PROVIDING AND CONSERVING COLLECTIVE GOODS

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To my family.

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CONTENTS

CHAPTER 1

INTRODUCTION

The provision and conservation of collective goods is challenging. In several instances, individuals' objectives conflict with their common interests. Already in 1965, Mancur Olson stated that "unless the number of individuals in a group is quite small, or unless there is coercion or some other special device to make individuals act in their common interest, rational, self-interested individuals will not act to achieve their common or group interests" (Olson, 1965, p.2). In consequence, a social dilemma may arise. One of the major challenges to behavioral and experimental economists is to determine the circumstances under which self-interest leads to inefficient collective outcomes, and which are the mechanisms and institutions that help to overcome these inefficiencies.

Collective goods are multifaceted. First, they differ in their manifestation. From playgrounds and parks in the neighborhood over public safety to a clean environment and stable climate – collective goods take several forms. Their manifestation determines the channel through which inefficiencies arise. Either, individuals contribute inefficiently low amounts to the provision of the collective good, or consume inefficiently high amounts of the collective good. The provision and conservation of collective goods can exhibit costs for the individual that differ strongly with context, and also between individuals. To investigate why the provision and conservation of certain collective goods fails and how it can be achieved, research needs to take into account these and other facets.

In recent decades, behavioral and experimental economics have importantly contributed to our understanding of the inefficiencies that arise in the context of collective goods provision and conservation. However, political and cultural changes as well as technological advancements constantly lead to the emergence of new social dilemmas. These cannot always be properly addressed by existing economic theory of human behavior. One such challenge to existing theory is the rise of information and knowledge as the key drivers of economic growth (OECD, 2013). Economic experiments can importantly contribute to our understanding of structural differences in decision-making in the digitized economy, thereby guiding the development of behavioral economics theory.

This thesis focuses on novel challenges and chances with regard to collective goods provision and conservation in the digitized economy. Information has a key role in a broad range of newly emerging social dilemmas. The advances in data collection and data mining have given rise to collective goods that rely on the voluntary provision of personal information. Instead of charging user fees, providers of information-based services collect customers' personal data in exchange for the usage of services. The collected data, in turn, increases the quality of services to the benefit of all customers. However, the recent privacy and data protection debate indicates that information-based collective goods may be severely distorted due to implicit costs of provision. In consequence, we are confronted with a social dilemma which is structurally different from existing paradigms.

The rise of knowledge as a key driver of economic growth also confronts us with novel challenges. In contrast to collective goods problems considered by the standard literature, knowledge creation is characterized by an important difference. Contributions are frequently not verifiable ex post. In consequence, the surplus from cooperation may be subject to subsequent competition. Given that economic growth increasingly relies on "knowledge capital" (OECD, 2013), it is crucial to determine the circumstances under which the provision of knowledge-based collective goods may be distorted.

There is ongoing exchange of information in the digitized economy. This provides us with a broad range of opportunities to counteract ill-informed decision-making. In recent years, research provided evidence that behavior is biased towards salient individual costs and benefits, often at the disregard of long-term costs for the collective. In the context of collective goods conservation, modern technologies can help to overcome this salience bias. Feedback provision, e.g., by the use of smart meters and web applications, can induce more sustainable decision-making. However, only little is known how individual costs of resource conservation interact with feedback provision.

OVERVIEW AND MAIN FINDINGS

Chapter 2 (*Providing personal information to the benefit of others*) is joint work with Bettina Rockenbach and Abdolkarim Sadrieh. We examine the willingness to provide real personal information to information-based public goods. In a laboratory experiment, we retrieve subjects' personal information and systematically vary the implicit cost of information provision. We find that in comparison to the provision of money, personal information may be under-provided when it exhibits a high implicit provision cost.

Chapter 3 (*Paying with your information: The efficiency-enhancing effects of data provision*) is joint work with Bettina Rockenbach and Abdolkarim Sadrieh. We examine whether the large gains of the provider of an information-based public good may crowd-out the willingness to provide real personal information. In contrast to the former study, personal information is not particularly implicitly costly. We introduce a "big player" who cannot contribute to the information-based public good, but profits greatly from the provided information. We find that the provision of information is less susceptible to the gains of the "big player" than the provision of money. This benefits both the big player and the collective of contributors.

Taken together, Chapter 2 and Chapter 3 demonstrate that the provision of personal information to information-based public goods exhibits important structural differences to the provision of money. From a methodological perspective, two findings are especially important. First, implicit costs determine which types of information are provided. Second, absent these costs, decision-making importantly deviates from the predictions of outcome-based models of fairness. Given the amplified importance of personal information in the digitized economy, we conclude that research needs to account for these and other structural differences between material and information-based public goods. From a policy perspective, the results of the first study demonstrate that the effectiveness of information-based public goods may be seriously limited. The implication is that providers of information-based public goods need to take into account structural distortions which can arise from implicit costs of information provision. In some instances, it may be more beneficial to charge user fees or collect taxes to provide a certain public good. On the other hand, the second study shows that absent high implicit provision costs, the collection of personal information may be more beneficial than the charging of user fees or the levy of taxes. This demonstrates that policy makers are well-advised to take individuals' concerns for privacy and data protection seriously and develop mechanisms that effectively protect individuals from data misuse. Only then, we can secure the potentials of information-based public goods.

Chapter 4 (*Cooperation and the prospect of competing over jointly created surplus*) examines the provision of collective goods when individual contributions are ex post unverifiable and the surplus from cooperation is subject to subsequent competition.[1] In this situation, steep incentives may impede the willingness to provide the collective good in the first place. In a laboratory experiment, I study the willingness to cooperate in anticipation of competing in a winner-take-all contest. I find that in comparison to a proportional-share contest which leads to more equitable outcomes, the winner-take-all contest seriously impedes cooperation. The results of this study suggest that the anticipation of winner-take-all competition leads to an unwillingness to cooperate in the first place, and the experience of not being able to recoup investments into cooperation may erode the willingness to cooperate even further.

From a methodological point of view, the results of Chapter 4 demonstrate that the interaction between surplus generation and surplus sharing exhibits important structural differences to the standard paradigm of collective goods provision. Given that in certain competitive environments (e.g., in contests) individual decision-making systematically deviates from the theoretical prediction, inefficient outcomes may arise more frequently un-

[1]Financial support by the Research Unit Design and Behavior is gratefully acknowledged.

der certain rules for the allocation of surplus. Given the amplified relevance of knowledge-based collective goods, research has a key role in developing mechanisms that help to overcome these inefficiencies. From a policy perspective, the findings of Chapter 4 demonstrate that there may be an economic cost to steep incentives that reaches far beyond concerns for unequal opportunities and growing inequality. Steep incentives may importantly deteriorate social cohesion as both the anticipation and the experience of falling short can erode the willingness to act prosocially. Policy makers are well-advised to reconsider the pervasiveness of steep incentives in the organizational structure of firms, the industry, job markets and the educational system.

Chapter 5 (*Crowding-in sustainable consumption under varying opportunity costs*) is joint work with Thomas Lauer and Christopher Zeppenfeld.[2] We examine how feedback on resource consumption affects decision-making under varying opportunity costs of resource conservation. In a laboratory experiment, subjects face a tedious real-effort task that can be simplified by the use of a shortcut. However, the use of the shortcut leads to a real waste of resources. Each time a shortcut is used, a blank sheet of paper is shredded. In one condition, we provide feedback on accumulated resource wasting and constantly confront subjects with a stream of their total paper waste. In the other condition, the paper waste is hidden within the shredding bin. We vary the time restriction to induce different opportunity costs of resource conservation. Our results demonstrate that the effect of feedback indeed differs with the opportunity costs of resource conservation. Feedback on accumulated resource wasting does not have an impact on resource consumption when opportunity costs are low. However, when opportunity costs are high, feedback on accumulated resource-wasting reduces resource consumption.

From a methodological point of view, Chapter 5 demonstrates that the efficacy of feedback interventions is highly context-dependent. Thus, the interplay between feedback and individual opportunity costs should be a

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cornerstone of any intervention design. From a policy perspective, the results of our study demonstrate that modern technologies indeed have the potential to induce the conservation of collective goods. However, feedback interventions need to carefully take into account that in the field, high individual costs of conservation may be externalized.

CHAPTER 2

PROVIDING PERSONAL INFORMATION TO THE BENEFIT OF OTHERS

Abstract

The provision of personal information can create public benefits, e.g., by reporting location data to control traffic flows or by sharing one's health status to improve disease control. We experimentally study the willingness to provide personal information to information-based public goods and find that – in comparison to the provision of material public goods – information-based public goods may be under-provided. We present evidence that this under-provision results from implicit (emotional and cognitive) costs making subjects reluctant to share certain personal information. Our results demonstrate that implicit costs of information provision may seriously limit the effectiveness of information-based public goods.

2.1 INTRODUCTION

The provision of personal information frequently creates public benefits. For example, the effectiveness of public disease control crucially depends on the individual's willingness to report a suspected illness to a national health authority. The accuracy of policy measures critically depends on the individual's willingness to report her socio-demographic data. The exactness of traffic information importantly depends on the individual's willingness to share her current location.

Without doubt, the provision of personal information creates benefits for the public in all these examples. However, in contrast to material contributions to public goods, like donating money to construct a playground in the neighborhood, personal information exhibits an important structural difference: Beyond explicit transaction or opportunity costs, the provider of personal information may incur implicit provision costs. These implicit provision costs can include cognitive costs of information compilation, "emotional" costs of expected negative effects of information leakage or fear of ostracism, self-image concerns or disutility from cognitive dissonance. Then, preference heterogeneity with regard to provision may even be more pronounced than in material public goods games (Fischbacher and Gächter, 2010). The question is whether and how the implicit costs of information provision influence the willingness to provide information-based public goods. Answering this question is particularly important as the advances in data collection and data mining that enable far-reaching analyses and inference have improved tremendously. However, their success crucially relies on individuals' willingness to provide personal information. The current privacy debate creates great concern about the unbiasedness and completeness of the collected data. Research shows that people hold idiosyncratic preferences for privacy, and that these preferences strongly depend on the context and also on the type of personal information (see Aquisti et al., 2015; Acquisti et al., 2016). Although the topic's relevance amplifies with the rapid technological advancements, to the best of our knowledge no study investigates the willingness to provide personal information to the public benefit.[1] However, to develop effective policy measures, we need to understand the role of emotions and cognition in information provision (e.g., van Winden, 2015).

In this paper, we experimentally study the willingness to provide personal information to information-based public goods, and compare this to the willingness to provide money to material public goods. We set up four treatments, varying the unit of provision (*information* vs. *money*), and the explicit net cost of provision (*cost* vs. *no cost*). In the *information* treatments, subjects provide real personal information about themselves (e.g., about their preferences, past behavior or physical characteristics). Because,

[1]The existing experimental literature focuses on trade-offs between individual costs and benefits (e.g., Benndorf and Normann, forthcoming; Schudy and Utikal, 2017; Feri et al., 2016; Beresford et al., 2012; Jentzsch et al., 2012; Tsai et al., 2011; Huberman et al., 2005).

as discussed above, the willingness to share personal information may depend on implicit costs of information provision, we systematically vary these costs. To gain a proxy for the implicit costs of information provision, we conducted a survey study asking (different) participants to rate how cognitively difficult and how emotionally demanding it would be for them to answer certain questions asking for particular personal information.

Our experimental results show that information provision to the benefit of others is treated differently than money provision. Information rated low in implicit provision costs is provided much more frequently than information rated high in implicit provision costs, where emotional costs seem to loom larger than cognitive cost. As a result, when explicit net provision costs are zero (i.e., when the individual monetary benefit from provision equals the individual monetary costs), selective information provision even leads to under-provision compared to the provision of money.

Our study provides evidence that already in an abstract laboratory setting where we can guarantee for subjects' privacy and data protection, implicit costs lead to selective information provision, and may thus lead to under-provision of information-based public goods (compared to material public goods). We conclude that high implicit costs of information provision can lead to structural distortions of information-based public goods that cannot be overcome by a reduction of the explicit net cost of information provision.

2.2 RELATED LITERATURE

In recent years, various disciplines have contributed to the growing literature on privacy. A recent survey by Acquisti et al. (2016) illustrates the broad scope of the issue that touches several disciplines, e.g., the legal sciences, philosophy, computer sciences, and economics. Most of the economics literature focuses on the informational dimension of privacy and takes a regulatory perspective on the "trade-offs associated with the balancing of public and private spheres between individuals, organizations, and governments" (Acquisti et al., 2016, p. 443). Another growing literature focuses on the behavioral dimension of privacy. These papers assume implicit concerns for privacy and focus on the individual's cost-benefit-calculus. Recent surveys and experimental studies provide insight into the individual willingness to trade personal information for individual benefits. Acquisti et al. (2016) and Acquisti et al. (2015) summarize the empirical evidence by the following remarks: 1) The implicit cost of information provision is context-dependent. This means that an individual may provide personal information in one situation, but may refrain from information provision in another situation. Further, 2) implicit costs are subjective and idiosyncratic. This means that in the same situation, some individuals will provide their personal information while other individuals will not. In what follows we focus on experimental papers providing evidence that preferences for data privacy 1) are susceptible to the framing of decisions, lead to 2) selection effects under varying privacy conditions, 3) selective information provision, and that 4) different privacy policies lead to differences in decision-making.

In a recent experiment Marreiros et al. (2017) provide evidence of framing effects on privacy preferences. The authors examine respondents' willingness to provide personal information after evaluating excerpts from newspaper articles that relate to the issue of data privacy. Treatments vary the content of the excerpts and highlight either a positive or a negative aspect of companies' privacy policies. Information provision is not incentivized and subjects are paid a flat fee for participating in the survey. Marreiros et al. (2017) find that the likelihood of information provision decreases and stated preferences for privacy become more conservative when respondents are presented with negative aspects of companies' privacy policies. In a field experiment, Acquisti et al. (2013) examine framing effects on the stability of the value assigned to privacy protection. Subjects are given the choice between two Visa gift cards, one of which is loaded with \$12 and programmed such that the name of the subject will be linked to the completed transactions, and one of which is loaded with \$10 and states that the name of the subject will not be linked to any completed transaction. Treatments vary subjects' initial endowments, and the ordering of choices.

Acquisti et al. (2013) find that the value assigned to privacy protection is affected both by initial endowments and the ordering of choices. In a laboratory experiment, Grossklags and Acquisti (2007) study framing effects with regard to the willingness to sell and the willingness to protect personal information. Subjects participate in a quiz and their body weight is recorded. Treatments vary whether subjects are given the opportunity to pay to protect their personal information from release to their group, or to sell their personal information for release to their group. Grossklags and Acquisti (2007) find that subjects' average willingness to sell is higher than their average willingness to protect. Further, subjects opt into selling (not protecting, respectively) their personal information already for very small amounts of money.

In a laboratory experiment, Huberman et al. (2005) study selection effects with regard to the willingness to sell personal information in a reverse second-price auction. The subject with the lowest price is paid the second-lowest price and has to reveal information on her weight and age to the other participants (but remains anonymous). Huberman et al. (2005) find that the subgroup of subjects with less socially desirable traits is likely to demand a higher price in the auction. In another experiment, Feri et al. (2016) investigate subjects' willingness to sell personal information about outcomes in an IQ test that is linked to their names in order to receive a shopping voucher at a discounted price. With a probability of 50% percent, a data breach occurs. Then, the subject's personal information is potentially being revealed to the other participants. Whether the data breach actually leads to revelation depends on another random draw: Subjects can sell their personal information in two periods of which one is randomly selected at the end of the experiment. If the subject sold her data in that period and a data breach has occurred, her personal information is revealed to all participants. Treatments vary the feedback subjects receive at the end of the first period. In one treatment, subjects are informed about the occurrence of a data breach. In another treatment, no feedback is provided. Feri et al. (2016) find that feedback induces the subgroup of subjects with below-median test results to sell less personal information.

Benndorf and Normann (forthcoming) experimentally study selective information provision. Treatments vary the elicitation method: In one treatment, the willingness to accept is elicited using a Becker-DeGroot-Marschak mechanism (Becker et al., 1964). In another treatment, the willingness to accept is elicited using a take-it-or-leave-it-offer. In both treatments, subjects are asked to sell different bundles of personal information to a German telecommunications company which uses the retrieved information for market research. Benndorf and Normann (forthcoming) retrieve different information sets and vary the anonymity of the respondent (e.g., subjects are asked to provide a digital copy of their Facebook account). The authors find evidence of considerable heterogeneity in subjects' preferences for privacy that depend on the type of information. Further, the likelihood of information provision is higher if the subject remains anonymous. In another paper, Schudy and Utikal (2017) study the willingness to sell personal information with varying recipients. In one experimental treatment, recipients are living in the same city. In another experimental treatment, recipients are peers from a different city. The authors also use the Becker-DeGroot-Marschak mechanism to elicit the willingness to sell personal information. Further, the type of information is varied. The authors find that the likelihood of information provision is higher if information is not potentially embarrassing, and if the recipient is not a proximate peer.

Three studies provide evidence that decision-making differs under varying privacy policies. In a laboratory experiment, Tsai et al. (2011) examine subjects' willingness to purchase from retailers with varying privacy indicators. Subjects are asked to use a search engine and make real purchases of a specific good. Treatments vary the availability of privacy indicators and the price of the good. Tsai et al. (2011) find that the likelihood of a purchase is higher if the retailer engages in privacy protection. Further, subjects are willing to pay higher prices to purchase from these retailers. In another experiment, Jentzsch et al. (2012) find contrasting evidence. The authors study subjects' willingness to purchase cinema tickets from retailers with varying privacy policies and prices. The main findings are that the majority of subjects purchases from the retailer with the lower price.

However, in line with the findings by Tsai et al. (2011), Jentzsch et al. (2012) also observe that a non-negligible fraction of subjects is willing to purchase from the retailer with the higher price if this retailer requests less personal information, or promises not to use it for marketing purposes. In a field experiment, Beresford et al. (2012) find similar results. The authors also examine subjects' willingness to purchase from two competing online retailers with different privacy policies. One of the two retailers requires the provision of more sensitive personal information than the other. Treatments vary the price of the retailer requesting more sensitive personal information. Interestingly, Beresford et al. (2012) find no evidence of implicit costs of information provision. Subjects purchase from the retailer with the lower price. If prices are equal, then there is no difference in the likelihood to purchase from one retailer or the other.

2.3 VOLUNTARY INFORMATION PROVISION

2.3.1 THE MODEL

We set up a public goods game in which payoffs increase in the other players' provision. The game consists of $n \geq 2$ players indexed by $i = 1, ..., n$. Player i's utility function in the game with money provision is given by:

$$U_i(g_i, g_j) = e - cg_i + \alpha \sum_{j \neq i} g_j. \tag{2.3.1}$$

Each player i is endowed with e monetary units. Players simultaneously decide how many units $g_i \in \{0, ..., e\}$ to provide to the public good. For each unit player i provides, she incurs an explicit monetary net provision cost of $c \geq 0$. Each unit provided by the $n-1$ other players increases player i's utility by $0 < \alpha < 1$.[2]

[2]Note that in contrast to the standard objective function in most public goods settings, parameter α constitutes a constrained marginal per capita return in our model since only the $n-1$ other players of the group benefit from player i's provision. The objective function is equal to the standard objective function if the net cost of provision is set to $c = 1 - \alpha$.

There is an important structural difference between the provision of money and the provision of information. While the units of provision are not distinguishable in the game with money provision, the units of provision may exhibit different implicit provision costs in the game with information provision (see discussion in Section 2.2). That is, in addition to the explicit net cost of provision, player i may incur an implicit cost of provision if she provides personal information. Further, this implicit cost may differ for different types of information. We account for this by adding an additional cost term to the player's utility function and allowing that the of units of provision are distinguishable.

In the game with information provision, each player i is endowed with a set of items of private information $\Theta_i = \{\theta_{i1}, \theta_{i2}, ..., \theta_{im}\}$. We assume that the number of items m contained in a player's information set Θ_i is identical for all n players. Each player i receives base utility v_{ik} from item θ_{ik},[3] i.e., her endowment is worth $\sum_{k=1}^{m} v_{ik}$ units of utility. Let $x_{ik} \in \{0, 1\}$ be player i's choice variable that indicates whether player i provides the kth item of her information set Θ_i, with

$$x_{ik} = \begin{cases} 0 & \text{if player } i \text{ does not provide } \theta_{ik} \\ 1 & \text{if player } i \text{ provides } \theta_{ik}. \end{cases}$$

Player i's decision is given by the choice vector $x_i = (x_{i1}, x_{i2}, ..., x_{im})$. Player i's utility function in the game with information provision is given by:

$$U_i(x_{ik}, x_{jk}) = \sum_{k=1}^{m} (v_{ik} - (c + \gamma_{ik}) x_{ik}) + \alpha \sum_{j \neq i} \sum_{k=1}^{m} x_{jk}. \qquad (2.3.2)$$

The utility function, for most of its parts, corresponds to the standard objective function in public goods settings. The main structural difference is that we integrate an additional cost variable γ_{ik} which captures the implicit provision cost that may vary between players as well as between items. The implicit cost may include cognitive costs of information compilation,

[3] For example, knowing the own preferences, past behavior or physical characteristics is valuable in the individual's current and future decision-making.

"emotional" costs of expected negative effects of information leakage, fear of ostracism, self-image concerns or disutility from cognitive dissonance. In general, we expect the implicit provision costs to be linked to the cognitive and emotional load of the requested information.

In the game with information provision, players simultaneously decide for each item θ_{ik} of their information set Θ_i whether to provide the item or not. We assume that the provision of an item does not reveal the personal information to any other player. As in the game with money provision, player i incurs an explicit (monetary) net provision cost of $c \geq 0$ that is equal for all items. Each item provided by the $n-1$ other players increases player i's utility by $0 < \alpha < 1$.

All differences between Equation 2.3.1 and Equation 2.3.2 stem from two structural differences between money and information: First, in the case of information the units of provision are distinguishable, while this does not hold for money. This is captured by the indicator choice variable x_{ik} and the individual value of information v_{ik} in Equation 2.3.2. Second, we integrate an additional cost term, the implicit provision cost γ_{ik}, in Equation 2.3.2. All other model parameters, especially the net cost from the own provision c and the return α from another player's provision do not differ between the two models.

2.3.2 HYPOTHESES

A rational player i maximizes her utility with respect to the items she provides. Obviously, as long as there is a positive net cost to the provision of information (no matter whether explicit or implicit) the dominant strategy is not to provide information. Hence, in the only utility maximizing equilibrium with positive costs, player i provides no information. If both the explicit net provision costs and the implicit provision costs are equal to zero ($c = 0$ and $\gamma_{ik} = 0$), the player is indifferent between providing and not providing information. Without costs, any mixture of provision and non-provision may be in equilibrium. The own payoff-maximizing equilibria in the game with monetary contributions have similar characteristics. In the equilibrium with explicit net costs of provision, players do not provide money to the public good, while with zero explicit net costs, any level of provision is possible. If we assume that players are concerned about others' material payoffs, it is likely that the pareto-efficient equilibrium is selected, leading to higher provision levels absent explicit net provision costs.

Hypothesis 1. *Both in the info and money condition, with positive explicit net provision costs the provision level is lower than with zero explicit net provision costs.*

Our second hypothesis concerns the difference between information and money provision. Conceivably, the provision of information involves higher costs than the provision of money since any implicit costs of information provision top off the explicit net costs. Hence, ceteris paribus we expect higher provision levels of money than information, assuming that the implicit costs are not zero for all items.

Hypothesis 2. *The provision level in the money condition is higher than in the information condition.*

In the case in which players hold social preferences the predictions of Hypothesis 1 remain unchanged. This is true because the explicit net costs of provision generally drive the cost-benefit calculus of players towards less provision, no matter whether or not they have an additional other-regarding utility.[4] However, if a player maximizes joint utility of the group, even items with a positive implicit provision cost may be provided. From the utility function in Equation 2.3.2, we see that in the social optimum of the game player i provides all items with certainty which exhibit zero implicit provision costs (which is in contrast to the set of Nash equilibria under $c = 0$ where player i is just indifferent with regard to provision). Further, to

[4]In a symmetric game with inequity-averse players, the prediction is even more extreme, because any provision – with or without explicit net costs – leads to a deterioration of the own payoff position compared to that of the other players.

maximize joint utility, she also provides items with a low implicit provision cost.[5] This means that if a player maximizes joint utility and incurs only low implicit costs of information provision, she provides more information than a player who incurs high implicit costs.

