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Essays in behavioral game theory

Dissertation thesis

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Declaration

I hereby declare that this thesis is my original work which I have worked out by my own. All sources, references, and literature used or excerpted during elaboration of this work are properly cited and listed in complete reference to the due source.

Štěpán Veselý

Brno, April 2016

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Dissertation abstract

I report results from a series of experiments on behavior in the prisoner's dilemma game, guessing game, public goods game, ultimatum game and a pure matching coordination game. Two broad topics underlie these investigations: fairness and social norms. More specifically, this work makes the following main contributions: 1) I study instrumental fairness and active deception, finding evidence for both. I offer a new classification of agents with respect to these behaviors. 2) I find inter-game learning in the prisoner's dilemma against three out of five strategies employed. 3) I check the robustness of a previous interesting finding that when feedback is withheld, transfer of "meaningful" learning to partially dissimilar games is stronger than when feedback is given (this study differs from the rest in that it does not focus on fairness or norms). 4) I report evidence showing that initially more cooperative groups are relatively more able to sustain cooperation in the long run. 5) I demonstrate that unless it is excessively tough, tough bargaining can be effective. 6) I provide a robustness check of a new incentive-compatible norm elicitation method showing that respondents submit equally accurate ratings with and without incentives. 7) I use this recently introduced norm elicitation method in the context of environmental behavior and establish that shared normative beliefs indeed predict environmental behaviors.

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INTRODUCTION

This thesis presents results from a series of experiments studying strategic behavior in settings that can be formalized as simple games, such as prisoner's dilemma, public goods game, and ultimatum game. Besides my focus on strategic social interactions, and the use of the experimental method, what unifies this collection of investigations is the focus on fairness (Chapters 1, 2, 7 and 8) and social norms, such as the norm of honesty (Chapters 1, 2 and 3), cooperation (Chapters 4 and 6) and of behaving pro-environmentally (Chapter 9). In addition, Chapters 4 and 5 study learning in strategic settings.

The topics of my thesis all have a long tradition in social psychology – social norms (Sherif, 1936; Deutsch & Gerard, 1955; Ajzen, 1991), fairness (Adams, 1965; Walster et al., 1978; Loewenstein et al., 1989), cooperation (Oskamp & Kleinke, 1970; Dawes, 1975, 1980; Kelley & Thibaut, 1978), and negotiation (Bazerman & Neale, 1982; Neale & Bazerman, 1992). All these topics also received considerable attention in behavioral economics – social norms (Krupka & Weber, 2009, 2013), fairness (Fehr & Schmidt, 1999; Cappelen et al., 2007), cooperation (Zelmer, 2003; Chaudhuri, 2011), and negotiation (Güth et al., 1982; Roth, 1995). Along with for example Gintis (2011), I believe that most surviving boundaries between social psychology and behavioral economics are unnecessary and unproductive, apart from (to some extent) certain methodological differences concerning incentive-compatibility of participants' responses (see Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001; and Chapter 8 in this work), and experimental deception (see Hertwig & Ortmann, 2001, 2008; and Chapter 3 in this work). Many of the past disciplinary boundaries have, of course, become dissolved already. For example, the empirical study of negotiation was mostly limited to psychology in the 1970's, then mostly limited to economics in the 1980's, but since then it has been clearly of interest to scholars in both disciplines. Part of the reason why I entitled my

thesis “Essays in behavioral game theory” is that game theory and behavioral game theory provide a framework that can be (and is) used by both psychologists and economists and enables a fruitful and effective communication between the two disciplines.

The study of fairness has become part of the social science mainstream (e.g. Güth et al., 1982; Fehr & Schmidt, 1999). Early studies such as Mitzkiewitz & Nagel (1993), Hoffman et al. (1994, 1996) and Suleiman (1996) were among the first to show that what appears as fairness in behavior can be decomposed into fairness driven by genuine, intrinsic fairness preferences and into strategic or instrumental fairness driven by strategic motives, such as the avoidance of sanctions. The flow of studies deconstructing observed fairness keeps growing to this date (e.g. Dana et al., 2006, 2007; Andreoni & Bernheim, 2009; Koning et al., 2011). Chapters 1, 2, 7 and 8 contribute to this literature.

Chapters 1 and 2, specifically, contribute to the strand of literature that shows that people behave less fair when others cannot clearly interpret their behavior (van Dijk et al., 2004; Dana et al., 2006, 2007; Andreoni & Bernheim, 2009; Koning et al., 2011). When others cannot clearly interpret the behavior of agents this creates a “moral wiggle room” which allows agents to make more selfish choices with relatively little threat of punishment from others. These two chapters also study deception – the participant needs to lie actively in order to create the moral wiggle room. Several studies examined this topic, but unlike in the previous studies (with the exception of Besancenot et al., 2013) participants are given a fine-grained choice set of possible deception in my experiment.

Chapter 7 studies tough bargaining using the ultimatum game paradigm. Game theoretical analysis based on subgame perfection and virtually all behavioral studies suggest that in this

game it is always the first mover (the proposer) who can afford to be tough to a greater or lesser degree. The second mover (the responder) should be never tough according to subgame perfect equilibrium analysis; and according to behavioral findings, he never demands more than half of the surplus[1]. In my experiment I show what would happen if the responder initially *did* demand more than half of the surplus. I find results in line with some theoretical arguments suggesting that such a tough strategy could be effective in the long run (Camerer et al., 2002; Napel, 2003).

Chapter 8 shows that very generous offers in ultimatum games are perceived as less fair and less appropriate than equitable offers. Hence, given that very generous offers are also relatively more costly, there does not seem to be any reason for the proposer to make such offers (unless, perhaps, he meets a tough responder, as in the experiment in Chapter 7). I also show that very generous offers are perceived as less fair than appropriate[2]. Thus, there are differences in perceived appropriateness and perceived fairness as far as very generous offers are concerned. Interestingly, I find no such differences when eliciting perceptions concerning either equitable offers or more or less stingy offers. Thus, while people are able to distinguish what is fair from what is socially appropriate, they see a given offer as equally (un)fair and (in)appropriate when this offer is either equitable or more or less stingy.

Social norms research will benefit from the development of valid and reliable measurement methods that can be used to capture norms (Krupka & Weber, 2013; Kimbrough & Vostroknutov, 2015). In Chapter 8, I make a contribution to this line of research by

[1] In this thesis, I call the responder "tough" if she rejects equitable or higher offers. I call the proposer "tough" if she offers small offers (say, 30% of the surplus or less – the precise amount would depend on the specific setting). I only study the effect, on proposer's behavior, of the responder being "tough" in this work.
[2] I.e., when participants rate the offers, appropriateness scores assigned to very generous offers are on average higher than fairness scores assigned to very generous offers.

comparing a novel incentive-compatible method for norm elicitation (Krupka & Weber, 2013) with an unincentivized version of this protocol.

In Chapter 9, I adopt the aforementioned method due to Krupka & Weber (2013) to elicit widely shared beliefs concerning 1) social appropriateness, 2) environmental effect, and 3) difficulty of a wide range of environmental behaviors. I show that pro-environmental behaviors perceived to be more socially appropriate and easier to perform are more likely to be chosen.

In Chapters 4 and 6, I study the traditional problem of cooperation in social dilemmas. In Chapter 4, I show that with experience people become more successful against certain types of opponents in the prisoner's dilemma.

Chapter 6 shows that initial group cooperation matters for long-run cooperation success. While we see some tendencies towards the typical cooperation decay as the interaction progresses (Andreoni, 1988; Croson, 1996b; Croson et al., 2005; Neugebauer et al., 2009), groups that are initially more cooperative remain more cooperative in the long run. Interestingly, this effect seems to become weaker and weaker as the game progresses. However, our use of a relatively simple design precludes a straightforward interpretation of this observation. Overall, Chapter 6 suggests that initially more cooperative groups are to some extent better able to overcome social dilemmas, and it poses an important new question: how long can we expect the beneficial effect of initial cooperativeness to last?

Like Chapter 4, Chapter 5 studies learning in a strategic setting – the guessing game. In this chapter I provide a robustness check of Rick and Weber's (2010) interesting finding that

when feedback about game outcomes is withheld from players, they are *better* able to transfer "meaningful" knowledge (e.g. how to discover the game's Nash equilibrium) obtained in one guessing game to a subsequent, partially dissimilar guessing game. I, however, find no evidence of better transfer learning between partially dissimilar guessing games when feedback is withheld (compared to the case when feedback is given). I discuss processing depth as a possible moderator of the effect of (absent) feedback on transfer learning.

Chapter 3 is a note on the current state of the debate concerning the use of deception in experiments. While experimental economists present some rather convincing arguments against it (and while I follow these recommendations in all experiments presented here), I show that there is in fact very little empirical research that would provide support against or for the use of experimental deception. Without such evidence many social psychologists appear reluctant to stop using experimental deception (Hertwig & Ortmann, 2008).

Before proceeding, let me briefly comment on the issue of external validity of lab experiments (for a more thorough discussion see e.g. Smith, 1982; Levitt & List, 2007; Falk & Heckman, 2009; Camerer, 2011). Social scientists sometimes disagree about the relevance and importance of external validity in case of lab studies. I tend to agree with those who say that the strength of laboratory experiments lies primarily in testing and developing theories, which includes testing the effects of experimental manipulations[3]. This view also maintains that external validity or generalizability is not necessarily of immediate interest when performing laboratory experiments (Falk & Heckman, 2009).

[3] Few single experiments in the social sciences serve to test or develop an entire theory, of course, unless the theory is extremely narrow. The process of testing and developing theories is incremental in nature.

In agreement with this position, I did not design my experiments so as they *necessarily* posses high external validity (e.g., the settings in which subjects make decisions in my experiments are more or less stylized) or high generalizability to different populations (e.g., I use convenience samples of university students).

Laboratory research is done under the assumption that the effects found in the lab can be in principle replicated outside the lab (see e.g. Smith, 1982; Falk & Heckman, 2009; Camerer, 2011). However, outside the lab other factors not present in the lab in the same form or intensity – e.g. social norms, habits, wealth – can interact with the effects one has discovered in the lab (e.g., the effects found in the lab might be weakened in certain subpopulations which were not represented in the lab). Thus, if one wants to be sure (as far as possible) that his findings from the lab will be replicated in the everyday world outside the lab, he needs to reproduce (as much as possible) all potentially relevant factors in the lab as they appear in the everyday world. Such an endeavor might sometimes even make sense (e.g. if the purpose of the lab experiment is designing an intervention to be applied in the field).

Rather than trying to ensure external validity and generalizability in the way described in the previous paragraph, the researcher interested in external validity of some lab results may follow up with a new study testing the regularities discovered in the lab in a field setting that interests him[4]. This is a very sensible and increasingly common approach[5]. Thus, if one wants to test the generalizability of specific lab findings (including the findings reported in this thesis) to some field setting and/or to certain subpopulations, one should conduct new studies

[4] There are usually many different field settings to which one might want to generalize. People sometimes forget this when talking about external validity. It would perhaps be more accurate to speak of external validities, plural.

[5] See e.g. Henrich et al. (2001), Fehr & List (2004), Falk (2007), Levitt & List (2007), Harrison et al. (2007), Herrmann et al. (2008), Shang & Croson (2009), Klein et al. (2015).

in the field (or in the lab, but reproducing carefully the conditions in the field setting that interests him).

CHAPTER 1: ULTIMATUM GAME WITH ASYMMETRIC INFORMATION: A STUDY OF DECEPTION AND FAIRNESS[6]

Abstract

The aim of the experiment reported here is to examine decision making in ultimatum games with symmetric and asymmetric information. In the asymmetric information condition, subjects (n = 134) were given an opportunity to deceive their partner. I found that participants indeed deceived their partners. Specifically: in the asymmetric information condition they reported to their partner they were given what amounted to only 56% of the actual endowment. Subsequently, they allocated only 28% of the actual surplus to their partner in the asymmetric information ultimatum game, whereas in the symmetric information game, where deception was not possible, they allocated 45% of the surplus to their partner. The larger the surplus to be divided, the more deception was present. Women deceived significantly more than men and allocated less money to their partner in asymmetric information ultimatum game (the effect size of gender was small, though).

[6] This chapter is based on Vesely (2014b).

1. INTRODUCTION

Ultimatum game (UG) was introduced by Güth et al. (1982). UG rules are simple. It is a game for two players, usually with only one round. The players have a sum of money to be divided between them. One of the players is the proposer, the other is the responder. The roles are assigned randomly. The proposer makes an offer how to divide the money and the responder either accepts or rejects the offer. If the offer is accepted by the responder, the money is split according to the proposer's offer. However, if the responder rejects the offer, neither player gets anything. In both cases, the game is over.

In this chapter I examine whether proposers tend to deceive responders when they have an opportunity due to asymmetric information: i.e. when responders do not know the size of the surplus to be divided and proposers can give them false information about it. Alternatively, proposers can act honestly and give responders true information.

This study builds upon van Dijk et al. (2004) and Koning et al. (2011) and related research, but in my study proposers deceived the other player actively (unlike in van Dijk et al., 2004) and to a degree they themselves chose (unlike in both van Dijk et al., 2004 and Koning et al., 2011).

Deception in the ultimatum bargaining context has attracted considerable attention. However, with a few exceptions (Boles et al., 2000; Croson et al., 2003; Koning et al., 2011; Besancenot et al., 2013; Kriss et al., 2013), deception by commission (active deception) was not enabled in previous research, and perhaps more importantly, only very limited choice set was offered to participants (to deceive vs. not deceive), with the exception of the study by Besancenot et

al. (2013). My design addresses these shortcomings by enabling *many degrees* of *active deception*.

My main research objectives, i.e. to study deception and fairness in a strategic setting, can be addressed by comparing proposers' behavior under two information conditions: in an asymmetric information UG and in the standard UG.

I make the following predictions: First, proposers will take advantage of the asymmetric information condition. Specifically, they will misrepresent the size of the surplus, especially in case of large surpluses. This prediction will be tested in hypotheses H1 and H3. My first hypothesis (H1) is that surplus size stated by proposers in asymmetric information UG will be lower than the real surplus size. This is a test of the presence of deception. My third hypothesis (H3) is that misrepresentation of the surplus will be positively related to the size of the actual surplus. (We will return to these hypotheses at the end of the next section.)

By looking at the surplus size stated by proposers in the asymmetric information UG, we will be able to say not only what is the proportion of people who actively deceive, but also to what degree, on average, people deceive. This was not possible in previous research, which offered to participants only a very limited choice set ("deceive" vs. "not deceive", e.g. van Dijk et al., 2004; Gneezy, 2005; Koning et al., 2011; for an exception see Besancenot et al., 2013 – discussed in the next section). Research relying on such a binary choice set is unable to tell us anything about the average degree of deception (or about any other aspects of the distribution of the degrees of deception preferred/chosen by participants). To avoid this shortcoming I use a large choice set of possible degrees of deception in this study.

My second prediction is that proposers will be less fair[7] in the asymmetric information condition, which will show that they use fairness strategically in the standard UG, rather than having genuine preferences for fairness. When speaking of "fairness" in this chapter, I invoke the equality and equity fairness ideals (see e.g. Cappelen et al., 2007). In the present setting, these ideals require equal division of the real surplus between the proposer and the responder (the two ideals coincide in the present setting).

The second prediction will be tested in hypothesis H2: offers made in asymmetric information UG will be lower than offers in symmetric information UG. This is a test of the "fairness preferences" vs. the "fear of rejection" explanation of behavior in UG (Suleiman, 1996; van Dijk et al., 2004). If the null hypothesis of no difference between the symmetric information UG offers and the asymmetric information UG offers is rejected, this will support the fear of rejection explanation. As "fear of rejection" has been repeatedly found to affect UG offers, as described in the next section, the test of H2 represents an attempt to replicate this effect in a different setting (different payoffs, number of games, subject pool, and a different deception mechanism). The test of hypothesis H2 can thus serve as a useful robustness check of previous findings on fear of rejection.

2. THEORETICAL BACKGROUND AND FORMER RESEARCH

From the game theoretic perspective, assuming players are rational and self-regarding, the proposer should offer the smallest fraction of the surplus to responder, and the responder should accept it. More concretely, a rational and self-regarding responder whose utility is monotonically increasing in money should accept anything > 0, because it maximizes her

[7] I.e., they will make offers further away from the equal split of the real surplus (and closer to zero).

gains (if she rejects, she will get 0 because of the rules of the game – see description in the previous section). A rational and self-regarding proposer knows this, so he should offer the smallest fraction of the surplus > 0 and have the rest for himself. These moves represent a subgame-perfect Nash equilibrium of the game[8].

UG represents an attractive problem precisely because players' behavior does not adhere to the game theoretic prediction outlined above. Proposers usually offer approximately 40% to 50% of the total sum, with 50/50 split being the modal offer; and half of the responders usually reject offers below about 20% to 30% of the total sum, see e.g. Güth et al. (1982), Fehr & Schmidt (1999), Croson et al. (2003).

2.1 FAIRNESS VS. FEAR OF REJECTION

According to the fairness hypothesis proposers act fair (give high offers) even when they do not have to fear rejection. Conversely, according to the fear of rejection hypothesis, they propose relatively lower offers the less they have to fear rejection (see Suleiman, 1996).

Three main types of experimental design can be used to vary fear of rejection present in an UG setting: a comparison of UG and a dictator game, a comparison of modified UGs with different δ parameters, and a comparison of standard symmetric information UG and asymmetric information UG.

[8] There is another subgame-perfect Nash equilibrium in which the rational and self-regarding proposer offers 0 and the rational and self-regarding responder accepts. However, under the assumption that the responder will in fact be *indifferent* between accepting and rejecting an offer of 0, the rational and self-regarding proposer, if he anticipates this, will offer more than 0.

Kahneman et al. (1986), Forsythe et al. (1994), Hoffman et al. (1994), and van Dijk & Vermunt (2000) rely on the first type of design: they compare dictator game and UG. In dictator game there is no fear of rejection on the proposer's part because the responder cannot reject proposer's offer. In UG he/she can and in that case neither player gets anything (which is the scenario the proposer fears – ending up empty handed). The above mentioned authors theorize that if proposers give equally high offers both in UG and in dictator game, it is due to their fairness preferences. If, on the other hand, proposers offer more in UG than in dictator game, it can be concluded that offers in UG are partially driven by fear of rejection. The results of the aforementioned studies usually (with the exception of van Dijk & Vermunt, 2000) support the fear of rejection hypothesis.

Suleiman (1996) and van Dijk et al. (2004) modify UG by inclusion of a discount factor δ, which expresses the relative power of the responder to reject the offer made by proposer. Specifically, if the responder rejects an offer, each player's final outcomes are determined by multiplying the proposed allocation to the respective player by the δ factor. UG and dictator game are cases with extreme δs (0 in case of UG and 1 in case of dictator game)[9]. Thus, when rejecting an offer in UG, for example, the proposed allocations to both players are multiplied by $\delta = 0$ and both players get nothing. Whereas when rejecting an offer in games with non-zero δs, both players end up with a non-zero fraction of the original proposal (equal to the original proposal multiplied by δ). The reasoning in Suleiman (1996) and van Dijk et al. (2004) is analogical to that described in the previous paragraph: if proposers offer equally high offers both in games with high δs and in games with low δs, it is due to their fairness preferences, otherwise higher offers in games with lower δs are partially driven by fear of

[9] One can imagine situations where δ is outside the 0-1 interval. However, such situations are not studied in Suleiman (1996) and van Dijk et al. (2004).

rejection. The findings of Suleiman (1996) and van Dijk et al. (2004) support the fear of rejection hypothesis.

Finally, a comparison of standard symmetric information UG and asymmetric information UG was employed e.g. in Mitzkiewitz & Nagel (1993), Pillutla & Murnighan (1995), Straub & Murnighan (1995), Croson (1996a), Kagel et al. (1996), Boles et al. (2000), Croson et al. (2003), van Dijk & Vermunt (2000), van Dijk et al. (2004), Shalvi et al. (2011b), and in the present study. The basic comparison works on the same principle as in the two experimental designs outlined above: a comparison of proposer's allocations in a low fear of rejection situation and in a high fear of rejection situation.

In this third type of design, the low fear of rejection situation is implemented as asymmetric information UG, whereas the high fear situation is the usual UG. Past findings on asymmetric information UG again support the hypothesis that offers in UG are partially driven by fear of rejection. For example in van Dijk et al. (2004) proposers divided a sum of chips under two conditions: symmetric information UG and asymmetric information UG. In both treatments the chips had double monetary value for the proposer. The asymmetric information condition differed from the symmetric information condition in that the responder did not know about the different value of chips. In the symmetric information condition he/she was aware of it. Therefore for any offer of *n* chips there was presumably more fear of rejection in the symmetric information condition than in the asymmetric information condition. For instance, when the proposer offered 50 chips out of a 100-chip pie in both conditions, the responder perceived this as an equal split in the asymmetric information condition, but as a 50-for-the-responder/100-for-the-proposer split in the symmetric information condition. As a result, in van Dijk et al. (2004) proposers offered more chips to recipients in the symmetric information

condition (M = 58.71) than in asymmetric information condition (M = 50.56). Significant differences in the same direction between asymmetric information UG and standard UG are found by other studies, such as Straub & Murnighan (1995), Croson (1996a), Croson et al. (2003) and Shalvi et al. (2011b).

2.2 DECEPTION IN ASYMMETRIC INFORMATION UG

If the responder does not know the actual surplus size (or the actual value of the surplus to either of the parties), the proposer can take advantage of this and deceive the less informed other player.

Most older studies of asymmetric information UG enable only indirect (passive) deception. That is the proposer cannot actively lie – he/she cannot explicitly state a false size of the surplus or an incorrect value of the surplus. The proposer is only given the opportunity to behave as if the (incorrect) knowledge about the surplus size or the value of the surplus the other player has was in fact correct. We can return to the example from van Dijk et al. (2004). A chip in the surplus has double monetary value for the proposer. The proposer knows it, the responder does not. The responder most likely incorrectly assumes that the chips have equal value for both players. Hence, the proposer is given the opportunity to behave as if the responder's incorrect assumption about the value of the chips was in fact correct. This constitutes the opportunity for indirect or passive deception. (Note that the proposer can alternatively behave in agreement with what is actually true about the value of the chips, which would lead to him/her making different distributive choices – namely compensating for the differential value of the chips by increasing his/her offer.)

In this chapter I study proposers' active deception. There seem to be only five older studies dealing with this topic: Boles et al. (2000), Croson et al. (2003), Koning et al. (2011) and two recent articles: Besancenot et al. (2013) and Kriss et al. (2013).

Koning et al. (2011) used modified UG and enabled active deception by proposers. Nevertheless, deception in their study had only two possible degrees (present – absent).

Kriss et al. (2013) designed an asymmetric information UG experiment where the proposer divided a surplus which was (with equal probability) either small or large. The receiver did not know the actual size of the surplus. The proposer could send a (possibly false) cheap talk message about the size of the surplus to the responder along with his/her offer. Again, deception in Kriss et al. (2013) had just two possible degrees (present – absent).

In Boles et al. (2000) and Croson et al. (2003) subjects can only deceive the other player via cheap talk, so their design is somewhat different from the present one. In experiments with cheap talk (communication between players), the responder knows that the proposer delivers an unverifiable message (e.g. about the size of the surplus). In my experiment the situation is different – the responder can never tell whether information about the size of the surplus comes from the proposer, because all information is delivered to him/her by the experimenter. The information available in asymmetric information UG and in symmetric information UG is thus indistinguishable from the responder's perspective in my design[10].

Besancenot et al. (2013) also use cheap talk type of communication. Otherwise, their study employs (independently) a design that is quite similar to mine: the actual surplus size is

[10] This does not constitute deception on the experimenter's part, because all participants were informed about this procedure beforehand.

picked with even chances among integers in the interval [50; 100]. The distribution is common knowledge, however the surplus size actually drawn is known only to the proposer who can misrepresent its size in a cheap talk message to the responder.

2.3 GENDER DIFFERENCES

Additionally, I want to check whether there are gender differences in decisions made in symmetric and asymmetric information UG. In the standard UG, there seem to be no robust gender differences in proposers' distributive decisions (see e.g. Solnick, 2001; Schmitt et al., 2008; McGee & Constantinides, 2013). As far as I know, no gender differences have been reported in proposers' offers in partial information UG.

As Childs (2012) observes, the evidence whether men or women are more prone to deception is inconclusive (and he reviews some recent papers). For instance, Childs (2012) found that equal portion of women and of men lied in a sender-receiver game. Similarly, Holm & Kawagoe (2010) found no notable gender differences in lying in this game. However, Dreber & Johannesson (2008) found that more men than women lied in this game. This is just one example of the mixed evidence regarding gender differences in deception (for other examples see e.g. the survey by Rosenbaum et al., 2014). Therefore, I do not expect to find any strong gender differences.

The problem of gender differences in UG is tangential to this study, but since gender differences in lying in strategic games represent a new area of study (see Dreber & Johannesson, 2008), I believe it could be of use to some readers to report my findings briefly.

2.4 THE PRESENT STUDY

This chapter contributes to the investigation of active deception and fairness of distributional choices in strategic settings.

My first hypothesis (H1) is that surplus size stated by proposers in asymmetric information UG will be lower than the real surplus size. This is a test of the presence of deception.

Unlike the present study, most previous studies enabled only indirect/passive deception. More recently, a couple of studies enabled active deception via cheap talk messages (e.g. Croson et al., 2003; Besancenot et al., 2013). However, cheap talk communication may increase suspicion on the responder's part, since he/she is aware that the message about the surplus size he/she receives from the proposer can be false. Responder's suspicion is not a problem in my design, or much less so.

Like the present study, Koning et al. (2011) enable active non-cheap talk deception. The present design, however, enables many degrees of deception, whereas in Koning et al. proposers could choose between just two alternatives: to lie, or not, which is too restrictive.

My second hypothesis (H2) is that offers made in asymmetric information UG will be lower than offers in symmetric information UG. This is a test of the "fairness preferences" vs. the "fear of rejection" explanation of behavior in UG (cf. Suleiman, 1996; van Dijk et al., 2004). If the null hypothesis is rejected, this will support the fear of rejection explanation.

Finally, I hypothesize that deception increases in the size of the money at stake: misrepresentation of the surplus will be positively related to the size of the actual surplus (H3). This hypothesis is based on the notion that deception is safer and potentially more profitable in case of larger surpluses[11].

3. METHOD

3.1 PARTICIPANTS

A convenience sample of 134 subjects participated in this study (67 men, 67 women), mostly university students. Their age ranged from 17 to 58 years (M = 24.3, SD = 4.6). They had no prior experience with UG. Although it is possible that some participants knew each other, they could neither choose, nor learn with whom they interact.

3.2 PROCEDURES

The study was presented to participants as a research on decision making. UG was explained to them in neutral terms (similarly to how the game is described in the Introduction). Their comprehension was checked. Subsequently, they played six incentivized one-round UG

[11] A given stated surplus size (say, 40 CZK) in the asymmetric information UG has the same chance of being considered true by the responder irrespective of the actual surplus size, as the responder has no way of learning the actual surplus size. If the actual surplus size is large (say, 200 CZK) the proposer can misrepresent it more than if the actual surplus size is small (say, 60 CZK) *and end up with the same stated surplus size* (40 CZK, in this example). In this sense, commiting larger misrepresentations is "safer" (and more profitable) if the original surplus size is large rather than small.

The reason why I speak about deception being more or less "safe" in the first place follows from an assumption that *for a given offer the fact that the stated surplus size is considered true rather than false by the responder increases the chance of acceptance*. This assumption seems reasonable because if the stated surplus size is considered by the responder to be untrue, by implication the real surplus size will be (by design – see the next section) considered by him/her to be larger than the stated surplus size. By implication, a given offer will then be considered to be less fair than if the stated surplus size were believed to be true by the responder. This will, in turn, lead to a higher chance of rejection (assuming fairness preferences as e.g. in Fehr & Schmidt, 1999).

games (players were rematched in each game without replacement), as described below in more detail[12].

All participants' decisions remained anonymous throughout the experiment. Only the experimenter knew each participant's decisions, i.e. the experiment was "single-blind" (which was common knowledge)[13].

The size of the surplus divided in each game ranged between 20 and 200 units of experimental crowns (EC). The possible range of the surplus size was known to subjects in both experimental conditions (described below), the probability distribution over this range that determined the amount of money to be divided was not known to participants. The mean actual surplus size and its distribution were identical in both experimental conditions.

Participants were informed beforehand that 1/24 of games played for EC will be randomly selected and the amounts will be paid to the respective participants privately after completion of both stages of the experiment (with exchange rate 100 EC = 100 CZK). The use of probabilistic money in this study warrants a few comments. On the one hand, the use of probabilistic money in decision experiments is common. For instance, in Straub & Murnighan (1995) there was only a 2 out of 1813 chance that a particular decision will be chosen and rewarded in cash. In van Dijk et al. (2004, Experiment 1) the chance was 15 out of 108, in Boles et al. (2000) it was 50%, and in Besancenot et al. (2013) it was 25%. In various other

[12] Experimental instructions are available in Appendix A.

[13] Such single-blind designs (in which participants do not know the identity of those they are interacting with, but the experimenter is in principle able to observe who made which decisions – and the participants are aware of this) are common, although it is sometimes observed that players behave more pro-socially in single-blind than in double-blind experiments (in double-blind designs neither the participants nor the experimenter are able to observe who made which decisions, see e.g. Hoffman et al., 1994, 1996). However, according to Bolton et al. (1998) and Engel (2011) allocations in dictator game (which is usually sensitive to factors in the experimental design) are independent of the presence vs. absence of experimenter observation. A similar result is reported by Laury et al. (1995) in case of the public goods game.

settings, what can be seen as "probabilistic incentives" were used e.g. by Starmer & Sugden (1991), Cubitt et al. (1998), Bardsley (2000), Bardsley & Sausgruber (2005), and in studies using the strategy method (see Brandts & Charness, 2011 for a review)[14]. While there is some evidence that the use of probabilistic incentives need not affect choice at least in some settings[15], their use cannot be considered wholly unproblematic.

In the context of my experiment the biggest issue is that probabilistic incentives may be too weak to motivate decisions[16]. The advantage of using powerful incentives is that they can help reduce the influence of unobserved factors, such as social desirability, on decisions (see e.g. Smith, 1982). One should note that in the present study the use of probabilistic incentives is common to both experimental conditions (described below), thus using them should not preclude comparisons between the conditions. I will, however, discuss instances in which the use of probabilistic incentives might have affected my results in section 5.

The experiment had two stages. (Participants knew beforehand that the experiment would consist of several parts, but they received the instructions for each part only after finishing the previous part.) In stage 1, all participants had the role of proposer. Participants made their stage 1 decisions in a paper-and-pencil format. No feedback with respect to the acceptance of their proposals was given to them until after the experiment was completed. In stage 1, responders were described as "other participants in this study" in the instructions. When

[14] Experiments using what I call "probabilistic incentives" are, by my definition, characterized by two features: only a fraction of the decisions will turn out to be payoff-relevant, and the specific decisions which will eventually turn out to be payoff-relevant are unknown to the participants ex ante. The present experiment fits this definition. Note: The mechanism which determines which decisions will eventually become payoff-relevant may be different in different experiments – compare e.g. Cubitt et al. (1998) and Bardsley (2000) – and I hasten to add that the use of the word "probabilistic" is not meant to imply that this mechanism is necessarily stochastic in nature.

[15] See Cubitt et al. (1998) and Brandts & Charness (2011); but see Sadiraj & Sun (2012) and Cox et al. (2015) who report payment protocol effects on bargaining decisions and on choice under risk, respectively.

[16] Other concerns about probabilistic incentives arise when studying decisions under risk (see Starmer & Sugden, 1991; Cubitt et al., 1998; Cox et al., 2015, 2016).

making their decisions in stage 1, participants did not know they will be later (in stage 2) deciding as responders.

Participants in the role of proposers made their decisions in six games in stage 1: in three games in symmetric information condition (SC) and in three games in asymmetric information condition (AC).

In SC the actual surplus size was known to both proposer and responder.

In AC, the actual surplus size was known to the proposer, not to the responder. The proposer knew that the responder would not be given information about the actual surplus size. Instead, the surplus size stated by the proposer in stage 1 was announced by the experimenter as the actual surplus size to the responder in stage 2 (this procedure was common knowledge).

Therefore, in AC the proposer decided upon his/her division of the surplus and upon the size of the surplus announced to the responder in stage 2 as the actual surplus (this was the opportunity for active deception on the proposer's part)[17].

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[17] This footnote summarizes the instructions more concretely. However, for all details the reader is referred to the instructions available in Appendix A.

In AC, the proposer was asked, in stage 1, to select an amount X (which had to be between 20 EC and the real surplus and a multiple of 20). When choosing X, the proposer was informed that the experimenter will inform the responder that the "amount being divided" (in quotation marks in the instructions) in a given UG is equal to X. The proposer was additionally informed that the responder will know that the "amount being divided" could be *either* the real surplus *or* that it could be an amount selected by the proposer (X); BUT that the responder will never learn which of the two is actually the case, and that in AC the responder will never learn what the real surplus is.

Subsequently, in stage 2, the responder was told (via an e-mail sent by the experimenter, see below the description of stage 2) what is the "amount being divided" (in quotation marks in the instructions) for each UG and what is the proposed allocation to him/her. In SC, the "amount being divided" stated to the responder by the experimenter in stage 2 was in fact the actual surplus size. In AC, the "amount being divided" stated to the responder by the experimenter in stage 2 was in fact the surplus size stated by the proposer (i.e., the X from the preceding paragraph). However, the responder in stage 2 was *not* experimentally deceived, as he was informed

The SC and AC conditions are illustrated by three examples in Table 1.

Table 1: Three illustrative examples of stage 1 conditions

	Example 1	Example 2	Example 3
Condition	Symmetric information	Asymmetric information	Asymmetric information
Actual surplus size	60	60	60
Surplus size stated by proposer	Proposer cannot state surplus size.	40	60
Surplus size announced to responder as the actual surplus size	60	40	60
Did proposer deceive the responder?	Deception is not possible.	Yes	No

The order of presentation of SC and AC conditions in stage 1 was randomly determined in each session (i.e., sessions were assigned to order of presentation). Participants first made three proposals in one of the conditions, then response sheets from that condition were collected, and participants received instructions and a response sheet for the second condition[18].

Immediately after completing stage 1, participants received the first part of (printed) instructions for stage 2: They were informed that in the upcoming weeks they will receive six randomly selected anonymous offers (three made in SC, three in AC) from other participants

that the actual amount being divided may or may not be identical to the "amount being divided" stated to the responder by the experimenter in stage 2.

[18] For example Cox et al. (2008, 2016) raise the concern of cross-task contamination in within-subject designs (or, more broadly, of the equivalence of within-subject and between-subjects designs). This concern is relevant also for the present experiment. The safest but also the most costly (in terms of money and statistical power) way how to avoid cross-task contamination is reliance on between-subjects designs with a single – payoff-relevant – decision per subject. In the present study, I took the approach of randomizing the order of presentation of experimental conditions. Regression results reported in Table 2 have subsequently shown that there was no significant cross-task contamination between decisions in SC and AC.

in the experiment (excluding themselves) via an e-mail sent by the experimenter. They were informed that the e-mail will state the six offers and the respective surplus sizes. Participants were informed that a given surplus size stated in the e-mail will be the real surplus size if the respective offer has been made in SC, and it will be the surplus size stated by the respective proposer if the respective offer has been made in AC. Participants were also informed that it will *not* be indicated in the e-mail in which condition a given offer was made, and that the offers will be presented in no particular order. Participants learned that they will be asked to indicate acceptance or rejection of each of the offers by sending an e-mail to the experimenter, who will subsequently pay out 1/24 of randomly selected decisions via a bank transfer.

Participants then gave their contact details and bank account numbers and left the session. After stage 1 data have been collected from all participants in this study, the e-mail correspondence through which stage 2 was implemented took place. The e-mail from the experimenter indicated the six "amounts being divided" and the six anonymous offers from six different proposers and asked the responder to choose whether to accept or reject each offer. The responder then sent his/her decisions to the experimenter via e-mail. Three of the offers each participant received were made in SC and three in AC (of which the responders have been informed in the printed instructions for stage 2 distributed after stage 1[19]). The offers were presented in random order and the condition in which a given offer was made was not indicated. The "amount being divided" indicated in the e-mail was the real surplus in SC, while in AC it was the surplus stated by the proposer (reproduced exactly in the e-mail). Responders were made aware of the fact that each of the "amounts being divided" stated in the e-mail may or may not be equal to the actual surplus in a given game.

[19] This information was not restated in the e-mail.

Comments on the procedure: Implementing stage 2 via an e-mail correspondence can be considered unusual in an experimental study (on the other hand, using electronic communication is common in questionnaire studies in psychology). One should note, however, that the (stage 1) decisions which are really important for the present study took place before participants knew anything about the implementation of (or their role in) stage 2, and thus the key measures obtained in stage 1 could not have been in any way influenced by the way stage 2 was implemented.

In retrospect, stage 2 could have perhaps been more easily and naturally implemented during the session immediately after stage 1, using the strategy method (Selten, 1967) and/or the "minimally acceptable offer" method (e.g. Blount, 1995).

3.3 MEASURES

The main variables of interest are the following: the amount offered by the proposer in symmetric information UG, the amount offered by the proposer in asymmetric information UG, actual surplus size (manipulated by the experimenter), surplus size stated by the proposer in asymmetric information UG, and two measures of misrepresentation of the surplus size by the proposer in the asymmetric information UG, referred to as $\text{Misrepresentation}_1$ and $\text{Misrepresentation}_2$.

By design, the stated surplus size must always be a multiple of 20 EC and within the 20-200 range (the same is true for the actual surplus size).

The variable Misrepresentation equals 1 - (stated surplus size / actual surplus size). In calculating $\text{Misrepresentation}_2$, but not $\text{Misrepresentation}_1$, cases of actual surplus equal to 20 EC are ignored. The distinction between these two measures is made because deception has no sense – and it never took place – when actual surplus is 20 EC.

4. RESULTS

4.1 HYPOTHESIS H1

In order to base the analysis on strictly independent observations, I use an approach similar to that used e.g. in Ambrus & Pathak (2011) and Thöni et al. (2012). I calculate the average proportion of the real surplus admitted (stated) by each proposer in AC[20] and test whether the sample average of this variable is different from 1. With one sample *t*-test I reject the null that the average proportion of the real surplus admitted by proposers in AC (M = 0.56, SD = 0.23) does not differ from 1, thus supporting H1: $t(133) = -22.374$, $p < .001$, effect size $r = .89$ (large effect); mean difference = -0.44 (95% *CI* = -0.48, - 0.41)[21]. The conclusion is that proposers deceive the other player in UG with asymmetric information, and the deception is substantial (on average, proposers state they have been given what only amounts to 56% of the actual surplus). Descriptively speaking, 96% of subjects in this study engaged in deception at least once and 43% deceived in all three games in AC.

[20] I average within subject by summing the subject's three stated surpluses in AC and divide this by the sum of that subject's three real surpluses.

[21] Unless otherwise stated, all *p*-values reported in this work are two-sided. I repeat all *t*-tests in this chapter using appropriate non-parametric procedures. The non-parametric tests yield qualitatively similar results as the *t*-tests in all cases. Testing H1 with Wilcoxon rank-sum test yields $p < .001$ (the tested null posits the median proportion of the real surplus admitted by proposers in AC does not differ from 1).

4.2 HYPOTHESIS H2

I first calculate the average proportion of the real surplus offered by each proposer in SC and in AC[22]. Next, I test whether the sample averages of these two variables differ. With dependent *t*-test for repeated measures I find support for hypothesis H2: the average proportion of the real surplus offered by proposers in SC is 0.45 (SD = 0.07), which is significantly different from the average proportion of the real surplus offered by proposers in AC (M = 0.28, SD = 0.11), $t(133) = 16.873$, $p < .001$, effect size $r = .83$ (large effect); mean difference = 0.17 (95% *CI* = 0.15, 0.19)[23].

