

# **Institutions, Behavior, and the Environment: An experimental approach**

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von  
M.Sc. Jens Rommel

Präsident der Humboldt-Universität zu Berlin  
Prof. Dr. Jan-Hendrik Olbertz

Dekan der  
Lebenswissenschaftlichen Fakultät  
Prof. Dr. Richard Lucius

Gutachter  
(1) Prof. Dr. Markus Hanisch  
(2) Associate Professor Eric Coleman, PhD  
(3) Prof. Dr. Oliver Mußhoff

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## SUMMARY

This research project focuses on the use of economic experiments for institutional analysis. The dissertation consists of one conceptual and four empirical essays. Specifically, it investigates how contextual factors and institutions affect behavior.

In the first essay, three theoretical perspectives on institutional change are distinguished. These are linked to empirical work in Experimental Economics. It becomes apparent that three types of economic experiments can be differentiated by the definition of institutions. The empirical essays expand on these considerations. In the second essay, commuters' transportation mode choice is analyzed by means of a framed field experiment. It is shown that loss aversion and status concerns drive behavior. In the third essay, a public goods game is employed to investigate the effect of endowment heterogeneity and leading by example on voluntary contributions to a jointly used sanitation facility. It is demonstrated that in a structurally similar decision context, choices are driven by experiences from other contexts. The fourth essay examines the impact of the so-called status quo bias on pro-environmental behavior. In a field experiment, it is shown that forcing a decision has a positive effect on pro-environmental behavior. The fifth essay investigates whether experiments can be used as a tool for environmental awareness building. Even participants with negative experiences from taking part in the dilemma experiment show a greater likeliness of behaving environmentally friendly in comparison to a control group.

In a concluding part, the essays are synthesized. One outcome is an analytical framework for developing economic experiments on institutional change. Constitutional rules (i.e., rules on how to change the rules) are identified as a promising starting point for future research.

**Keywords:** Behavioral Economics; Experimental Economics; Institutional Economics; Methodology

## ZUSAMMENFASSUNG

Das Kerninteresse der Arbeit gilt den Einsatzmöglichkeiten ökonomischer Experimente in der Institutionenanalyse. Die Dissertation setzt sich aus einem konzeptionellen und vier empirischen Aufsätzen zusammen. Anhand der Arbeiten wird nachvollzogen, wie sich verschiedene Kontextfaktoren und Institutionen auf Verhalten auswirken.

Im ersten Aufsatz werden drei theoretische Blickwinkel auf institutionellen Wandel unterschieden. Diese werden dann mit empirischen Arbeiten in der experimentellen Wirtschaftsforschung in Beziehung gesetzt. Es wird deutlich, dass sich drei Arten von ökonomischen Experimenten anhand ihres Institutionenbegriffs unterscheiden lassen. Die empirischen Aufsätze greifen diese Überlegungen auf. Im zweiten Aufsatz findet für die Analyse der Verkehrsmittelwahl von Pendlern unter verschiedenen Politikszenarios ein „Framed Field Experiment“ Anwendung. Es wird gezeigt, wie Verlustaversion und Statusängste die Entscheidungen der Pendler erklären können. Im dritten Aufsatz kommt ein „Public Goods Game“ zum Einsatz, um den Einfluss von ökonomischer Ungleichheit und Vorbildverhalten auf Beiträge zu einer gemeinsam genutzten sanitären Einrichtung zu untersuchen. Es kann gezeigt werden, dass in einer strukturell ähnlichen Entscheidungssituation Teilnehmerinnen sich häufig so verhalten, wie es ihnen aus anderen Zusammenhängen bekannt ist. Der vierte Aufsatz untersucht den Einfluss des sogenannten Status-Quo-Effekts auf umweltfreundliches Verhalten. In einem Feldexperiment wird gezeigt, dass sich ein Zwang zur Entscheidung positiv auf umweltfreundliches Verhalten auswirken kann. Der fünfte Aufsatz geht der Frage nach, ob sich Experimente als didaktisches Werkzeug zur Schaffung größeren Umweltbewusstseins einsetzen lassen. Im Vergleich zu einer Kontrollgruppe zeigen selbst Teilnehmende mit negativen Erfahrungen aus dem Experiment einen positiven Effekt auf umweltfreundliches Verhalten.

In einem Fazit findet eine Synthese der Aufsätze statt. Ein Ergebnis ist die Entwicklung eines analytischen Rahmens für die Durchführung ökonomischer Experimente zur Untersuchung institutionellen Wandels. Konstitutionelle Regeln, also Regeln zur Änderung von Regeln, werden als erfolgversprechender Ansatzpunkt für die zukünftige Forschung identifiziert.

**Schlagwörter:** Verhaltensökonomie; Experimentelle Wirtschaftsforschung; Institutionenökonomie; Methodologie

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## **1. RESEARCH AGENDA**

### **1.1. Problem statement**

Society faces a number of pressing challenges. The benefits of economic and social development remain out of reach for many people on the globe. Universal primary education, basic health, sanitation and hygiene, food security, or gender equity are only some of the issues that must be resolved in the years to come (United Nations, 2000). At the same time, global environmental change has reached alarming levels, and in a number of domains, planetary boundaries have been passed (Rockström et al., 2009). In many important aspects, solving these problems calls for behavioral change (World Bank, 2015). For instance, carbon emissions can be substantially reduced by household-level interventions aiming at behavioral change (Dietz et al., 2009), and to achieve improved sanitation practices, the provision of toilets and pipes – the “hardware” – must be complemented by a change in attitudes and habits – the “software” (Mosler, 2012).

Understanding how people make decisions in complex contexts is crucial to enhance policy analysis (World Bank, 2015). Traditionally, economists have regarded this as a matter of investigating people’s responses to incentives, and more recently, behavioral economists have started to explicitly acknowledge the role of cognition in decision-making (e.g., Thaler, 1994; Kahneman, 2011). Individual decisions are embedded in and interact with a wider social context. Ultimately, it is the combination of incentives and strategic interaction, cognition, and institutions that determine decision-making in practice. In empirical work, the complex relationships among the multiple levels and factors involved make it difficult to establish causal relationships. In Economics, the use of experiments has successfully addressed this methodical challenge. The manipulation of a variable that is exogenous to the process of interest allows for the construction of a proper counterfactual for the identification of causal effects (Harrison & List, 2004).

Over the first decades of its existence, the field of Experimental Economics has witnessed the application of a number of basic games involving abstract decision tasks, and numerous empirical studies have aimed at testing economic theory under controlled laboratory conditions. In some respects, the evidence has confirmed theoretical predictions; in others, economic theory has been rejected. For example, basic laws of competition and market power are fairly accurately reproduced under laboratory conditions with actual human subjects (Friedman & Sunder, 1994). By contrast, experimental economists have

repeatedly demonstrated that humans behave less selfishly than the *homo economicus* model would normally predict; this has led to the integration of fairness concerns or so-called other-regarding preferences into mainstream economic theory (e.g., Fehr & Schmidt, 1999). Over time, Experimental Economics has become more specialized and dispersed. However, two important recent trends can be identified.

First, there is growing interest in using experiments for the study of institutions (Ostrom, Gardner, & Walker, 1994; Ménard, 2001; Robin & Staropoli, 2008; Beckmann & Padmanabhan, 2009). In particular, in a recently created branch of Experimental Economics, institutions are subject to endogenous change (e.g., Gülerk, Irlenbusch, & Rockenbach, 2006; Ertan, Page, & Putterman, 2009; Dal Bó, Foster, & Putterman, 2010; Sutter, Haigner, & Kocher, 2010). As part of the experimental game, participants can select into groups that differ in their institutions (e.g., Gülerk, Irlenbusch, & Rockenbach, 2006) or adopt a different set of institutions as part of a group whose composition is held constant over time in repeated interactions (e.g., Sutter, Haigner, & Kocher, 2010). However, the link between empirical experimental work and the wide range of theoretical concepts on institutions and institutional change (e.g., Bromley, 1989; North, 1990; Knight, 1992; Bowles, 1998; van den Bergh & Stiglitz, 2003; Greif & Laitin, 2004; Hodgson, 2004; Aoki, 2007; Kingston & Caballero, 2009) is often only weakly established.

Second, increasingly experimental economists have left the lab, extending their research to non-student subjects and more explicitly recognizing context because “it is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects” (Harrison & List, 2004, p. 1022). A quickly growing body of empirical literature uses framed field experiments to explore the impact of a wide range of contextual factors on cooperation and collective action in natural resource management and agriculture (Cárdenas & Ostrom, 2004; Viceisza, 2012). In Development Economics, randomized controlled trials have become popular for assessing the effectiveness of policy interventions (Banerjee & Duflo, 2011). In spite of the growing empirical evidence, a vast number of potentially interesting factors remains unexplored. For example, the impact of “leading by example” or endowment heterogeneity has been studied in the lab (e.g., Levati, Sutter, & van der Heijden, 2007), but it is unstudied in the field where these factors exhibit context-specific interactions that are important for understanding actual decision-making. Framed field experiments may also complement other research methods if assessing the effect of policies would not be viable otherwise.

For instance, it is fairly difficult to study coordination and strategic interaction in transportation under different policy scenarios. Here, experimental games can complement other research methods.

## **1.2. Research purpose and scope**

This dissertation contributes to these two strands of research in five essays,. A key conceptual contribution lies in establishing links between theoretical work in Institutional Economics and empirical work in Experimental Economics. Institutions are at the core of many questions in Economics, and it is widely acknowledged that they play a key role in the economic system (North, 1990; Acemoglu, Robinson, & Woren, 2012). The overall role of human agency in driving institutional change, the mechanisms of such change, and actors' motivations to change an existing institution or create a new one are subject to a theoretical debate (Bromley, 1989; North, 1990; Knight, 1992; Hodgson, 2004). However, there is only limited empirical evidence on the topic because it is methodically challenging to study institutional change (Alston, Eggertsson, & North, 1996; Schlüter, 2010). Thus, an additional contribution of the dissertation lies in developing an improved understanding of how economic experiments are potentially useful for the empirical analysis of dynamically-changing institutions.

In summary, the **overarching research objective** is to explicate the multidirectional links between behavior, institutions, and the environment. The three **guiding questions** are:

1. How are institutions understood in Experimental Economics, and can we distinguish between concepts in empirical work?
2. How does behavior vary under different institutions, and how do institutions interact with contextual variables?
3. Finally, what are the specific conditions under which experiments are a useful method for analyzing dynamically changing institutions?

The empirical part employs framed field and natural field experiments to investigate in some detail the pressing challenges described above. The commonality among the empirical essays is that they are strongly embedded in a specific field setting that involves the complex interaction of contextual variables with institutions. The aim is to highlight the role of context in mediating outcomes through the interaction of contextual variables with

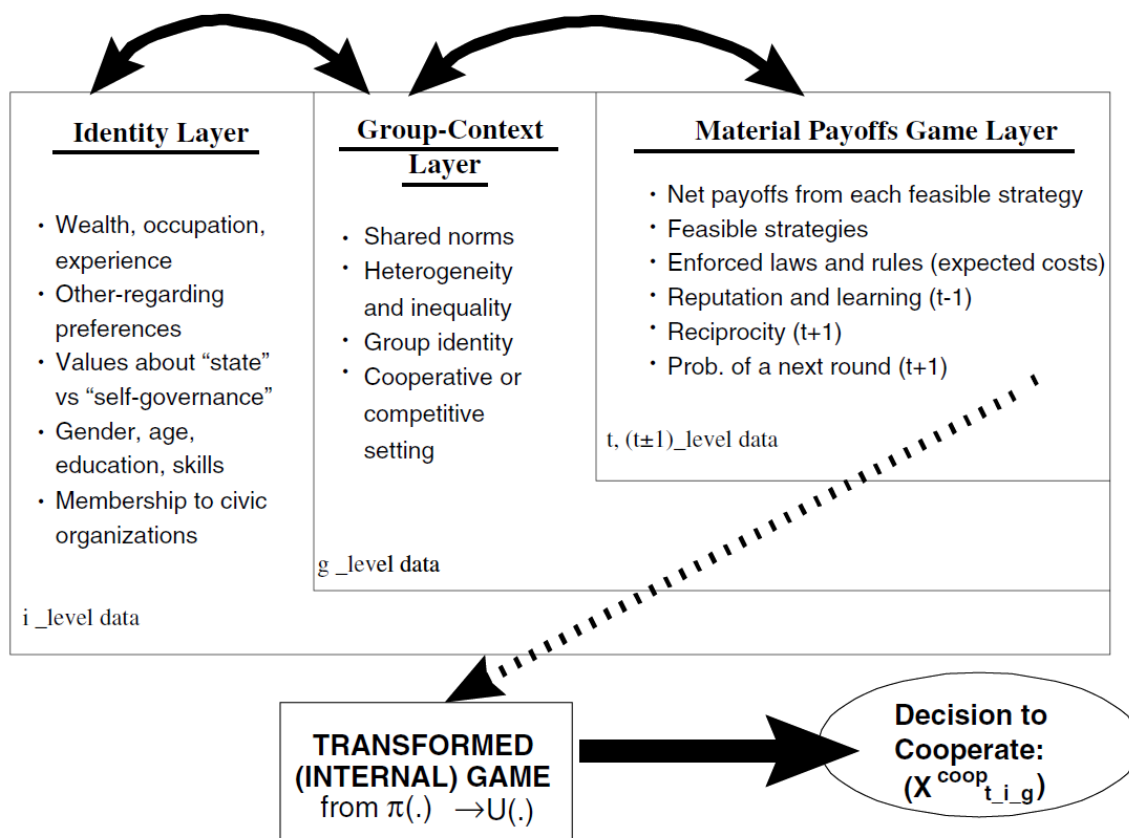
existing institutions. For this purpose, it is important to first develop a map of potentially influential variables.

### 1.3. A framework for analyzing decisions in framed field experiments

Analytical frameworks are used as heuristics to structure the research process. One aim is to assist empirically-working scholars with a set of potentially important variables and factors to consider for analysis in a specific empirical setting. In contrast to theories or models, analytical frameworks are not explicit on the direction or size of causal effects. They usually provide a starting point for empirical analysis, help to delineate boundaries of a research project, or highlight particular aspects of a more complex system.

Cárdenas & Ostrom (2004) have developed a framework for the analysis of decision-making data gathered from framed field experiments in common pool resource dilemmas. The central unit of analysis is the decision of an individual whether or not to cooperate. This decision is nested in multiple layers of information (Figure 1).

**Figure 1:** Framework for the analysis of the levels of information for deciding to cooperate



Source: Cárdenas & Ostrom, 2004, p. 310



The framework posits that participants in an experiment transform the formal game into an internal game and that three layers of information are used in deciding whether to cooperate or defect. The first layer is the material payoff layer. It consists of the set of feasible strategies and the associated payoffs of an outcome for a one-shot decision. It is temporally dynamic because it allows the development of reputation and learning from previous rounds and reciprocity in subsequent rounds ( $t \pm 1$  in Figure 1). The second layer is the group-context layer which is based on a player's perception of the other players. Players may assess whether norms are shared with other players or what the degree of inequality and heterogeneity within the group may be. The third layer, the identity layer, consists of individual level characteristics that may affect the decision to cooperate. The framework is used to indicate the single contributions of the four empirical essays.

#### **1.4. Contribution of the papers and organization of the thesis**

The **first paper** establishes the foundation of the thesis by reviewing the experimental literature in Ecological Economics. The paper develops a link between existing empirical work and the various definitions of institutions and theories of institutional change. It shows that three types of experimental practice can be distinguished in a structure–agency framework. These practices are mirrored in (1) a structural, (2) an agent-centered, and (3) an evolutionary viewpoint on institutions. The paper sets the frame for the thesis and contributes to the first and third guiding question by defining which concepts of institutions can underlie an empirical project and by asking questions about what we have to consider if we are interested in studying institutional *change* by means of economic experiments.

The **second paper** demonstrates the structural viewpoint as defined in the first paper and contributes to answering the second question. In a coordination game where subjects can choose between the bus or the car (i.e., a framed field experiment that focuses on mode choice), traffic policy is exogenously varied to study its impact on the behavior of participants.

In a similar approach, the **third paper** alters the sequence of play by introducing “leading by example.” In one treatment of a voluntary contribution mechanism public good game, subjects play in parallel; in another treatment, one participant moves first, and then, the other players decide, being aware of the first person's contribution. It extends the approach of the second paper by altering an important variable of the group context layer: the

distribution of wealth. Participants also receive information on the distribution of wealth. The paper also contributes to answering the third question. Specifically, it demonstrates that in the absence of a behavioral rule in one context, rules and norms known from other contexts can substantially alter behavior.

The **fourth paper** is based on a natural field experiment. Participants do not know they are part of an experiment and decide in their natural environment (cf. Harrison & List, 2004). The study explores the role of what is perceived to be the status quo decision in a specific context. In particular, it demonstrates that if subjects are forced to decide either for or against a “no junk mail” sticker, their choices differ from a situation in which the status quo is not to use a sticker. The paper contributes to answering the second and third questions by exploring how higher level rules and norms affect decision-making.

The **fifth paper** takes a normative viewpoint by asking whether economic experiments could be used as a tool for achieving pro-social behavioral change. Specifically, it investigates how the experience of play in a social dilemma game affects subsequent pro-social behavior as measured by a donation. The paper contributes to answering the second question by investigating how experience in one context transfers to another context. Table 1 displays a summary of the empirical papers, distinguishing between the layers of information players assess.

**Table 1:** Main topics of empirical papers by information layers

<b>Paper Number</b>	<b>Material Payoffs Game Layer</b>	<b>Group-Context Layer</b>	<b>Identity Layer</b>
II	Coordination problem; no dominant strategy; dynamics over time and interaction with alterations in payoff	Expectations on the behavior of others	Self-image as a car driver (upper middle class) or bus user (lower middle class)
III	Social dilemma with dominant strategy but possibility to set a positive example in the leadership treatment	Heterogeneity in wealth (in the game) is explicitly communicated to players	Interaction of leadership skills and experience with leadership institution in the game

IV	Individual benefits depend on the interest in receiving ads and the benefits from saving paper	Perception of what is the social norm	Self-image with respect to environmental behavior
V	Main interest in altruism (other-regarding preferences) by means of a dictator game	Perception of what is the social norm	Self-image with respect to environmental behavior; learning and experience

Source: own design

The dissertation culminates in a **concluding part**. Here, the work is summarized, and the lessons learned for institutional analysis are synthesized. A major outcome of this synthesis is a research framework for the study of evolutionary institutional change as defined in the **first paper**. The **empirical papers** contribute to the development of this framework by suggesting specific aspects that could be potentially important in the study of institutional change.

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## 2. LITERATURE REVIEW (P1): WHAT CAN ECONOMIC EXPERIMENTS TELL US ABOUT INSTITUTIONAL CHANGE IN SOCIAL–ECOLOGICAL SYSTEMS?

Jens Rommel  
Humboldt-Universität zu Berlin  
Department of Agricultural Economics

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**Abstract:** Today, economic experiments are well accepted in mainstream economics. They are also widely applied in ecological economics, often focusing on institutions. Yet, many empirical studies in this field lack a sound theoretical foundation of institutions and institutional change. In this paper, I show that in a structure–agency framework three theoretical viewpoints on institutions can be distinguished. They can be viewed as (1) structures that shape incentives, (2) cognitive media, embedded in beliefs and cognition of agents, or (3) evolving from a process of agents’ choices and structural aspects. Using examples from the empirical experimental literature, I argue that all three viewpoints are mirrored in experimental practice, and that these can be organized into an institutional typology of economic experiments. Placing special emphasis on experiments with endogenous institutional choice and their relationship to evolutionary economic theory, I discuss under which conditions experiments are a useful method for the analysis of social–ecological systems, concluding that methodological and methodical innovations are an important prerequisite for challenging the dominant paradigm of neoclassical economics.

**Keywords:** Evolutionary economics; Experimental economics; Institutional change; Methodology; Social–ecological systems

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## **2.1. Introduction**

In the economic literature there are at least three ways in which institutions are distinguished in a structure–agency framework. First, institutions can be understood as *structures* that enable or constrain agents’ choices. Second, institutions can be understood as embedded in *agents*, for example through norms, which may be socially or culturally determined, eventually being responsible for behavioral differences. Third, institutions may evolve as the result of both agents’ choices and the impact of structures on agents’ preferences for institutions. This *evolutionary* viewpoint implies that institutions are ontologically inseparable from structures and agents (Hodgson, 2004).

Implicitly, these three different conceptualizations of institutions are mirrored in the empirical practice of experimental economics. For example, classical market experiments involve the exogenous variation of trading rules (*structures*) to test subjects’ behavior under different auctioning mechanisms (Ketcham et al., 1984). The much-cited ultimatum game experiments, conducted across fifteen small-scale societies, assume that culturally determined fairness norms embedded in game participants (*agents*) are reflected in experimental play (Henrich et al., 2004). More recent experiments also allow subjects to change the rules of an experiment (within certain limits), while at the same time investigating the *evolution* of participants’ preferences for institutions as the result of their past choices, game outcomes, or initial institutions (Botelho et al., 2005).

The aim of this paper is to spell out more explicitly these relationships between the theoretical work on institutions and the empirical practice of experimentalists by means of an “institutional typology of economic experimentation,” which allows for the classification of economic experiments by their conceptualization of institutions. By accentuating these links, it is possible to expatiate on the connection of institutional economics to experimental methods. Ultimately, a sound theory–method link will be important in attracting more empirical projects in the relatively young field of economic experimentation, which would allow for “endogenous institutional choice.” As I will argue, this is related to the debate on the endogenous evolution of institutions in the economic system (Bowles, 1998; Brousseau and Raynaud, 2011; Field, 1984; Herrmann-Pillath, 2013; Nelson and Winter, 1982; van Bergh and Stagl, 2003).



Although the typology developed here is more generally applicable, my examples are drawn mostly from the field of ecological and environmental economics, for at least three reasons. First, empirical applications in environmental management have been among the first to use experiments with a specific focus on the role of institutions (Ostrom et al., 1994). This initial study has encouraged a large empirical literature using experimental methods in this field (List and Price, 2013). The importance of institutions for governing global ecosystems is also of high practical relevance, with humanity facing potentially disastrous global environmental change (Young, 2002). Second, experiments in ecological economics often combine the interaction of participants' behavior with temporal and spatial dynamics of ecological systems, making them more prone to a dynamic and evolutionary perspective than experimental work based on mainstream economic theory. Third, the field is currently witnessing a particularly lively debate on methodology and methods, including an ongoing debate on the use of qualitative vs. quantitative methods and the use of mixed methods, often also explicitly referring to economic experiments (Beckmann and Padmanabhan, 2009; Ménard, 2001; Poteete et al., 2010; Robin and Staropoli, 2008; Schlüter, 2010).

The remainder of the paper is structured as follows. First, starting with a definition of social-ecological systems, I briefly review theories of institutions and institutional change and organize them into a structure–agency framework. Second, following a short introduction to the use of experiments in economics, I present some examples of the empirical literature in light of these theories. Next, I discuss how the developed concepts may facilitate insights from experimental research on institutional change in social-ecological systems. Finally, I summarize and conclude.

## **2.2. Social-ecological systems and institutions**

In the most general way, a social-ecological system (SES) comprises a social system, an ecological system, and the interactions between the two. Traditionally, ecosystem analysis has studied the impact of human and geophysical drivers of ecosystem change in isolation. The definition of a SES goes beyond this view. SESs are defined as coherent systems with multiple (often non-linear) interactions that span across (hierarchically linked) scales, which consist of critical resources, whose flows and uses are affected by both social and ecological factors, and which are dynamic and adaptive (Redman et al., 2004).

According to Ostrom (2009), a SES can also be viewed as comprising four “first-level core subsystems,” namely: (1) a resource system, (2) resource units, (3) a governance system, and (4) users. A set of “second-level” variables that are useful for SES analysis, and which can be sorted by these core sub-systems, has also been identified. Institutions are part of both social sub-systems, i.e. the governance systems and users. More specifically, the overall property-rights system, operational, collective-choice, and constitutional rules are variables of the governance system, while norms are a variable of the user system (Ostrom, 2009). Focusing on this point, and in the spirit of structuration theory (Giddens, 1984), the following sections will focus on understanding the structure–agency dynamics of institutions and institutional change.

### **2.2.1. Institutions between structure, agency, and evolution**

There is still relatively little agreement on the basic concepts in institutional economics, or in the words of Arild Vatn (2005): “Just as there are many theories of what institutions are and what they do, there are certainly also many different ways of explaining their development and change.” It is not the aim of this paper to present an exhaustive review of this ongoing debate; others have already done this (Hodgson, 2006; Hodgson, 1998; Schmid, 2004; Vatn, 2005; Zikos and Thiel, 2013). Rather, I would like to show that it is useful to distinguish between (1) structural, (2) agent-based, and (3) evolutionary approaches of institutions (Hodgson, 2004), with the aim to better understand the concept of institution on which particular, empirically grounded economic experiments are based.

Vatn (2005) distinguishes two ways of defining an institution. Institutions can either be viewed as “external reward structures” that constrain (North, 1990) or enable (Bromley, 1989) agents, or as “internalized motivations” of economic agents (Berger and Luckmann, 1967). The structural view is most prominently represented by Douglass North, who defines institutions as “humanly devised constraints that structure political, economic and social interaction” (North, 1991). To the contrary, Berger and Luckmann (1967) view institutions as embedded in economic agents, representing a “reciprocal typification of habitualized actions by types of actors.” The two viewpoints are sometimes combined, resulting in an understanding of institutions as consisting “of cognitive, normative, and regulative structures and activities” (Scott, 1995).

Evolutionary economic theory emphasizes that institutions are endogenous to the economic process (Herrmann-Pillath, 2013; Hodgson, 1998; van Bergh and Stiglitz, 2003) and

ontologically not reducible to their structural or agent-based aspects (Hodgson, 2004). Both economic agents – with their shared beliefs and (limited) cognition – and structures play a role in the establishment and change of institutions. It has been pointed out that especially the cognitive aspects have been much neglected in (institutional) economics. A psychological or even neurological perspective on context-dependence, framing of decisions, habitualization, and their temporal dynamics could shed new light on the processes that constitute institutional choice (Herrmann-Pillath, 2013; Hodgson, 2004; Schlüter, 2009). Put differently, preferences for institutions are endogenous, dynamically changing over time, and domain-specific (Herrmann-Pillath, 2013).

### **2.2.2. Institutional change**

By definition, “[i]nstitutional change covers both the process of changing an existing institution and the establishment of an institution in a field where no institution has existed before” (Vatn, 2005). In an overview on theories of institutional change, Vatn (2005) draws a distinction between spontaneous and designed institutional change. This change can come from “below,” as the result of deliberative action, or it may result from “pure” spontaneous change. Theories of “designed institutional change” can be categorized into those “driven by efficiency considerations” and those “driven by the intent to protect specific interests or values” (Vatn, 2005).

More generally, *objective* institutional design is viewed as exogenous to the affected agents; it is principally limited to an alteration of the incentive structure; and it assumes stable preferences (Alexander, 2005; Goodin, 1996; Thiel et al., 2014). This perspective is equivalent to what I will call the structural perspective on institutional change throughout this paper. In contrast, *subjective* institutional design is assumed to affect actors’ beliefs and cognition (Aoki, 2011) and will in the following also be called the *agent*-based view on institutional change. In a third, *evolutionary* viewpoint, these two are combined in a process, with feedback loops existing between agents and structures. Further, it is assumed that agents can impact institutions through choice. Institutions governing social-ecological systems will also relate to properties of the two systems; they may be affected by the framing of the discourse, or the underlying epistemology. Under these conditions, institutions may also evolve as outcomes of social learning processes (Ison et al., 2014; Ison et al., 2007).

A major achievement was made by Hodgson (2004) in singling out the structure and agency aspects of institutions in the evolutionary framework: institutions as structures<sup>1</sup> affect the distribution of resources, agents' preferences, and subsequent institutional choice of agents (cf. Hodgson, 2004; van Bergh and Stiglitz, 2003). In this process, institutions and preferences co-evolve. It may thus be difficult to find an (objective) starting point for analyzing institutional change. This chicken and egg problem was also stressed by Field (1984), who shows the limits of game theoretical reasoning and the efficiency view on institutions in neoclassical microeconomics (cf. Vatn, 2005). Likewise, Hodgson (2004) emphasizes that in the study of institutional change it:

“is simply arbitrary to stop at one particular stage in the explanation and say ‘it is all reducible to individuals’ just as much as to say it is ‘all social and institutional’. The key point is that in this infinite regress, neither individual nor institutional factors have legitimate explanatory primacy. The idea that all explanations have ultimately be in terms of individuals (or institutions) is thus unfounded. Once we admit that the individual is socially determined then we have an explanatory infinite regress, and neither individuals nor institutions can be the legitimate final term.”

Summing up, institutions can be viewed as structures exogenous to the agent, or as cognitive media embedded solely in the agent. In contrast, evolutionary institutional economics highlights the inseparable co-evolution of institutional structures and agents' cognition, beliefs, behavior, and choice. In the following, I will show that the three viewpoints of institutions outlined here are implicitly reflected in the practice of experimental economists.

### **2.3. An institutional typology of economic experiments**

Different theoretical schools of thought in institutional economic theory can be linked to a particular set of accepted methods. The proponents of new institutional economics and its offspring, transaction cost economics, use to a large extent the same methodical toolbox as neoclassical economics (Beckmann and Padmanabhan, 2009). In contrast, the so-called

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<sup>1</sup> Hodgson writes: “A social structure is a set of significant relations between individuals that can lead to causal interactions. Social structures can involve rules, norms, meanings, communication and much else. These relations can be acknowledged or unacknowledged by the individuals involved. Furthermore, social structures can survive the demise of particular individuals that once related to them. Accordingly, the study of human social systems is more than the study of human individuals, because society embodies relations and properties in addition to those of the individuals themselves. Although structures frame and condition behaviours, they are neither reducible nor ontologically equivalent to them” (Hodgson, 2004).

classical institutionalism and evolutionary economic theory have availed themselves largely to economic history and case study analysis (Schlüter, 2010). To some extent, both strands acknowledge the importance of economic experiments. Empirical studies are relatively silent, however, about their theoretical understanding of institutions and institutional change in specific contexts. After a short introduction, the following sections review empirical work in experimental economics with respect to the underlying concept of institutions.

### **2.3.1. A brief introduction to experimental economics**

With several Nobel laureates having come from the field, experiments are now a well-accepted method of mainstream economics. Experiments are sometimes employed to study individuals' risk preferences in simplified decision tasks. More commonly, however, the focus is on the strategic interaction of multiple players. Typically, experiments in economics involve monetary rewards contingent on performance. This helps to overcome the incentive problems and social desirability biases of survey research. A key advantage is that contextual factors, available information, the framing, rules of the game, or subject composition can be exogenously manipulated, allowing the identification of causal effects – a challenge in observational studies (Parmeter and Pope, 2013).

Various classifications of experiments exist in the literature. It is usually helpful to distinguish experiments by the type of game played (Viceisza, 2012), which is important for making theoretical predictions on participants' play, using the various equilibrium concepts offered by behavioral game theory (Camerer, 2003). Also, the subject pool (e.g. students vs. farmers), the level of experimental control (e.g. strict computer-based lab experiments vs. field settings with sometimes open communication), and the reward medium (monetary vs. non-monetary rewards related to the context of the game) provide useful criteria along which a classification of an experiment and an assessment of its external validity are possible (Harrison and List, 2004).

Along these lines, (1) lab experiments, (2) artefactual field experiments, (3) framed field experiments, and (4) natural field experiments can be distinguished.<sup>2</sup> Lab experiments use students as a subject pool and rely on abstract definitions of rules, while artefactual field experiments use non-student subjects. Framed field experiments add local context and try to reduce the level of abstraction, and in natural field experiments subjects typically do not

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<sup>2</sup> A summary of experimental work until the early 1990s can be found in chapter 9 of Friedman and Sunder (1994).

know that they participate in an experimental study, and researchers observe subjects' actual behavior in context. The following three sections develop a typology that is based on the underlying concept of institutions.

### **2.3.2. Experiments on institutions as structural alternatives**

Traditionally, economic experiments have been employed to evaluate alternative market institutions, for instance in auction design (Ketcham et al., 1984; McCabe et al., 1989). By following this approach:

“experiments allow one to investigate the incentive and performance properties of alternative exchange institutions, and, with respect to institutional design, they provide a low-cost means of trying, failing, altering, trying etc. This process uses theory, loose conjecture, intuitions about procedural matters and, most important, repeat testing to understand and improve the features of the institutional rules being examined” (McCabe et al., 1993).

In other words, behavior and “performance” under exogenously determined institutions are in the center of interest; the focus is on evaluating objectively designed institutions (cf. Thiel et al., 2014).

Such experiments have also been widely applied in an environmental context, for instance to test the effect of alternative policy designs in environmental markets on behavior and efficiency in the lab (Tucker et al., 2013). A good example of this approach can be found in Chidambaram et al. (forthcoming), who conducted a framed field experiment on a mode choice coordination problem, in order to understand the effect of traffic policy on commuters' behavior. Subjects repeatedly choose between using the bus or the car. In several treatments, traffic policies are exogenously varied and changes in behavior are analyzed.

From a theoretical perspective, these experiments understand institutions as exogenous structures which constrain or enable economic agents and define the choice set. They are, thus, most consistent with the theoretical viewpoints of North (1990) or Bromley (1989), and subscribe to an (objective) planning perspective on institutional change (Alexander, 2005).

### **2.3.3. Experiments with institutions embedded in agents**

A second group of experiments views institutions as embedded in agents' cognition. Often these experiments focus on fairness, or the "emotional costs" of violating rules and norms, which are assumed to be socially or culturally acquired by subjects. A common way to implement this empirically is to compare behavior across different groups of subjects. Henrich et al. (2004) show that playing the same ultimatum game yields very different results across fifteen small-scale societies, which they ascribe to culturally determined differences in fairness norms. In an honor-based money box experiment, instead of varying the subject pool, Schlüter and Vollan (2011) use questionnaire data on religious background and other socio-economic variables as proxies for internalized social norms to measure delta-parameters, i.e. the "moral costs" of rule violation.

Another way to study differences in behavior is to expose the same subject pool to different framings of an otherwise identical game, implicitly also assuming that it "is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects" (Harrison and List, 2004). Indeed, it can be shown that the same game yields fairly different results depending on the contextual framing used (Cronk, 2007; Dufwenberg et al., 2011; Rege and Telle, 2004). In a similar vein, Rommel et al. (2014) show that priming subjects with either a positive or a negative experience affects pro-environmental behavior as measured by a donation to an environmental organization. These findings clearly violate preference stability as posited by mainstream economic theory, and put into question a purely structural view on institutions, since the observed deviations from stable preferences can be explained only by cognitive aspects. However, environmental applications of cross-cultural or cross-national (within-culture) experiments, and experiments on framing effects are still relatively rare (Gerkey, 2013; Prediger et al., 2010; Rege and Telle, 2004; Vollan, 2012).

In this second group of experiments, institutions are not exogenously varied by the researcher. They are assumed to be embedded in agents and evoked by either selecting a particular subject pool or by stimulating beliefs in agents through modifications or experimental framing. Subjects are asked to perform decision tasks that are in other respects comparable. This brings this group of experiments close to an understanding of institutions as "shared beliefs" (Bar-Tal, 2000; Berger and Luckmann, 1967).

#### **2.3.4. Experiments with endogenous institutional choice and evolutionary change**

A third group of experiments substantially differs from the other two. Here, rules are not fixed over the course of the experiment, but evolve as the result of participants' experience, behavior, and (aggregate) choice. In one way or another, institutions are endogenously varied by participants, most commonly in iterated or multi-step games (e.g. Botelho et al., 2005; Dal Bó et al., 2010; Decker et al., 2003; Ertan et al., 2009; Guillen et al., 2007; Gülerk et al., 2006; Kroll et al., 2007; Reuben and Riedl, 2009; Sutter et al., 2010). Subjects make two decisions: (1) they individually decide on what to do in the game (for instance how much to extract from a common pool resource), interacting with others under a certain set of rules; and (2) they also decide on what the rules of the game shall be (for instance whether or not sanctioning systems should be implemented). Institutional preferences are then aggregated and put into practice in subsequent rounds. In the SES context, some experiments also add temporal or spatial dynamics of ecological systems to such games with endogenous institutional choice (e.g. Janssen et al., 2008; Otto and Wechsung, 2014).

The basic rationale of this approach has been perhaps best described by Botelho et al. (2005), who state that:

“inferring preferences from the outcome of play under different institutions is a difficult, if not impossible, task. The solution to this problem is to *expand the experimental design to allow subjects to directly choose which institution they would prefer to operate under*. There are many possible ways in which subjects could choose an institution. There could be a direct referendum vote, some committee or public choice process, a bargaining process, or migration to locales that are differentiated by institutions” (emphasis in the original).

This is important, they argue, because “[i]f subjects in the field have mechanisms by which they can avoid, lobby or self-select into or out of institutions, we must consider the effects of those margins of choice before drawing conclusions about which institutions are best. Another way to express this is to consider if the laboratory environment that takes a particular institution as fixed is correctly modeling the naturally occurring environment in it's [sic!] salient features, if that environment includes ways in which subjects can endogenously opt out of that institution” (Botelho et al., 2005).



Sutter et al. (2010) provides a good example of such an experiment. They compare contributions with endogenous sanctioning and reward institutions to a reference scenario, in which institutions have been enforced exogenously by the researcher. In this study and in most other studies, without explicit recognition, two ontologically different viewpoints on institutions are taken: in one case, institutions are altered endogenously from bottom-up in *subjective* design; whereas in the other case, institutions are *objectively* designed.

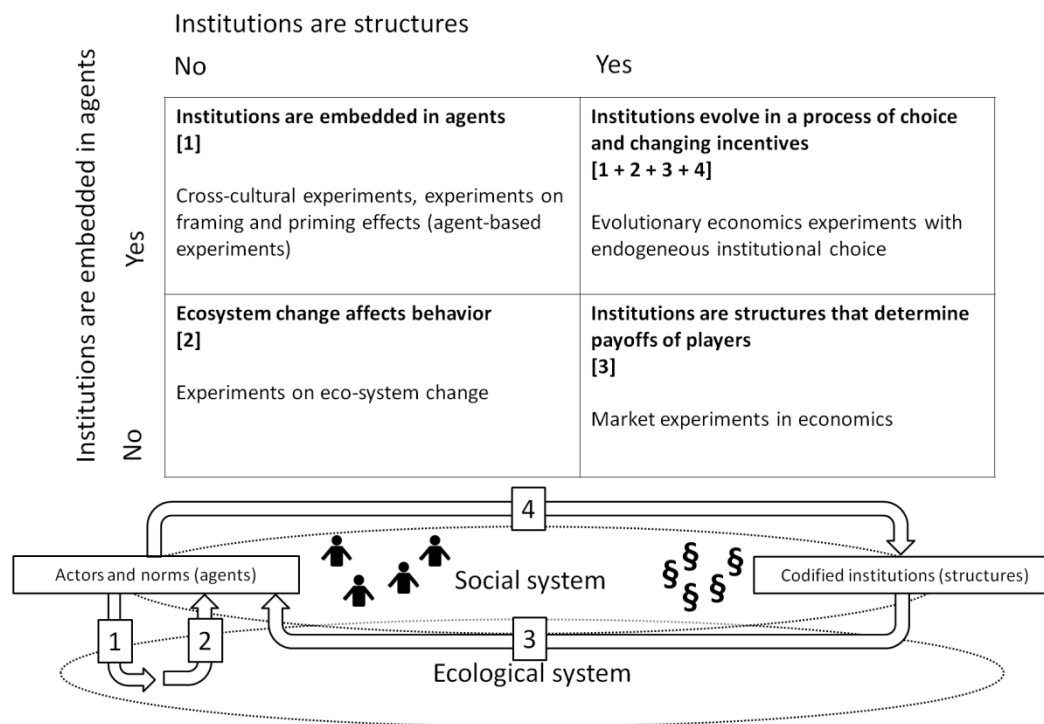
Applications in ecological economics typically focus on the self-governed commons and the evolution of sanctioning or reward mechanisms (Bischoff, 2007; Carpenter, 2000; Walker et al., 2000). The field experiments conducted by Cárdenas (2004), for instance, employ a protocol in which local resource users play a CPR game and have the possibility to implement endogenous (monitoring) institutions.