Hypothesis 3. *The likelihood of provision is higher for information with low implicit costs than for information with high implicit costs.*

2.4 THE EXPERIMENT

We experimentally study the willingness to provide personal information to information-based public goods, and compare this to the willingness to provide money to material public goods. In this section, we present the experimental design, explain how we measure the implicit costs of information provision, and provide information on the experimental procedure.

2.4.1 EXPERIMENTAL DESIGN

We examine the voluntary provision of personal information to the benefit of others, and compare it to the voluntary provision of money. To examine whether there are structural differences between the provision of money and information, we set up four treatments in a 2×2-design where we vary the unit of provision (*information* vs. *money*) and the explicit monetary net provision costs (*cost* vs. *no cost*). The experimental treatments are summarized in Table 2.1.

Table 2.1: Treatments.

	information	*money*
cost	INFO	MONEY
no cost	INFO_NC	MONEY_NC

[5]Precisely, all items with $\gamma_{ik} < -c + \alpha(n-1)$ are provided in the social optimum of the game.

All treatments are one-shot paper-and-pencil experiments with a group size of four players. In the *money* treatments, subjects are endowed with 20 monetary units and decide how many units to contribute to the public good. In the *information* treatments, subjects receive 20 questions and decide which of them to answer. Answering a question ("providing an item") means providing to the information-based public good. Each item and each unit is worth €0.30, i.e., the total endowment of a subject equals €6.00=20×€0.30. In Table 2.2, the marginal payoffs from the own provision and from the provision of the other group members are given for the *cost* and the *no cost* condition (marginal payoffs are identical for the *information* and the *money* condition). The decision to keep an item (i.e., not provide the information) or a monetary unit gives the player €0.30 while each of the other players receives zero, both in the *cost* and the *no cost* condition. The decision to provide an item (i.e., provide the information) or a unit gives €0.12 to the provider and each of the other players in the *cost* condition, and hence incurs a monetary net provision cost of €0.18. In the *no cost* condition, the decision to provide an item or a unit does not change the payoff of the provider, but gives €0.12 to each of the other players. Hence, the monetary net provision cost is zero in the *no cost* condition. By comparing provision levels under the different net monetary costs of provision, we can investigate whether information provision can be incentivized in the same way as money provision.

Table 2.2: Marginal payoffs.

	cost condition		*no cost* condition	
	self	others	self	others
item/unit kept	€0.30	€0.00	€0.30	€0.00
item/unit provided	€0.12	€0.12	€0.30	€0.12

2.4.2 IMPLICIT PROVISION COSTS

As the provision of information may incur implicit provision costs (in addition to the explicit costs), we assessed the implicit costs of the 20 items (questions) in a survey study in a large undergraduate economics class. In Table 2.3, the 20 items and corresponding questions are given.

Table 2.3: Items in the *information* treatments.

No.	Item	Question	Cognitive load		Emotional load		Comb.
			mean	median	mean	median	measure
1	gender	Are you male or female?	1.0948	1	1.4976	1	0.5066
2	eye color	What is your eye color?	1.4787	1	1.5024	1	0.6939
3	age	What is your age?	1.1848	1	1.7062	1	0.7300
4	subject of study	Are you currently enrolled and if so, what is your subject of study?	1.1517	1	1.8057	1	0.8198
5	shoe size	What is your shoe size?	1.4739	1	1.8246	1	0.9511
6	study duration	If you are studying, how long have you been studying so far?	1.3365	1	1.9336	2	0.9924
7	height	What is your height?	1.4455	1	1.9526	2	1.0516
8	zip code	What is your zip code?	1.6825	1	1.9621	1	1.1796
9	clothes	How often do you return clothing to the seller as unused after having actually used it?	1.8057	1	1.9573	2	1.2513
10	credit	How often do you overdraw your bank account?	1.5071	1	2.1659	1	1.2714
11	season	What is your favorite season?	2.1754	2	1.6635	1	1.3497
12	size	What is the size of your clothing?	2.1706	2	2.2038	2	1.6791
13	cheating	How often have you cheated in exams?	2.1185	2	2.3223	2	1.7319
14	travel	Where would you like to travel?	2.7488	2	1.5924	1	1.8464
15	sex	How often do you have sex per week?	1.8057	1	2.7867	3	1.9600
16	weight	What is your weight?	2.1043	2	2.7014	3	2.0284
17	lying	How often do you lie to your best friend?	2.3744	2	2.5308	2	2.0573
18	drink	What is your favorite drink?	2.8294	3	1.9668	2	2.0692
19	actor	Who is your favorite actor?	4.0616	4	2.3602	2	3.3502
20	song	What is currently your favorite song?	4.4692	5	2.1801	2	3.6644

A total of 211 respondents separately evaluated each of the 20 items, concerning the cognitive load ("When answering this question I have to think...") and the emotional load ("When answering this question I feel...").[6] Answers to the cognitive load question are on a 6-point Likert scale from ("I have to think") "very little" to "very hard". Answers to the emotional load question are on a 6-point Likert scale from ("I feel") "very uncomfortable" to "very comfortable".[7] Table 2.3 presents the mean and median item evaluation, where "very little" and "very comfortable" were coded with a value of 1, while "very hard" and "very uncomfortable" were coded with a value of 6. In the table, items are sorted on the basis of a combined measure of both evaluations.[8]

2.4.3 PROCEDURE

During 2013, we conducted two sessions of each of the four treatments in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany. We recruited our participants using the Online Recruitment System for Economic Experiments (Greiner, 2015). Overall, 212 participants participated with 55% female and 45% male, and mostly participants were students from economics, social sciences, and business administration. Before the experiment, a random draw determined the order in which items were put into each participant's envelope in the *information* condition.

Each experimental session was one-shot paper-and-pencil and lasted around one hour. The written instructions in the *information* treatments informed the participant that the experiment involves a decision to either keep information for herself, or to truthfully answer the question and thereby

[6] Translated questionnaires can be found in the Appendix.

[7] We find no order effects in the evaluation of items. Spearman's rank correlation coefficient between the order of presentation and the mean and median item assessment is insignificant both for the cognitive and emotional load (p-values range from p=0.5908 to p=0.8750).

[8] We combine both evaluations by building the Euclidean distance from the most positive evaluation on both dimensions, i.e., from the origin of $(1, 1)$. The Euclidean distance is calculated for each item separately by the following formula:

$$d(x, y) = \sqrt{(\text{mean}_{\text{cognitive load}} - 1)^2 + (\text{mean}_{\text{emotional load}} - 1)^2}.$$

provide it to the group account.[9] In the *money* treatments, the written instructions stated that the experiment involves a decision to either keep money or to provide it to the group account. In all treatments, participants received 20 sheets of paper in an envelope. In the *information* treatments, the term "information" and one of the 20 questions given in Table 2.3 were printed on each sheet of paper. To provide an item to the group account, the participant was asked to write the answer to the question on the respective sheet of paper.[10] We ensured that answers to the questions could not be ascribed to a participant's identity. This was also stated clearly in the instructions. In the *money* treatments, the term "money" and a text input field were printed on each sheet of paper. To provide a unit to the group account, the participant was asked to write the word "group" on the respective sheet of paper.[11] The instructions stated clearly that no participant would receive feedback about individual provision levels.

After participants made their provision decisions, they were asked to put all sheets back into the envelope and hand it over to the experimenter. While the experimenter calculated the payoffs in a separate room, participants answered a short questionnaire. In this questionnaire, we also asked for participants' beliefs regarding their group members' decisions. Belief elicitation was not incentivized. After the experiment, participants were paid anonymously in a separate room. On average, participants earned €10.20 in INFO (min: €5.20; max: €15.10), €14.90 in INFO_NC (min: €12.90; max: 15.70), €10.00 in MONEY (min: €6.10; max: €13.70), and €15.50 in MONEY_NC (min: €13.30; max: €15.70) including the show-up fee of €2.50.

We collected between 52 and 56 independent observations for each of the four treatments. By conducting one-shot experiments, we ensured statistical independence of all observations. If not stated otherwise, statistical comparisons between treatments are based on two-sided Mann-Whitney U-

[9] Translated instructions are provided in the Appendix.
[10] We emphasized that the answers should be truthful.
[11] Pictures of the experimental setup can be found in the Appendix.

tests (MWU), and comparisons within treatments are based on two-sided Wilcoxon signed-rank tests (WSR).

2.5 RESULTS

We present our experimental results in two steps. In part one, we analyze treatment differences with regard to average provision levels (see Section 2.5.1). We find that (1) with positive explicit net provision costs the provision level is lower than with zero explicit net provision costs, and (2) that the provision level in the *money* treatment is higher than in the *information* treatment if explicit net provision costs are zero. In part two, we analyze the likelihood of item provision in the *information* treatments (see Section 2.5.2). We find that (3) subjects engage in selective information provision, i.e., items with a low implicit provision cost are more likely provided than items with a high implicit provision cost.

2.5.1 INFORMATION AND MONEY PROVISION

In this section, we analyze the effect of explicit net provision costs on the provision of information and money. We further study differences between information and money provision. In line with Hypothesis 1, we expect higher average provision levels in the *no cost* condition than in the *cost* condition. Further, in line with Hypothesis 2, we expect higher average provision levels in the *money* than in the *information* treatments.

Table 2.4: Average provision levels.

	INFO	INFO_NC	MONEY	MONEY_NC
Avg. provision level	9.23 (1.11)	17.80 (0.61)	8.12 (1.01)	19.29 (0.47)
% of endowment	46.15	89.02	40.58	96.44
Ind. obs.	52	56	52	52

Notes: The table reports average provision levels and standard errors (in parentheses) by treatment.

As shown in the first row of Table 2.4, subjects' average provision levels are close to 50% if provision is costly to them. On average, subjects provide 9.23 items in INFO, and 8.12 units in MONEY. If the provision exhibits zero net costs, average provision levels are higher, and subjects provide 17.80 items in INFO_NC and 19.29 units in MONEY_NC. With regard to the effect of explicit net provision costs on the provision of money and information, we find that average provision levels are significantly lower in presence of explicit net provision costs both in the *info* and the *money* condition (p=0.0000, for both comparisons). This finding is in line with Hypothesis 1.

Result 1. *Both in the info and the money condition, with positive explicit net provision costs the provision level is significantly lower than with zero explicit net provision costs.*

With regard to differences between the provision of information and money, we find mixed results. Hypothesis 2 needs to be rejected for the *cost* condition where we find no statistically significant differences between average provision levels (INFO vs. MONEY: p=0.5144). However, we cannot reject Hypothesis 2 for the *no cost* condition in which subjects provide significantly more money than information (INFO_NC vs. MONEY_NC: p=0.0010).

Result 2. *With positive explicit net provision costs, there is no statistically significant difference in the provision of information and money. With zero explicit net provision costs, provision levels are higher in the money condition than in the info condition.*

2.5.2 SELECTIVE INFORMATION PROVISION

In this section, we investigate the impact of implicit provision costs on the likelihood of information provision. There is a significant negative correlation between the item's combined measure of implicit provision costs and the respective average provision level in both treatments (Spearman's rank correlation coefficient, INFO: p=0.0013; INFO_NC: p=0.0061). To test how the implicit provision cost affects the likelihood of information provision, we run a probit regression where the likelihood of item contribution is the dependent variable.

Table 2.5: Likelihood of item contribution.

	(1)	(2)	(3)
expl. cost	-1.3388***	-1.3624***	-1.6501***
(0 = *no cost*, 1 = *cost*)	(0.22)	(0.22)	(0.54)
combined measure cognitive and emotional load	-0.1825***		
(Euclidean distance from origin)	(0.04)		
mean cognitive load		-0.0239	0.0066
(1 = very low, 6 = very high)		(0.03)	(0.05)
mean emotional load		-0.6175***	-0.7311***
(1 = very low, 6 = very high)		(0.10)	(0.17)
expl. cost × mean cognitive load			-0.0520
			(0.06)
expl. cost × mean emotional load			0.1905
			(0.20)
constant	1.5255***	2.5660***	2.7419***
	(0.18)	(0.29)	(0.47)
number of observations	2,160	2,160	2,160
number of subjects	108	108	108
Pseudo-R-squared	0.1885	0.2010	0.2014

Notes: Probit regression clustered by subject, robust standard errors in parentheses. Dependent variable: Contribution of item (0 = no, 1 = yes). *: p<0.1, **: p<0.05, ***: p<0.01.

Table 2.5 reports the results from the probit regression. In model (1), the first coefficient replicates the above finding that explicit monetary net provision costs have a significant negative effect on the likelihood of item provision. The second coefficient shows that the higher an item scores on the combined cognitive and emotional dimension, the less likely it is to be provided. In model (2), we disentangle the effect of cognitive and emotional load. We find that while both coefficients are negative, only the mean emotional load has a significant negative effect on the likelihood of item provision.[12] In model (3), we interact the explicit monetary provision costs with the mean cognitive and emotional load. Both interactions are insignificant, meaning that the impact of the implicit provision costs on the likelihood of item provision does not differ across the cost conditions. The significant negative effect of the emotional load and the combined measure yields support for Hypothesis 3.

Result 3. *The higher the item's implicit provision cost, the lower is the likelihood of the item's provision.*

To further investigate our finding that selective information provision caused by implicit costs of information provision may explain the under-provision of information compared to money, we set up a new experimental study with new subjects, described in the following section.

2.5.3 PROVISION OF INFORMATION WITH LOW IMPLICIT COSTS

To investigate the finding that with zero explicit net provision costs, provision levels are higher in the *money* condition than in the *information* condition, we set up a new experimental study with new subjects to test whether this effect is primarily driven by the implicit costs in the *information* condition. In this new experiment, we replicate the design described above (see Section 2.4), but with a *reduced* set of questions. We reduced the set of items in the *information* condition to the 10 questions with the lowest combined measure of cognitive and emotional load (see upper part of Table 2.3) and accordingly reduced the endowment in the *money* condition

[12]However, both measures are significantly correlated (Spearman's rank correlation coefficient between mean cognitive load and mean emotional load: 50.85%, p=0.0221).

to 10 units. The new experiment comprises four treatments, summarized in Table 2.6.

Table 2.6: Treatments *reduced* condition.

	information	*money*
cost	INFO_RED	MONEY_RED
no cost	INFO_RED_NC	MONEY_RED_NC

To ensure payoff equivalence between all treatments, the marginal payoffs are doubled (compare Table 2.7). All other parameters remain unchanged.[13]

Table 2.7: Marginal payoffs *reduced* condition.

	cost condition		*no cost* condition	
	self	others	self	others
item/unit kept	€0.60	€0.00	€0.60	€0.00
item/unit provided	€0.24	€0.24	€0.60	€0.24

Overall, 212 subjects participated in the four treatments of the *reduced* question set experiment. To enable the statistical comparison between the four *main* treatments and the treatments of the *reduced* question set experiment, in the following we present provision levels as percentages of endowments (relative provision levels).

Table 2.8 contains three observations in support of our hypothesis that the high implicit provision costs induce the under-provision of items in INFO_NC as compared to the observed provision of money in MONEY_NC.

[13]The procedure in the *reduced* question set experiment followed the same protocol as in the four *main* treatments (see Section 2.4.3). All sessions were conducted during 2013. Overall, 212 subjects participated with 59% female and 41% male, and most participants were students from economics, social sciences and business administration. In the *reduced* question set experiment, on average participants earned €10.20 in INFO_RED (min: €5.70; max: €15.00), €15.20 in INFO_RED_NC (min: €13.10; max: €15.70), €9.70 in MONEY_RED (min: €6.40; max: €14.80), and €15.50 in MONEY_RED_NC (min: €14.50; max: €15.70) including the show-up fee of €2.50.

Table 2.8: Relative provision levels.

	information		*money*	
	cost	*no cost*	*cost*	*no cost*
main	46.15%	89.02%	40.58%	96.44%
reduced	45.71%	92.50%	33.46%	97.50%

Notes: The table reports provision levels as percentages of endowment (=20 in *main*, =10 in *reduced*).

First, we see that when the questions in the *information* treatments only concern the items with low implicit provision costs (INFO_RED_NC), relative provision levels are weakly significantly higher than in the full question set INFO_NC (p=0.0998) in the *no cost* condition. Second, we observe that this is only true for information, but not for money. Here, relative provision levels are not significantly different between MONEY_NC and MONEY_RED_NC (p=0.4950). Third, when explicit net cost are zero, we do not observe the under-provision (compared to money) we observe in the full question set. Relative provision levels are not significantly different between INFO_RED_NC and MONEY_RED_NC (p=0.2322).

Result 4. *If subjects are only confronted with items with low implicit provision costs, there is no statistically significant difference between the provision of information and money. In particular, there no longer is under-provision in case of zero explicit net costs.*

In Table 2.9, we provide the results from a probit regression that is run on our observations in INFO_RED and INFO_RED_NC, and replicates the analysis of Table 2.5 in Section 2.5.2. Again, the dependent variable is the likelihood of item provision. As before, the explicit monetary provision cost has a significant negative effect on the likelihood of item provision. The results replicate our findings summarized in Result 3 that subjects engage in selective information provision. Although subjects were only confronted with the 10 items with the lowest intrinsic costs, there is a variance in the

Table 2.9: Likelihood of item contribution (*reduced* treatments).

	(1)	(2)
explicit monetary provision cost	-1.5789***	-1.5823***
(0 = *no cost*, 1 = *cost*)	(0.26)	(0.26)
combined measure cognitive and emotional load	- 0.8531***	
(Euclidean distance from origin)	(0.16)	
mean cognitive load		0.0414
(1 = very low, 6 = very high)		(0.12)
mean emotional load		-1.1100***
(1 = very low, 6 = very high)		(0.22)
constant	2.2751***	3.4461***
	(0.30)	(0.49)
number of observations	1,080	1,080
number of subjects	108	108
Pseudo-R-squared	0.2382	0.2400

Notes: Probit regression clustered by subject, robust standard errors in parentheses. *: p<0.1, **: p<0.05, ***: p<0.01. Dependent variable: Contribution of item (0 = no, 1 = yes).

intrinsic costs and the regression shows that the higher an item scores on the combined cognitive and emotional dimension, the less likely it is provided. Again, the emotional load has a significant negative effect on information provision, while the mean cognitive load is insignificant.

2.6 CONCLUSION

In this paper, we experimentally study the provision of personal information to the benefit of others, and compare this to the provision of money. There is an important structural difference between money and information: Information provision exhibits an implicit cost that varies with the type of information. We account for this by including an additional cost parameter in our model and hypothesize that a player's decision to contribute to an information-based public good does not only depend on the explicit provision cost, but also on the implicit cost of information provision. This leads to different predictions for the provision of information-based public goods as compared to monetary public goods. If players' information sets strongly vary in implicit costs of information provision, information-based public goods will be underprovided as compared to material public goods. Further, selective information provision can be expected if the information requested is heterogeneous in implicit provision costs.

In a laboratory experiment, we test our hypotheses using a 2×2-design where we vary the unit of provision (*information* vs. *money*), and the explicit net provision costs (*cost* vs. *no cost*). We study real information provision, i.e., subjects provide real personal information about their own preferences, past behaviors and physical characteristics. In the two *information* treatments, we exogenously vary the cognitive and emotional load of the information we retrieve to induce different implicit costs of information provision. In line with the recent experimental literature (e.g., Benndorf and Normann, forthcoming; Schudy and Utikal, 2017; Feri et al., 2016; Huberman et al., 2005), we observe selective information provision both in presence and absence of explicit net provision costs. This even leads to under-provision of information as compared to money provision when explicit net provision costs are zero. Furthermore, in line with the literature we observe that information provision varies with incentives, i.e., we observe more information provision if explicit net costs are absent (compare Beresford et al., 2012; Jentzsch et al., 2012; Tsai et al., 2011).[14]

To the best of our knowledge, we are the first to investigate information provision that creates public benefits. We show that already in an abstract laboratory setting where we can guarantee for subjects' privacy and data protection, and where the information we retrieve is not especially private or complex, implicit costs lead to selective information provision.

[14]An interesting future direction would be to examine how subjects hold each other responsible for under-provision of information if it is implicitly costly. As Cappelen et al. (2010) show, the majority of subjects do not hold others responsible for impersonal factors beyond individual control. This could also be the case if implicit costs impede the provision of personal information. Then, one would observe lower punishment rates in an information-based public goods game than in a money-based public goods game.

In the most extreme case, information-based public goods are even under-provided in comparison to material public goods. We suggest that future research should investigate whether and how information with high implicit provision costs can be elicited to avoid that information-based public goods are biased.

2.A EXPERIMENTAL APPENDIX

2.A.1 TRANSLATED INSTRUCTIONS

The instructions are translated from German. The sample instructions belong to treatment INFO (MONEY). All deviations in the other treatments are given in square brackets.

Instructions of the information treatments

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us.

Depending on your own and the other participants' decisions, you can earn money in this experiment.

During the experiment, you are not allowed to talk to other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and all payments. All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a person who makes a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

At the end of the experiment, you receive 2.50 Euro for showing up. During the experiment, you can earn additional money. On the following pages we explain to you the exact course of the experiment.

Groups and rounds

- You are part of a group with a total of **4 members**.
- The experiment consists of **only one round**.

Course of the experiment

- You receive **20 information sheets** [*INFO_RED/INFO_RED_NC: **10 information sheets***] in a **white envelope** labeled with the word "Information". Each of the information sheets contains a question about you. Take out all 20 information sheets [*INFO_RED and INFO_RED_NC: 10 information sheets*] of the white envelope, and decide for each of the information sheets whether you want to **provide the information about you for the group or keep it for yourself**:
  - If you provide your information for the group, **the whole group** profits from this. For this purpose, please write the **truthful** answer to the question on the information sheet. Please put the answered information sheet back into the white envelope. For each answered information sheet which you put into the white envelope, **each group member (including yourself) receives 0.12 Euro** [*INFO_RED: **0.24 Euro***].

    [*INFO_NC: For each answered information sheet which you put into the white envelope, you **yourself receive 0.30 Euro, and each of the other three group members receives 0.12 Euro**.*]

    [*INFO_RED_NC: For each answered information sheet which you put into the white envelope, you **yourself receive 0.60 Euro, and each of the other three group members receives 0.24 Euro**.*]
  - If you keep your information to yourself, **only you profit from this**. For this purpose, please leave the information sheet completely empty. Please put the empty information sheet back into

the white envelope. For each empty information sheet you put into the white envelope, **you (but none of the other group members) receive 0.30 Euro** [*INFO_RED/INFO_RED_NC:* ***0.60 Euro***].

- Your **payoff** is calculated as follows:

0.12 Euro	times	the number of information sheets which you and the other member of your group have provided for the group
[*INFO_RED/ INFO_RED_NC: 0.24 Euro*]		[*INFO_NC/INFO_RED_NC: the number of information sheets which have been provided by the other members of your group*]
+		
0.30 Euro	times	the number of information sheets you have kept for yourself
[*INFO_RED: 0.60 Euro*]		[*INFO_NC: 20 (=number of information sheets in your envelope)*]
[*INFO_RED_NC: 0.60 Euro*]		[*INFO_RED_NC: 10 (=number of information sheets in your envelope)*]
=		Your payoff

- After all participants have made their decisions, **all 20 information sheets** [*INFO_RED/INFO_RED_NC:* ***10 information sheets***] **should be put back into the white envelope**.

- No participant of the experiment gets to know how many and which information sheets the other participants have provided and how many they have kept for themselves. Each participant only learns **her own payoff** after the experiment.

- As soon as all participants have made their decisions the lab team collects the white envelopes.
- Then, the lab team hands out a **questionnaire**. Please fill in the questionnaire while the lab team calculates your payoffs. After you have filled in the questionnaire, please stay at your cabin until we separately call you for payment.

Data protection

- **Please note**: The information about the group members which has been put into the white envelopes will be statistically evaluated anonymously, and destroyed by the experimenter immediately thereafter. At no time will information be assigned to the person of a single participant. Information will not be passed to third parties. Information will not be used for any other than research purpose, especially not for any direct or indirect commercial purpose.

Good luck and thank you very much for your participation!

Instructions of the money treatments

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us.

Depending on your own and the other participants' decisions, you can earn money in this experiment.

During the experiment, you are not allowed to talk to other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and all payments. All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a person who makes a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

At the end of the experiment, you receive 2.50 Euro for showing up. During the experiment, you can earn additional money. On the following pages we explain to you the exact course of the experiment.

Groups and rounds

- You are part of a group with a total of **4 members**.
- The experiment consists of **only one round**.

