

Christian-Albrechts-Universität zu Kiel

**Investigations on the voluntary provision of  
local and global public goods**

Inaugural-Dissertation

zur Erlangung des akademischen Grades eines  
Doktors der Wirtschafts- und Sozialwissenschaften  
der Wirtschafts- und Sozialwissenschaftlichen Fakultät  
der Christian-Albrechts-Universität zu Kiel

vorgelegt von

Carlo Gallier, M. Sc.

aus Mannheim, Deutschland

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*To*

*Catherine & Linus*

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# 1 Introduction

Curbing climate change, limiting ozone depletion, combating air pollution, halting ocean degradation, reducing river and lake pollution - a list of various environmental challenges that could be continued at will. What all these diverse challenges have in common is the structure of a social dilemma. However, these dilemmas arise on different scales at the local and global level, which poses specific challenges. In five related but distinct essays, this thesis examines both global and local social dilemma situations in order to design institutional mechanisms that successfully address these challenges. After this introductory chapter, the thesis is structured along three parts and a schematic overview is provided in Table 1.1.

In a nutshell, Part I addresses aspects of international climate negotiations to testbed institutions that aim to foster cooperation among sovereign agents at the global level. Based on a laboratory experiment, Chapter 2 investigates whether groups of sovereign and heterogeneous agents are able to reach an agreement on how to share the cost of providing a public good. Relating to this first essay, Chapter 3 captures recent developments in international climate negotiations by considering pre-existing public good contribution levels and giving heterogeneous participants the opportunity to increase contributions to the public good beyond the pre-existing contribution level, maintain the current level, or even undo efforts. Part II forms the bridge to more localized public good problems. Chapter 4 takes into account that local and global public goods are not necessarily mutually exclusive and analyzes voluntary contributions to public goods at different spacial levels. To investigate whether individuals' narrow concerns for local outcomes can harm efficiency, the online experiment is based on participants recruited from two neighboring cities in Germany. Part III comprises results of two experiments focussing on more localized public good problems. Chapter 5 takes into account that local public good problems have authorities with permissions to intervene and regulate *de jure*, but *de facto* costly monitoring and sanctioning can preclude the emergence of formal and even informal regulations. Based on an artefactual field experiment in Indonesian fishing communities, it tests the effects of non-enforced extraction limits on extraction behavior in a common-pool resource game. Chapter 6 is based on a laboratory experiment to test whether democratic participation can be used to increase compliance with imposed regulations at the local level. I test if, how, and why the effect of a simple contribution rule in a public goods game depends on how the rule has been implemented: whether it was democratically chosen or externally imposed.

Although each chapter is based on a stand-alone working paper or journal article and thus includes a separate introduction, I want to provide a broader rationale of the re-

Table 1.1: Schematic overview of the thesis

<b>Part</b>	<b>Chapter</b>	<b>Method</b>
<b>I. Global Public Goods</b>	Voting for burden sharing rules in public goods games	Laboratory experiment
	Ratchet up or down? An experimental investigation of global public good provision in the United Nations Youth Associations Network	Artefactual field experiment via the internet
<b>II. Multi-level Public Goods</b>	Leveling up? An inter-neighborhood experiment on parochialism and the efficiency of multi-level public goods provision	Artefactual field experiment via the internet
<b>III. Local Public Goods</b>	Non-binding restrictions, cooperation, and coral reef protection: Experimental evidence from Indonesian fishing communities	Framed field experiment
	Democracy and compliance in public goods games	Laboratory experiment

Note: Schematic overview of the conceptual and methodological approach of the thesis.

search conducted in this introductory chapter. I start by giving some background on social dilemma situations in the realm of environmental and resource economics (Section 1.1). Then, I examine various facets of global and local public good problems (Section 1.2). In Section 1.3, I outline the methodological approach of the thesis. I discuss the focus and contribution of this thesis in Section 1.4 and Section 1.5 concludes.

## 1.1 Social dilemmas in environmental and resource economics

Social dilemmas meet two criteria. First, individuals receive a higher payoff for a socially defecting choice than for a socially cooperative choice, no matter what the other individuals in society do. Second, all individuals are better off if all cooperate than if all defect (e.g., Dawes 1980). In the realm of environmental and resource economics, two polar cases of social dilemmas can be classified: public good and common-pool resource problems.

Benefits of public goods are non-rival and non-excludable in consumption. Benefits are non-rival when a unit of the good can be consumed by one individual without reducing the quantity available for consumption by another person. Climate protection, for instance, gives a non-rival benefit, since people who benefit from mitigating climate

change do not reduce the benefits for others. If the benefits of a good are available to all once the good is supplied, then the benefits are non-excludable. Climate protection is also non-excludable because it is impossible to prevent others from benefiting from mitigating climate change.

The first welfare theorem in economics states that private competitive markets lead to Pareto efficient allocations, as long as certain conditions are fulfilled (e.g., Debreu 1959). However, the conditions under which markets are efficient are restrictive and in reality there are many *distortions* (e.g., Fullerton and Stavins 1998). As analyzed by Samuelson already in 1954, the important point about public goods is that the private market generally does not guarantee efficient provision. Non-excludability poses a free-rider problem because a provider cannot keep non-contributors from consuming the good's benefits. Free-riding incentives will prevent individuals from contributing to a public good because they will get the good's benefits for free once it is provided by others and prevent uncoordinated action to meet an efficient allocation (e.g., Bergstrom et al. 1986; Sandler 1998).

Common-pool resources refer to resource systems in which exclusion of beneficiaries is impractical and benefits are rival in the sense that every use of the resource has an effect on others by decreasing the resource left for all others (e.g., Ostrom et al. 1994). As Bromley (1992) emphasizes, it is the institutional arrangement as much as the resource itself that gives rise to the common-pool resource problem.<sup>1</sup> Open-access resources, where no property rights have been established and the resource is non-excludable and open to all, give rise to Hardin's "tragedy of the commons" (Hardin 1968). It is rational for resource users to consider marginal revenue and marginal extraction costs, but to ignore scarcity rents (e.g., Stavins 2011). In the open ocean, for instance, each fisherman receives the full benefit of fishing and each fisherman's choices have an effect on other fishermen. These impacts are not taken into account and drive the resource stock below the efficient level.

In social dilemmas, individual rationality is not sufficient for collective rationality and Olson argued already in 1965 that "rational, self-interested individuals will not act to achieve their common or group interest" (Olson 1965, p. 2). This gives rise to an intense research on designing adequate institutions to overcome these dilemmas (e.g., Chaudhuri 2011). However, social dilemmas arise on different scales at the local and global level, which poses specific challenges (e.g., Sandler 1997). In Section 1.2, I outline the specific conditions of social dilemmas at both the global and local level.

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<sup>1</sup>Bromley (1992) distinguishes between four regimes in relation to common-pool resources: state property, common property, individual property, and non-property (open-access). Open-access - the absence of property rights - leads to degradation of the resource. For a detailed discussion of types of property regimes, see, e.g., Bromley (1992), Feeny et al. (1996), Ostrom et al. (1999).

Table 1.2: Taxonomy of public good problems in the realm of environmental and resource economics

		← Temporal →	
		Intragenerational	Intergenerational
↑ Geographical	Local	Intragenerational local public goods <i>Ground water pollution clean-up</i>	Intergenerational local public goods <i>Reduction of acid rain, depletion of local ecosystems</i>
	Global	Intragenerational global public goods <i>Ocean pollution clean-up</i>	Intergenerational global public goods <i>Limiting ozone depletion, curbing climate change</i>

Note: Classification according to the geographical and temporal dimension of public good problems follows Sandler (1997; 2004). Examples in italics.

## 1.2 The character of local and global public goods

In order to provide a clear taxonomy, I distinguish between local and global public goods as well as intra- and intergenerational public goods. A corresponding classification in the realm of environmental and resource economics is provided in Table 1.2. Although both public good and common-pool resource problems are social dilemma situations, they are strategically not equivalent (e.g., Apesteguia and Maier-Rigaud 2006). At the global level, however, both pose similar challenges and I follow the literature (e.g., Kaul et al. 1999; Sandler 2004; Nordhaus 2006) and use the generalizing label *public good problem*, knowing that both types of dilemma are distinct.

Global public goods are non-rival and non-excludable and, therefore, qualitatively not different from other - more local - public goods. However, in term of beneficiaries - the *publicum* - global public goods differ. Sandler (1997; 1998; 2004) follows a strict geographical approach and defines global public goods as those with no geographical limits of non-rival and non-excludable benefits and, therefore, beneficial to the world at large. Conversely, this defines local public goods as public goods that deliver non-rival and non-excludable benefits to a more limited geographical area. In addition, Nordhaus (2006) emphasizes the temporal dimension of public good problems and states that the impact of a variety of global public goods depends upon a stock of a variable that accumulates over time. Depending on the depreciation rate, this could have long-lasting consequences for many generations. In this sense, intergenerational

public goods, in contrast to intragenerational public goods, provide non-rival and non-excludable benefits among and within generations (e.g., Sandler 1999).<sup>2</sup>

Like limiting ozone depletion, curbing climate change is an ideal example of an intergenerational global public good. Efforts by any agent to mitigate greenhouse gases limit the atmospheric concentration of greenhouse gases globally (e.g., Sandler 2004). Furthermore, the impact of greenhouse gases depends upon the concentration in the atmosphere. Especially, carbon dioxide, identified to be the main driver of climate change, has an atmospheric residence time with a half-life in the order of a century or more and long-lasting consequences affecting future generations (e.g., Nordhaus 2006; 2007). The depletion of ecosystems or acid rain, for instance, are more localized public good problems. Benefit spillovers from abatement are less far reaching in these cases and tend to affect multiple generations in a well-defined region (e.g., Sandler 2004). Further examples of intergenerational local public goods are curbing desertification, the preservation of wetlands, and the removal of pollutants like lead, and dioxins (e.g., Sandler 1997). Other public goods provide either regional or global benefits just for the current generation. The clean-up of ground water pollution, for example, provides benefits for the current generation in a well-defined region. The benefits of ocean pollution clean-up, in contrast, affect the current generation on a global level (e.g., Sandler 1997). Undoubtedly, all public good problems pose severe challenges (see Section 1.1). Market failures and resulting inefficiencies are the largest for global and especially intergenerational global public goods affecting a vast number of people for long periods of time (e.g., Nordhaus 2006).

Global public goods are not a new phenomenon. Globalization, however, enhances the transfer of public good spillovers (e.g., Sandler 2004) and global public goods become more important as a result of technical change and the decline in transportation and communication costs (e.g., Nordhaus 2006; 2007). Or to put it in the words of Sandler: "Technology continues to draw the nations of the world closer together and, in doing so, has created novel forms of global public goods [...] that have diminished somewhat the relevance of economic decisions at the nation-state level" (Sandler 1998, p.221). He thereby also emphasizes that global public goods create additional economic and political challenges since the effect of one country might be very minor for solving

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<sup>2</sup>More nuanced definitions emphasize that the world is not only divided along geographical and temporal lines and that this should be taken into account when defining local and global public goods (e.g., Kaul et al. 1999). In addition to Sandler (1998) and Nordhaus (2006), Kaul et al. (1999) stress also a sociological dimension. Following Kaul et al. (1999), a global public good provides non-rival and non-excludable benefits to more than one geographical region for many generations and it does not discriminate against any population group. Though it is conceptionally possible to separate the geographical from the sociological dimension, I abstain from this subdivision since these dimensions are almost congruent in many examples of application.



market failures at the global level. Before I provide a detailed discussion of the specific challenges in the voluntary provision of global and local public goods relevant for this thesis in Section 1.4, I outline and discuss the methodological approach of the thesis in Section 1.3.

### **1.3 Experiments in environmental and resource economics**

Carried out within a controlled environment, economic experiments improve our understanding about how incentives, values, norms, and choices interact. Experimentation has therefore made its transition from a topic to a tool (Samuelson 2005) and has become another methodological approach within environmental and resource economics (e.g., Shogren and Nowell 1992; Shogren and Hurley 1999; Ehmke and Shogren 2009; Sturm and Weimann 2006; Cherry et al. 2008). In the following, I outline on which experimental techniques and tools this thesis relies on in order to provide new insights on the voluntary provision of global and local public goods.

#### **1.3.1 A useful tool**

In economics experimentation began in earnest during the 1960s with the work of Vernon Smith (1962; 1965; 1967).<sup>3</sup> In very general terms, (economic) experiments are defined as controlled data generation processes, in which the most factors which affect behavior are held constant and only one factor of interest is varied at a time (e.g., Croson and Gächter 2010). Experiments have a long tradition in environmental and resource economics<sup>4</sup> and experimentation on how to design and implement institutions to foster cooperation in social dilemma situations has been a subject of intense research since the beginning (e.g., Shogren and Nowell 1992; Kling 2008).<sup>5</sup> Following this tradition, my work relies on experiments for at least three reasons.

First, I use economic experiments as a tool to testbed institutional designs that aim to foster cooperation in the voluntary provision of public goods both at the global and more localized level. The purpose of a testbed is to determine if institutions can be implemented, how they work once they are implemented and which institutions perform best (e.g., Plott 1994; Sturm and Weimann 2006; Ehmke and Shogren 2009). One important asset of economic experiments to testbed institutions is that, in principle, they provide *ceteris paribus* observations of incentivized individual economic agents

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<sup>3</sup>Notable exceptions are Chamberlin (1948), Mosteller and Noguee (1951) and Flood (1958).

<sup>4</sup>For selective surveys of laboratory experiments related to environmental and resource economics, see, e.g., Sturm and Weimann (2006) and Noussair and van Soest (2014).

<sup>5</sup>Peter Bohm (e.g., Bohm 1972) as well as Charles Plott (e.g., Plott 1983) provide early examples of using laboratory experiments to test different mechanisms to voluntarily provide public goods.

and allow to make causal inferences (e.g., Levitt and List 2007).

Second, in general, experiments are used to test the predictive power of theories and axioms underlying the theory (e.g., Shogren and Hurley 1999; Sturm and Weimann 2006; Cherry and Dickinson 2008; Ehmke and Shogren 2009). Within the scope of this thesis, I follow this line of research and use experiments to test the power of hypotheses on predicted reactions to changes in exogenous parameters.

Third, researchers use experiments to look for behavioral patterns and explore, for instance, how people reveal their demand and construct preferences and beliefs (e.g., Sturm and Weimann 2006; Cherry and Dickinson 2008). In this light, my thesis relies on experimentation in order to investigate individual preferences, resentments, and patterns in the voluntary provision of global and local public goods.

Finally - even if this is not directly related to this thesis - I want to briefly mention that experiments are used as a tool to teach (e.g., Ehmke and Shogren 2009).<sup>6</sup> Furthermore, Ehmke and Shogren (2009) highlight that experiments can - in principle - also be used outside the classroom to inform policy-makers, businesses, and households about how new policies or regulations work.

### 1.3.2 Experimental challenges, methods, and techniques

A fundamental question is whether findings from an economic experiment can provide reliable inference outside the laboratory. Whatever we observe in an experiment might not be valid and relevant outside the laboratory, since the experimental environment differs from the world beyond (e.g., Levitt and List 2007). According to Smith's precept of *parallelism* (e.g., Smith 1980; 1982; Plott 1987), it should be possible to conclude that experimental results apply outside the laboratory if similar *ceteris paribus* conditions hold in both cases. However, different factors that systematically vary between the laboratory and the outside world have been identified to question Smith's precept of parallelism and, therefore, the external validity of economic experiments.

First, in a laboratory experiment subjects enter an environment in which they are aware that their behavior is monitored, recorded, and scrutinized. Such scrutiny can be responsible for the sense of role obligations of being an experimental subject and the willingness to reply expectedly, i.e., the experimenter demand effect (e.g., Zizzo 2010), and exaggerate, for example, pro-social preferences (e.g., Levitt and List 2007). Second, the context in experimental settings influences subjects' behavior and factors beyond the experimenter control may cause subjects not to play the game that the experimenter

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<sup>6</sup>Bergstrom and Miller (2000) present a set of experiments to teach microeconomic principles. Following this line, Giamattei and Lambsdorff (2015) provide with classEx a web-based ready-made software tool to carry out experiments in the classroom.

has in mind (e.g., Hertwig and Ortmann 2001; Levitt and List 2007). Third, the experimenter creates and restricts a set of actions a subject is allowed to take and defines the temporal aspect of a task. In naturally occurring environments, in contrast, the choices are often unlimited and one should take into account that identical preferences might be expressed in different ways in and outside the laboratory due to different options (e.g., Levitt and List 2007). Fourth, Henrich et al. (2010), for instance, point out that experiments are largely based on students recruited from western, educated, industrialized, rich, and democratic societies who self-select into the experiments. Since subjects in an experiment differ systematically from agents in many situations outside the laboratory, potential subject pool biases are likely to question the generalizability of experimental results (e.g., Levitt and List 2007; Henrich et al. 2010). Fifth, Levitt and List (2007) highlight the differences between low stake laboratory experiments and the world beyond. However, they also point out that large increases in stakes do not necessarily lead to proportional changes in participants' decisions and that it is, thus, important to adequately adjust for differences in stakes across settings when generalizing results.

In order to address these factors, three different types of experiments are employed in this thesis. According to the nomenclature of Harrison and List (2004), *laboratory experiments* - as experiments that employ a standard subject pool of students and an abstract framing - are the basis for the work in my thesis. In order to reach greater applicability of the results, parts of the thesis are based on participants from non-standard subject pools recruited in the field rather than in the classroom for participation in *artefactual field experiments*. Finally, a *framed field experiment* is conducted with participants from a non-standard subject pool using field context rather than abstract terminologies.<sup>7</sup>

A further distinction can be made between the technologies how economic experiments are conducted. The first economic experiments relied on very basic tools. Paper, pencils, blackboards, and posters have been used to explain instructions as well as to capture and process participants' decisions (e.g., Kagel and Roth 1995). In the 1990s, tools for conducting experiments via local computer networks were developed and became the primary tool of conducting laboratory experiments (e.g., Horton et al. 2011).<sup>8</sup> Computer-mediated experiments made it easier to conduct interactive exper-

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<sup>7</sup>*Natural field experiments* are defined the same way as *framed field experiments* but the environment is one where the subjects naturally undertake these tasks and where the subjects do not know that they are in an experiment. As the focus of the thesis is on laboratory, artefactual, and framed field experiments rather than natural field experiments, I abstain from a discussion of field experiments. For an overview of field experiments and randomized control trials in environmental and resource economics, see, e.g., List and Price (2016).

<sup>8</sup>Most prominently, Urs Fischbacher provided with z-Tree a software package for conducting and developing computer-mediated experiments via local computer networks (Fischbacher 2007).

iments, collect data, and also increase control over the flow of information. By now researchers have developed tools to get around local computer networks and get past physical laboratories with all their limitations and restrictions. In this line, parts of this thesis contributes to the growing literature in economics postulating an *online laboratory* (Bainbridge 2007) by conducting experiments via the internet to overcome limitations of laboratory experiments and get access to non-standard subject pools and combine participants from a variety of different countries.

Experiments conducted via the internet are a subject of intense discussion and some of their greatest advantages can be summarized as follows:<sup>9</sup> First, running experiments via the internet provides the opportunity to reach more diverse subject pools in terms of education, profession, age etc. (e.g., Hergueux and Jacquemet 2015). Second, via the internet it is possible to increase anonymity, for instance by inducing “double blindness” between experimenter and subject (e.g., Eckel and Wilson 2006). Third, without the physical limitations of laboratories, it is possible to run experiments simultaneously or asynchronously with a large number of participants (e.g., Reips 2002). Fourth, using the internet makes it possible to run experiments at different sites which provides the opportunity of comparing and combining decisions across countries and cultures (e.g., Eckel and Wilson 2006).

However, there are requirements of economic experiments that create methodological and practical problems for the online laboratory. Following Eckel and Wilson (2006) as well as Horton et al. (2011), methodological aspects of online experiments that challenge experiments’ internal validity are diverse: It is difficult to monitor the identity of participants; participants may read instructions carelessly, make decisions too quickly, or may be distracted; participants may not believe that there are other subjects involved in the experiment, that the experiment is conducted as described in the instructions, and that they are going to be paid; participants may exhibit a lack of trust toward others on the internet; and participants may selectively drop out of the experiment. Further practical problems include the logistics of recruiting and paying participants as well as restrictions to the experimental design. While the implementation of one-shot experiments is straightforward, repeated games require a substantial technical effort (e.g., Horton et al. 2011).

Comparative studies on experiments conducted both in the laboratory and online find that if these challenges are adequately considered within the design of online experiments, then there is strong parallelism between individuals’ behavior in these two types of experiments. Consequently, online experiments can be just as valid as labor-

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<sup>9</sup>See, e.g., Anderhub et al. 2001; Reips 2002; Shavit et al. 2001; Eckel and Wilson 2006; Charness et al. 2007; Horton et al. 2011; Hergueux and Jacquemet 2015 for discussions of methodological aspects of online experiments.

atory experiments (e.g., Horton et al. 2011; Hergueux and Jacquemet 2015).

## 1.4 Focus and contribution of the thesis

The overall objective of my thesis is to study the voluntary provision of global and local public goods in order to design and testbed institutions that aim at addressing these challenges. Against the conceptual and methodological approach outlined in Sections 1.2 and 1.3, five different experiments were conducted, which are structured along three parts (see Table 1.1). In the following, I will discuss the most relevant challenges in the provision of global and local public goods for this thesis and its corresponding contribution in greater detail.

**Part I** The first part of the thesis presents results of two experiments on different challenges in the voluntary provision of global public goods. Both Chapter 2 and Chapter 3 address two specific aspects of international climate negotiations to testbed institutions that aim to foster cooperation among sovereign agents to halt climate change.

*First*, a fundamental challenge in providing global public goods is that it requires global collective action (e.g., Sandler 2004). This leads to what William Nordhaus refers to as the *Westphalian dilemma* (e.g., Nordhaus 2006).<sup>10</sup> On the global stage, the crux is that there is no jurisdictional control through a central authority with enforcement capacities comparable to those of a national government within the nation-state (e.g., Barrett 1990; Sandler 2004; Nordhaus 2007; 2015). Consequently, the voluntary provision of public goods becomes the decisive field of research on how to overcome collective action problems without a central authority with corresponding enforcement capacities (e.g., Löschel and Rübberke 2014; Noussair and van Soest 2014; Nyborg et al. 2016). This immediately gives rise to the question whether groups of sovereign individuals are able to set themselves institutions in social dilemma situations. The first experiment that allows sovereign individuals to collectively set up an institution to solve a social dilemma situation is Ostrom et al. (1992). They find that around half of their participants implement a decentralized peer-to-peer punishment scheme, if they have the chance, and that efficiency levels are higher, if the institution is adopted than

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<sup>10</sup>Following the Treaty of Westphalia in 1648, the system of sovereign states established the central principles of modern international law. States are sovereign and have the fundamental right of political self-determination, they are legally equal, and are free to manage their internal affairs without interventions of other states (e.g., Kaul et al. 1999; Nordhaus 2015). Consequently, international obligations may be imposed on a sovereign state only with its consent. The requirement of reaching unanimity makes it considerably more difficult to reach universal and binding international agreements (e.g., Nordhaus 2006).

if groups fail to adopt the institution. Further experiments suggest that groups are also able to voluntarily impose centralized institutions, i.e., rules specifying what penalties will be imposed under what conditions, and set up a body that observes rule violations and imposes penalties (e.g., Feld and Tyran 2002; Tyran and Feld 2006; Dal Bó et al. 2010). Results by Markusson et al. (2013) and Kamei et al. (2015) suggest that individuals prefer decentralized compared to centralized schemes. Individuals also tend to choose reward over punishment schemes (e.g., Sutter et al. 2010) and prefer to sanction below rather than above average cooperators (e.g., Noussair and Tan 2011). However, within the literature on endogenous institutions in social dilemmas, heterogeneities between agents, which are a major source of controversies in international climate negotiations, remain sparsely studied. Margreiter et al. (2005) and Noussair and Tan (2011) are notable exceptions and suggest that heterogeneity among group members confounds coordination on an institutional scheme.

Chapter 2 contributes to this literature by investigating whether groups of heterogeneous agents, differing in their initial endowment, reach an agreement on how to share the costs of providing a public good. Against the background of international climate negotiations, we consider a set of three different burden sharing rules: equal contributions from all group members to the public good, equal payoffs for all group members, and contributions to the public good that are proportional to participants' endowment. We use a laboratory experiment to provide participants with the opportunity to choose between these burden sharing rules and impose the rules also exogenously as controls. Despite the fact that preferences for the burden sharing rules differ among participants, we find that most groups manage to agree upon a common scheme and successfully avoid an uncoordinated action. Furthermore, our results reveal both opportunities and risks of burden sharing negotiations. Average efficiency levels increase, if agents reach an agreement on how to share the cost. However, average efficiency levels decrease and are below an externally imposed voluntary contribution mechanism, if agents fail to reach an agreement.

*Second*, another aspect of global collective action and the provision of global public goods is that it requires transboundary cooperation (e.g., Sandler 2004). This could be especially demanding since a variety of experiments show that cooperation behavior substantially differs across countries (e.g., Weimann 1994; Burlando and Hey 1997; Henrich et al. 2001; Henrich et al. 2006; Herrmann et al. 2008). These results come from running the same experiment in a number of different countries and then comparing the results across locations. Herrmann et al. (2008), for instance, investigate the effect of costly punishment in public good games conducted in 16 subject pools around the world. They find that some subject pools punish above average contributors as

much as below average contributors. In some subject pools, this antisocial punishment is strong enough to erode the cooperation enhancing effect of decentralized punishment. These papers provide valuable insights, but it is not clear if evidence from intranational comparisons can be extrapolated for international concerns (e.g., Adler and Graham 1989; Chuah et al. 2007). Yet, there have been only a few published studies that experimentally investigate international social dilemma situations. They provide mixed results. Castro (2008) as well as Matsumoto and Hwang (2011), for example, find that mixing groups leads to less cooperative behavior. Carpenter and Cardenas (2011) conducted an international common-pool resource experiment with participants from Columbia and the US. They show that mixing groups leads to a decrease in extractions by American participants, but to an increase in extraction choices by Colombian participants. Based on prisoner's dilemma with Australian and Japanese participants, Yamagishi et al. (2005) find that participants from Australia and Japan cooperate more in international than in intranational groups. A crucial feature of international climate negotiations is that they need to cover a variety of different countries and regions. So far, only participants from up to six countries have been combined in experiments on international social dilemmas. The international public goods experiment by Buchan et al. (2009) is based on a sample of participants from Argentina, Iran, Italy, Russia, South Africa, and the United States. Brick and Visser (2015) investigate the perception of different burden sharing rules in a threshold public good game drawing on a sample of participants from the United States, the European Union, China, India and South Africa. They report evidence of self-interested use of burden sharing rules only among participants from China and the United States.

In Chapter 3, we capture important heterogeneities and varieties in global public good problems by collaborating with the United Nation Youth Associations Network (UN-YANET) and establishing an international subject pool with more than 130 participants from more than 50 countries. By conducting an artefactual field experiment via the internet we enabled individuals from different countries to participate in the experiment. To address wealth heterogeneity among parties in international climate negotiations, participants from developed and developing countries make decisions on high and low endowments, respectively. Furthermore, the experiment addresses recent developments in international climate negotiations by capturing pre-existing public good contribution levels and giving participants the opportunity to increase contributions to the public good beyond the pre-existing contribution level, maintain the current level, or even undo efforts. We find that reducing pre-existing contributions by taking something out of the public good appears to be a strong behavioral barrier. Consequently, public good provision levels are higher when starting with an intermediate provision

level compared to a pure giving frame.

**Part II** To build a bridge to the analysis of more localized public goods, the second part of the thesis takes into account that local and global public goods are not necessarily mutually exclusive. In many cases local and global public goods are interrelated by joint provision benefits. Reducing multi-level air pollutants like tropospheric ozone, for instance, causes interrelated effects varying from local health benefits to global climate stability (e.g., Shindell et al., 2012). In other cases, local and global public goods do co-exist and individuals have to decide not just whether to contribute, but also how much to contribute to which type of good. Cornes and Sandler (1984) provide first theoretical insights on interdependent public goods with benefits linked on multiple levels. This simultaneity of local and global public goods gives rise to an increasing number of experiments on voluntary contributions in multi-level public good games (e.g., Wachsman 2002; Wit and Kerr 2002; Blackwell and McKee 2003; Buchan et al. 2009; Güth and Sääksvuori 2012; Fellner and Lünser 2014; Beekman et al. 2017; Chakravarty and Fonseca 2017). These experiments suggest a tendency towards contributing to the local public good, if the social returns of the local and global public good are equal (e.g., Blackwell and McKee 2003; Fellner and Lünser 2014; Chakravarty and Fonseca 2017). Furthermore, they report that the most efficient public good attracts voluntary contributions (e.g., Blackwell and McKee 2003; Fellner and Lünser 2014). Based on an international subject pool Buchan et al. (2009) show that the individual as well as country level of globalization increases individuals' propensity to contribute to the global public good. In this line, social identity theory (e.g., Tajfel and Turner 1979; Turner et al. 1979) and experiments on in-group favoritism (e.g., Chen and Li 2009) give rise to the question whether individual resentments can affect contribution behavior in multi-level public good games.

Chapter 4 is based on an artefactual field experiment conducted via the internet in order to test whether individual's local identity and narrow concern for local outcomes (parochialism) can harm efficiency in a setting where different public goods co-exist and can be provided at different spatial levels. In order to conduct the experiment in a setting conducive to parochial behavior, we recruit more than 600 citizens of two neighboring cities in Germany. In our inter-neighborhood intra-region design, participants allocate an endowment between a personal account, a local public good account, and a regional public good account. The experimental design varies across two dimensions: whether participants learn that the small group contains only members of the participant's own neighborhood or not and the relative productivity of the local and regional public good. By comparing groups in which participants were or were not



aware that the small group consisted of their local neighbors, we find that both groups increased their contributions to the regional public good as the marginal return of the regional public good is increased. Importantly, this increase is not statistically smaller in the group where neighborhood attachment was made salient. In other words, we find evidence for parochialism, but parochialism does not interfere with efficiency.

**Part III** The third part presents results of two experiments which address different challenges in the provision of more localized public goods.

*First*, although the provision of local public goods is perceived as less problematic (e.g., Nordhaus 2006), it nevertheless poses severe challenges. In contrast to global public goods, more localized social dilemmas often do have de jure authorities with respective permissions to intervene and regulate, but de facto authorities can either be ignorant or lack the resources needed to monitor, sanction, and enforce interventions (e.g., Feeny et al. 1996; Kroll et al. 2007). Even if authorities with respective enforcement capacities exist on paper, the outcome of many local public good problems is more likely to confirm predictions of a regime without any formal institutional framework (e.g., Ostrom et al. 1994). Cardenas and Carpenter (2008), for instance, highlight that especially communities in developing countries rely on informal local norms and rules to provide local public goods and regulate common-pool resources. Evidence from the field illustrates the likelihood of many failures, and some notable successes of self-organized small scale local social dilemma situations underline the importance of informal institutions to solve social dilemma situations in the absence of central authorities (e.g., Ostrom 1990). However, monitoring and sanctioning costs could even hamper the emergence of informal institutions which aim at fostering cooperation.

Chapter 5 takes into account that costly monitoring and sanctioning can preclude the emergence of centralized and even decentralized institutions to preserve coral reef fisheries in Indonesia. Coral reef protection in Indonesia is an ideal example of a local public good with de jure competences of the Indonesian government to regulate fisheries, but de facto the government lacks capacities to actually monitor and sanction. The experiment relies on an extension of a standard common-pool resource game in order to explore the potential of different management approaches to reduce overfishing. We conduct an artefactual framed field experiment on the effect of non-enforced extraction limits in Indonesian fishing communities. We explore whether individual extraction decisions vary according to three non-binding recommended extraction levels originating from a democratic process, a group leader, or an external authority. We find a strong effect of the external treatment, a weaker effect of the democratic treatment and no effect of the leadership treatment. Closer inspection reveals that the results are

driven by one of the three sites where the experiment was conducted – that having the highest levels of ethnic and religious diversity. The absence of effects in two of the three sites suggests that a non-binding recommendation may often be insufficient in promoting the cooperative behavior that underpins contemporary approaches to managing coral reefs, and most importantly, underlines the importance of designing institutions and evaluating effects on a community-by-community basis.

*Second*, the idea that democratic participation may affect how individuals will respond to policies has a long tradition. Already in 1839 de Tocqueville wrote: “It is not always feasible to consult the whole people, either directly or indirectly, in the formation of the law; but it cannot be denied that, when such a measure is possible, the authority of the law is much augmented.” (de Tocqueville 1838, p. 228). In this line, a series of laboratory experiments on endogenous institutions in social dilemma situations tend to find support for a *democracy premium*. Decentralized (e.g., Sutter et al. 2010; Markusson et al. 2013; Kamei et al. 2015) as well as centralized (e.g., Feld and Tyran 2002; Tyran and Feld 2006) punishment institutions seem to be more effective in increasing contributions to a public good in case they are endogenously chosen than if they are externally imposed. This gives rise to the idea that local authorities with respective enforcement capacities may abstain from top-down regulations and use participation in order to increase compliance. Dal Bó et al. (2010) identify and quantify different drivers of the effect of democratic participation and conclude that a deterrent policy is more likely to be respected if it is democratically chosen than externally given. Since many interactions in real life related to cooperation are subject to non-deterrent policies (e.g., Ostrom 1990; Kroll et al. 2007), it remains an important and as yet unanswered question whether the findings by Dal Bó et al. (2010) can be extrapolated into a setting with a non-deterrent intervention and whether participation improves agents’ willingness to follow rules although they face incentives not to do so.

In Chapter 6, I combine the key design elements of Tyran and Feld (2006) and Dal Bó et al. (2010) in order to test if, how, and why the effect of a non-deterrent contribution rule in a public goods game depends on how the rule has been implemented: democratically chosen or externally imposed. The rule prescribes full contributions to a public good and is backed by a weak and non-deterrent sanction for those participants who do not comply. I complement the existing literature on endogenous institutions in social dilemma games, by using a randomization strategy in order to investigate to what extent the effect of democratic participation is driven by self-selection into the rule, information transmitted via voting, and democracy per se. I find that contributions to the public good are significantly higher if the rule is democratically chosen than if it is democratically rejected. By contrast, when treatments are exogenously given, the

contribution rule has no effect. A naive comparison suggests that the contribution rule is more effective, if it is democratically chosen than externally imposed. However, my decomposition reveals that this difference is not directly attributed to democratic participation. Democratic participation does not affect participants' contribution behavior if I take into account self-selection into treatments and the information transmitted by revealing the outcome of the referendum.

## **1.5 General lessons**

What are the general lessons of these five essays with regard to designing institutions that aim at fostering the voluntary provision of public goods both at the global and more localized level?

Experiments in Part I of the thesis are devoted to the provision of global public goods absent jurisdictional control through a central authority and within the context of international climate negotiations. Even if the experimental results cannot be transferred one-to-one to the real world, Chapter 2 reveals both opportunities and risks of international climate negotiations. It shows that sovereign and heterogeneous agents are able to reach an agreement on how to share the costs of providing a public good. Furthermore it shows that cooperation increases, if an agreement is reached. However, if agents fail to reach an agreement, cooperation collapses. Thus, Chapter 2 emphasizes the importance of small steps in the process of international climate negotiations in order to avoid that negotiations fail. Relating to this, the online experiment conducted among the international members of the United Nations Youth Associations Networks in Chapter 3 underlines the importance of a status-quo within the process of international climate negotiations. The results reveal that reducing the public good by undoing what has been already achieved appears to be a strong behavioral barrier. This finding provides support for the pledge and review process within the Paris Agreement. Even though Nationally Determined Contributions are, from a current perspective, not sufficient to limit global warming to below 2°C above pre-industrial level as intended and absent a central authority with respective enforcement capacities that precludes a downward revision of national targets.

Part II of the thesis shows that individuals' narrow concerns for local outcomes do not harm efficiency in a setting where different public goods co-exist and can be provided at different spatial levels. It indicates that a higher efficiency of the higher level public good is associated with increasing contributions and that this is irrespective of whether agents' local attachment was made salient or not. The implications of the experiment are relevant to policy as they give an indication that naturally grown types of local identity play a considerable role in individuals' cooperation behavior, but does not

necessarily imply a penalty on efficiency in a situation in which public goods can be provided simultaneously at different spatial levels.

Having said this, what can we learn from the experiments in Part III dedicated to more localized public good problems? Chapters 5 and 6 come to the conclusion that participatory processes have a rather limited effect in fostering cooperation in local social dilemma situations. The framed field experiment conducted in Indonesian fishing communities presented in Chapter 5 does not support the hypothesis that participatory processes in general reduce individual extractions from a common-pool resource. It shows only a weak effect of non-binding recommendations originating from a democratic process and finds that recommendations from a group leader have no effect on individual extraction decisions. Further inspections reveal that the results are driven by one of the three sites where the experiment was conducted. The experimental results underline that institutions and evaluations must be made on a case-by-case basis, even in a rather localized setting with seemingly similar conditions. Within a laboratory experiment, it is shown in Chapter 6 that the effect of a non-deterrent contribution rule is more effective in increasing public good provision levels if treatments are democratically chosen than if they are externally imposed. However, the experiment reveals that this difference is not directly attributed to democratic participation. It shows that the effect is a conglomerate of different sub-effects of participation, especially the effect of self-selection into the intervention and of the information transmitted by voting, and that participation does not affect agents' contribution decisions per se. Thus, Chapter 6 does not support the hypothesis that democratic participation directly affect how individuals respond to policies and can be used as an instrument to increase individuals' willingness to comply with non-deterrent policies.

This introductory chapter provided an overview of the broader rationale of the research conducted in this thesis in order to motivate the specific research questions addressed, to outline the scientific contributions, and to draw some general lessons. The detailed literature reviews, explanations, and analyses are provided in the following chapters.

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## 2 Voting for burden sharing rules in public goods games

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**Abstract:** We experimentally investigate whether groups of heterogeneous agents can reach an agreement on how to share the costs of providing a public good. Thereby, we explore the performance of different burden sharing rules being implemented either endogenously or exogenously. In case of an endogenously implemented burden sharing rule, subjects vote for different burden sharing schemes either by unanimity or majority vote. Despite the fact that preferences for the allocation schemes differ among agents, most groups agree upon a common scheme, and consequently avoid an uncoordinated action. Our results reveal both the opportunities and risks of burden sharing negotiations. We find average efficiency levels to increase in case an agreement is reached. If groups however fail to agree upon a common rule, cooperation collapses and efficiency levels decrease compared to a voluntary contribution mechanism being exogenously imposed. Most importantly, agents who face a voting decision on average receive higher payoffs than agents in an exogenously implemented voluntary contribution mechanism and do not earn less than participants in any externally determined burden sharing rule.

## 2.1 Introduction

Inducing contributions to public goods remains an important endeavor and is particularly demanding when sovereign agents have different interests due to heterogeneous preferences. Free riding incentives prevent the voluntary provision of a public good and every institutional design which aims at countervailing free riding has to address the question of a fair distribution of the costs and benefits from providing public goods. The experimental literature suggests rule-based contribution schemes which are inspired by different fairness norms as being effective in enhancing cooperation (e.g., Orzen 2008; Dannenberg et al. 2014; Kesternich et al. 2014; 2017). In such a scheme, agents have to agree upon a common group provision level of the public good by knowing that the associated costs are distributed among the agents according to an exogenously predetermined burden sharing rule.

While the previous papers devoted to this issue show that exogenously imposed burden sharing rules are able to increase efficiency levels in contrast to a voluntary contribution mechanism (VCM), we argue that the external implementation of a certain burden sharing scheme is rarely a feasible or desirable option due to sovereign agents. As an important example, in most international negotiations parties have to voluntarily agree upon a joint decision. Regarding international climate negotiations as an important application, a variety of rules for carbon emissions reductions among participating countries is conceivable and it constitutes a major challenge for participants to agree upon a common rule (e.g., Lange et al. 2007; 2010; Kesternich et al. 2014). Burden sharing rules being currently discussed within international climate policy are inspired by different fairness norms. These fairness norms comprise, for instance, sovereignty, calling for equal contributions from all agents (e.g., equal percentage reductions of emissions), or need, which implies relatively low obligations for developing countries and economies in transition and consequently shifts the burden towards agents with substantial economic capacity (e.g., Johansson-Stenman and Konow 2010). In most international negotiations, the consensus principle serves as the primary decision making tool. Even though the term consensus does not necessarily equal unanimity but is rather defined as “the absence of any formal objection” (e.g., Schwarte 2011) in many conventions, it is often claimed that a consensus enables one single party to block and delay decisions. Therefore, ongoing debates in international climate negotiations on possible changes in the decision-making process include calls for a supranational authority or an international framework being able to enforce the distribution of global mitigation efforts, for instance, through adjusting the consensus principle by establishing some form of majority voting (e.g., Vogel 2014). Also in the management of local commons like irrigation systems, different rules that regulate the distribution of costs

and benefits of cooperation among group members are subject to discussions. Ostrom (1990) demonstrates that governing schemes which are characterized by rules that distribute costs and benefits equivalently lead to comparatively high cooperation levels. Even in case of local public goods where enforcement problems appear less severe, an agenda-setter such as a local government may give agents the opportunity to agree on a contribution scheme by themselves instead of determining the burden sharing exogenously.

Our experimental design enables us to analyze the performance of different burden sharing rules being endogenously implemented by the participating agents themselves, both to an exogenously given *VCM* and to a setting where allocation schemes are predetermined by an agenda setter. We thereby apply a design with players differing in their initial endowments which, based on our previous findings, creates substantial tensions in the different rule-based contribution schemes (Kesternich et al. 2017). In particular, we investigate whether (i) sovereign and heterogeneous agents are able to agree upon a joint burden sharing rule, whether (ii) agreement formation does affect contribution behavior, and whether (iii) efficiency gains from exogenously implemented schemes persist in a setting with endogenously induced institutions.

Our three-player repeated public goods game is implemented either as a *Single-* or as a *Multi-Phase Game*. Subjects in the *Multi-Phase Game* play a collective-choice and a contribution phase. In the collective-choice phase, subjects vote for a joint burden sharing rule in a repeated voting decision. In order to acknowledge different levels of sovereignty, we implement either unanimity or majority vote. Furthermore, we consider three different burden sharing rules: equal contributions from all group members to the public good (*eqcont*), equal payoffs among all group members (*eqpay*), and contributions to the public good that are proportional to the participants' initial endowment (*propcont*). If a burden sharing rule obtains a sufficient majority in the collective-choice phase, it is implemented in the contribution phase. In case a sufficient majority cannot be reached after three voting rounds, the agreement formation has failed and subjects end up in a *VCM*. In the *Single-Phase Game*, subjects only play the contribution phase and make their contributions to the public good according to a burden sharing scheme being exogenously predetermined. In our experimental setting, all rule-based contribution schemes are based on the principle of "the smallest common denominator" which ensures veto power for all negotiating parties and comprises two steps: First, all agents can propose a minimum group contribution level to the public good. Then, the minimum of all proposals is selected and allocated among subjects according to a (predefined) burden sharing rule.

Our findings indicate that most groups agree upon a common scheme, and consequently

avoid an uncoordinated action, given that preferences for the allocation schemes differ among agents. The results reveal both the opportunities and risks for burden sharing negotiations. In case an agreement is reached we find that average efficiency levels increase compared to an externally implemented *VCM*. If groups however fail to agree upon a common rule, cooperation collapses and efficiency levels decrease compared to a *VCM* being exogenously imposed. Most importantly, we observe that it is beneficial to provide endogenous institution formation on a joint burden sharing agreement at the aggregate level, i.e., including both groups that reach a consensus upon a joint burden sharing rule and groups that fail to agree upon a common scheme. On average, agents who face a voting decision receive higher payoffs than participants in the external implemented *VCM*.

The remainder of this paper is organized as follows: Section 2.2 provides an overview of the related literature. Section 2.3 describes the experimental design and derives our theoretical predictions. We present the experimental results in Section 2.4 and Section 2.5 concludes.

## **2.2 Related literature**

As soon as the provision of public goods involves heterogeneous agents, any voluntary contribution mechanism has to cope with burden sharing issues. By summarizing the literature on contribution patterns in heterogeneous public goods games, Ledyard (1995) and Zelmer (2003) reveal that heterogeneity lowers average contribution rates in public goods experiments. Fisher et al. (1995) find evidence that players with high marginal per capita returns contribute less in heterogeneous groups with agents receiving low benefits from the public good compared to a homogeneous setting. Tan (2008) reports that heterogeneity with respect to contribution costs lowers cooperative behavior. Ambiguous effects have been shown with respect to the effect of inequality of initial endowments, indicating both negative (e.g., Anderson et al. 2008; Cherry et al. 2005; van Dijk et al. 2002) and potential positive effects (e.g., Chan et al. 1996; 1999). Georgantzis and Proestakis (2011) show positive impacts of heterogeneity in lab endowments on contribution levels, at least as long as subjects are not aware of the real wealth inequality of the group members. By tying contribution behavior to social norms based on different fairness principles, Reuben and Riedl (2013) reveal that players contribute proportionally to their endowments if punishment is allowed in a setting with endowment heterogeneity.

There are two additional strands of experimental literature which are particularly important for our research questions. Recent papers study the effect of rule-based contribution schemes on the voluntary provision of public goods. In such allocation schemes,



players first agree upon a common group provision level of a public good which is based on a minimum contribution mechanism. The associated costs for providing the public good are then distributed among players according to a specific predetermined burden sharing rule. Orzen (2008) and Dannenberg et al. (2014) show the benefits of this contribution mechanism for groups of homogeneous players in contrast to a VCM. When players are identical, there is only one fair sharing norm which assigns the same burden to all agents. In contrast, different burden sharing rules - inspired by different fairness norms - may seem plausible as soon as agents are heterogeneous. Remarkably, a burden sharing rule that aims at equalizing payoffs leads to substantial cooperation gains for providing a public good that generates heterogeneous benefits to agents (Kesternich et al. 2014). Further experimental investigation, however, indicates that efficiency gains from these rule-based contribution schemes are limited in case of endowment heterogeneity and that the perception on various contribution norms is sensitive to the circumstances under which the different positions accrued (Kesternich et al. 2017). In all these experiments, the different allocation rules are exogenously imposed instead of being endogenously determined by the group itself. An external implementation is rarely a feasible option among sovereign agents, e.g., for the provision of international public goods, and moreover may not be desirable in many cases. Our experimental design is tied to a growing number of experiments on the emergence and effects of endogenous institutions in social dilemma situations. An important challenge always remains whether or not institutions can emerge endogenously within groups of sovereign agents with conflicting interests. For instance, Plott and Levine (1978) as well as Hoffman and Plott (1983) have already studied how agents with opposing interests form agreements, and they emphasize that particular procedures which limit or favor the accumulation of information are at least as important as the structure of preferences in determining outcomes. Experimental studies show that sanctioning or rewarding mechanisms are successfully formed in public good games and that this also affect the behavior of the respective agents (e.g., Sutter et al. 2010; Gürerck et al. 2014). Thus, the leading question concerning the effect of endogenously implemented institutions covers important aspects of procedural fairness: Individuals do not only care about outcomes, but also pay attention to the conditions and processes leading to these outcomes (e.g., Frey et al. 2004; Frey and Stutzer 2005). Many experiments focusing on the impact of endogenously implemented institutions on cooperation behavior tend to find a *democratic premium*: Democratically implemented institutions lead to more cooperative behavior when compared to the same institutions being implemented exogenously (e.g., Grimm and Mengel 2009; Sutter et al. 2010; Dal Bó et al. 2010; Gürerck et al. 2014). Dal Bó et al. (2010) attribute this observation to both

a self-selection and a democracy effect. While players who vote for a certain institution may differ from those who oppose it (self-selection effect), the democracy effect disentangles potential differences in the decision behavior between endogenously or exogenously determined settings. In the same manner, empirical studies indicate that stronger direct democratic participation rights are associated with an increase in cooperation and a decrease in, for instance, tax evasion (e.g., Pommerehne and Weck-Hannemann 1996; Frey 1998).

## 2.3 Experimental design and theoretical predictions

Participants in the *Multi-Phase Game* play a collective-choice and a contribution phase. In the collective-choice phase, subjects themselves determine the contribution scheme to be implemented in the contribution phase. Participants in the *Single-Phase Game* play only the contribution phase whereby the different contribution schemes are exogenously given.

### 2.3.1 Single-Phase Game

In the *Single-Phase Game*, the contribution phase contains one of four different allocation rules: three rule-based contribution schemes (*eqcont*, *eqpay*, and *propcont*) and a standard voluntary contribution mechanism (*VCM*). In all contribution schemes, the payoff to player  $i$ ,  $\pi_i$ , is determined by a linear public goods game and given by

$$\pi_i = e_i - q_i + bQ$$

where  $e_i$  marks the initial endowment,  $q_i$  the individual contribution to the public good,  $b$  the marginal per capita return from the public good for player  $i$ , and  $Q = \sum_{j=1}^n q_j$  the aggregated provision level. Players differ with respect to their initial endowment. Each group of  $n = 3$  players consists of one “low-type” player (*type20*) with an initial endowment of  $e_{type20} = 20$ , one “middle-type” player (*type30*) with  $e_{type30} = 30$ , and one “high-type” player (*type40*) with  $e_{type40} = 40$ . Thus, there is a total group endowment of  $E = \sum_{j=1}^n e_j = 90$ . The endowment as well as contribution levels and payoffs are expressed in LabDollar (LD). Finally, we assume the marginal benefit from the public good to be  $b = 0.6$  and the marginal costs for investing into the public good to be equal to one.

In the baseline (*VCM*), agents simultaneously determine their individual contributions to the public good  $q_i$ . Since their marginal costs from investing into the public good exceed their individual benefits, standard theory predicts full free riding and zero contributions for all players and individual payoffs of  $\pi_i = e_i \forall i$ .

Our rule-based contribution schemes comprise two stages: In the first stage, the minimum stage, all players simultaneously suggest a minimum group provision level  $Q_i^{min} \in [0, 90]$ .<sup>11</sup> The smallest suggested proposal consequently determines the lower bound for aggregated contributions to the public good in the second stage, i.e.,  $Q^{min} = \min_{j \in S} Q_j^{min}$  where  $S$  is the set of players in a group. In the individual contribution stage, the minimum individual contribution level,  $q_i^{min}$ , is derived from the binding group minimum provision level,  $Q^{min}$ , according to a specific burden sharing rule, i.e.,  $q_i^{min}(Q^{min})$ , whereby subjects have to contribute at least the minimum contribution level  $q_i^{min}$ , i.e.,  $q_i \geq q_i^{min}$ . However, players can exceed these minimum requirements and voluntarily contribute more. We cover three different rule-based contribution schemes:

*Equal-contribution scheme (eqcont)* - If a rule-based contribution scheme requires equal contributions from all players to the public good, the individual minimum contribution levels are given by

$$q_i^{min} = \frac{1}{n} Q^{min}$$

for sufficient low group provision levels. Thus, the binding minimum proposal,  $Q^{min}$ , is equally distributed across all group members. However, it should be taken into account that in all burden sharing schemes, it is obligatory that the desired aggregated provision level  $Q^{min}$  is provided by the group and that  $q_i^{min} \in [0, e_i]$ . Therefore, it appears that *eqcont* may require higher contributions from subjects with high endowments if the equal contribution rule would require contributions from low-type players to exceed their initial endowment. More formally, contributions of all group members are given by  $q_i^{min} = \frac{1}{n} Q^{min}$  for  $Q^{min} \in [0, 60]$ . If  $Q^{min} \in [60, 80]$ , minimum contributions of *type20* players are  $q_{type20}^{min} = e_{type20}$  and therefore lower than those of *type30* and *type40* players,  $q_{type30}^{min} = q_{type40}^{min} = \frac{1}{n-1} (Q^{min} - e_{type20})$ . Following this logic, if  $Q^{min} \in [80, 90]$ , minimum contributions are given by  $q_{type20}^{min} = e_{type20} < q_{type30}^{min} = e_{type30} < q_{type40}^{min} = \frac{1}{n-2} (Q^{min} - e_{type20} - e_{type30})$ .

In the *eqcont* scheme, *type20* and *type30* players have a weakly dominant strategy to suggest  $Q_i^{min} = E$ . For them, the marginal benefit from increasing their group's binding minimum contribution level exceeds its costs for all  $Q_i^{min} \in [0, E]$ . Choosing  $Q_i^{min} < E$  would either reduce their own payoff if they set the binding minimum, or would not change the payoff if their minimum proposal was not pivotal. Following this intuition, *type40* players have a weakly dominant strategy to choose  $Q_i^{min} = 78$  since their marginal payoff is only positive if  $Q^{min} \leq 80$ .<sup>12</sup> Since the smallest pro-

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<sup>11</sup>In our experiment, integer multiples of three are required for  $Q^{min}$ .

<sup>12</sup>Mathematically, the weakly dominant strategy of *type40* players is to choose  $Q^{min} = 80$ , but the min-

posal is binding and we assume agents to choose an equilibrium strategy which maximizes their payoff in all subgames, an equilibrium that comprises those strategies which are weakly dominant in all subgames, i.e., a subgame perfect equilibrium in weakly dominant strategies, is characterized by  $Q^{min} = 78$  with  $q_{type20}^{min} = 20$  and  $q_{type30}^{min} = q_{type40}^{min} = 29$ . Given that  $q_i = q_i^{min}$ , this results in individual payoffs of  $\pi_{type20} = 46.8$ ,  $\pi_{type30} = 47.8$  and  $\pi_{type40} = 57.8$  and, consequently, an aggregated group payoff of  $\Pi = 152.4$ .<sup>13</sup>

*Equal-payoff scheme (eqpay)* - If a rule-based contribution scheme aims at reaching equality in payoffs among all group members and if all types of players have different initial endowments but the same marginal benefits from the public good, equating  $\pi_{type20} = \pi_{type30} = \pi_{type40}$  and solving for  $q_i^{min}$  implies that

$$q_i^{min} = e_i - \frac{1}{n}(E - Q^{min})$$

for sufficiently high group provision levels. Note that in the *eqpay* scheme, the desired aggregated provision level  $Q^{min}$  may not entirely allow for a payoff equalization since we do not take a direct redistribution of initial endowments between group members into account and  $q_i^{min} \in [0, e_i]$ . This implies that payoff equalization among all group members could be achieved if  $Q^{min} \in [30, 90]$ . If  $Q^{min} < 30$ , payoffs are equalized as far as possible. More formally, if  $Q^{min} \in [9, 30]$ ,  $q_{type20}^{min} = 0$  and  $q_{type30}^{min} = q_{type40}^{min} = e_j - \frac{1}{n-1}(e_{type40} + e_{type30} - Q^{min})$ . If  $Q^{min} \in [0, 9]$ ,  $q_{type20}^{min} = q_{type30}^{min} = 0$  and  $q_{type40}^{min} = Q^{min}$ . For instance, if  $Q^{min} = 24$ , this scheme would require  $q_{type20}^{min} = 0$ ,  $q_{type30}^{min} = 7$  and  $q_{type40}^{min} = 17$ . But nevertheless, payoff equality is not reached due to the endowment heterogeneity. If  $q_i = q_i^{min}$  individual payoffs are then given by  $\pi_{type20} = 34.4$ ,  $\pi_{type30} = \pi_{type40} = 37.4$ .

In the *eqpay* scheme, we expect all players to suggest full contribution levels, i.e.,  $Q_i^{min} = E$ . *type20* and *type30* players have a weakly dominant strategy to suggest  $Q_i^{min} = E$ . By anticipating the weakly dominant strategy of *type20* and *type30* players, *type40* players also maximize their payoff by suggesting  $Q_i^{min} = E$ . This allocation would lead to individual minimum contributions of  $q_{type20}^{min} = e_{type20}$ ,  $q_{type30}^{min} = e_{type30}$  and  $q_{type40}^{min} = e_{type40}$ , given that  $q_i = q_i^{min}$  it determines equal individual payoffs for all group members of  $\pi_{type20} = \pi_{type30} = \pi_{type40} = 54$  and a group payoff of  $\Pi = 162$ .<sup>14</sup>

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imum group provision level has to be an integer multiple of three. Therefore, the weakly dominant strategy is to choose the greatest integer multiple of three below 80.

<sup>13</sup>An illustration is provided in Figure 2.5 in the appendix.