Walker et al. (2000) are also interested in comparing outcomes for different voting schemes on allocation rules in a common pool resource dilemma, but they additionally include treatments without voting (exogenous variation) in their design. They find that “voting substantially increases the efficiency of the outcomes achieved” (Walker et al., 2000). These experiments highlight that understanding how and by whom rules are changed, and how interactions with ecological systems take place needs more attention in empirical work.

## **2.4. Synthesis and discussion**

It is notable that virtually none of the experimental applications with endogenous institutional change explicitly refer to the structure–agency dynamics outlined above or, for that matter, evolutionary concepts in economic theory more generally. Sometimes, contradictory theoretical concepts are used in empirical work. Otto and Wechsung (2014), for example, vary the subject pool (students vs. farmers vs. administrators), compare different monitoring rules, *and* let subjects choose among these rules. Distinguishing more explicitly between a structural, cognitive, and evolutionary perspective on institutions, as well as recognizing the impact of ecosystem dynamics on behavior, could increase the analytical clarity and precision in behavioral research dealing with the institutional analysis of SES. Towards this aim, and drawing on the previous sections, the following figure provides a synthesis of the proposed typology.

**Figure 2:** A typology of economic experiments for the institutional analysis of social-ecological systems



Source: own design

The upper part of the figure summarizes the classification of experiments by the underlying concept of institutions, providing also examples of such experiments. The lower part of the figure shows a simplified version of a social-ecological system, which emphasizes the institutional aspects of the social component and leaves aside ecological sub-systems, for didactic reasons (cf. Ostrom, 2009). The focus is on structural aspects, such as codified laws (symbolized by the section sign) which constrain or enable agents' choice sets, and on agency aspects (symbolized by the human characters) that may differ across agents. The numbered arrows indicate (unidirectional) causal links. These are echoed in the brackets of the upper part if the respective experiment type takes into account a particular causal link.

The lower left quadrant is reserved for experiments that do not focus on institutions at all. In a general sense, this includes all experiments that vary contextual factors, such as income distributions or group sizes, without explicitly relating these to (short-term) structural institutional change. In SES research, these experiments include studies focusing on the effect of ecosystem change and dynamics on behavior (e.g. Cárdenas et al., 2013; Janssen and Ostrom, 2008; Janssen, 2010; Moxnes, 1998). The upper left quadrant shows

the class of experiments focusing on differences in agents, and sub-subsequent differences in behavior affecting the ecological sub-systems (cf. section 2.3.3). The lower right quadrant is reserved for experiments that (comparatively) study the effect of exogenously varied rules without taking into account ecosystem aspects (cf. section 2.3.2). The upper right quadrant includes all other links and also allows for variation in the (structural) rules of the game (cf. section 2.3.4).

Although the upper right quadrant reflects the most realistic and complete viewpoint of institutional change in SES, it may practically not always be feasible or even desirable to include all possible feedback loops and channels of causation in empirical work. When faced with the decision of how to design a particular experiment, the proposed typology may be used to pragmatically decide on useful analytical abstractions in the spirit of evolutionary economic theory. Evolutionary economics rejects most of the assumptions of neoclassical economics, such as utility maximization or methodological individualism (Herrmann-Pillath, 2013; Hodgson, 2004). Partly because of this definition *ex negativo*, it has not always been clear what evolutionary economics stands for, or which methods are compatible with its approach. Although this has led to some fragmentation of the field, including a debate on the ontology of evolutionary economics, and a lack of applied empirical work (Herrmann-Pillath, 2001, 2013), the theories and methods used in neoclassical economics are not *generally* rejected. Rather, emphasis is put on the justification of why “bits and pieces” are sought to be transferred into a particular empirical context.

If properly legitimated, the pragmatic use of “analytical abstractions” (Hodgson, 2006) may lead to the application of all four forms of experiments discussed above.<sup>3</sup> A key aspect added in the figure above is the *interaction* of social and ecological systems. Not only do agents and structures affect each other, but agents also affect their (physical) environment and vice versa. Although, at least since the seminal work of Nicholas Georgescu-Roegen, the thermodynamic foundations of economic systems are well-known in ecological

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<sup>3</sup> Geoffrey Hodgson writes on this point: “In some circumstances it is legitimate to take the individual as given, as a simplifying abstraction. But individuals nevertheless remain to be explained. The task may be postponed, but it does not disappear. Similarly, the origins of the institutional structures that carry transaction costs also require an explanation. Furthermore, these deeper questions of explanation become vital once issues such as the influence of culture, the emergence and durability of institutions, and long-term economic development, become agendas of enquiry. It is important to understand how individual interactions lead to new institutional developments. But especially in the long term, it is also important explain – at least in principle – how institutions or circumstances can affect individuals and alter their perceptions and goals” (Hodgson, 2004).

economics (cf. Herrmann-Pillath, 2013), experiments have not been explicitly considered as a method in this context.

Today, experimental protocols are often transferred across contexts, without spending much attention on the applicability in new settings. Although this procedure allows for the – desirable – comparison of experimental findings across nations or cultures, it is somewhat questionable as to how far processes of endogenous institutional choice are really taking place within the same meta-framework (cf. Otto and Wechsung, 2014). In other words, in future it will also be important to think more seriously about constitutional rules, i.e. rules on the change of rules. Considering these aspects opens up new opportunities for both conceptual and empirical work, because institutional change driven by agents' choices is highly diverse with respect to the *mechanism* through which consensus is reached and institutions are altered (Levine and Nalebuff, 1995). As noted by Botelho et al. (2005), institutions could change through “a direct referendum vote, some committee or public choice process, a bargaining process, or migration to locales that are differentiated by institutions.”

In this context, several questions of practical relevance come up. Can institutions be altered by majority vote, and should all users be equally powerful in such a vote? Should institutional change be induced through an open communication phase and then be implemented only if agreed upon unanimously? Should there be a cost involved in bringing about institutional change? What are the default rules that are in place if no agreement on altering an institution is reached? Answering some of these questions will be necessary to better understand the meta-level of constitutional rules.

On a related note, it will be important to pay due respect to the frequency and way in which institutions change. Some of the institutions governing collectively managed common pool resources are several hundred years old (Ostrom, 1990). In such settings it may be difficult to study the institutional *choice* of agents experimentally, because participants may be unaware of the history of the institutions and their change when these are not part of their immediate personal experience. It is also important to evaluate an empirical context with respect to the way institutions change. Not in all instances are agents the main driver of (local) institutional change. Global market forces may, for instance, have a strong structural (and sometimes destructive) effect on locally evolved institutions.

The potential of experiments on cognition and framing effects in social-ecological systems is still poorly developed and could be further explored, also taking into account experimental work within environmental psychology. For the most part, environmental psychologists and ecological economists have worked in isolation. Questions of increased variability in ecosystems may play a role, focusing for example on the *perception* of resource availability uncertainties in social-ecological systems, where relatively little is known about drivers of and barriers to institutional change embedded in agents. This also allows further exploration of the effect of cultural, religious, ideological contexts, or cognitive biases (Liebig and Rommel, 2014) on institutional choice.

For all of these questions, the typology developed in this paper is of heuristic value, as it allows for the grouping and structuring of empirical research in institutional ecological economics. In particular, it has been shown that economic experiments can be employed in line with the ontology of evolutionary economic theory.

## **2.5. Summary and conclusion**

In this paper, I have developed an institutional typology of economic experiments. Theories that understand institutions as structures can be linked to classical experiments on alternative (market) institutions. Theories that see institutions as shared beliefs are related to cross-cultural comparisons, and framing effects in experiments. Evolutionary theories of institutional change are mirrored in economic experiments that employ endogenous institutional choice, i.e. subjects are allowed to change the rules of the game. Further, I have discussed the potential and limitations of such experiments in the analysis of social-ecological systems, and I have pointed out some unresolved issues with respect to constitutional rules, i.e. rules on how to change the rules.

In the past, experiments on social-ecological systems have sometimes focused on the unidirectional impact of humans on ecosystems. Others have looked at the effect of exogenous ecosystem change on human behavior. Experiments on institutions have studied behavioral change under alternative sets of rules, often focusing on the social system and neglecting ecosystem impacts. The typology developed in this paper combines these aspects, and allows for interactions among them, promoting a complex, adaptive system perspective on SES. It also recognizes the thermodynamic foundations of economic systems, and is consistent with evolutionary economics – a field that has suffered from a lack of rigid methods.

Evolutionary economics differs substantially from the dominant paradigm of neoclassical economics and its offspring, new institutional economics. Evolutionary theories embrace a more realistic, less autistic, and less formalized way to address questions of economic change. For example, the concept of *general* equilibrium and maximization behavior of agents are rejected with the aim to “bring back life back to economics” (Hodgson, 1996). These theories provide a better fit for addressing the complex questions of institutional change in social-ecological systems. Ultimately, the dominance of the neoclassical school can only be challenged with a well-developed methodical tool box, and it will be critical to provide policy-relevant knowledge to address the many challenges of global environmental change.

Everyone engaged in research knows how important the use of rigid methods is, especially in the science–policy dialogue. I believe that the various forms of economic experiments have a lot to contribute to the growing field of (evolutionary) institutional economics. Notwithstanding the necessity of a sound theoretical base, I hope that the ideas developed in this paper provide a rough guide for an experimental and more empirically oriented approach for the institutional analysis of social-ecological systems.

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### 3. EMPIRICAL STUDY I (P2): COMMUTERS' MODE CHOICE AS A COORDINATION PROBLEM: A FRAMED FIELD EXPERIMENT ON TRAFFIC POLICY IN HYDERABAD, INDIA

Bhuvanachithra Chidambaram  
Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Marco A. Janssen  
Arizona State University  
School of Human Evolution and Social Change, Center for the Study of Institutional Diversity

Jens Rommel  
Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Dimitrios Zikos  
Humboldt-Universität zu Berlin  
Department of Agricultural Economics

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**Abstract:** All major Indian cities face a severe transport crisis, with the number of cars on the road increasing every day. Policy makers are trying to keep pace with this growth by supplying more roads, largely neglecting demand-side policy measures. We have developed an economic experiment to investigate behavioral responses of citizens to such measures. Drawing on a sample of 204 white-collar commuters from Hyderabad, India, we model mode choice as a coordination problem and analyze how bus subsidies, increased parking costs, and public information on preferential car use can affect mode choice. We find that pecuniary treatments are effective for shifting behavior towards socially more desirable outcomes and increasing total benefits. Mode choice is relatively unaffected by socio-economic variables like gender, education or income but is significantly affected by actual traffic behavior. We discuss limitations of the applied sampling, conclude with a critical evaluation of the strengths and weaknesses of economic experiments in transportation research, and offer an outlook on how further experimentation could enrich the policy debate.

**Keywords:** Coordination game; Experimental economics; Hyderabad; India; Mode switching; Public transport

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### **3.1. Introduction**

India’s economic growth is being fueled by the development of its cities, as the rapidly growing service sector has created many jobs in the urban centers of the sub-continent. Rising incomes have led to an expanding middle class and, in combination with population growth, to a tremendous increase in the number of cars in Indian cities. Especially in the megacities of Mumbai, Delhi, and Kolkata and the emerging megacities of Bangalore, Chennai, and Hyderabad, this has led to a “transport crisis.” Authorities face the challenge of how to limit the growth of individualized traffic, find adequate ways of promoting public transport, and, more generally, develop a sustainable transport system (Pucher et al., 2005).

In attempting to confront this crisis, policy makers have largely focused on supply-side measures. With limited success, the dominant paradigm has been to increase supply of roads (Chidambaram, 2011) or focus on promotion of large-scale infrastructure projects, such as metro-rails (Mohan, 2008; Ramachandraiah, 2007). On the other hand, demand-side measures have not been adequately addressed in the policy debate on this issue (Chidambaram, 2011; Pucher et al., 2005). We believe that, to evaluate the scope of such

measures, it is important to understand the preferences and actual decision-making processes of traffic participants, which can however be difficult and costly to do in practice. Stated preferences methods such as choice experiments have been widely applied in transport economics (cf. Ben-Akiva and Lerman, 1985; Ortúzar and Willumsen, 2011), and traffic psychologists have used survey research to investigate the role of attitudes in mode choice (Gardner and Abraham, 2007). Yet, these studies have not usually addressed the interdependence of individual choices and subsequent dynamics, which game theory and experimental economics explicitly do take into account. Furthermore, subjects in such settings are motivated by payoffs which, in principle, should largely determine their decision-making (Smith, 1976).

In transport modeling, various theoretical approaches have attempted to address the interdependence of travelers in mode or route choice (Hollander and Prashker, 2006). From the perspective of game theory, choosing between public and private transport can be understood as a coordination problem, because using a car appears to be more attractive when the number of cars is small, whereas public transport seems more attractive during periods of heavy traffic (Vugt et al., 1995).

In spite of their great potential, empirical applications building on game theory and using the empirical toolkit of experimental economics are still relatively rare in transportation research (de Jong, 2012). Also, focus has typically been on market modeling (de Jong, 2012), with only a few studies analyzing individual choices (e.g. Gabuthy et al., 2006; Hartman, 2012; Selten et al., 2007). As far as we are aware, no economic experiment has yet used traffic framing to study behavioral effects of demand-side policy measures in a developing country. Drawing on middle-class white-collar commuters from Hyderabad as a subject pool, we develop here an economic experiment to test behavioral response to three popular measures that have been discussed in the policy debate on demand-side policies (Wootton, 1999), namely, increasing parking costs, subsidizing bus fares, and providing information to enhance coordination. All three of these measures can be implemented by urban authorities, as opposed to fuel taxes, for instance, which would require policies on the state or central government levels. In the experiment, we investigate the effects of these measures on solving the coordination problem while analyzing the socio-economic determinants of mode choice and the role interdependence plays for individual choices.

In the next section, we provide background information on the transport policy debate and the neglected role of demand-side measures in urban India, focusing on Hyderabad. The third section introduces some key theoretical concepts, describes our empirical approach, and lays out the key research hypotheses. Sections four and five present the results and discuss our findings with regard to the policy debate. The concluding section assesses our approach and offers an outlook on how further experimentation could enrich the policy debate.

### **3.2. Hyderabad's transport crisis and the role of demand-side policy measures**

According to a recent report on Hyderabad's traffic challenges, the city faces a severe mobility crisis (CSE, 2011). Every day, more than 600 vehicles are being added to the existing population of about three million vehicles. In fact, vehicle growth is faster than population growth by a factor of four, which is seen as one reason for the steady decline of travel speed over the last two decades (CSE, 2011). Frequent violation of traffic rules further adds to the problem (Dandona et al., 2005). Air pollution from traffic, especially from trucks, regularly reaches unacceptable levels and has contributed to an increase in respiratory disease and eye irritation (CSE, 2011; Sharma et al., 2010).

In spite of the ambitiously set targets for increasing the share of public transport to 80 percent in the City Development Plan (GHMC, 2006), the actual share of public transport has been constantly declining and is projected to drop from the current 35 percent to only 12 percent by 2031 (CSE, 2011). It will be important for the authorities to embrace more realistic planning goals in the future. Currently, the share of car trips is around 20 percent (CSE, 2011, p. 24). Under the Jawaharlal Nehru National Urban Renewal Mission (GoI, 2005) – a national funding scheme for urban infrastructure development – Hyderabad has received a tremendous amount of funding for investing in infrastructure (HPEC, 2011), the vast majority of which has been spent on constructing new roads and flyovers (CSE, 2011). However, these major public works have not led to substantial relief of the city's tense traffic situation. One of the key reasons for this is that road expansion has not been combined with the promotion of public and non-motorized transport (CSE, 2011). Also, the bus transport system has not been improved; instead, a costly and risky large-scale metro-rail is being planned. In the metro-rail planning process, citizens' preferences and attitudes have been widely neglected (Mohan, 2008; Ramachandraiah, 2007).



According to a recent study, more than 90 percent of surveyed citizens in Hyderabad perceive frequent congestion as a major problem for the city; there is great discontent with the public transport system, which is rated as “good” by only 16 percent of the population, as compared to 40 percent which describe it as “poor”; the majority of people believes that there is insufficient space for the use of bicycles and walking; and congestion caused by illegal parking is seen as a severe problem by about 92 percent (CSE, 2011). It would seem to follow from this that, from a citizen’s perspective, parking costs and parking restrictions, subsidized and improved public transport, and restricted car use should be the most relevant demand-side measures to consider for Hyderabad. It is claimed that complementing supply-side measures with policies to manage demand can help in solving the problem of traffic congestion in cities (Bull, 2003; Ferguson, 1990). However, the question remains concerning what the chances of success for demand-side measures could be in an arena dominated by strong desires for technical solutions and monumental constructions as symbols of development.

Generally speaking, reducing the use of cars has not often been achieved voluntarily. Instead, demand-side management must use “carrots and sticks” to control travel behavior (Meyer, 1999), including measures such as parking costs, parking prohibitions, vehicle use restrictions, road pricing, provision of traffic information or subsidies for public transport (Bull, 2003; Ison and Rye, 2008). Pricing or coercive restrictions all have their particular merits and demerits, which have been extensively discussed in the literature (cf. Gärling and Schuitema, 2007; Schade and Schlag, 2003). In the Indian context, both India’s National Urban Transport Policy and the Jawaharlal Nehru National Urban Renewal Mission have highlighted the importance of demand-side measures, such as funds for subsidized bus fares (GoI, 2006). However, implementation has been slow, with only three Indian cities – Jaipur, Delhi and Mumbai – having yet achieved some implementation of bus subsidies (Cropper and Bhattacharya, 2007; Tiwari and Jain, 2012). Until now, discouraging or restricting the use of cars has not been considered as an option by India’s metropolitan authorities. Most likely, such measures are seen as being rather unpopular among the newly emerging middle and upper classes, so the underlying urban political economy is preventing their effective implementation.

Based on the policy debate just outlined, we have developed an economic experiment aimed at shedding some light on the effects three demand-side measures – bus subsidies, parking costs, and externally provided information – might have on individual behavior.

More importantly, this experiment allows us to explore the socio-economic determinants of behavioral factors and to analyze the interdependence of individual choices.

### 3.3. The experiment

Different methods exist for studying traffic behavior, a variety of which are summarized in the following table in terms of some of their particular strengths and weaknesses. It also includes examples of recently published studies from this journal.

**Table 2:** Schematic Methodical Literature Review with Empirical Examples in Relation to this Study

Method	Strengths	Weaknesses	Recent TR-A Example
Lab experiments	Behavioral control	Artificiality	Sunitiyoso et al. (2011)
	(Monetary) incentives	Design limitations, with only a few	
	Interaction of agents	factors which can be studied	
Framed field experiments	Behavioral control	Artificiality	This study
	(Monetary) incentives	Design limitations, with only a few	
	Interaction of agents	factors which can be studied	
Natural field experiments and field trials	Some behavioral control	Design limitations, with only a few	Schuitema et al. (2010)
	Behavior under real world incentives	factors which can be studied	
		Relatively low control, survey information often hard to obtain	
		High costs	

Agent-based Modeling	Study of interaction and dynamics in complex systems over long periods of simulation  Large combinations of factors and their interaction can be studied	Artificiality  Empirical calibration often difficult	Guo et al. (2013)
Survey-based stated and revealed preferences methods	Random sampling and subsequent statistical generalization  Detailed quantitative information on respondents and preferences	No interaction  No incentives  In some cases of stated preferences “hypothetical bias”  Sometimes “social desirability bias”	Beck et al. (2013)
Participatory methods and case study research	Allows complex narratives  In-depth understanding of motivations	Often very context-specific  Difficult to establish causal relationships  Difficult to measure effects	Wahl (2013)

Source: own formulation

The table shows that various tradeoffs exist when choosing a method, notably between different kinds of experiments.<sup>4</sup> Controlled lab and framed field experiments and agent-

<sup>4</sup> Experiments in which subjects undertake a task in their natural environment and typically do not know that they are part of an experimental study are called “natural field experiments” within the taxonomy of Harrison and List (2004). An example of such an experiment in the transport realm would be Knockaert et al. (2012). In this study, participants were sampled through noting down license plates of frequent commuters on the Dutch highway A12 and contacting them by mail. In addition, snowball sampling was used. Participants then received rewards for avoiding morning rush-hour traffic. Subjects knew they were part of an experimental study, but their behavior was observed in a natural context which is sufficient for qualifying as a “natural field experiment” within the Harrison and List (2004) taxonomy. In a developing-country context, such

based modeling allow the study of interacting agents and emergent properties in dynamic systems. They are well-suited for testing general behavioral hypotheses, but it is sometimes difficult to extrapolate their results to the actual world. Survey methods offer a better fit in this regard and allow for rich data collection on many different aspects of actual transport behavior. However, it is often difficult to understand interaction between respondents. Case study research provides in-depth insights which cannot be obtained by any other method. However, it is more prone to response bias, and it is more difficult to establish causal theoretical links from such studies. In the end, we agree that pluralism and application of mixed-method designs will be important for helping us to understand complex phenomena from diverse methodological perspectives (see Poteete et al., 2010).

### **3.3.1. Behavioral Game Theory in Transportation Research**

A variety of theoretical models exist for studying the interdependent behavior of individuals in transport research using game theory (Hollander and Prashker, 2006).<sup>5</sup> With respect to mode choice, “commuting by car is more attractive” when “fewer individuals choose to go by car because of a lower probability of time delays caused by traffic jams and/or parking problems. However, when the number of other people commuting by car exceeds a certain limit, the choice for public transportation may yield greater outcomes for oneself because congestion can be avoided” (Vugt et al., 1995).<sup>6</sup> Expectations about the behavior of others, hence, become critical for determining choice and subsequent utility and payoffs.

Coordination problems have been widely studied under lab conditions in macroeconomics or in so-called market entry games (see, for example, Cooper, 1999 and Erev and Rapoport, 1998 for reviews). Lab applications in transportation research are, however, still rare (de Jong, 2012). The following table summarizes some key experimental studies in relation to our own.

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experiments can be logistically very demanding, however. It is a particular strength of “framed field experiments” that they can be carried out more easily. They can also be more directly related to economic theory, whereas “natural field experiments” have the great advantage of higher external validity. For a critical review on the relationship of the two, see Harrison (2013).

<sup>5</sup> Most empirical experimental applications focus on coordination problems. When the focus is on environmental externalities, the underlying game resembles a social dilemma (Vugt et al., 1995). There is an emerging literature (Chidambaram, 2011; Frischmann, 2012) which is trying to integrate the economics of infrastructure with the literature on common-pool resources, under the assumption that car drivers, by “subtracting road space,” may create a “tragedy of the commons” (Hardin, 1968), with each car user reducing the utility of infrastructure use for everyone else.

<sup>6</sup> Note that this relationship holds only if public transport is unaffected or less affected by congestion. Such coordination problems also exist for route choice or the starting time of a journey (Selten et al., 2007).

**Table 3:** Schematic Empirical Literature Review in Relation to this Study

Reference	Description and Results	Subject Pool and Mode of Conduct
Gabuthy et al. (2006)	Route choice as a coordination problem  Predicted Nash equilibrium is not reached; behavior is sensitive to tolling regime	96 management students from Lyon, France  Computer-based lab experiment
Hartman (2012)	Route choice as a coordination problem under an efficient toll (external travel cost added to others is internalized through the toll)  Travel time is substantially reduced by the toll, and the outcome under the toll is close to efficiency	180 subjects (not further specified)  Computer-based lab experiment
Iida et al. (1992)	Route choice as a coordination problem  No equilibrium reached and substantial fluctuations until the end	40 civil engineering students from Kyoto, Japan  Computer-based lab experiment
Schneider and Weimann (2004)	Congestion externalities as a social dilemma  Treatment which allows subjects to play with more than one car increases efficiency (externality is partly internalized)	40 economics students from Magdeburg, Germany  Computer-based lab experiment
Selten et al. (2007)	Route choice behavior as a coordination problem  Fluctuations persist; no convergence to Nash equilibrium; subjects who switch a lot earn less	216 economics students from Bonn, Germany  Computer-based lab experiment
Sunitiyoso et al. (2011)	Mode choice as a social dilemma (public good game); treatments with communication and feedback/information on	15 post-graduate students from the University of West England, Bristol, UK  Computer-based lab experiment without financial

	contributions in other groups	incentives
	No statistically significant effect of the treatments on contributions to the public good	
Vugt et al. (1995)	Mode choice between coordination problem (travel time) and social dilemma (environmental concerns) with a psychological focus on attitudes/social values	56 citizens of Maastricht, the Netherlands, recruited through newspaper advertisement Computer-based lab experiment
	Social-value orientations can explain mode choice	
Zieglmeier et al. (2008)	Departure time as a coordination problem	128 students from Strasbourg, France
	Congestion occurs according to prediction; information, number of drivers and costs of delay have no effect on behavior	Computer-based lab experiment
This study	Mode choice as a coordination problem	204 commuters from Hyderabad, India
	Treatments of different policy measures	Pen-and-paper field experiment

Source: own formulation

As the table shows, in spite of their growing popularity, “framed field experiments” have not yet been applied in transportation research. According to Harrison and List (2004), economic field experiments can be categorized according to the nature of the subject pool, available information, the commodity traded, trading rules, the stakes, and the environment subjects are placed in. Using a non-student subject pool and a contextual framing, our experiment would qualify as a framed field experiment.

Coordination games are also largely absent from the growing body of “lab-to-the-field” experiments (Viceisza, 2012). Thus, our study differs from previous experiments in the transport realm in at least three important aspects. Firstly, by altering payoffs and framing, we have studied the effect of policy measures on behavior. Secondly, we have conducted the first lab-to-the-field experiments with non-student subjects in transportation research.

Thirdly, we have conducted the first experiment on traffic behavior in a developing-country context.

In addition to context-related framing and nonstandard subject pools, experimental research on the collective management of natural resources has often gained additional insights from employing post-experimental surveys to gather socio-demographic information on experimental subjects and using this data in econometric models (Hayo and Vollan, 2012; Vollan, 2008; Werthmann, 2011). We have taken up this approach, as it allows us to identify individual characteristics which may affect mode choice in an experimental context.

### **3.3.2. Experimental Design and Hypotheses**

Our experiment was a coordination game with  $n$  ( $= 6$ ) players who each make a choice to use a bus ( $x = 0$ ) or car ( $x = 1$ ) for commuting. The payoff for the individual player depends on their own decisions and those of the other players, as depicted in Table 4.<sup>7</sup> Payoffs are inversely U-shaped and follow Greenshields' (1935) speed-flow model.<sup>8</sup> The social optimum is to have one car and five participants riding the bus. Note that the individual in the car will have a higher payoff in the social optimum. Therefore, the social optimum is unstable. If players are selfish and rational, and expect that others are too, they will choose the car if they expect there are zero, one or two other participants choosing it. If participants expect three or four other participants to choose the car, choosing the bus leads to better earnings. The participant is indifferent if she expects all others to choose the car. Hence, there are various Nash equilibria, dependent on the expectations of the participants.

Participants' payoffs include operating costs as well as travel time. The latter depends on the choices of the other players, since both cars and buses use road space. In spite of low car ownership, the streets of Hyderabad are already heavily congested, so already at low fractions of car use in the experiment (e.g. when going from one to two car users out of six), congestion is taken into account by a reduction in payoffs for both, car users and bus users. If more persons will use the car, congestion will get worse and the payoffs for both modes will be further reduced.<sup>9</sup>

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<sup>7</sup> The payoffs in the table are presented in "tokens." One token is equal to one Rupee paid to the subject after the game.

<sup>8</sup> For more details on how the payoffs were calculated, see the supplementary material below.

<sup>9</sup> Note that, in another game, one could assume separate bus lanes. This would result in a lower decrease in payoffs for choosing the bus. Travel time would stay constant independent of cars, and comfort would be

**Table 4:** Payoffs for Baseline Rounds

<b>Players' Aggregate Choices</b>	<b>0/6</b>	<b>1/5</b>	<b>2/4</b>	<b>3/3</b>	<b>4/2</b>	<b>5/1</b>	<b>6/0</b>
<b>(Number of Cars/Number of Bus Users)</b>							
Individual payoffs if car	-	23	18	14	9	4	0
Individual payoffs if bus	15	14	13	12	9	0	-
Summed payoffs	90	93	88	78	54	20	0

Source: own formulation

The instructions of the experiment were framed as a traffic mode-choice decision situation: White-collar participants ( $n = 204$ , grouped in 34 experiments) were asked to picture a situation where every morning they would decide to take either a bus or car to commute to their offices. Individual payoffs depended on the decisions of five other commuters who also face the same binary choice. If many participants chose the car, travel times would increase and, hence, the expected benefit from car travel would decrease. As in actual traffic behavior, the choices of other players are known only after a particular round and only in aggregation (i.e. the total number of car drivers and bus passengers). A table depicting payoffs, based on each player's own choices combined with those of the other five subjects, was handed out to each participant on paper (see section two in supplementary material).

Based on the policy debate outlined in section 2, the following hypotheses were formulated.

**H1:** Increasing the attractiveness of taking a bus versus a car by changing the payoff structure through a bus subsidy or parking cost will lead to a higher proportion of participants choosing the bus.

**H2:** Coordination improves when participants do not have to rely solely on self-coordination. By providing them with a model of how to behave when facing an ill-defined problem, the sum of payoffs can be increased.

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slightly reduced (crowding in the bus). The specific design of payoffs will then depend on commuters' preferences for travel time and comfort.



To test these hypotheses, we designed the following treatments: A baseline scenario was combined with three treatments in a mixed within- and between-subjects design, which is summarized in the following table.<sup>10</sup>

**Table 5:** Overview of the Experimental Design

	<b>Exercise 1</b>	<b>Exercise 2</b>	<b>Exercise 3</b>
	<b>(6 rounds)</b>	<b>(6 rounds)</b>	<b>(6 rounds)</b>
Bus subsidy group, 17 groups of six players, 102 subjects	Baseline treatment	Bus subsidy treatment (+3 tokens for taking the bus)	Public coordination
Parking cost group, 17 groups of six players, 102 subjects	Baseline treatment	Parking cost treatment (-3 tokens for taking the car)	Public coordination

Source: own formulation

Table 5 shows that each of the 204 subjects needed to make 18 binary decisions. Groups were randomly assigned to either the bus subsidy or the parking cost group. Both treatments modified payoffs by either adding or subtracting three tokens to/from the baseline payoffs, providing pecuniary incentives to change behavior.<sup>11</sup> The psychological effects of the framing might have induced loss aversion in the parking cost treatment, whereas the bus subsidy may have been perceived as a gain (Tversky and Kahneman, 1991).

In the public coordination treatment (testing H 2) players were informed that, to enhance coordination and increase social benefits, a “central planner” would announce one player who would be allowed to take the car in one particular round and that every player would be allowed to do so exactly once.<sup>12</sup> There was to be no enforcement or sanctioning

<sup>10</sup> Our rationale for choosing this design was based on several pre-tests run by the authors with German graduate students. The results of this testing are not published and were only used to improve the instructions and questionnaire. In principle, within-subject designs allow for more powerful statistical testing, as all fixed effects such as gender are effectively controlled for within subjects. On the other hand, within-designs are more prone to demand, learning, ordering effects and fatigue. A recent discussion on the topic can be found in Charness et al. (2012).

<sup>11</sup> Note that there are other important transport policies which are beyond the scope of this paper. These include, for instance, the promotion of shorter and fewer trips, higher car-occupancy ratios, and non-motorized transport.

<sup>12</sup> This was done by announcing the ID Code of the player, which was only known by the respective player, not by the others.

implemented regarding this restriction – a condition which was also explained to the subjects. Thus, each participant was still free in her or his decisions, meaning that it was possible to choose the car when it was “someone else’s turn” or to take the bus when one’s car-taking turn was announced. Assuming a “purely economic” approach to rule violation<sup>13</sup> (Becker, 1968), observed treatment effects would, thus, not result from a change in the payoff structure but rather from a change of expectations regarding the behavior of others or from “moral discomfort.”

Aggregate payoffs were maximized if only one subject chose the car. In the two treatments which modified payoffs, group payoffs were also maximized if everyone went by bus, although with different distributional effects. Individual strategies deviated from the social optimum, as Nash best responses depend on subjects’ expectations of what others will do. In the baseline and public coordination treatments, it was individually rational to choose the car if a player expected two or less other players to also take it. Players were indifferent when three other cars were expected. When treatments were implemented, this changed. Here subjects were able to increase their payoffs by taking the car only if not more than one other player chose it as well. Thus, in all treatments, the Nash best response clashed with the social optimum, defined as the combination of choices which maximizes aggregate payoffs.

### **3.3.3. Sampling and Practical Conduct of the Experiment**

The experiment was run with 204 subjects in 34 groups on 21 days in August and September 2012 in Hyderabad. Subjects were recruited according to their familiarity with the task, because it is not necessarily the case “that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects” (Harrison and List, 2004). In other words, subjects should have experience with *both* car and bus use. In Hyderabad, white-collar workers commuting to their offices fulfill these conditions best, and participants were recruited from offices throughout the city, including employees from private companies, government bureaucrats and universities. Some of the organizations were known to the authors, others were contacted by telephone from the telephone directory. Break rooms or conference rooms of respective buildings where participants worked were used to run the experiment, and in most cases participants knew each other. Results were announced to all players after each round, though

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<sup>13</sup> Here we refer to the rule that a player is “allowed” to take the car only in one out of six rounds. However, no monetary consequences resulted from violating this rule.

maintaining the anonymity of their choices. Subjects were not allowed to talk, and they were seated in a way that they could not directly see each other. A short general introduction was given by a facilitator. After questions from players were answered, subjects then received written instructions for the first exercise. After completion, instructions for the next exercise were distributed. Following the experiment, a brief survey was conducted which contained questions on the socio-economic background of participants, their everyday traffic behavior and their attitudes regarding different policy measures and traffic-related problems.

A typical session lasted about one and a half hours. Subjects received a show-up fee of 200 Rupees (about four US Dollars) plus their variable experimental earnings.<sup>14</sup> We provide a description and summary statistics of the variables used in the analysis in the following table.

**Table 6:** Description and Summary Statistics of Independent Variables

<b>Variable Name</b>	<b>Description</b>	<b>Obs.</b>	<b>Mean</b>	<b>SD</b>	<b>Min</b>	<b>Max</b>
MALE	= 1 if subject is male	204	0.71	0.45	0	1
AGE	Age in years	203	31.00	9.95	17	60
MARRIED	= 1 if married	202	0.49	0.50	0	1
INCOME1	= 1 if income below 5,000 Rupees per month	172	0.08	0.27	0	1
INCOME2	= 1 if income between 5,000 and 15,000 Rupees per month	172	0.22	0.42	0	1
INCOME3	= 1 if income between 15,000 and 50,000 Rupees per month	172	0.49	0.50	0	1
INCOME4	= 1 if income between 50,000 and 100,000 Rupees per month	172	0.16	0.37	0	1
INCOME5	= 1 if income above 100,000 Rupees per month	172	0.05	0.21	0	1
OWNSCAR	= 1 if households owns a car	204	0.11	0.32	0	1
OWNSBIKE	= 1 if households owns a motorbike	204	0.50	0.50	0	1
CARFREQ1	= 1 if respondent is	203	0.50	0.50	0	1

<sup>14</sup> Median, mean, standard deviation, minimum, and maximum of the variable earnings were 234, 237, 28.55, 146, and 322 Rupees, respectively. For our sample, the mean earnings are approximately equal to half of a daily wage. One survey item asked about the degree of agreement with the statement “I could understand the instructions of exercise 1,” to which more than 90 percent of the subjects chose “strongly agree” or “agree” on a five-point Likert scale. About half of the sampled subjects were using a car at least once a month. This gives us some evidence for claiming that the instructions were clear enough for subjects and that familiarity with the task was likely among our selected sample.

CARFREQ2	never using the car = 1 if respondent is using the car up to ten times a month	203	0.33	0.47	0	1
CARFREQ3	= 1 if respondent is using the car up between ten and 20 times a month	203	0.08	0.27	0	1
CARFREQ4	= 1 if respondent is using the car more than 20 times a month	203	0.09	0.28	0	1
BUSFREQ1	= 1 if respondent is never using the bus	202	0.20	0.40	0	1
BUSFREQ2	= 1 if respondent is using the bus up to ten times a month	202	0.31	0.46	0	1
BUSFREQ3	= 1 if respondent is using the bus up between ten and 20 times a month	202	0.17	0.38	0	1
BUSFREQ4	= 1 if respondent is using the bus more than 20 times a month	202	0.33	0.47	0	1

Source: own calculations, based on field data

The table shows that only eleven percent of the participants actually own a car.<sup>15</sup> On the other hand, half the participants use a car at least once a month. It should be noted that the Indian situation is very different from that in Western countries. Cheap labor allows even middle-class people to have drivers. Especially in Hyderabad, “travel agencies” offering relatively cheap car rental are prevalent and it is not uncommon to rent cars with drivers on a daily basis. Often only professional drivers have licenses; thus, the use of data on driving licenses is not very informative regarding people who make the decision to use cars. Sampling more “upper class” participants might have resulted in a sample with higher car ownership. On the other hand, this subject pool would very likely have problems relating to the option of taking a public bus. Indeed, about a fifth of the sample was not using buses at all at the time of the experiment. After careful assessment of the pros and cons of sampling different subject pools, we decided to look for a target population for whom deciding between bus and car was really an option.

<sup>15</sup> Exact figures on car ownership of private households are not available for Hyderabad. There are about 300,000 cars registered – including all vehicles which are used for commercial purposes – in the city (CSE 2011, p.25). With a total population of about eight million people, these figures give us some indication that car ownership is above the average in our sample. In the game, the situation was described as “using your own car.” This may be a small drawback in the framing of the instructions, as there are several ways to use a car without owning one in Hyderabad. On the other hand, for most middle class participants owning a car is within reach, at least in the medium term. The game abstracted from this point, ownership of driver licenses, and having more than one person in a car, which could all be important points to explore in further research.

