# EXPERIMENTS ON CLIMATE CHANGE MITIGATION

Inauguraldissertation
zur Erlangung des Doktorgrades
der Wirtschafts- und Sozialwissenschaftlichen Fakultät
der Universität zu Köln
2021

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Tag der Promotion: 29. April 2021

*To Nicolas.*

# ACKNOWLEDGEMENTS

I thank my supervisor Achim Wambach for giving me the opportunity to study in an excellent environment. I am truly grateful for his support, guidance, encouragement, and advice. I am also indebted to the other members of the thesis committee: Oliver Gürtler and Johannes Münster.

Thanks to the wonderful people around me at ZEW and our partner universities, I had a great time during my PhD. I am thankful for many fruitful discussions and the inspiring atmosphere. My special thanks go to my colleagues and co-authors: Carlo Gallier, Florian Gössl, Vitali Gretschko, Martin Kesternich, Daniel Osberghaus, Mike Price, Alexander Rasch, Clemens Recker, Christiane Reif, Kathrine von Graevenitz and Lisa Vorbeck. It was a particular pleasure to share the PhD experience with Marius Alt, Elke Groh and Madeline Werthschulte.

I thank my friends and my parents-in-law for their continuous encouragement.

I thank my parents for always believing in me, and my sisters, Anika and Laura, for our wonderful, unique bond.

Finally, I dedicate this thesis to Nicolas, the love of my life. I am deeply grateful for his unlimited support and his unconditional love.

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# CHAPTER 1.

# INTRODUCTION

Climate change is one of the biggest challenges of our time, and it raises economic questions with major consequences for our world. The very fact that makes it so challenging to implement effective climate protection measures is that they have characteristics of a *public good*. A public good is defined as a good individuals cannot be excluded from using (non-excludability), and its use does not reduce its availability to others (non-rivalry). These characteristics lead to an incentive to free-ride on the provision of the public good, e.g., the climate change mitigation efforts, by others. The consequence is a *social dilemma* in which individual incentives are at odds with collective group interests. Without intervention, this results in an inefficient provision of the good by private markets. For *global* public goods such as climate change mitigation, the inefficiencies are even higher as its impacts are indivisibly spread around the entire globe (Nordhaus, 2006). In this context, mitigating (human-induced) climate change can be considered the world's greatest market failure (Stern, 2007).

The efficient provision of public goods requires collective action. But while national governments have the power and legal authority to establish laws and institutions within their territories to internalize externalities, e.g., air pollutants, and provide public goods, e.g., clean air, this is not the case on the global level. There exists no workable market or legal mechanism by which disinterested majorities or majorities without unanimity can force reluctant free-rider countries into mechanisms that provide global public goods (Nordhaus, 2006).

The social dilemma of climate protection is subject to a large body of literature within (but not limited to) the field of environmental economics. The aim is to develop effective and efficient mechanisms and policy instruments to prevent inefficient resource allocation. As incorporating behavioral features into economic models can have substantial practical values for specific policy questions (Chetty, 2015), insights from psychology and behavioral economics also gained importance for climate change research. Croson and Treich (2014) explain this with the complexity of environmental issues due to the global scope and the long-term perspective as well as the observation that environmental problems are often associated with moral feelings.

To inform policymakers on how to design effective climate change mitigation policies, three aspects are critical:

1. Peoples' perception of environmental problems.
2. The motives to engage in climate change mitigation.
3. The formation and performance of institutions that foster climate change mitigation.

With this thesis, I contribute to the literature on climate change mitigation by providing insights from experimental studies in behavioral environmental economics concerning the three above mentioned questions. Each of the three chapters provides a stand-alone analysis that contributes to answering one of the three questions by applying an appropriate empirical or experimental method. Each essay formulates a specific research question and comprises both the contribution to the existing literature, the methodological approach, and the discussion of the results. In the following, I will first describe the objective of behavioral environmental economics and its connection to experimental economics. After that, I will provide an overview of the following chapters and how they contribute to the three above mentioned aspects by analyzing behavioral aspects using experimental methods.

The field of *behavioral environmental economics* extends the concept of the rational and self-interested homo economicus by attitudes and preferences for environmental goods and policy-making. In doing so, behavioral environmental economics applies the idea behind behavioral economics to a specific topic. Thaler (2016) states that characterizing optimal behavior is an essential building block of any kind of economic analysis. These theories must be augmented by additional descriptive theories derived from data rather than axioms in order to be able to predict actual behavior (Thaler 2016). First studies that incorporated insights from psychology into economic models were implemented by Simon (1955), Kahneman and Tversky (1979), and Thaler (1980). Since then, a large body of research has addressed concepts such as loss aversion, present bias, and reciprocity. These concepts are also of great relevance in the context of climate change. Furthermore, concepts of psychology and behavioral economics have also been directly integrated into research on environmental economics. For example, they help to access environmental valuation using stated preferences and understand the willingness to accept/willingness to pay discrepancy as well as implications of risk (mis-)perception for the regulation of environmental risks (Croson and Treich 2014).

In this thesis, the focus is on beliefs, environmental attitudes, and social norms that influence individual behavior and are also likely to increase the possibilities of obtaining international agreements compared to standard models (Brekke & Johansson-Stenman, 2008). Clark, Kotchen, and Moore (2003) find that altruistic motives and pro-environmental attitudes enhance pro-environmental behavior. Anderson, Bernauer, and Balietti (2017) show that fairness principles have a strong effect on participants' willingness to pay for climate change mitigation as they take into account players' capacities and responsibilities. It has further been shown that non-price behavioral interventions, e.g., social comparisons, can be a powerful climate policy instrument as they have the potential to significantly reduce the energy consumption of private households (Andor & Fels, 2018).

Incorporating behavioral science insights into the design of policy instruments can help increase their effectiveness. Standard theory may not adequately predict actual behavior in the environmental context (Shogren & Taylor, 2008; Venkatachalam, 2008) and thus the effect of policies on behavior (Chetty, 2015). Furthermore, these insights might offer alternative policy tools (e.g., changing default options or framing) (Chetty, 2015) that might even make use of individuals' deviations from the homo economicus (e.g., nudges) (Croson & Treich, 2014). For effective measures, both predictable general patterns of behavior, as well as contextual behaviors, are crucial (Kesternich, Reif, & Rübbelke, 2017). This is particularly relevant in the context of climate change mitigation because measures are implemented at different levels and target different audiences. Research on behavioral environmental economics can inform policymakers and agents for "top down" approaches as well as "bottom up" approaches (Croson & Treich, 2014). The first refers to measures by governments or quasi-governmental agencies. The latter describes situations in which specific decision frameworks are set, e.g., firms respond to consumers' preferences toward the environment with corporate responsibility or green products. While standard theory provides a valuable reference point, it is to be complemented by a practical perspective that takes considers actual behavior (Kesternich et al., 2017).

The description and analysis of actual behavior require empirical data, e.g., on electricity consumption or tax payments and data generated in controlled environments, this is by experimental methods. With experiments, one can observe single behavioral patterns or institutional designs by holding most factors that influence behavior constant. Simultaneously, the factor of interest is varied so that causal inference can be drawn (Croson & Gächter, 2009). The foundation for the development of experimental economics was laid by expected utility theory that creates a basis for the analysis of individual choice under uncertainty, and game theory, which allows analyzing strategic interactions between individuals (for a comprehensive history of economic experiments, see Weimann and Brosig-Koch 2019). At the beginning of the 1970s, stronger coordination between researchers in experimental economics emerged and enabled the development of a joint methodology to study reproducible patterns (Weimann & Brosig-Koch, 2019). For the application of experimental methods for research in behavioral environmental economics, Sturm and Weimann (2006) define three fields: experiments allow to test theoretical hypotheses for individual behavior in the context of a social dilemma. Also, experiments provide a testbed to evaluate and compare institutional designs and help elicit individual preferences for public goods.

Testing a specific theory or a model requires its closest possible representation, this is a high degree of internal validity (Weimann & Brosig-Koch, 2019). *Laboratory experiments* can achieve this. In these experiments, participants meet in an accordingly equipped room and play a specific game or solve a task on a computer. Laboratory experiments allow a high degree of control of the influencing factors by the experimenter. The conditions under which the players participate are as equal as possible for all and external influences are reduced to a minimum. Laboratory experiments have been used intensively to study individual and group behavior in public goods games. For example, Fehr and Gächter (2000) find that voluntary contributions to a public good decrease over time, but that the existence of a sanctioning opportunity increases average contributions. It even keeps contributions at a stable level over time. Falk, Fehr, and Fischbacher (2005) analyze different theories of fairness as possible motives for the willingness to sanction non-cooperative behavior. They find that cooperators impose sanctions on defectors by considering other players' individual payoffs rather than group payoffs.

The external validity of an experiment refers to its capacity to make a statement about reality (Weimann & Brosig-Koch, 2019). This is of particular interest in the context of policy advice. The external validity of an experimental study may be increased by changing the subject pool or other factors that can influence the decision of interest. These factors include the nature of the task or trading rules applied, the nature of the stakes, and the environment in which the subjects operate (Harrison & List, 2004). The experimental settings are still controlled, but controls are less artificial. According to Harrison and List (2004), these kinds of experiments can be categorized as follows: an *artefactual field experiment* uses the same setting as a laboratory experiment but with a nonstandard subject pool. A *framed field experiment* incorporates field context in either the commodity, task, or information. In a natural field experiment, the environment is one where the subjects naturally undertake an observable task. Nevertheless, it is possible to change institutional framework conditions or other variables of interest exogenously. One key factor that distinguishes this last experimental category from all previous ones is that participants are not aware of their participation in the experiment. (As is common in the literature, I will refer to natural field experiments as *field experiments* in the following.) Since a detailed discussion of the characteristics of the different experimental methods would go beyond the scope of this introduction, I will only mention two aspects that differ between the two most common methods, lab and field experiments.[1] First, the advantage of a field experiment – the possibility to observe a decision in a natural environment – also represents its limitation. In contrast to the laboratory, research designs in field experiments are limited by the framework conditions given, for example, by practice partners. Furthermore, the extent to which one can use results from a field experiment to predict the impact of a similar treatment or program is limited as they often do not clearly identify why a program worked. Compared to lab experiments, they are also difficult to replicate (List & Price, 2016). Second, in both types of experiments, the subject sample need not be representative. Subjects who participate in a lab experiment are (mainly) students who voluntarily registered for the participation. Participants of a field experiment belong to a group with a particular characteristic, such as purchasing a specific product or being employed by a company. The main advantage of these controlled experiments is that they automatically create a counterfactual by randomly assigning participants to either a treatment or a control group (Harrison & List, 2004). This means that the participants of both groups have the same characteristics in expectation, so that an observed effect is a treatment effect and not a selection effect. For the optimal research design, the respective characteristics of the two methods must be carefully assessed, depending on their advantages and disadvantages for the respective research question.

An insight from experimental studies (lab as well as field) that has important implications for behavioral environmental economics is the "willingness to pay/willingness to accept disparity" (List & Price, 2016). This describes the observation that subjects value a good differently when they own it compared to when they are looking to acquire it. This questions one of the fundamental assumptions of neoclassical models, that preferences for any good are independent of endowment (List & Price, 2016). The evidence that a loss of something (e.g., of high air quality) is valued higher than the gain from an improvement (e.g., of the air quality) has significant impacts for environmental policies such as quota allocations and reallocations or issuing and termination of licenses (Knetsch, 1990). For example, a pollution control program is likely to be valued higher if it is perceived as reducing harm than if it is perceived as an improvement (Knetsch, 1990). Specific settings in which field experiments provide important insights are energy and resource economics. Field experiments in this area address a central issue of environmental economics – the externality problem – and foster a deeper understanding of individual behavior and the factors underlying the private provision of public goods (and bads) (Price, 2014). Findings suggest that both behavioral aspects such as salience and social norms as well as neo-classical factors such as prices or search costs influence the residential consumption of water and energy

[1] For a comprehensive methodological discussion of laboratory experiments please refer to Weimann and Brosig-Koch (2019) and for the different types of field experiments please refer to Harrison and List (2004).

(Price, 2014). For example, social comparisons (Brent, Friesen, Gangadharan, & Leibbrandt, 2017) and targeted messages (List & Price, 2016) have been shown to be effective at reducing energy use. The possibility to disentangle causal mechanisms that drive consumer behavior, e.g., warm glow and altruism, makes field experiments an attractive method to inform policymakers beyond traditional economic incentives, such as prices and subsidies (Brent et al., 2017).

Despite a similar terminology, the concept of *natural experiments* does not belong to the controlled experiments in the aforementioned meaning. Instead, it refers to naturally occurring changes imposed, for example, by the specific design of a policy measure, the introduction of a new policy, or an external shock (Harrison & List, 2004). The effect of such a change is analyzed by comparing those affected to a naturally occurring comparison group to mimic the control group. This setting implies that the researcher has no influence on, this is no control over the specifics of the treatments or the place and time when it is imposed. The advantage of a natural experiment is that it reflects individuals' choices in a natural setting, facing natural consequences that are typically substantial.

As described above, the discussed methods provide insights on individual behavior and decision-making in the context of climate change mitigation and inform policymakers. The following three aspects are essential focal points for effective measures. This is (i) peoples' perception of environmental problems, (ii) their motives to engage in climate change mitigation, and (iii) formation and performance of institutions that foster climate change mitigation.

One factor determining *peoples' perception of environmental problems (i)* is their belief in the existence of global climate change (i.e., the acceptance of the scientific consensus that human-caused global warming is happening, hereinafter referred to as climate change belief). The belief in climate change is an important driver of pro-environmental attitudes and a pre-condition for the support of public and private mitigation and adaptation strategies (e.g., Dietz 2020; van Valkengoed and Steg 2019; Spence et al. 2011; Sibley and Kurz 2013). More concrete, pro-environmental attitudes (Lange & Ziegler, 2012) and the acknowledgment of a personal contribution to climate change (Diederich & Goeschl, 2014) have been shown to increase the willingness to contribute to climate mitigation. In this context, it is decisive to understand the determinants of belief formation and updating processes. Prior empirical research has studied the determinants of climate change beliefs, finding not only substantial geographic variation in climate change beliefs (Howe et al., 2015) but robust effects of political attitudes (Albright et al., 2019; Shao and Hao, 2019;

Ziegler, 2017 among others), education (Ballew et al., 2020), and gender (van der Linden, 2015).

In Chapter 2 of this thesis entitled "Floods and Climate Change Beliefs: The Role of Distance and Prior Beliefs"[2] my co-author Daniel Osberghaus and I exploit a major flood event as a natural experiment on climate change belief updating. Our analysis adds to the prior empirical literature by investigating geo-located panel data covering beliefs before and after a natural disaster elicited by a survey. We find a causal effect of flood experience on climate change beliefs for people living within a 1 km radius of the flood. Furthermore, we show that the influence of the flood experience depends on beliefs held prior to the flood, such that existing beliefs are confirmed. As public support of climate change mitigation and adaptation strategies is dependent on beliefs about climate issues, particularly the belief in the existence of global climate change. Using variation in the spatial distance to a major flood event, we analyze the causal effect of flood experience on the belief in climate change. The primary data source is a panel survey covering 22,251 observations from 11,194 geo-located households collected in Germany between 2012 and 2015. Flood experience is assessed based on satellite imagery of a major flood event in June 2013. We find that flood experience had a significant positive effect on the beliefs in the existence of climate change for those respondents living close to the flooded area. However, the effect decreases sharply with distance. Going more into detail on the different effects of prior beliefs, we show that the flood confirmed people in their belief in climate change if they had believed in climate change already before the flood occurred, while the spatial proximity had no measurable effect on pre-flood skeptics. In other words, personal flood experience has an effect on people who believed in the existence of climate change before the flood but does not cause skeptics to believe in the existence of climate change. These results imply that climate skeptics may not be influenced by the experience of extreme weather events at their doorsteps.

From an individual perspective, strategies to mitigate climate change involve environmental programs for voluntary compensation of individual greenhouse gas emissions. If firms combine their private goods with the carbon compensation, consumers can choose between different varieties (concerning the embodied carbon emissions) of the same good or service.

[2] I presented this project at the 6th World Congress of Environmental and Resource Economists (Gothenburg, Sweden 2018), the Seminar in Applied Microeconomics (University of Cologne 2018), the AURÖ workshop for young researchers of the Verein für Socialpolitik (University of Osnabrück 2018), the 2nd BETA-ZEW Workshop (ZEW Mannheim 2018), the workshop “Natural Experiments and Controlled Field Studies” (University of Munich, Ohlstadt 2018) and the 3rd Workshop on Experimental Economics for the Environment (University of Hamburg 2018). Daniel Osberghaus and I both designed the research, performed the statistical analyses and wrote the manuscript. Daniel Osberghaus collected the data.

To design such programs effectively, knowledge about agents' *preferences and motives regarding climate protection measures (ii)* is of utmost importance. This refers to both consumers and firms. While there is growing evidence exploring the response of individual end-users to green goods (e.g., Engelmann, Munro, & Valente, 2017; Feicht, Grimm, & Seebauer, 2016; Lange, Schwirplies, & Ziegler, 2017; Munro & Valente, 2016), there is a lack of empirical evidence on motivations underlying Corporate Environmental Responsibility supply (Croson & Treich, 2014).

In Chapter 3, with the title "The (un-)intended consequences of labels in voluntary carbon offsetting programs: evidence from a field experiment among firms"[3], I present findings of a study which is joint work with Martin Kesternich, Michael K. Price, and Kathrine von Graevenitz. A growing literature explores individual responses to carbon offsetting programs as a means for pro-environmental behavior. However, it is not clear to what extent these findings are informative about firm behavior. We contribute to this literature by studying how firms respond to green programs such as carbon neutral services. The evidence of this field experiment suggests that firms seem to avoid disclosure of their pro-environmental activities. Building upon theoretical insights, we provide field-experimental evidence on the role of pro-environmental signaling for voluntary carbon offsets among firms. We collaborate with a courier service company from Poland, which mainly offers business-to-business deliveries booked through a web-shop. Our experimental setting enables the sender of a delivery to offset the related carbon emissions by paying a price add-on while we vary the opportunity to signal this pro-environmental act to the receiver of the delivery on the label of the delivery. Based on more than 5,600 orders from 124 unique business clients, we find that senders are not more likely to enroll in the carbon offsetting program if they can signal the pro-environmental behavior to the receiver. In contrast, if senders have a choice, more than half of those who enroll in the offsetting program prefer to keep the signal private. Our results suggest that firms' motives for carbon offsetting are

[3] This study is registered in the AEA RCT Registry under the unique identifying number AEARCTR-0002646. I presented this project at the 4th HeiKaMaxY workshop for young researchers (KIT Karlsruhe 2018), the 3rd Workshop on Experimental Economics for the Environment (University of Hamburg 2018), the 24th Annual Conference of the European Association of Environmental and Resource Economists EAERE (University of Manchester, UK 2019), the annual meeting of the Verein für Socialpolitik (University of Leipzig 2019), the 7th Beijing Humboldt Forum (University of International Business and Economics Beijing, China 2019), the workshop "Natural Experiments and Controlled Field Studies" (University of Munich, Ohlstadt 2019), the Seminar at the Department of Economics, Finance, and Legal Studies of the University of Alabama (Tuscaloosa, US 2019), the 1st RWI Empirical Environmental Economics Workshop (RWI Essen 2019) and the 4th Workshop on Experimental Economics for the Environment (University of Munster 2019). Financial support by the German Federal Ministry of Education and Research under the funding line for "Polish-German Sustainability Research (STAIR)" (BMBF FKZ: 01LX1606A) and the German Federal State of Baden-Württemberg under the international research program "Strengthening Efficiency and Competitiveness in the European Knowledge Economies" (SEEK) is gratefully acknowledged. My co-authors and I designed the research, performed the statistical analyses and wrote the manuscript.

multidimensional and that the signal to business partners may not be desirable. A program designer aiming to increase the share of carbon neutral shipments should allow for flexibility on whether to disclose this information.

At the United Nations Framework Convention on Climate Change (UNFCCC) Conference of the Parties (COP21) in Paris in 2015, an agreement was formulated that sets the goal of "holding the increase in the global temperature to well below 2°C" to reduce the risks and impacts of climate change (UNFCCC 2015). For this collective climate goal to be reached, it has to be translated into a common climate commitment. However, despite this ambitious collective goal, there is a lack of ambition regarding individual abatement strategies of the countries (Cramton, Ockenfels, & Tirole, 2017). This is because self-interested countries have an incentive to rely on the emissions reductions of others without undertaking own efforts. The idealized solution to overcome this free-rider problem is a Climate Club (Nordhaus, 2015). This refers to an agreement by a subset of countries to undertake emission reductions and to sanction nonparticipants in order to create an incentive for them to enter the club and undertake emission reduction themselves. In reality, the formation of such an institution that allows for the sanction of nonparticipants conflicts with the fact that countries are sovereign, have the fundamental right of political self-determination, and are equal under law.

In Chapter 4, with the title "Endogenous Sanctioning Institutions in Public Goods Games"[4], my co-author Carlo Gallier and I study two institutions that represent the two extremes of sanctioning institutions in terms of the ability to sanction nonparticipants. With this, we contribute to understanding the *formation and performance of institutions that foster climate change mitigation* (iii). These insights help to design policies or set framework conditions such that institutions effectively provide climate change mitigation. We study the effectiveness of an exclusive sanctioning institution on cooperative behavior. This is an institution that imposes a deterrent sanctioning rule on its members while having no effect on the others. For this purpose, we compare the implementation frequency and welfare of this institution with an inclusive institution, where the majority decision determines whether the contribution and sanction rule is imposed either on all members of a group or on none. We conduct a two-stage laboratory experiment consisting of a vote on the costly institution formation and a public goods game. We find that giving participants the option to form an

[4] I presented this project at the 5th workshop on Experimental Economics for the Environment (Heidelberg University 2020), the 5th Winter School on Applied Microeconomics (Heinrich-Heine-Universität Düsseldorf, Saas-Fee 2020) and the ZEW-University of Mannheim Experimental Seminar (ZEW Mannheim 2020). Financial support by the German Federal Ministry of Education and Research under the funding line for "Economics of Climate Change II" (BMBF FKZ: 01LA1813B) is gratefully acknowledged. Carlo Gallier and I both designed the research, performed the statistical analyses and wrote the manuscript.

inclusive institution is significantly more welfare enhancing than giving them the option of forming an exclusive institution. Furthermore, our results suggest that the performance of an exclusive institution is enhanced by a positive spillover effect of yes-voters on the contributions of no-voters. However, this effect wears off over time. Understanding the role exclusive institutions can play in providing public goods is particularly important in situations where inclusive institutions are not feasible as they might interfere with states' sovereignty or actors' individual freedom.

# Chapter 2.

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# Floods and climate change beliefs: the role of distance and prior beliefs

*Joint work with Daniel Osberghaus*

**Abstract**

Public support of climate change mitigation and adaptation strategies is dependent on beliefs about climate issues, in particular the belief in the existence of global climate change. By using variation in the spatial distance to a major flood event, we analyze the causal effect of flood experience on the belief in the existence of climate change. The primary data source is a panel survey covering 22,251 observations from 11,194 geo-located households collected in Germany between 2012 and 2015. Flood experience is assessed based on satellite imagery of a major flood event in June 2013. We find that flood experience had a significant positive effect on the beliefs in the existence of climate change for those respondents living close to the flooded area. However, the effect decreases sharply with distance. Going more into detail on the different effects of prior beliefs, we show that the flood confirmed people in their belief in climate change if they had believed in climate change already before the flood occurred, while the spatial proximity had no measurable effect on pre-flood skeptics. These results imply that climate skeptics may not be influenced by the experience of extreme weather events at their doorsteps.

## 2.1 Introduction

Despite a strong consensus within the scientific community on contemporaneous global climate change, public opinions on its existence and anthropogenic causes diverge (Cook et al., 2013). Nonetheless, the belief in the existence of climate change (i.e. the acceptance of the scientific consensus that human-caused global warming is happening, hereinafter referred to as climate change belief) is an important driver of pro-environmental attitudes and a pre-condition for the support of public and private mitigation and adaptation strategies (Arbuckle, Morton, & Hobbs, 2013; Dietz, 2020; Hine et al., 2013; Sibley & Kurz, 2013;

Spence et al., 2011; Vainio & Paloniemi, 2013; van Valkengoed & Steg, 2019). Prior empirical research has studied the determinants of climate change beliefs, finding not only substantial geographic variation in climate change beliefs (Howe, Mildenberger, Marlon, & Leiserowitz, 2015; Ziegler, 2017) but robust effects of political attitudes (Albright & Crow, 2019; Shao & Hao, 2020; Ziegler, 2017), education (Ballew, Pearson, Goldberg, Rosenthal, & Leiserowitz, 2020), and gender (van der Linden, 2015). In a meta-analysis of determinants of climate change beliefs, Hornsey et al. (2016) find that demographics are dominated in predictive power by values, ideologies, and political orientation. Furthermore, their study suggests that "evidence" on climate change is searched and processed in a way that can be referred to as motivated reasoning (Druckman & McGrath, 2019). Hornsey et al. (2016) conclude that skeptics cannot be "converted" through facts alone; communication strategies have to go with their ideologies. In this context it is crucial to understand how prior beliefs influence the belief updating process after new information emerges, such as the personal experience of an extreme weather event. Although establishing a causal link between a single extreme weather event and climate change is challenging, it is likely that anthropogenic climate change increases the frequency and severity of extreme weather events, such as heat waves, intense precipitation, and floods (Hirabayashi et al., 2013; Intergovernmental Panel on Climate Change (IPCC), 2014; IPCC, 2012; Roudier et al., 2016). Therefore, most scholars assume that experience of such events strengthens personal beliefs in climate change (Akerlof, Maibach, Fitzgerald, Cedeno, & Neuman, 2013; Deryugina, 2013; Ripberger et al., 2017). Van der Linden (2015) finds that experiential factors explain significantly more variance in climate change risk perception than cognitive or socio-demographic characteristics. However, the effect seems to differ depending on the kind of event, its proximity (Howe, Boudet, Leiserowitz, & Maibach, 2014) and its characteristics (Taylor, De Bruin, & Dessai, 2014). Furthermore, belief updating regarding climate change issues has been shown to vary with peoples' experience and their characteristics, for instance, with political orientation (Bohr, 2017; Boudet, Giordono, Zanocco, Satein, & Whitley, 2020; Ogunbode, Liu, & Tausch, 2017), general worldviews (Capstick & Pidgeon, 2014) and the individual level of engagement in the issue of global warming (Myers, Maibach, Roser-Renouf, Akerlof, & Leiserowitz, 2013). More generally, Leiserowitz finds that debate on global warming and climate policy is characterized by the predisposition of groups of individuals to select, ignore, and interpret risk information in different ways (Leiserowitz, 2006).

If prior beliefs shape the belief updating process in the aftermath of an event, this can be interpreted as a form of motivated reasoning (Druckman & McGrath, 2019; Zanocco et al., 2018) or more concretely as confirmation bias (McFadden & Lusk, 2015; Whitmarsh, 2011).

Confirmation bias and its role in the processing of information on climate change have been discussed previously (McFadden & Lusk, 2015; Sunstein, 2006; Whitmarsh, 2011). McFadden and Lusk (2015) find that the assimilation of information on global warming is influenced by prior beliefs. Still, its role in climate change belief updating after extreme weather experience has rarely been analyzed empirically.

Based on a unique dataset, we contribute to the literature on the effects of extreme weather experience on climate change beliefs in several dimensions. First, we provide a longitudinal analysis of the effects of a flood event on climate change beliefs while controlling for unobserved heterogeneity at the household level. Such longitudinal data at the individual level are indispensable but rarely available for the analysis of belief updating processes (Shao & Hao, 2020). The flood event was induced by heavy precipitation and occurred in Eastern and Southern Germany in 2013. While climate projections for Continental Europe vary regionally and seasonally, a trend towards more heavy precipitation in Germany is predicted (IPCC, 2014). Complementary to available studies on flood effects which are based on cross-sectional or post-event data (Albright & Crow, 2019; Spence et al., 2011; Whitmarsh, 2008), we estimate a causal effect of flood experience on climate change beliefs. Second, we differentiate the effect with regard to spatial distance to the flood event by using detailed satellite imagery, thereby adding to the available studies on spatial proximity (Albright & Crow, 2019; Howe et al., 2014; Osberghaus, Schwirplies, & Ziegler, 2013; Thieken et al., 2016). Third and finally, the data set enables us to observe beliefs held by individuals before and after an extreme weather event and to examine heterogeneous impacts of this event with regard to prior beliefs. We can, therefore, assess the effects on climate change skeptics and believers separately and ask, does a major flood event convince skeptics of the existence of climate change, and does it prevent believers from switching to climate skepticism? To the best of our knowledge, this is the first empirical investigation of the heterogeneous effects of a single extreme weather event on climate change beliefs, dependent on prior beliefs. We thus contribute new empirical evidence to the question of whether belief updating in the aftermath of an extreme weather event is subject to confirmation bias.

Our contribution to the literature on extreme weather events on climate change beliefs is three-fold: (i) we use panel survey data and objective flood experience data to estimate the causal effect of flood experience on individual beliefs in the existence of climate change; (ii) we assess the role of spatial distance to the flood event; (iii) we analyze how individual belief updating differs depending on prior climate-related beliefs.

## 2.2 Data and analysis

In this section we first present the datasets used in this study (Section 2.2.1) and then the details of the statistical analysis (Section 2.2.2).

### 2.2.1 Data and study context

Beliefs about climate change are measured by a comprehensive panel survey of 11,194 household heads in Germany. There were four annual waves between 2012 and 2015, resulting in an unbalanced panel with 22,251 observations in total. A detailed description of the dataset and summary statistics of all used variables are presented in the Appendix to Chapter 2, Table S2.4 -Table S2.6. The households are geo-coded at the street-level. The sample is broadly representative of the total stock of households in Germany. Respondents were asked whether they believe that global climate change is already taking place, will take place in the future, or will not occur at all (Table S2.7). We code those responses stating that climate change is already occurring as "climate change believers" (*CCB*=1, 81.9 percent) and the remainder as "climate change skeptics" (*CCB*=0, 18.1 percent).

Differences in socio-economic variables between pre-flood climate change believers and pre-flood climate change skeptics are reported in Table S2.8 (mean comparisons) and Table S2.9 (probit model). The data show that skeptics tend to be male, less educated, politically conservative, older household heads who are predominantly located in Eastern Germany. These observations are in line with previous literature on determinants of climate change beliefs in Germany (Ziegler, 2017) and other parts in the world such as the UK (van der Linden, 2015) and the US (Ballew et al., 2020; Hornsey et al., 2016).

In June 2013, shortly after the second survey wave, heavy rainfalls triggered a major flood in large parts of Germany. The rivers Danube, Elbe, and their tributaries overflowed their banks and caused severe inundations of agricultural, industrial, and residential areas. Six hundred thousand people were affected by the flood, 80,000 had to be evacuated, and the incident caused 14 casualties (Thieken et al., 2016). The insured damage amounted to €1.65 billion ($1.8 billion), while the total economic damage to households, businesses, and public infrastructure was estimated at €5.66 billion ($6.2 billion). In terms of the numbers of people affected and economic damage, the flood of June 2013 was an extreme event in Germany. The incident was also covered extensively in the national mass media, including the possible attribution of the event to global climate change. Internet search data illustrate the perceived relevance of the flood event in June 2013 and the perceived relation of the event to climate change: Google searches for the terms "flood" and "climate change" during 2013 peaked in early June, exactly when the flood occurred (Figure S2.2). According to survey data, which were collected in the first wave of the panel used in this paper in 2012, a majority of 91 percent of all household heads expected climate change to trigger either flood or heavy rain events in Germany. This percentage is higher than for any other weather event type, such as heat waves or storms (Osberghaus et al., 2013). Hence, we think it is safe to assume that respondents were aware of the potential connection between the flood event and global climate change. Due to the extensive media coverage, we also assume that the vast majority of the participants in our panel were aware of the flood event and may have been affected in terms of their climate change beliefs.

To combine the survey data with spatial data of the flood event in 2013, we use satellite imagery of areas flooded during the event in June 2013 (NASA, 2017; ZKI & DLR, 2017). By using objective remote-sensed satellite data, we do not have to rely on self-reported flood experience data, which can be biased due to the respondent's personal characteristics (Bohr, 2017; Boudet et al., 2020; Capstick & Pidgeon, 2014; Druckman & McGrath, 2019; Myers et al., 2013; Ogunbode et al., 2017; Zanocco et al., 2018). We calculate the spatial distance of the midpoint of the respondent's street to the closest boundary of the flooded area as the central measure of flood experience. We can thereby assess whether and how the treatment effect of flooding on climate change beliefs varies with spatial distance to the flood.

We hypothesize that while the flood may have affected climate change beliefs throughout the country, the spatial distance to the event is crucial for the belief updating process. It can influence the perception of the likelihood and severance of extreme weather events and thereby the belief in climate change. In a similar vein, it could be shown that people living in closer proximity to shorelines express greater belief in climate change (Milfont, Evans, Sibley, Ries, & Cunningham, 2014).

### 2.2.2 Statistical procedure

The statistical analysis of the causal effect of flood experience on climate change beliefs relies on difference-in-differences estimation (DiD) at the household level. In the simplest form, the DiD model compares time trends of individuals (before and after some treatment) from the "treated" versus the "control" group. If after the flood – but not before – the time trend of the treated group differs significantly from the control group, the post-flooding difference between those groups is interpreted as a causal effect. In our case, the treatment is the flood event, and treated respondents are identified via their spatial proximity to flooded areas, using different cutoffs in the range of 0.5 km to 20 km. The high-resolution satellite data allow for a detailed analysis of different radii around the flooded areas. We amend the DiD model by fixed effects which control for any time-invariant respondent-specific characteristics, such as personality traits, gender, location, etc. For assessing whether prior beliefs shaped the response to the flood event, we employ probit models separately for pre-flood climate change skeptics and believers to estimate the marginal effect of flood experience on the probability of switching to the respective other belief.

The DiD model compares time trends of treated versus untreated respondents, while controlling for fixed effects at the household level. The DiD estimation equation is the following:

$$y_{it} = \gamma T_t F_i + \mu_i + \vartheta_t + \varepsilon_{it} \qquad (1)$$

The dependent variable $y_{it}$ depicts the belief in the existence of climate change ($CCB$ in the baseline) of the respondent *i* in year *t*. The time indicator $T_t$ equals zero before the flood and one afterward; $F_i$ is a flood experience variable as defined in Table S2.4; $\varepsilon_{it}$ denotes the error term. The coefficient $\gamma$ represents the estimate of the treatment effect and is the main parameter of interest. It can be described as the difference between the time trends of $y_{it}$ in the treatment group versus the control group. We include household- and year-fixed effects which absorb the effect of all observed and unobserved time-invariant household-specific factors ($\mu_i$) and general time-specific effects, such as nation-wide variations of climate change beliefs ($\vartheta_t$). This baseline model is estimated as an OLS regression with standard errors clustered at the household level. (The complete results are available in Table S7.) As pre-treatment analyses, we assess the pre-flood time trends of $y_{it}$ by graphical and statistical means and find no significant pre-treatment trend differences between households with different levels of $F_i$ (paragraph "Pre-treatment analyses" in the Appendix to Chapter 2). Alternative specifications are used in a series of robustness checks and extensions presented in the Appendix to Chapter 2.