To complement the above analysis, I estimate two linear regression models (see Table 2) with Condition (SC = 0, AC = 1), Order of presentation (SC first = 0, AC first = 1), Gender (male = 0, female = 1), Age, and the Gender by Condition interaction as fixed factors. The dependent variable is the proportion of the real surplus offered[24].

Model 2 includes the same predictors as Model 1. In addition, it includes Subject ID as a random factor (not reported in Table 2), and Experience as a repeated measures fixed factor (first three games = 1, last three games = 2)[25]. Covariance structure employed in Model 2 is

[22] I average within subject by summing the subject's three offers in SC and divide this by the sum of that subject's three surpluses in SC. Likewise, I average within subject by summing the subject's three offers in AC and divide this by the sum of that subject's three real surpluses in AC.

[23] The alternative hypothesis H2 is supported by rejecting the null of no difference between the average proportion of the real surplus offered by proposers in SC and AC. Testing H2 with Wilcoxon signed-rank test yields $p < .001$.

[24] I average within subject by summing the subject's three offers in SC and divide this by the sum of that subject's three surpluses in SC. Likewise, I average within subject by summing the subject's three offers in AC and divide this by the sum of that subject's three real surpluses in AC. Subject averages are used as observation units in the regressions.

[25] Because we use two observations per participant, there can be a clustering of effects in participants' responses, which can lead to correlated errors. This lack of independence is handled by including Subject ID as a random factor in Model 2.

"unstructured", REML is employed as the estimation method. The dependent variable is the same as in Model 1, i.e. the proportion of the real surplus offered[26].

Table 2: Regression models of the proportion of real surplus offered

	Model 1	Model 2
	Unstandardized coefficients (std. error)	Unstandardized coefficients (std. error)
Intercept	0.477 (0.031)***	0.492 (0.037)***
Condition	-0.143 (0.016)***	-0.143 (0.014)***
Order of presentation	0.003 (0.011)	0.003 (0.013)
Gender	0.015 (0.016)	0.015 (0.016)
Age	-0.001 (0.001)	-0.001 (0.001)
Gender * Condition	-0.056 (0.023)*	-0.056 (0.020)**
Experience		-0.007 (0.010)

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$. Number of observations = 268 in both Models (two observations per subject, one in each condition). Model 1's $R^2 = .471$. Coding of variables: Condition (SC = 0, AC = 1), Order of presentation (SC first = 0, AC first = 1), Gender (male = 0, female = 1), Experience (first three games = 1, last three games = 2).

Both models reported in Table 2 produce very similar estimates, and they lead to the same conclusion as the above *t*-test of H2: The highly statistically significant negative coefficient on Condition indicates that offers (expressed in terms of the proportion of the real surplus) are lower in AC than in SC.

The regression results also indicate that while there are no overall gender effects, Gender interacts with Condition in affecting offers. The statistically significant Gender * Condition interaction term indicates, specifically, that women in AC (coded as 1) offer smaller proportions of the real surplus than participants in the other "cells" combined (i.e., women in

[26] I average the within subject by summing the subject's three offers in SC and divide this by the sum of that subject's three surpluses in SC. Likewise, I average within subject by summing the subject's three offers in AC and divide this by the sum of that subject's three real surpluses in AC. Subject averages are used as observation units in the regressions.

SC and men in SC and AC, all coded as 0). Gender differences are studied in more detail below in section 4.4.

As Table 2 shows, no other variables predict offers. This is reassuring in case of the Order of presentation and Experience variables: There were no significant spillovers between the AC and SC conditions. Participants made similar decisions whether they first participated in AC or in SC. Likewise, participants made on average similar decisions in the first three games they played and in the last three games.

4.3 HYPOTHESIS H3

In 97 cases out of 402 (24.13%) the proposers do not misrepresent the actual surplus in AC. When cases of actual surplus equal to 20 EC are ignored, actual surplus is not misrepresented in 63 cases out of 368 (17.11%) in AC. To test H3 I calculate the correlation between each of two measures of misrepresentation of the surplus and the size of the actual surplus in AC[27]. In order to use strictly independent observations to test H3, misrepresentation is first averaged within each subject[28]. Pearson correlation between misrepresentation of the surplus and the size of the actual surplus in AC (again averaged within subject across decisions in AC) is equal to .173 (p = .045) when computed for $\text{Misrepresentation}_1$, and r = .148 (p = .088) when computed for $\text{Misrepresentation}_2$. The correlations are small, and only marginally significant in case of $\text{Misrepresentation}_2$. Hypothesis H3 is at least weakly supported (accepting the more

[27] The variable Misrepresentation equals 1 - (stated surplus size / actual surplus size). In calculating $\text{Misrepresentation}_2$, but not $\text{Misrepresentation}_1$, cases of actual surplus equal to 20 EC are ignored.

[28] I average within subject by summing the subject's three stated surpluses in AC and divide this by the sum of that subject's three real surpluses. The obtained value is subtracted from 1 to obtain a measure of misrepresentation.

liberal 10% α-level) by rejecting the null of no correlation between the respective variables: proposers are somewhat more willing to deceive responders when larger pies are at stake.

4.4 ADDITIONAL RESULTS

Using *t*-test for independent samples, I found no significant differences between the average proportion of the surplus offered in SC by male (M = 0.45, SD = 0.08) and female proposers (M = 0.46, SD = 0.06), $t(132) = -1.205$, $p = .23$, $r = .10$ (small effect); mean difference = -0.01 (95% *CI* = -0.04, 0.01)[29].

However, with *t*-test for independent samples I found significant differences between the average proportion of the surplus offered in AC by male (M = 0.30, SD = 0.11) and female proposers (M = 0.26, SD = 0.12), $t(132) = 1.397$, $p = .034$, effect size $r = .12$ (small effect); mean difference = 0.04 (95% *CI* = 0.00, 0.08)[30].

I also found that the average $\text{Misrepresentation}_1$ was higher in female proposers (M = 0.49, SD = 0.24) than in male proposers (M = 0.40, SD = 0.22), $t(132) = 2.050$, $p = .042$, effect size $r = .18$ (small effect); mean difference = 0.08 (95% *CI* = 0.00, 0.16). Similarly, $\text{Misrepresentation}_2$ was higher in female proposers (M = 0.49, SD = 0.24) than in male

[29] Before obtaining sample means, I average within subject by summing the subject's three offers in SC and divide this by the sum of that subject's three surpluses in SC. Subject averages are used as independent observations. (Note that the average surplus size was the same for men and women in SC.) Testing for gender differences in offers in SC with Mann-Whitney U test yields $p = .40$.

[30] I.e., men offer on average 4 percentage points more in AC than women. Before obtaining sample means, I average within subject by summing the subject's three offers in AC and divide this by the sum of that subject's three real surpluses in AC. These subject averages are used as independent observations. (Note that the average actual surplus size was the same for men and women in AC.) Testing for gender differences in offers in AC with Mann-Whitney U test yields $p = .028$.

proposers (M = 0.41, SD = 0.22), $t(132) = 1.985$, $p = .049$, effect size $r = .17$ (small effect); mean difference = 0.08 (95% *CI* = 0.00, 0.16)[31].

To summarize, there seem to be no gender differences in distributive decisions in SC and some small differences in distributive decisions and in active deception in AC.

4.5 RESPONDERS' BEHAVIOR

The behavior of responders is reported for the sake of completeness. However, one should note that responders' stage 2 behavior has no direct relevance for testing hypotheses H1-H3.

Acceptance rate was 92.8% overall, fairly similar to e.g. Croson (1996a). We thus see little variability in responders' decisions.

The logistic regression reported in Table 3 attempts to explain the existing variability in responders' choices by regressing choice (0 = reject, 1 = accept) on Visible surplus (in EC units, Visible surplus is equal to the actual surplus size in SC, and it is equal to the stated surplus size in AC), Offer percent (equal to the percentage of the Visible surplus offered to responder), and Offer size (in EC units).

Table 3: Logistic regression of acceptance decisions

	Unstandardized coefficient (std. error)	Odds ratio
Visible surplus	.000***	.93
Offer percent	.000***	1.05

[31] Before obtaining the sample mean misrepresentation, I average within subject by summing the subject's three stated surpluses in AC and divide this by the sum of that subject's three real surpluses. The obtained value is subtracted from 1 to obtain a measure of misrepresentation. Testing for gender differences in $\text{Misrepresentation}_1$ with Mann-Whitney U test yields $p = .032$. Testing for gender differences in $\text{Misrepresentation}_2$ with Mann-Whitney U test yields $p = .030$.

Offer size	.000***	1.19

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$. Number of observations = 804 (six observations per subject). Cox & Snell's $R^2 = .65$, Nagelkerke's $R^2 = .87$.

The estimates reported in Table 3 involve repeated observations (six per subject), which are not strictly speaking independent. Thus, we need to interpret the regression results with caution, because dependence in data can lead to an increased probability of Type I errors.

All predictors included in the model reported in Table 3 turn out highly statistically significant. Higher offers, both in absolute terms and relative to the Visible surplus (i.e., Offer size and Offer percent, respectively), increase acceptance odds, as one would expect. Ceteris paribus, increasing the size of the Visible surplus decreases the odds of acceptance.

5. DISCUSSION

I confirmed that offers in asymmetric information UG were lower than offers in standard UG. This is in line with previous studies that involved asymmetric information UG, e.g. Straub & Murnighan (1995), Croson (1996a), van Dijk & Vermunt (2000), van Dijk et al. (2004).

The average offer made in SC was equal to 45.02% of the pie, hence comparable to the normally observed values (e.g. Fehr & Schmidt, 1999).

It is worth noting that the average offer made in AC was equal to 51.13% of the stated size of the pie. Therefore both in SC and AC proposers offered roughly an equal share of the "visible" surplus. This indicates that even when the proposer does not in fact behave fairly, he/she feels the need to appear as if he/she did, because of strategic considerations, image

concerns or both[32]. One aspect of the results reported by van Dijk & Vermunt (2000) also suggests this. Proposers in their second experiment divided 100 chips. In the asymmetric information condition where responders were uncertain whether the surplus was 60 or 100 chips, 32% of the proposers offered 30 chips (i.e. a fair split if the surplus was 60 chips), whereas none of the participants made such an offer in the symmetric information condition.

This hint of indirect deception in van Dijk & Vermunt (2000) leads us to the results related to my main hypothesis. I found that proposers deceived the other player in UG with asymmetric information and the deception was substantial.

96% of subjects in the present study deceived at least once and 43% deceived in all three games in AC. The proportion of cases where deception was present was 82.88% (when cases with actual surplus equal to 20 EC are ignored). These results are broadly in accord with Boles et al. (2000), Koning et al. (2011), Besancenot et al. (2013), and Kriss et al. (2013).

In Koning et al. (2011) proposers in a modified UG actively deceived in 52.27% of cases. Kriss et al. (2013) found a 67%-89% proportion of subjects who send untrue messages. Active deception appears somewhat more prevalent in my experiment. However, the present design enables various levels of deception. In Koning et al. (2011) it was only possible to either state that the chips had double value for the proposer (i.e. not to deceive), or state that it had equal value for both players (i.e. deceive). In Kriss et al. (2013) the situation was basically the same. It is therefore quite likely that those subjects that in the current study deceived to a small degree would have been honest in Koning's et al. or Kriss's et al.

[32] Behavior motivated by image concerns has been found in other contexts, such as the dictator game or the public goods game, cf. Andreoni & Petrie (2004), Dana et al. (2007), Andreoni & Bernheim (2009), Zafar (2011).

experiments where a small degree of active deception was impossible. Hence, qualitatively speaking, the tendency toward active deception does not seem to be more prevalent in my experiment than in Koning et al. (2011) and Kriss et al. (2013).

In Besancenot et al. (2013) the proportion of false stated surplus sizes was 88.5%, i.e. very similar to my result (82.88%). Misrepresentation of the endowment was, however, roughly two times smaller in Besancenot et al. (2013), namely 20.5% (whereas it was 44% in my experiment). Differences in experimental design could perhaps account for this difference. Most notably, in the present case, 1 in 24 games was randomly selected and paid in cash, while in Besancenot et al. (2013), it was 1 in 4 games. We can speculate that proposers may realize that the monetary benefit from deception should be discounted by the subjective probability that their decisions will be actually picked for payment. This subjective discount factor is most likely higher in my experiment. Thus, subjects who presumably take this subjective discount factor into account would need to misrepresent the surplus more in my experiment than in Besancenot et al. (2013), which is in line with the data. Also, the cheap talk type of communication between players used in Besancenot et al. (2013) could have diminished the level of deception.

I also showed that proposers were somewhat more willing to deceive responders when larger endowments were at stake[33]. This is in line with Gneezy (2005) who found that players in a sender-receiver game lied more if they could gain more. Besancenot et al. (2013) also report

[33] The effect of stake size on misrepresentation I found was rather small (correlation of approximately .15). One can speculate that such an effect could be more pronounced if using certain rather than probabilistic rewards. It is well known that probabilistic and certain rewards are often perceived differently (e.g. Kahneman & Tversky, 1979; Gneezy & Potters, 1997; Holt & Laury, 2002; Harrison et al., 2007). One can thus, for example, speculate that a given distance between two stakes (say, 40 and 80 CZK) is perceived – in terms of value – to be more pronounced for certain rewards than for probabilistic rewards, which could in turn have implications for the link between stake size and misrepresentation tested in H3. The present research was, however, not designed to test such conjectures.

some descriptive results that support the existence of a positive relationship between actual surplus size and the degree of misrepresentation. Boles et al. (2000) also give some descriptive results supporting this finding.

As far as gender differences are concerned, I found no gender differences in distributive decisions in SC (which is in line with e.g. Solnick, 2001; Schmitt et al., 2008)[34] and some small differences in distributive decisions and in active deception in AC.

Given the generally mixed and/or negative results regarding differences in deception by gender (see e.g. Holm & Kawagoe, 2010; Childs, 2012 and studies cited therein) and practically no previously published findings on gender differences in deception in asymmetric information UG, any conclusive interpretation of my findings appears premature at least before they are found on replication. I would also recommend obtaining motivational information and beliefs from the proposers, since it may be the case that different motives and beliefs are triggered in female proposers (previous research for example indicates that women are less competitive than men – see e.g. Niederle & Vesterlund, 2007), which might lead to a different degree of deception in UG.

The main reason for deceiving others is usually self-interest. According to "standard" economic theory, people will lie whenever they can benefit from it and the possible costs are not too high (see e.g. Becker, 1968; Allingham & Sandmo, 1972; Gneezy, 2005). In the present case there is money at stake – players can increase the likelihood of securing more money when they lie about the surplus.

[34] Similarly, Bolton & Katok (1995) and Cadsby et al. (2010) report no gender differences in symmetric information *dictator* games, except for women's (compared to men's) higher propensity to donate half of their endowment when deciding in single-sex groups (see Cadsby et al., 2010).

On the other hand, there are usually reasons not to deceive others or to use deception only to a certain degree. This is evidenced by the fact that subjects in the current experiment misrepresented the actual surplus size just to a certain degree. On the one hand, this can be explained by the fact that the range of surplus sizes was common knowledge and proposers might have assumed that for low observable surplus sizes responders could suspect deception to be present (which might be thought to increase the risk of rejection). On the other hand, a complementary explanation is that deception comes at a psychological cost, such as feeling guilt, shame, embarrassment or fear, and/or damaging one's self-concept as an honest person (cf. Bodner & Prelec, 2003; Mazar et al., 2008; Ploner & Regner, 2013).

This psychological cost of deception and the related intrinsic aversion to deceiving others can have several different sources, which are an object of lively research and debate (see e.g. Mazar et al., 2008; Lundquist et al., 2009; Shalvi et al., 2011a, 2011b). It is not the purpose of the current study to identify the sources of the possible psychological cost of deception – a different design would be necessary[35]. So, only a brief overview follows[36].

Sánchez-Pagés & Vorsatz (2007) argue that people refrain from deception because they follow a truth-telling moral norm (breaking the norm would arguably result in a psychological cost to oneself). This view is indirectly supported by questionnaire data from Gneezy (2005) where 88-98% of participants rated lies described in two scenarios as (very) unfair. Mazar et al. (2008) posit that the norm of being honest is internalized as a part of self-concept. Subsequently, behaving dishonestly bears negatively on one's self-concept, which is aversive.

[35] For example a design manipulating perceived social norms which can in turn influence (the subjective cost of) dishonest behavior, see e.g. Wenzel (2005), Keizer et al. (2008), Mazar et al. (2008), Traxler (2010), Bucciol & Piovesan (2011).
[36] The reader may also consult the literature survey by Rosenbaum et al. (2014).

Overall, Mazar's et al. results support the view that paying attention to moral norms (e.g. when being reminded of them) curbs dishonest behavior.

Mazar et al. (2008) also study certain mechanisms by which the need to update one's self-concept after behaving dishonestly can be attenuated, such as rationalization and inattention to standards for honesty. Related to this, Shalvi et al. (2011a) show that the psychological cost of lying can be moderated by the ability to subjectively justify one's deception.

Lundquist et al. (2009) found that, all things being equal, aversion to lying increases in the size of the lie: i.e. subjects are more likely to lie when a small lie is sufficient to benefit them (while they are less likely to lie when a large lie is necessary to obtain the same benefit). Notice that this finding cannot be easily applied to my results, because in my experiment a small lie would typically yield a smaller (not the same) expected benefit than a large lie. Lundquist et al. (2009) also suggest aversion to lying is weaker when subjects believe other participants lie[37].

Finally, Gneezy (2005) found that aversion to lying decreases in one's own potential payoff secured by lying, while it increases, if the lie harms others. Following Gneezy (2005) we can, for example, reason that the degree to which a proposer lies strikes a balance between the effect (on lying) of his/her own expected gain obtained from lying and the effect of the expected reduction of the responder's gain. However, it is not possible to conclude which variables (such as those studied by Gneezy, Sánchez-Pagés & Vorsatz, Lundquist et al.,

[37] E.g. Fischbacher et al. (2001), Falk & Fischbacher (2002), Bardsley & Sausgruber (2005), Krupka & Weber (2009) similarly show that one's propensity to "good" vs. "bad" behavior is often conditional on (one's beliefs about) the behavior of others.

Mazar et al.) actually influence deception in my design. It could be interesting to undertake a comprehensive study of these factors in the future.

As pointed out by Boles et al. (2000, p. 256), UG is an "abbreviation" of more complex negotiations and research in more naturalistic settings is therefore called for. For example Lusk et al. (2006) and Levitt & List (2007) argue that behavior of subjects in laboratory experiments (where they know their behavior is under systematic scrutiny) is prone to be more pro-social and moral than in more natural settings. Hence, we might possibly expect even more deception in comparable everyday situations (see e.g. Pruckner & Sausgruber, 2013). On the other side, in everyday life the moral norm of truth-telling can be more salient, which could lead to less deception. Then again, in the lab lying can be possibly more easily categorized by the participant as something else than "lying" (e.g. as "competing"), which can protect his/her self-concept of an honest person despite deception (cf. Dana et al., 2007; Mazar et al., 2008), which can in turn lead to more deception in the lab. Drawing conclusions about the generalizability of lab findings to specific field settings of interest is thus difficult without studies specifically designed to address such issues (see also the discussion in the Introduction to this thesis).

The current study somewhat enhances the realism of experimental research on deception in UG by enabling active deception in various degrees (which is its main advantage over the sender-receiver game design and over Boles et al., 2000; Croson, 2003; Koning et al., 2011; and Kriss et al., 2013). A recent study independently used a similar design – see Besancenot et al. (2013). It might be interesting to include different forms of possible deception in prospective studies (e.g. face-to-face and written) between which the participants could

choose and to investigate their "attractiveness" to proposers and their "effectiveness" in deceiving the responders.

Deception is not uncommon in real-life bargaining and negotiation (see e.g. Boles et al., 2000; van Dijk et al., 2004 and references therein). When bargaining under incomplete information (e.g. when the private values of the stake are not publicly disclosed), UG with asymmetric information can be a particularly useful model. The results I obtained from this model situation show that subjects use active deception quite extensively to further their profit.

Among the options of dealing with this problem in everyday life is assessment of situational and dispositional factors that can influence the amount of deception. Since bargaining for larger stakes increases the degree of deception, it could be wiser to bargain for a number of smaller stakes (perhaps with independent parties), rather than for one large stake. Also, certain personality characteristics such as social value orientation can predict deception (see van Dijk et al., 2004).

Because in everyday life deception is usually detectable to a certain degree, the possibility of discovering proposers' misrepresentations could be introduced into prospective studies. This could lead to an interesting and meaningful investigation of learning in strategic settings (see e.g. Nagel & Tang, 1998; Golman & Page 2010; Vesely, 2012 for other examples). Several effects might be examined: 1/ the willingness of the responder to pay for an increased likelihood of detecting deceit; 2/ the adaptation of the proposer to the level of detectability; 3/ the adaptation of the level of detectability by the responder in response to proposer's changing (adaptive) behavior.

CHAPTER 2: IDENTIFICATION OF PLAYER TYPES IN ULTIMATUM BARGAINING GAMES[38]

Abstract

Using cluster analysis I identified three types of players in two versions of the ultimatum bargaining game (with symmetric and with asymmetric information availability). I call the player types Fair players, Moderate opportunists and Hard-nosed opportunists. The player types constitute 23.1%, 43.3% and 33.6%, respectively, of the sample of 134 experimental participants. The types differ only moderately with respect to their choices in the standard ultimatum game. In particular, Hard-nosed opportunists make less generous offers than the two other types. However, there are very pronounced differences in behavior in the asymmetric information game. Fair players make fairer offers, misrepresent the surplus less frequently and to a lesser degree than the two other types. Hard-nosed opportunists are in turn less fair and more deceptive than Moderate opportunists in the asymmetric information game. Moreover, it is possible to infer that Opportunistic types are more strongly driven by fear of rejection (strategic concerns) and have weaker fairness preferences than Fair types.

[38] This chapter is based on Vesely (2014a).

1. INTRODUCTION AND THEORETICAL BACKGROUND

Identification of player types and assessment of their prevalence constitutes an important problem. Formulating realistic models of choice and behavior (e.g. Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000) can prove to be difficult without an accurate knowledge of the prevalence of specific player types.

Player types can be classified with respect to various criteria, such as their distributive preferences and/or fairness views (e.g. Selten & Ockenfels, 1998; Andreoni & Miller, 2002; Frohlich et al., 2004; Büchner et al., 2007; Trhal & Radermacher, 2009; Cappelen et al., 2011, 2013a; Mollerstrom et al., 2015); their cooperativeness and/or tendency to contribute to a public good (e.g. Weimann, 1994; Brandts & Schram, 1999; Fischbacher et al., 2001; Kurzban & Houser, 2005; Albert et al., 2007; Bardsley & Mofatt, 2007; Kocher et al., 2008; Herrmann & Thöni, 2009; Fischbacher & Gächter, 2010; Thöni et al., 2012; Gächter et al., 2012, 2014); their propensity to punish social norms violation (e.g. Fehr & Fischbacher, 2004); or their (strategic) choice sophistication (e.g. Stahl & Wilson, 1995; Houser et al., 2004; Costa-Gomes & Crawford, 2006; Crawford & Iriberri, 2007; Burchardi & Penczynski, 2014; Brocas et al., 2014).

Surprisingly, player types are rarely classified jointly along more than one dimension (for exceptions see Albert et al., 2007; Brocas et al., 2014; Devetag et al., 2015). This study attempts to show the usefulness of classifying player types along more than one dimension. Classifying types along more than one dimension can point to facts not apparent when studying/classifying behavior along one dimension at a time. For example, the classification presented in this chapter provides a more nuanced picture than would two classifications considering separately each of the dimensions (fairness and deception). Two separate

classifications would lead to the fairly obvious conclusion that there are players who are more fair and then there are players who are less fair; and that (in case of the second classification) there are players who are more deceptive and there are players who are less deceptive. The players are, however, more complex than this, as my study shows.

This chapter exemplifies an approach that can be profitably used in many different contexts within behavioral game theory. While, for example, the well-known classifications of players according to their cooperativeness in social dilemmas (e.g. Fischbacher et al., 2001; Herrmann & Thöni, 2009; Fischbacher & Gächter, 2010) are interesting on its own, it would be even more interesting to consider other dimension(s) along with cooperativeness. Maybe the cooperators and free-riders are not homogenous types. Maybe when considering new dimension(s), one will realize that there are types high (low) on cooperation as well as on the other dimension(s), and also types high (low) on cooperation but low (high) on the other dimension (e.g. on norm compliance, see for example Kimbrough & Vostroknutov, 2015). Thus, classifying player types along more than one dimension can yield new important insights, and stimulate new important questions (e.g., if some types are not highly cooperative because of their high willingness to comply with norms, then why?).

In this chapter, I classify participants in a bargaining experiment into player types according to two broad criteria: fairness of their choices and their tendency to deceive the other player when given an opportunity (these criteria are operationalized using four variables described in the Method section).

My analysis is based on data from a previous experiment that studied deception and fairness in ultimatum bargaining games (UGs) with symmetric and with asymmetric information (Vesely,2014b; see Chapter 1 in this work).

UG with symmetric information is the standard UG introduced by Güth et al. (1982). It is a game for two players, usually with only one round. The players have a sum of money to be divided between them. One of the players is the proposer, the other is the responder. The roles are assigned randomly. The proposer makes an offer how to divide the money and the responder either accepts or rejects the offer. If the offer is accepted by the responder, the money is split according to the proposer's offer. However, if the responder rejects the offer, neither player gets anything. In both cases, the game is over. In the asymmetric information UG responders do not know the size of the surplus to be divided and proposers can give them false information about it (see e.g. Koning et al., 2011; Besancenot et al., 2013; Kriss et al., 2013 for studies using asymmetric information UG).

This chapter contributes to the literature in that it classifies player types jointly according to two broad dimensions – their fairness and their tendency towards deception[39]. While several papers (briefly reviewed below) explicitly or implicitly sorted players into types according to each dimension separately, no previous paper provides a categorization that takes both dimensions jointly into account.

Several papers enable differentiation of players who deceive their counterparts and those who are honest. Note, however, that the prevalence of player types seems to depend, among other

[39] More precisely, the classification is made according to four variables presented in the Method section (two measures of fairness and two measures of deception).

things, on the payoff structure, as well as on the absolute magnitude of rewards (see e.g. Gneezy, 2005).

For instance, in Koning et al. (2011) proposers in a modified UG actively deceived in roughly 52% of cases. Kriss et al. (2013) found that 67%-89% of subjects (depending on the specific condition) sent untrue messages. In Besancenot et al. (2013) the proportion of stated false surplus sizes was approximately 89%. In Vesely (2014b) 96% of subjects deceived at least once and 43% deceived whenever given an opportunity. Other examples include Gneezy (2005), Dreber & Johanesson (2008) and Childs (2012) who employed the sender-receiver game.

Cluster analysis is a convenient method for classification of objects according to one or several variables. Generally speaking, objects that are similar to each other are grouped into a common cluster, while they should be distinct from members of other clusters. There are many different clustering methods, see e.g. Kaufman & Rousseeuw (2008). Here I use the two-step cluster algorithm by Chiu et al. (2001).

Cluster analysis is a method relatively commonly used in psychology and economics (e.g. Tola et al., 2008; Nayum et al., 2015). But it has been only recently applied to classify players of strategic games: Brocas et al. (2014) classify players according to their choices in betting games and according to how much information potentially useful to play the games they acquire.

2. METHOD

2.1 EXPERIMENTAL DESIGN

The experiment is described in detail in Vesely (2014b) and in the previous chapter. Only the basic features are therefore outlined here. A total of 134 subjects played six one-round UGs. Players were rematched in each game. The size of the surplus divided in each game ranged between 20 and 200 units of experimental crowns (EC). The possible range of the surplus size was known to subjects in both experimental conditions, the probability distribution over this range that determined the amount of money to be divided was not known to participants.

The experiment had two stages. In stage 1, which is the one relevant here, all participants had the role of proposer. No feedback with respect to the acceptance of the proposals was given to them until after the experiment was completed. The offers from stage 1 were randomly distributed among participants in stage 2 to either accept, or reject them.

In stage 1 participants in the role of proposers made their decisions in six games: in three games in the symmetric information condition and in three games in the asymmetric information condition. In the symmetric information condition the actual surplus size was known to both proposer and responder. In the asymmetric information condition, the actual surplus size was known to the proposer, not to the responder. The proposer knew that the responder would not be given information about the actual surplus size. Instead, the surplus size stated by the proposer in stage 1 was announced by the experimenter as the actual surplus size to the responder in stage 2. Therefore, in the asymmetric information condition the proposer decided upon his/her division of the surplus and upon the size of the surplus

announced to the responder in stage 2 as the actual surplus (this was the opportunity for active deception on the proposer's part).

2.2 MEASURES

Fairness of players' choices and their deceptive behavior are operationalized using the following four variables: fairness of their choices in symmetric information UG (variable FAIR_SI), fairness of their choices in asymmetric information UG (variable FAIR_AI), relative frequency of deception in asymmetric information UG (variable DEC_FR), and degree of deception in asymmetric information UG (variable DEC_DEG).

FAIR_SI is calculated as the percentage of surplus offered to the responder averaged over the three decisions each proposer made in symmetric information UG.

FAIR_AI is calculated as the percentage of (real) surplus offered to the responder averaged over the three decisions each proposer made in asymmetric information UG.

DECEP_FR is calculated as the number of games where deception was present divided by the total number of asymmetric information games (i.e., three). Cases of actual surplus equal to 20 EC are ignored, because deception has no sense in these cases (see Chapter 1 and Vesely, 2014b).

DECEP_DEG equals one minus stated surplus size divided by actual surplus size. This measure is again averaged over the three decisions each proposer made in asymmetric information UG[40]. Cases of actual surplus equal to 20 EC are once again ignored[41].

3. RESULTS AND DISCUSSION

Using two-step cluster algorithm with Schwarz's Bayesian Criterion I obtained three clusters of players. The silhouette measure of cohesion and separation of this three-cluster solution is fairly good (above 0.5). Table 1 reports the cluster profiles – means and standard deviations for the three clusters. Each cluster profile provides a characterization of a player type. The first type is Fair type, the second type is Moderate opportunist, and the third type is Hard-nosed opportunist.

Table 1: Profiles of three player types / clusters of players

Cluster / Player type	FAIR_SI	FAIR_AI	DECEP_FR	DECEP_DEG
Fair players	0.50 (0.08)	0.45 (0.04)	0.39 (0.24)	0.11 (0.08)
Moderate opportunists	0.46 (0.04)	0.33 (0.05)	0.79 (0.16)	0.35 (0.10)
Hard-nosed opportunists	0.42 (0.10)	0.19 (0.06)	0.98 (0.08)	0.63 (0.12)

As can be seen from Table 1, Moderate opportunists fall somewhere between Fair players and Hard-nosed opportunists in all measured variables. There are 31 Fair players (23.1% of the

[40] I average within subject by summing the subject's three stated surpluses in AC and divide this by the sum of that subject's three real surpluses. The obtained value is subtracted from 1 to obtain DECEP_DEG.

[41] As choices with real surplus equal to 20 EC are excluded from the calculation of DECEP_FR and DECEP_DEG, the calculation of these measures is based on two instead of three choices for some subjects.

sample), 58 Moderate opportunists (43.3% of total) and 45 Hard-nosed opportunists (33.6% of total).

Fair players seem to possess genuine preferences for fairness, rather than to be driven by fear of rejection when making their offers (see Suleiman, 1996; Vesely, 2014b)[42]. They share roughly half of the surplus with the responder irrespective of the information condition (see the second and third column in Table 1). However, even this type is driven by fear of rejection to some extent, because FAIR_AI (M = 0.45) is significantly smaller than FAIR_SI (M = 0.50) according to a paired *t*-test: $t(30) = 3.44$, $p < .01$, effect size $r = .53$; mean difference = 0.05 (95% *CI* = 0.02, 0.08). The effect size *r* is quite large, in fact[43].

For Moderate and Hard-nosed opportunists, the effect size of information condition is even larger, which means they are more strongly driven by fear of rejection and have weaker fairness preferences than Fair players.

In particular, for Moderate opportunists, the difference between fairness of offers in symmetric (M = 0.46) and asymmetric information UG (M = 0.33) is significant according to a paired *t*-test: $t(57) = 14.35$, $p < .001$, effect size $r = .88$ (very large effect); mean difference = 0.13 (95% *CI* = 0.11, 0.15).

[42] Recent theoretical (Bodner & Prelec, 2003; Bénabou & Tirole, 2006, 2011) and experimental (Dana et al., 2007; Larson & Capra, 2009; Grossman & van der Weele, 2013; Feiler, 2014) research suggests that some people who behave fairly in asymmetric information situations may be only *in part* driven by "genuine" fairness preferences. In part they might be motivated by self-signaling, i.e. they behave fairly to appear fair to themselves rather than "really" caring about fair outcomes.

[43] I repeat all *t*-tests in this chapter using appropriate non-parametric procedures (Wilcoxon signed-rank test for repeated measures, and Wilcoxon rank-sum test for independent samples). The non-parametric tests yield qualitatively similar results as the *t*-tests in all cases.

The difference between fairness of offers in symmetric (M = 0.42) and asymmetric information UG (M = 0.19) is even more pronounced in case of Hard-nosed opportunists: $t(44) = 11.88$, $p < .001$, effect size $r = .87$ (very large effect); mean difference = 0.23 (95% *CI* = 0.19, 0.27).

Fair players misrepresent the surplus in somewhat over one case in three on average and on average, they misrepresent the surplus by 11% (DECEP_DEG = 0.11). In contrast, Hard-nosed opportunists lie basically every time (DECEP_FR = 0.98). Moderate opportunists lie in roughly 80% of cases, i.e. also quite a lot. However, Moderate opportunists misrepresent the surplus much less than Hard-nosed opportunists. DECEP_DEG = 0.35 on average in case of Moderate opportunists, whereas in case of Hard-nosed opportunists it is 0.63 on average, the difference is statistically significant according to independent samples *t*-test: $t(101) = 13.60$, $p < .001$, effect size $r = .80$ (large effect); mean difference = -0.28 (95% *CI* = -0.32, -0.24).

Quite fascinating is the fact that players of different types behave relatively similarly in the standard UG (compare FAIR_SI by player type in Table 1). To see whether the differences in FAIR_SI by player type are significant, I ran a one-way ANOVA with player type as an independent variable and FAIR_SI as an outcome variable. There was in fact an overall significant difference among player types with respect to their behavior in the standard UG, $F(2, 131) = 11.90$, $p < .001$, the effect size $\omega = .37$ was of medium size. Using post hoc tests to compare player types pair-wise shows there are significant differences between Hard-nosed opportunists and both other types ($p < .01$ in both cases), but only a marginally significant difference between Fair players and Moderate opportunists ($p < .1$ when using Tukey HSD, Bonferroni, Gabriel).

As a comparison, consider the differences among player types with respect to their offers in the asymmetric information UG (FAIR_AI) and with respect to deception (DECEP_DEG). These differences were also significant ($p < .001$ in both cases), but the effect sizes were much larger, $\omega = .89$ both for FAIR_AI and DECEP_DEG[44]. This shows that behavior across player types was only moderately different in the standard UG, but extremely different in the asymmetric information UG.

Motivations underlying behaviors in the standard UG are presumably rather different for different types. Namely, Fair players are motivated mostly by their fairness preferences, whereas opportunistic types are motivated to a larger degree by fear of rejection. This can be inferred from the comparisons between FAIR_SI and FAIR_AI that I performed above.

Using the strategy of comparing players' behavior in symmetric and asymmetric UGs enabled me to investigate whether players are motivated by fairness preferences, or by fear of rejection (or strategic concerns, to put it differently). Unlike previous studies that focused on this issue (e.g. Croson, 1996a; Koning et al., 2011; Besancenot et al., 2013; Kriss et al., 2013), the present study also demonstrates differences in terms of behavior and likely motivations across three different types of players in ultimatum bargaining games.

4. CONCLUSION

Although empirical identification of player types in different strategic games is an important research objective in economic psychology and behavioral economics, the types are usually

[44] $F(2, 131) = 262.02$ when FAIR_AI was the dependent variable. $F(2, 131) = 263.73$ when DECEP_DEG was the dependent variable. All pair-wise comparisons (Tukey HSD, Bonferroni, Gabriel) between player types with respect to both FAIR_AI and DECEP_DEG were significant at least at the .0001 level.

classified only along a single dimension (e.g. their level of strategic reasoning). I employed cluster analysis to classify agents interacting in two versions of the ultimatum game according to their fairness and deceptiveness. Three types emerged from the analysis, Fair players, Moderate opportunists and Hard-nosed opportunists. Fair players were fairer (more generous) and less deceptive than the Opportunistic types. Interestingly, differences among player types were only moderate in the standard symmetric information ultimatum game, but they were very large in the asymmetric information game.

Future research should focus on possible behavioral and personality correlates of player types identified in this chapter. There is a number of interesting questions concerning the types, for example: Does "type" predict behavior in other important strategic settings besides UG? Does it predict behavior only in contexts where deception is possible or also in other contexts? Is a given player's type stable in time? Are some types more stable than others across contexts and/or in time? Does type correlate with measures such as agreeableness or Machiavellianism (see e.g. Goldberg, 1993; Gunnthorsdottir et al., 2000; Spitzer et al., 2007)?

CHAPTER 3: EXPERIMENTAL DECEPTION: CURRENT STATE OF THE DEBATE

Abstract

In the short run, experimental deception can help accomplish valuable scientific goals. When it is otherwise not harmful towards participants it is therefore fairly often used in psychology. Even by current standards of their own discipline, however, some psychologists use more deception than they probably should: deception is used when it is unnecessary and when alternative non-deceptive procedures exist. Moreover, there are reasons, advocated mostly by experimental economists, why experimenters should recognize and optimally avoid the long-run risks of using even mild experimental deception. Nevertheless, empirical evidence concerning the long-term risks of deceiving participants is mixed and relatively scarce at this point.

1. INTRODUCTION

In this study I will only discuss issues related to deception by commission (i.e. providing false information). Omission, i.e. not providing all information, is acceptable to both psychologists and economists in most cases, see Hey (1998), McDaniel & Starmer (1998), Houser et al. (2008), Kimbrough & Vostroknutov (2015).

Experimental deception is often "subtle" and the deceived parties are not harmed. I will focus on this type of experimental deception[45]. Experimental economists shy away even from this type of deception, while it is sometimes acceptable for psychologists and other social scientists.

For example, in recent studies on choice in distributive games with a non-standard action set (e.g. action set including a "take" option), Leliveld et al. (2008), Leliveld et al. (2009, Experiment 3), Zhou & Wu (2011), Reinders Folmer & De Cremer (2012, Studies 2 and 3) and Tomasino et al. (2013) deceived participants about the real existence of their bargaining counterparts. All these papers were published in psychology or neuroscience journals. In contrast, deception was never used in related papers published in economics journals (List, 2007; Bardsley, 2008; Cappelen et al., 2013b; Dreber et al., 2013; Krupka & Weber, 2013). Necessity is often invoked as an argument in defense of deception. However, deception was not "necessary" in the non-economics studies cited in this paragraph (although, admittedly, using it was cheaper than not using it would have been).

The prevalence of experimental deception in studies published in certain major social psychology journals was 30% or higher in the 1960s-1990s (Hertwig & Ortmann, 2001, 2008

[45] An example of "serious" experimental deception would be promising subjects rewards for participation and then not paying them or falsely promising that an experiment is not harmful.

and sources cited therein), whereas "economics studies that use deception can probably be counted on two hands" (Hertwig & Ortmann, 2001, p. 395)[46].

Is it "better" to use experimental deception as is common in psychology or to avoid it as is standard in economics? In the evaluation of what is "better" I adopt a utilitarian perspective. This means I believe we need to evaluate the trade-off between benefits and costs of using deception for all parties involved.