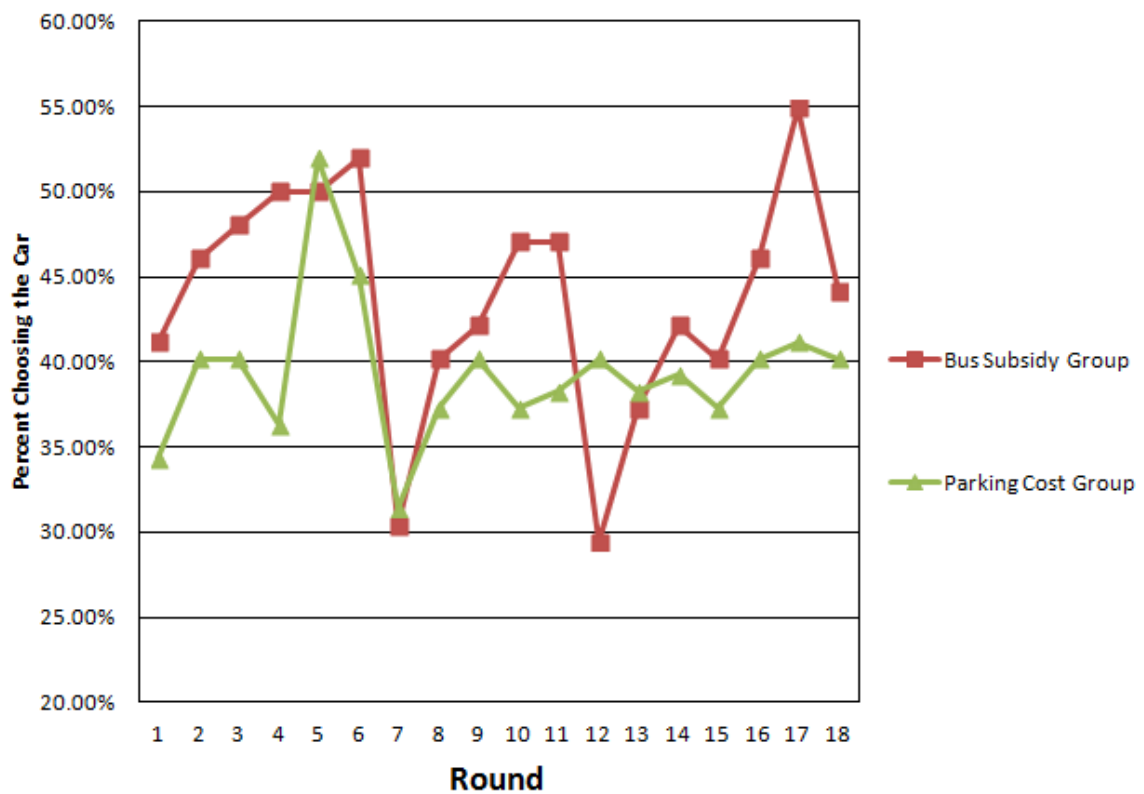
Compared to the general population of Hyderabad, our sample is apparently biased towards males with high income. On the other hand, we believe it is more representative of the commuting white-collar population, because it includes many subjects who qualify as part of the new emerging middle class of India, for whom car ownership is becoming an attainable goal.

### 3.4. Results

#### 3.4.1. Analyzing Treatment Effects

In the baseline condition, we see an increase of car usage over the rounds. Initially, about two of the participants chose a car on average. These figures increase to about three in round 6 as can be seen from the following figure.

**Figure 3:** Relative Frequency of Choosing the Car by Rounds and Between-subject Treatments



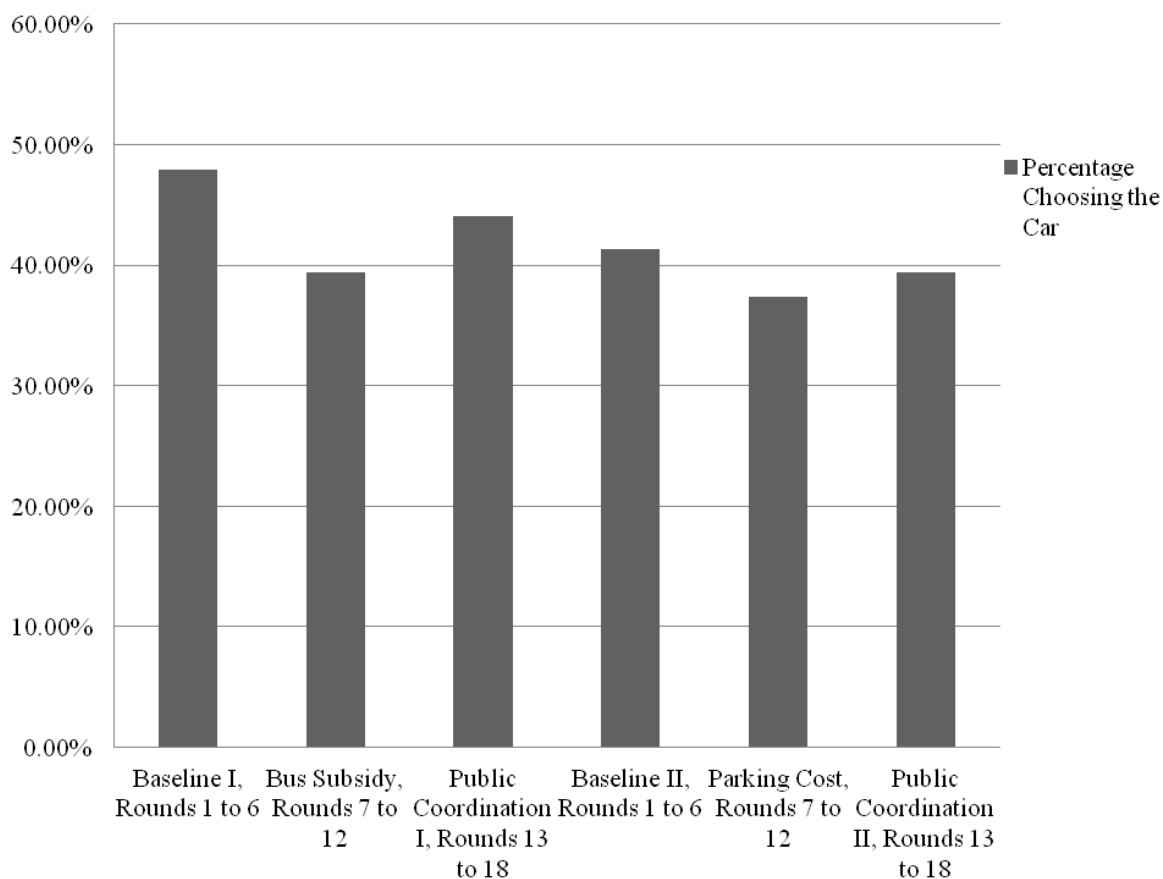
Source: own design, based on field data

Introduction of the bus subsidy or parking fee initially has a positive effect on the group, but over time we see an increase of choosing the car. The same happens when we move to the public coordination treatment. It can also be seen that, on average, players choose the car “too often.” In all rounds there is more than one car on average, suggesting inferior

outcomes for most groups. Recall that in the baseline rounds and in the public coordination treatment it is individually rational to choose the car only when two or less other players use one. On average, players stayed within the limit of having three or less cars in total in these rounds. In the two pecuniary treatments, it is individually rational to choose the car if one expects not more than one other player to use one. However, on average, choosing the car was fairly above the 33 percent which would have been individually rational.

Figure 4 depicts the average proportion of subjects choosing the car during the various exercises across the two different versions of the game.

**Figure 4:** Proportion of Car Users for each Exercise



Source: own design, based on field data

As can be seen from the figure, introducing the treatments reduces the number of cars relative to the baseline. The pecuniary treatments demonstrate a larger decrease in the number of cars chosen as compared to the public coordination condition. With the new treatment in round seven, we observe a sharp drop in cars chosen (see Figure 3).<sup>16</sup>

<sup>16</sup> This might be explained by a “restart effect.” In that case, the introduction of a new treatment acts as a new opportunity for coordination. What happens later in a particular treatment may also be influenced by results

Compared to the first six rounds, the number of cars is slightly lower in rounds 7 to 12, but, on average, this effect is not very large (Figure 4). Introduction of the public coordination treatment (in round 13) prompts a small difference. Aggregated across all six rounds, the bus subsidy treatment reduced the proportion of cars from 47.9% to 39.4% compared to the baseline treatment. Formal hypothesis testing by comparing the first round of each treatment reveals that the difference is not statistically significant at the five-percent level (McNemar's test  $\chi^2 = 3.10$ ;  $p = 0.0782$ ). If we compare rounds six and seven, however, the difference in the drop – more than 20% – becomes highly significant (McNemar's test  $\chi^2 = 11.52$ ;  $p = 0.0007$ ).

The parking cost treatment reduced the proportion of cars from 41.3% to 37.4%. Testing for the difference between subjects' decisions in rounds one and seven reveals, however, that this difference is only a small one and statistically not significant (McNemar's test  $\chi^2 = 0.27$ ;  $p = 0.6015$ ). The proportion of cars drops from 45% to 30% when comparing rounds six and seven, however, and this reduction is statistically significant (McNemar's test  $\chi^2 = 5.44$ ;  $p = 0.0196$ ).

Compared to the baseline scenario, introducing public coordination reduces the proportion of cars from 44.6% to 41.8% (McNemar's test for first round decisions  $\chi^2 = 0.00$ ;  $p = 1.0000$ ). The difference between the bus subsidy and the parking cost treatment is small, with proportions of 39.4% and 37.4%, respectively. Testing these proportions between subjects for the decisions made in round seven reveals that this difference is not statistically significant (Two-sample test of proportions  $z = -0.15$ ;  $p = 0.8795$ ). The difference between rounds 12 and 13, that is, the pecuniary treatments and the public coordination treatment, is also not significant (McNemar's test for the increase in the parking cost treatment  $\chi^2 = 1.60$ ;  $p = 0.2059$  and for the decrease in the bus subsidy treatment  $\chi^2 = 0.11$ ;  $p = 0.7456$ ). However, aggregated earnings of players are higher in the public coordination treatment (79.73 Rupees) as compared to the baseline scenario (75.94 Rupees). This difference of about five percent is statistically highly significant (Paired t-test  $t = -4.0543$ ;  $p = 0.0001$ ).

Summing up, the pecuniary treatments moderately reduce the number of cars on average. This effect is small, however, and is statistically significant only when comparing rounds 6 and 7, not when comparing the first rounds of the treatments. Also, the reduction of cars

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of earlier rounds. We have, thus, compared the *first* rounds of each treatment and *neighboring* rounds across treatments with each other.

induced by the public coordination treatment is relatively small. Yet it did help players to achieve significantly higher earnings in the public coordination condition.

Another way to evaluate the monetary demand-side policy measures used is from the perspective of total social benefits created and efficiency,<sup>17</sup> including for the “transport authority.” The table below depicts aggregated earnings and choices by treatments.

**Table 7:** Evaluating the Efficiency of Monetary Policy Measures

		<b>Baseline I</b> <b>(rounds 1 to 6)</b>	<b>Treatment I</b> <b>(Bus subsidy, rounds 7 to 12)</b>	<b>Baseline II</b> <b>(rounds 1 to 6)</b>	<b>Treatment II</b> <b>(Parking costs, rounds 7 to 12)</b>
Frequency of choosing the bus	of	319 (52.12%)	371 (60.62%)	359 (58.66%)	383 (62.58%)
Frequency of choosing the car	of	293 (47.88%)	241 (39.38%)	253 (41.34%)	229 (37.42%)
Earnings of all players in tokens		7,920	9,085	7,572	7,506
Total costs/income of the “transport authority” in tokens	None		Costs for subsidizing 371 bus rides x 3 = 1,113	None	Income from 229 times the parking fee x 3 = 687
Net benefits, including “transport authority” in tokens		7,920	7,972	7,572	8,193

Source: own calculations, based on field data

This table presents calculations taking into account the costs and benefits of the policy treatments. As can be seen, both policies increase total benefits over the particular rounds. But the parking cost policy generating earnings for the transport authority in the game and the bus subsidy introducing a cost are factors that should also be carefully considered. The

<sup>17</sup> This is also very often done in social dilemma experiments with punishment or communication. Typically, subjects – when given a choice – opt for sanctioning institutions (Gürrer et al., 2006). Cooperation typically increases with communication (Sally, 1995) and (costly) punishment (Fehr and Gächter, 2000). However, it has also been shown that efficiency is not always increased when punishment is costly. Gains from increased cooperation are sometimes outweighed by punishment costs (Ohtsuki et al., 2009; Bochet et al., 2006).



net benefit increases by 52 Rupees in the bus subsidy condition (from the second to the third column in the last row of the table) and by 621 Rupees in the parking cost condition (from column 4 to column 5 in the last row of the table). Under the bus subsidy condition, players' earnings are raised by 1,165 Rupees in total. However, this comes at a high cost of almost the same size for subsidizing bus rides (1,113 Rupees). Note that in the parking cost condition player earnings are reduced by only 66 Rupees while, at the same time, the parking fees generate an income of 687 Rupees for the transport authority. It can thus be said that, in our game, the parking cost measure appears to have been more efficient.

### 3.4.2. Analyzing the Socio-economic Determinants of Mode Choice

Table 8 presents six specifications of binary logistic regression models on subjects' mode choices in the experiment (0 = bus, 1 = car). The first five columns pool the data. The sixth column presents coefficient and standard error estimates of a random effects model. To study the effect of socio-economic characteristics on mode choice, we have included the independent variables described in Table 6 to models (3) to (6). The first model uses only the dummy variables on the treatment. The second model includes other variables from the game. The third model (3) tests for learning effects by including the round as an independent variable.<sup>18</sup> In model (4), we have included dummy variables on the treatments (PARKCOST, BUSSUB, and PUBCOORD), and in the fifth column (5) we have also included variables for the number of cars in the previous round (of a treatment) to test for changes in expectations based on what others have done in the previous round.<sup>19</sup> We use dummy variables to capture expected non-linearity in the effect (from the game design and the underlying coordination problem). Models (2), (4), and (5) also include the variable CARPREVROUND, indicating whether a player has chosen the car in the previous round, and the variable ALLOWED, a dummy variable indicating whether a particular player was the one who was "allowed" to take the car in a particular round of the public coordination

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<sup>18</sup> Due to multicollinearity – with the highest estimated variance inflation factors (VIF) being 12.30 in a linear probability model – specifications including both the round and the treatment dummy variables are not presented here. Estimation results of the respective linear probability models are presented in the supplementary material. A VIF indicates the degree of uncertainty with respect to a coefficient's standard error estimate in a linear regression. The square root of a VIF indicates how much larger a standard error estimate is due to correlation of the independent variables. Such bias in standard error estimates will also affect test statistics and p-values which ultimately may result in erroneous conclusions. Note that coefficient estimates are not affected by collinearity.

<sup>19</sup> In these two dynamic models, observations of the first round of a treatment (i.e. rounds 1, 7, and 13) have been excluded, which explains the smaller number of observations. In these models (columns 2, 5, and 6 in the table), 1CAR is a dummy variable taking the value of 1 for round  $t$  if there has been in total *one* player in round  $t - 1$  choosing the car; 2CARS takes the value of 1 if *two* players in the previous round have chosen the car, and so on. The reference category for this variable is "no cars," i.e. all players have chosen the bus.

treatment. Model (6) uses the same data as (5), but with a random effects model for estimation to address the serial correlation of the 18 choices observed per subject.

**Table 8:** Binary Logistic Regressions on Mode Choice in the Games

	(1) Pooled data	(2) Pooled data	(3) Pooled data	(4) Pooled data	(5) Pooled data	(6) Random effects
MALE			-0.2721 (0.1680)	-0.1984 (0.1464)	-0.1884 (0.1485)	-0.3185 (0.2349)
AGE			-0.0081 (0.0103)	-0.0084 (0.0092)	-0.0082 (0.0092)	-0.0107 (0.0153)
MARRIED			0.1183 (0.2035)	0.1912 (0.1783)	0.1865 (0.1784)	0.2624 (0.2866)
INCOME2			-0.4037 (0.3529)	-0.4412 (0.3222)	-0.4372 (0.3227)	-0.6001 (0.4228)
INCOME3			-0.0075 (0.3494)	-0.0556 (0.3169)	-0.0409 (0.3185)	-0.0163 (0.4050)
INCOME4			-0.1410 (0.3810)	-0.1328 (0.3458)	-0.0982 (0.3483)	-0.1398 (0.4748)
INCOME5			-0.0539 (0.4244)	-0.0020 (0.3960)	0.0323 (0.4003)	0.0364 (0.6191)
OWNSCAR			-0.0564 (0.2786)	0.0103 (0.2489)	-0.0090 (0.2540)	-0.0248 (0.3874)
OWNSBIKE			0.1592 (0.1900)	0.1062 (0.1663)	0.0935 (0.1686)	0.1528 (0.2411)
CARFREQ2			-0.0356 (0.1989)	-0.0703 (0.1762)	-0.0780 (0.1779)	-0.1149 (0.2458)
CARFREQ3			0.4485 (0.3315)	0.2617 (0.2994)	0.2536 (0.2955)	0.3831 (0.4395)
CARFREQ4			0.3126 (0.2951)	0.1338 (0.2578)	0.1422 (0.2607)	0.3044 (0.4048)
BUSFREQ2			-0.5822** (0.2364)	-0.4126** (0.2054)	-0.4007* (0.2062)	-0.5942* (0.3066)
BUSFREQ3			-0.4470 (0.2965)	-0.3562 (0.2561)	-0.3582 (0.2589)	-0.5119 (0.3599)
BUSFREQ4			-0.6678*** (0.2485)	-0.5176** (0.2142)	-0.5068** (0.2163)	-0.7706** (0.3182)
ROUND			-0.0028 (0.0068)			
PARKCOST	-0.2978** (0.1198)	-0.2741** (0.1109)		-0.1557 (0.1153)	-0.1924 (0.1181)	-0.2417 (0.1478)
BUSSUB	-0.2149* (0.1157)	-0.2023** (0.1026)		-0.2062* (0.1205)	-0.2259* (0.1202)	-0.3513** (0.1497)
PUBCOORD	-0.1166 (0.0757)	-0.3080*** (0.0839)		-0.3340*** (0.0946)	-0.3347*** (0.0942)	-0.4241*** (0.1203)
CARPREVROUND		0.8851*** (0.1257)		0.8210*** (0.1260)	0.7987*** (0.1336)	0.0193 (0.1188)
ALLOWED		1.0701*** (0.1837)		1.1425*** (0.2046)	1.1413*** (0.2041)	1.3699*** (0.2211)
1CAR		0.3590* (0.2044)			0.4282** (0.2168)	0.4381* (0.2650)
2CARS		0.4348** (0.2118)			0.5102** (0.2256)	0.6340** (0.2566)
3CARS		0.3838* (0.2182)			0.4076* (0.2336)	0.4553* (0.2629)
4CARS		0.2362 (0.2373)			0.2653 (0.2640)	0.3042 (0.2900)
5CARS		0.6003** (0.2865)			0.5809* (0.3258)	0.7345** (0.3375)
6CARS		0.5844 (0.4913)			0.6097 (0.4937)	0.5007 (0.5182)
Constant	-0.2165*** (0.0811)	-0.9172*** (0.2117)	0.5120 (0.4225)	0.1944 (0.3814)	-0.2231 (0.4148)	0.3015 (0.6576)
N	3672	3060	2988	2490	2490	2340
Pseudo R <sup>2</sup>	0.002	0.048	0.023	0.058	0.060	
Akaike Information	1.357	1.308	1.334	1.301	1.303	

Criterion						
$\sigma_u^2$						1.1321 (0.0994)
$\rho$						0.2803 (0.0354)
Log likelihood	-2487.8615	-1989.9046	-1975.2767	-1599.3429	-1595.2083	-1463.6772
$\chi^2$	10.36**	104.3256***	23.9937***	104.7039***	114.8549***	68.4975***

Standard errors (clustered for individuals in models 1 – 3) in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ; Source: own calculations

In all models, coefficients for the MALE variable are negative, indicating that women are more likely to choose the car. This is in line with our expectations, as women in India are usually more sensitive to security issues on public transport, such as harassment in buses. However, the coefficients are not very large and also not statistically significant at the five percent level. Coefficients of age and marital status are also relatively small and not statistically significant.

The effects of income are fairly small in the estimations. Especially respondents in the second-lowest income category are somewhat less likely to choose the car, when compared to the lowest income category. All other income categories are very similar to the lowest, as indicated by the coefficient estimates, which are close to zero. Our interpretation here is that the poorest participants in our sample may want to demonstrate their ambition for “upward mobility” by choosing the car more often, whereas the rich are more used to choosing the car. Overall these effects are fairly small, however.

It is a bit surprising that owning a car does not increase the propensity to choose one in the experiment. Three out of four estimated coefficients even have a negative sign, indicating a possible opposite effect. However, as pointed out earlier, owning a car may not be critical for the decision to use one. Looking at the coefficient estimates of the CARFREQ variables reveals that actually using a car has the expected positive effect on choices in the game. However, both the OWNSCAR and the CARFREQ variables are statistically not significant. The same applies to the coefficient of OWNSBIKE, which is small and statistically not significant.

The frequency of using the bus has the expected negative sign, and coefficients are comparatively large and statistically significant. It is notable that the effect can largely be attributed to the step from “not using the bus at all” (BUSFREQ1, the reference category) to one of the other categories, as coefficients of BUSFREQ2, BUSFREQ3, and BUSFREQ4 are fairly close to each other. The treatment dummy variables are jointly

significant, with the two pecuniary treatments being a little less effective than the public coordination treatment.<sup>20</sup> The effect of ROUND is virtually zero in model (3), indicating no substantial change of choices over time.

The coefficients of the variables added in models (3) to (5) show that the players change their behavior – only to a limited extent, however – depending on what others do in the game and depending on the particular treatment. The high and significant coefficient of CARPREVROUND in models (2), (4), and (5) suggests that players tend to repeat their choices, but the random effects model which addresses the serial correlation within subjects almost eliminates this effect. Thus, the overall effect can be assumed to be zero. Coefficients of the ALLOWED variable are large and statistically significant in all three models. This indicates that, when subjects are encouraged to choose the car in a particular round of the public coordination treatment, they are relatively likely to use this opportunity. The high coefficient estimates of the dummy variables of the number of cars in the previous round suggest that, even when many others choose the car, the likeliness of a particular player choosing the car remains relatively high. Taken together, the increase in the  $\chi^2$ -value from (4) to (5) is relatively small, however. A likelihood ratio test shows that the two models are equivalents.<sup>21</sup> Coefficients of the included dummies are relatively close to each other. This indicates that most of the increase in explanatory power can be attributed to the large difference between those cases where there are no cars (the reference category) to any other scenario. In other words, when no one chooses the bus in round  $t$ , the likeliness of a player choosing the car in round  $t + 1$  is substantially higher. If one or more players choose the car in round  $t$ , the effect on choices in round  $t + 1$  was relatively small.

### 3.5. Discussion

Our results show that the introduced treatments induce a modest increase in choosing the bus in the game. Increasing the parking costs or subsidizing the bus in the game has a positive effect, although in some cases these changes are statistically not significant. When also taking into account the costs and benefits for the “transport authority,” our results show that both policies produce net benefits, although with a substantially higher degree of

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<sup>20</sup> Likelihood ratio tests for the categorical variables with more than one category are presented in the supplementary material.

<sup>21</sup> The fairly stable Akaike Information Criterion shows that not much additional explanatory power is achieved by controlling for socio-economic heterogeneity. Choices are to a large extent the result of introduced treatments.

efficiency under the parking-cost condition. In this treatment, players' total earnings are reduced by less than 1%, whereas the raised income from parking fees represents more than 9% of all players' earnings in the baseline condition.

The non-pecuniary treatment is a little less effective in reducing the number of cars, yet significantly increases participants' earnings because of enhanced coordination. A larger sample and playing for more rounds – perhaps in a computerized version of the game – would certainly allow more reliable statistical tests. Previous work on coordination has shown that, after playing 50 rounds and more of a coordination game, no equilibrium emerges, even if additional information on others' behavior is provided to participants (cf. Schneider and Weimann, 2004; Selten et al., 2007; Ziegelmeyer et al., 2008).

Our experiment also shows that people tend to choose the car, even if such behavior is to their own disadvantage. One reason may be that boundedly rational subjects use diverse and imperfect models to predict what others will do. In some instances, this may result in socially inferior outcomes and impede the quick emergence of stable equilibria. Another interpretation could be that participants get the impression that other players take the car “too often,” thereby benefitting from their own “bus choice.” By also choosing the car, they may give up some of their payoff, but also reduce the payoff of others, as a form of strategic behavior – similar to costly punishment often observed in social dilemma experiments (Fehr and Gächter, 2000) – to influence other players' choices in subsequent rounds. Anecdotal evidence from the pretests with university students supports this interpretation. In those tests, choosing the car was very often motivated by a feeling of envy, the desire to punish and the objective of making others choose the bus in the game. It may be that in real life people support policies aimed at reducing the number of cars without necessarily feeling that this should have real consequences for their own behavior. Economic experiments are well-suited for detecting and exploring such cases where stated preferences and actual behavior diverge, because they link behavior to monetary incentives.

Controlling for observed socio-economic characteristics, the regression models have shown that age, gender, or income are rather unimportant for explaining the experimental behavior observed, whereas the introduced treatments had significant behavioral effects. The low level of statistical significance of observed socio-economic heterogeneity may be explained by the relatively homogeneous sample. The importance of treatment effects is indicated by the increase in explanatory power in the respective likelihood ratio test.

Further, some unobserved characteristics, such as political and environmental attitudes or perceived comfort and social status of cars and buses, may have influenced decision-making in the game. In the end, it is very important to understand how participants form expectations regarding the behavior of others, as this is critical for their payoffs and thus for mode choice in the game. Future research could pick up on these points to take a more detailed examination of these relationships by extending the sample to other target populations, gathering more data on attitudes, perceptions, and the formation of expectations about others' behavior, with regard to mode choice. Specifically, it will be important to take into account the low level of car use and ownership of private vehicles in our data. Few people in our sample own a car and only about half of the respondents regularly use one. This may have introduced bias in favor of the bus choice.

More interestingly from our perspective, the regressions suggest that some of participants' actual traffic behavior is – to some extent – “carried over” to the game. For instance, we found that participants who use buses in real life also have a higher propensity to choose this option in the game. Differences are particularly clear between the category of people who never use the bus and all remaining categories. Traffic measures affecting the use of either option may start virtuous or vicious feedback cycles. Impeding the use of cars can break the vicious cycle of having more cars on the road in our game. It is notable that the frequency of using the car has smaller coefficients in the regressions than the coefficients of bus frequency. Of course, the number of bus and car trips is not independent of each other. Yet, our experimental results give some indication that, *ceteris paribus*, encouraging people to use the bus may be a better strategy than decreasing the number of car trips.

The high coefficients of the ALLOWED variable indicate that people may take up a suggested model for coordination. The treatment effects show that this does not lead to an increase in the use of cars in the game. This finding indicates that the roles of policy and the public are important. Authorities of some cities, such as Shanghai, have experimented with an odd-even number license plates system during periods of heavy congestion, with drivers only being allowed to use certain main roads on alternate days, depending on their plates. The mechanism in our game works in a similar way, without monitoring or enforcement. It just provides players with a model to better predict what others may do. How such policies can actually work in practice and what the roles of monitoring and enforcement of such rules may be needs to be further clarified through more detailed analysis.

### 3.6. Conclusion

In this paper we have presented a framed field experiment on mode choice, run with commuters in Hyderabad, India. The results show that participants made their own decisions based on the expected decisions of others. In a given round, the more participants in an experimental group chose the car, the more likely it became that even more participants would choose the car in the following round. Subjects also became habituated, as indicated by a positive probability to stick with a choice from a previous round. We find that participants chose the car less frequently when we introduced monetary incentives for using public transport or avoiding the car. We have also shown that providing information to facilitate coordination helped subjects to improve their earnings. This effect works in two ways. Firstly, as shown in the regression analysis, when it is “their turn,” players were more likely to choose the car. Secondly, as indicated by the higher average payoffs, players exhibited a greater willingness not to take the car when it was “someone else’s turn.”

We acknowledge that generalization of the results to directly dictate traffic policies is neither possible nor desirable. The main goal of such experiments is rather to test more general hypotheses and advance our theoretical understanding of human decisions (Guala, 1999; Guala and Mittone, 2005; Schram, 2005). In addition, games like the one discussed in this paper might be particularly useful for participatory traffic planning that moves beyond one-dimensional surveys or qualitative methods (cf. Bickerstaff and Walker, 2001; Fouracre et al., 2006). It might be very costly to disentangle complex causal relationships by testing traffic policies in the field under controlled conditions. It might also be very difficult to quantitatively assess and understand interactions and subsequent dynamics of traffic behavior. In such a situation, framed field experiments like the one presented in this paper can provide rich sources of information on behavioral factors under different policy options, which may also guide further research such as surveys (cf. Mahmassani and Jou, 2000), although the limited sample may be a drawback, as only half of the people use the car on a regular base.

Experimental research in transportation economics could generate further interesting insights for demand-side measure policy debates and enrich the current discourses therein. For example, it would seem particularly relevant to combine policy measures and test whether the resulting change is more or less than the sum of its parts. The research presented in this paper was developed following such a logic and can hopefully lead the way towards experimenting with such an approach. Moreover, the possibility of oft policy

measures, such as awareness-raising campaigns, could be further explored in experimental settings so as to assess their potential contributions to achieving more sustainable transport systems. Just how far attitudes affect behavior and how these attitudes interact with experience and learning in a game constitute additional challenging questions for further research. Economic experiments on transport could also be useful in exploring the models people use for predicting the behavior of others and using such models for simulation. Another important issue to take up in the future is the number of motorbikes, which is growing at an even faster rate than the number of cars, in Indian cities. The positive – albeit small and statistically not significant – effect of OWNSBIKE may be a first indicator of a negative effect. In this context, it will also be important to look at policies which promote non-motorized transport (walking and bicycles) or which aim at reducing distances or frequency of motorized trips.

At this stage, our results suggest that soft policies alone might not be very effective. This finding, however, does not rule out the reasonable possibility of such policies functioning as multipliers in conjunction with other demand- or supply-side measures. Developing context-dependent games for different economic strata of the Indian society, e.g. a mode choice game on walking vs. taking the bus for the poor or games on using the car individually vs. using it jointly with others for the affluent, are promising extensions of the game developed in this paper. Sampling participants who can relate to the particular task at hand will be critical for the successful conduct of such experiments in the future. Again, investigating combinations of policy measures in games and then using these games as a starting point for discussion and gaining deeper insights into behavioral factors can be seen as a promising way ahead for a sector that, especially in the developing world, appears to be largely trapped in a vicious circle: more roads lead to more traffic which, in turn, fuels even more supply of infrastructure.



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#### 4. EMPIRICAL STUDY II (P3): SHARED TOILETS AS A COLLECTIVE ACTION PROBLEM: A FRAMED FIELD EXPERIMENT ON SANITATION IN HYDERABAD, INDIA

Jens Rommel

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Marco A. Janssen

Arizona State University  
School of Human Evolution and Social Change, Center for the Study of Institutional  
Diversity

In preparation for submission

**Abstract:** Interdependencies at the neighborhood level create a collective action problem for the provision of sanitation-related public goods. We study the effect of heterogeneity and leadership on cooperation in a framed economic field experiment. The game was conducted with 120 slum-dwellers from Hyderabad, India. We find that endowment heterogeneity has a negative effect on contributions in the game. Contrary to previous studies, “leading by example” decreases average contributions. However, the effect of leadership is positive and large for participants who are experienced leaders in actual life. We conclude that self-help group leaders can be entry points for sanitation-related community development.

**Keywords:** Collective Action; Heterogeneity; Hyderabad; India; Leadership; Public Good Game

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#### **4.1. Introduction**

Adequate sanitation facilities are crucial for human health and well-being. In fact, child mortality and infectious diseases are substantially reduced by improved hygiene practices (Curtis et al., 2011). However, access to toilets and clean water sources remain beyond reach for many people throughout the world. Progress in this area has been slow, and the millennium development goal of improved sanitation for 75 percent of the world’s population is likely to be unfulfilled. Sanitation is also intertwined with other challenges such as gender equality, hunger, and education (United Nations, 2012b). Governments and municipalities insufficiently invest in public infrastructures for sanitation. Furthermore, technical, cultural, behavioral, and political barriers to improved sanitation exist at various levels (Andres et al., 2014; Mosler, 2012; Chaplin, 1999; Engel & Susilo, 2014; Baruah, 2007; McFarlane, 2012; Kutter, 2014), and only recently have scholars and practitioners started to systematically investigate these issues (Black & Fawcett, 2008).

Although private in-house toilets are a preferred option for households in today’s modern society, public or otherwise shared toilets are widespread in the informal settlements of cities in the global south. In some instances, private toilets are too costly to construct, and in other cases, especially in crowded urban communities, private toilets are not in use due to high housing density (Black & Fawcett, 2008). For many poor urban dwellers, shared toilets remain a reality with which they must live. Consequently, research in development studies has recently started to focus on understanding sanitation behavior of urban dwellers when using public toilets (Mosler, 2012; Günther et al., 2012; Sonogo & Mosler, 2014). These studies have found that the number of users and perceived cleanliness of a toilet are strongly correlated, and toilets which are used by four or less people are perceived as clean in the majority of cases (Günther et al., 2012). In this line of argument, maintenance of shared toilets can be understood as a collective action problem for public good provision. Besides recognizing individual behavior and perceptions, it is also important to understand social interactions of multiple users, and economic experiments are a useful method for analyzing collective action problems that involve strategic interaction (Poteete et al., 2010).

In this paper, we study maintenance of sanitation facilities as a collective action problem by means of an experimental public good game, conducted in the field with 120 – mostly female – slum-dwellers from Hyderabad, a large and fast growing city in south India. To our knowledge, this is the first lab-in-the-field experiment focusing on the provision of shared sanitation as a collective action problem. From the literature, we identified two important factors that contribute to success or failure of collective action. First, we investigated economic inequality as a potential barrier to successful collective action. Secondly, we examined the effect organization of collective action and “leading by example” have on the provision of public goods. In addition, we used questionnaire data collected from game participants to understand the extent to which socio-economic characteristics affect decision-making in the game.

The paper is structured as follows. Section 2 introduces the study context and derives the two factors under investigation from the literature. Section 3 describes the game design and explains the sampling procedures and practical conduct of the experiment. Sections 4 and 5 present and discuss the results. Finally, Section 6 summarizes the paper and makes broader conclusions.

## **4.2. Sanitation in India as a Collective Action Problem**

### **4.2.1. Study Context**

South Asia is plagued by unavailability of toilets. Approximately 40 percent of the region’s population lacks access to sanitation facilities, which is more than any other region in the world (United Nations, 2012b). Of the 1.1 billion people of the world’s population without access to sanitation, more than half of these affected individuals live in India (United Nations, 2012b). Even worse, the situation is hardly improving, and South Asia is projected to miss the sanitation targets set by the Millennium Development Goals (United Nations, 2012a).

The costs of inadequate sanitation in India – including impacts on health, school and work attendance, water quality, and international tourism – are estimated to exceed \$50 billion USD per year (World Bank Water and Sanitation Program, 2011). Social benefits from one dollar invested in sanitation yield a social return on investment of up to five dollars, and with public spending of only 0.2 percent of GDP, India is investing insufficient financial resources into sanitation (World Bank Water and Sanitation Program, 2011). Funds for (mega-) urban development are largely directed at the needs of the middle classes (e.g., the

Jawaharlal Nehru National Urban Renewal Mission) and have predominately focused on large-scale infrastructure development of sewage systems or effluent treatment plants (Government of India, 2005). Women suffer disproportionately from this lack of sanitation. They are often compelled to defecate in public which makes them vulnerable to sexual harassment and assaults. In addition, women carry the main burden of poor child health (Truelove, 2011).

The manifold problems are most pronounced in poor neighborhoods. Knowledge and awareness on hygiene practices are low; child mortality and disease prevail in India's urban slums (Jalan & Somanathan, 2008). At the neighborhood level, powerful actors – so-called resident welfare associations in the affluent neighborhoods and “slum leaders” in the poor parts – act as political brokers who negotiate access to basic public services, including water, sanitation, or sewage for several hundred people (Zerah, 2009, 2007).

On a smaller scale, adjacent households often rely on informal practices of mutual self-help. Water and toilets are frequently shared among smaller groups of neighbors. Malpracticed hygiene and inadequate sanitation facilities affect the immediate environment and adjacent households. Examples include the contamination of water or the provision of breeding grounds for flies and mosquitoes (Clasen et al., 2007; Black & Fawcett, 2008). In crowded urban environments, individual incentives to invest in hygiene and sanitation are low and create a “collective action problem” (Olson, 1965).

#### **4.2.2. Heterogeneity and Leadership in the Voluntary Provision of Public Goods**

Mainstream economic theory predicts that unless “selective incentives” (i.e., private goods that are easily excludable from the use of others) are attached to public goods, free-riding on the contribution of others prevails. The voluntary provision of public goods will then occur at levels below social efficiency (Olson, 1965). In the same manner, unless privatized or centrally controlled by the government, open access common pool resources are said to become over-exploited, and users are projected to end up in a “tragedy of the commons” (Hardin, 1968).

This view of a collectively unsolvable social dilemma has been prominently challenged by Ostrom (1990), who has identified a number of institutional factors (e.g., the presence of monitoring and sanctioning systems or recognition by higher-level authorities) that enable communities to manage common pool resources collectively. In addition, a larger set of



“second level variables” that affect the likeliness of successful collective action have been proposed (Ostrom, 2009). Such approaches have also been extended to community development and collective action problems in development and public good provision (e.g., for accessing markets for agricultural produce or for organizing mutual self-help in rural finance) (Markelova et al., 2009; Bharamappanavara et al., in press). Heterogeneity and the presence of leadership are among the “second level variables” that can dictate success or failure of collective action.

The effect of community heterogeneity on successful collective action and cooperation is theoretically ambiguous and empirically contested (Baland et al., 2007; Baland & Platteau, 1997; Varughese & Ostrom, 2001; Poteete & Ostrom, 2004; Bharamappanavara et al., in press; Habyarimana et al., 2009). Heterogeneity can concern various dimensions (Baland et al., 2007), and it is difficult to operationalize in empirical work (Agrawal & Gibson, 1999). Heterogeneity may affect collective action through a diverse set of mechanisms. For instance, people who identify with a particular social group may prefer collaboration within this group, or they may more easily know how to punish and reward each other (Habyarimana et al., 2009). At the same time, these mechanisms are mediated by their structural institutional context (Bharamappanavara et al., in press; Varughese & Ostrom, 2001; Poteete & Ostrom, 2004).

Collective action may also be facilitated by the presence of leadership as “group formation often needs a catalyst, and the nature of the catalyst is crucial” (Thorp et al., 2005). Such leadership can emerge from individuals within the community or from outsiders who may also provide trainings, critical inputs, or social networks (Nkonya et al., 2008; Markelova et al., 2009). Leaders can enhance coordination and may be able to mobilize “critical masses” necessary for starting collective action processes (Calvert, 1992). More importantly, leaders may set an example for others to follow if followers do not know which actions are deemed appropriate (Hermalin, 1998). Especially in the experimental work on public good provision, obtaining information on what a first mover does is strongly correlated with the behavior of those who follow (Sutter & Rivas, 2014).

