THREE THEMES ON FIELD EXPERIMENTS AND ECONOMIC DEVELOPMENT

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ABSTRACT

We discuss the following three themes on the use of field experiments to study economic development: (1) We summarize the arguments for and against using experiments to gather behavioral data in the field; (2) We argue and illustrate that field experiments can provide data on behavior that can be used in subsequent analyses of the effect of behavioral social capital on economic outcomes; and (3) We illustrate that field experiments can be used as a development tool on their own to teach communities about incentives and strategic interaction.

1. INTRODUCTION

While there have recently been a considerable number of economic experiments run in developing countries, few have been run to answer questions pertaining directly to the development of the host countries.[1] We offer three thoughts on the use of field experiments to understand economic development. Our first theme is not new – we discuss the problems with basing analyses entirely on case study or survey data. However, this theme is important because we survey the opinions of a number of different authors and develop a large

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list of reasons for viewing experiments as complements to other empirical methodologies.

Our second theme is to offer a methodology for examining the links between behavior gathered in experiments and naturally occurring economic outcomes. To illustrate, in Section 3 we examine the connection between measured cooperativeness in a social dilemma experiment and economic well-being measured by individual monthly expenditures in the urban slums of Bangkok and Ho Chi Minh City.

Our last theme is more unconventional. After spending a considerable amount of time in the field conducting experiments with people who face social dilemmas in their daily lives, we have noticed that our experiments not only generate useful data, they also provide our participants with metaphors that they use in their daily lives. For example, people who live in rural Colombia and have participated in one of our common pool resource games tend to rely on their experience in the game when they discuss issues relating to their own extraction activities in the local ecosystem. To offer evidence that our experiments help generate prosocial norms in these communities (i.e. norms that bring outcomes closer to the social optimal when the social optimal differs from the Nash prediction), and therefore extract at more sustainable levels from the local commons, we argue that during subsequent visits people behave more cooperatively and this fact can not be explained entirely by selection (e.g. it is not the case that cooperators are the only ones who play again).

2. THEME 1 – MEASURING BEHAVIORAL PROPENSITIES

Ever since Smith (1982), economists have begun to look at experimental economics as a methodology, like econometrics, rather than as a boutique field in the profession. As this view continues to grow, researchers are realizing that experiments are just another way to gather data and that this particular method works well when incentives to reveal information truthfully are important. The theme that experiments complement other ways of gathering information about economically relevant behavior has also been widely advanced. We summarize these arguments with the hope of convincing development economists to consider experimental methods when information about individual behavior is sought.

Table 1 summarizes the literature on the reasons to use experiments to elicit behavioral information. Carpenter (2002) offers three reasons to supplement surveys with experiments. The first reason is that surveys often suffer from what most people call *hypothetical bias*, which means that people respond to situations

Table 1. Arguments Favoring the Use of Experiments in Behavioral Research.

Carpenter (2002)	Barr (2003)	Camerer & Fehr (Forthcoming)
Hypothetical bias	Control	Comparability
Idealized persona bias	Measurement	Replication
Incentive compatibility	Variation	
	Selectivity	

differently when the situation is hypothetical than when the situation is real. For example, in Carpenter et al. (2003) we note that 94% of Thai and Vietnamese survey respondents who report that a voluntary community project was organized in their neighborhood in the past year also respond affirmatively to the question, "Did you or someone in your household participate in those activities?" Taken at face value, this implies that there is no free-riding in these communities, which is clearly not the case based on the discussion we had with local leaders. This sort of bias is problematic because the effect is non-random (i.e. individuals are more likely to paint a rosy picture of themselves) and, therefore, it does not simply add noise to the data.

Hypothetical survey questions elicit bias for a number of other reasons which include what Carpenter (2002) describes as the *idealized persona bias* and the *surveyor effect*. The first bias, occurs when people respond to questions as the person that they wish they were rather than the person that they really are. The second effect, means that survey-takers often try to figure out what the researcher would like to hear and then respond in that way (or the opposite way). It is important to note that these biases are not restricted to surveys. For example, experiments can become contaminated when subjects react to the person running the experiment (the *experimenter effect*). However, the point is that these behaviors are often costly to the subjects in economic experiments, and they are not in surveys.

This leads us to the notion of *incentive compatibility*, which in this context essentially means that experimental participants often have an incentive to truthfully reveal private information (Smith, 1982). There are two benefits of incentive compatibility in experiments that have been used to measure the extent of other-regarding preferences in a population (see Camerer & Fehr, 2001; Carpenter, 2002) that we think are important: (1) at a minimum, paying participants based on what they do should make the task salient; and (2) in many experiments one must forego earnings to engage in non-selfish behavior. Considering the first benefit, Smith and Walker (1993) show that the variance in behavior falls when one compares experiments that are done hypothetically to those in which people are paid based on what everyone does (List & Lucking-Reiley, 2002; provide similar evidence from a field experiment). This fact indicates that payment, is

useful because it reduces noise in the data. Concerning the second benefit, most experiments based on an underlying game theoretic model assure that acting in one's self-interest will pay off in terms of maximizing expected monetary rewards. This is especially true in games that are dominance-solvable such as the linear public goods game. The implication of this fact is that it is materially costly for participants to engage in actions that are to the group's benefit (contributing in a public goods game) or that are to the group's detriment (rejecting offers in bargaining games). In this sense, many experiments used to measure other-regarding preferences help ensure that information is revealed truthfully, because in cases where preferred actions do not overlap with self interest participants must pay to behave pro- or asocially.

Barr (2003) focuses on the reasons that experiments generate data that are "cleaner," in the sense that they can be analyzed more directly and lead to clearer conclusions. The first benefit discussed by Barr is that experiments allow more control over the data generation process than surveys do. *Control* allows relationships to be identified and hypotheses to be separated by design rather than by statistical methods. Consider the classic identification problem: in naturally occurring markets demand and supply are observed together in a system of equations. Therefore, one can not identify the effect of price on the quantity demanded without controlling for the supply relationship. However, in the experimental lab the experimenter can exogenously change supply costs and isolate the demand relationship without worrying about endogeneity.[2]

Barr's second benefit of experiments is based on the observation that surveys suffer from *measurement* problems because they only allow us to gather data indirectly on preferences rather than on revealed or observed preferences. One example of this general problem is the hypothetical bias mentioned above. However, Barr also mentions the fact that measurement might be problematic when researchers have to infer preferences from past acts. To understand this idea, consider a situation in which the researcher is not particularly interested in the preferences of a group of people but needs to control for them in some other analysis. An example might be how altruistic people are. The researcher might survey current levels of charitable giving as a proxy for altruism, but there will surely be some residual difference between the unobserved variable, altruism, and charitable giving that will add noise to the analysis. Instead, the researcher could place individuals in a situation that allows them to actually make a donation (e.g. Cardenas & Carpenter, 2002; Eckel & Grossman, 1996). Further the experimenter can control the donation situation in such a way as to eliminate other explanations for giving (e.g. demonstrating one's social status).

A more practical benefit of experiments is what Barr (2003) calls *variation*: the fact that the experimenter can place individuals in a number of treatments

regardless of whether the treatments occur naturally. For example, imagine that a researcher is interested in whether microcredit programs actually improve living standards but microcredit associations only occur where there is enough homogeneity among community members. This means we can not attribute better outcomes with the institution because the institution is highly correlated with homogeneity. Instead, an experimenter (with deep pockets) could set up programs in a variety of neighborhoods and therefore generate treatments that would not have existed otherwise. Finally, Barr (2003) discusses the issue of *selectivity* which is the problem encountered in survey work where respondents are not randomized into treatments.

Camerer and Fehr (2001) discuss two benefits of experiments that are concerned more with the advantages of experiments over case studies. First, experiments with common protocols and experimenters can be compared across nations (e.g. Botelho et al., 2002; Croson & Buchan, 1999; Roth et al., 1991). *Comparability* is particularly important when juxtaposing experiments and case studies because it is almost impossible to identify causality using cases because the sample size is always one. The second reason to conduct experiments is *replication*. Not only can researchers compare experiments across cultures, they can also try to replicate them within cultures to check the robustness of previous results.

Harrison (forthcoming) contributes to this discussion by reviewing the general experimental literature on the magnitude of the hypothetical bias. An example of this work is illustrative. Imagine asking participants to state how much they would bid for a piece of art in a hypothetical second price sealed bid auction[3] and then compare that to how much people actually bid for the item in a real auction. Participants in real auctions bid approximately 40% of the stated, but hypothetical, willingness to pay of individuals in a hypothetical auction. This result suggests that there is a large difference in hypothetical values and real values.

While we encourage the use of economic experiments to measure behavioral propensities and norms, we realize that experiments are no panacea. Even the most celebrated feature of experiments – control – can never be perfect. Slight differences in protocols or frames, the location of the field lab (a school versus a church), the experimenter sex, race, or personality may all affect behavior (Hoffman et al., 1994; Kahneman & Tversky, 1984) and therefore one needs to be as careful as possible with the details of the experimental design.

In addition, experimenters are notorious for making inferences based on very small samples of 15 or 20 observations. The obvious advantage of surveys is that it is much easier to gather a large sample of responses. Likewise, while applied econometricians worry a lot about selection problems in survey data, little has been said about the selection problems associated with experiments. For example, are students who seek payment for their participation in an

experiment a random sample of the student population? This issue transfers to field settings as well. For example, in our own work (e.g. Cardenas, 2003b; Carpenter et al., 2003) we use experiments and exit surveys to examine the determinants of cooperation for people who face social dilemmas (e.g. extraction from commons or waste disposal) on a daily basis. However, all our parameter estimates are conditional on participation in the experiment. In other words, a complete analysis of cooperation in these communities would include a first-stage analysis of the process of deciding to participate or not and to do so we would need demographic and attitudinal data from a sample of community members who decided to not participate.

A final issue to consider is a version of the "in vitro" versus "in vivo" problem faced by biologists. This problem can be summarized by admitting that our experimental controls might remove other important behavioral determinants that are naturally occurring and would overwhelm or exacerbate whatever treatment effects we induce in the lab. This is essentially a problem of reducing complicated naturally occurring phenomena to manageable laboratory models while not knowing, a priori: (1) the relative magnitudes of the effects of different possible treatments; and (2) what all the possibly relevant treatments are. Along the same lines, while we suggest that conducting experiments in the field increases the external validity of the results, experiments are still novel events in most communities, and therefore, we must remain guarded in our interpretations of the data.

3. THEME 2 – THE IMPACT OF BEHAVIOR ON ECONOMIC PERFORMANCE

Experimental research in economics has concerned itself with the question of *why people behave as they do* while neglecting another question that might yield equally interesting, and perhaps more important, results. Specifically, our second theme recommends using experiments to ask, *how does behavior affect economic outcomes*? That is, instead of thinking of observed behavior as belonging on the left hand side of an analysis, why not use experiments to collect data that will subsequently be used on the right hand side of an analysis of economic performance, such as growth or health?

There has been a lot of related research on the link between individual and group characteristics, on one hand, and economic performance, on the other, which has been associated with the term *social capital* (e.g. Desdoigts, 1999; Knack & Keefer, 1997; Narayan & Pritchett, 1999; Putnam, 2000). Social capital is often broadly defined as the social aspects of society that facilitate transactions that

would otherwise be hard to contract for (e.g. work effort or collective action). More specifically, social capital typically refers to either the density of networks connecting individuals or individual norms or predispositions (e.g. trust and cooperativeness). Our claim is that much of the coevolving literature that criticizes the methods used in social capital research to measure behavior and analyze results (e.g. Durlauf, 2002a, b; Manski, 1993, 2000), and can be quelled by the adoption of field experiments. The reasons for this optimism include the fact that experiments: (1) incentivize participants, thereby potentially mitigating the hypothetical bias inherent in survey measures; and (2) produce less noisy and less biased measures of behavior. Experiments also allow us to control for factors that prevent the identification of relationships.

3.1. Behavior and Economic Outcomes

We have found only four examples of research that link behavior elicited in experiments to economic institutions or performance, and in only three of these studies does the implied causation run from behavior to outcomes. Henrich et al. (2001) analyze the links, at the societal level, between play in a simple bargaining game and how important cooperation is to production within a culture and how dependent people are on markets. In this case, they suggest that payoffs to cooperation and market integration determine the nature of fairness norms that evolve in societies. Specifically, societies in which the returns to cooperating in economic production are high (e.g. the Lamelara whale fishermen in Indonesia) and the level of market integration is high coordinate on fairness norms which require larger transfers from one player to another.[4]

Of more interest for our current purpose are the field studies described in Karlan (2002), Hoff and Pandey (2003), and Carter and Castillo (2002), who each use field experiments to measure behavioral propensities that are later used to predict economic outcomes. Karlan (2002) records play in a trust experiment and a public goods experiment. The players of these games are members of a group lending association in Peru, which is interesting because the author uses game behavior, in addition to a number of unspecified control variables, to predict individual default and savings rates in the year subsequent to participating in the experiment.