To summarize the discussion that follows: Using deception creates benefits for those using it. It can simplify experimentation considerably (see section 2.). However, using deception can also incur potentially serious costs to the scientific community. Namely, subjects may come to distrust the experimenters. In the long run this erosion of trust can wipe out most benefits of using deceptive designs, since distrusted experimenters lose control over (subjects' interpretation of) the experiments (see section 4.). Current evidence concerning the effects of experimental deception is mixed, scarce and limited in scope (see section 5.). This, however, should probably not be taken as a "license to cheat" subjects, as many experimental psychologists seem to think. Gathering more evidence on the effects of deception, avoiding deception and/or using existing and devising new methods that render the use of deception unnecessary (see section 3.) are other possible reactions to the present lack of conclusive evidence.

[46] See e.g. Marwell & Ames (1981), Kim & Walker (1984), Isaac & Walker (1988), Weimann (1994), Moxnes & van der Heijden (2003).

2. WHY TO USE DECEPTION

Experimental deception can be useful whenever one wants to investigate people's reactions to (relatively) unusual stimuli that are not under the experimenter's control. This is often the case when studying social interactions. As an example, let us consider how we could investigate the problem of conditional cooperation (i.e. whether people reciprocate cooperation with cooperation).

First of all, spontaneously occurring high levels of cooperation among anonymous participants in the lab are indeed rare. In the well-known public goods game[47] people initially cooperate to approximately 50% of the maximum level of cooperation available to them, and the level of cooperation declines over time when the game is repeated (e.g. Ledyard, 1995; Neugebauer et al., 2009). It is therefore clearly difficult to investigate how would people react to others' highly (or even maximally) cooperative behavior simply because such a stimulus is hard to come by.

Of course, one may repeat the public goods experiment a lot of times and hope that some high cooperators would show up. But this approach would be impractical and expensive.

Weimann (1994) who wanted to study how people respond to high levels of cooperative behavior consequently took recourse to experimental deception: the stimulus "high" and "low" cooperators in his experiment were not actually real people, but participants were not told so.

[47] This game is basically a multi-person prisoner's dilemma with a fine choice set. The (almost) continuous choice set makes it possible to cooperate a lot, somewhat or only a tiny bit – not just to cooperate, or to defect as in the standard prisoner's dilemma.

An alternative approach, chosen by Moxnes & van der Heijden (2003), is to use confederates instructed to cooperate a lot. This also constitutes deception because participants implicitly expect they are interacting with other untrained participants like themselves.

Both using direct deception as Weimann or pretend confederate-participants as Moxnes and van der Heijden is obviously more "practical" than waiting until the rare, highly cooperative behavior occurs spontaneously (for further discussion see e.g. Bonetti, 1998; Cook & Yamagishi, 2008; Sell, 2008). There are, however, some procedures that could be used instead of deception.

3. WHAT ARE THE ALTERNATIVES

The first, most straightforward alternative is to use artificial stimuli (e.g. computer programs acting similarly to humans) but not hide this fact from the participants. The problem with this approach is that people think and behave differently when knowing they interact with computers and not with other human beings (Rilling et al., 2004; Spitzer et al., 2007; Levy et al., 2011; Reinders Folmer & De Cremer, 2012; Ruff et al., 2013). For example factors such as fairness or altruism which are critical in many human interactions (e.g. Bolton & Ockenfels, 2000) might be virtually absent in human-computer interaction (cf. Falk et al., 2008).

Another alternative technique, well-known to behavioral economists (less so to social psychologists) is the so called "strategy method" (different modifications exist, see Selten, 1967; Fischbacher et al., 2001; Brandts & Charness, 2011). This method is actually quite simple. Let us say we conduct an experimental session with just two people who

simultaneously choose among two levels of cooperation. Importantly, each participant states (a) her unconditional choice (i.e. her chosen level of cooperation when not knowing what the other person chooses), and also (b) her conditional choices contingent on every possible choice by the other person (i.e. her chosen level of cooperation if the other person cooperates, and her chosen level of cooperation if the other person defects). The experimenter then randomly selects whose unconditional choice will be implemented and whose corresponding conditional choice will be implemented.

The main point is that when using the strategy method each participant states her choices for all possible contingencies that can occur. This enables the experimenter to study the effect of rare social stimuli. Moreover note that the conditional choices are not "hypothetical" for the participants, because the participants do not know ex ante which of their conditional choices will "take place" – because this depends on the simultaneously made decision by the other participant.

Strategy method is sometimes criticized because making decisions for all possible contingencies (as is the case in this method) seems to be a more "deliberate" process than just reacting to one event when it really happens. The evidence on comparability of the "cold" strategy method and the "hot" direct response method is not completely conclusive (see e.g. Cason & Mui, 1998; Brandts & Charness, 2000, 2011).

Whenever applicable, the strategy method might be preferable to simply eliciting hypothetical choices (which would be a third alternative to deception). The reason is that hypothetical choices are not incentive-compatible and might be therefore more prone to biased responding (Hertwig & Ortmann, 2001; see also Bodner & Prelec, 2003).

Another interesting alternative experimental protocol was introduced by Bardsley (2000), see also e.g. Bardsley & Sausgruber (2005), Skatova & Ferguson (2011), and Chapter 6 of this thesis. In Bardsley's (2000) experiment participants played 29 games with fictional opponents and one game with real opponents without knowing which one is the "real" one – but knowing that only one is real. This enabled the experimenter exogenous manipulation of the independent variable (the level of others' cooperation), while avoiding deception. Only the game against the real opponent was relevant for earnings from the experiment. (You can notice the similarity with the strategy method.)

4. WHY NOT TO USE DECEPTION

Now, the question is why should we as psychologists avoid experimental deception as do economists? The answer is not that deception would harm the participants. This is usually not the problem, or rather Institutional Review Boards deal with this issue (Sell, 2008). The main reason is that deception could harm other researchers. What do I mean by that?

For simplicity, let us assume that when participants learn that they have been deceived in an experiment, they do not mind[48]. However, when they participate in a next study, they may no longer have faith that they are told the truth by the experimenter (Hey, 1998; Ledyard, 1995; McDaniel & Starmer, 1998; Hertwig & Ortmann, 2001). It is reasonable to think that the more often they encounter or hear about experimental deception, the less faith they will have in the truthfulness of the experimental instructions (cf. Barrera & Simpson, 2012). Such a

[48] In reality, many participants do not mind but many *do* (Ortmann & Hertwig, 2002; Blatchley & O'Brien, 2007).

pollution of the subject pool can be devastating for certain types of experimental research. Two examples follow.

When taking part in new experiments, previously deceived subjects may for example no longer believe that they interact with other real people (the interactions in the lab are, for good reasons, typically not face to face). This would make experimental investigation of motives such as fairness, altruism or envy difficult, because these motives only or mostly arise in interactions with other humans, not with e.g. computer-simulated agents (see Rilling et al., 2004).

To use another example, previously or potentially deceived subjects may lose faith in that their decisions are really anonymous, which is crucial in many experiments (e.g. Hoffman et al., 1994; Shalvi et al., 2011a), and they might start thinking they are monitored, which could alter their behavior.

5. MORE RESEARCH IS NEEDED

Experimenters across disciplines should acknowledge the risks of using experimental deception. On the other hand, the magnitude of these risks ought to be empirically assessed. It could turn out that their magnitude is not large (see Barrera & Simpson, 2012). In that case mild experimental deception can be (more) acceptable.

Comparisons of participation rates, behaviors and beliefs in new experiment(s) measured in participants who were vs. who were not previously experimentally deceived could provide the basis for an informed choice concerning experimental deception. Since experimental

deception is, at the moment, rarely manipulated as an independent variable (but see Jamison et al., 2008; Barrera & Simpson, 2012), evidence on what effect it has on participants in subsequent experiments is far from conclusive.

We should be careful when interpreting results apparently showing no detrimental effects of deception. No effects could also mean that experimenters' default reputation for honesty is poor to begin with and that it cannot get much worse with one additional deception (Alberti & Güth, 2013 make a similar argument). Ironically, the labs and experimenters in disciplines, such as economics, that (are known to) never use experimental deception are in this respect better equipped to test the effects of deception. Psychologists will have to find a work-around[49].

Overall, existing evidence on the effects of experimental deception is mixed and fairly scarce (see e.g. Allen, 1983; Jamison et al., 2008; Barrera & Simpson, 2012; for reviews see Bonetti, 1998; Ortmann & Hertwig, 2002; Hertwig & Ortmann, 2008). Also, the evidence is limited in scope: for example, there are very few studies on the effect of experimental deception on decision making in strategic games (Allen, 1983; Jamison et al., 2008; Barrera & Simpson, 2012; Alberti & Güth, 2013). More research is needed.

[49] One possibility would be to boost participants' trust in the (in fact deceptive) experimental situation to ensure that the "surprise" when deception is revealed is salient. For example, a video feed from the room where one's computer-networked partners (or ostensible partners, in the experimental condition) are seated could be shown to participants. The video would be real in the control condition, but pre-recorded in the experimental condition where participants would actually play against computers.

6. CONCLUSION

This chapter summarizes the benefits and costs that need to be considered when deciding whether experimental deception should be accepted or rejected. The experimental economics tradition of not using deception seems, intuitively, like the safest option also for experimenters in psychology. There is good chance that deceptive designs erode subjects' trust in experimenters and, in turn, experimenters' ability to control subjects' beliefs and run controlled experiments. However, further empirical investigations can be performed to put this intuition to the test.

CHAPTER 4: TRANSFER LEARNING IN THE PRISONER'S DILEMMA[50]

Abstract

The aim of the experimental study reported here is to examine the existence of an effect of learning when initial and further encounters with undisclosed predetermined strategies are compared. This is one of the first systematic empirical studies of inter-game learning in the prisoner's dilemma game. Effect of learning in terms of higher gains in repeated encounters was found for the whole set of five strategies taken together (Tit for Tat, Deterrent, Bully, Benevolent and Random), and individually for three of the strategies employed (Tit for Tat, Deterrent, and Bully).

[50] This chapter is based in part on Vesely (2012) and on Chapter 10 of my diploma thesis (Vesely, 2011). However, the entire chapter is thoroughly revised, over 30 new papers are discussed in it, and the data are re-analyzed.

1. INTRODUCTION

1.1 THE GAME

Prisoner's dilemma (PD) is a game for two players who each have a choice whether to "cooperate", or "defect". The outcome of the decision of player A (whether to cooperate, or defect) is determined by the decision of player B (whether to cooperate, or defect), and vice versa. The decision of the other player is not known beforehand to his opponent (i.e., in the standard version of PD both players decide simultaneously).

At the end of one round of the game, each player ends up with precisely one of the following four possible outcomes (denoted here as *R*, *P*, *S* and *T*). In experiments, the outcomes often take the form of different amounts of money or points. The four possible outcomes in PD are as follows: when both players cooperate, both get *R* (or the reward for mutual cooperation), when both players defect, both get *P* (punishment for mutual defection). When player A (B) cooperates and player B (A) defects, player A (B) gets *S* (or sucker's payoff), while player B (A) gets *T* (temptation to defect). The preference ranking for the four outcomes from highest to lowest reward is $T > R > P > S$. It is also assumed that $2R > (T + S)$, or that the reward for mutual cooperation is greater than the average of temptation and the sucker's payoff (so as the two players cannot gain more by taking turns in one-sided defection than by mutual cooperation), see e.g. Ahn et al. (2001), Axelrod (2006, pp. 8-10).

1.2 RESEARCH OBJECTIVE

The task of this study is to test whether subjects play the PD game more successfully (i.e. receive higher gains) as they encounter certain strategies repeatedly and thus have the

opportunity to learn through experience how to react more successfully to them[51]. When playing against an unknown opponent, *adapting* to his or her strategy is usually preferable to playing a predetermined stationary strategy (Fudenberg & Levine, 1998; Airiau et al., 2007). I will test whether subjects *adapt* more successfully to their "opponents" (the five strategies I employed) as they learn more about them (i.e. in repeated encounters).

The present experiment is rather exploratory in nature, as there is no similar research testing for inter-game learning against different strategies in PD (next subsection reviews the existing literature). Because PD is a strategic environment of great interest to social scientists and because learning in strategic settings has been drawing increased attention, it makes sense to attempt to study learning in PD. More specifically, I want to see against which strategies people improve their performance when they encounter them repeatedly (this is my research objective in this chapter). I use five strategies, which are mostly variations of the well-known tit for tat[52].

It should be pointed out that the present research does *not* attempt to study the mechanism underlying learning in PD, it only studies whether learning will take place when repeatedly playing against each of a prespecified set of strategies. Subsequent research should, however, concentrate also on the underlying mechanism. Subsequent research should also use a wider and/or different set of strategies to complement findings from the current study.

[51] Specific research questions and hypotheses formulated to address this objective will be given in section 2.2.3.2.

[52] One strategy is the tit for tat itself, another one is a "less forgiving" tit for tat, the third one is a "more forgiving" tit for tat, the fourth is a "nasty, bullying" tit for tat, and the fifth one plays randomly. The strategies are described in detail in section 2.3.

Economists recently developed a large variety of learning models and fitted them to behavior in various iterated games (e.g. Roth & Erev, 1995; Stahl, 1996, 2000; Camerer & Ho, 1999; Bednar & Page, 2007; Arifovic & Ledyard, 2012; Haruvy & Stahl, 2012). This paper takes a different route by testing for the presence of learning in repeated play against prespecified strategies. However, it seems like an interesting possibility for future research to study how, in a given type of game (e.g., PD), the fit of a given learning model varies with "encountered strategy" (e.g., players might weigh some information comparatively more when playing against a particular strategy). Such an approach might be able to uncover some of the underlying mechanisms of learning in strategic settings.

1.3 FORMER RESEARCH ON INTER-GAME LEARNING

Even though PD has been studied for decades, systematic research concerning learning in repeated encounters with a given strategy, but *not* within a single iterated game, is virtually nonexistent. Most closely related to my study are experiments by Bereby-Meyer & Roth (2006) and Bruttel et al. (2012). In Bereby-Meyer & Roth (2006) players learn to cooperate in iterated (10-shot) PD games when playing such games repeatedly against other participants. More precisely, cooperation in period 1 of the 10-shot game increases from 61% to 93% between the first and last (20th) 10-shot supergame. Players also gradually learn to defect more near the end of each 10-shot supergame: cooperation in period 10 dropped from 22% to 7% between the first and last supergame. Similarly, Bruttel et al. (2012) find inter-game effect of learning in PD when human subjects are randomly rematched after each iterated game. These two papers indicate there is learning across iterated PD games. However, as these two experiments used human opponents, one cannot make any conclusions about the specific type

of opponent strategy against which people are able to improve through learning (in contrast, one will be able to draw such conclusions from the present experiment).

More distantly related to my experiment are studies on inter-game learning across more or less dissimilar games (for example studies on inter-game transfer between social dilemmas *and* coordination games – Knez & Camerer, 2000; Ahn et al., 2001; Bednar et al., 2012; Duffy & Fehrz, 2014). Such studies, however, differ from the present investigation especially in that they (1) do not focus solely on learning across iterated games of the *same* type (e.g., PD), but rather across games of different albeit typically not too dissimilar types, (2) they do not use predetermined strategies, but rather observe interactions between players. Nevertheless the literature on transfer learning across (somewhat dissimilar) games remains relevant in that it represents a wider context for my experiment.

The observation repeatedly reported in the literature on transfer learning across games (e.g. Knez & Camerer, 2000; Bednar et al., 2012; Grimm & Mengel, 2012) that people are able to gain experience in one game (say, how to coordinate in a Stag Hunt game) and transfer this experience to another more or less dissimilar game (say, how to cooperate in PD) supports my conjecture that *people might be able to learn how to play against a certain strategy in PD* (say, how to play effectively against the tit for tat strategy, cf. Axelrod, 2006) *and subsequently show better performance against this particular strategy when they encounter it later in the experiment*. (It is this type of conjecture that the present experiment explores.)

The transfer learning literature provides evidence, for example, of transfer learning across different 2x2 games (such as PD, see Bednar et al., 2012), across different 3x3 games (Grimm & Mengel, 2012), and across different 4x4 games (Haruvy & Stahl, 2012). Knez (1998) finds

transfer of playing the equal-payoff equilibrium from a 2-player conflict-of-interest game to a 3-player conflict-of-interest game. Helbing et al. (2005) find effect of learning in their experiments with The Route Choice Game: subjects who previously discovered that oscillatory cooperation with opponents brought them highest gains were able to apply this rule quickly to new games. Knez & Camerer (2000) report that previous efficient choices in weak-link coordination games tend to increase cooperation in subsequent PD games (Ahn et al., 2001 report similar results; Duffy & Fehrz, 2014, in contrast, report no significant transfer learning in either direction between PD and the Stag Hunt game)[53].

As previously pointed out, all these (mostly positive) positive findings of transfer between games seem to broadly support my expectation of improved play against at least some strategies in PD when these strategies are encountered repeatedly (specifically, when after a previous experience with a given strategy this strategy is later encountered in new iterated PD games).

However, I do not expect there will be transfer learning against all possible strategies in PD. On a priori grounds, it is difficult to tell against which strategies players will be able to improve. One of the objectives of the present experiment is to provide some initial insights

[53] Other examples include Cherry et al. (2003) who show that once people learn to make utility maximizing choices in a market-like environment, they start to make utility maximizing choices also in similar non-market settings. In their follow-up study, Cherry & Shogren (2007) establish such transfer effects also across more dissimilar environments. Gneezy et al. (2010) and Dufwenberg et al. (2010) find transfer learning in terms of using the dominant strategy between two versions of the Race game. Van Huyck et al. (1990) report some evidence of transfer learning in coordination games: inexperienced players are less able to coordinate on the payoff-dominant action in a game which also offers a "secure" option (i.e. in a game with two possible "focal points") than players with previous experience with another coordination game. Rick & Weber (2010, Experiment 1) report there is feedback-free transfer learning in dominance solvable normal form games. Players learn to stop violating the first two steps of iterated dominance and, which is the main point, they transfer this rule to similar but not identical games. In Rick & Weber (2010, Experiment 2) players who played a guessing game without feedback subsequently deviated less from Nash equilibrium in a second similar but not identical guessing game (see also Chapter 5 in this work).

concerning the type of strategies against which players in fact improve (I employ five different strategies in the experiment).

My expectation that participants will improve their performance against only some of the strategies is broadly in line with some results from the transfer learning literature (keeping in mind the abovementioned differences between my study and studies in this literature[54]). For example, unlike Rick & Weber (2010), Costa-Gomes & Crawford (2006) found no evidence of transfer learning across two-person guessing games played without feedback. Silverstein et al. (1998) found no significant transfer (in terms of cooperativeness) from an initial iterated PD game against a certain strategy to a new round of iterated games against real subjects (who previously also played against one of the strategies employed and also showed no transfer). Esposito et al. (2012) who studied spillovers between similar, but not identical weighed voting games also found no transfer learning[55].

[54] Specifically, former studies on transfer learning across games which I cite here (1) do not focus solely on learning across iterated games of the *same* type (e.g., PD), but rather across games of different types, and (2) they do not investigate changes in performance against *predetermined strategies*.

[55] Participants are sometimes able to transfer learning from environment A to environment B, but not the other way round: Kagel (1995) found that experience in first-price, sealed bid auctions led to marginally more profitable bidding in subsequent English auctions, but experience in English auctions did not improve performance in subsequent first-price auctions. In Sausgruber & Tyran's (2005) experiment subjects made better decisions when voting on a proposal to tax market transactions and redistribute tax revenues in an indirect tax regime if they previously voted in a direct tax regime. However, there was no evidence of transfer learning in the opposite direction, i.e. from the indirect to the direct tax regime. In Cason et al. (2012) successful coordination on the Pareto optimal equilibrium in a median effort coordination game led to more efficient play in subsequent weakest link coordination game (for a similar result see Devetag, 2005). Whereas when the game sequence was reversed, no transfer from the weakest link game to the median effort game occurred. Savikhin & Sheremeta (2013) report no transfer from a competitive situation (bidding contest) to a cooperative situation (public goods game) when decisions in these two settings are made in parallel. They find transfer learning in the opposite direction: parallel participation in the public goods game reduces overbidding in the contest (for related studies see Bednar & Page, 2007 and Cason & Gangadharan, 2013). Finally, Cooper & Kagel (2005) find transfer learning between two versions of the limit pricing game (Milgrom & Roberts, 1982) in case of two-player teams. In contrast, when (different) participants play the games individually, those participants with previous experience with one version of the game play less successfully in the new version of the game than inexperienced participants. But when participants play individually in a meaningful framework (using labels such as "low cost firm"), there is again transfer learning between the two versions of the game (see Cooper & Kagel, 2008).

2. METHOD

2.1 SUBJECTS

A convenience sample of 45 subjects participated in this study (18 men and 27 women). Their age ranged from 20 to 56 years (mean = 25.1 years, SD = 6.7, median = 24 years). Subjects were either university graduates or students. They had not prior experience with PD, usually with little or none previous formal knowledge of game theory (only six of the subjects admitted ever playing PD before, none of them extensively).

2.2 DESIGN AND PROCEDURES

Each subject played precisely fifteen iterated PD games. The games varied with respect to two factors: their length ("Length") and the type of predetermined opponent strategy the subject faced in a given game ("Strategy").

The Strategy factor has five levels (five different strategies: Tit for Tat, Random, Benevolent, Deterrent, Bully) and the Length factor has three levels (Closed, Semiclosed, Open). Crossing the two factors produces fifteen different "conditions" each participant encountered. They are listed in Table 1.

Table 1: Fifteen different PD games each participant played

Game no.	Game description
1	Closed Tit for Tat
2	Closed Random
3	Closed Benevolent
4	Closed Deterrent
5	Closed Bully
6	Semiclosed Tit for Tat
7	Semiclosed Random

8	Semiclosed Benevolent
9	Semiclosed Deterrent
10	Semiclosed Bully
11	Open Tit for Tat
12	Open Random
13	Open Benevolent
14	Open Deterrent
15	Open Bully

I balanced the order of presentation of the 15 games listed in Table 1 using a Latin square constructed with help of Bradley's (1958) algorithm. In this way I obtained the fifteen different orderings (A, B, C, …, O) of the fifteen games (1, 2, 3, …, 15), which are displayed in Table 2. The first subject encountered game order "A" (see Table 2), the second subject encountered game order B, and so on (the 16th participant, again, encountered game order "A", the 17th participant encountered game order "B", and so on).

Table 2: Latin square of game orders used in the experiment

Game order	Games (see Table 1 in which the games are described)														
A	1	2	15	3	14	4	13	5	12	6	11	7	10	8	9
B	2	3	1	4	15	5	14	6	13	7	12	8	11	9	10
C	3	4	2	5	1	6	15	7	14	8	13	9	12	10	11
D	4	5	3	6	2	7	1	8	15	9	14	10	13	11	12
E	5	6	4	7	3	8	2	9	1	10	15	11	14	12	13
F	6	7	5	8	4	9	3	10	2	11	1	12	15	13	14
G	7	8	6	9	5	10	4	11	3	12	2	13	1	14	15
H	8	9	7	10	6	11	5	12	4	13	3	14	2	15	1
I	9	10	8	11	7	12	6	13	5	14	4	15	3	1	2
J	10	11	9	12	8	13	7	14	6	15	5	1	4	2	3
K	11	12	10	13	9	14	8	15	7	1	6	2	5	3	4
L	12	13	11	14	10	15	9	1	8	2	7	3	6	4	5
M	13	14	12	15	11	1	10	2	9	3	8	4	7	5	6
N	14	15	13	1	12	2	11	3	10	4	9	5	8	6	7
O	15	1	14	2	13	3	12	4	11	5	10	6	9	7	8

It is important to note that *only the Strategy factor* is relevant for the present study, and I will "disregard" the Length factor in the main analyses. Before proceeding to the description of the

features of the experiment most relevant for this study, in the next subsection I first explain what I mean by "disregarding" Length in the main analyses.

2.2.1 GAME LENGTH

In both Closed, Semiclosed and Open games at least 30 trials are played in each iterated game[56], and for each condition 30 trials are registered. In Closed games players are told the game's duration is 30 moves (which is the actual duration of the game). In Semiclosed games players are told the game is about to last for 25-35 trials (the game was in fact randomly ended between moves 30-35[57]). In Open games subjects are not given information about the length of the game (the game was in fact randomly ended between moves 30-40[58]). Behaviorally, it is characteristic that end-game effects set in only towards the end of the game (in the last few trials) even in games with known finite end (i.e. "Closed" games in the present study, see e.g. Rapoport & Dale, 1966)[59]. In the main analyses in this chapter I will analyze only data from rounds 1-25 of each iterated game, thus discarding data that might be (most) affected by end-game effects. Since game behavior in rounds 1-25 across the three levels of Length can be reasonably expected to be very similar (Rapoport & Dale, 1966), I will not

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[56] At least 30 rounds are used in each game so as participants have sufficient experience with a given strategy in each game. This is important so as participants can to try out and figure out appropriate responses to specific strategies. While different authors have used a large variety of PD game durations in the past, even with the benefit of hindsight I feel like using games of about 20-30 rounds is a good choice for the present type of experiment. Of course, future experiments can use different game durations to check the robustness of my findings. It would be for example interesting to see what are the shortest games in which the type of learning I study here can occur. One can expect the "shortest game" to be different for different strategies – exploring this issue might be able to tell us something about the underlying mechanisms of learning across games.

[57] Between rounds 30 and 34 the probability of ending the game after a given round was 25%; after round 35 the game was ended for sure if it has not been already ended. The ending was determined for each participant prior to his/her session using a random numbers generator.

[58] Between rounds 30 and 39 the probability of ending the game after a given round was 25%; after round 40 the game was ended for sure if it has not been already ended. The ending was determined for each participant prior to his/her session using a random numbers generator.

[59] Backward induction (see e.g. Luce & Raiffa, 1957) has notoriously little bite in iterated PD with known finite end. I am not aware of any study showing backward induction can explain behavior in iterated PD with known finite end.

consider game Length in the main analyses. However, in section 3.1, I will, in addition, report regression analyses which control for Length and show that this factor indeed does not affect play in rounds 1-25 on which the main analyses are based.

2.2.2 STRATEGIES AGAINST WHICH PARTICIPANTS PLAY

Each subject will encounter five different strategies (Tit for Tat, Random, Benevolent, Deterrent, and Bully) precisely three times each (as is clear from Tables 1 and 2). The five strategies employed cover a relatively wide scope of possible strategic behavior in PD. However, they do not represent *all* possible or even all "typical" behaviors of possible opponents, and a modified set of strategies could have been used instead (I recommend that future research uses a modified set of strategies to complement the present study). Except for Random (see e.g. McClintock et al., 1963 who study this and similar strategies), the strategies are identical to or broadly based on the well-known tit for tat strategy (see e.g. Sermat, 1967; Oskamp, 1971; a variety of related strategies is described e.g. in Axelrod, 2006). I next describe each strategy in turn.

In the first move, Tit for Tat starts with cooperation. Tit for Tat responds to an opponent's cooperation in the *n*-th move by cooperation in the next move, while it responds to an opponent's defection in the *n*-th move by defection in the next move.

Random strategy chooses randomly between cooperation and defection with 50% probability in each round[60].

[60] Random strategy's moves were determined for each participant prior to his/her session.

In the first move, Benevolent starts with cooperation. Benevolent responds to an opponent's cooperation in the *n*-th move by cooperation in the next move, while it responds only to an opponent's third defection by defection in the next move, and afterwards it responds to every other opponent's defection by defection in the next move[61]. In all other cases, Benevolent cooperates[62].

In the first move, Deterrent starts with cooperation. Deterrent responds to an opponent's cooperation in the *n*-th move by cooperation in the next move and it responds to an opponent's defection in the *n*-th move by defection in the next move. But if its opponent defects right after mutual cooperation, Deterrent responds to it by two consecutive defections in the next two moves irrespective of what the other player does in those moves[63]. If at any future point its opponent defects again right after another mutual cooperation, Deterrent responds to this by three consecutive defections in the next three moves (irrespective of what the other player does in those moves). If at any future point its opponent defects again right after another mutual cooperation, Deterrent responds to it by four consecutive defections (irrespective of what the other player does in those moves). If at any future point its opponent defects again right after another mutual cooperation, Deterrent responds to it by five consecutive defections, etc. In all other cases Deterrent cooperates.

In the first move, Bully starts with cooperation. Bully responds to an opponent's defection in the *n*-th move by defection in the next move, and it responds to every third opponent's

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[61] This is why the strategy is called Benevolent (it lets more defections slide than the Tit for Tat).
[62] Benevolent thus responds to the *first* and *second* defection by an opponent by cooperation; subsequently, it responds to the *third* defection by defection, it responds to the *fourth* defection by cooperation, it responds to the *fifth* defection by defection, it responds to the *sixth* defection by cooperation, and so on.
[63] This is why the strategy is called Deterrent (this strategy will "deter" defection after mutual cooperation even more severely if such a defection takes place repeatedly, as described in the text).

cooperation by defection in the next move[64], otherwise it responds to an opponent's cooperation in the *n*-th move by cooperation in the next move.

2.2.3 HOW IS LEARNING STUDIED

The presence of learning is assessed by performing pairwise comparisons with respect to performance against a given strategy[65] at three points in time. Specifically, I compare the first vs. the second, the first vs. the third, and the second vs. the third iterated game against a given strategy. The analysis is based on subject totals (total gains in points) from rounds 1-25 of each iterated game. The specific hypotheses are outlined in subsection 2.2.3.2. The performance measures which are used to test the hypotheses are described in the next subsection (2.2.3.1).

2.2.3.1 MEASURES

The independent variable is whether the subject meets with the given strategy for the first, second, or third time.

The depended variable is performance measured by points obtained in the game. *The analyses will be based on subject totals (total gains in points) from rounds 1-25 of each iterated game*. Point values of the four possible outcomes in the game[66] are set as follows: $T = 5$, $R = 3$, $P = 1$, $S = 0$). Throughout this chapter, I compare performance (gains in points) in moves 1-25 only, to eliminate the influence of possible end-game effects, as discussed in section 2.2.1. The specific measures that will be compared to test my hypotheses are given in Table 3.

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[64] This is why the strategy is called Bully.
[65] Or set of strategies in case of hypotheses H16-H18.
[66] *T*, *R*, *P*, *S* – see description of the game in section 1.1.

Table 3: Measures used in hypothesis testing

Measure's code	Measure
"1"	Subject's gain in the first encounter with Tit for Tat[67]
"2"	Subject's gain in the second encounter with Tit for Tat
"3"	Subject's gain in the third encounter with Tit for Tat
"4"	Subject's gain in the first encounter with Random
"5"	Subject's gain in the second encounter with Random
"6"	Subject's gain in the third encounter with Random
"7"	Subject's gain in the first encounter with Benevolent
"8"	Subject's gain in the second encounter with Benevolent
"9"	Subject's gain in the third encounter with Benevolent
"10"	Subject's gain in the first encounter with Deterrent
"11"	Subject's gain in the second encounter with Deterrent
"12"	Subject's gain in the third encounter with Deterrent
"13"	Subject's gain in the first encounter with Bully
"14"	Subject's gain in the second encounter with Bully
"15"	Subject's gain in the third encounter with Bully
"16"	Subject's gain in the first encounter with all of the five strategies
"17"	Subject's gain in the second encounter with all of the five strategies
"18"	Subject's gain in the third encounter with all of the five strategies

The measures will be represented in the text by their code number given in Table 3 in quotation marks ("1", "2", etc.).

2.2.3.2 HYPOTHESES

My research questions can be formulated as follows: In the PD game, will subjects have higher gains in points in latter encounters with a particular Strategy than in previous encounters with that Strategy[68]? For which Strategies will the answer to the previous question be positive and for which Strategies will it be negative? To study these questions, I formulated 18 hypotheses (H1-H18) listed in Table 4.

[67] In all cases only rounds 1-25 are considered.
[68] Rounds 1-25 of an iterated game constitute one "encounter" with a Strategy.

Hypotheses H1-H15 test for the presence of learning in terms of improved play (higher gains) when comparing a latter encounter with a given strategy with an earlier encounter with that strategy.

To test hypotheses H16-H18, I sum within participant his/her gains from play against all five strategies when a given strategy is encountered for the first time (measure "16" in Tables 3 and 4)[69]; I do the same when a given strategy is encountered for the second time (measure "17" in Tables 3 and 4)[70], and I do the same when a given strategy is encountered for the third and last time (measure "18" in Tables 3 and 4)[71]. I then perform pairwise comparisons between measures "16", "17" and "18" to test H16-H18.

Table 4: Hypotheses H1-H18

No.	Opponent strategy	Encounters compared	Hypothesis
H1	All five strategies together	Third and first	"18" > "16"
H2	All five strategies together	Third and second	"18" > "17"
H3	All five strategies together	Second and first	"17" > "16"
H4	Tit for Tat	Third and first	"3" > "1"
H5	Tit for Tat	Third and second	"3" > "2"
H6	Tit for Tat	Second and first	"2" > "1"
H7	Random	Third and first	"6" > "4"
H8	Random	Third and second	"6" > "5"
H9	Random	Second and first	"5" > "4"
H10	Benevolent	Third and first	"9" > "7"
H11	Benevolent	Third and second	"9" > "8"
H12	Benevolent	Second and first	"8" > "7"
H13	Deterrent	Third and first	"12" > "10"
H14	Deterrent	Third and second	"12" > "11"
H15	Deterrent	Second and first	"11" > "10"
H16	Bully	Third and first	"15" > "13"
H17	Bully	Third and second	"15" > "14"
H18	Bully	Second and first	"14" > "13"

[69] I.e., I sum gains from the first encounter with Tit for Tat, Random, Benevolent, Deterrent, and Bully.
[70] I.e., I sum gains from the second encounter with Tit for Tat, Random, Benevolent, Deterrent, and Bully.
[71] I.e., I sum gains from the third encounter with Tit for Tat, Random, Benevolent, Deterrent, and Bully.

The descriptions of each measure "1", "2", …, "18" used to formulate the hypotheses in Table 4 are given in Table 3 above. Each respective null hypothesis posits no difference between sample averages of measures given in the fourth column of Table 4. For example, null hypothesis H4-null would read: "Subjects' gains in the third and first encounter with Tit for Tat do not differ, on average."

In hypotheses H1-H18 I test for the presence of learning *without postulating a specific learning mechanism*. Several possible learning mechanisms have been proposed in the past (e.g. Roth & Erev, 1995; Stahl, 1996; Camerer & Ho, 1999) and it is possible than any of them or a combination of these (or yet a different mechanism) can account for learning in the setting I study. However, as noted in the Introduction, it is not the purpose of this study to uncover a specific learning mechanism or to distinguish between possible learning mechanisms. The study was not designed for this purpose. At the same time, it is possible to test for the presence of learning while remaining agnostic about possible underlying mechanisms, which is precisely what I do here.

2.2.4 PROCEDURES

To make things as clear and natural to the subjects as possible, I told them it would be as if they played against several different players represented in the actual experimental setting by the experimenter. (It was obvious to the participants they are not going to encounter various real opponents, they understood that the experimenter, with whom they actually played/interacted, would be "representing" or "mimicking" different "players" or strategies.) The choices were labeled "cooperate" and "defect". These labels were written on cards that

the subjects showed in each trial, while the experimenter showed his choices (also "cooperate" and "defect") on similar cards. Each participant played 15 iterated games, as explained above. Participants were not told anything about any of the strategies beforehand.

Subjects were advised beforehand that it is not their task to defeat the experimenter, but to gain as many points as possible[72]. Each subject played with the experimenter individually (there was always one subject per session). Subjects were also informed that there will be other participants in exactly the same game situation, and that they should try to gain more points than the other participants (again, as opposed to gaining more points than the experimenter). These suggestions most probably tended to increase subjects' cooperation.

Instructing participants to gain as many points as possible is important because I test the hypotheses by comparing the number of points accumulated in the games under comparison (as explained in section 2.2.3.2). Unfortunately, I was not able to use material incentives to further increase participants' motivation to gain as many points as possible, as would be standard practice in experimental economics (Hertwig & Ortmann, 2001). A replication of the experiment using material incentives would therefore be welcome. It is quite possible that incentivizing the decisions would lead to better learning (Bereby-Meyer & Roth, 2006 found that the type of incentives employed affects learning in PD). Thus, the present experiment provides what might well be a lower bound of learning effects in PD.

[72] This instruction makes the task more comparable across subjects (if they were not told this, some of them would perhaps try to defeat the experimenter, while others would perhaps try to earn as many points as possible, whereas others might choose yet a different approach).

After the initial instructions and a careful, detailed explanation of the rules of the game (with comprehensive examples of possible combinations of moves and their outcomes), the experimenter did not comment in any way on the game interactions.

3. RESULTS

I test the hypotheses listed in Table 4 with dependent *t*-tests for repeated measures. To control for false positives I apply the Bonferroni correction, i.e. the alpha cut-off is set at .017 in the following analyses. In some sense, setting alpha at .017 is liberal. If one wanted to strictly control the experiment-wise error rate at the 5% level, one would have to set alpha for the individual comparisons at .0028.

I support hypothesis H1 that "18" > "16", $p < .001$ (see Table 5, row H1). I support hypothesis H2 that "18" > "17", $p = .015$ (see Table 5, row H2). I also support hypothesis H3 that "17" > "16", $p < .001$ (see Table 5, row H3)[73]. These tests show there is an effect of learning when third encounters with all strategies taken together were compared with first (H1), as well as with second encounters with all strategies taken together (H2). I also found an effect of learning when second encounters with all strategies taken together were compared with first encounters with all strategies taken together (H3)[74].

The effect sizes in case of H1-H3 tests are substantial (medium to large). In particular, there is a strong effect of learning when comparing the first and the second encounter with all strategies ($r = .60$), and a medium effect when comparing the second and the third encounter

[73] Discussion of the results is based on reported *t*-tests. As a robustness check, Table 5 also reports *p*-values obtained from Wilcoxon signed-rank (WSR) tests. In all cases, the parametric and non-parametric tests of H1-H18 lead to the same conclusions.

[74] Rounds 1-25 of an iterated game constitute one "encounter" with a Strategy.

(r = .36). This is consistent with the law of effect (Thorndike, 1927; Herrnstein, 1970; Roth & Erev, 1995). Understandably, the effect of learning is strongest when comparing the first and the third encounter, because the previous two effects "add up" (r = .71). Note that this is also generally the case when looking at the strategies individually (effect sizes are given in the sixth column of Table 5,).