<sup>14</sup>The payoff function of *type40* players is not monotonically increasing in  $Q^{min}$ . Therefore, depending on their beliefs about the other players' proposals, the best response of *type40* players is to propose  $Q_{type40}^{min} = 0$  or  $Q_{type40}^{min} = E$ . We assume *type40* players to anticipate the weakly dominant strategies

Table 2.1: Theoretical predictions according standard preferences

	$Q^{min}$	$q_{type20}$	$q_{type30}$	$q_{type40}$	$\Pi$	$\pi_{type20}$	$\pi_{type30}$	$\pi_{type40}$
<i>VCM</i>	0	0	0	0	90	20	30	40
<i>eqcont</i>	78	20	29	29	152.4	46.8	47.8	57.8
<i>eqpay</i>	90	20	30	40	162	54	54	54
<i>propcont</i>	90	20	30	40	162	54	54	54

Note:  $Q^{min}$ : binding minimum group contribution level;  $q_{type20}$  ( $q_{type30}$ ) ( $q_{type40}$ ): contribution of type20 (type30) (type40) players;  $\Pi$ : group payoff;  $\pi_{type20}$  ( $\pi_{type30}$ ) ( $\pi_{type40}$ ): payoff of type20 (type30) (type40) players.

*Proportional-contribution scheme (propcont)* - If a rule-based contribution scheme requires individual contributions to be proportional to the players' initial endowment, individual minimum contribution levels are given by

$$q_i^{min} = \frac{e_i}{E} Q^{min}.$$

Thus, the binding minimum proposal,  $Q^{min}$ , is distributed among group members according to their proportion of the total endowment, implying that individual minimum contributions increase with players' initial endowments. In this case, all players have a weakly dominant strategy to suggest full contribution levels, i.e.,  $Q^{min} = E$ , since their marginal benefit from increasing the group's minimum contribution level exceeds its costs for all  $Q^{min} \in [0, E]$ . This allocation is a subgame perfect equilibrium in weakly dominant strategies and would lead to individual minimum contributions of  $q_{type20}^{min} = e_{type20}$ ,  $q_{type30}^{min} = e_{type30}$  and  $q_{type40}^{min} = e_{type40}$ . Furthermore, given that  $q_i = q_i^{min}$  individual payoffs are  $\pi_{type20} = \pi_{type30} = \pi_{type40} = 54$  and the aggregated group payoff is  $\Pi = 162$ . Thus, assuming that subjects behave according to standard theory and that they play their own minimum contribution level, i.e.,  $q_i = q_i^{min}$ , we can derive the following hypothesis 1 regarding groups' payoffs:

**Hypothesis 1: Aggregate payoffs**

$$\Pi_{eqpay} = \Pi_{propcont} > \Pi_{eqcont} > \Pi_{VCM} = 90.$$

We summarize the theoretical predictions for all contribution schemes in Table 2.1.

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of their group members and since their payoff is maximized by  $Q_{type40}^{min} = E$  then, we expect *type40* players to propose full contributions.

### 2.3.2 Multi-Phase Game

In the first stage of the *Multi-Phase Game*, the collective-choice phase, subjects anonymously vote for one of the proposed rule-based contribution schemes (*eqcont*, *eqpay*, and *propcont*). We investigate two different voting mechanisms, namely majority and unanimity rule voting, each comprising three rounds of voting. After each round of voting, all group members are informed about the voting behavior of their group members and on whether an agreement was reached. In order to adopt an agreement, a rule-based contribution scheme has to receive two out of three votes (majority rule voting) or three out of three votes (unanimity rule voting). If the sufficient majority is obtained, the corresponding burden sharing rule is subsequently implemented in all periods of the contribution phase. In each period of the contribution phase, all players have to suggest a minimum group provision level. The corresponding costs are distributed among the members according to the burden sharing rule chosen in the collective-choice phase. If a majority cannot be obtained in the collective-choice phase, participants play the *VCM*. We summarize our experimental design in Table 2.2.

According to standard preferences, we expect *type20* and *type30* players to vote for the *eqpay* and *propcont* schemes more frequently when compared to the *eqcont* scheme. This can be explained by their expected payoffs which are comparatively higher in the *eqpay* and *propcont* schemes (see Table 2.1). In contrast, for *type40* players, the highest expected payoff appears in the *eqcont* scheme. Therefore, we expect *type40* players to vote for the *eqcont* scheme more frequently when compared to the *eqpay* and *propcont* scheme, at least under majority voting. Even though the highest expected payoff for *type40* players can be obtained under the *eqcont* scheme, where these players determine the minimum contribution level ( $Q_{type40}^{min} = Q^{min} = 78$ ), *type40* players may adjust their voting behavior and more frequently opt for an *eqpay* or *propcont* scheme which are clearly preferred by *type20* and *type30* players in order to avoid ending up in a *VCM*. Since the *VCM* is expected to be payoff-dominated by all rule-based contribution schemes, we expect participants to prefer any rule-based contribution scheme over a *VCM* and that subjects try to agree upon a joint burden sharing agreement.<sup>15</sup>

Furthermore, we expect an agreement to be more easily achieved under majority than under unanimity rule voting, and that groups agree on the *eqpay* and *propcont* schemes more frequently compared to *eqcont*. This has a simple statistical reason. Assuming that individuals make a random choice, the probability of reaching an agreement after three periods would amount to 0.99 under majority and to 0.30 under anonymity.

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<sup>15</sup>In a situation where *type20* and *type30* players suggest *eqpay* under unanimity voting, the *VCM* might emerge as an equilibrium if *type40* players do not expect the two other types to follow their weakly dominant strategies. Consequently, the best response of *type40* players is to propose  $Q_{type40}^{min} = 0$ , which corresponds to the *VCM*.

Table 2.2: Experimental design

Voting	Contribution scheme	Phases	No. of subjects (groups)
<i>Single-Phase Game</i>			
-	VCM	Contribution	45 (15)
-	<i>eqpay</i>	Contribution	45 (15)
-	<i>eqcont</i>	Contribution	45 (15)
-	<i>propcont</i>	Contribution	48 (16)
<i>Multi-Phase Game</i>			
Majority	VCM, <i>eqpay</i> , <i>eqcont</i> or <i>propcont</i>	Collective-choice and contribution	87 (29)
Unanimity	VCM, <i>eqpay</i> , <i>eqcont</i> or <i>propcont</i>	Collective-choice and contribution	93 (31)

However, in case players vote according to standard preferences, the clear dominance of the majority rule in terms of agreement formation might be counteracted if *type40* players under unanimity vote more frequently for the *eqpay* or *propcont* rules, which are also preferred by *type20* or *type30* players.

By relaxing the assumptions of standard preferences and allowing for other-regarding preferences, it can be expected that *type40* players also vote for the *eqpay* scheme. One prominent theory of other-regarding preferences, which also allows simple utility calculations, is the inequality aversion model by Fehr and Schmidt (1999) (F&S). They formalize the idea of inequality aversion by adding a disutility from disadvantageous inequality (weighted by parameter  $\alpha_i$ ) and a disutility from advantageous inequality (weighted by parameter  $\beta_i$ ) to a standard linear utility function. F&S themselves present the mean values  $\bar{\beta} = 0.315$  and  $\bar{\alpha} = 0.85$  for the inequality aversion parameters, which they both derive from individual behavior in ultimatum games. Blanco et al. (2011) use modified ultimatum and dictator games in order to obtain similar inequality aversion parameters. In their distribution ( $n = 61$ ), 56% of all subjects can be characterized by  $\beta_i > 0.5$ , and 33% of the participants have  $\beta_i > 0.67$ . In our case, for  $\beta_i > 0.67$ , a *type40* player prefers *eqpay* over the alternative burden sharing rules. For *type30* and *type40* players with  $\beta_i \geq 0$  and  $\alpha_i \geq 0$ , *eqpay* at least weakly dominates the alternatives.<sup>16</sup> Therefore, we can summarize our expectations in the following hypothesis:

<sup>16</sup>A detail description is provided in Section 2.6.2 in the appendix.

### Hypothesis 2: Voting in the collective-choice phase of the *Multi-Phase Game*

- (a) According to standard preferences, *type20* and *type30* participants vote for the *eqpay* and *propcont* schemes more frequently than for the *eqcont* scheme. In contrast, *type40* players vote for the *eqcont* scheme more frequently.
- (b) Participants prefer any rule-based contribution scheme to a *VCM* and therefore shall try to reach an agreement. They agree on the *eqpay* or *propcont* scheme more frequently than on the *eqcont* scheme, and an agreement is more frequently observed under majority than under unanimity rule voting.
- (c) By allowing for other-regarding preferences, it can be expected that *type40* players vote for the *eqpay* scheme more frequently than for the *eqcont* and *propcont* schemes.

Furthermore, in our experiment, the collective-choice institution itself becomes a treatment variable for distinguishing whether a contribution scheme is implemented exogenously by the experimenter or endogenously by the voting of the subjects. In order to distinguish between exogenously and endogenously implemented contribution schemes, we add the prefix *ex-* (i.e., *ex-VCM*, *ex-eqcont*, *ex-eqpay*, and *ex-propcont*) for the exogenous case and the prefix *end-* (i.e., *end-VCM*, *end-eqcont*, *end-eqpay*, and *end-propcont*) for the endogenous case. From a standard theoretical point of view, there should be no difference in the behavior in exogenously and endogenously implemented contribution schemes. But as already discussed in the literature review in Section 2.2, previous experimental studies have shown that the choice of an institutional setting itself can affect the level of cooperation in social dilemma situations. We are interested whether the democratic premium persists in endogenously determined burden sharing mechanisms. In order to disentangle these alternative predictions, our hypothesis H3 is as follows:

### Hypothesis 3: Endogenous and exogenous choice of contribution schemes

$$\Pi_{ex-i} = \Pi_{end-i} \text{ whereby } i = VCM, eqcont, eqpay, propcont$$

#### 2.3.3 Laboratory protocol

The experiment was run in July 2013 at the MaXLab laboratory of the University of Magdeburg in Germany. We used ORSEE (Greiner 2015) for recruiting participants and z-Tree software (Fischbacher 2007) for programming. We recruited 363 students



from different disciplines. Each student took part in one of 16 sessions, each one comprising between 21 and 24 subjects. On average, a session lasted about 60 minutes. In each session, we randomly created up to eight groups of three players with different initial endowments. Moreover, each group consisted of one *type20*, *type30* and *type40* player. All players remained in the same group and maintained their type throughout the whole experiment. During the collective-choice phase, subjects received information on the type classifications of other players and the outcome of the voting within their group. During the contribution stage, information on individual contributions to the public good, payoffs and corresponding average values within the group were transmitted via screen. Participants were not aware of their exact partners and the experiment did not allow any direct communication between participants.

At the beginning, participants received a set of experimental instructions which included written descriptions, numerical examples, and control questions. Furthermore, participants could make use of a simulator on their screen in order to verify the numerical examples, to answer control questions and to simulate different contribution decisions.<sup>17</sup> A session of the *Single-Phase Game* comprised 12 periods of the public goods game, the first two periods being practice. Additionally, in the *Multi-Phase Game*, the collective-choice phase with a maximum of three voting rounds preceded the public goods game. At the end of each session, one non-practice period of the public goods game was randomly chosen in order to determine individual earnings. The exchange rate between Euro and LD was 1:3. On average, participants earned 14.70 Euro. No additional show-up fee was paid.

## 2.4 Results

Our experimental design enables us to analyze the performance of different burden sharing rules being endogenously implemented by agents themselves both to an exogenously implemented *VCM* and to a setting where allocation schemes are predetermined by an agenda setter. While agenda setting appears not to be feasible - or at least highly controversial - in many situations, an endogenous institution formation faces the risk of coordination failure, i.e., it includes the risk that an agreement cannot be reached and therefore, that no institutional framework will be implemented at all. Following our main research question, that is to analyze whether efficiency gains from exogenously implemented schemes persist in a setting with institution formation, the sequence discussing our experimental results explicitly focuses on the comparison of payoffs and does not always reflect the order within the experiment.

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<sup>17</sup>Instructions and screenshots are provided in the appendix.

Table 2.3: Implemented contribution schemes in the *Multi-Phase Game*

	Majority rule voting	Unanimity rule voting	Total
<i>VCM</i>	1 (3.5)	13 (41.9)	14 (23.3)
<i>eqcont</i>	3 (10.3)	2 (6.5)	5 (8.3)
<i>eqpay</i>	18 (62.1)	10 (32.3)	28 (46.7)
<i>propcont</i>	7 (24.1)	6 (19.4)	13 (21.7)
Total	29 (100)	31 (100)	60 (100)

Note: Contribution schemes chosen by a group serve as one observation. If a sufficient majority, two out of three (majority rule voting) or three out of three (unanimity rule voting), cannot be obtained after the third voting round, participants play the *VCM*. Percentages in parentheses.

In a first step, we study the voting behavior in the *Multi-Phase Game* (Section 2.4.1). Subsequently, we compare payoffs from endogenously implemented contribution schemes at the aggregate level (i.e., including both groups that either managed to reach an agreement or who failed to agree upon a joint burden sharing rule) to a predetermined *VCM* (Section 2.4.2). We further examine differences in payoff levels in exogenously and endogenously implemented burden sharing schemes. In order to further elaborate potential differences in payoff levels, we additionally discuss individual contribution decisions (Section 2.4.3) and how they are related to minimum group contribution levels (Section 2.4.4).

### 2.4.1 Voting behavior

Analyzing voting behavior in the *Multi-Phase Game* reveals that almost 4 out of 5 groups successfully implement a joint burden sharing scheme (see Table 2.3). Remarkably, we find that approximately one third of all agreements (21 groups, 35%) are already established in the first voting round. As expected in hypothesis hypothesis 2b, a common agreement is more easily obtained under majority (97%) than under unanimity rule voting (58%) ( $p < 0.01$ , Fisher's exact test).

**Observation 1:** Despite expected differences in preferences on how to share the costs of providing a public good the majority of all groups successfully manages to agree upon a common burden sharing rule. Approximately one third of them obtain an agreement already in the first voting round. The equal-payoff scheme is the most frequently chosen burden sharing mechanism. If participants however fail to reach an agreement in the first voting round, they opt for the equal-payoff scheme less frequently, but rather for a proportional contribution scheme in the remaining two voting rounds.

In accordance with our theoretical prediction formulated in hypothesis 2a, we indeed find the preferred allocation rule to be dependent on subjects' initial endowments (see Figure 2.1 ). By considering all three rounds of the collective-choice phase, we find that agents do not randomly choose one of the proposed rule-based contribution schemes. Instead, the probability of an agent to opt for the *eqpay* scheme is significantly decreasing in the initial endowment (*type20*: 71%, *type30*: 46%, *type40*: 20%) ( $p < 0.01$ , Binomial test). Furthermore, *type40* players vote for the *eqcont* scheme more often than other group members (*type20*: 9%, *type30*: 12%, *type40*: 50%) ( $p < 0.01$ , Binomial test). The *propcont* scheme is most frequently chosen by *type30* players (*type20*: 20%, *type30*: 42%, *type40*: 29%) ( $p < 0.05$ , Binomial test). We find that *eqpay* is the burden sharing rule which is most frequently chosen in a successful agreement, being implemented in nearly half of the cases ( $p < 0.01$ , Binomial test). The *propcont* scheme is taken by 22% and the *eqcont* scheme by 8% of all groups. Finally, 23% of all groups fail to reach consensus (see Table 2.3).

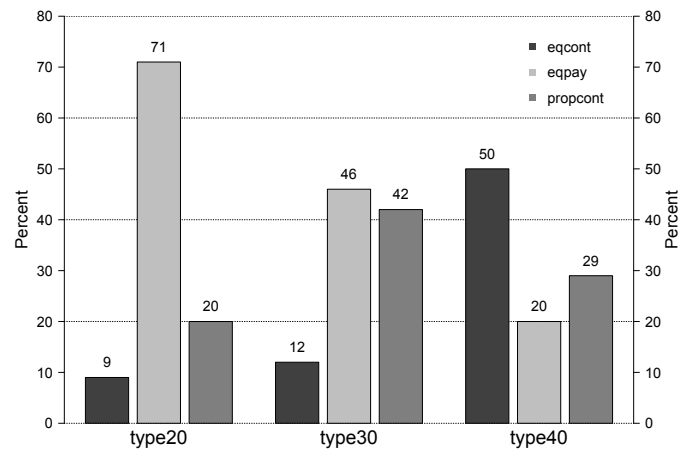
At first glance, standard theory of rational and selfish behavior performs considerably well when looking at subjects' voting behaviors. However, the dominance of the *eqpay* scheme, emerging in 62% of all cases under majority voting and still in 32% under unanimity voting (see Table 2.3), cannot be explained by standard theory but may rather coincide with other-regarding preferences. In particular, as indicated in hypothesis 2c, the decision behavior of *type40* players opting for *eqpay* may be driven by inequality aversion (e.g., F&S). If we assume that players prefer a burden sharing rule being at least at one minimum group contribution level payoff dominant and that players try to directly reduce inequality by implementing binding minimum contribution levels, i.e., not by implementing voluntary contributions exceeding the binding minimum contribution level, *type40* players prefer the *eqpay* to the *eqcont* as well as the *propcont* schemes for a sufficiently high level of disutility from advantageous inequality.<sup>18</sup>

Coordination failure could either reflect divergent preferences or, since direct communication was not allowed, a coordination problem, meaning that group members are per se willing to form an agreement but simply fail to vote for a certain burden sharing rule at the same time so that the required majority is not obtained. We find that agents change their voting behavior in case they fail to reach an agreement in the first round of the collective-choice phase. Especially under unanimity voting, *type20* play-

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<sup>18</sup>See Section 2.6.2 in the appendix for detailed description. We are aware that inequality might also be indirectly reduced by voluntarily exceeding the required individual minimum level. For instance, *type40* players might have an incentive to contribute more than required in order to reduce inequality. Anticipating this, *type20* and *type30* players might have an incentive to reduce the binding minimum contribution order to free ride on the voluntary contributions of the *type40* player. However, we focus on the case where players try to directly reduce inequality by implementing a binding minimum contribution level.

Figure 2.1: Individually chosen rule-based contribution schemes



Note: Individually chosen rule-based contribution scheme in all three rounds of the collective-choice phase of the *Multi-Phase Game* by types.

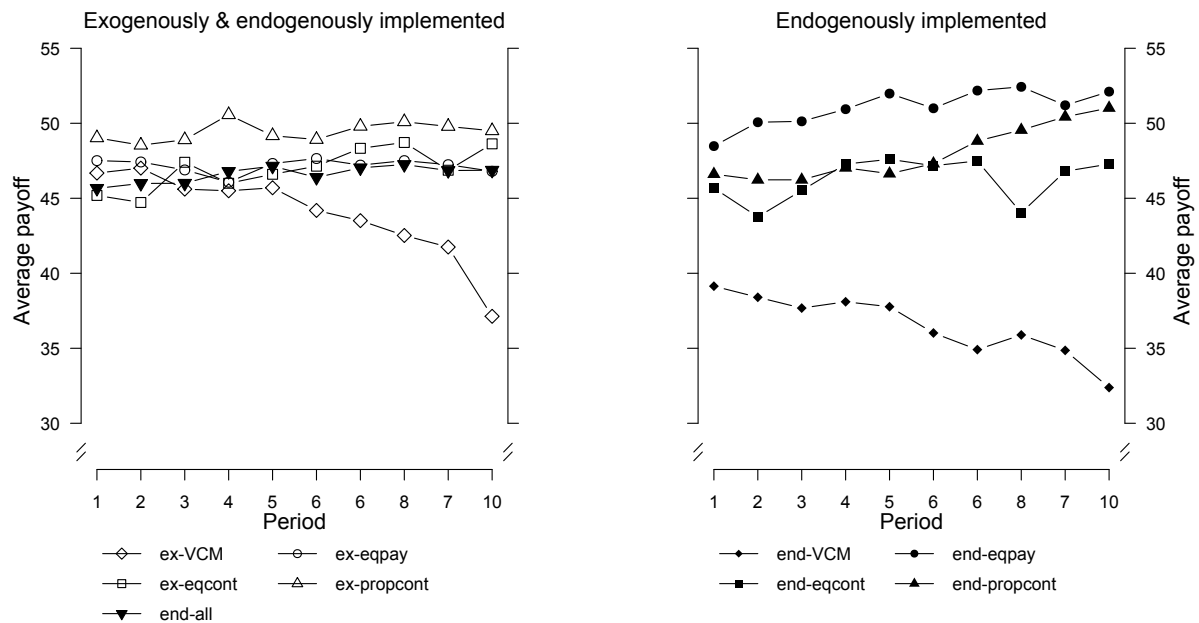
ers vote for the *eqpay* scheme less frequently and thus, more frequently for *propcont*. As discussed in our theoretical predictions, *type40* players seem to adjust their voting behavior under unanimity over time. While 61% of these players vote for the *eqcont* scheme in the first round, this share drops to 24% in the third round and the *propcont* scheme is selected in most cases by these types (57% of the votes). We do not observe this adjustment under majority voting. One possible explanation is that *type40* players become aware of their pivotal position under unanimity and accordingly adjust their voting behavior after the first round in order to avoid ending up in a *VCM*.

#### 2.4.2 Payoffs

Figure 2.2 illustrates average payoffs for each endogenously and exogenously implemented contribution scheme.<sup>19</sup> Across all periods, average aggregated payoffs for all groups who take part in the *Multi-Phase Game*, including those groups who fail to reach an agreement (*end-all*) amount to 46.6 LD. When considering all periods of the contribution phase, average payoff levels are not statistically different between the groups who take part in the *Multi-Phase Game* and those who participate in the exogenously implemented *VCM* (*end-all*: 46.6 vs. *ex-VCM*: 44.0, *p*-value: 0.128, Mann-Whitney U (MW-U) test). If we give groups more time for coordination and only consider the last five periods, the difference between the *Multi-Phase Game* (*end-all*: 46.9) and *ex-VCM* (41.8) becomes more pronounced and significant (*p*-value < 0.05, MW-U test). Moreover, aggregated payoffs under an endogenously induced institution formation (*end-all*) resemble the benefits from the different burden sharing rules obtained in the

<sup>19</sup>Detailed summary statistics are provided in Table 2.4

Figure 2.2: Payoffs by treatments



Note: Average payoffs in each period (excluding trial periods) of the game serve as one observation. On the left: payoffs in all exogenously implemented contribution schemes separately and the average of all endogenously implemented contribution schemes. On the right: payoffs in all endogenously implemented contribution schemes separately.

*Single-Phase Game* (*ex-eqcont*: 47.0, *ex-eqpay*: 47.2, *ex-propcont*: 49.4). Thus, enabling an endogenous institution formation on a joint burden sharing agreement is preferable to a situation where contributions to the public good are provided in a decentralized manner (*ex-VCM*). Moreover, institution formation is equally preferred to externally determined burden sharing rules. This latter observation may be driven by two important behavioral responses to the voting mechanism in our experiment.

**Observation 2:** Average payoffs under endogenous institution formation at an aggregate level (*end-all*), i.e., including those groups that did not reach an agreement, are higher than in a predetermined *VCM* in the last five periods. Compared to the benefits from the exogenously imposed burden sharing rules, endogenous institutions reveal similar payoffs at an aggregate level.

As discussed in Section 2.4.1, a broad majority of the groups manages to agree upon a joint distribution rule and individuals on average may simply contribute at similar rates in contrast to the exogenous case. Considering the endogeneity premium, we particularly investigate whether efficiency levels from those groups who reach consensus help to outweigh efficiency losses from coordination failure and if these efficiency gains

Table 2.4: Summary statistics - payoffs and contributions

	<i>end-all</i>	<i>ex-VCM</i>	<i>end-VCM</i>	<i>ex-eqcont</i>	<i>end-eqcont</i>	<i>ex-eqpay</i>	<i>end-eqpay</i>	<i>ex-propcont</i>	<i>end-propcont</i>
<b>All periods</b>									
$\Pi_{all}$	46.60	43.96	36.51	46.96	46.26	47.16	51.05	49.43	47.99
$\Pi_{type20}$	43.18	32.27	27.91	41.44	40.85	45.02	50.30	47.15	45.19
$\Pi_{type30}$	46.16	43.65	34.46	46.25	45.24	47.30	51.33	49.52	47.97
$\Pi_{type40}$	50.45	49.95	47.17	53.20	52.70	49.17	51.53	51.63	50.79
$q_{all}$	20.75	17.45	8.14	21.20	20.33	21.45	26.32	24.29	22.48
$q_{type20}$	14.16	13.13	6.74	16.73	15.74	13.60	17.07	16.58	15.27
$q_{type30}$	21.18	17.75	10.20	21.91	21.35	21.31	26.04	24.21	22.49
$q_{type40}$	26.89	21.46	7.49	24.96	23.89	29.45	35.84	32.09	29.68
<b>Last five periods</b>									
$\Pi_{all}$	46.89	41.82	34.81	47.94	46.56	47.28	51.78	49.62	49.43
$\Pi_{type20}$	43.59	35.75	25.40	42.91	41.45	44.57	51.26	47.46	47.48
$\Pi_{type30}$	46.64	41.06	33.35	46.74	45.23	47.67	52.24	49.67	49.45
$\Pi_{type40}$	50.40	48.65	45.68	54.17	52.99	49.61	51.86	51.73	51.35
$q_{all}$	21.10	14.77	6.01	22.42	20.69	21.60	27.23	24.53	24.28
$q_{type20}$	14.39	10.84	5.43	17.45	16.00	14.32	17.76	16.69	16.23
$q_{type30}$	21.33	15.53	7.47	23.62	23.40	21.21	26.78	24.48	24.26
$q_{type40}$	27.57	17.95	5.14	26.19	25.40	29.28	37.16	32.41	32.35

Note: Average payoffs and contributions both for all 10 periods (excluding trial periods) and the last five periods.  $\pi_{all}$ : average payoffs per group;  $\pi_{type20}$  ( $\pi_{type30}$ ) ( $\pi_{type40}$ ): averages payoffs of *type20* (*type30*) (*type40*) players;  $q_{all}$ : average contributions per group;  $q_{type20}$  ( $q_{type30}$ ) ( $q_{type40}$ ): average contributions of *type20* (*type30*) (*type40*) players. Exogenously implemented contribution schemes get the prefix *ex-* (i.e., *ex-VCM*, *ex-eqcont*, *ex-eqpay* and *ex-propcont*), endogenously implemented contribution schemes get the prefix *end-* (i.e., *end-all*, *end-VCM*, *end-eqcont*, *end-eqpay* and *end-propcont*).

differ across endogenously implemented burden sharing agreements. We study payoff levels in the different burden sharing schemes, both in the *Single-* and the *Multi-Phase Game*. We first summarize the results of the exogenously implemented contribution schemes and subsequently compare these findings to the agreement formation. Similar to previous findings on the minimum contribution mechanism (e.g., Kesternich et al. 2014), group payoffs are higher in rule-based contribution schemes than in the *ex-VCM* (44.0), if they are adopted by an agenda setter (see Figure 2.2 and Table 2.2). Averaged over all periods and agents, payoffs are highest in the *ex-propcont* setting (49.4). They amount to 47.0 in *ex-reqcont* and to 47.2 in *ex-reqpay*. Our regression analysis reveals that the exogenously implemented *propcont* burden sharing schemes increase efficiency as opposed to a *ex-VCM* ( $p$ -value  $< 0.05$ , Table 2.5 - Column 3).<sup>20</sup> Thus, the observed behavior is at least partly in accordance with hypothesis 1.

In case of an endogenous institution formation, substantial efficiency gains can be realized if the *end-reqpay* (51.1) scheme is implemented. Payoffs are lowest if groups fail to reach a burden sharing agreement and consequently have to play *end-VCM* (36.5) (see Figure 2.2 and Table 2.4). We find that the average payoffs per group are significantly higher in case one of the three rule-based contribution schemes (*reqcont*, *reqpay*, or *propcont*) is adopted than in case none of the schemes does receive a sufficient majority ( $p$ -value  $< 0.05$ , MW-U test (last five periods) and Table 2.5 - Column 1).

Furthermore, our regression analysis reveals that payoffs in endogenously implemented contribution schemes show different trends over time, depending on the contribution scheme implemented. We find decreasing payoffs in case coordination upon a joint burden sharing scheme has failed. In contrast, if one of the three proposed burden sharing rules is endogenously implemented, we observe positive trends over periods (Table 2.5 - Column 2). In addition, we find that *type40* and *type30* players receive significantly higher payoffs than *type20* players in all contribution schemes, either endogenously or exogenously implemented. However, these differences in payoffs are particularly pronounced in the *VCM*, and they are reduced in case a burden sharing rule is implemented (Table 2.5 - Columns 2 and 4).

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<sup>20</sup>A series of random-effects regression models is used in order to determine the payoffs of individuals. The discussion of the results is based on standard errors computed at group level. We consider individual level random effects, i.e., one observation of one individual corresponds to the panel variable and the period defines the time variable. All in all, our econometric analysis comprises 3630 observations. Due to missing sociodemographic information, we had to remove three out of the 366 participants from our economic analysis. Regarding the remaining 363 subjects, we have one observation for each of the 10 payoff-relevant periods.

Table 2.5: Random effects regressions of payoffs per subject

	1	2	3	4	5	6	7	8
	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$
Contribution scheme	all	all	all	all	VCM	eqcont	eqpay	propcont
Implementation	end	end	ex	ex	end & ex	end & ex	end & ex	end & ex
eqpay	14.09*** (1.61)	16.72*** (2.66)	3.53 (2.67)	2.79 (3.71)				
eqcont	9.09** (3.96)	8.69 (5.47)	3.25 (2.44)	-3.34 (4.03)				
propcont	11.28*** (2.40)	10.97**	5.51** (2.60)	4.05 (4.12)				
voting					-7.38*** (2.59)	-1.67 (4.00)	3.43* (1.86)	-1.47 (2.49)
period	0.14 (0.10)	-0.64*** (0.12)	-0.10 (0.13)	-0.88*** (0.17)	-0.77*** (0.11)	0.32** (0.13)	0.22 (0.15)	0.30** (0.15)
eqpay × period		0.97*** (0.18)		0.88** (0.37)				
eqcont × period		0.81*** (0.19)		1.25*** (0.24)				
propcont × period		1.20*** (0.24)		0.98*** (0.25)				
type30	3.13*** (0.75)	7.14*** (2.25)	3.90*** (0.66)	6.00*** (1.56)	6.01*** (1.48)	4.01*** (1.06)	1.78*** (0.65)	2.02** (0.93)
type40	7.38*** (1.15)	19.24*** (1.64)	7.84*** (1.12)	11.93*** (2.83)	15.32*** (1.72)	11.56*** (1.33)	2.35*** (0.68)	4.59*** (1.38)
eqpay × type30		-5.95*** (2.26)		-3.37* (1.88)				
eqpay × type40		-17.55*** (1.87)		-8.66*** (3.11)				
eqcont × type30		-3.26 (3.01)		-1.03 (1.81)				
eqcont × type40		-8.33** (3.39)		0.41 (3.11)				
propcont × type30		-4.71* (2.55)		-3.74* (1.93)				
propcont × type40		-13.92*** (2.47)		-8.05** (3.40)				

Note: Table 2.5 continues on the next page.



Table 2.5 (continued)

	1	2	3	4	5	6	7	8
	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$	$\pi_{it}$
Contribution scheme	all	all	all	all	VCM	eqcont	eqpay	propcont
Implementation	end	end	ex	ex	end & ex	end & ex	end & e	end & ex
male	2.45** (1.13)	1.63	1.30 (1.15)	1.73 (1.18)	0.37 (2.28)	2.36 (1.51)	1.82** (0.89)	2.38 (1.64)
exp	-0.03 (0.07)	-0.02 (0.07)	0.14* (0.07)	0.17** (0.07)	-0.03 (0.12)	0.10 (0.12)	0.11* (0.06)	0.09 (0.11)
Constant	31.52*** (1.93)	30.72***	38.54*** (2.53)	40.20*** (3.41)	41.16*** (3.34)	38.02***	42.74*** (2.04)	43.45*** (2.47)
Observations	1800	1800	1830	1830	870	600	1290	870
Number of Groups	180	180	183	183	87	60	129	87

Note: Standard errors in parentheses, adjusted for group clusters. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ .  $\pi_{it}$ : individual payoff of subject  $i$  in each period (excluding the trial periods);  $eqcont$  ( $eqpay$ ,  $propcont$ ) = 1 if subject  $i$  played  $eqcont$  ( $eqpay$ ,  $propcont$ ), 0 else; voting = 1 if subject  $i$  played the *Multi-Phase Game*, 0 else; period = period of contribution phase;  $type30$  ( $type40$ ) = 1 if subject  $i$  is  $type30$  ( $type40$ ), 0 else; male = 1 if subject  $i$  is male, 0 if female; exp = number of experiments subject  $i$  has taken part in MaXLab. Columns 1 and 2 consider observations from all endogenously implemented contribution schemes ( $end-VCM$ ,  $end-reqcont$ ,  $end-reqpay$ , and  $end-propcont$ ). Columns 3 and 4 consider all observations from exogenously implemented contribution schemes ( $ex-VCM$ ,  $ex-reqcont$ ,  $ex-reqpay$ , and  $ex-propcont$ ). Column 5 considers observations from the endogenously and exogenously implemented  $VCM$  ( $end-VCM$  and  $ex-VCM$ ). Column 6 considers observations from the endogenously and exogenously implemented  $reqcont$  ( $end-reqcont$  and  $ex-reqcont$ ). Column 7 considers observations from the endogenously and exogenously implemented  $reqpay$  ( $end-reqpay$  and  $ex-reqpay$ ). Column 8 considers observations from the endogenously and exogenously implemented  $propcont$  ( $end-propcont$  and  $ex-propcont$ ).

An endogenous institution formation affects payoff levels in the different burden sharing schemes. If participants decide to implement the  $reqpay$  scheme, payoffs are significantly higher than in the exogenously implemented counterpart ( $p$ -value  $< 0.05$ , MW-U test). With respect to the  $reqcont$  and  $propcont$  schemes, we cannot confirm that endogenously implemented institutions have an impact on efficiency levels. In contrast, we find that payoffs are significantly lower if no consensus is reached and players end up in an uncoordinated action than in a  $VCM$  being exogenously implemented ( $p$ -value  $< 0.05$ , MW-U test). Thus, hypothesis H3 has to be rejected. This observation emphasizes the risk of negotiation failures. The results of our regression analysis repeatedly confirm these findings (see Table 2.5 - Columns 5 to 7).

**Observation 3:** Average payoffs in an equal-payoff scheme are higher if this scheme is implemented by voting compared to a situation where it is exogenously implemented. If no consensus on a joint distribution rule is reached and agents end up in an uncoordinated action, they earn less than in the exogenously implemented VCM.

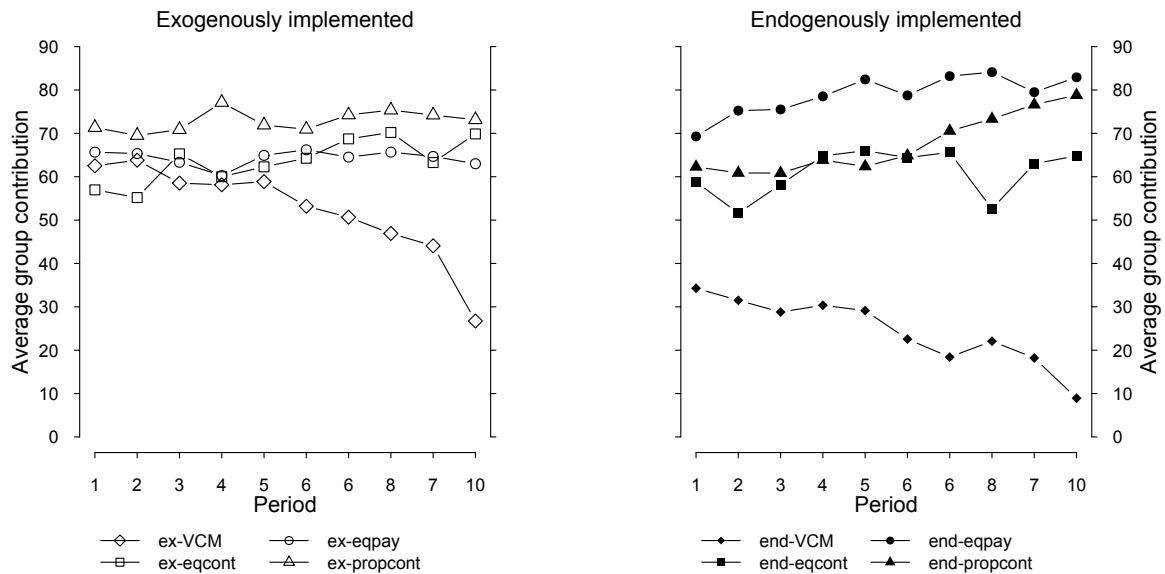
It could be argued that the differences in payoff levels between endogenously and exogenously implemented contribution schemes are driven by a self-selection effect described by Dal Bó et al. (2010). They show that cooperative subjects can separate themselves from uncooperative participants via voting and choosing treatments. We do not deny a possible self-selection effect in our experiment, but according to this argument, a sample selection effect should be relatively weak if groups already agreed on a common allocation rule in the first round of the collective-choice phase. By considering solely these groups and by focusing on the *eqpay* scheme, we find that voting still has a positive impact on payoff levels. Average payoffs in groups that agree on the *eqpay* scheme in the first voting round amount to 50.4 and are higher than in the exogenously implemented *eqpay* scheme (47.2). In this sense, we argue that our results are in line with the existing literature on the democratic premium and that they are at least partly driven by an effect of the endogenous choice rather than solely by a sample selection effect.

The allocation schemes clearly differ in the distribution of their benefits among the different players. Nevertheless, there is evidence that in case of exogenously implemented contribution schemes, the *propcont* scheme at least weakly payoff-dominates all other contribution schemes. Compared to the other contribution schemes, *type20* (47.2) and *type30* (49.5) players receive their highest payoff in *ex-propcont*. Even though *type40* players earn the most across all periods if all players have to contribute the same amount (53.2), this is not statistically different from their payoff in the *ex-propcont* scheme (51.6).

**Observation 4:** If an agenda setter determines the distribution rule exogenously, an allocation rule requesting proportional contributions from the different types of players leads to the highest average payoff per group across all periods and at least weakly payoff-dominates all other burden sharing rules. In case of an endogenous institution formation, groups benefit most if they agree upon an equal-payoff scheme.

Considering the endogenously implemented contribution schemes, the analysis reveals a different picture: The *eqpay* scheme leads to the highest average payoffs but is

Figure 2.3: Contributions by treatments



Note: Average group contributions in each period (excluding the trial periods) of the game serve as one observation. On the left (right): contributions in the exogenously (endogenously) implemented contribution schemes.

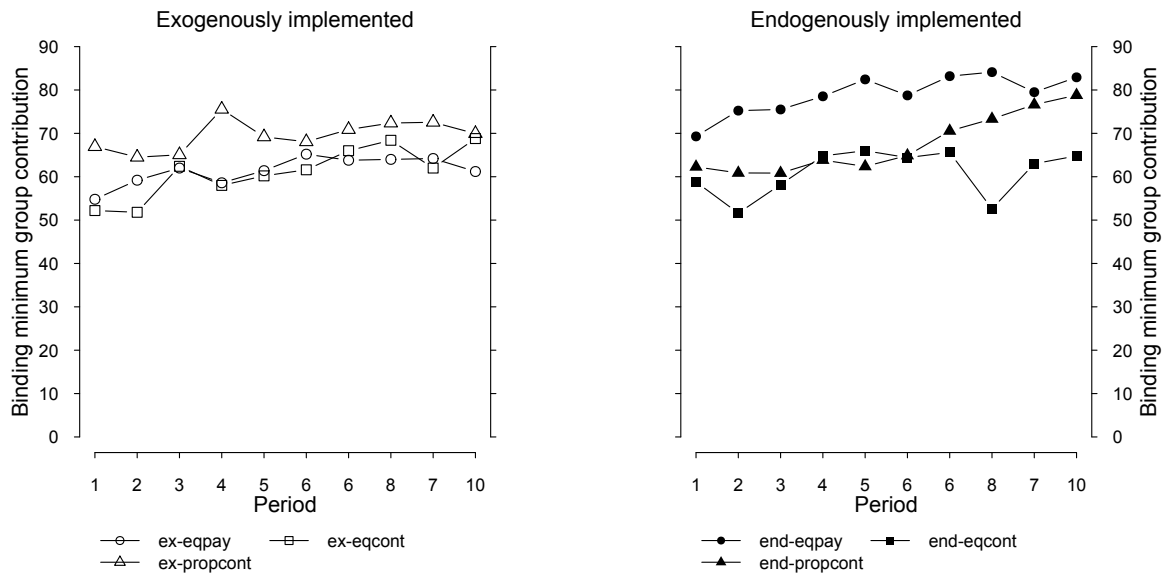
not payoff-dominant compared to the other contribution schemes. In addition, *type20* (50.3) and *type30* (51.3) players receive their highest payoff in *eqpay* compared to all other schemes. Once again, *type40* (52.7) players benefit most in the *eqcont* scheme. The difference between *eqcont* and *eqpay* (51.5) is significant for *type40* players ( $p$ -value  $< 0.1$ , MW-U test).

### 2.4.3 Contributions

In this part, we focus on contribution behavior in order to explore the differences in payoff levels in more detail. Figure 2.3 reports average group contribution levels for each either endogenously or exogenously implemented contribution scheme.

In the *Single-Phase Game*, if we focus on the last five periods, the contributions to the public good of all types of different players are higher in all burden sharing schemes than in the *VCM* (see Figure 2.3 and Table 2.4). As already discussed in the previous section, *ex-propcont* achieves the highest efficiency gains resulting from the highest average contributions (24.3). In this scheme, contributions from both *type30* (24.2) and *type40* players (32.1) are higher than in all other schemes. The contributions of *type20* players in *ex-propcont* (16.6) exceed those in *ex-eqpay* (13.6) but are marginally lower than in *ex-eqcont* (16.7). In the exogenous case, welfare gains in the *ex-propcont* scheme are particularly driven by increases in contributions from *type30* and *type40* players.

Figure 2.4: Binding minimum group contributions by treatments



Note: Average binding group minimum contribution levels in each period (excluding the trial periods) of the game serve as one observation. On the left (right): contributions in the exogenously (endogenously) implemented contribution schemes.

How are these findings related to the observations from endogenous institution formation? *propcont* achieves similar average contribution levels in the *Single-* and in the *Multi-Phase Game* (24.3 vs. 22.5,  $p$ -value: 0.397, MW-U test). This also applies to the *eqcont* scheme (21.2 vs. 20.3,  $p$ -value: 0.769, MW-U test). In contrast, average contribution levels increase in the *eqpay* scheme (21.5 vs. 26.3,  $p$ -value < 0.000) and collapse in the *VCM* (17.5 vs. 8.1,  $p$ -value < 0.000, MW-U test). In *eqpay*, all types of players contribute more to the public good than in the exogenous case if they agree upon this distribution rule. Moreover, total contributions are higher than in any other endogenously implemented burden sharing scheme.

#### 2.4.4 Minimum group contribution levels

We now examine binding minimum group contribution levels (see Figure 2.4 and Table 2.6) and individual minimum proposals in order to explore the voting effect of the *eqpay* scheme. As indicated by the contribution behavior of participants and their corresponding payoffs, we find that the average binding minimum group contribution level is higher in case the *eqpay* scheme is endogenously implemented (76.6) than in case the same scheme is exogenously implemented (61.4) ( $p$ -value < 0.1, MW-U test). In contrast, for *propcont*, we find that binding minimum group contribution levels tend to be higher if the rule is exogenously imposed (69.5 vs. 66.7), however the difference

Table 2.6: Summary statistics - binding minimum group contribution levels

	<i>ex-eqcont</i>	<i>end-eqcont</i>	<i>ex-eqpay</i>	<i>end-eqpay</i>	<i>ex-propcont</i>	<i>end-propcont</i>
All periods						
$Q^{min}$	61.14	60.66	61.44	76.55	69.51	66.67
$q_{type20}^{min}$	16.57	15.46	12.03	16.04	15.45	14.82
$q_{type30}^{min}$	21.25	21.31	20.31	25.39	23.17	22.22
$q_{type40}^{min}$	23.32	23.89	29.10	35.12	30.89	29.63
Last five periods						
$Q^{min}$	64.5	62.20	63.30	79.98	70.50	70.85
$q_{type20}^{min}$	17.24	15.37	13.12	17.06	15.67	15.74
$q_{type30}^{min}$	22.57	22.12	20.89	26.55	23.50	23.62
$q_{type40}^{min}$	24.68	24.71	29.29	36.38	31.33	33.49

Note: Average binding group and individual minimal contribution levels in for all 10 periods (excluding trial periods).  $Q^{min}$ : average binding minimum group contribution levels;  $q_{type20}^{min}$  ( $q_{type30}^{min}$ ) ( $q_{type40}^{min}$ ): average binding contribution levels of *type20* (*type30*) (*type40*) players. Exogenously implemented contribution schemes get the prefix *ex-* (i.e., *ex-eqcont*, *ex-eqpay* and *ex-propcont*), endogenously implemented contribution schemes get the prefix *end-* (i.e., *end-eqcont*, *end-eqpay* and *end-propcont*).

is not statistically significant ( $p$ -value: 0.839, MW-U test).

By focusing on groups who failed to coordinate at the entire contribution level, we find that *type40* players predominantly set the binding minimum group contribution level in *eqpay* (*ex-eqpay*: 41.8%, *end-eqpay*: 50.6%). More precisely, we find that *type40* players make the smallest proposals for the binding minimum group contribution level on average, independent from the method of implementation. In case the *eqpay* scheme is chosen in the collective-choice phase, they propose a binding contribution level of 81.2 on average. In case the scheme is implemented exogenously, they propose a contribution level which amounts to 73.9 on average. Moreover, *type30* players suggest an average binding contribution level of 83.6 in the endogenously implemented and 77.2 in the exogenously implemented case; *type20* players suggest amounts of 85.7 and 77.9, respectively. All types of players increase their average proposals in case the *eqpay* scheme is endogenously determined by contrast with the exogenously implemented *eqpay* scheme. In contrast, when looking at *propcont*, minimum contribution proposals are about five LD lower from each type of player under voting compared to the situation where an agenda setter implements the distribution rule. *type40* players propose a binding minimum contribution level of 73.6 on average (79.1 in *ex-propcont*), *type30* players suggest 77.1 (82.9 in *ex-propcont*) and *type20* players 74.2 (79.7 in *ex-propcont*). There is one speculative explanation for this observation, stating that groups need

more time for coordination in the endogenously implemented *propcont* scheme. If groups agree on an *eqpay* scheme, binding group contribution levels tend to increase immediately from the first period onwards. In contrast, when looking at the endogenously implemented *propcont* rule, group contribution levels are lower compared to *end-eqpay*. They remain rather constant in the first five periods, start to increase from the sixth period onwards and exceed group contribution levels determined by the exogenously implemented *propcont* rule only in the second half of the contribution phase (see Figure 2.4). One possible explanation could be that groups in the endogenously implemented *propcont* scheme are comparatively heterogeneous with respect to their preferred burden sharing rule. This occurs since the *propcont* scheme is most frequently implemented if coordination in the first rounds of the collective-choice phase fails. In this sense, we argue that the heterogeneity of groups in the endogenously implemented *propcont* scheme limits cooperation gains - at least in the first half of the contribution phase where players act rather cautious in order to learn more about their group members.

## 2.5 Summary and concluding remarks

In this experiment, we investigate whether the endogenous choice of different rules on how to share the costs of providing a public good affects individual contribution behavior. While agenda setting appears not to be feasible - or at least highly controversial - in many situations, endogenous institution formation faces the risk of coordination failure. At the aggregate level, i.e., including both groups that reach consensus upon a joint burden sharing rule and groups that fail to agree upon a common scheme, our results show that those participants who face a voting decision receive, on average, higher payoffs than participants in an exogenously implemented *VCM*. Moreover, they do not earn less than participants in any externally implemented burden sharing rule. This is driven by two important results. At first, despite different preferences for how to share the costs of providing a public good among heterogeneous agents, we find that groups manage to agree upon a common burden sharing rule in most cases. Second, endogenously and exogenously implemented rule-based contribution schemes are effective in increasing the level of contributions to the public good. While we observe this positive effect of an endogenous institutional choice on contributions to the public good, there is also a negative effect. If group members fail to agree upon a rule-based contribution scheme in the collective-choice phase of the *Multi-Phase Game*, they have to play the *VCM*. In this endogenously implemented case, contributions are lower than in case the *VCM* is exogenously implemented. Regarding the endogenously implemented *VCM*, one can speculate that group members are disappointed about the outcome

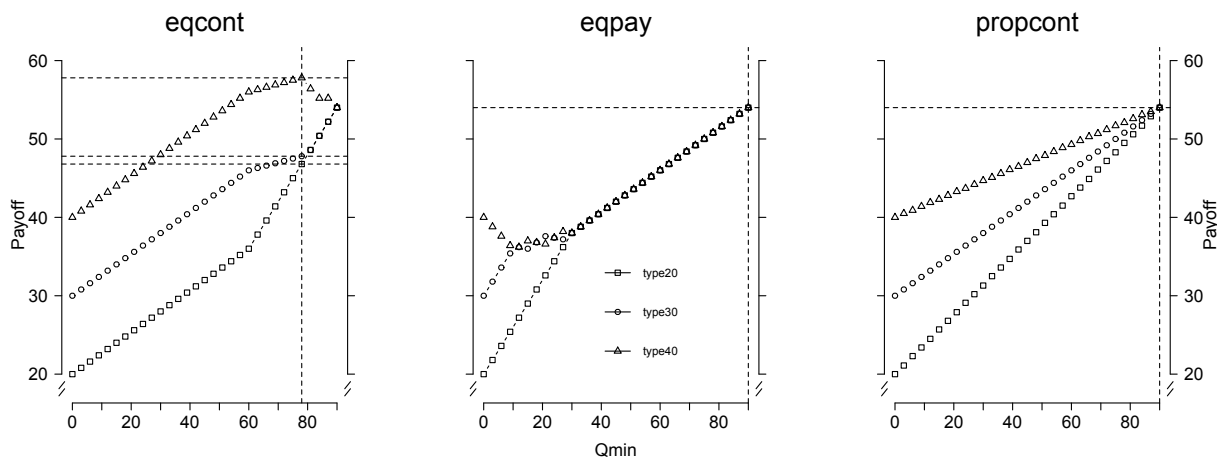
of the election and behave accordingly uncooperative. Nevertheless, the low contribution levels in this scheme may also be driven by a sample selection effect induced by the collective-choice phase of the *Multi-Phase Game*. In our experimental design, we cannot exclude that the endogenously implemented VCM “collects” all uncooperative participants who failed to implement a common rule-based contribution scheme. Therefore, our results indicate that an endogenously induced formation on a joint burden sharing agreement offers both chances and risks, depending on whether or not an agreement is reached. However, our results show that institution formation does not harm overall payoffs at the aggregated level. In particular, average payoffs are higher if agents face the opportunity to agree upon a joint burden sharing rule compared to an uncoordinated action. This observation may reveal interesting conclusions for a local agenda-setter as it shows that the benefits of the choice between different appropriate designed burden sharing rules do outweigh the risks.

As a matter of course, this implication has to be treated with caution because our experimental design shows at least one limitation since it automatically enforces the binding minimum contribution levels. Therefore, our design does not take into account that sovereign agents may deviate from initially accepted contribution patterns. However, any burden sharing mechanism in our experimental design only implements provision levels that all players have agreed upon previously. In addition, in the *Multi-Phase Game*, players perceiving to be “overruled” by their group members are always able to block any further cooperation by suggesting low minimum group contribution levels to the public good. The lowest common denominator principle therefore ensures that all subjects have veto power. Moreover, the perception on what constitutes a fair burden sharing norm may differ across countries and societies. Whether our results can be transferred to subjects with different cultural backgrounds remains an open question and deserves further research. In addition, to bring our experimental design closer to international negotiations with many players being involved, larger group sizes with players of different types might be taken into account in future research. Furthermore, one potentially meaningful extension would be to relax the assumption that all agents are fully informed about the cost and benefits from investing into the public good, i.e., the voting procedure could take place behind a “veil of ignorance” (Rawls 1971).

## 2.6 Appendix

### 2.6.1 Figures

Figure 2.5: Theoretical predictions according to standard preferences



Note: Binding minimum group contribution levels on x-axis and corresponding payoffs for each player per group (*type20*, *type30*, *type40*) on the y-axis. Players are assumed to choose and propose the minimum group contribution level which maximizes their payoff. Dotted lines indicate type-specific equilibrium quantities and corresponding payoffs.

### 2.6.2 Voting behavior and other regarding preferences

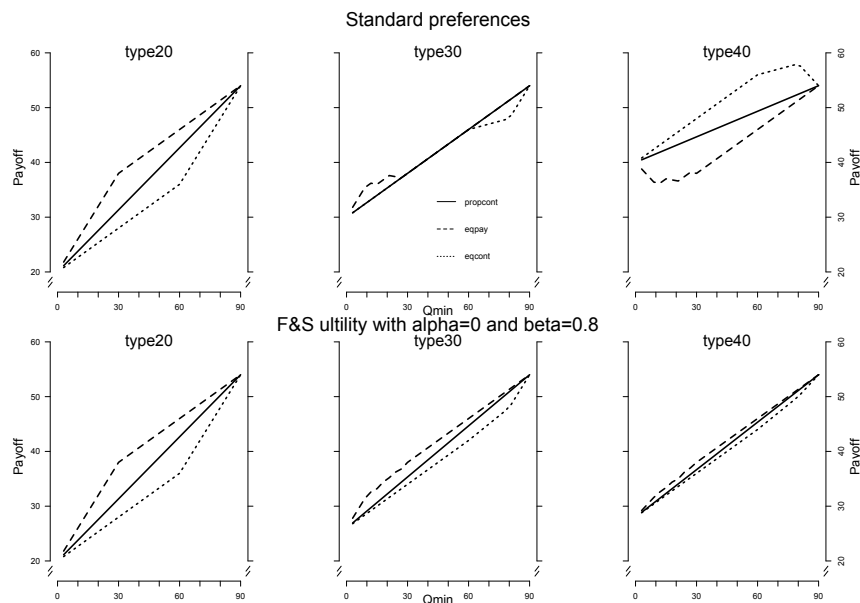
F&S introduce the idea that actors may be averse to inequality. They formalize the idea of inequality aversion by introducing the following utility for player  $i$ , given the payoffs for all other players  $j$ :

$$U_i(\pi_i, \pi_j) = \pi_i - \frac{\alpha_i}{N-1} \max\{\pi_j - \pi_i, 0\} - \frac{\beta_i}{N-1} \max\{\pi_i - \pi_j, 0\}.$$

For  $\alpha_i, \beta_i > 0$ , this implies that player  $i$  derives disutility from inequality. The second term in the utility function represents disutility from disadvantageous inequality (in case of  $\pi_j > \pi_i$ , weighted by  $\alpha_i$ ), while the third term reflects disutility from advantageous inequality (in case of  $\pi_i > \pi_j$ , weighted by  $\beta_i$ ). F&S assume  $\beta_i < 1$  and  $\alpha_i \geq \beta_i$ . By assuming that participants prefer the burden sharing rule which leads at least at one minimum group contribution level  $Q^{min} \in [0, 90]$  to a strict higher expected payoff in comparison to the other rules, the preference order for the different types of players with standard preferences is as follows:



Figure 2.6: Voting behavior according to standard and other-regarding preferences



Note: Binding minimum group contribution levels on x-axis and corresponding payoffs for each rule-based contribution scheme (*eqcont*, *eqpay* and *propcont*) the y-axis separately for *type20*, *type30* and *type40* players. Upper half (lower half): according to standard preferences (other-regarding preferences).

$$\begin{aligned}
 \text{type20} \quad \pi_{i,eqpay} &> \pi_{i,propcont} > \pi_{i,eqcont} \\
 \text{type30} \quad \pi_{i,eqpay} &> \pi_{i,propcont} > \pi_{i,eqcont} \\
 \text{type40} \quad \pi_{i,eqcont} &> \pi_{i,propcont} > \pi_{i,eqpay}.
 \end{aligned}$$

Assuming standard preferences, *type20* and *type30* players would, thus, vote for *eqpay* and *type40* players for *eqcont*. These are the predictions we derived in Section 2.3 although here we relaxed the assumption that participants take only equilibrium minimum contribution levels into account and assume that all possible minimum contribution levels  $Q^{min} \in [0,90]$  are considered. Figure 2.6 shows in the upper section the expected payoffs for the different types of players in the different burden sharing rules and illustrates the voting behavior. This picture changes if we allow for inequality aversion and compare F&S utility values. In particular, if we assume  $\beta_i = 0.67$ , *type40* players are indifferent between *eqpay* and the other schemes, and both other types strictly prefer *eqpay*. For values  $\beta_i > 0.67$ , all types of players strictly prefer *eqpay*. This holds independent from the value for  $\alpha_i$ . Figure 2.6 shows in the lower section the utility values for the different types of players in the different burden sharing rules by assuming a F&S utility function with  $\beta_i = 0.8$  and  $\alpha_i = 0$ .