In the *Trust Game* (TG), a first-mover can send as much of her endowment as she wants to an anonymous second-mover. The second-mover can then return any amount that she wants to. The game is not trivial because transfers from the first- to the second-mover are tripled along the way by the experimenter, making the game a social dilemma. Sending money is potentially socially efficient, but the second-mover has no material incentive to return anything (Berg et al., 1995).

Karlan finds that players who return more in the trust game (which he interprets as being more trustworthy) repay loans at significantly higher rates and save more voluntarily. These results are also economically significant – a doubling of one's trustworthiness (from 25 to 50% returned) reduces one's default rate by 7%. Surprisingly however, he also shows that with an number of unspecified control variables people who "trust" more in the TG save less and drop out of the credit association more often, indicating that the trust component of the trust game may actually be a better measure of risk-seeking than trust.

Hoff and Pandey (2003) examine the impact of expectations on performance in a production task experiment. The purpose of the experiment is to test whether the caste system continues to form the expectations concerning social exchange of people in rural India, despite having been outlawed decades ago. In this experiment, 642 school children took part by solving puzzles for money; the more they solved, the more they earned. In the main treatment and with the flavor of the study conducted by Fershtman and Gneezy (2001), the experimenter announced the family name (and therefore the caste membership) of each participant at the beginning of the session. Hoff and Pandey show that introducing this information reduces the productivity of high caste members in a tournament setting and is debilitating for lower caste participants. In carefully constructed auxiliary treatments, they isolate two forces that drive this reduction in productivity: (1) for upper caste members, interacting with lower caste members reduces the intrinsic motivation to complete the task; and (2) for lower caste members, information on caste signals that the "game" is no longer fair and will be tilted to favor those with more class status. They figure, why try hard if the game is not fair?

These results are important because they not only show that caste affects expectations and performance, they provide an estimate of how big this effect is. In the main treatment, the relative performance of the lower caste members can fall by almost half when caste is announced indicating that the expectation of an unfair playing field causes lower caste member to, essentially, give up. Such an effect, if externally valid, would go a long way to explain existing differences in educational attainment and economic success. Furthermore, these results illustrate that expectations and norms can be very robust to changes in the legislated set of institutions. Just like behaviors have been slow to change in the United States and South Africa since the end of segregation and apartheid, one should not expect that outlawing caste in India will rectify the injustices suffered by the lower castes in the near future.

Lastly, Carter and Castillo (2002) compare experimental measures of trust, trustworthiness, and altruism from communities in South Africa to family per capita expenditures as a measure economic well-being. The hypothesis driving this study is the same as the assertion of Fukuyama (1995), that prosocial norms

like trusting and being trustworthy should translate into better economic outcomes because they allow transactions to occur in all instances even though contracts may or may not be enforceable.

We will briefly summarize the design and important results of the Carter and Castillo (2002) experiment, but leave the details to the readers of their paper. Their participants were recruited from 14 South African communities split evenly between urban and rural settings. The average participant was 43 years old and had six years of formal education. The authors had participants play both the TG and a similarly framed *Dictator Game* (DG). In the DG (Forsythe et al., 1994) the first-mover simply allocates any fraction of a fixed pie, of known size, to a second-mover. The second-mover has no say in the allocation and must, therefore, be content with whatever she is given. The reason for having participants play both games is that the difference between what one sends in the TG and how much one sends in the DG is a measure of a participant's un-confounded trust (after controlling for individual characteristics). That is, trusting motivations may be confounded by altruistic motivations in the standard TG.

Carter and Castillo realize that the norms they measure in their exit survey may be endogenous to economic well-being as measured by expenditures and, therefore, employ a two-stage approach for their analysis. In the first stage of their community-level analysis they instrument for a survey-based measure of associational social capital (however it is hard to imagine that the instrument is not also endogenous). In the second stage they regress expenditures on control variables, the predicted value of the associational measure and behavior in the game. These regressions suggest that, controlling for other influences, a 10% increase in median trustworthiness (in urban communities) as measured by experimental behavior translates into a 7% increase in living standards.

3.2. Endogeneity, Behavior, and Economic Outcomes (a detailed example)

Because we want to emphasize the link between outcomes and behavior we conducted our own version of the Carter and Castillo (2002) analysis using data from a *Voluntary Contribution Mechanism* (VCM) experiment. In the VCM participants contribute any portion of their endowment to a public good that benefits the entire group. In most versions of this game (i.e. in the linear game) contributing is dominated by free riding, but the social optimum occurs when everyone contributes fully. We conducted this experiment with 240 people who live in urban slums in Bangkok and Ho Chi Minh City under the assumption that behavior in the experiments would be a better measure of community cooperation than those elicited by surveys. We test whether there is a causal relationship

between the cooperative norms we measure in our experiments and people's living standards. The details of our communities, experimental design, and exit survey are presented in Appendices A–C.

Like Carter and Castillo, we use family expenditures as a proxy for economic well-being, and the two-stage least squares method to control for endogeneity between expenditures and cooperation. However, we adopt a semilog functional form (i.e. we only take logs of the dependent variable, expenditures) and, more importantly, we also search for an instrument for cooperative behavior that meets the exogeneity criteria. It is not hard to imagine a scenario in which cooperative propensities translate into better economic outcomes, but it could also be the case that high living standards can afford people the luxury of being more cooperative (i.e. they may be more willing to forego the free rider's payoff, Olson, 1965).

To begin our analysis we show that there is a positive relationship between cooperative predispositions and living standards. The details of the analysis are only worth worrying about if such a relationship exists. In Fig. 1 we graph this relationship for Bangkok (left panel) and Ho Chi Minh City (right panel). The hypothesized relationship clearly exists in the Thai data ($p = 0.02$), but the effect of contribution propensities on expenditures in Vietnam looks weak ($p = 0.54$), at best.

As mentioned above, we want to instrument for cooperation in our experiment to control for the possibility of endogeneity. However, the choice of a proper instrument is not easy because it needs to be correlated with contributions in the public goods experiment but it also needs to have no direct effect on expenditures. The second criteria ensures that there is no feedback effect (i.e. it should not be correlated with the error term).

In Appendix E we present the details of our estimation strategy and highlight the problem of finding good instruments in these situations. To summarize our procedures, we notice that there are structural reasons to believe that age and sex do not directly affect expenditures in our communities because unemployment is so high and many people engage in the production of handicrafts that are sold directly on the market. Given this environment, unless older community members or men receive different prices for their goods, incomes (and expenditures because people save little in these communities) will not vary systematically by age or sex.

Table 2 presents the results of our analysis in which the dependent variable is the natural log of the sum of an individual's surveyed expenditures on rent, entertainment, food, and transportation and we include fixed effects for the five communities in each location. We also include a variety of individual controls. In terms of standard demographic controls, we include years of schooling, whether or not a person owns her home, the size of the household, the number of years

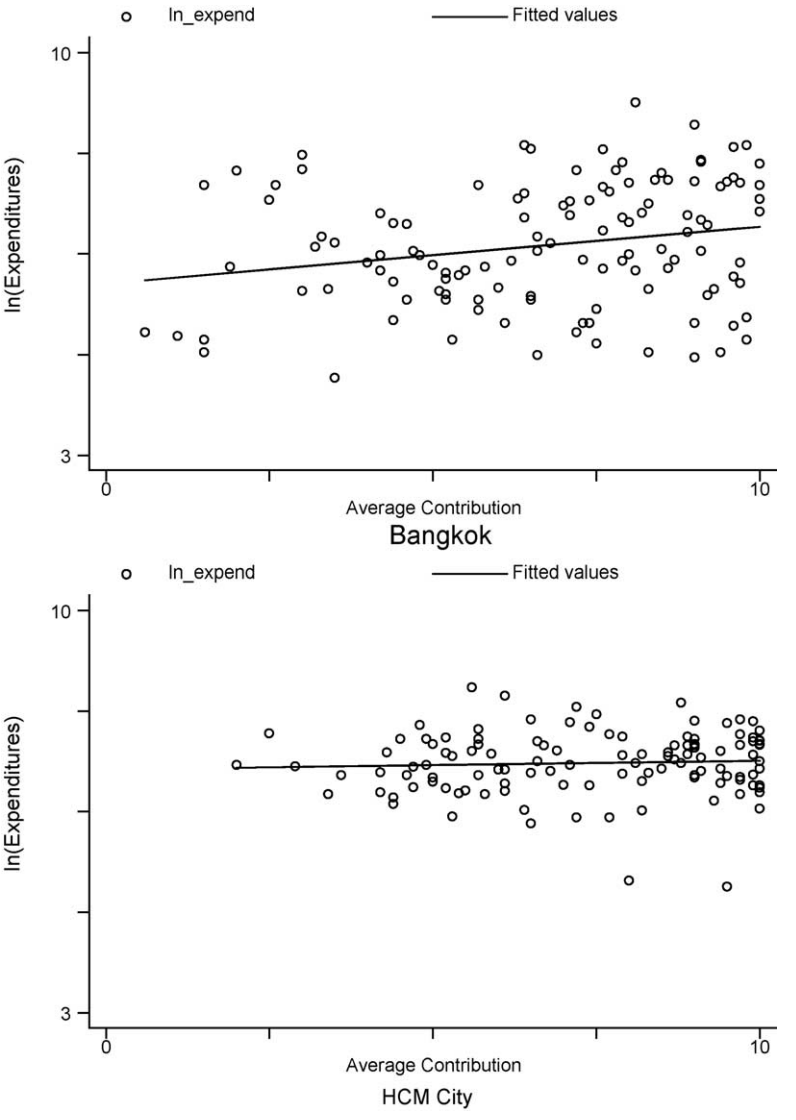

Fig. 1. The Uncontrolled Relationship Between Experimentally Measured Contribution Propensities and Living Standards.

Table 2. Dependent Variable is Natural Log of Expenditures.

	OLS		OLS		2SLS	
	BKK	HCM	BKK	HCM	BKK	HCM
Avg. Contribution			0.12**	−0.02	0.31**	−0.08
			(0.05)	(0.03)	(0.13)	(0.12)
Schooling	0.05*	0.03*	0.05*	0.03*	0.05*	0.04*
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Own home	−0.89***	−0.27	−0.77***	−0.28	−0.57*	−0.33
	(0.24)	(0.25)	(0.24)	(0.25)	(0.29)	(0.27)
Household size	0.02	0.01	0.03	0.01	0.04	0.008
	(0.04)	(0.02)	(0.04)	(0.02)	(0.04)	(0.03)
Residence	0.001	−0.003	0.002	−0.003	0.004	−0.004
	(0.01)	(0.004)	(0.01)	(0.005)	(0.01)	(0.005)
Homogeneous	−0.09	−0.16	−0.14	−0.19	−0.21	−0.29
	(0.25)	(0.25)	(0.25)	(0.25)	(0.27)	(0.31)
Cooperation scale	−0.15*	0.02	−0.15*	0.02	−0.15*	0.05
	(0.08)	(0.03)	(0.08)	(0.04)	(0.08)	(0.06)
Chat	−0.13	−0.02	−0.08	0.003	−0.01	0.07
	(0.13)	(0.11)	(0.12)	(0.11)	(0.14)	(0.17)
Describe neighbors	0.02	0.21*	0.01	0.21*	−0.01	0.20*
	(0.19)	(0.11)	(0.19)	(0.11)	(0.20)	(0.11)
Participate	0.005	0.003	−0.26	−0.04	−0.69	−0.19
	(0.38)	(0.24)	(0.38)	(0.25)	(0.50)	(0.37)
Leader	0.17	0.21	0.19	0.22	0.22	0.26
	(0.26)	(0.19)	(0.25)	(0.19)	(0.27)	(0.21)
Community fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	110	96	110	96	110	96
Adj. R^2	0.26	0.17	0.29	0.17	0.18	0.12
Hausman *p*-value					0.10	0.55

Note: Avg. Contribution is instrumented for with age and sex in the 2SLS model.
*Indicates significance at the 10% level.
**Indicates significance at the 5% level.
***Indicates significance at the 1% level.

the respondent has lived in the slum, and an indicator variable which takes the value of one when the respondent says that her community is ethnically homogeneous.