### **4.3. The Experiment**

#### **4.3.1. The Public Good Game**

Our study is based on a standard linear voluntary contribution mechanism public good game (Isaac et al., 1984) with  $n (= 5)$  players. In the game, each player decides how much

to contribute from initial endowments of size  $e_{ir}$  ( $= 5, 10, \text{ or } 15$  Indian Rupees, contingent on treatment in our study). The game is played repeatedly for  $r$  ( $= 8$ ) rounds. In each round, a player  $i$  decides how much he or she wants to contribute to a common fund, and his or her profits  $\pi_{ir}$  in round  $r$  are calculated as follows

$$\pi_{ir} = \frac{a(\sum_{j \neq i}^{n-1} x_{jr} + x_{ir})}{n} + e_{ir} - x_{ir}$$

where  $x_{ir}$  is player  $i$ 's contribution,  $x_{jr}$  are the other  $(n - 1)$  players' contributions in round  $r$ , and  $a$  ( $= 2$ ) is a constant which satisfies  $1 < a < n$  (the dilemma condition). In a finitely repeated game, free-riding (i.e., contributing zero) is a dominant strategy. The social optimum, which is defined as the strategy that maximizes aggregate payoffs  $\sum_{i=1}^r \sum_j^n x_{jr}$ , is to contribute everything. Typically, in a linear VCM public good game, players start contributing half of their initial endowment on average, and contributions decrease over time (Chaudhuri, 2011).

#### 4.3.2. Hypotheses and Treatments

Several economic experiments have investigated the effect of asymmetric endowments on contributions in public good games. Most studies in the lab find that absolute contributions increase and relative contributions decrease with increasing endowments (Chan et al., 1996; Cherry et al., 2005; van Dijk & Wilke, 1995; Chan et al., 1999; Aquino et al., 1992). Buckley & Croson (2006) find that endowment heterogeneity has no impact on individual contributions. Van Dijk & Grodzka (1992) study endowment heterogeneity and manipulate the information available to subjects. Participants are either aware or unaware of other players' endowments (and hence heterogeneity). However, information on heterogeneity does not strongly affect actual contributions. In a meta-analysis of public good games, Zelmer (2003) concludes that in the typical case, endowment heterogeneity reduces contributions. Few experiments though struggle with investigating endowment heterogeneity in actual field settings with non-student participants.

Cárdenas et al. (2002) studied income inequality for natural resource use – framed as firewood extraction – in rural Colombia. They find that those who have lower opportunity costs in alternative income opportunities (as induced by the experimental treatments) harvest more of a common resource. Notably, with asymmetry in alternative incomes, total harvesting of firewood decreases, therewith reducing the pressure on ecosystems. Hayo & Volland (2012) demonstrate that socio-demographic heterogeneity, which is calculated at the

group level from questionnaire data, leads to less cooperative play in a “grazing game” conducted with farmers in Namibia and South Africa.

There is a relatively large empirical experimental literature on “leading by example” (Andreoni, 2006) in the lab. In experimental games, leadership in public good games is typically introduced through sequential contributions. In a first step, one or more leaders decide on their contributions; in the next step, leaders’ contributions are announced, and others follow with their decisions. It is assumed that leaders are inclined to set a positive example by contributing large amounts and that followers will also contribute more contingent on their leaders’ contributions (Sutter & Rivas, 2014).

Empirically, these presumptions have been repeatedly demonstrated. The experimental literature on leadership typically finds a positive effect on outcomes in both coordination (Foss, 2001; Wilson & Rhodes, 1997) and social dilemma games (Potters et al., 2007; Gächter & Renner, 2003; Güth et al., 2007; Moxnes & van der Heijden, 2003). Sutter & Rivas (2014) summarize the recent experimental literature from the lab, and in almost all cases, leadership increases average contributions, and followers’ contributions are positively correlated with their leaders’ contributions. Our experiment is closely related to the work of Levati et al. (2007) who ran a similar experiment in the lab. For India, it has also been shown that religious heterogeneity – comparing mixed Hindu or Muslim groups with homogeneous groups – can undermine the positive effects of leadership in the lab (Keuschnigg & Schikora, 2014). To our knowledge, there is no research studying leadership in a framed field experiment.

Based on this literature, the following two hypotheses are formulated:

**H1:** Endowment heterogeneity has a negative effect on average contributions to the public good.

**H2:** Leading by example has a positive effect on average contributions to the public good.

#### **4.3.3. Experimental Design**

To test the hypotheses, two factors – distribution of endowments and leadership – were varied in a full factorial within-subjects design. The order of treatments was randomized in

four blocks.<sup>22</sup> The random order within-subjects design was chosen to avoid confounding of treatments with communities, group dynamics, or learning (Charness et al., 2012; Friedman & Sunder, 1994). Table 9 summarizes the experimental design.

**Table 9:** Overview of Experimental Design

	Treatment 1	Treatment 2	Treatment 3	Treatment 4
Name of treatment	Baseline	HetNoLead	HomoLead	HetLead
throughout the rest of the paper				
Heterogeneity	No	Yes	No	Yes
Leader	No	No	Yes	Yes
Endowments in Indian Rupees (Players 1 to 5)	10, 10, 10, 10, 10	5, 5, 10, 15, 15	10, 10, 10, 10, 10	5, 5, 10, 15, 15
Number of players per group	5			
Sum of group endowments per round	50			
Total endowments per player over all eight rounds	80			
Number of subjects	120			
Number of decisions per subject	2			
Number of distinct decisions in sample	240			
Order of Treatment	Randomized in Blocks			

Source: Self-design

Each subject was required to decide eight times how much to contribute from an initial endowment. In all four treatments, endowments of other players were known to everyone. Yet, the identity of these players was kept secret. Anonymity was ensured, and neither endowments nor contributions could be related to a particular player (cf. supplementary material/experimental instructions).

<sup>22</sup> The order of treatments in these four blocks and additional information on ordering effects are provided as part of the supplementary material.

The game framing referred to sanitation-related public goods. A session started with a general introduction to the problem of sanitation, and the rules of the game were then read to all 20 participants of a session. Participants were asked to picture a scenario in which they would share a toilet with four other neighbors. For maintenance, cleaning, and small repairs, some money would have to be collected. This money would be collected anonymously in a box, and an opaque box was presented to participants in the game instructions. Some Rupee coins were placed in the box to demonstrate this in practice. In the next step, another box, which was transparent this time, was shown to the participants. Again, the game facilitator added some coins to the box. Now, participants could see how much money was in the box. It was then highlighted that with this type of box, the participants could easily recognize how much money previous contributors have added to the total fund.

It was pointed out that such differences in boxes/organizational set-up would also exist in the game. More specifically, participants were told that in half of the games, one participant would be randomly and anonymously selected to be the “first mover,”<sup>23</sup> and the remaining four participants could make their contributions knowing how much money had been added to the box by the first person. The first mover was not the same person during the entire game, and in each of the four rounds, the person to contribute first was newly selected. Furthermore, it was explained that, as in real life, some people have more money than others, and such heterogeneity would also be part of the game.<sup>24</sup> For both conditions – heterogeneity and leading by example – paper sheets on which subjects would later indicate their decisions were used in the demonstration phase to explain the game’s tasks. In order to ensure an accurate understanding of the rules, the game facilitator posed several questions of understanding to randomly selected participants at the end of the demonstration phase. During all stages, participants were free to ask questions.

By drawing lots from a box, subjects were then randomly assigned to one of four groups per session, each of which consisted of five participants. A group of two or three research assistants was randomly assigned to each group for organizing the game. Disposers with printed sheets were distributed to participants. These sheets involved visualization, and following pre-testing in the field, these sheets were designed in a way that they could also

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<sup>23</sup> We strictly avoided the word “leader” because it has a strong connotation in Indian society. We wanted to study the basic principles of “leading by example” in an anonymous setting. Additional information on the practical implementation of the “leading by example” treatments is provided in the supplementary material.

<sup>24</sup> We ensured and explained to participants that total endowments over all rounds of the game would always add up to 80 Rupees though.

be easily accessed by illiterate subjects (cf. supplementary material). All decisions were made in private, and communication was not permitted during the game. Following players' decisions in a particular round, the results were verbally announced and noted on a white board in English as well as the local language, Telugu. Research assistants received training to always announce these results in the same way. After eight rounds of play and with the help of trained field staff, a short survey was conducted. Finally, subjects were paid on the basis of their decisions in all eight rounds.

#### 4.3.4. Community and Participant Selection

Based on knowledge gathered from 2010 to 2012 during three field visits to Hyderabad, five slum communities were selected for conducting the game. These communities were chosen in close consultation with local partners. Locations were considered with respect to their sanitation situations. Subjects were recruited only when they could relate to the game's framing. In other words, problems with sanitation and water access were prevalent within each subject's community. This requirement was important because it "is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects" (Harrison & List, 2004). Hyderabad's slums are very diverse, and communities were also selected to reflect some of this diversity, especially with respect to population (Muslim vs. Hindu), location (urban vs. peri-urban), or tenure status (recognized and semi-formal vs. unrecognized and temporary).

In each community, subjects were selected in collaboration with local partners and NGOs working on the ground. These partners also provided spaces for conducting the experiments (e.g., schools, community halls, yards, or temple compounds). The game was played in five communities. Based on our own assessments and knowledge as well as those of our partners, Table 10 summarizes some selected characteristics of these communities.

**Table 10:** Characteristics of Selected Communities

Name of Community	Fathullaguda	NTR Nagar	Rasoolpura	Singareni Colony Slum	Rakshapuram
Location	Peri-urban	Urban	Urban	Urban	Urban
Population density	Low	Medium	High	High	Medium
Tenure status	Partly	Notified	Notified	Not	Notified

		notified				notified	
Poverty incidence		High	Medium	High		Very high	Medium
Housing conditions		Mostly semi-permanent	Mix of permanent and temporary housing	Mix of permanent and temporary housing		Only temporary	Mostly permanent
Degree of economic inequality		Medium	High	High		Low	Medium
Level of collective action (SHGs)		Medium	High	Low		Very low	Medium
Sanitation/toilets		Some private and some shared toilets/open defecation	Some private and some shared toilets/open defecation	Mostly private toilets, some shared toilets		Mostly open defecation	Mostly private toilets, some shared toilets
Water access/availability		Medium	Medium	Low		None	Medium
Drainage/storm water		Medium	Medium	Medium		None	Low

Source: Self-design

The game was played with 24 groups of five individuals which led to 120, mostly female, slum dwellers in six experimental sessions. A typical experimental session lasted approximately two and a half hours. Subjects received a participation fee of INR 100. In addition, they earned money depending on their own and their group's decisions in the game. The median and mean earnings from the game (excluding the INR 100 participant fee) were INR 117.8 and INR 121.2 (SD = 17.517), respectively, with a minimum and maximum of INR 69.4 and INR 175.2, respectively. Thus, participants earned INR 221.2 on average, which was approximately four USD at the time of the experiment or equal to

slightly more than a daily wage for slum-dwellers in Hyderabad. Table 11 describes and presents summary statistics for some key variables of participants that were collected in the post-experimental survey and used in the regression analysis.

**Table 11:** Characteristics of Participants

Variable Name	Description	N	Mean	SD	Min	Max
FEMALE	= 1 if participant is female	120	0.90	0.30	0.00	1.00
AGE	= age in years	118	33.29	10.94	15.00	65.00
MARRIED	= 1 if married	120	0.87	0.34	0.00	1.00
DAILYWAGE	= daily wage in Indian Rupees	120	72.23	116.22	0.00	600.00
WEALTH	PCA Wealth Index <sup>25</sup>	120	0.00	1.50	-4.56	2.40
READANDWRITE	= 1 if participant can read and write	120	0.53	0.50	0.00	1.00
YEARSINSCHOOL	= years of formal schooling	120	5.06	4.76	0.00	15.00
YEARSINAREA	= years participant lives in the neighborhood	116	13.66	8.41	0.00	40.00
SHGMEMBER	= 1 if participant is member in a self-help group	120	0.52	0.50	0.00	1.00
SHGLEADER	= 1 has leadership position in a self-help group	120	0.19	0.40	0.00	1.00
HINDU	= 1 if participant states Hinduism as religion	120	0.74	0.44	0.00	1.00

Source: Self-design

Clearly, the vast majority of participants were female. Sanitation and its related problems affect women more than men, thus the focus on recruiting females. Also, the level of education in the sample is rather low with an average of only five years spent in school and almost half of the participants being illiterate. About half of the participants were members in a self-help group, and approximately one fifth of the participants had a leadership

<sup>25</sup> The index has been constructed from a list of household assets based on the method developed by Filmer & Pritchett (2001). It includes seven variables: possession of color TV (1), mobile phone (2), motorbike (3), electric fan (4), water filter (5), LPG cylinder for cooking (6), and private water tap in the house (7).



position in one of these groups. Three quarters of all participants were practicing Hindus, which is relatively close to the census figures for Hyderabad with its large Muslim, considerable Christian, and prevalence of other minority populations.

## 4.4. Results

### 4.4.1. Treatment Effects

Table 12 displays summary statistics for contributions in the public good game. Data are pooled across all eight rounds and 120 participants.

**Table 12:** Contributions by Treatments

	N	Mean contribution in INR	Standard Deviation	Minimum	Maximum
Pooled Data (Full Sample)	960	5.15	3.143	0	15
Treatment 1 (Baseline)	240	5.458	2.855	0	10
Treatment 2 (HetNoLead)	240	5.275	3.587	0	15
Treatment 3 (HomoLead)	240	5.004	2.730	0	10
Treatment 4 (HetLead)	240	4.863	3.311	0	15

Source: Self-design

Participants contributed approximately half of their endowments on average. This rate is similar to rates typically found in such experiments and close to what has been found in rural India (Ledyard, 1995; Chaudhuri, 2011; Chakravarty et al., 2010; Bouma et al., 2014).

Differences between treatments are statistically significant at the five percent level (Kruskal-Wallis-Test,  $\chi^2(3) = 9.040$ ,  $p = 0.0288$ ). Pair-wise differences between treatments are relatively small yet statistically significant. Under heterogeneous endowments participants contribute slightly less ( $M = 5.068$ ,  $SD = 3.455$ ) than under homogeneous

endowments ( $M = 5.231$ ,  $SD = 2.799$ ). This difference is statistically significant (Mann-Whitney U-test,  $z = 2.310$ ,  $p = 0.0209$ ). A larger difference exists between the leadership ( $M = 4.933$ ,  $SD = 3.032$ ) and non-leadership conditions ( $M = 5.367$ ,  $SD = 3.240$ ). This difference is statistically significant at the ten percent level (Mann-Whitney U-test,  $z = 1.926$ ,  $p = 0.0542$ ). Here, contrary to literature, “leading by example” decreases contributions. We will explore possible explanations for this behavior below.

#### 4.4.2. Socio-Economic Heterogeneity: Regressions on Contributions

Table 13 presents three specifications of multilevel regressions on contributions in the game. Three-level models are used with decisions (level 3) nested in players (level 2), nested in groups of five subjects (level 1).<sup>26</sup>

**Table 13:** Multilevel Models on Contributions

	(1) Static	(2) Dynamic	(3) Dynamic with socio-economic covariates
HETNOLEAD	-0.1960 (0.1908)	-0.2943 (0.2066)	-0.2205 (0.2123)
HOMOLEAD	-0.4542** (0.1907)	-0.5042** (0.2069)	-0.5528*** (0.2128)
HETLEAD	-0.6085*** (0.1908)	-0.6444*** (0.2067)	-0.6877*** (0.2136)
2NDROUNDTREAT	0.2561* (0.1499)	0.3926** (0.1639)	0.4221** (0.1686)
ENDOWMENT	0.3526*** (0.0213)	0.3469*** (0.0234)	0.3514*** (0.0241)
ROUND	-0.0760** (0.0327)	-0.0933*** (0.0359)	-0.1047*** (0.0370)
OWN CONTRIBUTION T-1		0.0033 (0.0305)	-0.0029 (0.0313)
CONTRIBUTIONSOTHERS T-1		-0.0306* (0.0166)	-0.0234 (0.0169)
FEMALE			-0.0885 (0.7930)
AGE			-0.0402* (0.0231)
MARRIED			-1.4199** (0.5930)
DAILYWAGE			-0.0014 (0.0019)
WEALTH			-0.0353 (0.1329)

<sup>26</sup> Further model specifications and robustness checks are presented in the supplementary material.

READANDWRITE			0.4345 (0.6073)
YEARSINSCHOOL			-0.1414* (0.0735)
YEARSINAREA			-0.0253 (0.0295)
SHGMEMBER			-0.8863* (0.4994)
SHGLEADER			0.6146 (0.5476)
HINDU			-0.6290 (0.4980)
Constant	2.1525*** (0.4126)	2.8517*** (0.5928)	7.0890*** (1.5209)
Ins1_1_1 Constant	0.2266 (0.2110)	0.3825* (0.2147)	0.2249 (0.2473)
Ins2_1_1 Constant	0.5019*** (0.0867)	0.4449*** (0.1040)	0.4514*** (0.1108)
Insig_e Constant	0.7369*** (0.0245)	0.7469*** (0.0271)	0.7497*** (0.0277)
<i>N</i>	960	840	798
$\chi^2$	291.1926	243.6627	258.2344
$p > \chi^2$	0.0000	0.0000	0.0000
Log likelihood	-2197.6137	-1941.7962	-1852.2193

Source: Own calculations; Standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

The first model includes only static game data, namely dummy variables for the respective treatments with BASELINE as the reference category and participants' endowments in Indian Rupees (5, 10, or 15). The variable 2ROUNDTREAT is a dummy variable indicating whether a treatment is applied the second time in the game. ROUND is a continuous variable indicating the round from 1 to 8 to test for a decay effect.

The second model extends the first model by adding two dynamic variables, namely lagged contribution and lagged contribution of others in the previous round ( $t - 1$ ).<sup>27</sup> The third model adds socio-demographic variables to control for heterogeneity. The large  $\chi^2$  statistics indicate that the overall explanatory capacity of the models is high. The estimates show relatively large effects on contributions of the treatment variables. Also, ENDOWMENT,

<sup>27</sup> There are no lagged variables available for the first round, hence the lower number of observations and differences in coefficients in the second column. Adding socio-economic variables further reduces the number of observations due to missing observations in the questionnaire. Specifically, the model in the third column is based on 114 out of 120 participants.

2ROUNDTREAT, AGE, MARRIED, READANDWRITE, YEARSINSCHOOL, the SHG variables, and HINDU are important factors affecting contributions in the game.

The effect of ROUND is negative, indicating decay in contributions over time. However, similar to what is typically found in related field experiments (Chakravarty et al., 2010; Werthmann, 2011; Bouma et al., 2014), this effect is relatively small. It is also partly outweighed by the relatively large effect of 2NDROUNDTREAT. Both lagged variables show only small effects. Participants' choices in  $t - 1$  are relatively independent of contributions in round  $t$ . The same applies to the contribution of others. Each Rupee the others contribute in  $t - 1$  is, on average, matched with about -0.03 Rupees in  $t$ , indicating no strong evidence of conditional cooperation (cf. Fischbacher et al., 2001).

#### 4.4.3. The Role of Leadership

Analyzing the effect of leadership reveals that, contrary to what is typically found in the literature, leading by example reduces contributions in our experiment. Table 14 presents summary statistics for all 480 leadership decisions by leadership status in the game and being an actual leader in a self-help group.

**Table 14:** Contributions by Leadership Status in Game and Actual Life

Participant is leader in the game	Participant is in leadership position in a self-help group in actual life	Number of observations	Mean contribution	SD of mean contribution
No	No	312	5.074	3.074
Yes	No	76	4.5	3.031
No	Yes	72	4.514	2.907
Yes	Yes	20	5.9	2.553

Source: Self-design

It is easy to see that leadership decisions differ strongly by experience with a leadership position in actual life. When selected as a leader, SHG leaders contribute 5.9 Rupees as compared to 4.5 Rupees for non-SHG leaders. These relationships also hold when controlling for other characteristics by regression analysis as shown in Table 15.

**Table 15:** Multilevel Regression on Leaders' and Followers' Contributions in the Game

	(1)	(2)	(3)	(4)	(5)
	Leaders in the game	Leaders in the game dynamic	Leaders in the game dynamic with interaction term	Followers in the game	Followers in the game dynamic
HETLEAD	0.0443 (0.5136)	-0.4613 (0.4722)	-0.4261 (0.4710)	-0.0602 (0.2183)	-0.0130 (0.2440)
2ROUNDTREAT	1.3143** (0.6406)	1.0893** (0.5516)	1.0863* (0.5640)	-0.0464 (0.3155)	0.2260 (0.3585)
ENDOWMENT	0.3652*** (0.0927)	0.4057*** (0.0800)	0.4185*** (0.0817)	0.3027*** (0.0349)	0.3031*** (0.0395)
ROUND	-0.0619 (0.1636)	-0.0612 (0.1205)	-0.0760 (0.1308)	-0.0968 (0.1127)	-0.1301 (0.1299)
FEMALE	1.0625 (1.0881)	1.3281 (0.9467)	1.3760 (0.9723)	-0.0770 (0.9002)	-0.1517 (0.8551)
AGE	-0.0508 (0.0363)	0.0371 (0.0322)	0.0398 (0.0343)	-0.0633** (0.0259)	- 0.0704*** (0.0246)
MARRIED	-1.6134* (0.9625)	-0.7978 (0.8564)	-0.5458 (0.9210)	-1.5081** (0.6814)	-1.4172** (0.6360)
DAILYWAGE	0.0015 (0.0027)	0.0024 (0.0022)	0.0021 (0.0023)	-0.0020 (0.0022)	-0.0015 (0.0020)
WEALTH	-0.1229 (0.2239)	-0.0900 (0.1986)	-0.1099 (0.2047)	-0.1159 (0.1464)	-0.0854 (0.1380)
READANDWRITE	0.7136 (1.0385)	0.0013 (0.9451)	0.1659 (0.9981)	1.3107* (0.6856)	1.3382** (0.6352)
YEARSINSCHOOL	-0.1830 (0.1206)	0.0097 (0.1139)	0.0140 (0.1178)	- 0.2203*** (0.0816)	- 0.2336*** (0.0768)
YEARSINAREA	-0.0397 (0.0422)	-0.0089 (0.0344)	-0.0035 (0.0359)	-0.0171 (0.0316)	-0.0243 (0.0310)
SHGMEMBER	-1.4486* (0.7790)	-1.1624* (0.6369)	-1.1820* (0.6571)	-0.6219 (0.5562)	-0.6083 (0.5285)
SHGLEADER	2.2856*** (0.8631)	1.3694* (0.7109)	3.1424 (2.0482)	0.3300 (0.6154)	0.2690 (0.5819)
HINDU	-0.7700 (0.7432)	-0.2043 (0.6438)	-0.3412 (0.6614)	-1.2042** (0.5454)	-1.2136** (0.5225)
OWN CONTRIBUTION T-1		0.3821*** (0.0845)	0.4058*** (0.1028)		0.0930* (0.0498)

CONTRIBUTIONSOTHERS		0.0691**	0.0822**		-0.0532**
T-1		(0.0332)	(0.0412)		(0.0264)
OWN CONTRIBUTION T-1			-0.0382		
x SHGLEADER			(0.1903)		
CONTRIBUTIONSOTHERS			-0.0714		
T-1 x SHGLEADER			(0.0913)		
LEADERCONTRIBUTED				0.0821*	0.0747
				(0.0462)	(0.0515)
Constant	4.6833*	-4.3284*	-5.1893*	7.3690***	8.3096***
	(2.5238)	(2.5741)	(2.9284)	(1.6493)	(1.7729)
<hr/>					
lns1_1_1					
Constant	-0.0401	-5.2641	-0.7724	-0.2243	0.1432
	(0.4951)	(452.8226)	(1.8228)	(0.4416)	(0.3523)
<hr/>					
lns2_1_1					
Constant	0.0980	-0.8296	-0.6494	0.5441***	0.3206*
	(0.9393)	(3.3830)	(2.5793)	(0.1161)	(0.1792)
<hr/>					
lnsig_e					
Constant	0.7602***	0.6617***	0.6401***	0.6999***	0.7140***
	(0.2288)	(0.1842)	(0.1877)	(0.0449)	(0.0554)
<hr/>					
N	90	78	78	366	315
$\chi^2$	45.5379	96.9355	91.1925	106.7929	101.6673
$p > \chi^2$	0.0001	0.0000	0.0000	0.0000	0.0000
Log likelihood	-214.0202	-172.7603	-174.6926	-860.7163	-744.0824

Source: Own calculations; Standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

It is seen that SHG leaders contribute substantially more when leading in the game (2.29 Rupees in Model 1) while there is hardly an effect of being an SHG leader when following (Models 4 and 5). Analogous to the regression table above, Models (2) and (3) include the dynamic variables. Of note, the effect of one's own previous choices and the contribution of others becomes much larger in the leadership decisions, and the overall effect of actual leadership (variable SHGLEADER) becomes smaller, although with 1.37 Rupees, it is still relatively large (cf. Model 2). It seems plausible that actual leaders know how to lead whereas inexperienced participants, in a situation that is new to them, look for orientation and anchors in past choices.

To investigate this idea, Model (3) introduces two interaction terms: OWN

CONTRIBUTION T-1  $\times$  SHGLEADER and CONTRIBUTIONSOTHERS T-1  $\times$  SHGLEADER. It is now possible to distinguish between the effects of previous choices of leaders and non-leaders directly. Although the effects are not statistically significant different from zero, we find that the effect of previous contributions becomes even larger for non-leaders and smaller for leaders, which is indicated by the negative signs of the coefficient estimates for the newly introduced interaction terms. In other words, there is strong heterogeneity of the leadership treatment on experimental subjects contingent on their own experiences with leading in the game. Participants who are SHG leaders in actual life contribute larger amounts if they are leading; non-leaders – more than in other treatments – use previous decisions as an orientation when asked for a choice in a seemingly unfamiliar task.

Models (4) and (5) show that leaders' decisions encourage followers to contribute more. Each Rupee is matched by about 0.08 Rupees on average. Note that this effect is at the individual level. Thus, an increase in the leader's contribution stimulates four times this amount in total contributions of followers. In other words, the negative impact of the leadership condition in the game must be attributed to low contributions of first movers rather than to followers.

#### **4.5. Discussion**

In our experiment, heterogeneity has led to a small reduction of roughly two percent of total endowments in contributions to the public good. Thus, we fail to reject the first hypothesis and find some support for a negative effect of endowment heterogeneity on contributions. Although this effect is smaller than the 14.51 percent found on average in studies conducted in the lab, it is within the 95 percent confidence interval reported in the respective meta-analysis (Zelmer, 2003, p. 307).

In the daily practice of shared sanitation facilities, it may be more difficult to organize contributions to a public good in the presence of economic inequality. For wealthier users, access to private toilets may be within reach; however, the poorest individuals may find it difficult to contribute anything at all. In the relatively homogeneous – albeit extremely poor – communities we have studied, this factor seems to be less of a problem. This is not to say that other dimensions of heterogeneity may not matter for organizing collective action. The fast growing city of Hyderabad attracts migrants from all over India with diverse cultural and linguistic backgrounds. A lack of communication possibilities between

community members may at times be a greater barrier to cooperation than wealth inequality. Also, heterogeneity is embedded in the broader context of a particular community, and perceptions on what is a fair contribution may differ between cases.

In our game, we did not find much support for conditional cooperation. With the exception of leaders in the game, most participants did not condition their contributions on other participants' decisions. If these findings would mirror actual behavior, virtuous cycles of cooperation may be difficult to start. On the other hand, a complete break-down of cooperation is also less likely to occur, and we did not observe a strong decay effect in our game. From discussions with game participants, we know that small charitable contributions to festival expenses in the community follow individual considerations rather than beliefs and perceptions about others' behaviors. Contributions are typically not varied much, and often suggestions on the size of contributions serve as anchors, used by money collectors when raising funds for various communal activities.

Contrary to what is typically found in the experimental literature, leading by example does not increase contributions in our experiment. Thus, we reject the second hypothesis. In the experiment, leaders, on average, fail to contribute more and to set a positive example. Interestingly, there is strong heterogeneity in leaders' decisions. In particular, we found that those participants who have leading roles in actual life are well aware of the importance of setting a positive example. For this group, the second hypothesis cannot be rejected. Actual leaders' contributions, when randomly selected to lead in the game, were substantially higher. Regression analysis has also shown that followers condition their contributions on leaders' decisions, thereby further aggravating the positive effect on aggregate contributions. Hence, if actual leaders accept a leadership role in contributing to the public good, overall cooperation may increase.

These findings have implications for the practical organization of voluntary contributions to public goods. Increasing transparency and publicly announcing contributions seems useful, especially if leaders are not random. With an awareness of this phenomenon, communities may organize collections in a way that experienced leaders are the first to contribute, ensuring that these contributions become known to followers. Alternatively, leaders themselves may find ways to set a positive example and to communicate their decisions. It also follows from our results that with random leadership, inexperience makes things worse. In this case, anonymous collections are better than a sequential organization.



In our game, contributions of leaders were anonymous. It is an open question how people's behavior would have changed if contributions were known and could be linked to faces, social status, and information on caste or religion. Although experimental ethics forbid the conduct of experiments that link decisions and identities, other methods may be used to explore these factors. In small communities, information on social status, the possibility to build up a reputation, and the potential threat of peer punishment are further important issues to investigate.

#### **4.6. Summary and Conclusions**

Around the globe, a lack of access to adequate sanitation exposes the poor to serious health risks. Significant associated costs must be borne, especially by women and children in urban slums. In the absence of private toilets, neighbors often share toilets. In this paper, we have demonstrated that maintenance of such shared toilets can be viewed as a collective action problem, and we have used an economic experiment to study the impact of leading by example and economic heterogeneity on contributions to a public good. In our study, both factors exhibit a negative effect on cooperation, although leadership is positive if participants are experienced in this role.

In our experiment, experienced leaders came from the ubiquitous Indian self-help group system. Leaders were not different from non-leaders in our data, which suggests that experience with leadership matters more than personal characteristics. Thus, a careful rotation of leadership roles might have the potential to create substantive co-benefits in other domains such as sanitation. The ability to set a positive example is not necessarily an inborn character trait, but it can be learned with practice.

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## 5. EMPIRICAL STUDY III (P4): ACTIVE AND FORCED CHOICE FOR OVERCOMING STATUS QUO BIAS: A FIELD EXPERIMENT ON THE ADOPTION OF “NO JUNK MAIL” STICKERS IN BERLIN, GERMANY

Georg Liebig

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Jens Rommel

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

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**Abstract:** Consumers around the world are burdened by large amounts of unaddressed junk mail. Attaching “No junk mail” stickers to mailboxes offers a simple solution for protecting against unwanted ads. Presumably, the use of such stickers can be increased if consumers deliberately decide either for or against receiving junk mail. This conjecture of status quo bias was tested in a field experiment, run with more than 900 households in Berlin, Germany. In one treatment, stickers were put into mailboxes, facilitating active choice. In a second treatment, stickers were attached halfway onto the outsides of mailboxes, forcing consumers to either remove or fully attach them. It was found that roughly a fifth of the sample attached a sticker after treatment. With uptake of more than 21, as compared to 16 percent, the forced choice was more effective than the active choice treatment. The findings highlight the importance of green nudges and defaults for promoting pro-environmental behaviour. Implications for landlords of the presented interventions are discussed. The field of social norms is identified as a promising area for extending the scope of the present study.

**Keywords:** Active Choice; Behavioural Economics; Green Nudge; Junk Mail; Status Quo Bias

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## 5.1. Introduction

On a global scale, every year one million trees are cut to produce junk mail (McKenzie-Mohr, p. 28), and it is estimated that each household in Germany receives about 30 kg of unaddressed ads per year in its mailbox (WWF 2011, p. 4). Often these papers are thrown away unread, with more than a quarter of the German population reading junk mail only occasionally or not at all (Ifak Institut 2013). But consumers can protect themselves against junk mail by attaching a sticker stating “No junk mail, please!” to their mailboxes. It is common knowledge in Germany that these stickers can easily be fabricated at home or can be bought at low cost from stationary shops. They have been reported to be effective in stopping delivery of junk mail, are well known, and about one quarter of German households uses them (IfD 2013). Yet, large differences exist in the proportion of stickers across houses. Specifically, in some houses one can observe that landlords force tenants to actively decide either for or against junk mail by attaching a two-sided printed sign to their mailboxes: One side reads “Ads, yes please,” the other “No ads, please.” This way, tenants are constantly reminded of their choice either for or against junk mail, and, typically, if forced to decide, more people will opt against junk mail. The low cost of attaching such a sticker gives rise to the presumption that adoption in houses where there is no such forced choice is low due to status quo bias.

In this paper, we report results from a field experiment conducted with more than 900 households in Berlin, Germany, focused on the role of status quo bias in the adoption of mailbox stickers. We analyze here a single, simple outcome variable: whether or not people have actually attached to their mailboxes a “No junk mail, please!” sticker provided to them as part of this study. In one treatment of our experiment, consumers receive a sticker *in* their mailboxes, meaning that they need to *actively* decide either for or against using it. In another treatment, consumers are *forced* to decide: The sticker is halfway attached on the *outside* of their mailboxes, prompting them either to remove or fully attach it.

This study seeks to contribute to the small but growing empirical literature on green nudges and status quo bias in behavioural consumer research (Allcott 2011; Garner 2005; Kallbekken and Sælen 2013; Kallbekken et al. 2013; Ölander and Thøgersen in press; Pichert and Katsikopoulos 2008; Sunstein and Reisch 2013). Here we focus especially on



the role of active and forced choices as a means to achieve consumer sovereignty, a topic that has received relatively little attention in the literature so far (Sunstein 2013a).

The remainder of this paper is organized as follows. In the next section we provide some background regarding the context of our experiment, review the literature on status quo bias in consumer research, and develop two hypotheses. In succeeding sections, we describe the study and present its results, which are then discussed in light of the literature. In a concluding section, we summarize our findings and present an outlook on future research.

## **5.2. Background, Literature, and Hypotheses**

### **5.2.1. Environmental Context and Study Area**

Global environmental change has reached alarming levels. In some domains, planetary boundaries – safe limits within which humans can act without major and irreversible distortions of the earth system – have been passed (Rockström et al. 2009a; Rockström et al. 2009b). The resulting task, to transform societies towards sustainability, is a key policy challenge for the 21<sup>st</sup> century (WBGU 2011). It has been suggested that, by focusing on a few simple non-coercive interventions to stipulate behavioural change at the household level, much can be achieved (Schultz, in press). For example, it is estimated that the United States could at low cost eliminate up to 20 percent of its households’ direct greenhouse gas emissions through such means (Dietz et al. 2009). It has also been shown that defaults and the (social) marketing of sustainable products as “normal” can facilitate environmentally friendly consumption behaviour (Rettie et al. 2014; Sunstein and Reisch 2013; Thaler and Sunstein 2008).

Here, we focus on consumer willingness to avoid paper waste by attaching “No junk mail” stickers to their mailboxes. In this respect, it is worth mentioning that the paper and pulp industry is highly energy-intensive and, with an increase in output of more than 75 percent over the past twenty years, has experienced tremendous growth (Fleiter et al. 2012). According to its national umbrella organization (VdP 2013), in Germany the sector processes more than 10,000 cubic meters of solid pulpwood (p. 56) and uses more than 18,000 gigawatts of electricity (p. 57), which is equivalent to roughly three percent of the country’s overall electric energy use. The major share of these resources is used for producing printed paper (p. 57), most of which are consumed by private households. In

2010, the mean per capita consumption of paper in Germany was about 250 kg, as compared to a world average of 57 kg.

With one million trees cut globally, junk mail has a huge environmental impact and makes up large proportions of consumed paper and household paper waste (McKenzie-Mohr 2012; Resse 2005; WWF 2011). A study in North Shore City, New Zealand, has found that, by using a simple sticker, the amount of junk mail dropped by more than 70 percent, from 3.65 kg to 1.1 kg per month (Gregory and Roberts 2005, p. 209). In Germany such stickers are potentially even more effective, as in the past courts have decided in favour of consumers when stickers were ignored by deliverers (n.a. 2014). There are differing estimates on the adoption rate of stickers in Germany, ranging from about 15 percent (Ifak Institut 2012) to about 25 percent (IfD 2013). The following tables show the adoption of stickers disaggregated by age, income, and city size.

**Table 16:** Adoption of “No junk mail”-stickers by age for the years 2009 to 2013

Year	Total	14-29 years	30-44 years	45-59 years	More than 60 years	Sample Size
2013	25.50%	24.00%	26.80%	25.50%	25.50%	24,543
2012	25.10%	22.80%	26.10%	25.30%	25.80%	26,063
2011	25.20%	21.90%	26.90%	25.00%	26.30%	20,271
2010	24.30%	21.30%	25.80%	24.90%	24.70%	21,062
2009	23.60%	21.40%	24.70%	24.10%	23.90%	21,068

Source: IfD 2013

**Table 17:** Adoption of “No junk mail”-stickers by income groups for the years 2009 to 2013

Year	Below 1,500 €	1,500 to 2,500 €	2,500 to 3,500 €	Above 3,500 €
2013	23.6%	25.9%	25.3%	26.6%
2012	24.2%	25.3%	24.5%	26.3%
2011	23.8%	24.8%	25.9%	26.2%

2010	22.6%	23.9%	25.7%	25%
2009	22.2%	23%	23.5%	26.3%

Source: IfD 2013

**Table 18:** Adoption of “No junk mail”-stickers by population size for the years 2009 to 2013

Year	Below 20,000 people	20,000 to 100,000 people	Above 100,000 people
2013	20.1%	25.3%	33.3%
2012	19.6%	25.2%	33%
2011	20%	25.3%	32.6%
2010	18.9%	24%	32.4%
2009	17.9%	23.3%	32.2%

Source: IfD 2013

As can be seen, no large differences exist between age and income groups. However, stickers are used more in large cities than in small towns. It is also noteworthy that many people are not interested in reading junk mail, and large amounts of papers are thrown away unread. One survey found that more than 20 million Germans – or more than a quarter of the population – read junk mail only about once a month or less (Ifak Institut 2013). Given annual expenditures of about 2.5 billion Euros for unaddressed junk mail in Germany (TNS Infratest 2013), there is even a large savings potential for the advertisement industry via more precise targeting of consumers, with additional benefits for consumers and positive environmental side effects for society as a whole.

The experiment being reported in this paper was run in Berlin: Germany’s largest city, with a population of about 3.4 million people, as well as being the country’s capital and its political and cultural centre. Environmentalism, vegetarianism, and sustainable consumption, evident through for instance the purchase of organic and fair trade products from local food cooperatives, are relatively wide-spread, especially among the young possessing a high level of education in the inner city. The city is divided into twelve

administrative districts (*Bezirke*), which are similar in population size: ranging from about 220,000 in Spandau to about 370,000 in Pankow. These districts strongly differ in size, from about 20 km<sup>2</sup> for Friedrichshain-Kreuzberg to almost 170 km<sup>2</sup> for Treptow-Köpenick. During the cold war, from 1961 to 1989, the city was divided by the Berlin wall. Some of the differences between the former Eastern and Western parts stemming from this separation have remained, especially in the city's outskirts.