### 2.6.3 Instructions

[Translated from German]<sup>†</sup>

#### **General information** [For all participants]

Please read the instructions carefully and contact us by opening the door or giving a hand signal if you have any questions. Please do not talk to each other and do not use any electronic devices such as mobile phones, smart phones, or the like throughout the whole experiment. In the experiment you are now taking part in, you can earn money depending on your decisions and those of your teammates. Your payoff from the experiment is calculated in LaborDollars (LD) and the exchange rate between € and LD is 1:3, i.e., 3 LD equals 1 €. During the experiment, you make your decisions **anonymously**. Only the experimenter will know your identity and your data will be treated confidentially. This experiment consists of two parts that will be carried out consecutively: (1) **voting** and (2) **game**. Please read the rules of the **game** in the following. After that, you will get details about the **voting procedure**.

#### **Rules of the Game**

**Three players** will take part in the game, i.e., apart from you, there are two other players. All in all, your group of three players has an initial endowment of 90 LD. One of the players is provided with an initial endowment of 20 LD (“type20” in the following). Another player (“type30”) is provided with an initial endowment of 30 LD and another one is provided with an initial endowment of 40 LD (“type40”). Whether you are type20, type30, or type40 will be **drawn by lot** and announced before the voting. Your task in the game (which is the same for your teammates in your group) is to decide what amount of LD you are willing to contribute to a **joint project**. Your **contribution,  $q$ , to the project** can range between 0 and 20 LD if you are **type20**. Your contribution can range between 0 and 30 LD if you are **type30** and between 0 and 40 LD if you are **type40**.

The individual payoff (in LD) for each one of the three players is derived as follows:

$$\text{Payoff} = (\text{initial endowment of player} - \text{contribution of player}) + 0.6 \cdot (\text{total sum of contributions by all players})$$

Assume you are **type20**, then your payoff (in LD) is:

$$\text{Payoff} = (20 - \text{your contribution}) + 0.6 \cdot (\text{total sum of contributions by all players})$$

That means, if, for example, the other two players contribute a total of 70 LD and you contribute 10 LD to the project, then your payoff is

$$\text{Payoff} = (20 - 10) + 0.6 \cdot (70 + 10) = 58$$

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<sup>†</sup>Explanatory notes are given in square brackets.

Whereas, if the other two players contribute a total of 70 LD and you contribute nothing, your payoff is

$$\text{Payoff} = (20 - 0) + 0.6 \cdot (70 + 0) = 62$$

If you are **type30**, your payoff (in LD) is:

$$\text{Payoff} = (30 - \text{your contribution}) + 0.6 \cdot (\text{total sum of contributions by all players})$$

If you are **type40**, your payoff (in LD) is:

$$\text{Payoff} = (40 - \text{your contribution}) + 0.6 \cdot (\text{total sum of contributions by all players})$$

**Multi-Phase Game** [Only for participants in the Multi-Phase Game]

The **game** has **two stages**. In **stage 1**, you decide which minimum contribution,  $Q_{min}$ , the group shall make. The other players in your group state as well which minimum contribution,  $Q_{min}$ , they would like to have for the group. The minimum of the suggestions,  $\min(Q_{min})$ , is set as the **minimum contribution of the group**. Then in **stage 2**, you decide about your contribution,  $q$ , to the project, whereby for every player a lower limit,  $q_{min}$ , for the individual contribution is calculated from  $\min(Q_{min})$  according to a specific rule. There are **three rules** to vote from:

**Rule 1 "Equal Contribution"**:  $q_{min}$  is determined from  $\min(Q_{min})$  so that the **minimum contributions**,  $q_{min}$ , of all players are as equal as possible\* so that every player contributes at least one third of the group's minimum contribution,  $\min(Q_{min})$ , i.e.,  $q_{min} = \frac{1}{3}\min(Q_{min})$ .

**Rule 2 "Equal Payoff"**:  $q_{min}$  is determined from  $\min(Q_{min})$  so that the **payoffs** off all players are equal or at least adjusted as far as possible.\*

**Rule 3 "Proportional Contribution"**:  $q_{min}$  is determined from  $\min(Q_{min})$  so that the **minimum contributions**,  $q_{min}$ , of all players are proportional to their initial endowment. I.e., the higher the initial endowment the higher is the minimum contribution,  $q_{min}$ , to the joint project by the player.

Examples for the rules 1-3 with a minimum contribution of the group  $\min(Q_{min}) = 45$ .

	Rule 1 "Equal Contribution"		Rule 2 "Equal Payoff"		Rule 3 "Proportional Contribution"	
	Lower limit of contribution	Payoff	Lower limit of contribution	Payoff	Lower limit of contribution	Payoff
Type20	15	32	5	42	10	37
Type30	15	42	15	42	15	42
Type40	15	52	25	42	20	47

Note: We assume each player chooses their lower limit as the contribution, i.e.,  $q = q_{min}$ .

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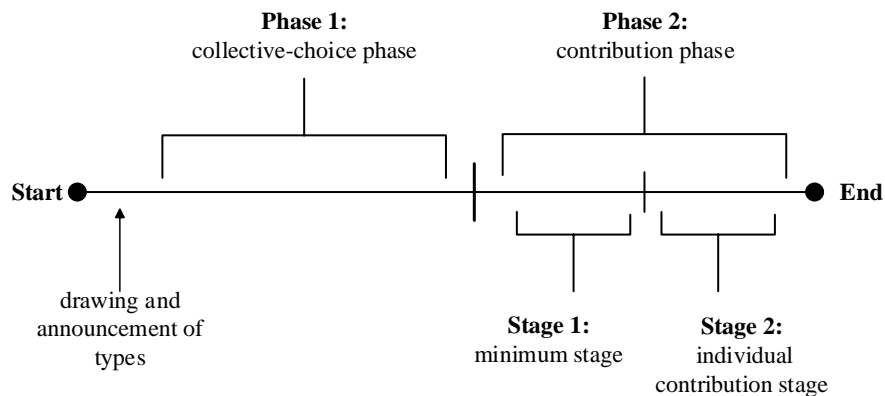
\*Please note that the adjustment is subject to the condition that the minimum contribution of the group to the joint project is reached.

Please use the **simulator** to understand the examples. On your screen you will find an Excel file named "**simulator**". You can enter your desired **minimum contribution of the group**,  $\min(Q_{min})$ , in the simulator. For each rule (rule 1 "Equal Contribution", rule 2 "Equal Payoff", rule 3 "Proportional Contribution") the individual minimum contributions,  $q_{min}$ , and the corresponding **payoffs** to each player as well as the payoff to the group are calculated. Please note that only the corresponding minimum contributions are calculated, i.e. the minimum contribution of the group,  $\min(Q_{min})$ , is distributed to the players according to the different rules. Of course, you can also contribute **more** than the calculated minimum contribution, but only as long as your contribution does not exceed your initial endowment. The game consists of **ten separate rounds**, in which you always play the same game, you remain the same type and you interact with the same two participants. In each round, you will be informed about the proposals of the minimum contribution ( $Q_{min1}$  to  $Q_{min3}$ ), the contributions ( $q_1$  to  $q_3$ ) and the payoffs ( $payoff_1$  to  $payoff_3$ ) of all players in your group as well as the average values (D). At the end of the experiment you will receive the **payoff of one of the ten rounds** in € (3 LD = 1 €). The round that will be disbursed is chosen **randomly**. Therefore, in **each** round, you should act as if it was relevant to disbursement. In the beginning, there will be **two trial rounds** which are **not relevant to disbursement**.

### Voting

The members of a group decide for themselves which rule will be applied for the distribution of the group's minimum contribution,  $\min(Q_{min})$ . The voting is a **majority vote** [alternative wording for unanimity rule voting: the voting is a **unanimity rule**] (between rule 1 "Equal Contribution", rule 2 "Equal Payoff" and rule 3 "Proportional Contribution"), i.e. if at least two of the [alternative wording for unanimity rule voting: if all of the] three group members vote for the same rule, it will be applied. There is a maximum of three voting rounds. If there is no rule that has received at least two of the three votes after the **third** voting, the game will be played **without stage 1**, i.e., no minimum contribution is determined and each player just states their contribution to the project and the payoffs will be determined as mentioned above. Illustration 1 shows course of the experiment in a nutshell.

Illustration 1:



**Control Questions (please answer, use the simulator if necessary)**

- Assume, the three players have stated 10, 20 and 30 respectively as the proposal for the minimum contribution. What is the group's minimum contribution  $\min(Q_{min})$ ? The group's minimum contribution,  $\min(Q_{min})$ , is: \_\_\_\_\_
- Assume, the group's minimum contribution,  $\min(Q_{min})$ , to the project is 30. What is your minimum contribution and payoff if you have agreed on the following rules, if all the players contribute their minimum contribution and you are **type20**? (Tip: Use the simulator)

	Rule 1 "Equal Contribution"	Rule 2 "Equal Payoff"	Rule 3 "Proportional Contribution"
My minimum contribution $q_{min}$			
My payoff			

- Assume, you could not agree on a rule in your group. Afterwards you make as a **type30** a contribution of 20 LD. The other two players contribute 0 LD and 10 LD. What is your payoff? My payoff is: \_\_\_\_\_
- We have the same situation as in 3.) and the other players in your group have contributed their whole initial endowment to the project. Which of the following contributions gives you the highest payoff as a **type40**? (please tick)  0 LD  10 LD  20 LD  40 LD
- We have the same situation as in 3.) and the other players in your group have contributed their whole initial endowment to the project. Which of the following contributions gives the group the highest payoff if you are a **type40**? (please tick)  0 LD  10 LD  20 LD  40 LD

**Single-Phase Game** [Only for participants in the Single-Phase Game]

[Only for participants in the exogenously implemented voluntary contribution mechanism (ex-VCM)]

In order to ease you the calculation, you will find an Excel file names “**simulator**” on your screen. For example, you can enter suggestions for your contribution and the contributions of the other group members into the simulator. The payoffs to each player as well as the payoff to the group are calculated.

[Only for participants in the exogenously implemented rule-based contribution schemes (ex-eqcont, ex-eqpay, ex-propcont)]

The **game** has **two stages**. In **stage 1**, you decide which minimum contribution,  $Q_{min}$ , the group shall make. The other players in your group state as well which minimum contribution,  $Q_{min}$ , they would like to have for the group. The minimum of the suggestions,  $min(Q_{min})$ , is set as the minimum contribution of the group. Then in **stage 2**, you decide about your contribution,  $q$ , to the project, whereby for every player a lower limit,  $q_{min}$ , for the individual contribution is calculated from  $min(Q_{min})$  according to a specific rule. The following rule will be applied:

[Only for participants in the exogenously implemented equal contribution rule (ex-eqcont)]

**Rule “Equal Contribution”**:  $q_{min}$  is determined from  $min(Q_{min})$  so that the minimum contributions,  $q_{min}$ , of all players are as equal as possible\* so that every player contributes at least one third of the group’s minimum contribution,  $min(Q_{min})$ , i.e.,  $q_{min} = \frac{1}{3}min(Q_{min})$ .

Examples for the rule “Equal Contribution” with a minimum contribution of the group  $min(Q_{min}) = 45$ .

	Rule “Equal Contribution”	
	Lower limit of contribution	Payoff
Type20	15	32
Type30	15	42
Type40	15	52

Note: We assume each player chooses their lower limit as the contribution, i.e,  $q = q_{min}$ .

[Only for participants in the exogenously implemented equal payoff rule (ex-eqpay)]

**Rule “Equal Payoff”**:  $q_{min}$  is determined from  $min(Q_{min})$  so that the payoffs off all players are equal or at least adjusted as far as possible.

Examples for the rule “Equal Payoff” with a minimum contribution of the group  $\min(Q_{min}) = 45$ .

	Rule “Equal Payoff”	
	Lower limit of contribution	Payoff
Type20	5	42
Type30	15	42
Type40	25	42

Note: We assume each player chooses their lower limit as the contribution, i.e,  $q = q_{min}$ .

[Only for participants in the exogenously implemented proportional contribution rule (ex-propcont)]

**Rule “Proportional Contribution”:**  $q_{min}$  is determined from  $\min(Q_{min})$  so that the minimum contributions,  $q_{min}$ , of all players are proportional to their initial endowment. I.e., the higher the initial endowment the higher is the minimum contribution,  $q_{min}$ , to the joint project by the player.

Examples for the rule “Proportional Contribution” with a minimum contribution of the group  $\min(Q_{min}) = 45$ .

	Rule “Proportional Contribution”	
	Lower limit of contribution	Payoff
Type20	5	42
Type30	15	42
Type40	25	42

Note: We assume each player chooses their lower limit as the contribution, i.e,  $q = q_{min}$ .

[Only for participants in the exogenously implemented rule-based contribution schemes (ex-eqcont, ex-eqpay, ex-propcont)]

Please use the **simulator** to understand the examples. On your screen you will find an Excel file named “simulator”. You can enter your desired **minimum contribution of the group**,  $\min(Q_{min})$ , in the simulator. For the rule “Equal Contribution” [alternative wording for ex-eqpay: “Equal Payoff”] [alternative wording for ex-propcont: “Proportional Contribution”] the individual minimum contributions,  $q_{min}$ , and the corresponding **payoffs** to each player as well as the payoff to the group are calculated. Please note that only the corresponding minimum contributions are calculated, i.e. the minimum contribution of the group,  $\min(Q_{min})$ , is distributed to the players according to the different rules. Of course, you can also contribute more than the calculated minimum

contribution, but only as long as your contribution does not exceed your initial endowment. The game consists of **ten separate rounds**, in which you always play the same game, you remain the same type and you interact with the same two participants.

*[Only for participants in the exogenously implemented voluntary contribution mechanism (ex-VCM)]*

In each round, you will be informed about the contributions ( $q_1$  to  $q_3$ ) and the payoffs ( $payoff_1$  to  $payoff_2$ ) of all players in your group as well as the average values (D).

*[Only for participants in the exogenously implemented rule-based contribution schemes (ex-eqcont, ex-eqpay, ex-propcont)]*

In each round, you will be informed about the proposals of the minimum contribution ( $Q_{min1}$  to  $Q_{min3}$ ), the contributions ( $q_1$  to  $q_3$ ) and the payoffs ( $payoff_1$  to  $payoff_3$ ) of all players in your group as well as the average values (D).

*[Only for participants in the Single-Phase Game]*

At the end of the experiment you will receive the **payoff of one of the ten rounds** in € (3 LD = 1 €). The round that will be disbursed is chosen randomly. Therefore, in each round, you should act as if it was relevant to disbursement. In the beginning, there will be **two trial rounds** which are **not relevant to disbursement**.

**Control Questions (please answer, use the simulator if necessary)** *[Only for participants in the Single-Phase Game]*

- Assume that your contribution as Type40 to the joint project is 20 LD. The contributions of the two other group members are 0 and 10 LD. What is your payoff?  
My payoff is \_\_\_\_\_
- Assume that your contribution as a Type20 to the joint project is 0 LD. The contributions of the two other group members are 10 and 20 LD. What is your payoff?  
My payoff is \_\_\_\_\_
- Assume all the other players in your group have contributed their whole initial endowment to the project. Which of the following contributions gives you the highest payoff as a type40? (please tick)  0 LD  10 LD  20 LD  40 LD
- Assume all the other players in your group have contributed their whole initial endowment to the project. Which of the following contributions gives the group the highest payoff if you are a type40? (please tick)  0 LD  10 LD  20 LD  40 LD

*[Only for participants in the exogenously implemented rule-based contribution schemes (ex-eqcont, ex-eqpay, ex-propcont)]*

- Assume, the three players have stated 10, 20 and 30 respectively as the proposal for the minimum contribution. What is the group's minimum contribution  $\min(Q_{min})$ ? The group's minimum contribution,  $\min(Q_{min})$ , is: \_\_\_\_\_



[Only for participants in the exogenously implemented equal contribution rule (ex-eqcont)]

- Assume, the group's minimum contribution,  $\min(Q_{min})$ , to the project is 30. What is your minimum contribution and payoff in the "Equal Contribution" rule, if all the players contribute their minimum contribution and you are **type20**? (Tip: Use the simulator)

	Rule "Equal Contribution"
My minimum contribution $q_{min}$	
My payoff	

[Only for participants in the exogenously implemented equal payoff rule (ex-eqpay)]

- Assume, the group's minimum contribution,  $\min(Q_{min})$ , to the project is 30. What is your minimum contribution and payoff in the "Equal Payoffs" rule, if all the players contribute their minimum contribution and you are **type20**? (Tip: Use the simulator)

	Rule "Equal Payoffs"
My minimum contribution $q_{min}$	
My payoff	

[Only for participants in the exogenously implemented proportional contribution rule (ex-propcont)]

- Assume, the group's minimum contribution,  $\min(Q_{min})$ , to the project is 30. What is your minimum contribution and payoff in the "Proportional Contributions" rule, if all the players contribute their minimum contribution and you are **type20**? (Tip: Use the simulator)

	Rule "Proportional Contributions"
My minimum contribution $q_{min}$	
My payoff	

**General information** *[For all participants]*

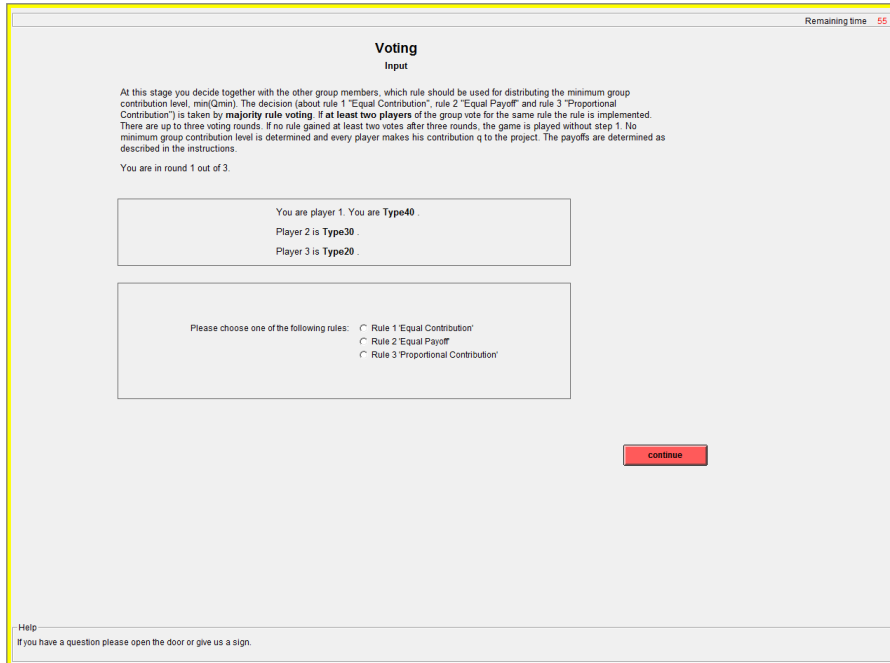
If you have answered all the questions, please give us a sign. We will then check your answers. The game will start (with explanations on the screen) when all participants have answered the control questions correctly.

**Good luck! The MaXLab-Team**

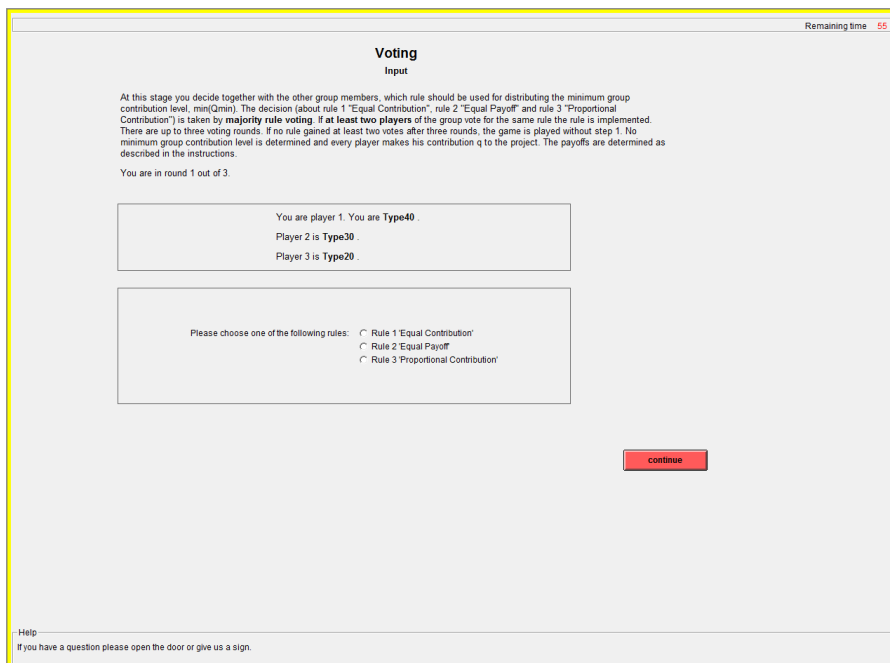
## 2.6.4 Screenshots

[Translated from German]<sup>†</sup>

Screenshot of the collective-choice phase of the *Multi-Phase Game* (majority rule voting)



Screenshot of the contribution phase of the *Multi-Phase Game* (eqcont)



<sup>†</sup>Explanatory notes are given in square brackets.

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### 3 Ratchet up or down? An experimental investigation of global public good provision in the United Nations Youth Associations Network

*A previous version of this chapter has appeared as:*

Gallier, C., M. Kesternich, A. Löschel and I. Waichman (2017). Ratchet up or down? An experimental investigation of global public good provision in the United Nations Youth Associations Network. ZEW Discussion Paper No. 17-071.

This article can be downloaded via  
<http://ftp.zew.de/pub/zew-docs/dp/dp17071.pdf>

**Abstract:** From a current perspective, the Paris Agreement is not sufficient to limit the global mean temperature to below 2°C above pre-industrial level as intended. The Agreement stipulates that parties review, compare, and ratchet up efforts to combat climate change over time. Within this process, commitments heavily depend on what has been already achieved and this status-quo represents an important reference point serving either as commitment advice or potential threat. We present an experimental study that is specifically designed to incorporate the effect of a status-quo via pre-existing contribution levels under endowment heterogeneity in a game, in which participants make voluntary contributions to a public good. Our participants are sampled from the United Nations Youth Associations Network, representing participants from 51 countries. Members from developed and developing countries take decisions against the background of different initial levels of endowments and pre-existing contributions. Our analysis indicates that starting with ambitious pre-existing contribution levels can foster aggregate mitigation levels. Falling behind status-quo contribution levels by reducing the public good appears to be a strong behavioral barrier. These observations might provide support for the basic structure of the Paris Agreement with Nationally Determined Contributions and the possibility to adjust them, even if a downward revision of national targets may not be precluded.

### 3.1 Introduction

During the United Nations Climate Change Conference (COP21) in 2015 and its resulting Paris Agreement, 195 countries agreed to set out a global action plan to avoid dangerous climate change by limiting global warming to well below 2°C above pre-industrial level. The agreement is based on “bottom-up” Nationally Determined Contributions (NDCs), which are voluntarily imposed, not legally binding and, finally, not enforceable.<sup>21</sup>

Recent assessments evaluating the effects of current mitigation policies raise doubts that countries are on track to meet the globally agreed target (UNFCCC 2015b). Consequently, as formulated already in Article 3 of the Paris Agreement, “[t]he efforts of all Parties will present a progression over time [...]” (UNFCCC 2015a) to ratcheting up and crank up ambitions over time. As a dynamic review mechanism, the global stocktake assesses the achievements of the parties under the Agreement and its long-term goals every five years, starting in 2023 (UNFCCC 2015a, Article 14).<sup>22</sup>

That is, countries will review, compare and potentially adjust their efforts periodically during the stocktake. In that respect, Article 4.11 states “A Party may at any time adjust its existing [NDC] with a view to enhancing its level of ambition, [...]” (UNFCCC 2015a). It shows that parties are not required to stick to a particular NDC once submitted but may replace existing policies with alternative approaches. The important point is that while a downward revision of existing NDCs is very likely to provoke stark criticism of the international community, in principal it remains a legally available option under the Paris Agreement (Center for Climate and Energy Solutions 2017). Countries’ future commitments will therefore depend on their individual assessment of what has been already achieved, both by themselves and the other countries. The status-quo of already achieved emission reductions may serve as a typical reference point, which may either advice countries to show commitment and strengthen their ambitions or appears as potential threat leading countries to reverse actions already undertaken.

In fact, again, while countries are encouraged to increase their ambitions during the stocktake, an adjustment of the NDCs to less ambitious emission reduction targets may also be possible. The decision of the current US government to withdraw from the

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<sup>21</sup>One of the core elements of the Agreement consists in encouraging parties to submit individual pledges for achieving substantial emission reductions at the national level to make efforts transparent and comparable. However, there is scepticism whether countries will deviate from their sovereign commitments (e.g., Barrett and Dannenberg 2016).

<sup>22</sup>The parties agreed on the global stocktake as central review mechanism to assess collective progress towards the globally agreed target. The global stocktake should provide countries with information for strengthening their ambitions and submitting new NDCs in the two years following the stocktake. In 2018, stock of international efforts is taken for the first time. The results of this assessment are then used to inform the decisions on more ambitious NDCs, which have to be submitted by 2020. This will be followed with a global stocktake every five years (Article 14).

Paris Agreement and to reverse policies initiated by the previous government serves as an example for the latter case. The question is then: ratchet up or down during the stocktake?

To address this question, we experimentally investigate the impact of the status-quo on future climate actions with the possibility of increasing and decreasing ambition levels under wealth heterogeneity, i.e., poor and rich countries, in the negotiations. Our experimental design builds upon the canonical public good games (e.g., Ledyard 1995; Chaudhuri 2011) to capture the tension between the individual interest to free ride on contributions of other group members and the joint interest to provide the public good, i.e., the global mitigation target, at a social optimal level. We thereby focus on individuals' pre-existing climate mitigation actions, which have been implemented exogenously. To capture the effect of the status-quo, participants in our experiment choose their actions against the backdrop of these pre-existing mitigation efforts.

Previous experimental studies devoted to the global climate tragedy account for pre-existing efforts by restricting the choice of players in a way that they can only add on top of what has been already achieved (e.g., Milinski et al. 2008; Tavoni et al. 2011; Barrett and Dannenberg 2012; 2014). By contrast, our experimental design allows for both increasing and decreasing ambition levels compared to the status-quo; most importantly players can undo existing efforts by taking pre-existing contributions from the public good. In case decisions are mutually exclusive (i.e., either only give or take from the status-quo), some studies find that such status-quo framing matters (e.g., Andreoni 1995; Park 2000; Khadjavi and Lange 2015; Gächter et al. 2017), while others do not observe that it changes behavior (e.g., van Dijk and Wilke 1997; Sell and Son 1997; Cubitt et al. 2011; Dufwenberg et al. 2011; Cox et al. 2013). A recent such study by Gächter et al. (2017) reveals that differences between giving and taking frames are associated with a decrease in reciprocity in take decisions and the mixed results can be traced back to individual differences in attitudes and beliefs. In a setting where the status-quo allows for both, ratcheting up and down pre-existing contributions, van Soest et al. (2016) find that cooperation collapses where participants can give and take compared to a pure giving frame, while Khadjavi and Lange (2015) do not observe differences. However, all these studies do not address heterogeneities between the parties, a crucial feature and a major source of ongoing controversies in climate change negotiations.

In our experiment we take differences in wealth into account and divide participants into two groups to reflect the persisting dichotomy between rich ("Annex-I") and poor ("Non-Annex-I") countries in current international climate policy.<sup>23</sup> Even though the

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<sup>23</sup>We are aware of one public good experiment with heterogeneous players extending the strategy set

Paris Agreement tries to avoid this bilateral structure, it continues to be of crucial importance to climate change negotiations (Tørstad and Sælen 2017). Building up on the reference point in a setting with an intermediate public good provision level compared to the standard giving setting with no taking option our experimental design thereby provides a more differentiated view on a potential contribution norm or burden sharing mechanism. Pre-existing provision levels thereby may either serve as coordination advice (since they might suggest a focal point for a possible contribution norm) or appear as a potential threat if parties undo climate action already implemented in the past, e.g., if they fear that other group members will contribute too little.

Another novelty of our study is that we investigate the impact of the status-quo on future climate actions for the case of poor and rich players and also capture the international dimension of global public goods (e.g., Barrett 2010) by collaborating with the United Nations Youth Associations Network (UNYANET). We recruited 139 individuals from 51 different countries. Wealth heterogeneity was implemented such that group members from a developing country start with a lower endowment than members from developed countries.<sup>24</sup>

Our results reveal that starting with pre-existing public goods provision levels and allowing both for giving and taking decreases contributions (i.e., additional efforts beyond the status-quo) compared to the standard giving frame. We find that both *poor* and *rich* agents contribute around 30% of their disposable endowment to the public good in both settings. However, reducing pre-existing contributions by taking out of the group account appears to be a strong behavioral barrier for the vast majority of participants. Consequently, in our setting, public goods provision is higher when starting with a pre-determined contribution level compared to a pure giving frame. While our experimental results cannot be generalized and directly inform international climate negotiations, we provide the important insight that the mere existence of a status-quo plays an important role for cooperation. The majority of individuals tend to abstain from exploiting or reversing existing public goods provision if they have the oppor-

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to taking (see McCarter et al. 2011). In case participants differ with respect to wealth, it is shown that highly endowed subjects are more likely to give to the public good, while low endowed subjects are more likely to take from the public good. While this study provides valuable first insights, the experiment does not include a pure giving frame and, therefore, does not allow for comparing potential differences in efficiency levels between these two different institutions.

<sup>24</sup>One might argue that the personal wealth status rather than the wealth status of the respective home country matters for the individual contribution decision in the public goods game. Since delegates in real negotiations typically are expected to represent their countries' views rather than their personal interests, we are confident that assigning players different roles based on the economic circumstances of their home country instead of considering personal wealth or income status provides a proper way to address our research question. Moreover, we control for the personal wealth status in our regression analysis without finding any significant impact of the personal income on the outcome variables.

tunity to do so. This observation might provide support for the basic structure of the Paris Agreement with Nationally Determined Contributions and the possibility to adjust them, even if a downward revision of national targets might not be precluded.

The remainder of this paper is organized as follows. Section 3.2 presents the experimental design and procedure of the study. Results are presented in Section 3.3. A concluding discussion is provided in Section 3.4.

### **3.2 Experimental design and procedure**

Our experiment is built upon a standard 4-player public goods game. To implement the status-quo with pre-existing contributions, we adopt the design developed by Khadjavi and Lange (2015) for the case of poor and rich parties. We distinguish between two treatment conditions: a pure giving condition (GIVE) without any pre-existing contributions and a condition allowing for both giving and taking of pre-existing efforts (GITA). In both conditions, each group consists of two *poor* and two *rich* agents. *Rich* agents receive an initial endowment of 30 tokens, while *poor* agents receive an initial endowment of 10 tokens. In the public goods game, every player has to decide how many tokens to keep for herself and how many tokens to contribute to the group account, reflecting the group climate change mitigation efforts. Each player earns 0.4 token for each token invested in the group account, regardless of whether she contributed to the group account. Under these parameters, it is in individuals' material self-interest to keep the entire endowment in their private account, however, from the group's perspective, it is socially optimal if the entire endowment of all group members is invested into the public account.

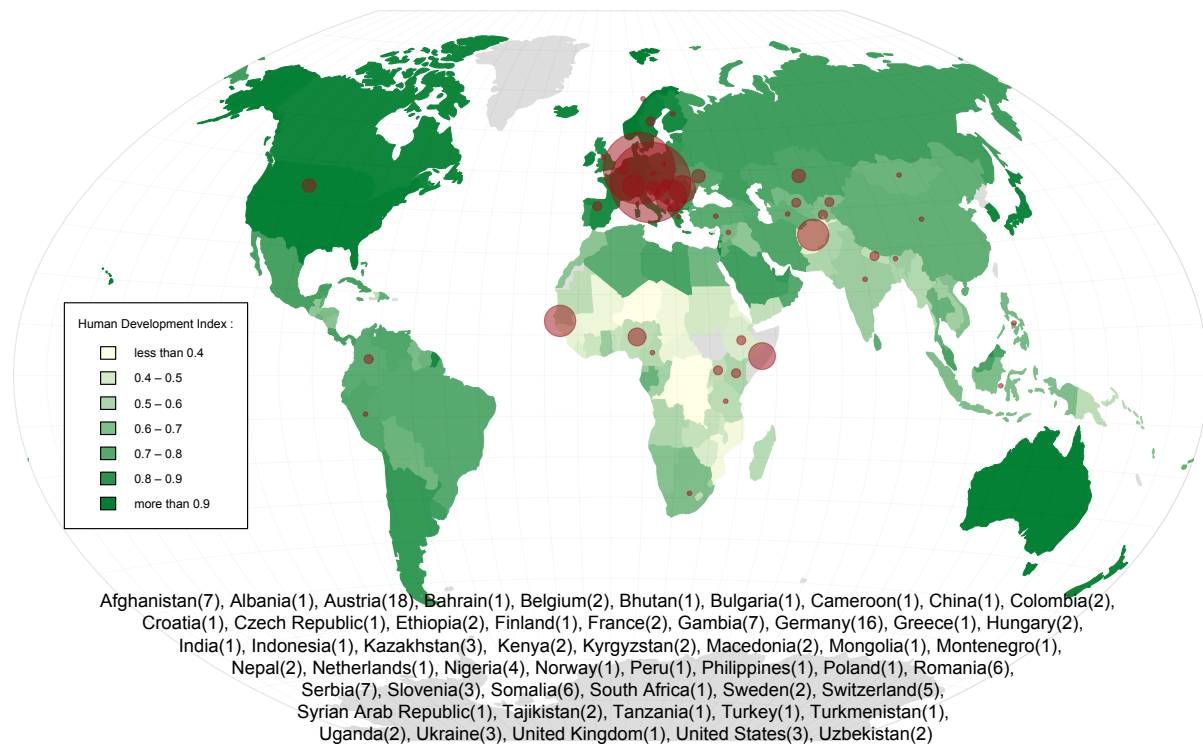
The only difference between GIVE and GITA is that in the first case the entire endowment is initially assigned to the individuals, and hence the status-quo public good provision is zero. By contrast, in GITA, 40% of the initial endowment (i.e., 12 tokens and 4 tokens for the *rich* and *poor* players, respectively) is initially allocated to the public good. Hence, in this treatment the status-quo of contributions is 40% of the individual initial endowment and players have the opportunity to increase contributions to the public good beyond the status-quo, to maintain the current levels, or even to undo efforts by taking existing contributions out of the public account.

Given our parameters and assuming standard preferences, the unique Nash equilibrium in dominant strategies is identical in both treatments: zero public good provision. In GIVE players should give nothing to the public good and, accordingly, in GITA agents are expected to undo existing efforts by taking all contributions out of the group account.

To capture the international dimension of the global climate tragedy, we conducted



Figure 3.1: Subject pool by country and Human Development Index



Note: Countries with a Human Development Index (HDI) of 0.7 and above are considered as countries with a high or very high human development (i.e., developed). Countries with a HDI below 0.7 are considered to have a medium or low development (i.e., developing). Red circles of different diameter indicate where participants come from. The larger the circles, the more participants come from the respective country.

the experiment in collaboration with the United Nations Youth Associations Network (UNYANET). The aim of UNYANET is to strengthen the collaboration between its members, the UN and further international organizations, e.g., by organizing Model United Nation (MUNs) sessions.<sup>25</sup> Individual members of the network were contacted by the national organizations via email, inviting them to take part in a scientific study on individual decision making conducted in collaboration with UNYANET. The email was distributed via UNYANET's official communication network. Participants were informed that they could earn an additional individual earning depending on their decisions and the decisions of their peers. In addition, participants knew that UNYANET receives a show-up fee of 2 US-Dollars for each participant. Subjects were informed that 10 percent of all participants were randomly selected after the experiment to receive their payment. We did not disclose any further information of the context or the aim of the experiment at this stage of the study.

As a first step, UNYANET members who followed the link in the invitation email had

<sup>25</sup>Further information on the UNYANET network are provided in the appendix.

to complete an online registration in order to be able to participate in the experiment. During the registration, participants had to provide socio-demographic information (age, sex, income, education, religion, nationality, and residence). This information was required to assign high and low endowments to participants from developed and developing countries, respectively.<sup>26</sup> After the registration, participants received an individual email with a unique access code and could take part in the online experiment. Conducting the experiment via the internet offered advantages for our study (e.g., Eckel and Wilson 2006; Horton et al. 2011). Most importantly, using the internet enabled us to conduct the experiment synchronously at different sites, which provides the opportunity of comparing individual decisions across regions and countries.

In total, 139 UNYANET members from 51 countries followed our invitation and took part in our online experiment. Figure 3.1 shows the distribution of participants according to countries and the respective economic development. Before entering their allocation decision, individuals were guided through detailed instructions on the procedure, and a set of numerical examples.<sup>27</sup> We use an ex-post protocol to match participants into groups of four players and to calculate group contribution levels and payoffs.<sup>28</sup> The average payment for the selected participants was a purchasing power equivalent to \$25 in the respective countries. After the allocation decision, we asked participants to answer questions thereby eliciting their attitudes, e.g., towards trust and climate change.

The characteristics of the participants from developed and developing countries are displayed in Table 3.1. In both samples the average age is 25, and around 50% are students (no significant differences between the samples). However, the share of female participants is lower in developing countries than in developed countries (36% vs. 53%,  $p$ -value: 0.057,  $\chi^2$ -test). In addition, participants from developed countries are more likely to define themselves as belonging to a lower income group within their home countries than participants from developing countries (3.14 vs. 2.52,  $p$ -value: 0.000, Mann–Whitney U test). As to personal attitudes, we find a considerably higher trust level (47% vs. 20%,  $p$ -value: 0.002,  $\chi^2$ -test) for participants from developed countries than for participants from developing countries (47% of the participants from developed countries agree with the statement that "most people can be trusted", but only 20% of all participants from developing countries). Moreover, it appears that participants from developing countries assign a higher weight to the need for fighting climate change than participants from developed countries (4.53 vs. 4.88,  $p$ -value: 0.004, Mann–Whitney U test). We take into account these individual characteristics

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<sup>26</sup>Details on the classification and assignment are provided in the appendix.

<sup>27</sup>We provide experimental details, invitation letters, instructions, and screenshots in the appendix.

<sup>28</sup>Details on the matching protocol are provided in the appendix.

Table 3.1: Sample characteristics

	Developed countries	Developing countries
Participants (in #)	89	50
Age (in years)	25.6	25.9
Female (in %)	52.8	36.0
Student (in %)	57.3	48.0
Income (category)	2.86	3.48
City (category)	3.1	2.82
Trust (in %)	47.1	20.0
Importance of climate protection (category)	4.53	4.88

Note: "Age" (in years); "Female" (female dummy); "Student" (student dummy); "Income": 1 indicates the lowest and 5 the highest income group in the participant's country, based on self-assessment; "City": grew up in big city (1) to rural village (4); "Trust": % of indicating "Most people can be trusted"; "Importance of climate change protection": importance of fighting climate change: not important (1) to very important (5).

and differences between the two subsamples, which might affect decision behavior, by adding them as explanatory variables within our regression analysis provided in the following section.

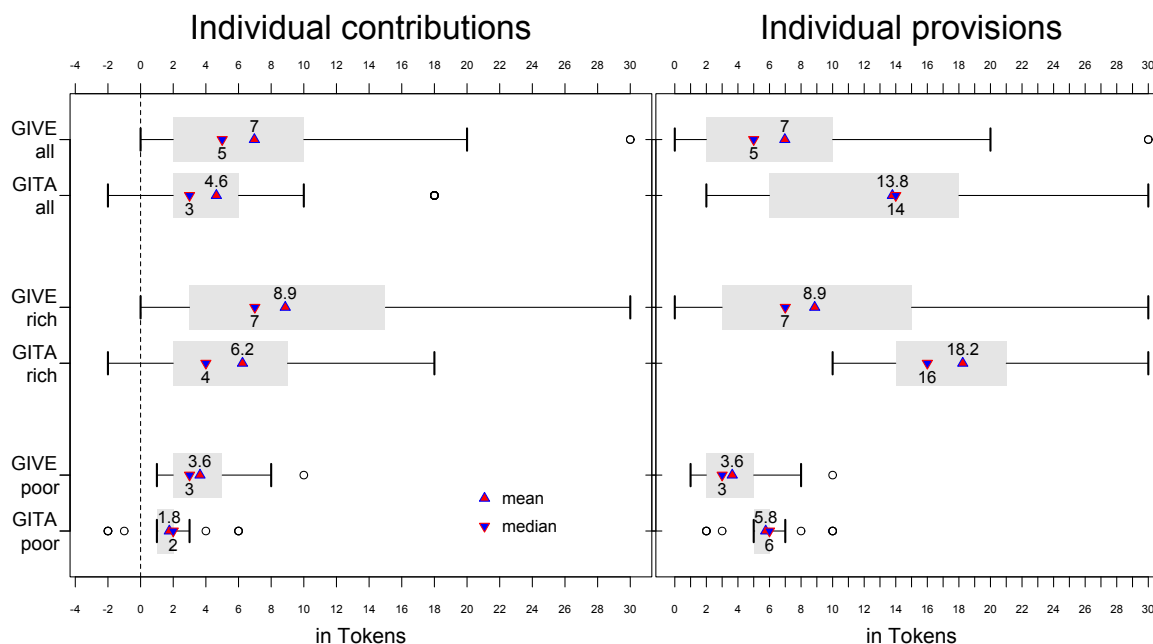
### 3.3 Results

Figure 3.2 illustrates our results for individual contributions (i.e., efforts beyond the status-quo) and individual provision levels (i.e., including both the status-quo and individual contributions) across treatment conditions.<sup>29</sup> We find that average individual contributions in GITA (4.6 tokens) fall below those in GIVE (7.0 tokens). Regression analyses of the data show that these differences are significant (Table 3.3 – column 1,  $p$ -value: 0.024). While participants contribute less in GITA, the vast majority abstains from undoing pre-existing contribution levels. Only 10% take out of the public good, which suggests that falling behind the status-quo appears to provoke a strong behavioral barrier in our experiment. Further evidence for this argument can be found by focusing on full free-riding behavior. In GITA, not a single individual fully free-rides. In GIVE, 4.4% of all participants act purely selfishly, i.e., zero contributions (4.35% vs. 0.00%,  $p$ -value: 0.120, Fisher's exact test).

By looking at the relative contributions (i.e., in percentage of disposable endowment),

<sup>29</sup>Detailed summary statistics are provided in Table 3.2.

Figure 3.2: Individual contributions and provisions by treatments and types



Note: Individual contributions (i.e., efforts beyond the status-quo) to the public good (left) and provision levels (i.e., including both the status-quo and individual contributions) (right) in absolute terms across treatments (GIVE vs. GITA) aggregated for both types of players (all) as well as separately for *rich* and *poor* players. The horizontal lines show the range of individual contributions (provisions). Outliers are presented separately. The ends of the “box” indicate the lower and upper values of the interquartile range, i.e., the middle 50 percent of the variable.

we find that participants contribute, on average, around one third of their disposable endowment to the climate account in both treatments GIVE and GITA (GITA: 32% vs. GIVE: 33%,  $p$ -value: 0.985, Mann-Whitney U test).

We now analyse the behavior of *rich* and *poor* players separately. Average contributions from *rich* players in GITA (6.2 tokens) fall below that in GIVE (8.9 tokens) ( $p$ -value: 0.071, Table 3.3 – column 2). Only 6.7% of the *rich* participants reduce existing climate mitigation efforts (see Table 3.1). In this line, neither the share of contributors (91.1% vs. 93.2%,  $p$ -value: 0.513, Fisher’s exact) nor the share of participants choosing the most selfish option (0% vs. 6.8%,  $p$ -value: 0.117, Fisher’s exact) differs significantly between GITA and GIVE. *Poor* participants decide to transfer only about half of the amount in GITA than in GIVE (1.8 vs. 3.6,  $p$ -value: 0.003, Table 3.3 – column 2). But also for them, the possibility to fall behind the pre-existing contribution level appears to be a strong behavioral barrier. 16% of the *poor* participants decide to undo pre-existing contribution levels. Neither the share of givers (100% vs. 84.0%,  $p$ -value: 0.110, Fisher’s exact) nor the share of strong free riders (0% vs. 0%) differ significantly across treatments for poor agents. Comparing the absolute reductions in individual contributions for both types between GITA and GIVE (*rich*: -1.9 vs. *poor*: -2.6) does not

Table 3.2: Summary statistics - individual contributions and provisions

	Developed ( <i>rich</i> )		Developing ( <i>poor</i> )	
	GIVE	GITA	GIVE	GITA
<b>Mean contribution level</b>	8.86	6.24	3.64	1.76
% of contributions < 0	-	6.67	-	16.00
% of contributions = 0	6.82	2.22	0.00	0.00
% of contributions > 0	93.18	91.11	100.00	84.00
% of endowment	29.53	20.80	36.40	17.60
% of disposable endowment	29.53	34.67	36.40	29.33
<b>Mean provision level</b>	8.86	18.24	3.64	5.76

Note: Individual contributions (i.e., efforts beyond the status-quo) to the public good (upper part) and provision levels (i.e., including both the status-quo and individual contributions) (lower part) in absolute terms across treatments (GIVE vs. GITA) separately for *rich* players from developed countries and *poor* players from developing countries.

reveal any significant differences in our sample (captured by interaction term GITA x Poor,  $p$ -value 0.739, Table 3.3 – column 2).

There is statistical evidence that higher trust levels increases giving, but this holds only for rich players ( $p$ -value: 0.001, Table 3.3 – column 3). The remaining control variables (age, female, student, income and city) do not significantly affect our results.

Even though GITA reduces individual contributions compared to GIVE (4.6 vs. 7.0,  $p$ -value: 0.024, Table 3.3 – column 1), average individual provision levels (i.e., including both the status-quo and individual contributions) in GITA are about twice as high as in GIVE (13.8 vs. 7.0,  $p$ -value: 0.000, Table 3.3 – column 4; Figure 3.2 – right-hand side). As discussed, participants in GITA hardly reduce their status-quo contributions. More precisely, in GIVE and in GITA *rich* (30% vs. 35%,  $p$ -value 0.406, Mann-Whitney U test) and *poor* (29% vs. 36%,  $p$ -value 0.411, Mann-Whitney U test) players contribute on average around 30% of their disposable income to the public good. Individual provision levels both for *rich* players (18.2 vs. 8.9,  $p$ -value 0.000, Table 3.3 – column 5) and *poor* players (5.8 vs. 3.6,  $p$ -value 0.000, Table 3.3 – column 5) are, therefore, higher in GITA than in GIVE.

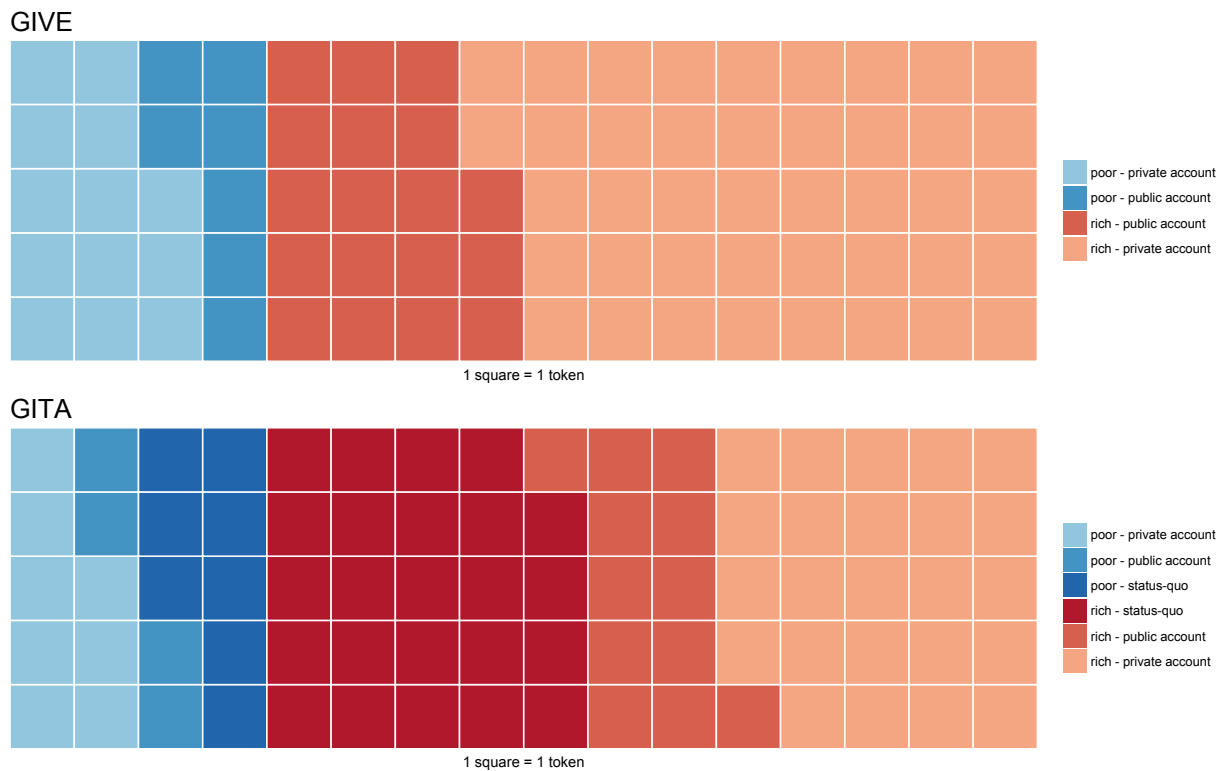
The private provision of public goods among heterogeneous agents always raises issues on a fair burden sharing, i.e., who should bear the costs of providing the public good? We now compare provision at the group level (see Figure 3.3). Groups in GIVE contribute on average 25 tokens to the public good. The two *poor* players per group contribute together 7 tokens and the two *rich* players contribute 18 tokens. Con-

Table 3.3: Individual contributions and provisions

	Individual contribution levels			Individual provision levels		
	(1)	(2)	(3)	(4)	(5)	(6)
GITA	-2.33** (1.02)	2.62* (1.44)	-2.74* (1.51)	6.81*** (1.22)	9.38*** (1.44)	9.26*** (1.51)
Poor		-5.22*** (1.23)	-4.02* (2.14)		-5.22*** (1.23)	-4.02* (2.14)
GITA x Poor		0.74 (1.57)	1.54 (1.65)		-7.26*** (1.57)	-6.46*** (1.65)
Trust			5.31*** (1.60)			5.31*** (1.60)
Trust x Poor			-5.11*** (1.73)			-5.11*** (1.73)
Climate			0.11 (1.04)			0.11 (1.04)
Climate x Poor			0.08 (1.35)			0.08 (1.35)
Age			0.04 (0.12)			0.04 (0.12)
Female			-0.39 (1.02)			-0.39 (1.02)
Student			-0.50 (1.29)			-0.50 (1.29)
Income			-0.87 (0.63)			-0.87 (0.63)
City			0.36 (0.49)			0.36 (0.49)
Constant	6.97*** (0.801)	8.86*** (1.14)	7.71 (4.76)	6.97*** (0.80)	8.86*** (1.14)	7.71 (4.76)
R-squared	0.037	0.184	0.334	0.184	0.520	0.603
AIC	895.234	887.155	768.232	945.941	876.155	768.234
Observations	139	139	122	139	139	122

Note: OLS regressions, with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Dependent variable are participants' individual contribution to the public good in Model (1) – (3) and the corresponding provision level in Model (4) – (6).

Figure 3.3: Public good provisions at group level by treatments and types



Note: Each square corresponds to one token contributed to the public good on average per group in GIVE (GITA) on top (below). Squares are highlighted in different colors for different participants (*poor* participants from developed countries in blue vs. *rich* participants from developing countries in red). In GITA, also initial contribution levels are highlighted separately.

sequently, nearly 28% of all contributions are provided by *poor* participants and the remaining 72% by their *rich* partners, which shows that participants coordinate on an equalizing redistribution that offsets the initial differences in endowments. This trend is even more pronounced in GITA, where two *rich* players contribute 12 tokens to the public good and the two *poor* players contribute 4 tokens. Taking into account the status-quo level (8 tokens for the *poor* and 24 tokens for the *rich* players), the overall provision level increases to 48 tokens per group. Therefore, 75% of the total public good level is provided by the *rich* agents. This is remarkable since *rich* players' status-quo contribution was already three times higher beforehand compared to the *poor* players' levels. We thus find that an institutional design which is build on pre-existing provision levels characterizing the status-quo – even though the taking option fully ensures cooperation to be voluntarily – generates higher benefits than a pure giving frame even if players differ with respect to their initial wealth.

### 3.4 Summary and concluding remarks

The Paris Agreement arguably gives new hope to climate change negotiations (e.g., Bodansky 2016) but from a current perspective, it seems insufficiently ambitious to limit the global mean temperature increase to 2°C (UNFCCC 2015b). According to Article 3 of the Agreement, the global efforts to combat climate change at a dangerous level will experience a progression over time. As a dynamic mechanism to review the voluntarily implemented NDCs of the different Parties, the global stocktake assesses the achievements of the international community every five years, starting in 2023 (UNFCCC 2015a, Article 14). As countries will review, compare and potentially adjust their efforts periodically during this process, their future commitments depend on what has been already achieved. This status-quo reflects an important reference point serving either as a commitment advice for strengthening the ambition or even revising it downwards.

We experimentally investigate the impact of the status-quo on future climate actions in a stylized negotiation setting reflecting the persisting dichotomy (Tørstad and Sælen 2017) between rich (“Annex-I”) and poor (“Non-Annex-I”) countries. Our results stress the importance of the status-quo for cooperative behavior. The analysis suggests that starting with pre-existing mitigation levels can foster aggregate mitigation levels. An important insight is that falling behind the pre-determined status-quo builds a strong behavioral barrier. The vast majority of players tends to abstain from reversing existing mitigation efforts. Of course, our experimental setup reflects only one particular aspect of the global stocktake as a review mechanism, namely its potential to affect players’ cooperative behavior via the status-quo. Low and highly endowed agents contribute around 30% of their disposable endowment to the climate account both in a setting with and without pre-determined contribution levels. Consequently, a status-quo comprising 40% of the initial wealth resulting in an increase in overall ambition levels. The possibility to revise contributions later might have led to more ambitious contribution levels initially (Center for Climate and Energy Solutions 2017). Given our results, the higher pre-determined contribution levels might indeed result in more climate mitigation efforts. The robustness of this finding against the background of different pre-existing contribution levels provides an avenue for future research.

Finally, whether the global stocktake process fosters the implementation of the Paris Agreement and increases overall climate actions will also depend on a variety of different factors, which we did not consider in our experiment including, e.g., strategic behavior over multiple periods, sequential decision making, and an open and non-anonymous discussion about targets and intentions. This provides an interesting and important route for further research.



## 3.5 Appendix

### 3.5.1 Experimental details

**Payoffs and treatments** The material incentive for each agent  $i$ ,  $\pi_i$ , is given by the following equation:

$$\pi_i = w_i^t - a_i^t + h \left( \sum_{j=1}^n e_j^t + \sum_{j=1}^n a_j^t \right).$$

Where  $w_i^t$  is  $i$ 's endowment in treatment  $t$ ,  $a_i^t \in A_i^t$  represents the transfer of  $i$  in treatment  $t$ ,  $A_i^t$  is the corresponding strategy set,  $h$  denotes the per capita return to the public good with  $0 < h < 1 < hn$  and  $e_i^t$  is  $i$ 's initial allocation to the public good in treatment  $t$ . We choose  $n = 4$  and  $h = 0.4$ . Agents differ with respect to their initial endowment. Each group consists of two *rich* agents and two *poor* agents. *Rich* agents receive an initial endowment of 30 tokens and *poor* agents, in contrast, receive an initial endowment of 10 tokens. Our experiment design consists of two treatments summarized in Table 3.4. Treatments differ in the initial allocation to the public good,  $e_i^t$ , the endowment  $w_i^t$ , and the corresponding action set available  $A_i^t$ . We use a pure giving frame (GIVE) as baseline. In this voluntary contribution mechanism, no initial allocations to the public good are made ( $e_{poor}^{GIVE} = e_{rich}^{GIVE} = 0$ ). Consequently, agents are endowed with  $w_{poor}^{GIVE} = 10$  and  $w_{rich}^{GIVE} = 30$  and have the following actions sets:  $a_{poor}^{GIVE} \in [0, 10]$  and  $a_{rich}^{GIVE} \in [0, 30]$ . In our second treatment, agents can either add to or subtract from the public good. In this giving and taking (GITA) frame, both *rich* and *poor* agents made an initial contribution of 40% of their endowment to the public good, i.e.,  $e_{poor}^{GITA} = 4$  and  $e_{rich}^{GITA} = 12$ . In GITA, the remaining endowments are given by  $w_{poor}^{GITA} = 6$  and  $w_{rich}^{GITA} = 18$  and, consequently, poor agents can decide on transfers between  $-4$  and  $6$  tokens ( $a_{poor}^{GITA} \in [-4, 6]$ ) and rich agents between  $-12$  and  $18$  tokens ( $a_{rich}^{GIVE} \in [-12, 18]$ ).