For analyzing how the effect of flooding is shaped by prior climate change beliefs, we employ two statistical approaches: First, we classify respondents into four groups: (1) believers before and after the flood, (2) those who switched from believers to skeptics after the flood, (3) skeptics before and after the flood, and (4) those who became believers after the flood. The descriptive statistics show that belief changes occur in both directions, but the percentage of pre-flood skeptics changing their minds is much higher (54.4%) than the respective share of pre-flood climate change believers (6.4%). In separate probit models for the two cross-sections of pre-flood climate change believers (groups 1 and 2) and pre-flood climate change skeptics (groups 3 and 4), we estimate the marginal effect of flood experience $F_i$ on the probability of switching to the respective other belief, while controlling for covariates. The estimation equation is the following:

$$Prob(CCBchange_i = 1) = \gamma F_i + \delta C_i + \varepsilon_i \qquad (2)$$

The binary variable $CCBchange_i$ takes the value of one if the respondent $i$ changed his or her opinion about the existence of global climate change after the flood, compared to before the flood event, and zero otherwise. A positive coefficient of $F_i$ in the sample of pre-flood climate change skeptics implies that flood experience is associated with higher chances to believe in climate change after the flood. Likewise, a negative coefficient of $F_i$ in the sample of pre-flood climate change believers signals that flood experience tends to prevent respondents from switching to climate change skeptics. In these probit estimations, all values in the covariates matrix $C_i$ were observed before the flood event, marginal effects are average marginal effects with covariates as observed, and standard errors $\varepsilon_i$ are clustered at the federal state level.

Second, we repeat the DiD model as presented before but separate the sample into pre-flood believers and pre-flood skeptics. Here, in both samples, positive treatment effects mean that flood experience strengthens the belief in the existence of climate change. Compared to the probit model approach, this approach controls for unobserved heterogeneity at the household level, and the estimated treatment effects are not potentially biased by spurious correlation.

## 2.3 Results

Turning to the results, we first present the estimation results of the causal effect of flood experience on individual beliefs in the existence of climate change with regard to the proximity in Section 2.3.1. We then analyze the role of prior climate-related beliefs on individual belief updating in Section 2.3.2.

### 2.3.1 Climate change beliefs are affected by close flood events

We first present a simple comparison of the time trends of individual beliefs in the existence of global climate change (*CCB*) for respondents with different levels of flood experience measured by distance to the flooded are (Table 2.1). The last column shows the differences in the changes in the proportion of households that believe in climate change, within and outside the respective radius. The share of households living within a 1 km radius shows the highest increase in *CCB* with a rise by 7 percentage points. These descriptive results suggest that the time trend is more positive for respondents living close to the flooded areas, at least if the radius around the flooded areas is sufficiently small.

Table 2.1: Descriptive difference-in-differences analysis

| Mean value of *CCB* (Number of observations) | Pre-flood (2012, 2013) | Post-flood (2014, 2015) | Difference (time trend, rounded) | Difference in differences (rounded) |
|---|---|---|---|---|
| Outside 0.5 km radius | 0.80 (10,291) | 0.84 (11,500) | +0.04 | +0.02 |
| Within 0.5 km radius | 0.74 (223) | 0.80 (237) | +0.06 | |
| Outside 1 km radius | 0.80 (10,050) | 0.84 (11,223) | +0.04 | +0.03 |
| Within 1 km radius | 0.76 (464) | 0.82 (514) | +0.07 | |
| Outside 5 km radius | 0.80 (9,079) | 0.84 (10,099) | +0.04 | +0.00 |
| Within 5 km radius | 0.77 (1,435) | 0.82 (1,638) | +0.04 | |
| Outside 10 km radius | 0.80 (8,233) | 0.84 (9,168) | +0.04 | -0.00 |
| Within 10 km radius | 0.78 (2.281) | 0.82 (2,569) | +0.04 | |

The DiD estimates of the causal flood effects on *CCB* confirm this result. Figure 2.1 presents the estimated treatment effects for different levels of spatial proximity to the flooded areas on *CCB*. The full results of some of these estimations are reported in

Table S2.10.

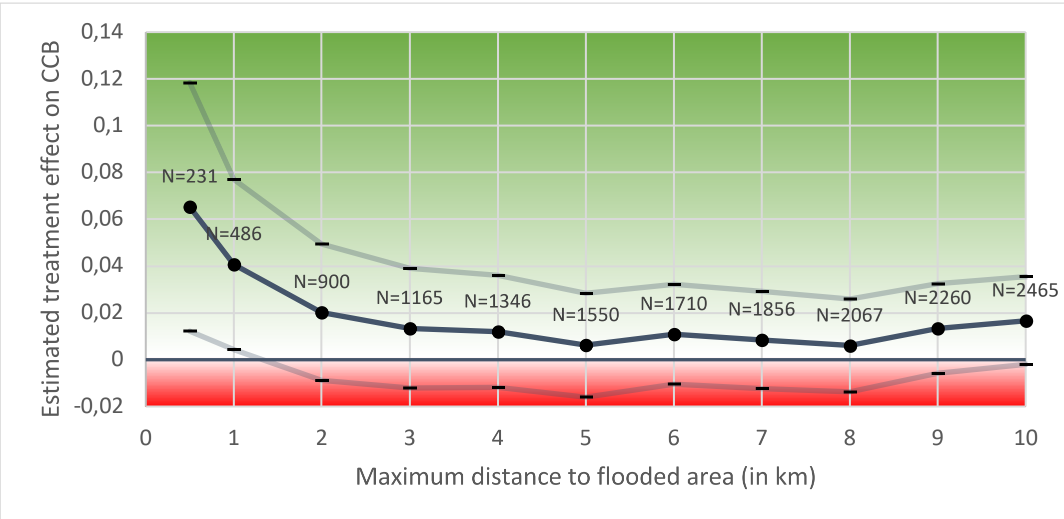



Figure 2.1: Estimated treatment effects of spatial proximity to the flooded areas on CCB

*Notes:* Point estimates and 90% confidence intervals of the treatment effects, based on DiD estimations with household-fixed effects, with varying distance thresholds for the identification of treated households. Indicated values are numbers of treated households (N). The total number of households is 11,194 in all specifications.

We find a statistically significant, positive treatment effect for respondents living very close to the flooded areas. The effect is strongest for those living within a 0.5 km distance and gets insignificant when the distance exceeds 1 km. This shows that climate change belief updating differs between households which experienced the flood in a closer proximity compared to those households which lived further away. As indicated by the year effects reported in

Table S2.10, belief in climate change existence was increasing everywhere in Germany in the time after the flood (especially in 2014). Still, what can be shown here, *CCB* values of respondents living very close to the affected areas showed a more positive time trend than the national average. More concretely, the time trend of *CCB* for households within the 1 km radius was by four percentage points higher than the respective time trend for other households. Several robustness checks demonstrate that this effect is robust regarding alternative model specifications, distance variables, and sample selections (see Appendix to Chapter 2, section on robustness checks). However, other measures of flood experience, which are less related to spatial proximity, show no significant effects on *CCB*, such as financial damages in the district, or percentage of the flooded area in the municipality (robustness checks RC5-RC9). These findings, together with the sharp decline of estimated treatment effects depicted in Figure 2.1, are consistent with the interpretation that flood effects on climate change beliefs depend crucially on the spatial proximity to the affected areas.

### 2.3.2 Flood events have different effects on climate believers and climate sceptics

So far, the analysis combines both the pre-flood climate change believers and skeptics. This does not allow us to distinguish differences in the reactions of both groups and to identify which group drives the estimated effect. Although an intuitive assumption would be that the result is driven by skeptics accepting climate change after experiencing the flood in their direct neighborhood, it might also be the case that pre-flood believers who were directly confronted with the flood were less likely to switch to skepticism than less affected believers. Therefore, we separate the sample along pre-flood climate beliefs and assess the correlation of flood proximity with the probability to change to the respective other belief. First, we present the shares of respondents who switch to the respective other belief for different levels of spatial proximity to the flood (Table 2.2; relations of the switching behavior with other variables are displayed in Table S2.11). These descriptive data provide first hints that the share of pre-flood skeptics who switch to believers may not be positively related to flood proximity. In fact, the share of skeptics who change their belief is lower in areas close to the flood than in other regions. Second, we estimate the marginal effect of spatial proximity on the probability to switch to the respective other belief. Table 2.3 presents the results of the respective probit models, separated for pre-flood skeptics and pre-flood believers.

Table 2.2: Descriptive statistics of climate change beliefs (CCB) pre- and post-flood, changing behavior

| Sample | 5,639 respondents (100.0%) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Pre-flood beliefs | Pre-flood skeptics (*CCB*=0): 1,205 (21.4%) | | | | Pre-flood believers (*CCB*=1): 4,434 (78.6%) | | | |
| Proximity to flooded areas | Less than 1 km away from flooded areas: 64 (5.3%) | | More than 1 km away from flooded areas: 1,141 (94.7%) | | Less than 1 km away from flooded areas: 203 (4.6%) | | More than 1 km away from flooded areas: 4,231 (95.4%) | |
| Post-flood beliefs | Changing to believers: 31 (48.4%) | Remaining skeptics: 33 (51.6%) | Changing to believers: 625 (54.8%) | Remaining skeptics: 516 (45.2%) | Remaining believers: 194 (95.6%) | Changing to skeptics: 9 (4.4%) | Remaining believers: 3,956 (93.5%) | Changing to skeptics: 265 (6.5%) |

*Notes:* Only one observation per household included. Only respondents included who participated before and after the flood event.

Table 2.3: Probit models of climate change belief switching behavior

| **Estimation sample**<br>Dependent Variable<br>Description of dependent variable | **Pre-flood skeptics**<br>CCBto1<br>Changing to believers | **Pre-flood believers**<br>CCBto0<br>Changing to skeptics |
|---|---|---|
| *Flood experience* | -0.043 (0.053) | -0.045** (0.022) |
| *Fem* | 0.054** (0.025) | -0.004 (0.013) |
| *Age* | -0.002 (0.002) | 0.001*** (0.000) |
| *Income* | 0.018 (0.032) | -0.013 (0.010) |
| *HHSize* | -0.007 (0.021) | 0.009** (0.004) |
| *Home* | 0.000 (0.043) | -0.008 (0.013) |
| *Educ* | -0.004 (0.030) | 0.003 (0.007) |
| *Left* | 0.105** (0.047) | -0.030*** (0.010) |
| Pseudo-R2 | 0.031 | 0.034 |
| Number of observations | 942 | 3,528 |

*Notes:* Reported values are average marginal effects on the probabilities to switch to the respective other belief. Flood experience relates to the place of residence within a 1 km radius around the flooded areas. For the interpretation note that the marginal effects in both columns are to be interpreted in opposing directions: while the flood experience reduces the likelihood that a subject switches from climate change skeptic to believer (not significant), it significantly reduces the likelihood of believers of switching to skeptics. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors (reported in parentheses) are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively, based on two-sided z-tests. The number of observations is lower than in Table 2.2 due to missing values in the covariates. The estimated probit coefficients are reported in Table S2.12.

In the subgroup of pre-flood believers, the effect is significantly negative. Pre-flood believers living within a 1 km radius to the flooded areas were, therefore, less likely to switch to climate skeptics than those believers who live farther away. The probability of becoming a climate skeptic was 4.5 percentage points lower for those in direct proximity to the flooded area compared to those farther away. The magnitude of this effect is considerable, given that the overall share of believers changing their belief is as low as 6.4 percent.

In the subgroup of pre-flood skeptics, the marginal effect of flood proximity is statistically not discernible from zero. It is important to note that this does not necessarily mean that skeptics did not react to the flood event. It shows that skeptics' belief updating process, in contrast to pre-flood believers, was independent of the spatial proximity to the flood event. Actually, a large proportion of the skeptics in 2013 (58.5 percent) switched to believers in 2014. For the whole panel, this change was the largest between two consecutive annual waves. Both between 2012 and 2013 and between 2014 and 2015, the respective shares were considerably lower (33.7 percent both times). Although our data do not allow a causal attribution of this development to the flood event, these figures might hint at an overall effect of the flood on climate change skeptics independent of their spatial proximity. The results of the probit analysis reveal additional information about the characteristics of the switchers. Politically left-oriented respondents tended to switch to or retain beliefs in the existence of climate change, female skeptics had a higher probability of switching to believers, and switching to skepticism was positively related to age.

While the estimation of switching behavior via probit models allows for intuitive interpretations, they do not account for unobserved heterogeneity, unlike DiD models with fixed effects. Therefore, we repeat the DiD estimation of *CCB* in the subgroups of pre-flood skeptics and pre-flood believers. The results (presented in Table S2.13) confirm the previous findings in Table 2.3, corroborating that the estimated flood effects in the probit models are reasonable. The estimated treatment effect of flood proximity on *CCB* is insignificant for pre-flood skeptics and significantly positive for pre-flood believers (~3.3 percentage points). We can, therefore, conclude that spatial proximity to the major flood event of 2013 in Germany did not affect climate change beliefs of pre-flood skeptics. It did, however, prevent believers from becoming climate skeptics.

These results are consistent with previous findings that motivated reasoning might drive the belief updating process after the personal experience of an extreme weather event (Myers et al., 2013; Zanocco et al., 2018). The difference in the assessment of the event by skeptics and believers also points to confirmation bias. In contrast to the conclusions of existing (often cross-sectional) studies, our panel study shows that the experience of an extreme weather event on their doorstep does not increase the likelihood of accepting the existence of climate change among people who are skeptical about the existence of climate change ex-ante. Overall, we find an effect of living very close to the flooded areas in the full sample.

Furthermore, we can show that this effect is entirely driven by *ex-ante* climate change believers who, under the impression of the nearby flood event, feel confirmed in their beliefs and are therefore less prone to switch to climate skeptics.

## 2.4 Conclusion

Based on data from a representative panel of 11,194 households in Germany and objective flood experience data, we have empirically analyzed the causal effect of a natural disaster on climate change beliefs and whether this effect differs depending on prior climate change beliefs. We further observed variation in the treatment intensity by varying the proximity to the affected area. To the best of our knowledge, this is the first time that it could be shown that the proximity to an extreme weather event plays a substantial role in confirming beliefs in the existence of climate change, while it has no effect on the beliefs of climate skeptics. In the full sample, we find a significant causal effect of spatial proximity to the event. We further find that this effect is driven by the group of pre-flood climate change believers. The comparison of affected (defined as within a 1 km radius around the flooded area) and unaffected pre-flood believers reveals that the latter are more likely to switch to skeptics, which means that the close flood proximity prevents respondents from becoming skeptics. We do not find a significant effect of spatial proximity for climate skeptics. However, it is essential to note that the lack of a significant effect does not necessarily reflect a failure to update skeptical beliefs. On the contrary, it can be observed that the majority of the pre-flood climate change skeptics evolve into believers over the entire period of the survey. However, unlike the pre-flood believers, this development is independent of their spatial proximity to the flood. There are two explanations for this observation: (a) an effect of the flood on all participants of the survey regardless of their spatial proximity and (b) other nation-wide effects such as the extensive media coverage on climate change. This opens interesting perspectives for further research on the channels through which belief updating works.

## 2.5 Appendix to Chapter 2

### *a) Data*

The main data source is the Eval-MAP panel conducted annually between 2012 and 2015 as part of a research project financed by the German Federal Ministry of Education and Research (BMBF). The survey institute forsa approached the same ~10,000 households annually and asked the household heads (defined as the person typically making financial decisions for the household) to complete an online questionnaire. Non-internet users participated through an electronic device fitted to the TV. The pool of households is representative for Germany in terms of household size and location. Households that moved between the survey waves were excluded. There might be *ex-ante* self-selection of risk-seeking individuals into flood-prone areas. However, survey data on the same sample show that natural hazards are of little relevance for (re-)location decisions of households in Germany (Kahsay & Osberghaus, 2018). The number of respondents used in this analysis varies between 4,738 in 2013 and 5,971 in 2015, of which 1,892 participated in all four years. In sum, the panel dataset used in this analysis includes 22,251 observations from 11,194 households. The topic of the questionnaire was not communicated beforehand, and the respondents were awarded a small financial incentive (consumer rewards) for completing the questionnaire. The survey consisted of closed questions only. For further information on the panel data and the research project, please refer to http://www.rwi-essen.de/forschung-und-beratung/umwelt-und-ressourcen/projekte/eval-map/.

Variable descriptions, key statistics, and correlations of all used variables are presented in Table S2.4 - Table S2.6. More details on the shares of climate change believers and skeptics in the different time periods and the exact wording of the question are summarized in Table S2.7. In the baseline specification, "don't know" responses are excluded from the analysis. In robustness check RC2, they are included as "climate change skeptics" (all robustness checks are presented in the chapter "Robustness checks"). We analyze differences between skeptics and believers by mean comparison tests (Wilcoxon ranksum tests, reported in Table S2.8) and a multivariate non-linear (probit) regression (Table S2.9). Both statistical methods yield similar results, showing that skeptics tend to be male, relatively less educated, politically conservative, older, and located in Eastern Germany.

The spatial proximity to the flood event of respondents is elicited by various variables. In the baseline specification, we define those respondents as "flood experienced" who live within a radius of 1 km around the flooded area. This threshold as well as the underlying indicator is varied in different robustness checks. Two households are located within the flooded area. They are always included in the "flood experienced" category. While there may be different effects for directly flooded households and households in close proximity to the flood, the group of directly flooded households is too small for a meaningful statistical analysis.

## b) Tables

Table S2.4: Description and sources of all used variables

| Variable group | Variable | Description | Data source |
|---|---|---|---|
| Climate beliefs | *CCB* | Belief in the existence of global climate change | Household panel |
| | *ACCB* | Belief that human activity is the main cause of climate change | Household panel |
| Flood experience | *FloodDist* | Shortest distance between residence of household and flooded area | DLR (2017) and NASA (2017) |
| | *FloodDistx* | *FloodDist* is lower than x km (*FloodDist05* refers to 0.5 km) | DLR (2017) and NASA (2017) |
| | *FloodDist1d5* | *FloodDist1,* without households living between 1 and 5 km radius (“doughnut”) | DLR (2017) and NASA (2017) |
| | *FloodIns* | Percentage of flood insurance policies with pay-out in district | GDV (2017) |
| | *FloodTA* | Flooded share of total area in municipality | DLR (2017) and NASA (2017) |
| | *FloodTAd* | *FloodTA* is larger than zero | DLR (2017) and NASA (2017) |
| | *FloodRA* | Flooded share of residential area in municipality | DLR (2017) and NASA (2017) |
| | *FloodRAd* | *FloodRA* is larger than zero | DLR (2017) and NASA (2017) |
| Other variables | *Fem* | Household head is female | Household panel |
| | *Age* | Age of household head in years | Household panel |
| | *Income* | ln of Household income in €/month | Household panel |
| | *HHSize* | Household size in persons | Household panel |
| | *Home* | Homeowner | Household panel |
| | *Educ* | Household head is highly educated (at least college degree) | Household panel |
| | *Left* | Household head is politically left oriented (leaning towards social democrat, green, left (socialist) or “pirates” party) | Household panel |
| | *East* | Household resides in East Germany | Household panel |
| | *Damage* | Household reports financial or health-related flood damage experience | Household panel |
| | *Risk* | Household head’s self-reported level of risk seeking on a scale from 0 to 10 | Household panel |
| | *Patience* | Household head’s self-reported level of patience on a scale from 0 to 10 | Household panel |

Table S2.5: Descriptive statistics of all used variables. Missing data are excluded.

| Variable group | Variable | Mean | Median | Min | Max | S.D. (within) | S.D. (betw.) | N (observations) | N (households) |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variables | *CCB* | 0.82 | 1 | 0 | 1 | 0.20 | 0.35 | 22,251 | 11,194 |
| | *ACCB* | 0.45 | 0 | 0 | 1 | 0.24 | 0.46 | 22,182 | 11,164 |
| Flood experience | *FloodDist* | 69.70 | 36.33 | 0 | 274.33 | 0 | 72.20 | 22,251 | 11,194 |
| | *FloodDist05* | 0.02 | 0 | 0 | 1 | 0 | 0.14 | 22,251 | 11,194 |
| | *FloodDist1* | 0.04 | 0 | 0 | 1 | 0 | 0.20 | 22,251 | 11,194 |
| | *FloodDist1d5* | 0.05 | 0 | 0 | 1 | 0 | 0.21 | 20,156 | 10,393 |
| | *FloodDist5* | 0.14 | 0 | 0 | 1 | 0 | 0.35 | 22,251 | 11,194 |
| | *FloodDist10* | 0.22 | 0 | 0 | 1 | 0 | 0.41 | 22,251 | 11,194 |
| | *FloodDist20* | 0.39 | 0 | 0 | 1 | 0 | 0.49 | 22,251 | 11,194 |
| | *FloodIns* | 0.52 | 0.08 | 0.02 | 12.5 | 0 | 1.37 | 22,251 | 11,194 |
| | *FloodTA* | 0.00 | 0 | 0 | 0.51 | 0 | 0.01 | 22,251 | 11,194 |
| | *FloodTAd* | 0.13 | 0 | 0 | 1 | 0 | 0.34 | 22,251 | 11,194 |
| | *FloodRA* | 0.00 | 0 | 0 | 0.66 | 0 | 0.01 | 22,251 | 11,194 |
| | *FloodRAd* | 0.09 | 0 | 0 | 1 | 0 | 0.29 | 22,251 | 11,194 |
| Other variables | *Fem* | 0.33 | 0 | 0 | 1 | 0 | 0.48 | 22,244 | 11,192 |
| | *Age* | 53.54 | 54 | 18 | 89 | 0.71 | 13.71 | 22,251 | 11,194 |
| | *Income* | 7.83 | 7.92 | 5.52 | 8.69 | 0.14 | 0.53 | 19,097 | 9,933 |
| | *HHSize* | 2.19 | 2 | 1 | 6 | 0.26 | 1.06 | 21,649 | 10,977 |
| | *Home* | 0.57 | 1 | 0 | 1 | 0.09 | 0.49 | 22,168 | 11,174 |
| | *Educ* | 0.45 | 0 | 0 | 1 | 0.12 | 0.49 | 21,563 | 10,951 |
| | *Left* | 0.40 | 0 | 0 | 1 | 0.17 | 0.47 | 20,492 | 10,644 |
| | *East* | 0.14 | 0 | 0 | 1 | 0 | 0.35 | 22,251 | 11,194 |
| | *Damage* | 0.32 | 0 | 0 | 1 | 0 | 0.47 | 5,776 | 5,776 |
| | *Risk* | 5.20 | 5 | 0 | 10 | 0.77 | 1.91 | 11,530 | 7,543 |
| | *Patience* | 5.93 | 6 | 0 | 10 | 0.87 | 2.26 | 11,530 | 7,547 |

*Notes:* Variables with no within-standard deviation (S.D. (within) = 0) are constant over time. For the variable Damage only data of 2012 are used. For Risk and Patience data are available only for the years 2012 and 2014.

Table S2.6: Correlation matrix (spearman's rho) of key variables.

| | Variable | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | *CCB* | 1.00 | | | | | | | | | |
| 2 | *ACCB* | 0.22 | 1.00 | | | | | | | | |
| 3 | *FloodDist* | | 0.04 | 1.00 | | | | | | | |
| 4 | *FloodIns* | -0.03 | -0.04 | -0.59 | 1.00 | | | | | | |
| 5 | *FloodTA* | -0.03 | -0.03 | -0.53 | 0.33 | 1.00 | | | | | |
| 6 | *FloodRA* | -0.02 | -0.03 | -0.45 | 0.28 | 0.83 | 1.00 | | | | |
| 7 | *Fem* | 0.04 | 0.02 | -0.03 | 0.03 | | 0.02 | 1.00 | | | |
| 8 | *Age* | -0.09 | -0.04 | 0.03 | -0.04 | | | -0.12 | 1.00 | | |
| 9 | *Income* | | | 0.03 | -0.02 | -0.03 | -0.05 | -0.17 | -0.05 | 1.00 | |
| 10 | *Educ* | 0.05 | | | 0.02 | 0.04 | 0.04 | | -0.16 | 0.23 | 1.00 |

*Notes:* Missing data are excluded. Number of observations varies between 19,090 and 22,251. Only correlations which are significant at the 1% level are included.

Table S2.7: Number of household heads in the four survey periods and their statements regarding the existence of global climate change.

| Global climate change… | Coded as… | 2012 | 2013 | 2014 | 2015 | All periods |
|---|---|---|---|---|---|---|
| | | Before the flood | | After the flood | | |
| … is already taking place | "Climate change believers" (*CCB*=1) | 4,815 (83.4%) | 3,560 (75.1%) | 5,002 (86.8%) | 4,841 (81.1%) | 18,218 (81.9%) |
| … will take place in future | "Climate change skeptics" (*CCB*=0) | 698 (12.1%) | 922 (19.5%) | 561 (9.7%) | 864 (14.5%) | 3,045 (13.7%) |
| … will not occur at all | "Climate change skeptics" (*CCB*=0) | 263 (4.5%) | 256 (5.4%) | 203 (3.5%) | 266 (4.5%) | 988 (4.4%) |
| Total | | 5,776 | 4,738 | 5,766 | 5,971 | 22,251 |
| Don't know (excluded from the main analysis, included in robustness check RC2) | | 153 | 189 | 190 | 216 | 748 |

*Notes:* Answers to the question "In the media, there is currently a lot of reports and discussions on global climate change. What do you think, with which of the following statements do you agree most?"

Table S2.8: Descriptive statistics (number of households and means) of various socio-economic variables for samples of “climate change skeptics” versus “climate change believers”.

| Variable | Sample of pre-flood “climate change skeptics” | | Sample of pre-flood “climate change believers” | | Mean difference (skeptics–believers) |
|---|---|---|---|---|---|
| | Mean | N | Mean | N | |
| *Fem* | 0.31 | 1,712 | 0.35 | 6,339 | -0.04*** |
| *Age* | 54.91 | 1,714 | 51.13 | 6,339 | +3.78*** |
| *Income* | 7.80 | 1,424 | 7.79 | 5,347 | n.s. |
| *HHSize* | 2.20 | 1,636 | 2.24 | 6,076 | n.s. |
| *Home* | 0.59 | 1,708 | 0.53 | 6,303 | +0.06*** |
| *Educ* | 0.37 | 1,612 | 0.46 | 6,035 | -0.09*** |
| *Left* | 0.30 | 1,526 | 0.43 | 5,718 | -0.13*** |
| *East* | 0.18 | 1,714 | 0.12 | 6,339 | +0.06*** |
| *Damage* | 0.28 | 536 | 0.32 | 2,779 | -0.04* |
| *Risk* | 5.05 | 535 | 5.20 | 2,773 | n.s. |
| *Patience* | 6.01 | 536 | 5.84 | 2,773 | n.s. |

*Notes:* Only one observation per household is included. All variables are measured in the Eval-MAP panel before the flood 2013. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively, and are based on the Wilcoxon Ranksum Test. Non-significant differences (p>0.1) are indicated as “n.s.”.

Table S2.9: Probit models of climate change beliefs (CCB)

| Dependent Variable<br>Sample | CCB<br>All observations, pooled sample | CCB<br>One observation per household, only pre-flood |
|---|---|---|
| *Fem* | 0.025*** | 0.025*** |
| *Age* | -0.002*** | -0.003*** |
| *Income* | -0.018** | -0.018** |
| *HHSize* | 0.002 | 0.006* |
| *Home* | 0.000 | -0.014 |
| *Educ* | 0.031*** | 0.055*** |
| *Left* | 0.086*** | 0.089*** |
| Year: 2012 | base category | - |
| Year: 2013 | -0.072*** | - |
| Year: 2014 | 0.046*** | - |
| Year: 2015 | -0.005 | - |
| Pseudo-R2 | 0.044 | 0.035 |
| Number of observations | 17,856 | 6,249 |
| Number of households | 9,503 | 6,249 |

*Notes:* Average marginal effects, based on the probit model. Standard errors are clustered at the household level (column 2) or at the federal state level (column 3). Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Table S2.10: DiD models of climate change belief (dependent variable: CCB), flood experience measured by binary distance variables

| Model | DiD05 | DiD1 | DiD5 | DiD10 | DiD20 |
|---|---|---|---|---|---|
| Treatment variable: distance to flooded areas less than… | 0.5 km | 1 km | 5 km | 10 km | 20 km |
| Treatment effect ($T_t$*$F_i$) | 0.065** | 0.041* | 0.006 | 0.017 | 0.008 |
| Year: 2012 | base category | base category | base category | base category | base category |
| Year: 2013 | -0.063*** | -0.062*** | -0.063*** | -0.063*** | -0.063*** |
| Year: 2014 | 0.036*** | 0.036*** | 0.037*** | 0.034*** | 0.035*** |
| Year: 2015 | -0.004 | -0.005 | -0.004 | -0.006 | -0.006 |
| Adjusted $R^2$ | 0.473 | 0.473 | 0.472 | 0.473 | 0.472 |
| Number of observations | 22,251 | 22,251 | 22,251 | 22,251 | 22,251 |
| Number of households | 11,194 | 11,194 | 11,194 | 11,194 | 11,194 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The adjusted $R^2$ include variance explained by the fixed effects. The variable $T_t$*$F_i$ indicates observations from the treatment group ($F_i$=1, household lives within the indicated radius around the flooded areas) and from periods after the flood ($T_t$=1, observation from 2014 or 2015).

Table S2.11: Mean values of flood proximity and other variables for four household groups defined by belief updating behavior

| Group | Group 1 | Group 2 | Group 3 | Group 4 | Full sample |
|---|---|---|---|---|---|
| Description | believers before and after the flood | believers before, skeptics after the flood | skeptics before and after the flood | skeptics before, believers after the flood | |
| Number of households | 4,150 | 284 | 549 | 656 | 5,639 |
| *FloodDist1* | 0.05 | 0.03 | 0.06 | 0.05 | 0.05 |
| *Fem* | 0.33 | 0.30 | 0.28 | 0.29 | 0.32 |
| *Age* | 52.6 | 55.7 | 57.3 | 56.0 | 53.6 |
| *Income* | 7.81 | 7.75 | 7.80 | 7.82 | 7.81 |
| *HHSize* | 2.22 | 2.22 | 2.16 | 2.17 | 2.21 |
| *Home* | 0.56 | 0.56 | 0.61 | 0.62 | 0.57 |
| *Educ* | 0.46 | 0.45 | 0.39 | 0.39 | 0.44 |
| *Left* | 0.45 | 0.34 | 0.27 | 0.36 | 0.42 |
| *East* | 0.12 | 0.17 | 0.20 | 0.17 | 0.14 |
| *Damage* | 0.33 | 0.29 | 0.30 | 0.24 | 0.32 |
| *Risk* | 5.23 | 5.11 | 5.34 | 5.82 | 5.20 |
| *Patience* | 5.87 | 6.07 | 5.91 | 6.28 | 5.92 |

*Notes:* Only one observation per household is included. Only respondents included who participated before and after the flood event. All variables (except *FloodDist1*) are measured in the Eval-MAP panel before the flood 2013. For some variables, the number of observations is lower than the group size due to missing values. Bold entries indicate variables with significant (p<0.1) differences between group 1 and 2, or between group 3 and 4. For the calculation of p-values, all available observations are used, based on the Pearson test of independence corrected for complex survey data, to account for possible correlations of multiple observations per respondent.

Table S2.12: Estimated coefficients of probit models of climate change belief changing behavior

| Dependent Variable | CCBto1 | CCBto0 |
|---|---|---|
| Description of dependent variable | Changing to CCB=1 | Changing to CCB=0 |
| Estimation sample | Only pre-flood climate change skeptics | Only pre-flood climate change-believers |
| *FloodDist1* | -0.111 | -0.378** |
| *Fem* | 0.142** | -0.032 |
| *Age* | -0.006 | 0.011*** |
| *Income* | 0.046 | -0.109 |
| *HHSize* | -0.019 | 0.073** |
| *Home* | 0.000 | -0.064 |
| *Educ* | -0.011 | 0.021 |
| *Left* | 0.273** | -0.249*** |
| Pseudo-R2 | 0.031 | 0.034 |
| Number of observations | 942 | 3,528 |

*Notes:* Flood experience relates to the place of residence within a 1 km radius around the flooded areas. Only one observation per household included. Federal state fixed effects are always included. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The estimated average marginal effects are reported in Table 3 in the main text.

Table S2.13: DiD models of climate change belief (dependent variable: CCB), separated for pre-flood belief groups.

| Model | DiD1 | DiD1skep | DiD1believ |
|---|---|---|---|
| Sample | All | Pre-flood skeptics | Pre-flood believers |
| Treatment variable ($F_i$) | *FloodDist1* | *FloodDist1* | *FloodDist1* |
| Treatment effect ($T_t$*$F_i$) | 0.041* | -0.015 | 0.034** |
| Number of observations | 22,251 | 4,127 | 14,877 |
| Number of households | 11,194 | 1,747 | 6,466 |

*Notes:* Household fixed effects and year effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The variable $T_t$*$F_i$ measures treatment intensity (in terms of $F_i$) and whether the observation is from a period after the flood ($T_t$=1, observation from 2014 or 2015).

### c) *Figures*

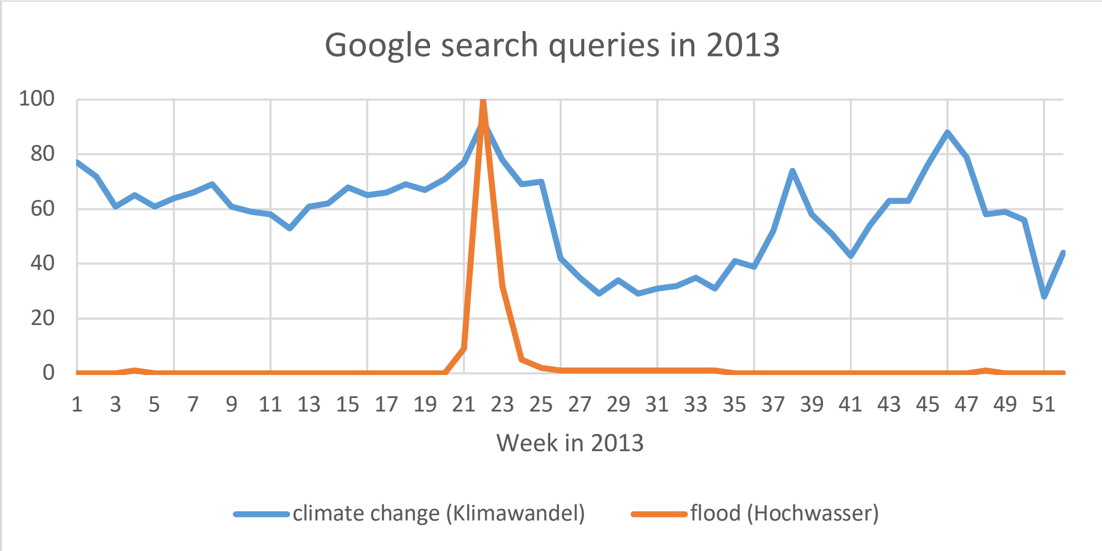



Figure S2.2: Google Trend data showing the development of the search queries for the terms "Klimawandel" (climate change) and "Hochwasser" (flood).

*Notes:* The graphs compare the search volume over time. The peak in early June coincides with the time of the flood event. Source: Google Trends, https://trends.google.de/trends/explore?date=2013-01-01%202013-12-31&geo=DE&q=Klimawandel,Jahrhundertflut (accessed 20.11.2018).

### d) *Alternative dependent variable: Belief in anthropogenic climate change*

In this section, we use the belief in human causes of climate change (belief in anthropogenic climate change, variable *ACCB*) as an alternative dependent variable $y_{it}$. These data were derived by asking those who believe in the existence of climate change what they think is responsible for climate change: mainly human activity, mainly natural processes, or whether both sources are equally responsible. We construct a dummy variable with the label "anthropogenic climate change believers" (*ACCB*=1) for those who stated climate change is mainly caused by human activity. The other respondents and the general climate change skeptics are labeled as "anthropogenic climate change skeptics" (*ACCB*=0). We carry out all tests that were presented for *CCB* above for *ACCB* as well to present as complete a picture as possible. Results show similar patterns for switching behavior.

Table S2.14 shows that right before the flood in 2013, 45.3 percent of respondents attributed climate change mainly to human activities. This share was only a bit larger than of those who perceived man and nature equally responsible. The share of "anthropogenic climate change believers" increased slightly after the flood by 1.8 percentage points.