We also include a few standard social capital variables. *Cooperation scale* is the sum of three questions meant to measure the respondent's predisposition to cooperate, *Chat* is a likert scale response to how often the respondent chats with her neighbors, *Describe Neighbors* is another likert scale measure of whether the respondent thinks of her neighbors as strangers, friends, or family, *Participate*

takes the value of one when the respondent says that she (or another member of her family) has volunteered in the community within the last year, and *Leader* indicates whether or not the respondent was identified as a community leader.[5]

In the first set of regressions we show that many of our demographic control variables have the anticipated signs. Expenditures (and living standards) are increasing in educational attainment and significantly so in each city. People who own their own homes have lower expenditures, but only significantly so in Bangkok. This result makes sense given home ownership in these communities means one of two things: the homeowner has paid cash for the residence or the "homeowner" is squatting. In either case, the respondent pays no rent or mortgage. Expenditures are increasing in the size of the household, but the coefficient is tiny and insignificant in every case which probably picks up the fact that these people spend all their earnings regardless of family size.

The social capital regressors are not significant with two exceptions. The first relationship is interesting. The more like-family participants describe their neighbors in Ho Chi Minh City, the higher are their living standards. The second relationship is more puzzling. The higher people score on the cooperation personality scale, the lower are their living standards. However, this may make sense if they are more likely to be taken advantage of.

The next two sets of regressions illustrate our main results – cooperation measured in the experiment is associated with higher living standards in Bangkok but not in Ho Chi Minh City. Starting with the two-stage least squares results we see that our controls are mostly unchanged when we add our predicted value of cooperation, but in Bangkok, there is a significant effect of contributions on expenditures ($p < 0.05$) which supports the hypothesis that cooperative predispositions translate into better economic outcomes.

Notice that the *p*-value on the Hausman statistic is relatively large in both cases. Here the Hausman test asks whether the 2SLS estimates are systematically different from the OLS estimates that assume that the relationship is uni-directional from contributions to expenditures. The high Vietnamese *p*-value indicates that the OLS regressions are just as efficient as the 2SLS regressions. This makes sense because neither model fits particularly well with the Vietnamese data. However, the *p*-value is at the boundary of significance in the Thai case, indicating that there may be significant feedback from expenditures to contributions.

In terms of economic significance, cooperative norms in Bangkok have an effect that is similar in magnitude to the trust results found in Carter and Castillo (2002). Changing from a free rider to a contributor in our experiment is associated with a 3% increase in living standard.

Summarizing, we have seen three pieces of evidence that illustrate why it might be useful to examine the effect of measured behavioral propensities on economic

performance. We have seen that trustworthiness affects loan repayment, and savings rates in Peru, it affects living standards in South Africa, and cooperativeness affects living standards in Thailand. Before moving on, we also note that the lack of a formal theory of social capital hinders econometrically estimating the effects of social capital. For example, our correlations are weak in Vietnam, but his might be due to the fact that we are estimating the wrong reduced form.

4. THEME 3 – EXPERIMENTS AS PEDAGOGICAL TOOLS

Our third and final theme is that running experiments in the field can be important, not only for researchers, but also for the participants in the experiment.[6] When things go well, field experiments can play a pedagogical role by asking participants to reflect, in an interactive and strategic environment, on the problems that they face in their daily lives. Also, as the participants interact with each other in their local context, new norms, values, or attitudes may emerge concerning behavior in real social dilemmas. However, when things do not go particularly well, there is danger that interactions in experiments might leave participants with metaphors that might move their community further from a social optimal. Perhaps the important point is that, regardless of the experiment and its outcome we need to be more responsible in debriefing our participants because something is always left behind.[7]

As an illustration of a situation where we think participants have learned something useful from their experience in an experiment and debriefing workshops that follow the experiments, we will discuss our work in rural Colombian villages where the villagers depend economically and environmentally on the use of common-pool resources. We ran experiments and workshops during 2001, returned to the same villages several months later to run the same and similar experiments, and found that mean individual behavior shifted towards cooperation during the second visit.[8]

4.1. Our Experiment

As part of a study on cooperation in rural communities and the effect of different institutions on behavior, we ran a large number of experiments in several rural villages in Colombia. In these villages participants played a five-player *common pool resource* (CPR) experiment which modeled their local existence of extracting from an ecosystem for direct benefits while having to preserve the ecosystem to maintain other indirect benefits (e.g. prevent erosion).

The protocols for these experiments are provided in Appendix D. We ran games with 20 rounds divided in two stages. In each round players, in groups of five, had to choose a level of extraction from a CPR between 1 and 8 units. The incentives and payoffs were constructed so that each player had an incentive to over-extract (i.e. pick 8) at the symmetric Nash equilibrium, and the group as a whole had an incentive to extract the minimum (i.e. pick 1).[9] This incentive structure recreates a typical tragedy of the commons. During the first stage (Rounds 1–10) players had to make their decisions in a non-cooperative environment with no communication and the only feedback players received was the aggregate level of extraction.

In the second stage of each session (Rounds 11–20), the rules were changed and several new incentive structures were introduced. Some of these rules included material incentives (taxes applied to over-extraction or subsidies to resource conservation), voting mechanisms to apply regulations, and face-to-face communication (See Ostrom et al., 1994 for an extensive experimental exploration of different institutions within a common-pool resource design). Because we are interested in the change in behavior between the two visits, we restrict our attention to the first 10 periods which were conducted using identical procedures during both visits.

4.2. The Samples

We returned to three of the same villages we had visited before to repeat experiments and to conduct a few new experiments with variations in the rules at the second stage.[10] The time difference between the first and second visit varied. Table 3 summarizes the two visits for each of the three villages.

The recruitment for the second visit was made through the same channels we used in the first visit: local leaders and NGOs located in the field who had been

Table 3. CPR Experiments in the Field.

Villages	First Visit			Second Visit			Months After 1st Visit
	Date	Number of Players	Sessions ($n = 5$)	Date	Number of Players	Sessions ($n = 5$)	
Sanquianga	May 2001	130	26	Aug 2002	80	16	15
La Vega	Aug 2001	130	26	Feb 2002	50	10	6
Neusa	Mar 2001	140	28	Dec 2002	30	6	20
Totals		400	80		160	32	

interacting with these communities for some time. Upon arrival, we would spend a day or two spreading the word around the village. The invitation was made to all adults who were part of households that depended, to any degree, on the extraction of resources from the surrounding forests or ecosystems.[11] Further, when asked if it mattered whether potential participants had participated before, we showed no particular preference but invited people to tell others that had not come during the first visit to participate as well. We suspected that this would open a process of dissemination of information from "experienced" players to "fresh" ones, although the time between the visits – six months for the shortest case and 20 for the longest – might reduce this.

4.3. The Experimental Data

Recall that the decision variable, x_i was the level of extraction by player i, where $i = 1, 2 \ldots 5$, ranged between 1 and 8 units, and that the symmetric Nash equilibrium was achieved when $x_i = 8$, and that the social optimum could be reached if $x_i = 1$, for every player in the group. At the Nash equilibrium the individual earnings in one round would be Col$320, while at the social optimum every player would earn Col$758; however, a player wishing to deviate and extract 8 units when everyone else chose the social optimal level of extraction would earn Col$880 instead.

Consistent with previous data on similar experiments, at the group level one observes neither a convergence towards the Nash equilibrium nor towards the social optimum. Within groups we observe that there are a variety of strategies and types of players choosing cooperative and individualistic levels of extraction. Therefore, the social efficiency achieved during this first stage is somewhere in between the two benchmarks.

The distribution of decisions (level of extraction) is shown in the panels of Fig. 2. The first column illustrates behavior from the first visit. The second column shows behavior from the second visit. The first row is the data aggregated across all three villages and each separate village is depicted in the rows below the line.

Clearly there is a change in behavior between the two visits. We can see that the fraction of high levels of extraction is reduced, and the fraction of decisions in favor of a group-oriented outcome are increased. The Wilcoxon and Mann-Whitney tests for differences in distributions between the first and second visits confirm that the aggregate data distributions are different, and at the village level, only in the case of Sanquianga (denoted S) do we fail to reject the null hypothesis. The case of Sanquianga will be elaborated on later.

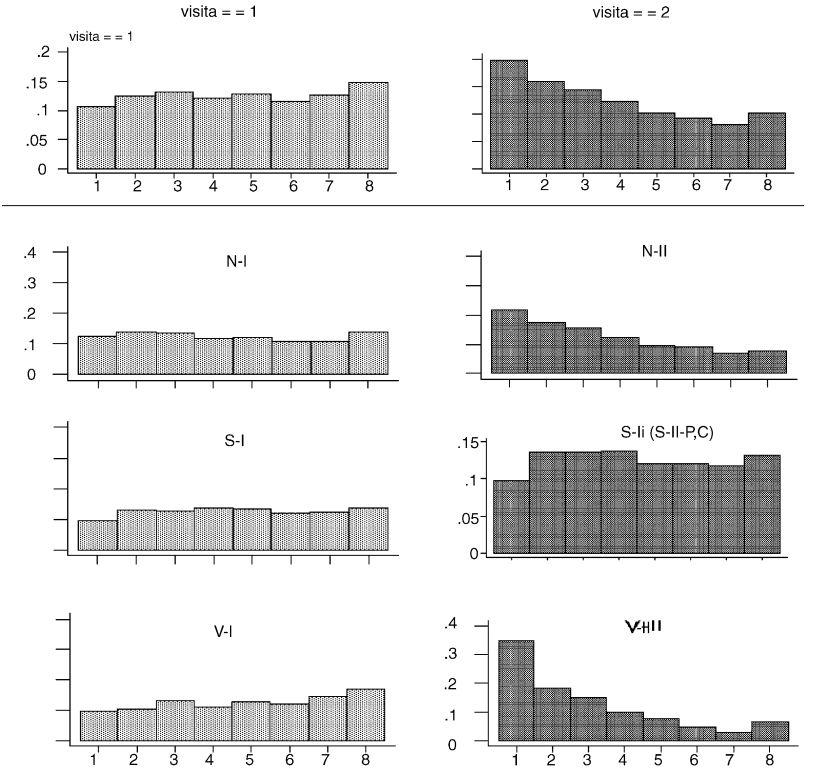

Fig. 2. The Distribution of Extraction Decisions. *Note:* Left side is first visit, right side is second visit. The top histograms are pooled data, below are across the 3 villages.

4.4. The Community Workshops and Behavioral Shifts

It is important to note that, one or two days after we concluded the initial series of experiments, we invited the participants and others interested, to be part of a workshop in which we presented our preliminary findings and discussed the similarities between the experiments and the economic activity of the villagers. During these workshops a great deal of debate was generated about what the best strategy was for the group and for each individual during the game. However, participants would also link play in the game to extraction activities they face in reality. Clearly the workshops allowed many opinions to be shared and contrasted and the discussion invariably refocused on issues relating to the community use of the local commons. We believe that these workshops may have a role in explaining

the differences between visits. That is, we hypothesize that the experiments and workshops provided mechanisms that clearly illustrated, and fostered pro-social behavior in these communities.

In addition to the data presented in Fig. 2, we also have anecdotal evidence that after the experiment and the workshops villagers continued to discuss their experiences, their strategies, and the consequences of those strategies. However, we do not know whether such discussion spread through the village and was internalized by the rest of the people that eventually ended up participating during the second visit, or it was only at the moment of recruiting that the norm was spread by the experienced participants.

Obviously, there are alternative explanations of the shift in behavior that have nothing to do with the evolution or reinforcement of cooperative norms. We will discuss two of them. First, the shift towards cooperation might simply be the result of selection. If, for whatever reason, cooperators are more likely to play the game again, the shift towards cooperation during the second visit might simply be the results of non-random sampling. To test this alternative explanation we first note that the second visits were roughly evenly distributed between repeat players and newcomers, overall. Of the 30 players in Neusa, 20 had participated before, 23 of the 50 participants in La Vega had participated during the first visit, but only five of 80 participated before in Sanquianga. If selection is driving the difference between visits we expect to see two things in the data: (1) repeater behavior should be distributed more cooperatively than first-timer behavior; and (2) first-timer behavior in the two sets of experiments should be the same. The first conjecture says that cooperators are more likely to play again and the second conjecture says that there are no dissemination or prosocial effects (i.e. selection explains all the difference).