Course of the experiment

- You receive **20 money sheets** [*MONEY_RED/MONEY_RED_NC:* ***10 money sheets***] in a **blue envelope** labeled with the word "Money". Take out all 20 money sheets [*MONEY_RED and MONEY_RED_NC: 10 money sheets*] of the blue envelope, and decide for each of the money sheets whether you want to **provide the money for the group or keep it for yourself**:

– If you provide your money for the group, **the whole group** profits from this. For this purpose, please write the word “GROUP” on the money sheet. Please put the labeled money sheet back into the blue envelope. For each labeled money sheet which you put into the blue envelope, **each group member (including yourself) receives 0.12 Euro** [*MONEY_RED: **0.24 Euro***].

  [*MONEY_NC: For each labeled money sheet which you put into the blue envelope, you **yourself receive 0.30 Euro, and each of the other three group members receives 0.12 Euro**.*]

  [*MONEY_RED_NC: For each labeled money sheet which you put into the blue envelope, you **yourself receive 0.60 Euro, and each of the other three group members receives 0.24 Euro**.*]

– If you keep your money to yourself, **only you profit from this**. For this purpose, please leave the money sheet completely empty. Please put the empty money sheet back into the blue envelope. For each empty money sheet you put into the blue envelope, **you (but none of the other group members) receive 0.30 Euro** [*MONEY_RED/MONEY_RED_NC: **0.60 Euro***].

- Your **payoff** is calculated as follows:

0.12 Euro	times	the number of money sheets which you and the other member of your group have provided for the group
[*MONEY_RED/ MONEY_RED_NC: 0.24 Euro*]		[*MONEY_NC/MONEY_RED_NC: the number of money sheets which have been provided by the other members of your group*]
+		
0.30 Euro	times	the number of money sheets you have kept for yourself
[*MONEY_RED: 0.60 Euro*]		[*MONEY_NC: 20 (=number of money sheets in your envelope)*]
[*MONEY_RED_NC: 0.60 Euro*]		[*MONEY_RED_NC: 10 (=number of money sheets in your envelope)*]
=		Your payoff

- After all participants have made their decisions, **all 20 money sheets** [*MONEY_RED/MONEY_RED_NC:* ***10 money sheets***] **should be put back into the blue envelope**.

- No participant of the experiment gets to know how many money sheets the other participants have provided and how many they have kept for themselves. Each participant only learns **her own payoff** after the experiment.

- As soon as all participants have made their decisions the lab team collects the blue envelopes.

- Then, the lab team hands out a **questionnaire**. Please fill in the questionnaire while the lab team calculates your payoffs. After you have filled in the questionnaire, please stay at your cabin until we separately call you for payment.

Good luck and thank you very much for your participation!

2.A.2 SURVEY STUDY

The questionnaire from the survey study contained all 20 items as given in Table 2.3.

Translated questionnaire from survey study

Dear students, in this questionnaire, we would like to ask you to evaluate 20 questions for a research project of the Chair in Experimental and Behavioral Economics. For this purpose, please read each question carefully and then indicate how you evaluate the question. Thank you very much for your participation!	Are you male or female? ○ male ○ female How old are you? [ ] What is your subject of study? [ ]
When answering this question... What is your size? ...I have to think... very little very hard ○ ○ ○ ○ ○ ○	...I feel... very comfortable very uncomfortable ○ ○ ○ ○ ○ ○

2.A.3 PICTURES

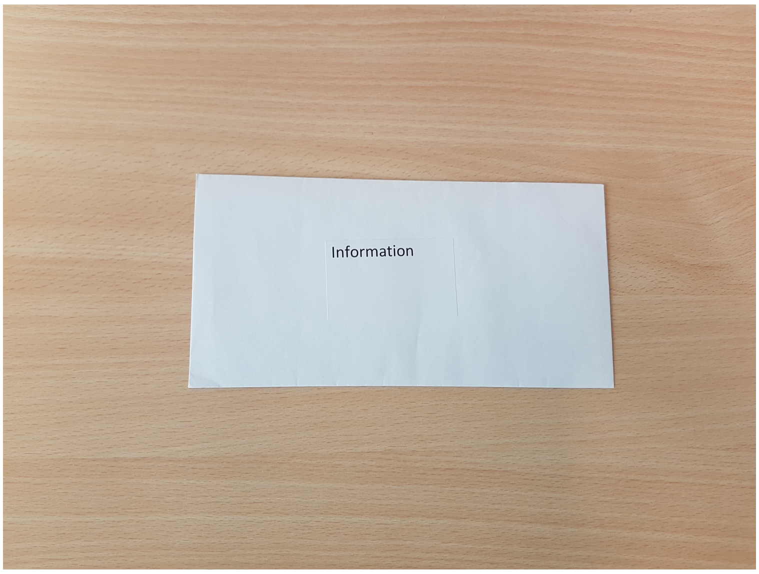


Figure 2.A.1: Envelope information treatments.

Figure 2.A.2: Envelope money treatments.

Figure 2.A.3: Information sheets.

Figure 2.A.4: Money sheets.

CHAPTER 3

PAYING WITH YOUR INFORMATION: THE EFFICIENCY-ENHANCING EFFECTS OF DATA PROVISION

Abstract

Internet services are often free of charge, but ask for customers' personal data in exchange for usage. Collecting personal data enables service providers to improve services to the benefit of customers, but also increases the providers' profits. We experimentally study whether the provision of information-based public goods is susceptible to gains of a "big player". We find that in presence of a "big player" the provision of personal information is more efficient than the provision of money. Our results demonstrate that collecting personal data instead of charging user fees may be profitable both for the service provider and the customer.

3.1 INTRODUCTION

Asking for customers' personal data in exchange for the usage of Internet services is ubiquitous. The quality of services improves in customers' willingness to share their personal data. The more users enable the collection of their location data, the more accurate is the traffic information by navigation apps. The more users enable the collection of their contact data, the higher is the quality of job suggestions by networks for professionals. While the provision of personal data benefits the collective of customers, it also generates extensive profits for the service provider. We can hardly object that "data are becoming the new raw material of business", "an economic input almost on a par with capital and labour".[1] Without exception,

[1] http://www.economist.com/node/15557443

the five most valuable firms worldwide are providers of internet services or data-driven businesses.[2] The question is whether awareness of the extensive profits of the service provider may crowd-out the willingness of customers to share their personal data to the benefit of others.

In this paper, we experimentally study how the willingness to provide personal information to information-based public goods varies with the presence of a "big player". Experimental subjects provide real personal information (e.g., about past behavior or physical characteristics). In analogy to providers of internet services, a "big player" creates value from the provided information, which is partly distributed among the contributors (i.e., by improving the service), but to a large extent kept by the big player. As a control, we study an information-based public good with the same benefits for the subjects, but without a big player. In this treatment, the surplus from provision is directly distributed among contributors. To control for the effects of information as the unit of provision, we study the same two scenarios (with and without a big player) with money as the unit of provision.

We find that the provision of information is less susceptible to the gains of a big player than the provision of money. In the experiment, the presence of the big player crowds out the willingness to provide money, but no such effect is observed for information. Our results demonstrate that the collection of personal data may generate more social surplus than the collection of user fees. Thus, the benefits from data collection reach far beyond the mere improvement of services. Given the current privacy and data security debate, we conclude that policymakers are well-advised to secure the potentials of data-driven business by the development and maintenance of mechanisms that effectively protect customers from data misuse.

[2] In the first quarter of 2017 Alphabet (Google's parent company), Amazon, Apple, Facebook and Microsoft collectively accounted for over $25bn in net profit (https://www.economist.com/news/leaders/21721656-data-economy-demands-new-approach-antitrust-rules-worlds-most-valuable-resource).

3.2 THE EXPERIMENT

We experimentally investigate whether awareness of the extensive profits of a passive big player crowd-out the willingness to provide personal information to information-based public goods. In the following, we present the experimental design, our hypotheses and the experimental procedure.

3.2.1 EXPERIMENTAL DESIGN

Subjects play a public goods game in groups of four active contributors and one passive big player. The unit of provision is information. The big player cannot contribute to the public good, but profits from the provision of information by the active group members. The four active group members receive 10 information sheets with questions retrieving personal information.[3] To provide information to the public good, group member i has to truthfully answer the question.

The provision of information benefits all group members as well as the big player. For each unit provided by group member i, each of the other group members receives a return of $\alpha = 0.24$ while the big player profits threefold ($3\alpha = 0.72$). We assume that for the information provider, the explicit provision cost equals the benefit from provision, i.e., the net costs of provision are zero. If no information is provided, neither the other group members nor the big player receive a benefit. Thus, the active group member i's payoff function is given by:

$$\pi_i^a = e + \alpha \sum_{j \neq i} \text{number of units provided by group member } j, \tag{3.2.1}$$

i.e., group member i's payoff increases in the number of units provided by any of the other active group members $j \neq i$. The big player's payoff function is given by:

$$\pi^b = e + 3\alpha \sum_{j=1}^{4} \text{number of units provided by group member } j. \tag{3.2.2}$$

[3]Note that the information we retrieved is the same as in Section 2.5.3.

The big player's payoff increases in the number of units provided by any of the four active group members. All group members as well as the big player start with the same initial endowment of $e = 6$.

We control for the effects of the presence of the big player by a *baseline* treatment without a big player. In the *baseline* treatment, the active group members' payoff function π_i^a is identical to that in the *big player* treatment (compare Equation 3.2.2). To control for effects of information as the unit of provision, we set up two additional treatments (with and without a big player) where money is the unit of provision. In an otherwise identical environment, subjects receive 10 money sheets. To provide a money sheet to the public good, group member i has to write the word "GROUP" on it. The resulting four treatments are summarized in Table 3.1.

Table 3.1: Treatments.

	information	*money*
baseline	INFO_BASE	MONEY_BASE
big player	INFO_BIG	MONEY_BIG

Note that treatment INFO_BASE and MONEY_BASE are identical to INFO_RED_NC and MONEY_RED_NC in Section 2.5.3. We use the data of the *no cost* treatments of experiment in Section 2.5.3 as the *baseline* observations in the present study since there are no differences in the provision of information and money. This allows for a controlled comparison of information and money provision in presence of the big player.

3.2.2 HYPOTHESES

Since the net provision cost is zero, any combination of provision levels is in equilibrium, both in the *information* and *money* treatments. If subjects care about others' payoffs, it is likely that the equilibrium with full provision is selected. The presence of the big player does not make a difference under the assumption of standard preferences. Predictions change if we assume that players hold other-regarding preferences and are inequality-averse with regard to monetary outcomes (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). Then, full provision no longer is in equilibrium in the *big player* treatments. Since all group members as well as the big player start with the same initial endowment of e, and given the higher return for the big player, any unit provided increases the difference in outcomes between the big player and the provider. Hence, provision to the public good always makes an inequality-averse player worse off. Engel and Rockenbach (2011) provide experimental evidence that indeed subjects' willingness to provide money to a material public good is lower if there are positive externalities on wealthy third parties. We expect lower average provision levels in the *big player* treatments than in the *baseline* treatments.

Hypothesis 1. *Average provision levels are lower in the big player treatments than in the baseline treatments.*

Since for both units of provision (information and money) the payoff functions are identical, outcome-based theories yield the same predictions and lead us to expect no differences between the provision of information and money.

Hypothesis 2. *Average provision levels do not differ between the information and money treatments.*

3.2.3 PROCEDURE

We collected one-shot paper-and-pencil decisions. By conducting one-shot experiments, we ensured statistical independence of all observations. A total of 192 participants (57% female and 43% male) participated in the experiment. All sessions were conducted in 2013 in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany. We recruited our participants using the Online Recruitment System for Economic Experiments (Greiner, 2015). Written instructions informed the participants about the course of the experiment.[4] We ensured that answers to the questions in the *information* treatments could not be ascribed to a participant's identity. This was stated clearly in the instructions. In the *big player* treatments, active group members were labeled "group members A". The big player was labeled "group member B". At the end of the experiment, participants answered a short questionnaire. In this questionnaire, we also asked for participants' beliefs regarding their group members' decisions. Sessions lasted around one hour.

On average, participants earned €15.20 in INFO_BASE (min: €13.10; max: 15.70), €15.10 in INFO_BIG (min: €13.10; max: €15.70), €15.50 in MONEY_BASE (min: €14.50; max: €15.70), and €14.00 in MONEY_BIG (min: €9.70; max: €15.70). In comparison, big players earned more than twice as much. On average, big players earned €34.90 in INFO_BIG (min: €29.40; max: €37.30), and €30.40 in MONEY_BIG (min: €19.30; max: €37.3) including the show-up fee of €2.50.

In addition to the 52 independent observations in treatment INFO_BASE and MONEY_BASE (compare section 2.5.3), we collected 44 independent observations in treatment INFO_BIG and MONEY_BIG. Furthermore, eleven passive big players participated in INFO_BIG and MONEY_BIG. If not stated otherwise, statistical comparisons between treatments are based on two-sided Mann-Whitney U-tests (MWU).

3.3 RESULTS

In Figure 3.1, average provision levels are given by treatment. Evidently, the average provision level is close to full provision in both *information* treatments. Interestingly, we find that there is no significant difference between average provision levels in INFO_BASE (9.25) and INFO_BIG (9.16, two-sided MWU-test: p=0.8989). This result is in contrast to Hypothesis 1.

[4] Translated instructions can be found in the Appendix.

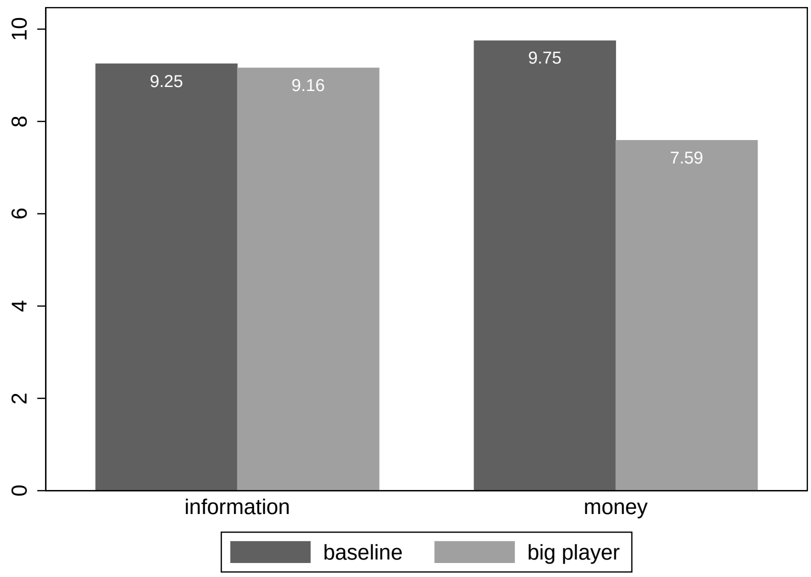


Figure 3.1: Average provision levels.

Result 1. *Information provision does not differ significantly between the baseline and big player condition.*

To examine whether the unit of provision drives this result, we compare average provision levels in the *information* treatments to the *money* treatments. In contrast to Hypothesis 2, we find that in the *big player* treatments money is significantly under-provided as compared to information (p=0.0351). However, in the *baseline* treatments there is no significant difference between the provision of information and money (p=0.2322). This is in line with Hypothesis 2.

Result 2. *In absence of the big player, information and money provision do not differ significantly. In presence of the big player, money provision is significantly lower than information provision.*

If we compare average provision levels between the two *money* treatments, we find significantly lower average provision levels in MONEY_BIG (7.59) than in MONEY_BASE (9.75, p=0.0006). This result supports Hypothesis 1.

Result 3. *Money provision is significantly lower in the big player condition than in the baseline condition.*

3.4 CONCLUSION

While the collection of personal data in exchange for the usage of Internet services is ubiquitous, it is unknown whether this form of payment is more or less efficient than the charging of monetary user fees. In a laboratory experiment, we show that information provision is less susceptible to the gains of a big player than the provision of money. Since both the providers of information and the big player benefit from an overall higher provision level, this is beneficial to all parties. In the experiment, the presence of the big player crowds out the willingness to provide money. Information provision does not vary with the presence of the big player.

Our experimental results are in line with the empirical observation that people willingly provide their personal data in exchange for the usage of Internet services. Therein, the potential gains of service providers who collect and use the data for their own business undertakings seems to be of minor importance. All actors involved benefit from this situation. Customers gain from an increased quality of data-based services, and firms realize rents from data collection. From a business perspective, our experiment confirms the profitability of asking for customers' personal data instead of charging user fees. However, in light of the recent privacy and data protection debate, these mutual benefits for firms and customers seem to be at risk. We conclude that policymakers are well-advised to secure the potentials of data-driven business by the development and maintenance of mechanisms that effectively protect customers from data misuse.

3.A EXPERIMENTAL APPENDIX

3.A.1 TRANSLATED INSTRUCTIONS

The instructions are translated from German. For the instructions of INFO_BASE and MONEY_BASE, see translated instructions of the INFO_RED_NC and MONEY_RED_NC treatments in Section 2.A.1.

Instructions of INFO_BIG

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us.

Depending on your own and the other participants' decisions, you can earn money in this experiment.

During the experiment, you are not allowed to talk to other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and all payments. All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a person who makes a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

At the end of the experiment, you receive 2.50 Euro for showing up. During the experiment, you can earn additional money. On the following pages we explain to you the exact course of the experiment.

Groups and rounds

- In this experiment, there are two types of participants, **group member A and group member B**. At the beginning of the experiment it is **randomly** determined whether you are group member A or B.
- Each group consists of 5 group members, 4 group members A and 1 group member B. At the beginning of the experiment, group member B receives an **initial endowment** of 6.00 Euro.
- The experiment consists of **only one round**.

Course of the experiment

- **Each group member A** receives **10 information sheets** in a **white envelope** labeled with the word “Information”. Each of the information sheets contains a question about the respective group member. Each of the group members A takes out all 10 information sheets of the white envelope, and decides for each of the information sheets whether she wants to **provide the information about herself for the group or keep it for herself**:
  - If a group member A provides her information, the **whole group including group member B profits** from this. For this purpose, group member A writes the truthful answer to the question on the information sheet. Group member A puts the answered information sheet back into the white envelope. From the answered information sheets that the group members A have put into the white envelopes, an **anonymized frequency statistic about the provided information is generated for group member B**. For each answered information sheet, **group member B receives 2.04 Euro at the end of the experiment**, whereof 1.) she keeps **0.72 Euro for herself**, 2.) has to give **0.60 Euro to the group member A who has provided the respective information sheet**, and 3.) **has to give 0.24 Euro to each of the other group members A**.

- If a group member A keeps her information for herself, **only this group member A herself profits from this**. For this purpose, group member A leaves the information sheet completely empty. Group member A puts the empty information sheet back into the white envelope. For each empty information sheet group member A puts into the white envelope, **this group member herself (but neither group member B nor any of the other group members A) receives 0.60 Euro**.

- **Group member B** also receives a white envelope labeled with the word "Information". Group member B waits while the group members A finish their information sheets.

- The **payoff of a group member A** is calculated as follows:

0.24 Euro	times	the number of information sheets which have been provided by the **other** group members A
+		
0.60 Euro	times	10 (= number of information sheets in the envelope)
=		group member A's payoff

- The **payoff of group member B** is calculated as follows:

6.00 Euro		initial endowment
+		
2.04 Euro	times	number of information sheets that have been provided by all group members A
-		
(1 $\times$ 0.60 Euro	times	number of information sheets that have been provided by all group members A
+		
3 $\times$ 0,24 Euro)		
= 6.00 Euro		
+ 0.72 Euro	times	number of information sheets that have been provided by all group members A
=		Group member B's payoff

- After all group members A have made their decisions, **all 10 information sheets should be put back into the white envelope**. The lab team collects the white envelopes and generates the **anonymized frequency statistic about the provided information of the group members A** for group member B. After that, the lab team hands out **white envelopes** to all participants. Group member B receives the anonymized frequency statistic about the provided information of the group members A in this envelope.

- No group member A gets to know how many and which information sheets the other group members A have provided and how many they have kept for themselves. Group member B only gets to know how many information sheets the group members A have provided, but does not learn the identity of a certain group member A that has provided a certain number of information sheets. Each group member only learns **her own payoff** after the experiment.

- After the lab team has collected the white envelopes, all group members receive a **questionnaire**. Please fill in the questionnaire while the lab team calculates your payoffs. After you have filled in the questionnaire, please stay at your cabin until we separately call you for payment.

Data protection

- **Please note**: The information about the group members which has been put into the white envelopes will be statistically evaluated anonymously, and destroyed by the experimenter immediately thereafter. At no time will information be assigned to the person of a single participant. Information will not be passed to third parties. Information will not be used for any other than research purpose, especially not for any direct or indirect commercial purpose.

Good luck and thank you very much for your participation!

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us.

Depending on your own and the other participants' decisions, you can earn money in this experiment.

During the experiment, you are not allowed to talk to other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and all payments. All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a person who makes a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

At the end of the experiment, you receive 2.50 Euro for showing up. During the experiment, you can earn additional money. On the following pages we explain to you the exact course of the experiment.

Groups and rounds

- In this experiment, there are two types of participants, **group member A and group member B**. At the beginning of the experiment it is **randomly** determined whether you are group member A or B.
- Each group consists of 5 group members, 4 group members A and 1 group member B. At the beginning of the experiment, group member B receives an **initial endowment** of 6.00 Euro.
- The experiment consists of **only one round**.

Course of the experiment

- **Each group member A** receives **10 money sheets** in a **blue envelope** labeled with the word "Money". Each of the group members A takes out all 10 money sheets of the blue envelope, and decides for each of the money sheets whether she wants to **provide the money for the group or keep it for herself**:
  - If a group member A provides her money, the **whole group including group member B profits** from this. For this purpose, group member A writes the word "GROUP" on the money sheet. Group member A puts the labeled money sheet back into the blue envelope. From the labeled money sheets that the group members A have put into the blue envelopes, an **anonymized frequency statistic about the provided money is generated for group member B**. For each labeled money sheet, **group member B receives 2.04 Euro at the end of the experiment**, whereof 1.) she keeps **0.72 Euro for herself**, 2.) has to give **0.60 Euro to the group member A who has provided the respective money sheet**, and 3.) **has to give 0.24 Euro to each of the other group members A**.
  - If a group member A keeps her money for herself, **only this group member A herself profits from this**. For this purpose, group member A leaves the money sheet completely empty. Group member A puts the empty money sheet back into the blue envelope. For each empty money sheet group member A puts into the blue envelope, **this group member herself (but neither group member B nor any of the other group members A) receives 0.60 Euro**.
- **Group member B** also receives a blue envelope labeled with the word "Money". Group member B waits while the group members A finish their money sheets.

- The **payoff of a group member A** is calculated as follows:

0.24 Euro	times	the number of money sheets which have been provided by the **other** group members A
+		
0.60 Euro	times	10 (= number of money sheets in the envelope)
=		group member A's payoff

- The **payoff of group member B** is calculated as follows:

6.00 Euro		initial endowment
+		
2.04 Euro	times	number of money sheets that have been provided by all group members A
-		
(1 × 0.60 Euro	times	number of money sheets that have been provided by all group members A
+		
3 × 0,24 Euro)		
= 6.00 Euro		
+ 0.72 Euro	times	number of money sheets that have been provided by all group members A
=		Group member B's payoff

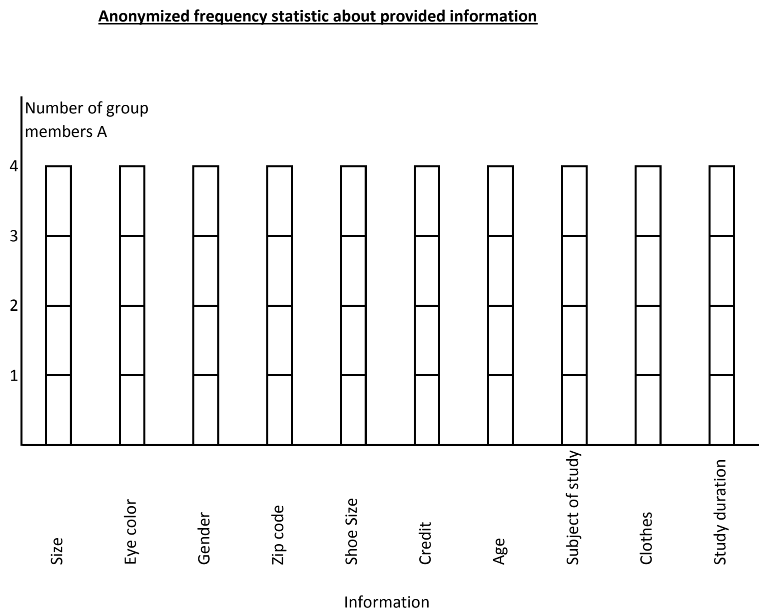
- After all group members A have made their decisions, **all 10 money sheets should be put back into the blue envelope**. The lab team collects the blue envelopes and generates the **anonymized frequency statistic about the provided money of the group members A** for group member B. After that, the lab team hands out **blue envelopes** to all participants. Group member B receives the anonymized frequency statistic about the provided money of the group members A in this envelope.