Table S2.14: Number of household heads in the four survey periods and their statements regarding the existence of anthropogenic causes of global climate change.

| Global climate change… | Coded as… | 2012 | 2013 | 2014 | 2015 | All periods |
|---|---|---|---|---|---|---|
| | | Before the flood | | After the flood | | |
| … is mainly caused by humans | "Anthropogenic climate change believers" (*ACCB*=1) | 2,415 (42.0%) | 2,142 (45.3%) | 2,704 (47.1%) | 2,794 (47.0%) | 10,055 (45.3%) |
| … is equally caused by humans and natural sources | "Anthropogenic climate change skeptics" (*ACCB*=0) | 2,867 (49.8%) | 2,128 (45.0%) | 2,642 (46.0%) | 2,642 (44.4%) | 10,279 (46.3%) |
| … is mainly caused by natural sources | | 210 (3.7%) | 200 (4.2%) | 197 (3.4%) | 253 (4.3%) | 860 (3.9%) |
| … will not occur at all | | 263 (4.5%) | 256 (5.4%) | 203 (3.5%) | 266 (4.5%) | 988 (4.5%) |
| Total | | 5,755 | 4,726 | 5,746 | 5,955 | 22,182 |
| Don't know (excluded from the analysis) | | 12 | 11 | 9 | 15 | 47 |

*Notes:* Answers to the question "In your opinion, who is responsible for climate change?"

Belief in anthropogenic causes for climate change is positively correlated with household size and a tendency to vote for left-wing parties, while it is negatively correlated with age, homeownership, and education (Table S2.15). These findings are in line with observations from other studies (Kahn & Kotchen, 2011; Mccright & Dunlap, 2011) except for education (Bohr, 2017; Mccright & Dunlap, 2011; Taylor et al., 2014). The data show further that the share of respondents who believe in *ACCB* increases over time.

Table S2.15: Probit models of anthropogenic climate change beliefs (ACCB)

| Dependent Variable | ACCB | ACCB |
|---|---|---|
| Sample | All observations, pooled sample | One observation per household, only pre-flood |
| *Fem* | 0.011 | 0.007 |
| *Age* | -0.002*** | -0.001*** |
| *Income* | 0.003 | 0.003 |
| *HHSize* | 0.010* | 0.012*** |
| *Home* | -0.024** | -0.034** |
| *Educ* | -0.017* | -0.014 |
| *Left* | 0.138*** | 0.143*** |
| Year: 2012 | base category | - |
| Year: 2013 | 0.035*** | - |
| Year: 2014 | 0.057*** | - |
| Year: 2015 | 0.061*** | - |
| Pseudo-R2 | 0.022 | 0.023 |
| Number of observations | 17,831 | 6,234 |
| Number of households | 9,486 | 6,234 |

*Notes:* Average marginal effects, based on the probit model. Standard errors are clustered at the household level (column 2) or at the federal state level (column 3). Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Table S2.16 describes the baseline results of the DiD analysis for *ACCB* with regard to different spatial proximities to the flood event. The strongest treatment effect can be observed for households within a radius of 1 km. This confirms the findings from the main analysis above, that the distance to the flooded area influences belief updating. Similarly, Figure S2.3 depicts the estimated treatment effects for different radii around the flooded area, and broadly confirms the finding that the effect decreases with increasing distance to the event.

Table S2.16: DiD models of anthropogenic climate change belief (dependent variable: ACCB), flood experience measured by binary distance variables

| Model | DiD05 | DiD1 | DiD5 | DiD10 | DiD20 |
|---|---|---|---|---|---|
| Treatment variable: distance to flooded areas less than… | 0.5 km | 1 km | 5 km | 10 km | 20 km |
| Treatment effect ($T_t$*$F_i$) | 0.051 | 0.066*** | 0.019 | 0.023** | 0.025** |
| Year: 2012 | base category | base category | base category | base category | base category |
| Year: 2013 | 0.028*** | 0.028*** | 0.028*** | 0.028*** | 0.028*** |
| Year: 2014 | 0.047*** | 0.045*** | 0.046*** | 0.043*** | 0.039*** |
| Year: 2015 | 0.051*** | 0.049*** | 0.049*** | 0.047*** | 0.042*** |
| Adjusted $R^2$ | 0.533 | 0.534 | 0.533 | 0.533 | 0.533 |
| Number of observations | 22,182 | 22,182 | 22,182 | 22,182 | 22,182 |
| Number of households | 11,164 | 11,164 | 11,164 | 11,164 | 11,164 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The adjusted $R^2$ include variance explained by the fixed effects. The variable $T_t$*$F_i$ indicates observations from the treatment group ($F_i$=1, household lives within the indicated radius around the flooded areas) and from periods after the flood ($T_t$=1, observation from 2014 or 2015).

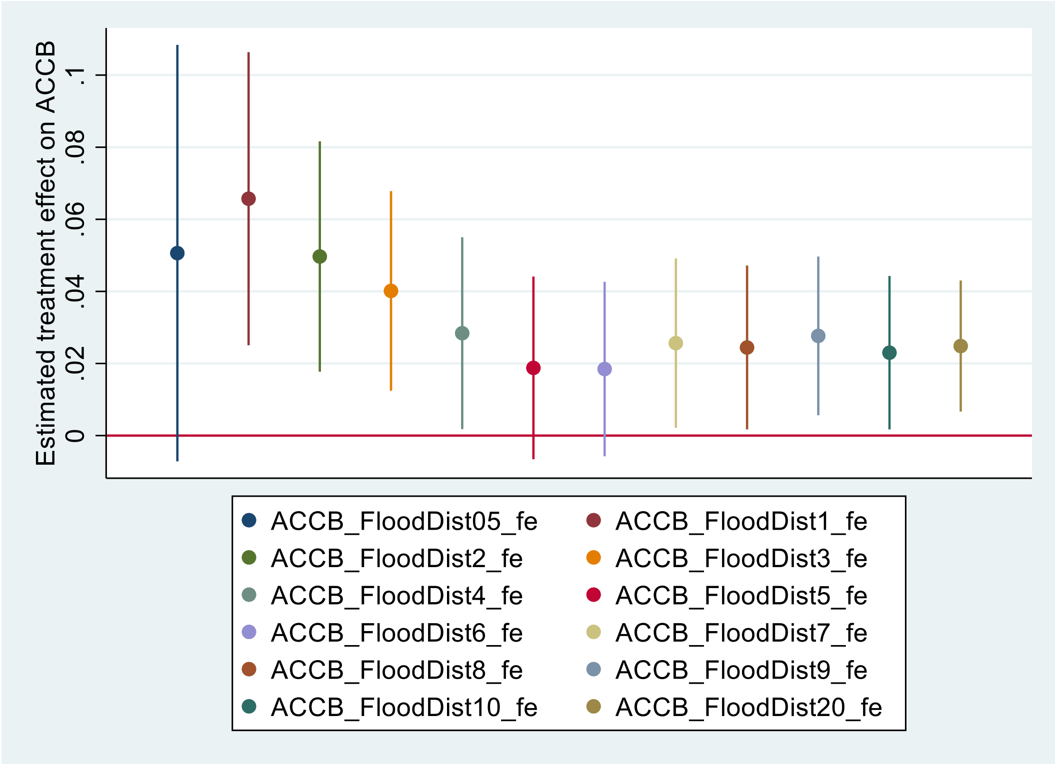



Figure S2.3: Estimated treatment effects of spatial proximity to the flooded areas on ACCB.

*Notes:* Point estimates and 90% confidence intervals of the treatment effects, based on DiD estimations with household-fixed effects, with varying distance thresholds for the identification of treated households.

Table S2.17: Descriptive statistics of anthropogenic climate change beliefs (ACCB) pre- and post-flood, changing behavior

| Sample | 5,613 respondents (100.0%) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Pre-flood beliefs | Pre-flood skeptics (*ACCB*=0): 3,122 (55.6%) | | | | Pre-flood believers (*ACCB*=1): 2,491 (44.4%) | | | |
| Proximity to flooded areas | Less than 1 km away from flooded areas: 165 (5.3%) | | More than 1 km away from flooded areas: 2,957 (94.7%) | | Less than 1 km away from flooded areas: 102 (4.1%) | | More than 1 km away from flooded areas: 2,389 (95.9%) | |
| Post-flood beliefs | Changing to believers: 46 (27.9%) | Remaining skeptics: 119 (72.1%) | Changing to believers: 685 (23.2%) | Remaining skeptics: 2,272 (76.8%) | Remaining believers: 84 (82.4%) | Changing to skeptics: 18 (17.7%) | Remaining believers: 1,853 (77.6%) | Changing to skeptics: 536 (22.4%) |

*Notes:* Only one observation per household included. Only respondents included who participated before and after the flood event.

By following the above procedure, we also analyze the effects of the proximity to the flood on belief updating, here belief in mainly anthropogenic causes of climate change. We observe that the patterns are similar to the beliefs about existence of climate change: 44.4 percent belief in anthropogenic causes of climate change before the flood (Table S2.17). About 20 percent of these change to skeptics after the flood. The probability of switching to skeptics is higher for those who live farther than 1 km away from the flooded area. This is confirmed in probit models (Table S2.18). Here we estimate that the probability to switch from believers to skeptics is 7.8 percent lower if the participant lives in a 1 km radius around the flooded area. As in the case of *CCB*, there is no significant effect on respondents who did not believe in anthropogenic climate change before the flood.

Table S2.18: Probit models of anthropogenic climate change belief switching behavior

| Estimation sample<br>Dependent Variable<br>Description of dependent variable | Pre-flood skeptics<br>ACCBto1<br>Changing to believers | Pre-flood believers<br>ACCBto0<br>Changing to skeptics |
|---|---|---|
| Flood experience | 0.057 | -0.078* |
| Fem | 0.009 | 0.029* |
| Age | -0.000 | 0.003*** |
| Income | -0.013 | -0.013 |
| HHSize | 0.024*** | 0.006 |
| Home | -0.009 | 0.014 |
| Educ | -0.023** | 0.044** |
| Left | 0.049** | -0.046*** |
| Pseudo-R2 | 0.015 | 0.019 |
| Number of observations | 2,487 | 1,967 |

*Notes:* Reported values are average marginal effects on the probabilities to switch to the respective other belief. Flood experience relates to the place of residence within a 1 km radius around the flooded areas. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The number of observations is lower than in Table S2.17 due to missing values in the covariates.

### *e) Pre-treatment analyses*

For a causal interpretation of the estimated treatment effect, the assumption of parallel pre-treatment trends in the dependent variable is crucial. In this section, we, therefore, assess the time trend of *CCB* prior to the flood event and evaluate whether there are differences between different levels of various flood experience variables ($F_i$).

First, we assess the time trends of *CCB* graphically, separating the households based on different levels of binary flood experience variables. The graphs, depicted in Figure S2.4, suggest that (the few) households living very close to the flooded areas (*FloodDist05*=1) may have a different time trend in terms of *CCB* than other households. The other variables capturing flood experience do not seem to imply pre-treatment differences in the time trend. However, this analysis does not allow any conclusions about the statistical significance of trend differences.

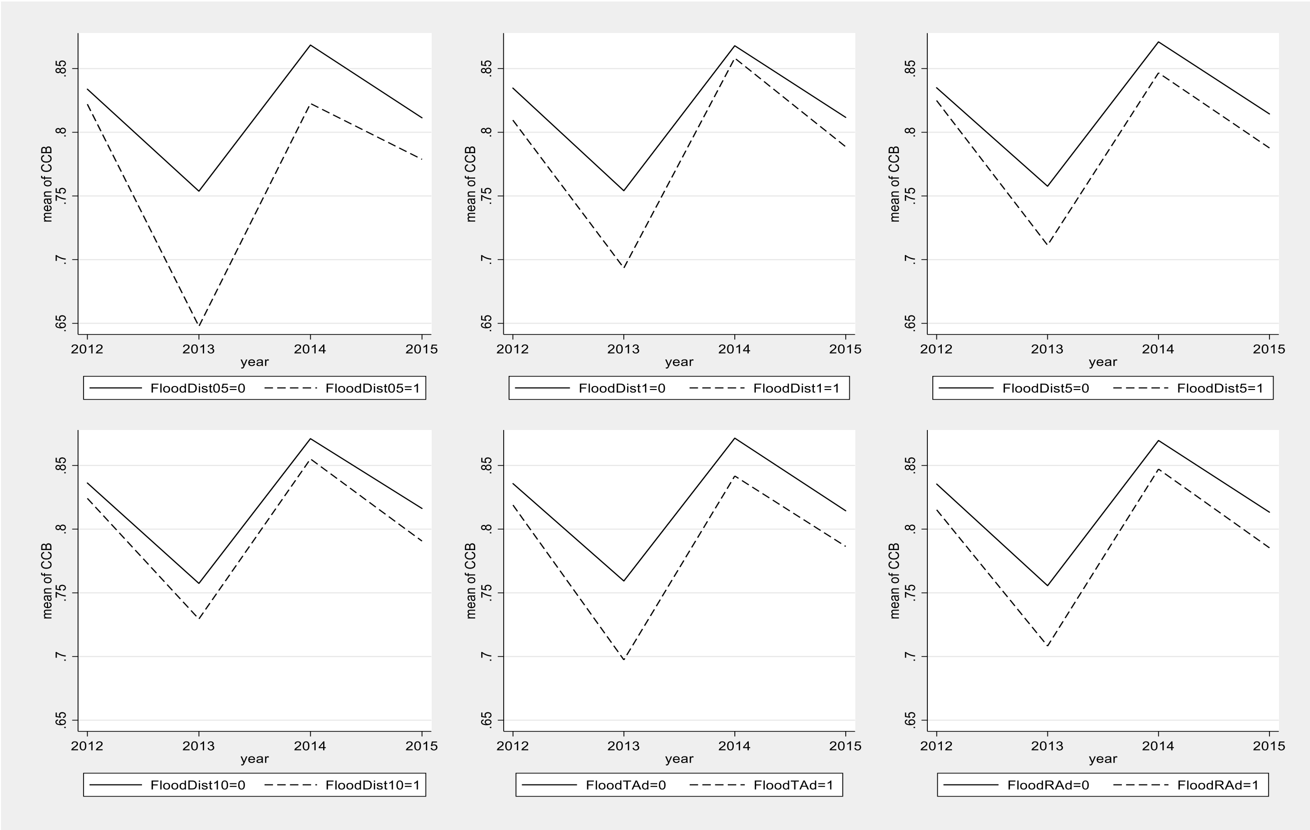



Figure S2.4: Graphical pre-treatment analyses. Mean values of CCB for different flood experience (treatment) groups. The different flood experience variables are explained in Table S2.4.

The assumption of parallel *CCB* trends before the flood can also be assessed by regressing *CCB* on a series of interactions of year dummies with the $F_i$ variables (Autor, 2003). Basically, the time dummy $T_t$ which indicates pre- and post-treatment observations in equation (1) is split up in several year dummies. The resulting estimation model is the following:

$$y_{it} = \sum_{k=2013}^{2015}(\gamma_k T_k F_i) + \mu_i + \vartheta_t + \varepsilon_{it} \quad t = 2012, \ldots, 2015 \quad (3)$$

The variable $T_k$ is an indicator variables which equals one if $k=t$ and zero otherwise. Then the estimated coefficient $\gamma_{k=2013}$ captures possible differences in the pre-flood time trend. Given identical pre-treatment trends for different levels of $F_i$, $\gamma_{k=2013}$ should be insignificant. Table S2.19 summarizes the estimated coefficients of $\gamma_{k=2013}$ from regressions of equation (3) with different $F_i$ variables. The estimated pre-treatment effect is insignificant in all specifications, which is reassuring for the assumption of parallel pre-treatment trends for all $F_i$ variables.

Another possibility to assess pre-treatment time trends is the regression of *CCB* on a linear time trend, using only pre-treatment observations, separately for the treatment and control group. We run these regressions using all binary flood experience variables as group separators. Differences in the estimated time trends are tested for significance using the Wald chi-squared test. The results are also summarized in Table S2.19. In half of the possible specifications, some minor pre-treatment differences are observable, but not for the flood experience variable *FloodDist1* which is the preferred variable for the baseline model.

Table S2.19: Results of statistical pre-treatment analyses

| Variable $F_i$ | Estimated coefficient $\gamma_{k=2013}$ | Difference of pre-treatment time trends |
|---|---|---|
| *FloodDist* | 0.000 | n.a. |
| *FloodDist05* | -0.047 | -0.094* |
| *FloodDist1* | 0.019 | -0.035 |
| *FloodDist5* | 0.000 | -0.036* |
| *FloodDist10* | -0.004 | -0.016 |
| *FloodIns* | -0.007 | n.a. |
| *FloodTA* | -0.408 | n.a. |
| *FloodTAd* | -0.022 | -0.045** |
| *FloodRA* | 0.156 | n.a. |
| *FloodRAd* | -0.011 | -0.027 |

*Notes:* Column 2: Estimated coefficients of a pre-treatment effect (effect in year 2013) in a DiD estimation of CCB with treatment effects estimated for every year. Column 3: Differences in the estimated pre-treatment time trends (treatment group minus control group). The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. Significance levels are obtained from the Wald chi-squared test for inter-group coefficient comparisons, using the STATA commands suest and test.

### f) Robustness checks

In this section, we report various robustness checks regarding alternative specifications, definition of the dependent variable, flood experience variables, and samples. We focus on the main results, i.e. (1) a positive flood effect on climate beliefs for households living very close to the flooded areas, and (2) a negative effect of flood proximity on the switching behavior of pre-flood believers, while there is no significant effect of spatial proximity on the switching behavior of pre-flood skeptics. For assessing the robustness of result (1), we compare to the baseline DiD regression results reported in

Table S2.10. For assessing result (2), we refer to the probit models reported in Table 2.3 as the baseline. Unless otherwise indicated, we use *FloodDist1* as the main flood experience variable.

The first series of robustness checks focusses on the specification of the dependent variable, *CCB*. In robustness check (RC) 1 we reformulate *CCB*, using all three outcomes of the original questionnaire item. Remember that in the baseline model, we define those respondents who expect climate change to occur in the future as climate skeptics. In RC1 we instead use an ordinal dependent variable capturing all three outcomes (climate change will not occur at all, will occur in future, is already occurring). Then the probit models of switching behavior are replaced by ordered probit models, estimating the existence and strength of belief changes. In RC2, we label those respondents who answered "don't know" as climate skeptics (*CCB*=0) and then repeat the statistical analyses as in the baseline. While the general effect of spatial proximity in RC1 becomes marginally insignificant, the remaining results remain robust (Table S2.20 and Table S2.21).

Table S2.20: RC1 and RC2: DiD models of climate change belief

| Robustness check<br>Model | Baseline<br>DiD1 | RC1<br>DiD1_3 | RC2<br>DiD1_dk |
|---|---|---|---|
| Description of RC | | *CCB* with 3 outcomes | Don't know as skeptics |
| Treatment effect ($T_t$*$F_i$) | 0.041* | 0.043 | 0.043** |
| Year: 2012 | base category | base category | base category |
| Year: 2013 | -0.062*** | -0.072*** | -0.067*** |
| Year: 2014 | 0.036*** | 0.046*** | 0.029*** |
| Year: 2015 | -0.005 | -0.004 | -0.010 |
| Number of observations | 22,251 | 22,251 | 22,973 |
| Number of households | 11,194 | 11,194 | 11,408 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The variable $T_t$*$F_i$ indicates observations from the treatment group ($F_i=1$, household lives within the indicated radius around the flooded areas) and from periods after the flood ($T_t=1$, observation from 2014 or 2015).

Table S2.21: RC1 and RC2: Probit models of climate change belief switching behavior

| Robustness check | Baseline | | RC1 | | RC2 | |
|---|---|---|---|---|---|---|
| Description of RC | | | *CCB* with 3 outcomes | | "Don't know" as *CCB*=0 | |
| Estimation sample | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers |
| Dependent Variable | CCBto1 | CCBto0 | Change of *CCB* | Change of *CCB* | CCBto1 | CCBto0 |
| *Flood experience* | -0.111 | -0.378** | -0.006 | 0.036** | -0.054 | -0.435** |
| *Fem* | 0.142** | -0.032 | 0.040 | 0.032 | 0.063 | -0.027 |
| *Age* | -0.006 | 0.011*** | -0.005 | -0.010*** | -0.005 | 0.011*** |
| *Income* | 0.046 | -0.109 | 0.034 | 0.111 | 0.064 | -0.080 |
| *HHSize* | -0.019 | 0.073** | 0.006 | -0.074** | -0.019 | 0.072** |
| *Home* | 0.000 | -0.064 | 0.055 | 0.060 | 0.041 | -0.090 |
| *Educ* | -0.011 | 0.021 | -0.008 | -0.032 | 0.013 | 0.005 |
| *Left* | 0.273** | -0.249*** | 0.182* | 0.257*** | 0.275*** | -0.288*** |
| Number of observations | 942 | 3,528 | 942 | 3,528 | 1,106 | 3,574 |

*Notes:* Reported values are estimated coefficients of the probit models (columns 2, 3, 6 and 7) and ordered probit models (columns 4 and 5), respectively. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

In the second series of robustness checks, we choose alternative variables for measuring flood experience. In the baseline specification, personal flood experience is approximated by a binary indicator of living within a certain radius around the closest flooded area (e.g., *FloodDist1*). In order to capture further dimensions of flood experience, we derive the following flood indicators: In RC3, we use the continuous variable *ln(FloodDist).* For interpreting these results, note that high values of *FloodDist* mean higher spatial *distance*, hence the signs of the $F_i$ coefficients should change. In RC4, we again base the measurement on the binary variable *FloodDist1*, but exclude households living between the 1 km radius and the 5 km radius (treatment variable *FloodDist1d5*, "doughnut estimator"). The results confirm that in terms of a general effect in the DiD setting, spatial proximity is most relevant for households very close to the flooded area (Table S2.22). They also confirm that spatial proximity, regardless its specific formulation, is only relevant for pre-flood believers (Table S2.23).

Table S2.22: RC3 and RC4: DiD models of climate change belief

| Robustness check | Baseline | RC3 | RC4 |
|---|---|---|---|
| Model | DiD1 | DiDln | DiD1d5 |
| Description of RC | | $F_i$: *ln(FloodDist)* | $F_i$: “Doughnut” |
| Treatment effect ($T_t$*$F_i$) | 0.041* | -0.004 | 0.040* |
| Year: 2012 | base category | base category | base category |
| Year: 2013 | -0.062*** | -0.063*** | -0.062*** |
| Year: 2014 | 0.036*** | 0.051*** | 0.037*** |
| Year: 2015 | -0.005 | 0.011 | -0.002 |
| Number of observations | 22,251 | 22,248 | 20,156 |
| Number of households | 11,194 | 11,192 | 10,130 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. In column 3, two households have to be omitted because they live directly in the flooded area and have a spatial distance of zero.

Table S2.23: RC3 and RC4: Probit models of climate change belief switching behavior

| Robustness check | Baseline | | RC3 | | RC4 | |
|---|---|---|---|---|---|---|
| Description of RC | | | $F_i$: *ln(FloodDist)* | | $F_i$: “Doughnut” | |
| Estimation sample | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers |
| Dependent Variable | CCBto1 | CCBto0 | CCBto1 | CCBto0 | CCBto1 | CCBto0 |
| *Flood experience* | -0.111 | -0.378** | 0.025 | 0.074** | -0.144 | -0.617** |
| *Fem* | 0.142** | -0.032 | 0.145** | -0.037 | 0.107* | 0.021 |
| *Age* | -0.006 | 0.011*** | -0.006 | 0.012*** | -0.006 | 0.010*** |
| *Income* | 0.046 | -0.109 | 0.047 | -0.107 | 0.077 | -0.058 |
| *HHSize* | -0.019 | 0.073** | -0.019 | 0.072** | -0.027 | 0.089*** |
| *Home* | 0.000 | -0.064 | 0.001 | -0.067 | 0.046 | -0.052 |
| *Educ* | -0.011 | 0.021 | -0.008 | 0.027 | -0.026 | 0.013 |
| *Left* | 0.273** | -0.249*** | 0.274** | -0.251*** | 0.338*** | -0.250*** |
| Number of observations | 942 | 3,528 | 942 | 3,527 | 858 | 3,196 |

*Notes:* Reported values are estimated coefficients of the probit models. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Next, we use flood experience variables which are not based on spatial distance to the flooded areas. In RC5, we opt for *FloodTA*, which indicates the flooded share of the total area in the municipality of the respondent. The variable *FloodTAd* indicates municipalities with a positive value of FloodTA (used in RC6). In RC7 and RC8, we slightly vary these variables, now measuring the flooded share of the residential area in the respective municipality (*FloodRA* and *FloodRAd*). Finally, in RC9 we focus on the economic damages and use the share of flood insurance policies in the district which filed a claim due to the flood event (*FloodIns*). These measures mirror the economic and social impact the flood had in the direct surrounding of the respondent. Figures S4 and S5 depict the flooded shares of residential areas (*FloodRA*) and the share of insurance policies affected (*FloodIns*), respectively. Table S2.24 shows that all these flood intensity measures had no causal effect on climate beliefs in the general sample. This reconfirms our hypothesis that spatial distance is more decisive for flood effects on climate beliefs. In Table S2.25, we find effects of *FloodTA* and *FloodRA* on the switching behavior of pre-flood believers, and again no effect on pre-flood skeptics.

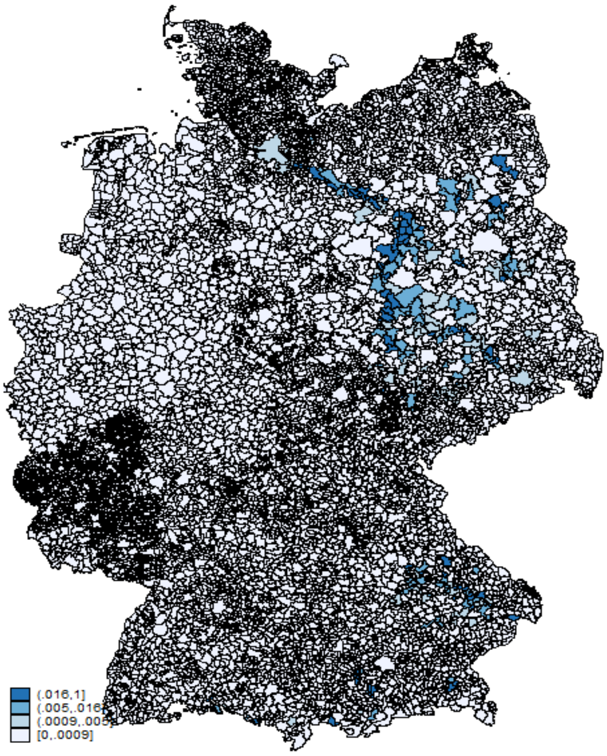



Figure S2.5: Map of the share of flooded residential areas in the municipalities in Germany (FloodRA) (own calculation based on ZKI/DLR 2017 and NASA 2017).

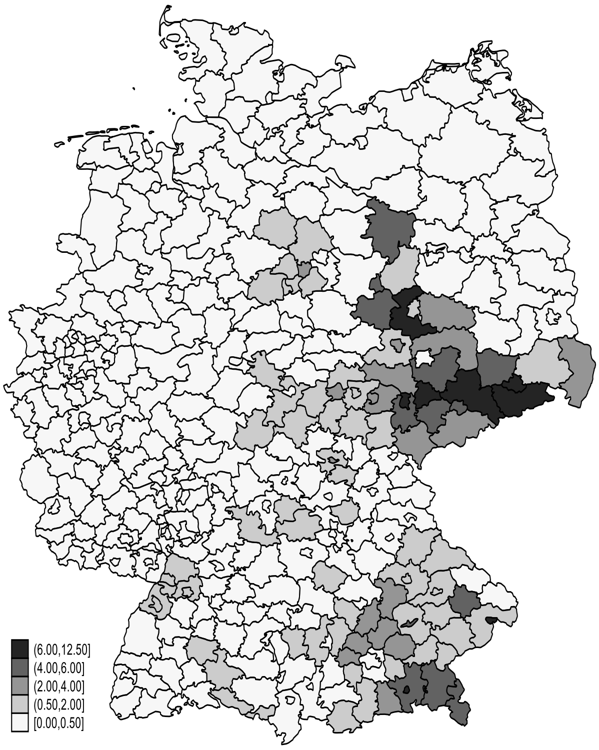



Figure S2.6: Map of the share of flood insurance policies filing a claim due to the flood event in the districts in Germany (FloodIns) (own calculation based on GDV 2017).

Table S2.24: RC5-RC9: DiD models of climate change belief

| Robustness check<br>Model | Baseline<br>DiD1 | RC5<br>DiDTA | RC6<br>DiDTAd | RC7<br>DiDRA | RC8<br>DiDRAd | RC9<br>DiDIns |
|---|---|---|---|---|---|---|
| Description of RC | | $F_i$:<br>*FloodTA* | $F_i$:<br>*FloodTAd* | $F_i$:<br>*FloodRA* | $F_i$:<br>*FloodRAd* | $F_i$:<br>*FloodIns* |
| Treatment effect ($T_t$*$F_i$) | 0.041* | 0.255 | 0.023 | 1.233 | 0.016 | -0.004 |
| Year: 2012 | base category | base category | base category | base category | base category | base category |
| Year: 2013 | -0.062*** | -0.063*** | -0.062*** | -0.063*** | -0.062*** | -0.062*** |
| Year: 2014 | 0.036*** | 0.037*** | 0.035*** | 0.037*** | 0.036*** | 0.040*** |
| Year: 2015 | -0.005 | -0.003 | -0.006 | -0.004 | -0.004 | -0.004 |
| Number of observations | 22,251 | 22,251 | 22,251 | 22,251 | 22,251 | 22,251 |
| Number of households | 11,194 | 11,194 | 11,194 | 11,194 | 11,194 | 11,194 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Table S2.25: RC5, RC7, and RC9: Probit models of climate change belief switching behavior

| Robustness check<br>Description of RC | RC5<br>$F_i$: *FloodTA* | | RC7<br>$F_i$: *FloodRA* | | RC9<br>$F_i$: *FloodIns* | |
|---|---|---|---|---|---|---|
| Estimation sample | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers |
| Dependent Variable | CCBto1 | CCBto0 | CCBto1 | CCBto0 | CCBto1 | CCBto0 |
| *Flood experience $F_i$* | -0.231 | -6.807*** | -4.446 | -38.168** | -0.028 | 0.013 |
| *Fem* | 0.142** | -0.033 | 0.143** | -0.033 | 0.140** | -0.029 |
| *Age* | -0.006 | 0.011*** | -0.006 | 0.012*** | -0.006 | 0.011*** |
| *Income* | 0.048 | -0.116 | 0.050 | -0.117 | 0.050 | -0.115 |
| *HHSize* | -0.019 | 0.076** | -0.018 | 0.077** | -0.020 | 0.075** |
| *Home* | 0.002 | -0.065 | 0.003 | -0.062 | 0.000 | -0.064 |
| *Educ* | -0.012 | 0.023 | -0.014 | 0.024 | -0.010 | 0.023 |
| *Left* | 0.273** | -0.251*** | 0.274** | -0.250*** | 0.271** | -0.246*** |
| Number of observations | 942 | 3,528 | 942 | 3,528 | 942 | 3,528 |

*Notes:* Reported values are estimated coefficients of the probit models. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

In the probit models presented in Table 2.3, the number of observations is necessarily restricted to those with available data for all covariates. To maximize the sample size, we omit all covariates and test the effect of *FloodDist1* (only conditional on federal state fixed effects) on switching behavior in RC10 (Table S2.26). It remains absent for skeptics and significant for believers.

Table S2.26: RC10: Probit models of climate change belief switching behavior

| Robustness check<br>Description of RC | Baseline | | RC10<br>without covariates | |
|---|---|---|---|---|
| Estimation sample | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers |
| Dependent Variable | CCBto1 | CCBto0 | CCBto1 | CCBto0 |
| *Flood experience* | -0.111 | -0.378** | -0.021 | -0.366*** |
| *Fem* | 0.142** | -0.032 | - | - |
| *Age* | -0.006 | 0.011*** | - | - |
| *Income* | 0.046 | -0.109 | - | - |
| *HHSize* | -0.019 | 0.073** | - | - |
| *Home* | 0.000 | -0.064 | - | - |
| *Educ* | -0.011 | 0.021 | - | - |
| *Left* | 0.273** | -0.249*** | - | - |
| Number of observations | 942 | 3,528 | 1,205 | 4,434 |

*Notes:* Reported values are estimated coefficients of the probit models. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Next, we turn to the clustering of estimated standard errors. In the baseline specification of the DiD regression, standard errors are clustered at the household level to allow for an unrestricted covariance structure for each household, but thereby we impose independence between households and thus disregard the possibility of spatial correlations. By clustering over communities (RC11), districts (RC12), or federal states (RC13) we allow for unrestricted covariance structure within each spatial unit, but still impose independence between the units. In all these specifications the baseline results are strengthened – the effect of spatial proximity on climate beliefs is more significant (Table S2.27).

Table S2.27: RC11-RC13: DiD models of climate change belief

| Robustness check<br>Model | Baseline<br>DiD1 | RC11<br>DiD1 | RC12<br>DiD1 | RC13<br>DiD1 |
|---|---|---|---|---|
| Description of RC | Clustering over 11,194 households | Clustering over 3,366 communities | Clustering over 402 districts | Clustering over 16 federal states |
| Treatment effect ($T_t$*$F_i$) | 0.041*<br>(0.022) | 0.041**<br>(0.020) | 0.041**<br>(0.017) | 0.041**<br>(0.016) |
| Year: 2012 | base category | base category | base category | base category |
| Year: 2013 | -0.062*** | -0.062*** | -0.062*** | -0.062*** |
| Year: 2014 | 0.036*** | 0.036*** | 0.036*** | 0.036*** |
| Year: 2015 | -0.005 | -0.005 | -0.005 | -0.005 |
| Number of observations | 22,251 | 22,251 | 22,251 | 22,251 |
| Number of households | 11,194 | 11,194 | 11,194 | 11,194 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. Clustered standard errors of the estimated treatment effect in parentheses.

Regarding the sample characteristics, the results could be affected by panel attrition (Brüderl & Ludwig, 2015) – flood affected households, believers or skeptics may systematically leave or remain in the panel. We therefore rerun the DiD regressions excluding households participating either only before or only after the flood (RC14). Finally, we restrict the sample to households living in the municipalities with positive values of *FloodTA*, hence municipalities which were covered by the DLR satellite imagery and where some flooding has occurred (RC15). In both sample restrictions, the main results stay qualitatively identical (Table S2.28 andTable S2.29).

Table S2.28: RC14 and RC15: DiD models of climate change belief

| Robustness check<br>Model | Baseline<br>DiD1 | RC14<br>DiD1 | RC15<br>DiD1 |
|---|---|---|---|
| Description of RC | | Sample: only respondents pre- and post-flood | Sample: Only municipalities with flooding |
| Treatment effect ($T_t$*$F_i$) | 0.041* | 0.041* | 0.066** |
| Year: 2012 | base category | base category | base category |
| Year: 2013 | -0.062*** | -0.062*** | -0.077*** |
| Year: 2014 | 0.036*** | 0.037*** | 0.006 |
| Year: 2015 | -0.005 | -0.005 | -0.023 |
| Number of observations | 22,251 | 17,081 | 2,211 |
| Number of households | 11,194 | 6,472 | 1,121 |

*Notes:* Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

Table S2.29: RC15: Probit models of climate change belief switching behavior

| Robustness check<br>Description of RC | Baseline | | RC15<br>Only municipalities with flooding | |
|---|---|---|---|---|
| Estimation sample<br><br>Dependent Variable | Pre-flood skeptics<br>CCBto1 | Pre-flood believers<br>CCBto0 | Pre-flood skeptics<br>CCBto1 | Pre-flood believers<br>CCBto0 |
| *Flood experience* | -0.111 | -0.378** | -0.174 | -0.436*** |
| *Fem* | 0.142** | -0.032 | 0.261 | -0.640*** |
| *Age* | -0.006 | 0.011*** | -0.001 | 0.022 |
| *Income* | 0.046 | -0.109 | -0.062 | -0.485 |
| *HHSize* | -0.019 | 0.073** | 0.022 | -0.345*** |
| *Home* | 0.000 | -0.064 | 0.465 | -0.083 |
| *Educ* | -0.011 | 0.021 | 0.424 | -0.027 |
| *Left* | 0.273** | -0.249*** | -0.239 | -0.627** |
| Number of observations | 942 | 3,528 | 111 | 323 |

*Notes:* Reported values are estimated coefficients of the probit models. Only one observation per household included. Only respondents included who participated before and after the flood event. Values of covariates are measured before the flood, standard errors are clustered at the federal state level. Federal state fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively.