Concerning the first conjecture, Fig. 3 shows the distribution of decisions for these two types of players at the second experiment. Although nonparametric tests conclude that the two distributions are different (i.e. mean extraction is slightly lower among repeaters), clearly in both cases there is a strong mode at the social optimum indicating there are significantly many cooperators among the first-timers. This suggest that selection is not driving our result. Further, Fig. 4 indicates that the second conjecture is also incorrect. If we restrict our attention to only the inexperienced players, the people playing during the second visit are significantly more cooperative.[12]

Another possible explanation for the shift in behavior that we see is that when we showed up in these villages the second time and announced that there would be another round of experiments, we changed our participant's orientation from one-shot game mode to repeated game mode.[13] Seeing us a second time may have made villagers ask themselves, "Are these guys with money going to keep coming

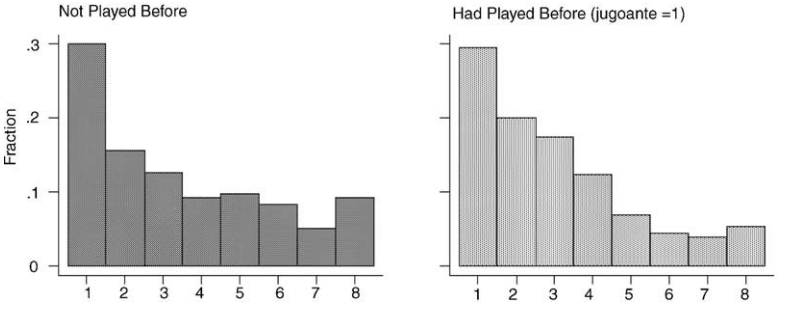

Fig. 3. Is Selection a Factor in Behavior During the Second Visit?

back here and if they are should I be more cooperative?" One must admit two things about this alternative. One, this hypothesis would endow our participants with a lot more strategic sophistication (and lower discount rates) than is typically seen among experimental participants[14] and two, such a hypothesis is consistent with Figs 3–5. If our participants are sophisticated, they may reason that more cooperation is warranted in a repeated game with uncertain endpoint which is what we see in Fig. 2. Likewise, the re-orientation should motivate both repeaters and first-timers to be more cooperative as in Figs 4 and 5.

Pl. check for figure 5 cited in text.

We also have one bit a evidence that suggests that the more powerful explanation is that repetition affects social preferences. This evidence comes from a cross national experiment we conducted with students in Middlebury, Vermont and Bogotá, Colombia. In this experiment (Cardenas & Carpenter, 2003) participants played a standard CPR game for 15 periods and then were allowed to donate any portion of their earnings to real conservation funds. When we regressed the fraction of one's earnings donated on one's extraction level at the end of the game, we find a significant correlation (controlling for other factors) that indicates that cooperative behavior in the CPR stage is associated with more generosity in the donation stage.

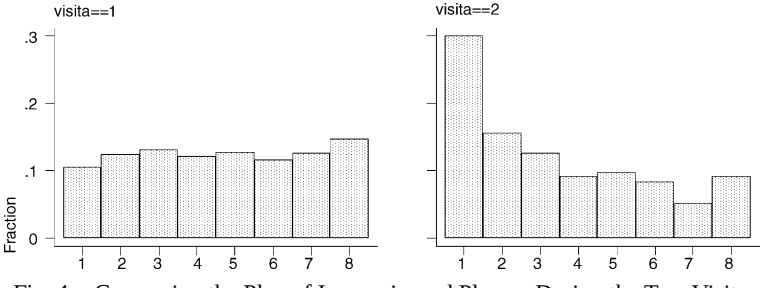

Fig. 4. Comparing the Play of Inexperienced Players During the Two Visits.

However, the repeated game hypothesis is not inconsistent with our hypothesis that exposure to the game affects community norms, it simply places emphasis on one specific mechanism. Our conjecture is that playing the game and participating in the workshops after the games shed light on the institutional and strategic dimensions of dilemmas that these villagers encounter in their daily lives. Their participation gives villagers a venue in which norms are clarified, reinforced and/or developed. There are a number of microfoundations for this phenomenon. One foundation is based on the rationale of the folk theorem and might be triggered either by each participant realizing she will interact with the other participants for the rest of her life or by the fact that we come more than once to conduct experiments. Another possibility, the one we favor, is that prosocial norms are fostered by participation because interactions near the social optimum reinforce other-regarding or social preferences (e.g. altruism) among the villagers. The point is that strategizing from a repeated game posture is consistent, not inconsistent, with the development of norms of cooperation.[15]

4.5. Sanquianga

As one can see in Fig. 2, behavior in Sanquianga diverges from the other two villages, but there were also many fewer returning participants. In this village households are spread along the banks of a mangrove forest in Sanquianga National Park in clusters of tens or hundreds of households. Recruitment consisted of inviting a few participants from each beach. Also, during this second visit we targeted the population of fishermen that depended on resources such as fish and shrimp while in the first visit we had focused on households depending on mollusks. Therefore, we have two possible explanations for the difference between this village and the other two. The norms that could have emerged from discussions following the experiments and workshops after the first visit did not reach others who are more geographically isolated, or there is less communication and fewer interactions among households that depend on different resources.

5. CONCLUDING THOUGHTS

We see our three themes as methodological recommendations for those studying the problems of economic development. Very roughly speaking, one purpose of development economics is to seek changes through economic policies and institutional designs that induce socially desired behaviors by agents. These behaviors, in turn, ultimately produce aggregate outcomes that reduce poverty and

increase the well-being of most of the population. At the core of the development task is the understanding of individual behavior and behavioral responses to institutional changes. The growing behavioral and experimental work on central issues that relate to individual decision making and development issues such as attitudes towards risk, preferences for the environment, a willingness to voluntarily contribute to public goods, or preferences that include the outcomes of others, can greatly complement the new work on micro-foundations of development economics that has emerged around the issues of norms, asymmetric information, and transaction costs in development (see Bardhan & Udry, 1999; Hoff & Stiglitz, 2001 for example).

The recent work by development economists such as Duflo (2003) are recognizing the need to incorporate elements from behavioral economics into the study of why the conventional economic model of rationality cannot fully account for the data gathered in the field on the decisions made, for instance, by the rural poor. She even calls for more carefully designed real and natural experiments outside of the university lab to better understand why the "poor but neoclassical," or the "poor but rational" models still fail to explain behavior and outcomes in developing countries.

Modern textbooks in development economics have begun to discuss some of the key micro-foundations of economic decisions and outcomes when there are asymmetries of information in, for example, credit or land contracts that create inefficiencies. These texts are also beginning to recognize the importance of factors like social norms and the relevance of strategic interaction, and some even include short introductions to game theory to study development problems as ones of strategic interactions (see Ray, 1998). Risk, for instance, is often incorporated in the current teaching and policy making in development, although it is far from settled in the behavioral and experimental literature how risk exactly affects economic behavior (or how best to measure it). The same can be said when considering the cases of including other-regarding preferences, a central issue in the analysis of the social dynamics among the poor, or in the study of attitudes of individuals about discounting the future – the latter issue being critically important for evaluating development policies and infrastructure projects.

Experimental and survey-based work demonstrates that institutional, demographic or incentive factors can widen the dispersion of behavior with respect to individuals discounting future outcomes, and this phenomenon has consequences for the study of development and therefore for the evaluation of benefits and costs of projects (see Harrison et al., 2002). Correlating experimental measures of risk aversion and discount rates (a la Barr & Truman, 2000; Binswanger, 1980; Kirby et al., 2002) might answer old but still unsettled debates about the rationality of "peasants" such as the claim that people in developing

countries are poor because they have higher discount rates. This may also dovetail with the development myth that poor people are poor because they are "too fair" which prevents the differential accumulation of capital and growth.

Likewise, the current debates in behavioral and experimental economics over the psychological effects of distributive allocations and fairness in choices and outcomes can clearly have implications for modeling and evaluating the role that the persistence of inequality has on development. The approaches suggested here could help in the incorporating of these elements in the study of individual preferences and the microeconomic foundations of the modern theories of development where individuals are modeled for many of the cases as self-regarding optimizers within a context of incomplete information, risk, and missing credit or capital markets (Ray, 2000).

Much of the experimental evidence surveyed here shows that in settings that differ substantially from both the student lab and the developed or industrialized world, in general, there are certain regularities about economic behavior that are not necessarily in line with some of the assumptions at the foundation of conventional development economics. Further, exploring the possibility to explain economic outcomes with economic experiments (e.g. income, expenditures or social outcomes), offers the ability to conduct controlled analyses at the individual level. For instance, calibrating development policy models according to certain cultural or social norms that can be discovered through experiments can allow development projects to better allocate scarce resources. An example is the design of policies that make better use of the predispositions of many individuals to engage in cooperative or collective actions that would augment the social efficiency of intervention efforts.

Furthermore, as participatory research methods have demonstrated in many previous instances, the possibility of beneficiaries of development projects to get involved in the research makes them more intrinsically motivated stakeholders in the resulting projects. Experiments may be a key way to engage in such programs and motivate stakeholders. Our preliminary analysis showing more experimental cooperation in villages we revisited months after conducting a first set of experiments suggests that patterns of community behavior can respond to these sorts of participatory research.

While we have identified three themes to discuss in this paper, other important themes exist and should be explored in future work. For example, the World Bank has recently begun to think hard about the role of culture in economic development (see Rao & Walton, 2004). Although there has also been a spate of experimental work that tests for nation-level differences in student behavior (e.g. Ashraf et al., 2003; Croson & Buchan, 1999; Roth et al., 1991), we need to resist conducting more cross-national experiments as the basis for cross-cultural claims. One of the

benefits of Henrich et al. (2001) is that the researchers examine differences in behavior by rather distinct cultural groups so that behavioral differences could be attributed broadly to "culture."

Another theme worth exploring is the use of experiments as a test bed for new institutions aimed at development goals. Efforts in the design of market institutions in the industrialized world using experimental methods find examples in the areas of electricity markets, auctions and labor markets as in the case of entry level market for medical doctors (Roth, 2002). For the case of development in poor regions, the idea is to test and revise institutions on a smaller scale before full implementation. Initiating institutional changes in a small field pilot allows policy makers to examine the allocative efficiency of the program and the individual response to the change in the incentives, before incurring large setup costs. This theme is developed rather well in McCabe (2003) and implemented in Tanaka (2003) who experimentally examines differing mechanisms for land consolidation as a means to inform real consolidation attempts in eastern Europe.

Another idea that one could explore is the testing and implementing a program to build on the lessons we have learned from our second visits to villages where experiments have been conducted in the past. We might push for a more systematic follow-up of longitudinal cooperative experiments in the field to build, sustain and introduce effective norms of pro-social behavior. For instance, with only three villages it is difficult to explore the weight that the time in between the two visits could have had on the change in behavior towards cooperation. Also, it could offer an interesting setting for exploring the cultural evolutionary capabilities of a few cooperative "mutants" to spread a norm of cooperation and how well such a norm could survive in a population with other, less prosocial norms.

Testing these behavioral regularities using experimental methods across institutional settings according to asymmetries of information, endowments or power, or for different types of interdependences across agents, have proven to be valuable, and could complement the progress that development economics has made in the recent decades in the modeling of strategic interactions among social actors. Further, these apply not only to the economic actors that benefit or suffer from the search for development, but also for the case of the social planners where the same behavioral assumptions can be made. Experimental approaches could enhance the now vast empirical base from field case studies and surveys that this area of study has used for decades. Behavioral foundations from experimental data can allow us to design better and more realistic models of rationality where information and human data processing capacity are limited, where preferences are more rich, and where the context or the institutional setting affects the valuation that individuals make of their options and constraints.

NOTES

1. For a review of this literature see Cardenas and Carpenter (2004).
2. This point is also made in Kinder and Palfrey (1993) in the context of the experimental study of political institutions and behavior.
3. This mechanism is also known as the Vickrey auction. The winner is the highest bidder but she only has to pay the second highest bid.
4. However, we should note that this analysis does not allow for the possible endogenous nature of fairness norms and market integration or payoffs to cooperation. For example, it might also be the case that fairness norms allow people to achieve higher payoffs to cooperative enterprises instead of the other way around.
5. See Carpenter et al. (2003) for a more detailed description of these variables.
6. To one degree or another this point has previously been made in Plott (1987).
7. Another setting in which this theme is even more salient is conducting economic experiments with children.
8. This discussion is based on the experiments conducted for Cardenas (2003a).
9. Participants were paid in cash, and, on average, earned US$5. This was a substantial amount of money to our participants.
10. However, as always, the new rules were announced only after the first stage of 10 rounds was finished.
11. In the case of Sanquianga we invited households that depended on firewood, mollusks, shrimp and fishing from their surrounding mangrove forests; in the case of La Vega we invited households that depended on firewood and water from the microwatershed of the village; in the case of Neusa households engaged in water extraction and trout fishing in a major water reservoir in the village.
12. The first of these two facts also suggests that an explanation offered by one of the referees that returning players tried to get new players to be cooperative to take advantage of them might have some traction, but the effect is small.
13. One of our reviewers offered this alternative.
14. See the discussion of strategic sophistication in Camerer (2003) and the survey of individual discount rates in Harrison et al. (2002).
15. Remember Axelrod (1984).

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APPENDIX A: CARPENTER ET AL. (2003) COMMUNITY DETAILS

Communities in Bangkok

Community 1
Geographically distinct section of famous Klong Toey slum located on a huge swath of land surrounding the Port of Thailand. The area has a large number of neighborhood-based NGOs including the Duang Prateep Foundation (founded by a Magsaysay Prize recipient living in the community) working to improve the physical conditions and community residents.