### **5.2.2. Consumer Choice and Status Quo Bias**

It is now widely accepted that conventional economic theory and its *homo oeconomicus* model fail to provide an adequate representation of human behaviour, which is sometimes characterized by anomalies and biases rather than utility maximization and (calculative) rationality. A large body of research in cognitive psychology and behavioural economics over several decades has shown that humans do reciprocate cooperative behaviour even when non-cooperation would yield them larger benefits; losses are valued higher than gains; and decisions are influenced by framing and priming (Ariely 2009; Kahneman 2011; Thaler 1992). Several scholars have called for the use of such findings in public policy and for the design of choices more generally (Camerer et al. 2003; Sunstein and Thaler 2003; Thaler and Sunstein 2003, 2008). In the United States (Sunstein 2013b) and the United Kingdom (Dolan et al. 2010), behavioural economics approaches are now being explicitly considered in public policy and administrative processes. Behavioural economics findings have also entered the OECD Consumer Policy Toolkit (Lissowska 2011) and are considered useful for facilitating pro-environmental behaviour at the national level in France (Oullier and Saunero 2011) or the supra-national level in the European Union (European Commission, DG Environment 2012). In a widely cited definition, the so-called nudge approach of “libertarian paternalism”, which seeks to actively influence the context of consumer choice, is described as follows: “A nudge, as we will use the term, is any aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid” (Thaler and Sunstein 2008, p. 6).

Nudges point towards the importance of social norms. People follow the behaviour of others, a finding that has been exploited in social marketing (Allcott 2011; Burchell et al. 2013; McKenzie-Mohr 2012; Rettie et al. 2014). A particularly strong phenomenon, which is also the focus of this paper, is people’s tendency to avoid change by sticking with the

status quo or default options, termed status quo bias in the literature (Anderson 2003; Samuelson and Zeckhauser 1988). Defaults can become effective through three principal mechanisms: (1) *implied endorsement*, where the default option may be perceived as a recommendation; (2) *cognitive bias*, where deviating from a default may be felt as a loss; and (3) *inertia* or “going with the flow”, where deviating from a default requires additional effort (Smith et al. 2013). Consequently, changing defaults can have tremendous effects also on pro-environmental behaviour of consumers, as demonstrated by the application of green defaults in many domains (Pichert and Katsikopoulos 2008; Sunstein and Reisch 2013, in press).

With respect to status quo bias, it has been pointed out that many choices require a default option, and default options are often inevitable. For instance, it has been shown that the legislative default is a powerful predictor of consent regarding organ donation (Abadie and Gay 2006). Yet, there is no simple way to avoid a default in national legislation. Either citizens have to opt out or have to opt in from a default. An alternative, over reaching societal consensus on all issues involving defaults is to “force people to make their choices explicit” (Sunstein and Thaler 2003, p. 178). In the organ donation example, one option could be forcing people to make a decision regarding donor status when receiving a driver’s license, or, more moderately, reminding them of the default option and potential benefits from behavioural change in this situation. Along these lines, the green nudge presented in this paper uses prompts to facilitate more deliberation in consumer choice. It aims at reducing status quo bias and improving consumer sovereignty by (1) reducing the effort to reach an active decision and (2) confronting consumers with a forced choice.

### **5.2.3. Hypotheses and Treatments**

Three groups of consumers can be distinguished regarding their attitudes towards “No junk mail” stickers: People in the first group are not interested in stickers at all, either because they want to receive ads, they do not believe in the effectiveness of stickers, or they do not want to use a sticker for some other reason. People in the second group do not want to receive ads and they are already using a sticker. Meanwhile, those in the third group do not want to receive ads and are interested in using a sticker, but they do not yet use one for some reason. During the experiment, groups one and three – all consumers not using a sticker – were treated, although the intervention was targeted at group three. Our presumption was that, for this third group, the adoption rate of stickers would increase if they were made available in the right place at the right time, prompting a decision.

Otherwise, this group, adhering to the status quo, would not adopt stickers. We reasoned that a good place and time to think about stickers would be when checking for new mail in front of the mailbox. Hence, as part of our study, households received stickers to test the first, more general, hypothesis:

**H1:** People will attach stickers to their mailboxes if these are easily available and it requires little effort to do so.

The second hypothesis asks about the importance of a forced choice option:

**H2:** Forcing consumers to decide either for or against receiving ads increases uptake as compared to a scenario in which choices can be more easily avoided or postponed.

To test the first hypothesis, in two different treatments, all households not already using a sticker received a short note with some background information on the initiative and a sticker. In the first treatment, henceforth called *ACTIVE*, stickers and notes were put *into* the mailboxes. People had to attach stickers themselves, thus allowing postponement of the decision, for instance by taking the sticker into their apartment. In the second treatment, henceforth called *FORCED*, stickers and notes were placed on the *outside* of the mailbox. Stickers were attached to mailboxes in a way that would allow people to either easily remove or permanently attach them to their mailboxes, thus forcing them to decide on an option. In the following section, the method and experimental procedure are explained in greater detail.

### **5.3. Method and Experimental Design**

It is often difficult to identify causal relationships in observational data, especially if these are cross-sectional. Particularly in the environmental domain, survey data are also prone to all kinds of misreporting, such as with social desirability bias. Economic experiments or quasi-experimental data can help overcoming these challenges by observing people's actions under actual incentives, and causality can then be established through the exogenous manipulation of factors of interest under controlled conditions (Parmeter and Pope 2013). It may be useful here to distinguish between four types of experiments in economics: (1) conventional lab, (2) artefactual field, (3) framed field, and (4) natural field (Harrison and List 2004). In natural field experiments, sometimes just termed 'field experiments', participant behaviour is observed in a context that is not artificially created or manipulated by the researcher. Typically, subjects also do not know that they are part of

an experimental study. When moving from the lab to the field or vice versa, researchers face trade-offs of control and external validity. Lab experiments allow for control of many factors that influence decision-making, whereas field experiments allow potentially greater external validity and obtaining of observational data in specific contexts. Under this typology, the experiment reported in this paper qualifies as a (natural) field experiment.

The experiment was run in September and October 2012 in Berlin. Sampling covered all twelve districts of the city, with the number of mailboxes roughly proportional to a district's population size and, wherever possible, also proportional to size within subdivisions (*Stadtteile*) of these districts. Data on population size were taken from official statistics (Amt für Statistik Berlin-Brandenburg 2012). It was not possible to obtain a full random sample of households or houses due to the unavailability of population data at this level. Within a district, a couple of arbitrarily selected streets were covered by the study. Although this has resulted in some spatial clustering, we believe it is fair to say that the sample represents the population of Berlin relatively well. More importantly, using a between-subjects design, assignment to each treatment was randomized at the house level, thus ensuring an optimal experimental design.

As part of the study, households were supplied with a small sticker in coloured print that was 35 mm by 70 mm in size (Figure 5).

**Figure 5:** Sticker used in the study



Stickers did not display any brand or logo and avoided identification with any organization. However, they did have an environmental framing to trigger pro-

environmental preferences and to highlight the relationship between avoiding junk mail and reducing negative environmental impacts. To increase salience and uptake, stickers were distributed in combination with small flyers stating “Attach the sticker – Save paper – Protect the environment” in German (Figure 6).

**Figure 6:** Flyer used in the study



These flyers, printed on black and white paper and 75 mm by 105 mm in size, also served as additional reminders, which have been found to be effective for achieving behavioural change in other studies (Cadena and Schoar 2011; Garner 2005; Karlan et al. 2010).

In the distribution phase, data were collected at the level of each house, including the total number of mailboxes and the number of mailboxes already equipped with a sticker. After twelve to sixteen days, houses were revisited to note down the main outcome variable: the number of households that had fully attached the sticker to their mailboxes. The study covered 125 houses with a total of 1,327 mailboxes. Out of these, 386, or 29.09 percent, were already equipped with a sticker. In total, then, 941 mailboxes did not have a sticker, and only these received one as part of our study in one of the treatments. In 57 houses the sticker was put into the mailbox (*ACTIVE* condition), while in 68 houses stickers were attached outside (*FORCED* condition).

#### **5.4. Results**

As a first step, it seems useful to compare the proportion of stickers in our sample with other samples. The 2012 figures from a large national survey on the adoption of “No junk



mail” stickers (IfD 2013) show that, at that time, from a sample of 26,063 people, 25.1 percent used a sticker. Although there is a statistically significant difference to our figure of 29.09 percent (Two-tailed two-sample test of proportions;  $z = -3.2602$ ;  $p = 0.0011$ ), it should also be pointed out that the same 2012 survey found 33 percent of respondents to use a sticker in cities with a population of more than 100,000 (Table 18). Taken together, it is fair to say that no substantial difference is to be found between the two figures and that the baseline adoption rate is similar to what others have found in survey-based research.

Looking at treatment effects and analyzing the data at the mailbox level reveals that 81 out of 507 (15.98 percent) of subjects attached the sticker in the *ACTIVE* condition, as compared to 94 out of 434 (21.66 percent) in the *FORCED* condition. The difference in proportions between treatments is statistically significant at the five percent level (One-tailed two-sample test of proportions;  $z = 2.2334$ ;  $p = 0.0128$ ), suggesting higher effectiveness of the *FORCED* treatment.

The following table displays selected variables of the collected data, disaggregated by neighbourhoods.

**Table 19:** Mailbox sticker data disaggregated by districts

District	Mailboxe s Total	Mailboxe s with stickers before treatment	Mailboxe s without stickers before treatment	Mailboxe s treated with <i>ACTIVE</i>	Mailboxe s treated with <i>FORCE</i> <i>D</i>	Stickers due to treatme nt	Sticker s due to <i>ACTIV</i> <i>E</i>	Stickers due to <i>FORCE</i> <i>D</i>	Total stickers before and after treatme nt	Proportio n of stickers before treatment	Uptake relative to mailboxe s treated	Uptake <i>ACTIV</i> <i>E</i>	Uptake <i>FORCE</i> <i>D</i>	Proportio n of stickers total
Mitte	104	22	82	45	37	20	10	10	42	21.15%	24.39%	22.22%	27.03%	40.38%
Pankow	178	68	110	51	59	32	11	21	100	38.20%	29.09%	21.57%	35.59%	56.18%
Reinickendorf	72	24	48	23	25	6	3	3	30	33.33%	12.50%	13.04%	12.00%	41.67%
Spandau	91	26	65	28	37	12	3	9	38	28.57%	18.46%	10.71%	24.32%	41.76%
Charlottenbur g-														
Wilmerdorf	182	82	100	78	22	11	8	3	93	45.05%	11.00%	10.26%	13.64%	51.10%
Friedrichshain														
-Kreuzberg	82	11	71	50	21	25	14	11	36	13.41%	35.21%	28.00%	52.38%	43.90%
Schöneberg-														
Tempelhof	122	31	91	47	44	18	8	10	49	25.41%	19.78%	17.02%	22.73%	40.16%
Marzahn-														
Hellersdorf	68	0	68	38	30	6	6	0	6	0.00%	8.82%	15.79%	0.00%	8.82%
Neukölln	124	38	86	43	43	13	6	7	51	30.65%	15.12%	13.95%	16.28%	41.13%
Treptow-														
Köpenick	113	44	69	31	38	10	8	2	54	38.94%	14.49%	25.81%	5.26%	47.79%
Steglitz-														
Zehlendorf	81	0	81	37	44	16	3	13	16	0.00%	19.75%	8.11%	29.55%	19.75%
Lichtenberg	110	40	70	36	34	6	1	5	46	36.36%	8.57%	2.78%	14.71%	41.82%
TOTAL	1,327	386	941	507	434	175	81	94	561	29.09%	18.60%	15.98%	21.66%	42.28%

Source: own calculations

The columns on the relative frequencies of uptake refer to the number of mailboxes treated. It can be seen that uptake was particularly high in the inner city, headed by Friedrichshain-Kreuzberg, Germany's stronghold of the Green Party<sup>28</sup>, with about 35 percent of subjects attaching the stickers provided for them by the study. With the exception of Marzahn-Hellersdorf and Steglitz-Zehlendorf, at the end of the study more than 40 percent of sampled consumers were using a sticker in all districts. These figures indicate a substantial increase – in most cases of more than ten percent – in the use of stickers, compared to both our baseline of 29 percent or the large-scale survey baseline (IfD 2013) of 33 percent.

To better understand differences at the district level, Table 20 shows the lower and upper bounds of 95 percent confidence-level intervals (exact binomial distribution) of the proportion of stickers before and after treatment.

**Table 20:** Confidence intervals (95 percent) of sticker proportions before and after treatment by districts

District	Lower bound before treatment	Upper bound before treatment	Lower bound total after treatment	Upper bound total after treatment
Mitte	.1375726	.3025688	.3087472	.5045677
Pankow	.3103478	.4576965	.4855874	.6359152
Reinickendorf	.2265634	.4542961	.3015212	.5388867
Spandau	.1958658	.3899882	.31501	.5256721
Charlottenburg-Wilmersdorf	.3768553	.5258808	.4359604	.5856539
Friedrichshain-Kreuzberg	.0689121	.2273606	.3295502	.5530336
Schöneberg-Tempelhof	.179633	.3408757	.3139321	.4942378
Marzahn-Hellersdorf	0	.052803	.0330725	.1822232
Neukölln	.2268317	.3955711	.3237369	.5031819

<sup>28</sup> In the 2013 general elections, Hans-Christian Ströbele, running in this district, received roughly 40 percent of the first vote, being Germany's only candidate of the Green Party to gain a direct mandate for parliament. In comparison, on average the Green Party received 8.4 percent of the second votes in Germany in the same elections.

Treptow-Köpenick	.2990672	.4856416	.3830371	.5739031
Steglitz-Zehlendorf	0	.0445203	.1173315	.3008627
Lichtenberg	.2740242	.4607895	.3248276	.5160541

Source: own calculations

Statistically significant differences at the 5 percent level between districts exist when there is no overlap between confidence intervals. It can be seen that, before treatment, especially Steglitz-Zehlendorf and Marzahn-Hellersdorf have substantially lower proportions of stickers that are also statistically significant from all other districts. Also Friedrichshain-Kreuzberg has a low ratio of stickers, which is statistically significantly different from proportions in Pankow, Charlottenburg-Wilmersdorf, and Treptow-Köpenick. Further, Charlottenburg-Wilmersdorf, the district with the highest proportion of mailboxes with stickers before treatment, is statistically different from Mitte, which has a comparatively low proportion.

After treatment, Steglitz-Zehlendorf and Marzahn-Hellersdorf are still statistically significantly different from all other districts and have a much lower proportion of stickers. Yet the differences between all other districts have evened out following the treatment, with no further statistically significant differences remaining at the five percent level.

The data can also be analyzed at the house level, which could be important with respect to evaluating the treatment's up-scaling potential, as landlords would appear to have an interest in reducing junk mail. In the following table, we present summary statistics and compare the proportion of stickers at the house level between treatments.

**Table 21:** Summary statistics of proportions at the house level by treatments

Variable	N	Median	Mean	SD	Min	Max
Proportion of stickers per house	68	.0732323	.1437085	.1784171	0	.6666667
<i>ACTIVE</i>						
Proportion of stickers per house	57	0	.2156595	.2930255	0	1

Source: own calculations

Due to the high number of zeros in the sample – indicating houses where nobody attached a sticker – there is no statistical difference in medians between treatments (Rows 2 and 3 in Table 21; Wilcoxon–Mann–Whitney test;  $z = 0.595$ ;  $p = 0.5517$ ).

## **5.5. Discussion**

First and foremost, the results demonstrate that people can be motivated to reduce paper waste from junk mail, flyers, and menus by a simple intervention – making stickers easily available. From the 1,327 mailboxes in our sample, 386 (29.09 percent) had a sticker attached before our intervention. After we distributed the stickers, this figure increased substantially by 175 to 561 stickers, meaning 42.28 percent of all mailboxes. This is equivalent to an absolute increase of 13.19 percent and a relative increase of 45.34 percent. Clearly, this leads to acceptance of the first, more general, hypothesis **H1**.

The two treatments differed in terms of their uptake. With a difference in proportions of more than five percent – which is also statistically significant at the five percent level – we accept **H2**. In line with the literature on status quo bias, going with the forced choice is more effective as an intervention than simply distributing stickers for free. Our example of forcing consumers to decide either for or against a sticker is a relatively harmless choice that is easily *reversible* and reversible at *low cost*. Further, the *consequences* of the decision, at least on an individual level, are not very substantial. In our view, these criteria will also be useful when evaluating forced choice interventions in other contexts. If decisions can be made at low cost and they are easily reversible, making a deliberate choice by being *forced* to decide seems to be a preferable option, especially when the costs of the intervention are small (a simple sticker in our example) and the consequences of sticking with a default are large (30 kg of paper waste per household each year in our example).

Based on this estimate of 30 kg of junk mail per German household each year (WWF 2011), we calculate that about 5,250 kg – 175 new stickers x 30 kg – less junk mail will be distributed annually due to our intervention. Projecting these figures for all 1.9 million households in Berlin, reveals a large saving potential. Up-scaling the intervention for the

whole city, assuming that 13.19 percent, or roughly 250,000 households more, would adopt stickers, could reduce paper waste by up to 7,500 tons per year. This could also help consumers to save time and money on waste disposal. Handled properly, the advertisement industry could save costs on junk mail that is thrown away unread without having an effect on potential customers. In addition, society as a whole could enjoy environmental benefits, assuming that the production of ads is adjusted accordingly.

In this context, it is important to consider who could have an interest in applying the “green nudge” presented in this paper. In Germany, landlords would perhaps have a great interest in preventing waste, with the aim of saving costs for cleaning up and disposal of discarded junk mail. In many instances, tenants receive “No junk mail” stickers along with their rental contracts. A key problem, however, is that the decision to attach the sticker can be postponed or the sticker can get lost somewhere among their files. It directly follows from our study that landlords should rather attach a sticker half-way to their new tenants’ mailboxes. They may even change the default status by attaching a sticker or using two-way signs. It would also be interesting to see how uptake of stickers would change if these were to be distributed under the landlord’s authority. Further, it would be important to look at variations of the interventions we have presented here. In particular, it could be interesting to develop an (experimental) test of interaction among neighbours. Clearly, in the way our intervention was designed, observations are not fully independent from each other. If many neighbours attach stickers to their mailboxes, this may motivate others to follow. Attaching stickers could then be perceived as a social norm. Ultimately, our design cannot control for such spillover effects and interactions among neighbours. With more resources available, one could treat only one mailbox per house to rule out such effects.

## **5.6. Summary and Conclusion**

In this paper we have reported results from a field experiment on the adoption of “No junk mail” stickers. We have found that people are more likely to use stickers when they are *forced* to make a fairly simple and reversible decision about using them. It has also been pointed out that forced choice is a preferable option if (1) the intervention is cheap, (2) potential benefits for society are large, (3) potential negative consequences for individual consumers are low, and (4) choices are easily reversible. If all four conditions are met, little can be said against forcing consumers to become explicit about their choices.

It seems an open question, and an interesting task for future research, to develop a test for a finer-grained separation of the status quo bias effect found in our study. It could be useful to differentiate between implied endorsement, social norms, loss aversion, or inertia as distinct mechanisms that might be responsible for consumers' decisions. Here, we were not able to separate these. In future research, a promising modification could be to change the framing of stickers. Here, we have used an environmentally framed sticker which may not have been appealing for everyone in our sample. Indeed, the low uptake of stickers in some areas of the city whose populations are rather reluctant regarding environmentalism (e.g., Marzahn-Hellersdorf) indicates that framing may be an important factor contributing to success or failure. Research in this direction could provide further insights on personalized defaults. There is a strong notion that social norms and implied endorsement may matter in such instances. Also, it could be helpful to extend the methods employed in this paper for exploring the motivations of consumers in greater detail. Experiments may be combined with qualitative interviews or surveys to learn more about the diverse possible drivers for banning junk mail.

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## 6. EMPIRICAL STUDY VI (P5): GAME PARTICIPATION AND PRESERVATION OF THE COMMONS: AN EXPERIMENTAL APPROACH

Jens Rommel

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Sergio Villamayor-Tomas

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Malte Müller

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

Christine Werthmann

Humboldt-Universität zu Berlin  
Department of Agricultural Economics

In preparation for submission

**Abstract:** Framed field experiments are widely applied to study behaviour in common pool resource management. Anecdotal evidence suggests that participation in such experiments improves cooperation in actual field settings. Yet, formal tests of such relationships are absent from the literature. We present results from a field experiment, investigating how participation in a two person prisoner's dilemma game affects pro-environmental behaviour, as measured by donations to an environmental foundation. We find that subjects who participate in the prisoner's dilemma game donate slightly more than subjects who do not participate in the game. Participants who are paired with a cooperative player in the prisoner's dilemma also donate more. We further find that donations substantially increase with income and concern for environmental issues.

**Keywords:** Cooperation; Environmental Behaviour; Experimental Economics; Field Experiment; Prisoner's Dilemma

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## **6.1. Introduction**

Over the last two and a half decades, our understanding of self-governed common pool resource management has improved substantially. The use of multiple methods has played an important role in this process (Poteete et al. 2010). Case study analysis has led the way to develop new theory (Ostrom 1990), showing that a “tragedy of the commons” (Hardin 1968) is not an inevitable outcome of common pool resource settings. Lab experiments have advanced our understanding of the role communication, punishment, or rewards play in shaping behaviour in social dilemma games (e.g., Ostrom et al. 1994; Fehr and Gächter 2000).

For the last fifteen years, economic experiments on common pool resource management have been brought to the field to understand cross-cultural differences in fairness norms and to reduce bias from so-called WEIRD (Western Educated Industrialized Rich Democratic) subjects or abstract decision-situations (Henrich et al. 2004; Henrich et al. 2010). As pointed out by scholars, adding local context to the game is important because it “is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects” (Harrison and List 2004).

Today, field experiments are widely applied, and increasingly used to inform policy-making (Viceisza 2012). Many experimentalists, especially from the “Colombian school” around Juan-Camilo Cárdenas, notice positive effects of game playing on collective management of common pool resources (Cardenas and Carpenter 2005; Lopez 2008). On a related note, the effect of playing games in teaching is widely studied – with mixed evidence (Randel et al. 1992). In Economics, class room games have a long history (Dimand 2005) and can improve teaching effectiveness, for instance in Environmental Economics classes (Frank 1997). For the field settings described above, such type of evidence is absent. It is an open question whether economic games have any short and/or long term effect on behaviour in environmental management. If such effects exist, one would like to know whether these are positive and large in the absence of other external interventions.

A key practical challenge to test the effect of game playing in the field is to find and to measure an adequate outcome variable. A recent large-scale research project, implemented in several Indian villages, studies the effect of playing “water management games” on geo-physical variables such as the water table (Meinzen-Dick 2013; Meinzen-Dick et al. 2014). At a smaller scale and in a different context, our paper follows a similar approach. In a simple experiment, we investigate how being paired with a “cooperator” or “defector” in a two person prisoner’s dilemma game affects cooperative behaviour in the short term, as measured by a post-game anonymous donation to an environmental foundation. According to our general hypothesis, the existence and direction of effects promoted by game playing depends on whether such an experience is positive or negative, i.e., whether it is an experience of cooperation or defection, respectively. The two types of players in the prisoner’s dilemma game, i.e., cooperator vs. defector, serve us as treatments which we compare to a control group of participants who do not play the game. This allows us to (1) test whether social dilemma game playing can have a short term effect on environmentally cooperative behaviour by itself and (2) the direction of such potential change.

The paper sheds light on the merits and demerits of conducting social dilemma games in the field for inducing behavioural change. Also from personal experience, we know that conducting experiments in difficult contexts may evoke or aggravate conflicts in a particular community. Such effects could potentially violate ethical principles in experimental research (cf. Teele 2014). With the aim to develop adequate field tools built on economic games, it is, thus, important to develop these carefully. This paper can be seen as an attempt to shed some light in that direction.

The remainder of the paper is structured as follows. In the next section, we introduce the experiment, develop the hypotheses, and describe the sample. Then, we present and discuss the results of the game. Finally, we summarize and conclude.

## **6.2. The study**

Conflicts over natural resources are the norm rather than the exception. Rarely will one encounter empirical settings where, in one way or the other, distributional conflicts over common pool resources do not prevail. When extraction cannot be monitored and agreements cannot be enforced, resource users may find themselves trapped in a situation where individually rational behaviour makes everybody worse off, i.e., a social dilemma.

In the behavioural sciences a large literature is concerned with the question of how pro-environmental behaviour can be advanced. One approach promotes the use of “green nudges” – small changes in the choice architecture that maintain freedom of decision makers – with the aim to make the socially desirable choice easier (Lucas et al. 2008; Osbaldiston and Schott 2012; Sunstein and Reisch 2013, 2014; Croson and Treich 2014). There is also a large experimental literature on charitable donations (e.g., Shang and Croson 2005, Landry et al. 2006) that seeks to identify social factors that determine the size of contributions. Our study is related to this literature as it seeks to identify factors that influence environmental behaviour and uses a charitable donation as an outcome variable. Yet, the broader aim is to develop experimental games into educational tools for application in field settings.

Experimental Economics try to reproduce these real-life dilemma settings in framed field experiments to study cause, effect, and solution mechanisms with greater external validity (Harrison and List 2004). One of the most replicated findings in Experimental Economics is that people participating in repeated social dilemma games start with initially high levels of cooperation; however, in the absence of sanctioning, rewards, or communication, cooperation typically breaks down over time (Fehr and Gächter 2000; Balliet 2010).<sup>29</sup> The simplest version of a social dilemma game is the two person prisoner’s dilemma. In this game, independent of the other player’s strategy, it is individually rational to choose the non-cooperative strategy. This behaviour leads to a Pareto-inferior Nash equilibrium; both players would be better off if they could make binding agreements on cooperation.

In our experiment, we let a group of participants engage in such a prisoner’s dilemma game. Rather than in the outcomes of the game itself, we are interested in understanding how different experience of participation affects subsequent behaviour. If positive effects could be demonstrated, such games may, indeed, be helpful in the field. We formulate the following hypotheses:

**H1:** Positive experience – being paired with a cooperative player – in a prisoner’s dilemma game results in positive behavioural change, as measured by a donation, after the game.

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<sup>29</sup> There is a large and converging literature on pro-social behaviour in Economics and Social Psychology. For a recent review and comparison of these two branches focusing specifically on pro-environmental behaviour see Turaga et al. (2010).

**H2:** Negative experience – being paired with a non-cooperative player – in a prisoner’s dilemma game results in negative behavioural change, as measured by a donation, after the game.

To test these hypotheses we have developed the following experiment. In a first step, subjects play a prisoner’s dilemma game which, for easier understanding, is framed as a decision to invest in a common project. In a second step, subjects play a framed dictator game which generates the main variable of interest. Monetary payoffs of the prisoner’s dilemma are depicted in Table 22.

**Table 22:** Payoff Table Prisoner’s Dilemma

		Player 1	
		Invest	Don’t invest
Player 2	Invest	2 Euro, 2 Euro	3 Euro, 0 Euro
	Don’t invest	0 Euro, 3 Euro	1 Euro, 1 Euro

Source: own elaboration

In each cell, the first number denotes Player 1’s payoff, and the second number denotes Player 2’s payoff. The social optimum is that both players invest, and in (Nash) equilibrium none of the players invests. Participants in this game are compared to a group of people who has not played the prisoner’s dilemma game, but are still asked to donate. For all groups, the outcome variable we study is the amount of an anonymous donation to an environmental foundation, which can also be understood as a dictator game (Blanco et al. 2012). The environmental foundation chosen is the “Deutsche Bundesstiftung Umwelt” (DBU), a foundation active in promoting research and project implementation concerned with environmental protection.<sup>30</sup>

We played the game with 45 participants whom we recruited during an open door event at a German university. Every year, on a Saturday in late spring or early summer, the city of Berlin organizes the so-called “Lange Nacht der Wissenschaften” (long night of sciences), during which several university and other research institutes open their doors and present

<sup>30</sup> We have selected this foundation to add environmental context to the game. At the same time we were interested in choosing an organization that does not evoke strong emotions in subjects. If we would have chosen a large and well-known organization such as Greenpeace, a participant’s attitude towards this organization may have dominated the donation decision. Ultimately, this would have resulted in more variation in the data and less statistical power. Every participant has received a handout describing the work of DBU. It was made clear to participants that none of the involved researchers was affiliated with DBU or would in any way benefit from the donation. The handout is available from the authors on request.

their research and teaching program to the interested public. Typically, the event starts in the afternoon and lasts until midnight. Our experiment took place at a small stall presenting research on social-ecological systems, including posters and further information on the use of framed field experiments for their analysis.<sup>31</sup> Visitors of the event were approached with a small standardized text presenting the idea of the game and asking for participation.<sup>32</sup> It was explained to them that, as a compensation for participation, everybody would receive five Euros. Everybody was also told that the reward, or parts of it, could be donated, and a “voting booth” was prepared and visible to visitors for this purpose. Donations were only matched to questionnaire data and assigned treatment using a code, making it impossible to link size of the donations to names. If visitors agreed to participate, they were presented with further instructions, printed out on paper. These instructions explained that with a 16 out of 45 probability, they would have to fill a small questionnaire; with a 29 out of 45 probability they would play a game – the prisoner’s dilemma game explained above. To maintain anonymity and to reduce logistical demands, visitors playing the prisoner’s dilemma game were paired with student players with whom we played the game in two lectures in the week before the event.

The students were told that we would play the game with visitors at the open door event and that they would receive payoffs from the game in the week after the event. They received written instructions depicting the payoffs, similar to those of the visitors.<sup>33</sup> Out of the 29 students who participated, 17 played cooperatively, i.e., “Invest,” and 12 played uncooperatively, i.e., “Don’t invest.” Note that students were also paid, but their decisions are not part of the analysis of this study. The sole purpose of students’ decisions was to assign visitors in the event to the “cooperation experience” vs. “defection experience” treatments in a meaningful and random way. Also note that beside the 29 visitors paired

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<sup>31</sup> A recent overview and a typology of such experiments can be found in Rommel (2014). Note that in the classification of Harrison and List (2004) our (donation) experiment can be regarded as a (natural) field experiment. In the donation part, subjects – although they play a game as part of our study – do not know that they are participating in an experiment on the effect of playing or not playing a game. Arguably, the context is rather artificial though. The prisoner’s dilemma game played by subjects qualifies as an artefactual field experiment, however.

<sup>32</sup> An English translation of the text reads as follows: “We are a group of resource economists from this university and we frequently conduct experiments to study behaviour in environmental decision-making. Today, we would like to give you the chance to participate in such an experiment to give you the opportunity to learn about our work. If you are interested, we will play – with some probability – a small game with you. It will not take more than 15 minutes. For your participation you will get compensated with five Euros. If you want or if feel for some reason uncomfortable taking this money you may anonymously donate this sum or parts of it to an environmental foundation.” All additional materials and texts are available from the authors on request.

<sup>33</sup> Students received English instructions, whereas visitors received everything in German. We have spent great care, however, that, apart from language, instructions of the prisoner’s dilemma game are the same for both parties.



with student players, a control group of 16 visitors complemented the experimental design which is summarized in Table 23.

**Table 23:** Experimental Design and Procedure

In total 45 visitors participate, everybody receives 5 Euro		
After a short general introduction, participants are randomly assigned to one of three treatments (Step 1)		
Baseline	Treatment 1	Treatment 2
CONTROL	COOPGAME	DEFECTGAME
16 participants	17 participants	12 participants
	Rules of the game are read out and handed over in written form (Step 2)	
	Decision in Prisoners' dilemma game (Step 3)	
	Receive information on game outcome; get paid; put payments to your pocket (Step 4)	
	Receive envelope with five Euros; possibility to donate (Step 5)	
	Sign receipt (Step 6)	
Short one-page questionnaire (Step 7)	Short two-page questionnaire (with three additional questions for those who played the game) (Step 7)	

Source: own elaboration

It can be seen that steps 1 and steps 5 to 7 were common to all 45 participants: everyone was approached with the same text, everyone received an envelope with five Euros<sup>34</sup>; everyone signed a receipt for the money; and everyone filled a small self-administered questionnaire. Participants in the CONTROL group went only through these stages. Participants in the other two treatments played the game in between those stages. The rules of the prisoner's dilemma game were explained to them, and they were informed that, to calculate their payoffs after their decisions, they would be paired with a student whose

<sup>34</sup> In all cases the envelope contained one 10 Cent coin, two 20 Cent coins, one 50 Cent coin, two 1 Euro coins, and one 2 Euro coin (= 5 Euro in total). This allowed participants to choose any amount between 0 and 5.00 Euro in steps of 10 Cents. Like all other decisions, donations were made fully in private and a voting booth was provided for this purpose. In this booth we placed a box into which subjects should put the envelopes, even when these are empty, i.e. when all money was taken home.

monetary payoff would also depend on the decision made in the game. Participants did not know anything about the distribution of cooperators or defectors among students. They were informed about what participants were paid according to the payoffs presented in Table 22 above, depending on the decision of the student with whom they were matched, as well as their own decision. After this, they were informed about the game outcome, they received payments in cash (if any), and they were asked to put away the money they received (if any). Table 24 describes the sample and the variables used in the results part of the paper and presents some summary statistics on the participants.

**Table 24:** Variable Description and Summary Statistics

Variable Name	Description	N	Mean	SD	Min	Max
PLAYSGAME	= 1 if either COOPGAME or DEFECTGAME treatment (participant plays the game)	45	0.64	0.48	0	1
COOPPARTNER	= 1 if COOPGAME treatment (paired with cooperator in the game)	45	0.38	0.49	0	1
FEMALE	= 1 if participant is female	45	0.51	0.51	0	1
YEAROFBIRTH	= year of birth	44	1977.25	12.52	1947	1994
UNIVDEGREE	= 1 if participant holds university degree	45	0.69	0.47	0	1
INCOME	= 1 if monthly household income is 2,000 Euros or more	43	0.56	0.50	0	1
ENVPROBLEMS	5-point Likert scale agreement with statement “I am interested in environmental issues.”  (1 = high agreement; 5 = low agreement)	45	1.36	0.61	1	3
ENVEVERYDAY	5-point Likert scale agreement with statement “In my daily decision-making, I consider the environmental impact.”  (1 = high agreement; 5 = low agreement)	45	1.60	0.58	1	3

	agreement)						
DONBEHAV	5-point Likert scale agreement with statement “I regularly donate money.”	44	2.68	1.34	1	5	
	(1 = high agreement; 5 = low agreement)						
KNOWDBU	= 1 if participant knows “Deutsche Bundesstiftung Umwelt” (DBU)	44	0.23	0.42	0	1	

Source: own calculation

It can be seen that from the 45 subjects participating, 22 were male and 23 female. On average, subjects were highly educated with more than two thirds holding a university degree. The average year of birth was about 1977, which is approximately equal to an average age of 36 years. Participants’ households are rather well off, with more than half of the sample having a monthly income of 2,000 Euros or more. Most people agree with the statements “I am interested in environmental issues” and “In my daily decision-making, I consider the environmental impact.” Most people do not regularly donate money. The environmental foundation, we have selected for the donation part, was known by about a quarter of participants.

## 6.3. Results

### 6.3.1. Game results and treatment effects

The vast majority of participants in the experiment – 22 out of 29 – cooperated in the prisoners’ dilemma game. Matching them with the student sample resulted in the following outcomes (Table 25).

**Table 25:** Frequency Table Prisoner’s Dilemma Game

		Visitor at open door event		
		Invest	Don’t invest	Total
Student	Invest	13	4	17
	Don’t invest	9	3	12
	Total	22	7	29

Source: own elaboration

It can be seen that most of the students and visitor do not behave rational and selfish. Roughly half of the matched pairs (13) achieve the socially optimal outcome and only three matches result in the Nash equilibrium.<sup>35</sup> Table 26 describes the main treatment effects on the key outcome variable and some useful binary transformations – the incidence of zero donations, of full donations, and of donations of more than five Euros.<sup>36</sup>

**Table 26:** Outcomes by Treatments

Outcome Variable	CONTROL treatment	DEFECTGAME treatment	COOPGAME treatment
Mean Donation in Euro (SD)	2.91 (2.28)	3.38 (2.04)	4.24 (2.02)
Zero donations (Absolute; relative frequency)	4 subjects; 25.00%	2 subjects; 16.67%	1 subject; 5.88%
Full donations (Absolute; relative frequency)	7 subjects; 43.75%	6 subjects; 50.00%	10 subjects; 58.83%
Donations of more than five Euro (Absolute; relative frequency)	0 subjects; 0.00 %	0 subjects; 0.00 %	3 subjects; 7.65%  (all of them cooperators themselves)

Source: own calculations

<sup>35</sup> There are various possibilities for explaining the larger proportion of cooperators in the visitor sample. For instance, visitors may care less about rewards because they are wealthier; they may also have lower opportunity costs of time in the context of the exhibition and self-select into a study which they may find interesting for other reasons. Note that we are not interested in this difference, however. The student sample serves us just as a source of exogenous cooperative and uncooperative decisions to be matched with visitors for practical reasons.

<sup>36</sup> When we opened the box containing the envelopes with donations, we have found three envelopes containing more money than the five Euros everyone was equipped with. Subjects were asked to put away the money they earned in the prisoner's dilemma game. Because the donation was anonymous and took place in the voting booth we could not control how much people actually put into the envelopes. We could, thus, also not rule that somebody was adding more than the five Euros to the envelope. In fact, all those three envelopes contained seven Euros. In all three cases participants have played the game and in all three cases the outcome was C, C (Invest, Invest), i.e. subjects have received two Euros from the game. We thus believe that these three participants have added the two Euros gained in the game to the donation.

It can be seen from the table that compared to the CONTROL treatment, donations increase by 47 Cents in the DEFECTGAME treatment. Compared to DEFECTGAME, another 86 Cents more are donated in the COOPGAME treatment. Comparing the CONTROL to the COOPGAME treatment, we observe a difference of 1.14 Euros. Owing to the small sample size, differences in medians between the three treatments are statistically not significant at the five percent level (Kruskal-Wallis equality-of-populations test; d. f. = 2;  $\chi^2 = 2.654$ ;  $p = 0.2653$ ).

Similarly, the proportion of zero donations steadily decreases from the CONTROL to the COOPGAME treatment. Formal testing of the differences of approximately 20 percent between CONTROL and COOPGAME reveals that the difference is statistically significant at the ten percent level (One-sided two-sample test of proportion;  $z = 1.5308$ ;  $p = 0.0629$ ).<sup>37</sup> In line with these results, the proportion of full donations (of five Euros and more) increases in the experiment.<sup>38</sup> Three participants donated more than the five Euros they were endowed with. This difference is statistically significant at the five percent level (One-sided two-sample test of proportion;  $z = -1.7624$ ;  $p = 0.0390$ ) between the CONTROL and COOPGAME treatments and statistically significant on the ten percent level between the DEFECTGAME and COOPGAME treatments ( $z = -1.5369$ ;  $p = 0.0622$ ).

Summing up, independently from the particular measure, participating in the prisoner's dilemma game had a positive effect on altruistic behaviour in the dictator's game. The positive effect was larger for participants who had a positive cooperative experience than for participants who were confronted with a negative cooperative experience. In the next section we will challenge these findings by controlling for socio-demographic heterogeneity, which is of particular importance due to the fairly small sample and the absence of pre-game cooperation tests.

### **6.3.2. Game decisions and socio-economic heterogeneity**

Table 27 presents three different specifications of OLS regression models where the amount donated is the dependent variable. The independent variables are presented in Table 24 above.

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<sup>37</sup> The test statistics and p-values for the difference between CONTROL and DEFECTGAME are  $z = 0.5318$  and  $p = 0.2974$  and  $z = 0.9392$  with  $p = 0.1738$  for the difference between DEFECTGAME and COOPGAME.