**Data collection and matching** Participants were recruited within the United Nations Youth Associations Network (UNYANET). UNYANET is the international umbrella organization of the United Nations (UN) Youth and Students associations with members from Europe, Asia, Africa and America. It was founded in 2011 and is seated in Geneva, Switzerland (for further information, see <http://unyanet.org/>). An invitation letter was distributed via the national contact points to the respective national members by email. Members who followed the link in the email first had to complete a pre-registration, providing some sociodemographic information (age, sex, income, education, religion, nationality, and residence). We used this information to

Table 3.4: Experimental design

	Endowment	Initial contribution	Action space
<b>GIVE</b>	poor $w_{poor}^{GIVE} = 10$	$e_{poor}^{GIVE} = 0$	$a_{poor}^{GIVE} \in [0, 10]$
	rich $w_{rich}^{GIVE} = 30$	$e_{rich}^{GIVE} = 0$	$a_{rich}^{GIVE} \in [0, 30]$
<b>GITA</b>	poor $w_{poor}^{GITA} = 10$	$e_{poor}^{GITA} = 4$	$a_{poor}^{GITA} \in [-4, 6]$
	rich $w_{rich}^{GITA} = 30$	$e_{rich}^{GITA} = 12$	$a_{rich}^{GITA} \in [-12, 18]$

Note: Groups consist of two *poor* and two *rich* agents each. *Poor* and *rich* agents receive an endowment of 10 and 30 tokens, respectively. In *GIVE*, no initial contributions to the public good are made. *Poor* agents can decide to transfer between 0 and 10 tokens. *Rich* agents can decide to transfer between 0 and 30 tokens. In *GITA*, *poor* and *rich* agents made an initial contribution to the public good of 4 and 12 tokens, respectively. *Poor* agents can decide to transfer between -4 and 6 tokens. *Rich* agents can decide to transfer between -12 and 18 tokens.

assign participants to the different subgroups, i.e., assigning low and high endowment to participants from developing and developed countries, respectively. After the pre-registration, members received an email with a unique access code to start the online experiment immediately. This procedure ensured that each participant could take part only once in the study. We used LimeSurvey, a free open source software tool (for further information, see <https://www.limesurvey.org>), for programming the pre-registration and the experiment. The design was optimized for either the use of a personal computer, a laptop, a tablet, or a smartphone. We used an ex-post protocol to match participants into groups of four players. Groups always consisted of two participants from developing countries and two participants from developed countries. Since our sample does not consist of participants from developing and developed countries in equal parts, we have re-matched participants to determine payoffs for all participants. Participants, also those who have been re-matched, have been paid once. For re-matched participants it was randomly determined which group levels were considered to calculate payoffs. All payments were adjusted for purchasing power parity.

**Assignment to treatments and classification of subjects** Based on the information from the pre-registration, we assigned low and high endowments to participants from developing and developed countries, respectively. The classification into developed

and developing countries was carried out using the Human Development Index (HDI). The HDI combines life expectancy, education, and per capita income indicators to rank countries according to their human development. Countries with a HDI of 0.7 and above are considered as countries with a high or very high human development. Countries with a HDI below 0.7 are considered to have a medium or low development. Consequently, participants from a country with a HDI below 0.7 are assigned to be *poor* and receive an initial endowment of 10 tokens. By contrast, participants from a country with a HDI of 0.7 and above are assigned to be *rich* and receive an initial endowment of 30 tokens. Afterwards, participants were randomly assigned to treatments (GIVE or GITA), which was done independently after the information in the pre-registration stage.

**Statistical analysis** In our one-shot experiment, we treated  $a_i^t$  (the transfer of individual  $i$  in treatment  $t$ ) as an independent observation. Assigning participants to treatments (GIVE or GITA) and types (*poor* or *rich*) results in the following number of observations: 69 observations in GIVE (44 *rich* and 25 *poor*) and 70 in GITA (45 *rich* and 25 *poor*). Statistical inference is based on linear regression models with robust standard errors and indicator variables for being assigned to GITA (GITA=1 if subject is assigned to GITA, 0 otherwise) and *poor* ( $poor = 1$  if subject is assigned to *poor*, 0 otherwise). Table 3.3 presents results from different model specifications.

## 3.5.2 Instructions

### Invitation letter (pre-registration) - First page

**ZEW**

Zentrum für Europäische  
Wirtschaftsforschung GmbH  
Centre for European  
Economic Research

Contact:

Carlo Gallier

Telefon +49 (0)621/1235-338

Telefax +49 (0)621/1235-226

E-Mail [onlinestudy@zew.de](mailto:onlinestudy@zew.de)

Mannheim, January 13, 2016

Distinguished delegate,

in cooperation with the United Nations Youth Associations Network (UNYANET), the Centre for European Economic Research (ZEW) is carrying out an

**online-study on individual decision making.**

Details concerning the research agenda and the results of the study will be circulated among all registered persons subsequently to the study.

**Why should I participate?**

The success of our study crucially depends on a high participation rate among UNYANET members from all over the world. The more delegates participate in the study, the better the insights we obtain from our analysis. Therefore, your personal participation in this survey is very important for the success of our research project.

Furthermore, you can **earn money**:

To support the activities of your network, UNYANET will receive **2 US-Dollars for each participant**.

Additionally, you can earn an **individual payment**. Whether you will get an individual payment and how much you will get depends on two factors: (i) Your decision in the study as well as the decisions of other participants. (ii) After all participants have made their decisions, we randomly select 10 percent of all participants who will receive their individual payment. These selected participants will receive an individual payment of about **50 US-Dollars** on average.

**How can I become a participant?**

In a first step we invite you to register at:

<http://www.zew.de/onlinestudy-registration>

using a computer or smartphone. Completing the **registration** will take no more than **5 minutes** of your time. Please note that we are looking for a balanced sample. **That means depending on the number of registrations, it might be the case that not all of the registered persons will be able to participate in the study in the end.** If you have been selected to participate in the study, you will receive your **personal log-in details within the next 14 days** via email. Otherwise all your data will be deleted subsequently to the study.

Centre for European Economic Research (ZEW) Mannheim  
Registered Office: Mannheim · Local Court Mannheim HRB 6554  
Chairwoman of the Supervisory Board: Minister Theresia Bauer MdL  
Executive Directors: Prof. Dr. Clemens Fuest, Thomas Koht

Baden-Württembergische Bank · BIC: SOLADEST600 · IBAN: DE04600501017496502782 · VAT-No. DE188318292

L 7, 1 · 68161 Mannheim · Germany

Mailing address:

P.O. Box 10 34 43 · 68034 Mannheim · Germany

Phone +49(0)621/1235-01, Fax -222

[www.zew.de](http://www.zew.de) · [www.zew.eu](http://www.zew.eu)

## Invitation letter (pre-registration) - Second page

### What happens to my personal data?

Your data will be used solely for scientific purposes and will be treated **anonymously as well as confidentially**. Only researchers of ZEW will know the participants' identity and data will be utilized in accordance with the personal data protection legislation.

### Who is behind this study?

The Centre for European Economic Research (ZEW) in Mannheim, Germany is a non-profit and independent research institute. Founded in 1990 on the basis of a public-private initiative in the Federal State of Baden-Württemberg in co-operation with the University of Mannheim, ZEW is one of Germany's leading economic research institutes, and enjoys a strong reputation in Europe. This online-study on individual decision making is part of a larger research project funded by the German Leibnitz Association. More information about ZEW is available at <http://www.zew.eu>.

### Do you have any questions left?

We are glad to help you with any questions or inconveniences. Do not hesitate to contact: [onlinestudy@zew.de](mailto:onlinestudy@zew.de). Please note that details concerning the research agenda and the results of the study will be distributed among all registered persons as soon as the study is completed.

Thank you, distinguished delegate, for your support of our research project!

Yours sincerely,

Carlo Gallier  
Dr. Martin Kesternich  
Prof. Dr. Andreas Löschel

Centre for European Economic Research (ZEW) Mannheim  
Registered Office: Mannheim · Local Court Mannheim HRB 6554  
Chairwoman of the Supervisory Board: Minister Theresia Bauer Mdl.  
Executive Directors: Prof. Dr. Clemens Fuest, Thomas Kohl

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L 7, 1 · 68161 Mannheim · Germany  
Mailing address:  
P.O. Box 10 34 43 · 68034 Mannheim · Germany  
Phone +49(0)621/1235-01, Fax -222  
[www.zew.de](http://www.zew.de) · [www.zew.eu](http://www.zew.eu)

## **Invitation email (experiment)**

Dear [NAME],

Many thanks for your interest in participating in our study. Please use the following link to start the study:

[www.zew.de/onlinestudy-2016](http://www.zew.de/onlinestudy-2016)

Your personal log-in details are: [CODE]

Please remember, this study is carried out by the Centre for European Economic Research (ZEW) in cooperation with the United Nations Youth Association Network (UNYANET). To support the activities of your network, UNYANET will receive 2 US-Dollar for each participant. Additionally, you can earn an individual payment. Whether you will get an individual payment and how much depends on your decisions in the study as well as on the decisions of other participants. Furthermore, after all participants have made their decisions, we randomly select 10 percent of all participants who will receive the individual payment. These selected participants will receive an individual payment of about 50 US-Dollar (adjusted for purchasing power parity) on average.

If you have any question, do not hesitate to contact us ([onlinestudy@zew.de](mailto:onlinestudy@zew.de)).

Yours sincerely,

Carlo Gallier

Dr. Martin Kesternich

Prof. Dr. Andreas Löschel

### **Instructions and questionnaire**

*[Brackets are used to indicate the alternative wording [DEVELOPED vs. DEVELOPING] and parameters [GIVE vs. GITA] used in the different treatments.]<sup>†</sup>*

#### **- Screen 1 -**

### **Welcome!**

Dear [Name],

Thank you very much for your interest in contributing to our research. You can start the Online-Study immediately. Prior to participating, please read through the most important information briefly summarized here:

- Participation will take no more than 10 minutes of your time.
- The study consists of both a decision task and a questionnaire.
- You will receive detailed information on the decision task during the study.
- All explanations are carried out as described:
  - By "money" we mean real amounts of money which will be paid out.
  - By other "participants" we mean real members of the UNYANET network who participate in this study just like you.

Note: Please always use the provided buttons and not your internet browser for navigation. Otherwise a successful completion of the study cannot be guaranteed.

Please click "Next" to learn more about the expense allowance.

#### **- Screen 2 -**

**Your expense allowance consists of two parts:** For each complete participation, UNYANET receives a fixed amount of 2 US-Dollars. Additionally, an individual payment can be obtained in the decision task. Whether you will receive the individual payment and how much you will get, depends on two factors:

1. Your decision in the study as well as the decisions of other participants.
2. After all participants have made their decisions, we randomly select 10 percent of all participants who will receive their individual payment.

Please click "Next" to learn more about the expense allowance.

#### **- Screen 3 -**

We now start the explanation and the procedure of the decision task. Please carefully read through the following instructions.

### **Explanation and procedure of the decision task**

---

<sup>†</sup>Explanatory notes are given in square brackets.

In this section, you have the chance to determine your individual payment.

**Who is involved in the study?** We create groups of four participants; in addition to you there are three other participants in your group. Members of your group are from a variety of countries. We categorize countries according to the Human Development Index (HDI) published by the United Nations Development Programme. The HDI is a summary of average achievement in key dimensions of human development: a long and healthy life, being knowledgeable and have a decent standard of living. The HDI divides countries into four different groups. For our study, we classify countries of “very high human development” and “high human development” as developed countries and we classify countries of “medium human development” and “low human development” as developing countries. Like you one other participant is from a [DEVELOPED: developed] [DEVELOPING: developing] country. The two remaining participants are, in contrast, from [DEVELOPED: developing] [DEVELOPING: developed] countries.

The decision you will make affects both you and the other members of your group. Therefore, your individual payment will be influenced by both your decision and the decisions of the other group members. Please note, that all other members of your group possess exactly the same amount of information and face the same decision as you do.

**What is the decision task?** During the study your individual payment is calculated in tokens. Subsequently to the study your payment will be converted from tokens into US-Dollars at the following exchange rate:

$$1 \text{ Token} = 1.5 \text{ US \$ [adjusted for purchasing power parity]}$$

Please note, the value of one token is the same in all countries. [GIVE: In this decision task, you and the other member of your group from a [DEVELOPED: developed] [DEVELOPING: developing] country possess an initial endowment of [DEVELOPED: 30] [DEVELOPING: 10] tokens in a private account. The two remaining participants from [DEVELOPED: developing] [DEVELOPING: developed] countries possess, in contrast, an initial endowment of [DEVELOPED: 30] [DEVELOPING: 10] tokens in their private accounts. Your task is to decide on the transfer of tokens between your private account and the group account: That is how many of the [DEVELOPED: 30] [DEVELOPING: 10] tokens in your private account you transfer to the group account. You will be asked to indicate your transfer on a slider as shown below. If you move the slider to the right, you decide to transfer tokens from your private account to the group account. Please note, your transfer can be between 0 and [DEVELOPED: 30] [DEVELOPING: 10] tokens (only integer numbers).] [GITA: In this decision task, you and the other



member of your group from a [DEVELOPED: developed] [DEVELOPING: developing] country possess an initial endowment of [DEVELOPED: 18] [DEVELOPING: 6] tokens in a private account. The two remaining participants from [DEVELOPED: developing] [DEVELOPING: developed] countries possess, in contrast, an initial endowment of [DEVELOPED: 18] [DEVELOPING: 6] tokens in their private accounts. In addition, there are 32 tokens in a joint group account shared by all members of your group. Your task is to decide on the transfer of tokens between your private account and the group account: That is how many of the [DEVELOPED: 18] [DEVELOPING: 6] in your private account you transfer to the group account and how many of the 32 tokens you leave in the group account respectively. Your transfer is related to the group account as described below: A positive transfer is a transfer of tokens from your private account to the group account. Respectively, a negative transfer is a transfer of tokens from the group account to your private account. You will be asked to indicate your transfer on a slider as shown below. If you move the slider to the left, you decide to transfer tokens from the group account to your private account (negative transfer). If you move the slider to the right, you decide to transfer tokens from your private account to the group account (positive transfer). Please note, you are from a [DEVELOPED: developed] [DEVELOPING: developing] country and your transfer can be between [DEVELOPED: -12] [DEVELOPING: -4] and [DEVELOPED: 18] [DEVELOPING: 6] tokens (only integer numbers). Participants from [DEVELOPED: developing] [DEVELOPING: developed] countries can transfer [DEVELOPED: -4] [DEVELOPING: -12] and [DEVELOPED: -12] [DEVELOPING: -4] and [DEVELOPED: 18] [DEVELOPING: 6] [DEVELOPED: 6] [DEVELOPING: 18] tokens (only integer numbers).]

**How do the accounts differ from each other?** Your individual payment depends on your decision as well as on the decisions of the other three group members according to the rules explained below. You receive a payment from tokens in your private account and you and the other three group members receive a payment from tokens in the group account.

- **Private account:** It's only you who can transfer tokens from your private account to the group account or vice versa. For every token which is in your private account, you will receive 1 token. The other three group members do not receive any payment from your private account. However, every participant equally owns a respective private account.
- **Group account:** Apart from you, the other three group members can also transfer tokens from their private account to the joint group account. For every token which is in the group account, you and the other three group members will receive a payment of 0.4 tokens.

Your total individual payment consists, consequently, of two parts: Part one: the amount of tokens in your private account, and Part two: the payment from the group account.

Please click "Next".

- Screen 4 -

### Examples

Before deciding on your transfer, this page will provide you with two examples of different transfers and how these decisions affect payments.

**Example 1:** [GIVE: Assume you are Participant A. You are from a [DEVELOPED: developed] [DEVELOPING: developing] country. You decide on a transfer of 2 tokens from your private account to the group account. Participants B decides to transfer 2 tokens from its private account to the group account. C decides to transfer 3 tokens and D decides to transfer 1 token. The group account now contains 8 ( $2+2+3+1$ ) tokens. According to the rules, this amount of tokens is multiplied by 0.4 and all group members benefit equally from it. Consequently, everybody receives 3.2 tokens from the group account. Your total individual payment is given by your initial endowment minus your transfer plus the payment from the group account: [DEVELOPED: 30] [DEVELOPING: 10]-2+3.2= [DEVELOPED: 31.2] [DEVELOPING: 11.2].] [GITA: Assume you are Participant A. You are from a [DEVELOPED: developed] [DEVELOPING: developing] country. You decide on a transfer of 2 tokens from your private account to the group account. Participants B decides to transfer 2 tokens from its private account to the group account. C decides to transfer 3 tokens and D decides to transfer 1 token. The group account now contains 40 ( $32+2+2+3+1$ ) tokens. According to the rules, this amount of tokens is multiplied by 0.4 and all group members benefit equally from it. Consequently, everybody receives 16 tokens from the group account. Your total individual payment is given by your initial endowment minus your transfer plus the payment from the group account: developed [DEVELOPED: 18] [DEVELOPING: 6] - 2+16= [DEVELOPED: 32] [DEVELOPING: 20] tokens.]

**Example 2:** [GIVE: Assume you are Participant A. You are from a [DEVELOPED: developed] [DEVELOPING: developing] country. You decide on a transfer of 6 tokens from your private account to the group account. Participants B decides to transfer 2 tokens from its private account to the group account. C decides to transfer 3 tokens and D decides to transfer 1 token. The group account now contains 12 ( $6+2+3+1$ ) tokens. According to the rules, this amount of tokens is multiplied by 0.4 and all group members benefit equally from it. Consequently, everybody receives 4.8 tokens from

the group account. Your total individual payment is given by your initial endowment minus your transfer plus the payment from the group account: [DEVELOPED: 30] [DEVELOPING: 10]-6+4.8= [DEVELOPED: 28.8] [DEVELOPING: 8.8].] [GITA: Assume you are Participant A. You are from a [DEVELOPED: developed] [DEVELOPING: developing] country. You decide on a transfer of 2 tokens from the group account to your private account. Participant B decides to transfer 2 tokens from its private account to the group account. C decides to transfer 3 tokens and D decides to transfer 1 token. The group account now contains 36 (32-2+2+3+1) tokens. According to the rules, this amount of tokens is multiplied by 0.4 and all group members benefit equally from it. Consequently, everybody receives 14.4 tokens from the group account. Your total individual payment is given by your initial endowment minus your transfer plus the payment from the group account: [DEVELOPED: 18] [DEVELOPING: 6] -(-2)+14.4= [DEVELOPED: 34.4] [DEVELOPING: 22.4] tokens.]

Please click "Next".

**- Screen 5 -**

### **Contribution Decision**

[GIVE: At this stage, you now have to decide upon your transfer. Please note, that the other members of your international group also indicate their decisions. You have [DEVELOPED: developed] [DEVELOPING: developing] tokens in your private account. Please indicate the amount of tokens you want to transfer: 0-[DEVELOPED: 30] [DEVELOPING: 10].] [GITA: At this stage, you now have to decide upon your transfer. Please note, that the other members of your international group also indicate their decisions. The group account contains 32 tokens and you have [DEVELOPED: 18] [DEVELOPING: 6] in your private account. Note again, any negative transfer between [DEVELOPED: -12] [DEVELOPING: -4] tokens and -1 is a transfer from the public account to your private account. Accordingly, any positive transfer between 0 and [DEVELOPED: 18] [DEVELOPING: 6] tokens is a transfer from your private account to the public account.]

Confirm: Please confirm your choice by clicking "Next".

**- Screen 6 -**

**Belief Group 1:** Irrespective of your own decision: What do you think is the transfer chosen by the other group member from a [DEVELOPED: developed] [DEVELOPING: developing] country? [GIVE: Please indicate the amount of tokens 0-[DEVELOPED: 30] [DEVELOPING: 10].] [GITA: Note again, any negative transfer between [DEVELOPED: -12] [DEVELOPING: -4] and -1 is a transfer from the public account to the private

account. Accordingly, any positive transfer between 0 and [DEVELOPED: 18] [DEVELOPING: 6] is a transfer from the private account to the public account.]

**Belief Group 2:** Irrespective of your own decision: What do you think is the transfer chosen by the other group members from [DEVELOPED: developing] [DEVELOPING: developed] countries? [GIVE: Please indicate the amount of tokens 0-[DEVELOPED: 10] [DEVELOPING: 30].] [GITA: Note again, any negative transfer between [DEVELOPED: -4] [DEVELOPING: -12] and -1 is a transfer from the public account to the private account. Accordingly, any positive transfer between 0 and [DEVELOPED: 6] [DEVELOPING: 18] is a transfer from the private account to the public account.]

Please click "Next".

**- Screen 7 -**

Please use the following scale to indicate how much you agree with the following statement:

- To me, the decision task was easy to understand.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer

**- Screen 8 -**

Please use the following scale to indicate how you classify yourself:

- In general, are you rather risk seeking or do you avoid to be exposed to risk? 0 (risk-averse) to 5 (prepared to take risks)  1  2  3  4  5  No answer
- Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?  Most people can be trusted  You need to be very careful  No answer

**- Screen 9 -**

Please indicate for each of the following actions whether you think it can always be justified, never be justified, or something in between, using this scale:

- Claiming government benefits to which you are not entitled.  Never justifiable  Mostly not justifiable  Neutral  Mostly justifiable  Always justifiable  No answer
- Avoiding a fare on public transport.  Never justifiable  Mostly not justifiable  Neutral  Mostly justifiable  Always justifiable  No answer
- Cheating on taxes if you have a chance.  Never justifiable  Mostly not justifiable  Neutral  Mostly justifiable  Always justifiable  No answer

**- Screen 10 -**

- Please classify the area where you grew up.  Big city  Small city  Rural village  No answer

People see themselves and how they relate to the world differently. Using this scale, can you indicate how strongly you agree or disagree with each of the following statements about how you see yourself?

- I see myself as a world citizen.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer
- I see myself as part of the [home country] nation.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer
- I see myself as an autonomous individual.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer

**- Screen 11 -**

If you use the following products or services, do you use them to contact people living in other parts of your country, or also people living in other countries?

- Postal mail.  Local area  Other parts of my country  Other countries  No answer
- Phone.  Local area  Other parts of my country  Other countries  No answer
- Internet.  Local area  Other parts of my country  Other countries  No answer

Consider the following geographical areas. How often do you travel, either for work or for vacation, in each of them?

- To other countries within my continent.  Every week  Every month  Every year  Less often  Never  No answer
- To other countries outside my continent.  Every week  Every month  Every year  Less often  Never  No answer

How often do you do the following activity?

- Watch a television program or movie from a different country.  Every day  Every week  Less often  Never  I do not have access  No answer
- Watch or listen to an international news source (CNN International, BBC World, Euronews, etc.).  Every day  Every week  Less often  Never  I do not have access  No answer
- Read an international news source (Time, The Economist, Le Monde, etc.).  Every day  Every week  Less often  Never  I do not have access  No answer

**- Screen 12 -**

- Do you belong to a religion or religious denomination?  Roman Catholic  Protestant  Orthodox (Russian/Greek/etc.)  JEW  Muslim  Hindu  Buddhist  Do not belong to denomination  Other:
- Please use the following scale to indicate how religious you are (1: not religious at all, 5: very religious).  1  2  3  4  5  No answer

**- Screen 13 -**

- Please use the following scale to indicate how important it is for you to own a piece of land (1: not important at all, 5: very important).  1  2  3  4  5  No answer
- Please use the following scale to indicate how important it is for you to maintain small species at the Amazon forest in South America (1: not important at all, 5: very important).  1  2  3  4  5  No answer

Using this scale, can you indicate how strongly you agree or disagree with each of the following statements concerning principles underlying potential rules to allocate climate change mitigation costs across countries:

- Countries with high income levels must pay a larger share of the costs than countries with low income levels. This option says that countries with greater ability to pay should pay more.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer
- Countries with currently high emissions levels must pay a larger share of the costs than countries with currently low emission levels. This option says that those countries that are currently polluting more should pay more.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer
- Countries with a history of high emissions levels must pay a larger share of the costs than countries with a history of low emissions. This option recognizes that CO<sub>2</sub> builds up in the atmosphere over many years. Thus, countries which polluted more in the past should pay more because they caused more of the problem.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer
- Countries with emissions per person greater than an agreed amount must pay, and they must pay more the higher their emission per person are.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer

- Each country shall reduce its emissions by an equal percentage rate. That is, a countries' overall share in global emissions remains constant.  Strongly disagree  Disagree  Neutral  Agree  Strongly agree  No answer

**- Screen 14 -**

Using this scale, can you indicate how important the following global challenges are in your opinion:

- International efforts in combating climate change.  Very important  Important  Neutral  Moderately important  Not important  No answer
- Securing world nutrition and eradicating poverty.  Very important  Important  Neutral  Moderately important  Not important  No answer
- Combating epidemics.  Very important  Important  Neutral  Moderately important  Not important  No answer
- Stabilizing the international financial system.  Very important  Important  Neutral  Moderately important  Not important  No answer
- Combating terrorism.  Very important  Important  Neutral  Moderately important  Not important  No answer

**- Screen 15 -**

**What comes next?**

After all participants have made their decisions, the ZEW research team will screen all decisions. To support the activities of your network, UNYANET will receive 2 US-Dollars for each participant. Furthermore, 10 percent of all participants will be randomly selected and receive their individual payment. If you have been selected, you will be informed via email subsequently to the study.

**- Screen 16 -**

Did you enjoy taking part in this study and do you want to participate in other scientific studies of this type? We would be pleased to add your name to our member database and would be happy to invite you to further studies. As a matter of course, your data are not used for any other purposes. If you wish to withdraw your participation offer at any point of time, your data will be directly unsubscribed from the database.

Please click on "Yes", to add your name to our database.

**- Screen 17 -**

Thank you for your time and support! Do you have any questions left? Our research team would be glad to help you. Do not hesitate to contact: [onlinestudy@zew.de](mailto:onlinestudy@zew.de). Please note details concerning the research agenda and the results of the study will be circulated among all registered persons subsequently to the study.

### 3.5.3 Screenshots

Screenshots of the contribution decision<sup>†</sup>

[*poor player in GIVE*]

A screenshot of a web interface for a contribution decision. The top section has a dark blue background with white text: a red asterisk icon, followed by "At this stage, you now have to decide upon your transfer. Please note, that the other members of your international group also indicate their decisions. You have 10 tokens in your private account." Below this is a yellow instruction: "Please click and drag the slider handles to enter your answer." and a yellow warning icon with the text "Each answer must be between 0 and 10". The main area is white and contains the text "Please indicate the amount of tokens you want to transfer:" above a horizontal slider. The slider has a blue handle positioned at the value 5, with a small black box containing the number 5 above it. The slider's range is from 0 to 10. At the bottom, a dark blue bar contains the text "Please confirm your choice by clicking 'Next'."

[*poor player in GITA*]

A screenshot of a web interface for a contribution decision. The top section has a dark blue background with white text: a red asterisk icon, followed by "At this stage, you now have to decide upon your transfer. Please note, that the other members of your international group also indicate their decisions. The group account contains 32 tokens and you have 6 tokens in your private account." Below this is a yellow instruction: "Please click and drag the slider handles to enter your answer." and a yellow warning icon with the text "Each answer must be between -4 and 6". The main area is white and contains the text "Please indicate the amount of tokens you want to transfer:" above a horizontal slider. The slider has a blue handle positioned at the value 1, with a small black box containing the number 1 above it. The slider's range is from -4 to 6. At the bottom, a blue information icon is followed by the text: "Note again, any negative transfer between -4 tokens and -1 is a transfer from the public account to your private account. Accordingly, any positive transfer between 0 and 6 tokens is a transfer from your private account to the public account."

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<sup>†</sup>Explanatory notes are given in square brackets.



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## 4 Leveling up? An inter-neighborhood experiment on parochialism and the efficiency of multi-level public goods provision

*A previous version of this chapter has appeared as:*

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**Abstract:** Many public goods can be provided at different spatial levels. Evidence from social identity theory and in-group favoritism raises the possibility that where higher-level provision is more efficient, subjects' narrow concern for local outcomes (parochialism) could harm efficiency. Building on the experimental paradigm of multi-level public good games and the "neighborhood attachment" concept, we conduct an artefactual field experiment with 600 participants in a setting conducive to parochial behavior. In an inter-neighborhood intra-region design, subjects allocate an endowment between a personal account, a local, and a regional public good account. The between-subjects design varies across two dimensions: One informs subjects that the smaller local group consists of members from their own neighborhood ("neighbors"). The other varies the relative productivity at the two public goods provision levels. We find evidence for parochialism, but contrary to our hypothesis, parochialism does not interfere with efficiency. The average subject responds to a change in relative productivities at the local and regional level in the same way, whether aware of their neighbors' presence in the small group or not. The results even hold for subjects with above-median neighborhood attachment and subjects primed on neighborhood attachment.

## 4.1 Introduction

Public goods (PG) can be provided at different spatial levels. As a result, individuals often have to make a decision not just whether to contribute to PG, but how much to contribute at different levels: Should I contribute to wildlife conservation through donating to a conservation area in my neighborhood when I could also contribute to wildlife conservation through a regional wildlife initiative? Should I give more support to my local public radio station – or more to the national network? In the “level problem”, individuals need to come to a decision whether and how much to contribute at different levels, each involving groups of different size and differently sized benefits of contributing for others - and for oneself.

The multi-level public goods game (ML-PGG) is an extension of the standard PGG that experimental economists have been employing for some time now in order to understand more about individual behavior in the level problem (Wachsmann 2002; Wit and Kerr 2002; Blackwell and McKee 2003; Buchan et al. 2009, Güth and Sääksvuori 2012; Fellner and Lünser 2014; Chakravarty and Fonseca 2017).<sup>30</sup> While design details differ, the unifying feature of all ML-PGG is the nested structure of social dilemmas: Subjects can privately provide the PG in a smaller group at the lower level and in a larger group at the upper level, and all the smaller groups are fully contained within a larger group. This nested structure is what differentiates the ML-PGG from other extensions of the standard PGG to multiple PGs (e.g., Cherry and Dickinson 2008; Falk and Zehnder 2013; McCarter et al. 2014)<sup>31</sup> and what allows the level problem to be captured by design.

To the public economist, the ML-PGG offers an opportunity to re-examine the behavioral economics of the level problem. The behavioral phenomenon at the heart of the present paper is the possibility that individuals exhibit a narrow concern for their own local group in the level problem, attaching a consequently lower weight to outcomes for the larger group in which the local group is nested. Such parochial concerns could interfere with the efficiency of individuals’ contribution decisions across the different levels in major ways, in particular if the provision of public goods technically exhibits economies of scale over some relevant range. Such scale economies are a reg-

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<sup>30</sup>More recently, the ML-PGG has also been applied in quantitative biology to study the evolution of cooperation (Wang et al. 2011).

<sup>31</sup>ML-PGG designs so far capture the concurrence of PG dilemmas in two different ways. One set of designs involves an allocation tasks for an experimental endowment not just between a private and a single group account, but between a private and two group accounts that differ in group size, marginal per-capita return, and other structural features (Blackwell and McKee 2003; Buchan et al. 2009; Fellner and Lünser 2014; Chakravarty and Fonseca 2017). The other set of designs retains the standard allocation task between one private and one public account, but varies across treatments the externalities that the public account generates to different groups (Engel and Rockenbach 2011; Güth and Sääksvuori 2012).

ular feature of public goods such as education (Brasington 2003), municipal services (Reingewertz 2012), fire (Duncombe and Yinger 1993) and police services (Finney 1997) and are commonly captured in ML-PGG by higher aggregate returns to contributing to spatially higher levels. The implication is that “leveling up”, i.e., more contributions going to the higher provision level, is in the interest of social efficiency (Buchan et al. 2009; Güth and Sääksvuori 2012; Chakravarty and Fonseca 2017) and that behavioral mechanisms that impede “leveling up” impose a social cost. Parochialism, the object of our present study, is an obvious candidate for such a mechanism.

The study of parochialism, i.e., favoring one’s own group at the expense of efficiencies in the larger group, relates to the recent interest in economics in individuals’ social identity or sense of group attachment (Akerlof and Kranton 2000). Such attachment has been shown to have significant impact on contribution decisions in the standard PGG when the design allows group attachment to play a role. When subjects share a social identity through a group, they tend to behave more cooperatively, than on average, towards those they recognize as group members and less cooperatively towards outsiders.<sup>32</sup> Evidence for such in-group favoritism and out-group discrimination can sometimes be generated in standard PGGs in which the shared commonalities in the group are “minimal” (e.g., Tajfel and Turner 1979; Bernhard et al. 2006; Chen and Li 2009)<sup>33</sup> and most reliably in settings in which the shared social identity is naturally grown through direct social interaction (e.g., Charness et al. 2007; Goette et al. 2006) and in which it can lead to potentially significant efficiency losses (e.g., Bernhard et al. 2006; Ruffle and Sosis 2006). Our study of parochial behavior in the ML-PGG shares with the existing literature on group attachment a focus on in-group favoritism, but also differs because the nested architecture of the ML-PGG does not admit an out-group. Instead, it features several in-groups of different size and distance to the contributor. In such a setting, social identity very well might, but does not necessarily affect PG contributions and create a conflict between parochialism and efficiency.

In the present paper, we build on previous ML-PGG experiments with an allocation task between a personal account, a local public good account, and a regional public good account in order to investigate the presence and magnitude of the “leveling up” effect. We do so in the setting of an artefactual field experiment<sup>34</sup> that is naturally suited for parochialism to manifest itself and at the same time allows for a controlled variation of both the relative productivities of PG provision at different levels and of

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<sup>32</sup>In a setting conducive to discrimination against outsiders, Daskalova (2018) shows that the tendency to discriminate is especially strong in case subjects decide collectively in groups sharing a common identity.

<sup>33</sup>“Minimal” groups are defined by a group identity constructed around an arbitrary membership criterion, such as assignment of a color or a shared taste in art (Turner et al. 1979).

<sup>34</sup>We follow the nomenclature of Harrison and List (2004) in this characterization.

the awareness of a shared group attachment in the small (low level) group. The ideal setting for parochialism to assert itself is one in which individuals exhibit attachment to naturally grown groups at a local level, but not at a higher level. The experiment implements this setting by offering a choice of providing the PG at two levels, the local level being the neighborhood in which the subject lives and the regional level being the region in which the subjects' neighborhoods are all located. The affiliation of an individual to a neighborhood has a distinguished history in the literature as an identifier of attachment to a naturally grown group. Neighborhood affiliation is a well-established component of social identity in social psychology and sociology. Having been intensively studied for at least forty years<sup>35</sup>, neighborhood attachment correlates with other measures of "local social capital", the intensity of neighborhood ties, and the level of involvement of subjects in informal social activities in the neighborhood (Ringel and Finkelstein 1991; Moser et al. 2002; Bonaiuto et al. 2003; Brown et al. 2003; Lewicka 2005). Correspondingly, the neighborhood has since been used in economics as an appropriate level at which to investigate parochialism in trust relationships (Falk and Zehnder 2013; Meier et al. 2016), PG provision (Marschall 2004), and social dilemmas in general (Falk and Zehnder 2013).

Neighborhood affiliation is also a particularly meaningful concept in the present context because of the explicitly spatial nature of the ML-PG provision problem in our experimental implementation. Numerous PGs are provided at the neighborhood level because the neighborhoods in our experiment, municipal districts in German cities, are political entities that have their own neighborhood associations, their own physical infrastructure of social interaction such as community halls, and send their own delegates to the city council. The neighborhood therefore provides a direct connection to public decision-making in the real world. Attachment to one's neighborhood contrasts with that to one's region, the other level of PG provision implemented in our experiment. Emotional attachment to regions is generally weak (Lewicka 2011) because regions are considered by their inhabitants to be more abstract (Tuan 1975), spatially fuzzy (Laczko 2005), and often a product of government planning rather than historically grown (Paasi 2003). All of these characteristics apply to the region that is used as the higher provision level in our present experiment, providing the desirable differential in attachment compared to the neighborhood level.

The controlled variation of the awareness of shared group attachment comes from two treatment conditions, one in which subjects learn that the small group contains only members of the subject's own neighborhood and the other in which they do not. The controlled variation in the relative productivities of PG provision at the local and re-

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<sup>35</sup>See Lewicka (2011) for a survey.

gional level comes from two treatments that differ in the marginal per-capital return (MPCR) of the regional PG. Together with the recourse to naturally grown groups, this two-by-two design allows us to answer whether parochialism interferes with greater efficiency in ML social dilemmas. This gives rise to three distinct contributions: The first is the exogenous variation of a naturally grown social identity, thereby going beyond artificially induced (Beekman et al. 2017), minimal (Blackwell and McKee 2003; Chakravarty and Fonseca 2017) and anonymous groups (Fellner and Lünser 2014) and experiments without exogenous variation in place attachment (Buchan et al. 2009). The second is the particular neighborhood-within-a-region setting that provides a naturally grown multi-level structure allowing parochialism to assert itself at the local level. The third is a design that answers to the need for a randomized assignment of subjects to treatments in which both the salience of social identity and the relative contribution productivities in the ML-PGG differ. This allows a clean disentangling of the social identity dimension and the efficiency dimension, which is not possible on the basis of existing evidence. Such disentangling is required, however, in order to isolate whether social identity and efficiency (MPCR) interact negatively. Earlier experiments either vary the MPCR of one of the two PGs (Blackwell and McKee 2003; Fellner and Lünser 2014; Chakravarty and Fonseca 2017), vary the salience of the group affiliation at a constant MPCR (Chakravarty and Fonseca 2017), or examine home-grown variations in group affiliation (Buchan et al. 2009). Closest to our setting, Beekman et al. (2017) conduct a laboratory experiment in which they induce conflicts between groups and vary the relative productivity in a ML-PGG. None of the previous studies implements the full factorial design with naturally grown groups and randomized assignment that is required to test whether groups with a shared social identity at the local level respond less to changes in the MPCR than those without a shared social identity.

The results presented in this paper are based on data collected in an artefactual field experiment of the ML-PGG type in which over 600 participants decide online about the private provision of concurrent and perfectly substitutable public goods at two different levels. We have two main findings. First, we show that some of the results from lab-based ML-PGG experiments (Blackwell and McKee 2003; Fellner and Lünser 2014; Chakravarty and Fonseca 2017; Beekman et al. 2017) successfully transfer to our field setting, among them the MPCR effect: Increasing the MPCR of the regional PG attracts higher contributions. Similarly, the level-wise allocation of private contributions to different PG levels is not fully socially efficient: As the previous laboratory experiments, we find positive average contributions to the small group, even when the large group PG generates higher total benefits. Our second and main finding is that the strength of the MPCR effect does not vary with the presence of a naturally grown

social identity in the small group. Comparing groups in which subjects were or were not aware that the small group consisted of their local neighbors, we find that both groups increased their contributions to the regional PG as the MPCR of the regional PG increased. Importantly, this increase is not statistically smaller in the group where neighborhood attachment was made public. In other words, a higher efficiency of the regional PG was associated with a leveling up of contributions by subjects, and the leveling up was the same across groups, irrespective of whether subjects knew that the small group consisted of their neighbors. This finding is robust. It holds on average, but also for subjects with above-median neighborhood attachment and for subjects that have been procedurally primed on their neighborhood attachment, even though both types of subjects exhibit clearly more parochialism in their contributions to the smaller group PG than the rest of the population. In our artefactual field experiment, therefore, efficiency can be said to survive parochial bias.

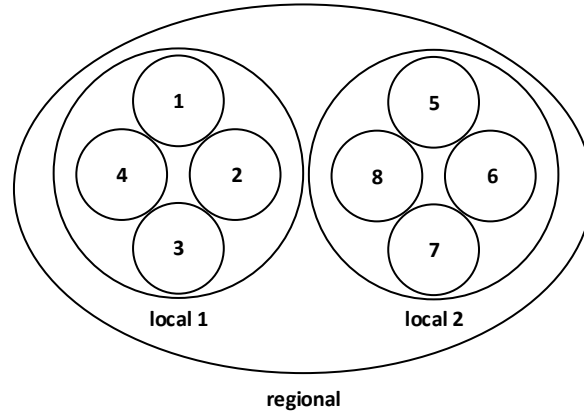
The remainder of the paper is organized as follows. Section 4.2 describes the experimental design and derives our theoretical predictions. Section 4.3 contains a detailed description of the experimental protocol. We present the experimental results and robustness checks in Section 4.4. Section 4.5 provides a summary discussion of our main findings and concludes.

## 4.2 Experimental design and theoretical predictions

### 4.2.1 Experimental design

Our experimental design implements a multi-level public goods game in which each subject is a member both of a small group consisting of four members and of a larger group of eight members. The larger group is composed of the small group of four plus additional four members, who are all members of the other small group. In the parlance of ML-PGG, the smaller groups are therefore “nested” (e.g., Güth and Sääksvuori 2012) in the larger group, with two small groups of four making up one large group of eight. In keeping with that literature, we will repeatedly refer in the paper (but not the instructions) to the small group and its PG as local and contrast that with the larger group and its PG being termed regional. Figure 4.1 illustrates the group composition. The decision task for subjects is to allocate an initial endowment across three different accounts: a private account that subjects retain for themselves, a PG that generates benefits to the member’s local group only, and a PG that provides benefits to the entire regional group. Formally, subject  $i$ ’s payoff,  $\pi_i$ , given the contribution decisions of all remaining seven subjects, of which three are in subject  $i$ ’s local group  $L_i$  and four in the other local group  $L_{-i}$  of which  $i$  is not a member, can be expressed as

Figure 4.1: Group composition



Note: Numbers represent different subjects in the experiment. The larger (regional) group is composed of two small groups (local 1 and local 2) of four subjects each.

$$\pi_i = e - q_i^l - q_i^r + \alpha^l \sum_{j \in L_i} q_j^l + \alpha^r \sum_{j \in L_i \cup L_{-i}} q_j^r$$

where  $e$  denotes the initial endowment,  $q_i^l$  the contribution of subject  $i$  to the local public good, and  $q_i^r$  the contribution to the regional public good.  $\alpha^l$  is the MPCR from the local public good and  $\alpha^r$  denotes the MPCR from the regional public good. The respective MPCRs fulfill the standard requirements for a social dilemma, with  $\alpha^l < 1$  and  $4\alpha^l > 1$  for the local as well as  $\alpha^r < 1$  and  $8\alpha^r > 1$  for the regional group.

The treatment conditions and parametric implementation of the design are summarized in Table 4.1 for stage 1 of the experiment and Figure 4.5 in the appendix provides a schematic diagram of the two-stage procedural implementation plus the number of subjects in each treatment. Table 4.1 shows that the experimental treatments vary along two dimensions, MPCR and social identity.

We start with the two MPCR conditions that vary the productivity of the regional PG. Subjects are randomly assigned to one of these conditions at the outset of the experiment and remain in the same MPCR condition until the end. As in Blackwell and McKee (2003), there are two MPCRs for the regional good while the MPCR for the local PG is always set at  $\alpha^l = 0.5$ . The total benefits (TB) of a one unit contribution to the local PG across the entire regional society of eight are therefore held constant at 2 units.<sup>36</sup> Condition LOW features a regional MPCR of  $\alpha^r = 0.25$  and corresponding TB of 2. In condition LOW, therefore, the TB of the local and the regional PG are the same

<sup>36</sup>With  $\alpha^l = 0.5$ , a contribution to the local PG of €1 by one subject generates €0.5 for four subjects in the local group only and therefore a total benefit of €2 for the entire group of eight.

while the price of contributing is lower in the local PG.<sup>37</sup> Condition HIGH features an MPCR of  $\alpha^r = 0.5$ , a corresponding TB of 4, which is larger than the TB of the local good, but the same price of contributing.<sup>38</sup>

The second treatment dimension, social identity, is implemented in two stages, corresponding to two consecutive decision tasks for each subject. In both stages, the treatment consists of whether subjects receive information that they share a group attachment with the members of the smaller and the larger group. The group attachment for the smaller group is residence in the same neighborhood, for the larger it is residence in the same region. The neighborhoods in the experiment are municipal districts, i.e., political entities with a typical population of several thousand inhabitants and an area of around five square kilometers that elect their own representatives to the city council. The region in the experiment is a metropolitan area including various cities and straddling several states with a population of over two million and in which the neighborhoods are located. The region does not function as a political entity. Four subjects from the same district are always combined in a small group. In a large group, two small groups from two different cities within the metropolitan area are matched. In the condition LABEL, subjects learn that the small group contains three other individuals that reside in the subject's own neighborhood and that the large group contains those three plus four individuals that reside in the same region as the subject. Subjects assigned to the treatment condition NOLABEL, on the other hand, are neither informed that the three other members of the smaller group share a common neighborhood with the subject nor that individuals in the larger group reside in the same region. Jointly, the two treatment dimensions of MPCR and social identity allow us to identify how contributions in a ML-PGG respond to naturally occurring forms of social identity.

Subjects take one allocation decision in stage 1 of the experimental session. There is no feedback after stage 1. The session then continues with stage 2, which is essentially a repeat of stage 1, but preceded by a priming task that follows the natural identity stimulation approach by Li et al. (2017) and Chen et al. (2014). First, subjects complete a questionnaire that contains a set of seven questions about their neighborhood and their involvement in neighborhood activities to make subjects' local identity salient. Second, subjects fill in a writing task in response to an open-ended question in order to gain a positive connotation of living in their neighborhood or the metropolitan region. There are two versions of the open-ended question. Given our interest in parochial

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<sup>37</sup>The price of giving for the individual contributor is the opportunity cost of contributing to the PG. At an MPCR of 0.5, the contributor receives €0.5 in PG for every €1, corresponding to a price of €0.5. At an MPCR of 0.25, the contributor only receives €0.25 and the price is €0.75.

<sup>38</sup>At  $\alpha^r = 0.25$  ( $\alpha^r = 0.5$ ), a contribution to the regional PG of €1 by one subject generates €0.25 (€0.5) for eight subjects in the regional group and therefore a total benefit of €2 (€4) for the entire group of eight.



Table 4.1: Experimental design - Stage 1

Treatment	Local Public Good (LGP)			Regional Public Good (RPG)		
	#	$\alpha^l$	TB <sup>l</sup>	#	$\alpha^r$	TB <sup>r</sup>
LOW-NOLABEL	4	0.5	2	8	0.25	2
LOW-LABEL	4	0.5	2	8	0.25	2
HIGH-NOLABEL	4	0.5	2	8	0.5	4
HIGH-LABEL	4	0.5	2	8	0.5	4

Note: Small (local) groups consist of four subjects each. Large (region) group consist of eight subjects.  $\alpha^l$  (TB<sup>l</sup>) is the MPCR (total benefit) from the local public good.  $\alpha^r$  (TB<sup>r</sup>) is the MPCR (total benefit) from the regional public good. In LABEL subjects are informed that the small (local) group contains other individuals from their own neighborhood and that the large (regional) group contains those three plus four individuals from the same region. In NOLABEL subjects are neither informed that the three other members of the small group are from their own neighborhood nor that individuals in the large group are from the same region.

preferences, the analysis of stage 2 decisions uses observations from the majority of subjects (74%) assigned to the *local prime* version in which subjects list positive aspects of living in their specific neighborhood.<sup>39</sup> After completing the writing task, subjects take their stage 2 allocation decision. To determine final payoffs to subjects, the group decision of one regional group in one of the two stages was randomly selected and the corresponding pay-offs computed at the end of the experiment.<sup>40</sup>

#### 4.2.2 Hypotheses

In a sequence of two one-shot ML-PGG, purely selfish individuals are predicted to allocate their entire endowment to their private account in both decisions. Given the parameter choices of the design, this prediction holds for all four treatment conditions, irrespective of the level of the MPCR (LOW or HIGH) and of the social identity information (LABEL or NOLABEL). The behavior of the average subject in PGG experiments, however, is not consistent with the assumption of purely selfish preferences (e.g., Ledyard 1995; Zelmer 2003; Chaudhuri 2011). This also holds for behavior in the ML-PGG for which previous experiments have found that individuals exploit the free-riding opportunities present in the ML-PGG to a significantly lower degree than predicted in the standard Nash equilibrium of purely selfish players (e.g., Blackwell and McKee 2003; Güth and Sääksvuori 2012; Fellner and Lünser 2014).

<sup>39</sup>The remaining 26% of subjects were assigned to the *regional prime* version in which they listed positive aspects of living in the metropolitan area.

<sup>40</sup>To prevent spillover or licensing effects, subjects are informed at the beginning of the experiment about these procedures.

The levels of cooperation observed in the ML-PGG can be traced back to well understood structural factors that explain cooperation in the linear PGG such as the MPCR. The conclusive evidence from the standard PGG that higher MPCRs induce higher contributions (e.g., Isaac et al. 1984; Ledyard 1995; Zelmer 2003; Chaudhuri 2011) carries over to ML-PGG. Like in the standard PGG, a higher MPCR increases efficiency through a higher productivity and, at the same time, a lower price of giving (Andreoni and Miller 2002). Group size, another structural factor, has been shown to have either no or at best a slightly positive effect on contributions in experiments involving the standard PGG (e.g., Isaac et al. 1994; Nosenzo et al. 2015; Diederich et al. 2016). In light of these results, previous findings from the ML-PGG that the small group receives higher contributions on average when its TB are the same as those of the larger group (Blackwell and McKee 2003; Fellner and Lünser 2014; Chakravarty and Fonseca 2017) are not in themselves evidence of a local bias, but may simply affirm that contributions in the PGG respond to the MPCR, but are largely irresponsive to group size. The finding that increasing the MPCRs for contributions to the larger-group PG leads to higher contributions (Blackwell and McKee 2003; Fellner and Lünser 2014) is also in line with these previous findings. Group size invariance can similarly explain the result that even at identical MPCRs for the smaller and larger group, contributions to the smaller group do not fall to zero (Blackwell and McKee 2003; Chakravarty and Fonseca 2017). Given the broad empirical support for a significant and positive MPCR effect, our first hypothesis is that an artefactual field experiment will validate the core findings of previous ML-PGG lab experiments. Comparing stage 1 contributions of subjects that face a lower MPCR for the regional PG ( $a^r = 0.25$ ) than for the local PG ( $a^l = 0.5$ ) with stage 1 contributions of subjects that face equal MPCRs in both PGs ( $a^r = a^l = 0.5$ ), we predict a higher average share of endowments going to the regional PG when MPCRs are the same (and TB are higher). This would be in line with the results by Blackwell and McKee (2003) in a design with “minimal groups” and by Fellner and Lünser (2014) in a design without group identity and would reaffirm the dominance of the MPCR effect: The productivity of contribution to the regional public good is higher for the higher MPCR while the price of contribution is lower. Both mechanisms render contributing to the regional PG more attractive for subjects with social preferences, whether aware or unaware of a shared common identity. Applied to the design of the present experiment, this validation test leads to the following formulation.

**Hypothesis 1 (leveling up):** Average contributions to the regional public good will be higher in the HIGH MPCR treatment compared to the LOW MPCR treatment.

In other words, contributions are predicted to respond positively to increases in the MPCR for a PG benefiting the larger group, and the positive MPCR effect is expected

to be present both in settings in which subjects are aware of a shared neighborhood affiliation and in which they are not. The comparison of contributions that form the core test of hypothesis 1 therefore delivers a validation check on previous findings in the ML-PGG paradigm that have varied the MPCR of the regional PG both under anonymity and using minimal groups.

Following the validation exercise implicit in testing hypothesis 1, hypothesis 2 homes in on the core issue of this paper: Does a shared social identity in a subgroup of subjects engaged in a ML-PG provision problem lead to changes in contribution behavior that can unambiguously be judged to interfere with efficiency? In other words, does a parochial bias lead to efficiency losses in the level problem and if so, how big are these losses? Our strategy for establishing the presence and size of a parochial bias in the present ML-PGG is to examine the interaction effect between the shift in the MPCR in the regional good and the disclosure of a shared social identity in the local group. This strategy presents a clean test for the question how ML-PG provision is affected by a potential parochial bias on account of activated social identity and is a key step towards the question of efficiency.<sup>41</sup> Parochial altruism predicts that, relative to subjects in an anonymous setting, subjects aware of a shared local neighborhood affiliation attach greater weight to local outcomes (Bernhard et al. 2006) and will therefore have less of an inclination to level up in response to a higher MPCR for the regional PG. A shared social identity, in other words, prevents subjects' from leveraging a higher MPCR into a higher provision of PGs to the same extent as when identity is not revealed. Applied to the present experimental design, this tests formulates as

**Hypothesis 2 (leveling up with social identity):** The interaction effect between the MPCR treatment and the LABEL treatment is predicted to be negative: Relative to subjects without knowledge of their group composition, subjects aware that the local public good benefits exclusively their neighbors increase the contributions to the higher level by less when the MPCR of the regional public good increases.

The test of hypothesis 2 establishes the core result of our experiment. The remaining

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<sup>41</sup>An alternative approach could be based on a simple comparison of contribution levels to the local and regional PG across social identity treatments at constant MPCR for the regional PG. A strategy based on comparing levels across the LABEL/NOLABEL treatment is not sufficient, however. The LOW MPCR treatment is a poor setting for a comparison because the TBs of the local and the regional account are identical such that any combination of contributions to the local and regional PG that leaves their sum broadly unchanged has the same impact on total provision. Comparing levels in the HIGH MPCR treatment, on the other hand, is complicated by evidence from previous experiments that even in anonymous group settings, the local PG attract significant contributions despite its lower TBs. This sets a high baseline for an additional parochial bias to assert itself. Comparing total benefits across MPCRs is also problematic since productivity is exogenously higher in the HIGH MPCR condition.

three hypotheses add robustness.

Hypotheses 3 and 4 do not focus on the presence of the interaction effect, but its strength. We expect to find heterogeneity across subjects in how they respond to the disclosure of a common local affiliation among members of the small group. Subjects will be heterogeneous along a multitude of dimensions. The dimension of interest in the present design is a predisposition towards parochialism, and we explore two sources of predisposition. One predisposition is home-grown: We predict that among subjects who articulate a strong attachment to their own neighborhood in the post-questionnaire (henceforth “local patriots”), the change in contributions to the regional PG caused by an increased MPCR for the regional good is less than the change among the other subjects when they are in the LABEL treatment. No such effect should be present in the NOLABEL treatment. The reasoning is the same as that underlying hypothesis 2, with the only difference that subjects predisposed to parochialism are expected to exhibit a stronger form of the interaction effect. This conjecture is captured in the following hypothesis.

**Hypothesis 3 (local patriots):** The negative interaction effect between the MPCR treatment and the LABEL treatment is predicted to be greater in absolute terms for subjects who articulate high concern for members of their neighborhood compared to those who articulate low concern: Local patriots that are aware that their contributions to the local group benefit their “neighbors” are less inclined than others to level up when the MPCR of the regional public good increases.

The second source of predisposition toward parochial choices, namely through priming, provides the content of our fourth hypothesis. The priming procedure that subjects take prior to their stage 2 decision follows Li et al. (2017) and Chen et al. (2014) and is designed to activate an existing place attachment in subjects’ mind. Subjects that have undergone the local prime version of the procedure and are then assigned to the LABEL condition in the social identity treatment are therefore expected to exhibit a stronger concern for how their allocation decisions in stage 2 impact on members of the small group. The predictions for locally primed subjects are then essentially the same as in the case for a home-grown predisposition for parochialism: On average, subjects in the LABEL condition will increase their contributions towards the regional PG less as its MPCR doubles than subjects in the NOLABEL condition, who are unaware that the small group contains their “neighbors”.

**Hypothesis 4 (priming effect):** The negative interaction effect between the MPCR treatment and the LABEL treatment is predicted to be greater in absolute terms for subjects who have undergone the local priming procedure. After local priming,

subjects that are aware that their contributions to the local group benefit their "neighbors" are less inclined than others to level up when the MPCR of the regional public good increases.

### 4.3 Experimental setting and procedure

The inter-neighborhood intra-region experiment recruited participants from a total of four municipal districts, two each from two cities in Germany, Heidelberg and Mannheim, that are located within 25 km of each other in the same region, the Rhine-Neckar metropolitan region. Heidelberg, with a population 150,000, consists of 15 municipal districts; Mannheim, with a population of 330,000, consists of 17 districts. The recruitment procedure involved the distribution of around 12,000 invitation letters via mail to up to 3,000 randomly selected households in each of the four districts.<sup>42</sup> The letter invited the receiving household to have one member of voting age take part in a scientific study on decision making, conducted by the University of Heidelberg and the Centre for European Economic Research (ZEW) in Mannheim. The announced participation reward was set at €5. Subjects were informed that they could earn additional individual payments in the course of a 15-minute study. No other information on the context of the study was given in this initial invitation letter.