Table 5: Results of tests of hypotheses H1-H18 (the entire sample, n = 45)

Hypothesis	Variable	Mean (SD)	Mean difference ($CI_{95\%}$)	t	r (effect size)	p (t-test)	p (WSR test)
H1 ("18" > "16")	"18"	335.82 (11.63)	18.27 (12.72, 23.81)	6.636	.71	.000*	.000*
	"16"	317.62 (13.53)					
H2 ("18" > "17")	"18"	335.82 (11.63)	6.13 (1.23, 11.03)	2.522	.36	.015*	.007*
	"17"	329.69 (10.80)					
H3 ("17" > "16")	"17"	329.69 (10.80)	12.13 (7.25, 17.02)	5.006	.60	.000*	.000*
	"16"	317.62 (13.53)					
H4 ("3" > "1")	"3"	73.89 (1.48)	4.04 (1.97, 6.12)	3.929	.51	.000*	.000*
	"1"	69.84 (6.90)					
H5 ("3" > "2")	"3"	73.89 (1.48)	1.13 (0.31, 1.96)	2.779	.39	.008*	.011*
	"2"	72.76 (2.92)					
H6 ("2" > "1")	"2"	72.76 (2.92)	2.91 (1.00, 4.83)	3.062	.42	.004*	.006*
	"1"	69.84 (6.90)					
H7 ("6" > "4")	"6"	60.62 (9.35)	3.67 (-0.08, 7.42)	1.970	.28	.055	.048
	"4"	56.96 (8.22)					
H8 ("6" > "5")	"6"	60.62 (9.35)	0.29 (-3.76, 4.34)	0.144	.02	.886	.624
	"5"	60.33 (8.33)					
H9 ("5" > "4")	"5"	60.33 (8.33)	3.38 (-0.21, 6.97)	1.896	.27	.065	.031
	"4"	56.96 (8.22)					
H10 ("9" > "7")	"9"	80.33 (3.42)	1.27 (-0.03, 2.56)	1.972	.28	.055	.040
	"7"	79.07 (3.06)					
H11 ("9" > "8")	"9"	80.33 (3.42)	0.82 (-0.24, 1.88)	1.565	.23	.125	.116
	"8"	79.51 (3.14)					
H12 ("8" > "7")	"8"	79.51 (3.14)	0.44 (-0.76, 1.65)	0.745	.11	.460	.343
	"7"	79.07 (3.06)					
H13 ("12" > "10")	"12"	71.93 (2.60)	4.36 (2.32, 6.39)	4.316	.55	.000*	.000*
	"10"	67.58 (6.57)					
H14 ("12" >	"12"	71.93 (2.60)	1.11 (-0.57,	1.335	.20	.189	.380

"11")	"11"	70.82 (5.26)	2.79)				
H15 ("11" >	"11"	70.82 (5.26)	3.24 (0.75, 5.73)	2.626	.37	.012*	.002*
"10")	"10"	67.58 (6.57)					
H16 ("15" >	"15"	49.04 (4.06)	4.93 (3.13, 6.74)	5.392	.63	.000*	.000*
"13")	"13"	44.18 (5.31)					
H17 ("15" >	"15"	49.04 (4.06)	2.78 (1.26, 4.29)	3.694	.49	.000*	.001*
"14")	"14"	46.27 (4.80)					
H18 ("14" >	"14"	46.27 (4.80)	2.16 (0.28, 4.03)	2.240	.32	.030	.041
"13")	"13")	44.18 (5.31)					

Notes: $N = 45$ in all cases ($df = 44$ in all cases). The column "Mean difference" indicates the mean difference in values of variables included in the respective hypothesis test (and the 95% confidence interval of the difference). * $p < .017$.

When I tested for an effect of learning in encounters with specific strategies (hypotheses H4-H18, detailed results are given in Table 5 and below in the text), the results were as follows: I found an effect of learning in encounters with Tit for Tat, Deterrent and Bully, I found no statistically significant effect of learning in encounters with Benevolent and Random.

Tit for Tat:

With dependent *t*-test for repeated measures I confirmed hypothesis H4 that "3" > "1", $p < .001$, hypothesis H5 that "3" > "2", $p < .017$, and hypothesis H6 that "2" > "1", $p < .017$ (see Table 5, rows H4-H6 for details). The effect sizes are substantial (medium to large).

That means I found an effect of learning when the third encounter with Tit for Tat was compared with the first, as well as with the second encounter with Tit for Tat. I also found an effect of learning when the second encounter with Tit for Tat was compared with the first encounter with this strategy[75].

[75] Rounds 1-25 of an iterated game constitute one "encounter" with a Strategy.

Random:

With dependent *t*-test for repeated measures I did not find statistically significant differences between performance in the third ("6") and the first ("4"), the third ("6") and the second ("5"), or the second ("5") and the first ("4") encounters with Random strategy (see Table 5, rows H7-H9 for details). That means I found no evidence for an effect of learning when Random strategy was encountered repeatedly: hypotheses H7-H9 were not supported (the respective null hypotheses could not be rejected).

Benevolent:

With dependent *t*-test for repeated measures I did not find statistically significant differences between performance in the third ("9") and the first ("7"), the third ("9") and the second ("8"), or the second ("8") and the first ("7") encounters with Benevolent strategy (see Table 5, rows H10-H12 for details). That means I found no evidence for an effect of learning when Benevolent strategy was encountered repeatedly: hypotheses H10-H12 were not supported (the respective null hypotheses could not be rejected). Interestingly, there seems to be a somewhat stronger effect of learning between the second and the third encounter with Benevolent (r = .23) than between the first and the second encounter (r = .11), which is not consistent with the law of effect (however, these observations are not statistically reliable).

Deterrent:

With dependent *t*-test for repeated measures I found that "12" > "10", $p < .001$, and that "11" > "10", $p < .017$. Thus I supported hypotheses H13 and H15 (see Table5, rows H13 and H15 for details). I found no difference between "12" and "11" (i.e. between subjects' gains in the

third and in the second encounter with this strategy). Hypothesis H14 was not supported (see Table5, row H14). The effect sizes are moderate.

Bully:

With dependent *t*-test for repeated measures I confirmed that "15" > "13", $p < .001$, and "15" > "14", $p < .001$ (see Table 5, rows H16-H17). In other words hypotheses H16-H17 were supported: I found an effect of learning when the third encounter with Bully was compared with the first, as well as with the second encounter with Bully. I found a marginally significant effect of learning when the second encounter with Bully ("14") was compared with the first encounter with this strategy ("13"), see row H18 in Table 5. The effect sizes are substantial (medium to large). Interestingly, there seems to be a somewhat stronger effect of learning between the second and the third encounter with Bully ($r = .49$) than between the first and the second encounter ($r = .32$), which is not consistent with the law of effect.

3.1 ADDITIONAL RESULTS

As an alternative way how to post-hoc explore possible heterogeneity in learning across gender subsamples (similar to e.g. Schwartz & Fleishman, 1982; Bamberg, 2002; Bamberg et al., 2007; Thöni et al., 2012; Carbone & Duffy, 2014) I subsequently performed dependent *t*-tests for repeated measures for hypotheses H1-H18 for males and females separately (which is, of course, not the same as testing for gender differences). Setting the per comparison alpha cut-off at .017, as in the above analyses, the 36 *t*-tests resulted in 12 "significant" differences. Nevertheless, when I carried out Wilcoxon signed-rank tests to check for robustness of these *t*-tests, I obtained only one "significant" difference out of 36. This shows that the exploration of gender differences would not be very meaningful with the present data.

To complement the analyses presented in Table 5, I estimate five linear mixed models, separately for each of the five opponent strategies, with "Experience" with the strategy at hand (first encounter = 1, second encounter = 2, third encounter = 3; entered as a scale variable) as a repeated measures fixed factor, with game "Length" dummies (Closed, Semiclosed, Open – Open is the omitted category; see section 2.2.1 for description) and "Gender" (male = 0, female = 1) as fixed factors, and with "Subject ID" as a random factor (not reported in Table 6)[76]. I employ the "unstructured" covariance structure, and the REML estimation method in these models. In all models, the dependent variable is the subject's total gain in points in rounds 1-25 (subject totals in a given iterated game are used as observations). Table 6 reports the unstandardized coefficients (and standard errors) for each Model separately.

Table 6: Regressions (for each Strategy) of gains in PD on Experience, game Length and Gender

	Model 1 – Tit for Tat	Model 2 – Random	Model 3 – Benevolent	Model 4 – Deterrent	Model 5 – Bully
Intercept	69.98 (1.13)***	55.32 (2.40)***	79.24 (0.84)***	66.10 (1.41)***	40.90 (1.35)***
Experience	1.41 (0.36)***	1.95 (0.93)*	0.65 (0.32)*	1.92 (0.46)***	2.51 (0.44)***
Length = Closed game	-0.03 (0.40)	0.17 (1.88)	-1.02 (0.56)	-0.08 (0.78)	0.28 (1.00)
Length = Semiclosed game	-0.77 (0.40)	-0.29 (1.88)	0.05 (0.56)	0.26 (0.78)	-0.06 (0.83)
Gender	-0.04 (0.45)	0.32 (1.42)	-0.95 (0.67)	0.14 (0.73)	0.85 (0.83)

Notes: * $p < .05$, ** $p < .01$, *** $p < .001$. Number of observations = 135 in each Model (three observations per subject).

[76] Because we use three observations per participant and Strategy, there can be a clustering of effects in participants' responses, which can lead to correlated errors. This lack of independence is handled by including Subject ID as a random factor in the models.

The results from all five models indicate statistically significant learning effects. The highly significant coefficients on "Experience" in Models 1, 4 and 5, in particular, confirm the robustness of our previous positive findings for Tit for Tat, Deterrent, and Bully (see tests of hypotheses H4-H6, H13, and H15-H18 in Table 5).

While Models 2 and 3 suggest there are (smaller) learning effects also in the case of Random and Benevolent, we should not place too much weight on these estimates. Simple parametric and non-parametric tests reported in Table 5 did not find any learning effects for these two strategies, thus we have to conclude that we do not have robust evidence for learning in repeated encounters with Random and Benevolent.

It is reassuring that, as expected (see section 2.2.1), we see no statistically significant effects of game Length in Table 6.

4. CONCLUDING DISCUSSION

I supported the following hypotheses: H1-H6, H13, H15-H18. That means I found inter-game effect of learning when third encounters with all strategies taken together were compared with first, as well as with second encounters with all strategies taken together. I also found effect of learning when second encounters with all strategies taken together were compared with first encounters with all strategies taken together.

When effect of learning in encounters with particular strategies was tested individually, the results were as follows: I found effect of learning in encounters with Tit for Tat, Deterrent,

and Bully, I did not find robustly significant effect of learning in encounters with Benevolent and Random.

My positive results are very broadly in accord with e.g. Knez (1998), Knez & Camerer (2000), Helbing et al. (2005), Bereby-Meyer & Roth (2006), Dufwenberg et al. (2010), Gneezy et al. (2010), Rick & Weber (2010), Bruttel et al. (2012) and the positive results e.g. in Kagel (1995), Sausgruber & Tyran (2005), Cason et al. (2012). My negative results are broadly in line with the negative results e.g. in Kagel (1995), Silverstein et al. (1998), Cooper & Kagel (2005), Sausgruber & Tyran (2005), Cason et al. (2012), Esposito et al. (2012) (see Introduction where I also discuss the key differences between these studies and the present investigation).

The studies cited above usually show either that in some context learning does take place[77], or that in some context learning does not occur[78]. The present study is different in that I take a single context (the PD game) and explore whether learning takes place depending on the specifics of the interaction partners within the context. I control these specifics experimentally – they are the Strategies I employ. This approach tries to uncover the conditions under which learning can take place from a different perspective than when only looking at the contextual factors without manipulating the *behavior of agents* (i.e., "Strategies") in the context at hand. I believe this approach can be useful also when studying learning in different other contexts besides PD.

[77] E.g., efficient choices in weak-link coordination games tend to increase cooperation in subsequent PD games (see Knez & Camerer; 2000; Ahn et al., 2001; but see Duffy & Fehrz, 2014).
[78] E.g., experience in English auctions does not improve performance in subsequent first-price auctions (Kagel, 1995).

The experiment shows that the occurrence of an effect of learning depends on the particular opponent strategy encountered. We can speculate that subjects did not improve their performance against Benevolent, since it was already relatively easy to interact successfully with this particular strategy during the initial encounter with it. Future research should attempt to measure and systematically manipulate the "difficulty" of the encountered strategies.

The occurrence of an effect of learning possibly also depends on the whole mix of strategies encountered during the experiment (in the present experiment the "mix" consisted of the five strategies employed). The mix of strategies encountered provides a context in which players can learn – it is likely that context can facilitate or hinder learning to play against certain strategies (see e.g. Savikhin & Sheremeta, 2013). For example, in a different mix of strategies than the five strategies I use, the optimal strategy against Random (which is always defect) would perhaps be easier to follow. In the mix of strategies I used here, subjects could have found it psychologically difficult to act uncooperatively against Random, because cooperative behavior was generally successful against the other strategies in the mix. Future research should concentrate not only on specific strategies, but also on the role of the broader context (the mix of strategies encountered).

My results must be interpreted with caution because this study is one of the first inquiries into inter-game learning in the PD (thus, convergent evidence is needed) and my sample was not very large. An important limitation of the present study is that participants made hypothetical decisions without being materially motivated to make good decisions. Presumably, with strong material incentives the decisions could have been different (Camerer & Hogarth, 1999) and learning could have perhaps been better (cf. Bereby-Meyer & Roth, 2006).

Learning can be generally understood as changing behavior or cognition according to past information (Nagel & Tang, 1998). This study, however, remains agnostic as to the exact nature of the adaptations participants develop over the course of the experiment. The improved performance against some of the strategies I observed can be tentatively explained, for example, by updating subjective expected utilities of different possible actions (see e.g. Cohen & Axelrod, 1984; Moore & Moore, 1989; Roth & Erev, 1995) and/or by better recognition of opponent strategies' types, cf. Haruvy & Stahl (2012), Mengel (2012). After suffering losses an individual is likely to change his/her behavior (presumably to avoid further losses). When receiving satisfactory gains, the present behavior is likely to be reinforced and maintained (presumably because the decision maker anticipates future gains from the reinforced behavior). When better recognition of different opponents occurs, fewer losses are needed to abandon maladaptive behavior and fewer gains are needed to maintain adaptive behavior[79].

4.2 RECOMMENDATIONS FOR FURTHER RESEARCH

First, it would be desirable to replicate the present study with a modified set of opponent strategies (the number of strategies used, and consequently the complexity of the decision environment, can also play a role, see Grimm & Mengel, 2012).

Second, a more realistic/natural framing of the experiment might reveal that people are more efficient learners when dealing with familiar situations (cf. Fiddick et al., 2000). The use of natural framing may perhaps decrease decision makers' cognitive costs, which could facilitate optimal learning (cf. Grimm & Mengel, 2012; Mengel, 2012). Framing effects in strategic

[79] Other learning models and their various applications include e.g. Stahl (1996, 2000), Camerer & Ho (1999), Bednar & Page (2007), Arifovic & Ledyard (2012), Haruvy & Stahl (2012).

games have been studied e.g. in Brewer & Kramer (1986), Cooper & Kagel (2003), Abbink & Henning-Schmidt (2006), De Heus et al. (2010), Dufwenberg et al. (2011), and Fosgaard et al. (2014), but much work remains to be done[80].

Third, an inherent limitation of the study of learning is that many trials of PD must be played. Hence, it is difficult to obtain a wholly unbiased sample: the subjects must be at least somewhat interested in the task to be willing to participate, so the results might be biased towards, presumably, better performance of the subjects. Offering participants monetary incentives may provide partial solution to this problem, because greater proportion of the general population would be willing to participate.

[80] Especially in light of papers such as Bardsley (2008), Ferraro & Vossler (2010) or Fosgaard et al. (2014) that suggest a possibly major role of available choice set and/or confusion in game play.

CHAPTER 5: TRANSFER LEARNING WITH AND WITHOUT FEEDBACK[81]

Abstract

Influential learning models assume that learning takes place only in response to feedback. Interestingly, however, Rick and Weber (2010) [*Games and Economic Behavior*, *68*, 716-730] show experimentally that when feedback is withheld, transfer of "meaningful" learning (e.g. how to play Nash equilibrium) to partially dissimilar games is stronger than when feedback is given. In this comment we provide a robustness check of Rick and Weber's finding by studying guessing games with different number of players. Surprisingly, we find no evidence of better transfer learning between partially dissimilar guessing games when feedback is withheld. We discuss processing depth as a possible moderator of the effect of absent feedback on transfer learning.

[81] This chapter is based on a manuscript co-authored by Jan Řezáč and Jiří Špalek (Stepan Vesely being the first author).

1. INTRODUCTION

Economists recently started paying increased attention to learning and introduced influential learning models such as Roth & Erev (1995), Stahl (1996) and Camerer & Ho (1999). Learning can be broadly defined as systematic change in behavior or cognition in response to past information (cf. Nagel & Tang, 1998). A special kind of learning takes place when people change their behavior or cognition in response to their past *internal* experience (such as one's past attempts at solving a puzzle), rather than in response to *external* feedback (such as being told what is the puzzle's correct solution or how others tried to solve it). Weber (2003) and Rick & Weber (2010) report evidence supporting the existence of this type of learning without external feedback in guessing games and as well as in different normal form games. Danz et al. (2012), on the other hand, do not find statistically significant evidence of feedback-free learning in a 3x3 normal form game.

An interesting question is whether learning without external feedback can sometimes outperform learning with external feedback. Rick & Weber (2010, henceforth RW) address this question. In their Experiment 2, they find that within-game learning in a competitive guessing game is more successful with than without feedback (i.e., when given feedback, players converge more quickly on Nash equilibrium).

More interestingly, RW also found that players who first played a 10-round guessing game without feedback subsequently deviated *less* from Nash equilibrium (NE) in a second slightly dissimilar 10-round guessing game (with a different NE), compared to players who were given feedback in the first guessing game. In other words, transfer learning in terms of playing closer to NE was more successful without than with feedback. In this chapter, we test

the robustness of their finding. We extended the RW study by introducing smaller groups of subjects.

In everyday life, people routinely switch from one "game" to another often without fully knowing the outcomes of the first "game". There is a growing literature on transfer learning across partially dissimilar games (e.g. Cooper & Kagel, 2005, 2008; Gneezy et al., 2010; Cason et al., 2012; Haruvy & Stahl, 2012), but more research is needed especially on transfer learning without feedback. As a contribution to this literature, we check the robustness of RW's result on better transfer learning without feedback. RW use guessing games with 8-10 players in their experiment. We use 10-, 3- and 2-person guessing games (Nagel, 1995; Grosskopf & Nagel, 2008) in this replication. As noted by Ho et al. (1998) varying group size is interesting because in smaller groups any given participant exerts more influence on the mean choice in the guessing game, which could affect learning.

2. METHOD

A convenience sample of 178 students from Masaryk University in Brno participated in the experiment across 12 sessions which lasted approximately 40 min. Participants earned 166.4 CZK (approximately \$6.7) on average, including 50 CZK as a show-up fee[82]. The computerized experiment was programmed in z-Tree (Fischbacher, 2007). The experiment was organized and administrated with the software hroot (Bock et al., 2014). Information on age and gender of participants was not collected.

[82] Average hourly wage in a student job was about 80 CZK or \$3.2 at the time of the experiment.

In the experiment, participants consecutively played two slightly different 10-round guessing games. In Phase 1, they played 10 rounds of the "infinite threshold" (IT) guessing game. In Phase 2, they played 10 rounds of the "finite threshold" (FT) guessing game (see Ho et al., 1998; Rick & Weber, 2010). Participants first learned the rules for Phase 1, after Phase 1 was finished they learned the rules for Phase 2. The games are described in the next paragraph[83].

In each round of the guessing game, N players in a fixed group each choose a number in a given range $[s_1, s_2]$. The average of the N numbers chosen is then multiplied by a constant (p) to obtain a target number. In each round, the player whose choice is the smallest absolute distance from the target number wins a fixed monetary bonus, which, in case of a draw, is split equally among the winners[84]. In the IT game, $s_1 = 0$, $s_2 = 100$, $p = 0.7$ and NE = 0. In the FT game, $s_1 = 100$, $s_2 = 200$, $p = 1.3$ and NE = 200.

Participants played the games in fixed groups of either 2, 3 or 10 members. The 10-round IT game in Phase 1 was played either with or without feedback. By feedback we mean that each round participants were informed about their group's average choice, the target number, and their own profit from the round. The 10-round FT game in Phase 2 was always played with feedback.

Table 1 summarizes experimental conditions and the number of participants in each condition.

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[83] Prior to Phase 1, participants' ability to multiply and calculate mean was checked. A calculator was available on the decision screen throughout Phases 1 and 2.

[84] The bonus was equal to 10, 15 and 50 CZK when $N = 2$, 3 and 10, respectively. This means bonus earnings are the same in expectation irrespective of the group size (i.e., 5 CZK per subject and round). In RW, the bonus was 30-37.5 cents per subject and round in expectation (depending on the number of participants per session). The incentives in the two experiments are thus, roughly, comparable.

Table 1: Experimental conditions

Condition	Number of participants	Group size	Type of game in Phase 1	Type of game in Phase 2	Feedback in Phase 1
A	28	2	IT	FT	Yes
B	30	2	IT	FT	No
C	30	3	IT	FT	Yes
D	30	3	IT	FT	No
E	30	10	IT	FT	Yes
F	30	10	IT	FT	No

The key hypotheses concerning the effect of the presence of feedback on subsequent transfer learning are as follows:

H1: First-round choice in Phase 2 is closer to NE in condition B (no feedback) than in condition A (with feedback).

H2: First-round choice in Phase 2 is closer to NE in condition D (no feedback) than in condition C (with feedback).

H3: First-round choice in Phase 2 is closer to NE in condition F (no feedback) than in condition E (with feedback).

Notice that all three predictions are exactly in line with the result from RW that without feedback in Phase 1 participants deviate less from NE in the first round in Phase 2 than when receiving feedback in Phase 1[85]. The main difference between RW and the present experiment is that we use guessing games with different group sizes to provide a robustness check of RW's results.

[85] After the main part of the study we administered a brief incentivized cognitive ability test (CRT, Frederick, 2005). The results from this test are not reported here.

3. RESULTS AND DISCUSSION

In Table 2 we report average choices, by condition, in the first and last round in Phase 1, and in the first round, in rounds 1-3 (averaged), and in the last round in Phase 2[86].

Table 2: Guessing game choices (mean, SD)

Condition	Round 1 choice in Phase 1	Round 10 choice in Phase 1	Round 1 choice in Phase 2	Choice in rounds 1-3 in Phase 2	Round 10 choice in Phase 2
A	33.60 (18.54)	6.43 (19.66)	177.96 (31.48)	190.14 (13.84)	200.00 (0.00)
B	36.95 (24.2)	25.89 (24.54)	175.19 (33.03)	186.93 (17.65)	199.99 (0.03)
C	50.07 (23.36)	6.45 (14.78)	170.67 (27.76)	186.80 (14.64)	200.00 (0.00)
D	44.58 (24.31)	32.16 (24.37)	173.47 (26.32)	184.44 (17.87)	200.00 (0.02)
E	45.48 (19.20)	6.15 (5.37)	180.04 (24.75)	188.56 (17.51)	200.00 (0.00)
F	50.45 (23.48)	38.66 (22.87)	179.43 (18.23)	187.34 (14.05)	199.7 (1.32)

3.1 WITHIN-GAME LEARNING

We first ascertain whether there is within-game learning. Specifically, we first test whether last-round choices in Phase 1 are closer to NE than first-round choices in Phase 1. This is indeed the case in all conditions (all *p*s < .01, one-tailed *t*-test for repeated measures[87]). We next test whether last-round choices in Phase 2 are closer to NE than first-round choices in Phase 2. This is again the case in all conditions (all *p*s < .001, one-tailed *t*-test for repeated measures)[88].

Our results are thus in line with previous evidence of within-game learning in iterated guessing games played with feedback (e.g. Nagel, 1995; Ho et al., 1998) and without

[86] I.e. in rounds 1, 10, 11, 11-13, and 20 in the experiment overall, respectively.
[87] The respective null hypotheses in this and all subsequent cases in this chapter posit no difference between the compared sample averages.
[88] I repeat all parametric tests in this chapter using appropriate non-parametric procedures (Wilcoxon signed-rank test for repeated measures, and Wilcoxon rank-sum test for independent samples). Unless otherwise noted, the non-parametric tests yield qualitatively similar results as the *t*-tests.

feedback (Weber, 2003; Rick & Weber, 2010). Thus, within-game learning in guessing games with and without feedback appears to be a robust finding.

3.2 TRANSFER LEARNING

In Table 3 we report a series of two-tailed independent samples *t*-tests. The tests in the second column in Table 3 establish whether conditions A vs. B, C vs. D, and E vs. F differ in terms of choices in Round 1 in Phase 1. These tests indicate that randomization of participants into conditions with vs. without feedback was successful, as we cannot reject the null of no difference in first-round choices in Phase 1 in the feedback vs. in the no-feedback conditions.

Table 3: Comparisons of conditions A vs. B, C vs. D, and E vs. F

Comparison	Round 1 choice in Phase 1	Round 10 choice in Phase 1	Round 1 choice in Phase 2	Choice in rounds 1-3 in Phase 2
A vs. B	t_{56} = -0.590, p = .56	t_{56} = -3,318, p < .01	t_{56} = 0.326, p = .75	t_{56} = 0.768, p = .45
C vs. D	t_{58} = 0.891, p = .38	t_{58} = -4,940, p < .001	t_{58} = -0.402, p = .69	t_{58} = 0.561, p = .56
E vs. F	t_{58} = -0.898, p = .37	t_{58} = -7,579, p < .001	t_{58} = 0.108, p = .91	t_{58} = 0.299, p = .77

The tests in the third column in Table 3 provide statistical support for the finding that there is stronger within-game learning with than without feedback: choices in the feedback conditions A, C, and E are closer to NE than choices in the no-feedback conditions B, D, and F, respectively.

The next to last column in Table 3 gives the results of the tests of hypotheses H1-H3. None of the three null hypotheses of no difference between first-round choices in Phase 2 in the feedback conditions A, C, and E vs. in the no-feedback conditions B, D, and F, respectively,

can be rejected. Thus, transfer learning is equally successful in conditions with and without feedback[89].

The tests of hypotheses H1-H3 checked for immediate transfer learning (i.e. transfer learning apparent in the very first round of Phase 2). In the last column in Table 3, we perform similar tests for what Ho et al. (1998) call structural transfer (i.e. the ability of experienced participants to quickly converge to NE). However, we do not see participants who played without feedback in Phase 1 (conditions B, D, F) converge more quickly to NE in rounds 1-3 in Phase 2, compared to participants who played with feedback in Phase 1 (conditions A, C, E)[90].

Thus, we do not find support for RW's finding that transfer learning is more successful in conditions without feedback than in conditions with feedback. We find no support for either immediate or structural (delayed) transfer. We find no such support in the 10-person game (which is most similar to the 8- to 10-person games RW use), nor in 3- and 2- person games which were designed to serve as a stress-test of RW's result.

Incidentally, the pattern of behavior in the 10-, 3- and 2-person games in our experiments is strikingly similar, as can be seen in Table 2. Irrespective of the group size, participants converge more quickly to NE within the first 10-round game when given feedback than when feedback is withheld. At the beginning of the second 10-round game with switched NE,

[89] Qualitatively similar results are obtained using analyses of covariance to separately test H1-H3, with first-round choice in Phase 1 as a covariate, first-round choice in Phase 2 as the dependent variable, and condition (A vs. B, C vs. D, and E. vs. F) as independent variable.

[90] Qualitatively similar results are obtained using analyses of covariance to separately test for differences by condition (A vs. B, C vs. D, and E. vs. F) in first-round choices in Phase 2 (dependent variable), when first-round choice in Phase 1 is entered as a covariate.

participants play equally close to the new NE whether feedback has been provided or withheld in the first 10-round game. This fact is again true irrespective of the group size.

We should note that while our design is suitable for testing whether transfer learning is affected by the presence vs. absence of feedback (which is the purpose of this study), we cannot easily test whether transfer learning actually took place in either condition (compared to a control situation with no opportunity to learn). This is because we only use IT games in Phase 1 and FT games in Phase 2, and thus cannot compare first-round decisions of inexperienced participants in Phase 1 in the IT (FT) game to first-round decisions of experienced participants in Phase 2 in the IT (FT) game.

We can, however, test whether participants deviated less from the respective (different) NE in Phase 2 than in Phase 1. With a series of *t*-tests for repeated measures, we establish that the absolute deviation from NE is statistically significantly smaller in the first round of Phase 2 than in the first round of Phase 1 in conditions C, D, E and F (all ps < .01, one-tailed *t*-test), and marginally significantly smaller in conditions A and B (both ps < .10 in one-tailed *t*-test; both ps < .05 in one-tailed Wilcoxon signed-rank test). It therefore appears that transfer learning took place in all conditions to some extent.

4. CONCLUDING REMARKS

Transfer learning is relevant in many settings (e.g. Knez & Camerer, 2000; Cherry et al., 2003; Bednar et al., 2012; Grimm & Mengel, 2012; Haruvy & Stahl, 2012; see also the previous Chapter). RW was the first study to suggest and test the interesting possibility that transfer learning in a strategic setting can be more successful when participants initially learn

without feedback. This possibility is certainly relevant, as one often decides and learns without external feedback (feedback can be delayed, noisy, false, or simply absent). The basic logic behind the possibility of better transfer in feedback-free settings is that feedback can sometimes inhibit more in-depth processing of the problem at hand, and thus the knowledge that could have been obtained through in-depth processing is not available for transfer to other problems (see RW and references therein).

RW found that when players first play an IT (FT) guessing game for ten rounds and receive no feedback, they play closer to NE in a subsequent FT (IT) guessing game, compared to players who first play an IT (FT) guessing game for ten rounds and receive feedback. Shortly put, RW found evidence for better transfer learning in the context of a guessing game when feedback is withheld.

In this note, we replicated RW's experiment using groups of different sizes (2, 3 and 10 members). In neither case did we find support for better transfer learning when feedback was withheld in Phase 1. Thus, RW's interesting results may not be robust.

One difference between RW (Experiment 2) and our study is that RW used a paper-and-pencil procedure while we used a computerized format. Some studies indicate that presentation mode (computer vs. other mode) can affect cognition and choice (e.g. Ackerman & Lauterman, 2012; Oeberst et al., 2015). It is possible that the use of a computerized design leads to a more mechanical responding, which could have precluded the possibility of better transfer learning in the conditions without feedback in our study.

Another difference between the two studies is the intensity of contact between experimenter and participants during the session. In RW (Experiment 2), the experimenter had to record every subject's choice after each round, while there was almost no contact between the experimenter and subjects while they made their decisions in our experiment. The experimenter was present in the room but did not approach the participants. It is possible that participants in RW might have been more motivated to make good decisions due to more intensive contact with the experimenter, which could have lead to deeper cognitive processing and better transfer.

In their Experiment 3, RW replicated their finding of better transfer learning in guessing games played without feedback compared to games played with feedback. They administered Experiment 3 using Fischbacher's z-Tree, as we do here. Unlike us, they use the sequence FT game → IT game (we use the IT → FT sequence). More importantly, in RW's Experiment 3, participants write down their decisions twice (once on the computer screen, once on a record sheet). Moreover, at the end of each round, after submitting their choice, participants in RW's Experiment 3 were also asked to state their belief concerning group average choice. Consequently, participants in RW's Experiment 3 possibly processed their decisions more thoroughly than did participants in our experiment, which could have lead to better feedback-free transfer learning in RW's Experiment 3, as opposed to our experiment.

Despite possible new insights we can obtain by studying feedback-free learning, this chapter shows it is necessary to test the robustness of the effect the presence vs. absence of feedback has on transfer learning. In fact, we found no evidence for better feedback-free transfer learning in either of three guessing games with different group size. What seems like a possible reason for RW finding better transfer learning in guessing games under no feedback,

while we do not, is that participants in their two experiments could have processed their decisions in more depth. Subsequent research should explore this suggestion further. A limitation of the present study is the relatively modest sample size, which could prevent detection of reliable but small effects.

CHAPTER 6: GROUPS THAT ARE INITIALLY MORE COOPERATIVE COOPERATE MORE IN THE LONG RUN[91]

Abstract

Although factors that influence voluntary cooperation have been widely studied in the literature, the role which initial group contributions play in shaping subsequent contributions has not been thoroughly examined yet. We report results from a large-scale classroom experiment: Groups that are initially more cooperative continue to be more cooperative in subsequent rounds of a public goods game. Groups with more women are not more or less cooperative than groups with fewer women. Large groups are not more or less cooperative than small groups. The positive effect of within group communication on subsequent cooperation is, however, smaller in large groups.

[91] This chapter is based on a manuscript co-authored by Jiří Špalek (Stepan Vesely being the first author).

1. INTRODUCTION AND THEORETICAL BACKGROUND

Behavior of individuals facing situations called social dilemmas is a popular topic in psychology and economics (e.g. Dawes, 1975, 1980; Ledyard, 1995; Tyran & Feld, 2006; Herrmann et al., 2008; Fischbacher & Gächter, 2010; Bowles & Polania-Reyes, 2012; Fellner et al., 2013). The term social dilemma was first used by Dawes (1975, 1980) and it represents a situation in which individual interest is in conflict with social interest. A typical example of such a situation is voluntary provision of public goods. Assuming people have purely self-regarding preferences, theory predicts that social dilemma situations result in inefficient outcomes. In case of voluntary contribution to public goods this means that rational individuals seeking to maximize their own utility will not contribute anything to the public good (at least unless they perceive possible gains from establishing a good reputation, cf. Kreps et al., 1982). As a result, the public good will not be provided or it will be provided at a sub-optimal level (this second scenario is most typical in public goods experiments, as not all people have purely self-regarding preferences).

In this chapter we focus on the association between initial cooperation within a group and later cooperation in that group. We model this situation using a public goods game (PGG). Our research objective is, more specifically, to test for associations between contributions in the first round of a PGG and all subsequent rounds (i.e., rounds 2-9 in our study).

In PGG participants are asked to divide their initial endowment between a personal account and a public account, which they share with other participants in their group. The payoff structure motivates self-interested rational players to contribute nothing to the public account, but the efficient outcome for the group is for each subject to contribute their entire endowment to the public good. As is well known, PGG can model situations such as

cooperation in teams, charitable giving, fundraising or engaging in pro-environmental behavior (e.g. Viguier, 2004; Tyran & Feld, 2006; Shang & Croson, 2009; van der Heijden et al., 2009). See Ledyard (1995), Zelmer (2003) and Chaudhuri (2011) for reviews of the experimental literature.

When PGG is played repeatedly (e.g. taxpayers are asked to pay taxes every year, teams are assigned new tasks), it can be interesting to know whether previous behavior in the PGG predicts future behavior in the game. Do initial contributions within a group predict that group's long-run behavior? We provide a partial answer to this question by looking at the correlation between initial contributions (in round 1) to the public good within a group and subsequent contributions (in the next eight rounds) to the public good within that group[92].

Keser & van Winden (2000) and Gächter & Renner (2014) found strong positive correlation between the average group contribution in the first period and over all subsequent periods. I.e., groups that are more cooperative initially are more cooperative in the long term, according to Keser & van Winden (2000) and Gächter & Renner (2014). A recent paper by Dannenberg et al. (2015) reports very high positive correlations between the average group contribution in the first period and over all subsequent periods in threshold PGGs with an unknown (risky or ambiguous) threshold. On the other hand, they find very high negative correlation between the average group contribution in the first period and over all subsequent periods in a threshold PGG with known threshold.

[92] The initial contribution (in round 1) can be seen as an index of the types of players included in the group. In the first round, the players have not interacted with others in their group yet, therefore their decision to either contribute or free-ride is based on their "homegrown" type (as argued e.g. in Gächter et al., 2014).

However, Keser & van Winden (2000) and Gächter & Renner (2014) only report a positive correlation between average group contribution in the first period and *over all subsequent periods combined*. Similarly, Dannenberg et al. (2015) report positive correlation between average group contribution in the first period and *over all subsequent periods combined*. Here, we study the long-run effects of initial contributions in somewhat more detail by *separately* looking at the associations between initial group contribution to the public good (round 1) and every subsequent group contribution (rounds 2-9). This approach will lead to a better understanding of how initial contributions/cooperation in a group affect subsequent contributions/cooperation.

Moreover, we use considerably different procedures than previous related studies, especially in that we collect data in large groups in a classroom experiment. Thus our experiment also serves as a robustness check of previous findings.

Related to our investigation are also studies by Sonnemans et al. (1999), Croson et al. (2005), Guillen et al. (2010), Ambrus & Pathak (2011) and Kimbrough & Vostroknutov (2015). However, in general, these papers focus on a shorter time horizon than we do, i.e. they do not really study cooperation "in the long term" as we do here[93]. Ambrus & Pathak (2011) find moderate positive correlation between the average group contribution in the first period of a PGG and individual contributions in the second period. Croson et al. (2005), Guillen et al. (2010) and (in case of highly norm-following subjects) also Kimbrough & Vostroknutov (2015) find a positive correlation between the average group contribution in period t-1 (not necessarily the first period) and individual contributions in period t. Sonnemans et al. (1999)

[93] There are other studies thematically related to ours, e.g. Fischbacher et al. (2001), but they only focus on the shortest time horizon possible, i.e. they investigate conditional play within one game.

study PGG with two options (contribute or not) and find that subjects contribute with higher probability in period *t* when more other subjects in their group contributed in period *t*-1.

The underlying mechanism explaining why initially more cooperative groups (might) continue to be more cooperative in the long run is not the focus of the present study. We test for a link between initial and subsequent contributions *without postulating a specific underlying mechanism for this link*. Such a mechanism could be complex and the present study is not intended or designed to explore it (the interested reader is referred e.g. to Fischbacher & Gächter, 2010; Gächter et al., 2012; Gächter & Renner, 2014).

We thus only briefly mention two possible candidates for such a mechanism. A "homegrown type" explanation suggests that players tend to stick to their homegrown type throughout the game (e.g., cooperators continue to cooperate and free-riders continue to free-ride because of their type/disposition, see e.g. Messick & McClintock, 1968; Kuhlman & Marshello, 1975; Fischbacher et al., 2001; Fischbacher & Gächter, 2010). A second explanation could be based on some form of a consistency theory (see e.g. Cialdini et al., 1995; Bodner & Prelec, 2003; Oxoby, 2004; Falk & Zimmermann, 2011). If players begin to play for example cooperatively, they may dislike switching away from cooperative behavior later because of their preference for consistency. Unlike the first explanation, the second explanation does not assume a strong homegrown taste for either cooperation or free-riding on the players' part. The players might be even (but not necessarily) completely indifferent between cooperation and free-riding at first, but once they make an initial choice, they commit to it, as they have a taste for consistency. Both explanations sound interesting. We leave it to future research to explore possible underlying mechanisms of long-run effects of initial contributions.

2. METHOD

The study was conducted at Masaryk University in Brno in 2007-2013 as a classroom experiment across 52 sessions. In each session (class) there was precisely one group in which participants played an iterated PGG. A convenience sample of undergraduate students at the Faculty of Economics and Administration at Masaryk University participated in the study (n = 1644, 54.3% females, information on age was not collected).

PGG was played repeatedly over nine rounds in each group (partners design). At the end of each session, one randomly chosen participant received a non-monetary performance-dependent reward (extra points that could be used to pass an exam). One point earned during the experiment was exchanged to 0.05 points for passing the exam (passing the exam required at least 120 points in total).

In each round, a player could contribute zero, one, or two chips to the public account and the rest of the chips to his private account. Individual contributions were anonymous. Participants learned the sum of all contributions to the group account after each round.

The value of a chip allocated to the public account was 10 points for each player in the group (irrespective of whether he/she contributed). The value of a chip allocated to the private account was 40 points in rounds 1-3 and 7-9 and it was 20 points in rounds 4-6, for the player who allocated it to his/her account[94].

[94] Experimental instructions are available in Appendix D. The game parameters were varied primarily for educative purposes of the classroom experiment. For this reason, too, a group discussion was allowed between rounds 6 and 7. We do not focus on the effects of these manipulations here, as they are relatively well documented in the literature (e.g. Sally, 1995; Zelmer, 2003; Balliet, 2010). We called the points "CZK" (the

This payoff structure means that each player has the rational individual income maximizing strategy to contribute nothing to the public account and everything to his/her private account in every round, irrespective of what the others do. In contrast, the Pareto optimal (group welfare maximizing) strategy is for each player to contribute everything to the public account in each round (hence the game is a "mixed-motive" game or a "social dilemma"), cf. e.g. Andreoni (1988).

The group sizes differ somewhat across sessions (mean size = 31.6, SD = 6.5). In the analyses we therefore control for group size. We also control for the proportion of females in each group.

3. RESULTS AND DISCUSSION

Table 1 provides a first glimpse at our data. The proportions of chips contributed to the public account are given there for individual rounds (1, 2, …, 9), as well as averaged across rounds 1-9 (the entire game).