Community 2 (Ruam Samakkhi)
Located in a newly (last five years) urbanized section of inner Bangkok, along a small very contaminated klong (or canal). The entire community sits about six feet above the surface of a canal, a position that is maintained through the use of concrete stilts; brackish water sits below the housing structures, emanating odors into and around dwellings.

Community 3 (Trak Tan)
Located outside of central Bangkok in the adjoining province of Samut Prakan but the area around Trak Nan is entirely urban. Most of the land is owned by a variety of entities including a nearby Buddhist temple and private landlords but wealthy households have begun to build large, impressive homes in the midst of the crowded lanes. Solid waste is a major issue and garbage is everywhere; rats appear to be the most aggressive, problematic form of vermin in this community. This community is the wealthiest slum and has the largest average household size of all five slums.

Community 4
Located on the north and south of a major road (soi) running through downtown Bangkok. The housing stock is particularly poor in quality, and mostly composed of wood. Standing water and garbage is clearly common beneath the houses. The community's central location in Bangkok means that the value of real estate is quite high, therefore, the likelihood of eviction seems greater than at the other four locations.

Community 5 (Sin Samut/Prachatipat)
Located in suburban Pathum Thani province. Residents are dispersed in an almost rural environment along the banks of a large klong full of plants and animals.

Within the slum there are at least two distinct areas, differentiated by age and land ownership although both groups are very poor and earn significantly less than households from the other four settlements. The first settlement, which resides upon land owned by the Irrigation Department, is about 20 years old. The second settlement, existing for around 30 years, occupies land that was recently transferred from a member of the royal family to an insurance company. Both communities are actively being threatened with eviction. Intervention on the part of the Department of the Interior has given slum members the opportunity to purchase property through their savings groups. They are in the process of trying to assemble the required down payment. Unfortunately, there is not enough space to accommodate all the households even if all of the members of both communities were interested in moving there. Specific households – those living on land owned by the Irrigation Department – have been given the option of moving to other sites owned by the Housing Authority. There is considerable resistance within the community to this second option, because the land is distant, the residents must pay for the land, and they would need to find jobs in the new area, which would likely be difficult to do. In fact, a group has formed to resist attempts to move the community from along the edges of the canal.

Communities in Ho Chi Minh City

Community A (Tan Dinh)

Located in the central district (ancient Saigon) in a single triangular-shaped city block. The community is close to the Tan Dinh Market, a scene of much economic activity both day and night. Some residents have lived there since prior to the war but others (mostly recent migrants) live around the market without any permanent dwelling. The housing pattern is extremely dense; a mix of materials including plaster, brick, tile and cement with the occasional tin roof or siding. Quality of housing structures seems high (many consist of two stories) but conditions are extremely crowded with little floor area available per household. Despite high density, communal alleys and walkways are kept clean and most residents appear to have toilets/septic tanks as well as daily access to garbage collection.

Community B (District 2)

Bounded on one side by the Saigon River and on the others by rice fields, District 2 was recently rezoned by the City's People's Committee as urban land. The area remains relatively isolated and rural with no current access by car; work is underway on a highway that cuts through rice fields owned by community members

that will allow quick passage into the city across the river. While most households are very poor rice farmers and own simple wooden homes with roofs made of palm fronds, some community members have sold land near the planned highway and are constructing very large, modern plastered houses. Public services within the community are quite limited, even for the wealthier households. Most houses have piped water and electricity but there are few indoor toilets and garbage collection is unavailable. The community relies on public outdoor toilets that release waste into swampland; each household has a garbage pit in which to dispose of solid wastes.

Community C (District 8)
Located on one side of a small island that is formed by the meeting of three canals. Community uses a deteriorated wooden bridge to cross the canal; very poor housing conditions. The structures are predominantly one storey and few improvements have been made to the wooden and corrugated tin exteriors. Community resembles Bangkok because it is very urban in character, dilapidated in terms of built structures, has narrow pathways, and borders a canal full of garbage. Interesting array of small industry, including an industrial laundry, cottage shoe production and a small open-air market where merchants sell goods under thatched umbrellas. Little garbage collection.

Community D
Situated at the periphery in southwest Ho Chi Minh City in the portlands of the city where many migrants have moved to the city over different time periods. Streets and alleys are extremely old and narrow amid high-density warehouses. Appears homogeneous (primarily two stories high, plaster coated with many shared walls) with little evidence of any new construction. The People's Council suggested this slum because the basic infrastructure of the community is in a terrible condition. There are two lively street markets located on either end of the community selling primarily processed and unprocessed foods, some of which are made and sold by women of the community. Many of the men from this community find more or less regular employment in the port or nearby harbor.

Community E (Taan Binh)
Situated in the northeast area of Ho Chi Minh City – a peripheral zone that until eight years ago included agricultural land and activities. Most of the residents migrated from rural areas, and constructed their houses upon land that used to be a cemetery. There is great variety in housing styles and quality and differing access to piped water, electricity and drainage/sewage connections. Two canals

flow through this community and, while regularly dredged, are full of garbage and black water. Area is urbanizing very quickly and is rapidly becoming very polluted. The causes of deterioration include construction of dwellings without adequate planning, lack of a drainage system, and the direct disposal of garbage into canals as well as the operation of small-scale industry (especially in terms of dust, smoke and chemical agents).

APPENDIX B: CARPENTER ET AL. (2003) EXPERIMENT INSTRUCTIONS (THAILAND)

Thank you for participating in our study today. There will be three parts to the study: Exercise 1, Exercise 2, and an interview. For your participation you will be paid. The amount you will get paid depends on the decisions you and everyone else make during the exercises. You will be paid an additional 20 baht (US$ 0.50) for the interview at the end of the study. The money to conduct this study has been provided by a social research institution in the United States.

Any decisions you make in the exercises or responses you give during the interview will be strictly confidential. We will never tell anyone your responses or choices. To assure your responses are confidential, we ask you to not speak to each other until the entire study is completed.

Instructions for Exercise 1

To understand Exercise 1, think about how you allocate your time. You spend part of your time doing things that benefit you or your family only. You spend another part of your time doing things that help everyone in your community. For example, you spend part of your time doing things that only benefit you or your family and another part of your time doing things that benefit the entire community.

Specifically, you might spend part of your time hauling or purifying water for your family and you may spend part of your time cleaning or maintaining the community water supply which benefits everyone including you. Another example is that you spend part of your time working for pay or fixing your house. This activity only benefits your family. However, you might spend part of the time cleaning up the neighborhood which benefits everyone.

Exercise 1 is meant to be similar to this sort of situation where you must decide between doing something that benefits you only and something that benefits everyone in a group. There will be five decision making rounds. There are three other people in the group with you.

At the beginning of Exercise 1 we will give you an envelope to keep your money in. Keep this envelope with you at all times. At the beginning of each round everyone in the group will be given 10, 5 Baht coins. Each person in the group will then decide how many of these 10 coins to allocate to a group project and how many to keep from himself or herself. Everyone in the group benefits equally from the money allocated to the group project, but only you benefit from the money you keep.

We have designed both exercises so that you can make your decisions privately and so that no one else will ever know your choices. One at a time, you will come to a private location with your envelope and your 10 coins. Once there, you will allocate as many coins as you want to the group project. You will keep the remaining coins and put them in your envelope.

When all four members of the group have decided how many of the 10 coins to allocate to the group project, we will add up all the money. When we know the total, we will double it. Each person will then receive an equal share of the doubled amount. To distribute the proceeds from the group project for the round each person, one at a time, will return to the private location. When you are at the private location we will show you a card. On this card we will write how much each person in the group allocated to the group project but you will not know how much any specific person allocated to the group project.

We will also give each of you your share of the group project. Put your share in your envelope; it is for you to keep. Each person receives an equal share of the doubled amount regardless of how much money he or she contributed to the group project.

Here is an example to illustrate how the exercise works. Each person decides how much to allocate to the group project privately, so you will not know what anyone else has decided when you make your choice. Imagine that on the first round everyone in your group, including you, allocate 5 coins to the group project. In total there are $5 + 5 + 5 + 5 = 20$ coins in the group project. This is equal to 100 Baht. We will double this amount which makes the total 200 Baht. Each of you then receives an equal share of the 200 Baht. We would give you each 50 Baht. At the end of round one you will have 50 Baht from the group project and 25 Baht that you kept. You will have a total of 75 Baht in your envelope.

To continue the example, now say that it is the second round. Everyone in the group receives another 10 coins at the beginning of the round. Imagine that this time you allocate no money to the group project. Imagine that the other three people in your group allocate 5 coins to the group project. In total there are $0 + 5 + 5 + 5 = 15$ coins in the group project. We double this amount which makes the total 30 coins or 150 Baht. Each person receives an equal share of the 150 Baht.

Because we will only use 5 Baht coins, we will always round up to the next highest number that can be divided by 4. Four can not divide 30 evenly so we will round up to 32 coins or 160 Baht. This means you each would receive 8 coins or 40 Baht from the group project. At the end of round two you will have 40 Baht from the group project and 50 Baht that you kept. You will add another $40 + 50 = 90$ Baht to your envelope. In total you will have $75 + 90 = 165$ Baht in your envelope.

The rest of the group will also receive 40 Baht from the group project. In total, each of the other three group members will add $40 + 25 = 65$ Baht to their envelopes. They receive 40 Baht from the group project and have 25 Baht that they kept.

Let's continue the example for one more round. Everyone receives 10 coins at the start of the third round. Now say that you and two other players allocate everything to the group project and keep nothing. Say that the fourth group member allocates nothing to the group project. The group project will have a total of $0 + 10 + 10 + 10 = 30$ coins in it. We double this amount which makes the total 60 or 300 Baht. Each person receives an equal share of the 60 coins. Each person receives 15 coins or 75 Baht from the group project.

At the end of round three, you and the other two group members who allocated all 10 coins to the group project receive 15 coins from the group project. The fourth group member who kept all 10 coins adds the 10 coins she kept to the 15 coins she receives from the group project. In total she receives 25 coins or 125 Baht.

In total you have 75 from round $1 + 90$ from round $2 + 75$ from round $3 = 240$ Baht in your envelope at the end of round 3.

This is only an example. You will play 5 rounds and each of you will decide, on your own, how to allocate the 50 Baht you start each round with. Any money in your envelope at the end of the fifth round is yours to keep.

It is important that you understand how the exercise works. Are there any questions about how the exercise will proceed?

Instructions for Exercise 2 (Only to be Handed Out After Exercise 1 has been Completed)

Exercise 2 is very similar to Exercise 1, but there will be one difference in the procedures. The first part of each decision making round will be exactly the same as Exercise 1. There will be 5 decision making rounds and you will each receive 10, 5 Baht coins at the beginning of every round. You will each go to a private location and decide how much money to allocate to the group project and how much to keep. When everyone in the group has made this decision, we will calculate the

total contribution. We will then double the total contribution. Each person will receive an equal share of the doubled amount.

The only difference between Exercise 1 and Exercise 2 happens when you return to the private location to receive your share of the group project. We will let you see the card that shows how much each person in the group allocated to the group project and we will give you your share of the group project as in Exercise 1. However, Exercise 2 is different because you will also be given the chance to send a message to the rest of your group.

If you give us 1 Baht you can send a message to the rest of the group. You may send this message if you are unhappy with how many slips of paper the other people in your group are allocating to the group project. The message will be this picture (show the picture that is below). When you see this picture, you know that one of the group members has spent 1 Baht to tell the rest of the group that she is unhappy with the number of slips that were contributed by the other group members.

– unhappy face –

We will display any messages at the beginning of the next decision making round. When you come to the private location to choose how much to allocate to the group project, you will see any messages sent from someone at the end of the previous round.

At most you will see four messages if everyone sent a message. Here is an example. Imagine at the end of Round 6 you go to the private location to pick up your share of the group project and you see that everyone else in your group allocated more or less than you did to the group project. If you do not like this, you can spend 1 Baht to have the picture displayed at the beginning of the next round. When you go to the private location to decide how much to allocate to the group project during Round 7, you, and everyone else in the group will see the picture that you spent money to display.

Anyone who decides to send this message will do so anonymously. Nobody will know who the person was that sent the message. After everyone has seen the messages, we will take them down. You will have to spend 1 Baht at the end of each round if you want to continue to send a message to the group.

This is only an example; you will make the decision to spend 1 Baht to send a message to the group.

The rest of Exercise 2 is identical to Exercise 1. After each group member receives her share of the group project and decides whether or not to send a message to the group, she will return to her seat. When everyone has made this decision the decision making round is be finished.

Are there any questions about how the exercise will proceed?