<sup>38</sup> The test results are  $z = -0.3282$ ,  $p = 0.3714$  when comparing CONTROL and DEFECTGAME;  $z = -0.4706$ ,  $p = 0.3190$  when comparing DEFECTGAME and COOPGAME; and  $z = -0.8659$ ,  $p = 0.1933$  when comparing CONTROL and COOPGAME.

**Table 27:** OLS Regression Models on Donations

	(1)	(2)	(3)
PLAYSGAME	0.4625 (0.8089)	0.8533 (0.7541)	0.4377 (0.7319)
COOPPARTNER	0.8603 (0.7987)	0.6825 (0.7567)	0.6560 (0.7263)
FEMALE		-0.9634* (0.5566)	
YEAROFBIRTH		0.0628* (0.0324)	0.0822*** (0.0299)
UNIVDEGREE		-0.9970 (0.6508)	
INCOME		2.6938*** (0.7048)	2.9499*** (0.6858)
ENVPROBLEMS		-1.6785** (0.6242)	-1.5792*** (0.5313)
ENVEVERYDAY		0.8604 (0.6619)	
DONBEHAV		-0.3343 (0.2598)	
KNOWDBU		-0.1016 (0.7164)	
Constant	2.9125*** (0.5296)	-120.0077* (64.0144)	-159.1082** (58.9099)
<i>N</i>	45	41	42
adj. $R^2$	0.029	0.371	0.343
F	1.6541	3.3611***	5.2836***

Source: own calculations; Standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

The first model includes only the randomized treatment variables as independent variables. Hence, the constant term is equal to the average donation in the CONTROL treatment (cf. Table 26). The coefficient of PLAYSGAME can be interpreted as the effect of playing the game, i.e., the joint effect of COOPGAME and DEFECTGAME. The coefficient of

COOPGAME is the additional amount donated as an effect of being paired with a student playing cooperatively. Columns two and three also control for socio-economic heterogeneity from the survey data. While model 2 includes the full set of independent variables, model 3 is a more parsimonious specification.

Model 1 performs rather poorly in terms of F-statistic and  $R^2$  value. The models in the last two columns, however, show relatively high and statistically significant F-statistics, indicating a good overall explanatory power. The same applies to the relatively high  $R^2$  values which show that more than one third of the variance in donations can be explained by assignment to treatment and observed socio-economic heterogeneity.

It can be seen that in all columns, treatment effects are positive and relatively large. Owing to the small sample, they are not statistically significant, however. In all models, the effects of PLAYSGAME and COOPGAME add up to more than 90 Cent. In other words, in all three specifications, players who make a positive experience donate substantially more than those who do not play the game. More than 65 Cent of this sum can be attributed to the difference between the positive and negative experience.

The second column shows that a relatively large gender gap exists in donations: women donate about 90 Cents less. Models (2) and (3) show that the younger a participant, the more she donates. This effect is quite substantial. An increase in YEAROFBIRTH from 1960 to 1990 would, *ceteris paribus*, result in an increase in donation of about 2.75 Euros in model (3). Participants holding a university degree donate about one Euro less in the game. Very large, and statistically significant, effects can be observed for the income variable. Subjects with monthly incomes of more than 2,000 also donate substantially higher amounts – more than 2.50 Euros on average. Large and statistically significant effects can also be found for the coefficient of ENVPROBLEMS. A one level increase in disagreement reduces donations by more than 1.50 Euros. People concerned with the environment in their daily lives, donate smaller amounts. The effect of regular donations on donations in the game is rather small: as one would expect participants who typically donate also donate somewhat more in our experiment. We have also controlled whether subjects are affected by their knowledge of DBU, the environmental foundation the money was donated to. The effect of knowing DBU is very small in model (2).

## 6.4. Discussion

Earlier in this paper, we have formulated two hypotheses. First, we were interested in testing whether making a “positive experience” from being paired with a cooperative player, raises donations in the second phase of our experiment (**H1**). Second, we expected that a “negative experience” would decrease donations when compared to a control group (**H2**). The results just presented tend to support **H1**, but reject **H2**. Playing the game has a positive net effect, regardless of the absence of external intervention to steer learning. The effect is larger when subjects are paired with a cooperative partner. These results also hold when we control for observed socio-economic heterogeneity using regression analysis. The effect is rather small and statistically not significant, however. Socio-economic heterogeneity is more important in explaining donation behaviour in our game. Especially the young, wealthy, and those interested in environmental issues, donate substantially higher amounts, and large proportions of the observed variation can be explained by personal characteristics.

It is interesting that people who care about environmental issues in their daily lives donate less in the game. One may interpret this as the possibility to substitute monetary donations for pro-environmental behaviour in daily decision-making. Another interpretation would be that participants are aware of the crowding-out problem of intrinsic motivation (Cardenas et al. 2000; Gneezy and Rustichini 2000; Volland 2008). Conceptual work differentiates at least four types of environmentally significant behaviour (Stern 2000), including activism, non-activist behaviour in the public sphere, private-sphere environmentalism, and other environmentally significant behaviours. It will be important to distinguish these in future research. This would also mean to pay greater attention to inter-relationships of the various dimensions. Ultimately, this points towards a drawback of our approach. In our experiment, we observe only a small fraction of the large spectrum of possible pro-environmental actions – a fairly simple donation decision or “non-activist behaviour in the public sphere” as (Stern 2000) would put it. It would, of course, be overly optimistic to expect a lasting effect of participating in a small experiment on decision-making beyond the immediate context (cf. Huang et al. 2014 and Bernedo et al. 2014 for recent studies focusing on the duration of effects due to experimental manipulation). For investigating this question in greater detail, one would have to extend the experimental design substantially and observe participants’ behaviour over a longer period, as some scholars are already doing (Lopez 2008). Our intention however was to test short-term effects in the



absence of interventions other than game playing. The fairly large effect of income on donations could stem from a “warm glow effect” (Andreoni 1990), that is relatively cheap to buy for wealthier participants. In a replication, stake size as an additional factor that is manipulated as part of the experiment would help to study such effects.

A key difference of our game to the large-scale “water management games” currently conducted in the field (Meinzen-Dick 2013, Meinzen-Dick et al. 2014), is the level of interaction among participants. In our game, paired players, students and visitors, do not know each other and our outcome variable is limited to the amount donated to an environmental foundation. In practice, people gaming with each other may also interact in resource management. Gaming effects on water use may have lasting consequences for livelihoods and eco-systems in the actual world. One should, thus, not easily conclude that games as ours can contribute to an improved understanding of conflicts in all contexts and at all times.

More importantly, the “do no harm principle” of experimental ethics should be carefully evaluated in any field setting. If experimental research is directed at politically and economically vulnerable subjects, it is ethically advisable to start with a less vulnerable group in order to explore treatment effects and unforeseen harm (cf. Teele 2014). This is what we have tried to do in this paper. Although we find some support for a positive effect of game participation even if this experience is negative, we cannot rule out that repeated negative interaction undermines pro-environmental behaviour in actual field settings. We can also not rule out the interaction of assigned treatment with observed or unobserved socio-demographic heterogeneity. If we would find, for instance, that in spite of an *average* positive effect, specific sub-groups are harmed by particular treatments, such groups could be excluded from participation or at least from random assignment to treatment. Future research should pay more attention to identify such heterogeneity in treatment effects.

## **6.5. Summary and Conclusions**

Framed field experiments have become a common method to study behaviour in common pool resource dilemmas in specific field contexts. As experimentalists we often hear and experience that, beyond generating interesting scientific insights, the method has a lasting positive effect on participants’ understanding of resource dilemmas encountered in the field. In this paper, we have developed a field experiment to investigate how participation in a simple two person prisoner’s dilemma experiment affects subsequent donations to an

environmental foundation. We find that donations increase when participants play the game. This effect is larger for those who have a positive cooperation experience. However, total effects are relatively small, and socio-demographic heterogeneity explains a much greater proportion of variation in donations.

In future research, our approach could be extended to other samples and contexts. Repeated gaming and questionnaire data on environmental behaviour could yield further interesting insights. The topic could also be explored for environmental education more generally. Ultimately, in field contexts, one would also have to study how interaction in the game affects interaction in the field. In other words, the group interacting in the game sees each other again in actual life, and will have to “deal with” what happened in the game. Many of us can cite personal examples where – after playing a card or parlor game – friend- and relationships were put to a hard test or even ended. People can become quite emotional in gaming – for the good and for the bad.

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## 7. SUMMARY AND CONCLUSIONS

### 7.1. Key results and contributions

Experiments are employed to construct a proper counterfactual for the study of causal effects in Economics, and the method has been used to investigate the impact of institutions and norms on behavior. More recently, experimental economists have tried to analyze processes of institutional change by endogenizing institutions. Participants in the experiment have a certain say in the design of the rules of the game. Different types of experiments are based on different definitions of institutions and institutional change. Aside from explicating the different viewpoints and links to empirical work, the dissertation aimed at the empirical study of behavior under different institutions, the interaction of institutions with contextual variables, and an identification of factors that are important in the experimental investigation of institutional change in field settings.

The **first paper** has laid the foundation for the dissertation by showing that different schools of thought in Institutional Economics find a representation in the empirical practice of Experimental Economics. These links are implicit, and experimental economists rarely refer to theoretical work on institutions and institutional change, even if the direct empirical focus is on such change. A key contribution of this dissertation is to highlight these links, emphasizing theoretical consistency in empirical work. Furthermore, the paper has also identified a number of issues faced in the study of institutional change. For instance, the role of constitutional rules – deeper level rules on how to change existing rules – requires more attention in empirical work. Additional factors have emerged from the empirical applications. From different perspectives, the **four empirical papers** of this dissertation have investigated how institutions interact with the payoffs rules of an experiment (Material Payoffs Game Layer), social variables (Group-Context Layer), and individual variables (Identity Layer).

The **second paper** and **third paper** investigated the impact of exogenous institutions on behavior. The **second paper** studied traffic policies and the impact of socio-economic backgrounds. It found that payoffs had a large impact on decision-making. Making the bus more attractive or making the car less attractive by modifying payoffs changed subjects' choices (Material Payoffs Game Layer). Furthermore, subjects frequently switched between the bus and the car, indicating that their decisions also depend on what others do (Group-Context Layer). In concordance with prospect theory (Kahneman & Tversky, 1979), at the individual level, framing the policy intervention as a loss has led to a larger

drop in the use of cars in the game than framing the intervention as a gain. Moreover, self-image of using a particular transportation mode and personal experience were important (Identify Layer).

The **third paper** expanded on this idea as it moved more explicitly to the Group-Context Layer. Heterogeneity in wealth was manipulated, and participants also knew whether or not wealth was distributed unequally in the game. In addition, institutions were modified. In one version of the game, players contributed in parallel; in another version, leading by example was introduced. It was found that there are small negative impacts of wealth heterogeneity and leading by example with strong heterogeneity in the effect of the latter. In a situation where leadership does not matter, real-life leaders do not behave differently from others. However, in a situation where setting a positive example is important, leaders contribute substantially more than those who are not used to this role. Significantly more than in the benchmark case, decisions of the inexperienced were guided by the history of play, namely what others contributed in the previous round.

In the **last two papers**, the perspective became more agent-centered, and for the most part, structural aspects were neglected. In the **fourth paper**, it was shown that status quo matters for individual decision-making. As a default, if landlords push tenants to use “no junk mail” stickers, these are used more. It was demonstrated that by means of a small “nudge,” deliberate decisions are encouraged, and the use of stickers can be increased.

In the **fifth paper**, a simple prisoner’s dilemma game was embedded into a field experiment. It was shown that experience in the game had a positive impact on the donation to an environmental foundation. In this way, the role of cognition and context was highlighted. At least in the short run, experience with the game – even if it was negative – had a positive effect on pro-environmental behavior. The contributions of the empirical papers are summarized in the table.

**Table 28:** Summary of empirical papers

<b>Paper Number</b>	<b>Material Payoffs Game Layer</b>	<b>Group-Context Layer</b>	<b>Identity Layer</b>
II	Different institutions effective to achieve behavioral change;	Dynamically changing behavior; people adjust and switch between car	Self-image as a car driver (upper middle class) or bus user



	improvements are possible by implementing demand side policies	and bus often; expectation by assisting in coordination helps	(lower middle class); real-life experience Loss aversion
III	Leadership treatment negative; strong interaction with social positions	Heterogeneity negative;	Leadership skills and experience strongly interact with leadership institution in the game
IV	Status quo rule matters; anecdotal evidence on	Perception of what is the social norm	Self-image with respect to environmental behavior
V	Main interest in altruism (other-regarding preferences) by means of a dictator game	Perception of what is the social norm	Self-image with respect to environmental behavior; learning and experience

Source: own design

The third guiding question of the dissertation sought to investigate how useful economic experiments are for the study of institutional *change* and which contextual factors must be considered. The framework of Cárdenas & Ostrom (2004) assumes that the rules of the game are static. It was shown that this must not necessarily be the case and that – at least in the long run – institutions change. Such institutional change is subject to a set of deeper-level rules:

All rules are nested in another set of rules that define how the first set of rules can be changed. [...] What can be done at a higher level will depend on the capabilities and limits of the rules at that level and at a deeper level. Whenever one addresses questions about *institutional change*, as contrasted to ongoing actions within institutional constraints, it is necessary to recognize that:

1. Changes in the rules used to order action at one level occur within a currently “fixed” set of rules at a deeper level.
2. Changes in deeper-level rules usually are more difficult and more costly to accomplish, thus increasing the stability of mutual expectations among individuals interacting according to the deeper set of rules. (Ostrom, 2005, p. 58; emphasis in the original)

These deeper level rules can be both at the agent and structural levels. For instance, in a parliament, a formally devised (constitutional) rule exists on how to change laws. In more informal settings, a shared norm or shared expectations may form the constitutional basis for altering an existing rule. In both instances, it is methodically challenging to address the quickly unfolding complexity that is involved in dynamic processes of institutional change. One way to deal with such complexity lies in the careful and transparent decomposition of the empirical setting at hand. Experiments then allow the singling out of particular aspects, holding other aspects constant.

The commonality of the papers in this dissertation is that they investigate a broader range of context-specific aspects in the study of institutions and behavior by examining the interaction of institutions with individual or social variables. Although institutions are not endogenous in any of the empirical papers in the sense that they can be changed by participants, the empirical papers can jointly contribute to an improved understanding not only of static institutional analysis but also of institutional change. In particular, each of the papers highlights a set of more specific aspects that could be considered in the study of institutional change. A research framework which synthesizes the findings of all five papers is presented on this basis.<sup>39</sup>

## **7.2. The dynamic view on institutions: A framework for conducting economic experiments**

In the development of empirically grounded economic experiments, researchers face decisions on how to translate on-the-ground realities into models. Ideally, the basis of these decisions should be carefully documented, and such documentation can build upon the existing culture of openness and transparency in the methodical community. The following

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<sup>39</sup> Parts of the sub-chapter are based on a paper that I am writing with Christian Kimmich, and a preliminary version has been presented at a conference (Rommel & Kimmich, 2014).

list of questions aims to assist empirically-working scholars. The first two questions are more fundamental as they generally address the role of deliberate human action in altering existing institutions. Subsequent questions specifically explore the variables to be considered when modeling institutional change:

(1) How do institutions change in a specific empirical context? Institutions may emerge and change spontaneously and are not necessarily the result of deliberate human action (cf. Vatn, 2005, Chapter 8; Kingston & Caballero, 2009). In such cases, there is no role for experimental subjects in the design or change of an institution. Furthermore, if the timeframe under investigation is short, decisions are made in a high frequency, and if institutions change only at slow pace, it may be reasonable to assume exogenous institutions.

(2) Who changes a particular institution? Is it the parties affected or are institutions imposed by someone who is an outsider? Perceived legitimacy of a new rule can differ substantially depending on the way it is implemented. In particular, exogenously imposed rules typically perform worse than endogenously chosen rules (cf. Dal Bó et al., 2010). If institutions are imposed on the affected parties, experiments may be useful to evaluate institutional performance of alternative institutional options as it has been done in much of the classic work in the field. Studied participants then do not have a role in institutional change. For instance, in the **second paper**, subjects are exposed to traffic policies that are devised in a political process in which the individual may have only a limited say. Allowing subjects to change the rules of the game in the context of traffic policy is perhaps an interesting exercise but usually will have limited empirical grounding. Similarly, in the **fourth paper**, landlords are the ones who decide on the status quo institution whereas tenants are the ones who make the choices.

(3) What are viable institutional alternatives? What are the alternatives considered by those who intend to develop a new rule or change an existing rule? Where do these alternatives come from, and how many are there? For instance, in the **third paper**, it was demonstrated that in a new situation and in the absence of a behavioral model, some participants rely on their experiences and strongly condition their behaviors on what others have done in the past.

(4) What are the costs and benefits of institutional change, who bears the costs, and who benefits? If a new institution is established, this may entail costs of changing existing rules.

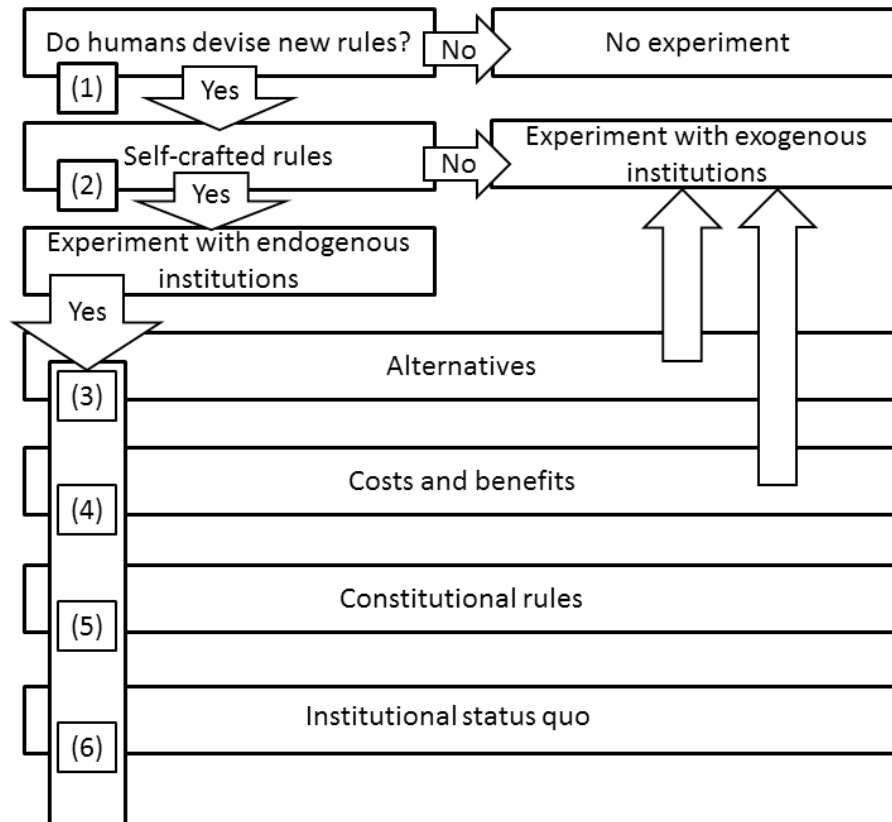
There may be initial costs for establishing a new institution; fixed and variable costs of running the new institution may also change. For instance, a new system may be needed to monitor and enforce a new rule. It is also important to know who has to bear how much of this cost and who may benefit from the new institution. In the **second paper**, the distributional consequences of institutional change became obvious. In the mode choice coordination problem, it was demonstrated that different demand side policy measures can have quite different outcomes in terms of the distribution of benefits. A bus price subsidy must be carried by the taxpayer to the benefit of those using the bus. In contrast, introducing parking costs creates revenues for everyone at the expense of those using the car.

(5) What is the mechanism of change? If only one person is involved in changing a rule, are there limits within which this person operates? Is this person known to everyone, and do reputational concerns play a role? If more than one person is involved, how is the decision reached? Is there open communication an anonymous vote or some other type of mechanism? Which majorities are needed? For instance, is a unanimous agreement needed or does the simple majority suffice to alter an existing institution? These questions all concern deeper level rules that are decisive for the change of existing rules. In some settings (e.g., in the legislative process in many parliaments), formal rules guide processes of legislative change. In more informal settings, such constitutional rules may also stem from culturally determined fairness norms. In the **third paper**, it has been demonstrated that people in their decision-making refer much more to what others have done when they have no experience with the task. Likewise, when a specific institution is at stake, in the absence of rules that guide the process of change, people may look for orientation by remembering or referring to situations that are similar in structure. They may also be influenced by their current mood or recent experiences as demonstrated in the **fifth paper**.

(6) What is the point of departure for institutional change? If a particular institution is already in place, the status quo is likely to determine people's perception of what is legitimate (cf. Kingston & Caballero, 2009). For instance, in the **fourth paper**, it is shown that the status quo rule affects perceptions of what is or what is not a socially accepted behavior. Under different status quo rules, not only behavior is likely to differ; also the willingness to accept new rules may be driven by the institutional status quo. Consequently, systems may evolve quite differently and may follow different paths,

depending on the institutional setup at the start. The following figure summarizes this framework:

**Figure 7:** A framework for developing economic experiments on institutional change (own design)



The figure displays how the formulated questions fit into the workflow of developing an economic experiment addressing institutional change. The first question is the most basic. If humans have no role in changing an institution, it is apparent that a behavioral experiment cannot help in answering the question of how an institution changes. If the institution under investigation is imposed upon the affected people, it might make sense to test their behaviors under different institutions. If rules are self-crafted, an experiment featuring endogenous institutions might be viable. As described above, various properties should then be regarded. The first two – alternatives and costs and benefits – are also relevant for experiments with exogenous institutions. Constitutional rules and status quo matter for experiments with endogenous institutional choice.

The framework developed here aims at the design phase of an empirical project. However, like the framework of Cárdenas and Ostrom (2004), it might also be used after the empirical part to structure and communicate results and decisions. Both frameworks might

even be combined, and questions (3) to (6) could be seen as an extension of the Material Payoffs Game Layer that is endogenous to considerations of the participant. Eventually, both frameworks complement each other, and their application has been demonstrated here.

### **7.3. Methodical limitations and future research**

Laboratory experiments in Economics are commonly criticized for being artificial. Even if some phenomena are well-established in the lab and have been replicated repeatedly, it is still difficult to accurately predict behavior outside the controlled setting. In the actual world, a great number of factors affect behavior, and for the experimenter, it is usually difficult to identify and control all of them. Furthermore, these factors unfold differently with different people in different contexts. Framed field experiments take up this critique by investigating more narrowly defined empirical questions and examining behavior of non-student subjects in specific field settings. Certainly, this is an improvement over the sole application of lab experiments, and moving back and forth between the lab and the field has been a productive way of knowledge generation in Economics (Harrison & List, 2004; Levitt & List, 2007; Levitt, List, & Reiley, 2010). Similarly, using non-student subject pools has been an important response to the critique of limited external validity (Henrich et al., 2004). However, framed field experiments also remain artificial, and they abstract from important aspects of a more complex reality.

Sometimes, it may be advisable to introduce specific aspects of such complexity into an experiment. For instance, Janssen et al. (2008) or Cárdenas, Janssen, & Bousquet (2013) are concerned with studying the interaction of institutions and behavior with non-linear eco-system dynamics. Kimmich (2013) points out that a farmer's decision whether or not to use energy-efficient equipment for an electric irrigation pump does not depend on cost-benefit considerations alone. The decision is linked to other farmers' decisions and embedded in village-level politics as well as the wider political economy of agricultural support. Much of the experimental work on endogenous institutions that has been reviewed in the dissertation (e.g., Sutter, Haigner, & Kocher, 2010) succeeds in accounting for at least some of this complexity in abstract laboratory experiments. However, much more remains to be done in this field. Ultimately, research boundaries must be defined and delineated, and the heuristic sketched in the previous chapter may help in doing so.

To address the issue of artificiality and external validity, experimental results must confront evidence from other methods, and combining multiple methods is more likely to produce the robust evidence needed for a better understanding of complex realities (Poteete, Janssen, & Ostrom, 2010; Prediger et al., 2010; Werthmann, 2011). With respect to static institutional analysis and the study of institutional change, qualitative methods will especially have to play an important role (Schlüter, 2010), and many aspects of institutions and their gradual change cannot be measured, or too little is known about how to measure them (Voigt, 2013; Rocco & Thurston, 2014).

The strand of empirical literature on the endogenous formation of institutions stems from the tradition of Experimental Economics that has always been much aware of economic theory. However, some of the more recent experimental applications are fairly limited in their theoretical underpinnings. Although mainstream economics has become increasingly aware of the role institutions play in the economic process both empirically (e.g., Acemoglu, Robinson, & Woren, 2012) and theoretically (e.g., North, 1990), Institutional Economics as a sub-field is still not well-established in the curricula. In the years to come, it will be important to bridge the gaps between theorists and empirically-working scholars. The framework developed above has made it apparent that constitutional rules and the institutional status quo are especially important fields for future research in Experimental Economics.

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## APPENDIX

**Appendix 1: Supplementary Material “Commuters’ Mode Choice as a Coordination Problem: A Framed Field Experiment on Traffic Policy in Hyderabad, India” (As published online by journal)**

# **Commuters' Mode Choice as a Coordination Problem: A Framed Field Experiment on Traffic Policy in Hyderabad, India**

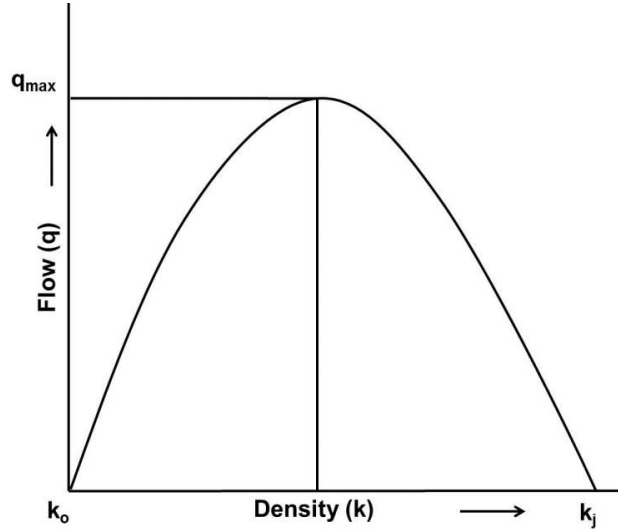
## **– Supplementary Material –**

- 1. Additional Background on Calculation of Payoffs**
- 2. Experimental Protocols and Instructions**
- 3. Payoff Table by Exercises**
- 4. Questionnaire**
- 5. Additional Graphs**
- 6. Additional Statistical Analysis**

## **1. Additional Background on Calculation of Payoffs**

The fundamental traffic flow parameters, vehicle speed, traffic flow (the number of vehicles expressed in terms of passenger car units passing a specified point during a stated period of time), and density, determine road capacity. Figure 8 displays the non-linear relationship between flow and density.

**Figure 8: Fundamental traffic flow diagram**



Source: own design

In the model traffic flow and increase until reaching a saturation point  $q_{\max}$ . After this point the flow decreases until  $k_j$ , the point where traffic breaks down completely. This relationship is captured by the equation:

$$f(Q) = V_f(\sum k_i) + \left[ \frac{V_f}{k_m} \right] (\sum k_i)^2 \quad (1)$$

where

$Q$	=	Traffic flow (PCUs <sup>40</sup> /hour)
$k_i$	=	Space required by vehicle $i$ measured in PCU
$\sum k_i$	=	Sum of space across all vehicles measured in PCU
$k_m$	=	Maximum road capacity in PCU
$V_f$	=	Vehicle speed under free flow conditions

A commuter's payoff in the game depends on the vehicle speed and its operating costs. Travel speed is dependent also on the choice of other players. A problem of interdependence and coordination emerges. In a particular round of the game, the payoff  $\pi$  – which we assume to include all costs and benefits of the particular mode choice – can be expressed as follows<sup>41</sup>

$$\pi = e - c \left[ \frac{k_i}{\sum k_i} \right] + \left[ \frac{k_i}{\sum k_i} \right] f(Q) \quad (2)$$

<sup>40</sup> When the traffic consists of different vehicle types, they are transformed into Passenger Car Units (PCU). For example, a private car could be calculated with 1PCU, motor bikes with 0.5 PCU, and a bus with 2 PCU.

<sup>41</sup> This equation stems from the standard common pool resource literature, assuming that road space is “appropriated.” See for example Walker, J. M., R. Gardner, and E. Ostrom (1990). Rent Dissipation in a Limited-Access Common-Pool Resource: Experimental Evidence. *Journal of Environmental Economics and Management* 19(3): 203–211.

Substituting Eq. (1) into Eq. (2) yields:

$$\pi = e - c \left[ \frac{k_i}{\sum k_i} \right] + \left[ \frac{k_i}{\sum k_i} \right] \left[ V_f (\sum k_i) + \left[ \frac{V_f}{k_m} \right] (\sum k_i)^2 \right] \quad (3)$$

where

$\pi$	=	Participant's payoff in a particular round in tokens
$e$	=	Participant's income/endowment of the round (set to 10 in our case)
$c$	=	Vehicle operating cost in units
$k_i$	=	Space required by vehicle $i$ measured in PCU
$\sum k_i$	=	Sum of space across all vehicles measured in PCU
$k_m$	=	Maximum road capacity in PCU
$V_f$	=	Vehicle speed under free flow conditions

The following table presents the parameter specifications used for arriving at the payoffs used in the game.

**Table 29:** Specifications of transport mode used in payoff equation

<i>Vehicle type</i>	<i>Free-flow speed (<math>V_f</math>)</i>	<i>PCU (<math>k_i</math>)</i>	<i>Vehicle operating cost (<math>c</math>)</i>
Private car	19	1	1
Public bus	10	3	6

Source: own design

Substituting the parameters from the table payoffs for private car  $\pi_c$  and payoffs for public bus  $\pi_b$  are calculated as follows

$$\pi_c = 10 - 1(1) + \left[ \frac{1}{\sum k_{i(c)}} \right] [19(\sum k_{i(c)}) - 4.75(\sum k_{i(c)})^2] \quad (4)$$

$$\pi_b = 10 - 3(6) + \left[ \frac{3}{\sum k_{i(b)}} \right] [10(\sum k_{i(b)}) - 2.75(\sum k_{i(b)})^2] \quad (5)$$

Calculated payoffs were then rounded (for simplification) to produce the payoff tables presented in section 3 of this document.



## **2. Experimental Protocols and Instructions**

## General Introduction

Welcome! My name is Bhuvanachithra Chidambaram and I am a researcher at Humboldt University in Berlin Germany. These are my research assistants Rajiv Reddy and Ravi Teja.

In the following one and a half hours, I will ask you to complete a series of written exercises in form of a small game. All the data gathered in these games will be treated with confidence and they will be used anonymously and solely for research.

After the game, you will be rewarded with earnings based on the decisions you and other people have made. Additionally, you will receive a show-up fee of 200 INR independent of your decisions in the game.

Before we begin, I would ask you to please turn off your mobile phones and other devices, so that there will be no disturbances during the experiment. Please also do not speak to other participants during the game. If you have a question, raise your hand and I will assist you.

In total, three exercises are distributed to you, one at a time. When you are handed an exercise, read the instructions carefully and complete the information that is requested. You may begin each exercise as soon as you have received it. Each exercise has six rounds. You will also receive an ID that will be used throughout the experiment.

*Each round, you will have to write your decision. Once you are done, please place your form closed on the desk. Then, I will collect the forms and note down your earnings based on your decision. After each round, the total number of cars and buses will be announced. When all six rounds are completed, please handover the sheets to me and I will distribute the next exercise.*

[DO FIRST, SECOND and THIRD EXERCISE]

We will now distribute a survey. It will take you approximately 10 minutes to complete the survey. Please answer all the questions. Meanwhile we will prepare your earnings. Once everyone has completed the survey and we have calculated your payment, we will call you to the front of the room, where you have signed in. We will pay each of you in cash and in private. Please gather your belongings and bring them with you. After you have received your payment, we would like to ask you to leave the room.

[DO SURVEY]

# Instructions Exercise 1

Your Participant Id is \_\_\_\_\_

*Note:*

Please do not talk with anyone while we are doing this exercise!

Please raise your hand if you have a question!

## ***Before you start playing, imagine the following situation:***

You are working in an office and every day you may go to your office by vehicle using the (city) roads and you got your own car. There is also a public bus leaving in front of your house going directly to your office. Accordingly, you can chose between public bus or your own car to travel to your office. Your car is faster than bus but if everyone takes cars then the road gets congested and so your car is not faster than the bus.

## ***Let's start the game!***

- You are one among six players in this exercise and this exercise consists of six rounds (i.e. each round is one month).
- You have two options: namely Car and Public Bus and you have earnings for both the options in each round which are given in the payoff table on page 2. In each round, your earnings also depend on the other people's choices.
- Now please make your choice in the table on Page 3 by writing either car or bus into column 4 of the table on page 3!
  - Once you are finished writing, place your form closed on the desk, as a signal to me. I will make a note of your decision and announce the total number of cars and buses on the road (column 5).
  - Then using the given payoff table, I will calculate the earnings and write them on your sheet, based on your decision (column 6).
  - Then the next round starts.

The above steps are repeated for 6 rounds in this exercise.

## Instructions Exercise 2

Your Participant Id is \_\_\_\_\_

*Note:*

Please do not talk with anyone while we are doing this exercise!

Please raise your hand if you have a question!

### ***Before you start playing, imagine the following situation:***

You are working in an office and every day you may go to your office by vehicle using the (city) roads and you got your own car. There is also a public bus leaving in front of your house going directly to your office. Accordingly, you can chose between public bus or your own car to travel to your office. Your car is faster than bus but if everyone takes cars then the road gets congested and so your car is not faster than the bus.

**The government has decided to subsidize the bus ticket which results in an increase of three tokens for taking the bus.**

### ***Let's start the game!***

- You are one among six players in this exercise and this exercise consists of six rounds (i.e. each round is one month).
- You have two options: namely Car and Public Bus and you have earnings for both the options in each round which are given in the payoff table on page 2. In each round, your earnings also depend on the other people's choices.
- **In this exercise, if you take Public Bus, you can add three points (+3) to your earnings as displayed in the following payoff table.**
- Now please make your choice in the table on Page 3 by writing either car or bus into column 4 of the table on page 3!
  - Once you are finished writing, place your form closed on the desk, as a signal to me. I will make a note of your decision and announce the total number of cars and buses on the road (column 5).
  - Then using the given payoff table, I will calculate the earnings and write them on your sheet, based on your decision (column 6).
  - Then the next round starts.

The above steps are repeated for 6 rounds in this exercise.

## Instructions Exercise 2

Your Participant Id is \_\_\_\_\_

*Note:*

Please do not talk with anyone while we are doing this exercise!

Please raise your hand if you have a question!

### ***Before you start playing, imagine the following situation:***

You are working in an office and every day you may go to your office by vehicle using the (city) roads and you got your own car. There is also a public bus leaving in front of your house going directly to your office. Accordingly, you can chose between public bus or your own car to travel to your office. Your car is faster than bus but if everyone takes cars then the road gets congested and so your car is not faster than the bus.

**The government has decided to impose a parking cost of three additional tokens for people choosing the car.**

### ***Let's start the game!***

- You are one among six players in this exercise and this exercise consists of six rounds (i.e. each round is one month).
- You have two options: namely Car and Public Bus and you have earnings for both the options in each round which are given in the payoff table on page 2. In each round, your earnings also depend on the other people's choices.
- **In this exercise, if you take your car, you have to subtract three points (-3) from your earnings as displayed in the following payoff table.**
- Now please make your choice in the table on Page 3 by writing either car or bus into column 4 of the table on page 3!
  - Once you are finished writing, place your form closed on the desk, as a signal to me. I will make a note of your decision and announce the total number of cars and buses on the road (column 5).
  - Then using the given payoff table, I will calculate the earnings and write them on your sheet, based on your decision (column 6).
  - Then the next round starts.

The above steps are repeated for 6 rounds in this exercise.

## Instructions Exercise 3

Your Participant Id is \_\_\_\_\_

*Note:*

Please do not talk with anyone while we are doing this exercise!

Please raise your hand if you have a question!

### ***Before you start playing, imagine the following situation:***

You are working in an office and every day you may go to your office by vehicle using the (city) roads and you got your own car. There is also a public bus leaving in front of your house going directly to your office. Accordingly, you can chose between public bus or your own car to travel to your office. Your car is faster than bus but if everyone takes cars then the road gets congested and so your car is not faster than the bus.

**The government has decided to implement a car rotation system. This means that each participant will have the chance to take the car once in the next six rounds. Each round a player ID which is not known to the others will be announced and each ID will be announced exactly once. However, there is no enforcement or change in payoffs and you are free to choose either the bus or the car.**

### ***Let's start the game***

- You are one among six players in this exercise and this exercise consists of six rounds (i.e. each round is one month).
- You have two options: namely Car and Public Bus and you have earnings for both the options in each round which are given in the payoff table on page 2. In each round, your earnings also depend on the other people's choices.
- **At the beginning of each round one player ID will be announced and this player is allowed to take the car in the following round. Everybody will be chosen exactly once.**
- Once you are finished writing, place your form closed on the desk, as a signal to me. I will make a note of your decision and announce the total number of cars and buses on the road (column 5).
  - Then using the given payoff table, I will calculate the earnings and write them on your sheet, based on your decision (column 6).
  - Then the next round starts.

The above steps are repeated for 6 rounds in this exercise.