Table 1: Fraction of chips contributed to the public account

Round(s)	Proportion of chips contributed to the public account (mean, SD)
1-9 (the whole game)	0.41 (0.11)
1	0.31 (0.09)
2	0.26 (0.11)
3	0.21 (0.10)
4	0.52 (0.10)

currency unit in Czech Republic) in the instructions, but participants were fully aware the game was not played for real money.

5	0.44 (0.12)
6	0.36 (0.12)
7	0.77 (0.18)
8	0.50 (0.25)
9	0.34 (0.22)

The overall cooperation in this experiment is relatively high (41%), peaking in round 7 (77%), i.e. in the round following the opportunity for group discussion. We do not observe the typical downward trend of contributions to the public account across rounds 1-9 (e.g. Andreoni, 1988; Croson, 1996b; Croson et al., 2005; Neugebauer et al., 2009). The likely reason is that game parameters are different in rounds 1-3, 4-6 and 7-9, so the effect of time is confounded by the changes in parameters. However, notice that within the three sequences of consecutive rounds with identical parameters (i.e., sequences consisting of rounds 1-3, 4-6 and 7-9) decreasing trends do occur. Three repeated-measures ANOVAs run separately for rounds 1-3, 4-6 and 7-9 confirm that these decreasing linear time trends in contributions are all significant at $p < .001$.

Now we turn to the main analysis. To reiterate, the research question we want to answer is: What is the association between initial contributions within a group (in round 1) and subsequent contributions in that group (in rounds 2-9)?

To answer this question we run nine separate regressions presented in Table 2. First, we regress the fraction of chips contributed to the public account averaged across rounds 2-9 on the fraction of chips contributed to the public account in round 1 (see Model (1) in Table 2). We next separately regress the fraction of chips contributed to the public account in specific rounds (2, 3, 4, …, 9) on the fraction of chips contributed to the public account in round 1 (Models (2)-(9)). Each of these regressions controls for group size and the proportion of

females in the group (see columns four and five in Table 2). Average group contribution in a round is the unit of observation in all regressions.

Table 2: Results of nine regressions of the fraction of chips contributed to the public account in latter rounds on the initial contribution to the public account

Model	Outcome variable is the fraction of chips contributed to the public account in round(s):	Standardized Beta coefficients for: Fraction of chips contributed to the public account in round 1	Group size	Proportion of females in group	Proportion of variance (R^2) explained by model
(1)	2-9	.47**	-.16	.09	.19
(2)	2	.73***	-.07	.12	.51
(3)	3	.64***	.03	-.19	.42
(4)	4	.40**	-.05	-.14	.14
(5)	5	.44**	-.04	-.09	.16
(6)	6	.48**	-.01	.04	.19
(7)	7	.22	-.33*	.20	.10
(8)	8	.18	-.19	.21	.04
(9)	9	.25#	-.13	.08	.02

Note: * $p < .05$, ** $p < .01$, *** $p < .001$, # $p < .10$.

The results in Table 2 show that the fact that a group starts out as more or less cooperative continues to affect cooperation up to the sixth round. This is in line with Keser & van Winden (2000) and Gächter & Renner (2014). Gächter & Renner (2014) explain this correlation by group members' beliefs concerning others' cooperativeness and by conditional cooperation (see also Fischbacher & Gächter, 2010; Gächter et al., 2012).

The association between initial and subsequent play in the PGG studied in our experiment is similar to the also positive association between these variables in threshold PGGs with unknown thresholds studied by Dannenberg et al. (2015). (In contrast, groups contributing little in early rounds in PGGs with *known* thresholds apparently feel they need to boost their

cooperation later in the game in order to reach the known required threshold, hence the negative correlation between initial and subsequent play in PGGs with *known* thresholds in Dannenberg et al., 2015[95]. When the threshold is not known, or when there is no threshold at all as in our experiment, such a pressure is, unsurprisingly, weaker or absent.)

The effect of initial contributions is most pronounced in the earlier rounds (see Models (2) and (3)). We can observe (see the standardized Betas in Table 2) that a one standard deviation increase in the initial average group contribution (round 1) predicts approximately a 0.5 standard deviation increase in contributions in the following rounds. This is true for the game overall (Model (1)) and for individual game rounds up to the sixth round.

The effect of initial contributions is, however, smaller and not statistically significant when looking at rounds 7-9 (although in round 9 it is marginally significant). Based on our data, it is not possible to say whether: (a) the effect of initial contributions "wears off" at the very end of the iterated game, and/or (b) whether it is the possibility to communicate prior to round 7 which accounts for its absence, and/or (c) whether the reason is that players change their reasoning at the end of the game. Further experimentation is needed to disentangle these alternative explanations.

We only find one other significant effect in our analyses (see below). As for the negative results, groups with a higher proportion of female members do not cooperate more (or less). This is consistent with much of economic and psychology literature that often discovers only minor, mixed or no differences in strategic behavior by gender (see e.g. Ortmann & Tichy, 1999; Zelmer, 2003; Croson & Gneezy, 2009).

[95] As Dannenberg et al. (2015) comment, the negative correlation reflects "the presence of groups that had a slow start but ultimately strived and managed to reach the threshold" (see note 13 in their paper).

We also fail to find a significant effect of group size (except in round 7). This is not very surprising since in Zelmer's (2003) meta-analysis group size has only a marginally significant effect on contributions.

The only other positive result we obtain is that larger groups contribute comparatively less in the round following the group discussion (see Model (7)). According to our Model (7), smaller groups are significantly more able to reap the benefits of communication, i.e. they are more collaborative after communication takes place (i.e. in round 7). Interestingly, Balliet (2010) reports the exact opposite in his meta-analysis (i.e. larger groups are more collaborative as a result of communication). It is important to note, however, that Balliet's study only includes groups that are much smaller (2-9 group members) than groups in the present study (21-46 members). Thus, our results complement Balliet's (2010) findings in an important way. Group size is confounded with marginal per capita return (MPCR) in our experiment, however. Thus the interpretation proposed here must be read with caution and confirmed by an experiment in which group size is not confounded with MPCR.

3.1 LIMITATIONS

This study has a number of limitations, which are mostly due to the fact that this was a classroom experiment. Despite its limitations, the experimental results support the interpretation that groups that are initially more cooperative continue to be more cooperative in the long run. More research on this issue in a cleaner setting is, however, needed.

One limitation of this study is that MPCR varies with group size, and thus the effect of group size cannot be separated from the effect of MPCR in our study. Testing for main effects of group size and MPCR was nevertheless not the objective of the present study (these effects have been repeatedly investigated by others, see e.g. Isaac & Walker, 1988; Zelmer, 2003). The fact that group size and MPCR are confounded in our design has to be acknowledged, however.

Also, as mentioned earlier, we cannot single out one specific reason for the diminished correlation between first-round cooperation and cooperation in rounds 7-9. More generally, a limitation of the present study is that game parameters change in round 4 and again in round 7. Strictly speaking, we thus cannot distinguish the effect of time (i.e., round position) from the effect of changing parameters. Subsequent research concerning the link between initial and subsequent cooperation should therefore rely on repeated games in which game parameters are kept constant (possibly games with an unknown end, to avoid end-game effects). That way, when we, for example, see a diminishing correlation between initial and subsequent play as the game progresses, we would be able to attribute this observation to time alone[96], which is not possible in the present study.

Finally, although individual decisions were fully anonymous, (many) participants knew each other since they were attending the same class. This could possibly influence their behavior (cf. Charness & Gneezy, 2008; but see Zelmer, 2003 – previous friendship among subjects did not have a significant effect on contributions in her meta-analysis). However, in non-experimental social dilemmas people also belong to a firm, a community, a neighborhood, or a country – in a similar way as in our experiment they belong to a class. So, the fact that

[96] In general, there can be different mechanisms underlying the effect of "time", of course (e.g. Andreoni, 1988; Croson, 1996b).

participants knew each other in our experiment may actually increase the external validity of our findings.

4. CONCLUSION

The last three decades of experimental research identified several factors that robustly affect cooperation in social dilemmas. Our experiment shows that one factor of substantial importance might be the initial behavior of the group. We find that groups that are initially more cooperative continue to be more cooperative in subsequent rounds of the public goods game.

Groups with more women are not more (or less) cooperative than groups with fewer women. Larger groups are not more (or less) cooperative than smaller groups. However, larger groups are less able to increase cooperation following a joint discussion within the group. Future research should try to study ways how to ensure greater positive effects of within-group communication on subsequent collaboration and efficiency in large groups. This is important as many groups facing social dilemmas outside the laboratory are large.

CHAPTER 7: IS TOUGH BARGAINING EFFECTIVE? AN ULTIMATUM EXPERIMENT

Abstract

Tough responders who previously turned down generous offers are able to elicit unusually high offers from proposers in the ultimatum game. This is especially true for "moderately" tough responders (who previously rejected a 55-45 split). While sophisticated tough play has been identified as a potentially effective long-run strategy in theory, this study is the first that empirically shows it can be effective in negotiation between humans.

1. INTRODUCTION

In the ultimatum game (UG) two players have a sum of money to be divided between them. One of the players is proposer, the other is responder. The proposer makes an offer how to divide the money and the responder either accepts or rejects the offer. If the responder accepts the offer, the money is split according to the proposed division. If the responder rejects the offer, neither player gets anything (Güth et al., 1982). The unique subgame-perfect Nash equilibrium for this game is that proposer offers the smallest fraction of the surplus to responder and the responder accepts it. However, in reality proposers usually offer approximately 40% to 50% of the endowment, with 50-50 split being the modal offer, and most responders reject offers below about 20% of the total sum (Roth, 1995; Güth & Kocher, 2014)[97].

In the present experiment I investigate whether a tough responder is able to "teach" the proposer to allocate unusually high portion of the surplus to him[98]. This possibility has been neglected so far in experimental literature. The likely reason for this gap is that tough bargaining is unusual in the lab. But the fact that some behavior is unusual does not mean it should be ignored. On the contrary, it can be interesting to study strategic reactions to relatively unusual stimuli, such as high contributors (e.g. Bardsley, 2000; Shang & Croson, 2009; Croson & Shang, 2013), grim trigger players (Guillen et al., 2010), or tough bargainers.

Studies on sophisticated play in repeated games (e.g. Ho et al., 1998; Tang, 2001; Camerer et al., 2002; Rapoport et al., 2002; Napel, 2003) suggest that when UG is played repeatedly, the

[97] The 50-50 split can be also seen as a natural focal point (Lindahl & Johannesson, 2009) and a social norm (Vesely, 2015).

[98] In this chapter, I define "tough" bargaining by the responder as rejecting equitable or higher offers. Rejecting near-equitable offers (say, 40% of the surplus) would also qualify as being "tough", in my opinion. However, I do *not* study the case of rejecting slightly less than equitable offers in this chapter.

responder may want to be tough and turn down large offers in earlier games in order to teach the proposer to make even larger offers in subsequent games. Sophisticated players develop multi-period forecasts and when matched with the same adaptive players repeatedly they can teach them by choosing strategies with poor short-run payoffs (e.g., responders can reject high offers) which will possibly change what adaptive players do (e.g., proposers might increase their offers), see Camerer et al. (2002).

Napel (2003) shows that in an agent-based simulation of mini-UG a successful player is persistent and stubborn in not quickly adapting his aspiration level in response to his opponent's behavior. Empirically, responders' tough bargaining could be effective if human proposers are sufficiently malleable or "adaptive". In this chapter I provide the first empirical evidence on the effect of responders' tough bargaining behavior on subsequent offers made by proposers.

To formulate my research question in specific terms, consider the following two scenarios.

In the first scenario, proposer A1 offers 50% of the surplus to responder B1. Responder B1 accepts the offer (thus, responder B1 is not tough[99]). Another player C1 subsequently learns about this (past) game between A1 and B1. Player C1 then proceeds to play a new UG with responder B1. How much will C1 offer to B1?

In the second scenario, proposer A2 offers 50% of the surplus to responder B2. Responder B2 rejects the offer (thus, responder B2 is tough). Another player C2[100] subsequently learns about

[99] I define being tough as rejecting equitable or higher offers.
[100] In principle, players C1 and C2 can be also thought of as being identical to A1 and A2, respectively. However, I do *not* study this repeated-game case in the present chapter.

this (past) game between A2 and B2. Player C2 then proceeds to play a new UG with responder B2. How much will C2 offer to B2?

Now, the research question is not as much how much will C1 offer to B1 and how much will C2 offer to B2 in absolute terms. My research question is, rather, whether C2 will offer to B2 *more* than C1 will offer to B1. If the answer is "yes", this will be evidence of tough bargaining being effective in eliciting high offers[101].

2. METHOD

2.1 PARTICIPANTS

A convenience sample of 267 undergraduate students (170 females) participated in the experiment across 18 sessions, which lasted about 30 min. Information on participants' age of was not collected. Participants earned 137.2 CZK (approximately $6.4) on average including 90 CZK show-up fee. The experiment was programmed in z-Tree (Fischbacher, 2007) and run in a computer lab at Masaryk University in Brno (CZ).

2.2 DESIGN

The design of the experiment follows a basic logic very similar to that of comparing players' "C" offers in the two hypothetical scenarios (with a tough and a non-tough responder "B") described in the Introduction.

[101] Different variations of these scenarios are studied in this chapter.

The experiment involves two separate consecutive UGs. The first UG is used to implement the experimental conditions (there are three experimental conditions and a control condition). The second UG is used to obtain the dependent measure, namely the proposer's "C" offer.

Before reviewing the procedural details (which are slightly complicated), it will be useful to consider what the experimental conditions are, first. These are given in Table 1, together with the number of participants in each condition.

Table 1: Experimental conditions

Condition	First UG offer by proposer A to responder B (out of 200 tokens)	First UG offer accepted by responder B? (Yes/No)	Number of participants C in each condition
Control	100	Yes	49
Low	100	No	52
Medium	110	No	56
High	130	No	56

Consider conditions Control and Low displayed in Table 1 first. You will notice these conditions are essentially identical to the two scenarios from the Introduction:

In the Control condition, proposer A1 offers 100 out of 200 tokens, i.e. 50% of the surplus, to responder B1 (see the second column in Table 1)[102]. Responder B1 accepts the offer (see the third column in Table 1). Another player C1, assigned to the Control condition, then learns about the previous game between A1 and B1. Player C1 then proceeds to play a second UG with responder B1. The last column in Table 1 indicates there were 49 such "C1" players assigned to Control.

[102] A1, B1, C1, A2, B2, C2 are just "names" of the players.

In the Low condition, proposer A2 offers 100 out of 200 tokens, i.e. 50% of the surplus, to responder B2 (see the second column in Table 1). Responder B2 rejects the offer (see the third column in Table 1). Another player C2, assigned to the Low condition, then learns about the previous game between A2 and B2. Player C2 then proceeds to play a second UG with responder B2. The last column in Table 1 indicates there were 52 such "C2" players assigned to the Low condition.

To see whether tough bargaining is effective I will test whether players C2 (i.e. players C assigned to Low) will offer significantly more on average in the second UG than players C1 (i.e. players C assigned to Control). If the answer is "yes", this will be evidence of tough bargaining being effective in eliciting high offers.

I will make the same kind of comparison between the second UG offers by players C in conditions Medium vs. Control, and High vs. Control. The design of the experiment is thus based on pairwise comparisons between the three experimental conditions and the control condition shown in Table 1.

As one can see in Table 1, in conditions Low, Medium and High, responder B is, by my definition, "tough" as she rejects offers equal to or above the 50-50 split. She is toughest in condition High (rejection of a 130-70 split), less tough in condition Medium (rejection of a 110-90 split), and only "slightly" tough in condition Low (rejection of a 100-100 split). In the Control condition, the responder accepts a 100-100 split, which is not tough.

2.3 PROCEDURE

Although the basic design involves two consecutive UGs and three players, the key features of the design, which have been described in the previous subsection, are quite straightforward. What is a little more complicated is how the design was implemented. However, whatever the complexities of the implementation, they do not change the basic logic of the experiment which has been already described.

We first need to consider how the four conditions (see Table 1) were implemented. As is apparent from Table 1, the conditions differ in the observed behavior of proposer A and responder B. I had to manipulate the observed behavior of A and B experimentally. This was done by using *fictious* proposer A and *fictious* responder B. For example, in condition Low, *fictious* proposer A made a 100-100 offer and *fictious* responder B rejected it (similarly in the other conditions, see Table 1). Using fictious players A and B was necessary because offers above the equal split are normally rare in the lab.

I randomly assign *real* players C (i.e., the participants) to one of the four conditions (see the numbers of real players C assigned to each condition in Table 1). From the previous paragraph it is clear that each of these real players C observes what is – actually – an experimentally manipulated behavior of *fictious* players A and B (in the first UG). The real player C then (in the second UG) has to make an offer to a *fictious* player B.

Now comes another important point. Players C *do not know* that players A and B which they observe are fictious. If a player C knew that "his" responder B is fictious, then player's C second UG decision would be "hypothetical". In this study I avoid the disadvantages of hypothetical, incentive non-compatible choice experiments on the one hand and of experimental deception on the other hand. To achieve this, I employ a modification of the

Conditional Information Lottery introduced in Bardsley (2000). Based on the arguments proposed by Bardsley (2000), I note that in a situation in which real and fictious opponents A and B *cannot be distinguished a priori* by players C, all players C should act towards "their" A and B as if they were interacting with real players A and B (for a detailed discussion see Bardsley, 2000).

Next comes another important step: How do I ensure that real and fictious opponents A and B cannot be distinguished a priori by players C? This is done by having one *real* player A and one *real* player B in each session. Thus, in each session, two real participants are randomly assigned to be *real* players A and B, respectively. These two players participate in the first UG.

Apart from one real player A and one real player B per session, all the remaining participants in each session are randomly assigned to be players C, which are all real. Of these players C, precisely one is randomly picked to observe the decisions of the two *real* participants A and B; let us call this participant "special-C". Let us call the remaining participants C "normal-C". Each normal-C participant is randomly assigned to a condition from Table 1. Each normal-C participant observes the first UG decisions of fictious participants A and B, as determined by the condition to which the normal-C has been assigned (thus, all normal-C participants in a given condition observe the same decisions of fictious A and B participants).

Next comes another important point: No player C knows whether she has been randomly picked to be the special-C or a normal-C. In other words, all participants C observe first UG decisions of player A and player B, but they do not know whether "their" players A and B are real or fictious. However, all participants C are told beforehand that they *could* be randomly

picked to become *either* special-C or normal-C (and thus that they will be paired with real A and B players, or with fictious ones).

I summarize the sequence of procedures in each session as follows:

1) Player roles (A, B, C) of the actual participants are randomly assigned at the beginning of the session. The roles are private knowledge. In each session, precisely one participant is assigned the role of real player A, precisely one participant is assigned the role of real player B, and the rest of the participants are assigned the role of player C (out of these, precisely one is special-C and the rest are normal-C; all players C are real)[103]. Participants in the role of player C do *not* know whether they are special-C or normal-C.

2) Real participants A and B make their decisions in the first UG.

3) Each participant C is informed about the decisions of one pair of players A and B in the first UG. Each participant C knows that the pair of players A and B she learned about could be real (if participant C has been chosen to be "special-C"), or fictional (if participant C has been chosen to be "normal-C"). Normal-C participants are randomly assigned to condition and observe the first UG behavior of fictious A and B players as specified for the selected condition in Table 1.

[103] This explains why of the 267 total participants in this study only 213 have been assigned to experimental conditions (as indicated in Table 1): as there were 18 sessions in total, 18 participants became real players A, 18 participants became real players B, and 18 participants became special-C players. These 54 participants *have not been assigned* to the experimental conditions and data from them are not included in the analysis in section 3. These 54 participants were needed solely to implement the Conditional Information Lottery procedure (Bardsley, 2000) in this experiment.

4) In the second UG, each participant C makes an offer to player B she is paired with. This offer will only be payoff-relevant if player B with whom participant C is paired is the real one (this, again, is known to participants).

5) Real player B accepts or rejects the offer from participant C with whom she is paired in the second UG.

6) Participant C (i.e., special-C) who was paired with the real player B is informed about participant B's decision in the second UG. Participants C (i.e., normal-C) who were paired with fictional players A and B are told so.

7) All participants are paid privately and leave (participants who were paired with fictional players receive just the show-up fee, the rest are paid according to their decisions plus the show-up fee).

In the first UG, the surplus size was 200 tokens (each worth 0.6 CZK), in the second UG, the surplus size was 1000 tokens (each worth 0.6 CZK). Instructions for normal-C players are available in Appendix E.

3. RESULTS AND DISCUSSION

The mean second UG offer was 433.1 tokens (SD = 93.9) in the Control condition; 477.8 tokens (SD = 154.5) in condition Low; 537.7 tokens (SD = 116.6) in condition Medium; and 483.0 tokens (SD = 113.3) in condition High.

A one-way independent measures ANOVA reveals a significant effect of condition on second UG offer size, $F(3, 209) = 6.54$, $p < .001$. After establishing this significant omnibus effect, I proceed to conducting planned pair-wise comparisons between each of the experimental conditions (Low, Medium, High) and the Control condition with respect to offers made by participants in the second UG. To control for false positives I apply the conservative Bonferroni correction, i.e. the alpha cut-off for each pair-wise comparison is set at .017 (instead of the uncorrected .05). I use both parametric *t*-tests and non-parametric Mann-Whitney U tests to test for differences between the Control condition and each of the experimental conditions. The tests proceed by attempting to reject the null of no difference between the central tendencies of second UG offers in the respective compared conditions. Table 2 presents the results.

Table 2: Second UG offers: Comparisons of experimental conditions with control

Compared conditions	Mean difference ($CI_{95\%}$)	*df*	*t*	*r* (effect size)	*p* (*t*-test)	*p* (Mann-Whitney U test)
Low vs. Control	44.8 (-6.1, 95.6)	99	1.747	.17	.084	.000*
Medium vs. Control	104.6 (63.3, 146.0)	103	5.018	.45	.000*	.000*
High vs. Control	50.0 (9.4, 90.6)	103	2.440	.24	.016*	.008*

Notes: * $p < .017$. The column "Mean difference" indicates the mean difference in offers between the compared conditions. Positive mean difference indicates that offers in a given experimental condition were on average higher than in Control.

The results presented in Table 2 indicate that second UG offers in the Low condition (with the least tough responder) are not robustly significantly higher than in the Control condition. There is, however, robust evidence that the two other experimental conditions, Medium and High, lead to higher second UG offers compared to Control.

In case of the Medium condition, participants offer on average over 100 tokens more to the (moderately tough) responder than do participants in the Control condition with a non-tough responder. The effect size is $r = .45$ (medium to large effect).

In case of the High condition, participants offer on average 50 tokens more to the (toughest) responder than do participants in the Control condition. The effect size is $r = .24$ (small to medium effect).

In sum, this experiment shows that tough bargaining can be – within limits – quite effective. Moderately tough bargainers in condition Medium were the most successful in eliciting high offers from participants.

It is not too surprising that the least tough responders in condition Low did not on average elicit reliably higher second UG offers compared to Control. Presumably, the level of the "treatment" was too low to be effective in condition Low.

It is interesting to note that this experiment's toughest responders in condition High were not as successful (compared to Control) as the moderately tough responders in condition Medium in eliciting high offers from participants in the second UG. When responders' play is (too) tough, there are at least three forces that may crowd out proposers' generous offers[104]:

(1) The threat that non-generous offers will be rejected is simply not credible enough (cf. Ellingsen & Johannesson, 2004).

[104] It would be interesting to see what would be the effect of even tougher bargaining, such as rejecting 150-50 splits in the first UG.

(2) The behavior of responders rejecting generous offers in the first UG may seem nasty and unmannered to some proposers who may want to punish the responders by very low offers in the second UG (Camerer & Thaler, 1995; Rotemberg, 2008).

(3) In some rare cases highly egalitarian responders prefer 50/50 allocations over generous offers (Murnighan & Saxon, 1998; Henrich et al., 2001; Bahry & Wilson, 2006). If proposers in conditions Medium and High think that responders have such preferences, they may propose an equal split instead of a very generous offer in the second UG.

How would tough bargaining fare in longer interactions? This is an especially important question because net benefits from tough bargaining are often going to be negative at first, but once a credible reputation for toughness is earned the bargainer would later reap the benefits of high offers without the need to reject low offers. This bears some resemblance to the case of decentralized punishment in the public goods game à la Fehr & Gächter (2000a) – the game has to be long enough for the punishment mechanism to yield net benefits (Fehr & Gächter, 2000a; Gächter et al., 2008; Chaudhuri, 2011). If tough bargaining can be somewhat effective in the short-run, as the present experiment shows, there is at least reason to believe it can be profitable on net in the long-run. I leave the exploration of this very important issue to subsequent research.

Finally, on a methodological note, as is apparent from section 2.3, implementing Bardsley's (2000) Conditional Information Lottery can be slightly complex. However, it has the serious

advantage of enabling experimental manipulation of (even unusual) stimuli, while avoiding the disadvantages of hypothetical choice experiments (i.e., when participants in an experiment are told they are paired with fictious opponents) and deception (i.e., when participants in an experiment are *falsely* told they are paired with real opponents). See e.g. Hertwig & Ortmann (2001) and Chapter 3 in this thesis for a discussion of the methodological issues concerning hypothetical choice and deception.

I am not aware of any study systematically comparing decisions obtained using the Conditional Information Lottery and decisions from a fully transparent design with real (or hypothetical) opponents. Such a comparison would serve to strengthen or qualify our confidence in the validity of decisions in Conditional Information Lottery designs. Similar comparisons are already available for the strategy method, and for the random lottery incentive system and other payment methods (Cubitt et al., 1998; Brandts & Charness, 2011; Cox et al., 2015). Despite the arguments in Bardsley (2000), it is conceivable that, empirically, participants tend to treat decisions in Conditional Information Lottery designs as hypothetical ones. It is also conceivable that participants are in some cases able to discern real and hypothetical scenarios/opponents a priori, thus rendering the Conditional Information Lottery procedure ineffective. Thus, empirical investigations of the validity of decisions in Conditional Information Lottery designs are needed.

4. CONCLUSION

Gneezy et al. (2003) note there are many imperfect equilibria in the UG supported by non-credible threats. I show that some of the theoretically non-credible threats become credible enough (behaviorally) when backed up by previous costly rejections. Tough responders in

conditions Medium and High elicit higher offers compared to offers to non-tough responders in the Control condition. On the other hand, proposers do not offer robustly significantly more to slightly tough responders in condition Low compared to Control.

Besides the question of tough bargaining's long-run effectiveness, future research can focus for example on the following questions: What are the limits of toughness beyond which bargaining cannot be successful? What are the factors that facilitate and inhibit the effectiveness of tough bargaining (e.g. communication, personality, prevalent social norms)?

Experimental economists have shown time and time again that subgame perfection has relatively little bite in UG. Perhaps surprisingly, this experiment is the first to show that behavior in UG can deviate even more radically from the subgame perfect equilibrium prediction in that a tough responder might be able to earn *most* of the surplus in the game (and not "just" more than nothing).

CHAPTER 8: ELICITATION OF NORMATIVE AND FAIRNESS JUDGMENTS: DO INCENTIVES MATTER?[105]

Abstract

Krupka and Weber (2013) [*Journal of the European Economic Association, 11,* 495-524] introduce an incentive-compatible coordination game as an alternative method for elicitation of normative judgments. I show, however, that people provide virtually the same responses in incentivized and non-incentivized versions of the Krupka-Weber game. Besides ratings of social appropriateness, I also elicit ratings of fairness of all possible offers in an ultimatum game. Ratings of social appropriateness and fairness are similar for low offers (below or equal to the equal split), but not for high offers which are judged to be more appropriate than fair.

[105] This chapter is based on Vesely (2015).

1. INTRODUCTION AND RESEARCH BACKGROUND

The study of social norms is becoming common in behavioral economics (Bardsley & Sausgruber, 2005; Wenzel, 2005; Croson et al., 2008; Bicchieri & Xiao, 2009; Krupka & Weber, 2009, 2013; Servátka, 2009; Gächter et al., 2013; Reuben & Riedl, 2013; Kimbrough & Vostroknutov, 2015; Thöni & Gächter, 2015). This can be seen as a continuation and extension of behavioral economists' intense interest in social preferences, such as fairness or altruism (Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000; Andreoni & Miller, 2002; Cappelen et al., 2007; 2013a, Thöni & Gächter, 2015, among many others).

While both preferences and norms can be, to an extent, inferred from observed behavior, eliciting perceived social preferences and norms directly from subjects can be another useful way of collecting data. However, economists often view self-report measures as problematic, because they are in theory just cheap talk: the experimenter (often) cannot rule out the possibility that respondents are not motivated to provide truthful and accurate responses unless he himself provides them with incentives to do so (see Smith, 1982; Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001; Trautmann & van de Kuilen, 2015).

Making subjects' responses incentive-compatible can be especially challenging when eliciting normative or fairness judgments, as "objective" criteria against which to compare the responses are not obvious. To deal with this methodological obstacle, Krupka & Weber (2013) introduce the first incentive-compatible method for elicitation of social norms (the KW game henceforth). A test of this method will be the focus of this chapter.

The KW game is a pure matching coordination game in which each participant receives a monetary reward if his or her rating of social appropriateness of a randomly chosen action

matches the modal rating of this action's appropriateness in his or her experimental session. Shared social norms are assumed to create focal points for the judgment of appropriateness on which participants can coordinate (Krupka & Weber, 2013).

The KW game has been used to measure perceived social appropriateness of actions in various games, such as the dictator game and gift-exchange game (Gächter et al., 2013; Krupka & Weber, 2013; Krupka et al., 2014; Chang et al., 2015; D'Adda et al., 2015; Erkut et al., 2015; Kimbrough & Vostroknutov, 2015). Burks & Krupka (2012) use the KW game to identify norms for on-the-job behavior among employees at a financial services company. Vesely & Klöckner (2015) use the KW game to elicit shared perceptions of different predictors of pro-environmental behaviors (social norms, difficulty, and environmental effect).

However, the aforementioned studies do not attempt to show that the KW game actually leads to different ratings of social appropriateness than a comparable incentive-free rating method. In this chapter, I therefore provide a comparison of the KW game to such a non-incentivized, but otherwise very similar rating method[106].

As mentioned above, in the experimental economics tradition material incentives are commonly used to motivate decisions. However, in some cases cash incentives do not seem to make a difference. For example, meta-analyses by Zelmer (2003) and Engel (2011) show that

[106] Xiao & Houser (2005, 2009) and Houser & Xiao (2011) use a game very similar to the KW game for classification purposes. Houser & Xiao (2011) find that coders who are financially rewarded for successful coordination on shared meanings of messages they classify are more responsive to classification criteria than coders in a traditional unincentivized content analysis treatment. However, as the authors mention "content analysis differs from our coordination game in multiple ways (e.g., evaluating according to what one thinks others believe as compared to what oneself believes)" (p. 7), thus the presence of incentives is only one possible explanation of the observed differences between coders' performance across the two treatments in Houser and Xiao's experiment.

the presence (vs. absence) of pecuniary incentives in public goods games and dictator games, respectively, does not robustly significantly predict decisions. To give another example more closely related to the present study, Gächter & Renner (2010) elicit beliefs about other group members' contributions to a common group project (the "public good"). Incentivized beliefs were more accurate in periods 2-10 of the experiment (i.e. when participants already had experience with others' behavior), but beliefs with and without incentivization did not differ in terms of accuracy in period 1. In their survey article, Camerer & Hogarth (1999) show that the effects of incentives tend to be "mixed and complicated" (p. 8) and "depend dramatically on the kind of task a person performs" (p. 20)[107]. It is therefore reasonable to ask whether incentivizing "correct" (i.e. modal) responses in the KW game has an effect.

So far, the KW game has been used to rate social appropriateness of actions (see citations above). However, its use can be easily extended to elicit shared judgments of fairness and other aspects of behavior. Hence, participants in my study rate social appropriateness and fairness of possible actions in the ultimatum game (UG, Güth et al., 1982). It is quite possible that incentivization will matter when eliciting one type of judgment, but not the other, for example because one type of judgment could be more difficult to make.

Fairness is often studied using UG and its variants. Still, experimental participants are seldom asked about their actual fairness perceptions, as noted by van Winden (2007). Elicitation of fairness (and normative) judgments is important because knowing the actual fairness and normative perceptions eliminates the need to model them based on a priori assumptions, casual observation and/or introspective intuitions. The KW game enables elicitation of impartial fairness perceptions of different actions in UG in an incentive-compatible way. This

[107] In some cases the effect of incentives also depends on the kind of person who performs the task (Ben-Ner et al., 2008).

has probably never been done before: for example in Leliveld et al. (2009), Reuben & van Winden (2010), Ruff et al. (2013), and Rustichini & Villeval (2014) participants rated fairness of actions in UG or similar games, but these ratings were not incentive-compatible.

2. METHOD

A convenience sample of 270 university students from all fields of study (173 females) participated in the study across 16 sessions, each of which lasted for approximately 1 h. Participants were on average 22.5 years old (SD = 2.3). Participants earned 166.5 CZK (approximately $7.7) on average including the show-up fee. The show-up fee was 90 CZK in the KW game treatment and 120 CZK in the Non-incentivized treatment.

The experiment was implemented as a 2 (elicitation method: KW game vs. Non-incentivized) * 2 (order of presentation: fairness rated first vs. social appropriateness rated first) * 11 (which offer is being rated: $0, $1, …, $10) mixed design with two dependent variables (fairness rating and appropriateness rating). "Elicitation method" and "order of presentation" are independent factors. Combination of these factors yields four independent conditions to which participants were randomly assigned. "Offer being rated" is a repeated measures factor.

I adapted the instructions from Krupka & Weber (2013) and I ran the experiment in a paper-and-pencil form, as they did[108].

[108] Experimental instructions are available in Appendix F. Two guessing games were played after the main part of the experiment and a brief cognitive ability test (CRT) was administered. The results from these tasks are not reported here.

Participants in all conditions rated social appropriateness and fairness of all possible actions a proposer can take in UG. There were 11 possible actions for the proposer (to offer \$0-\$10 in \$1 increments), hence there were 11 ratings of social appropriateness and 11 ratings of fairness per participant. Fairness and social appropriateness were rated on a four-point scale as e.g. "very unfair", "somewhat unfair", "somewhat fair" and "very fair".

In both the KW game treatment and in the Non-incentivized treatment participants were asked to rate the offers from the position of a third, neutral party that is not involved in the UG. Also, in both treatments, participants were asked to provide what they thought would be a "common" or "most frequent" rating, rather than their "personal" rating.

The key experimental manipulation is described next.

In the KW game treatment, but not in the Non-incentivized treatment, two possible actions in the UG were randomly (with replacement) selected at the conclusion of the experiment. Participant received a monetary reward (75 CZK) if his/her rating of social appropriateness matched the modal rating of social appropriateness in his/her session for the first selected action, and he/she received (another) 75 CZK if his/her rating of fairness matched the modal rating of fairness in his/her session for the second selected action.

In the Non-incentivized treatment participants rated the choices without incentives. However, they were also asked not to provide their "personal" ratings, but rather "what they think would be 'common' ratings", such as ratings "they think would be the most common among people in today's session". This request in the instructions points to a possible focal point (the modal

rating) and thus makes the Non-incentivized treatment as similar to the KW game treatment as possible, except for the incentivization.

3. RESULTS AND DISCUSSION

Table 1 presents means and standard errors of incentivized and non-incentivized ratings of social appropriateness and fairness of proposer's possible choices. The ratings are coded so that the lowest rating (e.g. "very unfair") has the value equal to 1, while the highest rating has the value equal to 4.

Table 1: Incentivized and non-incentivized ratings of proposer's choices: mean (S.E.)

Proposer's offer being rated	Ratings of social appropriateness in the KW game treatment	Ratings of social appropriateness in the Non-incentivized treatment	Ratings of fairness in the KW game treatment	Ratings of fairness in the Non-incentivized treatment
\$0	1.09 (0.03)	1.09 (0.03)	1.07 (0.03)	1.05 (0.03)
\$1	1.42 (0.05)	1.29 (0.05)	1.23 (0.04)	1.22 (0.04)
\$2	1.78 (0.04)	1.57 (0.06)	1.61 (0.05)	1.55 (0.05)
\$3	2.32 (0.06)	2.16 (0.06)	2.07 (0.05)	2.01 (0.06)
\$4	2.98 (0.05)	2.94 (0.05)	2.87 (0.05)	2.79 (0.05)
\$5	3.91 (0.03)	3.91 (0.02)	3.96 (0.02)	3.97 (0.02)
\$6	3.44 (0.06)	3.44 (0.05)	3.09 (0.06)	3.06 (0.06)
\$7	3.14 (0.07)	3.20 (0.07)	2.45 (0.07)	2.30 (0.07)
\$8	2.76 (0.08)	2.83 (0.08)	1.96 (0.07)	1.93 (0.07)
\$9	2.56 (0.09)	2.58 (0.10)	1.73 (0.08)	1.65 (0.08)
\$10	2.48 (0.10)	2.48 (0.11)	1.59 (0.09)	1.50 (0.08)

Note: There were 137 raters in the KW game treatment and 133 in the Non-incentivized treatment.

Table 1 shows, for example, that the mean rating of social appropriateness of the lowest possible offer (\$0) is 1.09 both in the KW game treatment and in the Non-incentivized treatment and that the mean rating of fairness of the \$0 offer is 1.07 in the incentivized KW

game treatment and 1.05 in the Non-incentivized treatment. The key comparisons in my experiment (to be reported below) will test for differences between ratings summarized in the second vs. the third column in Table 1 (i.e. between appropriateness ratings with vs. without incentives) and between ratings summarized in the fourth vs. the fifth column (i.e. between fairness ratings with vs. without incentives). No important differences between ratings with vs. without incentives are apparent from Table 1.

Rather than applying statistical tests to mean ratings of specific offers (e.g. \$0), I first fit four straight lines to each subject's data (as described in the following) and then test for differences between the slopes of the fitted lines. More specifically, this way I obtain four new variables, SlopeApp1 and SlopeApp2 (slopes of lines fitted to ratings of social appropriateness of offers \$0-\$5 and \$5-\$10, respectively) and SlopeFair1 and SlopeFair2 (slopes of lines fitted to ratings of fairness of offers \$0-\$5 and \$5-\$10, respectively). These new variables will be used in subsequent analyses[109].

Because part of the subjects rated social appropriateness first and fairness second and part of the subjects the other way round, I first test for the presence of order effects with a series of four *t*-tests. Using an independent samples *t*-test I find no significant differences in SlopeApp1 between subjects who rated appropriateness first and those who rated it second, $t(268) = 0.240$, $p = .81$, $r = .01$ (negligible effect); mean difference = 0.00 (95% *CI* = -0.02, 0.03)[110]. Similarly, I find no significant differences in SlopeApp2 between these two groups of subjects, $t(268) = -1.219$, $p = .22$, $r = .07$ (very small effect); mean difference = -0.04 (95%

[109] I thank Jon Baron for suggesting this approach to analysis and for helping me with it. While basing the tests on offers \$0-3, \$4-6 and \$7-10, rather than \$0-5 and \$5-10, would also have some appeal, this would be problematic because the ratings of \$4-6 have the shape of inverted U, and thus would be hard to approximate with a straight line.

[110] The respective null hypothesis in this and all subsequent cases in this chapter posits no difference, on average, between the compared slopes.

CI = -0.10, 0.02). Likewise, I find no significant differences in SlopeFair1 between these two groups of subjects, $t(268) = -0.612$, $p = .54$, $r = .04$ (negligible effect); mean difference = -0.01 (95% *CI* = -0.03, 0.02). Finally, I do not find any significant differences between these two groups of subjects in SlopeFair2, either, $t(268) = -0.105$, $p = .92$, $r < .01$ (negligible effect); mean difference = 0.00 (95% *CI* = -0.05, 0.05)[111]. Consequently, I pool the data from participants who rated social appropriateness as first and those who rated it as second in all subsequent analyses.

Now I proceed to the main comparisons which will show whether there is an effect of providing (vs. not providing) incentives to raters on the ratings of social appropriateness and/or fairness.