A total of 616 individuals from Heidelberg (323) and Mannheim (293) took part in the online experiment. They did so by following a link in the invitation letter with their personal electronic device.<sup>43</sup> To log in and start the experiment, participants entered an individual access code provided in the invitation letter. The access code prevented participants from taking part in the study more than once. After going through a series of detailed instructions on the procedures, the decision task, and a set of numerical examples, participants made their decisions.<sup>44</sup> The average participant completed the experiment in approximately 15 minutes. We used an ex-post matching protocol to calculate final payoffs.

The participation payment was set at €5. The initial endowment was set at €8. Participants earned an average of €18.38, which at 15 minutes average completion time compares favorably with the equivalent average hourly wage. Individual payments

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<sup>42</sup>Since one district (Bahnstadt) is considerably smaller than the other three districts, we distributed invitation letters to all 2,000 households living in this district. We provide a translated version of the invitation letter in the appendix.

<sup>43</sup>The programming was completed in LimeSurvey, a free open source software tool. The design was optimized for either the use of a personal computer, a laptop, a tablet, or a smartphone. In addition to the conventional link, the invitation letter contained a QR code to facilitate access to the online experiment.

<sup>44</sup>We provide a diagram of the experimental procedures, the invitation letter, instructions, and screenshots in the appendix.

were implemented by sending households a payment card which is good for cash in many large retail chains, petrol stations, and online shops.<sup>45</sup> Payment cards were charged with the individualized payments and sent out by mail four weeks after the conclusion of the experiment. All specifics regarding the payment procedure were disclosed to subjects prior to their first decision.

As part of the experimental procedure, subjects completed a post-questionnaire after the first decision task. The questionnaire collected information on characteristics of place attachment. We combine five measures commonly used in the place attachment literature into a score of participants' local identity (*local identity index*). These metrics are (1) whether participants deliberately decided to live in their neighborhood; whether they feel (2) happy, (3) proud, and (4) comfortable to be living in their neighborhood; and (5) how well they feel they identify with their neighborhood. Responses to each item were made on a five-point Likert scale from 1 (*not at all*) to 5 (*extremely*). The local identification score was calculated by standardizing responses to all five items and summing. At the end of the experiment, subjects completed another questionnaire collection information on core demographics (*age, sex, income, education, religiousness*), duration of residence in the region (*years region*) and the municipal district (*years neighborhood*).<sup>46</sup>

## 4.4 Results

### 4.4.1 Full sample

We begin our analysis by describing the pooled data from the stage 1 decision across all treatments and participants. There, 32.8% of the sample contribute their full endowment either to the local (LPG) or the regional public good (RPG) and leave nothing in their private account. 6.1% of participants allocate their entire endowment to their private account. In line with the overwhelming evidence in PGG experiments, it is modal behavior to contribute some, but not all of the endowment to PGs. This can also be seen in Figure 4.2, which plots participants' average contribution decisions to their private account and the LPG and RPG across all four treatment conditions.

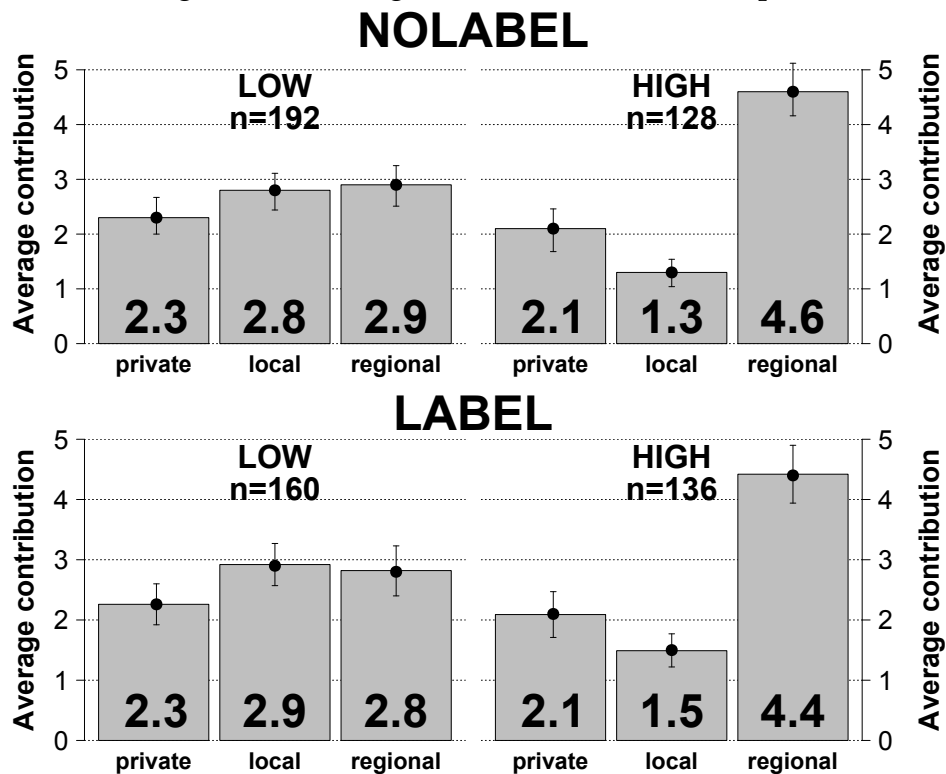
Hypothesis 1 conjectures that the artefactual field experiment will replicate a core finding of previous laboratory experiments, namely that a higher MPCR for the regional good causes significantly higher contributions by subjects. We test hypothesis 1 by comparing contributions to the RPG at different MPCRs in both the NOLABEL and

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<sup>45</sup>This procedure allowed us to pay subjects in an incentive compatible way without having to personally interact with them which would be problematic both for reasons of anonymity and logistics.

<sup>46</sup>Table 4.6 in the appendix reports the descriptive statistics of the post-questionnaire broken down by municipal districts.

Figure 4.2: Average contributions - full sample



Note: Average contributions to private, local, and regional account in the NOLABEL (upper half) and LABEL (lower half) of LOW (left) and HIGH (right) for the whole sample. Confidence intervals at the 95%-level.

the LABEL conditions. In the NOLABEL treatment, contributions to the RPG are significantly higher from an MPCR of 0.5 compared to an MPCR of 0.25 (2.9 vs. 4.6,  $p$ -value: 0.000, Mann-Whitney U (MW-U) test). The same is true in the LABEL treatment (2.8 vs. 4.4,  $p$ -value: 0.000, MW-U test). These tests are summarized in

**Result 1 (leveling up):** There is a positive MPCR effect: Average contributions to the regional public good are significantly higher in the HIGH MPCR treatment compared to the LOW MPCR treatment.

We note in passing that the positive MPCR effect observed in this experiment is a substitution effect between the two PGs. The average share of endowment allocated to the private account does not change significantly across the MPCR conditions (NOLABEL: 2.3 vs. 2.1,  $p$ -value: 0.309, MW-U test; LABEL: 2.3 vs. 2.1,  $p$ -value: 0.343, MW-U test). Average contributions to the LPG are significantly lower, however (NOLABEL: 2.8 vs. 1.3,  $p$ -value 0.000, MW-U test; LABEL: 2.9 vs. 1.5,  $p$ -value: 0.000, MW-U test). This observed substitution from the LPG to the RPG is well in line with the findings of Fellner and Lünser (2014), but contrasts with Blackwell and McKee (2003) who find that contributions are substituted from the private account towards the non-excludable public

good.<sup>47</sup>

Having validated one of the key findings from lab-based ML-PGG experiments in an artefactual field setting in result 1, we now turn to testing hypothesis 2, which forms the core of the paper. Following hypothesis 2, we expect that the strength of the MPCR effect will be smaller in the LABEL condition, which invokes naturally occurring social identity, compared to the NOLABEL condition, which does not even invoke minimal group identity. First note that, for the average subject, the effect of invoked social identity on contributions to the LPG is rather mild and fails to reach statistical significance (LOW: 2.8 vs. 2.9,  $p$ -value: 0.266, MW-U test; HIGH: 1.3 vs. 1.5,  $p$ -value: 0.363, MW-U test). This finding might be driven by individual differences in place attachment. In this section, we focus on the average subject before investigating potential differences in subject's place attachment and their impact on the MPCR effect in more detail in Sections 4.4.2 and 4.4.3. The test of hypothesis 2 is essentially a difference-in-difference test in which we compare whether the MPCR effect (the difference between LOW and HIGH MPCR) differs between the NOLABEL and LABEL condition. In the NOLABEL condition, the MPCR effect gives rise to an increase of €1.8 in contributions to the RPG, in the LABEL condition to an increase of €1.7, which is smaller. The difference between the MPCR effects is not statistically significant, however ( $p$ -value: 0.766, F-test). Invoking social identity had therefore no significant effect on the average subject's responsiveness to an increase in the MPCR of the RPG.<sup>48</sup> This is summarized in

**Result 2 (leveling up with social identity):** The inclination to level up is not affected by the revelation of neighborhood ties: For the average subject there is no statistical difference in the strengths of the MPCR effect, i.e., revealing a shared social identity does not significantly change the increase in the contributions to the regional public good when its MPCR increases.

The non-parametric test of hypothesis 2 is supported by regression analysis. Table 4.2 reports the results of an OLS model of participants' contributions to either the private, local, or regional account. The baseline are the contributions of subjects assigned to a treatment without social identity invoked (NOLABEL) and at an MPCR of 0.25 for the

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<sup>47</sup>As already noticed by Fellner and Lünser (2014), the results of Blackwell and McKee (2003) have to be interpreted with some caution as they are derived from only one independent observation per treatment.

<sup>48</sup>Beekman et al. (2017) induce conflicts to create out-group hostility and deepen in-group favoritism in a ML-PGG. They hypothesize that the effect of an increase in the MPCR might be smaller in cases where groups have previously competed against one another than in cases where groups share no previous interaction. However, they do not report statistical tests on size differences in treatment effects. Potential differences to our results would not be surprising, since we abstain from inducing conflicts and instead concentrate on a setting where preexisting identities might play a role.



Table 4.2: Individual contributions - full sample

	1	2	3	4	5	6
	$q^p$	$q^l$	$q^r$	$q^p$	$q^l$	$q^r$
	Private	Local	Regional	Private	Local	Regional
<i>HIGH</i>	-0.27 (0.260)	-1.49*** (0.212)	1.76*** (0.309)	0.26 (0.263)	-1.61*** (0.215)	1.87*** (0.311)
<i>LABEL</i>	-0.08 (0.241)	0.14 (0.244)	-0.07 (0.282)	-0.04 (0.249)	0.07 (0.252)	-0.03 (0.296)
<i>HIGH</i> × <i>LABEL</i>	0.09 (0.367)	0.06 (0.309)	-0.16 (0.444)	0.08 (0.377)	0.22 (0.317)	-0.23 (0.456)
Constant	2.33*** (0.169)	2.78*** (0.169)	2.88*** (0.188)	3.09*** (0.606)	2.59*** (0.547)	2.33*** (0.736)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	616	616	616	602	602	602

Note: OLS regressions,  $q^p, q^l, q^r \in [0, 8]$ , with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Statistically significant results reported above are robust to jointly estimating regressions (2), (3) and respectively (5), (6) as seemingly unrelated regression equations. Controls: *age*, *female*, *income*, *education*, *religious*, years of residency in the *neighborhood*, years of residency in the *metropolitan region*.

RPG (LOW). The dummy variable (*LABEL*) indicates assignment to treatment condition *LABEL* and the dummy variable *HIGH* assignment to a treatment with an MPCR of 0.5 for the RPG. The variable of interest is the interaction term of the two dummies (*HIGH* × *LABEL*), which captures whether contributions respond differently to a change in the MPCR when participants share a common local affiliation. We estimate both a simple model (first three columns of coefficients) as well as a richer model with further controls (second three columns). The controls comprise individual characteristics collected in the post-questionnaire: *age*, gender (*female*), *income*, *years of education*, the degree of *religious* affiliation (based on a five-point Likert scale from 1 “not at all” to 5 “extremely”), the time of living in the respective neighborhood (*neighborhood*) and in the metropolitan region (*region*). Furthermore, regressions in the second three columns contain dummy variables for the different neighborhoods in order to take neighborhood fixed effects into account.

The regression results reaffirm results 1 and 2: The coefficients associated with the dummy variable *HIGH* show that at a higher MPCR, average contributions to the RPG are higher and contributions to the LPG lower, supporting result 1. Also note that the coefficients estimated for *LABEL* have the predicted sign (positive for the LPG, neg-

ative for the RPG), but are small and statistically insignificant for all three accounts. Hence, while the dummy capturing the interaction effect is also insignificant throughout, supporting result 2 that there is no interaction effect, the overall relatively low level of parochialism prompts us to challenge our findings by a series of robustness checks.

#### 4.4.2 Robustness check I: Pre-existing heterogeneity in place attachment

The experimental procedures introduce two sources of heterogeneity in predisposition towards a local bias. One of the sources are pre-existing differences in place attachment: Some citizens identify much more with their own neighborhood than others. Following Candelo et al. (2017), the strength of social identity is essential, and for subjects for whom place attachment is relatively unimportant for their social identity, revealing a shared local affiliation may not be sufficient to induce a change in contribution behavior. Subjects with strong place attachment, on the other hand, may respond more strongly than the average person to such information. This is important for two reasons: One is that there are spatial contexts in which neighborhood attachment is very prominent (such as sectarian cities with minimal intra-neighborhood mobility, see, e.g., Meier et al. 2016). It is therefore useful to understand whether those for whom place attachment is an important component of social identity differ in their propensity to level up from the rest of the population. The other is that in real world settings in which contribution decisions to PG often have a sequential dimension, heterogeneity in the interaction effect could incite subjects with strong neighborhood attachment to be the first to contribute to the local PG, thus conceivably setting in motion a path towards parochialism that other participants subsequently follow (Vesterlund 2003; Andreoni and Petrie 2004).

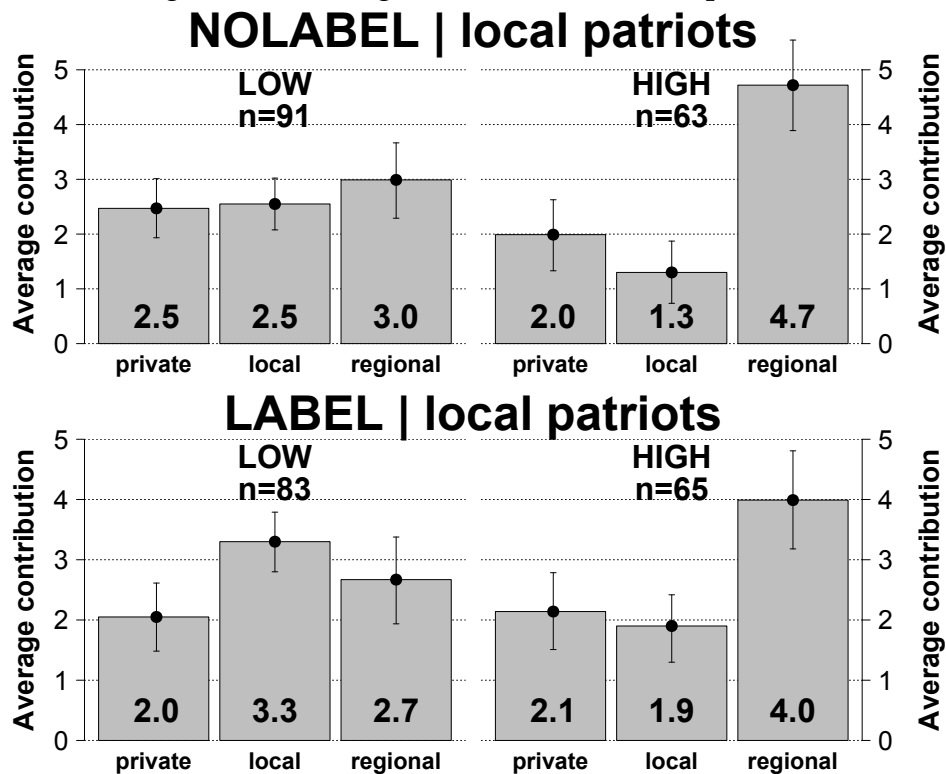
To test for the possibility of a heterogeneous interaction effect, we first identify the subsample of subjects for whom place attachment is likely to matter most. This identification relies on a composite index that measures the degree of group identity based on five questions concerning participants' affiliation with the neighborhood. Those above the median index value exhibit above-median place attachment.<sup>49</sup> As a shorthand, we refer to this group as "local patriots". Figure 4.3 plots the contribution behavior of local patriots in all four treatments.

As a first construct validity test, we find that local patriots, i.e., those that express

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<sup>49</sup>Place attachment is measured as the degree to which individuals agreed to the following five items: "deliberately decided to live in their neighborhood", "happy to live in their neighborhood", "proud to live in their neighborhood", "feel comfortable to live in their neighborhood", and "perceive identification" with their neighborhood" each measured on a five-point likert scale from 1 "not at all" to 5 "extremely".

Figure 4.3: Average contributions - local patriots



Note: Average contributions to private, local and regional account in the NOLABEL (upper half) and LABEL (lower half) of LOW (left) and HIGH (right) for the subsample of subjects with above-median place attachment (*local patriots*). Confidence intervals at the 95%-level.

above-median place attachment, exhibit a distinct contribution behavior compared to the average subject discussed in the previous section. For example, local patriots contribute significantly more to the LPG than subjects with a below median place attachment, but only when a shared neighborhood affiliation in the small group is revealed (LABEL), irrespective of the MPCR condition (LOW: 3.3 vs. 2.5,  $p$ -value: 0.013, MW-U test; HIGH: 1.9 vs. 1.3,  $p$ -value: 0.003, MW-U test). Unaware of the shared neighborhood in the small group (NOLABEL), their contribution behavior is indistinguishable from the rest of the sample (LOW: 2.5 vs. 3.0,  $p$ -value: 0.176, MW-U test; HIGH: 1.3 vs. 1.3,  $p$ -value: 0.933; MW-U test).<sup>50</sup> Local patriots therefore not only express above-median place attachment, they also contribute significantly more to the LPG than other subjects if and only if they know that the local group consists of neighbors.

In light of how local patriots' contribution behavior differs from that of other subjects, the presence and nature of the MPCR effect and the presence of an interaction

<sup>50</sup>This is consistent with the observation that local patriots also respond more strongly and statistically significantly to the social identity treatment than others. Knowledge about a shared neighborhood in the small group makes local patriots increase their contributions to the LPG by 1.2 (LOW MPCR,  $p$ -value < 0.05) and by 0.6 (HIGH MPCR,  $p$ -value < 0.10) relative to those without strong neighborhood attachment.

effect between MPCR and social identity information are obvious next questions. Hypothesis 3 predicts that this interaction effect will be present and, in light of result 2, significantly negative. To answer these questions, we first test for the MPCR effect by comparing in both social identity conditions the change in contributions to the RPG as the MPCR of the RPG doubles. Doubling the MPCR raises contributions to the RPG among local patriots by €1.7 ( $p$ -value: 0.000, MW-U test) without knowledge of shared neighborhood (NOLABEL) and by €1.3 ( $p$ -value: 0.000, MW-U test) with knowledge of shared neighborhood (LABEL). This finding reaffirms result 1: Local patriots also exhibit the positive MPCR effect. Testing hypothesis 3 requires a comparison of the MPCR effects across social identity treatment. Table 4.3 reports the coefficients of the regression analysis conducted for the reduced sample. The results reaffirm the MPCR effect (dummy HIGH) as well as the positive impact of revealed shared neighborhood on contributions to the LPG. The interaction effect, however, does not deliver statistically significant results.<sup>51</sup>

**Result 3 (leveling up by local patriots):** For subjects with above-median place attachment, revelation of neighborhood ties does not result in less leveling up: Despite the fact that we find a positive label effect for the local patriots (i.e., higher average contributions to the local account among subjects in the LABEL treatment), there is no statistical difference in the strengths of the MPCR effect.

Taken together, these findings have a number of implications. One is that the MPCR effect and the parochial bias affect contributions independently. Local patriots behave more cooperatively towards others when they are aware that others are also locals and they respond to changes in the MPCR of a PG in the predicted way. However, their response to a change in the MPCR is not modulated by parochialism. Put differently, the parochialism observed in local patriots, as evidenced in higher contributions to the LPG, is independent of how productive it is to provide the PG at a higher level.<sup>52</sup> This

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<sup>51</sup>This null result is not driven by our categorization of *local patriots* based on the above median identification. Applying stricter definitions of local patriots (such as the upper quartile of all participants) does not alter our results and does not provide statistical evidence for a significant interaction effect.

<sup>52</sup>One might worry that the absence of an interaction effect between LABEL and HIGH among local patriots is driven by the parametrization of our ML-PGG. In HIGH, the MPCRs ( $\alpha^l = \alpha^r = 0.5$ ) are equal for both the LPG and the RPG. Thus, subjects can benefit their close neighbors equally by either contributing to the LPG or the RPG. Contributing to the RPG generates additional benefits to non-neighbors at no cost to neighbors. This need not but might affect the intensity of a potential interaction effect depending on the importance of parochial motives. To examine this possibility further, we conduct, as an additional robustness check, a complementary treatment (MIX) with additional  $n = 116$  subjects. In MIX, the LPG has an MPCR of 0.5 while the MPCR in the RPG differs for neighbors (0.25) and non-neighbors (0.75). Contributing to the RPG therefore involves a trade-off between overall efficiency levels and benefiting the local group. We report the results of this robustness check in the appendix of the paper (Table 4.7). With respect to contribution patterns, there is

Table 4.3: Individual contributions - local patriots

	1	2	3	4	5	6
	$q^p$	$q^l$	$q^r$	$q^p$	$q^l$	$q^r$
	Private	Local	Regional	Private	Local	Regional
<i>HIGH</i>	-0.49 (0.398)	-1.25*** (0.307)	1.74*** (0.472)	-0.43 (0.399)	-1.33*** (0.318)	1.76*** (0.482)
<i>LABEL</i>	-0.42 (0.341)	0.75** (0.355)	-0.32 (0.416)	-0.45 (0.350)	0.77** (0.377)	-0.32 (0.450)
<i>HIGH</i> × <i>LABEL</i>	0.59 (0.530)	-0.19 (0.448)	-0.40 (0.646)	0.51 (0.532)	-0.14 (0.474)	-0.37 (0.671)
Constant	2.47*** (0.261)	2.55*** (0.248)	2.99*** (0.299)	4.17*** (0.889)	1.93*** (0.779)	1.90*** (1.11)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	302	302	302	294	294	294

Note: OLS regressions,  $q^p, q^l, q^r \in [0, 8]$ , with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Statistically significant results reported above are robust to jointly estimating regressions (2), (3) and respectively (5), (6) as seemingly unrelated regression equations. Controls: *age, female, income, education, religious*, years of residency in the *neighborhood*, years of residency in the *metropolitan region*.

independence, in turn, adds robustness to the sample average that is reported in result 2: Since the parochial bias of local patriots does not interact with the MPCR effect, it is clear that result 2, the zero effect on average, is not the outcome of countervailing effects among those with strong and those with weak place attachment.

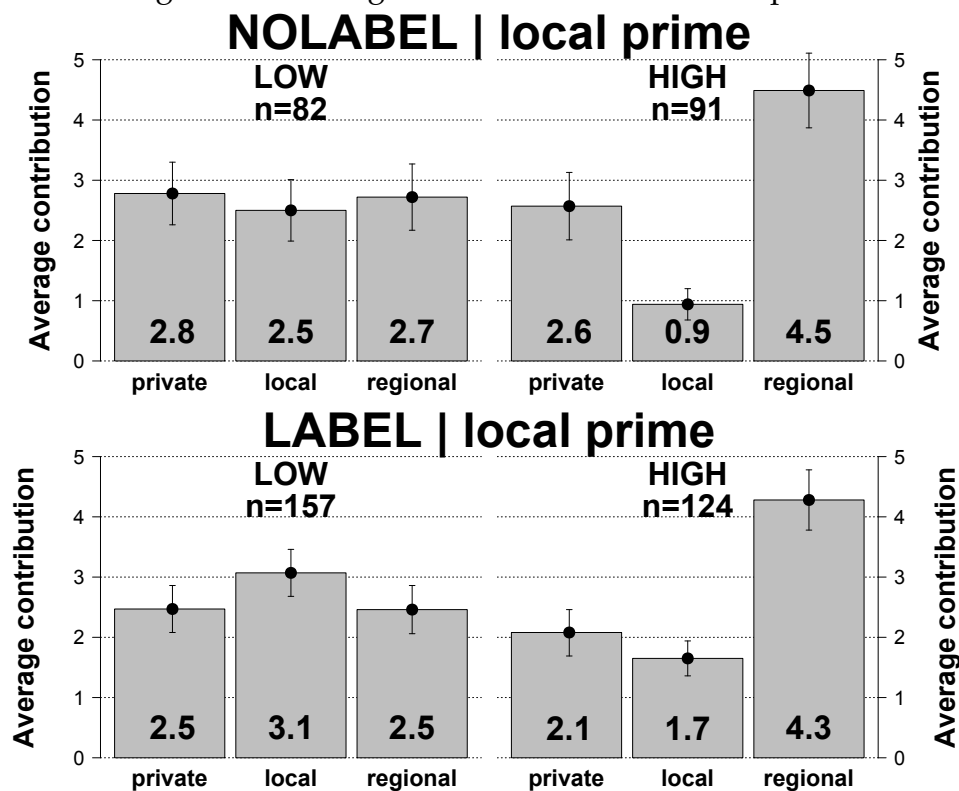
#### 4.4.3 Robustness check II: Priming for place attachment

Despite relying on naturally occurring forms of social identity that are expected to affect behavior more substantially than minimal group identity, a conceivable objection to our experimental design could be a concern that it insufficiently stimulates an existing predisposition towards behaving parochially. If true, the results based on observed behavior in the social identity treatments LABEL/NOLABEL would underestimate the true effect of social identity. A robustness check based on pre-existing heterogeneity in place attachment, as that conducted in the previous subsection, would not provide

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again no interaction effect between LABEL and MPCR in this treatment conditions, neither for the full sample (Table 4.7 - Panel A) nor for the subsample of local patriots (Table 4.7 - Panel B). We are therefore confident that the absence of an interaction effect between HIGH and LABEL is not exclusively driven by our parametrization of the MPCRs implemented in the HIGH treatment.

Figure 4.4: Average contributions - after local prime



Note: Average contributions in stage 2 to private, local and regional account in the NOLABEL | local prime (upper half) and LABEL | local prime (lower half) of LOW (left) and HIGH (right) for the sample restricted to locally primed subjects. Confidence intervals at the 95%-level.

a remedy if a social identity stimulus was indeed insufficient because the local patriots subsample is defined relative to the sample median rather than to an absolute benchmark.

To examine whether result 2 is robust against the possibility of an insufficient experimental stimulus, we use observations from stage 2 of the experiment, i.e., after the priming task. A total of 454 subjects underwent the local priming version that is of interest here. As a first test, we compare the behavior between the LABEL and the NOLABEL treatment in stage 2 across the MPCR conditions. Figure 4.4 plots the stage 2 contribution decisions across treatments for all locally primed subjects. As in the previous tests, there is a strong MPCR effect on contributions to the RPG in the control group under the NOLABEL condition. Both size and significance of the effects are comparable to the previous results, reaffirming result 1.<sup>53</sup>

Also, priming has the expected effect on contribution behavior, providing a manipulation check on the priming procedure: Locally primed subjects in the LABEL treatment

<sup>53</sup>Contributions to the RPG increase significantly (2.7 vs. 4.5,  $p$ -value: 0.000) for a doubling of the MPCR while contributions to the LPG decrease significantly (2.5 vs. 0.9,  $p$ -value: 0.000).

Table 4.4: Individual contributions - full sample - after local prime

	1	2	3	4	5	6
	$q^p$	$q^l$	$q^r$	$q^p$	$q^l$	$q^r$
	Private	Local	Regional	Private	Local	Regional
<i>HIGH</i>	-0.21 (0.383)	-1.56*** (0.287)	1.77*** (0.416)	-0.28 (0.402)	-1.50*** (0.303)	1.78*** (0.435)
<i>LABEL</i>	-0.31 (0.328)	0.57* (0.323)	-0.26 (0.343)	-0.34 (0.345)	0.59* (0.342)	-0.26 (0.368)
<i>HIGH</i> × <i>LABEL</i>	-0.18 (0.472)	0.14 (0.378)	0.05 (0.528)	-0.08 (0.495)	-0.07 (0.396)	0.15 (0.554)
Constant	2.78*** (0.261)	2.50*** (0.255)	2.72*** (0.277)	3.15*** (0.756)	1.93*** (0.646)	2.92*** (0.911)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	454	454	454	443	443	443

Note: OLS regressions,  $q^p, q^l, q^r \in [0, 8]$ , with robust standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . Statistically significant results reported above are robust to jointly estimating regressions (2), (3) and respectively (5), (6) as seemingly unrelated regression equations. Controls: *age, female, income, education, religious*, years of residency in the *neighborhood*, years of residency in the *metropolitan region*.

have significantly higher average contributions to the LPG than the control group both at a LOW MPCR (3.1 vs. 2.5,  $p$ -value: 0.054, MW-U test) and a HIGH MPCR (1.7 vs. 0.9,  $p$ -value: 0.001, MW-U test).<sup>54</sup>

Despite these effects, the interaction effect between an increase in the MPCR and invoking social identity is again insignificant. Table 4.4 reports the results of the regression analysis of the contributions decisions using the same estimation strategy as for Table 4.2 and Table 4.3. The results are similar to those for the restricted sample of stage 1 contributions by subjects with above-median place attachment presented in Table 4.3. The coefficient estimates in Table 4.4 reaffirm a positive MPCR effect (dummy *HIGH*) that leads to a substitution away from LPG to RPG. The results in Table 4.4 also confirm the presence of a pro-local bias induced by the revelation of shared neighborhood affiliation (dummy *LABEL*): Contributions to the LPG are higher. At the same time, Table 4.4 also reaffirms the lack of an interaction effect: The change in contributions caused by a doubling of the MPCR in the RPG is statistically indistinguishable between sub-

<sup>54</sup>While in LOW these additional contributions to the LPG accrue at the expense of lower contributions to both the private account (-0.3) and the RPG (-0.2), in HIGH we find additional contributions to the LPG primarily driven by lower contributions to the private account (-0.5) and a moderate decrease in RPG contributions (-0.2).

Table 4.5: Individual contributions - restricted sample - after local prime

	1	2	3	4	5	6
	$q^p$	$q^l$	$q^r$	$q^p$	$q^l$	$q^r$
	Private	Local	Regional	Private	Local	Regional
<i>HIGH</i>	-0.02 (0.524)	-1.99*** (0.441)	2.00*** (0.558)	-0.00 (0.565)	-2.06*** (0.485)	2.06*** (0.589)
<i>LABEL</i>	-0.04 (0.465)	0.33 (0.490)	-0.28 (0.471)	0.04 (0.510)	0.27 (0.530)	-0.32 (0.512)
<i>HIGH</i> × <i>LABEL</i>	-0.44 (0.663)	0.31 (0.561)	0.13 (0.724)	-0.52 (0.727)	0.30 (0.617)	0.23 (0.790)
Constant	2.58*** (0.359)	2.98*** (0.399)	2.44*** (0.380)	3.51*** (1.09)	2.41*** (0.930)	2.08* (1.22)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	229	229	229	225	225	225

Note: OLS regressions,  $q^p, q^l, q^r \in [0, 8]$ , with robust standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . Statistically significant results reported above are robust to jointly estimating regressions (2), (3) and respectively (5), (6) as seemingly unrelated regression equations. Controls: *age, female, income, education, religious*, years of residency in the *neighborhood*, years of residency in the *metropolitan region*.

jects primed for neighborhood attachment to whom a shared neighborhood affiliation in the local group is disclosed and primed subjects to whom it is not disclosed.

As a second test, we rerun the regression on a restricted sample of 229 subjects that were allocated to the same social identity condition in both stages. This eliminates a potential attenuation of the treatment effects in the NOLABEL treatment in stage 2 by subjects who were assigned to the LABEL treatment in stage 1 and were therefore aware of the composition of the small group. Table 4.5 reports the results of this exercise, which are in line with the evidence from the full sample.<sup>55</sup> Jointly, these tests of the effect of the local priming procedure on contributions leads to our final result.

**Result 4 (no interaction effect through priming):** For subjects who have undergone the local priming procedure, revelation of neighborhood ties does not result in less leveling up: Even though priming results in a positive label effect (i.e., higher average contributions to the local account among subjects in the LABEL treatment), there is still no statistical difference in the strengths of the MPCR effect

<sup>55</sup>Additional tests on other subsamples in which the first stage treatment assignment are taken into account in various ways were also conducted. These tests reaffirm the results reported in the main text.



exhibited by subjects primed for place attachment across the social identity treatment conditions.

This result also supports the previous finding that the MPCR and the social identity effects are unrelated, even when the propensity for place attachment is procedurally activated through a priming task.

#### **4.5 Summary and concluding remarks**

The starting point of this paper was the question whether naturally grown social identity, a well established source of biases in other-regarding behavior, also negatively affects the efficiency of multi-level public goods provision due to parochial concerns. Building on the experimental paradigm of the multi-level public goods game and the well-established concept of neighborhood attachment, it tests whether subjects who know that their contributions to the lower level public good specifically benefit their neighbors respond less to a higher MPCR in the higher level public good than subjects who are unaware of the shared neighborhood attachment.

Our evidence from an artefactual field experiment design brings three novel elements to bear on the question. One element is the field context that favors naturally grown social identity as a behavioral driver of parochial concerns. The second element is the particular neighborhood-within-a-region setting that allows parochialism to naturally assert itself at the local level. The third is a two-by-two design that varies both the public good's productivity across levels and the salience of social identity. This two-by-two design makes disentangling both dimensions in a formal test possible. Jointly, these elements confirm previous evidence that there is a positive MPCR effect in multi-level public goods, but also that level-wise allocations of public goods contributions do not efficiently respond to relative total productivities. Our results, including several robustness checks, show, however, that inefficiencies of level-wise allocations need not reflect parochialism. Even though subjects with above-median neighborhood attachment and subjects that have been procedurally primed on their neighborhood attachment exhibit clear evidence of parochialism and behave more cooperatively towards the local group if they know that the local group consists of neighbors, their response to a change in the MPCR is statistically identical to a situation where local identity and parochial bias cannot play any role by design. Most importantly, our results challenge the hypothesis that a shared social identity in the smaller group makes the average subject less responsive to a higher efficiency of contributing to the larger group. Parochialism, in other words, does not stand in the way of efficiency. This result is robust towards individual heterogeneity in social identity as it holds for subjects with above-

median neighborhood attachment and subjects primed towards such attachment, even though both groups exhibit clear evidence of parochialism. In sum, therefore, we find that even naturally grown types of social identity do not necessarily imply a parochialism penalty on efficiency in a situation in which public goods can be provided at more than one spatial level.<sup>56</sup>

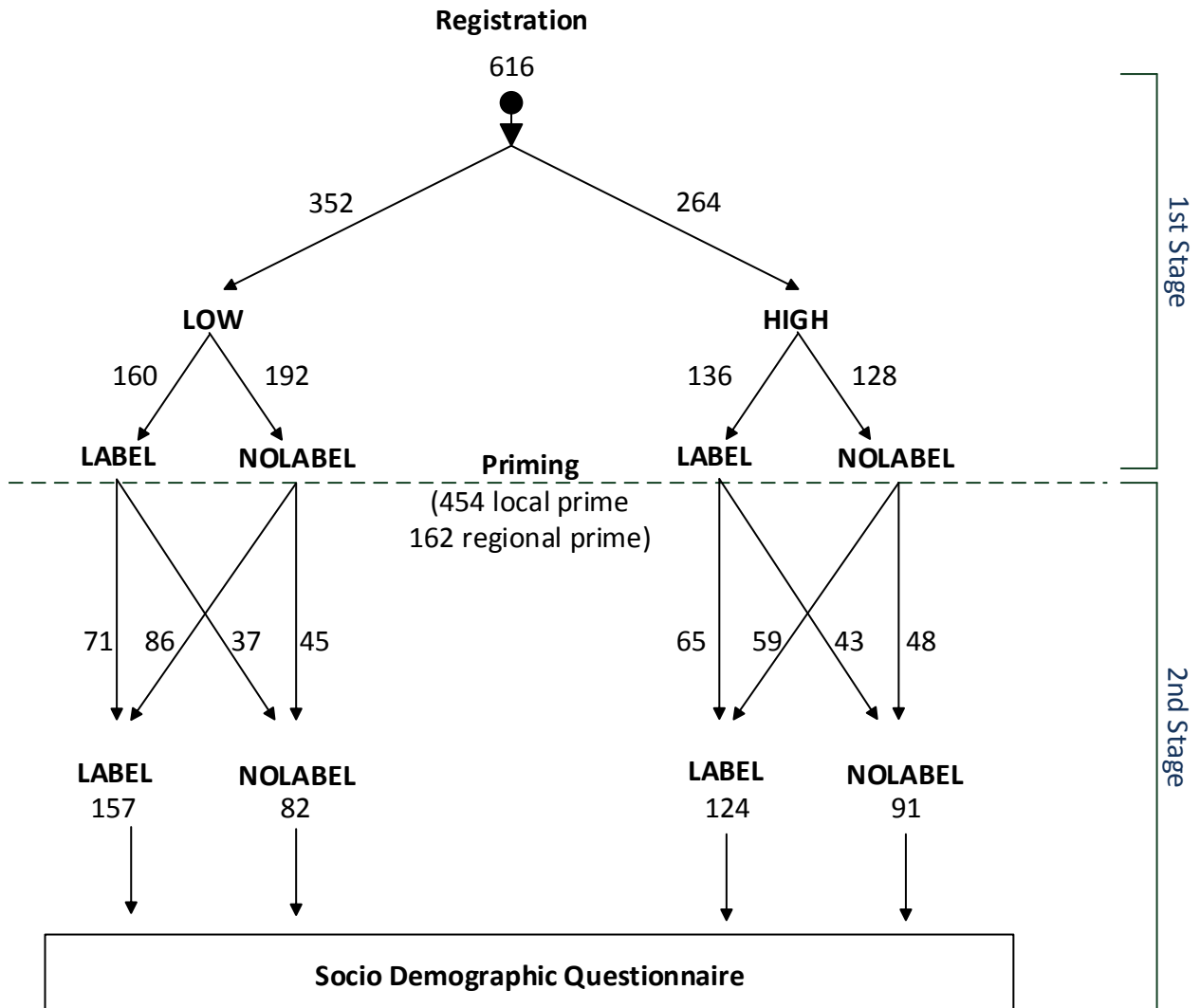
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<sup>56</sup>Note that we are not claiming that the private provision of public goods will be efficient, which would run counter to an overwhelming body of empirical and experimental evidence. We are only claiming that social identity will not increase the inefficiencies inherent in the social dilemma of PG provision.

## 4.6 Appendix

### 4.6.1 Figures

Figure 4.5: Illustration of the experimental procedure



Note: Schematic diagram of the experimental procedure and number of subjects in each treatment. At the beginning of stage 1, subjects are randomly assigned to LOW or HIGH and to NOLABEL or LABEL. Right after stage 1, subjects complete a questionnaire and a writing task to prime them on their local (local prime) or regional (regional prime) common identity. In stage 2, subjects are again randomly assigned to NOLABEL or LABEL. Finally, subjects complete a post-experimental questionnaire.

4.6.2 Tables

Table 4.6: Sample characteristics

	Total	Heidelberg		Mannheim	
		Bahnstadt	Neuenheim	Feudenheim	Schwetzingenstadt / Oststadt
Subjects (in #)	616	146	177	109	184
Age (in years)	35.6	29.0	38.1	47.2	31.5
Female (in %)	45.7	47.9	48.9	42.6	42.6
Income (in EUR)	2,087	2,027	2,117	2,550	1,832
Education (in years)	14.8	14.4	15.4	14.3	15.0
Religion	2.33	2.33	2.31	2.43	2.30
Region (in years)	17.5	7.4	19.5	35.2	13.2
Neighborhood (in years)	8.9	1.6	10.8	20.6	5.9
Local Identity Index	-0.04	-0.12	-0.05	0.29	-0.16
Local Patriots (in %)	49	45	47	65	44

Note: "Age" (in years); "Female" (female dummy); "Income" (1-5): 1 indicates the lowest (up to 500 euro disposable net income per month) and 5 indicates the highest (4000 euro or more disposable net income per month); "Education" (in years of education); Religion (1-5): 1 indicates no affiliation with religious community and 5 indicates very high affiliation with religious community; "Region" (years living in the Rhine-Neckar Metropolitan Region); "Neighborhood" (years living in city district); "Local Identity Index" (-1-1): -1 indicates very low local place attachment and 1 indicates very high local place attachment; "Local Patriots" (dummy for participants with above median "Local Identity Index").





Table 4.7: Individual contributions - complementary treatment

	1	2	3	4	5	6
	$q^p$	$q^l$	$q^r$	$q^p$	$q^l$	$q^r$
	Private	Local	Regional	Private	Local	Regional
Panel A - Full sample						
<i>MIX</i>	0.18 (0.39)	-0.74** (0.29)	0.55 (0.44)	0.22 (0.40)	-0.69** (0.29)	0.47 (0.46)
<i>LABEL</i>	-0.08 (0.24)	0.14 (0.24)	-0.07 (0.28)	-0.01 (0.25)	0.09 (0.25)	-0.08 (0.30)
<i>MIX</i> x <i>LABEL</i>	0.22 (0.53)	0.45 (0.44)	-0.66 (0.60)	0.17 (0.53)	0.46 (0.44)	-0.63 (0.61)
Constant	2.34*** (0.17)	2.78*** (0.17)	2.88*** (0.19)	2.58*** (0.73)	2.65*** (0.68)	2.77*** (0.88)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	468	468	468	454	454	454
Panel B - Local patriots						
<i>MIX</i>	-0.61 (0.50)	-0.46 (0.43)	1.07* (0.64)	-0.57 (0.52)	-0.56 (0.42)	1.13* (0.67)
<i>LABEL</i>	-0.42 (0.34)	0.75** (0.35)	-0.32 (0.42)	-0.49 (0.35)	0.77** (0.38)	-0.28 (0.45)
<i>MIX</i> x <i>LABEL</i>	1.22* (0.69)	-0.17 (0.61)	-1.05 (0.83)	1.34* (0.70)	-0.11 (0.66)	-1.24 (0.86)
Constant	2.47*** (0.26)	2.55*** (0.25)	2.98*** (0.30)	4.20*** (1.07)	1.77*** (0.97)	2.03*** (1.33)
Controls	no	no	no	yes	yes	yes
Neighborhood FE	no	no	no	yes	yes	yes
# of observations	236	236	236	229	229	229

Note: OLS regressions,  $q^p, q^l, q^r \in [0, 8]$ . Robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Statistically significant results reported above are robust to jointly estimating regressions (2), (3) and respectively (5), (6) as seemingly unrelated regression equations. Controls: age, female, income, education, religious, years of residency in the neighborhood, years of residency in the metropolitan region. In *MIX* the MPCR for the LPG is set at  $\alpha^l = 0.5$  and the total benefits (TB) of a one unit contribution to the LPG is given by 2 units. The MPCR for the RPG differs between members of the local group (neighbors) and members of the regional group from a different neighborhood (non-neighbors). The MPCR for the RPG is given by  $\alpha^r_{neighbor} = 0.25$  for neighbors and  $\alpha^r_{non-neighbor} = 0.75$  for non-neighbors. Therefore, contributing to the RPG in *MIX* involves a trade-off in term of benefiting neighbors. A one unit contribution to the RPG corresponds to a TB of 4 units, which is larger than the TB of the LPG.

### 4.6.3 Instructions

#### Invitation letter - First page [Translated from German]<sup>†</sup>

	<p>UNIVERSITÄT HEIDELBERG ZUKUNFT SEIT 1386</p>	 <p>Zentrum für Europäische Wirtschaftsforschung GmbH</p>
<p>– Invitation –</p>		
<p style="text-align: right;">June 2015</p>		
<p>Invitation to participate in a scientific study</p>		
<p>Dear Sir or Madam,</p>		
<p>The University of Heidelberg and the Centre for European Economic Research (ZEW) in Mannheim are jointly carrying out a scientific study on individual choice behaviour. This research project is funded by the Federal Ministry of Education and Research.</p>		
<p>Your household has been chosen randomly to actively support our research project by participating in a 15-minute online survey. You will receive a reward of 5 euros for your participation in any case. The choices you will make in the online survey will further increase the amount of money.</p>		
<p>No special previous knowledge is required. Please note that only one person per household can participate in this study and this person needs to have attained full age.</p>		
<p>You may immediately start with the online survey. To do so, please, register at the following website:</p>		
<p style="text-align: center;"><a href="http://www.zew.de/umfrage2015">www.zew.de/umfrage2015</a></p>		
<p>Your personal access key (valid until 28.06.2015) is:                      &lt;&lt;CODE&gt;&gt;</p>		
<p>Please, contact Dr Daniel Römer if you have any questions by calling 0621/1235-214 or send an email to <a href="mailto:umfrage@zew.de">umfrage@zew.de</a>. For further information, please, refer to the back of the page.</p>		
<p>We are looking forward to your participation in this survey that surely is also of interest to you, and thank you very much for supporting this research project.</p>		
		
<p>Prof Timo Goeschl, Ph.D.</p>		<p>Dr Daniel Römer</p>
<p>Research Centre for Environmental Economics Alfred Weber Institute of Economics University of Heidelberg</p>		<p>Environmental and Resource Economics, Environmental Management Centre for European Economic Research</p>

<sup>†</sup>Explanatory notes are given in square brackets.

## Invitation letter - Second page

Information sheet on content and procedure of the study

Who is organising this study?

This study is part of a joint research project of the University of Heidelberg and the Centre for European Economic Research (ZEW) in Mannheim. The University of Heidelberg is a public institute for education and research of the federal state Baden-Wuerttemberg. The ZEW is a non-profit research institute and member of the Leibniz Association. This research project is funded by the Federal Ministry of Education and Research.

How can I participate?

You may participate starting from today until at the latest 28.06.2015 (as long as the maximum number of participants has not been reached). You only need a device (e.g. computer, laptop, tablet or smartphone) with internet access and internet browser (e.g. Internet Explorer, Firefox, Safari, Chrome).

- **Computer/laptop:** To register, please, enter [www.zew.de/umfrage2015](http://www.zew.de/umfrage2015) into the address bar of your internet browser. It leads you to the start screen where you need to enter your personal access key. The access key is on the first page of the invitation letter. This access key allows you to participate in the study one time only. The online survey starts immediately after you have entered the key.
- **Tablet/smartphone:** To register, please, enter [www.zew.de/umfrage2015](http://www.zew.de/umfrage2015) into the address bar of your internet browser. Alternatively, you may also use an adequate app to read the QR code beside this paragraph. Both options lead you to the start screen where you need to enter your personal access key. The access key is on the first page of the invitation letter. This access key allows you to participate in the study one time only. The online survey starts immediately after you have entered the key.



What about my data?

The information you give is exclusively used for research purposes and analysed anonymously. Your personal data are exclusively used to transfer your reward to you and are neither related to the information given in the survey nor given to any third-party.

What influences the amount of reward I will receive?

Your reward consists of a standard reward and an additional reward. In any case, you will receive the standard reward of 5 euros for participating. The amount of the additional reward depends on your own choices and those of the other participants.

How will I receive my reward?

We want to transfer your reward to you without you having to give us your bank data. Therefore, you will receive a shopping voucher by mail about 4 weeks after participating. The value of the voucher equals the total amount of your reward (standard reward of 5 euros plus additional reward). You may use the voucher in many different local stores and online shops to pay for your shopping (e. g. Galeria Kaufhof, Karstadt, Media Markt, Saturn – for the complete list of stores accepting the voucher, please, refer to this website: <http://www.edenred.de/produkte/ticket-shopping-card/akzeptanzpartner.html>)

### Instructions and questionnaire

*[Translated from German. We use braces {} to indicate the alternative wording (LABEL vs. NOLABEL) and parameters (HIGH vs. LOW) used in the different treatments. The term city district (set in italics) stands for one of the four different municipal districts Bahnstadt, Feudenheim, Neuenheim, and Schwetzingen/Oststadt, depending on where the respective participant lives. In the following, we show the wording for the LOW-NOLABEL as the benchmark case and as described above, we also show the deviations in the other treatments by using square brackets.]<sup>†</sup>*

#### - Screen 1 -

### Welcome to our research study!

Dear participant, Thank you for showing interest in this research study. You can start with the tasks immediately. On this screen you can find the most important information regarding your participation:

- Participating takes on average 15 minutes.
- There are two tasks and some questionnaires.
- All tasks will be precisely explained to you in the course of the study.
- All instructions will be carried out as described:
  - By “money” we mean real amounts of money which will be definitely paid out to you.
  - By “other participants” we mean real people who also participate in this study just like you.

Note: Please always use the buttons provided and not your internet browser for navigation because otherwise, a successful completion of the survey cannot be guaranteed.

#### - Screen 2 -

**Your payment for participating consists of two parts:** In any case, you will receive a fixed amount of 5 euros if you complete both tasks and complete the questionnaires.

- In task 1 or task 2, an additional payment may emerge for you and the other study participants.
- A random procedure (comparable with a coin flip) at the end of the study will determine whether you receive the payment from either task 1 or task 2. Both outcomes are equally likely.
- We will definitely select and pay out one of the two tasks. You will receive the respective payment additionally to the fixed amount.

Thus, your total payment for participating in the study consists of the following:

---

<sup>†</sup>Explanatory notes are given in square brackets.



Your total payment = 5 euros + payment from either task 1 **or** task 2

- Screen 3 -

### Explanation and procedure of task 1

In this section, you can earn money in addition to the fixed amount.

**Who are the other participants?** All in all, 8 subjects actively take part in this decision task, namely you and 7 more participants. {LABEL: Apart from you, 3 other participants are, like you, inhabitants of city district. The other 4 participants are not inhabitants of city district but are from another area located in the Rhine-Neckar Metropolitan Region.}

**What is your task?** In this task, you and all other participants are provided with 8 euros at your free disposal. Please note that you receive this amount of money in addition to the fixed amount. Your task is to decide on how to distribute the 8 euros across three different pots. Please note that the whole amount of 8 euros has to be distributed completely to the three different pots. Please also note that the other participants face the same decision situation as you do.

**How do the pots differ from each other?** At the end of the study, the total sum of money allocated to all three pots is paid out to you and to the other participants according to the rules explained below. It depends on the pot (A, B, or C) whether either you, or you and 3 other participants, or you and 7 other participants benefit from the payment. Additionally, the total sum of money allocated to a certain pot may be multiplied before any payment from this pot is received.

**Pot A (private):** Only you can put an amount of money into this pot. For every euro that is contributed into this pot, you will receive 1 euro. The 7 other participants do not receive any payment from your pot. However, every participant owns a respective pot A as well.

**Pot B (4 participants {LABEL: city district}):** Apart from you, there are 3 more participants {LABEL: from city district} (in total, 4 participants) who can put an amount of money into this pot. Every euro which is contributed to this pot will be doubled and the respective sum will be equally distributed to all 4 participants. For every euro that is put into Pot B, you and the 3 other participants {LABEL: from city district} will consequently receive 0.50 euro each. The other group, which also comprises 4 participants, will not receive any payment from your Pot B. However, the other group owns a respective Pot B as well.

**Pot C (8 participants {LABEL: Rhine-Neckar Metropolitan Region}):** Apart from you, there are 7 other participants {LABEL: from the Rhine-Neckar Metropolitan Region} (in total 8 participants) who can put an amount of money into this pot. Every euro which

is put into this pot will be doubled {HIGH: quadrupled} and the respective sum will be equally distributed to all 8 participants. For every euro that is put into Pot C, you and the 7 other participants {LABEL: from the Rhine-Neckar Metropolitan Region} will consequently receive 0.25 euros {HIGH: 0.50 euros} each.

If task 1 is chosen for payment, your total payment is composed of the following elements:

$$\text{Total payment} = 5 \text{ euro (lump sum)} + 1.0 \times (\text{sum of euros of Pot A}) + 0.5 \times (\text{sum of euros of Pot B}) + 0.25 \text{ {high: 0.50} } \times (\text{sum of euros of Pot C})$$

### - Screen 5 -

Please enter the amount of money you want to put into each of the three pots. Please remember that you are provided with 8 euros at your free disposal for this task of decision-making and you can decide on how to distribute this amount of money to the three different pots. For this purpose, please fill out the fields by entering an amount between 0 and 8 euros:

- Pot A (private): \_\_\_€
- Pot B (4 participants {LABEL: city district}): \_\_\_€
- Pot C (8 participants {LABEL: Rhine-Neckar Metropolitan Region}): \_\_\_€

**Pot A (private):** Only you can put an amount of money into this pot. For every euro that is contributed to this pot, you will receive 1 euro. The 7 other participants do not receive any payment from your pot. However, each participant owns a respective pot A as well.

**Pot B (4 participants {LABEL: city district}):** Apart from you, there are 3 more participants {LABEL: from city district} (in total 4 participants) who can put an amount of money into this pot. Every euro which is put into this pot will be doubled and the respective sum will be equally distributed to all 4 participants. For every euro that is put into Pot B, you and the 3 other participants {LABEL: from city district} will consequently receive 0.50 euro each. The other group, which also comprises 4 participants, will not receive any payment from your Pot B. However, the other group owns a respective Pot B as well.

**Pot C (8 participants {Rhine-Neckar Metropolitan Region}):** Apart from you, there are 7 other participants {from the Rhine-Neckar Metropolitan Region} (in total 8 participants) who can put an amount of money into this pot. Every euro which is put into this pot will be doubled {HIGH: quadrupled} and the respective sum will be equally distributed to all 8 participants. For every euro that is put into Pot C, you and the

7 other participants {LABEL: from the Rhine-Neckar Metropolitan Region} will consequently receive 0.25 euros {HIGH: 0.50 euros} each.

**- Screen 6 -**

Before proceeding with task 2, we would like to gather some information about your district.

**- Screen 7 -**

Now we would like you to answer the following questions.

- How strongly do you identify yourself with the Rhine-Neckar Metropolitan Region? Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
- How strongly do you identify yourself with the district of *city district*? Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement

**- Screen 8 -**

Now we would like you to answer the following questions.

- Please indicate how much you, as an inhabitant of *city district*, agree with the following statements.
  - I deliberately decided to live in *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
  - I actively participate in local organizations and groups which mainly consist of members who are inhabitants of *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
  - I am happy to live in *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
  - I participate in neighborhood activities together with other inhabitants of *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
  - Recently, many new inhabitants have moved to city district. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
- Please describe the positive aspects of {LOCAL PRIME: city district. REGIONAL PRIME: Rhine-Neckar Metropolitan Region} briefly in your own words. Answer  
\_\_\_\_\_

**- Screen 9 -**

On the next screen, the second decision task starts. Please carefully read through the following explanations as they describe how your payment and the payments of the other study participants depend on your decision.

**- Screen 10 -**

**Explanation and procedure of task 2**

[See Screen 3]

**- Screen 11 -**

[See Screen 4]

**- Screen 12 -**

After you have made your decisions, we are interested in how you assess the behavior of the other participants in task 2.

- Regardless of your own decision: In your opinion, which decision did the other participants from your group {LABEL: from city district} make on average when they faced the same decision situation as you did?
  - Pot A: \_\_\_€
  - Pot B: \_\_\_€
  - Pot C: \_\_\_€
  
- Regardless of your own decision: In your opinion, which decision did the other participants {label: from the Rhine-Neckar Metropolitan Region} make on average when they faced the same decision situation as you did?
  - Pot A: \_\_\_€
  - Pot B: \_\_\_€
  - Pot C: \_\_\_€

**- Screen 13 -**

Now we would like to know your opinion about the comprehensibility of the tasks. Please indicate how much you agree with the following statement:

- The instructions for the tasks were clear. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement

- Regardless of your actual decision: How would you have distributed the 8 euros to Pot A, Pot B and Pot C in task 2 if it had been your aim to maximize the total payment for yourself?
  - Pot A: \_\_\_€
  - Pot B: \_\_\_€
  - Pot C: \_\_\_€
- Regardless of your actual decision: How would you have distributed the 8 euros to Pot A, Pot B and Pot C in task 2 if it had been your aim to maximize the total payment for all 8 participants in the decision task?
  - Pot A: \_\_\_€
  - Pot B: \_\_\_€
  - Pot C: \_\_\_€

**- Screen 14 -**

Thank you very much, you are almost done. Finally, we would like to ask you for some personal details.