In the baseline DiD estimations, we use OLS regressions, thereby ultimately estimating a linear probability model. Although OLS does not take into account the binary nature of the dependent variable, in DiD settings the linear probability model is usually preferred over non-linear alternatives, because non-linear approaches such as probit or logit are violating some important assumptions of the DiD model (Ai & Norton, 2003; Couttenier & Soubeyran, 2013; Monheit, Cantor, Delia, & Belloff, 2011). However, if we estimate the DiD model by conditional logit (controlling for unobserved heterogeneity at the household level, RC16), the main results remain unchanged (Table S2.30 and Table S2.31).

Table S2.30: RC16: DiD models of climate change belief

| Robustness check<br>Model | Baseline<br>DiD1 | RC16<br>DiD1cl |
|---|---|---|
| Description of RC | | Conditional logit instead of OLS |
| Treatment effect ($T_t$*$F_i$) | 0.041* | 0.618* |
| Year: 2012 | base category | base category |
| Year: 2013 | -0.062*** | -0.715*** |
| Year: 2014 | 0.036*** | 0.534*** |
| Year: 2015 | -0.005 | -0.077 |
| Number of observations | 22,251 | 3,954 |
| Number of households | 11,194 | 1,354 |

*Notes:* Reported values are estimated coefficients. Household fixed effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. In the conditional logit estimation, households which do not switch in terms of *CCB* or which only participate once are omitted.

Table S2.31: RC16: DiD models of climate change belief switching behavior, separated for pre-flood belief groups

| Robustness check | Baseline | | RC16 | |
|---|---|---|---|---|
| Description of RC | | | Conditional logit instead of OLS | |
| Sample | Pre-flood skeptics | Pre-flood believers | Pre-flood skeptics | Pre-flood believers |
| Treatment effect ($T_t$*$F_i$) | -0.015 | 0.034** | 0.217 | 1.351** |
| Number of observations | 4,127 | 14,877 | 2,399 | 1,497 |
| Number of households | 1,747 | 6,466 | 819 | 506 |

*Notes:* Reported values are estimated coefficients. Household fixed effects and year effects are always included. The stars *, **, *** denote significance levels of 10, 5, and 1 percent, respectively. The variable $T_t$*$F_i$ measures treatment intensity (in terms of $F_i$) and whether the observation is from a period after the flood ($T_t$=*1,* observation from 2014 or 2015). In the conditional logit estimations, households which do not switch in terms of *CCB* or which only participate once are omitted.

# CHAPTER 3.

# THE (UN-)INTENDED CONSEQUENCES OF LABELS IN VOLUNTARY CARBON OFFSETTING PROGRAMS: EVIDENCE FROM A FIELD EXPERIMENT AMONG FIRMS

*Joint work with Martin Kesternich, Michael K. Price, Kathrine von Graevenitz*

**Abstract**

A growing literature explores individual responses to carbon offsetting programs as a means for pro-environmental behavior. However, it is not clear to what extent these findings are informative about firm behavior. Building upon theoretical insights, we provide field-experimental evidence on the role of pro-environmental signaling for voluntary carbon offsets among firms. We collaborate with a courier service company from Poland, which mainly offers business-to-business deliveries booked through a web-shop. Our experimental setting enables the sender of a delivery to offset the related carbon emissions by paying a price add-on while we vary the opportunity to signal this pro-environmental act to the receiver of the delivery on the label of the delivery. Based on more than 5,600 orders from 124 unique business clients, we find that senders are not more likely to enroll in the carbon offsetting program if they can signal the pro-environmental behavior to the receiver. In contrast, if senders have a choice, more than half of those who enroll in the offsetting program prefer to keep the signal private. Our results suggest that firms' motives for carbon offsetting are multidimensional and that the signal to business partners may not be desirable. A program designer aiming to increase the share of carbon neutral shipments should allow for flexibility on whether to disclose this information.

## 3.1 Introduction

Mitigating the dangerous consequences of climate change is one of the biggest challenges of our generation. An effective global solution has not yet been forthcoming, and the policy response in wide parts of the world remains a patchwork of uni- or multilateral initiatives at the regional or national level. Even in highly developed regions, such as Europe, most countries so far remain unlikely to reduce their greenhouse gas emissions (GHGs) sufficiently to reach their national climate targets through formal institutions only. At the same time, climate change has come to be perceived as a severe issue within societies worldwide as demonstrated by the Fridays for the Future demonstrations and related movements, which suggests that citizens are prepared to further engage in voluntary approaches to reduce GHG emissions.

From a consumer perspective, one particular strategy to enable mitigation of GHG emissions involves improving transparency of embodied emissions in different fields of consumption, including housing, clothes, food, and mobility. Through coherent and stringent certification schemes and labels, consumers can differentiate and choose between different varieties (with respect to the embodied GHG emissions) of the same good or service. However, not all kinds of products and services are currently available in a carbon neutral version and in some cases a carbon neutral supply comes with prohibitively high abatement costs. In such cases, environmental programs for voluntary carbon compensation offer an essential opportunity to reduce GHG emissions indirectly. To design such programs effectively, information about the underlying motives of firms and their beliefs regarding customers' perceptions are of utmost importance. Two sources of motivation seem to be particularly relevant: First, some firms engage in voluntary carbon offsetting programs for motives that can be attributed to *insider-oriented* Corporate Social Responsibility (CSR) activities reflecting a delegated philanthropy on behalf of stakeholders like a firm's owner, employees or shareholders (Bauman & Skitka, 2012; Bénabou & Tirole, 2009). Secondly, the bundling of a private and a public good can be used as a mechanism for vertical market differentiation to meet preferences of final customers that explicitly demand (and are willing to pay a price add-on) a firm to engage in philanthropy on their behalf (Bénabou & Tirole, 2009). As CSR activities in this context are directed at target groups outside a firm, we will refer to this channel as *outsider-oriented*.

This study aims to improve our understanding of the role of these two channels for voluntary carbon-offsetting in a business-to-business context and to inform businesses and policymakers about the importance of specific design choices, which target the role of (mandatory) information disclosure. To do so, we collaborated with a courier service company from Poland, which mainly offers business-to-business deliveries booked through a web-shop. Our experimental setting enables the sender of a delivery to offset the related carbon emissions by paying a price add-on. We vary the opportunity to signal this pro-environmental act to the recipient of the delivery on the delivery label. In total, we observe 5,676 orders from 124 unique clients. Across all orders, 11.6% of all deliveries were sent in a carbon neutral way. If senders have a choice, more than half of those who enroll in the offsetting program prefer to keep the signal private. Our findings suggest that a mandatory signal may lead to a crowding-out of voluntary engagement. A program designer aiming to increase the share of carbon neutral shipments should allow for flexibility on whether to disclose this information.

The Courier, Express, and Parcel (CEP) services sector does not only provide an excellent surrounding for our experimental setup as it includes diverse interactions (short- as well as long-term) of firms from several economic sectors and, therefore, different motives for CSR activities. It is also a sector with rising emissions trends. Growth rates in Courier, Express, and Parcel services in Europe are expected to amount to 13% per year (based on estimations between 2013-2016, European Commission, n.d.) in part driven by increases in online shopping. This holds both for large European economies like Germany, where Courier, Express, and Parcel services are expected to grow to 4.3 billion deliveries per year in 2022 (compared to 3.35 billion in 2017) (BIEK 2018) but also for smaller economies like Poland (366 million deliveries in 2018, 209 million in 2014) (European Commission, 2020) where we conduct our field experiment. As a direct consequence of these market developments, road traffic volumes and emissions are expected to increase further. In 2018, lightweight duty trucks, which are often used for the 'last mile' deliveries, produced a total of 8 Mio t $CO_2$ or 13% of the total GHG emissions from transport in Poland (European Environment Agency, 2020). Comprehensive market penetration of non-fossil fuel-based transportation modes for Courier, Express, and Parcel services such as E-Scooters is feasible from a long-term perspective. However, in the short run, $CO_2$ offsets provide an effective way to compensate emissions due to the current composition of transportation fleets by reductions in emissions elsewhere. Also, there is evidence of a rising awareness of carbon offsetting in mobility or transport choices on the demand side. Stated preference approaches indicate that 47% of European travelers have a positive individual willingness to pay for the compensation of emissions resulting from their air travels (Brouwer, Brander, & Van Beukering, 2008) and 23% of people purchasing new vehicles are willing to offset $CO_2$ emissions (Lange & Ziegler, 2012). Experimental evidence suggests similar effect sizes, with about 30% of customers of long distance bus rides being willing to pay an add-on for a carbon neutral ticket (Kesternich, Löschel, & Römer, 2016).

The paper proceeds as follows: In Section 3.2, we discuss the related literature. In Section 3.3, we describe our theoretical considerations and the experimental design as well as its implementation. Section 3.4 presents results from the experiment, followed by a discussion of the underlying channels in Section 3.5. We conclude in Section 3.6.

## 3.2 Related Literature

From an economic point of view, products with carbon offsetting are classified as impure public (green) goods where individual contributions to a public good are explicitly tied to the own harm-related behavior (Cornes & Sandler, 1984; Kotchen, 2009). Recent theoretical work on impure public goods by Lai et al. (2017) suggests that bundling (e.g. linking sales to charitable contributions) enables profit-maximizing producers to differentiate their product, to attract new customers (i.e., those with a high valuation for the public good) and to relax price-competition. In their model, the decision to provide an impure public good goes hand in hand with profit maximization. This is in line with other theoretical work by Bagnoli and Watts (2003) stressing that the types of public goods provided are biased toward consumers with high participation value. Product bundles in the form of cause-related products can increase profits even through spillover effects on other products in a firms' product portfolio (Krishna & Rajan, 2009). An important assumption in this context is that customers perceive and reward product differentiation adequately (i.e., the number of customers with a high willingness to pay for the public good is sufficient).

While there is growing evidence exploring the response of individual end-users to green goods (e.g., Engelmann, Munro, and Valente 2017; Feicht, Grimm, and Seebauer 2016; Lange, Schwirplies, and Ziegler 2017b; Munro and Valente 2016), there is a lack of empirical evidence on how firms respond to green programs such as carbon neutral services. This particularly attributes to business-to-business decisions where private consumers with potentially heterogeneous environmental preferences are at the end of the supply chain. Various potential reasons exist why decisions on green goods within firms may differ from those at the individual level. First, the decision making process is much more complex as it usually comprises a series of single nested decisions or group decisions. Second, while strict profit-maximizing behavior on an individual level is often accompanied by other factors such as social norms or self-image concerns, less is known about the potential impacts of social preferences of firm owners or employees deciding on behalf of their employer. A related point concerns potential principal-agent issues as the employee (agent) may fail to internalize the employer's (principal) objective function. As a result, it is not clear to what extent research findings from analyses on the household level provide informative insights for firm behavior.

Empirical evidence on firms engaging in green goods or services is usually closely related to Corporate Social Responsibility (CSR) activities in general. Previous research has mainly focused on aspects targeting the willingness to pay of participants for pro-social behavior in a broader sense, competitiveness effects, and the financial benefits for the firms (see

Kitzmueller and Shimshack 2012 for an overview). In line with the theoretical findings by Lai et al. (2017) a recently conducted international cross-firm analysis shows that competition enhances firms' investments in CSR as a strategy for strengthening relationships with workers, suppliers, and customers (Ding, Levine, Lin, & Xie, 2020). A series of lab experiments explore sellers' investments in CSR under price competition (Feicht et al., 2016; Pigors & Rockenbach, 2016). An important insight from this research is that firms' CSR activities play a role in consumers' decisions and is taken into account in a competitive setting, at least as long as the price premium for social responsibility is not too high. This strand of literature provides the basis for our *outsider-oriented channel* for offsetting. However, communication of these CSR activities seems to be an essential requirement for a positive effect of CSR for firms: for firms with high advertising expenditures, CSR and firm value are positively related (Servaes & Tamayo, 2013). Transparency about responsibility activities (Buell & Kalkanci, 2020) as well as consumers' participation in firms' CSR initiatives (Du, Bhattacharya, & Sen, 2011) motivate consumer sales.

With regard to the *insider-oriented channel*, a further strand of literature investigates the effect of CSR activities on employees. CSR has been found to raise employee satisfaction, reducing, e.g., employee turnover and increasing the quality of employees (Bauman & Skitka, 2012; Bénabou & Tirole, 2009; Rodermans, 2020). On the other hand, (List & Momeni, 2020) report that CSR activities of firms can increase shirking at least among short-term employees due to moral licensing. That is, while CSR activities by firms are often used as pro-social signals, they may provoke evasive responses. However, this motive is out of scope for this paper as we do not observe employees' actions beyond the offsetting decision.

We contribute to this literature by examining two main channels through which firms may decide on their CSR engagement. For this end, we use an experimental approach to document behaviors (Roth, 1993) that are not yet comprehensively reflected in existing theory. In this sense, we use a field experiment as a descriptive method (Card, DellaVigna, & Malmendier, 2011) to disentangle insider- and outsider-oriented motives in a real-world setting.

## 3.3 Experimental design and conceptual framework

In this section, we describe the underlying rationale of the experiment and derive the different treatments. We further describe the implementation of the field experiment.

As discussed in the previous section, our field-experimental design is guided by recent theoretical advances in the commodity literature on bundling to stimulate the private provision of a public good. As Lai et al. (2017) argue, an important channel through which profit-maximizing producers benefit from linking sales to charitable contributions is that they can attract additional customers that have a high value for the public good and, consequently, are willing to pay a mark-up to the conventional product price. Additionally, the bundling of a private and a public good can be used as a mechanism for vertical market differentiation. Both cases result from the fact that customers have some demand (and a corresponding Willingness To Pay (WTP)) for firms to engage in philanthropy on their behalf (Bénabou & Tirole, 2009). As CSR activities in this context are directed at target groups outside a firm, we will refer to this channel as *outsider-oriented*. In contrast to that, *insider-oriented* CSR activities are primarily motivated by delegated philanthropy on behalf of stakeholders or insider-initiated corporate philanthropy (Bénabou & Tirole, 2009). This means, these activities can reflect the intrinsic values of the firm's owner, its employees or shareholders (Bauman & Skitka, 2012). These values may in part be influenced by social norms expressed by policymakers or the general public.

Although lab-experimental and theoretical research suggests that these channels are decisive for firms' engagement in CSR, they have not yet been studied in combination in the field. To analyze which role these channels play for the firms' engagement in CSR, we implement a field experiment where we observe a firm's decision on bundling a normal business activity with a pro-environmental activity, i.e., a CSR activity. We vary the firm's ability to signal this pro-environmental activity. This variation allows us to differentiate between insider and outsider-orientation as the channels driving a firm's decision.

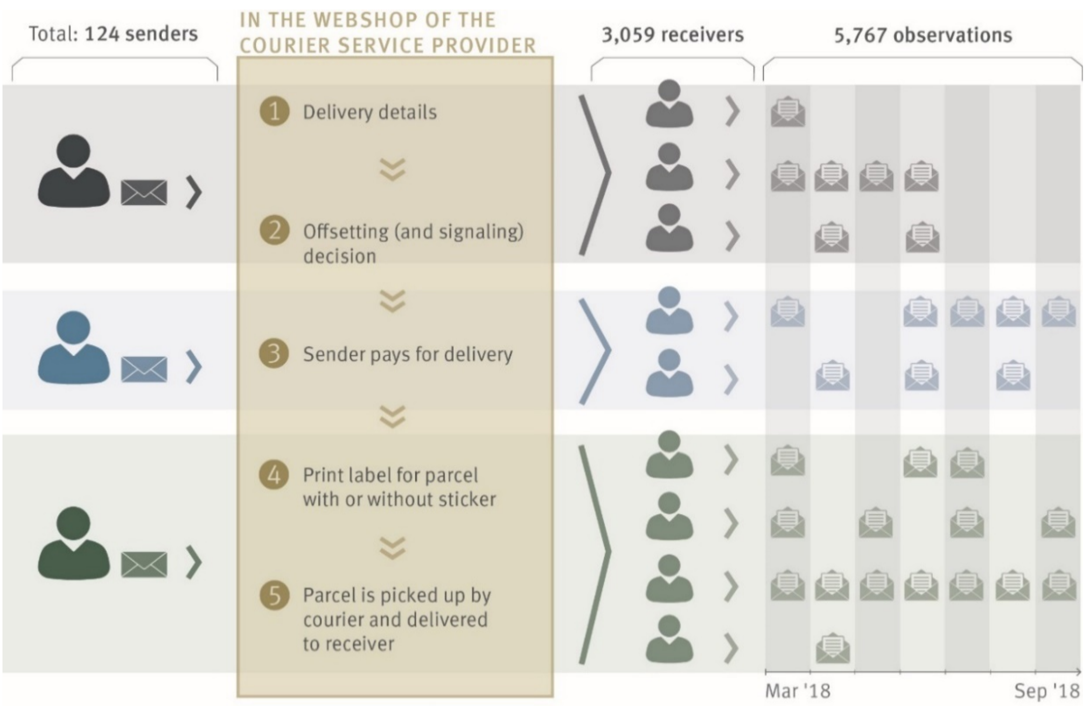
We study a situation in which firm $B_1$ has an (intermediate) good delivered to either a business partner (firm $B_2$) or a customer (C). $B_1$ decides whether to send this delivery in a standard way or a $CO_2$ neutral way, i.e., whether to bundle it with a pro-environmental activity. This specific situation[5] allows us to observe firm $B_1$'s initial decision to bundle a private good with a public good. $B_1$ observes the receiver's identity and holds a belief about the receiver's willingness to pay for the public good. The beliefs about the receiver's WTP might be formed based on knowledge about the receiver's geographical location, the economic sector, or the receiver's customers.[6] Based on the sender's beliefs about the WTP of the receiver, the sender chooses optimally to offset or not offset the emissions associated with the shipment.

[5] In contrast to the standard situation where the customer or business partner chooses from a variety of heterogenous products which differ in the associated $CO_2$ emissions.

[6] The same channels (insider- and outsider-oriented) also affect the decisions of a business partner ($B_2$), who purchases an intermediate product from $B_1$. He might pass the signal on to his customers. This means that receiver's WTP includes an assumption about the WTP of the end consumer at the end of the supply chain.

The experiment was conducted in cooperation with a Polish delivery service provider as part of its standard online ordering process. It took place between March 27 and September 30, 2018. The clients of the delivery service provider purchased a delivery service in a web-shop. They take on the role of the firm $B_1$ from the situation described above and we will refer to them as the 'senders' in the following. Their delivery was then picked up at a specified location and brought to the destinations (the 'receivers'). An overview of the decision situation is provided in Figure 3.1.

Figure 3.1: Overview of the sender receiver relationship and the ordering process




*Notes:* Every sender who purchased a delivery in the web-shop of the courier service provider had to go through the steps (1-5) of the ordering process listed in this figure. We observe a total of 124 senders, who have something delivered to a total of 3,059 receivers.

The experimental intervention was integrated into the standard ordering process in the web-shop as the last step before payment. We carried out three treatment variations, which were all implemented simultaneously and at the same stage of an identical online ordering process. Participants were assigned to one of the three treatments following a randomization procedure, building upon their unique customer IDs. The randomization was implemented by the courier service provider[7]. Participants remained within the same treatment over the entire intervention period (between-subjects design). In all treatment variations, senders had

[7] The random assignment was performed with the following code for existing clients: https://www.postgresql.org/docs/9.5/static/functions-math.html. New clients which became customers to the courier service provider during the ongoing experiment were assigned to treatments using the excel function randbetween(1;3). In June a relatively high number of new customers entered the web-shop. At this time the courier service provider launched a new website and introduced a small fee for orders via phone.

the opportunity to voluntarily offset carbon emissions by paying a price add-on on the delivery costs. There was no pre-set default. Both the underlying carbon emissions (in kg) and the price premium (in Zloty) were stated to the sender. There was also the opportunity to reveal further information on the carbon offsetting program by clicking on a link (see details in the Appendix to Chapter 3). We calculated the price add-on based on corresponding $CO_2$ emissions on a delivery-specific basis, including characteristics such as distance, weight, type of service (within a city, national or international), and transportation vehicles (bike, car, train) in close collaboration with myclimate, an independent international Non-governmental organization offering carbon compensation via climate protection projects. The offsetting price for 1 t of $CO_2e$[8] amounted to 15 € (or 63 PLN), which was the then current market price for carbon compensation. All emission values included a lump sum for overhead emissions (covering emissions from heating office spaces, office supplies, etc.), calculated based on actual historical data from the courier service company. A detailed description of the calculation can be found in the Appendix.

To be able to disentangle the behavioral patterns of the insider-oriented and outsider-oriented channels, we vary whether the offsetting decision was kept private for the sender or was disclosed to the receiver of the delivery. As the signal of carbon offsetting, we introduce a specifically for this experiment designed emblem. Depending on the treatment and the sender's decision, this emblem is part of the standard label or waybill, which senders have to print out by themselves and attach to their delivery. Thus, the decision on the $CO_2$ neutral delivery can be inferred by the receiver from this signal on the label. The sender had to print this label irrespectively of her offsetting decision. This means the sender did not incur additional transaction costs or impacts on the environment in case of offsetting. The emblem indicates that the delivery was sent carbon neutral by stating: "*The sender cares about the environment - your shipment is emission-free*." A screenshot of the whole label is provided in the Appendix. Before the order was complete, customers received a preview of the label (irrespectively of whether offsetting was selected or not). We introduced the label preview to ensure participants were fully informed about the consequences of their decision, i.e., the kind of information that was or was not transmitted with the label. A screenshot of this preview stage is provided in the Appendix.

We first consider the baseline condition ('no signal', T1) without any opportunity to add the emblem. That is, the decision on carbon offsetting remained private for the sender in any case. Senders were asked to select one of the following statements: "*Yes, I want to offset* $CO_2$

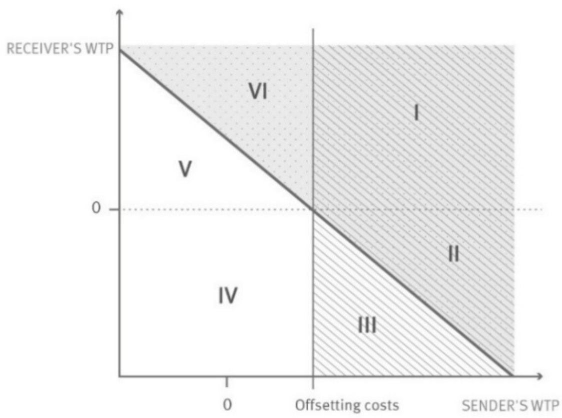
[8] Carbon dioxide equivalents is a scale to measure and subsume the effects of different gases on global warming. For reasons of simplicity we refer to the group of these gases as $CO_2$ or carbon emissions in the following.

*emissions*" or "*No, I do not want to offset $CO_2$ emissions*". (Screenshots of the decision situations of all three treatments are provided in the Appendix, including translations.) This treatment was designed to identify decisions made based on the *insider-oriented channel*: A sender chooses the $CO_2$-neutral option to meet the intrinsic pro-environmental value orientations of herself, the owner of the firm, its employees, or shareholders. Thus, the decision in the no signal-treatment is not driven by beliefs about the WTP of the end consumer.

In the 'mandatory signal' treatment (T2), the emblem was automatically attached to the label of the delivery, when the sender paid for the carbon offsetting. Thus, the statements were the same as in the baseline, but the description included the information that the decision would be indicated on the delivery. This treatment requires senders to form a belief about the receiver's perception of the signal and the offsetting. The receiver is either a business partner who can pass on the signal to the end consumer or the end consumer himself. Both the *insider-* and the *outsider-oriented channel* influence firms' decision-making in this treatment. The direction of the effect depends on the strength and the direction of the *outsider-oriented channel*. Thus, the total effect of the mandatory signal on offsetting rates compared to a setting without a signal can go either way: The signal can either increase (through 'crowding-in') or decrease (through 'crowding-out') offsetting rates.

The "optional signal" treatment (T3) allows senders to decide about the offsetting and at the same time whether to add the emblem to the label. Accordingly, the following three options appeared: "Yes, I want to offset *$CO_2$ emissions and please mark it on the waybill/label", "Yes, I want to offset $CO_2$ emissions, but I don't want to report this on the waybill/label" and "No, I do not want to offset $CO_2$ emissions."* This treatment serves a dual purpose. It was designed as a consistency check for the observations made in the other treatments. Furthermore, T3 allows us to directly distinguish between offsetting senders that believe their receivers to have a positive attitude towards carbon offsetting from those who believe their receivers to have a negative attitude.

The comparison of offsetting rates between the no signal and the mandatory signal treatment reveals the role played by the *outsider-oriented channel*. If senders believe receivers have a sufficiently high WTP for carbon offsetting, senders should offset more often with the mandatory signal than without the signal. This means the effect of the mandatory signal would result in a crowding-in of additional offsetting. If senders believe receivers have a negative attitude, they should offset less often in the mandatory signal treatment than in the no signal treatment. As a result, the mandatory signal could lead to a crowding-out of offsetting. Figure 3.2 displays the optimal offsetting behavior depending on the sender's and the receiver's WTP in each treatment. It illustrates the effects of the two channels in our experimental setting, such that the sender's WTP for the offsetting is motivated by the *insider-oriented channel* and the sender's belief about the receiver's WTP represents the *outsider-oriented channel*. In the no-signal treatment (T1), the sender offsets when her WTP exceeds the offsetting costs k (regions I + II+ III). In the mandatory signal treatment (T2), the sender offsets when the sum of her WTP and the WTP of the receiver is larger than k. The comparison of the offsetting rates in these two treatments reveals the crowding-in and crowding-out effect. Region III describes those situations in which there is offsetting in T1 but not in T2. Here the sender's WTP is high enough for her to offset in the absence of a signal (S>k), but the receiver's WTP is so low, that she would not offset in case of a mandatory signal (R<k-S). This illustrates the crowding-out effect of the signal. Region VI describes situations in which offsetting does not take place in T1 but in T2. Although the sender's WTP is not high enough to offset without signal (S<k), she offsets when the signal becomes mandatory as the receiver's WTP is high enough to reach k (S+R>k). This illustrates the crowding-in.




| TREATMENT | CONDITION | SENDER'S OFFSETTING DECISION | REGIONS IN FIGURE |
|---|---|---|---|
| in T1 | if S>k | offsetting | I+II+III |
| in T2 | if S+R>k | offsetting | VI+I+II |
| in T3 | if S+R>k and R>0 | offsetting with signal | I+VI |
| | if S>k and R>0 | offsetting without signal | II+III |

S: Sender's WTP
R: Receiver's WTP
k: Offsetting costs
III: Crowding-out
VI: Crowding-in
— k = S + R

Figure 3.2: Schematic illustration of offsetting rates depending on the interaction between the sender's WTP for the offsetting motivated by the insider-initiated channel and the sender's belief about the receiver's WTP representing the outsider-oriented channel.

We cannot make a clear prediction regarding the direction of the effect of the signal as it depends entirely on the sender's beliefs about the receiver's WTP for offsetting. For example, if the strength of the crowding-out was larger than the crowding-in, overall offsetting would be higher in T2 compared to T1. Given that senders can decide freely whether or not to signal their offsetting, the offsetting rate in T3 should be at least as high as the highest offsetting rate in T2 (VI + I + II) or T1 (I + II + III), depending on whether crowding-in or crowding-out determines senders' decisions.

## 3.4 Results

We observe a total number of 5,767 orders[9] by 124 unique clients during our field phase through the web-shop of the courier service provider. Across all observations, senders faced an average carbon footprint of 5.1 kg $CO_2$e for their delivery, resulting in a price premium of 0.33 PLN (0.074 EUR = 0.083 USD), which accounts for about 0.78% of the average total shipping charges per order. Throughout the experiment, we observe more than one order for about 88% of all participating senders. Most orders were sent within the same city (as city service). Individual city services accounted for 6.56 kg $CO_2$e on average. In comparison, carbon footprints for national (1.86 kg $CO_2$e) and international services (0.68 kg $CO_2$e) were considerably lower due to larger capacities for combining different deliveries with similar destinations (see Table 3.1).

Table 3.1: Delivery specifications for each type of service (mean values)

| All orders | | | | | |
|---|---|---|---|---|---|
| Service | Parcel weight (kg)[b] | Distance (km)[c] | $CO_2$ Emissions (kg)[d] | Offsetting price (PLN) | Observations |
| National service | 6.9 | 379 | 1.86 | 0.12 | 1,748 |
| City Service[a] | 7.15 | -- | 6.56 | 0.43 | 3,983 |
| International Service | 6.08 | 444 | 0.68 | 0.05 | 36 |
| | | | | | 5,767 |

*Notes:* Mean values of the delivery specifications for each type of service. Deviations from the total number of observations for each service (indicated in the very right column) due to data gaps are indicated for the variables concerned. Data gaps result from the fact that some data were not collected for all observations. [a]Distance was not relevant for the calculation of emissions and offsetting prices in city services. [b]3,980 observations for city service. [c]3,981 observations for city service. [d]3,978 observations for city service; 35 observations for international service.

Concerning the randomization process, which as previously explained, was executed by the courier service company, we observe more senders in the mandatory (N=45) and optional signal treatment (N=54) compared to the no signal treatment (N=25), resulting in variation in the number of orders across the different treatments (2,245 orders in T2, 2,488 orders in T3 and 1,024 orders in T1, see Table 3). As a randomization check, we first investigate potential differences in observable characteristics across treatments for the first order per sender. As shown in Table 3.2 we observe differences in shipment characteristics such as parcel weight, distance and shipment costs, which might be due to the high variance of the relatively small number of unique senders. Most notably, we observe quite similar numbers

[9] A complete protocol on the treatment of the raw data is provided in the Appendix.

of average orders per sender ID across treatments (41.4 in T1, 49.9 in T2, and 46.1 in T3), suggesting that treatment assignment did not substantially affect return rates (see Table 3.3)[10]. However, significant differences can be observed for the emissions values and offsetting prices in T2 compared to T1. We control for these characteristics in our analyses below.

Table 3.2: Summary statistics (mean and sd) of observable characteristics for first orders per sender

| First orders | | | | |
|---|---|---|---|---|
| Variable | No signal | Mandatory signal | Optional signal | All observations |
| Distance for national and international services (km)[a] | 374<br>(150) | 414<br>(166) | 338<br>(135) | 371<br>(149) |
| Final costs for senders (shipment costs and offsetting (PLN)) | 28.86<br>(21.39) | 33.8<br>(37.9) | 24.97<br>(24.75) | 27.95<br>(29.79) |
| Shipment costs excl. offsetting (PLN) | 23.84<br>(21.4) | 33.76<br>(37.94) | 24.96<br>(24.75) | 27.93<br>(29.8) |
| $CO_2$ Emissions produced (kg) | 1.78<br>(5) | 4.85<br>(11.9) | 2.84<br>(4.19) | 3.35<br>(8.03) |
| Offsetting price (PLN) | .12<br>(.32) | .31<br>(.75) | .19<br>(.27) | .22<br>(.51) |
| Parcel weight (kg)[b] | 5.35<br>(19.78) | 23.31<br>(76.68) | 5.02<br>(12.38) | 11.53<br>(47.56) |
| Info button request (%)[c] | 71<br>(49) | **11**<br>(32) | **4**<br>(2) | 16<br>(37) |
| Average time spent for offsetting decision (sec)[d] | 15<br>(12.52) | 38.94<br>(67.58) | 29<br>(50.4) | 30.71<br>(54) |
| Calculated off_emissions (kg)[e] | 3.56<br>(5) | 7.25<br>(10.93) | 5.86<br>(5.7) | 5.89<br>(7.95) |
| Calculated off_prices (PLN) | .22<br>(.31) | .54<br>(.76) | .36<br>(.36) | .4<br>(.54) |
| Observations | 25 | 45 | 54 | 124 |

*Notes:* Mean values of each variable for each treatment as well as for all observations and the standard deviations in brackets. Calculated for senders' first orders only. Deviations from the total number of observations (124) due to data gaps are indicated for the variables concerned. Data gaps result from the fact that some data were not recorded for all observations. Values in bold letters indicate a statistical significant difference from the baseline treatment "no signal" at a significance levels of 5 percent (chi2 tests). [a]The distance was not calculated for city services, 47 observations. [b]122 observations. [c]49 observations. [d]49 observations. [e]122 observations.

[10] In line with our expectations, subjects in the mandatory and optional signal treatments spent more time (39 and 29 seconds, respectively) for their first booking than those in the no signal treatment (15 sec) indicating that the signaling of the decision gave rise to more deliberation. However, the deliberation time declines substantially in repeated orders to 6-8 seconds depending on treatment. If we include all observations (i.e. multiple orders per sender) we also see that differences in observable characteristics become much smaller (Table 3.3).

Table 3.3: Summary statistics (mean and sd) of observable characteristics for all orders per sender

| All orders | | | | |
|---|---|---|---|---|
| Variable | No signal | Mandatory signal | Optional signal | All observations |
| Orders per client ID (unique senders) | 41.36<br>(60.86) | 49.9<br>(87.9) | 46.1<br>(73.52) | 46.5<br>(76.3) |
| Distance for national and international services (km)[a] | 396<br>(125) | 393<br>(141) | **366**<br>(140) | 380<br>(137) |
| Share of international services of all orders in the respective treatment (per cent)[b] | 0.39 | 0.62 | 0.72 | 0.62 |
| Share of national services (per cent)[c] | 45.36 | 18.66 | 34.57 | 30.31 |
| Share of city services (per cent)[d] | 54.26 | 80.71 | 64.71 | 69.07 |
| Final costs for senders (shipment costs and offsetting (PLN)) | 22.2<br>(15.91) | 21.1<br>(16.18) | 25.37<br>(24.81) | 23.13<br>(19.89) |
| Shipment costs excl. offsetting (PLN) | 22.18<br>(15.92) | 21.05<br>(16.18) | 25.33<br>(23.8) | 23.1<br>(19.89) |
| Emissions produced (kg)[e] | 4.42<br>(6.17) | **5.66**<br>(6.05) | 4.87<br>(5.14) | 5.1<br>(5.72) |
| Offsetting price (PLN) | .28<br>(.4) | **.37**<br>(.38) | .31<br>(.32) | .33<br>(.36) |
| Calculated off_emissions (kg)[f] | 4.3<br>(5.8) | **6.45**<br>(5.99) | **6.43**<br>(6.29) | 6.05<br>(6.15) |
| Calculated off_prices (PLN) | .28<br>(.38) | **.42**<br>(.41) | .41<br>(.41) | .39<br>(.41) |
| Parcel weight (kg)[g] | 2.9<br>(7.42) | 11.1<br>(38.23) | 5.19<br>(19.14) | 7.08<br>(27.34) |
| Info button request (per cent)[h] | .009 | .003 | .003 | .004 |
| Average time spent for offsetting decision (sec)[i] | 6.4<br>(10.1) | 7.82<br>(31.32) | 6.38<br>(27.41) | 6.97<br>(26.93) |
| Observations | 1,034 | 2,245 | 2,488 | 5,767 |

*Notes:* Mean values of each variable for each treatment as well as for all observations and the standard deviations in brackets. Calculated for senders' first orders only. Deviations from the total number of observations (5,767) due to data gaps are indicated for the variables concerned. Data gaps result from the fact that some data were not recorded for all observations. Values in bold letters are significantly different from the baseline treatment "no signal" at a significance level of 5 percent (simple linear regression, standard errors are clustered at sender level). [a]The distance was not calculated for city services; 1,784 observations. [B]36 observations. [c]1,748 observations. [d]3,983 observations. [e]5,761 observations. [f]Emission values and offsetting prices were recalculated based on parcel information such as weight and distance of delivery as some of the values were known to be miscalculated In the beginning if the experiment, 5 ,747 observations. Calculated values are used as a comparison and verification only since senders based their decisions on the information which was actually given to them. Further information on data processing is provided in the appendix. [g]Prices were recalculated; 5,764 observations. [h]4,334 observations. [i]4,331 observations.