APPENDIX C: CARPENTER ET AL. (2003) EXPERIMENT SURVEY

Experiment Date:
Community:
Group Number:
Player Color:

Record the participant's sex. Male or Female

1. What year were you born? 19__
2. How many years of schooling have you completed? _____ years

3. Does your family own its own house?	Yes	No	No Answer
	1	0	−9

4. How many people are there in your household (including you)? _____
5. How long have you lived in this community? _____ years

6. When new people come to your community, do they mostly come from the same village or region or do they come from many different places?	Same	Different	No Answer
	1	0	−9

7. Please tell me how much of a problem each of these issues is to you on a daily basis.

Issue	Not a Problem	A Small Problem	A Big Problem	No Answer
(a) Poor Health	0	1	2	−9
(b) Clean Water	0	1	2	−9
(c) Uncooperative Neighbors	0	1	2	−9
(d) Mosquitoes, Flies, Rats, Vermin	0	1	2	−9
(e) Garbage	0	1	2	−9
(f) other (specify) _____	0	1	2	−9

8. Have you had a problem with one of your neighbors in the last year?	Yes	No	No Answer
	1	0	−9

8a. [If yes] which one of the following describes how you reacted to your neighbor:

0 I ignored this person.
1 I gave this person a critical look.
2 I verbally expressed my dissatisfaction to this person.
3 I threatened this person.
4 Other (specify)
−9 No answer

9. Do you have piped water in your home?	1	0	−9
10. Do you Boil or Filter your drinking water?	1	0	−9
11. Do you have a toilet in your house?	1	0	−9

12. Does your community have any sort of garbage collection service?	Yes	No	No Answer
	1	0	−9

13. How often have you been ill in the past year?	Not at All	Not Often	Often	No Answer
	0	1	2	−9

14. Please tell me the last time you suffered from the following illnesses.

Illness	Never	More than One Year	Within One Year	Within Six Months	Within One Month	No Answer
a. Gastroenteritis or Diarrhea	0	1	2	3	4	−9
b. Asthma or Breathing problems	0	1	2	3	4	−9
c. Malaria	0	1	2	3	4	−9
e. Other (specify) _____	0	1	2	3	4	−9

15. How much does your household spend on transportation each day? _____

16. How much does your household spend on food each day? _____

17. How much does your household spend on rent or mortgage each month? _____

18. How much does your household spend for entertainment, including drinking, and the legal (or black market) lotteries each month? _____

19. Tell me a little bit about yourself. Do you agree with or disagree with the following statements?

Statement	Agree	Neutral	Disagree	No Answer
a. It is better to cooperate than compete. (+)	1	0	−1	−9
b. People should listen to their conscience when making decisions. (+)	1	0	−1	−9
c. People should forgive others when they are angry. (+)	−1	0	1	−9
d. It is amusing to play tricks on other people. (−)	−1	0	1	−9
e. People should revenge wrongs that are done to them. (−)	−1	0	1	−9
f. Confrontations should be avoided. (+)	1	0	−1	−9

Note: These statements come from internationally validated personality scales on cooperation. They are available at http://ipip.ori.org/ipip/new_home.htm

20. How often do you chat (talk informally) or spend time together with other people in your community?

1 A few times each week
2 A few times each month
3 A few times each year
4 Never
−9 No answer

21. How do you describe your immediate neighbors?
1 Like family
2 Like friends
3 Like strangers
−9 No answer

22. In some communities, neighbors will work on projects to help everybody in the community (for example: community clean-ups, developing drainage systems, or building a community hall).

22a. Do you remember such a project happening in your community in the past year?	Yes	No	No Answer
	1	0	−9

If yes, ask:

22b. Did you or someone in your household participate in those activities?	Yes	No	No Answer
	1	0	−9

22c. What kind of project was this?
1 Building/repairing houses for neighbors
2 Building/repairing a road/walkway
3 Building/repairing a wastewater drainage system
4 Collecting trash/cleaning community
5 Other (please specify_____)
−9 No answer

APPENDIX D: CARDENAS (2003A) EXPERIMENT INSTRUCTIONS (ENGLISH TRANSLATION)

These instructions were originally written in Spanish and translated from the final version used in the field work. The instructions were read to the participants from this script below by the same person during all sessions. The participants could interrupt and ask questions at any time.

Whenever the following type of text and font e.g. [. . . MONITOR: distribute *PAYOFFS TABLE* to participants . . .] is found below, it refers to specific instructions to the monitor at that specific point, when in *italics*, these are notes added to clarify issues to the reader. Neither of these were read to participants. Where the word "poster" appears, it refers to a set of posters we printed in very large format with the payoffs table, forms, and the three examples described in the instructions. These posters were hanged in a wall near to the participants' desks and where the eight people could see them easily.

Greetings . . .

We want to thank every one here for attending the call, and specially thank the field practitioner ____ (*name of the contact person in that community*), and ____ (*local organization that helped in the logistics*) who made this possible. We should spend about two hours between explaining the exercise, playing it and finishing with a short survey at the exit. So, let us get started.

The following exercise is a different and entertaining way of participating actively in a project about the economic decisions of individuals. Besides participating in the exercise, and being able to earn some prizes and some cash, you will participate in a community workshop in two days to discuss the exercise and other matters about natural resources. During the day of the workshop we will give you the earnings you make during the game. Besides a basic "show-up" prize for signing up and participate (examples: flash lamps, machetes, school kits, home tools), you will receive a cash bonus that will be converted into cash for purchases for your family. The funds to cover these expenditures have been donated by various organizations that support this study among which we have the Instituto Humboldt, el Fondo Mundial para la Protección de la Naturaleza, y la Fundación Natura.

I. Introduction

This exercise attempts to recreate a situation where a group of families must make decisions about how to use the resources of, for instance, a forest, a water source, a mangrove, a fishery, or any other case where communities use a natural resource. In the case of this community ____ (name of the specific village), an example would be the use of firewood or logging in the ____ (name of an actual local commons area in that village) zone. You have been selected to participate in a group of 8 people among those that signed up for playing. The game in which you will participate now is different from the ones others have already played in this community, thus, the comments that you may have heard from others do not apply necessarily to this game. You will play for several rounds equivalent, for instance, to years or harvest seasons. At the end of the game you will be able to earn some prizes in kind and cash. The cash prizes will depend on the quantity of points that you accumulate after several rounds.

II. The Payoffs Table

To be able to play you will receive a *PAYOFFS TABLE* equal to the one shown in the poster. [. . . *MONITOR*: show *PAYOFFS TABLE* in poster and distribute *PAYOFFS TABLE* to participants . . .]

This table contains all the information that you need to make your decision in each round of the game. The numbers that are inside the table correspond to points (or pesos) that you would earn in each round. The only thing that each of you has to decide in each round is the number of MONTHS that you want to allocate EXTRACTING THE FOREST (in the columns from 0 to 8).

To play in each round you must write your decision number between 0 and 8 in a yellow GAME CARD like the one I am about to show you [. . . MONITOR: show *yellow GAME CARDS* and show in the poster . . .]. It is very important that we keep in mind that the decisions are absolutely individual, that is, that the numbers we write in the game card are private and that we do not have to show them to the rest of members of the group if we do not want to. The monitor will collect the 8 cards from all participants, and will add the total of months that the group decided to use extracting the forest. When the monitor announces the group total, each of you will be able to calculate the points that you earned in the round. Let us explain this with an example.

In this game we assume that each player has available a maximum of eight MONTHS to work each year extracting a resource like firewood or logs. In reality this number could be larger or smaller but for purposes of our game we will assume eight as maximum. In the PAYOFFS TABLE this corresponds to the columns from 0 to 8. Each of you must decide from 0 to 8 in each round. But to be able to know how many points you earned, you need to know the decisions that the rest in the group made. That is why the monitor will announce in each round the total for the group. For instance, if you decide to use two months in the forest and the rest of the group together, add to 20 months in the forest, you would gain ____ points. Let us look at two other examples in the poster.

[. . . MONITOR: show poster with the *THREE EXAMPLES* . . .]

Let us look how the game works in each round.

III. The DECISIONS FORM

To play each participant will receive one green DECISIONS FORM like the one shown in the poster in the wall. We will explain how to use this sheet [. . . MONITOR: show the *DECISIONS FORM* in the poster and distribute the *DECISIONS FORMS* . . .]

With the same examples, let us see how to use this DECISIONS FORM. Suppose that you decided to play 5 in this round. In the yellow GAME CARD you should write 5. Also you must write this number in the first column A of the decisions form. The monitor will collect the 8 yellow cards and will add the total of the group. Suppose that the total added 26 months. Thus, we write 26 in the column

B of the decisions form [. . . MONITOR: In the poster, write the same example numbers in the respective cells . . .].

To calculate the third column (C), we subtract from the group total, MY MONTHS IN THE FOREST and then we obtain THEIR MONTHS IN THE FOREST which we write in column C. In our example, $26 - 5 = 21$. If we look at the PAYOFFS TABLE, when MY MONTHS are 5 and THEIR MONTHS are 21, I earn ____ points. I write then this number in the column D of the DECISIONS FORM.

It is very important to clarify that nobody, except for the monitor, will be able to know the number that each of you decide in each round. The only thing announced in public is the group total, without knowing how each participant in your group played. Let us repeat the steps with a new example [. . . MONITOR: Repeat with the other two examples, writing the numbers in the posters hanging in the wall . . .].

It is important repeating that your game decisions and earnings information is private. Nobody in your group o outside of it will be able to know how many points you earned or your decisions during rounds. We hope these examples help you understand how the game works, and how to make your decisions to allocate your MONTHS in each round of the game. *If at this moment you have any question about how to earn points in the game, please raise your hand and let us know* [. . . MONITOR: pause to resolve questions . . .].

It is very important that while we explain the rules of the game you do not engage in conversations with other people in your group. If there are no further questions about the game, then we will assign the numbers for the players and the rest of forms needed to play.

IV. Preparing for playing

Now write down your player number in the green DECISIONS FORM. Write also the place _____ and the current date and time __/__/__, __:__am/pm. In the following poster we summarize for you the steps to follow to play in each round. Please raise your hand if you have a question.

[MONITOR: Read the steps to them from the poster]

Before we start, and once all players have understood the game completely, the monitor will announce one additional rule for this group. To start the first round of the game we will organize the seats and desks in a circle where each of you face outwards. The monitor will collect in each round your yellow game cards. Finally, to get ready to play the game, please let us know if you have difficulties reading or writing numbers and one of the monitors will seat next to you and assist you with

these. Also, please keep in mind that from now on no conversation or statements should be made by you during the game unless you are allowed to. We will have first a few rounds of practice that will NOT count for the real earnings, just for your practicing of the game.

DECISIONS FORM

	Column A	Column B	Column C	Column D
Round No.	**MY MONTHS IN THE FOREST** (From your decision)	**TOTAL GROUP MONTHS IN THE FOREST** (Announced by the Monitor)	**THEIR MONTHS IN THE FOREST** [Column B minus Column A]	**MY TOTAL POINTS IN THIS ROUND** (Use your PAYOFFS TABLE)
Practice				
1				
2				
Total				

GAME CARD (Example)

GAME CARD	
PLAYER NUMBER:	1
ROUND NUMBER:April 24, 2002	
MY MONTHS IN THE FOREST:	

COMMUNITY RESOURCES GAME

(Summary Instructions)

In each round, you must decide how many months in a year between 0 and 8, you want to devote to extract resources from a forest. The points you earn in each round depend on your decision and the decisions by the rest of the group, according to the PAYOFFS TABLE (blue table). What do you need: To play you need a blue PAYOFFS TABLE, a green DECISIONS FORM, and several yellow GAME CARDS. Also you need a player number.

Steps to play in each round:

(1) Using the blue PAYOFFS TABLE, decide how many MONTHS IN THE FOREST you will play.
(2) In the DECISIONS FORM write your decision (MY MONTHS IN THE FOREST) in Column A for the round being played at that moment.
(3) In a yellow GAME CARD write the round number, and your decision MY MONTHS IN THE FOREST. Make sure it corresponds to the DECISIONS FORM. Hand the yellow game card to the monitor.
(4) Wait for the Monitor to calculate the total from all the cards in the group. The Monitor will announce the TOTAL GROUP MONTHS.
(5) In the green DECISIONS FORM write this total in Column B (TOTAL GROUP MONTHS IN THE FOREST).
(6) In the green DECISIONS FORM calculate Column C (THEIR MONTHS IN THE FOREST) equals to Column B minus Column A.
(7) In the green DECISIONS FORM write in Column D the total points you earned for this round. To know how many points you made, use the PAYOFFS TABLE and columns A and C (MY MONTHS and THEIR MONTHS). We will also calculate this quantity with the yellow cards to verify.
(8) Let us play another round (Go back to step 1).

Rule A: THERE IS NO COMMUNICATION WITHIN THE GROUP
Besides the rules described in the instructions that we just explained, there is an additional rule for the participants in this group:

You will not be able to communicate with any member of your group before, during or after you make your individual decision in each round. *Please do not make any comment to another participant or to the group in general*. After the last round we will add the points you earned in the game.