**- Screen 15 -**

Now we would like you to answer the following questions.

- Please indicate how much you, as an inhabitant of *city district*, agree with the following statements.
  - I am proud to live in the district of *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
  - I feel comfortable in the district of *city district*. Please choose one of the following answers:  Not at all  Slightly  Moderately  15  Very  Extremely  No statement
- Self-assessment of your personality: In general, are you a person willing to take risks or are you more risk-averse? Explanation about the scale: 0 (risk-averse) to 5 (prepared to take risks) Please choose one of the following answers:  0  1  2  3  4  5  No statement
- What is your opinion about the following three statements?
  - In general, people can be trusted. Please choose one of the following answers:  Strongly agree  Disagree  Agree  Strongly agree  No statement

- Nowadays people are not reliable anymore. Please choose one of the following answers:  Strongly agree  Disagree  Agree  Strongly agree  No statement
  - In dealing with strangers, it is better to be cautious before trusting them. Please choose one of the following answers:  Strongly agree  Disagree  Agree  Strongly agree  No statement
- Do you think that most people... Please choose one of the following answers:
  - ... would take advantage of you if they had the possibility to do so...
  - ...or would rather try to be fair to you?
  - No statement
- In your opinion, would you say that most of the time people... Please choose one of the following answers:
  - ... try to be helpful...
  - ...or only pursue their own interests?
  - No statement
- Please specify your gender: Please choose one of the following answers:  Male  Female
- How old are you? \_\_\_\_\_ years
- For how long have you been living in the Rhine-Neckar Metropolitan Region? For approximately \_\_\_\_\_years
- For how long have you been living in *city district*? For approximately \_\_\_\_\_ years
- Do you consider yourself a member of a certain religious community? Please choose one of the following answers:  Not at all  Slightly  Moderately  Very  Extremely  No statement
- What are your native languages? \_\_\_\_\_
- In total, how much money does your household have at its disposal (net income) per month? Please choose one of the following answers:  Up to less than 500€  500€ to 1,000€  1,000€ to 1,500€  1,500€ to 2,000€  2,000€ to 3,000€  3,000 to 4,000€  4,000€ or more  No statement

- Which party would you vote for if there were general elections to be held on next Sunday? Please choose one of the following answers:  CDU/CSU  SPD  Bündnis 90 / Die Grünen  FDP  Die Linke  AfD  I do not vote  No statement  Other: \_\_\_\_\_
- What is the highest level of education you have received? Please choose one of the following answers:  No qualification  Secondary School Leaving Certificate [Hauptschule]  O level [Mittlere Reife]  Advanced Technical College Certificate [Fachhochschulreife]  A level / Higher Education Entrance Qualification [Abitur]  Completed apprenticeship  University Diploma / Polytechnic Degree  No statement  Other qualification: \_\_\_\_\_

**- Screen 16 -**

You will definitely receive 5 euros for your participation. For the additional payment, there will be a procedure (comparable with a coin flip) which randomly determines whether you will receive the additional payment from task 1 or from task 2. Both outcomes are equally likely. We definitely choose and pay one of the two amounts. You will receive the respective payment additionally to the fixed amount.

Your total payment = 5 euros + payment from either task 1 **or** task 2

As soon as all participants have made their decisions, we will inform you about the resulting total payment. In order to allow you to receive your total payment without having to provide your personal bank data, you will receive a shopping voucher by mail approximately 4 weeks after your participation. The value of the voucher corresponds to your total payment. You can redeem the voucher to at numerous local shops and online shops (e.g. Galeria Kaufhof, Karstadt, Media Markt, Saturn). For this purpose, please enter your address:

- Name:\_\_\_\_\_
- Surname:\_\_\_\_\_
- Street Address:\_\_\_\_\_
- Zip Code:\_\_\_\_\_
- City:\_\_\_\_\_

Note: Your address will be used only for sending the shopping voucher and will not be transferred to third parties. Moreover, the data of the questionnaires will not be linked to your address data. If you have any questions concerning this research procedure,

please do not hesitate to contact the directors of the study by calling 0621/1235-214 or by email (umfrage@zew.de).

Did you enjoy taking part in this study and do you want to participate in other scientific studies of this type? We would be pleased to add your name to our members' database and would be happy to invite you to further studies. For this purpose, we only need your e-mail address. As a matter of course, your e-mail address is not used for any other purposes and if you wish to withdraw your participation offer at any future point, you will be directly unsubscribed from the database.

- E-mail Address: \_\_\_\_\_

- Screen 17 -

**Thank you very much! Your answers were stored.**



#### 4.6.4 Screenshots

[In German]<sup>†</sup>

Screenshot of the contribution stage

**LimeSurvey**  
**Wissenschaftliche Studie**

0%  100%

Bitte teilen Sie uns mit, welchen Geldbetrag Sie in jeden der drei Töpfe legen wollen. Zur Erinnerung: Für diese Entscheidungsaufgabe stehen Ihnen insgesamt 8 Euro zur freien Verfügung und Sie können diesen Betrag beliebig auf die drei Töpfe aufteilen. Hierzu tragen Sie bitte in jedes dieser Felder einen Betrag zwischen 0 und 8 Euro ein.

Topf A (Privat):  Euro

Topf B (Schwetzingerstadt/Oststadt):  Euro

Topf C (Metropolregion Rhein-Neckar):  Euro

Verbleibend: 8 Euro

Gesamt: 0 Euro

Zur Erinnerung:

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<sup>†</sup>Explanatory notes are given in square brackets.

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## 5 Non-binding restrictions, cooperation, and coral reef protection: Experimental evidence from Indonesian fishing communities

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**Abstract:** We conduct a framed field experiment in Indonesian fishing communities, with an eye towards evaluating alternative decision-making processes for setting extraction restrictions to preserve coral reef fisheries in the absence of stringent monitoring and enforcement. We explore whether the individual extraction decision varies according to three non-binding recommended extraction levels originating from (1) a democratic process, (2) a group leader or (3) an external source. For the sample as a whole, we find a strong effect of the external treatment, with a weaker effect of the democratic treatment and no effect of the leadership treatment. Closer inspection reveals that the results are driven by one of the three sites where the experiment was conducted – that having the highest levels of ethnic and religious diversity. There we find that democratic decision-making as well as information originating from outside the community reduces the extraction level, a result that is robust to regressions controlling for community and individual attributes. The absence of effects in two of the three sites suggests that a non-binding recommendation may often be insufficient in promoting the cooperative behavior that underpins contemporary approaches to managing coral reefs.

## 5.1 Introduction

The ongoing destruction of coral reef ecosystems ranks among the major drivers of global environmental change, with already more than a quarter of the world's reefs irrevocably damaged from the combined effects of climate change and local stressors (Burke et al. 2011). Beyond serving as repositories of biodiversity and marine nutrients, coral reefs provide a multitude of benefits to local communities, including storm surge protection and livelihood from fishing and tourism. Coral reefs are often located within open-access fisheries, making them vulnerable to overfishing and destructive fishing practices. This situation is aggravated by the weak formal and informal enforcement mechanisms characterizing fisheries management in many developing countries. The establishment of exclusive access privileges is increasingly seen as an effective response to countering the resulting overexploitation (Afflerbach et al. 2014). One such management strategy is referred to as Territorial Use Rights for Fisheries (TURF), which has gained traction in recent years largely due to its promotion by non-governmental organizations (NGOs). Nevertheless, the decision-processes underpinning the establishment of a TURF, particularly as regards the setting of an extraction rate that aligns the self-interest of individual fishers with the collective stewardship of the fishery, remains sparsely studied. Velez et al. (2010), Lopez et al. (2012) and Santis and Chávez (2015) are notable exceptions, focusing on the complementarity of informal and formal enforcement mechanisms using framed field experiments with fishers in Colombia and Chile, respectively.

The present study complements the work of these authors with an experiment of non-enforced limits on extractive behavior in fishing communities located in Sulawesi, Indonesia. The idea is to conduct an ex-ante analysis of alternative processes for reaching a decision on the extraction rate in a region where TURFs are planned but have not yet been implemented. We explicitly framed the experiment as extraction from a common-pool fishery for Indonesian fishers.<sup>57</sup> Our experimental design employs a common-pool resource (CPR) game that introduces treatments corresponding to alternative strategies for encouraging cooperative behavior. Drawing on Cardenas (2004), we specifically investigate whether recommendations originating either from a democratic decision process, a group leader decision or an external source affect participants' extraction behavior.

A distinguishing feature of our approach is that the recommendations are non-binding, with no formal or informal sanctioning mechanism introduced for non-compliance. Diverging from the substantial literature on monitoring and enforcement of state and

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<sup>57</sup>Following the nomenclature of Harrison and List (2004) our experiment can be classified as framed field experiment.

federal regulation, we abstain from introducing even a weak enforcement protocol (e.g., Cardenas et al. 2000; Moreno-Sánchez and Maldonado 2010) and, in addition, any informal internal sanctioning measures, e.g., self-imposed sanctions (e.g., Gatiso et al. 2015) or punishment (e.g., Ostrom et al. 1992; Vollan 2008). Our set-up thereby represents an extreme case that we believe characterizes the environment that our study subjects actually encounter, one under which highly costly monitoring and sanctioning precludes a credible deterrent to overfishing. Although many experiments document that costly monitoring and sanctioning devices are effectively used to increase cooperation in social dilemma situations (e.g., Casari and Plott 2003; Carpenter 2007), Villena and Chávez (2005) show that rational fishermen will not engage in monitoring when there are no economic incentives in place for the reporting of violators. Given the absence of compensation for monitoring within the TURFs in operation elsewhere in Indonesia, the question arises as to whether the process for determining the extraction level prior to the establishment of the TURF is, itself, sufficient to support compliance with a socially optimal extraction level.

Previous research suggests that participation in decision making affects behavior and increases individuals' willingness to cooperate in social dilemma situations (e.g., Ostrom and Nagendra 2006; Sutter et al. 2010). However, it has also been shown that the means by which community involvement is implemented can have a fundamental bearing on outcomes (e.g., Agrawal and Chhatre 2006; Cinner and Aswani 2007; Persha et al. 2011; Cox et al. 2014). Moreover, there is evidence that the effects of measures that attempt to foster cooperation in social dilemma situations perform differently depending on the underlying set of personal attributes and informal norms prevailing in the community (e.g., Ostrom 1990; Carpenter et al. 2004; Herrmann et al. 2008; Gächter et al. 2010; Vollan et al. 2017).

The present study expands on these themes with an experimental design that links different decision-making processes to different extractive outcomes, revealing how these outcomes are mediated by the socio-cultural setting in which the participants in the experiment reside. Among our main results, we find that non-binding recommendations originating from both a democratic process and an external source have a statistically significant effect in drawing participants toward the social optimum in the sample as a whole. Closer inspection reveals, however, that this result is driven by one of the three sites where the study was undertaken, that having the highest levels of ethnic and religious diversity. This result is robust to regressions controlling for individual and community level factors. Nevertheless, the absence of effects in two of the three sites underlines the importance of evaluating effects on a case-by-case basis, and suggests that non-binding recommendations may often have only a muted effect in encouraging

cooperative behavior.

The paper is organized as follows. In Section 5.2, background information on coral reef fisheries and TURFs in Indonesia as well as our subject pool are presented. The experimental design and procedures are presented in Section 5.3. Results are presented in Section 5.4. A summary discussion is provided in Section 5.5.

## **5.2 Background, community descriptions, and sampling**

### **5.2.1 The Indonesian context**

Harboring the largest expanse of reefs worldwide, Indonesia is heavily dependent on marine resources, with 54% of the country's animal protein coming from fish and seafood (Burke et al. 2011). A variety of stressors, including agricultural runoff and fishing activities, have put this resource base under severe duress. The World Bank (2014) reports that almost 65% of Indonesia's reefs are threatened by overfishing, and roughly half are threatened by destructive fishing practices.

The Indonesian government recognized the urgency of protecting the reefs decades ago. National and regional laws against destructive fishing practices and overfishing have been introduced over the years, but a lack of monitoring capacities has undermined law enforcement. Conservation NGOs have partially filled this void. A unifying principle of many early interventions was the establishment of Marine Protected Areas (MPAs). The record of MPAs, however, has been mixed, with poor management performance (e.g., Mora et al. 2006), non-compliance with existing rules (e.g., Pieraccini et al. 2017), the prioritization of conservation over economic development, and the non-involvement of local communities in the implementation process (e.g., Ferse et al. 2010) being cited as sources of ineffectiveness. TURFs represent an integrated approach to management that couples conservation with economic development goals by bestowing local fishers with exclusive access to their fishing grounds in the form of territorial use rights.

As documented in a meta-study undertaken by Afflerbach et al. (2014), a common trend characterizing the creation of TURFs is a diversity of stakeholders. While TURFs have existed in various forms for centuries, Afflerbach and colleagues (2014) find that in most contemporary cases TURFs have emerged from the collaboration of an NGO, a governmental unit, and/or a community organization. Such is the situation on the island of Sulawesi, where the creation of the TURFs is supported by international NGOs working in tandem with the Indonesian Ministry of Marine Affairs and Fisheries and respective regional governments, which hold the authority to transfer geographically assigned property rights to the communities. The communities, in turn, set operational

rules, define monitoring and enforcement procedures, and regulate harvest (e.g., Wilen et al. 2012).

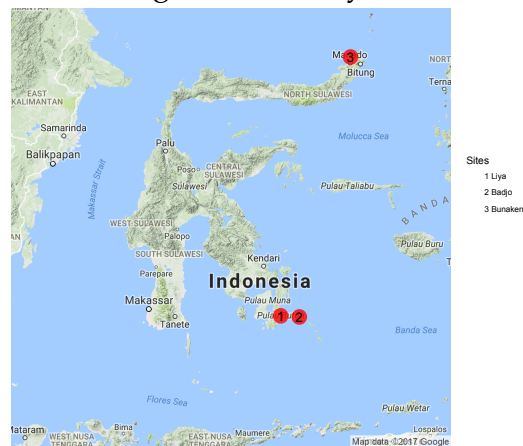
Monitoring itself, which is in principle carried out by local fisherman, is often costly in practice, particularly – as in Sulawesi – when the planned TURFs are large or located far from the coastline. Under such circumstances, rule enforcement may be correspondingly lax or even non-existent, including in cases where the rules are clear (De Alessi 2014). Depending on local socio-economic, political and environmental features, NGOs have consequently availed a mix of strategies to encourage voluntary compliance with the rules established under the TURF. Perhaps the most important question in gauging the scope for garnering support relates to the process by which a given community reaches decisions on exploitation and resource extraction. While a variety of decision-making procedures are possible during the planning phase of a TURF, our experimental approach broadly distinguishes between decisions reached by way of a democratic process, a group leader, or through an outside entity. This division largely captures the alternative channels through which NGOs may attempt to coordinate behavior in the Indonesian context, where rule setting is left to the villages managing the TURF, without a clear agreement about the procedure.

### **5.2.2 Community descriptions**

An immediate challenge in undertaking survey work in Indonesia is the country's rich tapestry of cultural and ethnic heterogeneity. Indonesia is home to more than 300 ethnic groups, and around 700 different languages are spoken across its 14,000 islands. The study site of Sulawesi, which is the fourth biggest Indonesian island in territory and the third biggest in population, embodies this heterogeneity, with at least 117 local ethnicities residing on the island (Ananta et al. 2015). While the main religion is Islam, Christians are also prevalent and comprise about 20% of the population.

Recognizing that this diversity conspires against drawing samples that allow the extrapolation of findings (e.g., Henrich et al. 2001; Herrmann et al. 2008), we selected culturally distinct communities to test the extent to which generalizations can be drawn. Specifically, we selected three sites from a set of 12 sites in which one of the international NGOs working in the region is in the planning phase of a program to establish TURFs (see Figure 5.1). Two of the sites are on Wakatobi, a small string of islands in South-East Sulawesi that are primarily populated by two different ethnicities, the Badjo and the Liya. Badjo communities are primarily organized around fishing and have governance structures that are largely democratic, with village leaders determined by elections. Liya communities occupy the same string of islands and are primarily populated by seaweed farmers, who augment their livelihood by part-time

Figure 5.1: Study sites



Note: Study sites are located on the island of Sulawesi in Indonesia. The study was conducted in Liya (1) and Badjo (2) on Wakatobi islands in South-East of Sulawesi and in Bunaken (3) in the North-East of Sulawesi.

fishing. Liya governance is hierarchically organized, with the village leader usually selected from one of several influential families. While both the Badjo and Liya are relatively cut-off from the remainder of the island, being connected primarily via ferries and planes, the third site, Bunaken, is situated on the main part of the island in the North-East of Sulawesi. Its centralized location, transportation linkages, and mix of ethnicities and religions makes Bunaken more prototypical of Sulawesi at large. Contrasting with the ethnically homogeneous and nearly exclusively Muslim communities of the Badjo and Liya, Bunaken is represented in our sample by 23 different ethnicities and a religious composition that is 64% Muslim. Governance structures in Bunaken also vary, but are typically comprised of a village council and village leader who is democratically elected.

### 5.2.3 Sampling and descriptive statistics

The sample comprises a total of 695 households distributed approximately evenly across the three communities. Bunaken has 10 villages while Liya and Badjo have 4 and 3 villages, respectively. Sampling proceeded according to a design which ensured that the number of households surveyed from every village was proportional to the village's population within the community. The households were selected by approaching every  $n^{\text{th}}$  household from an arbitrary starting point, with  $n$  determined according to the number of households in the village. If a household declined to participate, which occurred in three cases, the next  $n^{\text{th}}$  household was approached.

In addition to one member of each household – usually the head – participating in the CPR game, we administered a questionnaire upon initially approaching the house-

Table 5.1: Sample characteristics

	All sites	Badjo	Liya	Bunaken
<b>Social Capital</b>				
Participant trust level (linearized index from 0 to 1)	0.61	0.55 (0.01)	0.62 (0.01)	0.66 (0.01)
Participant locus of control level (linearized index from 0 to 1)	0.36	0.35 (0.01)	0.36 (0.01)	0.35 (0.01)
Association membership (head of household is member in party or association)	0.33	0.32 (0.03)	0.27 (0.03)	0.40 (0.03)
<b>Household demographics</b>				
Religion of head of household (Islamic believer)	0.87	1.0 (0)	0.99 (0.0)	0.64 (0.03)
Household fishing intensity (0 "Never" to 5 "More than 1-2 times per week")	3.16	3.25 (0.15)	3.58 (0.14)	2.70 (0.14)
Household expenditures (in IDR)	301,481	423,149 (60.52)	198,297 (21.24)	286,302 (15.2)
Head of household gender (male dummy)	0.81	0.76 (0.03)	0.87 (0.02)	0.80 (0.03)
Head of household age (age of head of household)	44.72	38.60 (0.95)	47.43 (0.88)	47.39 (0.77)
Education of head of household (at least primary education)	0.41	0.30 (0.03)	0.46 (0.03)	0.45 (0.03)
<b>Village characteristics</b>				
Internet (good internet connection)	0.19	0.33 (0.01)	0	0.25 (0.009)
Transport services (public transport services available)	0.66	0.66 (0.01)	1 (0)	0.66 (0.01)
Observations	695	225	230	240

Note: Summary of household demographics, social capital indices, and village characteristics. The column all sites denotes the average value of the other three sites. Standard errors are in parentheses. Village characteristics are available for 17 communities.



hold. The questionnaire elicited information on a range of socioeconomic variables that serve as a baseline measure of conditions prior to the introduction of the TURF. Table 5.1 presents descriptive statistics for a subset of these variables, which are subsequently used in a regression analysis that explores whether the estimated treatment effects vary when controlling for personal, household and village characteristics. The majority of variables come directly from questions pertaining to education, religion, age and other attributes, but two are derived from a battery of questions directed at the respondent's level of trust and perceived self-empowerment, referred to as the locus of control.<sup>58</sup> These questions are based on research on the locus of control and trust and then transformed into one indicator ranging from 0 to 1 using principal component analysis.<sup>59</sup>

## 5.3 Common-pool resource model, experimental design, and predictions

### 5.3.1 Common-pool resource model

In standard CPR games, individuals exert effort to extract a shared resource. Extraction is individually beneficial, but implies negative externalities (e.g., Ostrom et al. 1994). Externalities arise whenever the extraction effort by one individual affects the benefits of others. In order to analyze whether collective decision making is an appropriate approach to increase cooperation within Indonesian fishing communities, we use a CPR game based on Cardenas (2004). Rather than using a standard subject pool and neutral framing, we explicitly framed the experiment as extraction from a common-pool fishery for Indonesian fishers. The advantage of using a non-standard subject pool and framing the decision as extraction decision is that the decision is familiar to participants, who in turn bring the experience and context from their daily lives in reaching their decisions.

The design is based on a model of a group of five ( $n = 5$ ) homogeneous agents indexed by  $i$  who have access to a common-pool resource, e.g., a fish stock. All agents have a maximum labor endowment of 8 units of effort to spend and decide how much effort to spend on extracting,  $x_i \in [1, 8]$ . The instantaneous benefits of extraction accruing to

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<sup>58</sup>The importance of trust has long been recognized in a collective action problem (e.g., Ostrom 2000). We use seven questions similar to those of the World Value Survey to disentangle the trust nexus from all perspectives. The locus of control concept measures the person's belief in being able to control events that affect their lives. We apply the method developed by Levenson (1974) to elicit the multidimensionality of the locus of control that asks a set of interrelated questions adjusted to our context. The questions were readjusted to our context.

<sup>59</sup>The principal component analysis allows reducing several correlated variables into one indicator facilitating interpretation and analysis.

agent  $i$ ,  $E_i$ , are given by:

$$E_i = ax_i - \frac{1}{2}bx_i^2,$$

where  $a$  and  $b$  are positive constants. This implies diminishing returns from extraction and that instantaneous benefits received by an appropriator to be independent of the extraction of other appropriators. Additionally, agents receive benefits from conserving the shared resource,  $C_i$ . Benefits from conserving the resource, in contrast to extracting, depend on the total level of extraction and are given by:

$$C_i = \alpha \sum_{j=1}^5 (c - x_j),$$

where  $\alpha$  and  $c$  are positive constants. Note that this introduces a negative externality into the model because an agent's benefit from conservation decreases with total appropriation. Benefits from extraction,  $E_i$ , and conservation,  $C_i$ , define agent  $i$ 's individual payoff:

$$\pi_i = E_i + C_i = ax_i - \frac{1}{2}bx_i^2 + \alpha \sum_{j=1}^5 (c - x_j).$$

To assure comparability to the previous literature and keep the experiment as simple as possible, we use the same parametrization as Cardenas (2004), i.e.,  $e = 8$ ,  $a = 60$ ,  $b = 5$ ,  $\alpha = 20$  and  $c = 800$ . This transfers the experiment into a CPR game that is linear in payoffs with respective corner solutions.<sup>60</sup> Differentiation with respect to  $x_i$  yields that the optimal extraction for agent  $i$  is  $x_i^* = \frac{(a-\alpha)}{b} = 8$ . Given that  $x_i \in [1, 8]$ , the social optimum, in contrast, is attained if agents extract in a way that maximizes the joint payoff and is given by individual extraction levels of  $x_i^\circ = 1$ .

### 5.3.2 Experimental design and predictions

Based on the CPR game by Cardenas (2004), our experimental design covers four treatments reflecting regulatory approaches that apply to fishermen operating under TURFs and allows us to study the effect of alternative strategies to implement community participation in defining and following extraction plans. At the beginning of each session, which is administered from a team assembled from the University of In-

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<sup>60</sup>Support for this transformation comes from Cason and Gangadharan (2015). Their results suggest that non-linearity in payoffs complicate coordination in social dilemma games. Although our game is linear in payoffs with corner solutions we abstain from framing it as *public bad game* (e.g., Andreoni 1995) to address that common-pool resource and public good or bad games are distinct types of social dilemmas and not strategically equivalent (e.g., Apesteguia and Maier-Rigaud 2006).

Table 5.2: Experimental design

Treatment	Description	Sub. (obs.) per site			
		Badjo	Liya	Bunaken	Total
<i>autonomy</i>	Agents autonomously decide on per-capita effort level	60 (12)	60 (12)	60 (12)	180 (36)
<i>democracy</i>	Agents vote on the non-binding recommended per-capita effort level	55 (11)	60 (12)	60 (12)	175 (35)
<i>leadership</i>	Group leader decides on non-binding recommended per-capita effort level	55 (11)	50 (10)	60 (12)	165 (33)
<i>external</i>	External authority decides on non-binding recommended per-capita effort level	55 (11)	60 (12)	60 (12)	175 (35)

indonesia, participants were randomly assigned to groups of five. There is no change in the group throughout the experiment. In each session one of the four treatments was played. Each session consists of 10 static decision rounds. The experiment was consciously framed in such a way that participants were fully aware of participating in an experiment about fishing in coral reefs. We abstain from introducing direct face-to-face communication and focus on different regulatory approaches to implement community participation. This creates a situation more like that of a large group setting than a small group setting (e.g., Walker et al. 2000), thereby mimicking the circumstances when planned TURFs are large and the participating communities cover a vast area, as characterizes the study region.

Table 5.2 briefly summarized the experimental design. In the *autonomy* treatment, which we later refer to as the baseline, each agent autonomously and simultaneously, without any form of communication, decides how much effort to spend on extracting the common resource, i.e., chooses  $x_i \in [1, 8]$ , in each decision round. After each decision, agents are informed about the individual efforts and the corresponding payoffs. Since each agent decides individually, the predicted Nash equilibrium is that all agents spend their individually maximal level of effort, i.e.,  $x_i^* = 8$ , on extracting.

In the *democracy* treatment we introduce community participation by letting groups set their own recommendation on how much effort members should spend on extracting. In each decision round agents vote on the recommended effort level. This stylized structure of participation follows the lead of Walker et al. (2000), who limit direct communication, but allow for indirect communication via the signals through proposals

and voting. Deviating from the previous literature, we capture the high costs of monitoring and enforcement that prevail in the study region by making the recommendation in this and the other treatments non-binding. Although experimental results suggest that voting on binding contribution schemes can substantially increase efficiencies in the private provision of public goods (e.g., Kesternich et al. 2014) and the use of common-pool resources (e.g., Bernard et al. 2013; Walker et al. 2000), evidence on non-binding contribution schemes remains scarce. Kroll et al. (2007) investigate whether a non-binding vote fosters contributions in a public goods game. Their findings suggest that voting alone does not increase cooperation. Following Bernard et al. (2013), each group member in our experiment proposes a per-capita effort level, knowing that the median of all proposals will be selected to be the recommended effort level.

In the *leadership* treatment one group leader is randomly chosen among the members at the beginning of each of the 10 rounds.<sup>61</sup> The leaders thereby selected should not be considered as commanding traditional authority, influence, or experience, as, for example, in Javaid and Falk (2015). Our design instead follows the literature on leading by example in social dilemma situations<sup>62</sup>, with a key distinction being that the leader, who remains anonymous, decides upon a non-binding recommended per-capita effort level in each decision round on behalf of the group. Other work in this vein (e.g., Moxnes and van der Heijden 2003) has typically incorporated binding recommendations.

Under the *external* treatment, a staff member from the University of Indonesia gives a recommendation on a per-capita effort level in each decision round, i.e., how much effort group members should spend on extracting. The staff member thereby represents an external institution, like an NGO or scientist, which has been highlighted as one of the key factors in establishing a TURF (Afflerbach et al. 2014). Following previous experiments on external regulation in a social dilemma context (e.g., Cardenas 2004; Velez et al. 2010; Lopez et al. 2012), agents received in each decision round the recommendation to spend the socially optimal effort, i.e.,  $x_i^{\circ} = 1$ .<sup>63</sup>

Since recommendations are always non-binding, our different mechanisms do not affect the Nash equilibrium and, according to standard theory, agents should spend their maximum amount of effort on extracting the resource, i.e.,  $x_i^* = 8$ .

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<sup>61</sup>Güth et al. (2007) show that whether the leader is determined in a rotating order or once and for all does not have a significant effect on cooperation levels.

<sup>62</sup>See, e.g., Güth et al. (2007) for an overview.

<sup>63</sup>From the instructions and examples provided, it was clear to participants in all treatments that the group earns the maximum amount if every group member chose a level of extraction of one unit.

### 5.3.3 Experimental procedure

The experiments were conducted in 17 different villages within three sites in Sulawesi (see Figure 5.1). After completing the household questionnaire, participants received an invitation to the local community center where the game was conducted the following day. They were randomly allocated to different groups once they had registered and entered the community center. One session comprised ideally 4 groups of 5 players and lasted 10 rounds. In total, 36 sessions were conducted.

All groups were placed in the same room and the seating of the participants was arranged in a way that they could not see their fellow group members, ensuring anonymity. Once all participants had taken their seats, an instructor explained the experiment verbally owing to the low literacy level of the participants.<sup>64</sup> The instructions stipulated the setting of the experiment as being in fishery management, how own extraction rates are related to those of other group members, and the payment. At the end of the instructions, examples were presented to improve the understanding of the participants about the procedure. Then, a pilot round was played to help participants familiarize themselves with the task. Questions could be asked after the explanation of the instructions as well as after the pilot. The questions were answered within the larger audience to ensure that at any time everyone had the same information. A large poster with the payoff table was placed in a way that every participant could see it during the presentation of the instructions. In addition, laminated payoff tables were handed out to the participants before the pilot round started.<sup>65</sup> The experiment only started once everyone indicated that they had understood the rules and there were no further questions to discuss.

One assistant was assigned to each of the four groups per session, ensuring a smooth run of the experiment. In every round the participants had to select their extraction rate and then hand it to the group's assistant. The assistant transmitted the sheets to the researchers, who entered the rates in a computer, calculated the respective payoff, filled the payoff in the sheets and handed it back to the assistant who transmitted this information to the participants. Information about the individual extraction rate and payoff of all the group members were indicated on the sheet as well as the average payoff of the group. Thus, the participants always knew their own payoff and the payoff of all other group members.

The *autonomy* treatment was played as a standard CPR game without any interventions. In the *democracy* treatment, every participant gave a recommendation on a per-

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<sup>64</sup>In order to ensure that the instructions, which were in Bahasa Indonesia, did not suffer from any translation bias, we had them retranslated by an independent Bahasa Indonesia native speaker. We did not find any inconsistencies. Instructions in English are provided in the appendix.

<sup>65</sup>The payoff table is provided in Table 5.7 in the appendix.

capita extraction effort level and the median of these recommendations was shown in every group to each participant individually on a sheet of paper by the respective assistant. Care was taken to ensure that everyone understood the concept of the median before the game commenced. For the *leadership* treatment, one group leader per group was drawn randomly in each round and his recommended extraction effort was then shown to every participant individually on a sheet of paper by the respective assistant. In the *external* treatment the participants were shown the social optimum individually on a sheet of paper in every round by the respective assistant assigned to their group. A total of 695 individuals participated in the experiments, with 180 participants in the *autonomy*, 175 in the *democracy* treatment, 165 in the *leadership* treatment and 175 in the external treatment (see Table 5.2). At the end of the experiment, we randomly drew one out of the 10 rounds to be paid out. On average, the payoff was 28,505 Indonesian Rupiah (IDR) per person. We also added a show-up payment of 10,000 IDR per person, yielding an individual average earning of 38,505 IDR or 2.89 US-Dollar. This is around 86% of the daily working wage in Sulawesi.

## 5.4 Results

### 5.4.1 Extraction efforts

We first analyze participants' extraction rates across treatments aggregated in all three sites (see Figure 5.2). Turning to our *autonomy* treatment, we find that participants have an extraction effort of on average 4.3 hours. In the case of the *democracy* and *leadership* treatments, we find that the mean extraction efforts of 4.1 hours ( $p$ -value: 0.128) and 4.3 hours ( $p$ -value: 0.961) are statistically indistinguishable from the level in *autonomy*.<sup>66</sup> The recommendations from an external source, by contrast, lead to an average extraction effort of 3.6 hours, which is significantly below the extraction effort in *autonomy* ( $p$ -value: 0.000).<sup>67</sup>

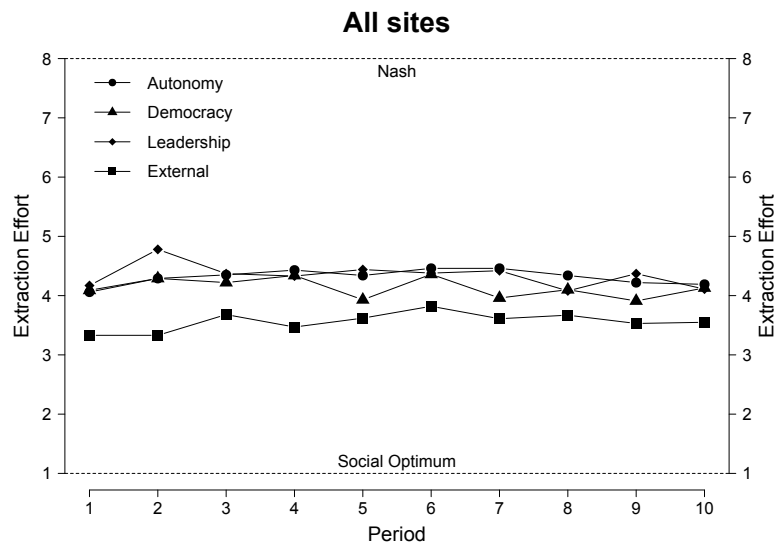
In line with Cardenas (2004), we find that extraction efforts in all treatments, and especially in *autonomy*, remain relatively stable throughout the experiment. While Cardenas (2004) shows that centralized punishment institutions and direct face-to-face communication substantially decreases the extraction level, our non-parametric analysis reveals that non-binding recommendations have a rather limited effect on participants'

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<sup>66</sup>The  $p$ -values in this section correspond to Mann-Whitney U tests, except where otherwise specified.

<sup>67</sup>One might speculate that the noticeable accumulation of mean extraction efforts of around 4 hours are a consequence of random decisions within the action space between 1 and 8 hours. Two features of the data rebut this. Across sites and treatments, we detect different extraction patterns (see Figure 5.7 in the appendix). Furthermore, according to Kolmogorov-Smirnov tests, we can clearly reject the hypotheses that extraction patterns are uniformly distributed across sites and treatments.

Figure 5.2: Extraction efforts by treatments



Note: Average extraction effort during the 10 periods of the autonomy, democracy, leadership and external treatment.

extraction behavior. Only recommendations from an external source have a statistically significant effect in drawing participants toward the social optimum.<sup>68</sup>

To gain further insight into the mechanism for reaching the extraction decision, we analyze the variance of the results across treatments. Applying a Levene's test as measure for the variance, we find that there are no differences between the variances in *democracy* and *leadership* compared to *autonomy* ( $p$ -value: 0.57 and  $p$ -value: 0.96, respectively). By contrast, we reject the equality of variances in *autonomy* and *external*, finding a significant decrease in the variation of the extraction in the *external* treatment ( $p$ -value: 0.000). This decrease likely reflects the additional information provided in the treatment, which is in line with Geest et al.'s (2017) finding of decreases in variation after providing information.

To explore the robustness of these non-parametric comparisons, we estimate a series of random effects models. The first model in Table 5.3 includes dummies for the three treatments, but as in the initial unconditional analysis, does not allow for differential effects by site or individual controls. The results confirm our non-parametric estimates. The coefficients on the external dummy is statistically significant at the one percent level. The second model controls for the different sites through site dummies, revealing only negligible differences relative to Model (1).

<sup>68</sup>An analysis of extractions in the first round only, reveals very similar results: *autonomy* vs. *democracy* ( $p$ -value: 0.13), *autonomy* vs. *leadership* ( $p$ -value: 0.96), and *autonomy* vs. *external* ( $p$ -value: 0.00).

Table 5.3: Random effects regressions of extraction efforts

	Extraction effort			
	(1)	(2)	(3)	(4)
Democracy	-0.18 (0.142)	-0.18 (0.142)	-0.37** (0.173)	-1.24*** (0.303)
Leadership	0.03 (0.147)	0.03 (0.148)	-0.02 (0.204)	-0.43 (0.364)
External	-0.75*** (0.168)	-0.75*** (0.168)	-0.62** (0.224)	-1.66*** (0.346)
Liya		-0.01 (0.133)	0.01 (0.203)	-0.74** (0.376)
Badjo		0.14 (0.133)	-0.40* (0.208)	-1.60*** (0.383)
Democracy x Liya				0.81** (0.381)
Democracy x Badjo				1.95*** (0.482)
Leadership x Liya				0.22 (0.517)
Leadership x Badjo				0.78 (0.474)
External x Liya				1.26** (0.510)
External x Badjo				1.93*** (0.508)
Trust index			0.23 (0.461)	-0.02 (0.459)
Locus of control			-1.25*** (0.417)	-1.23*** (0.415)
Association membership			-0.07 (0.148)	-0.06 (0.140)
Primary education			-0.23 (0.143)	-0.23* (0.139)
Muslim			0.34 (0.254)	0.53* (0.319)
Fishing intensity			-0.08 (0.055)	-0.06 (0.056)

Note: Table 5.3 continues on the next page.



Table 5.3 (continued)

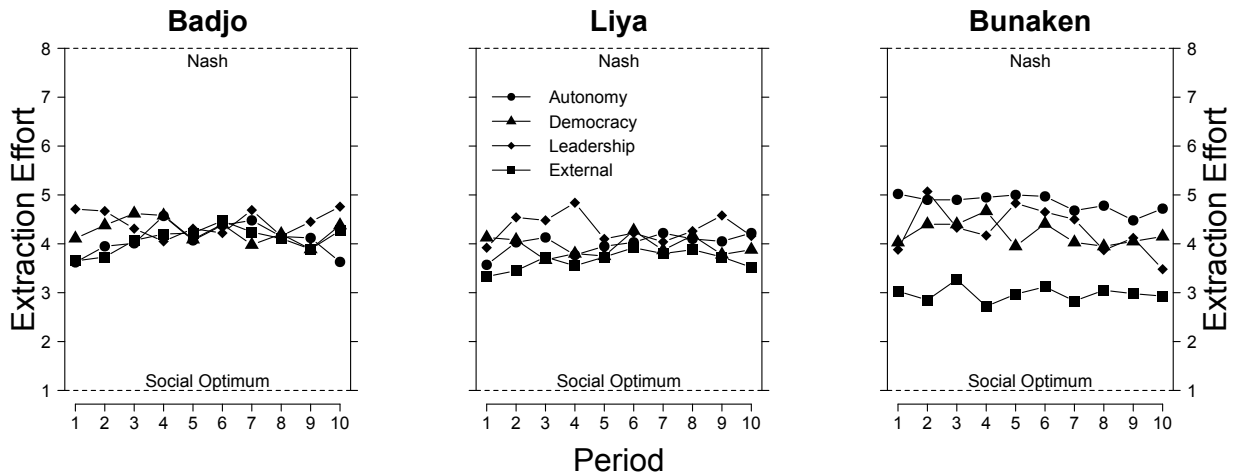
	Extraction effort			
	(1)	(2)	(3)	(4)
Household expenditures (in Mio. IDR)			-0.02** (0.000)	-0.015 (0.000)
Gender			0.12 (0.251)	0.082 (0.228)
Age			-0.00 (0.006)	-0.00 (0.006)
Internet			-0.05 (0.179)	-0.41** (0.191)
Transport services			-0.83*** (0.182)	-0.78*** (0.208)
Round			-0.01 (0.010)	-0.01 (0.010)
Constant	4.32*** (0.102)	4.30*** (0.126)	5.61*** (0.578)	6.09*** (0.561)
Observations	6,950	4,380	6,950	4,380

Note: Random effects regression with robust standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$  and \*\*\*  $p < 0.01$ . Dependent variable: Participants' extraction effort in each round of the experiment. Democracy: 1 if subject participates in *democracy* treatment, 0 otherwise. Leadership: 1 if subject participate in *leadership* treatment, 0 otherwise. External: 1 if subject participate in *external* treatment, 0 otherwise. Liya: 1 if subject from is from Liya, 0 otherwise. Badjo: 1 if subject is from Badjo, 0 otherwise. Controls (trust index, locus of control, association membership, primary education, Muslim, fishing intensity, household expenditures, gender, age, internet, transport services) are summarized and outlined in Section 5.2.

The third model introduces further controls for individual heterogeneity.<sup>69</sup> The effect of the *external* treatment is statistically significant at the one percent level and indicates a reduction in the extraction of about 0.62 hours, which is just slightly lower than the magnitude of the unconditional comparison. A statistically significant effect is now likewise seen for the *democracy* treatment, with an associated reduction in the extraction rate of 0.34 hours. Model (3) additionally reveals that behavior varies with socioeconomic factors. The positive and statistically significant coefficient on the locus of control is of particular interest, corroborating Kalamas et al. (2014) and Engqvist Jonsson and Nilsson (2014), who find that a higher locus of control is associated with stronger

<sup>69</sup>Model (3) includes the variables presented in Table 5.1 as controls. This results in a loss of about 180 observations owing to instances when a shortened version of the questionnaire was administered because the participant in the CPR game was not present at the household at the time of the interview. When this was the case, we did not record information on the locus of control. As shown in Table 5.4, the exclusion of this variable from the specification does not affect the remaining coefficient estimates markedly.

Figure 5.3: Extraction efforts by treatments and sites



Note: Average extraction effort in Badjo (left), Liya (middle) and Bunaken (right) during the 10 periods of the *autonomy*, *democracy*, *leadership* and *external* treatment.

pro-environmental behavior. Additionally, two other control variables – household expenditure, and transport services – are statistically significant at the 5% level or below, both of which have negative associations with extraction.

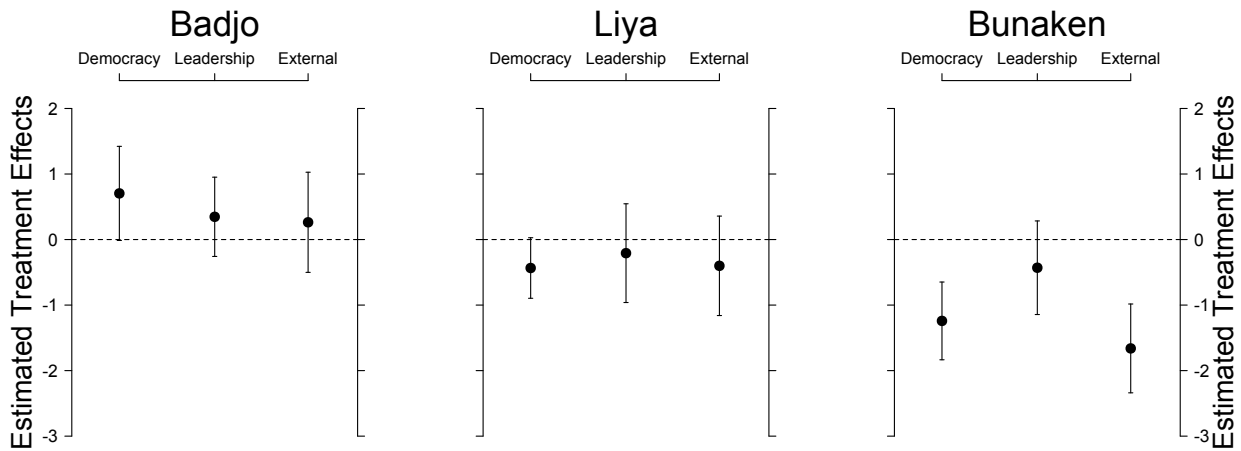
Recognizing the hierarchical structure of the data, we undertook robustness checks by specifying multilevel models with random effects at the site and individual level. While these models provided an improved fit to the data, as judging from a likelihood ratio test, the differences in the estimated coefficients were negligible. Likewise, the pattern of estimated treatment effects changed only marginally.<sup>70</sup>

To recap, Models (1)-(3) confirm the non-parametric results that recommendations originating from an external source decrease participants extraction behavior. In addition, if we take the different sites and individual characteristics into account in Model (3), we find that recommendations from a democratic process also have a statistically significant effect in reducing participants' harvest.

Next, we separate our analysis according to the three sites to gauge the extent of regional variation in extraction efforts and treatment effects. The main results of this analysis are presented in Figure 5.3. We start with a non-parametric analysis by comparing extraction efforts in *autonomy* across sites. Based on a Kruskal-Wallis test, we find that average extractions in our *autonomy* treatment differ significantly across the three sites ( $p$ -value: 0.002). More precisely, extraction efforts in *autonomy* are higher in Bunaken than in Liya ( $p$ -value: 0.001) and in Badjo ( $p$ -value: 0.003). Average extraction efforts in *democracy* and *leadership* are remarkably similar across regions. According to

<sup>70</sup>Results are provided in Table 5.6 and Figure 5.8 in the appendix.

Figure 5.4: Estimated treatment effects by sites



Note: Estimated treatment effects based on regression model in column (4) of Table 5.3. Confidence intervals at the 95%-level.

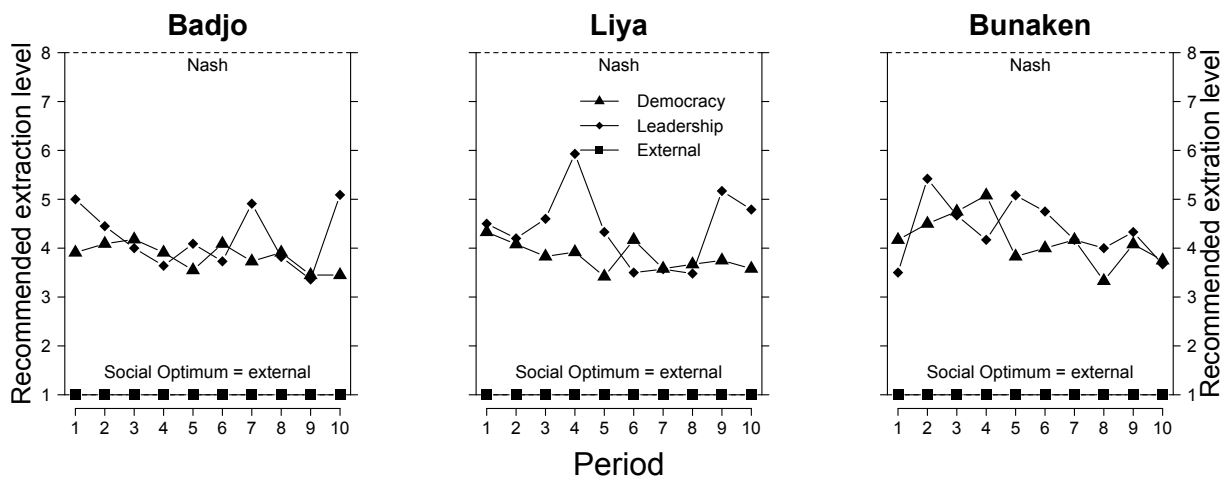
Kruskal-Wallis tests we do not find statistically significant differences in individual extraction effort decisions across the three regions ( $p$ -value: 0.248 and  $p$ -value: 0.794, respectively). In *external*, however, extraction efforts in Bunaken are significantly lower than in Badjo ( $p$ -value: 0.001) and Liya ( $p$ -value: 0.035).

The statistically significant effect of the *external* treatment in the pooled analysis is thus seen to be driven by one site, Bunaken, whose extraction effort of 3 hours is roughly 1.8 hours below the site's relatively high level of 4.8 hours in *autonomy* ( $p$ -value: 0.000). The *democracy* ( $p$ -value: 0.003) and *leadership* ( $p$ -value: 0.010) treatments are likewise statistically significant in Bunaken compared to the *autonomy* treatment, albeit with reductions in the extraction level that are substantially lower in magnitude than under the *external* treatment. In Liya and Badjo, by contrast, none of the treatments significantly bear on the extraction effort relative to each site's baseline extraction effort of 4 and 4.1 hours.<sup>71</sup>

Returning to Table 5.3, the fourth model includes the full range of control variables in addition to interactions between the site- and treatment dummies to allow the magnitude of the treatment effect to vary by site. Several of the effects presented in the table are seen to be statistically significant, but the resulting proliferation of coefficients makes it difficult to assess their magnitude. To ease interpretation, Figure 5.4 illustrates estimates of the deviation from the baseline by treatment and for each site, along with the corresponding 95% confidence interval. As in the non-parametric analysis, this comparison shows statistically significant treatment effects only in Bunaken.

<sup>71</sup>Given that we test multiple treatments across multiple sites, we corrected our results for multiple hypothesis testing by the corrections suggested in List et al. (2016). In no case are the statistically significant results negated by those corrections.

Figure 5.5: Recommended extraction efforts by treatments and sites



Note: Average recommended effort in Badjo (left), Liya (middle) and Bunaken (right) during the 10 periods of the *democracy*, and *leadership* treatment.

According to the point estimates, both the *democracy* and *external* treatments lower the extraction effort by about 1.3 and 1.7 hours, respectively. This highlights that the overall effect discussed above are driven by Bunaken.<sup>72</sup>

The more flexible specification of Model (4) also alters the interpretation of some of the control variables. Unlike in Model (2), household expenditures are no longer statistically significant, while the dummy indicating a good internet connection is highly significant and suggests a decrease in the extraction rate of about 0.41 hours, a three-fold increase in magnitude relative to Model (1). The locus of control index remains significant at a 1 percent level and increases in magnitude, with a one standard deviation increase in the index associated with a 0.44 decrease in the hours of extraction. Similar increases in magnitude are seen for the dummies indicating public transport, religion, and primary school education.

#### 5.4.2 Recommendations and compliance

To further explore what drives the regional heterogeneity, we analyze participants' recommendations and whether those recommendations are followed across sites. Across all sites and periods, participants recommend extracting on average significantly less in *democracy* than in *leadership* (3.95 vs. 4.34,  $p$ -value: 0.008).

Controlling for the different sites where the experiment has been conducted reveals

<sup>72</sup>Again applying a Levene's test to analyze different patterns in the variation of the extraction rates within and across sites, confirms the significant decreases in the variation of extractions in *external* compared to *autonomy* in all sites (Badjo:  $p$ -value: 0.054; Liya:  $p$ -value: 0.073; Bunaken:  $p$ -value: 0.000).

that this effect is driven by participants in Liya (see Figure 5.5). In Liya, the mean recommended extraction effort of 3.8 hours in *democracy* is roughly 0.6 hours below the recommendations in *leadership* ( $p$ -value: 0.007). In Badjo and Bunaken we do not find a statistically significant difference between the recommendations in *democracy* and *leadership*. However, compared across sites, we find only moderate differences with respect to the recommended extraction efforts. Most importantly, recommendations in Bunaken are not systematically lower than those in Badjo or Liya, which could help explain the heterogeneous treatment effects in extraction efforts.

Given the non-binding nature of the recommendation, a final question concerns the issue of compliance, i.e., whether the recommended extraction levels emerging from the treatments differ from those actually selected by the participants. Figure 5.6 presents these differences by treatments and sites, with negative differences indicating the extent to which the recommended extraction level falls below the chosen one, that is, the extent of non-compliance. For the case of *democracy* and *leadership*, the difference in compliance across sites is negligible in magnitude and statistically insignificant ( $p$ -value: 0.263 and  $p$ -value: 0.182, respectively, Kruskal-Wallis test).<sup>73</sup> Focusing on compliance with externally recommended non-binding extraction efforts, we find substantial differences across regions ( $p$ -value: 0.003, Kruskal-Wallis test). While compliance is lower in all sites, participants in Bunaken, in particular, show a significantly higher compliance than participants from Badjo (-1.98 vs. -3.09,  $p$ -value: 0.001) and Liya (-1.98 vs. -2.67,  $p$ -value: 0.035). The differences in treatment effects across regions are thus driven primarily by a relatively high extraction in our baseline treatment in Bunaken and consequently a comparatively high compliance in Bunaken in the *external* treatment.

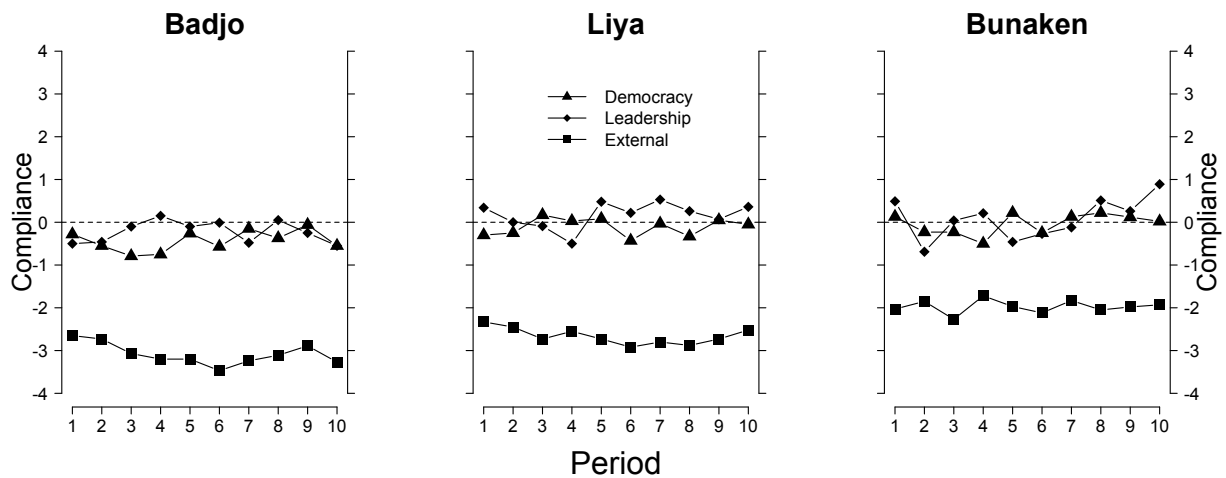
In order to complement the analysis on group level, we also investigated individual differences between participants' recommendations and actual extraction decisions in both the *democracy* and *leadership* treatment.<sup>74</sup> By comparing recommendations and extractions at the individual level, this analysis enables us to elicit whether participants set recommendations strategically. In our *democracy* treatment, we find that participants start to behave more strategically in the second-half of the experiment by recommending a lower extraction level than they actually adhere to themselves. With respect to our *leadership* treatment, we find that leaders follow their own recommendations in most of the cases. However, also in leadership we recognize a slight increase

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<sup>73</sup>We also include in our regression model the given recommendations in the groups (see Table 5.5 in the appendix). This allows us to analyse the extraction rate given the recommendation and controlling for several confounders. For both treatments, *democracy* and *leadership*, a low recommendation is associated with a lower extraction rate, on average, and the extraction effort increases with the recommendation level.

<sup>74</sup>Results are provided in Figures 5.9 and 5.10 in the appendix.

Figure 5.6: Differences between recommendations and extraction efforts by treatments and sites



Note: Average difference between recommendations and actual extraction efforts over periods across all participants from Badjo (left), Liya (middle) and Bunaken (right).

in strategically recommending low extraction efforts while pursuing higher individual extractions.

## 5.5 Summary and concluding remarks

This study examined collective resource management among fishing communities on the Indonesian island of Sulawesi. The island, one of the most biologically diverse expanses of coral reef fisheries globally, has been subject to extensive degradation from overfishing (Burke et al. 2011). In response, international NGOs and regional governments have teamed up to coordinate the establishment of TURFs, community-based management regimes for regulating access to the fishery. Following Handberg and Angelsen (2015), who highlight the importance of framed field experiments for the evaluation of proposed conservation measures, we undertake an ex-ante analysis of the establishment of a TURF reserve that is being overseen by an environmental NGO. An underlying premise of our analysis is the importance of understanding common resource management when costly monitoring precludes a strong enforcement regime, a common feature of TURFs globally. Using a common-pool resource game conducted in three culturally distinct sites on the island, we explored alternative strategies for garnering the requisite cooperation under this circumstance. The game involves individual fishers selecting a desired level of harvesting activity. Corresponding to alternative strategies for encouraging voluntary cooperation in the absence of enforcement, experimental treatments were introduced that consisted of different non-binding

resource extraction recommendations originating from a democratic process, a group leader decision or an external source that recommends a socially optimal level.