- No group member A gets to know how many money sheets the other group members A have provided and how many they have kept for themselves. Group member B only gets to know how many money sheets the group members A have provided, but does not learn the identity of a certain group member A that has provided a certain number of money sheets. Each group member only learns **her own payoff** after the experiment.
- After the lab team has collected the blue envelopes, all group members receive a questionnaire. Please fill in the **questionnaire** while the lab team calculates your payoffs. After you have filled in the questionnaire, please stay at your cabin until we separately call you for payment.

Good luck and thank you very much for your participation!

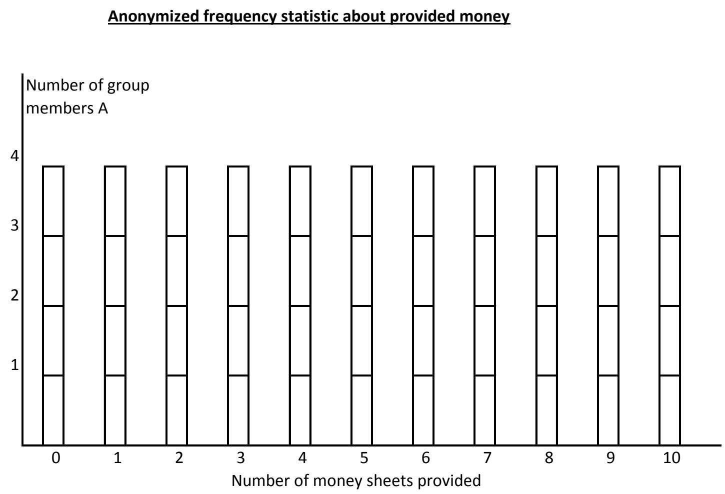
Frequency statistics for big player (information condition)

In the experiment, the bars were colored according to the number of group members A who provided a certain number of units.



Frequency statistics for big player (money condition)

In the experiment, the bars were colored according to the number of group members A who provided a certain item.



3.A.2 PICTURES

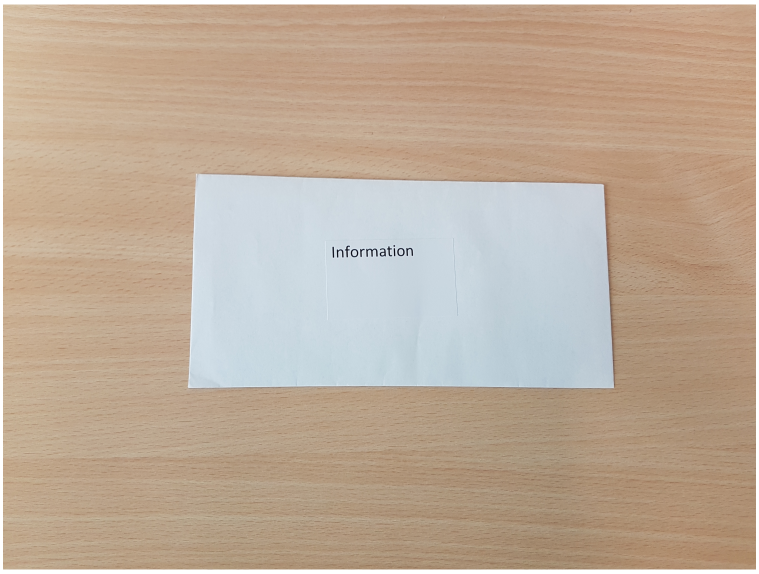


Figure 3.A.1: Envelope information treatments.

Figure 3.A.2: Envelope money treatments.

Figure 3.A.3: Information sheets.

Figure 3.A.4: Money sheets.

CHAPTER 4

COOPERATION AND THE PROSPECT OF COMPETING OVER JOINTLY CREATED SURPLUS

Abstract

Frequently, surplus from cooperation is subject to subsequent competition. Co-workers engage in teamwork and firms engage in joint innovation, and then compete for a share of a team bonus or industry profit. Steep incentives (e.g., provided by competitive promotion schemes or patent systems) may lead to an unwillingness to cooperate in the first place. I experimentally study the willingness to cooperate under the prospect of competing in a winner-take-all contest. In comparison to a proportional-share contest, the level of cooperation more than halves from 81% to 38% in this situation. The results demonstrate that steep incentives may seriously impede cooperation.

4.1 INTRODUCTION

Cooperation frequently creates surplus that is subject to subsequent competition. Especially in knowledge-intensive production (e.g., team work, research collaborations, joint innovation), individual contributions to joint production may be ex post unverifiable. Then, former cooperators engage in competitive rent-seeking to secure their share of the jointly created surplus (e.g., team members report their contributions to a joint project to secure their share of a team bonus, and firms increase their market supply to secure their share of industry profit). A problem may arise if the acquired share does not reflect the initial investment into cooperation (see Bayer, 2016).

Steep incentives are ubiquitous in the organizational structure of firms and in the industry. Tournament-like reward schemes and competitive promotion systems are installed to enhance worker productivity. Patent systems are implemented to foster technological progress. However, steep incentives may have adverse effects with regard to joint surplus creation. In the most extreme case, individuals can obtain exclusive rights over the jointly created surplus, leaving former cooperators uncompensated for their engagement. The question is whether steep incentives affect the willingness to cooperate in the first place.

In this paper, I provide evidence from a controlled laboratory experiment that investigates the emergence of cooperation in a two-stage game of cooperation and competition. The cooperation stage is implemented in a two-player threshold public goods game. Only if the group members contribute enough to a group project, they enter the competition stage. The competition stage is implemented in a generic contest game. In an otherwise identical environment, I systematically vary the allocation of surplus in the competition stage. In one treatment, cooperation creates a divisible rent that is allocated proportionally to the group members' investments (proportional-share mechanism). This treatment depicts a situation in which team members report their contributions to a joint project to secure a share of a team bonus, or in which firms set their market supply to secure a share of the industry profit. In the other treatment, cooperation creates an indivisible rent that is acquired by the winner of a lottery contest (winner-take-all mechanism). This treatment depicts a situation in which individual team members compete for promotion, or in which firms apply for patents. The contest environment allows for a controlled examination of the impact of different allocation rules on cooperation since the equilibrium predictions, best responses and expected equilibrium payoffs are equivalent between the proportional-share and the winner-take-all contest. Furthermore, parameters are chosen such that cooperation is also an equilibrium in both treatments if players are risk-averse. By the present study, I aim at answering the following research question: Does the prospect of competing in a winner-take-all competition impede the willingness to cooperate?

I find that in comparison to the proportional-share mechanism, the winner-take-all mechanism indeed reduces cooperation from 81% to 38%.

Interestingly, this result cannot be explained by differences in decision-making in the competition stage. There are differences in the cooperation stage, however. Already in the first period, significantly less subjects cooperate in the winner-take-all treatment as compared to the proportional-share treatment. However, this effect alone cannot explain the treatment effect. Earning less than the other group member significantly decreases the willingness to cooperate, but only the experience of being unable to recoup investments into cooperation can explain the treatment effect on cooperation.

This study shows that although winner-take-all competition likely elicits high individual investments, it seemingly can adverse effects on joint investments. From the results of this paper, it becomes clear that steep incentives may impede the emergence of cooperation. Thus, whenever joint production is desirable, organizations and regulators are well-advised to take into account that winner-take-all competition may have detrimental effects on cooperation. From a more general perspective, it seems that the economic costs of the "winner-take-all society" (Frank and Cook, 1995) may be severely underestimated if we do not consider the cooperation that does not take place.

4.2 RELATED LITERATURE

The debate concerning the economic costs of winner-take-all competition has a long tradition in economics and related fields.[1] However, although competition for jointly generated surplus is ubiquitous in many organizational settings and markets, the interaction between surplus generation and surplus sharing has received only limited attention in the experimental economics literature. To the best of my knowledge, there exists only one experimental study that focuses on the interaction between surplus generation and surplus sharing. Bayer (2016) investigates the willingness to cooperate when the surplus from cooperation is allocated in a proportional-share

[1]For an extensive discussion of the economic costs of the "winner-take-all society", see Frank and Cook (1995).

contest. The rent in the contest linearly increases in subjects' contributions into cooperation. Bayer (2016) finds strong cooperation in this setting. In contrast to the present paper, Bayer (2016) does not contrast cooperation under the prospect of competing in a proportional-share contest to competing in a winner-take-all contest.

Another related paper investigates decision-making in strategic alliances (Ke et al., 2013). Although the focus of their study is not on joint surplus generation, Ke et al. (2013) also combine cooperative elements and competition. In the first stage, two players form a strategic alliance and jointly compete against a third player in a lottery contest. The probability of winning depends on the total investment into competition by the alliance members and the third player. If the alliance wins, the alliance members compete against each other in a second lottery contest. Ke et al. (2013) find that investments in the first stage are lower under the prospect of winner-take-all competition as compared to the prospect of an equal split of the prize. However, with regard to the research question of this paper, no general conclusions can be drawn from the results of Bayer (2016) and Ke et al. (2013). In the former study, the prize increases linearly in contributions to cooperation while in the latter study, the game entails additional stochastic and strategic uncertainty.

Another strand of literature that is relevant with regard to the present study focuses on behavioral spillovers from cooperation on competition (and vice versa) in market environments (Brandts and Riedl, 2017; Cason and Gangadharan, 2013; Nicklisch, 2012; Suetens, 2008), and organizational settings (Buser and Dreber, 2015; Burks et al., 2009). The findings by these studies corroborate the hypothesis that winner-take-all competition may have negative effects on cooperation. For instance, Brandts and Riedl (2017), Buser and Dreber (2015), Cason and Gangadharan (2013) and Burks et al. (2009) provide evidence of negative spillovers from competition on cooperation when competition leads to very unequal outcomes. In contrast, Nicklisch (2012) and Suetens (2008) find positive spillovers from cooperation on competition when competition leads to rather equal outcomes.

A different line of experimental research which is also relevant with regard to the present study focuses on decision-making in contests. It is a well-known phenomenon that subjects engage in overinvestment (i.e., investments are higher than the Nash equilibrium prediction) and overspreading (i.e., investments cover the entire strategy space) in experimental contests (for a review, see Dechenaux et al., 2015; Sheremeta, 2013). Cason et al. (2010) provide evidence that if given the choice to opt out, subjects less frequently enter a winner-take-all than a proportional-share contest. With regard to differences in decision-making in winner-take-all and proportional-share contests, experimental evidence is mixed. Some studies report significantly higher investments in winner-take-all than in proportional-share contests (e.g., Cason et al., 2013; Eisenkopf and Teyssier, 2013). Others find no or only limited support that investments differ (e.g., Chowdhury et al., 2014; Masiliunas et al., 2014; Fallucchi et al., 2013; Shupp et al., 2013). With regard to the present study, differences in decision-making in the competition stage could have an impact on the willingness to cooperate. Thus, I set up two control treatments to account for these differences.

4.3 THE MODEL

To address my research question, I set up a two-stage game of cooperation and competition. Holding everything else constant, I vary the allocation mechanism in the competition stage. In Section 4.3.1, I show that under (risk-neutral) standard preferences and expected payoff maximization, the subgame-perfect Nash equilibria are identical between the two games. In Section 4.3.2, I show that cooperation is a subgame-perfect Nash equilibrium also under the assumption of risk aversion. The two stages of the game are given by:

1) *Cooperation stage.* Two players form a group and simultaneously decide how many points a_i of their endowment A to contribute to a group project and how many points to keep for themselves. The group project is only successful if total contributions $a_1 + a_2$ are equal

to or exceed a threshold T. No refund is paid if the group project is not successful, and no rebate is paid on excessive contributions.

2) *Competition stage.* If $a_1 + a_2 \geq T$, the group project is successful and the two players enter the competition stage. In the competition stage, each of the two players receives an endowment B. The two players simultaneously decide how many points b_i to invest into the acquisition of a prize V. Competition takes place in a contest with a winner-take-all or proportional-share allocation mechanism.

Player i's payoff function of the two-stage game is given by:

$$\Omega_i(a_i, a_j, b_i, b_j) = \begin{cases} A - a_i + \omega_i(b_i, b_j) & \text{if } a_1 + a_2 \geq T \\ A - a_i & \text{if } a_1 + a_2 < T. \end{cases}$$

$\omega_i(b_i, b_j)$ denotes the payoff player i realizes in the competition stage. I study competition in a contest environment following Tullock (1980). Player i's expected payoff in the competition stage is given by:

$$E\left[\omega_i(b_i, b_j)\right] = B + p_i(b_i, b_j)V - b_i.$$

The contest success function for both mechanisms is given by:

$$p_i(b_i, b_j) = \begin{cases} \frac{b_i}{b_i + b_j} & \text{if } b_1 + b_2 > 0 \\ 0 & \text{otherwise.} \end{cases}$$

In the winner-take-all mechanism, the contest success function defines the probability that player i wins the prize. In the proportional-share mechanism, the contest success function defines the proportion of the prize awarded to player i. The resulting payoff in the winner-take-all competition is given by:

$$\omega_i(b_i, b_j) = \begin{cases} B - b_i + V & \text{if player } i \text{ wins} \\ B - b_i & \text{if player } i \text{ does not win.} \end{cases}$$

In the proportional-share competition, the payoff is given by:

$$\omega_i(b_i, b_j) = B - b_i + \frac{b_i}{b_i + b_j} V.$$

I assume that $A < T < 2A$, meaning that (1) the group project is never successful if only one player contributes a positive amount, and (2) that group members do not have to contribute their entire endowment A to make the group project successful. Further, I assume that $B = \frac{T}{2}$ and $V \geq B$. The game is solved via backward induction.

4.3.1 STANDARD PREFERENCES

In the following, I focus on pure-strategy equilibria. Under the assumption of standard preferences and expected payoff maximization, the stage equilibrium of the competition stage is unique (Szidarovszky and Okuguchi, 1997) and given by equilibrium investments of:

$$b_i^* = b_j^* = \frac{1}{4} V.$$

This stage equilibrium holds for all first stage outcomes with $a_i + a_j \geq T$ since the threshold interaction in the first stage makes the second stage history-independent.[2] Expected equilibrium payoffs in the competition stage are given by:

$$E\left[\omega_i(b_i^*, b_j^*)\right] = B + \frac{1}{4} V.$$

Player i never contributes more in the first stage than she expects to earn in the second stage. The two-stage game has multiple subgame perfect Nash equilibria (SPNE) that can be classified by a set of cooperative pure-strategy SPNE and one uncooperative pure-strategy SPNE.

Proposition 1. *We have a set of cooperative SPNE with equilibrium investments of* $b_i^* = b_j^* = \frac{1}{4}V$ *in the competition stage, and equilibrium contributions of* $a_i^* + a_j^* = T$ *where* $a_i^*, a_j^* \in [T - (B + \frac{1}{4}V), B + \frac{1}{4}V]$ *in the cooperation stage. Furthermore, we have one uncooperative SPNE with*

[2] The second stage in the game presented by Bayer (2016) is not history-independent since the size of the prize in the second stage depends on contributions in the first stage.

equilibrium investments of $b_i^* = b_j^* = \frac{1}{4}V$ *in the competition stage, and equilibrium contributions of* $a_i^* = a_j^* = 0$ *in the cooperation stage.*

Proof. See Appendix. □

4.3.2 RISK AVERSION

The above SPNE predictions rely on the assumption of risk neutrality. However, under the given assumptions, cooperation is also a SPNE under the assumption of risk aversion. If players are risk-averse, expected utility in the winner-take-all competition is given by:

$$EU\left[\omega_i(b_i, b_j)\right] = -\frac{b_i}{b_i + b_j} e^{-\eta(B - b_i + V)} - \frac{b_j}{b_i + b_j} e^{-\eta(B - b_i)}$$

where $0 < \eta < 1$ measures the player's constant absolute risk aversion (compare Cornes and Hartley, 2003). In the winner-take-all mechanism, the Nash equilibrium prediction for the competition stage is given by:

$$b_i^* = b_j^* = \frac{1}{2}\frac{-1 + e^{\eta V}}{\eta(1 + e^{\eta V})},$$

Equilibrium investments are lower than under risk neutrality for any $\eta > 0$ (compare Abbink et al., 2010). The overall expected utility for both stages in the winner-take-all mechanism is given by:

$$\begin{aligned} EU\left[\Omega_i(a_i, a_j, b_i^*, b_j^*)\right] =& \frac{1}{2}\left(-e^{-\eta(A - a_i + B - \frac{1}{2}\frac{-1+e^{\eta V}}{\eta(1+e^{\eta V})} + V)}\right) \\ &+ \frac{1}{2}\left(-e^{-\eta(A - a_i + B - \frac{1}{2}\frac{-1+e^{\eta V}}{\eta(1+e^{\eta V})}}\right). \end{aligned}$$

In the competition stage with proportional-share mechanism, a risk-averse player's expected utility is given by:

$$EU\left[\omega_i(b_i, b_j)\right] = -e^{-\eta(B - b_i + \frac{b_i}{b_i + b_j}V)}.$$

The Nash equilibrium prediction is the same as under the assumption of risk neutrality, i.e., $b_i^* = b_j^* = \frac{1}{4}V$.

The overall expected utility for both stages in the proportional-share mechanism is given by:

$$EU\left[\Omega_i(a_i, a_j, b_i^*, b_j^*)\right] = -e^{-\eta(A-a_i+B+\frac{1}{4}V)}.$$

If she chooses not to contribute to the group project at all, a risk-averse player i's utility is given by:

$$U = -e^{-\eta A}.$$

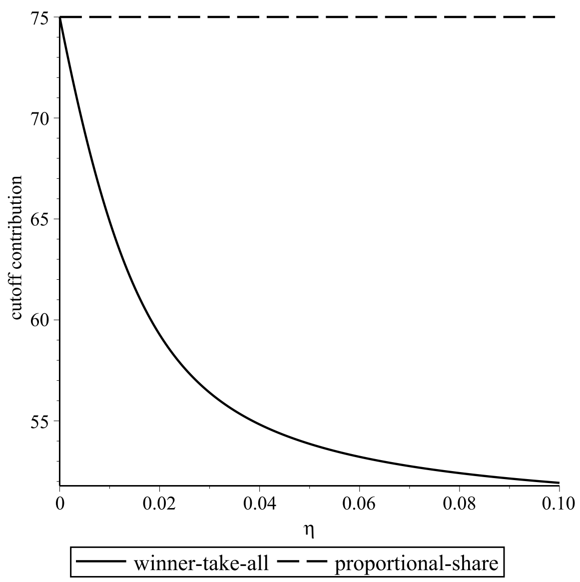


Figure 4.1: Cutoff contribution under risk aversion (A=75, B=50, V=100).

To determine the set of SPNE under risk aversion, the cutoff contribution has to be determined that exactly equals the utility from not cooperating with the expected utility from cooperation. Expected utility from cooperation in the proportional-share mechanism is higher than from not cooperating as long as $a_i \leq \hat{a} = \frac{1}{4}V + B$. In the winner-take-all mechanism, for cooperation to be a SPNE it must hold that:

$$a_i \leq \bar{a} = B + V + \frac{1}{2}\frac{2(ln(2) + ln(\frac{1}{1+e^{\eta V}}))(1 + e^{\eta V}) + 1 - e^{\eta V}}{\eta(1 + e^{\eta V})}.$$

$\bar{a}$ is the cutoff contribution that exactly equals the utility from not cooperating with the expected utility from cooperating in the winner-take-all mechanism. The set of cooperative SPNE in the winner-take-all mechanism becomes smaller as risk aversion increases. For cooperation to be a SPNE, this cutoff contribution may not fall below $\frac{T}{2}$. In the limit, we get:

$$\lim_{\eta \to \infty} \bar{a} = B.$$

Hence, since $B = \frac{T}{2}$ at least one cooperative pure-strategy SPNE with $a_i^* = a_j^* = \frac{T}{2}$ exists in the winner-take-all mechanism. For illustrative purposes, in Figure 4.1 the cutoff contributions $\hat{a}$ and $\bar{a}$ are given for the winner-take-all (solid line) and the proportional-share mechanism (dashed line) for $A = 75$, $B = 50$ and $V = 100$.

4.4 THE EXPERIMENT

To study the impact of anticipated winner-take-all competition on cooperation, I implement the model presented in Section 4.3 in a laboratory experiment. Holding expected equilibrium payoffs constant, the experimental treatments vary the allocation mechanism in the contest. The experiment is implemented in a repeated strangers matching protocol. Players are randomly rematched within matching groups of six in each of the 20 periods. In the experiment, I measure cooperation by the percentage of successful group projects, i.e., by the average success rate.

4.4.1 EXPERIMENTAL IMPLEMENTATION

The experiment is implemented by the following parameterization: The initial endowment in the cooperation stage is given by $A = 75$, and subjects can contribute $a_i \in \{0, 1, ..., 75\}$ to the group project. The threshold is set to $T = 100$. As outlined in the previous section, no refund is paid if the group project is not successful and no rebate is paid on excessive investments (i.e., payoffs in the cooperation stage are given by the amount the subject keeps for herself, $75 - a_i$). If the group project is successful and the two players enter the competition stage, each group member receives an endowment of $B = \frac{T}{2} = 50$. Subjects can invest $b_i \in \{0, 1, ..., 50\}$ into the acquisition of the indivisible prize (their share of the divisible prize). The prize in the competition stage is set to $V = 100$. Under the given parameterization, expected equilibrium payoffs in the competition stage are given by $E\left[\omega_i(b_i^*, b_j^*)\right] = 75$.

4.4.2 TREATMENTS

I set up four treatments which are given in Table 4.1. The main treatment variable is the allocation mechanism (*proportional-share* versus *winner-take-all*). In addition to the two main treatments SHARE and LOTTERY, I set up two control treatments SHARE_BASE and LOTTERY_BASE without a cooperation stage to account for differences in the competition stage. In conformity with the focal symmetric cooperative SPNE of the two-stage game where both players contribute 50 points to the group project and keep 25 points for themselves, subjects receive 20×(75-50)=500 points as initial endowment in the two baseline treatments to induce equivalence of expected equilibrium payoffs in all four treatments. The initial endowment of 500 points cannot be invested into competition in SHARE_BASE and LOTTERY_BASE.

Table 4.1: Treatments.

		allocation mechanism	
		proportional-share	*winner-take-all*
cooperation stage	*yes*	SHARE	LOTTERY
	no	SHARE_BASE	LOTTERY_BASE

4.4.3 HYPOTHESES

Under the assumption of risk-neutral standard preferences and expected payoff maximization, the set of cooperative SPNE is given by investments of $b_1^* = b_2^* = 25$ in the competition stage. In the cooperation stage, total contributions are given by $a_1^* + a_2^* = 100$ with $a_1^*, a_2^* \in \{25, ..., 75\}$. The uncooperative SPNE is given by $b_1^* = b_2^* = 25$ and $a_1^* = a_2^* = 0$ (see Proposition 1). (Expected) payoffs of the two-stage game are $E\left[\Omega_i(a_i^*, a_j^*, b_i^*, b_j^*)\right] = 100$ in the symmetric cooperative SPNE, and $\Omega_i(a_i^*, a_j^*, b_i^*, b_j^*) = 75$ in the symmetric uncooperative SPNE. Under the assumption of expected payoff maximization, the theoretical prediction does not depend on the allocation mechanism. Further, the symmetric cooperative SPNE also exists under risk aversion. Thus, we do not expect any differences between SHARE and LOTTERY with respect to cooperation. The Null Hypothesis is given by:

Null Hypothesis. *There is no difference in cooperation between LOTTERY and SHARE.*

It is a well-known phenomenon that subjects engage in overinvestment and overspreading in experimental contest games (see Dechenaux et al., 2015; Sheremeta 2013). Cason et al. (2013) and Eisenkopf and Teyssier (2013) provide evidence that overinvestment and overspreading could be more pronounced in the winner-take-all treatment of our experiment. Then, the realized payoffs would be lower as compared to the proportional-share treatments. This may create lower incentives to cooperate in the first place. Further, the experimental literature provides evidence that subjects less often choose to enter a winner-take-all contest (Cason et al., 2010). Strategic alliances are less successful if subjects expect to compete against each other over an indivisible prize as compared to equally sharing the prize (Ke et al., 2013), but subjects cooperate strongly if they expect to compete over a divisible rent (Bayer, 2016). Taken together, the experimental literature suggests that cooperation is more likely to emerge in SHARE than in LOT-

TERY. The Alternative Hypothesis is given by:

Alternative Hypothesis. *There is less cooperation in LOTTERY than in SHARE.*

4.4.4 PROCEDURE

The experiment was conducted during June 2017 in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany. The recruitment was organized via ORSEE (Greiner, 2015).[3] In compliance with the rules of the CLER, participants received a show-up fee of €4.00.