Rule B: COMMUNICATION WITH MEMBERS OF THE GROUP
Besides the rules described in the instructions that we just explained, there is an additional rule for the participants in this group:

Please make a circle or sit around a table with the rest of your group. Before making your decision in each round, you will be able to have an open discussion of maximum five minutes with the members of your group. You will be able to discuss the game and its rules in any fashion, except you *cannot use any promise or threat or transfer points. Simply an open discussion*. The rest of the rules hold.

We will let you know when the five minutes have ended. Then you will suspend the conversation and should make your individual decision for the next round. These decisions will still be private and individual as in the past rounds and cannot be known to the rest of the group or other people.

APPENDIX E: THEME TWO DATA APPENDIX.

In this section we discuss the details of how we estimated the effect of contributions in our voluntary contribution experiment (as a proxy for cooperative norms in the communities) on living standards in Southeast Asian urban slums. We focus on the Thai data because there seems to be a significant effect of contributions in Bangkok. The procedures for the Vietnamese data are identical.

In general, we consider the case where contributions are endogenous and follow the procedures detailed in Wooldridge (2002) Chapters 5 and 6. We begin by estimating the structural equation we are interested in omitting the possibly endogenous contribution variable. To linearize our proxy for well-being, monthly expenditures on transportation, rent, food and entertainment, we utilize the semi-log functional form. Therefore, let ln(y) be the natural log of monthly expenditures, x_1 be a vector of a subset of the exogenous variables, s be a vector of indicator variables for each community, and u a disturbance term. Using OLS we estimate:

$$\text{Ln}(y) = \beta_0 + x_1\beta_1 + s\beta_2 + u \tag{1}$$

yielding the following results:

Source	SS	df	MS	Number of Obs = 110
				$F(14, 95) = 3.68$
Model	44.1290864	14	3.1520776	Prob $> F = 0.0001$
Residual	81.3300553	95	0.856105846	$R^2 = 0.3517$
				Adj $R^2 = 0.2562$
Total	125.459142	109	1.1510013	Root MSE = 0.92526

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
schooling	0.0514642	0.0264625	1.94	0.055	−0.0010705	0.1039989
own home	−0.8954756	0.2423174	−3.70	0.000	−1.376536	−0.4144148
household	0.0244612	0.040029	0.61	0.543	−0.0550064	0.1039288
residence	0.000892	0.0093481	0.10	0.924	−0.0176663	0.0194503
homogeneous	−0.0883223	0.2538663	−0.35	0.729	−0.5923106	0.415666
coop scale	−0.148466	0.0790884	−1.88	0.064	−0.3054764	0.0085443
chat	−0.1274566	0.1271393	−1.00	0.319	−0.3798599	0.1249468
describe	0.0244423	0.1898824	0.13	0.898	−0.3525219	0.4014065
participate	0.0050698	0.3769052	0.01	0.989	−0.7431815	0.7533211
leader	0.1729975	0.2570973	0.67	0.503	−0.3374053	0.6834002
dumslum2	0.790265	0.370057	2.14	0.035	0.0556091	10.524921
dumslum3	0.2213256	0.2902193	0.76	0.448	−0.3548325	0.7974837
dumslum4	−0.0115286	0.3409118	−0.03	0.973	−0.6883241	0.6652669
dumslum5	−0.2080381	0.3402219	−0.61	0.542	−0.8834638	0.4673877
_cons	7.623612	0.8081779	9.43	0.000	6.019176	9.228048

which indicate that expenditures are significantly increasing in education attainment and decreasing in home ownership and our psychological scale.

As a second step we add the average contribution of an individual in the experiment (call this variable z) to the right hand side of the OLS regression and estimate:

$$\text{Ln}(y) = \beta_0 + x_1\beta_1 + s\beta_2 + \beta_3 z + u \tag{2}$$

which yields:

Source	SS	df	MS	Number of Obs = 110
				$F(15, 94) = 4.01$
Model	48.9517755	15	3.2634517	Prob $> F = 0.0000$
Residual	76.5073662	94	0.813908151	$R^2 = 0.3902$
				Adj $R^2 = 0.2929$
Total	125.459142	109	1.1510013	Root MSE = 0.90217

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
contr_avg	0.1183085	0.0486025	2.43	0.017	0.021807	0.21481
schooling	0.0498631	0.0258105	1.93	0.056	−0.0013842	0.1011104
own home	−0.7680987	0.2419953	−3.17	0.002	−1.248586	−0.2876114
household	0.0312309	0.039129	0.80	0.427	−0.0464606	0.1089224

(*Continued*)

ln_exp	Coef.	Std. Err.	*t*	$P > \|t\|$	[95% Conf. Interval]	
residence	0.0019786	0.0091257	0.22	0.829	−0.0161407	0.020098
homogeneous	−0.135744	0.2482961	−0.55	0.586	−0.6287417	0.3572538
coop scale	−0.1479022	0.077115	−1.92	0.058	−0.3010158	0.0052115
chat	−0.0836699	0.1252646	−0.67	0.506	−0.3323857	0.1650458
describe	0.0124144	0.1852095	0.07	0.947	−0.3553234	0.3801522
participate	−0.2616227	0.3834826	−0.68	0.497	−1.023036	0.4997911
leader	0.192055	0.2508033	0.77	0.446	−0.3059209	0.6900308
dumslum2	0.7007485	0.3626908	1.93	0.056	−0.0193826	1.42088
dumslum3	0.484624	0.3029448	1.60	0.113	−0.11688	1.086128
dumslum4	0.0415302	0.3331177	0.12	0.901	−0.6198828	0.7029432
dumslum5	−0.3146821	0.3346115	−0.94	0.349	−0.9790612	0.3496969
_cons	6.881881	0.8448709	8.15	0.000	5.20437	8.559392

and shows that there is some association between cooperation in our experiment and economic well-being. However, while we hypothesize that cooperative norms, measured by our experiment, contribute to higher living standards in urban slums, one could also argue (a la Olson, 1965) that higher living standards may allow people to act more cooperatively.

To explore the possibility that average contributions are endogenous, we employ the regression-based version of the Hausman test. To do so, let x be the vector of the entire set of exogenous variables. In our case the difference between x and x_1 is the inclusion of age and a female indicator in x that are not in x_1. As a first step we estimate the linear projection of our potentially endogenous variable, z, on x and s or:

$$z = \alpha_0 + x\alpha_1 + s\alpha 2 + e \tag{3}$$

which yields:

Source	SS	df	MS	Number of Obs = 110
				$F(16, 93) = 6.57$
Model	329.356434	16	20.5847771	Prob $> F = 0.0000$
Residual	291.534128	93	3.13477557	$R^2 = 0.5305$
				Adj $R^2 = 0.4497$
Total	620.890562	109	5.69624369	Root MSE = 1.7705

contr_Avg	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
age	−0.0409188	0.0165712	−2.47	0.015	−0.0738259	−0.0080117
female	−1.336999	0.3869786	−3.45	0.001	−2.105462	−0.5685366
schooling	−0.1039156	0.0603984	−1.72	0.089	−0.2238548	0.0160237
own home	−0.9967668	0.464191	−2.15	0.034	−1.918558	−0.0749755
household	0.0228283	0.0795972	0.29	0.775	−0.135236	0.1808926
residence	0.0014961	0.0180763	0.08	0.934	−0.0343998	0.037392
homogeneous	0.1063181	0.4939422	0.22	0.830	−0.8745533	1.087189
coop scale	0.0596058	0.1522766	0.39	0.696	−0.2427853	0.3619969
chat	−0.4009925	0.2434157	−1.65	0.103	−0.8843678	0.0823828
describe	0.3254487	0.3708631	0.88	0.382	−0.411012	1.061909
participate	1.806592	0.72985	2.48	0.015	0.3572546	3.255929
leader	0.0684413	0.5163314	0.13	0.895	−0.9568905	1.093773
dumslum2	0.7128512	0.7108851	1.00	0.319	−0.6988257	2.124528
dumslum3	−2.453313	0.5591678	−4.39	0.000	−3.56371	−1.342917
dumslum4	−0.505357	0.6533601	−0.77	0.441	−1.802801	0.7920866
dumslum5	1.309659	0.6593487	1.99	0.050	0.0003229	2.618994
_cons	8.607062	1.756644	4.90	0.000	5.118715	12.09541

We then save the residuals from this regression, call them e^{hat}, and add these residuals to our original estimation that included average contributions. That is, we now estimate:

$$\text{Ln}(y) = \beta_0 + x_1\beta_1 + s\beta_2 + \beta_3 z + \beta_4 e^{h\alpha t} + v \tag{4}$$

which yields:

Source	SS	df	MS	Number of Obs = 110
				$F(16, 93) = 4.00$
Model	51.1668445	16	3.19792778	Prob $> F = 0.0000$
Residual	74.2922973	93	0.798841906	$R^2 = 0.4078$
				Adj $R^2 = 0.3060$
Total	620.890562	109	5.69624369	Root MSE = 1.7705

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
e_hat	−0.2222069	0.1334426	−1.67	0.099	−0.4871974	0.0427836
contr_avg	0.3063222	0.1227468	2.50	0.014	0.0625713	0.5500731
schooling	0.0473187	0.0256161	1.85	0.068	−0.0035498	0.0981872
own home	−0.5656737	0.2688033	−2.10	0.038	−1.099464	−0.0318835

(*Continued*)

ln_exp	Coef.	Std. Err.	*t*	$P > \|t\|$	[95% Conf. Interval]	
household	0.0419893	0.0392998	1.07	0.288	−0.0360524	0.1200309
residence	0.0037055	0.0091001	0.41	0.685	−0.0143656	0.0217766
homogeneous	−0.2111057	0.2501159	−0.84	0.401	−0.7077862	0.2855749
coop scale	−0.147006	0.0763998	−1.92	0.057	−0.298721	0.0047089
chat	−0.014085	0.1309465	−0.11	0.915	−0.2741189	0.2459488
describe	−0.0067002	0.183846	−0.04	0.971	−0.3717819	0.3583815
participate	−0.6854456	0.4572931	−1.50	0.137	−1.593539	0.2226478
leader	0.2223408	0.2491359	0.89	0.374	−0.2723938	0.7170753
dumslum2	0.5584903	0.3693345	1.51	0.134	−0.1749348	1.291915
dumslum3	0.903053	0.3914314	2.31	0.023	0.1257479	1.680358
dumslum4	0.1258503	0.3338823	0.38	0.707	−0.5371738	0.7888744
dumslum5	−0.4841589	0.3467719	−1.40	0.166	−1.172779	0.2044613
_cons	5.703136	1.096212	5.20	0.000	3.526276	7.879996

According to Hausman, a test of whether contributions are endogenous is whether the coefficient on e^{hat} is significantly different from zero. The intuition for this test is that if contributions are exogenous then there should be no correlation between the errors in the structural equation and the errors in the above reduced form Eq. (3). That is $E(e^{\text{hat}}u)$ should be zero. Examination of this hypothesis yields:

$$e\_hat = 0 \;\; F(1,\ 93) = 2.77 \;\; \text{Prob} > F = 0.0992 \tag{1}$$

and we conclude that contributions are endogenous.

To control for the endogeneity of contributions, we use 2SLS, and therefore must find valid instruments for contributions in our experiment. According to Wooldridge (2002, p. 83) there are two important conditions for good instruments. First, the instruments must be correlated with the endogenous variable in the reduced form Eq. (3). Second, the instruments must be uncorrelated with the disturbance in the structural Eq. (2). We let our knowledge of the communities in our sample guide our choice of instruments. We argue that the elements in x that are not in x_1 (i.e. age and female) are reasonable instruments.

The first criteria, that our instruments are correlated with contributions, is easy to demonstrate. Our estimation of Eq. (3) indicates that both age and female are highly correlated with average contributions ($p = 0.015$ and $p = 0.001$, respectively). However, we also must argue why our instruments are orthogonal with respect to expenditures. There are no formal statistical tests for this criteria and, therefore, we: (a) let our knowledge of the communities in our sample provide some theoretical justification for the choice of age and female; and (b) show that neither age nor female improve our estimate of expenditures when we move them from the reduced form to the structural equation.

Participants in our communities live in extreme poverty, suffer high unemployment, and have few chances for educational attainment. The first of these facts implies that our participants save little and, therefore, their expenditures also closely approximate their earnings or wages. Therefore, for our current purposes we can speak in terms of wages and not expenditures. In the traditional theory of wage determination, factors such as age and sex correlate with wages: wages are increasing in age (although they may plateau) and men often earn more than women in the same job. The major reason we argue that age and sex are orthogonal to expenditures (i.e. wages) is that this theory of wages does not apply in the slums. Most people, who are employed, are employed in low-skilled jobs that are often female dominated in which there is little wage discrimination based on sex. Instead, all workers in these jobs are poorly paid (Macpherson & Hirsch, 1995). Further, younger, single members of the community are just a likely to be employed in these low skilled jobs as are older community members with families. The punchline is that under conditions of severe poverty, as in our communities, being a man or being older does not translate in to a higher wage or higher expenditures.