### **3. Payoff Tables by Exercises**

## Payoff-Table (Exercise 1)

Out of all six players		Your Earnings / Cost in each round (in points)			
Car (Nos.)	Bus (Nos.)	If you take 'car'		If you take 'bus'	
		Car (Numbers)	Car (Your points)	Bus (Numbers)	Bus (Your points)
0	6	0	---	6	15
1	5	1	23	5	14
2	4	2	18	4	13
3	3	3	14	3	12
4	2	4	9	2	9
5	1	5	4	1	0
6	0	6	0	0	---



## Payoff-Table (Exercise 2)

Out of all six players		Your Earnings / Cost in each round (in points)			
Car (Nos.)	Bus (Nos.)	If you take 'car'		If you take 'bus'	
		Car (Numbers)	Car (Your points)	Bus (Numbers)	Bus (Your points)
0	6	0	---	6	18
1	5	1	23	5	17
2	4	2	18	4	16
3	3	3	14	3	15
4	2	4	9	2	12
5	1	5	4	1	3
6	0	6	0	0	---

## Payoff-Table (Exercise 2)

Out of all six players		Your Earnings / Cost in each round (in points)			
Car (Nos.)	Bus (Nos.)	If you take 'car'		If you take 'bus'	
		Car (Numbers)	Car (Your points)	Bus (Numbers)	Bus (Your points)
0	6	0	---	6	15
1	5	1	20	5	14
2	4	2	15	4	13
3	3	3	11	3	12
4	2	4	6	2	9
5	1	5	1	1	0
6	0	6	-3	0	---

## Payoff-Table (Exercise 3)

Out of all six players		Your Earnings / Cost in each round (in points)			
Car (Nos.)	Bus (Nos.)	If you take 'car'		If you take 'bus'	
		Car (Numbers)	Car (Your points)	Bus (Numbers)	Bus (Your points)
0	6	0	---	6	15
1	5	1	23	5	14
2	4	2	18	4	13
3	3	3	14	3	12
4	2	4	9	2	9
5	1	5	4	1	0
6	0	6	0	0	---

#### **4. Questionnaire (Question 2 differed by treatments in the questionnaire)**

Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree	Not Applicable /Don't know
I chose the car in the exercise, because I could earn more money than choosing public bus.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I chose the car in the exercise, because in everyday life, I am used to take my own car.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I chose the public bus in the exercise, because I could earn more money than choosing the car.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I chose the public bus in the exercise because in everyday life, I am used to take the public bus.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I was considering the other players' earnings in my decision.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
My decisions were driven mainly by my self-interest.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
My decision in each round was affected by the results of the previous round.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>I had another motivation to choose the car, <i>please explain:</i></b>						

I had another motivation to choose the bus, *please explain*:

**Survey**

Player ID \_\_\_\_\_

- 1. How much do you agree or disagree with the following statements about your decisions in the first six rounds of the exercise?**

*Please indicate your selection by crossing a box in each row below (☒)*

- 2. How much do you agree or disagree with the following statement about the effect of the parking cost in rounds 7 – 12?**

*Please indicate your selection by crossing a box in each row below (☒)*

Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree
I opted for the bus because of the parking cost.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

- 2. How much do you agree or disagree with the following statement about the effect of the bus subsidy in rounds 7 – 12?**

*Please indicate your selection by crossing a box in each row below (☒)*

Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree
I opted for the bus because of the subsidy.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**3. How much do you agree or disagree with the following statements about the rotation in the rounds 13 – 18?**

Please indicate your selection by crossing a box in each row below (☒)

Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree
The rotation system changed my decision compared to rounds 1 – 6.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**4. The following table shows potential causes of the traffic jams in Hyderabad. What do you think is the impact of the listed factors on traffic jams?**

Please indicate your selection by crossing a box in each row below (☒)

Causes for traffic jam	Impact level to traffic jam				
	Very Low Impact	Low Impact	Medium Impact	High Impact	Very High Impact
Too many cars on the road	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Too many two-wheelers on the road	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Lack of public bus	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Narrow roads	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Parking cars on the road	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Improper driving behaviour	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Construction works on roads (widening roads, flyovers etc.,)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Poor traffic signals timing	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**5. Do you think if there are other potential causes of traffic jams in Hyderabad which we have missed? (Please explain briefly!)**

**6. How much do you agree or disagree with the following statements about the public bus system in Hyderabad?**

*Please indicate your selection by crossing a box in each row below (☒)*



Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree
The public buses in Hyderabad are very old.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The frequency of public buses in Hyderabad is too low.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The comfort of public buses in Hyderabad is too low.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The ticket prices for public buses in Hyderabad are too high.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The public buses in Hyderabad often arrive late.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The public buses in Hyderabad are often overcrowded.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The public buses in Hyderabad often do not stop.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The public buses are not well connected	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

throughout the city of Hyderabad	
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**7. How useful or useless do you think are the following traffic measures when implemented in Hyderabad for reducing traffic congestion?**

*Please indicate your selection by crossing a box in each row below (☒)*

<b>Traffic measures in Hyderabad</b>	<b>Very useful</b>	<b>Somewhat useful</b>	<b>Neither useful nor useless</b>	<b>Somewhat useless</b>	<b>Very useless</b>
High parking fee for private vehicles (motorised two-wheelers and cars)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Prohibiting private vehicles (motorised two-wheelers and cars) on road	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Odd-Even number plate car rotation system	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Constructing flyovers or road over bridges	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Widening existing roads	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Increasing bus frequency	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Reduced ticket fare for public bus	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Providing separate lane for Bus (or Bus Rapid Transit)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Increasing fuel (Petrol or Diesel) price	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
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**8. Do you think there are other traffic measures that could be useful for reducing the traffic congestion in Hyderabad? (Please explain briefly!)**

**9. Please specify your gender (Male/Female):** \_\_\_\_\_

**10. Please specify your age** \_\_\_\_\_

**11. What is your marital status? (Please circle!)**

- a. Single
- b. Married
- c. Other

**12. How many years of education do you have? \_\_\_\_\_ and what is your level of education? (Please circle!)**

a. Literate, Pls. circle your education level below:

Primary / Secondary / Higher secondary / Diploma / Graduate / Post graduate

b. If others, *pls. write here*

**13. What is your job status? (Please circle!)**

- a. Self-employed
- b. Employer
- c. Employee
- d. Seeking Job
- e. Student
- f. If others *pls. write here*

**14. What is your average monthly income? (Please circle!)**

- a. Below Rs.5000
- b. Rs. 6,000 – Rs. 15,000
- c. Rs. 15,000 – Rs.50,000
- d. Rs. 50,000 – Rs.1,00,000
- e. Above Rs.1,00,000 (>1 lakh)
- f. I do not know
- g. I do not want to answer

**15. Do you own any vehicle? (Please circle!)**

- a. Yes  
Pls. circle your vehicle type:  
  
Car / Motor-bike or Two-wheeler / Auto- rickshaw / Cycle.  
  
If others pls. specify \_\_\_\_\_
- b. No

**16. If “Yes” in question 15, then what is your average monthly vehicle operating cost (i.e. the fuel and maintenance cost)? (Please circle!)**

- a. Less than Rs. 50
- b. Rs. 50 - Rs. 100
- c. Rs.100 - Rs. 500
- d. Rs. 500 - Rs. 1,000
- e. Rs. 1,000 - Rs. 5,000
- f. Rs. 5,000 - Rs.10,000
- g. Above Rs. 10,000
- h. I do not know

**17. How often do you use the following means of transportation?**

*Please indicate your selection by crossing a box in each row below (☒)*

	<b>Never</b>	<b>(1 to 10 times per month)</b>	<b>(11 to 20 times per month)</b>	<b>(more than 20 times per month)</b>
Walking	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Car	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Public Bus	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Motor-bike	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Auto-rickshaw	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Cycle	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>If others, please write here:</b>				

**18. If you use public bus, then what is your average monthly expenditure for the travel cost by public bus? (Please circle!)**

- a. Less than Rs.50
- b. Rs.50 - Rs.100
- c. Rs.100 - Rs. 500
- d. Rs. 500 - Rs. 1,000
- e. Rs. 1,000 - Rs. 5,000
- f. Above Rs. 5,000
- g. I do not know

**19. How often do you get stuck in traffic jam in Hyderabad?**

*Please indicate your selection by crossing a box in each row below (☒)*

Always	Often	Sometimes	Rare	Never
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**20. Your household details in Hyderabad:**

*Please indicate your selection by crossing a box in each row below (☒)*

Household details	0	1	2	3	>3
How many people are there in your household in Hyderabad (including you)?		<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
How many cars are there in your household in Hyderabad (including your vehicle)?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

How many motor-bikes are there in your household in Hyderabad (including your vehicle)?	<input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/>
<b>If there are other vehicle types in your household, <i>please write here:</i></b>	

**21. How much do you agree or disagree with the following statements about this exercise?**

*Please indicate your selection by crossing a box in each row below (☒)*

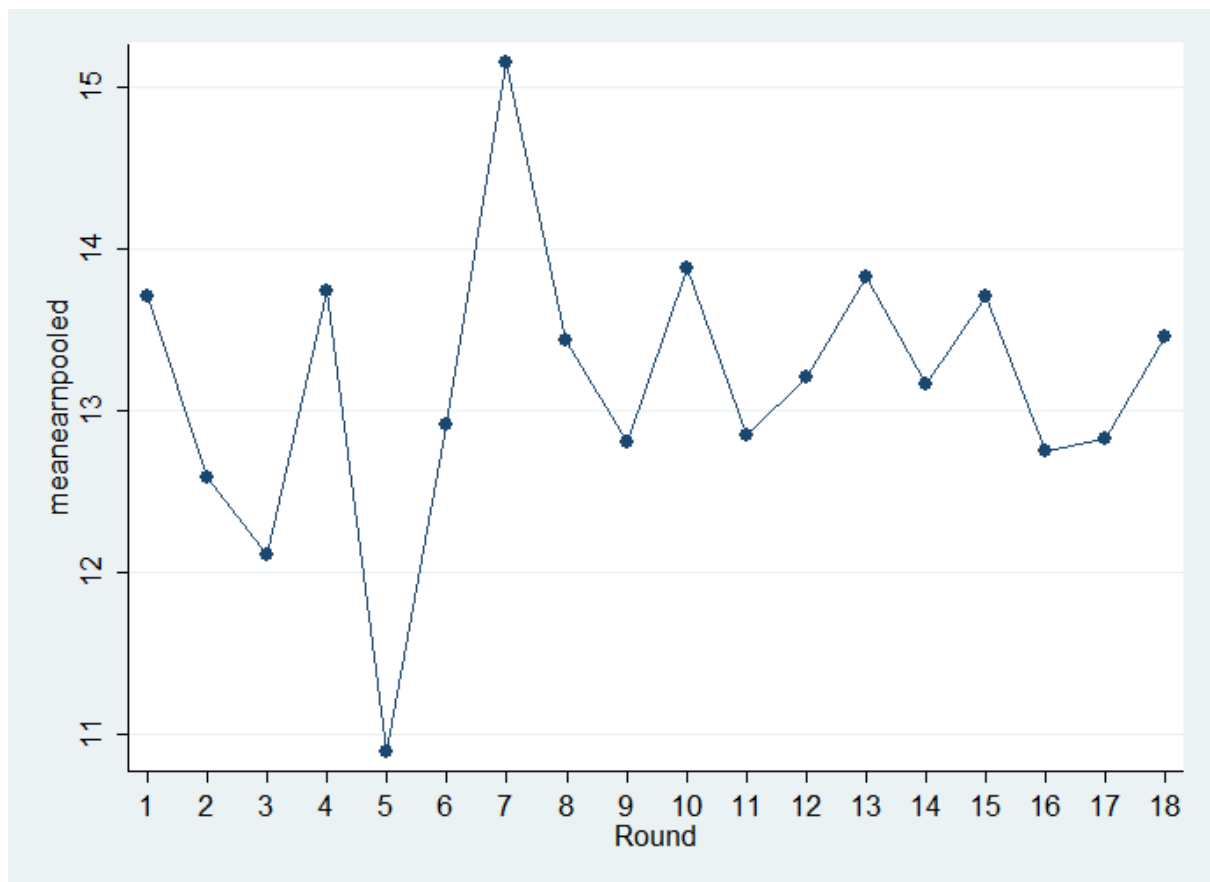
Statement	Fully agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Fully disagree
I could understand the instructions of exercise 1.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I could understand the instructions of exercise 2.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
I could understand the instructions of exercise 3.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>Do you want to tell us anything else about the game? <i>Please write here:</i></b>					

***Thank you very much for participating!***

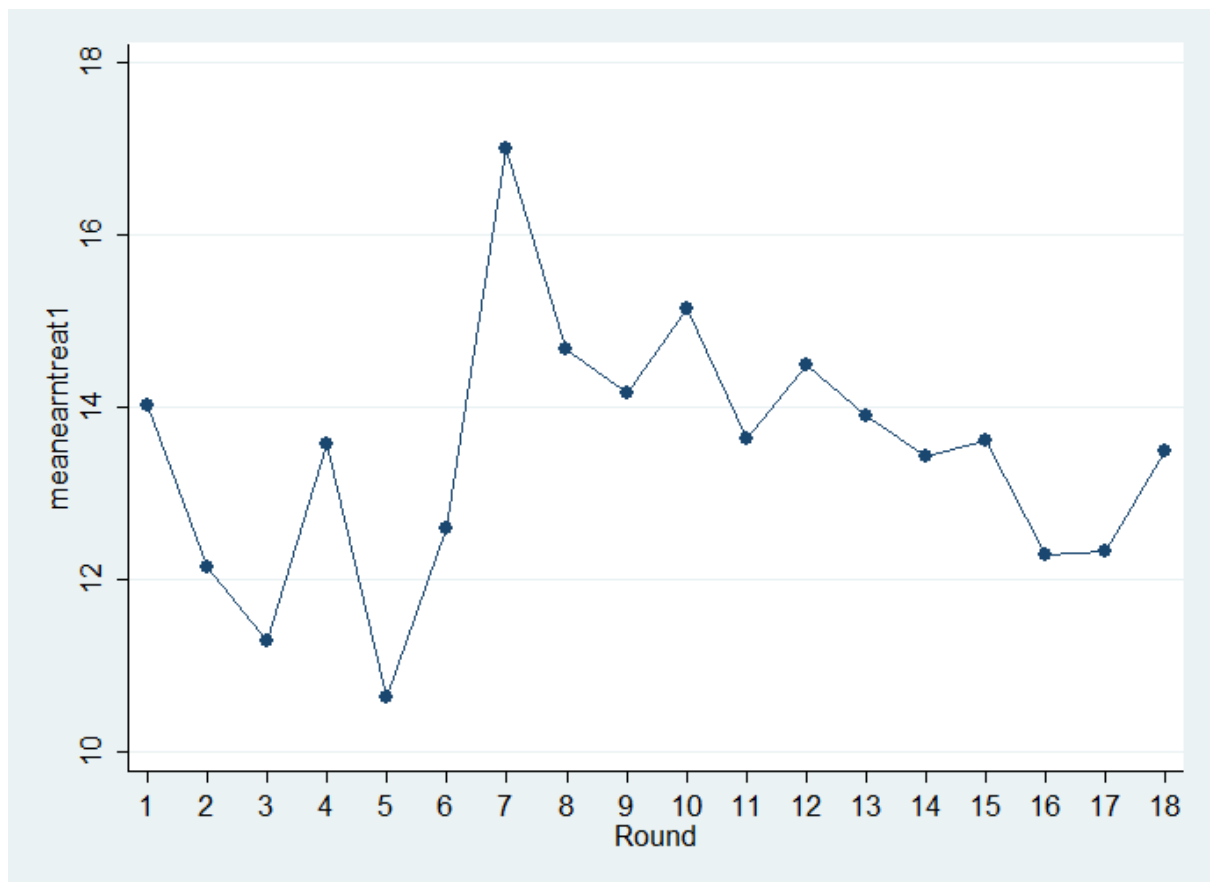
## **5. Additional Graphs**



**Figure 9:** Mean individual earnings by round; within subject treatments are introduced in rounds 7 and 13 (full sample)

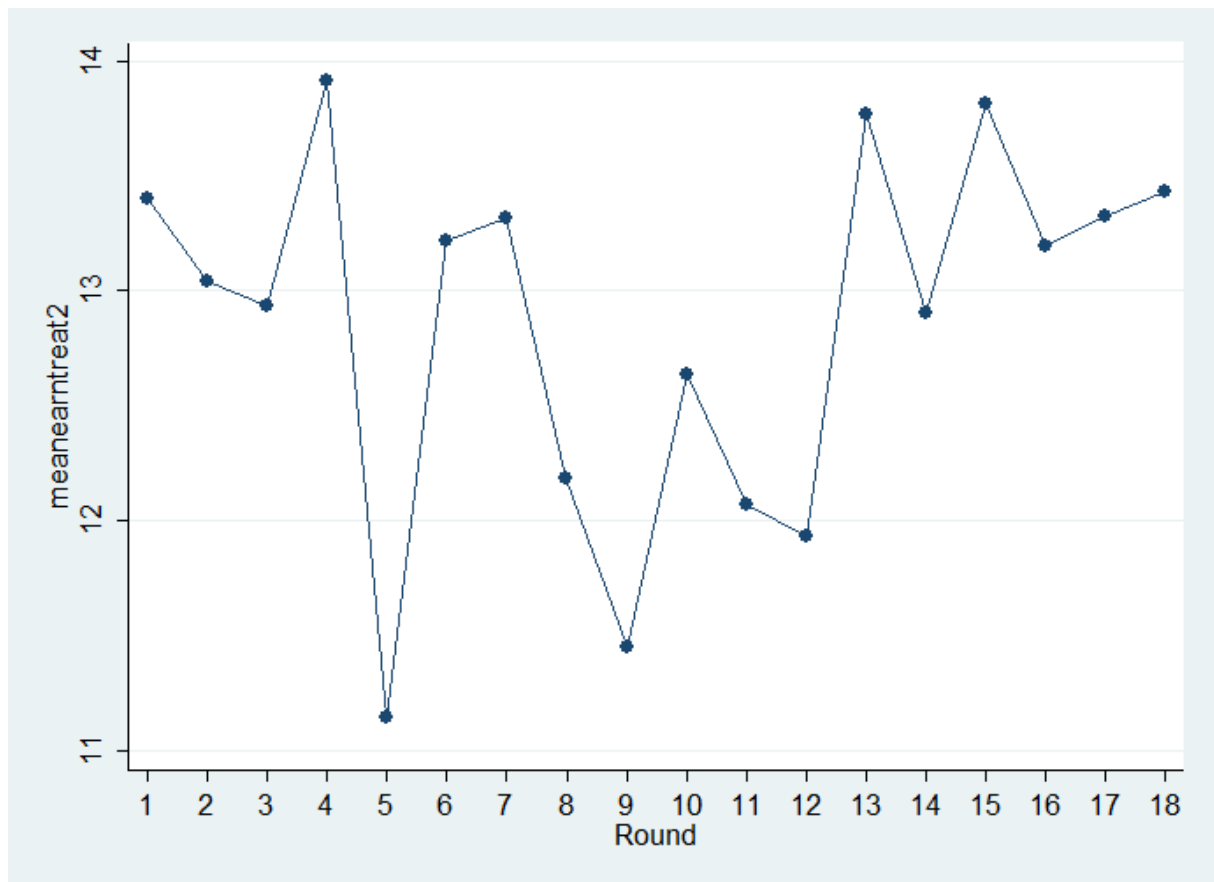


**Figure 10:** Mean individual earnings by round for treatment 1 (= bus subsidy) (half the sample)



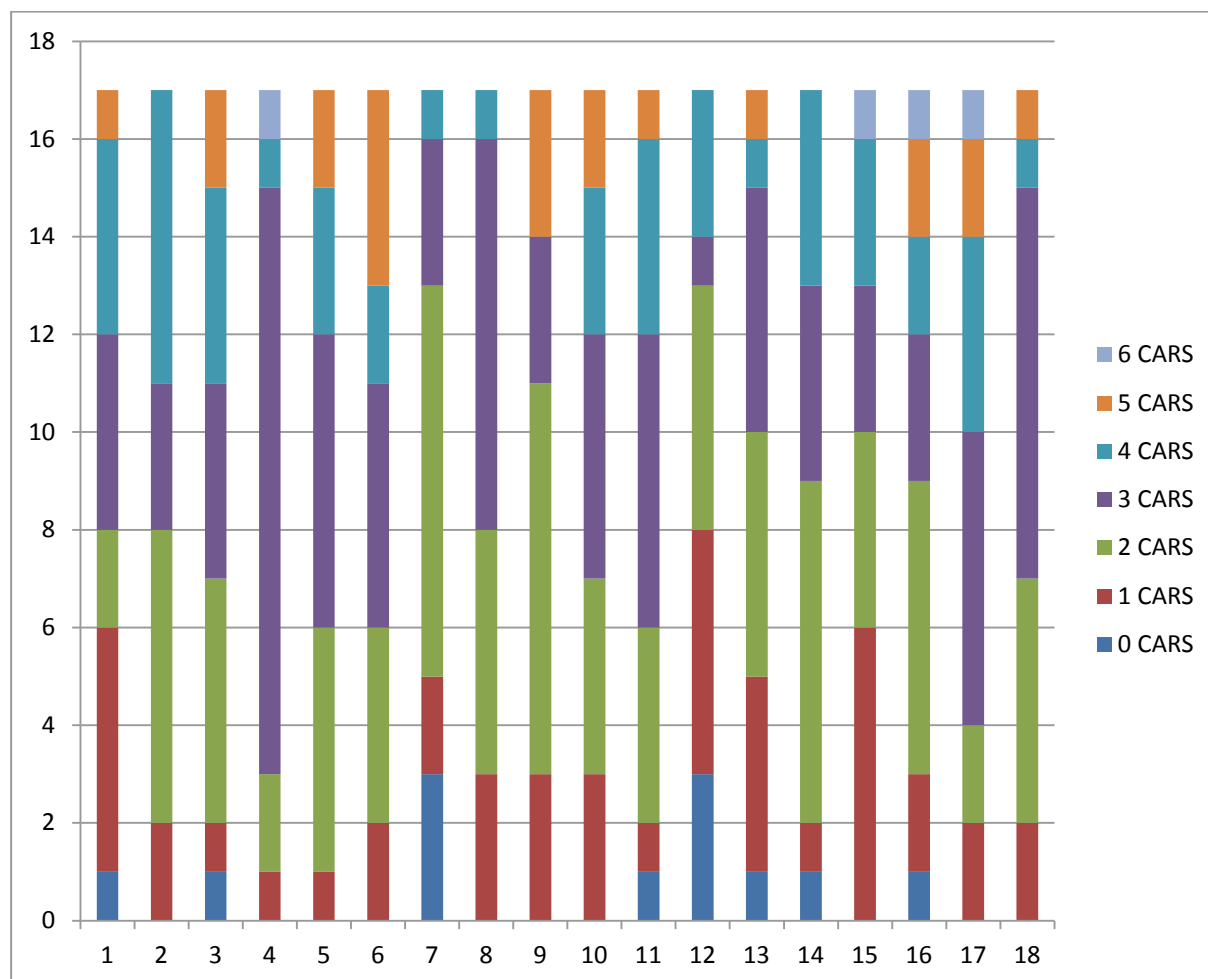
Earnings are higher because players taking the bus receive 3 Rs more in rounds 7 to 12.

**Figure 11:** Mean individual earnings by round for treatment 2 (=parking cost) (half the sample)

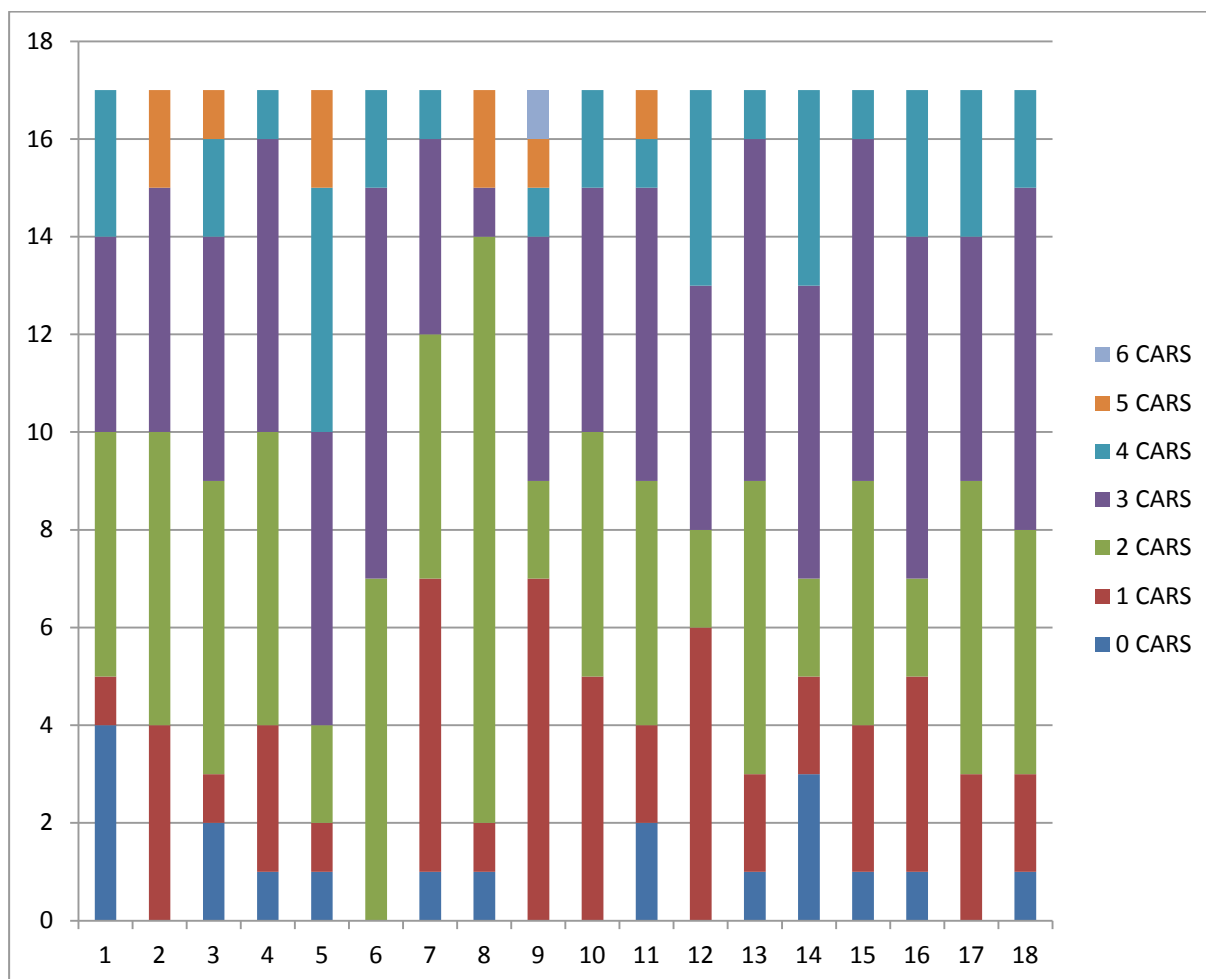


Earnings are lower because players taking the car receive 3 Rs less in rounds 7 to 12.

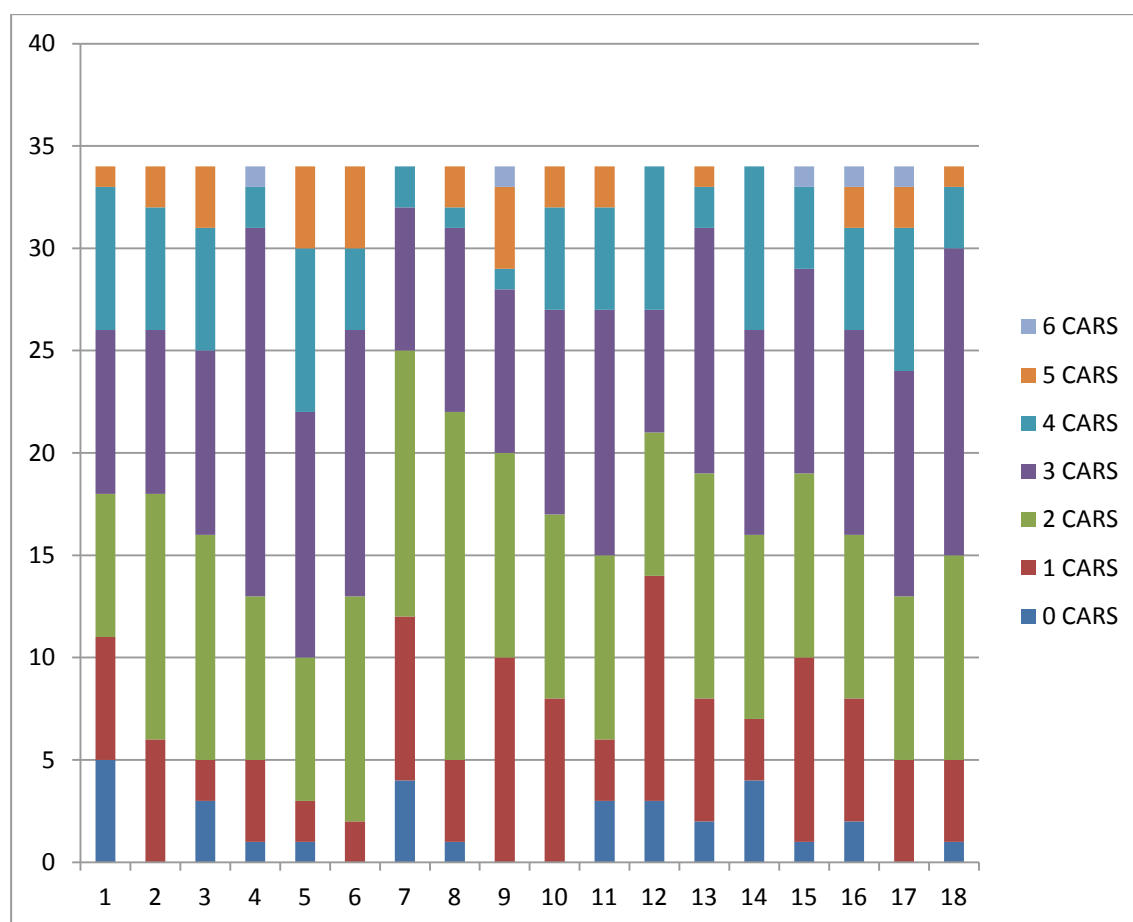
**Figure 12:** Absolute Frequencies of Group Outcomes by Rounds in the Bus Subsidy Treatment



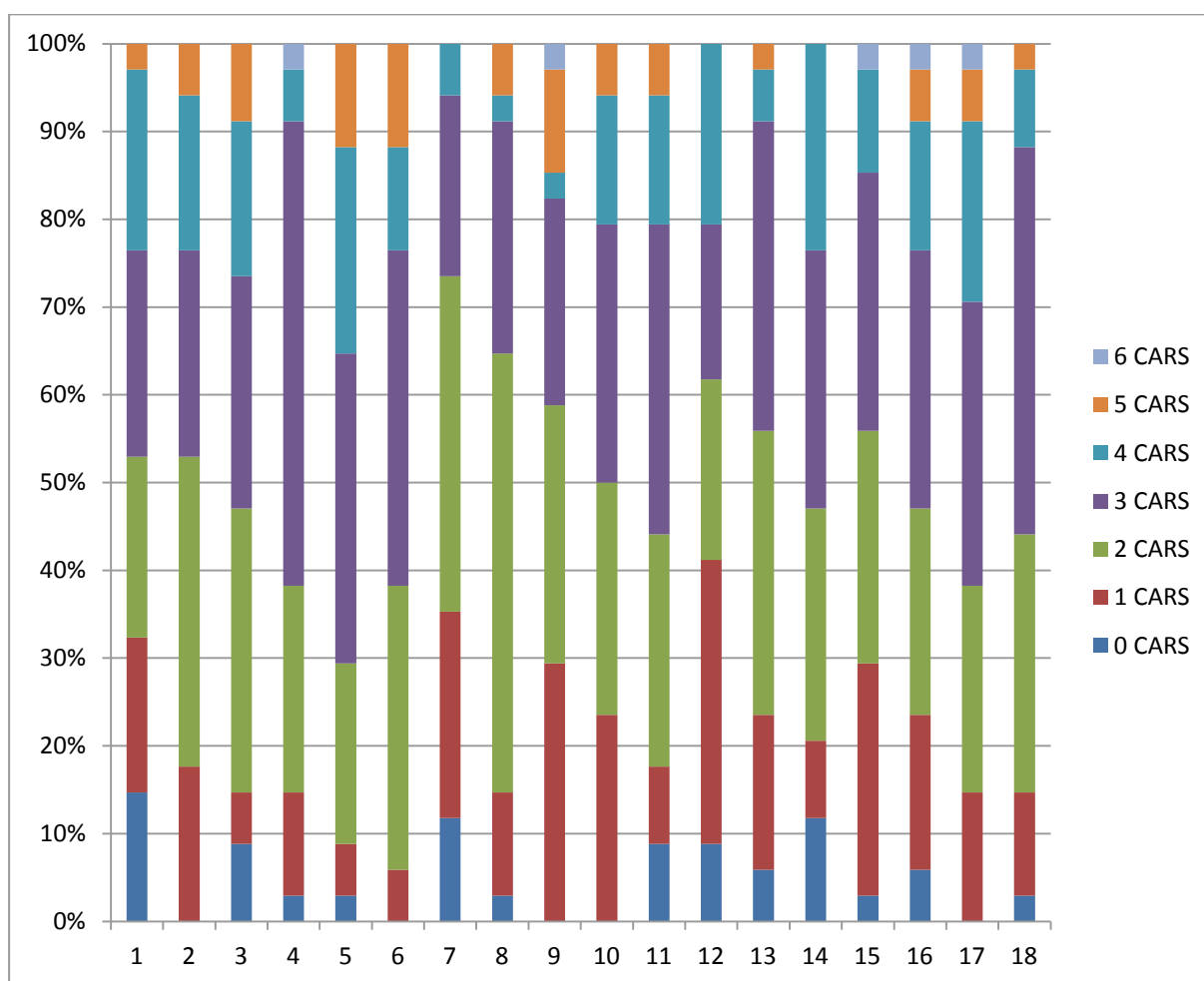
**Figure 13:** Absolute Frequencies of Group Outcomes by Rounds in the Parking Cost Treatment



**Figure 14:** Absolute Frequencies of Group Outcomes by Rounds in the Sample



**Figure 15:** Relative Frequencies of Group Outcomes by Rounds in the Sample



## **6. Additional Statistical Analysis**



**Table 30:** Linear Probability Models and Variance Inflation Factors

	(1)		(2)		(3)	
	OLS LPM	VIF	OLS LPM	VIF	OLS LPM	VIF
MALE	-0.0653 (0.0397)	1.22	-0.0653 (0.0397)	1.22	-0.0641 (0.0397)	1.19
AGE	-0.0020 (0.0024)	2.26	-0.0020 (0.0024)	2.26	-0.0019 (0.0024)	2.26
MARRIED	0.0277 (0.0486)	2.02	0.0277 (0.0486)	2.02	0.0280 (0.0484)	2.02
INCOME2	-0.0933 (0.0831)	3.01	-0.0933 (0.0830)	3.01	-0.0924 (0.0826)	3.00
INCOME3	-0.0014 (0.0836)	4.02	-0.0014 (0.0836)	4.02	-0.0008 (0.0833)	4.01
INCOME4	-0.0327 (0.0905)	3.13	-0.0327 (0.0905)	3.13	-0.0324 (0.0903)	3.13
INCOME5	-0.0124 (0.1011)	1.56	-0.0124 (0.1011)	1.56	-0.0120 (0.1009)	1.56
OWNSCAR	-0.0123 (0.0658)	1.60	-0.0123 (0.0658)	1.60	-0.0128 (0.0655)	1.59
OWNSBIKE	0.0375 (0.0449)	1.43	0.0375 (0.0449)	1.43	0.0372 (0.0447)	1.43
CARFREQ2	-0.0092 (0.0470)	1.32	-0.0092 (0.0470)	1.32	-0.0091 (0.0469)	1.32
CARFREQ3	0.1062 (0.0806)	1.19	0.1062 (0.0806)	1.19	0.1067 (0.0805)	1.19
CARFREQ4	0.0723 (0.0707)	1.41	0.0723 (0.0707)	1.41	0.0733 (0.0707)	1.41
BUSFREQ2	-0.1422** (0.0573)	2.05	-0.1422** (0.0573)	2.05	-0.1420** (0.0573)	2.05
BUSFREQ3	-0.1097 (0.0720)	1.80	-0.1097 (0.0720)	1.80	-0.1094 (0.0719)	1.80
BUSFREQ4	-0.1616*** (0.0595)	2.22	-0.1616*** (0.0595)	2.22	-0.1612*** (0.0595)	2.22
PARKCOST	-0.1454*** (0.0382)	3.06	-0.0449 (0.0288)	1.28		
BUSSUB	-0.1565*** (0.0404)	2.91	-0.0560* (0.0306)	1.27		
PUBCOORD	-0.2342*** (0.0550)	12.30	-0.0331 (0.0205)	1.33		
ROUND	0.0168*** (0.0042)	9.23			-0.0006 (0.0016)	1.00
Constant	0.5873*** (0.1014)		0.6460*** (0.1008)		0.6221*** (0.1003)	
<i>N</i>	2988		2988		2988	
Adjusted R <sup>2</sup>	0.0297		0.0266		0.0254	

Standard errors (clustered for participants) in parentheses

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

Table 30 presents three different specifications of a linear probability model on mode choice using the OLS estimator. Next to each column of coefficients we have added the variance

inflation factors (VIFs) for the variables by models. It can be seen that including both the round and the treatment dummy variables causes multicollinearity as indicated by the high VIFs for the PUBCOORD and ROUND variables in the first estimated model.

**Table 31:** Additional Specifications of Binary Logistic Regression

	(1) Full	(2) No income	(3) No CARFREQ	(4) No BUSFREQ	(5) No treatment dummies
MALE	-0.1984** (0.1000)	-0.2175** (0.0961)	-0.2028** (0.0995)	-0.1520 (0.0977)	-0.1912* (0.0985)
AGE	-0.0084 (0.0065)	-0.0094 (0.0061)	-0.0067 (0.0063)	-0.0097 (0.0064)	-0.0083 (0.0065)
MARRIED	0.1912 (0.1207)	0.1978* (0.1199)	0.1880 (0.1181)	0.2024* (0.1202)	0.1914 (0.1204)
INCOME2	-0.4412** (0.1768)		-0.4241** (0.1753)	-0.4251** (0.1759)	-0.4362** (0.1760)
INCOME3	-0.0556 (0.1678)		-0.0955 (0.1646)	-0.0078 (0.1670)	-0.0538 (0.1673)
INCOME4	-0.1328 (0.1980)		-0.1955 (0.1947)	-0.0492 (0.1963)	-0.1316 (0.1976)
INCOME5	-0.0020 (0.2595)		-0.0230 (0.2554)	0.1235 (0.2563)	0.0005 (0.2590)
OWNSCAR	0.0103 (0.1636)	0.1418 (0.1545)	-0.0039 (0.1507)	-0.0100 (0.1616)	0.0088 (0.1631)
OWNSBIKE	0.1062 (0.1012)	0.1518 (0.0967)	0.1046 (0.1009)	0.1494 (0.0972)	0.1061 (0.1010)
CARFREQ2	-0.0703 (0.1030)	-0.0624 (0.1014)		-0.0175 (0.1009)	-0.0701 (0.1028)
CARFREQ3	0.2617 (0.1840)	0.2146 (0.1810)		0.2637 (0.1785)	0.2621 (0.1834)
CARFREQ4	0.1338 (0.1707)	-0.0119 (0.1620)		0.3142* (0.1629)	0.1385 (0.1699)
BUSFREQ2	-0.4126*** (0.1283)	-0.4093*** (0.1264)	-0.3998*** (0.1247)		-0.4097*** (0.1280)
BUSFREQ3	-0.3562** (0.1497)	-0.3446** (0.1488)	-0.3370** (0.1451)		-0.3519** (0.1495)
BUSFREQ4	-0.5176*** (0.1335)	-0.5503*** (0.1317)	-0.5401*** (0.1285)		-0.5130*** (0.1332)
PARKCOST	-0.1557 (0.1254)	-0.1708 (0.1250)	-0.1476 (0.1252)	-0.1645 (0.1250)	
BUSSUB	-0.2062 (0.1295)	-0.1869 (0.1290)	-0.2141* (0.1292)	-0.1921 (0.1289)	
PUBCOORD	-0.3340*** (0.1087)	-0.3330*** (0.1084)	-0.3337*** (0.1086)	-0.3310*** (0.1083)	
CARPREVROUND	0.8210*** (0.0860)	0.8361*** (0.0858)	0.8305*** (0.0858)	0.8522*** (0.0855)	0.8223*** (0.0858)
ALLOWED	1.1425*** (0.1982)	1.1422*** (0.1980)	1.1427*** (0.1980)	1.1371*** (0.1977)	0.9684*** (0.1858)
Constant	0.1944 (0.2667)	0.0695 (0.2254)	0.1790 (0.2585)	-0.2749 (0.2386)	0.0193 (0.2576)
<i>N</i>	2490	2490	2490	2490	2490
Pseudo <i>R</i> <sup>2</sup>	0.058	0.054	0.056	0.053	0.055
Log likelihood	-1599.3429	-1605.3575	-1601.2620	-1607.3897	-1604.2007
$\chi^2$	195.5815***	183.5523***	191.7433***	179.4879***	185.8660***

Standard errors in parentheses

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

Table 31 shows four additional specifications of the logistic regression model from the paper. Using the same sample as in model 1, models 2, 3, 4, and 5 remove the sets of dummy variables for INCOME, CARFREQ, BUSFREQ, and the treatment dummies, respectively. This allows us to perform likelihood ratio tests to test for the increase in explanatory power of the respective nested model.