I conducted two sessions of each of the four treatments. The game was repeated for 20 periods to account for learning effects. The experiment was computerized and programmed utilizing the software package zTree (Fischbacher, 2007). After their arrival, participants received written instructions.[4] Participants' beliefs regarding the other player's contribution in the cooperation stage, and regarding the other player's investment in the competition stage were elicited. Belief elicitation was unincentivized and beliefs were reported simultaneously to participants' contribution and investment decisions, respectively.[5] Sessions lasted between 50 and 60 minutes. At the end of the experiment, participants answered a questionnaire.[6]

After the experiment, points were converted to Euro and paid in cash with an exchange rate of 250 points against 1.00 Euro plus the show-up fee of €4.00. On average, participants earned €10.70 in SHARE (min: €9.50; max: €11.80), €9.90 in LOTTERY (min: €8.20; max: €12.00),

[3]Participants were excluded who had previously participated in another, related experiment in which I studied cooperation under the prospect of competing in a Cournot market with varying degrees of substitution.

[4]Translated instructions are provided in the Appendix.

[5]Screenshots are provided in the Appendix.

[6]Further, they participated in an incentivized risk elicitation task and a distributional preferences task. These tasks were announced only after the main part of the experiment ended. I discarded the data since I find significant differences in the distributional preferences task between SHARE and LOTTERY. These point towards spillover effects from the main part of the experiment on the two tasks. Hence, the explanatory power of the elicited measures is questionable in the present study.

€11.20 in SHARE_BASE (min: €10.80; max: €12.00), and €10.90 in LOTTERY_BASE (min: €7.80; max: €13.70).

I planned to collect 8 independent observations per treatment. Due to no-shows, I collected 7 independent observations in treatment LOTTERY. In total, 186 participants participated in the experiment with 59% female and 41% male. A total of 45% of my sample were students from economics, social sciences, and business administration. If not stated otherwise, statistical comparisons between treatments are based on two-sided Mann-Whitney U-tests (MWU), and comparisons within treatments are based on two-sided Wilcoxon signed-rank tests (WSR).

4.5 RESULTS

I present the results of the experiment in three steps. In part one, I analyze treatment effects on decision-making in the competition stage (see Section 4.5.1). In part two, I proceed by the analysis of treatment effects on decision-making in the cooperation stage (see Section 4.5.2). In part three, I investigate what drives subjects' decision to cooperate.

Table 4.1: Aggregated outcomes.

	SHARE	LOTTERY	SHARE_BASE	LOTTERY_BASE
		Cooperation stage		
Success rate	0.81 (0.09)	0.38 (0.14)		
Contribution	43.59 (4.05)	25.40 (6.89)		
Belief about other's contribution	45.10 (3.58)	27.83 (7.07)		
Profit cooperation stage	31.41 (4.05)	49.60 (6.89)		
		Competition stage		
Bid	36.70 (1.53)	38.79 (1.80)	35.01 (1.08)	38.86 (1.27)
Belief about other's bid	40.19 (1.60)	40.20 (1.92)	35.08 (1.54)	40.16 (1.40)
Profit competition stage	63.30 (1.53)	61.21 (1.80)	64.99 (1.08)	61.04 (1.30)
Ind. obs.	8	7	8	8

Notes: The table reports averages and standard errors (in parentheses) of the cooperation stage (upper part) and the competition stage (lower part) based on independent observations (as reported in the last row) per treatment.

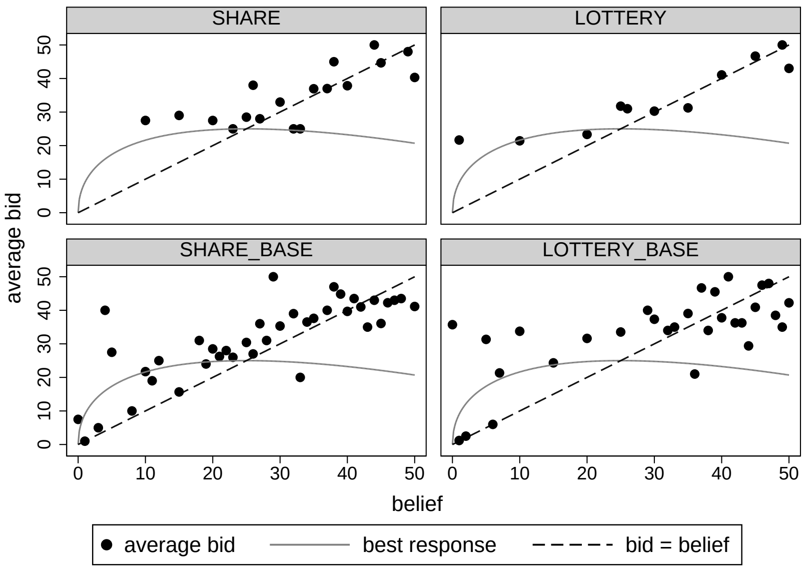


Figure 4.2: Average investments by given beliefs.

Following Rockenbach and Waligora (2016), I investigate subjects' average investments by given beliefs about the competitor's investment in Figure 4.2. The solid gray line gives the theoretical best response function, i.e., the optimal investment for a given belief. The dashed 45°-line gives the linear response of a player who equalizes her investment with her belief about the competitor's investment. In line with Rockenbach and Waligora (2016), I find that subjects' decision-making in the competition stage deviates strongly from the theoretical prediction. Data points are closely located to the dashed 45°-line but do not match the course of the theoretical best response function. In all four treatments, the correlation coefficient between average investments and average beliefs is positive and significant.[7]

[7]Spearman's rank correlation coefficient based on observations at the matching group level is equal to 69% (p=0.0580) in SHARE, equal to 86% (p=0.0137) in LOTTERY, equal to 71% (p=0.0465) in SHARE_BASE, and equal to 67% (p=0.0710) in LOTTERY_BASE.

An investment that matches the competitor's expected investment is suboptimal for any given belief below or above the Nash equilibrium investment. Below the Nash equilibrium, subjects' investments are too low if they match their investment with the competitor's expected investment. Consequently, their probability of winning the prize (their share of the prize, respectively) is inefficiently low. Above the Nash equlibrium, subjects' average investments are too high and consequently, they spend too many points given the marginal (expected) payoff they earn.

On average, subjects expect significantly higher investments by their competitors in LOTTERY_BASE (40.16 points) than in SHARE_BASE (35.08 points, two-sided MWU-test: p=0.0357). No such difference is observed between SHARE (40.19 points) and LOTTERY (40.20 points, p=0.8170). In SHARE_BASE, subjects expect significantly smaller investments than in SHARE (p=0.0357). There is no statistically significant difference between LOTTERY and LOTTERY_BASE (p=0.9079).

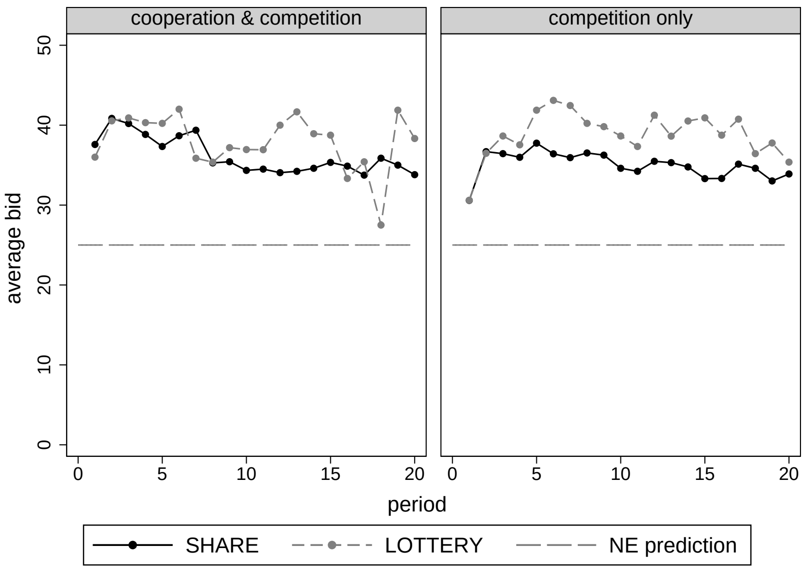


Figure 4.3: Average investments (competition stage).

In Figure 4.3, average investments are given by period. Average investments are clearly above the Nash equilibrium prediction of 25 points throughout all periods. I find statistically significant overinvestment in all four treatments (two-sided WSR-test, SHARE: p=0.0117; LOTTERY: p=0.0180; SHARE_BASE: p=0.0117; LOTTERY_BASE: p=0.0117). Average investments do not differ significantly between SHARE (36.70) and LOTTERY (38.79, two-sided MWU-test: p=0.4875). However, average investments are significantly higher in LOTTERY_BASE (38.86) than in SHARE_BASE (35.01, p=0.0587).[8]

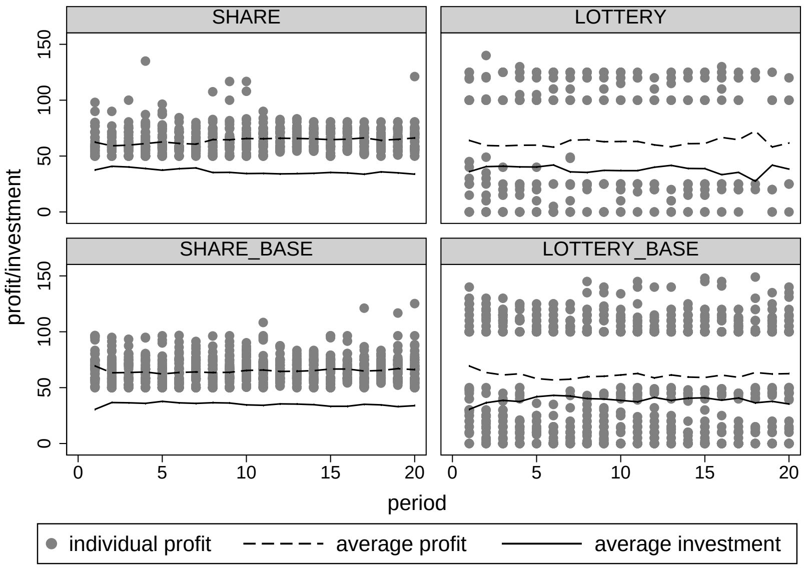


Figure 4.4: Individual payoffs in the competition stage.

Figure 4.4 shows average and individual payoffs in the competition stage. Average payoffs in the competition stage are 63.30 points in SHARE and 61.21 points in LOTTERY (compare Table 4.1). The difference is not statistically significant (two-sided MWU-test: p=0.4875). Average payoffs are higher in SHARE_BASE (64.99) than in LOTTERY_BASE (61.04,

[8]There is no difference in average investments between SHARE and SHARE_BASE (p=0.4008) or between LOTTERY and LOTTERY_BASE (p=0.8170).

p=0.0587). However, there is no statistically significant difference between SHARE and SHARE_BASE (p=0.4008), and also not between LOTTERY and LOTTERY_BASE (p=0.7285). Individual payoffs have a significantly higher standard deviation in LOTTERY than in SHARE (p=0.0012). With regard to the two main treatments, the findings of this section are summarized by the following result:

Result 1. *In the competition stage, average investments and average payoffs are not significantly different between SHARE and LOTTERY.*

4.5.2 TREATMENT EFFECTS ON COOPERATION

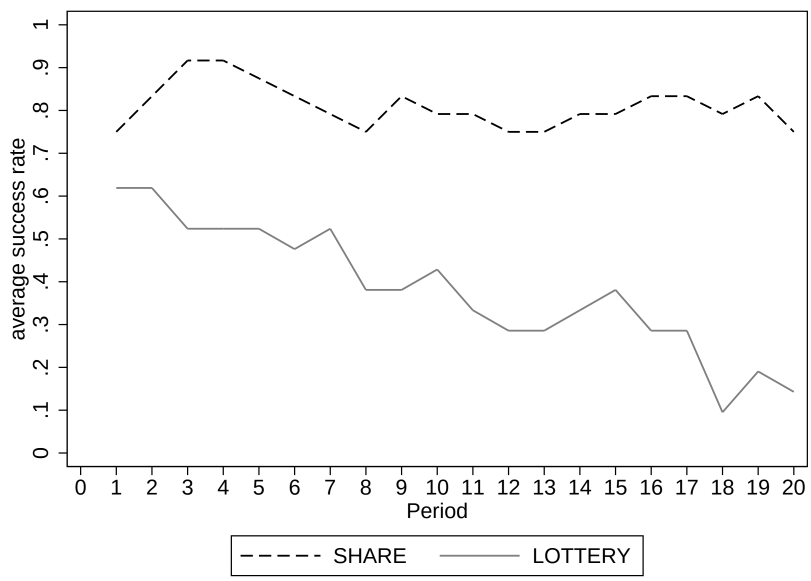


Figure 4.5: Average success rate by period.

In Figure 4.5, the average success rate is given by period for treatment SHARE and LOTTERY. Groups cooperate in both treatments, with average success rates of 81% in SHARE and 38% in LOTTERY (compare Table 4.1). In period 1, significantly more subjects contribute zero points in LOTTERY (19.05%) than in SHARE (4.17%, two-sided Fisher's exact test: p=0.041). From Figure 4.5, it can be seen that in SHARE the average success rate exhibits no significant time trend (Spearman's rank correlation coefficient: p=0.2351). However, in LOTTERY the average success rate decays significantly over time (p=0.0000). The average success rate is significantly higher in SHARE than in LOTTERY (two-sided MWU-test: p=0.0425).[9] This finding is not in line with the Null Hypothesis and supports the Alternative Hypothesis.

Result 2. *In the cooperation stage, the average success rate is significantly higher in SHARE than in LOTTERY.*

4.5.3 DETERMINANTS OF THE LIKELIHOOD OF COOPERATION

Table 4.2 presents the results from a probit regression where the likelihood to make a cooperative contribution is the dependent variable.[10] In line with Result 2, the regression results show that the winner-take-all mechanism has a significant negative effect on cooperation (compare model (1)). Further, cooperation decreases significantly over time. If I control for previous matchings between group members' contributions in model (2), the period effect becomes insignificant. As compared to a matching of two cooperative group members, any other match has a significant negative effect on cooperation. In line with the strangers matching protocol, the own cooperativeness in the previous period seems to drive decision-making more strongly than behavior of the former group member. Hence, in the following I control for the subject's own decision to cooperate in the previous period. In model (3), I study the impact of inequality in outcomes between players. To this end, a dummy variable is added that is equal to one if the subject's period income (the sum of incomes in stage one and two) in period $t-1$ was lower than that of her group member. The effect of this variable on cooperation is negative and significant. However, the treatment effect on

[9] A histogram of individual contribution choices and a figure of average contributions by matching group are provided in the Appendix.

[10] In line with our theoretical predictions, a contribution is classified as cooperative if it is at least 25 points.

Table 4.2: Likelihood of a cooperative contribution.

	(1)	(2)	(3)	(4)
LOTTERY	-1.1486**	-0.3808**	-0.5778*	-0.2070
(0=no, 1=yes)	(0.51)	(0.19)	(0.30)	(0.32)
period	-0.0541***	0.0017	-0.0142*	-0.0233***
	(0.01)	(0.01)	(0.01)	(0.01)
Both uncoop. (t-1)		-3.9080***		
(0=no, 1=yes)		(0.38)		
Self uncoop. × Other coop. (t-1)		-2.3244***		
(0=no, 1=yes)		(0.22)		
Self coop. × Other uncoop. (t-1)		-1.3730***		
(0=no, 1=yes)		(0.20)		
Self coop. (t-1)			3.2339***	3.3223***
(0=no, 1=yes)			(0.32)	(0.33)
Other earned more (t-1)			-0.8090***	
(0=no, 1=yes)			(0.11)	
Own payoff < 75 (t-1)				-1.1640***
(0=no, 1=yes)				(0.22)
constant	1.7459***	2.0513***	-0.7285*	-0.8710**
	(0.31)	(0.18)	(0.40)	(0.38)
Pseudo-R^2	0.1694	0.6883	0.6261	0.6414
Number of observations	1,800	1,710	1,710	1,710
Number of cluster	15	15	15	15

Notes: The table reports results from a random effects probit regression. Robust standard errors (in parentheses) are clustered on matching group. *: p<0.1, **: p<0.05, ***: p<0.01. Baseline category (matching): own contribution cooperative × other's contribution cooperative. Dependent variable: Cooperative contribution (0 = less than 25 points, 1 = equal to or more than 25 points).

cooperation remains significant.

Result 3. *The experience of being worse off than the other player significantly decreases the likelihood of cooperation in the subsequent period, but cannot explain the treatment effect on cooperation.*

Although earning less than the other group member is one of the main features of the winner-take-all mechanism, it cannot explain the treatment effect on cooperation. In model (4), I study the impact of having earned less than the initial endowment in the cooperation stage. The dummy is one if the subject earned less than 75 points. The effect of this variable on cooperation is also negative and significant. Further, the treatment effect is no longer significant.

Result 4. *The experience of earning less than the initial endowment significantly decreases the likelihood of cooperation, and can explain the treatment effect of the winner-take-all mechanism on cooperation.*

I conclude three things from the analysis: First, the treatment effect of the winner-take-all mechanism on cooperation cannot be explained by differences in decision-making or beliefs in the competition stage. Furthermore, average payoffs in the competition stage cannot explain the treatment effect. However, I find that significantly less subjects cooperate in the first period of LOTTERY than in the first period of SHARE. Matching patterns can explain the decay of cooperation in LOTTERY. The experience to earn less than the other player significantly decreases the likelihood of cooperation. However, this cannot explain the treatment effect on cooperation. The experience of earning less than the initial endowment explains the treatment effect on cooperation.

4.6 DISCUSSION

In line with the experimental literature, I find significant overinvestment in all treatments (e.g., see Dechenaux et al., 2015; Sheremeta, 2013). Furthermore, subjects' decision-making in the competition stage deviates strongly from the theoretical prediction (see Rockenbach and Waligora, 2016). Average investments in the competition stage are not significantly different between the two main treatments SHARE and LOTTERY, but between SHARE_BASE and LOTTERY_BASE. The former result is in contrast to Cason et al. (2013), and Eisenkopf and Teyssier (2013) who find significant differences between the two contest forms. However, it is in line with

Chowdhury et al. (2014), Fallucchi et al. (2013), Shupp et al. (2013), and Masiliunas et al. (2014) who also find no significant differences between the proportional-share and the winner-take-all contest under comparable parameterizations.

In the present study, selection effects could drive decision-making in the competition stage of SHARE and LOTTERY. If risk-averse, inequity-averse or loss-averse subjects refrain to cooperate in LOTTERY, this may have an impact on outcomes in the competition stage. However, in Section 4.5 it has been shown that average investments are significantly higher in SHARE than in SHARE_BASE while there is no statistically significant difference between LOTTERY and LOTTERY_BASE. If any, one would expect that selection effects lead to differences in the latter comparison. Behavioral spillovers from cooperation on competition as observed in Nicklisch (2012) and Suetens (2008) also cannot explain why there are differences between the two control treatments, but not between the two main treatments. One would expect that behavioral spillovers lead to lower investments. However, investments do not differ between the main and the control treatments. Overall, the evidence does not point towards systematic distortions due to selection and spillover effects.

In LOTTERY, average success rates are significantly lower than in SHARE. This result is in line with Cason et al. (2010) who observe significantly more entry in the proportional-share contest than in the winner-take-all contest. In their experiment, subjects perform a real-effort task and decide whether they want to enter a contest or be paid by a piece rate. My findings are also in line with Ke et al. (2013) who find that strategic alliances are more successful if they do not expect to compete in a lottery contest. Taken together, the results from the present study and the papers by Cason et al. (2010) and Ke et al. (2013) point towards an aversion against participating in winner-take-all contests.

In SHARE, the average success rate of 81% is remarkably high, given that the return from cooperation is rather low (compare Section 4.4). Croson and Marks (2000) define the step return in threshold public goods games by the aggregate group payoff from the public good divided by the total contribution threshold. In the present experiment, the theoretical step return is given by $\frac{150}{100} = 1.5$ in the symmetric cooperative SPNE. Due to overinvestment, the return to cooperation is even lower both in SHARE and LOTTERY. In groups with a successful group project, the average second stage payoff is 62 points in SHARE and 64 points in LOTTERY, meaning that the empirical step return is even below 1.25.[11] However, subjects cooperate strongly in SHARE. This is in line with Bayer (2016) who also observes strong cooperation if returns to cooperation are low. In contrast to Bayer (2016) who finds that investments are close to the equilibrium prediction, I find significant overinvestment both in SHARE and LOTTERY. Bayer (2016) attributes his findings to the allocation mechanism in the contest. The present experiment sheds doubt on this conclusion, and suggests that a possible explanation for the findings in Bayer (2016) may be that the prize increases in contributions.

4.7 CONCLUSION

In this paper, I study whether steep incentives are an impediment to cooperation. To this end, I set up an experimental game that allows for a controlled comparison of cooperation under the prospect of competing in a proportional-share or winner-take-all contest. By the experimental parameterization, I ensure that cooperation is a subgame-perfect Nash equilibrium even if players are risk-averse. I find remarkable differences between the two games. If players anticipate to compete in the proportional-share contest, cooperation is successful in 81% of all cases. If players anticipate to compete in the winner-take-all contest, the average success rate more than halves to only 38%. This result cannot be explained by differences in decision-making in the competition stage. I provide evidence that a significant fraction of subjects refrains from cooperation already in the first period and that matching patterns can explain the strong decay of cooperation in the winner-take-all treatment which is not observed in the proportional-

[11] Croson and Marks (2000) observe average success rates of only 63% for a step return of 3. However, the group size is five in their experiment.

share treatment. Earning less than the other player has a negative effect on the willingness to cooperate. However, only earning less than the initial endowment can explain the treatment effect.

The findings of this study show that indeed steep incentives may be an impediment to cooperation. Together with the experimental findings that subjects avoid to enter winner-take-all competition (Cason et al., 2010), that strategic alliances are less successful if expecting to compete against each other over an indivisible prize (Ke et al., 2013), and that winner-take-all competition leads to negative spillovers on cooperation (Buser and Dreber, 2015; Burks et al., 2009), the present study creates great concern that we do not underestimate the economic costs of the "winner-take-all society" (Frank and Cook, 1995) given the cooperation that may not take place. Steep incentives are ubiquitous in organizations, the job market, whole industries, and the educational system. In the majority of countries, we observe huge and growing disparities between the rich and the poor (Keeley, 2015), leading to differences in chances and economic outcomes. It is questionable whether living in a society where only a few get very much is favorable for social cohesion. The results of this study suggest that steep incentives may lead to an unwillingness to act prosocially in the first place, and that the experience of not being able to recoup investments into cooperation may erode prosocial behavior even further. I conclude that whenever the emergence of cooperation is desirable, we are well-advised to avoid strong disparities in outcomes.