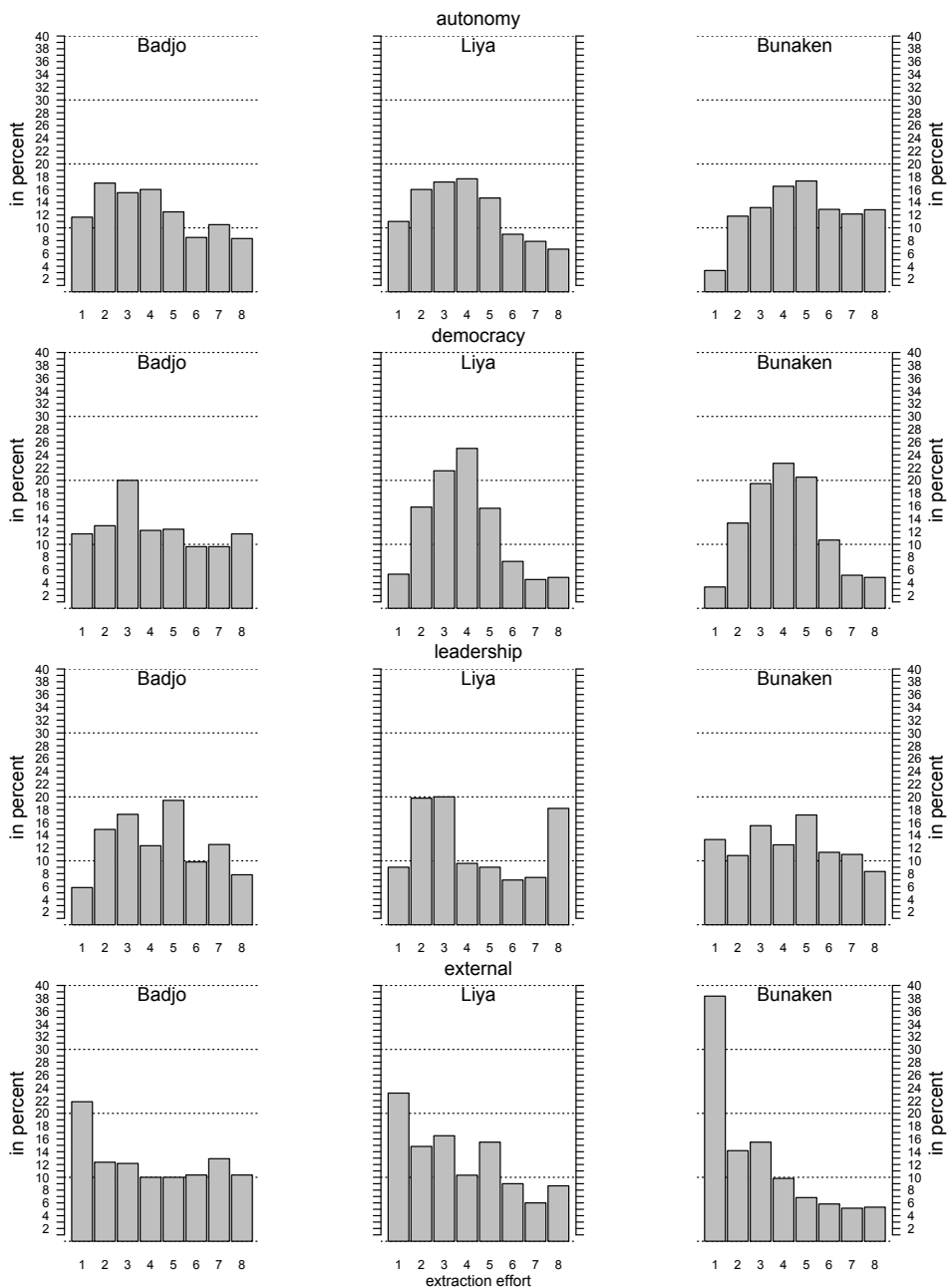
In line with the previous work on cross-cultural differences in behavior in social dilemma situations in cross-country (e.g., Henrich et al. 2001; Carpenter et al. 2004; Gächter et al. 2010) and inter-country (e.g., Kocher et al. 2012) experiments, we find substantial differences in participants' extraction behavior in our CPR game across the three sites. Although our pooled analysis suggests that non-binding recommendations originating from both a democratic decision process and an external source increase cooperation, a site-specific analysis revealed this result to be driven by one of the three sites, Bunaken, which has the highest levels of ethnic and religious diversity. The absence of this effect in two of the sites shows that caution is warranted in generalizing this conclusion to other sites in which TURFs are being considered.

As interaction with an external source, such as an NGO or scientist, is a feature of TURF-implementation in Indonesia and throughout the world (Afflerbach et al. 2014), the question arises as to the circumstances under which such interaction facilitates or stifles cooperation. Part of the answer likely depends on the community's diversity and its exposure to outside influences. In this regard, the low compliance but high extraction reduction of the *external* treatment in Bunaken together with the high compliance but somewhat lower extraction reduction of the *leadership* and *democracy* treatments suggests some promise for coupling external advocacy of the social optimum with either a top-down or community-based democratic decision process, particularly when enforcement is precluded. Recent work on cross-cultural management from Meyer (2017), which situates Indonesia as a country that values top-down decision-making and harbors hierarchical attitudes toward authority, provides some support for this course of action in Bunaken. Nevertheless, the heterogeneity of Indonesia's cultural landscape suggests that caution is warranted in applying it elsewhere, particularly given the significantly lower rates of compliance in the more remote and culturally homogenous communities of Badjo and Liya. The discrepancy in outcomes in these locations emphasizes the importance of the context in which fishermen live (Torres-Guevara and Schlüter 2016), and specifically the potential interactions between the socio-cultural setting and interventions targeted at increased cooperative behavior (e.g., Ostrom 1990; Herrmann et al. 2008). Further research should thus be directed at disentangling the root of different extraction levels, and in particular at identifying the circumstances under which non-binding recommendations from an external source serve to motivate conservation.

## 5.6 Appendix

### 5.6.1 Figures

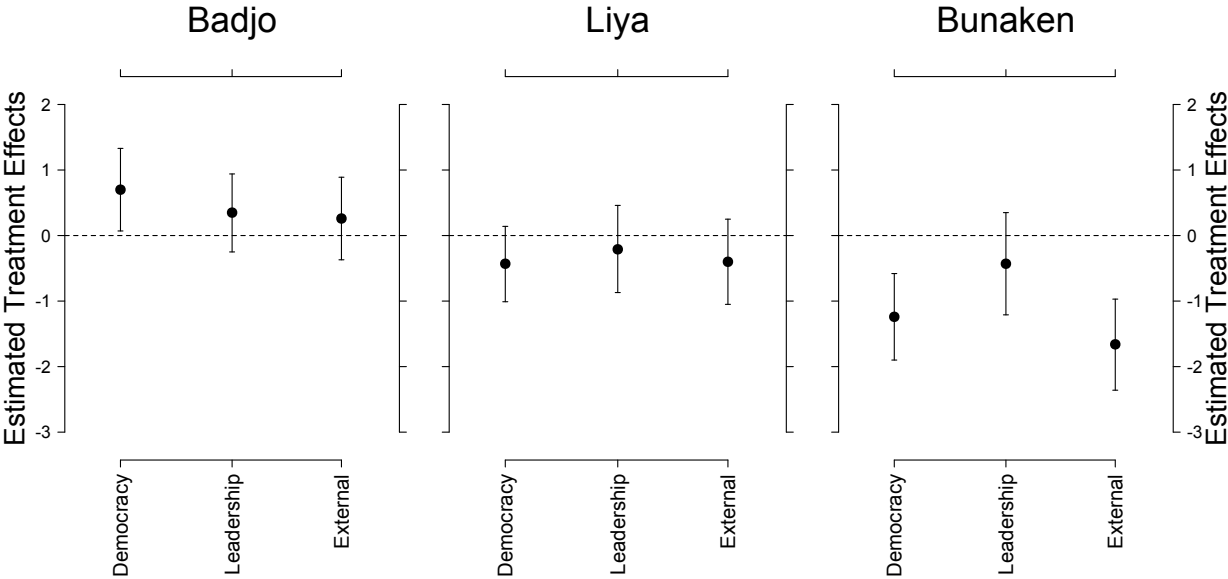
Figure 5.7: Histograms of extraction efforts by treatments and sites



Note: Histograms of average extraction efforts by treatment (*autonomy, democracy, leadership, external*) and site (Badjo, Liya, Bunaken) across all 10 periods of the experiment.

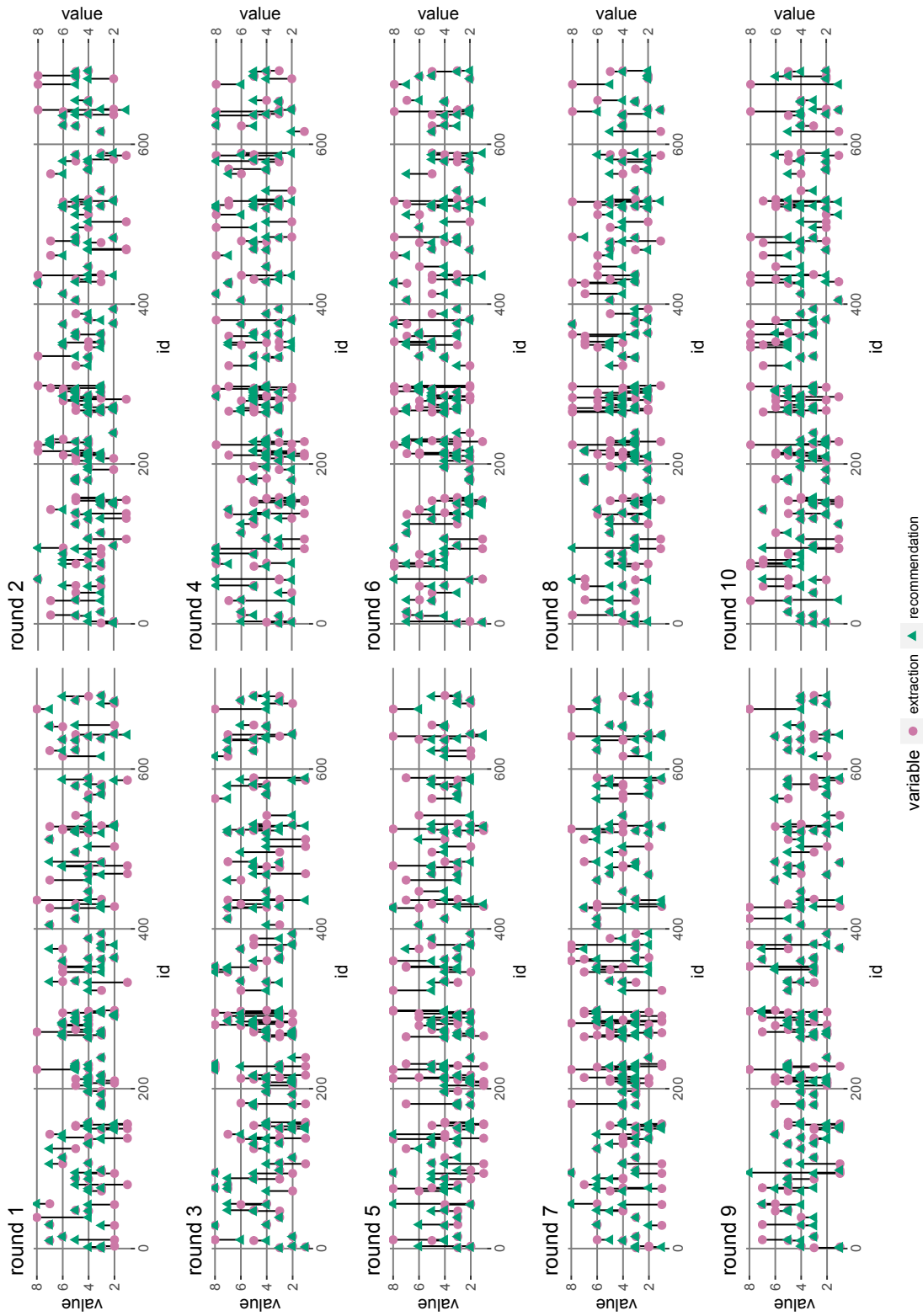


Figure 5.8: Estimated treatment effects by sites - hierarchical models



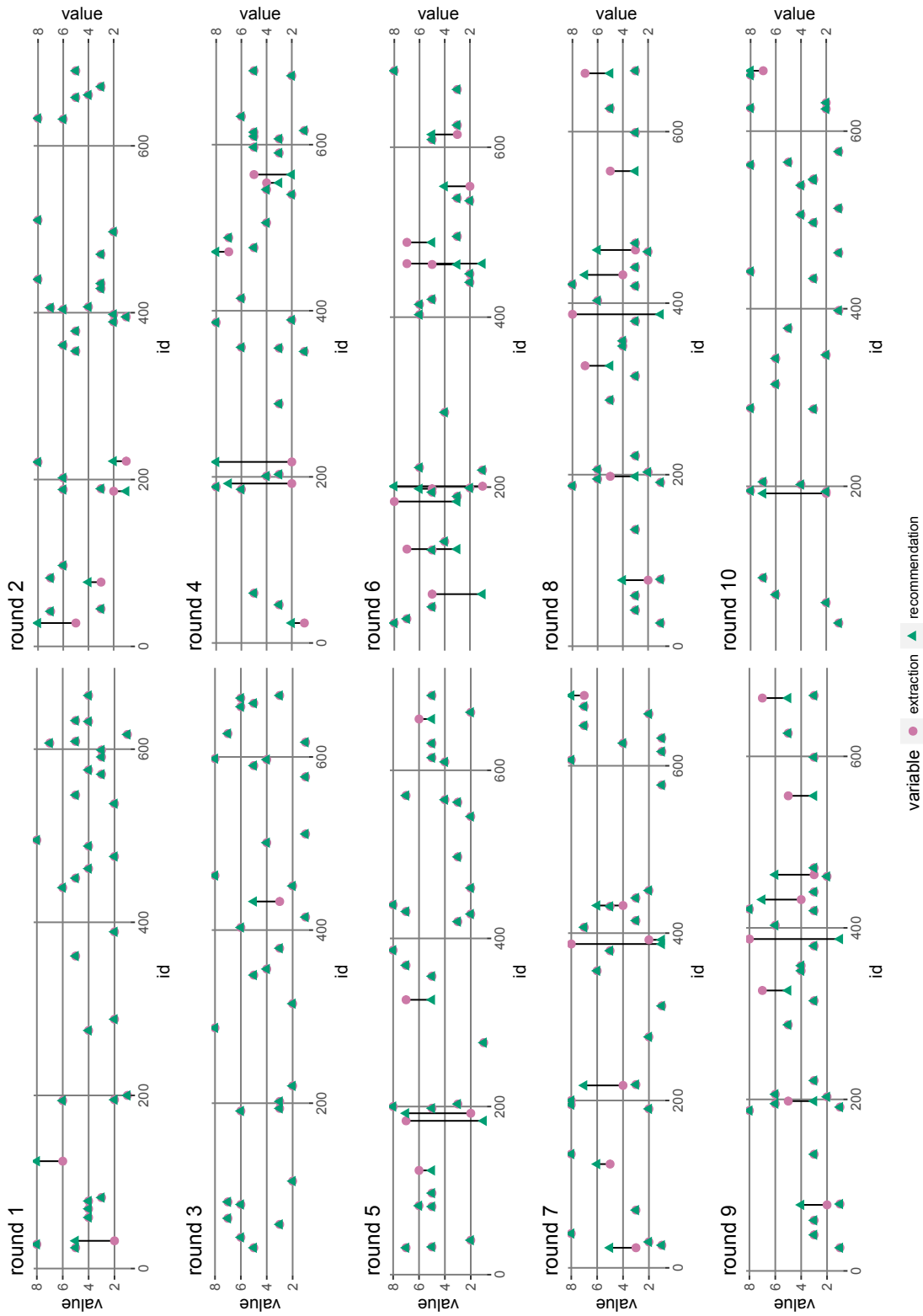
Note: Estimated treatment effects based on regression model in column (4) of Table 5.6. Confidence intervals at the 95%-level.

Figure 5.9: Individual recommendations and extractions in *democracy*



Note: Individual recommendations and extraction levels in *democracy* treatment separately for all ten rounds of the experiment. The x-axis orders the participants. The y-axis provides the scale for individual recommendations and extractions. Triangle symbols indicate individual recommendations. Point symbols indicate individual extractions. Parallel lines indicate the change from recommendation to extraction per participant.

Figure 5.10: Individual recommendations and extractions in leadership



Note: Individual recommendations and extraction levels in *leadership* treatment separately for all ten rounds of the experiment. The x-axis orders the participants. The y-axis provides the scale for individual recommendations and extractions. Triangle symbols indicate individual recommendations. Point symbols indicate individual extractions. Parallel lines indicate the change from recommendation to extraction per participant.

5.6.2 Tables

Table 5.4: Random effects regressions of extraction efforts without locus of control

	Extraction effort	
	(1)	(2)
Democracy	-0.19 (0.16)	-0.94*** (0.28)
Leadership	0.03 (0.17)	-0.33 (0.34)
External	-0.56*** (0.20)	-1.54*** (0.33)
Liya		-0.63* (0.36)
Badjo		-1.21*** (0.36)
Democracy x Liya		0.75** (0.36)
Democracy x Badjo		1.71*** (0.45)
Leadership x Liya		0.44 (0.47)
Leadership x Badjo		0.66 (0.44)
External x Liya		1.36*** (0.46)
External x Badjo		1.85*** (0.47)
Trust index	0.54 (0.41)	0.30 (0.43)
Association membership	-0.04 (0.13)	-0.00 (0.13)
Primary education	-0.17 (0.13)	-0.19 (0.12)
Muslim	0.22 (0.21)	0.341 (0.31)
Fishing intensity	-0.03 (0.031)	-0.01 (0.03)

Note: Table 5.4 continues on the next page.

Table 5.4 (continued)

	Extraction effort	
	(1)	(2)
Household expenditures (in Mio. IDR)	-0.01 (-0.01)	-0.01 (-0.01)
Gender	0.19 (0.19)	0.16 (0.18)
Age	0.00 (0.01)	0.00 (0.01)
Internet	-0.22 (0.15)	-0.46*** (0.17)
Transport services	-0.61*** (0.13)	-0.69*** (0.20)
Round	-0.01 (0.01)	-0.01 (0.01)
Constant	4.16*** (0.43)	4.81*** (0.46)
Observations	5,780	5,780

Note: Random effects regression with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Dependent variable: Participants' extraction effort in each round of the experiment. Democracy: 1 if subject participates in *democracy* treatment, 0 otherwise. Leadership: 1 if subject participate in *leadership* treatment, 0 otherwise. External: 1 if subject participate in *external* treatment, 0 otherwise. Liya: 1 if subject from is from Liya, 0 otherwise. Badjo: 1 if subject is from Badjo, 0 otherwise. Controls (trust index, association membership, primary education, Muslim, fishing intensity, household expenditures, gender, age, internet, transport services) are summarized and outlined in Section 5.2.

Table 5.5: Random effects regressions of recommendations and extraction efforts

	Extraction effort			
	<i>democracy</i>		<i>leadership</i>	
	(1)	(2)	(3)	(4)
Recommendation of 1			-1.28** (0.58)	-0.97 (0.90)
Recommendation of 2	0.23 (0.32)	0.31 (0.37)	-1.35*** (0.19)	-1.36*** (0.24)
Recommendation of 3	0.92*** (0.32)	1.07*** (0.37)	-0.87*** (0.19)	-0.89*** (0.24)
Recommendation of 4	1.27*** (0.34)	1.39*** (0.39)	-0.33** (0.16)	-0.16 (0.20)
Recommendation of 5	2.01*** (0.35)	2.04*** (0.40)	0.69*** (0.14)	0.68*** (0.13)
Recommendation of 6	2.16*** (0.39)	2.24*** (0.44)	1.12*** (0.19)	1.25*** (0.19)
Recommendation of 7	2.90*** (0.39)	3.01*** (0.44)	1.87*** (0.32)	2.09*** (0.25)
Recommendation of 8	3.85*** (0.48)	4.14*** (0.50)	3.05*** (0.26)	2.98*** (0.29)
Liya	0.19 (0.26)	0.11 (0.28)	-0.24 (0.30)	-0.17 (0.33)
Badjo	0.65** (0.27)	0.67** (0.29)	0.46 (0.70)	-1.05* (0.58)
Locus of control		-0.31 (0.57)		-1.00 (0.96)
Trust index	0.87 (0.64)	0.87 (0.68)	0.37 (0.70)	0.59 (0.81)
Association membership	0.12 (0.17)	0.26 (0.17)	-0.57** (0.28)	-0.56 (0.35)
Primary education	-0.11 (0.18)	-0.03 (0.19)	-0.03 (0.23)	-0.65** (0.28)
Muslim	-0.14 (0.21)	-0.04 (0.25)	0.27 (1.01)	0.38 (0.97)
Fishing intensity	-0.10* (0.05)	-0.10 (0.07)	0.01 (0.05)	0.00 (0.14)

Note: Table 5.5 continues on the next page.

Table 5.5 (continued)

	Extraction effort			
	<i>democracy</i>		<i>leadership</i>	
	(1)	(2)	(3)	(4)
Household expenditures (in Mio. IDR)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00* (0.00)
Gender	0.19 (0.24)	0.32 (0.28)	-0.02 (0.33)	-0.13 (0.47)
Age	-0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	-0.01 (0.02)
Internet	-0.49** (0.23)	-0.51** (0.26)	-0.55* (0.32)	0.28 (0.38)
Transport services	-0.65*** (0.24)	-0.75*** (0.27)	0.37 (0.73)	-0.99 (0.62)
Round	0.01 (0.02)	0.00 (0.02)	-0.03 (0.02)	-0.01 (0.02)
Constant	2.87*** (0.74)	2.62*** (0.97)	3.74*** (1.42)	5.96*** (1.41)
Observations	1,520	1,280	1,350	910

Note: Random effects regression with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Dependent variable: Participants' extraction effort in each round of the experiment. Recommendation 1 (2, 3, 4, 5, 6, 7, 8): 1 if recommendation is equal to 1 (2, 3, 4, 5, 6, 7, 8), 0 otherwise. Democracy: 1 if subject participates in *democracy* treatment, 0 otherwise. Leadership: 1 if subject participate in *leadership* treatment, 0 otherwise. External: 1 if subject participate in *external* treatment, 0 otherwise. Liya: 1 if subject from is from Liya, 0 otherwise. Badjo: 1 if subject is from Badjo, 0 otherwise. Controls (trust index, locus of control, association membership, primary education, Muslim, fishing intensity, household expenditures, gender, age, internet, transport services) are summarized and outlined in Section 5.2.

Table 5.6: Hierarchical regressions of extraction efforts

	Extraction effort			
	(1)	(2)	(3)	(4)
Democracy	-0.18 (0.155)	-0.30* (0.180)	-0.64** (0.261)	-1.24*** (0.338)
Leadership	0.031 (0.158)	-0.05 (0.195)	-0.55** (0.261)	-0.43 (0.400)
External	-0.75*** (0.155)	-0.67*** (0.196)	-1.86*** (0.261)	-1.66*** (0.355)
Liya			-0.83*** (0.261)	-0.74* (0.427)
Badjo			-0.74*** (0.261)	-1.60*** (0.364)
Democracy x Liya			0.57 (0.369)	0.81* (0.448)
Democracy x Badjo			0.80** (0.373)	1.95*** (0.471)
Leadership x Liya			0.86** (0.378)	0.22 (0.526)
Leadership x Badjo			0.89** (0.373)	0.78 (0.509)
External x Liya			1.52*** (0.369)	1.26*** (0.484)
External x Badjo			1.85*** (0.373)	1.93*** (0.475)
Trust index		0.46 (0.439)		-0.02 (0.443)
Locus of control		-1.19*** (0.443)		-1.23*** (0.430)
Association membership		-0.11 (0.142)		-0.06 (0.139)
Primary education		-0.14 (0.138)		-0.23 (0.142)
Muslim		0.23 (0.211)		0.53* (0.310)
Fishing intensity		-0.09* (0.052)		-0.06 (0.052)

Note: Table 5.6 continues on the next page.



Table 5.6 (continued)

	Extraction effort			
	(1)	(2)	(3)	(4)
Household expenditures (in Mio. IDR)		-0.00*		-0.00
		(0.000)		(0.000)
Gender		0.11		0.08
		(0.237)		(0.231)
Age		0.00		-0.00
		(0.006)		(0.006)
Internet		-0.13		-0.41*
		(0.177)		(0.214)
Transport services		-0.67***		-0.78***
		(0.148)		(0.223)
Round	-0.00	-0.01	-0.00	-0.01
	(0.006)	(0.008)	(0.007)	(0.009)
Constant	4.34***	5.19***	4.87***	6.09***
	(0.115)	(0.559)	(0.188)	(0.586)
Observations	6,950	4,380	6,950	4,380
Number of groups	3	3	3	3
LR $\chi^2(4)$	32.92			
Prob > $\chi^2$	0.00	0.00	0.00	0.00
LR $\chi^2(23)$				104
LR $\chi^2(12)$			66.54	
LR $\chi^2(15)$		66.27		

Note: Hierarchical regressions with robust standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Dependent variable: Participants' extraction effort in each round of the experiment. Democracy: 1 if subject participates in *democracy* treatment, 0 otherwise. Leadership: 1 if subject participate in *leadership* treatment, 0 otherwise. External: 1 if subject participate in *external* treatment, 0 otherwise. Liya: 1 if subject from is from Liya, 0 otherwise. Badjo: 1 if subject is from Badjo, 0 otherwise. Controls (trust index, association membership, primary education, Muslim, fishing intensity, household expenditures, gender, age, internet, transport services) are summarized and outlined in Section 5.2.

Table 5.7: Payoff table

Their effort extracting (in hours)	My effort extracting (in hours)							
	1	2	3	4	5	6	7	8
4	37880	39500	40880	42000	42880	43500	43880	44000
5	36880	38500	39880	41000	41880	42500	42880	43000
6	35880	37500	38880	40000	40880	41500	41880	42000
7	34880	36500	37880	39000	39880	40500	40880	41000
8	33880	35500	36880	38000	38880	39500	39880	40000
9	32880	34500	35880	37000	37880	38500	38880	39000
10	31880	33500	34880	36000	36880	37500	37880	38000
11	30880	32500	33880	35000	35880	36500	36880	37000
12	29880	31500	32880	34000	34880	35500	35880	36000
13	28880	30500	31880	33000	33880	34500	34880	35000
14	27880	29500	30880	32000	32880	33500	33880	34000
15	26880	28500	29880	31000	31880	32500	32880	33000
16	25880	27500	28880	30000	30880	31500	31880	32000
17	24880	26500	27880	29000	29880	30500	30880	31000
18	23880	25500	26880	28000	28880	29500	29880	30000
19	22880	24500	25880	27000	27880	28500	28880	29000
20	21880	23500	24880	26000	26880	27500	27880	28000
21	20880	22500	23880	25000	25880	26500	26880	27000
22	19880	21500	22880	24000	24880	25500	25880	26000
23	18880	20500	21880	23000	23880	24500	24880	25000
24	17880	19500	20880	22000	22880	23500	23880	24000
25	16880	18500	19880	21000	21880	22500	22880	23000
26	15880	17500	18880	20000	20880	21500	21880	22000
27	14880	16500	17880	19000	19880	20500	20880	21000
28	13880	15500	16880	18000	18880	19500	19880	20000
29	12880	14500	15880	17000	17880	18500	18880	19000
30	11880	13500	14880	16000	16880	17500	17880	18000
31	10880	12500	13880	15000	15880	16500	16880	17000
32	9880	11500	12880	14000	14880	15500	15880	16000

### 5.6.3 Instructions

*[Translated from Bahasa. Instructions were written in English, translated into Bahasa Indonesia by an native speaker and retranslated by another independent Bahasa Indonesia native speaker. Instructions were read out loud due to a high illiteracy rate.]<sup>†</sup>*

**Welcome** *[This information was given to all participants]*

Hello, Good Afternoon/evening... My Name is ... from UI and RWI. As already mentioned in the survey we are here for a research project about fishing behavior. As a complimentary part of our research, we are going to have a simulation. To guarantee a smooth process, we need to establish some rules first: Please do not talk to each other and do not use any electronic devices such as mobile phones, smart phones, or the like throughout the whole game. If you want to go to restroom, please do it now because we will have the simulation for about 2 hours. If any of you want to go to restroom, you may go now. We won't give permission to go to the restroom when the simulation has begun. During the game, you make your decisions anonymously. Only the researchers will know your identity and your data will be treated confidentially. In order to make these projects as useful as possible to the local population we heavily depend on exact, truthful, and comprehensive information. Are you ready?

**General Information** *[This information was given to all participants]*

In this simulation, you will be sharing a small fish site with four other people. You profit in two different ways from the fish site. By fishing you will earn money, but at the same time you receive benefits from conserving the fish stock. This means, if you refrain from fishing you will help the fish population to grow more sustainable and secure the future of the fish population. At the same time, a more amenable habitat will attract tourists from outside, who pay for the conserved environment. A healthy fish stock will then pay out for you in the future. You will be asked to decide on the amount of time you spend for fishing. Please note that the more time you use for fishing, the more fish you will get. However, at the time you will also reduce the stock of fish, which also means decreasing the profit gained from fish conservation as well as future gains from the fish population.

*[This information was given to participants in external treatment only]* Before you make a decision, you will get an official recommendation about how much time you should spend for fishing. Please notice that even though this is an official recommendation, it is not binding. This means that you and the other four people who share this fish site can spend more or less time fishing than officially recommended.

*[This information was given to participants in democracy treatment only]* In this simulation, you will be deciding together in your group about the number of hours each

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<sup>†</sup>Explanatory notes are given in square brackets.

participant should spend fishing. Each of the group members will propose how many hours each participant should spend fishing. Following this, the median of all the proposals, which is the third highest value proposed by your group, will be treated as the recommended time duration to fish for every member of the team. Please notice that even though this is a recommendation, it is not binding. This means that you and the other four people who share this fish site can spend more or less time fishing than recommended.

*[This information was given to participants in leadership only]* Before you make a decision, you will get a recommendation about the length of time to fish. This recommendation will be made by the head of this group. This leader will be randomly chosen at the time when the simulation starts. Please notice that even though this is a recommendation, it is not binding. This means that you and the other four people who share this fish site can spend more or less time fishing than recommended.

*[This information was given to all participants]* In this simulation, you can earn money, to an amount depending on your decisions and your group members' decisions. The decisions you take will determine how much you can earn during the course of the simulation, so please take your time and make your decisions after thinking carefully. This simulation will go on for ten separate rounds, during which you will play the same exercise and interact with the same four people. In each round you can earn money, and at the end of the simulation we will randomly draw one round and this round will then be paid out. Thus, each of your decisions may be the one that will be paid out in the end, so always think carefully about your decisions.

**Rules of the game** *[This information was given to all participants]*

Remember that in this simulation you will be sharing a small fish site with four other people. You will get benefits from the fish stock in two ways; earning money from fishing or from preserving the fish stock for the future. Your task now is to decide how many hours you want to spend fishing each day. You can spend between one and eight hours fishing per day. Remember, the more hours you spend fishing, the more fish you will catch, but the lower the fish stock.

*[This information was given to participants in external only]* Please notice that before you make your decision, you will receive an official recommendation on how much hours to spend fishing. This recommendation is official, but not binding. This means that you and the other group member can spend more or less hours fishing than officially recommended.

*[This information was given to participants in democracy only]* Please note that in this simulation you decide together with the other four group members how many hours each group member should spend fishing. Each of the group members can spend between

one and eight hours fishing each day. To decide collectively, each of the group members has to propose how many hours each group member should spend fishing. Afterwards, the median of all proposals will be imposed as non-binding recommendation. In other words, you and the other four group members should spend the median of all proposed hours fishing. Please notice that this recommendation is not binding, meaning that you and the other group members can spend more or less hours to fishing than recommended.

*[This information was given to participants in leadership only]* Please note, that at the beginning of each round one of the group members is selected randomly to become the group leader. The person chosen will be informed about the outcome whereas those not chosen will be informed about this outcome as well. The responsibility of the leader is to provide a recommendation on how many hours to be spent by each group member on fishing. This recommendation is not binding, meaning that you and the group members can spend more or less hours fishing than recommended.

*[This information was given to all participants]* Please remember, the simulation consists of ten separate rounds, in which you always play the same simulation and interact with the same four people. After each round, you will be informed about the amount of fish caught and the payoffs of all the players in your group. Please be aware that all the group members face the same decision like you. Your total earnings in each round depend on: (i) The number of hours you spend fishing, (ii) The number of hours the other 4 group members spend fishing. The payoff table [see Table 5.7] shows that the amount of your earning depending on the time that you and the other group members spend fishing per day. Remember your payoff depends on how many hours you and your group members spend fishing per day. Please note, your payoff increases with the hours you spend fishing, but the more hours you and your group spend on fishing, the lower is your benefit from conserving the fish stock.

### **Examples**

*[This information was given to participants in autonomy only]* Example 1: Imagine that you and the other group members spent one hour per day for activities related to fisheries. This means that you and your group members would get 37880 Rupiah at the end of the round. Example 2: Imagine that you and all the other group members spent eight hours per day for activities related to fisheries. This means that you and the other group members would get 16000 Rupiah at the end of the round. Example 3: Imagine that the other group members spent together ten hours per day for activities related to fisheries and you spend one hour per day for activities related to fisheries. This means that you would get 31880 Rupiah at the end of the round. Example 4: Imagine that the other group members spent together ten hours per day with activities related

to fisheries and you spend eight hours per day for activities related to fisheries. This means that you would get 38000 Rupiah at the end of the round.

[*This information was given to participants in external only*] Example 1: Imagine that you get a recommendation to spend one hour per day to fish. You and all other group members follow this recommendation and spend one hour per day fishing. Then you and the other group members earn 37880 Rupiah at the end of the round. Example 2: Imagine that you get a recommendation to spend one hour per day to fish, but you and all the group members spend eight hours per day fishing. Then you and the other group members earn 16000 Rupiah at the end of the round. Example 3: Imagine that you get a recommendation to spend one hour per day to fish. Then you spent exactly one hour per day to fish, but the other group members spent together ten hours per day to fish. Then you earn 31880 Rupiah at the end of the round. Example 4: Imagine that you get a recommendation to spend one hour per day to fish. However, it turns out that the other group members spent together ten hours per day fishing, and you spent eight hours per day fishing. Then you earn 38000 Rupiah at the end of the round.

[*This information was given to participants in democracy only*] Example 1: Imagine that you proposed to spend 1 hour per day to fish, and the four other group members proposed to spend 2, 4, 6, and 8 hours per day fishing. This means that you and all the group members should spend 4 hours per day on fishing. Example 2: Imagine that you proposed to spend 8 hour per day to fish, and the other four group members proposed to spend 1, 2, 4, and 6 hours per day fishing. This means that you and all the group members should spend 4 hours per day on fishing. Example 3: Imagine that you and all the group members proposed to spend one hour per day on fishing. This means that you and all the group members should spend 1 hour per day on fishing. By assuming that you and all the other group members follow this proposal you and all of the group members will earn 37880 Rupiah at the end of the round. Example 4: Imagine that you and all the group members proposed to spend eight hours per day on fishing. This means that you and all the group members should spend 8 hour per day on fishing. By assuming that you and all the other group members follow this proposal you and all the group members will earn 16000 Rupiah at the end of the round. Example 5: Imagine that you and all the group members proposed to spend 3 hours per day on fishing. However, all of the other group members together spend 10 hours per day on fishing and you spend one hour per day on fishing. Then you earn 31880 Rupiah at the end of the round. Example 6: Imagine that you and all the group members proposed to spend 3 hours per day on fishing. However, that all of the other group members together spend 10 hours per day on fishing and you spend 8 hours per day on fishing. Then you earn 38000 Rupiah at the end of the round.

[*This information was given to participants in leadership only*] Example 1: Imagine that you were recommended to spend one hour per day to fish. You and all the other group members follow this recommendation. Then you and your group members earn 37880 Rupiah at the end of the round. Example 2: Imagine that you were recommended to spend one hour per day to fish, but you and all the other group members spend eight hours per day on fishing. Then you and your group members earn 16000 coins at the end of the round. Example 3: Imagine that you were recommended to spend one hour per day to fish. You spend only one hour on fishing per day but the other group members spend ten hours per day fishing. Then you earn 31880 Rupiah at the end of the round. Example 4: Imagine that you were recommended to spend to spend one hour per day to fish. However, the other group members spend 10 hours per day fishing, and you spend 8 hours per day fishing. Then you earn 38000 Rupiah at the end of the round.

[*This information was given to all participants*] We will now play one round to help you to familiarize yourself with the simulation. This round will not count for the payoff. Are there any more questions? If not, I will began the game. And please, be reminded that no smartphones are allowed in this room, you should not talk to each other, and there is no permission to go to the restroom after the simulation has begun without losing all your payoffs.

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## 6 Democracy and compliance in public goods games

*A previous version of this chapter has appeared as:*

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This article can be downloaded via  
<http://ftp.zew.de/pub/zew-docs/dp/dp17038.pdf>

**Abstract:** I investigate if, how, and why the effect of a contribution rule in a public goods game depends on how it is implemented: endogenously chosen or externally imposed. The rule prescribes full contributions to the public good backed by a non-deterrent sanction for those who do not comply. My experimental design allows me to disentangle to what extent the effect of the contribution rule under democracy is driven by self-selection of treatments, information transmitted via the outcome of the referendum, and democracy per se. In case treatments are endogenously chosen via a democratic decision-making process, the contribution rule significantly increases contributions to the public good. However, democratic participation does not affect participants' contribution behavior directly, after controlling for self-selection of treatments and the information transmitted by voting.



## 6.1 Introduction

In this paper, I investigate whether the effect of a rule on how to act in a social dilemma situation depends on how it is implemented: endogenously chosen or externally imposed. Most importantly, my experimental design allows me to determine the drivers of the effect of democratic participation: self-selection of treatments, information transmitted via voting, or democracy per se. Since many interactions in real life related to cooperation are subject to non-deterrent policies, I focus on a rule which prescribes full contributions to a public good and is backed by a weak sanction for those who do not comply. For instance, in international environmental treaties between sovereign nations, like the Kyoto protocol, no third-party mechanism exists to enforce the agreement (e.g., Barrett 2010). Small scale common property goods, like fisheries, do have formal authorities in most cases, but authorities often lack the capacities to monitor, sanction and enforce (e.g., Ostrom 1990; Kroll et al. 2007). Therefore, this experiment is in general related to the vast literature on how to design policies in order to foster cooperation in social dilemma situations in the absence of strong enforcement mechanisms.<sup>75</sup>

However, not the policy itself but rather the process of how it is implemented is at the focus of my paper. Thus, I contribute to the growing economic literature which investigates whether the effect of a policy depends on how it is implemented. One of Elinor Ostrom's design principles characterising robust institutions for managing common-pool resources is that resource users affected by regulations should be authorized to participate in making and modifying the rules (Ostrom 1990). Initially this refers to the importance of local knowledge in devising effective rules. Further - and most of all - positive aspects of participation have been identified in several field studies. Participation is suggested to increase the willingness to follow rules or to avoid that externally imposed regulations crowd out voluntary cooperative behavior (e.g., Ostrom and Nagendra 2006). In this line, Bardhan (2000) shows that users of a common-pool resource tend to manage the resource more successfully when they are genuinely engaged in decisions on rules affecting their use. Further empirical findings by Pommerehne and Weck-Hannemann (1996) and Frey (1998), for example, suggest that income tax compliance in Switzerland increases with democratic participation. A central problem with the interpretation of these studies is that unobservable confounding factors such as self-selection into policies could affect the results.

A series of laboratory experiments aim at taking confounding factors into account in more controlled environments and suggest that the effect of democratic participation is rather nuanced. The majority of the experiments suggest a positive democracy

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<sup>75</sup>For overviews, see, e.g., Ledyard (1995), Chaudhuri (2011), and Zelmer (2003).

premium, i.e., that institutions are more effective if they are endogenously chosen via a democratic decision-making process than externally imposed (e.g., Grimm and Mengel 2009; Sutter et al. 2010; Gürer et al. 2014). However, other experiments provide a more differentiated and mixed picture. Sutter and Weck-Hannemann (2003), for instance, find that democratic participation in determining minimum contributions to a public good does not necessarily raise overall cooperation levels. Especially participants with relatively high obligations reduce contributions, if these are democratically determined. In a related experiment, Sutter and Weck-Hannemann (2004) show that cooperation collapses if groups democratically reject imposing minimum contribution levels. Tyran and Feld (2006) find that the effect of democratic participation can cut both ways. Based on a public goods game, they find that a simple contribution rule which aims at fostering cooperation is more effective in case it is endogenously chosen than the same rule externally imposed. If, by contrast, the rule is endogenously rejected, the effect of participation tends to be negative. Drawing on the experiment by Tyran and Feld (2006) and using samples of students and workers in China, Volland et al. (2017) conclude that the effectiveness of democratic participation depends on its conformity with societal values, norms, and rules. They find that participants cooperate on average the most if the contribution rule is exogenously imposed, what can be explained with a long history and great importance of authoritarian norms in China. One reason that prevents us from identifying a coherent effect based on these studies is that potential drivers of the effect democratic participation are not equally considered, identified, and quantified. Dal Bó et al. (2010) suggest a randomization technique to identify and quantify different drivers of the effect of democratic participation. Based on a prisoner's dilemma they introduce the opportunity to democratically impose a deterrent sanction on mutual defection which transforms their social dilemma game into a coordination problem. After taking potential confounding factors into account, they conclude that the deterrent policy is more likely to be respected if it is democratically chosen than externally imposed.

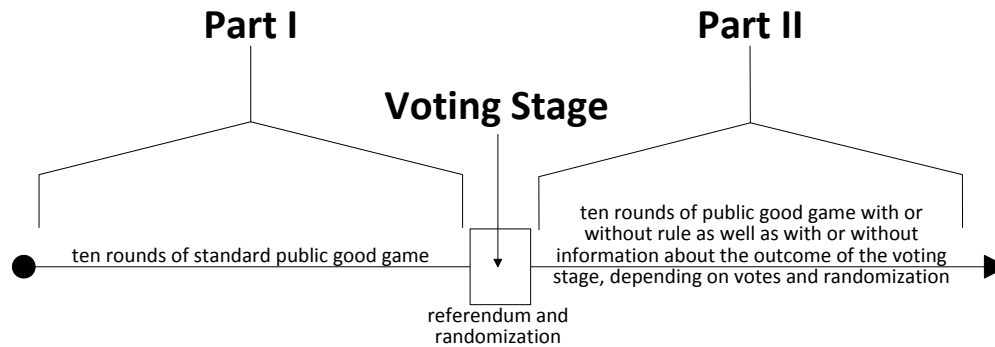
In this paper, I complement the existing literature by investigating whether democratic participation increases participants' compliance with a non-deterrent intervention. I focus on a non-deterrent intervention, what is of fundamental importance since it provides the opportunity to study how participants follow rules although they face incentives not to do so. Furthermore, it remains unclear whether the findings by Dal Bó et al. (2010) can be extrapolated into a setting with a non-deterrent intervention. Based on the experimental design by Tyran and Feld (2006), the rule in my experiment prescribes full contributions to the public good and a sanction for participants who do not comply with the obligation. The sanction is non-deterrent and zero contributions

remain the unique Nash equilibrium in dominant strategies. In addition, my paper extends previous experiments by identifying and separating three potential drivers of the effect of democratic participation. Theory on procedural utility suggests that people do not only value outcomes but also the processes. It indicates that being aware of the fact that the group imposed the policy may directly affect agents' behavior (Frey et al. 2004; Frey and Stutzer 2005). A second hypothesis is that democratic decision making could also affect behavior because it reveals information to agents on their partners' likelihood to favor a specific policy or not, affecting both the agents' beliefs about the partners' future behavior and thus their own behavior (Tyran and Feld 2006). Finally, while groups are randomly formed, they are not necessarily identical. One cannot exclude the possibility that there are unobservable factors that explain both responses to policies and the policy selected (Dal Bó et al. 2010). Adopting an identification strategy suggested by Dal Bó et al. (2010), I control for self-selection and separate the total effect of a democratically chosen contribution rule into a selection, information, and democracy effect.

Using a laboratory experiment, I observe that a contribution rule in a public goods game backed by a mild and non-deterrent sanction improves contributions, if treatments are democratically chosen. In line with Tyran and Feld (2006), I find that contributions to the public good are significantly higher if the rule is democratically chosen than if it is democratically rejected. By contrast, if the corresponding treatments are exogenously given the contribution rule does not affect participants' contribution behavior. A naive comparison of these effects suggest that the contribution rule is more effective in fostering contributions to the public good in case treatments are democratically chosen than in case treatments are externally given. However, my decomposition reveals that the apparently different effects are not driven by democratic participation per se. After taking into account effects of self-selection into treatments and the information transmitted via voting, democratic participation does not directly affect participants' contribution behavior. My findings thus suggest that the effect of democratic participation does not directly increase participants' willingness to comply with rules which are for the common good, but at odds with their individual free-riding incentives.

The remainder of this paper is organized as follows. Section 6.2 presents the experimental design of the study. Results are presented in Section 6.3. A concluding discussion is provided in Section 6.4.

Figure 6.1: Timeline for the experiment



Note: In Part I, subjects play ten round of a standard public good game. Depending on votes and randomization, subjects are assigned to treatments in Part II of the experiment.

## 6.2 Experimental design and procedure

The experimental design is based on a linear public goods game with subjects randomly and anonymously matched into groups of three for the entire experiment. The experiment consists of two parts (Part I and II) and a timeline for the experiment is provided in Figure 6.1.

In Part I, subjects participate in ten rounds of a standard public goods game. Each subject  $i$  receives an initial endowment of  $e = 20$  LabDollar (LD) in each round. Of this endowment an amount  $q_i$  is contributed to a public good, while the rest,  $e - q_i$ , goes to a private account. Subject  $i$ 's payoff ( $\pi_i$ ) is given by the private account plus the benefit from the group's contributions to the public good multiplied by the marginal per capita return of  $\beta = 0.5$ , i.e.,  $\pi_i = e - q_i + 0.5 \sum_{j=1}^3 q_j$ . Since  $\beta < 1 < n\beta$ , complete free-riding ( $q_i = 0$ ) is the dominant strategy for all subjects, according to the standard game theoretic prediction of purely selfish subjects. Full contributions to the public good ( $q_i = 20$ ) are, in contrast, socially optimal.

Right after the ten rounds of this standard public goods game (Part I) has been completed, the voting stage starts (see Figure 6.1). Subjects vote in a referendum on whether or not to enact a contribution rule in Part II of the experiment. The main focus of my experiment is to investigate whether and, if so, how the effect of the rule depends on the procedure of implementation. Therefore, I decided to keep the rule as simple and non-strategic as possible and abstain from introducing rather complex centralized (e.g., Cardenas et al. 2000) or decentralized sanctioning mechanisms (e.g., Carpenter 2007). Following Tyran and Feld (2006), the rule aims at fostering cooperation by prescribing full contributions to the public good backed by a fixed and automatically imposed sanction of  $s = 4$  for subjects who do not comply, i.e.,  $q_i < 20$ . In case the rule is

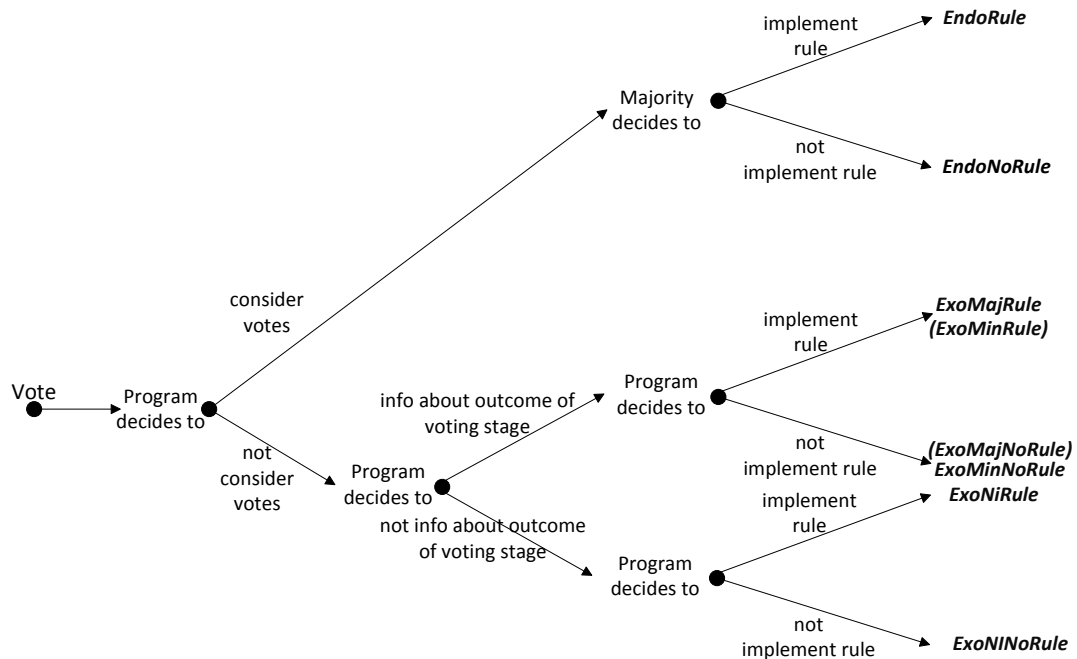
imposed, subject  $i$ 's payoff is given by:

$$\pi_i = \begin{cases} 20 - q_i + 0.5 \sum_{j=1}^3 q_j - 4 & \text{if } q_i < 20 \\ 20 - q_i + 0.5 \sum_{j=1}^3 q_j & \text{if } q_i = 20. \end{cases}$$

With  $s = 4$  the penalty for violating the proposed contribution is rather low and zero contributions to the public good remain the unique Nash equilibrium in dominant strategies. Since  $\beta = 0.5$ , partial contribution is never optimal. Complete free-riding yields a payoff of  $\pi_i(q_i = 0|q_{-i}) = 20 + 0.5 \sum_{j \neq i} q_j - 4$ . Compliance, in contrast, yields  $\pi_i(q_i = 20|q_{-i}) = 10 + 0.5 \sum_{j \neq i} q_j$ . Compliance is rational if and only if  $\pi_i(q_i = 0|q_{-i}) < \pi_i(q_i = 20|q_{-i})$ . This would require a sanction of  $s > 10$ . Thus, for  $s = 4$  full free-riding is the unique Nash equilibrium in dominant strategies, i.e.,  $\pi_i(q_i = 0|q_{-i}) > \pi_i(q_i = 20|q_{-i}) \forall i$ .

Participants in Tyran and Feld (2006) vote in a referendum on whether or not to enact the rule at the beginning of the experiment. In order to enhance the understanding of the game and give participants the opportunity to gain experiences, participants in my experiment play ten rounds of a standard public goods game before they vote (see Figure 6.1). Furthermore, Tyran and Feld (2006) do not explicitly control for confounding factors like self-selection and information transmitted by voting. Using a within-subject design, Tyran and Feld (2006) rely on the strategy method, in which subjects make contingent decisions for all possible outcomes of the referendum. According to standard game theoretic predictions, the strategy method should yield the same decisions as the direct-response method. However, the literature suggests that subjects make different decisions in contingent responses relative to situations where they face given and known decisions (e.g., Falk et al. 2005; Brandts and Charness 2011; Jordan et al. 2016). In this experiment, I rely on a direct-response design and adopt a randomization technique suggested by Dal Bó et al. (2010) to control for self-selection of treatments and to disentangle the effect of a democratically chosen contribution rule. Dal Bó et al. (2010) use a prisoner's dilemma with mutual defection as unique Nash equilibrium and introduce the opportunity to impose a sanction on unilateral defection. The sanction is comparatively strong and both mutual defection and cooperation are Nash equilibria. I investigate the effect of a rather weak and non-deterrent rule. This is of fundamental importance because strong and deterrent rules set strong incentives, thus cooperation and compliance seem easier anyway. Furthermore, many situations in real life, and especially in an environmental policy context, are subject to rather non-deterrent interventions. Either no supra authorities exist in order to monitor, enforce, and sanction any policy, or, in case authorities exist, they lack the resources to enforce policies.

Figure 6.2: Illustration of the randomization technique



Note: Randomization technique adopted by Dal Bó et al. (2010). After all participants have voted the program decides randomly whether to consider the votes. In case the votes are not considered, it randomly decides whether to reveal the information about the outcome of the voting stage and thereafter whether to implement the rule. Consequently, participants could be assigned randomly to eight different treatments. However, only six out of all the treatments are of primary interest, i.e., *EndoRule*, *EndoNoRule*, *ExoMajRule*, *ExoMinNoRule*, *ExoNiRule*, and *ExoNiNoRule*. The two remaining treatments, i.e., *ExoMinRule* and *ExoMajNoRule*, are not considered in the following analysis and, therefore, placed in parentheses.

I complement the existing literature by combining the experiments by Tyran and Feld (2006) and Dal Bó et al. (2010) in order to test whether the effect of a weak and non-deterrent contribution rule in a public goods game depends on how it has been implemented. The corresponding randomization strategy is summarized in Figure 6.2. First, all three participants per group vote simultaneously and anonymously in a referendum on whether to enact the rule or not. Second, the computer randomly chooses whether to consider the votes in each group. If the computer considers the votes, the majority wins. If the computer does not consider the votes, it randomly chooses whether or not to reveal the information regarding the outcome of the referendum and, in a second step, whether or not to impose the rule exogenously.

After the voting stage, subjects are assigned to treatments (see Figure 6.1). Depending on votes and the randomization technique, they are informed whether the computer randomly chose to consider votes and whether the rule is implemented.<sup>76</sup> In case par-

<sup>76</sup>Subjects were informed that the computer will randomly decide. However, following Dal Bó et al.

ticipants do receive the information about the outcome of the voting stage, they not learn the exact distribution of votes. They learn whether at least two subjects or at the most one subject per group voted for the rule.

The eight possible treatments are denoted as *EndoRule*, *EndoNoRule*, *ExoMajRule*, *ExoMinRule*, *ExoMajNoRule*, *ExoMinNoRule*, *ExoNiRule*, and *ExoNiNoRule* (see Figure 6.2). *Endo* denotes that the votes of the group were considered and *Exo* denotes that the computer overrode the votes. *Rule* denotes that the rule is implemented versus *NoRule*. In case the information regarding the outcome of the referendum is available, *Maj* denotes that the majority of the group supported the rule, *Min* denotes that only a minority supported the rule. *Ni* denotes that this information is not available.<sup>77</sup>

After Part II has been completed, agents fill in a final questionnaire on socio demographic characteristics as well as attitudes and values adapted from established value surveys (World Value Survey 2014). I included questions to measure participants' trust level, locus of control, political preferences and political commitments as well as their acceptance of authorities. The experiment was conducted at the mLab of the University of Mannheim, Germany. I used the experimental software z-Tree developed by Fischbacher (2007) for programming, and participants were recruited via ORSEE (Greiner 2015). I conducted 17 sessions between October 2016 and June 2017 with a total of 270 participants.<sup>78</sup> A session lasted on average slightly more than 60 minutes and participants earned on average 11.60 euros, with a maximum of 18.00 euros and a minimum of 5.00 euros.

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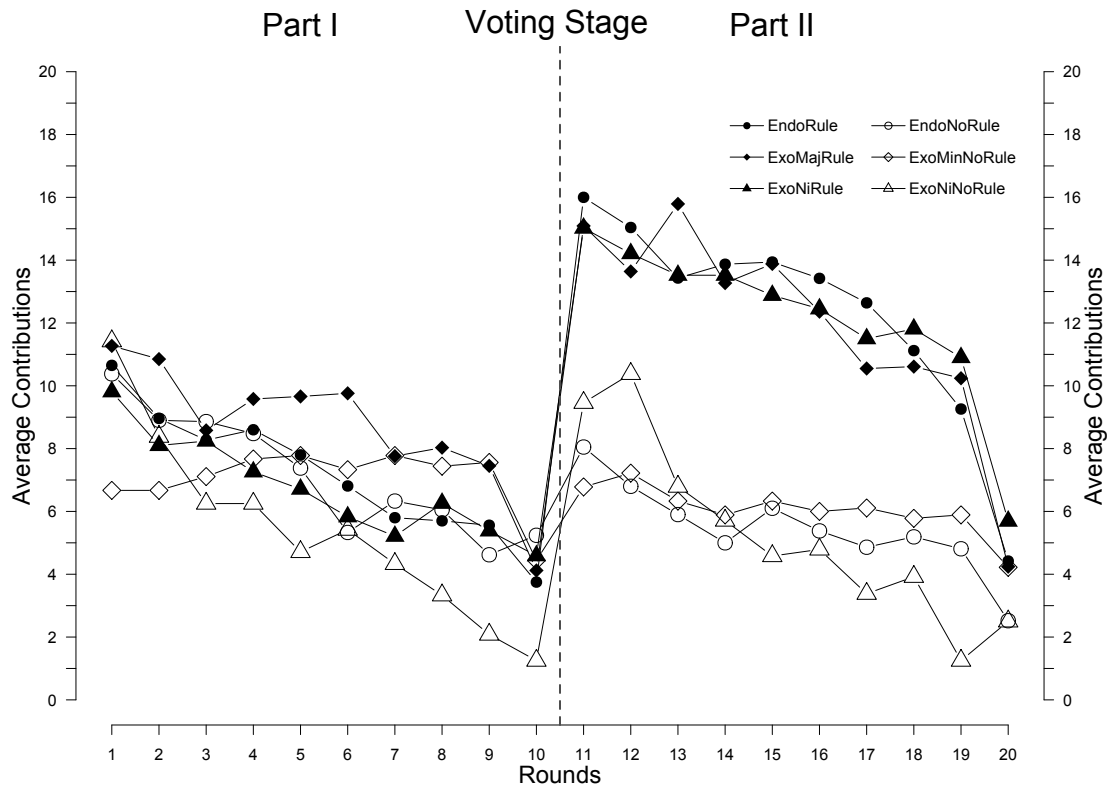
(2010) they were not informed about the exact probabilities. The instructions said that "the computer will randomly choose whether to consider the votes or not in your group", "it will randomly choose whether to reveal the outcome of the voting stage or not" and "it will randomly choose whether to implement the contribution rule or not". Instructions and screenshots are provided in the appendix.