Using an independent samples *t*-test I find no significant differences in SlopeApp1 between subjects in incentivized KW-game treatment and subjects in Non-incentivized treatment, $t(268) = -0.681$, $p = .50$, $r = .04$ (negligible effect); mean difference = -0.01 (95% *CI* = -0.04, 0.02).

Similarly, using an independent samples *t*-test I find no significant differences in SlopeApp2 between subjects in incentivized KW-game treatment and subjects in Non-incentivized control treatment, $t(268) = -0.077$, $p = .94$, $r < .01$ (negligible effect); mean difference = 0.00 (95% *CI* = -0.07, 0.06).

[111] These are results of *t*-tests applied to the entire data set. When performing the same analyses separately for data from treatments with and without incentives, the obtained results are qualitatively similar.

Next, with an independent samples *t*-test I find no significant differences in SlopeFair1 between subjects in KW-game treatment and subjects in control, $t(268) = 0.152$, $p = .88$, $r < .01$ (negligible effect); mean difference = 0.00 (95% *CI* = -0.02, 0.03).

Finally, with an independent samples *t*-test I find no significant differences in SlopeFair2 between subjects in KW-game treatment and subjects in control, $t(268) = 0.443$, $p = .66$, $r = .03$ (negligible effect); mean difference = 0.01 (95% *CI* = -0.04, 0.06).

To complement the analyses reported above, I also run two separate three-way mixed ANOVAs, one with social appropriateness rating as dependent variable, the other with fairness rating as dependent variable (the predictors are: the presence of incentives, whether appropriateness or fairness was rated first, and the offer being rated).

The first ANOVA reveals that appropriateness ratings differ across offers, $F(2.38, 630.83) = 483.61$, $p < .001$, effect size $\eta^2 = .65$ (large effect)[112]. Bonferroni-corrected pairwise comparisons between offers reveal, for example, that the equal split ($5 offer) is more appropriate than any other offer. Offering $6 is more appropriate than any other offer except $5. Offering $0-$2 is less appropriate than offering any higher offer[113]. Importantly, I found no significant effect of the presence of incentives on social appropriateness ratings $F(1, 266) = 0.75$, $p = .39$, effect size $\eta^2 = .003$ (negligible effect). There was also no significant effect of the order of presentation, $F(1, 266) = 0.50$, $p = .48$, $\eta^2 = .002$ (negligible effect).

The second ANOVA reveals that fairness ratings differ across offers, $F(3.28, 872.67) = 620.08$, $p < .001$, $\eta^2 = .70$ (large effect). Bonferroni-corrected pairwise comparisons between

[112] Greenhouse-Geisser adjusted *F* is used in both ANOVAs because of violation of sphericity.
[113] All reported differences are significant at $p < .05$ (after correction).

different offers reveal that the equal split is more fair than any other offer. Offering \$6 is more fair than any other offer except \$5, offering \$4 is more fair than any other offer except \$5 and \$6. Offering \$0 or \$1 is less fair than offering any higher offer[114]. Importantly, I found no significant effect of the presence of incentives on fairness ratings $F(1, 266) = 1.29$, $p = .26$, effect size $\eta^2 = .005$ (negligible effect). There was no significant effect of the order of presentation, either, $F(1, 266) = 0.50$, $p = .48$, $\eta^2 = .002$ (negligible effect).

To conclude, ratings differ across offers, while – which is the main result – incentives do not matter when rating social appropriateness or fairness of actions, at least not in simple bargaining situations such as UG. It is interesting that equal splits are more fair and more socially appropriate not only than all lower offers, but also than all higher offers. Offering more than the equal split simply does not pay in terms of appearing more fair and more socially appropriate (cf. Klein & Epley, 2014; Klein et al., 2015). This observation, together with the relative costliness of offers above the equal split, suggests an explanation of the rarity of "hyper-fair" offers in UG (similar conclusions can be drawn about donations in the dictator game based on the findings in Krupka & Weber, 2013).

The effect of the presence of incentives on ratings of both appropriateness and fairness is negligible ($r < .05$ in case of the four *t*-tests of the incentive effect reported above; $\eta^2 = .002$ for the effect of incentives in both ANOVAs reported above). Based on these results I draw the above conclusion that using incentives virtually does not change elicited normative/fairness judgments. Admittedly, the small effect sizes also mean that I do not have sufficient power to reject the null hypothesis (of no effect of incentives) if it is in fact false.

[114] All reported differences are significant at $p < .05$ (after correction).

However, I base my conclusion about the minimal effect of incentives not primarily on the significance tests, but rather on the estimated effect sizes.

One can think of at least two situations where cash incentives would not affect responses (Camerer & Hogarth, 1999 lay out similar arguments). Consider the following example first. A respondent understands, even absent incentivization, that an action is socially inappropriate. He is not sure, however, whether it is "very" or "somewhat" inappropriate. Offering him incentives for judging the action correctly will not make it any easier for him to discern between the "very" and "somewhat" inappropriate character of that action. This is a situation where incentives would not help because the task is too difficult. Conversely, the effect of incentives could be negligible in very easy tasks where participants do not need any additional motivation to provide the correct response. Thus, it remains an open question whether incentivization of socially shared judgments may not have an effect in more complex settings[115].

My results suggest that researchers using the KW elicitation protocol might want to follow the "do it both ways" recommendation by Hertwig & Ortmann (2001) and include an unincentivized KW protocol as a control in their future experiments. Consequently, when further evidence accumulates, a more general conclusion concerning the importance of material incentives for normative and fairness judgments can be drawn.

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[115] This study was not designed to explain *why* incentives may or may not work in the KW game. A different design would be needed for that. I thus stress that any attempt at such an explanation must be necessarily regarded as somewhat speculative. Besides my preferred explanation along the lines of Camerer & Hogarth (1999), one may invoke several other possible explanations. For example, crowding out of intrinsic motivation by extrinsic incentives (e.g. Deci, 1971; Frey & Oberholzer-Gee, 1997; Deci et al., 1999; Gneezy & Rustichini, 2000). It is conceivable that when participants are paid, they perceive the situation differently ("a job") than when they are not paid ("helping science", "having fun"), with the possible consequence of decreased intrinsic motivation under material incentives. Based on a casual observation of the participants, it does not seem likely to me that intrinsic motivation has been crowded out by the cash rewards in this particular case.

Incentivization is more likely to have an effect on "non-volunteer" participants with low intrinsic motivation to perform well or to provide truthful responses due to e.g. self-presentation bias or self-signaling (Bodner & Prelec, 2003). It could be also relatively more effective in supporting performance in time-consuming, lengthy tasks. Finally, could the fact that the raters evaluate a strategic situation (in which they are not involved, however) affect the results? The strategic nature of UG gives the proposer relatively little scope for ignoring distributive norms (see e.g. Rustichini & Villeval, 2014). It is therefore possible that incentives would be more effective when rating actions in a non-strategic game, such as the dictator game, because distributive norms might be generally less clear in the dictator game and stating them might be therefore more costly in terms of cognitive effort on the rater's part.

In the remainder of the chapter I at least briefly consider the similarities and differences between appropriateness and fairness ratings[116]. A paired *t*-test comparing SlopeApp1 and SlopeFair1 shows only marginally significant, small differences, $t(269) = -1.762$, $p = .08$, $r = .11$ (small effect); mean difference = -0.01 (95% *CI* = -0.02, 0.00). This *t*-test reveals that low offers ($0-$5) receive approximately the same social appropriateness and fairness ratings.

In contrast, there are significant differences between the two types of ratings when high offers are being rated. A paired *t*-test comparing SlopeApp2 and SlopeFair2 shows highly significant, large differences, $t(269) = 10.897$, $p < .001$, $r = .55$ (large effect); mean difference

[116] I pool data from the KW game treatment and from the Non-incentivized treatment in the tests that follow. The following tests assume comparability of the two types of ratings. I concede that this assumption might be seen as somewhat problematic. However, participants consecutively rated fairness and appropriateness of exactly the same offers in exactly the same format, thus the comparability of the ratings seems acceptable. Assuming comparability, paired *t*-test is an appropriate test for assessing whether there are differences in the two types of ratings. Readers who are, however, not willing to accept the assumption of comparability of the appropriateness and fairness ratings should disregard the following tests.

= 0.19 (95% *CI* = 0.15, 0.22). This comparison reveals that high offers ($5-$10) receive significantly higher social appropriateness ratings than fairness ratings.

I am not aware of any previous studies comparing fairness and appropriateness perceptions of strategic decisions. While comparing results across studies can be tricky because of differences in designs, looking at results reported in Krupka & Weber (2013), Rustichini & Villeval (2014), and Erkut et al. (2015) suggests a pattern of similarities and differences between fairness and appropriateness judgments broadly similar to that reported here:

1) In particular, in Rustichini & Villeval (2014) participants judge offers in UG exceeding approx. 66-71% of the pie as unfair; similarly, offers in dictator game exceeding approx. 69-71% of the pie are rated as unfair. In contrast, all dictator offers exceeding the 50-50 split are still judged as socially appropriate on average in Krupka & Weber (2013) and Erkut et al. (2015). I.e., high offers are unfair (Rustichini & Villeval), but appropriate (Krupka & Weber, Erkut et al.)

2) At the same time, there does not seem to be such a misalignment between these three papers when it comes to ratings of fairness and social appropriateness of low offers: in Rustichini & Villeval (2014) participants judge offers below approx. 20-23% of the surplus in UG and below approx. 19-22% in dictator game as unfair. Quite similarly, offering 30% of the surplus or less in dictator game is rated as inappropriate on average in Krupka & Weber (2013) and Erkut et al. (2015). I.e., low offers are both unfair and inappropriate.

The results in these three papers are thus qualitatively similar to results reported in this chapter, i.e. low offers score low on both appropriateness and fairness, while high offers are

rated as more appropriate than fair. New experiments are needed to obtain a deeper explanation of these observations.

4. CONCLUSION

The KW game is a promising technique of capturing socially shared judgments of appropriateness, fairness and potentially many other aspects of behavior. I elicit impartial ratings of social appropriateness and fairness of all possible offers in UG. However, I show that incentive-compatibility of this elicitation mechanism in not important in some contexts, such as the UG. It remains an open question whether incentives would affect judgments of social appropriateness and fairness in different, e.g. more complex contexts. I also find that ratings of social appropriateness and fairness are similar for low offers, but not for high offers which are judged to be more appropriate than fair.

CHAPTER 9: GLOBAL SOCIAL NORMS AND ENVIRONMENTAL BEHAVIOR[117]

Abstract

Social norms are important predictors of environmental behavior. However, the concept of social norms is rather fuzzy and measuring social norms is a challenge in many contexts. In this paper, we adopt a recently introduced incentivized method (Krupka & Weber, 2013) to elicit global, widely shared beliefs concerning social appropriateness, environmental effect, and difficulty of a wide range of environmental behaviors. Based on two seperate student samples (n=25 and n=61), we establish that these characteristics, as reflected in the elicited shared beliefs recorded in one of the samples, predict environmental behaviors in the second, separate sample. Pro-environmental behaviors perceived to be more socially appropriate and easier to perform, in particular, are more likely to be chosen. We show further that subjective social norms mediate the effect of global social norms on behavior, which improves our understanding of the normative processes underlaying pro-environmental action. As argued in the paper, the use of the Krupka-Weber method might moreover mitigate problems associated with conventional surveys, such as social desirability bias, consistency bias, and inattentive responding.

[117] This chapter is based on a manuscript co-authored by Christian A. Klöckner (Stepan Vesely being the first author).

1. INTRODUCTION AND THEORETICAL BACKGROUND

That other people's expectations and behaviors have an impact on the performance or non-performance of environmentally relevant actions is a well-established finding (e.g. Heath & Gifford, 2002; Schultz et al., 2007, 2014; Nolan et al., 2008). Normative messages have been successfully used in experiments and interventions designed to boost pro-environmental behaviors (Schultz et al., 2007, 2014; Goldstein et al., 2008; Nolan et al., 2008; Kormos et al., 2015). Normative influences on behavior, either in the form of social norms or in the form of personal norms, are also postulated in influential action models like the theory of planned behavior (Ajzen, 1991), the norm-activation theory (Schwartz & Howard, 1981), and the value-belief-norm theory (Stern, 2000).

This social impact on environmental behavior has been conceptualized in several ways in different theoretical and empirical traditions: Most importantly, a distinction between injunctive norms and descriptive norms has been made (Cialdini et al., 1990; Thøgersen, 2006). Following Cialdini et al. (1990) and Fehr & Gächter (2000b) we define injunctive social norms as widely shared beliefs of how one ought to behave in a given situation. We in turn define descriptive social norms as behaviors that are common in a given situation (Cialdini et al., 1990). With respect to the mechanisms behind the effects of social influence on behavior, there are several possible reasons for norm compliance, such as avoiding informal sanctions (Fehr & Gächter, 2000a) or recognizing social norms' legitimacy (Bicchieri, 2006).

In spite of its usefulness in theoretical models, the measurement of social impact is not as straightforward as it seems. Meaningful advances in understanding normative processes in the environmental domain are possible (see e.g. Thøgersen, 2006; Schultz et al., 2007) and

needed (Göckeritz et al., 2010) to improve on the way social norms are measured and theoretically conceptualized. In this paper, we employ a novel method for elicitation of social norms (introduced by Krupka & Weber, 2013) to measure social appropriateness of different environmental behaviors. Unlike most operationalizations of social norms in environmental research, the Krupka-Weber protocol taps widely shared understanding of social norms, rather than local family or peer norms.

Our study is the first application of the Krupka-Weber elicitation protocol in environmental research, where the method can prove especially useful because: (a) social norms are an important predictor of environmental behavior; (b) the method helps mitigate social desirability and consistency biases; (c) the method captures widely shared ("global") understandings of social norms, which are usually missing in current environmental research. We will return to these issues below.

1.1 THE KRUPKA-WEBER PROTOCOL

The Krupka-Weber elicitation method has been used so far mainly to explain behavior in various experimental games (Gächter et al., 2013; Krupka & Weber, 2013; Krupka et al., 2014; Chang et al., 2015; D'Adda et al., 2015). The method has helped solve some important puzzles, such as why changing the set of available actions leads to unexpected shifts in behavior (List, 2007; Bardsley, 2008; Krupka & Weber, 2013). The method was also used in a field setting to identify social norms for on-the-job behavior among employees at a financial services company (Burks & Krupka, 2012).

The Krupka-Weber elicitation method is based on a "coordination game" (henceforth, the KW game) in which participants rate social appropriateness of different behaviors. In our study, specifically, participants rate different alternative courses of action – e.g. recycling vs. not recycling paper – for 56 environmentally relevant behaviors on a 4-point scale ranging from very socially inappropriate to very socially appropriate.

A key aspect of the KW game is that each participant receives a monetary reward if his/her stated appropriateness rating of a randomly selected action matches the modal appropriateness rating of this action in his/her session. How can the participants figure out what is likely to be the modal rating? Krupka & Weber (2013) postulate that tacitly shared social norms are assumed to create focal points on which the raters can coordinate when submitting their ratings. This assumption is justified both theoretically and empirically: In coordination games (including the KW game) there are multiple Nash equilibria on which players can coordinate and assumptions of rationality and common knowledge invoked in game theory are unable to select one of these equilibria (Young, 1996; Sugden, 2001). At the same time, previous experimental research shows that participants in coordination games tacitly coordinate on one of the equilibria based on their shared cultural background, social norms and so on (Schelling, 1960; Mehta et al., 1994; Lindahl & Johanesson, 2009; Bardsley et al., 2010). To give an example, consider two drivers passing each other on a narrow street. Absent traffic rules, conventions, shared preferences and the like, there is no a priori reason for the drivers to coordinate on driving on the right side, or on driving on the left side. They have to rely on some tacitly shared understanding (a traffic rule in this case) to solve the game (Young, 1996). This implies that raters in the KW game should not submit their private normative judgments, but rather their best guess of normative judgments widely shared in the society (see also Krupka et al., 2014).

Social norms as obtained with the KW game have certain desirable properties, which we discuss in the following subsections.

1.2 GLOBAL AND LOCAL SOCIAL NORMS

With the KW game, we manage to capture global, widely shared normative judgments. Being widely shared is one of the characteristic features of social norms (Gibbs, 1965; Bicchieri, 2006). In contrast, the now standard operationalization of social norms in environmental research relies on respondents' beliefs about normative opinions and/or common behaviors of people close to the responder, such as family and friends (e.g. Heath & Gifford, 2002; Nigbur et al., 2010; for exceptions see Goldstein et al., 2008; Nolan et al., 2008). We call social norms elicited using this standard operationalization local social norms, because they are shaped by the responders' social micro-environment.

In the present study, we measure both types of social norms: global and local. We test whether the effect of global social norms on behavior is mediated by local social norms. This would make sense because we can think of small social groups, such as families, as creating their own local social norms in part by adopting portions of global social norms[118]. Because people can sometimes follow (global) social norms even if this does not agree with their more immediate motivations (Crawford & Ostrom, 1995; Binmore, 2010), it would also make sense if global social norms had an additional direct effect on behavior, not mediated by local social norms.

[118] Empirical research on the transmission of pro-environmental social norms within society seems to be lacking. Grønhøj & Thøgersen (2012) and Matthies et al. (2012), however, explore the related topic of pro-environmental norms transmission within families.

1.3 SOCIAL DESIRABILITY AND INATTENTION

In the KW game, each rater has a monetary incentive (the bonus for matching the modal rating) to state accurately and truthfully his/her belief about others' judgments concerning appropriateness of different possible environmentally relevant behaviors. Making the rating task incentive compatible in this way should mitigate biased responding and errors due to inattention and/or lack of motivation (Smith, 1982; Camerer & Hogarth, 1999; Hertwig & Ortmann, 2001; Bodner & Prelec, 2003; Gächter & Renner, 2010; Hagger et al., 2010).

Errors due to inattention. While incentivizing responses in order to sustain participants' attention may be unnecessary in cases when people can be expected to remain attentive throughout the task/study, it can be useful in lengthy and/or repetitive tasks (Camerer & Hogarth, 1999). In lengthy and repetitive tasks, participants' attention typically wanes without incentivization (Davies & Parasuraman, 1982). However, when, as in the present study, people are paid for only a small number of rating decisions randomly selected ex post, they should make all decisions with the same care as if any of them could be the one relevant for payment. The validity of this kind of incentivization procedure has been tested e.g. in studies on risk preferences where people make numerous choices between gambles, but only one of these choices is then randomly selected for payment (Starmer & Sugden, 1991; Cubitt et al., 1998; see, however, Cox et al., 2015).

Social desirability bias. A number of studies show that participants misreport their pro-environmental behaviors (McGuire, 1984; Corral-Verdugo, 1997; Lam & Cheng, 2002; Chao & Lam, 2011). Social desirability (e.g. Paulhus, 1984) is commonly invoked as a reason

behind the misreporting, see Hartig et al. (2001), Chao & Lam (2011). Socially desirable responding can be a problem not only in reporting behavior, but also in reporting pro-environmental intentions (Chao & Lam, 2011), motivations to adopt green innovations (Noppers et al., 2014), preservation values (Wiseman & Bogner, 2003), pro-environmental attitudes (Milfont, 2009), or environmental consciousness (Stocké & Hunkler, 2007). We acknowledge, however, that sometimes the role of social desirability in environmental research appears to be small to negligible (Kaiser, 1998; Kaiser et al., 1999; Milfont, 2009). We thus note that more research on the role of social desirability in reporting environmental behaviors and beliefs is needed.

While not telling the truth can make the respondent look better, it also has its costs (Rasinski et al., 1999). The subjective costs include the risk of detection and its consequences, disutility from harming others by lying (Gneezy, 2005), disutility due to one's intrinsic aversion to lying (Lundquist et al., 2009), and disutility from damaging one's self-image as an honest person (Fischbacher & Föllmi-Heusi, 2013). The Krupka-Weber elicitation method should reduce socially desirable misreporting because the costs of misreporting are higher when using the KW game than when using an unincentivized procedure (cf. Bodner & Prelec, 2003). If the participant biases his/her stated appropriateness ratings to look more desirable, he/she increases the risk of miscoordination with other raters which is costly in the KW game (assuming participants are not able to tacitly coordinate on biased ratings).

1.4 CONSISTENCY BIAS

People often like being and appearing consistent (Aronson & Carlsmith, 1962; Cialdini et al., 1995; Oxoby, 2004; Falk & Zimmermann, 2011). This seems to be the case also in the

context of environmental behavior (Kantola et al., 1984; Kaiser, 1998; Thøgersen, 2004). Being inconsistent may trigger the aversive feeling of cognitive dissonance (Festinger, 1957; Elliot & Devine, 1994). Moreover, appearing inconsistent may also give rise to perceived social pressure towards greater consistency (Rabin, 1994; Falk & Zimmermann, 2011).

Research findings will be biased if participants have a strong enough preference for appearing more consistent than they in fact are. For example, a participant can have personal norms that require her to behave pro-environmentally, but she does not. If she prefers to appear consistent, she may overstate her behavior, understate her personal norms, or both. As a result, the apparent level of variables and associations between them would be affected.

Findings in Elffers et al. (1987), Hessing et al. (1988) and Wenzel (2005), for example, suggest that respondents adjust their reported tax-paying behavior and/or their subjective social norms related to this behavior in such a way so as to appear consistent. Relatedly, results in Presser & Traugott (1992) suggest that non-voters who are interested in public affairs misreport about having voted to match their reported high level of interest in public affairs. In three studies, Falk & Zimmermann (2013) show that respondents adjust their subsequent answers in a questionnaire so as to retain consistency with their answers to previous items.

A related stream of evidence shows that people adjust their behavior in experiments when their beliefs are elicited first (Croson, 1999, 2000; Krupka & Weber, 2009; Ruström & Wilcox, 2009; but see Gächter & Renner, 2010; D'Adda et al., 2015). In this case, however, also other reasons besides a taste for consistency, such as learning, might be involved (see Weber, 2003; Ruström & Wilcox, 2009).

Following Krupka & Weber (2013) we elicit the environmental behaviors' appropriateness ratings from one group of participants, and then use the ratings to explain environmental behaviors in another, separate group of participants (drawn from the same subject pool). This way we give participants no opportunity to adjust their stated "global" social norms and stated behaviors so as to appear consistent.

2. METHOD

2.1 PARTICIPANTS

Two separate, non-overlapping groups of subjects participated in two parts of the study. 61 subjects participated in Part 1, and 25 in Part 2. Participants were students at Masaryk University from different fields of study[119]. The study was computerized (programmed in z-Tree, Fischbacher, 2007) and it was conducted in summer 2014 at Masaryk University in Brno.

Participants in Part 1 received a 250 CZK show-up fee (~ 12 USD; 100 CZK was about 4.8 USD at the time of the experiment). Participants in Part 2 received a 180 CZK show-up fee (~ 8.6 USD) and a performance bonus of 0-450 CZK (0-21.6 USD, approximately). Mean earnings in Part 2 were 468 CZK (~ 22.5 USD). All sessions lasted under 2 hours.

[119] The sample was a convenience sample. Participants' mean age = 22.7 years (SD = 3.1). 69% of participants were females.

2.2 PART 1 OF THE STUDY

In Part 1, 61 participants (a) answered a questionnaire (56 items), and (b) made decisions in nine decision tasks (not reported in this chapter). Questionnaire items were divided into six thematic blocks: garbage (7 items), water and power consumption (16 items), consumer behavior (11 items), garbage inhibition (7 items), automobile use (7 items), volunteering in nature-protection activities/political support (8 items). The order of presentation of the thematic blocks was randomized. The items were mostly based on Kaiser (1998)[120].

Each questionnaire item consisted of six sub-items:

(1) Behavior. Participants reported whether they most of the time performed the behavior in question. This was a multiple choice sub-item, usually with two possible responses, for example: "Do you wait to have a full load before doing your laundry?" with possible responses "Yes" and "No". We will call each possible response a "behavioral alternative" in what follows[121].

To make clear the description of the next two sub-items, we should point out there was always precisely one *positively formulated* behavioral alternative (e.g. the "Yes" answer to the question "Do you return dead batteries to collection points for dangerous waste?"), precisely one *negatively formulated* behavioral alternative (e.g. the "No" answer to the question "Do you return dead batteries to collection points for dangerous waste?"), and sometimes other alternatives as well (e.g. the "I do not use batteries" response to the batteries question).

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[120] See Appendix G.1 for the instructions for Part 1.
[121] See Appendix G.3 for a complete list of the Behavior sub-items and all possible responses (behavioral alternatives).

(2) Subjective descriptive social norm. Participants indicated how many people they personally know perform the *positively formulated* behavioral alternative in question. What the "positively formulated" behavioral alternative stands for is described in the previous paragraph. We used a 7-point response scale where 1 means "no one I personally know does this", 4 means "about half of the people I know do this" and 7 means "everyone I personally know does this".

(3) Subjective injunctive social norm. Participants indicated whether people close to them approved of the *positively formulated* behavioral alternative in question. We used a 7-point response scale where 1 stands for "people close to me very strongly disapprove of this behavior" and 7 stands for "people close to me very strongly approve of this behavior".

(4) Importance of social appropriateness. Participants indicated whether a behavior's social appropriateness was important for them when they decided whether to perform the behavior. We used a 7-point response scale where 1 stands for "social appropriateness of the behavior is completely irrelevant when I decide whether to perform that behavior or not" and 7 stands for "social appropriateness of the behavior is extremely important when I decide whether to perform that behavior or not".

(5) Importance of environmental effect. Participants indicated whether a behavior's environmental effect was important for them when they decided whether to perform the behavior. We used a 7-point response scale where 1 stands for "the effect the behavior has on the natural environment is completely irrelevant when I decide whether to perform that behavior or not" and 7 stands for "the effect the behavior has on the natural environment is extremely important when I decide whether to perform that behavior or not".

(6) Importance of difficulty. Participants indicated whether a behavior's difficulty was important for them when they decided whether to perform the behavior. We used a 7-point response scale where 1 stands for "the difficulty or inconvenience of performing the behavior is completely irrelevant when I decide whether to perform that behavior or not" and 7 stands for "the difficulty or inconvenience of performing the behavior is extremely important when I decide whether to perform that behavior or not".

2.3 PART 2 OF THE STUDY

In Part 2, each of 25 participants played 375 KW games in which he/she rated 125 alternative responses to the "Behavior" sub-items from Part 1[122]. Specifically, each participant rated the following characteristics of each possible behavioral alternative on a four- or six-point scale[123]:

- Social appropriateness: very socially inappropriate (coded as 1), somewhat socially inappropriate (2), somewhat socially appropriate (3), very socially appropriate (4).

- Effect on the environment: very harmful towards the environment (coded as 1), harmful towards the environment (2), somewhat harmful towards the environment (3), somewhat effective in protecting the environment (4), effective in protecting the environment (5), very effective in protecting the environment (6).

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[122] Recall there were 56 items in Part 1. These comprised 130 behavioral alternatives (responses) in total. However only 125 of the 130 responses were unique (a response is "unique" if it only appears once in the 130 responses). Three KW games per *unique* behavioral alternative were played – one in which social appropriateness was rated, one in which effect on the environment was rated, and one in which difficulty was rated (see below). This results in a total of 375 KW games being played.

[123] See Appendix G.2 for the instructions for Part 2.

- Difficulty: very difficult to carry out (coded as 1), somewhat difficult to carry out (2), somewhat easy to carry out (3), very easy to carry out (4).

Importantly, for one ex post randomly selected behavioral alternative (e.g. “Person A does not wait to have a full load before doing his/her laundry”), raters received a monetary reward (150 CZK) if their rating of social appropriateness of the selected behavioral alternative matched the modal appropriateness rating of the selected behavioral alternative in their session.

Similarly, for another randomly selected behavioral alternative, raters received a monetary reward (150 CZK) if their rating of difficulty of the selected behavioral alternative matched the modal difficulty rating of the selected behavioral alternative in their session. In the same way raters could receive another 150 CZK bonus for their rating of the effect on the environment.

All raters were therefore materially motivated to coordinate on the implicitly shared perceptions of social appropriateness, effect on the environment and difficulty when rating the behavioral alternatives.

3. RESULTS

3.1 DATA PREPARATION AND ANALYSIS STRATEGY

In our questionnaire, the positively formulated behavioral alternatives are usually those that are generally considered to be pro-environmental (e.g., Yes, I recycle), whereas the negatively

formulated behavioral alternatives are usually those generally considered to be environmentally unfriendly (e.g., No, I do not recycle). There are eight exceptions to this rule. In these eight cases the negatively formulated behavioral alternative was the pro-environmental one (e.g., No, I do not you use a tumble dryer).

For the purposes of subsequent analyses we created a set of 56 pro-environmental behavioral alternatives by including those eight negatively formulated pro-environmental behavioral alternatives, along with the remaining 48 positively formulated pro-environmental behavioral alternatives. Because in Part 1 we elicited the variables subjective descriptive social norm and subjective injunctive social norm only for the positively formulated behavioral alternatives, we reverse-coded these two variables in case of the eight negatively formulated pro-environmental alternatives (i.e., the value of the recoded variable is equal to 8 minus the original value).

Pro-environmental behavioral alternatives were chosen (reported) in 1700 out of 3416 cases (49.77%) in this study. Environmentally unfriendly behavioral alternatives were chosen in 1368 out of 3416 cases (40.05%). In the remaining 348 cases (10.19%) other alternatives (usually of the type "does not apply to me" or similar) were chosen[124].

Observation units for our analyses are single decisions participants make about performing or not performing each of the 56 pro-environmental behaviors. Thus, there is a total of 3416 behavioral choices analyzed, reported by 61 participants in Part 1 of the study. The behavioral choice is coded in a dichotomous way: 1 = reporting performing the environmentally friendly

[124] In Appendix G.3, I list all 56 questionnaire items asking about behaviors from Part 1 of the study. I also list all available behavioral alternatives (responses to the items) in Appendix G.3, classified as "pro-environmental", "environmentally unfriendly" and "other" responses.

alternative, 0 = reporting not performing the environmentally friendly alternative. Each choice was then regressed on a number of predictors obtained in Part 1 and Part 2 of the study, as outlined in the remainder of this subsection by means of a logistic regression. Due to the dependence of the data, the analyses were calculated with corrected standard errors according to the clustered structure of the data.

In Model 1, the global ratings of behavior's perceived social appropriateness, effect on the environment, and difficulty were included as predictors. All three predictors were obtained in Part 2 of the study (see section 2.3).

In Model 2, the subjective importance of each of these "global" factors was added, as well as the interactions between each of the three subjective importance variables and the respective global rating. To prevent false multi-collinearity both the importance variables and the ratings were centered before calculating the interaction term. While the global ratings were obtained in Part 2, the subjective importance variables were obtained in Part 1 of the study (see section 2.2).

In Model 3, subjective injunctive and descriptive norms measured in Part 1 were added to Model 1.

Finally, Model 4 extends Model 3 by adding the subjective importance of social appropriateness, environmental effect, and difficulty of behaviors, and the interactions between each subjective importance variable and the respective global rating.

3.2 THE MAIN ANALYSIS

Table 1 contains the results of the tests of the four models described above.

Table 1: Regression results

	Model 1		Model 2		Model 3		Model 4	
	B	OR	*B*	OR	*B*	OR	*B*	OR
Global social appropriateness	1.219***	3.385	1.221***	3.391	0.260	1.298	0.307#	1.359
Importance of appropriateness			-0.032	0.968			-0.056	0.946
Appropriateness * Importance			-0.165*	0.848			-0.064	0.938
Global environmental effect	0.126*	1.134	-0.027	0.974	0.070	1.073	-0.07	0.932
Importance of effect			0.335***	1.398			0.328***	1.389
Effect * Importance			0.094***	1.099			0.083**	1.087
Global difficulty	1.028***	2.796	1.071***	2.918	0.744***	2.104	0.774***	2.169
Importance of difficulty			-0.161***	0.852			-0.173***	0.841
Difficulty * Importance			0.045	1.047			0.082*	1.085
Injunctive norms					0.331***	1.392	0.303***	1.354
Descriptive norms					0.462***	1.588	0.483***	1.622
Model fit:								
Number of observations	3416		3416		3415		3415	
Number of clusters	61		61		61		61	
Pseudo R^2	.104		.183		.257		.316	
correctly classified	66.74%		69.17%		74.67%		77.07%	
Pearson Chi^2 goodness of fit	827.72		2826.86		1933.18		3380.52	
df	56		2640		1225		3270	
p goodness of fit	<.001***		.006**		<.001***		.087	

Note: * $p < .05$, ** $p < .01$, *** $p < .001$, # $p < .10$.

Model 1. In Model 1, we test the hypothesis that ratings of global social appropriateness, global perceptions of the effect of a behavioral alternative on the environment, and the global perception of the difficulty of a behavior predict whether a pro-environmental alternative is

selected or not[125]. Keep in mind that we use ratings on the three variables from a separate sample (obtained in Part 2) to predict the choices in Part 1.

Model 1 shows that all three ratings uniquely predict whether a pro-environmental behavioral alternative was chosen. Social appropriateness has the strongest impact – a one-unit increase in social appropriateness makes an alternative 3.4 times more likely to be chosen. Easier behavioral alternatives are more likely to be chosen than difficult ones[126], the odds ratio is 2.8. Alternatives with more positive effect on the environment are also more likely to be chosen, but the impact of this variable on choice is weak (odds ratio is 1.1), albeit statistically significant.

These results show that social appropriateness, effect on the environment, and difficulty as reflected in shared "global" ratings affect environmental behaviors of individual people (who did not provide the ratings).

Model 2. Model 1 only contained predictors obtained from raters in Part 2. In Model 2, we extend Model 1 by adding the following predictors obtained in Part 1 from people who also reported their behaviors: importance of appropriateness, importance of environmental effect, and importance of difficulty. We also add interactions between (rated) appropriateness and (reported) importance of appropriateness, between environmental effect and importance of the environmental effect, and between difficulty and importance of difficulty.

[125] This is the outcome variable in all four models.
[126] Note that high difficulty rating means the action is easy.

The first thing to notice about Model 2 is that the effects of social appropriateness and difficulty on choice are not substantially altered compared to Model 1. The impact of the variable effect on the environment, already weak in Model 1, disappears in Model 2.

We now turn to the three "importance" variables included in Model 2, and the respective interactions of the "importance" variables with the ratings.

Importance of social appropriateness does not affect choice. This means that whether a decision about a behavior is accompanied by strong or weak concerns about that behavior's social appropriateness does not matter for the outcome (i.e. the behavior being chosen or not). The interaction between rated social appropriateness and its reported importance significantly negatively affects whether a behavior is chosen. This means that when people perceive social appropriateness to be relatively more important, the effect of the rated social appropriateness on choosing a pro-environmental behavior becomes relatively less positive.

The significantly positive coefficient on importance of environmental effect means that the more the environmental effect of a pro-environmental behavior matters to people, the more likely they choose that pro-environmental behavior. The interaction between rated environmental effect and its reported importance significantly positively affects whether a behavior is chosen: When people are more concerned about environmental effects, then whether a behavior is perceived to have positive effects on the environment affects their choice more strongly than when they are not as much concerned about environmental effects.

Finally, we observe that the more difficulty matters to people, the less likely they will choose a pro-environmental behavioral alternative. This makes sense, because pro-environmental

alternatives are rated as more difficult/less easy (M = 2.68, SD = 0.62) than all the remaining alternatives combined (M = 3.68, SD = 0.34), the difference between the means is significant, $t(56) = -9.812$, $p < .001$ (paired *t*-test, two-tailed).

Model 3. Model 3 contains the same variables as Model 1, i.e. social appropriateness (which is a measure of global social norms), effect on the environment, and difficulty, together with two measures of local social norms: subjective injunctive social norm and subjective descriptive social norm.

Both measures of subjective social norms positively affect the likelihood that a pro-environmental action will be chosen: The more people close to the participant perform the behavior in question (descriptive norm) and the more strongly people close to the participant approve of the behavior (injunctive norm), the more likely that behavior is performed by the participant.

The inclusion of the two subjective norm measures attenuates the impact of the three rated variables on behavioral choice. First, the impact of the variable effect on the environment, already weak in Model 1, disappears in Model 3. Second, the impact of the variable difficulty on choice becomes smaller compared to Model 1 (the odds ratio decreases from 2.8 to 2.1). Third, after inclusion of injunctive norm and descriptive norm we observe a big drop in the effect of global social appropriateness on choosing pro-environmental behavior (the odds ratio decreases from 3.4 to 1.3). The direct effect of global social appropriateness on behavior is no longer significant in Model 3.

We next establish that there is an indirect, mediated effect of global social appropriateness on behavior via descriptive norms and via injunctive norms. Table 2 presents support for this claim.

Table 2: Local social norms predicted by global social norms

	Descriptive norms		Injunctive norms	
	B	*Beta*	*B*	*Beta*
Appropriateness	1.95***	.371	1.848***	.39
Importance of appropriateness	0.035	.032	0.069*	.07
Appropriateness * Importance	-0.162**	-.057	-0.183***	-.071
Number of observations	3416		3415	
Number of clusters	61		61	
R^2	.137		.154	

Note: * $p < .05$, ** $p < .01$, *** $p < .001$, # $p < .10$.

Table 2 reveals that descriptive norms, as well as injunctive norms are predicted by global social appropriateness. This means that behavioral alternatives that were rated as more socially appropriate (in Part 2) are more commonly performed by people close to participants in Part 1 (descriptive norms) and are more strongly approved by people close to participants in Part 1 (injunctive norms).

Overall, Table 2 supports our claim that global social norms affect choice of behavior via local social norms (injunctive norms and descriptive norms). First, global social appropriateness predicts injunctive norms and descriptive norms (see Table 2). Second, global social appropriateness predicts choice of behavior (see Model 1). Third, the effect of global social appropriateness on choice of behavior is no longer significant when injunctive norms

and descriptive norms are included in Model 3. Thus, the influence of global social appropriateness is fully mediated by injunctive norms and descriptive norms in Model 3.

Moving briefly forward to Model 4 (see Table 1), we see that in this model the influence of global social appropriateness on choice of behavior is only partially mediated by injunctive norms and descriptive norms: global social appropriateness retains a marginally significant influence on behavior choice when entered along with injunctive norms and descriptive norms into the regression. This means that in Model 4 global social norms have a mediated as well as a marginally significant direct effect on choice of behavior when the interaction with importance of appropriateness is also in the model.

Going back to Table 2, we can also observe that people feel the stronger social approval (injunctive norms) the more social appropriateness matters to them in choosing a behavior. Finally, Table 2 indicates that the interaction between social appropriateness and its importance significantly negatively affects both descriptive norms and injunctive norms. This means that when people perceive social appropriateness to be relatively more important, the positive effect of the rated social appropriateness on perceived descriptive norms and injunctive norms is relatively less pronounced.

Model 4. In Model 4 we include all predictors from the previous Models. When comparing Model 4 with Model 3, we can observe the pattern of variables' influence on the choice of pro-environmental behaviors is rather similar in the two models. The coefficients of the variables effect on the environment, difficulty, injunctive norms, and descriptive norms are virtually the same in the two models. As for social appropriateness, its effect on behavior is partially mediated by injunctive norms and descriptive norms (see above), but unlike in

Model 3, in Model 4 there is also a marginally significant direct effect of social appropriateness on behavior. One may prefer this specification over that in Model 3, because the three "importance" variables and their interactions with the respective ratings are controlled for in Model 4.

Next, we compare Model 4 with Model 2, focusing on the three "importance" variables and their interactions with the respective ratings. There is virtually no change in the estimate of importance of difficulty, nor in the difficulty by importance of difficulty interaction term.

The interaction between social appropriateness and its perceived importance, that we found in Model 2, is no longer significant in Model 4. Hence, Model 4 indicates that people are equally strongly influenced by social appropriateness irrespective of whether they find social appropriateness important or not.