4.A THEORETICAL APPENDIX

Proof of Proposition 1: Subgame perfect Nash equilibria (standard preferences)

Proof. Szidarovszky and Okuguchi (1997) prove that under the given assumptions, the stage equilibrium of the competition is unique. Suppose that $a_1^* + a_2^* = T$. For any $a_i < a_i^*$ player i would forgo additional payoff $B + \frac{1}{4}V$ and gain $a_i^* - a_i$. The maximum gain for the deviating player is given by $B + \frac{1}{4}V$ from contributing $a_i = 0$. This would make her (weakly) worse off than if she contributes a_i^*. For any $a_i > a_i^*$ player i would forgo payoff in magnitude of $a_i - a_i^*$ and would be strictly worse off. Suppose that $a_1^* + a_2^* = 0$. For any $a_i > a_i^*$ player i would forgo payoff in magnitude of a_i since her endowment does not suffice to make the group project successful since $A < T$. Hence, she would be strictly worse off from choosing $a_i > a_i^*$. $\square$

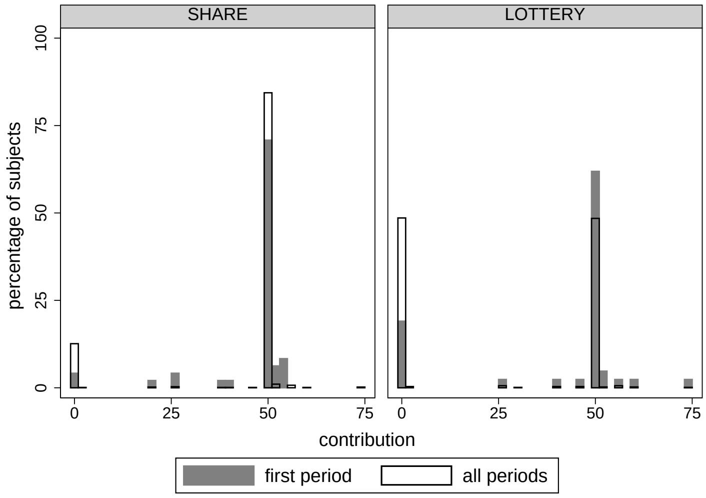


Figure 4.B.1: Distribution of contributions.

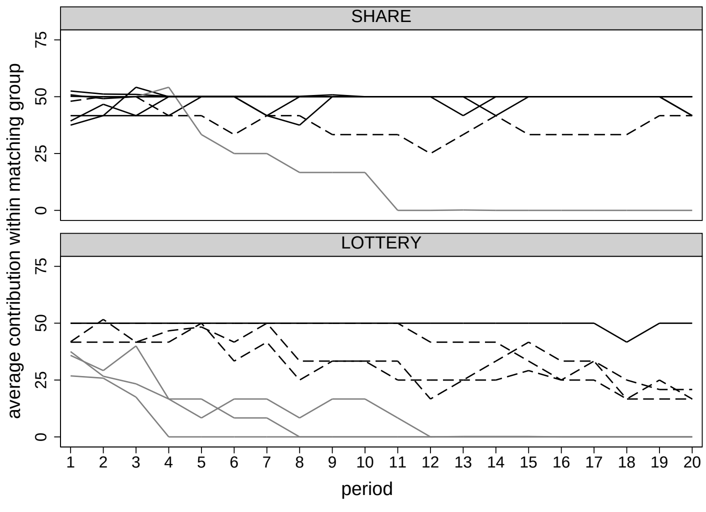


Figure 4.B.2: Average contribution by matching group.

4.C EXPERIMENTAL APPENDIX

4.C.1 TRANSLATED INSTRUCTIONS

In the following, sample instructions of treatment LOTTERY and SHARE_BASE are given. The instructions are translated from German.

Instructions of LOTTERY

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us. Please lift your hand, we will come to you and help you personally.

For showing up, you receive 4.00 Euro. You can earn additional money in this experiment depending on your own and the other participants' decisions. At the end of this experiment, you will receive your payment in cash.

Please do not talk to the other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and from all payments.

All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a participant who made a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

During the experiment your income is initially calculated in points. The total points your earned during the experiment will be paid out at the end of the experiment whereby

$$250 \text{ points} = 1 \text{ Euro}.$$

On the following pages we explain to you the exact course of the experiment.

Rounds and groups

- The experiment consists of **20 rounds** of which each round has the same structure.
- At the beginning of each round all participants are randomly assigned into **groups of two**. This assignment remains unchanged in the respective round. At the beginning of the next round all participants are randomly assigned into new groups of two.

Course of one round
Stage 1

- At the beginning of the first stage you and your group member each receive an **endowment A** of **75 points**.
- You and your group member decide simultaneously **how may points of your endowment A** you want to **contribute** to reach the second stage.
- Feasible contributions are integer values between 0 and 75 points. Points that are not contributed are directly added to your round income.
- Your group reaches the second stage of the round if you and your group member contribute **at least 100 points in total**. It holds that:

– If you and your group member contribute less than 100 points in total, the round finishes after the first stage. Contributed points are not paid back.

– If you and your group member contribute exactly or more than 100 points, your group reaches the second stage.

- Your **income of the first stage** of the round is given by (irrespective of whether your group has reached the second stage):

  Your income in the first stage = 75 - your contributed points

- At the end of the first stage, you and your group member are informed about the contributed points of both group members and the resulting incomes in the first stage.

Stage 2 (total group contribution of at least 100 points)

- At the beginning of the second stage you and your group member each receive an **endowment B** of **50 points**.
- In the second stage you and your group members can receive a **prize** of **100 points**.
- You and your group member simultaneously decide **how many points of your endowment B** to **invest** to increase the **probability** with which you receive the prize.
- Feasible investments are integer values between 0 and 50 points. Points that are not invested are added to your round income.
- The **probability** with which you receive the prize depends on how many points you invested and how many points your group member invested. The following rule applies:

$$\text{Probability with which you receive the prize} = \frac{\text{Points you invested}}{\text{Sum of points you and your group member invested}}$$

For you and your group member it holds that:

- The probability with which you receive the prize is equal to the number of points you invested divided by the sum of all points invested (by you and your group member).

- The probability with which you receive the prize is at least 0% and at most 100%.
- Each point you yourself invest increases the probability with which you receive the prize.
- Each point your group member invests reduces the probability with which you receive the prize.

It further holds that:

- If **no member of your group** invested points, no one receives the prize.
- If **only one member of your group** invested points, it receives the prize for certain.

- Your **income of the second stage** of the round is given by:

Your income in the second stage if you receive the prize =
50 - your invested points + 100
Your income in the second stage if you do not receive the prize =
50 - your invested points

- At the end of the second stage, you and your group member are informed about:
  - the points invested by you and your group member
  - the probabilities with which you and your group member receive the prize, respectively
  - which member of your group receives the prize
  - the resulting incomes of the second stage of both group members.

At the end of the round, you and your group member are informed about the round incomes (income of the first stage + income of the second stage if the second stage is reached) of both group members.

In the experiment, you receive the opportunity to use a calculator in the second stage. You can enter hypothetical values for the points invested by you and your group member. The calculator determines the hypothetical probability with which you receive the prize as well as the hypothetical probability with which your group member receives the prize. The calculator is available in the second stage of each round.

Income of the experiment

Your income of the experiment is the sum of the round incomes of all 20 rounds.

Instructions of SHARE_BASE

General information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us. Please lift your hand, we will come to you and help you personally.

For showing up, you receive 4.00 Euro. You can earn additional money in this experiment depending on your own and the other participants' decisions. At the end of this experiment, you will receive your payment in cash.

Please do not talk to the other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and from all payments.

All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a participant who made a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

During the experiment your income is initially calculated in points. The total points your earned during the experiment will be paid out at the end of the experiment whereby

$$250 \text{ points} = 1 \text{ Euro}.$$

You receive an initial endowment of 500 points. On the following pages we explain to you the exact course of the experiment.

Rounds and groups

- The experiment consists of **20 rounds** of which each round has the same structure.

- At the beginning of each round all participants are randomly assigned into **groups of two**. This assignment remains unchanged in the respective round. At the beginning of the next round all participants are randomly assigned into new groups of two.

Course of one round

- At the beginning of the round you and your group member each receive an **endowment** of **50 points**.
- You and your group members can receive a share of a **prize** of **100 points**.
- You and your group member simultaneously decide **how many points of your endowment** to **invest** to increase the **share** you receive of the prize.
- Feasible investments are integer values between 0 and 50 points. Points that are not invested are added to your round income.
- The **share** you receive of the prize depends on how many points you invested and how many points your group member invested. The following rule applies:

$$\text{Share of the prize you receive} = \frac{\text{Points you invested}}{\text{Sum of points you and your group member invested}}$$

<u>For you and your group member it holds that:</u>

- The share you receive of the prize is equal to the number of points you invested divided by the sum of all points invested (by you and your group member).
- The share you receive of the prize is at least 0 and at most 100 points.
- Each point you yourself invest increases the share you receive of the prize.
- Each point your group member invests reduces the share you receive of the prize.

It further holds that:

- If **no member of your group** invested points, no one receives the prize.
- If **only one member of your group** invested points, it receives the whole prize.

- Your **round income** is given by:

$$\text{Your round income} = \\ 50 \text{ - your invested points} + \text{your share of the prize} \times 100$$

- At the end of the round, you and your group member are informed about:
  - the points invested by you and your group member
  - the shares you and your group member receive of the prize, respectively
  - the resulting round incomes of both group members.

In the experiment, you receive the opportunity to use a calculator. You can enter hypothetical values for the points invested by you and your group member. The calculator determines the hypothetical share you receive of the prize as well as the hypothetical share your group member receives of the prize. The calculator is available in each round.

Income of the experiment

Your income of the experiment is the sum of the round incomes of all 20 rounds.

4.C.2 SCREENSHOTS

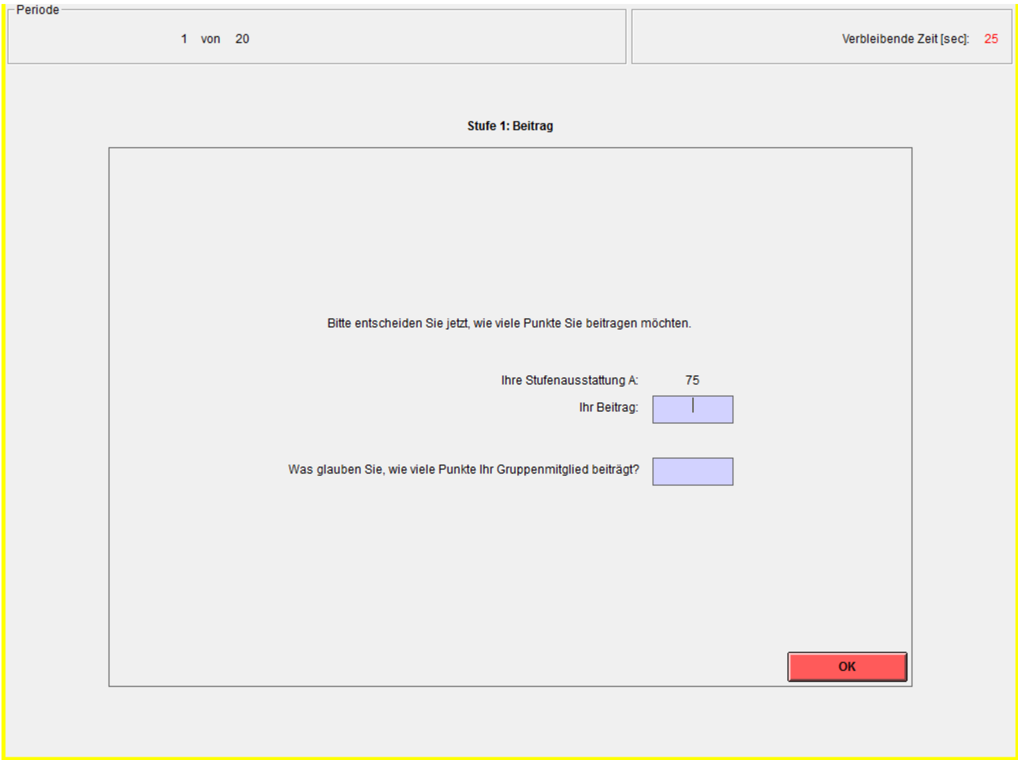


Figure 4.C.1: Screenshot cooperation stage.

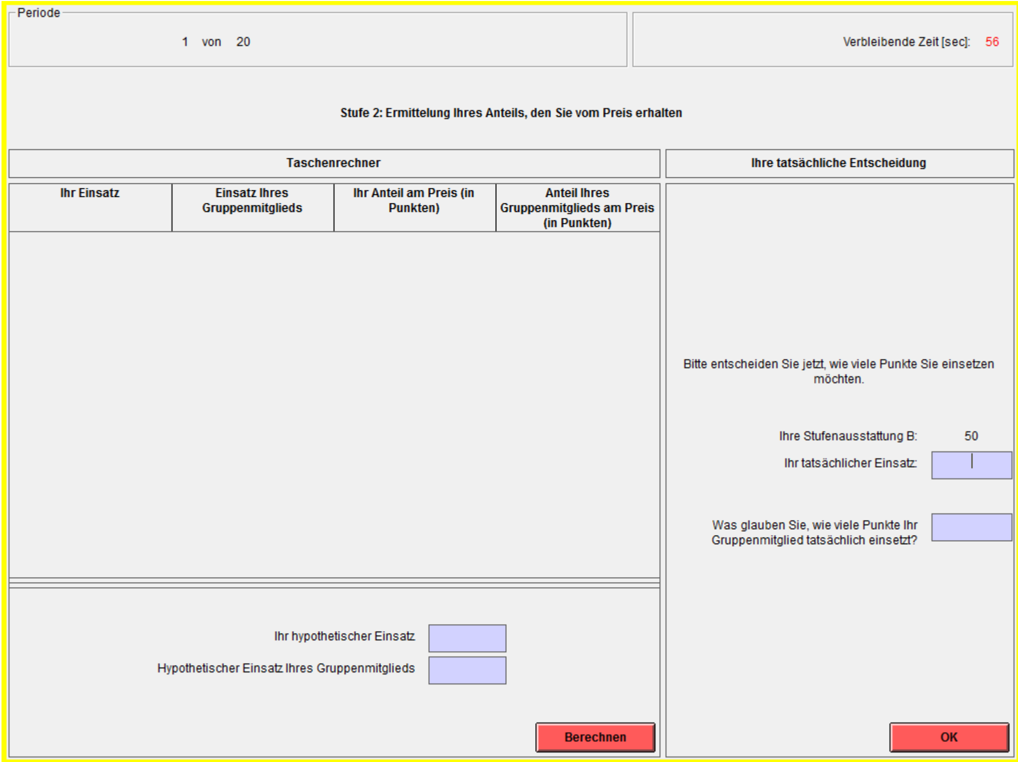


Figure 4.C.2: Screenshot competition stage.

CHAPTER 5

CROWDING-IN SUSTAINABLE CONSUMPTION UNDER VARYING OPPORTUNITY COSTS

Abstract

Consumption frequently imposes negative externalities on the environment. Feedback helps to induce resource conservation. However, little is known about how feedback affects behavior under varying individual opportunity costs (e.g., using public transport instead of driving by car may be less convenient on the countryside than in the city). In a laboratory experiment, we study real-resource consumption under varying opportunity costs. Subjects face a tedious real-effort task which can be simplified, but this leads to a real waste and a blank sheet of paper is shredded. Our results show that feedback provision affects decision-making differently depending on opportunity costs.

5.1 INTRODUCTION

Promoting sustainable consumption is challenging. Frequently, individual consumption is socially costly due to negative externalities on the environment. The problem is that these costs may seem negligible if the benefits from consumption are immediate and more perceptible. For example, the negative impact of increased CO2 emissions on the climate may not seem important the moment you decide to cover a distance by car and not by public transport. The negative impact of plastic waste on the environment may not seem important the moment you decide to buy a coffee to go. Sustainable consumption is the perfect example of a social dilemma: The individual has to trade off personal convenience with the objective of environmental conservation. A narrow view only captures current individual benefits and individual costs while a broader view accounts for the overall consequences of consumption, i.e., for negative externalities on the environment. Then, even if the individual has a preference to protect the environment, she may not live up to her ideals since her decision-making is biased towards the salient individual costs and benefits (Bordalo et al., 2012; Kahneman et al., 1982). This phenomenon is referred to as salience bias (Tiefenbeck et al., 2016). Hence, it is not the objective situation which determines behavior, but the subjective construal of the situation. To achieve behavioral change, mechanisms need to affect information processing (Crusius et al., 2012).

Recent field interventions aim to achieve behavioral change by different forms of feedback provision.[1] While these studies illustrate when and how feedback has an impact on resource consumption, only little is known about how feedback interacts with individual costs of behavioral change. In the field, it is difficult to control for these individual opportunity costs of resource conservation since typically, only baseline consumption and consumption after a treatment intervention are observable. It is clear, however, that attitudes towards resource conservation strongly vary with the necessity to consume. It may be rational for an individual to forgo comfort if there are convenient alternatives (e.g., not driving a car when living in a city with extensive public transport), but it may be too costly for her if convenient alternatives are not available (e.g., not driving a car when living on the countryside with sporadic public transport). Then, the costs of changing behavior may be too high to engage in resource conservation. As a consequence, we cannot derive final conclusions from the existing body of literature on how feedback provision affects decision-making under varying opportunity costs.

In this paper, we contribute to the existing literature by an examination of the impact of different feedback regimes on resource consumption in a laboratory environment where we can control and systematically vary the opportunity costs of resource conservation. We manipulate individual feedback on past resource consumption to influence resource conservation. We apply a novel experimental design to induce negative externalities in the

[1]See reviews by Abrahamse et al. (2005), Shippee (1980), Winett and Neale (1979).

laboratory. Subjects face a tedious real-effort task which they can simplify by using a shortcut. However, each time a shortcut is used, a blank sheet of paper is shredded. We thus create a situation in which the individual needs to forgo convenience (i.e., exert more effort) to save resources. We vary the salience of resource wasting across treatments. In the first condition, we provide feedback on accumulated resource wasting and constantly confront subjects with a live video stream of their total accumulated paper waste. In the second condition, the paper waste is hidden within the shredding bin, making it harder for the subject to grasp the total extent of her resource waste. Because, as discussed above, the reaction to feedback may vary with the necessity to consume, we vary the opportunity costs of resource conservation by manipulating the time available to solve the real-effort tasks. In one condition, the time limit is sufficient to solve all tasks without using shortcuts, i.e., shortcuts may be used only for convenience. In the other condition, the time limit is reduced severely making it impossible to solve all tasks without using shortcuts, i.e., shortcuts need to be used to secure the maximum payoff. This setup allows us to study the interplay of feedback and opportunity costs on real consumption decisions. Further, we set up two baseline treatments in which we only vary the time limit but where the use of shortcuts does not exhibit negative externalities.

Our results show that the effect of feedback indeed differs with the opportunity costs of resource conservation. If there is a negative externality, subjects use substantially fewer shortcuts in both time conditions. Subjects substitute shortcuts by increasing their own effort to avoid resource wasting. If necessary, they even give up part of their payoff to save resources. As long as the opportunity costs are low and resources are used for individual convenience only, feedback on accumulated resource wasting does not reduce the use of resources. However, if participants have to use the shortcut to avoid monetary losses, feedback on accumulated resource wasting decreases resource consumption.

Our study shows that feedback mechanisms need to focus on the interplay of information provision and economic incentives. Specifically, studies that intervene in individual resource consumption need to carefully take into account the opportunity costs associated with the intervention. In contrast to our laboratory setting in which opportunity costs cannot be externalized and have to be incurred by the subject (i.e., she has to "pay" for the conservation of resources through a lower monetary payoff), such an externalization may well be the case in the field. Opportunity costs and thus the efficacy of interventions are highly context-dependent and should thus be a cornerstone of any intervention design.

5.2 RELATED LITERATURE

Two fields of literature are especially relevant with regard to the present study. First, we review existing feedback interventions in the field and discuss the feedback mechanism proposed by this paper. Second, we review laboratory experiments which induce negative externalities and discuss our design choices. The question whether behavioral interventions can induce sustainable decision-making has a long tradition in economics and related fields (for a review, see Abrahamse et al., 2005; Shippee, 1980; Winett and Neale, 1979). In contrast to standard economic theory which assumes that decision-makers hold all the information necessary to make an optimal consumption choice, this literature assumes that the cost-benefit calculus is potentially biased due to a lack of information. Indeed, several studies provide evidence that consumption of resources like energy and water can be decreased by feedback interventions. In what follows, we focus on the behavioral dimension of feedback.

Early feedback interventions are based on so-called home reports. In these studies, feedback is provided on a regular basis, but focuses on past decision-making only. The results show that home energy reports can decrease household energy consumption by 2% (e.g., Allcott and Mullainathan, 2010; Costa and Kahn, 2013; Allcott and Rogers, 2014). Similar interventions for water consumption yield effects between 0% and 5% (Ferraro and Price, 2013; Mitchell and Chesnutt, 2013; Bernedo et al., 2014; Schultz et al., 2016; Brent et al. 2015). The rapid technological advancements in recent years allow for the provision of more timely feedback. In more recent feedback intervention studies, so-called smart meters are installed to provide timely feedback on aggregate resource consumption, e.g., via home displays or web applications. However, although participants can monitor their resource consumption on a more frequent basis, electricity consumption decreases by only 2% to 5% in these smart-metering studies (Degen et al. 2013, McKerracher and Torriti 2013, Buchanan et al. 2015). A recent study by Tiefenbeck et al. (2016) suggests that these rather moderate effects may be driven by the fact that feedback in all the former interventions only allows for the adaption of future behavior. In the authors' view, the segregation of feedback and consumption may make adaption prone to relapse. In their study, Tiefenbeck et al. (2016) provide real-time feedback on current consumption that allows for the immediate adaption of behavior. A smart meter is installed in participants' showers that measures water and energy consumption while subjects take a shower. The authors show that using this feedback mechanism decreases electricity and water consumption by 22%.

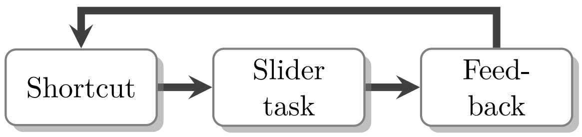


Figure 5.1: Timing in the experiment.

In this study, we vary feedback to determine its impact on decision-making under varying opportunity costs of resource consumption. In a laboratory experiment, subjects face a repeated individual decision task. In Figure 5.1, the sequence of one period is given. First, subjects determine the number of shortcuts they want to use. Then, they have to perform the slider task. Last, they receive feedback on resource wasting. Then, the sequence starts again. In one condition, feedback is provided only on the current period's waste of resources. Following the notion by Read et al. (1999) that people frequently fail to account for the overall consequences of their decision-making, we design a feedback mechanism that provides subjects with feedback on accumulated resource wasting. Thus, in the other condition, feedback is provided on the current plus all previous periods. As in Tiefenbeck et al. (2016), both feedback mechanisms of our experiment allow for the immediate adaption of behavior, as the decision to use shortcuts in the subsequent period immediately follows the feedback stage.

There is a growing literature which examines decision-making when it imposes negative externalities in laboratory or lab in the field settings. Falk and Szech (2013) experimentally study decision-making where the decision to accept an offer of 10 Euro leads to the death of a mouse. The authors' focus is on whether market interaction erodes moral behavior. The focus of the present study is different. We study decision-making over time and hypothesize that subjects' fail to account for the overall consequences of their decision-making. Further, since our focus is on negative externalities on the environment, the mouse-paradigm obviously is not suitable for the present study. Bartling et al. (2015) induce negative externalities in a laboratory experiment where the production of an "unfair product" exhibits zero costs, but exhibits a negative externality on a passive third party. In their experiment, the externality is implemented by a payoff reduction on behalf of a passive subject. Although this paradigm would allow us to study decision-making over time, for two reasons it is also not suitable for the present study. First, we aim to abstract from fairness concerns that may arise if negative externalities were imposed on a third party. Second, we focus on individual decision-making and thus abstract from peer effects. These would arise if a third party was involved. A third paradigm is introduced in Kirchler et al. (2016). Here, subjects can choose to take money, or donate a potentially larger sum to UNICEF for measles vaccines. In the experiment, taking money imposes a negative externality as the donation to UNICEF is decreased. Another related paradigm is used by Irlenbusch and Saxler (2015). In their study, a subject's decision to take money implements a negative externality as a default donation to a charity which sponsors warm meals for children is decreased. Alike in Bartling et al. (2015), the choice to generate a negative externality may also be guided by other-regarding concerns in Kirchler et al. (2016) and Irlenbusch and Saxler (2015). Hence, we opt for a different design. A fourth, more closely related paradigm is introduced by Löschel et al. (2017). In a framed field experiment, the authors elicit the demand for voluntary climate change mitigation and provide participants with the opportunity to buy European Union Allowances which are withdrawn from the European Emissions Trading Scheme. In the present study, we could induce a negative externality by reducing a given amount of permits. However, in this situation subjects would only influence how much other individuals can emit. We opt for a more direct link between decision-making and resource wasting. In another related laboratory experiment by Bühren and Daskalakis (2015), subjects are asked to imagine putting effort into energy saving activities. Like in our experiment, two treatments induce an actual waste of resources. Terrace heaters are installed outside the laboratory. The more slider tasks subjects solve, the earlier the heater is switched off.