We start our analysis by focusing on the first orders per customer ID and then extend the discussion to all orders to study long-term effects.

### 3.4.1 First orders

During the first order, 13% of all deliveries are offset (see Table 3.4). While the participation rate in the offsetting program is highest in the no signal treatment (16.0%), it reaches slightly lower levels in both mandatory signal (11.1%) and optional signal (13.0% = 7.4% + 5.6%). These descriptive insights are supported by the results of a series of binary logit regressions where the dependent variable *offsetting* equals one if a sender opts for the green delivery (see Table 3.5 where we report average marginal effects). Across all model specifications, we observe no significant differences between treatments[11]. There are also no differences in the characteristics of the deliveries such as weight, which are included as control variables (Table 3.5). We do not observe senders to be more likely to enroll in the carbon offsetting program if they can signal the pro-environmental behavior to the receiver. The evidence, though not statistically significant, suggests that offsetting rates decrease when the signal is introduced – even when it is optional.

Table 3.4: Offsetting rates for first orders across treatments

| | Offsetting decisions | | | |
|---|---|---|---|---|
| Treatment | No offsetting | Offsetting without signal | Offsetting with signal | Total |
| No signal | 21<br>(84.0%) | 4<br>(16.0%) | -- | 25<br>(100%) |
| Mandatory signal | 40<br>(88.9%) | -- | 5<br>(11.1%) | 45<br>(100%) |
| Optional signal | 47<br>(87.0%) | 4<br>(7.4%) | 3<br>(5.6%) | 54<br>(100%) |
| Observations | 108<br>(87.1%) | 8<br>(6.5%) | 8<br>(6.5%) | 124<br>(100%) |

[11] These results are confirmed by two-sample t-tests on mean differences, which are provided in the Appendix .

Table 3.5: Logit regression on offsetting rates in first orders

| | (1) Offsetting | (2) Offsetting | (3) Offsetting | (4) Offsetting | (5) Offsetting | (6) Offsetting | (7) Offsetting |
|---|---|---|---|---|---|---|---|
| Mandatory signal | -0.0489 | -0.0380 | -0.0462 | -0.0582 | -0.0537 | -0.0601 | -0.0569 |
| | 0.5742 | 0.6596 | 0.6025 | 0.5353 | 0.5610 | 0.5439 | 0.5779 |
| Optional signal | -0.0304 | -0.0280 | -0.0364 | -0.0578 | -0.0517 | -0.0691 | -0.0703 |
| | 0.7252 | 0.7375 | 0.6723 | 0.5254 | 0.5631 | 0.4609 | 0.4628 |
| shipping_costs[a] | | -0.0019 | -0.0031 | -0.0016 | -0.0018 | -0.0019 | -0.0018 |
| | | 0.3367 | 0.2511 | 0.5915 | 0.5566 | 0.5577 | 0.5589 |
| Offset price[b] | | | 0.0848 | -0.1164 | -0.1290 | -0.1324 | -0.1235 |
| | | | 0.4047 | 0.6783 | 0.6640 | 0.6557 | 0.6820 |
| ratio offset/shipping[c] | | | | 6.0486 | 5.4033 | 5.6589 | 5.5473 |
| | | | | 0.3157 | 0.3915 | 0.3717 | 0.3895 |
| order_volume[d] | | | | | -0.0006 | -0.0007 | -0.0007 |
| | | | | | 0.3556 | 0.3302 | 0.3573 |
| CSR[e] | | | | | | -0.0300 | -0.0340 |
| | | | | | | 0.7026 | 0.6745 |
| Sector[f] | | | | | | | -0.0027 |
| | | | | | | | 0.9692 |
| N | 124 | 124 | 124 | 121 | 121 | 117 | 114 |

*Notes:* Dependent variable: 1 if sender offsets and 0 if sender does not offset. Margins and p-values. Baseline: no signal treatment. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]share of offsetting costs in total shipping costs, [d]number of orders per sender, [e]1 if firm communicates some of CSR activity on firm website (own elicitation), [f]1 if firm is part of sector M according to EU nomenclature: "Professional, scientific and technical activities" (data from Polish commercial trade register NIP or KRS). In the appendix we estimate the model (1) with the minimum sample of 114 respondents. It does not change the results. Results do not change. A joint test shows that we are unable to reject the hypothesis that effects of the mandatory and optional signal are identical (p=0.7772; Table S3.16 in the Appendix).

We pool these first insights by formulating our first observation:

**Observation 1:** During their first order, senders are not more likely to enroll in the carbon offsetting program if they can signal the pro-environmental behavior to the receiver. Moreover, when senders have the choice, they often decide against adding the pro-environmental signal to the delivery when they offset.

To check whether these first insights are driven by the initial reactions to the program only, we now take a closer look at all orders.

### 3.4.2 All orders

Table 3.6 shows the offsetting rates across treatments for all orders. The rate remains relatively stable and highest in the no signal treatment (17.7%). Remarkably, if the pro-environmental signal is mandatory, enrollment in the program drops to 8.0%. In the optional signal treatment, the overall participation rate is 12.3%. Still, fewer senders decide to signal their pro-environmental behavior (5.1%) compared to those who enroll but do not disclose it (7.2%) (see Table 3.6).

Table 3.6: Offsetting rates for all orders across treatments

| | Offsetting decisions | | | |
|---|---|---|---|---|
| Treatment | No offsetting | Offsetting without signal | Offsetting with signal | Total |
| No signal | 851 (82.3%) | 183 (17.7%) | -- | 1,034 (100%) |
| Mandatory signal | 2,066 (92.0%) | -- | 179 (8.0%) | 2,245 (100%) |
| Optional signal | 2,182 (87.7%) | 178 (7.2%) | 128 (5.1%) | 2,488 (100%) |
| Observations | 5,099 (88.4%) | 361 (6.3%) | 307 (5.3%) | 5,767 (100%) |

In line with our descriptive insights, a pairwise treatment comparison based on a two-sample t-test suggests that program enrollment in the no signal treatment is more likely than in mandatory ($p<0.01$) and the optional signal treatment ($p<0.01$) (results are provided in Table S3.13). We also find that the optional signal outperforms the mandatory signal treatment ($p<0.01$). However, these t-tests do not account for possible serial correlation between decisions made by the same sender. To control for the fact that decisions of the same sender are likely to be correlated and for the differing characteristics between individual orders, we conduct a binary logit regression with clustered standard errors (Table 3.7). The results support the observation that senders are not more likely to engage in carbon offsetting if their pro-environmental behavior can be disclosed. Instead, the point estimates suggest that senders are more reluctant to offset emissions if the pro-environmental signal is automatically transmitted, though these effects are not statistically significant[12]. This means our results show a tendency towards crowding-out of pro-environmental activities if a mandatory signal is introduced. The third treatment confirms this pattern of more offsetting without than with a signal. However, in the third treatment, the total offsetting rate is larger than in T2 but – contrary to our expectations – lower than in T1. We do not observe statistically significant differences in offsetting rates between the optional signal treatment and the other two treatments. If senders have a choice on whether to disclose their decision, for a substantial share of deliveries participants actively decide to avoid it.

[12] We further apply a multinomial logit model to identify drivers of the decision to add the signal to the label in the optional signal treatment (T3) but do not find any significant effects on the signaling.

Table 3.7: Logit regression on offsetting rates in all orders

| | (1) Offsetting | (2) Offsetting | (3) Offsetting | (4) Offsetting | (5) Offsetting | (6) Offsetting |
|---|---|---|---|---|---|---|
| Mandatory signal | -0.0972 | -0.0973 | -0.0883 | -0.0743 | -0.0752 | -0.0547 |
| | 0.2121 | 0.2135 | 0.2409 | 0.3258 | 0.3216 | 0.4554 |
| Optional signal | -0.0540 | -0.0531 | -0.0501 | -0.0404 | -0.0409 | -0.0160 |
| | 0.5100 | 0.5173 | 0.5287 | 0.6089 | 0.6070 | 0.8441 |
| shipping_costs[a] | | -0.0003 | 0.0002 | -0.0004 | -0.0005 | -0.0006 |
| | | 0.6937 | 0.7095 | 0.5710 | 0.4828 | 0.6007 |
| Offset price[b] | | | -0.0880 | -0.0165 | -0.0078 | -0.0424 |
| | | | 0.1097 | 0.7213 | 0.8582 | 0.5738 |
| City[c] | | | | -0.0671 | -0.0650 | -0.0013 |
| | | | | 0.2042 | 0.2269 | 0.9878 |
| ratio offset/shipping[d] | | | | | -0.2960 | -3.8510 |
| | | | | | 0.0836 | 0.3494 |
| Time stamps [sec.][e] | | | | | | 0.0001 |
| | | | | | | 0.3241 |
| N | 5767 | 5767 | 5767 | 5767 | 5716 | 4316 |

*Notes:* Dependent variable: 1 if sender does offset and 0 if sender does not offset. Margins and p-values. Standard errors are clustered at sender level. Baseline: no signal treatment. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]1 if type of service is city service and 0 if it is international or national services, [d]share of offsetting costs in total shipping costs, [e]time senders spent for decision making on offsetting. In the appendix we estimate the model (1) with the minimum sample of 4,316 observations. Results do not change. A joint test shows that we are unable to reject the hypothesis that effects of the mandatory and optional signal are identical (p=0.4007; Table S3.17 in the appendix).

We summarize these insights in our second observation:

**Observation 2:** Including all orders, enrollment in the carbon offsetting program does not increase if the pro-environmental activity can be signaled. If senders have a choice, more than half of those who opt for the offsetting program prefer to keep their decision private.

### 3.4.3 Minimum detectable effect size

For further evaluation of our results, we put them into the context of a power analysis. We determine the minimum detectable effect size (MDE), which is given as the smallest detectable difference between the proportions of two treatments for which the experiment will (correctly) reject the $H_0$ (= no difference in participation rates) (Burlig, Preonas, & Woerman, 2020). We calculate the minimum detectable effect size for the given number of first and all observations per sender while taking into account the respective offsetting rate of the baseline treatment (power=0.8 and α=0.05).

That is, for the first orders, given that $N_{1,2}$=70 and $p_1$=0.16, we principally could have observed a significant treatment effect for the mandatory (T2) or optional signal (T3) treatment (i.e., significantly more offsetting in T2 and T3 compared to the no signal treatment) if $p_2$ or $p_3 > 0.44$. Or, in other words, we can say that if a treatment effect between T1 and T2 or T3 exists, it is smaller than 28 percentage points. In our sample, the treatment effect between T1 and T2 or T3 is smaller than zero. The corresponding calculation for treatment effect size between T2 and T3 with $N_{2,3}$=99 leads to a minimal detectable difference of 20 percentage points. In other words, based on our study, we can say that if a statistically significant treatment effect between T2 and T3 exists, it is smaller than 20 percentage points.

For the whole sample, we estimate the minimum detectable effect without accounting for serial correlation as follows. With $N_{1,2}$=3,279, the $H_0$ can be rejected at the smallest difference between the proportions of 4 percentage points. The actual difference between T1 and T2 is 10 percentage points and between T1 and T3 is 5 percentage points. If there were no serial correlation, the differences between the offsetting rates in our experiment would likely not be due to small sample sizes.

## 3.5 Discussion

Our experimental results show the *insider-oriented channel* can play an important role in offsetting decisions. Even without the ability to signal the pro-environmental decision, firms decide to offset for 17.7% of all deliveries. If the signal is mandatory, the offsetting rate is lower than without a signal. This points in the direction of a crowding-out effect. Firms motivated by *insider-oriented* CSR activities might reduce their participation in the mandatory signal compared to the no signal treatment as they assume end consumers to lack a positive WTP for carbon compensation. Furthermore, if firms can choose whether to signal their pro-environmental decision to their receivers, they do not signal for the majority of offset deliveries. This is, they seem to avoid the signal actively. Information disclosure of pro-environmental behavior does not increase offsetting rates. Hence, there is no guarantee that the *outsider-oriented channel* plays a positive role in enhancing participation in voluntary carbon offsetting programs. When interpreting these observations, one has to keep in mind that the results are not statistically significant.

Instead of using the label to signal their offsetting activity, it is possible that firms already use other channels to communicate their CSR activities. It would be sufficient for firm $B_1$ to report her activities on sustainability in an aggregated manner on the firm website, the annual reports or in communication with the relevant shareholders for complying with both the insider initiated philanthropy as well as outsider-oriented motives. The limited data on the firms' CSR activities we could access online were included as control variables in the logit regressions. Firms with and without CSR communication did not differ in their offsetting behavior (Table 3.5).

The *insider-oriented ch*annel subsumes the motives of different agents within a firm. Suppose the offsetting is a corporate decision, but it is not to be disclosed. In that case, it might also be the case that firms engage in pro-environmental activities as an internal signal to their employees. Our results might also reflect the principal-agent relationship between an employee and the employer. If his individual intrinsic motivation drives an agent who uses his leeway in decision-making to decide upon the offsetting for environmentally friendly behavior, he might avoid the signal. He might not want to show off by signaling the pro-social behavior (Frey, 1997) or might want to limit further questions concerning their decision both by their employers and the receiver. However, these motives are out of the scope of our experimental setting as we do not observe which individual decides on what basis.

For the *outsider-oriented channel,* the firms' beliefs about the end consumer's WTP for offsetting play a decisive role. When the sender of a delivery decides on disclosing her carbon offsetting decision, she makes assumptions regarding the receivers' environmental attitudes. The observation that senders in the mandatory and optional signal treatments spent more time (39 and 29 seconds, respectively) for their first booking than those in the no signal treatment (15 sec) suggests that they reflect about the perception of the signal.

In our experiment, all senders are businesses from construction, marketing, tourism, etc. In some sectors such as e.g. construction, pro-environmental signals may play a minor role as price competition is usually stiff and direct contact with end-consumers who might value a green value chain remains modest. Therefore, if a sender in these business relationships decides to enroll in the carbon offsetting program at all, e.g. due to insider-oriented motives, she might tend to avoid a pro-environmental signal. Such a signal may be interpreted as indicating potential cost savings (e.g., through cheaper deliveries) for the receiver. This could generally be different for service sectors such as marketing or tourism, where there is closer contact with end consumers who might value a green value chain. However, our regression analysis (Table 3.5) does not indicate that firms in the sector "Professional, scientific and technical activities", which includes services such as marketing, are more likely to participate in the offsetting.

The minimum information a sender of a delivery has about her receiver is his address. She might form her belief about the receiver's attitude based on her assumption about the distribution of political views in the receiver's geographical environment. We look at the last presidential election results in Poland in 2020 (National Electoral Commission, 2020). The election results demonstrate a clear geographical polarization in the political orientation. People living in the north-western part of the country and in the urban areas of the eastern part mainly voted for the *Platforma Obywatelska* (PO) supported by *Partia Zieloni* (Poland's Green Party). We refer to them as more liberal. Those in rural areas in the south-east and center of Poland voted mainly for the center-right and EU-critical *Prawo i Sprawiedliwość* (PiS). We refer to them as more conservative. Based on the first two digits of the receivers' zip codes, individual regions were identified and categorized either as liberal or conservative.[13] In our sample, senders mainly come from areas where a larger share of votes was for the more liberal candidate. Receivers are more spread out in different areas. Among receivers there is, therefore, more variation in political orientation as proxied by electoral outcomes. We find that in the no signal treatment (T1), deliveries sent to receivers in more liberal areas are offset more frequently (13% vs. 24%). In the mandatory signal treatment (T2), deliveries sent to receivers in more liberal areas are also offset more frequently (10% vs. 19%). In the optional signal treatment (T3), those deliveries are offset more frequently, that are sent to receivers in more conservative areas compared to those in more liberal areas (25% vs. 17%). But, for receivers in more conservative areas the share of non-signaled offsetting is higher than signaled offsetting. On the other hand, for receivers in liberal areas the share of signaled offsetting is higher than non-signaled offsetting. These observations suggest that polarization may play a role even if the evidence is not conclusive. Crowding-out of insider-motivated offsetting in regions where conservatism is dominant might arise from firms' beliefs that end consumers' WTP is too low compared to the offsetting cost in these regions.

## 3.6 Conclusion

In the political context and society, one can observe a push towards environmental programs that foster voluntary contributions and commitments of firms as well as individuals and non-state actors. We set up an experiment to examine how to effectively incorporate communication of pro-environmental action into the design of voluntary programs. Our treatments mimic programs in use as they differentiate between settings that include a label or certification and those that do not. Building on theoretical insights, we have conducted a

[13] Depending on the majority election results, the regions were divided into two categories corresponding to the two largest political camps: (a) the conservative incumbent and new president Andrzej Sebastian Duda of the ruling PiS party (32 regions) and (b) the more liberal Rafał Kazimierz Trzaskowski of the Civic Platform (PO) (42 regions). If no majority election result could be determined for a region, it was not assigned to any political camp (22 regions). For our analysis, we further distinguish between the service types and exclude city services as there is no variation in political orientation among receivers of city services. We focus on "national services" where deliveries travel between regions.

field experiment to shed light on the importance of pro-environmental signaling for voluntary carbon offsets among firms. We collaborated with a courier service company from Poland, which mainly offers business-to-business deliveries booked through an online system. Our experimental setting enables the sender of a delivery service to offset the related carbon emissions by paying a price add-on while we vary the opportunity to signal this pro-environmental act to the delivery receiver through the label on the delivery. By varying the option to signal, we disentangle *outsider-oriented motives* for offsetting from those that are *insider-oriented*. In total, we observe 5,676 orders from 124 unique clients. Across all orders, 11.6% of all deliveries are sent in a carbon neutral way. We find that senders are not more likely to enroll in the carbon offsetting program if they can signal the pro-environmental behavior to the receivers. In contrast, if senders have a choice, more than half of those who enroll in the offsetting program prefer to keep the offsetting private. While the sender's economic sector has no significant effect on program participation, senders may (correctly or incorrectly) infer that end-consumers have a low Willingness To Pay for carbon neutral deliveries. This study provides insights into firms' motivations to voluntarily contribute to climate protection and with that contributes to the body of literature on the behavioral aspects in environmental policymaking (e.g., List and Price 2016).

From a policy perspective, the findings provide valuable insights into the design of effective measures. Our results suggest that mandatory pro-environmental signaling can cause crowding-out in some market settings. There seems to be demand for offsetting without a signal. Effective offsetting programs should, therefore, not include a signal by default. A program designer aiming to increase the share of carbon neutral deliveries should allow for more flexibility on whether to disclose this information.

## 3.7 Appendix to Chapter 3

In the following, we provide additional information regarding the experimental design and the randomization of subjects as well as on data cleaning and statistical analysis.

### *Experimental design*

The following figures show the screenshots of the website with the instructions during the ordering process as they were shown to the customers of the courier service provider. The decision screen (Figure S3.3) and the preview of the label, which was printed and attached to the parcels by the customers (Figure S3.4 and Figure S3.5), were part of the ordering process. Depending on treatment and decision, the label contained the emblem indicating the carbon offsetting decision. A larger version of the emblem is provided in Figure S3.6. Translations of the decision screens for each treatment are given below with exemplary numbers.

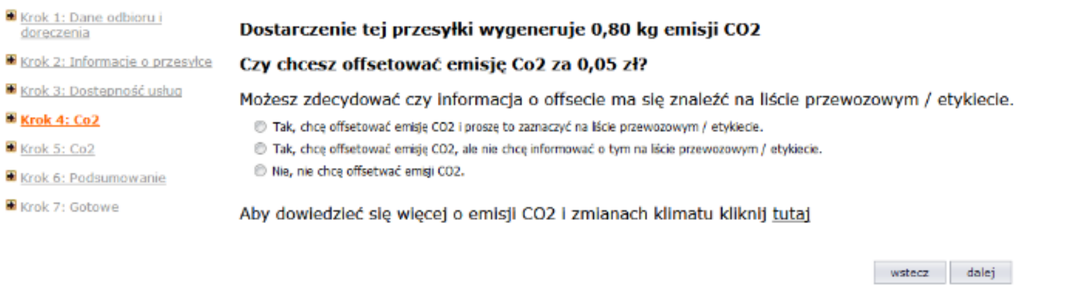



Figure S3.3: Screenshot of the choice treatment (T3), decision screen.


Figure S3.4: Screenshot of the preview of the label which in this case includes the emblem.


Figure S3.5: Preview of the label which in this case does not include the emblem.

T1. BASELINE CONDITION (offsetting without signal):

*The shipment will generate 6.92 kg of CO2 emissions.*
*Do you want to offset the Co2 emissions for 0.44 PLN?*

- *Yes, I want to offset CO2 emissions.*
- *No, I don't want to offset CO2 emissions.*

*To find out more about CO2 emissions and climate change click here.*

T2. MANDATORY SIGNAL TREATMENT:

*The shipment will generate 6.92 kg of CO2 emissions.*
*Do you want to offset the Co2 emissions for 0.44 PLN?*

- *Yes, I want to offset CO2 emissions.*
- *No, I don't want to offset CO2 emissions.*

*If you choose to offset, the waybill / label will indicate this fact.*
*To find out more about CO2 emissions and climate change click here.*

T3. OPTIONAL SIGNAL TREATMENT:

*The shipment will generate 6.92 kg of CO2 emissions.*
*Do you want to offset the Co2 emissions for 0.44 PLN?*
*You can decide whether the information about the offset is to be included in the waybill/label.*

- *Yes, I want to offset CO2 emissions and please mark it on the waybill/label.*
- *Yes, I want to offset CO2 emissions, but I don't want to report this on the waybill/label.*
- *No, I do not want to offset CO2 emissions.*

*To find out more about CO2 emissions and climate change click here.*

The last screen before completing the order showed a preview of the label and information on the total costs:

> *Delivery of this shipment will generate 6.92 kg of CO2 emissions.*
> *Shipping costs: 16,90 PLN*
> *Cost of CO2 offset: 0.44 PLN*
> *Total: PLN 17.34*


Figure S3.6: Emblem indicating the carbon neutral shipment. Own design.

*Notes:* The text translates into "The sender cares about the environment - your shipment is emission free", own design.

Customers could request further information on the offsetting. The courier service provider provided the following information on his website:

*Offset CO2*
*Each of us contributes to greenhouse gas emissions when we drive a car, fly an airplane, use energy, or even when we participate in parties. Traditional courier services, based on road transport, are also associated with emissions of pollutants into the air. However, it is possible to neutralize this negative impact on the environment by participating in CO2 offset projects. The essence of these projects is to pay for activities in another region of the world that will reduce CO2 emissions in the same amount as was produced for our needs. These measures could include, for example, planting trees, building renewable energy sources, or exchanging energy-intensive technologies for zero-emission technologies among communities that would not be able to finance such modernization on their own.*
*To promote the idea of CO2 offset, [name of the courier service provider company] supports a reforestation project in San Juan de Limay, Nicaragua. The project is implemented by myclimate, a non-profit organization. So far, 2 million trees have been planted on an area of 1,500 hectares, avoiding 346,000 tonnes of CO2 emissions. This corresponds to the total annual emissions of 90,000 passenger cars. You can read more about the project here.*

Information on the climate protection project as provided by the international initiative in charge of the carbon compensation:

*Project type: Land Use and Forestry*
*Project location: San Juan de Limay and Somoto, Nicaragua*
*Annual $CO_2$ reduction: ~70,000 t*

*This community-based reforestation initiative is situated upon a critical watershed that feeds into Nicaragua's most important estuaries, the Estero Real. This estuary is home to one of the biggest extensions of mangroves and migratory birds in the region and has been recognized by the Ramsar Convention on Wetlands of International Importance. By reforesting this region, the program plays an important role in regulating the hydrological cycle, providing important water and biodiversity benefits both locally and internationally and improves the quality of life of smallholder farmers.*

<u>Data cleaning</u>

The datasets obtained from the courier service provider overlapped so that some observations appeared two or three times when all datasets were collapsed. The duplicates were not identical but contained different information, especially on the weight of the parcel, the prices, emissions, and the sender or receiver addresses. The courier service provider explained this with manual changes in the orders after the client submitted it and with difficulties in the correct calculation of emissions and offsetting prices at the beginning of the experiment. Different versions of the same order can be identified by the number of the dataset or by order dates. We kept the first observation as this was most likely what participants saw during the ordering process. Emission values and offsetting prices were recalculated based on parcel information such as weight and distance of delivery. Recalculated values are used as a comparison and verification. Statistical analysis is based on reported values as these are the information on which senders based their decisions. The recalculated values are included in Table 3.3. The raw dataset without duplicates and test orders (manually deleted as well as orders paid by or sent from the office of the courier service provider) contained a total of 6,768 observations, which were checked for consistency and adjusted with the following steps.

- Four observations of 'weight' contained unreadable data and were set to missing; in one waybill number, a letter was deleted to obtain conformity.
- The following observations were deleted: one observation did not contain information on the customer ID of the sender. One client was assigned to two different treatments during the experiment: only the observations from the first assignment were kept, and the other 99 were deleted. According to the database, three clients made decisions that were not possible in their assigned treatment: we kept the observations until that error occurred and deleted the other 18 observations.
- An outlier with a total of 884 orders was excluded from the analysis (mean order_volume without the large customer is 46.5, median 15).

The final dataset contained a total of 5,767 observations.

Notes on the final dataset:

- Type of service 'suburban' was added to 'city service'.
- For five observations, the variable 'Print label' is 0. According to courier service provider every customer in the online shop has to print the label himself. Thus we considered this a mistake in the dataset and did not exclude these observations for the analysis.
- Some observations of the variables price (60), off_emissions (767), and off_price (785) were 0 in the raw data. At the beginning of the experiment, the courier service provider only provided data on these variables only for those orders with carbon compensation. Furthermore, at the beginning of the experiment, some errors in the calculation of these variables occurred. Since we cannot tell what clients saw when they made their offsetting decision, the zeros remain in the dataset and in the analysis. Alternative approaches would have been to replace 0 with missing or to delete first experiences. But: Clients had a specific experience also during their first orders, and this had an impact on the following orders.

*Randomization*

Subjects were randomly assigned to one of the three different treatments of the experiments and remained within that treatment for the whole duration of the experiment. For existing customers, the random assignment to the treatments was performed with the following code: https://www.postgresql.org/docs/9.5/static/functions-math.html New clients, which became customers to the courier service provider during the ongoing experiment, were assigned to treatments using the excel function randbetween(1;3). Customers placed their first orders at different times after the experiment has started. Table S3.8 shows the treatment and the month of the first order for all customers. The number of customers per treatment seems unbalanced, but we do not observe any patterns suggesting errors in the randomization strategy.

Table S3.8: Assignment to treatments over time

| | No signal | Mandatory | Optional | Total |
|---|---|---|---|---|
| Mar 2018 | 4 | 10 | 12 | 26 |
| Apr 2018 | 14 | 15 | 18 | 47 |
| May 2018 | 0 | 2 | 1 | 3 |
| Jun 2018 | 3 | 10 | 16 | 29 |
| Jul 2018 | 1 | 3 | 3 | 7 |
| Aug 2018 | 3 | 1 | 2 | 6 |
| Sep 2018 | 0 | 4 | 2 | 6 |
| Total | 25 | 45 | 54 | 124 |

In June, a relatively high number of new customers entered the web-shop. At this time, the courier service provider launched a new website and introduced a small fee for orders via phone, which was another channel to set orders. Customers who set their first order in the web-shop before June do not differ in their offsetting behavior than customers who entered in June or later (for descriptives, see Table S3.9 and Table S3.10).

Table S3.9: Offsetting behavior of those customers who placed their first order before June.

| Offsetting decision | Freq. | Percent | Cum. |
|---|---|---|---|
| 0 | 1,680 | 91.55 | 91.55 |
| 1 | 155 | 8.45 | 100.00 |
| Total | 1,835 | 100.00 | |

Table S3.10: Offsetting behavior of those customers who placed their first in June or later.

| Offsetting decision | Freq. | Percent | Cum. |
|---|---|---|---|
| 0 | 3,419 | 86.95 | 86.95 |
| 1 | 513 | 13.05 | 100.00 |
| Total | 3,932 | 100.00 | |

Table S3.11 shows an example of the $CO_2e$ emissions produced by a delivery of a 1-kg-delivery over a distance of 100 km for the three main kinds of services offered by the courier service company: city services, national services, and international services. City services are relatively carbon intensive as deliveries are often transported in small cars/vans. In contrast, for international services, usually, several deliveries are combined and shipped together. For real-time calculation of carbon emissions for national and international services during the ordering process, we built upon a predefined matrix containing zip codes and distances for a variety of routes. For city services, due to the high level of spatial fragmentation, offsetting costs were approximated by a fixed percentage of total shipping costs.

Table S3.11: Exemplary calculation of $CO_2e$ for the transport of a 1 kg parcel over a distance of 100 km for the three different services.

| **Service** | **kg $CO_2e$/Service (1 kg parcel, 100 km)** | **$CO_2$-offsetting price** |
|---|---|---|
| City Standard | 13 | 0,88 PLN (0.21 EUR) |
| National Standard | 0,782 | 0,05 PLN (0.01 EUR) |
| Internat. Standard | 1,360 | 0,13 PLN (0.03 EUR) |

*Notes:* The table shows examples of the calculated $CO_2$ emissions and offsetting costs of (the average parcel shipped through each of the) three types of services: city, national and international. The offsetting price for 1 t of $CO_2e$ amounted to 15 € (or 63 PLN). Exchange rate: 1 Zloty (PL) = 0,24 Euro.

<u>*Statistics*</u>

The following tables provide additional information for the statistical analysis, as referred to in the main text above.

Table S3.12: Two-sample t-test on mean differences in participation rates in first orders between treatments

| | obs1 | obs2 | Mean1 | Mean2 | dif | St_Err | t_value | p_value |
|---|---|---|---|---|---|---|---|---|
| No signal vs mandatory | 25 | 45 | .16 | .111 | .049 | .085 | .6 | .565 |
| Mandatory vs Optional | 45 | 54 | .111 | .13 | -.018 | .067 | -.3 | .781 |
| No Signal vs optional | 25 | 54 | .16 | .13 | .03 | .085 | .35 | .721 |

*Notes:* Two-sample t test with equal variances.

Table S3.13: Two-sample t-test on mean differences in participation rates in all orders between treatments

| | obs1 | obs2 | Mean1 | Mean2 | dif | St_Err | t_value | p_value |
|---|---|---|---|---|---|---|---|---|
| No signal vs mandatory | 1034 | 2245 | .177 | .08 | .097 | .011 | 8.35 | 0 |
| Mandatory vs optional | 2245 | 2488 | .08 | .123 | -.044 | .009 | -4.9 | 0 |
| No signal vs optional | 1034 | 2488 | .177 | .123 | .054 | .013 | 4.25 | 0 |

*Notes:* Two-sample t-test with equal variances.

Table S3.13 shows that the probability that a client compensates for the carbon emissions resulting from his delivery decreases both when a signal is mandatory and optional. The two-sample t-test does not control for serial correlation or differing characteristics of individual orders, which is why we prefer the logistic regression in Table 3.7 above.

The following tables provide the regression results with minimum samples for first orders only and for all orders. The minimum sample size was derived from the sample size of the model with the maximum number of covariates.

Table S3.14: Logit regression on participation rates in first orders, minimum sample

| | (1) Offsetting | (2) Offsetting | (3) Offsetting | (4) Offsetting | (5) Offsetting | (6) Offsetting |
|---|---|---|---|---|---|---|
| Mandatory signal | -0.0655 | -0.0445 | -0.0558 | -0.0493 | -0.0493 | -0.0560 |
| | 0.5142 | 0.6501 | 0.5823 | 0.6290 | 0.6296 | 0.5890 |
| Optional signal | -0.0584 | -0.0499 | -0.0618 | -0.0596 | -0.0603 | -0.0726 |
| | 0.5492 | 0.5894 | 0.5176 | 0.5277 | 0.5246 | 0.4518 |
| shipping_costs[a] | | -0.0025 | -0.0041 | -0.0040 | -0.0040 | -0.0015 |
| | | 0.3168 | 0.2259 | 0.2297 | 0.2310 | 0.6025 |
| Offset price[b] | | | 0.1057 | 0.1085 | 0.1081 | -0.1147 |
| | | | 0.3579 | 0.3473 | 0.3498 | 0.6868 |
| CSR[e] | | | | -0.0376 | -0.0369 | -0.0388 |
| | | | | 0.6497 | 0.6564 | 0.6333 |
| Sector[f] | | | | | 0.0062 | -0.0029 |
| | | | | | 0.9303 | 0.9677 |
| offsetting/shipping[c] | | | | | | 6.2959 |
| | | | | | | 0.3064 |
| N | 114 | 114 | 114 | 114 | 114 | 114 |

*Notes:* Dependent variable: 1 if sender does offset and 0 if sender does not offset. Margins and p-values. Baseline: no signal treatment. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]share of offsetting costs in total shipping costs, [e]1 if firm communicates some of CSR activity on firm website (own elicitation), [f]1 if firm is part of sector M according to EU nomenclature: "Professional, scientific and technical activities" (data from Polish commercial trade register NIP or KRS).

Table S3.15: Logit regression on participation rates in all orders with minimum sample

| | (1)<br>Offsetting | (2)<br>Offsetting | (3)<br>Offsetting | (4)<br>Offsetting | (5)<br>Offsetting |
|---|---|---|---|---|---|
| Mandatory signal | -0.0913 | -0.0919 | -0.0684 | -0.0648 | -0.0544 |
| | 0.2638 | 0.2634 | 0.3702 | 0.4020 | 0.4579 |
| Optional signal | -0.0388 | -0.0378 | -0.0247 | -0.0246 | -0.0159 |
| | 0.6635 | 0.6736 | 0.7696 | 0.7676 | 0.8449 |
| shipping_costs | | -0.0004 | 0.0004 | 0.0001 | -0.0006 |
| | | 0.6526 | 0.5917 | 0.9386 | 0.6073 |
| Offset price | | | -0.1915 | -0.1226 | -0.0432 |
| | | | 0.0340 | 0.2290 | 0.5698 |
| city | | | | -0.0437 | -0.0012 |
| | | | | 0.5398 | 0.9888 |
| ratio offsetting/shipping | | | | | -3.8492 |
| | | | | | 0.3506 |
| N | 4316 | 4316 | 4316 | 4316 | 4316 |

*Notes:* Dependent variable: 1 if sender does offset and 0 if sender does not offset. Margins and p-values. Baseline: no signal treatment. Standard errors are clustered at sender level. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]1 if type of service is city service and 0 if it is international oder national services, [d]share of offsetting costs in total shipping costs, [e]time senders spent for decision making on offsetting.

In addition to the analysis in Section 3.4 we provide the joint analysis of treatments T2 and T3. The following tables show the regression results with T2 as baseline treatment.