Importance of difficulty has about the same effect in Model 2 and in Model 4: When people care about the difficulty of behaviors, they are less likely to act pro-environmentally. While compared to Model 2, the estimated effect of difficulty somewhat drops in Model 4, a significant interaction of difficulty with importance of difficulty emerges in the latter model. Thus, Model 4 indicates that easier pro-environmental behaviors are more likely to be chosen than difficult ones, especially when people care about difficulty.

4. DISCUSSION

We begin our discussion with a summary of the key results. Result 1: Shared "global" ratings of social appropriateness, difficulty and environmental effect predict whether pro-

environmental actions are chosen or not. The rated characteristics affect behavior in the expected direction: more appropriate, more effective and easier behaviors are more likely to be selected.

Admittedly, the positive influence of the behavior's perceived environmental effect on choice of behavior is only small in Model 1. While this main effect vanishes in the remaining models, the interaction "effect on the environment" by "importance of environmental effect" is significant and positive in Models 2-4, which means that if people care about the effect on the environment, they are more likely to choose pro-environmental behaviors the more effective in protecting the environment these are perceived in general on a global cultural level (as captured by the ratings from Part 2).

Result 2: Result 1 in combination with the fact that participants who reported their behaviors were separate from the raters, and were not aware of the ratings, implies that raters are in fact able to capture widely shared properties of environmental behaviors in their ratings. Result 2 is further supported by the finding that the measure of global social appropriateness significantly predicts injunctive norms and descriptive norms on the individual level.

Result 3: Global social appropriateness affects behavior indirectly via injunctive norms and descriptive norms. Depending on model specification, there is also an additional marginally significant direct effect of social appropriateness on behavior, besides its mediated effect.

This paper shows that global perceptions of properties of environmental behaviors can serve to predict their choice (Result 1). We should point out that we elicited the global perceptions from a convenience student sample. This is not a problem for our research, because we are

interested in the relationships between the global variables and behaviors, rather than in absolute levels of variables. A related issue of theoretical and practical interest, however, is *which* tacitly shared perceptions raters use as coordination devices when submitting the ratings. For example, when student raters submit their ratings, do they try to coordinate on perceptions tacitly shared in the student community, or do they try to coordinate on even more widely shared perceptions? Burks & Krupka (2012) and Chang et al. (2015) make some initial steps in addressing this type of questions. The results in Chang et al. (2015) seem to suggest that members of different subgroups do not use subgroup-specific shared perceptions to guide them in the Krupka-Weber rating task, but rather more widely shared perceptions, *unless* their subgroup identity is made salient to them prior to the KW game.

As we discussed in the introduction, using the incentive-compatible KW game has several methodological benefits. Response noise due to inattention and social desirability is reduced. And consistency bias is eliminated because predictor and outcome variables are obtained from different people. The correlations we found thus cannot be as easily attributed to consistency and social desirability biases as is sometimes the case in conventional questionnaire studies (Hessing et al., 1988; Presser & Traugott, 1992; Falk & Zimmermann, 2013). This gives us increased confidence that social appropriateness, shared beliefs about the difficulty and to some extent also shared beliefs about the environmental effect matter for environmental behavior (see Result 1).

Our finding that behaviors' environmental effect has only a limited effect on choice seems to accord well with Poortinga et al. (2003) who found that, on average, the acceptability of 23 different energy-saving measures is not affected by the amount of energy a given measure saves.

Of course, we cannot be certain that the shared perceptions of behaviors' environmental effect represent an accurate measure of the behaviors' actual environmental effect. To shed more light on this issue, future studies can compare whether the environmental effect ratings are aligned with some objective measure of the behaviors' environmental effect.

Finding little or no link between environmental effect ratings and behaviors can also mean that *the ratings* are, after all, too noisy. It is possible that the raters do not have a good idea about the behaviors' environmental effects. In such a case, the Krupka-Weber elicitation method would not produce results any better than a more conventional elicitation method (Vesely, 2015; see also Gächter & Renner, 2010). This interpretation, while not necessarily correct, is a reminder that the KW game can be useful only when rating characteristics that are sufficiently widely known by the raters (see Result 2).

Turning now to Result 3, we found, first of all, that injunctive norms and descriptive norms both have direct effect on environmental behavior, which is broadly in line with previous research (e.g. Heath & Gifford, 2002; Schultz et al., 2007; 2014). People are more likely to choose behaviors that are more strongly approved by people close to them (injunctive norms) and that are more commonly performed by people close to them (descriptive norms).

Both injunctive norms and descriptive norms are statistically significant predictors of choice of behavior when entered simultaneously in the regression. This finding is not surprising, as the two social norm types are assumed to have somewhat different, albeit partially overlapping, functions (Deutsch & Gerard, 1955; Bicchieri, 2006; Göckeritz et al., 2010; see also a meta-analysis by Rivis & Sheeran, 2003).

We further found that global social norms (as reflected in the social appropriateness ratings) affect behavior indirectly via local social norms (injunctive norms and descriptive norms). Depending on model specification, there is also an additional marginally significant direct effect of global social norms on behavior, besides their mediated effect. This result is correlational in nature, so we do not make any strong claims about causality, and recommend instead further experimental exploration of the normative influences that our results suggest (global social norms → local social norms → behavior).

After commenting on our key results, we now turn to three other interesting findings from our study.

Result 4: Going back to Model 2, we found that when people perceive social appropriateness to be relatively more important, the effect of the rated social appropriateness on choosing a pro-environmental behavior becomes relatively less positive. This result, which may seem counter-intuitive at first sight, suggests that when people perceive appropriateness concerns to be relatively important, they put relatively less weight to small differences in appropriateness when making their choice.

Schultz et al. (2014) report a somewhat similar result: people with stronger personal norms concerning water conservation are relatively less affected by normative messages promoting conservation. Göckeritz et al. (2010) report another related result: people more personally involved in energy conservation are less influenced by social norms related to energy conservation.

It seems that people with either strong personal norms, strong personal involvement or strong appropriateness concerns (as in our study) need a stronger normative "push" to make them more likely to act pro-environmentally than people with weak personal norms, personal involvement or appropriateness concerns. Maybe this is the case because they more strongly rely on their own internal appropriateness standards. A complementary explanation offered by Göckeritz et al. (2010) and Schultz et al. (2014) is that people with strong personal norms, strong personal involvement (and possibly also those with strong appropriateness concerns) are less receptive to relatively superficial external influences.

Result 5: We saw that after adding injunctive norms and descriptive norms to the global ratings in Model 3, not only the impact of global social appropriateness on behavior was attenuated (which is not surprising), but so was the impact of global difficulty on behavior. This second observation is worth pointing out because it provides a further support for the view that social norms can serve as sources of information indicating feasible and adaptive courses of action (see Sherif, 1936; Deutsch & Gerard, 1955; Cialdini et al., 1990; Bamberg et al., 2007).

Result 6: The regression reported in Table 2 shows that people feel the stronger social approval (injunctive norms) the more social appropriateness matters to them (importance of appropriateness). The causality of this relation, however, is not obvious.

We conjecture that when social appropriateness matters to them, people become more receptive to signs of social approval. Another conjecture consistent with the data is that when behaviors are more approved of, social appropriateness of such behaviors becomes more important to people. Result 6 may even imply the following feedback loop: When, for

example, one's social context is not supportive of pro-environmental behaviors (low injunctive norm), then the decision-maker may not develop a concern for behaving appropriately (low importance of appropriateness), which may make him/her even less receptive to any signs of social support for pro-environmental behaviors, and so on. These conjectures should be tested in subsequent research.

Directions for future research. Normative processes represent a topic of theoretical interest for environmental researchers. We believe the present paper can inspire important new research in this area: How are global social norms formed? How are local social norms formed? What other factors influence the formation and maintenance of local social norms besides global social norms? Do global social norms really influence behavior also independently of local social norms? If so, what exactly does it mean that these (effective) global social norms are not absorbed into the local social norms? Why are not they absorbed?

Good understanding of normative processes can be also crucial when designing interventions, for example because there are certain bottlenecks – such as participants' baseline behavior, their personal involvement, and their identity – that can render normative messages ineffective (Wenzel, 2004; Schultz et al., 2007, 2014; Göckeritz et al., 2010; Fellner et al., 2013; Pruckner & Sausgruber, 2013). Thus, having a firmer grasp of normative processes underlying environmental behavior can also inform policy. A normative intervention strives to change behavior. To do that, does it have to change perceptions of global social norms, participants' own local social norms or both? Answering such questions seems to be another important direction for future research.

5. CONCLUSION

In this paper we adopt a new incentive-compatible technique for elicitation of shared beliefs concerning different environmental behaviors. The method can be used to elicit any number of characteristics relevant to environmental behaviors, provided that the knowledge of these characteristics is tacitly shared in the population.

On the one hand, the use of the Krupka-Weber protocol reduces certain problems associated with conventional surveys (social desirability bias, consistency bias, and inattentive responding). A technical advantage of using the KW protocol is also the fact that the shared beliefs can be elicited from a separate sample, as was the case in the present study.

On the other hand, measuring both the widely shared and the subjective perceptions of behaviors' characteristics may lead us to a more nuanced and accurate understanding of the normative processes underlying environmental behavior, and the processes of information transmission in general (e.g. transmission of beliefs concerning behaviors' feasibility and effectiveness).

We show that social norms, difficulty and (to some extent) environmental effect *as reflected in the widely shared global beliefs* matter for decisions to behave pro-environmentally. Our results further suggest that if we manage to change these global beliefs, the more proximal local and subjective variables might follow after. We specifically find that the effect of global social norms on choice of pro-environmental behaviors is mediated by subjective injunctive and descriptive social norms.

Of course, the causality of the links between the global variables, the proximal variables and behavior needs to be further examined. If the causal role of the global variables is confirmed, we may develop and try out new techniques for influencing the global beliefs with the goal of cultivating the local beliefs and promoting individuals' environmentally friendly behaviors.

CONCLUDING REMARKS

Rather than summarizing my results again I would like to at least briefly comment on one line of promising research related to both of the main foci of my thesis – social norms and fairness. (The reader who just needs a summary of the results is referred to short abstracts at the beginning of each chapter.)

A new avenue of research is the examination of how intrinsic social preferences differ from intrinsic preferences for compliance with social norms. And how these preferences complement each other in affecting behavior. Such questions already attracted researchers such as Binmore (2005), Gintis (2009, 2010), Gächter et al. (2013), Krupka & Weber (2013), Kimbrough & Vostroknutov (2015), see also Chapter 7 in this work. Answering these questions may have far-reaching implications for the theory of strategic behavior, which is currently dominated by social preference models.

Binmore (2005), Gintis (2009, 2010) and Kimbrough & Vostroknutov (2015), on the one hand, suggest that behaviors captured by influential social preference models such as Rabin (1993) and Fehr & Schmidt (1999) can be modeled with reference to social norms instead. According to a simplified version of this view, people act as if having what is called social preferences (e.g. fairness, reciprocity, altruism) because they, in the first place, follow the respective social norms (e.g. of fairness, reciprocity, altruism). The extent to which social norms are followed by individuals depends in turn on each individual's conformity (Kimbrough & Vostroknutov, 2015), and factors such as observability (Hoffman et al., 1994; Schram & Charness, 2015) and others' compliance (López-Pérez, 2008).

On the other hand, the widely used social preference models often provide parsimonious and reasonably accurate explanations of behaviors[127]. Even assuming that it is normative influences that drive behavior, social preference models might be therefore used at least as a "shorthand" for the underlying normative influences without the need to model social norms explicitly. Gächter et al. (2013), for example, show that a simple social preference model predicts behavior somewhat better than a model based on elicited social norms.

Thus, research should concentrate on empirical identification of both social norms and social preferences related to behaviors. One crucial empirical question is whether both social norms and social preferences will be needed in the prediction of behavior. The next empirical question is the question of causal links between these two kinds of explanatory variables. One potentially useful hypothesis I would like to suggest here is that the effect of social norms on behavior is *mediated* by social preferences. This hypothesis is inspired by a similar finding in Vesely & Klöckner (2015) who show that the effect of widely shared "global" social norms on behavior is mediated by the effect of subjective ("local") social norms on behavior.

[127] For a controversy concerning this issue see Binmore & Shaked (2010), Eckel & Gintis (2010), and Fehr & Schmidt (2010).

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APPENDICES

APPENDIX A: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 1

The following instructions are translated from Czech. They were presented in electronic form via e-mail (Stage 2 – decisions) and in paper form (all other parts).

APPENDIX A.1: INSTRUCTIONS FOR CONDITIONS ORDER SC-AC

[Introduction – page 1]

Introduction

Hello and thank you for participating in this experiment! The experiment will consist of a number of parts. Instructions for each part will be distributed sequentially. Please read the instructions carefully. If you have any questions, please raise your hand.

Note that all decisions you make during the experiment will be only known to the experimenter and otherwise they will be COMPLETELY anonymous.

Please read the description of the following situation first:

There are two people interacting in this situation, Person A and Person B.

In the first step, Person A is provisionally given an amount of money "M".

In the second step, Person A has to propose a division of M between himself/herself and Person B.

In the third step, Person B has to decide whether to accept or reject the division of M proposed by Person A. If Person B accepts the division, it will be implemented. If Person B rejects the division, neither person gets any part of M.

If you understand the above situation, please answer the questions on the next page.

[Introduction – page 2]

Questions:

Suppose the amount M is equal to 300 CZK. Suppose Person A proposes a division of M such that Person B should have 100 CZK. Suppose Person B accepts the division.

1a) How much money will Person A get?

1b) How much money will Person B get?

Suppose the amount M is equal to 300 CZK. Suppose Person A proposes a division of M such that Person A should have 100 CZK. Suppose Person B rejects the division.

2a) How much money will Person A get?

2b) How much money will Person B get?

[The experimenter checked the answers individually and explained what was unclear, if necessary.]

[Stage 1, SC condition]

You will now make three separate decisions in the situation described on the Introduction page. You will make decisions as Person A (i.e., the one who proposes the division of M).

You will be provisionally given three different amounts M in the range 20-200 EC (experimental crowns). The specific amounts are given below.

In each decision, you will be paired with a different Person B (i.e., you will be paired with three different persons in total). Each person B is another participant in this study.

Please propose a division of each amount M between yourself and the respective Person B.

Remember that each Person B will later decide whether to accept or reject your proposed division. Recall that if a given Person B accepts your proposal, it will be implemented, and if he/she rejects your proposal both of you get nothing from the respective amount M. Person B always knows both the respective M and the proposed amount to himself/herself.

Please indicate your decisions:

!! The proposed amount for Person B must range between 0 and the respective M. If Person B accepts a given proposal, you will get M minus the amount proposed for B.

1. decision, M_1 = [20-200] EC
Proposed amount for Person B:

2. decision, M_2 = [20-200] EC
Proposed amount for Person B:

3. decision, M_3 = [20-200] EC
Proposed amount for Person B:

Note: At the conclusion of the entire study (i.e., probably in a couple of weeks), the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B[128]. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please wait silently after having made your decisions. The experimenter will collect your decisions shortly. Thank you for your patience!

[128] A pair of decisions consists of a proposal and its acceptance/rejection.

[Stage 1, AC condition – questions]

You will now make three separate decisions in a situation similar to that described on the Introduction page. You will again make decisions as Person A (i.e., the one who proposes the division of M).

You will be provisionally given three different amounts in the range 20-200 EC (experimental crowns). The specific amounts are given on the next page.

In each decision, you will be paired with a different Person B (i.e., you will be paired with three different persons in total; all of them will be different from the three persons you have been informed that you will be paired in case of the previous three decisions). Each person B is another participant in this study.

How does the present situation differ from the situation described on the Introduction page: In each decision, you will be asked to choose an amount X, which must be between 20 and M and a multiple of 20 (recall that M is the total amount provisionally allocated to you to divide between you and Person B).

The experimenter will inform Person B that the "amount being divided" is equal to X. Person B will know that the "amount being divided" could be either the actual total amount (M) or that it could be an amount selected by you (X). BUT Person B will never learn which is actually the case and Person B will never learn what M is.

Before proceeding, please answer the following questions:

Suppose the amount M is equal to 300 CZK. Suppose Person A chooses X to be 200 CZK. Suppose Person A proposes a division of M such that Person B should have 100 CZK. Suppose Person B accepts the division.

1) How much money will Person A get?

2) How much money will Person B get?

3) How much money will Person B be told by the experimenter is the "amount being divided"?

[The experimenter checked the answers individually and explained what was unclear, if necessary.]

[Stage 1, AC condition – decisions]

Please choose X for each decision below and propose a division of each amount M between yourself and the respective Person B (proposed amount for Person B cannot be larger than X).

Remember that each Person B will later decide whether to accept or reject your proposed division. Recall that if a given Person B accepts your proposal, it will be implemented, and if he/she rejects your proposal both of you get nothing from the respective amount M. Person B always knows the respective X chosen by you and the proposed amount to himself/herself, never the actual M.

Please indicate your decisions:

!! X must be between 20 and M and a multiple of 20. The proposed amount for Person B must range between 0 and the respective X. If Person B accepts a given proposal, you will get M minus the amount proposed for B.

1. decision, M_1 = [20-200] EC
Amount X_1 presented to Person B as the "amount being divided":
Proposed amount for Person B:

2. decision, M_2 = [20-200] EC
Amount X_2 presented to Person B as the "amount being divided":
Proposed amount for Person B:

3. decision, M_3 = [20-200] EC
Amount X_3 presented to Person B as the "amount being divided":
Proposed amount for Person B:

Note: At the conclusion of the entire study (i.e., probably in a couple of weeks), the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please wait silently after having made your decisions. The experimenter will collect your decisions shortly. Thank you for your patience!

[Stage 2 – introduction]

Please state below your e-mail address and your bank account number.

In subsequent weeks, you will receive an e-mail from the experimenter with six randomly selected anonymous proposals from six different participants in this study.

Along with each proposal, you will also learn the respective "amount being divided". However, you will *not* be told whether the "amount being divided" is M or X – it will be X if Person A was asked to choose an X, otherwise it will be M. Three of the six "amounts being divided" will in fact be M and three of them will be X. The six amounts and the corresponding proposals will be presented to you in random order.

For each of these six proposals, you will be asked to choose whether you accept or reject it. You will then be asked to send your decisions by e-mail to the experimenter.

After all decisions are collected, the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

[Stage 2 – decisions]

Dear participant,

Please find below the six randomly selected anonymous proposals from six different participants in the decision making study you have been participating in.

Decision 1:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 1? Yes No

Decision 2:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 2? Yes No

Decision 3:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 3? Yes No

Decision 4:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 4? Yes No

Decision 5:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 5? Yes No

Decision 6:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 6? Yes No

For each decision separately, please indicate whether you accept or reject the proposal. Please send your decisions by e-mail to the experimenter.

If you accept a given proposal, it will be implemented. If you reject, both you and the respective Person A will get nothing in the respective decision. Note that in case of acceptance, Person A will get what is equal to the ACTUAL amount being divided (which may or may not be identical to the "amount being divided" stated in this e-mail) minus the proposed allocation to you.

After I receive decisions from all participants, I will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please let me know if you have any questions, otherwise please do not forget to send the e-mail with you decisions. Thank you!

APPENDIX A.2: INSTRUCTIONS FOR CONDITIONS ORDER AC-SC

[Introduction – page 1]

Introduction

Hello and thank you for participating in this experiment! The experiment will consist of a number of parts. Instructions for each part will be distributed sequentially. Please read the instructions carefully. If you have any questions, please raise your hand.

Note that all decisions you make during the experiment will be only known to the experimenter and otherwise they will be COMPLETELY anonymous.

Please read the description of the following situation first:

There are two people interacting in this situation, Person A and Person B.

In the first step, Person A is provisionally given an amount of money "M".

In the second step, Person A has to propose a division of M between himself/herself and Person B.

In the third step, Person B has to decide whether to accept or reject the division of M proposed by Person A. If Person B accepts the division, it will be implemented. If Person B rejects the division, neither person gets any part of M.

If you understand the above situation, please answer the questions on the next page.

[Introduction – page 2]

Questions:

Suppose the amount M is equal to 300 CZK. Suppose Person A proposes a division of M such that Person B should have 100 CZK. Suppose Person B accepts the division.

1a) How much money will Person A get?

1b) How much money will Person B get?

Suppose the amount M is equal to 300 CZK. Suppose Person A proposes a division of M such that Person A should have 100 CZK. Suppose Person B rejects the division.

2a) How much money will Person A get?

2b) How much money will Person B get?

[The experimenter checked the answers individually and explained what was unclear, if necessary.]

[Stage 1, AC condition – questions]

You will now make three separate decisions in a situation similar to that described on the Introduction page. You will make decisions as Person A (i.e., the one who proposes the division of M).

You will be provisionally given three different amounts in the range 20-200 EC (experimental crowns). The specific amounts are given on the next page.

In each decision, you will be paired with a different Person B (i.e., you will be paired with three different persons in total). Each person B is another participant in this study.

How does the present situation differ from the situation described on the Introduction page: In each decision, you will be asked to choose an amount X, which must be between 20 and M and a multiple of 20 (recall that M is the total amount provisionally allocated to you to divide between you and Person B).

The experimenter will inform Person B that the "amount being divided" is equal to X. Person B will know that the "amount being divided" could be either the actual total amount (M) or that it could be an amount selected by you (X). BUT Person B will never learn which is actually the case and Person B will never learn what M is.

Before proceeding, please answer the following questions:

Suppose the amount M is equal to 300 CZK. Suppose Person A chooses X to be 200 CZK. Suppose Person A proposes a division of M such that Person B should have 100 CZK. Suppose Person B accepts the division.

1) How much money will Person A get?

2) How much money will Person B get?

3) How much money will Person B be told by the experimenter is the "amount being divided"?

[The experimenter checked the answers individually and explained what was unclear, if necessary.]

[Stage 1, AC condition – decisions]

Please choose X for each decision below and propose a division of each amount M between yourself and the respective Person B (proposed amount for Person B cannot be larger than X).

Remember that each Person B will later decide whether to accept or reject your proposed division. Recall that if a given Person B accepts your proposal, it will be implemented, and if he/she rejects your proposal both of you get nothing from the respective amount M. Person B always knows the respective X chosen by you and the proposed amount to himself/herself, never the actual M.

Please indicate your decisions:

!! X must be between 20 and M and a multiple of 20. The proposed amount for Person B must range between 0 and the respective X. If Person B accepts a given proposal, you will get M minus the amount proposed for B.

1. decision, M_1 = [20-200] EC
Amount X_1 presented to Person B as the "amount being divided":
Proposed amount for Person B:

2. decision, M_2 = [20-200] EC
Amount X_2 presented to Person B as the "amount being divided":
Proposed amount for Person B:

3. decision, M_3 = [20-200] EC
Amount X_3 presented to Person B as the "amount being divided":
Proposed amount for Person B:

Note: At the conclusion of the entire study (i.e., probably in a couple of weeks), the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B[129]. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please wait silently after having made your decisions. The experimenter will collect your decisions shortly. Thank you for your patience!

[129] A pair of decisions consists of a proposal and its acceptance/rejection.

[Stage 1, SC condition]

You will now make three separate decisions in the situation described on the Introduction page. You will again make decisions as Person A (i.e., the one who proposes the division of M).

You will be provisionally given three different amounts M in the range 20-200 EC (experimental crowns). The specific amounts are given below.

In each decision, you will be paired with a different Person B (i.e., you will be paired with three different persons in total; all of them will be different from the three persons you have been informed that you will be paired in case of the previous three decisions). Each person B is another participant in this study.

Please propose a division of each amount M between yourself and the respective Person B.

Remember that each Person B will later decide whether to accept or reject your proposed division. Recall that if a given Person B accepts your proposal, it will be implemented, and if he/she rejects your proposal both of you get nothing from the respective amount M. Person B always knows both the respective M and the proposed amount to himself/herself.

Please indicate your decisions:

!! The proposed amount for Person B must range between 0 and the respective M. If Person B accepts a given proposal, you will get M minus the amount proposed for B.

1. decision, M_1 = [20-200] EC
Proposed amount for Person B:

2. decision, M_2 = [20-200] EC
Proposed amount for Person B:

3. decision, M_3 = [20-200] EC
Proposed amount for Person B:

Note: At the conclusion of the entire study (i.e., probably in a couple of weeks), the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please wait silently after having made your decisions. The experimenter will collect your decisions shortly. Thank you for your patience!

[Stage 2 – introduction]

Please state below your e-mail address and your bank account number.

In subsequent weeks, you will receive an e-mail from the experimenter with six randomly selected anonymous proposals from six different participants in this study.

Along with each proposal, you will also learn the respective "amount being divided". However, you will *not* be told whether the "amount being divided" is M or X – it will be X if Person A was asked to choose an X, otherwise it will be M. Three of the six "amounts being divided" will in fact be M and three of them will be X. The six amounts and the corresponding proposals will be presented to you in random order.

For each of these six proposals, you will be asked to choose whether you accept or reject it. You will then be asked to send your decisions by e-mail to the experimenter.

After all decisions are collected, the experimenter will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

[Stage 2 – decisions]

Dear participant,

Please find below the six randomly selected anonymous proposals from six different participants in the decision making study you have been participating in.

Decision 1:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 1? Yes No

Decision 2:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 2? Yes No

Decision 3:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 3? Yes No

Decision 4:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 4? Yes No

Decision 5:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 5? Yes No

Decision 6:
"Amount being divided": […]
Proposed allocation to you: […]
Do you accept proposal 6? Yes No

For each decision separately, please indicate whether you accept or reject the proposal. Please send your decisions by e-mail to the experimenter.

If you accept a given proposal, it will be implemented. If you reject, both you and the respective Person A will get nothing in the respective decision. Note that in case of acceptance, Person A will get what is equal to the ACTUAL amount being divided (which may or may not be identical to the "amount being divided" stated in this e-mail) minus the proposed allocation to you.

After I receive decisions from all participants, I will randomly select 1/24 of all pairs of decisions made by Persons A and B. Participants who made the randomly selected decisions will be paid out via bank transfer with an exchange rate 1 EC = 1 CZK.

Please let me know if you have any questions, otherwise please do not forget to send the e-mail with you decisions. Thank you!

APPENDIX B: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 4

The instructions were presented verbally using the following script.

Thank you for participating in this study. You will make a number of decisions in the situation shown on the following figure [the figure is available to the participant throughout the session].

		Player B	
		Cooperate	Defect
Player A	Cooperate	Player A: 3 points Player B: 3 points	Player A: 0 points Player B: 5 points
	Defect	Player A: 5 points Player B: 0 points	Player A: 1 point Player B: 1 point

As you can see in this figure, if both players cooperate, both get 3 points. If both players defect, both get 1 point.

When player A cooperates and player B defects, player A gets 0 points, and player B gets 5 points.

When player B cooperates and player A defects, player B gets 0 points, and player A gets 5 points.

Do you understand how this works?

Suppose Player A chooses cooperate and Player B chooses defect. How much points will each get?

Suppose Player A chooses cooperate and Player B chooses cooperate. How much points will each get?

Suppose Player A chooses cooperate and Player B chooses defect. How much points will each get?

Suppose Player A chooses defect and Player B chooses cooperate. How much points will each get?

The situation in the figure can be repeated a number of times, i.e. players A and B can make several decisions in the same situation repeatedly.

You and I will now in fact make repeated decisions in this situation. We will each indicate our decisions by placing these cards with “cooperate” and “defect” written on them face down on the desk [the cards are shown]. I will then turn the cards face up so that we see the decisions. Then we will proceed to the next decision. I will explain how many times the decisions will be repeated in a minute. Do you so far understand how this will work?

One thing to note is that I will not decide as “myself”, but in my decisions I will “represent” or “act like” different possible “players”. Do you so far understand how this will work?

We will now play 15 repeated “games”. In each of the games the situation shown in the figure you already know will be repeated a number of times. I will always tell you when a new game begins and how long it will take.

In each new game I may represent a different “player”. Within the game, however, the “player” you will interact with remains the same.

You will encounter the same “players” repeatedly throughout the session. The specific “players” will not be described to you. You will only learn about the “players” based on their decisions.

Do you understand how this will work? Do you have any questions?

Before we start it is important to note that your task is to gain as many points as possible, *not* to “defeat” the players I will be representing. There will be other participants in this study and you should try to gain more points than the *them*.

Lets start with game one. The game will have 30 rounds, i.e. you will make 30 decisions in it. After the game is finished, we move to the next game.

Lets move to game two. The game will have 25-35 rounds, i.e. you will make 25-35 decisions in it. The duration will be dermined randomly.

Lets move to game three. The duration will be dermined randomly (I will not tell you how long the game is, but I will tell you when it ends).

[And so on until game 15. The game’s “Length” and duration were, in each case, determined as explained in section 2.2 of the chapter.]

APPENDIX C: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 5

The following instructions are translated from Czech. Instructions were presented in computerized form. The layout of the computerized version was, of course, somewhat different from what is shown here.

APPENDIX C.1: INSTRUCTIONS FOR CONDITION C

Welcome and thank you for participating in our decision making experiment! You will be paid for your participation in the experiment. In addition to a 50 CZK participation bonus, the amount you receive will be determined during the experiment and will depend on your decisions and the decisions of two others you will be matched with. Your earnings will be paid to you in cash at the conclusion of the experiment.

If you have a question during the experiment, raise your hand and an experimenter will assist you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating this rule will be asked to leave the experiment and will not be paid.

There will be two parts to the experiment and a brief questionnaire at the end. The experiment is preceded by two simple control questions. At the conclusion of the experiment and after answering the questionnaire, you will receive your earnings, in private.

In the following experiment it is necessary to be able to calculate the mean and to multiply numbers. Please, answer the following control questions. We will continue after everyone correctly answers the questions.

A) How many is 2000 times 1.1?

B) What is the mean of the following three numbers: 5, 7, 9?

The first part of the experiment will last for 10 rounds. During each round, you will play a simple game with two other participants. There are therefore three participants including you.

Each of you will simultaneously choose a number from 0 to 100. You can choose any number in this range, including 0 to 100, by writing that number (up to two decimal places) in the appropriate place in the table on the next screen. This choice is private and will not be revealed to other participants.

Your payoff in each round will be determined as follows:

1) First, the average of the participants' choices (including yours) will be computed. This average will be computed by adding up the numbers chosen by each of the three participants and then dividing by three.

2) A target number will then be determined by multiplying the average of the participants' choices by 0.7. Thus:

Target number = 0.7 x (average of the participants' choices)

3) The person who chose a number closest to this target number, from either above or below, will win 15 CZK in that round. If there are two or more participants whose choices are exactly equally close to the target number, then these participants will equally divide the 15 CZK prize.

At the end of each round, the average of the choices and the target number will be displayed to participants on their screens.

Round	1	2	3	4	5	6	7	8	9	10
Your number choice										
Average of choices										
Target number										
Your earnings										

The second part of the experiment will last for 10 rounds. During each round, you will play a simple game with two other participants, who are the same as in the previous part. There are therefore three participants including you.

Each of you will simultaneously choose a number from 100 to 200. You can choose any number in this range, including 100 or 200, by writing that number (up to two decimal places) in the appropriate place in the table on the next screen. This choice is private and will not be revealed to other participants.

Your payoff will be determined as follows:

1) First, the average of the participants' choices (including yours) will be computed. This average will be computed by adding up the numbers chosen by each of the three participants and then dividing by three.

2) A target number will then be determined by multiplying the average of the participants' choices by 1.3. Thus:

Target number = 1.3 x (average of the participants' choices)

3) The person who chose a number closest to this target number, from either above or below, will win 15 CZK in that round. If there are two or more participants whose choices are exactly equally close to the target number, then these participants will equally divide the 15 CZK prize.

At the end of each round, the average of the choices and the target number will be displayed to participants on their screens.

Round	1	2	3	4	5	6	7	8	9	10
Your number choice										
Average of choices										
Target number										
Your earnings										

Please answer the following three questions. You will receive an additional 10 CZK per correct answer.

A) A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?

B) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

C) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

APPENDIX C.2: INSTRUCTIONS FOR CONDITION D

Welcome and thank you for participating in our decision making experiment! You will be paid for your participation in the experiment. In addition to a 50 CZK participation bonus, the amount you receive will be determined during the experiment and will depend on your decisions and the decisions of two others you will be matched with. Your earnings will be paid to you in cash at the conclusion of the experiment.

If you have a question during the experiment, raise your hand and an experimenter will assist you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating this rule will be asked to leave the experiment and will not be paid.

There will be two parts to the experiment and a brief questionnaire at the end. The experiment is preceded by two simple control questions. At the conclusion of the experiment and after answering the questionnaire, you will receive your earnings, in private.

In the following experiment it is necessary to be able to calculate the mean and to multiply numbers. Please, answer the following control questions. We will continue after everyone correctly answers the questions.

A) How many is 2000 times 1.1?

B) What is the mean of the following three numbers: 5, 7, 9?

The first part of the experiment will last for 10 rounds. During each round, you will play a simple game with two other participants. There are therefore three participants including you.

Each of you will simultaneously choose a number from 100 to 200. You can choose any number in this range, including 100 or 200, by writing that number (up to two decimal places) in the appropriate place in the table on the next screen. This choice is private and will not be revealed to other participants.

Your payoff will be determined as follows:

1) First, the average of the participants' choices (including yours) will be computed. This average will be computed by adding up the numbers chosen by each of the three participants and then dividing by three.

2) A target number will then be determined by multiplying the average of the participants' choices by 1.3. Thus:

Target number = 1.3 x (average of the participants' choices)

3) The person who chose a number closest to this target number, from either above or below, will win 15 CZK in that round. If there are two or more participants whose choices are exactly equally close to the target number, then these participants will equally divide the 15 CZK prize.

At the end of each round, the average of the choices and the target number will be displayed to participants on their screens.

Round	1	2	3	4	5	6	7	8	9	10
Your number choice										
Average of choices										
Target number										
Your earnings										

The second part of the experiment will last for 10 rounds. During each round, you will play a simple game with two other participants, who are the same as in the previous part. There are therefore three participants including you.

Each of you will simultaneously choose a number from 0 to 100. You can choose any number in this range, including 0 to 100, by writing that number (up to two decimal places) in the appropriate place in the table on the next screen. This choice is private and will not be revealed to other participants.

Your payoff in each round will be determined as follows:

1) First, the average of the participants' choices (including yours) will be computed. This average will be computed by adding up the numbers chosen by each of the three participants and then dividing by three.

2) A target number will then be determined by multiplying the average of the participants' choices by 0.7. Thus:

Target number = 0.7 x (average of the participants' choices)

3) The person who chose a number closest to this target number, from either above or below, will win 15 CZK in that round. If there are two or more participants whose choices are exactly equally close to the target number, then these participants will equally divide the 15 CZK prize.

At the end of each round, the average of the choices and the target number will be displayed to participants on their screens.

Round	1	2	3	4	5	6	7	8	9	10
Your number choice										
Average of choices										
Target number										
Your earnings										

Please answer the following three questions. You will receive an additional 10 CZK per correct answer.

A) A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?

B) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

C) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

APPENDIX D: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 6

The following instructions are translated from Czech. They were presented in paper form.

Game rules

The experiment consists of 9 rounds. In each round you will be endowed with 2 chips which you can invest either to your *"private"* account or to the *"group"* account. The value of a chip will be different when you invest it to your private account and when you invest it to the group account (see details in Game process). The goal of the game is to accumulate the largest possible amount.

You will be given *18 chips* (two chips for each round). You will be asked to indicate on each chip whether you want to put it to the group account (make a cross mark on the chip) or whether you want to put it to your private account (leave the chip blank). In each round you can:

- put both chips to the group account (two blank chips),
- put one chip to the private account and one chip to the group account (one blank chip and one chip marked with a cross),
- put both chips to the group account (two chips marked with a cross).

We will collect two chips from each of you in every round. Please leave these two chips face down on your desk – so that no one can see your decision.

Throughout the game you will write down your decisions in a *record sheet*. The record sheet is anonymous, you will only be asked to state your gender. You will write down the earnings from each round in the record sheet. By summing up the earnings, you will obtain the total earnings in individual parts of the game, as well as in the entire game. No one should be allowed to look at your record sheet during the game!

You will be asked to please return the record sheet to us after the game.

Game process

Rounds 1-3

By returning one blank chip you contribute to your "private" account 40 CZK [the game was not played for real money, of which the participants were aware], by returning two blank chips you contribute to your "private" account 80 CZK. Your return from contributing one or two chips (marked with a cross) to the "group" account is calculated as follows: after each round the total number of chips contributed by all present players to the "group" account is counted and the total is multiplied by 10. We will announce the resulting amount and you will add this amount to your "group" account [in the record sheet] whether you yourself in fact contributed anything to the "group" account or not.

The calculation of a given player's one-round earnings:

Profit from the "private account" (40 CZK times the number of chips invested by you to your private account)

+

Profit from the "group account" (10 CZK times the number of all chips invested to the group account)

Rounds 4-6
After round 3 the value of a chip invested to your private account decreases to 20 CZK. The value of a chip invested to the group account remains unchanged.

After round six, a *discussion* will take place, during which you will be able to agree on a strategy for subsequent rounds.

Rounds 7-9
The value of a chip invested to your private account again increases to 40 CZK. The value of a chip invested to the group account remains unchanged.

It is very important that no one communicates about his/her strategy with the other players (or the teacher), except for the discussion after round 6.

At the end of the game, we will randomly select one of you. The selected participant will receive 0.5% of his/her earnings in CZK in the form of points which will help you to pass the subject "Public economics". Thus, if you are the selected person and if you earn 5000 CZK during the game, you will receive 25 points for passing "Public economics".

APPENDIX E: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 7

The following instructions are translated from Czech. Instructions were presented in computerized form.

The following instructions are instructions for C players. Only data from normal-C players are analyzed in the chapter, as explained therein.

APPENDIX E.1: INSTRUCTIONS FOR THE CONTROL CONDITION

1) General description of the situation

This experiment focuses on a situation where two persons A and B are bargaining. One round of the bargaining process consists of just two steps.

First step: Person A proposes a division of X tokens between himself/herself and person B. (The tokens can be exchanged for money at the end of the experiment. The tokens were provided by the experimenter and they were randomly given to person A.)

Second step: Person B can either accept or reject the division made by person A. If he/she accepts the division, the tokens are split according to person A's offer. If person B rejects the division, neither person A, nor person B get anything. In both cases the bargaining process is over.

No additional communication is possible between persons A and B and they will remain anonymous throughout the experiment (and their identities will be never revealed by the experimenter).

2) Comprehension check

Please answer the following questions about the general situation in this experiment:

1)
Assume that person A proposes a division of 44 tokens between himself/herself and person B.

What is the minimum person A can offer to person B? _____ tokens

What is the maximum person A can offer to person B? _____ tokens

What is the minimum person A can offer to himself/herself? _____ tokens

What is the maximum person A can offer to himself/herself? _____ tokens

2)
Assume that person A proposes a division of 33 tokens between himself/herself and person B.
Assume that person A proposed 30 tokens to himself/herself and 3 tokens to person B.

If person B accepts this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

If person B rejects this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

3) Your role in the situation

You have been randomly chosen to be in the role of person A, i.e. the one who proposes a division of tokens between himself/herself and person B.

You have been given 1000 tokens to divide between yourself and person B by the experimenter.

4) Information on a previous round of bargaining

Before you proceed to make your choice, you will be given some additional important information.

While you were answering the comprehension questions, two randomly chosen people in this room, one in the role of person A (we will call him/her "Dan/Dana"), one in the role of person B (we will call him/her "Jan/Jana") already bargained over 200 tokens (each worth 0.6 CZK) with each other.

"Dan/Dana" will not make any other bargaining decisions in this experiment. "Jan/Jana" will be randomly paired with another participant in this room to bargain in another (final) round over 1000 tokens (each worth 0.6 CZK). (Note that "Jan/Jana" knew he/she would bargain in two rounds.) It is possible, but not certain (because there are several other people here) that the participant with whom "Jan/Jana" will be randomly paired next is YOU.