The study of negative externalities in laboratory environments has the advantage that it allows to systematically vary parameters that are exogenous and sometimes unobservable in the field. Laboratory experiments can thus provide complementary evidence informing the design and conduct of field interventions. However, it almost always creates an artificial link between the own decision and the imposed consequences on third parties. To control for potentially arising experimenter demand effects, it is necessary to hold potential confounds constant across treatments. To control for potentially arising experimenter demand effects, we do not install a default waste of resources, but let subjects actively engage in resource consumption. Within this setting, we examine how feedback affects behavior. The overall consequences of decision-making are the same in the resulting treatment comparisons, and this is known to subjects. By these design features, we aim to minimize potential confounds.

5.3 THE EXPERIMENT

The experiment consists of an individual decision task that is repeated for 50 periods over six treatments in a between-subjects design. Throughout the experiment, we abstract from peer effects, i.e., subjects do not interact with each other and no subject is informed about the other subjects' decisions or payoffs.

5.3.1 COURSE OF ONE PERIOD

In each period, subjects perform a real-effort task that consists of ten randomly positioned sliders which have to be positioned correctly to generate income (Gill and Prowse, 2012). Sliders can be moved to any integer between 0 and 100 inclusive. The slider has to be correctly positioned at position 50.[2] Each correctly positioned slider increases the subject's period income by €0.04. The task can be simplified by the use of shortcuts. The subject decides at the beginning of each period how many shortcuts she wants to use, i.e., how many sliders she wants to simplify. Every shortcut simplifies one slider. Whenever a subject uses a shortcut on a slider, the number of possible positions is reduced to three (0, 50, and 100). This makes it trivial to complete the task, i.e., to correctly position the slider at position 50. Hence, the use of shortcuts decreases individual effort costs. Each of the 50 identical periods has the following three stages:

1) *Shortcuts*. Subjects decide how many shortcuts to use to simplify sliders in the subsequent stage.

2) *Slider task*. Subjects have to correctly position 10 sliders and earn money for every correctly positioned slider.

3) *Feedback*. Subjects are provided with individual feedback on the negative externality imposed by their individual use of shortcuts.

The use of shortcuts imposes a negative externality. Whenever the subject uses a shortcut to simplify a slider, a blank (i.e., unused) sheet of paper is destroyed in a shredder. This is explicitly stated in the instructions. At the beginning of each period, subjects are asked how many sliders they want to simplify in the respective period (i.e., they have to enter an integer value x between 0 and 10). According to their choice of shortcuts, the

[2] The experiment is programmed such that the slider is never positioned at position 50 initially.

first x sliders on the subject's screen are simplified.[3] After the slider-task stage, the shredding of the paper takes place in a separate room. Subjects observe the shredding process via a live video stream. Each participant has a separate shredder and an individual webcam to avoid peer effects. The machinery noise is inaudible in the participants' room. At the beginning of the experiment, subjects verify that the stream is live by writing a symbol and their cabin number on a Post-It note. This personalized Post-It note is then attached to their individual shredder.

5.3.2 TREATMENTS

To address our research question, we set up four treatments in a 2×2-design. Additionally, we run two baseline treatments. Table 5.1 shows all six treatments. The experimental treatments vary the opportunity costs of resource conservation by different time limits to solve a given set of ten slider tasks (40 seconds vs. 75 seconds). Further, treatments vary the feedback provided to subjects about their current and accumulated cause of negative externalities (closed vs. open shredder bin).

Table 5.1: Treatments.

		Feedback		Baseline
		closed bin	open bin	
Time limit	75 seconds	CLOSED75	OPEN75	NOEXT75
	40 seconds	CLOSED40	OPEN40	NOEXT40

Time Limit. The first treatment variable is the time limit of the real-effort task to induce different opportunity costs of resource conservation. In line with Doerrenberg et al. (2015), we estimate that subjects should be able to solve all ten sliders within 75 seconds. Thus, there is no monetary incentive to use shortcuts and the opportunity cost of resource conservation is low. If, however, the time limit is set to 40 seconds, the opportunity cost

[3]Screenshots and translated instructions are provided in the Appendix.

of resource conservation is much higher, and there is a strong incentive to use shortcuts.

Feedback. The second treatment variable is feedback on resource consumption. Prior to the experiment, we cut open one side of the shredder bin. To provide accumulated feedback on resource wasting, the open side of the shredder bin is oriented towards the camera, meaning that the shredded paper is visible to subjects as it falls out the shredding bin.[4] Hence, in the OPEN setup, subjects can observe the total accumulated consequences of their current and past choices to use shortcuts. In the CLOSED setup, we provide feedback on current resource wasting only. Here, the closed side of the shredder bin is oriented towards the camera, i.e., only the process of shredding can be observed but the total paper waste stays hidden within the shredder bin. In other words, subjects can only observe the consequences of their current choice to use shortcuts.

Baseline. In addition to the four main treatments, we run two additional baseline treatments without negative externalities. That is, we run the same real-effort task over 50 periods and vary the time limit (40 seconds vs. 75 seconds) but the use of shortcuts does not lead to the shredding of paper. The two baseline treatments allow us to study decision-making in a situation in which the negative externality does not exist. We refer to these baseline treatments as NOEXT75 and NOEXT40.

Crossing. We cross the two main treatment variables in a fully factorial 2×2 design. The two baseline treatments only vary the time limit but do not impose a negative externality.

5.3.3 HYPOTHESES

This section presents a simple decision problem to formally guide our argumentation. The two variables that determine the economic environment are the time limit $\tau \in \mathbb{R}$ that specifies how long a period lasts and the feedback parameter $\phi \in \mathbb{R}$ that specifies how much information the decision-maker has about accumulated resource wasting. That is, ϕ_t is the information

[4]Pictures are provided in the Appendix.

provided on the number of shortcuts used in the past periods up to current period t. In each period t, the decision-maker chooses her real effort e_t and the number of shortcuts s_t. The per-period utility function reads as:

$$U_t(e_t, s_t; \tau, \phi) = u(e_t) - c(e_t, s_t) - d(s_t; \tau, \phi).$$

u is the direct utility from money and is increasing and concave in the provided effort e_t. We model two cost functions to account for the cost from real effort (c) and the cost from perceived environmental damage (d). We assume that the physical real-effort costs $c(e_t, s_t)$ do not directly depend on the time limit τ and the feedback ϕ, i.e., solving a slider is equally difficult under varying time limits, and also under varying feedback. However, τ and ϕ influence the real-effort costs indirectly since both variables have an impact on the perceived environmental costs $d(s_t; \tau, \phi)$, and thus affect an individual's decision how many shortcuts to use. The real-effort cost function c is increasing and convex in effort, and decreasing and convex in the number of shortcuts. This means that shortcuts lighten the burden from real effort. The perceived environmental cost function d is increasing and convex in the number of used shortcuts (and thus shredded sheets of paper).

In the experiment, under feedback condition OPEN, the decision-maker observes the total amount of shredded paper up until (and including) the current period t. Hence, she has the information set

$$\Phi_t^{OPEN} = \{s_1, s_2, ..., s_{t-1}, s_t\}.$$

This information set is contained in the feedback $\phi^{OPEN} = \sum_{j=1}^{t} s_j$. On the contrary, under feedback condition CLOSED, the decision-maker does only observe the amount of shredded paper in the current period t and has information set

$$\Phi_t^{CLOSED} = \{s_t\}.$$

This information set is contained in the feedback $\phi^{CLOSED} = s_t$. Thus, $\Phi_t^{CLOSED} \in \Phi_t^{OPEN}$ and therefore $\phi^{CLOSED} < \phi^{OPEN}$, i.e., the decision-maker considers less information under CLOSED than under OPEN feedback. We derive our predictions for the experiment by totally differentiating the first-order conditions of the per-period utility function.[5] With regard to the provided feedback, we expect that the number of used shortcuts decreases if more feedback is provided, i.e., $\frac{ds}{d\phi} < 0$.

Hypothesis 1. *The number of shortcuts decreases in provided feedback.*

With regard to the available time, we expect that the number of used shortcuts decreases if the time limit is higher, i.e., $\frac{ds}{d\tau} < 0$.

Hypothesis 2. *The number of shortcuts decreases if more time is available.*

With regard to the interaction between the feedback and the time limit, we expect that the effect of feedback on the use of shortcuts is stronger if less time is available, i.e., $\frac{ds}{d\tau d\phi} < 0$.

Hypothesis 3. *The number of shortcuts decreases stronger in the provided feedback if less time is available.*

5.3.4 PROCEDURE

The experiment was conducted in the Cologne Laboratory for Economic Research (CLER) at the University of Cologne, Germany. The recruitment was organized via ORSEE (Greiner, 2015). A total of 175 participants participated in the experiment with 55% female and 45% male. A total of 45% of our sample were students from economics, social sciences, and business administration. Neither the content of the experiment nor expected payoffs were stated in the invitation email. In compliance with the rules of the CLER, participants received a show-up fee of €4.00.

The experiment took place in two separate rooms. This enabled us to install the shredders in the second room and to set up a constant real-time video stream via webcam to the other room. We intentionally avoided to install the shredders in the same room where the participants were seated

[5] See Appendix for the derivation of the hypotheses.

to abstract from peer effects and to ensure the safety of participants. The machinery noise was inaudible in the participants' room. The experiment was programmed utilizing the software package zTree (Fischbacher, 2007).

We conducted nine sessions of the four main treatments with four participants each to ensure the correct implementation of the experiment. The control treatments (NOEXT40 and NOEXT75) were conducted in one session each. Here, no shredders were needed. This enabled us to run sessions with a higher number of participants. Two research assistants operated the shredders in the second room. The instructions clearly stated that participants were not recorded via webcam, and webcams on the participants' desktops were covered with tape.

All instructions were provided on screen.[6] The picture of the shredder was streamed on the right half of participants' desktops from the beginning of the session. To ensure credibility of the shredding operation, we let participants mark a Post-It note with their cubicle number and an individual drawing at the beginning of the experiment.[7] The Post-It notes were collected and put on the shredder assigned to the participant before the main part of the experiment began. To ensure that the slider task could be performed without distractions, we asked participants to turn off their mobile phones and other electronic devices, and to put them in an envelope that would stay within the participant's cubicle throughout the entire experiment.

To make participants familiar with the slider task, two incentivized practice periods were run prior to the main 50 periods of the experiment. In the first practice period, participants had to position ten non-simplified sliders, and in the second period, they had to do the same for ten simplified sliders. In both practice periods, each solved slider generated a payoff of €0.04, i.e., incentives were the same in the practice and main periods. Only after the two practice periods, the on-screen instructions made participants familiar with the use of shortcuts and the negative externality imposed.

[6]Translated instructions are provided in the Appendix.
[7]Pictures of the experimental setup are provided in the Appendix.

After the 50 main periods, participants were asked whether they wanted to learn the total number of blank sheets of paper that were shredded due to their choices in the experiment. After that, participants were asked to guess the number of paper sheets that were shredded due to their use of shortcuts. On the next screen, participants were informed about their total number of correctly positioned sliders and their individual payoff that was the sum of period incomes from all 50 periods and the two practice periods of the experiment. Depending on their revelation choice, information about the total number of blank sheets of paper was also provided. After this summary, participants were given the opportunity to donate to the World Wide Fund for Nature (WWF) for the conservation of the Amazonian forest. The donation decision was not announced in any previous period or the instructions. In total, €154.87 were donated to the WWF. Participants were asked to fill out a short questionnaire asking for demographics and participants' perceptions and considerations during the experiment. Overall, 28,805 blank sheets of paper were shredded.

On average, participants earned €24.20 in NOEXT75 (min: €22.00; max: €24.80), €23.80 in CLOSED75 (min: €19.00; max: €24.80), €23.30 in OPEN75 (min: €19.00; max: €24.80), €23.20 in NOEXT40 (min: €14.00; max: €24.70), €19.20 in CLOSED40 (min: €9.00; max: €24.60), and €18.60 in OPEN40 (min: €4.00; max: €24.60). We collected between 35 and 36 independent observations for each of the four main treatments, and 16 independent observations for each of the two control treatments. By focusing on individual decisions and not providing feedback about other participants' decisions, we ensured statistical independence across observations on the participant level. If not stated otherwise, statistical comparisons between treatments are based on two-sided Mann-Whitney U-tests (MWU), and comparisons within treatments are based on two-sided Wilcoxon signed-rank tests (WSR).

5.4 RESULTS

We present our experimental results in three steps. In part one, we analyze how the different levels of information on resource wasting affect the consumption of resources (i.e., the number of used shortcuts) and individual effort (i.e., the number of solved sliders) when opportunity costs are low (i.e., the time limit is 75 seconds). In part two, we investigate trade-offs between individual benefits and resource conservation when opportunity costs are high (i.e., the time limit is 40 seconds). In part three, we analyze how our feedback mechanism on accumulated resource wasting influences decision-making. Table 5.1 provides the summary statistics of the main variables across treatments.

Table 5.1: Aggregated outcomes.

	NOEXT75	CLOSED75	OPEN75	NOEXT40	CLOSED40	OPEN40
Solved sliders	499.88	488.53	483.50	494.06	406.83	368.91
	(0.09)	(2.71)	(3.56)	(2.29)	(14.92)	(17.96)
% of NOEXT		97.73%	96.72%		82.34%	74.67%
Used shortcuts	243.31	86.89	91.50	357.94	211.14	147.49
	(50.99)	(20.12)	(26.48)	(29.78)	(29.72)	(26.91)
% of NOEXT		35.71%	37.61%		58.99%	41.20%
Payoff (in €)	20.00	19.54	19.34	19.76	16.27	14.76
	(0.00)	(0.11)	(0.14)	(0.09)	(0.60)	(0.72)
Independent obs.	16	36	36	16	36	35

Notes: The table reports averages and standard errors (in parentheses) based on independent observations per treatment. Percentages of NOEXT are calculated by using the average of individual totals relative to the average total in NOEXT in the respective time condition (note that relative payoffs equal relative solved sliders).

5.4.1 THE EFFECT OF THE NEGATIVE EXTERNALITY

We begin our analysis by examining the baseline treatment NOEXT75 where the use of shortcuts does not impose any externalities, and the time limit is 75 seconds. Under the assumption that solving the sliders exhibits effort costs, standard theory would predict that participants use any shortcut available to minimize these effort costs and to be able to solve all 500 sliders. Indeed, on average subjects solve almost all sliders in NOEXT75 (499.88, compare Table 5.1). The shortcut is used for 243.31 of 500 sliders on average, i.e., even if there is no negative externality subjects substitute almost 50% of shortcuts by own effort.

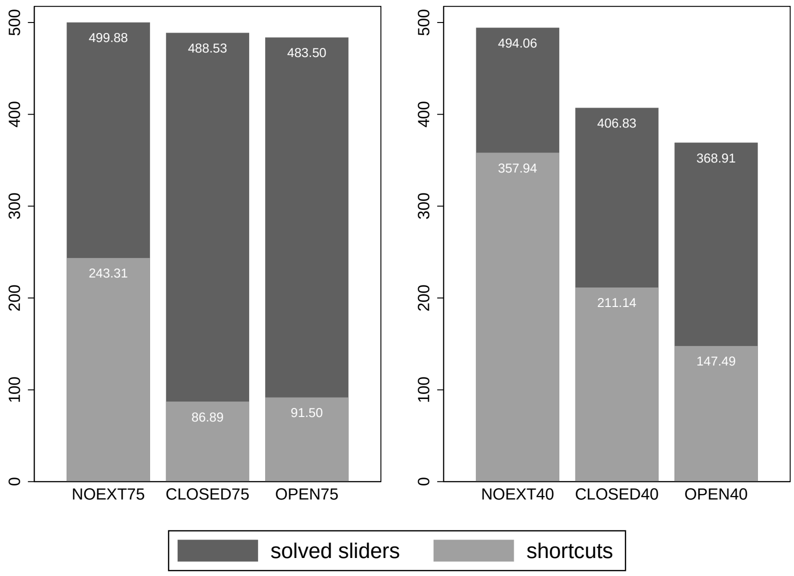


Figure 5.2: Used shortcuts and solved sliders.

As shown in Figure 5.2, subjects substitute shortcuts by own effort even more when the shortcut imposes a negative externality.[8] In CLOSED75, the average number of solved sliders (488.53) is still close to 500 sliders. However, the average number of used shortcuts drops by 64% to 86.89. Although both solved sliders and used shortcuts are significantly lower in CLOSED75 than in NOEXT75 (two-sided MWU-test: solved sliders: p=0.0000; used shortcuts: p=0.0055), there is a remarkable difference which becomes clear when looking at the relative changes. In CLOSED75, subjects on average solve 98% of the sliders solved in NOEXT75. At the same time, they use only 36% of the shortcuts. The reduction of used shortcuts is significantly higher than the reduction of solved sliders (two-sided WSR-test:

[8]A figure of the use of shortcuts and solved sliders by period is provided in the Appendix.

p=0.0000), meaning that subjects substitute considerable amounts of resource consumption by own effort when there is a negative externality.[9] We observe similar patterns when subjects are provided with feedback on accumulated resource wasting in OPEN75. On average, subjects solve 97% of the sliders that are solved in NOEXT75, but use only 38% of the shortcuts. The difference between both relative measures is again significant (two-sided WSR-test: p=0.0008). Thus, both in CLOSED75 and OPEN75, the negative externality leads subjects to substitute resource consumption by own effort.

The above analysis reveals that subjects react similarly to the imposition of a negative externality in CLOSED75 and OPEN75, i.e., irrespective of the feedback on accumulated resource wasting. The comparison of CLOSED75 and OPEN75 reveals that confronting participants with the overall consequences of their choices does not affect their decisions to use the shortcuts. Neither the average number of solved sliders (two-sided MWU-test: p=0.3125) nor the number of used shortcuts (p=0.8593) is statistically different between CLOSED75 and OPEN75. This means that if subjects mainly consume for convenience (because there is no time pressure), feedback on accumulated resource wasting does not affect decision-making. This finding does not support Hypothesis 1.

Result 1. *When opportunity costs of resource conservation are low, feedback on accumulated resource wasting does not have a statistically significant effect on resource consumption.*

One possible explanation for this result could be that resources are mainly consumed for convenience in this setting. The time limit of 75 seconds is sufficient to solve all sliders without using shortcuts. In fact, 31% of

[9]To test this difference in differences, we first calculate the difference in solved sliders by deducting each subject's individual number of solved sliders in CLOSED75 from the average number of solved sliders in NOEXT75. Then, we calculate the difference in used shortcuts by deducting each subject's individual number of used shortcuts from the average number of used shortcuts in NOEXT75. The resulting differences are then compared using a Wilcoxon signed-rank test.

the subjects in CLOSED75 and OPEN75 do not use a single shortcut and still manage to solve a total of 485 sliders on average (in comparison, only 13% of the subjects in NOEXT75 do not use any shortcut and solve 500 sliders on average). It seems that subjects already only use as many shortcuts as necessary to solve all sliders when their use of shortcuts imposes a negative externality in CLOSED75. The trade-off between individual monetary benefits and resource conservation is rather weak under the time limit of 75 seconds, meaning that there may be no room for further improvements. In the following section, we analyze whether this result changes under the tighter time limit of 40 seconds.

5.4.2 OPPORTUNITY COSTS AND RESOURCE CONSERVATION

First, we check the effectiveness of the time manipulation by comparing the average number of solved sliders in the practice period where no shortcuts are available. While on average subjects solve 7.65 out of 10 sliders within 75 seconds (NOEXT75, CLOSED75 and OPEN75), they only solve 4.01 out of 10 sliders within 40 seconds (NOEXT40, CLOSED40 and OPEN40). Hence, by our time manipulation, we reduced subjects' capability to solve the 10 sliders by mere effort provision by roughly one half, thereby increasing the opportunity costs of resource conservation. In the following, we investigate the induced trade-off between individual monetary benefits and resource conservation.

As expected, we see from Figure 5.2 that when there is no negative externality, a reduction of time from 75 to 40 seconds leads to a higher average number of used shortcuts of 357.94 in NOEXT40 as compared to 243.31 in NOEXT75 ($p=0.0882$). The reduced time limit has a similar effect when the use of shortcuts imposes a negative externality. Here, the average number of used shortcuts is 211.14 in CLOSED40 as compared to 86.89 in CLOSED75 ($p=0.0024$). This result is in line with Hypothesis 2.

Result 2. *A stricter time limit leads subjects to use significantly more shortcuts.*

Turning to the impact of feedback on resource consumption in the high opportunity costs treatments, we find the following: In comparison to NOEXT40, the imposition of a negative externality already leads to significantly less resource consumption in CLOSED40 (p=0.0087). Furthermore, the provision of feedback on accumulated waste leads to an even lower level of used shortcuts in OPEN40 as compared to CLOSED40 (p=0.0708). In relative terms, subjects consume 41% less in CLOSED40 than in NOEXT40. Feedback on accumulated consequences leads subjects to consume 59% less in OPEN40 than in NOEXT40. Thus, feedback on accumulated resource wasting indeed affects decision-making differently under varying opportunity costs. The result is in line with Hypothesis 3.

Result 3. *When opportunity costs of resource conservation are high, feedback on accumulated resource wasting significantly decreases the use of shortcuts.*

Under high opportunity costs, we observe that the average number of solved sliders is only 368.91 in OPEN40 as compared to 406.83 in CLOSED40 and to 494.06 in NOEXT40. In consequence, on average subjects earn €5.00 less in OPEN40 than in NOEXT40, which equals more than 20% of the potential earnings in this experiment. Indeed, payoffs are significantly lower both in OPEN40 and CLOSED40 than in NOEXT40 (p=0.0000 in both comparisons), but also between these two treatments (p=0.0794). In comparison, no such difference is observed between CLOSED75 and OPEN75 (p=0.3125). This means that when opportunity costs of resource conservation are high, subjects substitute shortcuts by own effort and even by own payoff.

5.4.3 GENERAL AVOIDANCE OR EXPERIENCE

Recall from Section 5.3 that feedback on accumulated resource consumption should have an effect on decision-making only after the subject actually used shortcuts in any of the previous periods. However, it could also be that the picture of the open shredder bin on subjects' desktops results in a general avoidance to use shortcuts to avoid seeing the paper waste. In this section, we provide evidence that the former is the case. In Figure 5.3, the average number of shortcuts is compared between period t in which the subject uses the shortcut for the first time and the subsequent period $t+1$.

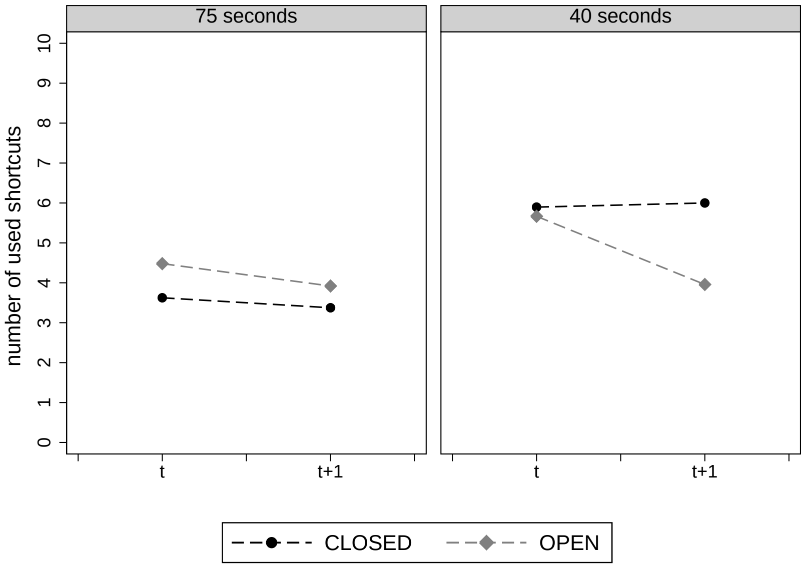


Figure 5.3: Number of shortcuts — first vs. second use.