Table S3.16: Logit regression on offsetting rates (offs.) in first orders, baseline: mandatory signal

| | (1)<br>Offs. | (2)<br>Offs. | (3)<br>Offs. | (4)<br>Offs. | (5)<br>Offs. | (6)<br>Offs. | (7)<br>Offs. |
|---|---|---|---|---|---|---|---|
| No signal | 0.0489 | 0.0380 | 0.0462 | 0.0582 | 0.0537 | 0.0601 | 0.0569 |
| | 0.5742 | 0.6596 | 0.6025 | 0.5353 | 0.5610 | 0.5439 | 0.5779 |
| Optional signal | 0.0185 | 0.0099 | 0.0098 | 0.0004 | 0.0020 | -0.0090 | -0.0134 |
| | 0.7772 | 0.8809 | 0.8810 | 0.9950 | 0.9763 | 0.8955 | 0.8509 |
| shipping_costs[a] | | -0.0019 | -0.0031 | -0.0016 | -0.0018 | -0.0019 | -0.0018 |
| | | 0.3367 | 0.2511 | 0.5915 | 0.5566 | 0.5577 | 0.5589 |
| Offset price[b] | | | 0.0848 | -0.1164 | -0.1290 | -0.1324 | -0.1235 |
| | | | 0.4047 | 0.6783 | 0.6640 | 0.6557 | 0.6820 |
| offsetting/shipping[c] | | | | 6.0486 | 5.4033 | 5.6589 | 5.5473 |
| | | | | 0.3157 | 0.3915 | 0.3717 | 0.3895 |
| order_volume[d] | | | | | -0.0006 | -0.0007 | -0.0007 |
| | | | | | 0.3556 | 0.3302 | 0.3573 |
| CSR[e] | | | | | | -0.0300 | -0.0340 |
| | | | | | | 0.7026 | 0.6745 |
| Sector[f] | | | | | | | -0.0027 |
| | | | | | | | 0.9692 |
| N | 124 | 124 | 124 | 121 | 121 | 117 | 114 |

*Notes:* Dependent variable: 1 if sender does offset and 0 if sender does not offset. Margins and p-values. Baseline: mandatory signal treatment. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]share of offsetting costs in total shipping costs, [d]number of orders per sender, [e]1 if firm communicates some of CSR activity on firm website (own elicitation), [f]1 if firm is part of sector M according to EU nomenclature: "Professional, scientific and technical activities" (data from Polish commercial trade register NIP or KRS).

Table S3.17: Logit regression on participation rates in all orders, baseline: mandatory signal

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Offsetting | Offsetting | Offsetting | Offsetting | Offsetting |
| No signal | 0.0972 | 0.0973 | 0.0883 | 0.0743 | 0.0752 |
| | 0.2121 | 0.2135 | 0.2409 | 0.3258 | 0.3216 |
| Optional signal | 0.0433 | 0.0442 | 0.0383 | 0.0338 | 0.0343 |
| | 0.4007 | 0.3909 | 0.4416 | 0.4878 | 0.4851 |
| shipping_costs[a] | | -0.0003 | 0.0002 | -0.0004 | -0.0005 |
| | | 0.6937 | 0.7095 | 0.5710 | 0.4828 |
| Offset price[b] | | | -0.0880 | -0.0165 | -0.0078 |
| | | | 0.1097 | 0.7213 | 0.8582 |
| City[c] | | | | -0.0671 | -0.0650 |
| | | | | 0.2042 | 0.2269 |
| offsetting/shipping[d] | | | | | -0.2960 |
| | | | | | 0.0836 |
| N | 5767 | 5767 | 5767 | 5767 | 5716 |

*Notes:* Dependent variable: 1 if sender does offset and 0 if sender does not offset. Margins and p-values. Baseline: mandatory signal treatment. Standard errors are clustered at sender level. Independent variables: [a]price for delivery including offsetting, [b]costs for offsetting, [c]1 if type of service is city service and 0 if it is international oder national services, [d]share of offsetting costs in total shipping costs.

# Chapter 4.

# Endogenous sanctioning institutions in public goods games

*Joint work with Carlo Gallier*

**Abstract**

We study the effectiveness of an exclusive sanctioning institution on cooperative behavior. This is an institution that imposes a deterrent sanctioning rule on its members while having no effect on the others. For this purpose, we compare the implementation frequency and welfare of this institution with an inclusive institution, where the majority decision determines whether the contribution and sanction rule is imposed either on all members of a group or on none. We conduct a two-stage laboratory experiment consisting of a vote on the costly institution formation and a public goods game. We find that giving participants the option to form an inclusive institution is significantly more welfare-enhancing than giving them the option of forming an exclusive institution. Furthermore, our results suggest that the performance of an exclusive institution is enhanced by a positive spillover effect of yes-voters on the contributions of no-voters. However, this effect wears off over time. Understanding the role exclusive institutions can play in providing public goods is particularly important in situations where inclusive institutions are not feasible as they might interfere with states' sovereignty or actors' individual freedom.

## 4.1 Introduction

Combating climate change requires collective action by sovereign nations. The absence of a supranational authority makes it necessary that states voluntarily commit to providing *climate change mitigation.* However, it has proven to be challenging to make progress in the multilateral framework that was created for this purpose – the United Nations Framework Convention on Climate Change (UNFCCC) (Nordhaus, 2019; Weischer, Morgan, & Patel, 2012), because of strong incentives for free-riding (Nordhaus, 2015). The blueprint of an ideal system to overcome this problem is Climate Clubs (Nordhaus, 2015). This term refers to an agreement by a subset of countries to undertake emission reductions and sanction nonparticipants to create an incentive for them to enter the club and undertake emission reduction themselves (Nordhaus, 2015). In reality, the implementation of this ideal form of a Climate Club is hindered by the fact that these penalties, e.g., in the form of boarder adjustment taxes in the context of domestic carbon prices, cannot be easily implemented against the background of legal frameworks, such as international trade agreements. In this paper, we study two institutions that represent the two extremes of sanctioning institutions in terms of the ability to sanction nonparticipants. We refer to an institution, in which a sanctioning rule also applies to nonparticipants, as *inclusive institution*.

The landscape of existing Climate Clubs promotes dialogue and/or implementation of specific activities (Weischer et al., 2012) but without such sanctions against nonmembers. This shows that sanctioning other countries is difficult in reality. Institutions more common in practice are *exclusive institutions*. That is a voluntary coalition by a subgroup of states or actors – i.e., *insiders* –, without such an opportunity to impose a sanction against nonparticipants of the coalition – i.e., *outsiders*.

An example of such endogenously formed institutions, in which sovereign states commit to contribute to a public good and submit to its enforcement by building a jurisdiction, is the *EU Effort Sharing Legislation* implemented by the European Union (EU) to comply with the Paris Agreement. The EU's greenhouse gas emissions reduction target under the Paris Agreement amounts to at least 40%[14] by 2030 (DG CLIMA, 2021a). To reach this target, the EU Member States have implemented an exclusive institution – the *EU Effort Sharing Legislation*, in the form of a commitment to binding annual national emissions reduction targets and agreed to a penalty for non-compliance. The group of insiders of this institution - i.e., the EU Member States – is expanded by two additional voluntary members in 2021: Iceland and Norway have committed to applying the same rules and will have the same obligations and flexibilities as EU Member States (DG CLIMA 2021b). All other states are outsiders, i.e., they are not subject to the regulation but benefit from the emission reduction efforts.

[14] It is currently proposed to increase the EU's 2030 greenhouse gas emissions reduction target even further to 55% greenhouse on the way to climate neutrality by 2050 (DG CLIMA, 2020).

This legislation is one of the primary measures of the EU's climate and energy policy framework as it covers sectors, including transport, buildings, agriculture, non-ETS industry, and waste, that account for almost 60% of total domestic EU emissions (DG CLIMA, 2021b). The member states' compliance with the agreement is enforced by the European Commission, which has confirmed that all 28 member states complied with their obligations in 2013-2017 (DG CLIMA, 2020). However, it is projected that the 2030 targets can only be achieved with additional measures and then only by a small margin. One country that is expected to miss its targets even with additional measures is Germany (DG CLIMA, 2020). The Effort Sharing Legislation foresees that member states that miss their annual targets will be subject to sanctions: in the first instance, they have to achieve the missing emissions reduction in the next year multiplied by a factor of 1.08 as a penalty (DG CLIMA, 2021b). Additionally, the Commission may launch a formal infringement procedure, which can lead to a financial penalty (DG COMM, 2021).

As this example shows, there are actors who pursue the endogenous formation of voluntary institutions for climate protection, commit to contributions and submit to sanctioning rules. To better understand the role such institutions can play in providing public goods, we design an experimental study. We investigate the endogenous formation of formal sanctioning institutions in a laboratory experiment. Once the costly institution is formed by a majority vote, a deterrent sanctioning is automatically enforced by a central authority. We vary to whom the sanctioning rule applies and implement an *exclusive institution* and an *inclusive institution treatment*. In the exclusive treatment, only those subjects become insiders to the institution who voted in favor of it and thus committed to contributing to the public good. The others are outsiders, and although they benefit from the public good, they cannot be sanctioned for defecting. If an inclusive institution is formed, all members of a group become subject to the sanctions. Understanding the role exclusive institutions can play in providing public goods is particularly important in situations where inclusive institutions are not feasible. This might be the case if the development of a legal framework is too complex due to the interference with states' sovereignty or actors' individual freedom than it would be for an exclusive institution.

The focus of this paper is the investigation of the performance of exclusive institutions. To this end, we compare the implementation frequency and welfare of the exclusive institution against an inclusive institution as a benchmark. Our central research questions are as follows:

1. In which treatment (inclusive or exclusive) are more institutions formed?
2. Is the possibility of forming an institution more welfare-enhancing in an inclusive or an exclusive setting?

We first analyze a theoretical model to shed light on these questions. Due to the multiplicity of equilibria, we cannot derive clear predictions regarding the two questions. In our experiment, we observe that inclusive institutions are more frequently formed than exclusive institutions and also yield higher welfare. However, we find that exclusive institutions perform better than predicted by the mixed strategy equilibrium.

We then focus on explorative research questions and analyze the contribution behavior of those subjects who are not subject to a sanctioning rule. The resulting research questions are:

3. How much do yes-voters and no-voters contribute if an institution is not formed?
4. In an exclusive setting, do no-voters contribute more if an institution is formed or not?

Theory predicts no differences in contributions for both questions. In contrast, we find that no-voters in the inclusive treatment contribute significantly more than yes-voters if no institution is formed. In the exclusive treatment, it is the other way round. In the exclusive treatment, we observe that the formation of an institution positively affects the contributions of outsiders. The outsiders' (voluntary) contributions further increase in the number of insiders.

The remainder of the paper is structured as follows. Section 4.2 provides a review of related literature. In Section 4.3, we present our theoretical considerations. Section 4.4 introduces the experimental design and the hypotheses. The results are presented in Section 4.5, followed by concluding remarks in Section 4.6.

## 4.2 Related literature

With this paper, we contribute to the literature on endogenous sanctioning institutions. In an early experimental study by Yamagishi (1986), subjects in an adapted version of the standard public goods game were given the opportunity to finance a punishment fund. The punishment rule was designed so that the least cooperative group member was punished depending on the height of the punishment fund. The sanctioning proved successful in enhancing cooperation in the public good. In the seminal paper by Ostrom, Walker, and Gardner (1992) on commitments among common pool resource users, subjects had the opportunity to impose an informal sanction on each other. The results show that subjects are willing to pay a fee higher than predicted to place a sanction. However, the net earnings cannot be increased. Later experiments show that endogenous sanctioning institutions are a more effective mechanism for increasing contributions to public goods than rewards (e.g., Sefton, Shupp and Walker, 2007) and nonmonetary sanctions (e.g., Masclet *et al.*, 2003). Despite these observations that cooperation tends to be higher with endogenously formed sanctioning institutions, a recent literature overview shows that a significant share of subjects fails to implement sanctioning institutions (Dannenberg & Gallier, 2019). The implementation of such a sanctioning institution is itself referred to as a second-order public good problem (Kosfeld, Okada, & Riedl, 2009). Therefore, the focus of studies on endogenous sanctioning institutions is on the implementation mechanism and the implemented sanctioning regimes. In the following, we will present findings of the literature on both of the two aspects and link our contribution to these previous studies.

### *Endogenous sanctioning institutions - implementation mechanisms*

Andreoni and Gee (2012) compare the exogenous and endogenous implementation of a mechanism that sanctions the lowest contributor in a public goods game. They find that groups implement the mechanism 85% of the time and that average individual net earnings are similar if the same mechanism is exogenously imposed. In the endogenous setting, the mechanism is implemented if a certain funding threshold is reached. All 4 subjects of a group are endowed with 4 tokens each, of which they can choose to contribute $e_i, 0 \leq e_i \leq 4$ to a hiring fund. If the sum reaches 8 the threshold is met. In our paper, we use an implementation mechanism in which subjects can also decide to contribute an endowment to fund a sanctioning institution, and the threshold can be reached by 2 subjects. In contrast to their work, we only allow for symmetric investments by subjects, which means that our decision situation is closer to voting. In Andreoni and Gee (2012), once implemented, the sanctioning mechanism applies to all subjects. However, the key point of this paper is that we vary to whom the mechanism applies. In contrast to the majority of the literature in this field, which studies inclusive sanctioning institutions that govern all group members once implemented, we study the formation of exclusive sanctioning institutions that govern only those who voted in favor of it. To this end, we implement two treatments: subjects either vote on the formation of an inclusive or of an exclusive institution.

A simple approach to letting subjects decide on an institution is to give them a choice between two options. If subjects can freely choose between a world without and with a costly peer sanctioning, they migrate to a world with sanctioning over time and strongly cooperate (Gürerk, Irlenbusch, & Rockenbach, 2006). Deciding on the implementation of sanctioning institutions through voting represents a more complex coordination mechanism, as it takes into account that subjects can be outvoted. Letting subjects vote has been shown to affect cooperation in public goods games positively. The first evidence on this was provided by Tyran and Feld (2006). In a two-stage game subjects first vote on the implementation of a non-deterrent sanctioning and then play a public goods game. They find that the endogenous implementation of this non-deterrent law increases the efficiency of the public good provision compared to no law as well as to an exogenously imposed law. Tyran and Feld (2006) explain their findings by the fact that voting may be interpreted as a signal of the willingness to cooperate. In this paper, we also implement a voting stage prior to a public goods game. This allows us to investigate if the formation of an exclusive institution affects the contribution behavior of no-voters. The findings by Tyran and Feld (2006) are supported by similar studies (e.g., Dal Bó, Foster and Putterman, 2010; Baldassarri and Grossman, 2011). Sutter, Haigner, and Kocher (2010) show that the existence of a voting opportunity suffices to increase contributions independent of whether a sanctioning institution is implemented or not.

Our paper is also closely related to Kosfeld, Okada, and Riedl (2009). They analyze the endogenous implementation of a sanctioning institution that only applies to its funding members. For the implementation decision, they employ a two-stage mechanism that allows subjects to condition their funding decision on other subjects' behavior. Thereby, they allow for conditional cooperation in the implementation stage. As a consequence, subjects can decide to only fund an institution if all members voted in favor of it, this is if the institution is inclusive. Kosfeld, Okada, and Riedl (2009) find that most institutions are grand organizations; that is, all subjects are members. A comparison with a baseline without the opportunity to form an institution reveals that this opportunity increases and stabilizes total contributions to the public good. In contrast to them, we employ a straightforward voting mechanism that does not allow for conditional cooperation.

### *Endogenous sanctioning institutions - sanctioning regimes*

Sanctioning regimes studied in the context of endogenous institution formation can be divided into two categories: informal sanctioning, i.e. (costly) punishment of other group members, peers, after learning of their contributions; and formal sanctioning, i.e., the enforcement of sanctions by a central authority or a group. Findings on both are ambiguous. Kamei, Putterman, and Tyran (2015) find that subjects achieve high levels of cooperation at low cost by implementing informal sanctions, even if they have the option to implement formal sanctions of modest cost. Fehr and Williams (2018) find that a peer sanctioning institution performs as well as an elected central sanctioning institution in terms of cooperation rates in the beginning. But, welfare improvement over time is more rapid with central sanctioning. Markussen, Putterman, and Tyran (2014) compare a formal deterrent sanctioning rule to peer punishment. They find that a majority of subjects vote for low-cost deterrent formal sanctioning and reach high efficiency. However, with subjects' experience, peer punishment outperforms formal deterrent sanctions, which might in parts be explained by the cost advantage of the peer punishment. Their observations further suggest that deterrent sanctions are preferred to non-deterrent formal sanctions. The experimental design in this paper is closely related to Markussen, Putterman, and Tyran (2014). We implement a low-cost deterrent formal sanctioning that sanctions contributions to the private account with a fixed fine, which is exogenously determined.

*Leadership in public good provision*

In our experiment, we inform subjects about their group members' voting behavior regarding the institution formation before they make their public good contributions. This allows us to shed light on the influence of a yes-voters' self-commitment on outsiders' contribution behavior. Thus, the exclusive institution in our paper may in parts be related to the literature on leadership in public good provision. This literature typically considers sequential public goods games and investigates the influence of the first mover's decision on others' contributions. In certain situations, leadership can solve coordination and cooperation problems (Garretsen, Stoker, & Weber, 2020), for example, if subjects learn that they are critical for meeting implementation thresholds (McEvoy, 2010). Furthermore, high contributions by the first mover are welfare-enhancing in the presence of a conditional cooperator whose contributions to a public good are positively correlated with others' contributions. This is especially important as a considerable share of the overall population are conditional cooperators (Chaudhuri, 2011; Kosfeld, 2020). Güth, Levati, and Sutter (2004) provide evidence that groups with a leader outperform those without a leader. But, leading by example is less effective in fostering contributions than sanctions (Gürerk, Lauer and Scheuermann, 2018).

In this literature, a leader is typically determined by chance or according to a specific rule. A leader who is elected by followers enhances the group's outcome compared to a randomly selected leader (Brandts, Cooper, & Weber, 2015). Our setting is closer related to studies that investigate voluntary leadership. Haigner and Wakolbinger (2010) find that voluntary leaders outperform groups with involuntary leaders. Rivas and Sutter (2011) study a setting in which each subject can volunteer to contribute before the other group members. Each group can consist of as many leaders as volunteers. They find that voluntary leadership increases contributions significantly compared to exogenously enforced leadership. In our paper, a leader is not explicitly named as such, neither exogenously nor endogenously. But, subjects have the opportunity to go ahead by deciding to vote for the formation of an exclusive institution. Accordingly, all members of a group can send a signal to their respective others. In our setting, the institution formation implies a credible commitment to full public good contributions. This allows us to test the influence of self-commitment on outsiders. Credibility has been found to enhance the effectiveness of a leader's promise on his followers' cooperation behavior (Helland, Hovi, & Sælen, 2018).

In summary, we contribute to the emerging literature on exclusive institutions (e.g., McEvoy, 2010; Gerber, Neitzel and Wichardt, 2013). Exclusive institutions are of great practical relevance as they do not interfere with states' sovereignty or actors' individual freedom. Furthermore, they can be implemented in settings in which inclusive institutions are not feasible. Setting the framework conditions such that an inclusive institution can be formed is more difficult than setting the framework conditions that enable the formation of an exclusive institution. We conduct a lab experiment that enables us to directly compare exclusive with inclusive institutions to observe how exclusive institutions perform compared to inclusive institutions.

## 4.3 Theoretical predictions

In this section, we introduce an illustrative theoretical framework to describe the decision situation of our experiment formally and derive a set of theoretical predictions.

We are concerned with one primary issue here: the formation of sanctioning institutions to coordinate public good contributions. In particular, we distinguish between two kinds of institutions that differ in to whom they apply. In an *inclusive setting,* all members of a group become subject to the sanctioning institution once an implementation threshold is reached. In an *exclusive setting,* only those who voted in favor of it become subject to the institution if they reach the implementation threshold. In both settings, those who are subject to an institution are called *insiders*. In the exclusive setting, it is possible that a formed institution does not affect all members of a group. Those to whom the sanctioning rule of a formed institution does not apply are called *outsiders*. To formalize this notion of institution formation, we consider a two-stage game. In Stage 1, subjects vote on forming a costly sanctioning institution that enforces a contribution rule in a public goods game with a deterrent sanctioning. The sanctioning implies a fine $s$ which is assumed to be exogenously given. In Stage 2, subjects play a public goods game. For this, we consider groups of $n$-subjects who obtain utility from consuming a private and a public good. The private good yields private benefits for the subject only. A subject's contribution to the public good is beneficial for all $n$ group members. There are $n \geq 2$ subjects, each of whom has a private endowment $E = E_1 + E_2$ with $E_1, E_2 > 0$. Here, $E_1$denotes a subject's endowment in the first stage, which she can invest in the costly institution formation. $E_2$ denotes subjects' second stage endowment for the public goods game. Each subject can contribute $C_i \epsilon\ [0, E_2]$ to the public good in Stage 2, while $E_2 - C_i$ goes into the private good. If no institution is formed, given the contribution of all subjects ($C_i,\ldots,C_n$), subject $i$'s total profit is given by

$$\pi_i = E_1 + E_2 - C_i + m * \sum_{j=1}^{n} C_j. \tag{1}$$

The parameter $m$ describes the marginal per capita return (MPCR) from contributing to the public good. We consider a setting in which it holds that $\frac{1+\bar{C}}{\bar{n}*\bar{C}} < m < 1 < nm$. Here $1 < \bar{n} < n$ denotes the threshold for institution formation. This is, if at least $\bar{n}$ subjects vote yes, the institution is implemented. For the further analysis, we assume that the institution is formed if at least half of the group members vote in favor of it. Such majority voting implies that $\bar{n}$ is equal to the smallest integer larger or equal $\frac{n}{2}$. $\bar{C}$ denotes the minimum contribution as set by the contribution rule, which is not sanctioned. We assume that the fine $s$ is large enough so that those subjects, to whom the rule applies, have an incentive to contribute $\bar{C}$. With $m < 1$, the individual incentives are against contributing if no institution is formed. More concrete, zero contribution is the dominant action for every rational and purely self-interested subject. Each subject's profit is maximized by contributing zero to the public good regardless of the other subjects' contributions. The condition $nm > 1$ implies that contributions to the public good are welfare enhancing. The condition $m > \frac{1+\bar{C}}{\bar{n}*\bar{C}}$ ensures, that an equilibrium exists in which an exclusive institution is formed.

The profit of an outsider and of members of groups in which no institution is formed is given by $\pi_i$ as defined by (1). When a contribution rule is in place, all insiders who contribute less than $\bar{C}$ to the public good are sanctioned with a deduction in the amount of $s$. Subject $i$'s profit as an insider in Stage 2 is consequently equal to

$$E_2 - C_i + m * \sum_{j=1}^{n} C_j - \begin{cases} 0 \text{ if } C_i \geq \bar{C} \\ s \text{ if } C_i < \bar{C} \end{cases}$$

The formation of the sanctioning institution is costly. The institution is formed if at least $\bar{n} = \frac{n}{2}$ subjects vote in favor of the institution. If an institution is implemented, each yes-voter pays a fee $k_i$ which we normalize to 1. The level of the individual fee equals the endowment each subject receives in stage 1, this is $k_i = E_1 = 1$. Each no-voter pays nothing. If no institution is implemented, no payments are made.

The profit of a yes-voter if an institution is formed is given by

$$E_1 + E_2 - C_i + m * \sum_{j=1}^{n} C_j - 1 - \begin{cases} 0 \text{ if } C_i \geq \bar{C} \\ s \text{ if } C_i < \bar{C} \end{cases}$$

In our setting, the sanctioning is deterrent, i.e., $s > \bar{C} * (1 - m)$.[15] Hence, it is optimal for an insider to contribute $\bar{C}$ to the public good.

We use the profit functions above to derive analytic predictions for our experiment. In doing so, we solve our illustrative model by backward induction and thus start with the subjects' contribution behavior to the public good in Stage 2 of the game.

### *Contributions in a public goods game*

If no institution is formed, a subject maximizes her profit by contributing 0 to the public good. As a consequence, the profit of all subjects is equal to E. If an inclusive institution is formed, all members of a group are subject to the sanctioning regime. As the sanctioning fee is deterrent it is individually rational for all subjects to contribute $\bar{C}$ to the public good. In this case, the yes-voters will make a profit of $E - \bar{C} + n * m * \bar{C} - 1$. No-voters will make a profit of $E - \bar{C} + n * m * \bar{C}$. As indicated above, a necessary condition for an inclusive institution being formed in equilibrium is that yes-voters earn a higher profit with than without an institution. This is the case if $m > \frac{1+\bar{C}}{n*\bar{C}}$. If an exclusive institution is formed, all members of a group who voted in favor of the institution in the first stage become insiders. All no-voters become outsiders. As the sanctioning is deterrent all insiders contribute $\bar{C}$ to the public good, and all outsiders contribute 0. Suppose $\bar{n}$ subjects vote yes, then an

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[15] If the insider contributes $C_i = 0$ her profit is: $\pi_i^{in}(0) = E + m * \sum_{j \neq i} C_j - k_i - s$. If the insider contributes $C_i = \bar{C}$ her profit is: $\pi_i^{in}(\bar{C}) = E - \bar{C} + m * \sum_{j \neq i} C_j - k_i + m * \bar{C}$. From the two equations it follows that an insider will contribute $C_i = \bar{C}\ if\ s > \bar{C} * (1 - m)$ and $C_i = 0\ if\ s < \bar{C} * (1 - m)$, thus the institution is deterrent if $s > \bar{C} * (1 - m)$. All contributions different from 0 and $\bar{C}$ are strictly dominated by one of them.

institution is formed, and an insider will make a profit of $E - \bar{C} + \bar{n} * m * \bar{C} - 1$ and an outsider will make a profit of $E + \bar{n} * m * \bar{C}$. As indicated above, a necessary condition for an exclusive institution being formed in equilibrium is that yes-voters earn a higher profit with than without an institution. This is the case if $> \frac{1+\bar{C}}{\bar{n}*\bar{C}}$ .

This is summarized in our first proposition:

<u>Proposition 1:</u> *If an exclusive institution is formed, all insiders contribute $\bar{C}$ to the public good, and outsiders contribute 0. If an inclusive institution is formed, everyone contributes $\bar{C}$. If no institution is formed, everyone contributes 0.*

### *Voting for institution formation*

In Stage 1, subjects make a binary decision about forming a sanctioning institution. We now consider equilibria for the inclusive institution and the exclusive institution.

The following holds for both types of institutions: Suppose that $\hat{n} \leq \bar{n} - 2$ vote in favor of the institution. In this case, no institution is formed, and no subject can change the outcome with her vote, i.e., no subject is pivotal for the formation of an institution. Hence, no subject has a deviation incentive. This directly implies that there exists an equilibrium in which $\hat{n} \leq \bar{n} - 2$ subjects vote yes.

<u>Proposition 2:</u> *In both settings, there exists an equilibrium in which no institution is formed. In such an equilibrium, up to $\bar{n} - 2$ subjects vote yes, and the others vote no.*

Suppose that in the inclusive setting $\bar{n}$ subjects vote yes. In this case, an institution is formed. No no-voter has an incentive to become a yes-voter because this would not influence the institution formation but decrease her profit of $E - \bar{C} + n * m * \bar{C}$ by the costs of 1. No yes-voter has an incentive to become a no-voter because then no institution would be formed, and as a result, her profit would be reduced from $E - \bar{C} + n * m * \bar{C} - 1$ to E.

Suppose that in the exclusive setting $\bar{n}$ subjects vote yes. In this case, an institution is formed. No no-voter has an incentive to become a yes-voter because this would decrease her profit of $E + \bar{n} * m * \bar{C}$ to $E - \bar{C} + \bar{n} * m * \bar{C} - 1$. No yes-voter has an incentive to become a no-voter because then no institution would be formed, and as a result, her profit would decrease from $E - \bar{C} + \bar{n} * m * \bar{C} - 1$ to E.

<u>Proposition 3:</u> *In both settings, there exists an equilibrium in which $\bar{n}$ subjects vote yes (and an institution is formed).*

In the inclusive setting, the expected profit of a yes-voter given that all other group members vote in favor of the institution with probability $x$ is given by

$\sum_{i=0}^{\bar{n}-2}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * E + \sum_{i=\bar{n}-1}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * [E - \bar{C} + n * m * \bar{C} - 1].$

Similarly, this subject's expected profit when voting no is given by

$\sum_{i=0}^{\bar{n}-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * E + \sum_{i=\bar{n}}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * [E - \bar{C} + n * m * \bar{C}].$

The difference between the expected profit when voting for and the expected profit when voting against the institution can be written as

$$\binom{n-1}{\bar{n}-1} * x^{\bar{n}-1} * (1-x)^{n-\bar{n}} * [n * m * \bar{C} - \bar{C} - 1] - \sum_{i=\bar{n}}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i}. \quad (2)$$

Analogously, the expected profit of a yes-voter in the exclusive setting given that all other group members vote in favor of the institution with probability $x$ is given by

$\sum_{i=0}^{\bar{n}-2}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * E + \sum_{i=\bar{n}-1}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * [E - \bar{C} + (i+1) * m * \bar{C} - 1].$

The subject's expected profit when voting no is given by

$\sum_{i=0}^{\bar{n}-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * E + \sum_{i=\bar{n}}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * [E + i * m * \bar{C}].$

In the exclusive setting, the difference between the expected profits is given by

$$\binom{n-1}{\bar{n}-1} * x^{\bar{n}-1} * (1-x)^{n-\bar{n}} * [\bar{n} * m * \bar{C} - \bar{C} - 1] - \sum_{i=\bar{n}}^{n-1}\binom{n-1}{i} * x^i * (1-x)^{n-1-i} * [\bar{C}(1-m) + 1]. \quad (3)$$

A subject that is uncertain about the other group members' voting behavior, i.e., she believes that they vote in favor of the institution with a probability $x \in (0,1)$, faces the following trade-off for her own voting decision. On the one hand, voting in favor of the institution has an upside. It implies a chance of being pivotal for the institution formation. A subject that is pivotal always prefers an institution over no institution. On the other hand, voting in favor of the institution also has potential downsides if the institution was formed even without the subject's yes-vote. In the inclusive and the exclusive setting yes-voting then results in an unnecessary payment. In the exclusive setting, there is the additional downside of committing to a higher contribution. For $x = 0$ the trade-off does not exist. Both the chance of being pivotal and the chance of the institution being formed anyway is zero.

We want to show that a mixed strategy equilibrium exists. In the mixed-strategy equilibrium, the two effects mentioned above are balanced such that subjects are indifferent between voting yes and no. For $x = 1$, the institution will be formed independently from the subject's vote. Thus, she votes no. Given that expressions (2) and (3) are continuous in $x$ and negative for $x = 1$, it suffices to find an $x$ for that the expressions are positive to prove the existence of a mixed-strategy equilibrium. The probability of being pivotal is increasing in $x$ for all $x$ smaller than $(\bar{n} - 1)/(n - 1)$ and decreasing in $x$ for all higher values. In contrast to that, the probability of the institution being formed anyway is monotonically increasing in $x$. Hence, voting in favor of the institution becomes less and less attractive for larger values of $x$. For values of $x$ sufficiently close to zero, the chance of being pivotal is larger than the chance of the institution being formed anyway. As a consequence, there always exist values of $x$ close to zero such that expressions (2) and (3) are positive, i.e., the subject prefers voting in favor of the institution. Hence, a mixed strategy equilibrium exists. The fact that the relative attractiveness of voting in favor of the institution decreases monotonically with increasing x implies that it is unique.

Next, we show that in the mixed-strategy equilibrium of the inclusive setting, the probability of voting in favor of the institution is larger than in the mixed-strategy equilibrium of the exclusive setting. To do so, we compare (2) and (3). Given that $[n * m * \bar{C} - \bar{C} - 1]$ is larger than $[\bar{n} * m * \bar{C} - \bar{C} - 1]$ and $\bar{C}(1 - m) + 1$ is larger than 1, expression (2) is larger than (3) for all $x \in (0,1]$. Take a value $x_e^*$ for which a subject is indifferent in the exclusive setting. Then this subject strictly prefers voting in favor of an inclusive institution for all $x \in (0, x_e^*]$. Vice versa, take a value $x_i^*$ for which a subject is indifferent in the inclusive setting. Then this subject strictly prefers voting against an exclusive institution for all $x \geq x_i^*$. Intuitively, being pivotal is relatively more beneficial in the inclusive setting than in the exclusive setting, making voting yes more attractive in the inclusive setting than in the exclusive setting. Furthermore, the downside of voting in favor of the institution if the institution was formed anyway is larger in the exclusive setting than in the inclusive setting. This is because, in the inclusive setting, subjects would have to contribute $\bar{C}$ anyway, whereas in the exclusive setting, subjects are only committed to contributing $\bar{C}$ if they vote yes. Hence, voting yes is more attractive in the inclusive setting than in the exclusive setting for all $x \in (0,1]$.

<u>Proposition 4:</u> *There exists a mixed strategy equilibrium in both treatments. The probability with which a subject votes yes is larger in the mixed-strategy equilibrium of the inclusive setting than in the mixed-strategy equilibrium of the exclusive setting.*

## 4.4 Experimental design and hypotheses

In this section, we present the design of our experiment and the procedural details (Section 4.4.1) and derive the hypotheses (Section 4.4.2).

### 4.4.1 Experimental design and procedural details

One session of the experiment consists of 20 rounds, each of which is divided into two stages: stage 1 (*voting stage*) and stage 2 (*contribution stage*). Across treatments, participants are divided into groups of $n = 4$ members at the beginning of each round (stranger matching). In stage 1, each subject receives an individual endowment of 1 Experimental Currency Unit (ECU) and has to decide whether to invest $k = 1$ to implement a sanctioning institution for stage 2. If the sum of a group's contributions at the voting stage reaches a threshold of $c = 2$ ECU, the institution is formed, and the corresponding rule is implemented for stage 2. An overview of the timing of the experiment is provided in Figure 4.1.

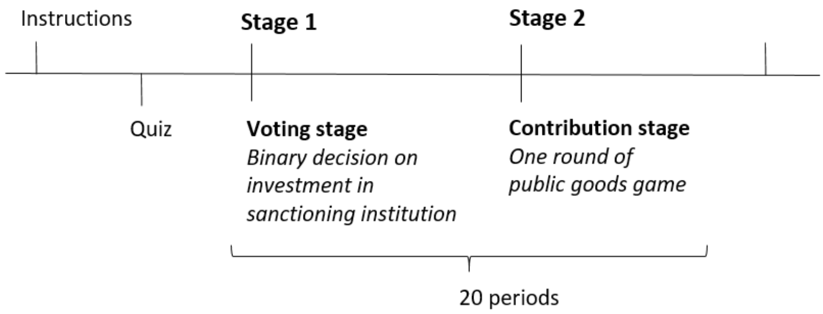



Figure 4.1: Timing of the experiment (both treatments)

Our experimental design varies whether subjects can form an inclusive (T1) or an exclusive (T2) sanctioning institution. In T1, all members of a group become insiders if an institution is formed. In T2, only those who voted in favor of it become insiders if an institution is formed, whereas those who voted against it become outsiders.

In both treatments, an institution is formed if at least two subjects vote yes, i.e. $\bar{n} = 2$. If an institution is formed, each yes-voter pays 1 ECU. If the threshold is not met, subjects do not pay and earn 1 ECU in stage 1. Subjects who voted no, also earn 1 ECU in stage 1. The parameters are set such that in both treatments an equilibrium exists in which the institution is formed.

In the contribution stage, subjects participate in a linear public goods game. Each subject receives an individual endowment of 20 ECU, which she can contribute to a private and a public good, respectively. We set the marginal per capita return, $m = 0.65$, such that the socially optimal payoff per subject exceeds the payoff when each individually optimizes. This holds even for the subgroup of two subjects necessary to implement an institution in the exclusive treatment. Each ECU invested in the public good pays a return of 0.65 ECU

($m = 0.65$) to each of the four group members. This corresponds to an aggregate social return of 2.6 ECU.