<sup>77</sup>Only six out all eight possible treatments are of central importance for the following analysis, i.e., *EndoRule*, *EndoNoRule*, *ExoMajRule*, *ExoMinNoRule*, *ExoNiRule*, *ExoNiNoRule*. The program, however, decides randomly whether to consider votes, and in case the votes are not considered, whether to reveal the information of the voting stage and thereafter to implement the rule. It is thus possible that the votes are not considered, the rule exogenously implemented (not implemented) and participants informed that a majority of their group members are against (in favor of) the rule, i.e., *ExoMinRule*, *ExoMajNoRule*. The function of the information treatments is to provide the necessary intermediate steps between the exogenously imposed and democratically chosen treatments. This is done by comparing participants with the same information of the outcome of the referendum across treatments, i.e., *EndoRule* vs. *ExoMajRule* and *EndoNoRule* vs. *ExoMinNoRule*. Following this logic, there is no equivalent for *ExoMinRule* and *ExoMajNoRule* under democracy. Therefore, both treatments are not considered in my analysis.

<sup>78</sup>Panel A of Table 6.2 summarizes the number of participants by treatment and vote. The two treatments *ExoMinRule* and *ExoMajNoRule* are irrelevant for my analysis, therefore the results are based on the 213 participants in the treatments of primary interest, i.e., *EndoRule*, *EndoNoRule*, *ExoMajRule*, *ExoMinNoRule*, *ExoNiRule*, and *ExoNiNoRule*.

### 6.3 Results

Figure 6.3: Contributions by treatment



Note: Average contributions to the public good in LabDollar (LD) by round and vote stage results. In Part I, all participants play a voluntary contribution mechanism. After Part I participants vote in a referendum on whether or not to enact the contribution rule. Depending on individual votes and the randomization strategy described in Section 6.2 participants are assigned to treatments in Part II. *EndoRule* (*EndoNoRule*): contribution rule is democratically chosen (rejected). *ExoMajRule* (*ExoMinNoRule*): contribution rule is externally imposed (not imposed) and participants receive the information that the majority (minority) of their group supported the rule. *ExoRule* (*ExoNoRule*): contribution rule is externally imposed (not imposed).

Average contributions to the public good over rounds and across treatments in both parts of the experiment are summarized in Figure 6.3. In the first part of the experiment, both the level of contributions to the public good as well as the pattern are comparable to other voluntary contribution mechanisms (e.g., Ledyard 1995; Zelmer 2003; Chaudhuri 2011). In Part I, participants contribute on average 6.99 LD to the public good and contributions decrease over rounds with an average of 10.40 LD in round 1 and 3.63 LD in round 10. Although all participants played a standard voluntary contribution mechanism without any interventions in the first ten rounds, there are differences across treatments in terms of participants' contributions, especially at the end of Part I. According to a Kruskal-Wallis test, contributions cannot be considered



as equal across treatments in the last five ( $p$ -value: 0.066) and three ( $p$ -value: 0.040) rounds of the first part of the experiment. More precisely, in round 10 participants in *ExoNiNoRule* contribute on average 1.25 LD to the public good and therefore significantly less than participants in the other treatments.<sup>79</sup> Therefore, even before the voting stage and the assignment to treatments, participants cannot be considered statistically identical in terms of contribution levels.

In line with previous evidence on the restart effect in prisoner's dilemma games (e.g., Andreoni and Miller 1993) and public goods games (e.g., Andreoni 1988), contributions increase at the beginning of the second part of the experiment (see Figure 6.3). The increase is much larger in case the rule is implemented, which leads to substantial differences across treatments in Part II ( $p$ -value: 0.000, Kruskal-Wallis test) and especially in round 11 ( $p$ -value: 0.000, Kruskal-Wallis test).<sup>80</sup>

In order to estimate and disentangle the effect of democratic participation, I follow Dal Bó et al. (2010) and initially focus on behavior in the first round of Part II, i.e., round 11. Since participants cannot be considered statistically identical in terms of cooperation levels in the first part of the experiment, I use individual differences in contribution levels between round 11 (i.e., the first round of Part II) and round 10 (i.e., the last round of Part I) as primary outcome variable.

### 6.3.1 Voting behavior

The vast majority of the 270 participants wanted the rule to be introduced. More precisely, significantly more participants vote in favor of the rule than against it: 196 (72.59%) yes-voters versus 74 (27.41%) no-voters ( $p$ -value: 0.000, Binomial test).

**Result 1:** Participants vote for rule in the majority of all cases.

The approximately 73% of participants voting for the rule are clearly above the 50% obtained by Tyran and Feld (2006) and 53% by Dal Bó et al. (2010). However, the intervention of Dal Bó et al. (2010) differs substantially from the rule used by Tyran and Feld (2006) and in this experiment. The intervention by Dal Bó et al. (2010) affects the equilibrium of their prisoner's dilemma. It is comparatively strong intervention and both mutual defection and cooperation are Nash equilibria. This could explain the comparatively low level of support.<sup>81</sup> I use the same contribution rule as Tyran and Feld (2006), however, the experiments differ in their protocols. In the experiment

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<sup>79</sup>Differences are robust to corrections for multiple hypothesis testing proposed by List et al. (2016).

<sup>80</sup>In Part II of the experiment, the differences are also robust to corrections for multiple hypothesis testing (List et al. 2016).

<sup>81</sup>See Section 6.2 for a detailed discussion of the differences in interventions.

Table 6.1: Determinants of voting behavior

	Dependent variable: Yes	
	Coefficients	Average marginal effects
<i>Coop. Part I: Own</i>	0.08** (0.042)	0.02** (0.012)
<i>Coop. Part I: Others</i>	-0.03 (0.020)	-0.01 (0.006)
<i>Trust</i>	-0.06 (0.123)	-0.02 (0.036)
<i>Locus of control</i>	0.15** (0.058)	0.04*** (0.016)
<i>Obey authority</i>	0.14 (0.179)	0.04 (0.052)
<i>Democrat</i>	-0.01 (0.069)	-0.00 (0.020)
<i>Pol. commitment</i>	-0.45*** (0.158)	-0.13*** (0.044)
<i>Female</i>	-0.41** (0.205)	-0.12* (0.058)
<i>Age</i>	0.00 (0.020)	0.00 (0.060)
<i>Constant</i>	-0.23 (0.900)	
Observations	216	216
Log likelihood	-112.031	

Note: Probit regression. Coefficients (average marginal effects) with robust standard errors in parentheses in column 1 (2). \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Dependent variable (*Yes*): 1 if participant votes for rule and 0 otherwise. *Coop. Part I: Own* (*Others*): average own contributions (contributions of others) in Part I. *Trust*: index for stated trust level between 0 (low) and 1 (high). *Locus of control*: stated locus of control on a scale between 1 (low) and 10 (high). *Obey authority*: index for stated respect for authorities between 0 (high) and 1 (low). *Democrat*: stated importance of living in a democratic system on a scale between 1 (low) and 10 (high). *Pol. commitment*: index for stated political commitment between 0 (low) and 1 (high).

by Tyran and Feld (2006) participants do not interact before they vote on whether or not to impose the contribution rule. In my experiment, in contrast, participants play ten rounds of a standard public goods game before they vote (see Figure 6.1). The experience they have made in Part I and the enhanced understanding of the experiment could drive the differences in voting behavior.

I define the variable *Yes*, which is a binary variable for whether participants vote in favor of the rule or not, in order to analyze participants voting behavior in more detail via estimating a regression model. Results are shown in Table 6.1. In line with Dal Bó et al. (2010) my results suggest that participants own contributions to the public good in Part I of the experiment (*Coop. Part I: Own*) are positively and significantly correlated with voting for the rule. More cooperative participants are more likely to vote for enacting the rule. In this line, voting for the rule is negatively correlated with the average contributions of the other group members in Part I (*Coop. Part I: Others*). However, the effect does not reach the conventional significant levels. In addition, my findings suggest that participants who are convinced to be able to control events that affect their lives (*Locus of control*) are more likely to vote for the rule. Furthermore, males are more likely to favor the rule compared to females (*Female*). Finally, I find that that participants' political commitment (*Pol. commitment*) affects participants' voting behavior significantly. Participants reporting a strong political commitment vote significantly less frequently for enacting the rule than participants with a weak commitment.

### 6.3.2 Endogenous vs. exogenous treatments effects: Aggregated analysis

The main results of Part II of the experiment are summarized in Table 6.2. Panel A shows the number of observations by vote and treatments. Treatment effects shown in Panel B are estimated by the differences in individual contribution levels between round 11 (i.e., the first round of Part II) and 10 (i.e., the last round of Part II).<sup>82</sup>

To start the aggregated analysis I derive the total treatment effect (*TotalTrE*) of the democratically chosen rule by comparing the treatment effect in case the rule is democratically chosen (*EndoRule*) with the effect if it is democratically rejected (*EndoNoRule*). By randomly assigning participants to treatments and conditioning on individual votes, I can decompose this *TotalTrE* into four components: the exogenous treatment effect (*ExoTrE*), the effect of revealing the information about the outcome of the referendum (*InfoE*), the effect of self-selection into treatments (*SelE*), and the effect of democratic participation (*DemoE*).<sup>83</sup>

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<sup>82</sup>The two treatments *ExoMinRule* and *ExoMajNoRule* are not required to estimate and disentangle the effect of democracy and therefore not considered in Table 6.2. A summary of all individual contributions in Part I and Part II of the experiment is given in Table 6.6 in the appendix.

<sup>83</sup>A graphical illustration of the decomposition strategy is provided in Figure 6.5 in the appendix.

Table 6.2: Summary statistics - individual level data

Considering votes			Not considering votes			
Vote	EndoRule	EndoNoRule	Information available		Information not available	
			ExoMajRule	ExoMinNoRule	ExoNiRule	ExoNiNoRule
Panel A. Votes						
No	14	14	4	6	13	6
Yes	70	7	29	3	29	18
Total	84	21	33	9	42	24
Panel B. Treatment effects (Differences in individual contributions between round 11 and 10)						
No	10.14	1.5	-0.5	1.67	5.54	6.17
Yes	12.67	5.43	12.55	3.67	12.62	8.88
Total	12.25	2.81	10.97	2.33	10.43	8.21

Note: Panel A summarizes the number of observations by vote and result of the voting state across treatments. Treatment effects are summarized in Panel B. Treatment effects are measured by differences in individual contribution levels between round 11 (i.e., the first round of Part II) and round 10 (i.e., the last round of Part I).

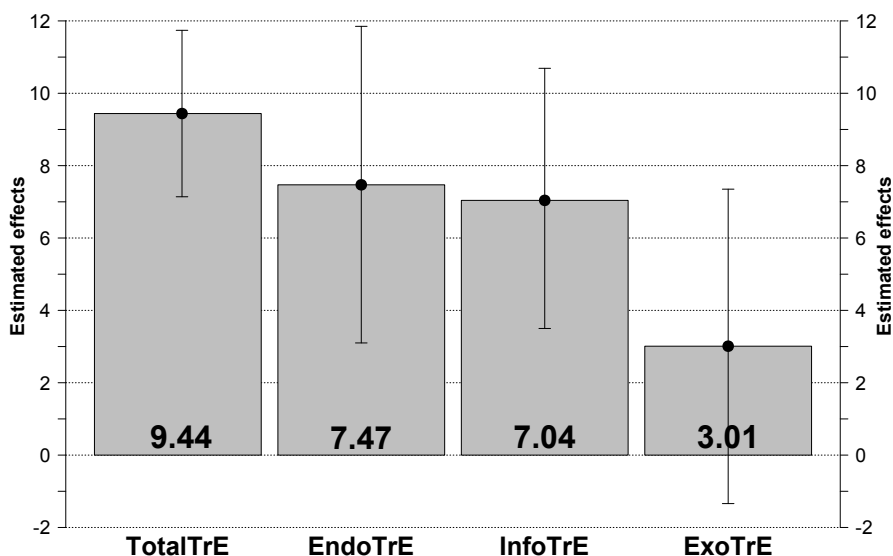
In order to structure the analysis, I extend the analysis of Dal Bó et al. (2010) by explicitly addressing the effect of information transmitted via the results of the referendum. In this sense, I denote as  $g(v|M, I, R)$  the proportion of subjects who vote  $v \in \{Y, N\}$  (in favor or against the rule) given the procedure of implementation  $M \in \{Endo, Exo\}$  (democratically chosen or randomly by the computer), the information available about the outcome of the voting stage  $I \in \{Maj, Min, Ni\}$  (majority or minority support the rule or no information available), structure of the experiment  $R \in \{Rule, NoRule\}$  (rule imposed or not), and let  $q(v|M, I, R)$  be the difference between contributions in round 11 and round 10 of participants who voted  $v$  given the structure of the experiment  $R$ , the amount of information available  $I$ , and the procedure of implementation  $M$ .

The statistical inference in this section is based on a series of linear regression models. Since the decomposition relies on ex-post estimates of linear combinations of regression coefficients, all regressions are estimated with indicator variables for the different treatments that are separated for yes- and no-voters and without a constant.<sup>84</sup> To ease interpretation, results of the decomposition analysis are illustrated in Figure 6.4 and summarized in Table 6.3.

*Total Treatment Effect* - The first two columns in Panel B of Table 6.2 show that the increase in individual contributions at the beginning of Part II is substantially stronger if

<sup>84</sup>Results are provided in Table 6.7 in the appendix.

Figure 6.4: Decomposition analysis



Note: Estimated total treatment effect (*TotalTrE*), endogenous treatment effect (*EndoTrE*), information treatment effect (*InfoTrE*), and exogenous treatment effect (*ExoTrE*). Confidence intervals at the 90%-level. The information effect (*InfoE*) is given by the difference between *InfoTrE* and *ExoTrE*. The selection effect (*Sele*) is given by the *TotalTrE* and the difference between *EndoTrE*. The democracy effect (*DemoE*) is given by the difference between *EndoTrE* and *InfoTrE*.

the rule is democratically chosen than democratically rejected: 12.25 vs. 2.81. Following Dal Bó et al. (2010), I can calculate the *TotalTrE* as weighted average of individual contributions by participants' voting behavior if I use the proportion of participants who vote for and against the rule as weights.

$$TotalTrE = \sum_{v \in \{Y, N\}} [g(v|Endo, Maj, Rule)q(v|Endo, Maj, Rule) - g(v|Endo, Min, NoRule)q(v|Endo, Min, NoRule)].$$

This shows that participants contribute on average 9.44 LD more to the public good in case the rule is democratically chosen than democratically rejected (*p*-value: 0.000, Table 6.3 - row 1).<sup>85</sup> This first observation can be summarized by establishing the following result:

**Result 2:** Contributions are higher if the rule is democratically chosen than if it is democratically rejected.

<sup>85</sup> $TotalTrE = (\frac{14}{84} * 10.14 + \frac{70}{84} * 12.67) - (\frac{14}{21} * 1.50 + \frac{7}{21} * 5.43) = 9.44$ . If not mentioned otherwise, the statistical analysis in this section is based on linear regression models presented in Table 6.7 in the appendix. Test statistics and *p*-values correspond to Wald tests based on respective linear combinations of regression coefficients presented in Table 6.7.

In case both treatments are exogenously imposed and participants do not receive the information about the outcome of the referendum, the rule does not significantly increase contribution levels (*ExoNiRule*: 10.43 vs. *ExoNiNoRule*: 8.21,  $p$ -value: 0.357). Moreover, by simply comparing these two effects I can replicate the finding by Tyran and Feld (2006) that the effect of the rule is stronger under democracy than if treatments are exogenously given (9.44 vs. 2.22,  $p$ -value: 0.024).<sup>86</sup>

However, the *TotalTrE* captures at least two changes. A change in treatments (*EndoRule* vs. *EndoNoRule*) and, by design, a change in the proportion of yes- and no-voters across treatments. Furthermore, under democracy participants do know the outcome of the referendum. This is not the case if treatments are exogenously given and could also affect their behavior. Before we take potential effects of self-selection into treatments and the information transmitted via voting into account the naive comparison between endogenously and exogenously implemented treatments could be biased. By conditioning on the proportion of yes- and no-voters or the contributions per treatment, I can separate the *TotalTrE* into an endogenous treatment effect (*EndoTrE*) and a selection effect (*SelE*).

*Endogenous Treatment Effect* - The *EndoTrE* leaves the proportion of yes- and no-voters constant and captures only the endogenous change in the structure of the experiment.

$$\begin{aligned} \text{EndoTrE} = \sum_{v \in \{Y, N\}} g(v|Endo, Maj, Rule) [q(v|Endo, Maj, Rule) \\ - q(v|Endo, Min, NoRule)]. \end{aligned}$$

In other words, it measures the effect of changing treatments democratically assuming that the proportion of yes- and no-voters is the same in both treatments. With 7.47 LD the effect loses some of its strength, but contributions are still significantly higher in case the rule is democratically chosen than if it is rejected ( $p$ -value: 0.006, Table 6.3 - row 1).<sup>87</sup> That the *EndoTrE* is de facto slightly below the *TotalTrE* indicates a weak and positive effect of self-selection into treatments under democracy.

*Selection Effect* - The *SelE* is given by the difference between the *TotalTrE* and the *EndoTrE*. It captures the effect of the change in the proportion of yes- and no-voters in *EndoRule* and *EndoNoRule* leaving the contributions constant across treatments.

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<sup>86</sup>Under democracy, a comparison of average contributions in *EndoRule* (12.25) and *EndoNoRule* (2.81) reveals a treatment effect of 9.44 LD. Following this logic and simply comparing average contribution in *ExoNiRule* (10.43) and *ExoNiNoRule* (8.21) leads to a treatment effect of 2.22 LD in case treatments are exogenously imposed.

<sup>87</sup> $\text{EndoTrE} = \frac{14}{84} * (10.14 - 1.50) + \frac{70}{84} * (12.67 - 5.43) = 7.47.$

$$SelE = \sum_{v \in \{Y, N\}} [g(v|Endo, Maj, Rule) - g(v|Endo, Min, NoRule)]q(v|Endo, Min, NoRule).$$

The effect of self-selection is given by 1.97 LD.<sup>88</sup> In line with Dal Bó et al. (2010) this indicates that yes-voters show the tendency to contribute more to the public good than no-voters across treatments. However, the selection effect is statistically indistinguishable from zero ( $p$ -value: 0.282, Table 6.3 - row 1).

*Exogenous Treatment Effect* - The *ExoTrE* captures the change in contributions to the public good due to an exogenous change in treatments in case participants do not receive any information about the outcome of the voting stage. As in the endogenous treatment effect, it leaves the proportion of yes- and no-voters constant across treatments in order to take the effect of self-selection into account.

$$ExoTrE = \sum_{v \in \{Y, N\}} g(v|Endo, Maj, Rule)[q(v|Exo, Ni, Rule) - q(v|Exo, Ni, NoRule)].$$

The *ExoTrE* is given by 3.01 LD and statistically indistinguishable from zero ( $p$ -value: 0.253, Table 6.3 - row 1).<sup>89</sup> In case treatments are exogenously given, the rule does not affect participants' contribution behavior. In line with Tyran and Feld (2006), this leads to the following result:

**Result 3:** In case treatments are exogenously imposed, the rule does not increase contributions.

*Information Treatment Effect* - Analogous to the *ExoTrE* the information treatment effect (*InfoTrE*) captures the change in contributions due to an exogenous change in treatments and leaves the proportion of yes- and no-voters constant. In addition, the information about the outcome of the voting stage is revealed.

$$InfoTrE = \sum_{v \in \{Y, N\}} g(v|Endo, Maj, Rule)[q(v|Exo, Maj, Rule) - q(v|Exo, Min, NoRule)].$$

I can calculate this effect as 7.04 LD.<sup>90</sup> In case treatments are exogenously given and the information about the outcome of the referendum is revealed the rule does significantly affect participants' contribution behavior ( $p$ -value: 0.002, Table 6.3 - row 1).

<sup>88</sup>  $SelE = (\frac{14}{84} - \frac{14}{21}) * 1.5 + (\frac{70}{84} - \frac{7}{21}) * 5.43 = 1.97.$

<sup>89</sup>  $ExoTrE = \frac{14}{84} * (5.54 - 6.17) + \frac{70}{84} * (12.62 - 8.88) = 3.01.$

<sup>90</sup>  $InfoTrE = \frac{14}{84} * (-0.50 - 1.67) + \frac{70}{84} * (12.55 - 3.67) = 7.04.$

*Information Effect* - In order to isolate the effect of the information transmitted by the voting stage, I use the difference between the information treatment effect (*InfoTrE*) and the exogenous treatment effect (*ExoTrE*). Therefore, the information effect (*InfoE*) leaves the proportion of yes- and no-voters, the treatments and how they have been imposed constant and only captures the effect of revealing the outcome of the voting stage.

$$\begin{aligned} InfoE = & \sum_{v \in \{Y, N\}} g(v|Endo, Maj, Rule) \\ & [(q(v|Exo, Maj, Rule) - q(v|Exo, Min, NoRule)) \\ & - (q(v|Exo, Ni, Rule) - q(v|Exo, Ni, NoRule))]. \end{aligned}$$

It is given by 4.03 LD, but the difference is statistically not different from zero ( $p$ -value: 0.239, Table 6.3 - row 1).<sup>91</sup>

*Democracy Effect* - Finally, the democracy effect (*DemoE*) captures the effect of choosing treatments democratically. It is measured by the difference between the endogenous treatment effect (*EndoTrE*) and the information treatment effect (*InfoTrE*). It leaves the proportion of yes- and no-voters, the information available and respective treatments constant. Only the procedure how treatments have been implemented changes.

$$\begin{aligned} DemoE = & \sum_{v \in \{Y, N\}} g(v|Endo, Maj, Rule) \\ & [(q(v|Endo, Maj, Rule) - q(v|Endo, Min, NoRule)) \\ & - (q(v|Exo, Maj, Rule) - q(v|Exo, Min, NoRule))]. \end{aligned}$$

The *DemoE* is given by 0.43 LD and indicates that democratic participation does not affect contributions directly ( $p$ -value: 0.900, Table 6.3 - row 1).<sup>92</sup> This leads to the following result:

**Result 4:** After controlling for self-selection into treatments and information transmitted via voting, democratic participation does not affect contributions.

*Decomposition* - Having calculated all the different effects, I can decompose the total treatment effect of 9.44 LD into four components. It can be rewritten as  $TotalTrE = ExoTrE + InfoE + SelE + DemoE$ .

The *TotalTrE* is given by the effect of the rule if treatments are exogenously imposed ( $ExoTrE = 3.01$ ), the effect of revealing the outcome of the referendum ( $InfoE = 4.03$ ),

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<sup>91</sup>  $InfoE = \frac{14}{84} * (-0.50 - 1.67) + \frac{70}{84} * (12.55 - 3.67) - (\frac{14}{84} * (5.54 - 6.17) + \frac{70}{84} * (12.62 - 8.88)) = 4.03$ .

<sup>92</sup>  $DemoE = \frac{14}{84} * (10.14 - 1.50) + \frac{70}{84} * (12.67 - 5.43) - (\frac{14}{84} * (-0.50 - 1.67) + \frac{70}{84} * (12.55 - 3.67)) = 0.43$ .



Table 6.3: Summary of aggregated effects

		<i>TotalTrE</i>	<i>EndoTrE</i>	<i>Sele</i>	<i>ExoTrE</i>	<i>InfoTrE</i>	<i>InfoE</i>	<i>DemoE</i>
Basis ↓								
(1)	Differences in contributions between round 11 and 10	9.44*** (1.379)	7.47*** (2.626)	1.97 (1.813)	3.01 (2.605)	7.04*** (2.185)	4.04 (3.400)	0.43 (3.416)
(2)	Contributions in round 11	7.95*** (1.583)	7.02*** (2.579)	0.93 (2.03)	6.47*** (2.32)	7.47** (3.652)	0.93 (4.327)	-0.45 (4.471)
(3)	Average contributions in Part II	6.85*** (1.926)	6.52*** (1.919)	0.33** (0.161)	7.56*** (2.701)	6.41* (3.706)	-1.15 (4.587)	0.109 (4.174)

Note: Estimated effects with standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Estimates are based on differences between individual contributions in round 11 and 10 (row 1), individual contributions in round 11 (row 2), and average contributions in Part II of the experiment (row 3). Estimates and standard errors are based on coefficients and weighted linear combinations of coefficients of regressions presented in column (1), (2), and (3) of Table 6.7 in the appendix, respectively.

the effect of self-selection into treatments under democracy ( $Sele = 1.97$ ), and, finally, the effect of democratic participation itself ( $DemoE = 0.43$ ). The decomposition shows that the effect of democratic participation is not statistically significant and is also rather low in magnitude. My results suggest that the democratic participation explains only 5% of the total treatment effect.

The absence of a direct effect of democratic participation is robust to restricting the analysis to contributions in the first round of Part II of the experiment (see Table 6.3 - row 2) and expanding the analysis to average contributions in all ten rounds of the second part (see Table 6.3 - row 3).<sup>93</sup> By considering only the average contributions in the first round of Part II, contributions to the public good are on average significantly higher in case the rule is democratically chosen than if it is democratically rejected (16.00 vs. 8.05,  $p$ -value: 0.000) and the *TotalTrE* is given by 7.95 LD. The *TotalTrE* consists of the effect of the exogenously imposed treatments ( $ExoTrE = 6.47$ ,  $p$ -value: 0.007), the information effect ( $InfoE = 0.997$ ,  $p$ -value: 0.818), the effect of self-selection into treatments ( $Sele = 0.929$ ,  $p$ -value: 0.649), and the direct effect of democratic participation ( $DemoE = -0.446$ ,  $p$ -value: 0.921). By expanding the analysis to all ten rounds of Part II of the experiment, the *TotalTrE* amounts to 6.85 LD ( $p$ -value: 0.001). This effect can be decomposed into a *ExoTrE* of 7.56 LD ( $p$ -value: 0.005), a *InfoE* of -1.15 LD ( $p$ -value: 0.799), a *Sele* of 0.33 LD ( $p$ -value: 0.040), and a *DemoE* of 0.109 LD ( $p$ -value: 0.979). In case the analysis is restricted to contributions in round 11 or expanded to the average

<sup>93</sup>Contributions in the in round 11 and all ten rounds of Part II are shown in Table 6.6 in the appendix. A summary of all estimated aggregated effects is given in Table 6.3.

of contributions in all ten rounds of Part II, I find that the rule increases contributions significantly even in case treatments are exogenously given. Furthermore, when giving participants time to learn and coordinate by considering contributions in all ten rounds of Part II, the effect of self-selection into treatments becomes more important and statistically significant.

### 6.3.3 Endogenous vs. exogenous treatments effects: Individual level analysis

When I control for self-selection into treatments and the information transmitted via the outcome of the election, the aggregated analysis in Section 6.3.2 shows that democratic participation does not affect participants' contribution behavior. However, the effect may vary across individual types of participants, especially yes- and no-voters, and could also depend on further individual characteristics.

To take individual heterogeneity into account, I estimate a series of linear regression models separately for yes- and no-voters and control for additional individual attributes. I use linear regressions models with individual differences in contributions between round 11 (i.e., the first round of Part II) and 10 (i.e., the last round of Part I) as dependent variable. The difficulty is that participants in groups that choose or reject the rule democratically may be different from those participants in exogenously imposed treatments. I can derive unbiased estimates by conditioning on participants' votes. Furthermore, I can disentangle the effect of democratic participation by conditioning on the information available and the structure of the experiment.

To ease ex-post comparisons of coefficients across the procedure of implementation (*Endo* vs. *Exo*), the information available (*Maj* vs. *Min* vs. *Ni*), and the structure of the experiment (*NoRule* vs. *Rule*), all regressions are estimated without a constant and indicator variables for the different treatments. Table 6.4 contains the regression results. More precisely, I can estimate the information effect by comparing contributions under externally imposed treatments with treatments under exogenously imposed treatments where the outcome of the election is revealed, i.e., *ExoNiRule* vs. *ExoMajRule* if the rule is implemented and *ExoNiNoRule* vs. *ExoMinNoRule* if it is not imposed. Following this logic, I estimate the democracy effect by comparing contributions in case the rule is democratically chosen or rejected with contributions in case treatments are externally imposed and the information about the referendum are available, i.e., *ExoMajRule* vs. *EndoRule* if the rule is implemented and *ExoMinNoRule* vs. *EndoNoRule* if it is not. The main results of this analysis are summarized in Table 6.5. Among participants who voted in favor of the rule, i.e., yes-voters, there is no evidence that the information transmitted via voting affect participants' contribution levels, if the rule is imposed. According to Table 6.4 - column 1, yes-voters contribute 12.62

Table 6.4: Individual contributions

	Dependent variable: Differences in contributions between round 11 and 10			
	Yes-voter		No-Voter	
	(1)	(2)	(3)	(4)
<i>EndoRule</i>	12.67*** (1.259)	27.92*** (6.422)	10.14*** (2.454)	-2.27 (9.692)
<i>EndoNoRule</i>	5.43* (2.917)	22.51*** (7.658)	1.50 (0.964)	-13.32 (10.096)
<i>ExoMajRule</i>	12.55*** (2.155)	26.08*** (6.939)	-0.50 (0.459)	-11.03 (10.592)
<i>ExoMinNoRule</i>	3.67** (1.551)	17.16** (7.357)	1.67** (0.721)	-12.15 (9.34)
<i>ExoNiRule</i>	12.62*** (2.426)	28.13*** (6.558)	5.54* (3.105)	-6.35 (9.89)
<i>ExoNiNoRule</i>	8.88*** (1.716)	21.89*** (6.558)	6.17** (3.019)	-6.85 (9.463)
<i>Coop. Part I: Own</i>		-0.508 (0.374)		-1.42 (0.83)
<i>Coop. Part I: Others</i>		-0.08 (0.200)		0.59 (0.432)
<i>Trust</i>		-0.684 (1.068)		-0.28 (1.837)
<i>Locus of control</i>		0.65 (0.522)		-0.89 (1.09)
<i>Obey authority</i>		3.50** (1.652)		-2.61 (2.83)
<i>Democrat</i>		-1.56*** (0.523)		1.63 (1.284)
<i>Pol. commitment</i>		0.79 (1.44)		3 (2.779)
<i>Female</i>		0.64 (1.436)		3.56 (3.276)
<i>Age</i>		-0.07 (0.186)		0.13 (0.187)
Observations	156	123	57	42
Adj. R <sup>2</sup>	0.644	0.726	0.403	0.565

Note: OLS regressions. Standard errors, in parentheses, are clustered at the group level. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . In order to ease the ex-post comparisons of coefficients, all regressions are estimated without a constant. Regressions are done for yes-voters (column 1, 2) and no-voters (column 3,4) separately. Differences in individual contributions between round 11 and 10 are the dependent variable. The independent variables are indicator variables for the different treatments. Further control variables, see Section 6.3.1.

Table 6.5: Summary of individual effects

		Information effect		Democracy effect	
		Rule	No Rule	Rule	No Rule
		<i>ExoMajRule</i>	<i>ExoMinNoRule</i>	<i>EndoRule</i>	<i>EndoNoRule</i>
		<i>vs.</i>	<i>vs.</i>	<i>vs.</i>	<i>vs.</i>
		<i>ExoNiRule</i>	<i>ExoNiNoRule</i>	<i>ExoMajRule</i>	<i>ExoMinNoRule</i>
Yes-voters	(1) Without control variable	0.07 (3.245)	5.22** (2.313)	0.12 (2.496)	1.76 (3.304)
	(2) With control variables	2.05 (2.943)	4.73 (4.481)	1.84 (2.391)	5.35 (5.53)
No-voters	(3) Without control variable	6.04* (3.139)	4.5 (3.104)	10.64*** (2.496)	-0.17 (1.204)
	(4) With control variables	4.67 (8.088)	5.30 (3.699)	8.75* (4.871)	-1.17 (2.40)

Note: Estimated effects with standard errors in parentheses. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Estimations are based on differences between individual contributions in round 11 and 10. Estimates and standard errors in row 1 (2, 3, 4) are based the comparison of coefficients of regressions presented in column 1 (2, 3, 4) of Table 6.4, respectively, and shown separately for yes- and no-voters with and without consideration of control variables.

LD if the rule is exogenously given and 12.55 LD if it is exogenously imposed and, in addition, the information about the outcome of the election is revealed ( $p$ -value: 0.983, Table 6.5 - row 1). In case the rule is not imposed (8.88 vs. 3.67), in contrast, I find evidence for an information effect ( $p$ -value: 0.027, Table 6.5 - row 1). However, this effect is not robust to controlling for further individual characteristics (21.89 vs. 17.16,  $p$ -value: 0.296, Table 6.5 - row 2). Furthermore, I do not find an effect of democratic participation among yes-voters. Neither in case the rule is implemented (12.67 vs. 12.55,  $p$ -value: 0.962, Table 6.5 - row 1) nor in case it is not implemented (8.88 vs. 5.43,  $p$ -value: 0.560, Table 6.5 - row 1).<sup>94</sup> This supports Result 4 that democratic participation does not affect contributions after controlling for self-selection and taking into account the information effect.

For no-voters, revealing the outcome of the election does affect participants' contributions when the rule is externally imposed (5.54 vs. -0.5,  $p$ -value: 0.061, Table 6.5 - row

<sup>94</sup>This is robust to controlling for further individual attributes (see Table 6.4 - column 2 and Table 6.5 - row 2).

3). However, this information effect is not robust and not statistically significant when I control for further individual characteristics (-6.35 vs. -11.03,  $p$ -value: 0.567, Table 6.5 - row 4). When the rule is not imposed, the information about the outcome of the referendum does also not have a statistically significant effect (6.17 vs. 1.67,  $p$ -value: 0.155, Table 6.4 - row 3). In case the rule is not imposed, there is no effect of participation (1.67 vs. 1.50,  $p$ -value: 0.891, Table 6.5 - row 3). The effect of participation is, in contrast, positive and statistically significant if the rule is implemented. More precisely, the difference in contribution levels between round 11 (i.e., the first round of Part II) and 10 (i.e., the last round of Part I) is -0.5 LD if the rule is externally imposed and participants receive the information about the outcome of the referendum. In case the rule is democratically chosen, this difference amounts to 10.14 LD (Table 6.4 - column 3). This shows a positive democracy premium among no-voters if the rule is imposed ( $p$ -value: 0.000, Table 6.5 - row 3). This effect is robust to regressions controlling for additional individual characteristics (-11.03 vs. -2.27,  $p$ -value: 0.082, Table 6.5 - row 4). However, this effect is not strong enough to drive aggregate results.

## **6.4 Summary and concluding remarks**

This experiment contributes to the literature on endogenous institutions in social dilemma situations by investigating if, how, and why democratic participation increases participants' willingness to comply with a non-deterrent rule which aims at fostering cooperation. My experimental design enables me to identify and separate potential drivers of the effect of democratic participation. I can determine to what extent the effect is driven by self-selection into the rule, information transmitted by voting, and democracy per se. Tyran and Feld (2006) report that a non-deterrent contribution rule is more effective if it is endogenously chosen by voting than externally imposed. However, they do not explicitly take into account effects of self-selection and information transmitted via voting. Focusing on a deterrent intervention which transforms their prisoner's dilemma into a coordination problem, Dal Bó et al. (2010) suggest a randomization strategy to derive an unbiased estimate of the effect of democratic participation.

I complement the existing literature by combining the key elements of Tyran and Feld (2006) and Dal Bó et al. (2010) experiments to test whether the effect of a non-deterrent contribution rule depends on whether it has been endogenously chosen via a democratic decision-making process or externally imposed and, if so, to what extent this is driven by self-selection into treatments, the information transmitted via voting, and democratic participation per se. Investigating a non-deterrent contribution rule enables me to investigate the willingness of participants to follow a rule which is for the

common good, but at odds with their individual free-riding incentives. This is a central characteristic of many interactions in real life social dilemmas which are subject to non-deterrent policies which do not affect underlying incentive schemes. In an environmental policy context, for instance, either no supranational authorities exist in order to enforce international environmental policies (e.g., Barrett 2010), or, in case authorities exist at the local level, they lack capacities and resources to actually enforce policies (e.g., Ostrom 1990; Kroll et al. 2007). Furthermore, deterrent rules set strong incentives and, thus, there is no conflict between cooperation and compliance what increases participants' willingness to follow the rule.

In line with the existing literature, I find that contributions to the public good are significantly higher if the rule is democratically chosen than if it is democratically rejected. In case treatments are exogenously given, in contrast, the contribution rule does not affect participants' contribution behavior. A naive comparison would suggest that the contribution rule is more effective in fostering contributions to the public good in case treatments are endogenously chosen than in case treatments are externally given. However, this comparison neglects potential confounding factors and does not necessarily prove that democratic participation increases participants' willingness to comply with a non-deterrent contribution rule. More precisely, my decomposition reveals that the apparently different effects are not directly driven by democratic participation per se. Democratic participation does not affect participants' contribution behavior if I take into account self-selection into treatments and the information transmitted by revealing the outcome of the referendum.

Of course, it is very difficult to make direct generalizations from my experiment, not at least because of the fact that I study students' behavior in an environment where they know they are being observed which might lead to higher willingness to follow the rule. Furthermore, due to my randomization strategy and the high amount of participants supporting the rule, participants are distributed unequally across treatments. It becomes thus more difficult to provide evidence for a statistically significant effect of democratic participation in my experiment. However, my findings not only indicate that democratic participation does not directly and significantly affect participants' compliance with a non-deterrent contribution rule, it is also shown that democratic participation explains only about 5% of the overall treatment effect. Therefore, the effect of participation appears to be neither of statistical nor economic significance.

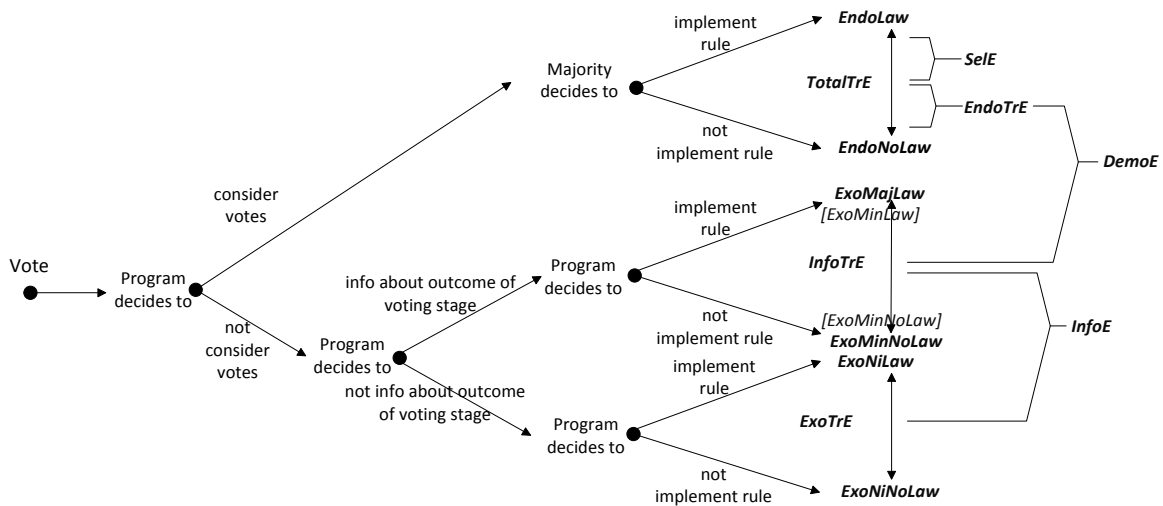
This is not necessarily a contradiction to the postulate that democratic participation actually affects behavior. My findings rather show that the effect of choosing a non-deterrent intervention which aims at fostering cooperation in a social dilemma situation is a conglomerate of different sub-effects of participation. Differences with the

existing literature suggest that the effect of participation depends on the type of the intervention. While Dal Bó et al. (2010) find a positive democracy premium in case of a deterrent contribution rule, my experiment does not provide evidence that people are more willing to follow a weak and non-deterrent rule if it is democratically chosen than externally given. This suggests that democratic participation can motivate people to comply with rules which are in their own interest, but not necessarily with rules which are at odds with their individual free-riding incentives. Finally, abstracting from important aspects of democratic decision-making like, for instance, direct communication, deliberation, and different decisions rules, I follow the experimental literature and reduce democratic participation to voting. It is not the purpose of this paper to capture democratic decision-making in all this facets, but this could be a an interesting and important route for further research.

## 6.5 Appendix

### 6.5.1 Figures

Figure 6.5: Illustration of the decomposition analysis



Note: The total treatment effect ( $TotalTrE$ ) captures the difference in contributions in  $EndoRule$  and  $EndoNoRule$ . The information treatment effect ( $InfoTrE$ ) is derived by comparing contributions in  $ExoMajLaw$  and  $ExoMinNoLaw$  and the exogenous treatment effect ( $ExoTrE$ ) by comparing contributions in  $ExoNiLaw$  and  $ExoNiNoLaw$ . The information effect ( $InfoE$ ) captures the difference between  $InfoTrE$  and  $ExoTrE$ . The  $TotalTrE$  can be decomposed into a selection effect ( $SelE$ ) and the endogenous treatment effect ( $EndoTrE$ ). The democracy effect ( $DemoE$ ) captures the difference between  $EndoTrE$  and  $InfoTrE$ .



6.5.2 Tables

Table 6.6: Summary statistics - individual level data (all)

Considering votes		Not considering votes							
		Information available				Information not available			
Vote	EndoRule	EndoNoRule	ExoMajRule	ExoMinRule	ExoMajNoRule	ExoMinNoRule	ExoNiRule	ExoNiNoRule	
Panel A. Votes									
No	14	14	4	9	8	6	13	6	
Yes	70	7	29	3	37	3	29	18	
Total	84	21	33	12	45	9	42	24	
Panel B. Average contributions in Part I									
No	5.52	6.80	7.65	7.41	5.29	7.75	5.9	5.38	
Yes	7.52	7.87	8.85	10.27	6.25	5.63	7.11	5.33	
Total	7.19	7.16	8.71	8.13	6.08	7.04	6.74	5.34	
Panel C. Contributions at the end of Part I (round 10)									
No	3.43	5.93	2.50	0.56	1.25	5.00	4.23	1.67	
Yes	3.81	3.86	4.34	5.33	3.35	3.33	4.76	1.11	
Total	3.75	5.24	4.12	1.75	2.98	4.44	4.60	1.25	
Panel D. Treatment effects (Differences in individual contributions between round 11 and 10)									
No	10.14	1.5	-0.5	12.22	6.25	1.67	5.54	6.17	
Yes	12.67	5.43	12.55	4.67	6.70	3.67	12.62	8.88	
Total	12.25	2.81	10.97	10.33	6.62	2.33	10.43	8.21	
Panel E. Contributions at the beginning of Part II (round 11)									
No	13.57	7.43	2.00	12.78	7.5	6.67	9.77	7.83	
Yes	16.49	9.29	16.90	10.00	10.05	7.00	17.38	10.00	
Total	16.00	8.05	15.09	12.08	9.60	6.78	15.02	9.46	
Panel F. Average contributions in Part II									
No	10.90	5.24	4.45	8.92	5.84	6.25	8.26	4.17	
Yes	12.60	5.90	13.00	12.23	6.53	4.87	13.00	4.87	
Total	12.31	5.46	11.97	9.75	6.41	6.06	12.15	5.28	

Note: Panel A summarizes the number of observations by vote and result of the voting state across treatments. Average contributions in Part I of the experiment are summarized in Panel B. Individual contributions at the end of Part I (i.e., the last round of Part I) and the first round of Part II (i.e., the first round of Part II) are summarized in Panel C and E, respectively. Differences in individual contribution levels between the first round of Part II and the last round of Part I are summarized in Panel D. Average contributions in Part II of the experiment are summarized in Panel E.

Table 6.7: Estimates for aggregated effects

	Dependent variable:		
	(1)	(2)	(3)
	Differences in contributions between round 11 and 10	Contributions in round 11	Average contributions in Part II
<i>EndoRule-Y</i>	12.67*** (1.272)	16.49*** (0.839)	12.60*** (1.12)
<i>EndoNoRule-Y</i>	5.43* (2.947)	9.29*** (3.033)	5.9*** (1.573)
<i>ExoMajRule-Y</i>	12.55*** (2.177)	16.90*** (1.180)	13*** (1.516)
<i>ExoMinNoRule-Y</i>	3.67** (1.567)	7.00* (3.991)	4.87 (3.736)
<i>ExoNiRule-Y</i>	12.62*** (2.451)	17.38*** (1.256)	13.90*** (1.784)
<i>ExoNiNoRule-Y</i>	8.89*** (1.73)	10.00*** (2.072)	5.64*** (2.102)
<i>EndoRule-N</i>	10.14*** (2.3936)	13.57*** (2.474)	10.9*** (1.8)
<i>EndoNoRule-N</i>	1.50 (0.940)	7.43*** (1.914)	5.24*** (1.59)
<i>ExoMajRule-N</i>	-0.50 (0.448)	2.00 (1.791)	4.45** (1.95)
<i>ExoMinNoRule-N</i>	1.67** (0.7037)	6.67*** (1.862)	6.65** (2.73)
<i>ExoNiRule-N</i>	5.54* (3.029)	9.77*** (2.837)	8.26*** (2.517)
<i>ExoNiNoRule-N</i>	6.17** (2.945)	7.83*** (2.548)	4.17* (2.349)
Observations	213	213	213
Adj. R <sup>2</sup>	0.612	0.795	0.776

Note: OLS regressions. \* $p < 0.1$ , \*\* $p < 0.05$  and \*\*\* $p < 0.01$ . Differences in individual contributions between round 11 and 10 are the dependent variable in column (1). In column (2), individual contributions in round 11. In column (3), average contributions in all ten rounds of Part II are the dependent variable. Standard errors, in parentheses, are clustered at the group level. In order to ease the ex-post estimates of weighted linear combinations of coefficients, all regressions are estimated without a constant and indicator variables for the different treatments that are separated for yes- and no-voters. Indicator variables for yes-voters (no-voters) receive the corresponding suffix -Y (-N).

### 6.5.3 Instructions

[Translated from German]<sup>†</sup>

#### Welcome!

Thank you for taking part in this experiment. Please do not talk to other participants and turn off all electronic devices such as phones for the whole course of this session. Please read the instructions carefully and raise your hand if you have any questions. This experiment regards individual decision behaviour. At the end of the experiment, you will receive an individual payment anonymously and in cash. Your payment will be based on the decisions you and your fellow participants will have taken as well as a random component. During the experiment, your payment will be calculated in so-called LaborDollar (LD). After the experiment, the total sum of LD will be converted into euros. The exchange rate is:

$$2 \text{ LD} = 1 \text{ euro.}$$

During the experiment, you will take your decisions **anonymously**. Only the experimenter will know about your identity. Of course, all provided information will be treated in strict confidence.

#### Rules of the experiment

The experiment consists of **two parts (Part I and Part II)**. For the whole course of the experiment, all participants are divided into groups of three. The group constellations do not change and every participant inside their respective group will face the same decision scenarios.

#### Part I

In Part I, we will ask you and your fellow participants to take decisions in ten separate rounds. At the beginning of each round, you and your fellow group members will be endowed with 20 LD, respectively. You (as well as your fellow group members) will then have to decide on the amount of LD that you wish to contribute to a joint project. Your contribution,  $q$ , can be between 0 and 20 LD.

The individual payment (in LD) for all three participants is calculated as follows:

$$\text{Payment} = (20 - \text{Contribution of the participant}) + 0.5 \cdot (\text{Total sum of contributions})$$

As an example, if the other two group members contribute together 40 LD while your contribution is 10 LD, your individual payment will be calculated as follows:

$$\text{Payment} = (20 - 10) + 0.5 \cdot (40 + 10) = 35$$

If on the other hand, both group members contribute 40 LD in total and you refrain from paying by entering 0 LD, your individual payment will be calculated as follows:

---

<sup>†</sup>Explanatory notes are given in square brackets.

$$\text{Payment} = (20 - 0) + 0.5 \cdot (40 + 0) = 40$$

Part I consists of ten separate rounds. In each round, you will face the same decision task and interact with the same two group members. After each decision, you will be informed on the average values as well as the contributions and payments regarding the other two group members. At the beginning, there will be two test rounds. They are not relevant for disbursement.

## **Part II**

As in Part I, we will ask you and your fellow participants to take decisions in ten separate rounds. You will be part of the same group, which remains unchanged in its constellation. Again, at the beginning of each round, you and your fellow group members will be endowed with 20 LD, respectively. The decision tasks are the same as in Part I. You (as well as your fellow group members) will have to decide on the amount of LD that you wish to contribute to a joint project. Your contribution,  $q$ , can be between 0 and 20 LD. Contrary to Part I, it is now possible to introduce a contribution rule. It stipulates that all group members shall contribute the total sum of LDs endowed at the beginning ( $q = 20$ ) to the joint project. Participants who do not abide by this rule shall pay a fee of 4 LD.

If a participant adheres to the rule ( $q = 20$ ), their individual payment will be calculated as follows:

$$\text{Payment} = (20 - 20) + 0.5 \cdot (20 + \text{Total sum of contributions made by all the other group members.})$$

If a participant refrains from adhering to the rule ( $q < 20$ ), their individual payment will be calculated as follows:

$$\text{Payment} = (20 - \text{Contribution of the participant}) + 0.5 \cdot (20 + \text{Total sum of contributions made by all the other group members}) - 4$$

As an example, if the other two group members contribute a total sum to the tune of 40 LD while your contribution is 10 LD, your individual payment will be calculated as follows:

$$\text{Payment} = (20 - 10) + 0.5 \cdot (40 + 10) - 4 = 31$$

If on the other hand, both group members contribute 40 LD in total and you refrain from paying by entering 0 LD, your individual payment will be calculated as follows:

$$\text{Payment} = (20 - 0) + 0.5 \cdot (40 + 0) - 4 = 36$$

Whether the rule is introduced or not depends on the following: Firstly, the group decides on introduction of the rule by majority vote. Secondly, it is decided at random, whether the group's decision will be taken into account. After the voting, you will be informed on whether the group's decision will be taken into consideration.

- If the group's decision is taken into account, you will be informed on the voting results. The decision will be taken based on the group's majority. For example, if two out of the three group members vote in favour of the rule, it will be introduced. If only one group member is in favour, the rule will not be introduced.
- If the group's decision is not taken into account, the decision on introducing the contribution rule will be taken at random. Furthermore, it is decided at random, whether you will be informed about the voting results.

In total, the experiment is made up of 20 separate rounds (10 rounds for Part I and 10 rounds for Part II). At the end of the experiment, you will receive the payment of one of the 20 rounds in euros. The round which will serve as the basis of your payment will be selected at random. For this reason, we recommend you to decide for each round as if it was the basis of your payment.

**Control Questions (please fill in)**

1. Suppose that in Part I, your contribution to the joint project amounted to 15 LD. The other two group members paid 15 LD in total. What is your individual payment?  
My payment is \_\_\_\_\_
2. Suppose that in Part I, your contribution to the joint project amounted to 5 LD. The other two group members paid 15 LD in total. What is your individual payment? My payment is \_\_\_\_\_
3. Suppose that in Part I, the two other group members contributed their total initial sum to the joint project. Which contribution would produce the maximum individual payment (please tick)?  0 LD  5 LD  10 LD  15 LD  20 LD
4. Suppose that in Part I, the two other group members contributed their total initial sum to the joint project. Which contribution would produce the maximum payment for your group (please tick)?  0 LD  5 LD  10 LD  15 LD  20 LD
5. Suppose that in Part II, the contribution rule was implemented and your contribution to the joint project amounted to 20 LD. The other two group members paid 20 LD in total. What is your individual payment? My payment is: \_\_\_\_\_
6. Suppose that in Part II, the contribution rule was implemented and your contribution to the joint project amounted to 10 LD. The other two group members paid 20 LD in total. What is your individual payment? My payment is: \_\_\_\_\_
7. Suppose that in Part II, the contribution rule was implemented and the two other group members contributed their total initial sum to the joint project, respectively. Which contribution would produce the maximum individual payment (please tick)?  0 LD  5 LD  10 LD  15 LD  20 LD

8. Suppose that in Part II, the contribution rule was implemented and the two other group members contributed their total initial sum to the joint project, respectively. Which contribution would produce the maximum payment for your group (please tick)?  0 LD  5 LD  10 LD  15 LD  20 LD

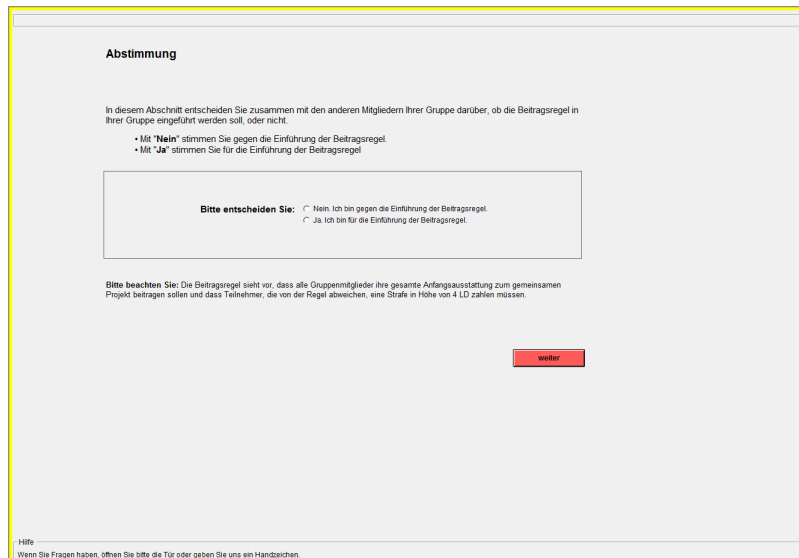
Please raise your hand after you finished answering all questions. We will then check your answers. The experiment will start once all participants have successfully completed this test.

**Good luck!**

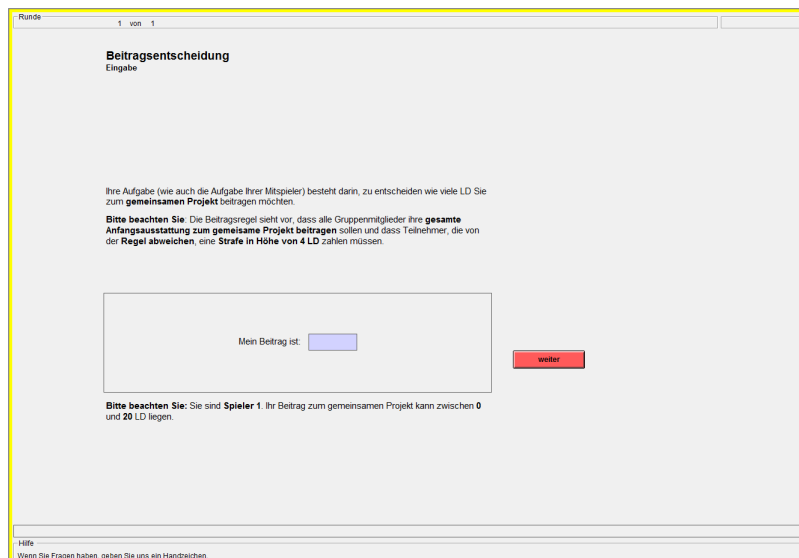
## 6.5.4 Screenshots

[In German]<sup>†</sup>

Screenshot of the voting stage



Screenshot of the contribution stage [In Part II of the experiment with contribution rule implemented]



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<sup>†</sup>Explanatory notes are given in square brackets.

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## Eidesstattliche Erklärung

Ich erkläre hiermit an Eides Statt, dass ich meine Doktorarbeit

*Investigations on the voluntary provision of local and global public goods*

selbstständig und ohne fremde Hilfe angefertigt habe und dass ich alle von anderen Autoren wörtlich übernommene Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehenden Ausführungen meiner Arbeit, besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert habe.

Mannheim, August 2018

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Carlo Gallier