There is no difference in the average number of used shortcuts in the first period in which subjects actually use a shortcut (two-sided MWU-test: CLOSED75 vs. OPEN75: p=0.4534, CLOSED40 vs. OPEN40: p=0.7307).[10] Further, there is no significant difference in the number of subjects who never use a shortcut between CLOSED75 and OPEN75 (two-sided Fisher's exact test: p=1.000) as well as between CLOSED40 and OPEN40 (p=0.415). These findings suggest that the observed treatment effect cannot be ascribed to a general avoidance of subjects to use the short-

[10] In this and the following comparisons, we exclude subjects who never use a shortcut, and also two subjects who first use the shortcut in the final period. The resulting number of independent observations is 24 subjects in CLOSED75, 25 subjects in OPEN75, 29 subjects in CLOSED40, and 24 subjects in OPEN40.

cut when the shredder bin is open. However, when we focus our analysis on subjects' decision-making after the first use of a shortcut, we find differences between the two feedback mechanisms in the high opportunity cost treatments. Recall that the average number of used shortcuts is not different between CLOSED75 and OPEN75 as well as between CLOSED40 and OPEN40 in period t. In period $t+1$, there is also no significant difference between CLOSED75 and OPEN75 (p=0.4439). However, the average number of used shortcuts is significantly lower in period $t+1$ of OPEN40 than in period $t+1$ of CLOSED40 (p=0.0373). That is, being confronted with the shredding process for the first time in OPEN40 has a significant impact on decision-making.

Table 5.2: Number of used shortcuts.

	(1)	(2)
Period	-0.0161***	-0.0164***
	(0.00)	(0.00)
OPEN	0.0889	-0.2398
(0=no, 1=yes)	(1.13)	(1.07)
Time limit	4.5892***	3.0317***
(0=75sec, 1=40sec)	(1.20)	(1.03)
OPEN × Time limit	-2.5898*	0.2345
(0=75sec, 1=40sec)	(1.53)	(1.51)
Adaption after 1st use		-0.6860***
		(0.21)
constant	-1.8963**	2.2444***
	(0.91)	(0.76)
number of observations	7,150	5,100
number of subjects	143	102
Wald Chi-squared	96.05***	103.46***

Notes: Random-effects panel tobit regression, robust standard errors in parentheses. *: p<0.1, **: p<0.05, ***: p<0.01. Dependent variable: Number of used shortcuts in a given period, left-censored to 0.

To further examine whether the open shredder bin has an impact on decision-making that goes beyond the initial effect after observing the shredding process in OPEN, we run a panel tobit regression on all independent observations of the main treatments in our sample. The results are presented in Table 5.2. We see that there is a significant overall decrease of used shortcuts over time. In line with Hypothesis 2, time pressure leads to a significantly higher level of used shortcuts. The interaction between OPEN and time pressure is only significant in model (1), but the effect vanishes once we control for adaption after the first use of shortcuts in model (2). Hence, we conclude that the initial effect of observing the wasted resources drives our treatment effect of feedback on accumulated resource wasting under high opportunity costs.

5.5 CONCLUSION

In this paper, we experimentally study the impact of different feedback regimes on resource consumption in a laboratory environment in which we systematically vary the opportunity costs of resource conservation. We manipulate individual feedback on accumulated resource wasting to influence individual resource conservation.

We find that the effect of feedback differs with varying opportunity costs of resource conservation. When the use of shortcuts imposes a negative externality, substantially fewer shortcuts are used in all treatments. Subjects substitute resource consumption by own effort and – in the high opportunity cost treatments – even by own payoff. Feedback on accumulated resource consumption does not have an additional effect in the treatments with low opportunity costs. However, in the treatments with high opportunity costs, it reduces resource consumption even further. This effect can be attributed to a shift in consumption that results from the initial effect of observing the waste resulting from shortcut use.

Our study shows that feedback mechanisms need to focus on the interplay of information provision and economic incentives. Specifically, studies that intervene in individual resource consumption need to carefully take into account the opportunity costs associated with a behavioral change. In contrast to our laboratory setting in which opportunity costs cannot be externalized and have to be incurred by the subject (i.e., she has to “pay” for the conservation of resources by a lower monetary payoff), such an externalization may well be the case in the field. Opportunity costs and thus the efficacy of interventions are highly context-dependent and should thus be a cornerstone of any intervention design.

5.A THEORETICAL APPENDIX

The decision problem is given by:

$$\max\{e_t, s_t\} U_t(e_t, s_t; \tau, \phi) = u(e_t) - c(e_t, s_t) - d(s_t; \tau, \phi).$$

We assume that the physical real-effort costs $c(e_t, s_t)$ are independent of the time limit τ and the feedback ϕ, i.e., solving a slider is equally difficult under a high and a low time limit, and also under varying feedback. However, τ and ϕ influence the real-effort costs indirectly since both variables influence the perceived environmental costs $d(s_t; \tau, \phi)$, and thus affect an individual's decision how many shortcuts to use.
The first-order conditions are given by:

$$F(e, s; \tau, \phi) = u_e - c_e = 0$$

and

$$G(e, s; \tau, \phi) = -c_s - d_s = 0,$$

where subscripts denote the partial derivative with respect to that variable. We assume that all functions are continuously differentiable. Further, we assume that utility from money is increasing and concave in effort ($u_e > 0$, $u_{ee} < 0$), that effort costs increase and are convex in effort ($c_e > 0$, $c_{ee} > 0$), and that effort costs are decreasing and convex in used shortcuts ($c_s < 0$, $c_{ss} > 0$). Further, we assume that perceived environmental costs are increasing and convex in provided feedback ($d_s > 0$, $d_{ss} > 0$). Further, the marginal perceived environmental costs increase in provided feedback ($d_{s\phi} > 0$) and in the available time ($d_{s\tau} > 0$). Further, we assume that $c_{es} < 0$, $c_{se} < 0$. Thus, we have the following partial derivatives of the first-order conditions:

$$F_e = u_{ee} - c_{ee} < 0$$
$$F_s = -c_{es} > 0$$
$$F_\phi = 0$$
$$F_\tau = 0$$
$$G_e = -c_{se} > 0$$
$$G_s = -c_{ss} - d_{ss} < 0$$
$$G_\phi = -d_{s\phi} < 0$$
$$G_\tau = -d_{s\tau} < 0.$$

Totally differentiating the first-order conditions yields:

$$F_e de + F_s ds + F_\tau d\tau + F_\phi d\phi = 0$$
$$G_e de + G_s ds + G_\tau d\tau + G_\phi d\phi = 0.$$

Effect of Varying the Time Limit: We divide both total derivatives by $d\tau$ and set $\frac{d\phi}{d\tau} = 0$, because our treatment variables are orthogonal, i.e., the time limit does not influence the provided feedback. Rearranging yields

$$F_e \frac{de}{d\tau} + F_s \frac{ds}{d\tau} = -F_\tau$$
$$G_e \frac{de}{d\tau} + G_s \frac{ds}{d\tau} = -G_\tau.$$

By Cramer's rule, we get:

$$\frac{ds}{d\tau} = \frac{\mathbf{det}\begin{pmatrix} F_e & -F_\tau \\ G_e & -G_\tau \end{pmatrix}}{\mathbf{det}\begin{pmatrix} F_e & F_s \\ G_e & G_s \end{pmatrix}}.$$

We have:

$$\mathbf{det}\begin{pmatrix} F_e & -F_\tau \\ G_e & -G_\tau \end{pmatrix} = F_\tau G_e - F_e G_\tau$$

$$\begin{aligned} &= -F_e G_\tau \\ &= -(u_{ee} - c_{ee})(-d_{s\tau}) \\ &< 0 \end{aligned}$$

and

$$\mathbf{det}\begin{pmatrix} F_e & F_s \\ G_e & G_s \end{pmatrix} = F_e G_s - F_s G_e$$

$$= (u_{ee} - c_{ee})(-c_{ss} - d_{ss}) - (-c_{es})(-c_{se}).$$

For a clear prediction, we need the following assumption with regard to the concavity of the utility function $u(e)$:

$$u_{ee} < c_{ee} - \frac{(-c_{es})^2}{(c_{ss} + d_{ss})}. \tag{5.A.1}$$

Then, we have:

$$\frac{ds}{d\tau} < 0,$$

i.e., the more time is available, the fewer shortcuts the decision-maker uses.

Effect of Varying the Feedback: We divide both total derivatives by $d\phi$ and set $\frac{d\tau}{d\phi} = 0$, because our treatment variables are orthogonal, i.e., the provided feedback does not influence the time limit. Rearranging yields

$$F_e \frac{de}{d\phi} + F_s \frac{ds}{d\phi} = -F_\phi$$
$$G_e \frac{de}{d\phi} + G_s \frac{ds}{d\phi} = -G_\phi.$$

By Cramer's rule, we get:

$$\frac{ds}{d\phi} = \frac{\mathbf{det}\begin{pmatrix} F_e & -F_\phi \\ G_e & -G_\phi \end{pmatrix}}{\mathbf{det}\begin{pmatrix} F_e & F_s \\ G_e & G_s \end{pmatrix}}.$$

We have:

$$\begin{aligned} \mathbf{det}\begin{pmatrix} F_e & -F_\phi \\ G_e & -G_\phi \end{pmatrix} &= F_\phi G_e - F_e G_\phi \\ &= -F_e G_\phi \\ &= -(u_{ee} - c_{ee})(-d_{s\phi}) \\ &< 0. \end{aligned}$$

Thus, given our assumption in Equation (5.A.1), we get:

$$\frac{ds}{d\phi} < 0,$$

i.e., the more feedback is provided, the fewer shortcuts the decision-maker uses.

Interaction Effect between Time Limit and Feedback: We differentiate $\frac{ds}{d\phi}$ with respect to the time limit τ to analyze how the change through a variation of feedback provision depends on the time limit. We get

$$\frac{ds}{d\phi d\tau} = \frac{\frac{d}{d\tau}\left[-F_e G_\phi\right](F_e G_s - F_s G_e) - \frac{d}{d\tau}\left[F_e G_s - F_s G_e\right](-F_e G_\phi)}{(F_e G_s - G_e F_s)^2}.$$

Under the given assumptions, the sign of $\frac{ds}{d\phi d\tau}$ depends on the sign of

$$\begin{aligned}\frac{d}{d\tau}\left[-F_e G_\phi\right] &= -\left(F_e G_{\phi\tau}\right) \\ &= -(u_{ee} - c_{ee})(-d_{s\phi\tau}),\end{aligned}$$

and

$$\begin{aligned}\frac{d}{d\tau}\left[F_e G_s - F_s G_e\right] &= F_e G_{s\tau} \\ &= (u_{ee} - c_{ee})(-d_{ss\tau}).\end{aligned}$$

We assume that $d_{ss\tau} < 0$ and $d_{s\phi\tau} > 0$. Then, we have:

$$\frac{ds}{d\phi d\tau} < 0,$$

i.e., the impact of feedback on the use of shortcuts is stronger under a strict time limit.

5.B EXPERIMENTAL APPENDIX

5.B.1 TRANSLATED INSTRUCTIONS AND SCREENSHOTS

In the experiment, the instructions were provided on screen. The instructions are translated from German. The sample instructions belong to treatment OPEN75 and CLOSED75. All deviations in treatments CLOSED40 and OPEN40 are given in square brackets.

Instructions of the four main treatments

— Screen 1 —

Instructions for the experiment — general information

We welcome you to this economic experiment. It is very important that you read the following instructions carefully. If you have questions, please get in touch with us. Please lift your hand, we will come to you and help you personally.

In today's experiment, it is not allowed to keep personal belongings (books, lecture notes, etc.) or mobile phones and other electronic devices within reach or to use them. Thus, please put your bags and personal belongings behind your seat. Please turn off your mobile phone and ensure that the alarm is deactivated. Please put your mobile phone and other electronic devices into the envelope that has been placed in your cabin. Please do not seal the envelope, this will be done by the lab team. The envelope remains in your cabin during the entire experiment and must not be opened. During the experiment, it is not allowed to talk to the other participants of the experiment. Non-compliance with this rule leads to exclusion from the experiment and from all payments.

For the duration of the experiment, you are assigned a shredder. For safety reasons and to ensure the participants' anonymity, the shredder is located in the second room of the laboratory. A live video of the shredder is streamed on your screen in real time for the entire duration of the experiment. Please verify that you can see a video of the shredder on the right half of your screen. Please note: The webcam of your desktop is deactivated. We would like to ask you to mark your shredder such that at any time you can verify that it is your shredder you see. For this purpose, please use the Post-It note that has been place in your cabin. The form of your marking is up to you. The lab team will collect your Post-It note before the experiment begins and will mark your assigned shredder.

You can earn money in this experiment. For showing up for this experiment, you receive 4.00 Euro. During the experiment, you can earn additional money. The amount you earn at the end of the experiment depends only on your own decisions and is not influenced by the decisions of the other participants.

All decisions are made anonymously, i.e., none of the other participants gets to know the identity of a participant who made a certain decision. All payments are made anonymously, too, i.e., no participant gets to know the payoff of the other participants.

After the experiment, we would like to ask you to fill in a questionnaire while we prepare your payment. After you have filled in the questionnaire, please stay at your cabin until we separately call your cabin numbers for payment.

Good luck and thank your for your participation!

— Screen 2 —

Practice period 1

On the following screen, you will see 10 sliders. Each slider can be moved to any possible position between 0 and 100. The start position of each slider is determined randomly. For each slider which you position exactly on position 50 before the time expires, you earn 4 Cents. You have 75 seconds [*CLOSED40/OPEN40: 40 seconds*] to position the 10 sliders. Your round income is calculated as follows:

Round income = Number of correctly positioned sliders × 4 Cent

— Screen 3 —

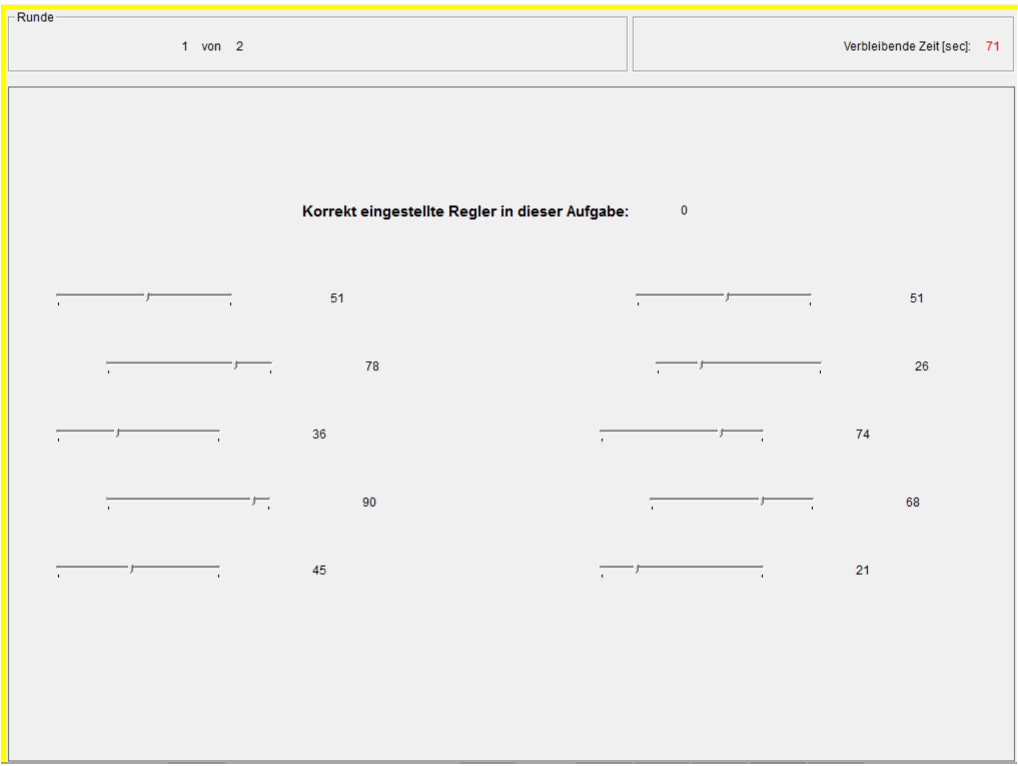


Figure 5.B.1: Screenshot difficult practice period.

— Screen 4 —

Practice period 2

On the following screen, you will see 10 sliders. Each slider can be moved to three possible positions (0, 50, 100). The start position of each slider is 0. For each slider which you position exactly on position 50 before the time expires, you earn 4 Cents. You have 75 seconds [*CLOSED40/OPEN40: 40 seconds*] to position the 10 sliders. Your round income is calculated as follows:

$$\textbf{Round income} = \textbf{Number of correctly positioned sliders} \times \textbf{4 Cent}$$

— Screen 5 —

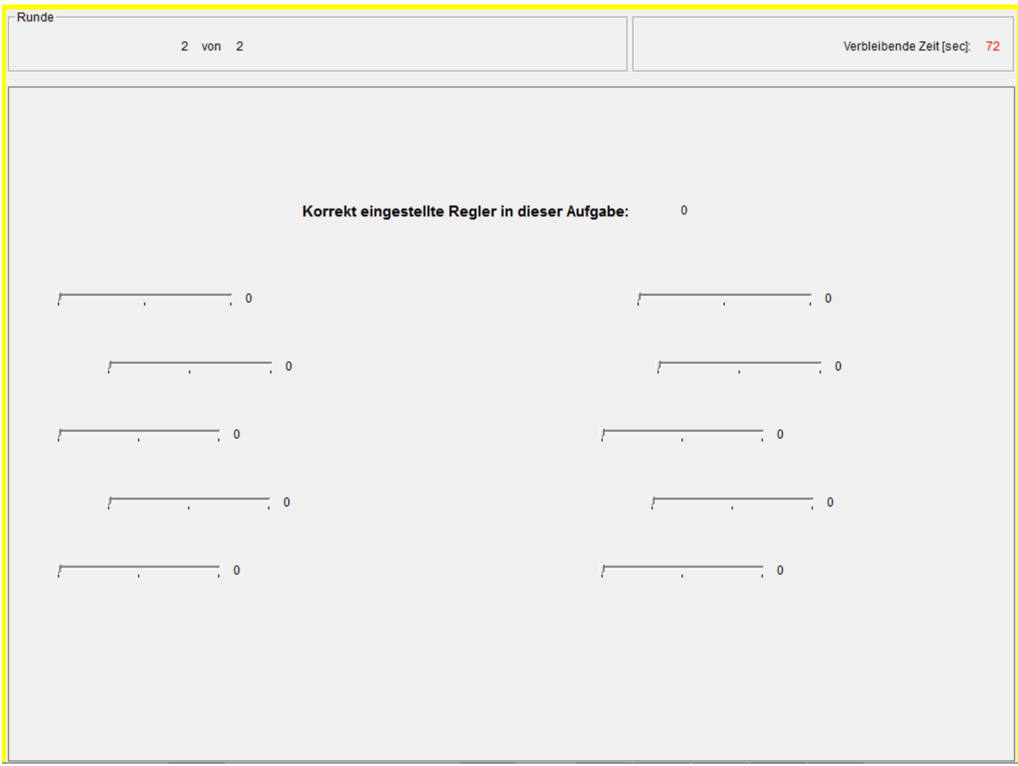


Figure 5.B.2: Screenshot difficult practice period.

— Screen 6 —

Main part of the experiment

The main part of the experiment consists of 50 rounds with 10 sliders each. As in practice period 1, each slider can be moved to any possible position between 0 and 100. The start position of each slider is determined randomly and is an integer value between 0 and 100. For each slider which you position exactly on position 50 before the time expires, you earn 4 Cents. You have 75 seconds [*CLOSED40/OPEN40: 40 seconds*] in each round to position the 10 sliders. Your round income in each of the 50 rounds is calculated as follows:

Round income = Number of correctly positioned sliders × 4 Cent

Simplification

Before the start of each round, you are given the opportunity to indicate how many of the 10 sliders you want to simplify in this round. As in practice period 2, each simplified slider has three possible positions (0, 50, 100) to which the slider can be moved. The start position of each simplified slider is 0. Please note: Of the 10 sliders of one round the simplified ones are displayed first (ordered from the top).

For each slider you simplify, after the time for positioning the sliders expires, a blank sheet of paper will be shredded in the other room by the shredder which you marked. In each round, it applies that:

Number of simplified sliders = number of shredded sheets of paper

— *Screen 7* —

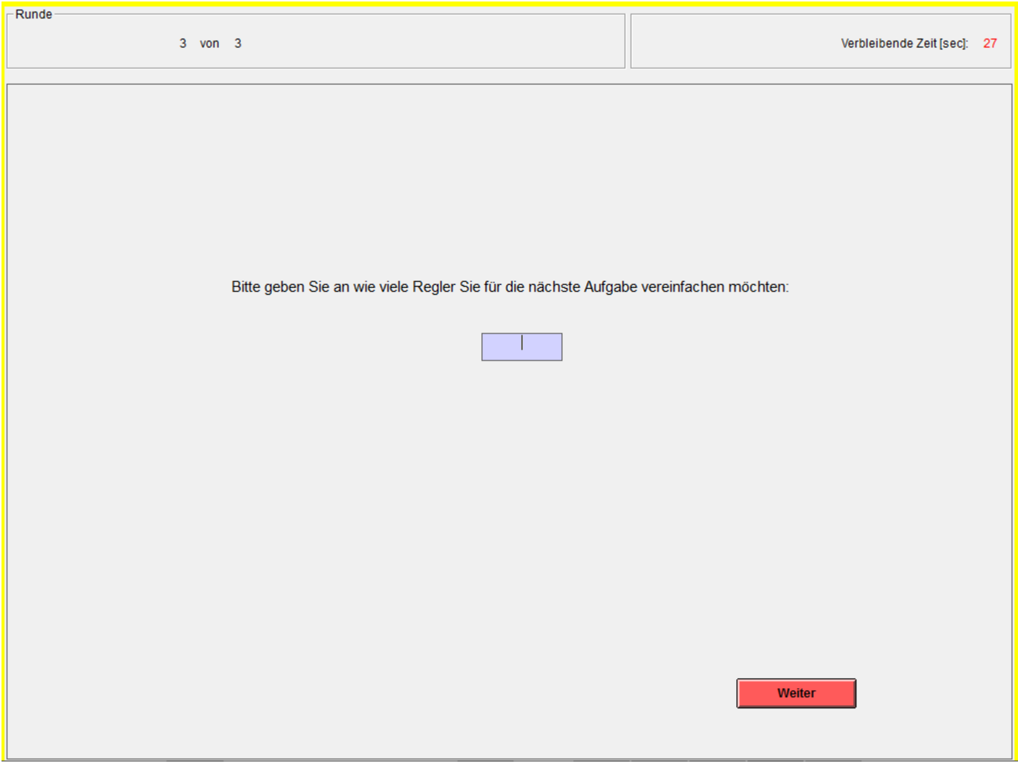


Figure 5.B.3: Shortcut decision screen.

5.B.2 PICTURES

Figure 5.B.4: Closed shredder bin.

Figure 5.B.5: Open shredder bin.

Figure 5.B.6: Experimental setup.

Figure 5.B.7: Shredder marking.

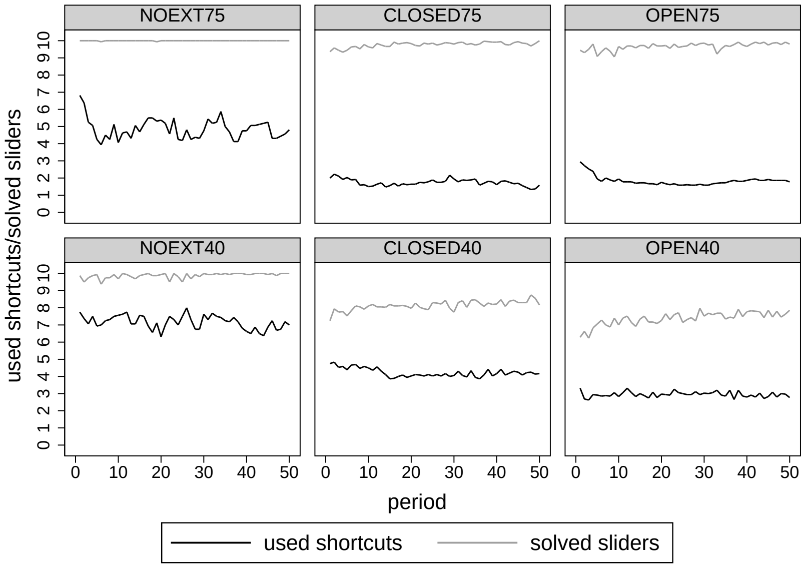


Figure 5.C.1: Used shortcuts and solved sliders by period.

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