Subjects' earnings from the private good equal the number of ECU they do not contribute to the public good. Let $C_i$ be subject $i$'s contribution to the public good. Every subject for whom the contribution rule applies is sanctioned with a deduction in the amount of $s = 15$, if she does not contribute her full endowment of 20 ECU to the public good. The sanction $s$ is high enough to deter free-riding fully, and it is privately rational to contribute all of one's endowment to the group. In both treatments, the individual payoff for an insider per period is given by:

$$21 - C_i + 0.65 * \sum_{j=1}^{n} C_j - 1 - \begin{cases} 0 \text{ if } C_i \geq 20 \\ 15 \text{ if } C_i < 20 \end{cases}$$

The profit of an outsider in the exclusive treatment and of members of groups in which no institution is formed is given by:

$$(21 - C_i) + 0.65 * \textstyle\sum_{j=1}^{n} C_j.$$

If no institution is formed, a purely rational and self-interested subject will choose to set $C_i$=0. If all group members play selfishly, the profit in stage 2 per member is 20 ECU. The group welfare-maximizing level of contribution is $C_i$=20. If all group members chose this amount, they would each earn 52 ECU from the public good. From this follows, the cooperation premium, i.e., the difference between the earnings without and with a sanctioning institution, amounts to 32 ECU in the inclusive setting. In the exclusive setting, where the institution applies only to a subset of the group members, the minimum cooperation premium is 6.[16] Consequently, a subject should be willing to pay any amount less than or equal to this cooperation premium to form the institution. Even though predicted gains from the inclusive institution are larger than those from the exclusive institution, we decided to consider equal implementation costs. It allows the comparison of two groups of the same size with the same threshold (two yes votes for establishing the institution).

[16] Minimum cooperation premium in the exclusive setting: $r_{ce} = (20 - 20 + 0.65 * 2 * 20) - (20) \rightarrow r_{ce} = 26 - 20 = 6$
Cooperation premium in the inclusive setting: $r_{ci} = (20 - 20 + 0.65 * 4 * 20) - (20) \rightarrow r_{ci} = 52 - 20 = 32$

### *Procedural details and implementation*

All five sessions of the experiment were conducted in September 2020 with subjects from the pool of the Cologne Laboratory for Economic Research as an online experiment. We use otree software (Chen, Schonger, and Wickens, 2016) for programming and ORSEE (Greiner, 2004) for recruiting. Each session lasted about 60 minutes, and participants earned on average 17.82 €, including a participation fee of 2.50 €.

At the beginning of each session, 24 participants were randomly divided into two cohorts of 12. Per session, one cohort played treatment T1 and the other T2. Thus, each session consisted of two cohorts of 12 subjects each who played the two treatments parallel to reduce session effects between treatments. In total, each treatment was conducted five times for a total of 60 participants per treatment. Subjects were given the instructions (provided in the Appendix to Chapter 4) and a quiz, which had to be answered correctly before the experiment started. Participants then played the game for 20 periods, of which all were paid out. To minimize repeated game effects, participants were randomly and anonymously rematched into new groups of four at the beginning of each period.

The following procedure was held constant in every round: in stage 1, all group members simultaneously and anonymously decided whether to form an institution. Subjects were informed about the voting decisions of their group members and whether an institution would be in place in stage 2. In stage 2, subjects simultaneously and anonymously determined the amount of their contribution to the public good. They were informed about the aggregated contribution of their group members and the individual share each of the four members earned from the public good. In each period, an overview of their decisions and the endowment from the previous rounds as well as their then-current account balance was displayed to them. To rule out a negative payout at the end of the experiment, participants were given an initial endowment of 60 ECU at the beginning of the session[17]. To remove experimenter effects, all sessions were run by the same person. Technical details on the implementation of this online experiment as well as the instructions and screenshots of the different stages of the experiment are provided in the Appendix to Chapter 4.

[17] This was calculated as the sum of a loss that would result for a subject, who invested in the institution but contributed only 19 ECU, over all 20 rounds.

### 4.4.2 Hypotheses

Given our theoretical considerations and the parametrization of our experimental design, we can derive the following set of hypotheses for participants' public good contributions in stage 2 of the experiment with inclusive and exclusive sanctioning schemes.

Three types of equilibria exist in both treatments. A pure strategy equilibrium in which exactly two subjects vote in favor of the institution and the institution is formed. A pure strategy equilibria in which no subject votes in favor of the institution and no institution is formed. Furthermore, there exists a mixed-strategy equilibrium for inclusive institutions (T1) in which all subjects vote for the institution with 84% probability. There is an equilibrium for exclusive institutions (T2) in which all subjects vote for the institution with 28% probability. We cannot make a prediction which of the equilibria will be played. However, note that the pure strategy equilibria in which institutions are formed require a high level of coordination; the strangers matching in our experiment virtually rules out coordination. Additionally, the pure strategy equilibria in which no institutions are formed are payoff dominated by the mixed-strategy equilibria. If the mixed strategy equilibria are played, more institutions are formed in T1 than in T2. Although we would expect higher institution formation rates in T1 compared to T2, we cannot derive a clear theoretical prediction.

The focus of this paper is on the investigation of the performance of exclusive institutions. Thus, the analysis in the following section compares the implementation frequency and welfare of the exclusive institution against an inclusive institution as a benchmark. Due to the above mentioned multiplicity of equilibria, we cannot derive clear predictions regarding this comparison.

We then explore the contribution behavior of those subjects who are not subject to a sanctioning rule. From the propositions above, we derive the following hypotheses for this second part of the analysis.

*Hypothesis 1: If no institution is formed, yes- and no-voters contribute the same.*

*Hypothesis 2:*

a) *The contribution of a no-voter in the exclusive setting does not depend on whether an institution is formed.*
b) *Furthermore, the contribution of an outsider in the exclusive setting does not depend on the number of insiders.*

## 4.5 Results

We begin our analyses by comparing institution formation rates and welfare across treatments (Sections 4.5.1 and 4.5.2). This is followed by an analysis of the voting behavior (Section 4.5.3). The last two Sections provide an analysis of the contribution behavior of no- and yes-voters across treatments (4.5.4) and outsiders in the exclusive treatment (4.5.5).

We begin every part of this analysis with cohort averages because they constitute the only truly independent observations (Burger & Kolstad, 2009). However, the small sample size of five cohorts per treatment limits statistical power. Thus, we complement the non-parametric tests with regression analyses on the subject-level across treatments.

### 4.5.1 Institution formation rates

Over all sessions, the share of formed institutions amounts to 90% in the inclusive setting and 71% in the exclusive setting (Figure 4.2). A Mann-Whitney-U-test shows that the difference is statistically significant (p=0.03)[18]. In both treatments, institutions can be formed by at least two and a maximum of four group members voting in favor of the institution. In the inclusive treatment, most groups (48%) form institutions by three subjects (27% are formed by two and 15% by four subjects). In the exclusive treatment, we observe slightly smaller institutions: 36% of all groups form the institutions by two subjects (29% by three and 6% by four subjects). A detailed discussion of individual voting behavior and the resulting distribution of institution size is provided further below.

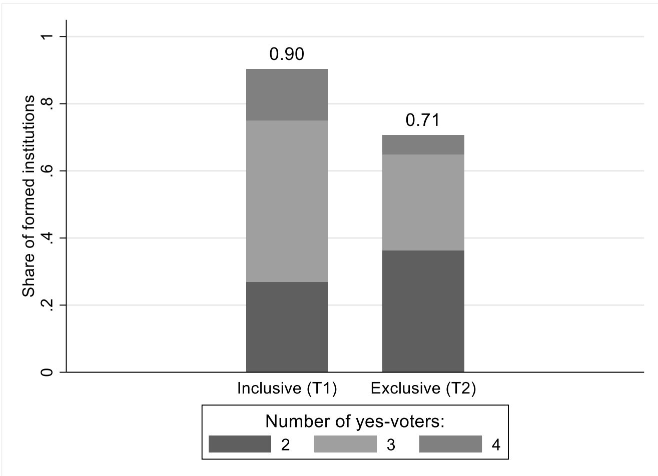



Figure 4.2: Share of formed institutions in each treatment and the number of yes-voters that formed the institutions.

[18] Non-parametric results. Statistical test on cohort level data. Analysis on cohort level ensures independency of observations. There are five cohorts per treatment. One cohort of each treatment was run in the same session.

Concerning research question 1, we can derive the following first result:

**Result 1**: In an inclusive setting, more institutions are formed than in an exclusive setting.

### 4.5.2 Welfare analysis

The average profit for a subject per round in the inclusive setting amounts to 48 ECU and in the exclusive setting to 38 ECU ($p<0.01$, Mann-Whitney-U-test). As one would expect, the profit is higher if an institution is formed (inclusive: 49 ECU, exclusive: 43 ECU) compared to a situation without an institution (inclusive: 33 ECU, exclusive: 28 ECU). As this might be caused by a possible selection effect, we do not make statistical analyses. Figure 4.3 shows the development of the profits in both treatments over time. In the inclusive setting, the average profits of each subject (black line) increase over time. With regard to previous findings in the public goods literature, it seems that the inclusive institution reduces the otherwise in public goods games observed time trend towards lower contributions (e.g., Fehr and Gächter, 2000). However, the average profits in the exclusive setting show a negative time trend. As the share of institution formation is relatively high in both treatments, the average profits are relatively close to the profits with an institution (grey line).

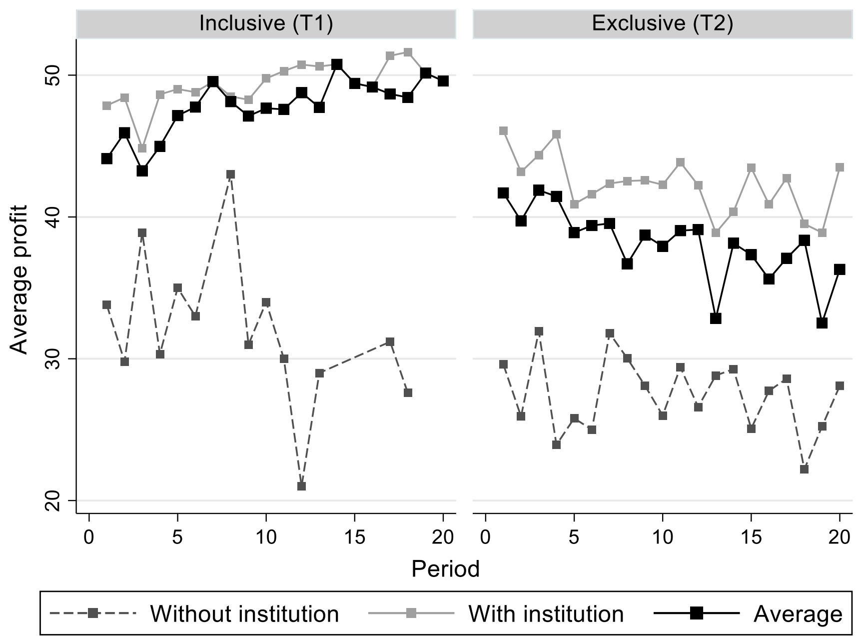



Figure 4.3: Average profit per subject over time.

*Notes*: Average and with institution overlap if the share of formed institutions equals 1.

Research question 2 can be answered as follows:

**Result 2**: Giving participants the option to form an inclusive institution is significantly more welfare-enhancing than giving them the option of forming an exclusive institution.

### 4.5.3 Voting behavior between treatments

Due to the multiplicity of equilibria, no hypothesis about the formation frequency between the two treatments can be made. However, we observe more yes-votes for the inclusive institution than for the exclusive institution. This difference is in line with the relation of the mixed strategy equilibria. Comparing the two treatments with their respective theoretically predicted equilibrium (see Figure 4.4) shows that in the inclusive treatment, the share of yes-votes is significantly below the mixed-strategy equilibrium prediction (p=0.04, Mann-Whitney-U-test). In the exclusive treatment, it is significantly higher (p=0.04, Mann-Whitney-U-test). In both cases, the share of yes-votes seems to develop in the direction of the respective mixed-strategy equilibrium. (Further analyses on the voting behavior compared to the coordinated and the mixed-strategy equilibria are provided in the Appendix to Chapter 4.)

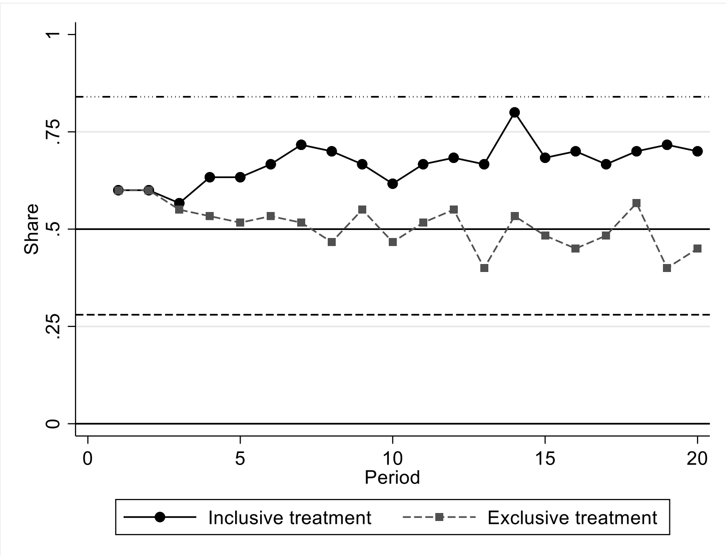



Figure 4.4: Share of votes in favor of an institution.

*Notes:* The dashed line marks the mixed strategy equilibrium in the exclusive treatment. The dash-dotted line marks the mixed strategy equilibrium in the inclusive treatment.

We find that the difference in voting between treatments is driven by opposed time trends, as illustrated in Figure 4.4. While the difference is not significantly different between treatments in the first rounds, it becomes different over time. This is due to a positive time trend of yes votes in the inclusive treatment and a negative time trend in the exclusive setting (regression results are provided in Table S4.4 in the Appendix to Chapter 4.)

**Result 3**: The share of yes votes is higher in the inclusive treatment than in the exclusive treatment (p=0.007). In the first rounds, voting behavior does not differ between treatments but the difference becomes significant over time. This is caused by a positive time trend in the share of yes votes in the inclusive and a negative time trend in the exclusive treatment.

### 4.5.4 Contribution behavior between treatments if no institution is formed

Those who are subject to the sanctioning institution contribute on average an equal amount of 18.9 ECU in both treatments.[19] However, the difference in contributions is slightly greater when no institution is formed: in the inclusive treatment, subjects contribute on average 7.2 ECU and 4.3 ECU in the exclusive treatment.

Going more into detail about what drives this difference, we observe that a relatively high share of subjects contributes their full endowment (24%) in the inclusive treatment. In comparison, only 7.4% do so in the exclusive treatment. Furthermore, we find that in the absence of an institution, no- and yes-voters contribute differently. (Yes-voters are those subjects who voted in favor of the institution, and no-voters are those who voted against it in the respective round.) If no institution is formed in the inclusive treatment, no-voters contribute on average 7.6 ECU, whereas yes-voters contribute 5.4 ECU. In the exclusive treatment, it is the other way round: no-voters contribute 3.8 if no institution is formed, and yes-voters contribute 7.2 ECU (Figure 4.5). The differences between yes- and no-voters are statistically significant in both treatments (both p=0.04, Wilcoxon signed-rank test). Thus, we can reject hypothesis 1.

[19] To compare the performance of the exclusive institution to a setting without a possibility of sanctioning we draw on literature. In a similar setting (public goods game with n=4, stranger matching and an endowment of 20 but with a lower MPCR of 0.4) subjects contribute on average 4.5 of an endowment of 20 (Fehr & Gächter, 2000). In a public goods game with an MPCR of 0.65 subjects contribute on average 12.4 out of 20 (Kosfeld et al., 2009).

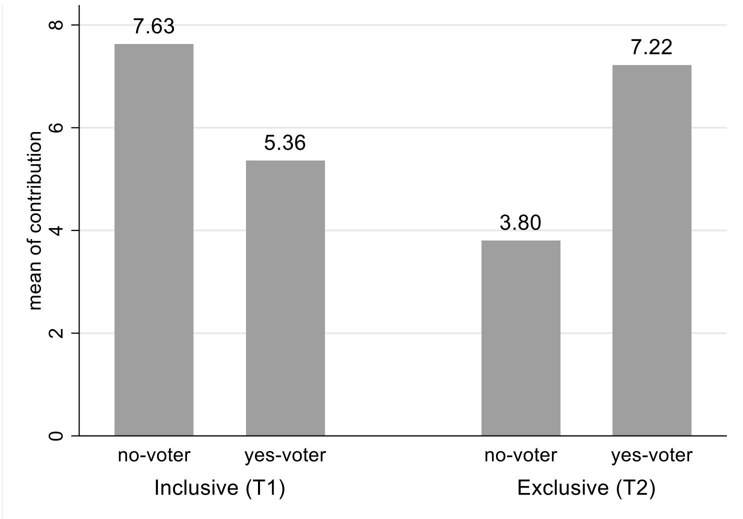



Figure 4.5: Average contributions of yes- and no-voters if no institution is formed

We further conduct panel-regressions to investigate the effect of voting behavior on contributions if no institution is formed (Table 4.1). We observe the same countervailing directions of the effect for the inclusive and the exclusive treatments as described above (in T1, yes-voter contribute 4 ECU less than no-voters, $p<0.05$; in T2, yes-voter contribute 3 ECU more than no-voters, $p<0.01$). Additionally, the analysis results show an overall time trend towards lower contributions for the exclusive treatment, which does not differ between yes- and no-voters.

Table 4.1: Contributions of no- vs. yes-voters if no institution is formed in the inclusive (T1) and exclusive (T2) setting.

| | (1)<br>T1<br>Contribution | (2)<br>T2<br>Contr. | (3)<br>T1<br>Contr. | (4)<br>T2<br>Contr. | (5)<br>T1<br>Contr. | (6)<br>T2<br>Contr. |
|---|---|---|---|---|---|---|
| Yes-vote | -3.994** | 3.140*** | -3.797* | 3.091*** | -3.287 | 3.161** |
| | (2.014) | (0.801) | (2.016) | (0.797) | (2.933) | (1.563) |
| Period | | | -0.141 | -0.0870** | -0.125 | -0.0859* |
| | | | (0.115) | (0.0429) | (0.132) | (0.0479) |
| Yes-vote x Period | | | | | -0.0686 | -0.00586 |
| | | | | | (0.283) | (0.113) |
| _cons | 8.334*** | 3.920*** | 9.323*** | 5.004*** | 9.210*** | 4.992*** |
| | (1.173) | (0.757) | (1.421) | (0.931) | (1.503) | (0.968) |
| *N* | 116 | 352 | 116 | 352 | 116 | 352 |

*Notes*: Random-effects panel regression. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Concerning research question 3 we can say that:

**Result 4**: No-voters in the inclusive treatment contribute significantly more ($p=0.04$, Wilcoxon signed-rank test) than yes-voters if no institution is formed, while in the exclusive treatment, it is the other way round ($p=0.04$, Wilcoxon signed-rank test).

### 4.5.5 Contribution behavior of no-voters in the exclusive treatment

To find out whether the formation of an exclusive institution by the other members of their groups affects no-voters, we compare their contribution behavior in situations without and with exclusive institutions. We find that no-voters in the exclusive setting contribute on average 3.8 ECU if no institution is formed and 6.5 ECU if an institution is formed, though this difference is not significant (p=0.14, Wilcoxon signed-rank test). Figure 4.6 reveals the distribution behind these averages. While the share of no-voters contributing 0 is not affected by the formation of an institution, the share of those who contribute 20 increases from 3.5% to 20.1% (p=0.04, Wilcoxon signed-rank test).

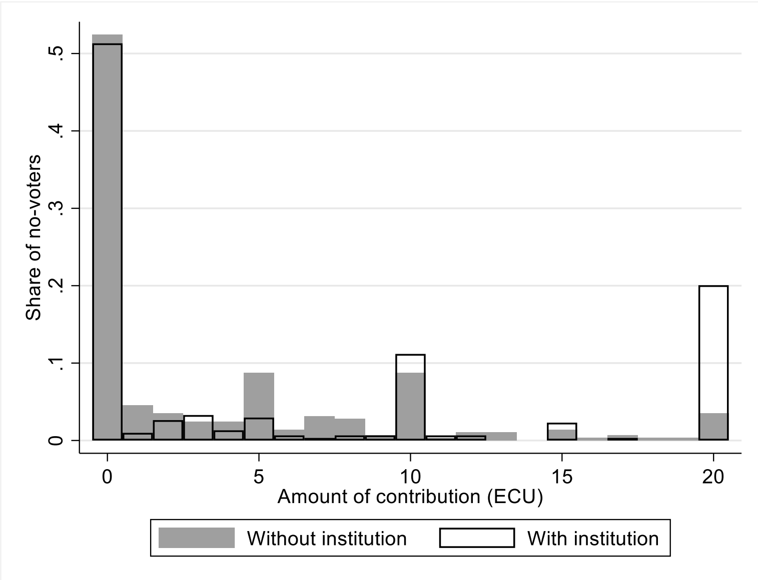



Figure 4.6: Distribution of no-voters' contributions without and with an institution (exclusive treatment).

To further investigate the influence of an institution on no-voters' contribution decisions, we conduct a regression analysis. In contrast to the non-parametric tests on the cohort-level, the regression models show a significant influence of the institution formation on no-voters (Table 4.2), who contribute on average 3 ECU more in case an institution is formed (p<0.01). Based on the regression analysis, hypothesis 2a can be rejected. When looking at time effects, we observe that the overall contributions of no-voters decrease. Including the interaction effect of institution formation and periods further reveals that no-voters' contribution remains stable over time if no institution is formed but decreases if an institution is formed.

Table 4.2: No-voters' contributions with regard to institution formation over time.

| | (1) Contribution | (2) Contribution | (3) Contribution |
|---|---|---|---|
| Institution | $3.050^{***}$ | $2.771^{***}$ | $5.108^{***}$ |
| | (0.455) | (0.458) | (0.965) |
| Period | | $-0.134^{***}$ | -0.0261 |
| | | (0.0377) | (0.0544) |
| Institution x Period | | | $-0.206^{***}$ |
| | | | (0.0750) |
| _cons | $4.156^{***}$ | $5.752^{***}$ | $4.411^{***}$ |
| | (0.880) | (0.954) | (1.062) |
| *N* | 590 | 590 | 590 |

*Notes*: Random-effects panel regression. Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

This time trend is illustrated in Figure 4.7. That is, contributions of no-voters without institution start at a lower level and remain stable over time, while contributions of no-voters with institution start at a relatively higher level and decrease over time.

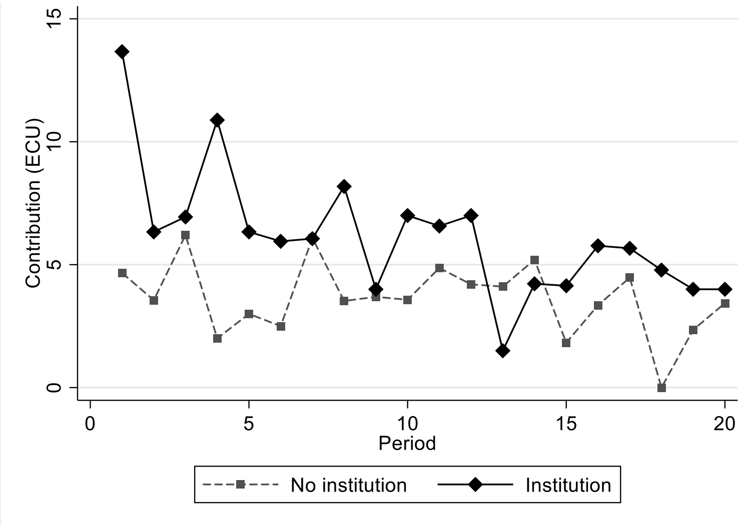



Figure 4.7: Average contributions of no-voters over time (exclusive setting)

Research question 4 can be answered as follows:

**Result 5**: In the exclusive setting, no-voters contribute more if an institution is formed than when it is not. The effect decreases over time.

Building upon the results from above, we now go more into detail about how no-voters' contributions depend on the coalition's size within their group. In other words, we look at how outsiders' contributions differ depending on the number of insiders in his group. We find that outsiders contribute significantly more in the case of three insiders (8.6 ECU) compared to a situation with two insiders (5.8 ECU; p=0.04, Wilcoxon signed-rank test). Additional panel-regressions confirm these results and show that a no-voter's contribution increases by 2.5 ECU if two of his group members commit to contribute to the public good compared to a setting without an institution. If the third group member is also an insider, the contributions increase by another 1.9 ECU on average. (Hypothesis 2b can be rejected.) Furthermore, the regressions reveal a negative time trend of contributions driven by those outsiders with two insiders (Table 4.3). For a subject, who wants to contribute 20 ECU independently of the formation of an institution, it is profitable to vote in favor of the institution. Even if the institution were formed without his vote, his benefit through the increase in the outsiders' contributions (1.89 ECU*0.65=1.23 ECU) would exceed his costs (1 ECU).

Table 4.3: Outsider‘s contribution depending on the number of insiders in his group (exclusive treatment)

| | (1)<br>Contribution | (2)<br>Contribution | (3)<br>Contribution | (4)<br>Contribution |
|---|---|---|---|---|
| 2 insiders | $2.546^{***}$ | $2.334^{***}$ | $4.535^{***}$ | $4.301^{***}$ |
| | (0.484) | (0.485) | (0.990) | (1.061) |
| A third insider | $1.889^{***}$ | $1.703^{***}$ | $1.561^{**}$ | $2.221^{*}$ |
| | (0.655) | (0.654) | (0.654) | (1.259) |
| Period | | $-0.126^{***}$ | -0.0263 | -0.0262 |
| | | (0.0376) | (0.0541) | (0.0541) |
| 2 insiders x period | | | $-0.191^{**}$ | $-0.169^{**}$ |
| | | | (0.0749) | (0.0831) |
| A third insider x period | | | | -0.0691 |
| | | | | (0.113) |
| _cons | $4.187^{***}$ | $5.678^{***}$ | $4.443^{***}$ | $4.453^{***}$ |
| | (0.883) | (0.958) | (1.065) | (1.072) |
| *N* | 590 | 590 | 590 | 590 |

Notes: Random-effects panel regression. Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Result 6**: In the exclusive setting, outsiders contribute more in the case of three insiders (8.6 ECU) compared to a situation with two insiders (5.8 ECU). The difference is statistically significant (p=0.04, Wilcoxon signed-rank test).

## 4.6 Conclusion

The provision of climate change mitigation can be enhanced by an effective institutional design that sanctions non-compliance with a contribution rule. However, implementing the ideal form of such an institution that applies a sanction to all those who benefit from the public good often fails in reality when it interferes with states' sovereignty or actors' individual freedom or requires unanimity. In this paper, we investigate the effectiveness of an alternative institution, which applies a sanctioning rule only to those who submitted to it.

To better understand the role such institutions can play in providing public goods, we design an experimental study. We investigate the endogenous formation and effectiveness of *inclusive and exclusive institutions* in the context of public good provision. We implement a two-stage laboratory experiment consisting of (i) a voting stage to form an institution and (ii) a public goods game. Once a costly institution is formed by a majority vote, a deterrent sanctioning is automatically enforced by a central authority. We vary to whom the sanctioning rule applies. In the *exclusive treatment,* only those subjects become insiders to the institution who voted in favor of it and thus committed to contributing to the public good. The others are outsiders, and although they benefit from the public good, they cannot be sanctioned for defecting. In the *inclusive treatment,* all members of a group become subject to the sanctioning rule if an institution is formed.

We find that giving participants the option to form an inclusive institution is significantly more welfare-enhancing than giving them the option of forming an exclusive institution. This has two reasons. First, inclusive institutions are more likely to be formed. Second, the contributions to the public good are higher if an inclusive institution is formed compared to an exclusive institution as the contribution rule governs all subjects of a group.

To shed more light on the effects of both settings, we compare the contribution behavior of yes- and no-voters for the case where no institution is formed. Given that no institution is formed in the exclusive treatment, we observe that yes-voters contribute significantly more than no-voters. In contrast to that, yes-voters contribute significantly less than no-voters in the inclusive treatment if no institution is formed. This suggests that different types of subjects vote to form an institution, depending on the coverage of the sanctioning rule. On the one hand, those subjects vote in favor of an exclusive institution who contribute more even if no institution is formed. On the other hand, those subjects who contribute less if no institution is formed are more likely to vote in favor of an inclusive institution. This might suggest that exclusive institutions are formed by rather pro-social subjects, whereas inclusive institutions are formed by more selfish subjects. This observation can be related to findings by Sutter, Haigner, and Kocher (2010), who investigate informal punishment (and reward) institutions. They observe that pro-social subjects select into endogenously implemented institutions so that cooperation under endogenous institutions is higher than under exogenous institutions. Our study allows investigating the influence of the type of an endogenous sanctioning institution (inclusive vs. exclusive) on this selection effect. Our observations in the exclusive treatment are in line with theirs. However, we observe that inclusive institutions are formed by subjects that contribute little if no institution is formed.

In our experiment, we inform subjects about their group members' voting behavior regarding the institution formation before they make their public good contributions. This allows us to shed light on the influence of a yes-voters' self-commitment on outsiders' contribution behavior in the exclusive treatment. We find that the formation of an institution positively affects the contributions of outsiders. The outsiders' (voluntary) contributions increase in the number of insiders. However, this effect wears off over time. This suggests a spillover effect of the exclusive institutions, enhancing their performance and making them more attractive. These findings may in parts be related to the literature on leadership in public good provision, which provides evidence that a high contribution of a leader positively affects the contribution of others.

## 4.7 Appendix to Chapter 4

The Appendix first provides and further analysis of the observed voting behavior in comparison to the equilibrium predictions. We then describe the technical details of the experimental procedure and provide the instructions of the experiment in a German (original) and an English (translated) version. This is followed by screenshots of the different stages of the experiment.

### a) *Supplementary figures and tables*

Table S4.4: Voting behavior by treatment.

| | (1) Yes-vote | (2) Yes-vote | (3) Yes-vote |
|---|---|---|---|
| Exclusive treatment | $-1.032^{***}$ | $-1.032^{***}$ | -0.355 |
| | (0.384) | (0.384) | (0.411) |
| Period | | 0.0000236 | $0.0375^{***}$ |
| | | (0.00631) | (0.00972) |
| Exclusive treatment x period | | | $-0.0676^{***}$ |
| | | | (0.0130) |
| _cons | $0.954^{***}$ | $0.954^{***}$ | $0.588^{**}$ |
| | (0.273) | (0.281) | (0.293) |
| *N* | 2400 | 2400 | 2400 |

*Notes*: Probit regression. Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

### b) Coordination and patterns in voting behavior

Comparing the observed data with the theoretically predicted equilibria in Figure 4.4 shows that in the inclusive treatment, the share of yes-votes is lower than the mixed strategy equilibrium prediction. In the exclusive treatment, it is higher. In both cases, the share of yes-votes seems to develop in the direction of the respective mixed strategy equilibrium. However, the share of yes-votes in the exclusive treatment is also relatively close to the coordinated equilibrium, in which always two group members vote in favor of the institution. To investigate further, if the voting behavior is instead in line with a coordinated or a mixed strategy equilibrium, we look at the distribution of the different voting behaviors in groups in detail. In doing so, we first present the distribution of votes in the observed data (histograms in the center of Figure S4.8 and Figure S4.9 below). In the histograms on the right-hand sides of each of the two Figures, we plot the distribution of yes-voters per group if subjects played the mixed strategy equilibrium and mix with the probabilities of 84.3% (T1) and 28.2% (T2), respectively. In the two histograms on the left of each Figure, we take the actual average voting behavior (T1: 66.92%; T2: 50.83%) and simulate the distribution of yes-voters per group under the assumption that subjects mix with this probability; this is we exclude any coordination in the direction of a pure strategy equilibrium. The main observation for both treatments is that there is no statistically significant difference between the observed data and the simulation (Two-sample Kolmogorov-Smirnov test). This means that there is no indication of coordination between subjects within a group in our data. We observe that the difference between the observed data and the mixed strategy equilibrium in the exclusive treatment is that subjects vote in favor of the institution more often than theoretically predicted. In contrast to that, in the inclusive treatment, subjects vote less often in favor of the institution than theoretically predicted.

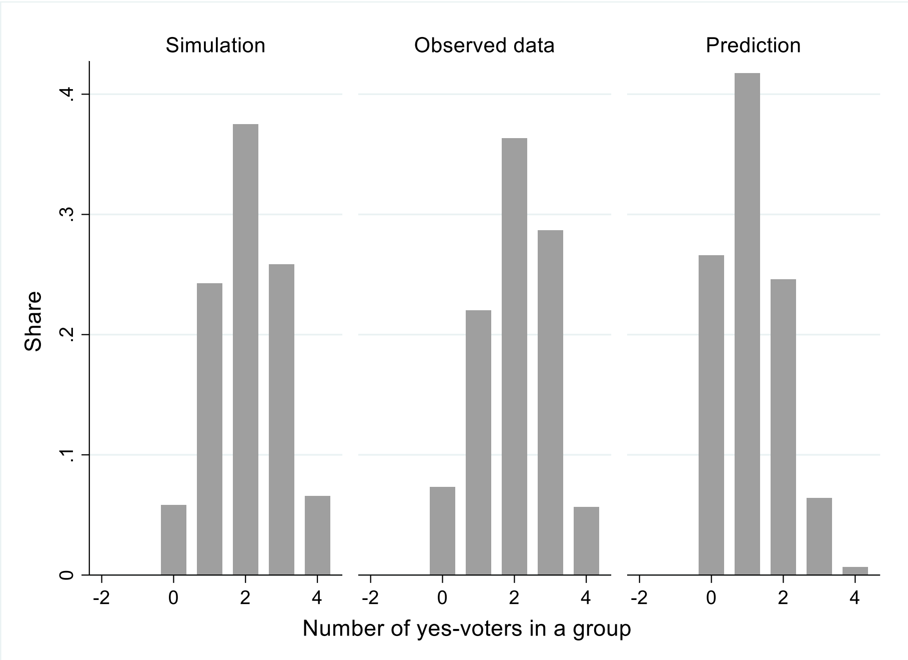



Figure S4.8: Distribution of yes-voters per group (Exclusive treatment)

*Notes:* Distribution of yes-voters per group (i) from observed data; (ii) if subjects played the mixed strategy equilibrium and mix with the calculated probability of 28.2% (see Section 4.4.2); (iii) if subjects played a mixed strategy with actual average voting behavior from the observed data.

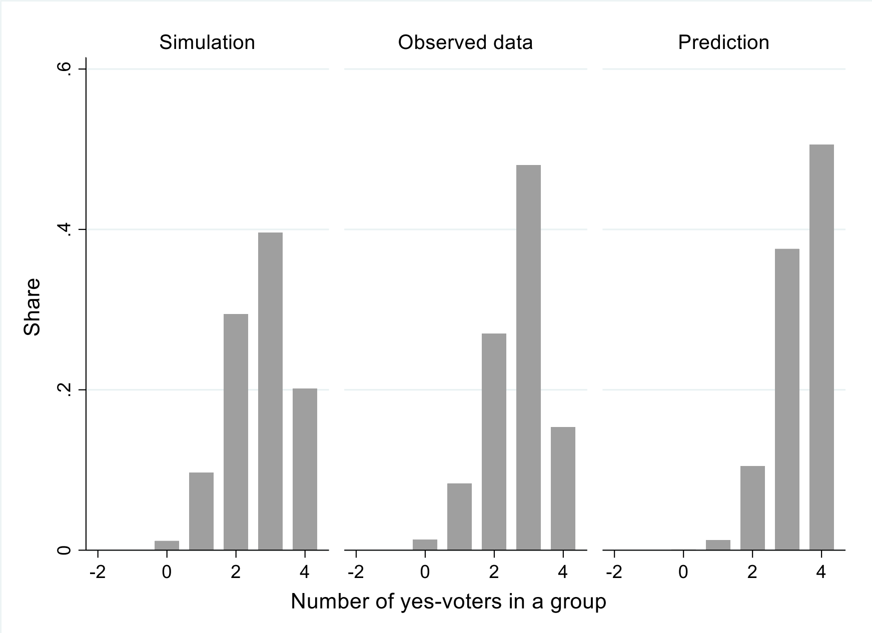



Figure S4.9: Distribution of yes-voters per group (Inclusive treatment)

*Notes:* Distribution of yes-voters per group (i) from observed data; (ii) if subjects played the mixed strategy equilibrium and mix with the calculated probability of 84.3% (see Section 4.4.2); (iii) if subjects played a mixed strategy with actual average voting behavior from the observed data.