Additionally, those people who are not employed often earn money in the handicrafts or food preparation industries. The products that these people create are often sold directly on the market. Given there is no reason to expect discrimination in the price that men or women or old or young craftspeople can get for these handicrafts, then neither age nor sex will correlate directly with expenditures.

Given this argument for the use of age and female as instruments for contributions, we use 2SLS to estimate the reduced form Eq. (3) and then use the predicted values of contributions in our structural equation. The system is:

$$Z = \alpha_0 + x\alpha_1 + s\alpha_2 + e \tag{5}$$

$$\mathrm{Ln}(y) = \beta_0 + x_1\beta_1 + s\beta_2 + \beta_3\hat{z} + u$$

and the results are:

First-stage regressions

Source	SS	df	MS	Number of Obs = 110
				$F(16, 93) = 6.57$
Model	329.356434	16	20.5847771	Prob > $F = 0.0000$
Residual	291.534128	93	3.13477557	$R^2 = 0.5305$
				Adj $R^2 = 0.4497$
Total	620.890562	109	5.69624369	Root MSE = 1.7705

contr_avg	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
schooling	−0.1039156	0.0603984	−1.72	0.089	−0.2238548	0.0160237
own	−0.9967668	0.464191	−2.15	0.034	−1.918558	−0.0749755
household	0.0228283	0.0795972	0.29	0.775	−0.135236	0.1808926
residence	0.0014961	0.0180763	0.08	0.934	−0.0343998	0.037392
homogeneous	0.1063181	0.4939422	0.22	0.830	−0.8745533	1.087189
sum19	0.0596058	0.1522766	0.39	0.696	−0.2427853	0.3619969
chat	−0.4009925	0.2434157	−1.65	0.103	−0.8843678	0.0823828
describe	0.3254487	0.3708631	0.88	0.382	−0.411012	1.061909
participate	1.806592	0.72985	2.48	0.015	0.3572546	3.255929
leader	0.0684413	0.5163314	0.13	0.895	−0.9568905	1.093773
dumslum2	0.7128512	0.7108851	1.00	0.319	−0.6988257	2.124528
dumslum3	−2.453313	0.5591678	−4.39	0.000	−3.56371	−1.342917
dumslum4	−0.505357	0.6533601	−0.77	0.441	−1.802801	0.7920866
dumslum5	1.309659	0.6593487	1.99	0.050	0.0003229	2.618994
age	−0.0409188	0.0165712	−2.47	0.015	−0.0738259	−0.0080117
female	−1.336999	0.3869786	−3.45	0.001	−2.105462	−0.5685366
_cons	8.607062	1.756644	4.90	0.000	5.118715	12.09541

Instrumental variables (2SLS) regression

Source	SS	df	MS	Number of Obs = 110
				$F(15, 94) = 3.47$
Model	36.7720807	15	2.45147205	Prob > $F = 0.0001$
Residual	88.687061	94	0.943479373	$R^2 = 0.2931$
				Adj $R^2 = 0.1803$
Total	125.459142	109	1.1510013	Root MSE = 0.97133

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
contr_avg	0.3063222	0.133397	2.30	0.024	0.0414593	0.5711851
schooling	0.0473187	0.0278387	1.70	0.092	−0.0079556	0.1025931
own	−0.5656737	0.2921261	−1.94	0.056	−1.145697	0.0143496
household	0.0419893	0.0427097	0.98	0.328	−0.0428118	0.1267904
residence	0.0037055	0.0098897	0.37	0.709	−0.0159307	0.0233418
homogeneous	−0.2111056	0.2718173	−0.78	0.439	−0.7508052	0.3285939
sum19	−0.147006	0.0830287	−1.77	0.080	−0.3118614	0.0178494
chat	−0.014085	0.1423082	−0.10	0.921	−0.2966412	0.2684711
describe	−0.0067002	0.1997975	−0.03	0.973	−0.4034027	0.3900024
participate	−0.6854456	0.4969703	−1.38	0.171	−1.672192	0.3013005

(*Continued*)

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
leader	0.2223408	0.2707523	0.82	0.414	−0.3152442	0.7599258
dumslum2	0.5584903	0.40138	1.39	0.167	−0.238459	1.35544
dumslum3	0.903053	0.4253941	2.12	0.036	0.0584231	1.747683
dumslum4	0.1258503	0.3628517	0.35	0.729	−0.5946003	0.8463009
dumslum5	−0.4841589	0.3768597	−1.28	0.202	−1.232423	0.2641048
_cons	5.703136	1.191325	4.79	0.000	3.337732	8.06854

Note: Instrumented: contr_avg
Instruments: schooling own household residence homogeneous sum19 chat describe participate leader dumslum2 dumslum3 dumslum4 dumslum5 age female.

One way to indirectly test the second criteria for age and female being good instruments is to remove them, one at a time, from the reduced form and place them in the structural equation to see if they have any direct effect on expenditures. If they are significant in the structural equation we know they should be correlated with the disturbance in the structural Eq. (without either instrument) because of omitted variable bias. We begin by pulling age out first which yields the following structural estimate:

Instrumental variables (2SLS) regression

Source	SS	df	MS	Number of Obs = 110
				$F(16, 93) = 3.68$
Model	46.4053062	16	2.90033164	Prob $> F = 0.0000$
Residual	79.0538355	93	0.850041242	$R^2 = 0.3699$
				Adj $R^2 = 0.2615$
Total	125.459142	109	1.1510013	Root MSE = 0.92198

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
contr_avg	0.2232058	0.1507206	1.48	0.142	−0.0760953	0.522507
age	−0.0104164	0.0102461	−1.02	0.312	−0.0307631	0.0099303
schooling	0.0300109	0.0314338	0.95	0.342	−0.0324104	0.0924322
own	−0.6434669	0.2876486	−2.24	0.028	−1.21468	−0.0722539
household	0.0388732	0.0406554	0.96	0.341	−0.0418604	0.1196067
residence	0.0039151	0.0093895	0.42	0.678	−0.0147305	0.0225608
homogeneous	−0.2305954	0.2587179	−0.89	0.375	−0.7443578	0.283167
sum19	−0.1387247	0.07923	−1.75	0.083	−0.2960597	0.0186104
chat	−0.046101	0.1387002	−0.33	0.740	−0.3215322	0.2293302

(*Continued*)

ln_exp	Coef.	Std. Err.	*t*	$P > \|t\|$	[95% Conf.	Interval]
describe	−0.0064143	0.1896462	−0.03	0.973	−0.3830142	0.3701855
participate	−0.5216146	0.4984874	−1.05	0.298	−1.511512	0.4682825
leader	0.3055642	0.2697189	1.13	0.260	−0.2300441	0.8411724
dumslum2	0.5850738	0.3818827	1.53	0.129	−0.1732696	1.343417
dumslum3	0.6808907	0.4591228	1.48	0.141	−0.2308363	1.592618
dumslum4	0.0660831	0.3493973	0.19	0.850	−0.6277506	0.7599168
dumslum5	−0.3581917	0.3785643	−0.95	0.347	−1.109945	0.3935619
_cons	6.741017	1.523469	4.42	0.000	3.715711	9.766324

Note: Instrumented: contr_avg
Instruments: age schooling own household residence homogeneous sum19 chat describe participate leader dumslum2 dumslum3 dumslum4 dumslum5 female.

We then try pulling out female:

Instrumental variables (2SLS) regression

Source	SS	df	MS	Number of Obs = 110
				$F(16, 93) = 2.45$
Model	6.86838744	16	0.429274215	Prob > $F = 0.0039$
Residual	118.590754	93	1.2751694	$R^2 = 0.0547$
				Adj $R^2 = 0.0547$
Total	125.459142	109	1.1510013	Root MSE = 1.1292

ln_exp	Coef.	Std. Err.	*t*	$P > \|t\|$	[95% Conf.	Interval]
contr_avg	0.4777686	0.2582924	1.85	0.068	−0.035149	0.9906862
female	0.3403503	0.4100434	0.83	0.409	−0.4739147	1.154615
schooling	0.0564639	0.0341883	1.65	0.102	−0.0114273	0.1243552
own	−0.3897272	0.4003403	−0.97	0.333	−1.184724	0.4052692
household	0.0330619	0.0508044	0.65	0.517	−0.0678255	0.1339493
residence	0.0035343	0.0114993	0.31	0.759	−0.019301	0.0263696
homogeneous	−0.25766	0.3209444	−0.80	0.424	−0.894992	0.3796719
sum19	−0.1538981	0.0968828	−1.59	0.116	−0.3462882	0.038492
chat	0.0559768	0.1857311	0.30	0.764	−0.3128483	0.4248018
describe	−0.0892615	0.2526791	−0.35	0.725	−0.591032	0.4125091
participate	−0.9815057	0.6789927	−1.45	0.152	−2.329851	0.3668393
leader	0.2881416	0.3245967	0.89	0.377	−0.3564432	0.9327264
dumslum2	0.4036084	0.5025562	0.80	0.424	−0.5943686	1.401585

(*Continued*)

ln_exp	Coef.	Std. Err.	t	$P > \|t\|$	[95% Conf.	Interval]
dumslum3	1.305413	0.692504	1.89	0.063	−0.0697628	2.680589
dumslum4	0.1947282	0.4299236	0.45	0.652	−0.6590149	1.048471
dumslum5	−0.691582	0.5043822	−1.37	0.174	−1.693185	0.310021
_cons	4.54998	1.961716	2.32	0.023	0.6544014	8.445558

Note: Instrumented: contr_avg
Instruments: female schooling own household residence homogeneous sum19 chat describe participate leader dumslum2 dumslum3 dumslum4 dumslum5 age.

Based on these two regressions, we see that in neither case does moving an instrument add to the structural estimate.

We have two things left to show. First, we need to show that the 2SLS estimates are inconsistent with the standard OLS results. Second, we use more instruments than we have endogenous variables to instrument for (i.e. $2 > 1$) and therefore we need to worry about over-identification. The first task is a straight forward application of the Hausman test which yields:

—- Coefficients —-

	(b) Consistent	(B) Efficient	($b - B$) Difference	sqrt(diag($V\_b - V\_B$)) S. E.
contr_avg	0.3063222	0.1183085	0.1880137	0.1139682
schooling	0.0473187	0.0498631	−0.0025444	0.0015423
own	−0.5656737	−0.7680987	0.202425	0.1227039
household	0.0419893	0.0312309	0.0107583	0.0065214
residence	0.0037055	0.0019786	0.0017269	0.0010468
homogeneous	−0.2111056	−0.135744	−0.0753617	0.045682
sum19	−0.147006	−0.1479022	0.0008961	0.0005432
chat	−0.014085	−0.0836699	0.0695849	0.0421803
describe	−0.0067002	0.0124144	−0.0191146	0.0115867
participate	−0.6854456	−0.2616227	−0.4238229	0.2569085
leader	0.2223408	0.192055	0.0302858	0.0183583
dumslum2	0.5584903	0.7007485	−0.1422581	0.0862325
dumslum3	0.903053	0.484624	0.418429	0.2536389
dumslum4	0.1258503	0.0415302	0.0843201	0.0511123
dumslum5	−0.4841589	−0.3146821	−0.1694768	0.1027317
_cons	5.703136	6.881881	−1.178745	0.7145194

Note: b = consistent under Ho and Ha; obtained from regress; B = inconsistent under Ha, efficient under Ho; obtained from ivreg.

Test: Ho: difference in coefficients not systematic

$$\text{chi2}(1) = (b - B)'[(V\_b - V\_B)^{\wedge}(-1)](b - B) = 2.72$$

$$\text{Prob} > \text{chi2} = 0.0990$$

The chi-squared test indicates that the estimates are different and this is further confirmation of the endogeneity of contributions.

As for the over-identification problem there are a number of tests that can be applied. As seen below, in each case we fail to reject the null hypothesis that the over-identifying restrictions are valid.

Tests of overidentifying restrictions:

Sargan $N \times R$-sq test	1.090 Chi-sq(1)	P-value $= 0.2965$
Sargan $(N - L) \times R$-sq test	0.931 Chi-sq(1)	P-value $= 0.3346$
Basmann test	0.930 Chi-sq(1)	P-value $= 0.3347$
Sargan pseudo-F test	0.931 F(1,94)	P-value $= 0.3370$
Basmann pseudo-F test	0.930 F(1,93)	P-value $= 0.3372$