Adding INCOME dummies to the models significantly increases their explanatory power (LR test;  $\chi^2(4) = 12.03$ ;  $p = 0.0171$ ) on the five percent level. Adding CARFREQ does not lead to a significant increase (LR test;  $\chi^2(3) = 3.84$ ;  $p = 0.2795$ ). Adding BUSFREQ improves the model significantly (LR test;  $\chi^2(3) = 16.09$ ;  $p = 0.0011$ ). Likewise, adding the treatment dummies improves the model (LR test;  $\chi^2(3) = 9.72$ ;  $p = 0.0211$ ).

**Appendix 2: Protocols and Materials “Shared Toilets as a Collective  
Action Problem: A Framed Field Experiment on Sanitation in  
Hyderabad, India” (As used in the experiment)**

## **1. Instructions**

## **Sanitation Game Instructions and Specifications**

Number of participants: always recruit 20 people. Then, we will separate them randomly into four groups.

Start: Everyone gathers. Once everyone sits down, Bharath starts with welcoming participants and some background on the game:

1. Namaste! Welcome everybody!
2. Language: Does everybody understand Telugu? If not, which languages do you speak?  
Ensure translation into Hindi/Urdu.
3. Introduction:

We are a group of researchers from Germany interested in water and sanitation issues. We are also interested in collective action in the community more generally. Today, we would like to play a small game with you. It is only a game and we cannot promise you any action beyond this. We are researchers and we will write a study, we cannot come up with any practical action, and we cannot promise you any immediate help or money for your community.

All of today's data will be used in anonymous form and for research purposes only. **We will not share any information with government agencies or anyone else in your community.**

During the game, it is important that you **do not talk. Also do not use mobile phones!** Please **listen carefully when we explain the game!** If there are any questions/doubts please raise your hand and we will answer your question.

For participating in the game, **everyone will receive 100 Rupees.** All of you will get these 100 Rupees no matter what happens in the game. During the game **you can earn additional money** depending on your decisions in the game and the decisions of others. Please remember, **all your decisions are made in private and will not be known by the other players!** It is thus very important that you understand the rules of the game. After the game, each of you will be asked to fill a questionnaire to provide us with some background on you and your household. In total this will not take more than three hours of your time.

If you do not feel comfortable to play the game, you can leave now. Of course, you can also leave at any other time if you do not feel comfortable.

Did everybody understand this? Are there any doubts/questions so far?

**[Answer questions if any, else start explaining the game]**

4. Bharath explains the scenario and the game:

**Scenario:** Please picture the following scenario. You do not have a private toilet and rely on a public toilet shared by the community. The municipality or NGOs do not provide maintenance and cleaning for this toilet and you as a community have to organize these things yourselves.

To organize this collective task you would like to collect money. For doing so you put a plastic bottle next to the toilet where all members of your community can put money. You can organize the collection of this money in two different ways. For collecting this money you could either use transparent or opaque bottles.

**[Show both bottles]**

One bottle is opaque. You cannot see how much others have put. Contributions are anonymous.

**[Show opaque bottle and place money]**

The other bottle is transparent. In this case you can see how much others have contributed before you.

**[Show transparent bottle and place money]**

The game we will play works in a similar way. Sometimes you can see what others have contributed, sometimes you can't.

The game will be played in **groups of five people** and we will play the game for **eight rounds**. **In each round, each of you will get five, ten, or fifteen Rupees.** One Rupee in the game is also one Rupee of your payment afterwards. We have organized the game in a way that everyone will get a total of 80 Rupees over the course of all eight rounds. In each round you see how much money you have and you also know how much the others in your group have. You do not know who these people are however. After each round the results will be announced on a white board **[show white boards]**.

**[Distribute decision sheets and explain how to decide]**



In each round of the game, you have the possibility to put some or all money in the bottle or to keep all the money for yourself. The money which is put in the bottle will be used for the community. The benefit from this is exactly twice the amount you have contributed as a group. This benefit will be distributed evenly among all group members, independent of your individual contributions. You can also picture a scenario where an organization has promised to double all the funds you have collected.

In each round, all five players will make a decision using the provided sheets **[hold up sheets again]**. In some rounds, you make a first move. Then, one randomly selected person from your group is selected and the decision is announced **[show transparent bottle again; show sequential game sheets]**. For the player who has been selected to be the first mover, we will cross out the second column of the decision sheets. Please behave as if you would decide again to ensure no one will notice that you are the one selected **[demonstrate in group play]**.

Let's have some small examples **[use sheets and circles decisions for visualization]**:

Five participants:

**Example 1:** everybody has 10 Rupees. Everybody contributes 4 Rupees – in total 20 Rupees. This will be doubled which makes 40 Rupees in total. Everybody gets 8 Rupees and ends up with 14 **[show on white board]**.

**Example 2:** two participants have 5 Rupees, one has 10, and two have 15. The two with 5 put everything and nobody else puts anything. In total there are 10 Rupees in the bottle. This will be used for cleaning and the benefit is 20. Everybody gets 4 Rupees and what has remained from the endowments **[show on white board]**.

**Example 3:** Nobody puts anything in the bottle. Everybody keeps his starting money **[show on white board]**.

Did you understand everything? Any doubts/questions?

**[Answer questions if any, else continue]**

**Ask some participants test questions:** What happens if everybody has ten Rupees and everybody gives 4 Rupees? How much does everybody have in the end? What happens if everyone contributes everything?

**[Explain and show again the two different bottles. Show the two different types of sheets. Explain again how choices are made on paper by circling the amount. Show on sheets. Explain again that this has real monetary consequences. One Rupee in the game is one real Rupee afterwards. We will pay you after the game! Questions? Remind that people should not talk. Remind that all decisions are private and cannot be known by anybody else in the group.]**

5. Start the game: Bharath announces that now groups are formed and everyone draws a lot from the box. Bharath announced how everyone finds their group. Group leaders find their participants by ID code.
6. Participants get their ID codes (keep it!).
7. Participants go to their table and receive their folders.
8. Game starts, afterwards questionnaire and payment.

## **2. Protocol Sheets for Data Collection**

## Group Sheet Sanitation Game

(Please write readable!)

Session ID: \_\_\_\_\_

Date (DD/MM/YY): \_\_\_\_\_

Place: \_\_\_\_\_

Starting Time (Hour/Min): \_\_\_\_\_

Ending Time (Hour/Min): \_\_\_\_\_

Color Code (Circle!):    A   B   C   D   E

What is first (Circle!):            Parallel (everybody same time)            Sequential (First Mover)

Player IDs (Copy to the other two sheets):

	Player 1	Player 2	Player 3	Player 4	Player 5
Player ID					

Group Conductor (Your Name): \_\_\_\_\_

Assistant (Your Assistant's/Translator's Name): \_\_\_\_\_

Did some people talk/communicate during the game?<sup>42</sup>

☐ YES

☐ NO

Notes for special things that may have happened (Everything matters! Note down!):

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<sup>42</sup> Use back site for more information/details!

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Parallel Moves

Player Number	Player 1	Player 2	Player 3	Player 4	Player 5	Public Good (Sum of Contributions)	Public Good * 2 (Doubled Sum)	Each Player Gets (doubled divided by 5)
Player ID (write down!)								
Contributes in Round 1								
Keeps in Round 1								
Total in Round 1								
Contributes in Round 2								
Keeps in Round 2								
Total in Round 2								
Contributes in Round 3								
Keeps in Round 3								
Total in Round 3								
Contributes in Round 4								

Keeps in Round 4								
Total in Round 4								
<b>KEPT IN TOTAL</b>  <b>(40 – Sum (Contributions))</b>						<b>Sum all rounds</b>	<b>Sum all rounds x 2</b>	<b>Everybody gets in total from parallel</b>
<b>PAYOFF PARALLEL</b> <b>(Kept plus what everybody gets)</b>								

**Don't write in the shaded area!**

### Sequential Moves (First Mover)

Player Number	Player 1	Player 2	Player 3	Player 4	Player 5	ID of the First Mover	Public Good (Sum of Contributions)	Public Good * 2 (Doubled Sum)	Each Player Gets (doubled divided by 5)
Player ID (write down!)									
First Mover (Tick if yes!)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>				
Contributes in Round 1									
Keeps in Round 1									
Total in Round 1									
First Mover (Tick if yes!)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>				
Contributes in Round 2									
Keeps in Round 2									
Total in Round 2									
First Mover (Tick if yes!)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>				



Contributes in Round 3									
Keeps in Round 3									
Total in Round 3									
First Mover (Tick if yes!)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>				
Contributes in Round 4									
Keeps in Round 4									
Total in Round 4									
<b>KEPT IN TOTAL</b>  <b>(40 – Sum (Contributions))</b>							<b>Sum all rounds</b>	<b>Sum all rounds x 2</b>	<b>Everybody gets in total from sequential</b>
<b>PAYOFF PARALLEL</b> <b>(Kept plus what everybody gets)</b>									
<b>TOTAL PAYOFF</b>									

Don't write in the shaded area!

**3. Questionnaire**

## Post-Experimental Questionnaire Sanitation Game

(Please write readable!!)

### Panel Identification and Personal Details

1. Participant ID (Copy from sheet!): \_\_\_\_\_
2. Gender: Male ☐ Female ☐
3. Age: \_\_\_\_\_ years
4. Are you married? Yes ☐ No ☐
5. What is your current occupation? \_\_\_\_\_
6. Daily wage in INR: \_\_\_\_\_
7. Daily wage of spouse in INR: \_\_\_\_\_
8. Can you read and write? Yes ☐ No ☐
9. How many years did you spend in school? \_\_\_\_\_
10. Up to what level have you studied (degree)? \_\_\_\_\_
11. What is your religion? Hindu ☐ Muslim ☐ Christian ☐ Other: \_\_\_\_\_
12. What is your caste?  
Scheduled Tribe ☐ Scheduled Caste ☐ Other Backward Caste ☐ Minorities ☐ Not applicable ☐  
Other: \_\_\_\_\_
13. What is your mother tongue? Telugu ☐ Urdu ☐ Hindi ☐ Other: \_\_\_\_\_
14. How many years have you lived in this area? \_\_\_\_\_ years
15. Are you member of a Self-help Group (SHG)? Yes ☐ No ☐  
15.a. If yes, what is the name SHG? \_\_\_\_\_  
15.b. What is your role in the SHG? Member ☐ Leader ☐

### Household Details

<b>16. Please tell me, taking all people who live in your household (including men, women and children), how many members are there in your household?</b>  <i>(By household I mean all the people who usually live in this house and eat from the same kitchen as you do.)</i>			<b>Number</b>
	<b>a. Men (14 years old and above)</b>		
	<b>b. Women (14 years old and above)</b>		
	<b>c. Children (below 14 years)</b>	<b>Boys</b>	
<b>Girls</b>			

17. Are you the head of this household? Yes ☐ No ☐

17.c.

W

What is the occupation of the head of the household? \_\_\_\_\_

**18. In what kind of house do you live?**

**18.d.** Type of house: Pucca ☐ Kutchra ☐

**18.e.** Tenure: Own ☐ Rent ☐

**18.f.** Size of house in square yards: \_\_\_\_\_ square yards

**18.g.** How old is the house (in years)? \_\_\_\_\_ years

**19. Which of the following things do you own (tick whatever is owned by household!)?**

Colour-TV ☐ Mobile Phone ☐ Motorbike ☐ Electric fan ☐ Water filter ☐ Bicycle ☐

**20. What do you typically use for cooking meals (tick only one option!)?**

Biomass (Wood/dung/coal) ☐ Kerosine ☐ LPG ☐ Other: \_\_\_\_\_

**Sanitation and Water Situation**

---

**21. Where do you typically get your water from (tick only one option!)?**

Private water tap ☐ Private bore well ☐ Public bore well ☐ Water tanker delivery ☐

Other: \_\_\_\_\_

**22. What do you typically use for drinking (tick only one option!)?**

Untreated water ☐ Boiled water ☐ Water which has been filtered through cloth ☐

Purified with water filter ☐ Purchase drinking water ☐ Other: \_\_\_\_\_

**23. What do you typically use for sanitation (tick only one option!)?**

Private toilet ☐ Public pay toilet ☐ Community toilet ☐ No toilet/"open space" ☐

Other: \_\_\_\_\_

**23.h.** If you pay for the toilet, what are the monthly costs (per person)? \_\_\_\_\_ Rs. per month for one person

**23.i.** If you have private toilet, how much did it cost to construct it? \_\_\_\_\_ Rs.

**23.j.** If you have a private toilet, how old is it (years)? \_\_\_\_\_ years

**24. In the last four weeks, did you suffer from vomiting or loose motion?** Yes ☐ No ☐

**25. In the last four weeks, did anybody in your family/household suffer from vomiting or loose motion?** Yes ☐ No ☐

**Game Details**

---

**26. Would you like to play the game again?** Yes ☐ No ☐

**27. How easy or difficult was it for you to understand the game?**

Very easy ☐ Easy ☐ Neither easy nor difficult ☐ Difficult ☐ Very difficult ☐

**28. How much do you agree or disagree with the statement "In the game, those who have more money in the game should also contribute more to the common fund."?**

Strongly agree ☐ Agree ☐ Neither agree nor disagree ☐ Disagree ☐ Strongly disagree ☐

**29. How much do you agree or disagree with the statement "In the game, I followed the action of the first mover."?**

Strongly agree ☐ Agree ☐ Neither agree nor disagree ☐ Disagree ☐ Strongly disagree ☐

#### **4. Additional Explanation on Experimental Design and Summary Statistics of Order Effects**

## Implementation of Leadership Treatment

The leadership treatment was implemented anonymously. In a first step, every player had to decide on a contribution if randomly selected as a leader (cf. FIRST MOVE and SECOND MOVE columns on player sheets above). Then, all folders were handed in to field assistants. Hidden from participants, one folder was picked and the contribution of the player was noted down. The second column was marked by a large cross so that the respective person knew she was selected as a leader and did not have to make another move. All folders were handed back to participants and four out of five participants had to make their second choice. To avoid identification of the “leader,” first movers were also asked to write something on the player sheet.

## Additional Information on Design and Order Effects

The order of within-subject treatments was randomized using four variations of the sequence of play. In two versions, subjects started to play the first four rounds in the leadership/sequential game condition and then moved to the other condition in rounds 5 to 8 (Order 3 and Order 4 in **Table 32** below). In two other versions of the game, subjects started with parallel contributions (rounds 1 to 4) and then moved to the sequential game in rounds 5 to 8 (Order 1 and Order 2 in **Table 32** below).

In alternate rounds, subjects were either equally endowed (uneven rounds for Order 1 and Order 3, even rounds for Order 2 and Order 4) or endowment heterogeneity was introduced (even rounds for Order 1 and Order 3, uneven rounds for Order 2 and Order 4). In order to avoid confounding of sequence of play with communities, each order was prepared one time per session resulting in a total of four versions for twenty subjects. Subjects drew lots for groups and, thus, also for the order of play. Teams of group assistants that organized the session were also randomly assigned to one of the groups per session.

**Table 32:** The Four Possible Sequences of the Game

Treatment in Round								
	1	2	3	4	5	6	7	8
Order 1	Baseline	HetNoLead	Baseline	HetNoLead	HomoLead	HetLead	HomoLead	HetLead
Order 2	HetNoLead	Baseline	HetNoLead	Baseline	HetLead	HomoLead	HetLead	HomoLead

Order 3	HomoLead	HetLead	HomoLead	HetLead	Baseline	HetNoLead	Baseline	HetNoLead
Order 4	HetLead	HomoLead	HetLead	HomoLead	HetNoLead	Baseline	HetNoLead	Baseline

---

It can be seen that, by design, the number of observations per treatment was equal for each of the rounds. The only exception is the first session, in which only one order was played. In all sessions, each treatment was played two times by every player. **Table 33** displays contributions by treatments for all rounds and for round 1. It can be seen that, on average, contributions are lower in the first round – although not substantially in treatment 2 – and that the order of treatment effects – again with the exception of treatment 2 – is the same in round 1 and in all rounds.

**Table 33:** Contributions by Treatments

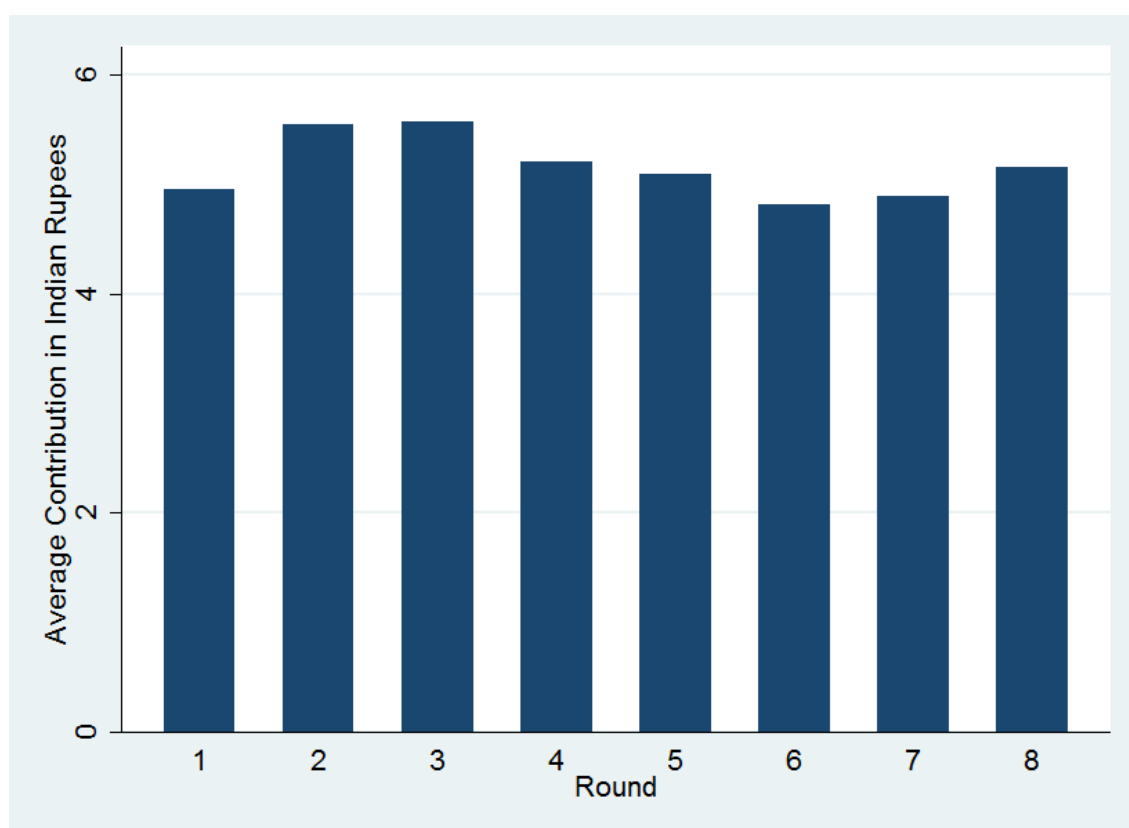
	Mean contribution in INR all rounds	Standard deviation all rounds	Mean contribution in INR round 1	Standard deviation round 1
Treatment 1 (Baseline)	5.458	2.855	4.84	2.608959
Treatment 2 (HetNoLead)	5.275	3.587	5.16	3.036994
Treatment 3 (HomoLead)	5.004	2.730	4.44	1.916594
Treatment 4 (HetLead)	4.863	3.311	4.2	2.614065

---

There seems to be no strong effect of sequence of play on contributions and average contributions are relatively stable over time (cf. Figure 16; note that the frequency of treatments is balanced in rounds).



**Figure 16:** Contributions by Rounds



**5. Statistical Model Selection**

**Table 34: Model Comparison**

	(1)	(2)	(3)	(4)	(5)
	Multi Level Model with session dummies (three levels)	Multi Level Model (three levels)	Multi Level Model (two levels)	Random Effects	Fixed Effects
HETNOLEAD	-0.2189 (0.2121)	-0.2205 (0.2123)	-0.2290 (0.2132)	-0.1982 (0.2548)	-0.2306 (0.2110)
HOMOLEAD	-0.5500*** (0.2126)	-0.5528*** (0.2128)	-0.5671*** (0.2137)	-0.5853** (0.2554)	-0.5493*** (0.2115)
HETLEAD	-0.6779*** (0.2135)	-0.6877*** (0.2136)	-0.7263*** (0.2144)	-0.8215*** (0.2559)	-0.6650*** (0.2124)
ENDOWMENT	0.3514*** (0.0241)	0.3514*** (0.0241)	0.3495*** (0.0242)	0.3679*** (0.0289)	0.3446*** (0.0240)
ROUND	-0.1039*** (0.0370)	-0.1047*** (0.0370)	-0.1071*** (0.0371)	-0.1221*** (0.0443)	-0.0996*** (0.0368)
OWN CONTRIBUTION T-1	-0.0098 (0.0314)	-0.0029 (0.0313)	0.0057 (0.0313)	0.3377*** (0.0305)	-0.1103*** (0.0327)
CONTRIBUTIONSOTHERS T-1	-0.0311* (0.0172)	-0.0234 (0.0169)	0.0097 (0.0160)	0.0558*** (0.0131)	-0.0304* (0.0179)
FEMALE	0.1602 (0.8224)	-0.0885 (0.7930)	-0.1990 (0.8311)	-0.0292 (0.3927)	
AGE	-0.0370 (0.0238)	-0.0402* (0.0231)	-0.0496** (0.0245)	-0.0280** (0.0117)	
MARRIED	-1.3605** (0.5959)	-1.4199** (0.5930)	-1.5762** (0.6527)	-1.0618*** (0.3099)	
DAILYWAGE	-0.0013 (0.0019)	-0.0014 (0.0019)	-0.0013 (0.0020)	-0.0009 (0.0010)	
WEALTH	-0.0070 (0.1389)	-0.0353 (0.1329)	-0.1141 (0.1369)	-0.0218 (0.0651)	
READANDWRITE	0.5088 (0.6144)	0.4345 (0.6073)	0.6075 (0.6538)	0.3108 (0.3092)	
YEARSINSCHOOL	-0.1548** (0.0753)	-0.1414* (0.0735)	-0.1364* (0.0763)	-0.0850** (0.0361)	
YEARSINAREA	-0.0263 (0.0330)	-0.0253 (0.0295)	-0.0131 (0.0275)	-0.0102 (0.0130)	
SHGMEMBER	-0.7910 (0.5123)	-0.8863* (0.4994)	-0.7549 (0.5225)	-0.5106** (0.2470)	
SHGLEADER	0.4114 (0.5632)	0.6146 (0.5476)	0.9198 (0.5791)	0.4659* (0.2757)	
HINDU	-0.4344	-0.6290	-0.8171	-0.3804	

	(0.5340)	(0.4980)	(0.5028)	(0.2397)	
SESSIONDUMMY1	1.7722 (1.2312)				
SESSIONDUMMY 2	1.4855 (1.1874)				
SESSIONDUMMY 3	0.3075 (1.1848)				
SESSIONDUMMY 4	0.4803 (1.1833)				
SESSIONDUMMY 5	0.9996 (1.2748)				
2NDROUNDTREAT	0.4214** (0.1685)	0.4221** (0.1686)	0.4310** (0.1693)	0.4200** (0.2022)	0.4274** (0.1676)
Constant	5.9365*** (1.8825)	7.0890*** (1.5209)	6.6615*** (1.5177)	2.0781** (0.8712)	3.4275*** (0.5153)
<hr/>					
lns1_1_1					
_cons	0.3258 (0.2492)	0.2249 (0.2473)	0.6357*** (0.0971)		
<hr/>					
lns2_1_1					
_cons	0.4509*** (0.1112)	0.4514*** (0.1108)			
<hr/>					
lnsig_e					
_cons	0.7488*** (0.0276)	0.7497*** (0.0277)	0.7536*** (0.0279)		
<hr/>					
<i>N</i>	798	798	798	798	798
<i>R</i> <sup>2</sup>					0.273
F					31.7783
AIC	3747.8779	3750.4386	3759.4756		3334.4105
BIC	3878.9769	3858.1271	3862.4820		3376.5495
$\chi^2$	261.8256	258.2344	261.2513	436.0738	
$\chi^2$ Comparison Model	119.7898	110.1240	99.0870		

Standard errors in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 34 displays five different specifications of the econometric model. The first two columns are multi-level models with decisions (level 3) nested in players (level 2) nested in groups of five subjects (level 1). The third column uses two levels with decisions (level 2) nested in players (level 3). Models (4) and (5) present estimates of a random and a fixed effects model for a dynamic balanced panel. The first round is dropped because lagged variables for  $t + 1$  are part of the independent variable list.

A Hausman test rejects the null hypothesis of equal coefficients ( $\chi^2 = 2897.90$ ;  $df = 7$ ;  $p = 0.0000$ ). Thus, the random effects model, with a very high likelihood, produces inconsistent estimates. Coefficients of the multilevel models are close to the consistent (but inefficient) fixed effects model.

If we compare model (2) against a linear OLS regression, the null hypothesis of a zero variance component of the player is rejected ( $\chi^2 = 99.09$ ;  $df = 1$ ;  $p = 0.0000$ ). If we compare the three-level model (2) with the two-level model (3), a likelihood ratio test rejects the null hypothesis that the variance component of groups is zero ( $\chi^2 = 11.04$ ;  $df = 1$ ;  $p = 0.0004$ ). The three-level model is, thus, preferred over the two-level model.

In addition we control for fixed effects of sessions by adding a set of dummy variables to model (1). A likelihood ratio test shows that the additional explanatory value is rather small ( $\chi^2 = 3.59$ ;  $df = 5$ ;  $p = 0.6095$ ). We, thus, decide for model (2).

**Appendix 3: Supplementary Material “Game Participation and Preservation of the Commons: An experimental approach” (As used in the experiment)**

## Original Instructions and Material for the Paper

### „Game Participation and Preservation of the Commons: An experimental approach”

1. Introduction
2. Rules of the Game/Instructions
3. Feedback on Outcome of Prisoner’s Dilemma Game
4. Instructions and Receipt Donation
5. Questionnaire

## 1. Introduction



**Herzlich willkommen zu unserer Befragung!**

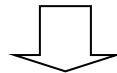
WissenschaftlerInnen setzen zu Forschungszwecken vermehrt ökonomische Experimente ein. Dabei werden in der Regel Entscheidungen getroffen, die Konsequenzen für eine tatsächliche monetäre Auszahlung haben. Wir möchten Ihnen heute Abend die Möglichkeit geben, an solch einem Spiel teilzunehmen.

**Insgesamt gibt es 45 Umschläge (=TeilnehmerInnen)!**

**Jede(r) TeilnehmerIn erhält unabhängig vom Ausgang eine Aufwandsentschädigung in Höhe von fünf Euro.**

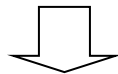
Sollte Ihnen dies unangenehm sein, haben Sie die Möglichkeit das gesamte Geld oder einen Teil des Geldes zu spenden. Natürlich können Sie das Geld auch behalten. Alle hier gewonnenen Daten werden nur anonymisiert und nur zu Forschungszwecken verwendet.

**Zufallsauswahl eines Umschlags**



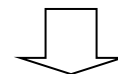
**29 Umschläge mit Spiel – Paarung mit Studierenden der HU**

**(insgesamt ca. 15 min)**



**16 Umschläge nur Befragung**

**(insgesamt ca. 8 min)**



Erklärung der Spielregeln	-
Spiel	-
Ergebnis des Spiels	-
Erhalt der Aufwandsentschädigung mit Möglichkeit der Spende	Erhalt der Aufwandsentschädigung mit Möglichkeit der Spende
Quittierung des Erhalts	Quittierung des Erhalts
Fragebogen (ca. 5 min)	Fragebogen (ca. 5 min)

## 2. Rules of the Game/Instructions

Herzlich willkommen bei der Langen Nacht der Wissenschaften,

vielen Dank für Ihre Bereitschaft an unserer Befragung teilzunehmen!

Wir wollen heute ein ökonomisches Spiel mit Ihnen durchführen, in dem Sie eine Entscheidung treffen müssen. **Die Höhe der Auszahlung in diesem Spiel hängt von Ihrer eigenen Entscheidung und der Entscheidung eines Mitspielers/einer Mitspielerin ab.** Die Kosten hierfür trägt die Universität.

Das Spiel wird insgesamt nicht mehr als 15 Minuten dauern. Zunächst erhalten Sie eine kleine Einführung in die Regeln. Alle Daten werden anonymisiert und nur zu Forschungszwecken verwendet. Niemand wird Ihre Entscheidung und Ihre Auszahlung Ihnen persönlich zuordnen können.

**Bitte lesen Sie sich die Regeln aufmerksam durch. Wenn Sie etwas nicht verstanden haben, so stehen wir Ihnen für Fragen jederzeit zur Verfügung.**

### Einführung und Regeln des heutigen Spiels

Sie erhalten einen Euro. Diesen Euro können Sie in ein gemeinsames Projekt mit Ihrem Mitspieler/Ihrer Mitspielerin investieren. Sie können das Geld auch behalten. **Ihr Mitspieler/Ihre Mitspielerin ist Studierender der Humboldt-Universität, der die Entscheidung für dieses Spiel in einer Vorlesung vor einigen Tagen getroffen hat und nächste Woche eine Auszahlung erhalten wird, die auch von Ihrer Entscheidung abhängt.** Sie werden zufällig mit einem dieser Studierenden gepaart. Ihre Auszahlung aus dem Spiel hängt somit sowohl von der Entscheidung des Studierenden, als auch von Ihrer eigenen Entscheidung ab. Sie erhalten Ihr Geld nach dem Spiel. Es gibt vier Möglichkeiten:

- Beide Spieler investieren in das gemeinsame Projekt.
- Nur Sie investieren in das gemeinsame Projekt.
- Nur Ihr Mitspieler investiert in das gemeinsame Projekt.
- Keiner der Spieler investiert in das gemeinsame Projekt.

Entsprechend Ihrer Entscheidung und der Entscheidung Ihres Mitspielers entstehen folgende Auszahlungen. **Das Geld erhalten Sie bar nach dem Spiel.**

		Entscheidung des Mitspielers	
		MitspielerIn investiert.	MitspielerIn investiert nicht.
Ihre Entscheidung	Ich investiere.	Jeder Spieler erhält 2 Euro.	Ich erhalte nichts, mein(e) MitspielerIn erhält 3 Euro.
	Ich investiere nicht.	Ich erhalte 3 Euro, mein(e) MitspielerIn erhält nichts.	Jeder Spieler erhält 1 Euro.

### Gibt es bis hierher Fragen?

Nun erklären wir Ihnen, wie Sie uns Ihre Entscheidung mitteilen. Durch Ankreuzen auf der nächsten Seite wählen Sie bitte **eine** der beiden Optionen (investieren/nicht investieren)!

Danach stecken Sie das Blatt bitte in den dafür vorgesehenen Umschlag. Ich werde den geschlossenen Umschlag dann einem Kollegen übergeben, der Sie nicht sieht **und Ihre Entscheidung nicht Ihnen persönlich zuordnen** kann. Dieser Kollege bereitet dann auch die Auszahlung vor, welche ich Ihnen in wenigen Minuten übergeben werde. So kann niemand Ihre persönliche Entscheidung nachvollziehen – sie ist **vollkommen anonym**.

Nachdem Sie den Ausgang des Spiels erfahren haben und ihre Auszahlung eingesteckt haben, erhalten Sie außerdem Ihre Aufwandsentschädigung mit der Möglichkeit der Spende. Hierzu erhalten Sie nach erfolgreichem Spiel eine gesonderte Anleitung.

In einem letzten Schritt möchten wir Sie bitten, einen kurzen Fragebogen auszufüllen.

Vielen Dank für Ihre Teilnahme!

## Entscheidung

Bitte entscheiden Sie sich, trennen Sie diesen Zettel ab und stecken Sie ihn in den dafür vorgesehenen braunen Umschlag!

Spieler ID: \_\_\_\_\_

Bitte wählen Sie **eine** der folgenden Möglichkeiten!

Ich investiere in das gemeinsame Projekt. ☐

Ich investiere nicht in das gemeinsame Projekt. ☐

Zur Erinnerung hier noch einmal die Auszahlungsmatrix:

		Entscheidung des Mitspielers	
		Mein Mitspieler investiert.	Mein Mitspieler investiert nicht.
Ihre Entscheidung	Ich investiere.	Jeder Spieler erhält 2 Euro.	Ich erhalte nichts, mein Mitspieler erhält 3 Euro.
	Ich investiere nicht.	Ich erhalte 3 Euro, mein Mitspieler erhält nichts.	Jeder Spieler erhält 1 Euro.

### 3. Feedback on Outcome of Prisoner's Dilemma Game

Das Ergebnis des Spiels:

**Ihre Entscheidung:**

Sie investieren ☐

Sie investieren nicht ☐

**Entscheidung des Studierenden:**

Investiert ☐

Investiert nicht ☐

		Entscheidung MitspielerIn			
		Mein(e) MitspielerIn investiert.		Mein(e) MitspielerIn investiert nicht.	
<b>Ihre Entscheidung</b>	<b>Ich investiere.</b>	Jeder Spieler erhält 2 Euro.	<input type="checkbox"/>	Ich erhalte nichts, mein(e) MitspielerIn erhält 3 Euro.	<input type="checkbox"/>
	<b>Ich investiere nicht.</b>	Ich erhalte 3 Euro, mein(e) MitspielerIn erhält nichts.	<input type="checkbox"/>	Jeder Spieler erhält 1 Euro.	<input type="checkbox"/>

Sie erhalten: \_\_\_\_\_ Euro

Der Studierende erhält: \_\_\_\_\_ Euro

## 4. Receipt Donation



Liebe Teilnehmerin, lieber Teilnehmer,

wie angekündigt erhalten Sie fünf Euro für die Teilnahme an unserer Befragung. Die fünf Euro finden Sie im beiliegenden Umschlag. **Sie haben die Möglichkeit, einen Teil des Geldes oder die gesamte Summe der „Deutschen Bundesstiftung Umwelt“ (DBU) zu spenden. Natürlich können Sie das Geld auch behalten.** Die Staffelung der Münzen gestattet es Ihnen, jeden beliebigen Betrag von 0,00 bis 5,00 Euro in 10-Cent-Schritten zu spenden.

Zur Information: Die DBU ist eine der größten Stiftungen in Europa. Sie fördert innovative beispielhafte Projekte zum Umweltschutz aus den Bereichen Umwelttechnik, Umweltforschung/Naturschutz und Umweltkommunikation.

Durch eine Spende an die DBU unterstützen Sie Projekte, von denen alle in der Gesellschaft profitieren können.

**Bitte nehmen Sie so viel Geld aus dem Umschlag wie Sie behalten möchten, und stecken anschließend den Umschlag in die dafür vorgesehene Box. Bitte stecken Sie Ihren Umschlag in jedem Fall in die Box, auch wenn der Umschlag leer ist. Eine Zuordnung Ihrer Spende zu Ihrem Namen erfolgt nicht.**

Wir möchten Sie außerdem bitten, uns den Erhalt der fünf Euro auf diesem Zettel zu quittieren. Dies dient allein der Abrechnung der bewilligten Mittel.

Vielen Dank für Ihre Teilnahme!

**Hiermit bestätige ich, dass ich fünf Euro für die Teilnahme an der Studie zum Spendenverhalten bei der „Langen Nacht der Wissenschaften“ an der Landwirtschaftlich-Gärtnerischen Fakultät erhalten habe.**

Vorname: \_\_\_\_\_

Familienname: \_\_\_\_\_

Berlin, den 8.6.2013

Unterschrift: \_\_\_\_\_

## 5. Questionnaire

## Fragebogen für TeilnehmerInnen

1. TeilnehmerInnennummer: \_\_\_\_\_

2. Ihr Geschlecht:

Männlich ☐ Weiblich ☐

3. Ihr Geburtsjahr: \_\_\_\_\_

4. Wieviele Personen leben derzeit dauerhaft in Ihrem Haushalt? \_\_\_\_\_

5. Über welches Nettoeinkommen verfügt Ihr Haushalt monatlich?

Weniger als 1.000 Euro ☐ 1.000 Euro bis weniger als 2.000 Euro ☐  
2.000 Euro bis weniger als 3.500 Euro ☐ 3.500 Euro und mehr ☐

6. Was ist ihr höchster erzielter Schulabschluss?

Kein Schulabschluss ☐ Haupt-/Realschule/Polytechnische ☐ Gymnasium/Gesamtschule/Erweiterte ☐  
Oberschule ☐ Oberschule ☐

7. Haben Sie eine Ausbildung oder ein Hochschulstudium (mit Abschluss) absolviert?

Hochschulstudium ☐ Berufsausbildung ☐ Weder noch ☐

8.

Wie sehr stimmen Sie folgenden Aussagen zu oder lehnen diese ab?

		Stimme voll zu	Stimme eher zu	Weder noch	Lehne eher ab	Lehne voll ab
I.	Ich interessiere mich sehr für Umweltfragen.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
II.	Ich achte bei meinen Alltagsentscheidungen auf mögliche Konsequenzen für die Umwelt.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
III.	Ich spende regelmäßig einen Teil meines Einkommens.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

9. Waren Sie mit der Arbeit der „Deutschen Bundesstiftung Umwelt“ vor der Teilnahme an dieser Befragung vertraut?

Ja ☐ Nein ☐

Wenn ja: Wie bewerten Sie die Arbeit der Deutschen Bundesstiftung Umwelt?

Sehr positiv ☐ Eher positiv ☐ Weder noch ☐ Eher negativ ☐ Sehr negativ ☐

**10. In welcher der folgenden Situationen erwarten Sie am ehesten, dass egoistisches Verhalten zu einem gesellschaftlichen Schaden führen kann (soziales Dilemma)?**

Angeln am eigenen  
Teich  
☐

Angeln in internationalen  
Gewässern  
☐

Angeln mit Angelschein am  
See  
☐

Weiß nicht  
☐

**11. Glauben Sie, dass die Teilnahme am Spiel Ihr Spendenverhalten beeinflusst hat?**

Ja, ich habe **mehr** gespendet, als ich es ohne das Spiel getan hätte. ☐

Ja, ich habe **weniger** gespendet, als ich es ohne das Spiel getan hätte. ☐

Nein, das Spiel hat mein Spendenverhalten **nicht** beeinflusst. ☐

**12. Wenn Sie nur die Höhe ihrer eigenen Auszahlung berücksichtigen, was wäre die beste Strategie im Spiel gewesen?**

Investieren  
☐

Nicht investieren  
☐

Abhängig von der Entscheidung  
des anderen Spielers  
☐

Weiß nicht  
☐