The theoretical predictions are based on the assumption that subjects contribute 0 if they are outsiders and 20 if they are subject to the sanctioning institution (insiders). However, we observe deviations from these predictions in our experiment. As described above, in the inclusive treatment, insiders' contributions are close to 20 (18.9 ECU), and those of outsiders are larger than 0 (7.2 ECU). Consequently, the formation of an institution is less profitable than theoretically predicted, and in turn, the empirically optimal yes-voting rate is below the mixed strategy equilibrium rate. This is in line with our observation. We observe similar patterns in the exclusive treatment as insiders contribute close to 20 (18.9 ECU) and outsiders contribute more than 0 (5.2 ECU). Furthermore, the formation of an institution has a positive effect on outsiders’ contribution behavior. Thus, institution formation is more attractive than theoretically predicted, which can explain the observation that the observed yes-voting rate is higher than the theoretically predicted.

**Result**: In the inclusive setting, the share of yes-voters is significantly smaller than the mixed strategy equilibrium (p=0.04, Mann-Whitney-U-test). In the exclusive setting, the respective share is significantly larger (p=0.04, Mann-Whitney-U-test).

We now turn to patterns in voting behavior. Investigating the individual voting behavior over time, we find that about half of the subjects submit the same vote in each period. In the inclusive treatment, 46.7% of subjects vote yes in at least 90% of all 20 periods, and 10% always vote no. The pattern is similar but less distinctive in the exclusive treatment, with 28.3% of subjects voting yes in at least 90% of the periods and 20% always voting no (Figure S4.10).

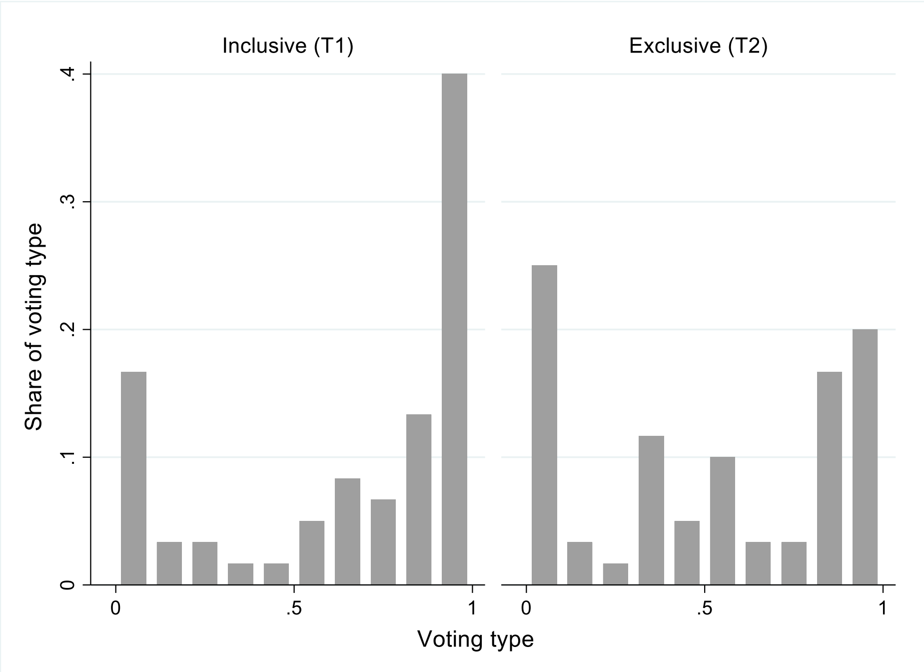



Figure S4.10: Distribution of patterns in voting behavior

*Notes:* Voting types are defined by the share of a subject's yes votes over all periods.

From these data, we can say that about half of the subjects show a relatively stable voting behavior. In the following, we investigate if being pivotal for forming an institution in the last period influences the subjects' voting behavior. A subject who was pivotal in the previous round might assume to be pivotal again and thus be more likely to vote yes in the next round (Table S4.5). We do not find that subjects who had been pivotal for institution formation in the previous round are more likely to vote in favor of it in the next round.

Table S4.5: Voting behavior of a subject who was pivotal for institution formation in the previous round

| | (1) Inclusive Yes-vote | (2) Exclusive Yes-vote |
|---|---|---|
| Subject pivotal | 0.301 | 0.0884 |
| | (0.191) | (0.136) |
| Period | $0.0371^{***}$ | $-0.0271^{***}$ |
| | (0.0108) | (0.00936) |
| _cons | $0.708^{**}$ | 0.185 |
| | (0.339) | (0.278) |
| *N* | 1140 | 1140 |

*Notes*: The variable 'subject pivotal' is 1 if the subject had voted no in the previous round and one of his group members had voted yes. This is the subject would have been pivotal for the formation of an institution. Standard errors in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

### *c)* Experimental Procedure

The experiment was initially planned for implementation in the rooms of the Cologne Laboratory for Experimental Economic Research at the University of Cologne. However, due to the Corona pandemic, the laboratory was closed as of March 2020, so the experiment was transferred to an online format. For this purpose, participants were invited to the respective sessions by mail via ORSEE (Greiner, 2004) as usual. With the confirmation email, they received a link to a Zoom webinar. The settings in Zoom were such that the participants of the experiment could only see and hear the experimenter via video. They could not turn on their video and only communicate with the experimenter and only via the chat function. This way, we could ensure that the participants were anonymous for each other and could not communicate with each other. After the attendees had been checked against the registration list to ensure that only those who had registered could participate, participants were randomly selected using a random number generator. (This was done using the following website, which randomly draws a specified number of observations from a total set using the PHP function random_int(): https://www.ultimatesolver.com/de/zufall-teilmenge) The selected participants were each sent a link to the experiment via Zoom chat. The experiment took place entirely through otree. Upon completing the experiment, participants were automatically redirected to a second app programmed in otree, which retrieved participant data for payout and stored it on a separate server. Those who were not drawn to participate received a direct link to the payout form. All subjects were paid out via PayPal. In order to test the organizational and technical processes, two test sessions were conducted in advance. These data are not reported here. In order to avoid the loss of a session in case a participant would be logged out of the experiment for technical reasons, two reserve participants were appointed in each session. They received the same instructions and comprehension questions and were on stand by to be able to step in at any time. However, they were not called in any session.

*d) Instructions*

This section provides the instructions in German (original) and English (translated) for treatment 1 and 2. The instructions for the two treatment differ in one paragraph only. Thus, we will first present the instructions for T1 as a whole and then for T2 only the paragraph in which the instructions for T2 deviate from T1.

Herzlich Willkommen!

Zum Ablauf:

1. Im Folgenden werden Ihnen zunächst die **Instruktionen** zu diesem Experiment angezeigt. Bitte lesen Sie diese sorgfältig durch. Es ist wichtig, dass Sie diese richtig verstehen.
2. Nach Abschluss der Instruktionen werden Ihnen **Verständnisfragen** gestellt. Es geht weiter, sobald alle Teilnehmer alle Fragen korrekt beantworten haben.
3. Danach beginnt das eigentliche Experiment mit insgesamt 20 Runden.

Falls Sie Fragen zum Experiment haben, können Sie diese jederzeit per Chat in Zoom an die Experimentatoren richten (im Adressfeld des Chats steht richtigerweise ‚Alle Diskussionsteilnehmer‘). Ihre Nachrichten im Chat sind für die anderen Experimentteilnehmer nicht sichtbar.

Das Experiment dauert voraussichtlich eine Stunde.

Vielen Dank für Ihre Teilnahme.

**Instruktionen**

In diesem Experiment können Sie in Abhängigkeit von Ihren Entscheidungen und den Entscheidungen der anderen Teilnehmer Geld verdienen. Ihre Entscheidungen im Experiment treffen Sie **anonym**. Nur der Experimentator erfährt Ihre Identität, wobei Ihre Angaben vertraulich behandelt werden.

Während des Experiments wird Ihre Auszahlung in experimentellen Währungseinheiten (ECU) angegeben. Nach dem Experiment werden Ihre ECU-Punkte zu einem Kurs von

50 ECU = 1 Euro

in Euro eingetauscht und ausgezahlt.

Zusätzlich zu den Auszahlungen, die Sie auf der Grundlage Ihrer Entscheidungen während des Experiments erzielen, erhalten Sie ein Teilnahmeentgelt in Höhe von 2,50 Euro.

Jeder Teilnehmer erhält zu Beginn des Experiments ein Startkapital von 50 ECU. Diese einmalige Zahlung kann für eventuelle Verluste während des Experiments verwendet werden. Sie können Verluste jedoch jederzeit vermeiden. Während des Experiments wird Ihnen in jeder Runde Ihr jeweils aktueller Kontostand inklusive des Startkapitals angezeigt.

Nach dem Experiment werden Ihre gesamten Auszahlungen aus dem Experiment plus das Startkapital und das Teilnahmeentgelt per PayPal ausbezahlt.

Das Experiment besteht aus 20 Runden. Zu Beginn jeder Runde werden die Teilnehmer zufällig in Vierergruppen eingeteilt. Sie agieren also in einer Gruppe mit 3 weiteren Teilnehmern. Die Zusammensetzung der Gruppen ändert sich zufällig zu Beginn jeder Runde. In jeder Runde wird Ihre Gruppe daher aus verschiedenen Teilnehmern bestehen.

Sie erhalten keine Informationen über die Identität der anderen Teilnehmer, weder während noch nach dem Experiment. Auch die anderen Teilnehmer erhalten keine Informationen über Ihre Identität. Alle Interaktionen in diesem Experiment erfolgen anonym.

In jeder Runde besteht das Experiment aus zwei Stufen. Wir stellen den Ablauf zunächst rückwärts dar: In der zweiten Stufe müssen Sie entscheiden, wie viele ECU Sie zu einem gemeinsamen Projekt Ihrer Gruppe beitragen. In der ersten Stufe müssen Sie gemeinsam mit den anderen Mitgliedern Ihrer Gruppe entscheiden, ob Sie für die zweite Stufe eine Beitragsregel einführen und einen Verwalter einsetzen wollen, der diese Regel durchsetzt. Die Rolle des Verwalters übernimmt der Computer.

Im Folgenden wird der Ablauf des Experiments im Detail beschrieben:

**Detaillierte Informationen zum Experiment**

In jeder Runde stellt sich die Situation für jeden Teilnehmer wie folgt dar:

Stufe 1 (nur T1)

In der **ersten Stufe** jeder Runde erhalten alle Mitglieder einer Gruppe jeweils 1 ECU und stimmen darüber ab, ob sie für Stufe 2 eine Beitragsregel einführen und einen Verwalter einsetzen wollen. Die Beitragsregel besagt, dass in Stufe 2 jedes Gruppenmitglied 20 ECU zum gemeinsamen Projekt beigetragen muss. Der Verwalter (simuliert durch den Computer) kontrolliert, ob die Beitragsregel eingehalten wurde und führt bei Nichteinhaltung einen Punktabzug durch.

> **Die Beitragsregel und damit der Verwalter werden eingeführt, wenn mindestens 2 Gruppenmitglieder für diese stimmen.** Alle Gruppenmitglieder, die für die Beitragsregel gestimmt haben, zahlen je 1 ECU. Wenn ein Verwalter eingesetzt wird, sind alle Mitglieder der Gruppe dazu verpflichtet in der 2. Stufe 20 ECU zum gemeinsamen Projekt beizutragen. Halten sie sich nicht an diese Regel, werden ihnen nach der 2. Stufe jeweils 15 ECU abgezogen.
>
> **Die Beitragsregel und der Verwalter werden nicht eingeführt, wenn weniger als 2 Teilnehmer für diese stimmen.** Wird der Verwalter nicht eingesetzt, zahlt kein Teilnehmer für den Verwalter und es entsteht keine Beitragsregel für die Entscheidung in Stufe 2.

Ablauf: Alle Gruppenmitglieder stimmen gleichzeitig und anonym darüber ab, ob sie eine Beitragsregel und damit einen Verwalter einsetzen möchten, oder nicht. Nachdem alle Gruppenmitglieder abgestimmt haben, werden sie über das Ergebnis informiert, ob für Stufe 2 eine Beitragsregel gilt und ein Verwalter eingesetzt wird.

Stufe 2

In der **zweiten Stufe** jeder Runde erhalten alle Mitglieder einer Gruppe jeweils 20 ECU. Ihre Aufgabe besteht darin, diese zwischen einem privaten und einem gemeinsamen Projekt der Gruppe aufzuteilen. Diese Projekte unterscheiden sich in der Auszahlung für Sie und die Mitglieder Ihrer Gruppe wie folgt:

- Für jeden ECU, den Sie zum **privaten Projekt** beigetragen haben, **erhalten Sie 1 ECU**.
- Jeder ECU, den Sie oder ein anderes Mitglied Ihrer Gruppe zum **gemeinsamen Projekt** beigetragen haben, wird mit 2,6 multipliziert und gleichmäßig zwischen allen vier Gruppenmitgliedern aufgeteilt. D.h. für jeden ECU, den Sie oder ein anderes Gruppenmitglied in das gemeinsame Projekt beigetragen haben, **erhält jedes Gruppenmitglied 0,65 ECU**.

Die Einnahmen aus dem gemeinsamen Projekt werden nach dieser Formel für alle Gruppenmitglieder berechnet. Bitte beachten Sie: Jedes Gruppenmitglied erhält die gleichen Einnahmen aus dem gemeinsamen Projekt, unabhängig davon, wie viel es eingezahlt hat, d.h. jedes Gruppenmitglied profitiert von allen Beiträgen zum gemeinsamen Projekt.

Ablauf: Alle Gruppenmitglieder geben gleichzeitig und anonym an, wieviel sie zu dem gemeinsamen Projekt beitragen möchten. Der Anteil der 20 ECU, die sie nicht zum gemeinsamen Projekt beitragen, geht automatisch in das private Projekt jedes Gruppenmitglieds. Die Summe aus den ECU, die sie zum privaten Projekt beitragen plus die ECU, die sie zum gemeinsamen Projekt beitragen, beträgt also jeweils 20 ECU. Nachdem alle Gruppenmitglieder ihren Beitrag eingegeben haben, werden sie informiert, was die anderen Mitglieder ihrer Gruppe zum gemeinsamen Projekt beigetragen haben, ob es gegebenenfalls einen Punktabzug gibt und wie hoch ihre Auszahlung in der jeweiligen Runde ist.

Wir erläutern nun, aus welchen einzelnen Bestandteilen Ihre **Auszahlung in jeder Runde** zusammengesetzt ist.

1. Stufe:

- 1 ECU, falls der Verwalter nicht eingesetzt wird oder Sie dagegen gestimmt haben.
- 0 ECU, falls der Verwalter eingesetzt wird und Sie dafür gestimmt haben.

2. Stufe:

- Ihr Beitrag zum privaten Projekt + 0,65 * Gesamtbeiträge aller Mitglieder Ihrer Gruppe ins gemeinsame Projekt
    - 15 ECU Abzug, wenn für Sie eine Beitragsregel gilt und Sie sich nicht daran halten.

**<u>Zusammenfassung:</u>**

- Das Experiment umfasst 20 Runden, die jeweils aus 2 Stufen bestehen.
- Zu Beginn jeder Runde werden die Teilnehmer zufällig in Vierergruppen eingeteilt.
    - Entscheidung in der 1. Stufe: Alle Teilnehmer stimmen gleichzeitig und anonym über das Einsetzen einer Beitragsregel und eines Verwalters ab.
    - Entscheidung in der 2. Stufe: Alle Teilnehmer beobachten das Abstimmungsergebnis ihrer Gruppe und treffen die Beitragsentscheidungen für das private und das gemeinsame Projekt.
    - Alle Teilnehmer werden über die Beiträge der anderen Mitglieder Ihrer Gruppe informiert. Außerdem erhalten sie Informationen über einen möglichen Punktabzug und ihre Auszahlung in der Runde.
- Auszahlung eines Spielers in einer Runde
  = 1 ECU + 20 ECU
  - Beitrag für den Verwalter (Wert entweder 0 oder 1)
  - Beitrag zum privaten Projekt (Wert zwischen 0 und 20)
  - Beitrag zum gemeinsamen Projekt (Wert zwischen 0 und 20)

+ Auszahlung aus dem privaten Projekt (1*Wert zwischen 0 und 20)
+ Auszahlung aus dem gemeinsamen Projekt (0,65*Gesamtbeiträge aller Gruppenmitglieder zwischen 0 und 80)
- Punktabzug bei Nichteinhaltung einer geltenden Beitragsregel (Wert entweder 0 oder 15)

Paragraph on stage 1 for T2:

Stufe 1 (only T2)

In der **ersten Stufe** jeder Runde erhalten alle Mitglieder einer Gruppe jeweils 1 ECU und stimmen darüber ab, ob sie für Stufe 2 eine Beitragsregel einführen und einen Verwalter einsetzen wollen. Die Beitragsregel besagt, dass in Stufe 2 jedes Gruppenmitglied, das dafür gestimmt hat, 20 ECU zum gemeinsamen Projekt beigetragen muss. Der Verwalter (simuliert durch den Computer) kontrolliert, ob die Beitragsregel eingehalten wurde und führt bei Nichteinhaltung einen Punktabzug durch.

**Die Beitragsregel und damit der Verwalter werden eingeführt, wenn mindestens 2 Gruppenmitglieder für diese stimmen.** Alle Gruppenmitglieder, die für die Beitragsregel gestimmt haben, zahlen je 1 ECU. Wenn ein Verwalter eingesetzt wird, sind diejenigen Mitglieder der Gruppe, die für die Beitragsregel gestimmt haben, dazu verpflichtet in der 2. Stufe 20 ECU zum gemeinsamen Projekt beizutragen. Halten sie sich nicht an diese Regel, werden ihnen nach der 2. Stufe jeweils 15 ECU abgezogen.

**Die Beitragsregel und der Verwalter werden nicht eingeführt, wenn weniger als 2 Teilnehmer für diese stimmen.** Wird der Verwalter nicht eingesetzt, zahlt kein Teilnehmer für den Verwalter und es entsteht keine Beitragsregel für die Entscheidung in Stufe 2.

Ablauf: Alle Gruppenmitglieder stimmen gleichzeitig und anonym darüber ab, ob sie eine Beitragsregel und damit einen Verwalter einsetzen möchten, oder nicht. Nachdem alle Gruppenmitglieder abgestimmt haben, werden sie über das Ergebnis informiert, ob für Stufe 2 eine Beitragsregel gilt und ein Verwalter eingesetzt wird.

Welcome!

About the procedure:

1. In the following, you will first see the instructions for this experiment. Please read them carefully. It is important that you understand them correctly.
2. After completing the instructions, you will be asked comprehension questions. It will continue as soon as all participants have answered all questions correctly.
3. After that, the actual experiment will begin with a total of 20 rounds.

If you have questions about the experiment, you can always send them to the experimenters via chat in Zoom (the address field of the chat correctly says 'All panelists'). Your messages in the chat will not be visible to the other experiment participants.

The experiment is expected to last one hour.

Thank you for your participation.

## Instructions

In this experiment you can earn money depending on your decisions and the decisions of the other participants. You make your decisions in the experiment **anonymously**. Only the experimenter will know your identity, and your information will be kept confidential.

During the experiment, your payoff will be in experimental currency units (ECU). After the experiment, your ECU points will be exchanged at a rate of

50 ECU = 1 Euro

for Euros and paid out.

In addition to the payoffs, which you receive based on your decisions during the experiment, you will receive a participation fee of 2.50 euros.

Each participant will receive a seed money of 50 ECU at the beginning of the experiment. This one-time payment can be used for any losses during the experiment. However, you can avoid losses at any time. During the experiment, your current account balance including the seed money will be displayed in each round. After the experiment, your total payoffs from the experiment plus the seed money and the participation fee will be paid out via PayPal.

The experiment consists of 20 rounds. At the beginning of each round, participants are randomly divided into groups of four. So they act in a group with 3 other participants. The composition of the groups changes randomly at the beginning of each round. Therefore, in each round your group will consist of different participants.

You will not receive any information about the identity of the other participants, neither during nor after the experiment. The other participants will also not receive any information about your identity. All interactions in this experiment will be anonymous.

In each round, the experiment consists of two stages. We first present the process backwards: In the second stage, you have to decide how many ECU you contribute to a joint project of your group. In the first stage, you must decide together with the other members of your group, whether to introduce a contribution rule for the second stage and appoint an administrator to enforce that rule. The role of the administrator is played by the computer.

The following is a detailed description of how the experiment works:

### Detailed information about the experiment

In each round, the situation for each participant is as follows:

<u>Stage 1 (T1 only).</u>

In the **first stage** of each round, all members of a group receive 1 ECU each and vote on whether to implement a contribution rule and appoint an administrator for stage 2. The contribution rule states that in stage 2, each group member must contribute 20 ECU to the joint project. The administrator (simulated by the computer) checks whether the contribution rule has been complied and deducts points in case of non-compliance.

> **The contribution rule and thus the administrator are introduced if at least 2 group members vote for it.** All group members who voted for the contribution rule pay 1 ECU each. If an administrator is introduced, all members of the group are obliged to contribute 20 ECU to the joint project in the 2nd stage. If they do not comply with this rule, 15 ECU will be deducted from each of them after the 2nd stage.
>
> **The contribution rule and the administrator will not be implemented if less than 2 participants vote for it.** If the administrator is not implemented, no participant will pay for the administrator and no contribution rule will be implemented for the decision in stage 2.

Procedure: All group members vote simultaneously and anonymously on whether or not to implement a contribution rule and thus an administrator. After all group members have voted, they are informed about the result, whether a contribution rule applies for stage 2 and an administrator is used.

Stage 2

In the **second stage** of each round, all members of a group receive 20 ECU each. Your task is to divide this between a private project and a joint project of the group. These projects differ in payoff for you and the members of your group as follows:

- For every ECU you contributed to the **private project**, **you will receive 1 ECU**.
- Each ECU that you or another member of your group contributed to the **joint project** will be multiplied by 2.6 and divided equally between all four group members. That is, for each ECU you or another group member contributed to the joint project, **each group member will receive 0.65 ECU.**

The income from the joint project is calculated according to this formula for all group members. Please note: Each group member receives the same income from the joint project regardless of how much they have contributed, i.e. each group member benefits from all contributions to the joint project.

Procedure: All group members simultaneously and anonymously indicate how much they would like to contribute to the joint project. The part of the 20 ECU they do not contribute to the joint project automatically goes to the private project of each group member. So the sum of the ECU they contribute to the private project plus the ECU they contribute to the joint project is 20 ECU each. After all group members have entered their contribution, they are informed what the other members of their group have contributed to the joint project, if there is a point deduction, and what their payoff is in that round.

We will now explain the individual components that make up your **payoff in each round**.

1st stage:

- 1 ECU if the administrator is not appointed or you voted against it.
- 0 ECU, if the administrator is appointed and you voted for it.

2nd stage:

- Your contribution to the private project + 0.65 * total contributions of all members of your group into the joint project.
  - 15 ECU deduction if there is a contribution rule for you and you do not follow it.

**Summary:**

- The experiment includes 20 rounds, each consisting of 2 stages.
- At the beginning of each round, participants are randomly divided into groups of four.
  - Decision in the 1st stage: all participants vote simultaneously and anonymously on the appointment of a contribution rule and an administrator.
  - Decision in the 2nd stage: all participants observe the voting result of their group and make the contribution decisions for the private and the joint project.
  - All participants are informed about the contributions of the other members of their group. They will also receive information about a possible point deduction and their payoff in the round.
- Payoff of a player in a round
  = 1 ECU + 20 ECU
  - Contribution to the administrator (value either 0 or 1)
  - Contribution to the private project (value between 0 and 20)
  - Contribution to the common project (value between 0 and 20)
  + payout from the private project (1*value between 0 and 20)
  + payout from the joint project (0.65*total contributions of all group members between 0 and 80)
  - Point deduction for non-compliance with an applicable contribution rule (value either 0 or 15).

Paragraph on stage 1 for T2:

<u>Stage 1 (only T2)</u>

In the **first stage** of each round, all members of a group receive 1 ECU each and vote on whether they want to introduce a contribution rule and appoint an administrator for stage 2. The contribution rule states that in stage 2, each group member who voted in favor of the rule must contribute 20 ECU to the joint project. The administrator (simulated by the computer) checks if the contribution rule has been respected and performs a point deduction in case of non-compliance.

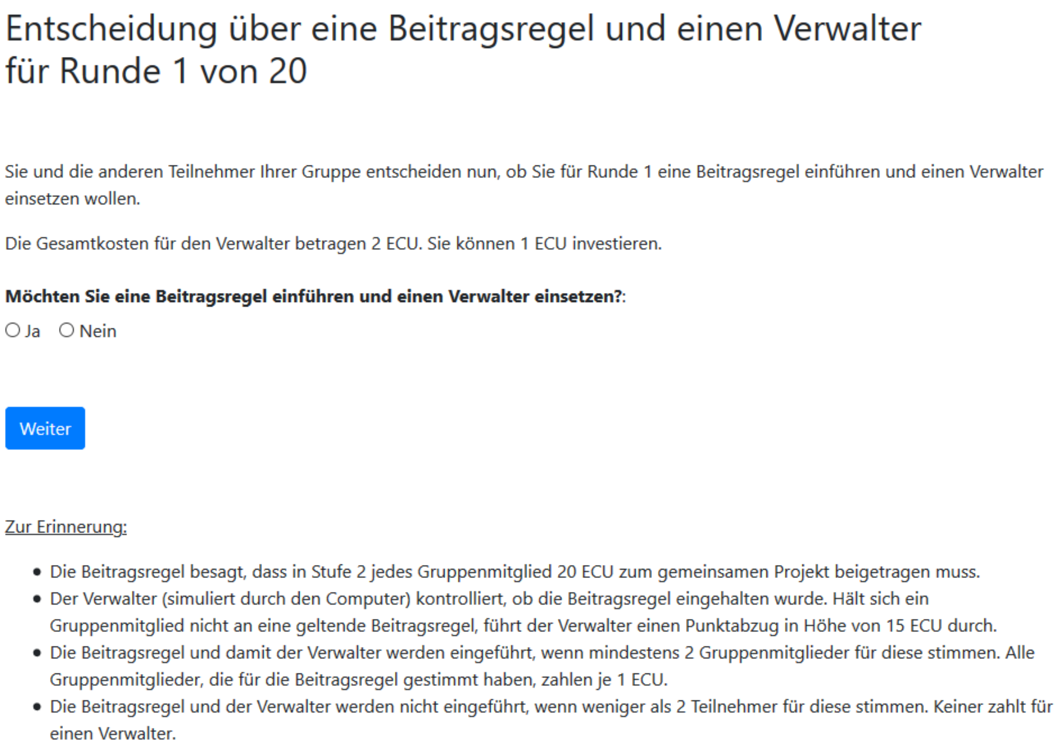
**The contribution rule and thus the administrator are introduced if at least 2 group members vote for it.** All group members who voted in favor of the contribution rule pay 1 ECU each. If an administrator is established, those members of the group who voted in favor of the contribution rule are obliged to contribute 20 ECU to the joint project in the 2nd stage. If they do not comply with this rule, 15 ECU will be deducted from each of them after the 2nd stage.

**The contribution rule and the administrator will not be implemented if less than 2 participants vote for them.** If the administrator is not implemented, no participant will pay for the administrator and no contribution rule will be created for the decision in stage 2.

Procedure: All group members vote simultaneously and anonymously on whether or not to implement a contribution rule and thus an administrator. After all group members have voted, they are informed about the result, whether a contribution rule applies for stage 2 and an administrator is used.

e) Screenshots of the experiment for the inclusive treatment (T1) and exclusive treatment (T2).

**1.** The voting stage was similar for T1 and T2:



## Entscheidung über eine Beitragsregel und einen Verwalter für Runde 1 von 20

Sie und die anderen Teilnehmer Ihrer Gruppe entscheiden nun, ob Sie für Runde 1 eine Beitragsregel einführen und einen Verwalter einsetzen wollen.

Die Gesamtkosten für den Verwalter betragen 2 ECU. Sie können 1 ECU investieren.

**Möchten Sie eine Beitragsregel einführen und einen Verwalter einsetzen?**:

○ Ja ○ Nein

Weiter

Zur Erinnerung:

- Die Beitragsregel besagt, dass in Stufe 2 jedes Gruppenmitglied 20 ECU zum gemeinsamen Projekt beigetragen muss.
- Der Verwalter (simuliert durch den Computer) kontrolliert, ob die Beitragsregel eingehalten wurde. Hält sich ein Gruppenmitglied nicht an eine geltende Beitragsregel, führt der Verwalter einen Punktabzug in Höhe von 15 ECU durch.
- Die Beitragsregel und damit der Verwalter werden eingeführt, wenn mindestens 2 Gruppenmitglieder für diese stimmen. Alle Gruppenmitglieder, die für die Beitragsregel gestimmt haben, zahlen je 1 ECU.
- Die Beitragsregel und der Verwalter werden nicht eingeführt, wenn weniger als 2 Teilnehmer für diese stimmen. Keiner zahlt für einen Verwalter.

**2.** Display of the result of the voting decision (T2):

# Ergebnis der Abstimmung für Runde 1 von 20

Sie haben **gegen** eine Beitragsregel gestimmt.

Anzahl der anderen Gruppenmitglieder, die für die Beitragsregel gestimmt haben: **2**

Ergebnis der Abstimmungsentscheidung Ihrer Gruppe:

**Für 2 Mitglieder Ihrer Gruppe gilt in dieser Runde eine Beitragsregel. Für Sie gilt keine Regel.**

Ihre Kosten für den Verwalter betragen: 0 ECU.

Weiter

**3.** Contribution stage (T2):

# Investitionsentscheidung in Runde 1 von 20

**Für 3 Mitglieder Ihrer Gruppe gilt in dieser Runde eine Beitragsregel. Für Sie gilt keine Regel.**

Die Beitragsregel besagt, dass 20 ECU zum gemeinsamen Projekt beigetragen werden müssen. Wird die Regel nicht eingehalten, werden nach der 2. Stufe 15 ECU abgezogen.

Sie haben gegen eine Beitragsregel gestimmt und Ihre Kosten für den Verwalter betragen 0 ECU.

Sie und die anderen Mitglieder Ihrer Gruppe entscheiden nun, wieviel Sie von 20 ECU jeweils zum privaten und zum gemeinsamen Projekt beitragen möchten. Mit dem Rechner unten können Sie für verschiedene Szenarien Ihre Auszahlung in dieser Runde bestimmen.

Der Anteil der 20 ECU, den Sie nicht zum gemeinsamen Projekt beitragen, geht automatisch in Ihr privates Projekt.

**Wieviel möchten Sie zum gemeinsamen Projekt beitragen?**

0

Weiter

**Ihr aktueller Kontostand (inkl. Startkapital) beträgt: 60 ECU**

Zur Erinnerung:

- Für jeden ECU, den Sie zum privaten Projekt beigetragen haben, erhalten Sie 1 ECU.
- Jeder ECU, den Sie oder ein anderes Mitglied Ihrer Gruppe zum gemeinsamen Projekt beigetragen haben, wird mit 2,6 multipliziert und gleichmäßig zwischen allen vier Gruppenmitgliedern aufgeteilt. D.h. für jeden ECU, den Sie oder ein anderes Gruppenmitglied in das gemeinsame Projekt beigetragen haben, erhält jedes Gruppenmitglied 0,65 ECU.

Rechner:

Ihr Beitrag zum gemeinsamen Projekt (0-20):

Gesamtbeitrag der anderen 3 Gruppenmitglieder zum gemeinsamen Projekt (0-60):

Berechne

In diesem Szenario würde Ihre Auszahlung in dieser Runde ECU betragen .

**4.** Results of the contribution decision (T2):

## Ergebnis Beitragsentscheidung in Runde 1 von 20

**Für Sie und 1 weiteres Mitglied Ihrer Gruppe galt in dieser Runde eine Beitragsregel.**

Ihr Beitrag zum gemeinsamen Projekt: 18 ECU

Gesamtbeitrag der anderen Gruppenmitglieder zum gemeinsamen Projekt: 0 ECU

Gesamtauszahlung des gemeinsamen Projekts (= 18 ECU * 2,6): 46,80 ECU

Individuelle Auszahlung jedes Gruppenmitglieds aus dem gemeinsamen Projekt (= Gesamtauszahlung/4): 11,70 ECU

Ihre Auszahlung in dieser Runde beträgt: 21 – 1 – 2 – 18 + 2 + 11,70 – 15 = -1,30 ECU

= 1 ECU + 20 ECU

- Beitrag für den Verwalter: 1 ECU

- Beitrag zum privaten Projekt: 2 ECU

- Beitrag zum gemeinsamen Projekt: 18 ECU

+ Auszahlung aus dem privaten Projekt: 2 ECU

+ Auszahlung aus dem gemeinsamen Projekt: 11,70 ECU

- Punktabzug bei Nichteinhaltung der Regel: 15 ECU

Ihr aktueller Kontostand (inkl. Startkapital) beträgt: 58,70 ECU

| Runde | Beitragsregel und Verwalter für Sie | Anzahl weiterer Gruppenmitglieder mit Beitragsregel und Verwalter | Ihr Beitrag zum gemeinsamen Projekt | Gesamtbeitrag der anderen Gruppenmitglieder zum gemeinsamen Projekt | Anteil jedes Gruppenmitglieds aus dem gemeinsamen Projekt | Ihr Punktabzug | Ihre Auszahlung |
|---|---|---|---|---|---|---|---|
| 1 | Ja | 1 | 18 ECU | 0 ECU | 11,70 ECU | 15 ECU | -1,30 ECU |

**5.** Display of the result of the voting decision (T1):

# Ergebnis der Abstimmung für Runde 1 von 20

Sie haben **gegen** eine Beitragsregel gestimmt.

Anzahl der anderen Gruppenmitglieder, die für die Beitragsregel gestimmt haben: **3**

Ergebnis der Abstimmungsentscheidung Ihrer Gruppe:

**Für Sie und die anderen Mitglieder Ihrer Gruppe gilt in dieser Runde eine Beitragsregel.**

Ihre Kosten für den Verwalter betragen: 0 ECU.

Weiter

**6.** Results of the contribution decision (T1):

# Ergebnis Beitragsentscheidung in Runde 1 von 20

**Für Sie und die anderen Mitglieder Ihrer Gruppe galt in dieser Runde die Beitragsregel.**

Ihr Beitrag zum gemeinsamen Projekt: 20 ECU

Gesamtbeitrag der anderen Gruppenmitglieder zum gemeinsamen Projekt: 0 ECU

Gesamtauszahlung des gemeinsamen Projekts (= 20 ECU * 2,6): 52,00 ECU

Individuelle Auszahlung jedes Gruppenmitglieds aus dem gemeinsamen Projekt (= Gesamtauszahlung/4): 13,00 ECU

Ihre Auszahlung in dieser Runde beträgt: 21 – 1 – 0 – 20 + 0 + 13,00 – 0 = 13,00 ECU

= 1 ECU + 20 ECU

- Beitrag für den Verwalter: 1 ECU

- Beitrag zum privaten Projekt: 0 ECU

- Beitrag zum gemeinsamen Projekt: 20 ECU

+ Auszahlung aus dem privaten Projekt: 0 ECU

+ Auszahlung aus dem gemeinsamen Projekt: 13,00 ECU

- Punktabzug bei Nichteinhaltung der Regel: 0 ECU

Ihr aktueller Kontostand (inkl. Startkapital) beträgt: 73,00 ECU

| Runde | Beitragsregel und Verwalter eingesetzt | Ihr Beitrag zum gemeinsamen Projekt | Gesamtbeitrag der anderen Gruppenmitglieder zum gemeinsamen Projekt | Anteil jedes Gruppenmitglieds aus dem gemeinsamen Projekt | Ihr Punktabzug | Ihre Auszahlung |
|---|---|---|---|---|---|---|
| 1 | Ja | 20 ECU | 0 ECU | 13,00 ECU | 0 ECU | 13,00 ECU |

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