If "Jan/Jana" will in fact be paired with YOU to participante in another (final) round of bargaining, you will be given information about what decisions "Jan/Jana" and "Dan/Dana" made in the first round.

If "Jan/Jana" will be paired with SOMEONE ELSE in this room (not you), you will be given information about what decisions a hypothetical player A and a hypothetical player B made in the first round.

YOU WILL NOT KNOW whether you are given information about REAL participants "Jan/Jana" and "Dan/Dana", or about HYPOTHETICAL players.

After you receive the information about the first round, you will make your own decision in the role of player A dividing 1000 tokens. When making your decision, it is entirely possible you are paired with another real player (i.e. with "Jan/Jana"). IF THIS IS THE CASE, he/she can accept or reject your division. If he/she accepts your division, the tokens are split accordingly. If he/she rejects your division, neither he/she, nor you get anything. In both cases the (final) round is over and the experiment will end soon.

If, however, you are in fact not paired with a real participant ("Jan/Jana"), you would be paired with a hypothetical player B. HOWEVER, WHEN MAKING YOUR DECISION YOU SHOULD ASSUME THAT YOU ARE PAIRED WITH THE REAL PARTICIPANT ("JAN/JANA") WHICH IS ENTIRELY POSSIBLE. Only if you are paired with the real participant "Jan/Jana", will the division of tokens in the following round have any financial consequences for both of you (i.e., each token will be worth 0.6 CZK), otherwise you will just receive the participation fee.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Now, you are given the following information about the first round of bargaining:
Person A offered 100 tokens to himself/herself and 100 tokens to the other person B. Person B accepted this offer, i.e. person A got 100 tokens, and person B got 100 tokens in that round.

YOU WILL NEXT BE PAIRED WITH THIS PERSON B. It can be the real participant ("Jan/Jana"), or it can be a hypohetical player, as described above. You will make your decision next.

5) Your decision

Please indicate your proposed division of 1000 tokens between yourself and person B.

Proposed allocation to yourself: _____ tokens

Proposed allocation to person B: _____ tokens

Remember, person B has the option to either accept or reject your proposed division. If person B accepts your division, the tokens are split according to your decision. If person B rejects your division, neither you, nor person B get anything. In both cases, this (final) round is over. WHEN MAKING YOUR DECISION, PLEASE ASSUME THAT PERSON B IS THE REAL PARTICIPANT "JAN/JANA", WHICH IS ENTIRELY POSSIBLE. Note that only if you are paired with the real "Jan/Jana", will your division of tokens have any financial consequences for both of you, otherwise you will just receive the participation fee.

6) Person B’s decision

Person B accepted [rejected] your offer. I.e.:

You receive: _____ tokens

Person B receives: _____ tokens

[Alternatively: “You were not paired with the real participant “Jan/Jana”, i.e. you will just receive the participation fee of 90 CZK.”]

Please wait until the experimenter calls you to collect your earnings.

APPENDIX E.2: INSTRUCTIONS FOR CONDITION LOW

1) General description of the situation

This experiment focuses on a situation where two persons A and B are bargaining. One round of the bargaining process consists of just two steps.

First step: Person A proposes a division of X tokens between himself/herself and person B. (The tokens can be exchanged for money at the end of the experiment. The tokens were provided by the experimenter and they were randomly given to person A.)

Second step: Person B can either accept or reject the division made by person A. If he/she accepts the division, the tokens are split according to person A's offer. If person B rejects the division, neither person A, nor person B get anything. In both cases the bargaining process is over.

No additional communication is possible between persons A and B and they will remain anonymous throughout the experiment (and their identities will be never revealed by the experimenter).

2) Comprehension check

Please answer the following questions about the general situation in this experiment:

1)
Assume that person A proposes a division of 44 tokens between himself/herself and person B.

What is the minimum person A can offer to person B? _____ tokens

What is the maximum person A can offer to person B? _____ tokens

What is the minimum person A can offer to himself/herself? _____ tokens

What is the maximum person A can offer to himself/herself? _____ tokens

2)
Assume that person A proposes a division of 33 tokens between himself/herself and person B.
Assume that person A proposed 30 tokens to himself/herself and 3 tokens to person B.

If person B accepts this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

If person B rejects this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

3) Your role in the situation

You have been randomly chosen to be in the role of person A, i.e. the one who proposes a division of tokens between himself/herself and person B.

You have been given 1000 tokens to divide between yourself and person B by the experimenter.

4) Information on a previous round of bargaining

Before you proceed to make your choice, you will be given some additional important information.

While you were answering the comprehension questions, two randomly chosen people in this room, one in the role of person A (we will call him/her "Dan/Dana"), one in the role of person B (we will call him/her "Jan/Jana") already bargained over 200 tokens (each worth 0.6 CZK) with each other.

"Dan/Dana" will not make any other bargaining decisions in this experiment. "Jan/Jana" will be randomly paired with another participant in this room to bargain in another (final) round over 1000 tokens (each worth 0.6 CZK). (Note that "Jan/Jana" knew he/she would bargain in two rounds.) It is possible, but not certain (because there are several other people here) that the participant with whom "Jan/Jana" will be randomly paired next is YOU.

If "Jan/Jana" will in fact be paired with YOU to participante in another (final) round of bargaining, you will be given information about what decisions "Jan/Jana" and "Dan/Dana" made in the first round.

If "Jan/Jana" will be paired with SOMEONE ELSE in this room (not you), you will be given information about what decisions a hypothetical player A and a hypothetical player B made in the first round.

YOU WILL NOT KNOW whether you are given information about REAL participants "Jan/Jana" and "Dan/Dana", or about HYPOTHETICAL players.

After you receive the information about the first round, you will make your own decision in the role of player A dividing 1000 tokens. When making your decision, it is entirely possible you are paired with another real player (i.e. with "Jan/Jana"). IF THIS IS THE CASE, he/she can accept or reject your division. If he/she accepts your division, the tokens are split accordingly. If he/she rejects your division, neither he/she, nor you get anything. In both cases the (final) round is over and the experiment will end soon.

If, however, you are in fact not paired with a real participant ("Jan/Jana"), you would be paired with a hypothetical player B. HOWEVER, WHEN MAKING YOUR DECISION YOU SHOULD ASSUME THAT YOU ARE PAIRED WITH THE REAL PARTICIPANT ("JAN/JANA") WHICH IS ENTIRELY POSSIBLE. Only if you are paired with the real participant "Jan/Jana", will the division of tokens in the following round have any financial consequences for both of you (i.e., each token will be worth 0.6 CZK), otherwise you will just receive the participation fee.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Now, you are given the following information about the first round of bargaining:
Person A offered 100 tokens to himself/herself and 100 tokens to the other person B. Person B rejected this offer, i.e. neither person A, nor person B got anything in that round.

YOU WILL NEXT BE PAIRED WITH THIS PERSON B. It can be the real participant ("Jan/Jana"), or it can be a hypohetical player, as described above. You will make your decision next.

5) Your decision

Please indicate your proposed division of 1000 tokens between yourself and person B.

Proposed allocation to yourself: _____ tokens

Proposed allocation to person B: _____ tokens

Remember, person B has the option to either accept or reject your proposed division. If person B accepts your division, the tokens are split according to your decision. If person B rejects your division, neither you, nor person B get anything. In both cases, this (final) round is over. WHEN MAKING YOUR DECISION, PLEASE ASSUME THAT PERSON B IS THE REAL PARTICIPANT "JAN/JANA", WHICH IS ENTIRELY POSSIBLE. Note that only if you are paired with the real "Jan/Jana", will your division of tokens have any financial consequences for both of you, otherwise you will just receive the participation fee.

6) Person B’s decision

Person B accepted [rejected] your offer. I.e.:

You receive: _____ tokens

Person B receives: _____ tokens

[Alternatively: “You were not paired with the real participant “Jan/Jana”, i.e. you will just receive the participation fee of 90 CZK.”]

Please wait until the experimenter calls you to collect your earnings.

APPENDIX E.3: INSTRUCTIONS FOR CONDITION MEDIUM

1) General description of the situation

This experiment focuses on a situation where two persons A and B are bargaining. One round of the bargaining process consists of just two steps.

First step: Person A proposes a division of X tokens between himself/herself and person B. (The tokens can be exchanged for money at the end of the experiment. The tokens were provided by the experimenter and they were randomly given to person A.)

Second step: Person B can either accept or reject the division made by person A. If he/she accepts the division, the tokens are split according to person A's offer. If person B rejects the division, neither person A, nor person B get anything. In both cases the bargaining process is over.

No additional communication is possible between persons A and B and they will remain anonymous throughout the experiment (and their identities will be never revealed by the experimenter).

2) Comprehension check

Please answer the following questions about the general situation in this experiment:

1)
Assume that person A proposes a division of 44 tokens between himself/herself and person B.

What is the minimum person A can offer to person B? _____ tokens

What is the maximum person A can offer to person B? _____ tokens

What is the minimum person A can offer to himself/herself? _____ tokens

What is the maximum person A can offer to himself/herself? _____ tokens

2)
Assume that person A proposes a division of 33 tokens between himself/herself and person B.
Assume that person A proposed 30 tokens to himself/herself and 3 tokens to person B.

If person B accepts this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

If person B rejects this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

3) Your role in the situation

You have been randomly chosen to be in the role of person A, i.e. the one who proposes a division of tokens between himself/herself and person B.

You have been given 1000 tokens to divide between yourself and person B by the experimenter.

4) Information on a previous round of bargaining

Before you proceed to make your choice, you will be given some additional important information.

While you were answering the comprehension questions, two randomly chosen people in this room, one in the role of person A (we will call him/her "Dan/Dana"), one in the role of person B (we will call him/her "Jan/Jana") already bargained over 200 tokens (each worth 0.6 CZK) with each other.

"Dan/Dana" will not make any other bargaining decisions in this experiment. "Jan/Jana" will be randomly paired with another participant in this room to bargain in another (final) round over 1000 tokens (each worth 0.6 CZK). (Note that "Jan/Jana" knew he/she would bargain in two rounds.) It is possible, but not certain (because there are several other people here) that the participant with whom "Jan/Jana" will be randomly paired next is YOU.

If "Jan/Jana" will in fact be paired with YOU to participante in another (final) round of bargaining, you will be given information about what decisions "Jan/Jana" and "Dan/Dana" made in the first round.

If "Jan/Jana" will be paired with SOMEONE ELSE in this room (not you), you will be given information about what decisions a hypothetical player A and a hypothetical player B made in the first round.

YOU WILL NOT KNOW whether you are given information about REAL participants "Jan/Jana" and "Dan/Dana", or about HYPOTHETICAL players.

After you receive the information about the first round, you will make your own decision in the role of player A dividing 1000 tokens. When making your decision, it is entirely possible you are paired with another real player (i.e. with "Jan/Jana"). IF THIS IS THE CASE, he/she can accept or reject your division. If he/she accepts your division, the tokens are split accordingly. If he/she rejects your division, neither he/she, nor you get anything. In both cases the (final) round is over and the experiment will end soon.

If, however, you are in fact not paired with a real participant ("Jan/Jana"), you would be paired with a hypothetical player B. HOWEVER, WHEN MAKING YOUR DECISION YOU SHOULD ASSUME THAT YOU ARE PAIRED WITH THE REAL PARTICIPANT ("JAN/JANA") WHICH IS ENTIRELY POSSIBLE. Only if you are paired with the real participant "Jan/Jana", will the division of tokens in the following round have any financial consequences for both of you (i.e., each token will be worth 0.6 CZK), otherwise you will just receive the participation fee.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Now, you are given the following information about the first round of bargaining:
Person A offered 90 tokens to himself/herself and 110 tokens to the other person B. Person B rejected this offer, i.e. neither person A, nor person B got anything in that round.

YOU WILL NEXT BE PAIRED WITH THIS PERSON B. It can be the real participant ("Jan/Jana"), or it can be a hypohetical player, as described above. You will make your decision next.

5) Your decision

Please indicate your proposed division of 1000 tokens between yourself and person B.

Proposed allocation to yourself: _____ tokens

Proposed allocation to person B: _____ tokens

Remember, person B has the option to either accept or reject your proposed division. If person B accepts your division, the tokens are split according to your decision. If person B rejects your division, neither you, nor person B get anything. In both cases, this (final) round is over. WHEN MAKING YOUR DECISION, PLEASE ASSUME THAT PERSON B IS THE REAL PARTICIPANT "JAN/JANA", WHICH IS ENTIRELY POSSIBLE. Note that only if you are paired with the real "Jan/Jana", will your division of tokens have any financial consequences for both of you, otherwise you will just receive the participation fee.

6) Person B’s decision

Person B accepted [rejected] your offer. I.e.:

You receive: _____ tokens

Person B receives: _____ tokens

[Alternatively: “You were not paired with the real participant “Jan/Jana”, i.e. you will just receive the participation fee of 90 CZK.”]

Please wait until the experimenter calls you to collect your earnings.

APPENDIX E.4: INSTRUCTIONS FOR CONDITION HIGH

1) General description of the situation

This experiment focuses on a situation where two persons A and B are bargaining. One round of the bargaining process consists of just two steps.

First step: Person A proposes a division of X tokens between himself/herself and person B. (The tokens can be exchanged for money at the end of the experiment. The tokens were provided by the experimenter and they were randomly given to person A.)

Second step: Person B can either accept or reject the division made by person A. If he/she accepts the division, the tokens are split according to person A's offer. If person B rejects the division, neither person A, nor person B get anything. In both cases the bargaining process is over.

No additional communication is possible between persons A and B and they will remain anonymous throughout the experiment (and their identities will be never revealed by the experimenter).

2) Comprehension check

Please answer the following questions about the general situation in this experiment:

1)
Assume that person A proposes a division of 44 tokens between himself/herself and person B.

What is the minimum person A can offer to person B? _____ tokens

What is the maximum person A can offer to person B? _____ tokens

What is the minimum person A can offer to himself/herself? _____ tokens

What is the maximum person A can offer to himself/herself? _____ tokens

2)
Assume that person A proposes a division of 33 tokens between himself/herself and person B.
Assume that person A proposed 30 tokens to himself/herself and 3 tokens to person B.

If person B accepts this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

If person B rejects this offer, how many tokens will each get?

How many tokens will person A get? _____ tokens

How many tokens will person B get? _____ tokens

3) Your role in the situation

You have been randomly chosen to be in the role of person A, i.e. the one who proposes a division of tokens between himself/herself and person B.

You have been given 1000 tokens to divide between yourself and person B by the experimenter.

4) Information on a previous round of bargaining

Before you proceed to make your choice, you will be given some additional important information.

While you were answering the comprehension questions, two randomly chosen people in this room, one in the role of person A (we will call him/her "Dan/Dana"), one in the role of person B (we will call him/her "Jan/Jana") already bargained over 200 tokens (each worth 0.6 CZK) with each other.

"Dan/Dana" will not make any other bargaining decisions in this experiment. "Jan/Jana" will be randomly paired with another participant in this room to bargain in another (final) round over 1000 tokens (each worth 0.6 CZK). (Note that "Jan/Jana" knew he/she would bargain in two rounds.) It is possible, but not certain (because there are several other people here) that the participant with whom "Jan/Jana" will be randomly paired next is YOU.

If "Jan/Jana" will in fact be paired with YOU to participante in another (final) round of bargaining, you will be given information about what decisions "Jan/Jana" and "Dan/Dana" made in the first round.

If "Jan/Jana" will be paired with SOMEONE ELSE in this room (not you), you will be given information about what decisions a hypothetical player A and a hypothetical player B made in the first round.

YOU WILL NOT KNOW whether you are given information about REAL participants "Jan/Jana" and "Dan/Dana", or about HYPOTHETICAL players.

After you receive the information about the first round, you will make your own decision in the role of player A dividing 1000 tokens. When making your decision, it is entirely possible you are paired with another real player (i.e. with "Jan/Jana"). IF THIS IS THE CASE, he/she can accept or reject your division. If he/she accepts your division, the tokens are split accordingly. If he/she rejects your division, neither he/she, nor you get anything. In both cases the (final) round is over and the experiment will end soon.

If, however, you are in fact not paired with a real participant ("Jan/Jana"), you would be paired with a hypothetical player B. HOWEVER, WHEN MAKING YOUR DECISION YOU SHOULD ASSUME THAT YOU ARE PAIRED WITH THE REAL PARTICIPANT ("JAN/JANA") WHICH IS ENTIRELY POSSIBLE. Only if you are paired with the real participant "Jan/Jana", will the division of tokens in the following round have any financial consequences for both of you (i.e., each token will be worth 0.6 CZK), otherwise you will just receive the participation fee.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Now, you are given the following information about the first round of bargaining:
Person A offered 70 tokens to himself/herself and 130 tokens to the other person B. Person B rejected this offer, i.e. neither person A, nor person B got anything in that round.

YOU WILL NEXT BE PAIRED WITH THIS PERSON B. It can be the real participant ("Jan/Jana"), or it can be a hypohetical player, as described above. You will make your decision next.

5) Your decision

Please indicate your proposed division of 1000 tokens between yourself and person B.

Proposed allocation to yourself: _____ tokens

Proposed allocation to person B: _____ tokens

Remember, person B has the option to either accept or reject your proposed division. If person B accepts your division, the tokens are split according to your decision. If person B rejects your division, neither you, nor person B get anything. In both cases, this (final) round is over. WHEN MAKING YOUR DECISION, PLEASE ASSUME THAT PERSON B IS THE REAL PARTICIPANT "JAN/JANA", WHICH IS ENTIRELY POSSIBLE. Note that only if you are paired with the real "Jan/Jana", will your division of tokens have any financial consequences for both of you, otherwise you will just receive the participation fee.

6) Person B’s decision

Person B accepted [rejected] your offer. I.e.:

You receive: _____ tokens

Person B receives: _____ tokens

[Alternatively: “You were not paired with the real participant “Jan/Jana”, i.e. you will just receive the participation fee of 90 CZK.”]

Please wait until the experimenter calls you to collect your earnings.

APPENDIX F: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 8

The following instructions are translated from Czech. They were presented in paper form. Response sheets were collected at the conclusion of each Part of the experiment and instructions and response sheets for the next Part were then distributed.

APPENDIX F.1: INSTRUCTIONS FOR CONDITION IAF: INCENTIVIZED KW GAME USED, APPROPRIATENESS RATED FIRST, FAIRNESS RATED SECOND

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 90 CZK. You may receive some additional money based on your choices and the choices of others during the experiment. The experiment will have five parts [instructions for parts III-V are available upon request from the author; results from these parts of the experiment are not reported in this paper].

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid. All participants in today's session are given exactly the same set of instructions.

Please wait to turn the page until the experimenter asks you to do so.

General description of Part I of the experiment

In this experiment, you will read description of a situation in which an “Individual A” and an “Individual B” make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A “COMMON” RATING, RATHER THAN YOUR “PERSONAL” RATING.

For *each* of the possible choices, you will indicate whether taking that action would be “socially appropriate” and “consistent with what most people expect Individual A ought to do” or “socially inappropriate” and “inconsistent with what most people expect Individual A ought to do.”

By socially appropriate we mean behavior that most people agree is the “proper” thing to do and most people expect that person A ought to do it.

Based on your ratings of social appropriateness you will be able to earn money today. At the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

To give you an idea of how the experiment will proceed, we will go through an example and show you how you will indicate your responses. On the next page you will see an example situation.

Example situation (part 1 of the description)

Individual A is at a local coffee shop near campus. While there, Individual A notices that someone has left a wallet at one of the tables. Individual A must decide what to do. Individual A has four possible choices: take the wallet, ask others nearby if the wallet belongs to them, leave the wallet where it is, or give the wallet to the shop manager. Individual A can choose one of these four options.

The table below presents a list of the possible choices available to Individual A. For each of the choices, you will be asked to indicate whether you believe choosing that option is: very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate.

You will rate *each* action on social appropriateness. To rate an action, you would place a checkmark (X) in the corresponding box (see the table below and the next page).

Choice	Appropriateness ratings			
	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Take the wallet				
Ask others nearby if the wallet belongs to them				
Leave the wallet where it is				
Give the wallet to the shop manager				

Example situation (part 2 of the description)

If this example situation were one of the situations for this study, you would consider each of the possible choices in the table below and, for each choice, indicate the extent to which you believe taking that action would be "socially appropriate" and "consistent with what most people expect Individual A ought to do" or "socially inappropriate" and "inconsistent with what most people expect that Individual A ought to do". You should provide what you think would be a "common" rating, rather than your "personal" rating.

For example, suppose you believe that most people think that taking the wallet is "very socially inappropriate", asking others nearby if the wallet belongs to them is "somewhat socially appropriate", leaving the wallet is "somewhat socially inappropriate" and giving the wallet to the shop manager is "very socially appropriate". Then you would have rated the actions in the following way:

Choice	Appropriateness ratings			
	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Take the wallet	X			
Ask others nearby if the wallet belongs to them			X	
Leave the wallet where it is		X		
Give the wallet to the shop manager				X

Are there any questions about this example situation or about how to indicate your responses?

Monetary bonus

As mentioned before, at the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. (In case of a tie between two or more most frequent ratings, one of them will be selected randomly to serve as "the" most frequent one.) If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

For instance, suppose we randomly selected the possible choice "Leave the wallet where it is". If your rating of this choice had been "somewhat socially inappropriate" then you would receive 75 CZK, in addition to the 90 CZK participation fee, if this was the response selected by most other people in today's session. Otherwise, you would receive only the 90 CZK participation fee.

Are there any questions about the monetary bonus?

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER THAT YOU WILL EARN MONEY (75 CZK) IF YOUR RATING OF ONE RANDOMLY SELECTED CHOICE IS THE SAME AS THE MOST COMMON RATING OF THAT CHOICE PROVIDED IN TODAY'S SESSION.

Rate Individual A's choices

Individual A's choice	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

Part II of the experiment

This part of the experiment is similar to Part I of the experiment. In this part, you will read DESCRIPTION OF THE SAME SITUATION AS IN PART I in which an "Individual A" and an "Individual B" make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. You should provide what you think would be a "common" rating, rather than your "personal" rating.

For *each* of the possible choices, you will indicate whether taking that action would be "fair" and "consistent with what most people think is a fair and just thing to do" or "unfair" and "inconsistent with what most people think is a fair and just thing to do" in the situation.

By fair we mean behavior that most people agree is the "fair" or "just" thing to do.

Based on your ratings of fairness you will be able to earn money today. At the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. (In case of a tie between two or more most frequent ratings, one of them will be selected randomly to serve as "the" most frequent one.) If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

On the following page you will read description of a situation and a table on the page after that presents a list of possible choices available to Individual A in that situation.

For *each* of the possible choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box.

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER THAT YOU WILL EARN MONEY (75 CZK) IF YOUR RATING OF ONE RANDOMLY SELECTED CHOICE IS THE SAME AS THE MOST COMMON RATING OF THAT CHOICE PROVIDED IN TODAY'S SESSION.

Rate Individual A's choices

Individual A's choice	Very unfair	Somewh at unfair	Somewh at fair	Very fair
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

APPENDIX F.2: INSTRUCTIONS FOR CONDITION IFF: INCENTIVIZED KW GAME USED, FAIRNESS RATED FIRST, APPROPRIATENESS RATED SECOND

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 90 CZK. You may receive some additional money based on your choices and the choices of others during the experiment. The experiment will have five parts [instructions for parts III-V are available upon request from the author; results from these parts of the experiment are not reported in this paper].

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid. All participants in today's session are given exactly the same set of instructions.

Please wait to turn the page until the experimenter asks you to do so.

General description of Part I of the experiment

In this experiment, you will read description of a situation in which an “Individual A” and an “Individual B” make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A “COMMON” RATING, RATHER THAN YOUR “PERSONAL” RATING.

For *each* of the possible choices, you will indicate whether taking that action would be “fair” and “consistent with what most people think is a fair and just thing to do” or “unfair” and “inconsistent with what most people think is a fair and just thing to do” in the situation.

By fair we mean behavior that most people agree is the “fair” or “just” thing to do.

Based on your ratings of fairness you will be able to earn money today. At the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

To give you an idea of how the experiment will proceed, we will go through an example and show you how you will indicate your responses. On the next page you will see an example situation.

Example situation (part 1 of the description)

Individual A is at a local coffee shop near campus. While there, Individual A notices that someone has left a wallet at one of the tables. Individual A must decide what to do. Individual A has four possible choices: take the wallet, ask others nearby if the wallet belongs to them, leave the wallet where it is, or give the wallet to the shop manager. Individual A can choose one of these four options.

The table below presents a list of the possible choices available to Individual A. For each of the choices, you will be asked to indicate whether you believe choosing that option is: very unfair, somewhat unfair, somewhat fair, or very fair.

You will rate *each* action on fairness. To rate an action, you would place a checkmark (X) in the corresponding box (see the table below and the next page).

Choice	Fairness ratings			
	Very unfair	Somewhat unfair	Somewhat fair	Very fair
Take the wallet				
Ask others nearby if the wallet belongs to them				
Leave the wallet where it is				
Give the wallet to the shop manager				

Example situation (part 2 of the description)

If this example situation were one of the situations for this study, you would consider each of the possible choices in the table below and, for each choice, indicate the extent to which you believe taking that action would be "fair" and "consistent with what most people think is a fair and just thing to do" or "unfair" and "inconsistent with what most people think is a fair and just thing to do". You should provide what you think would be a "common" rating, rather than your "personal" rating.

For example, suppose you believe that most people think that taking the wallet is "very unfair", asking others nearby if the wallet belongs to them is "somewhat unfair", leaving the wallet is "somewhat unfair" and giving the wallet to the shop manager is "very fair". Then you would have rated the actions in the following way:

Choice	Fairness ratings			
	Very unfair	Somewhat unfair	Somewhat fair	Very fair
Take the wallet	X			
Ask others nearby if the wallet belongs to them			X	
Leave the wallet where it is		X		
Give the wallet to the shop manager				X

Are there any questions about this example situation or about how to indicate your responses?

Monetary bonus

As mentioned before, at the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. (In case of a tie between two or more most frequent ratings, one of them will be selected randomly to serve as "the" most frequent one.) If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

For instance, suppose we randomly selected the possible choice "Leave the wallet where it is". If your rating of this choice had been "somewhat unfair" then you would receive 75 CZK, in addition to the 90 CZK participation fee, if this was the response selected by most other people in today's session. Otherwise, you would receive only the 90 CZK participation fee.

Are there any questions about the monetary bonus?

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER THAT YOU WILL EARN MONEY (75 CZK) IF YOUR RATING OF ONE RANDOMLY SELECTED CHOICE IS THE SAME AS THE MOST COMMON RATING OF THAT CHOICE PROVIDED IN TODAY'S SESSION.

Rate Individual A's choices

Individual A's choice	Very unfair	Somewh at unfair	Somewh at fair	Very fair
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

Part II of the experiment

This part of the experiment is similar to Part I of the experiment. In this part, you will read DESCRIPTION OF THE SAME SITUATION AS IN PART I in which an "Individual A" and an "Individual B" make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. You should provide what you think would be a "common" rating, rather than your "personal" rating.

For *each* of the possible choices, you will indicate whether taking that action would be "socially appropriate" and "consistent with what most people expect Individual A ought to do" or "socially inappropriate" and "inconsistent with what most people expect Individual A ought to do."

By socially appropriate we mean behavior that most people agree is the "proper" thing to do and most people expect that person A ought to do it.

Based on your ratings of social appropriateness you will be able to earn money today. At the end of the experiment today, we will randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine which response was selected by the most people here today. (In case of a tie between two or more most frequent ratings, one of them will be selected randomly to serve as "the" most frequent one.) If you give the same response as that most frequently given by other people, then you will receive an additional 75 CZK. This amount will be paid to you, in cash, at the conclusion of the experiment.

On the following page you will read description of a situation and a table on the page after that presents a list of possible choices available to Individual A in that situation.

For *each* of the possible choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box.

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER THAT YOU WILL EARN MONEY (75 CZK) IF YOUR RATING OF ONE RANDOMLY SELECTED CHOICE IS THE SAME AS THE MOST COMMON RATING OF THAT CHOICE PROVIDED IN TODAY'S SESSION.

Rate Individual A's choices

Individual A's choice	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

APPENDIX F.3: INSTRUCTIONS FOR CONDITION NIAF: INCENTIVES NOT USED, APPROPRIATENESS RATED FIRST, FAIRNESS RATED SECOND

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 120 CZK. You may receive some additional money based on your choices and the choices of others during the experiment. The experiment will have five parts [instructions for parts III-V are available upon request from the author; results from these parts of the experiment are not reported in this paper].

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid. All participants in today's session are given exactly the same set of instructions.

Please wait to turn the page until the experimenter asks you to do so.

General description of Part I of the experiment

In this experiment, you will read description of a situation in which an “Individual A” and an “Individual B” make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A “COMMON” RATING, RATHER THAN YOUR “PERSONAL” RATING.

Ffor *each* of the possible choices, you will indicate whether taking that action would be “socially appropriate” and “consistent with what most people expect Individual A ought to do” or “socially inappropriate” and “inconsistent with what most people expect Individual A ought to do.”

By socially appropriate we mean behavior that most people agree is the “proper” thing to do and most people expect that person A ought to do it.

Remember, you should provide what you think would be a “common” rating (e.g. the most common rating among people in today’s session), rather than your “personal” rating.

To give you an idea of how the experiment will proceed, we will go through an example and show you how you will indicate your responses. On the next page you will see an example situation.

Example situation (part 1 of the description)

Individual A is at a local coffee shop near campus. While there, Individual A notices that someone has left a wallet at one of the tables. Individual A must decide what to do. Individual A has four possible choices: take the wallet, ask others nearby if the wallet belongs to them, leave the wallet where it is, or give the wallet to the shop manager. Individual A can choose one of these four options.

The table below presents a list of the possible choices available to Individual A. For each of the choices, you will be asked to indicate whether you believe choosing that option is: very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate.

You will rate *each* action on social appropriateness. To rate an action, you would place a checkmark (X) in the corresponding box (see the table below and the next page).

Choice	Appropriateness ratings			
	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Take the wallet				
Ask others nearby if the wallet belongs to them				
Leave the wallet where it is				
Give the wallet to the shop manager				

Example situation (part 2 of the description)

If this example situation were one of the situations for this study, you would consider each of the possible choices in the table below and, for each choice, indicate the extent to which you believe taking that action would be "socially appropriate" and "consistent with what most people expect Individual A ought to do" or "socially inappropriate" and "inconsistent with what most people expect that Individual A ought to do". You should provide what you think would be a "common" rating, rather than your "personal" rating.

For example, suppose you believe that most people think that taking the wallet is "very socially inappropriate", asking others nearby if the wallet belongs to them is "somewhat socially appropriate", leaving the wallet is "somewhat socially inappropriate" and giving the wallet to the shop manager is "very socially appropriate". Then you would have rated the actions in the following way:

Choice	Appropriateness ratings			
	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Take the wallet	X			
Ask others nearby if the wallet belongs to them			X	
Leave the wallet where it is		X		
Give the wallet to the shop manager				X

Are there any questions about this example situation or about how to indicate your responses?

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER, YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING (E.G. THE MOST COMMON RATING AMONG PEOPLE IN TODAY'S SESSION), RATHER THAN YOUR "PERSONAL" RATING.

Rate Individual A's choices

Individual A's choice	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

Part II of the experiment

This part of the experiment is similar to Part I of the experiment. In this part, you will read DESCRIPTION OF THE SAME SITUATION AS IN PART I in which an "Individual A" and an "Individual B" make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. You should provide what you think would be a "common" rating, rather than your "personal" rating.

For *each* of the possible choices, you will indicate whether taking that action would be "fair" and "consistent with what most people think is a fair and just thing to do" or "unfair" and "inconsistent with what most people think is a fair and just thing to do" in the situation.

By fair we mean behavior that most people agree is the "fair" or "just" thing to do.

Remember, you should provide what you think would be a "common" rating (e.g. the most common rating among people in today's session), rather than your "personal" rating.

On the following page you will read description of a situation and a table on the page after that presents a list of possible choices available to Individual A in that situation.

For *each* of the possible choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box.

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER, YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING (E.G. THE MOST COMMON RATING AMONG PEOPLE IN TODAY'S SESSION), RATHER THAN YOUR "PERSONAL" RATING.

Rate Individual A's choices

Individual A's choice	Very unfair	Somewhat unfair	Somewhat fair	Very fair
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

APPENDIX F.4: INSTRUCTIONS FOR CONDITION NIFF: INCENTIVES NOT USED, FAIRNESS RATED FIRST, APPROPRIATENESS RATED SECOND

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 120 CZK. You may receive some additional money based on your choices and the choices of others during the experiment. The experiment will have five parts [instructions for parts III-V are available upon request from the author; results from these parts of the experiment are not reported in this paper].

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid. All participants in today's session are given exactly the same set of instructions.

Please wait to turn the page until the experimenter asks you to do so.

General description of Part I of the experiment

In this experiment, you will read description of a situation in which an "Individual A" and an "Individual B" make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING, RATHER THAN YOUR "PERSONAL" RATING.

For *each* of the possible choices, you will indicate whether taking that action would be "fair" and "consistent with what most people think is a fair and just thing to do" or "unfair" and "inconsistent with what most people think is a fair and just thing to do" in the situation.

By fair we mean behavior that most people agree is the "fair" or "just" thing to do.

Remember, you should provide what you think would be a "common" rating (e.g. the most common rating among people in today's session), rather than your "personal" rating.

To give you an idea of how the experiment will proceed, we will go through an example and show you how you will indicate your responses. On the next page you will see an example situation.

Example situation (part 1 of the description)

Individual A is at a local coffee shop near campus. While there, Individual A notices that someone has left a wallet at one of the tables. Individual A must decide what to do. Individual A has four possible choices: take the wallet, ask others nearby if the wallet belongs to them, leave the wallet where it is, or give the wallet to the shop manager. Individual A can choose one of these four options.

The table below presents a list of the possible choices available to Individual A. For each of the choices, you will be asked to indicate whether you believe choosing that option is: very unfair, somewhat unfair, somewhat fair, or very fair.

You will rate *each* action on fairness. To rate an action, you would place a checkmark (X) in the corresponding box (see the table below and the next page).

Choice	Fairness ratings			
	Very unfair	Somewhat unfair	Somewhat fair	Very fair
Take the wallet				
Ask others nearby if the wallet belongs to them				
Leave the wallet where it is				
Give the wallet to the shop manager				

Example situation (part 2 of the description)

If this example situation were one of the situations for this study, you would consider each of the possible choices in the table below and, for each choice, indicate the extent to which you believe taking that action would be "fair" and "consistent with what most people think is a fair and just thing to do" or "unfair" and "inconsistent with what most people think is a fair and just thing to do". You should provide what you think would be a "common" rating, rather than your "personal" rating.

For example, suppose you believe that most people think that taking the wallet is "very unfair", asking others nearby if the wallet belongs to them is "somewhat unfair", leaving the wallet is "somewhat unfair" and giving the wallet to the shop manager is "very fair". Then you would have rated the actions in the following way:

Choice	Fairness ratings			
	Very unfair	Somewhat unfair	Somewhat fair	Very fair
Take the wallet				
Ask others nearby if the wallet belongs to them				
Leave the wallet where it is				
Give the wallet to the shop manager				

Are there any questions about this example situation or about how to indicate your responses?

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very unfair, somewhat unfair, somewhat fair, very fair. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER, YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING (E.G. THE MOST COMMON RATING AMONG PEOPLE IN TODAY'S SESSION), RATHER THAN YOUR "PERSONAL" RATING.

Rate Individual A's choices

Individual A's choice	Very unfair	Somewh at unfair	Somewh at fair	Very fair
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

Part II of the experiment

This part of the experiment is similar to Part I of the experiment. In this part, you will read DESCRIPTION OF THE SAME SITUATION AS IN PART I in which an "Individual A" and an "Individual B" make decisions. For this situation, you will be given a description of possible choices available to Individuals A and B.

After you read the description of the situation and possible choices, you will be asked to evaluate the different possible choices available to Individual A. You should provide what you think would be a "common" rating, rather than your "personal" rating.

For *each* of the possible choices, you will indicate whether taking that action would be "socially appropriate" and "consistent with what most people expect Individual A ought to do" or "socially inappropriate" and "inconsistent with what most people expect Individual A ought to do."

By socially appropriate we mean behavior that most people agree is the "proper" thing to do and most people expect that person A ought to do it.

Remember, you should provide what you think would be a "common" rating (e.g. the most common rating among people in today's session), rather than your "personal" rating.

On the following page you will read description of a situation and a table on the page after that presents a list of possible choices available to Individual A in that situation.

For *each* of the possible choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box.

Description of the situation and choices and their rating

Suppose that Individual A is randomly paired with another person, Individual B in an experiment. The pairing is anonymous, meaning that neither individual will ever know the identity of the other individual with whom he or she is paired.

In the experiment, first Individual A will make a choice. After learning the choice made by Individual A, Individual B will make a choice. The experimenter will record the choices, and then both individuals will be paid money based on their choices. Suppose that neither individual will receive any other money for participating in the experiment, but a small participation fee ($1).

In each pair, Individual A will initially receive $10 and Individual B will initially receive $0.

First, Individual A will have the opportunity to give any amount of his or her $10 to Individual B. The choice must be made in $1 increments (see the table on the next page for all possible choices).

Second, after learning about Individual's A choice, Individual B will then have the opportunity to either accept or reject Individual's B proposed division of the money. If Individual B accepts the division, Individual's A choice is implemented. If Individual B does not accept the division, neither individual gets anything (but the small participation fee, $1, which they get in any case, irrespective of the decisions made during the experiment).

The choices by Individual A and Individual B will determine how much money each participant will receive, privately and in cash, at the end of the experiment.

The table on the next page gives a list of the possible choices available to INDIVIDUAL A. For each of the choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark (X) in the corresponding box in the table on the next page. You should rate the offers from the position of a third, neutral party that is not involved in the situation described above in any way.

REMEMBER, YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING (E.G. THE MOST COMMON RATING AMONG PEOPLE IN TODAY'S SESSION), RATHER THAN YOUR "PERSONAL" RATING.

Rate Individual A's choices

Individual A's choice	Very socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Very socially appropriate
Individual A offers $0 (out of $10) to Individual B				
Individual A offers $1 (out of $10) to Individual B				
Individual A offers $2 (out of $10) to Individual B				
Individual A offers $3 (out of $10) to Individual B				
Individual A offers $4 (out of $10) to Individual B				
Individual A offers $5 (out of $10) to Individual B				
Individual A offers $6 (out of $10) to Individual B				
Individual A offers $7 (out of $10) to Individual B				
Individual A offers $8 (out of $10) to Individual B				
Individual A offers $9 (out of $10) to Individual B				
Individual A offers $10 (out of $10) to Individual B				

APPENDIX G: INSTRUCTIONS FOR EXPERIMENT REPORTED IN CHAPTER 9

The following instructions are translated from Czech. They were presented in computerized form. The layout of the computerized version was, of course, somewhat different from what is shown here, e.g. subjects indicated their choices by clicking on radio-buttons in the computerized version.

APPENDIX G.1: INSTRUCTIONS FOR PART 1

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 250 CZK.

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

General description of Part 1 of the study

On the following screens, you will read descriptions of behaviors divided into six thematic blocks. You will be asked to provide the following information for each behavior:

First, for each of the behaviors, please indicate if you most of the time personally perform this behavior. Therefore, answer “no” if you never perform this behavior or if you perform it only rarely. Answer “yes” if you always or usually perform this behavior.

Second, for each of the behaviors, please indicate how many people you personally know do you think perform the behavior in question. You can indicate the prevalence of each behavior on a scale from 1 (“no one I personally know does this”) to 7 (“everyone I personally know does this”). The midpoint of the scale can be interpreted as “about half of the people I know do this”.

Third, for each of the behaviors, please indicate whether people close to you approve of the behavior in question. You can indicate this on a scale from 1 (“people close to me very strongly disapprove of this behavior”) to 7 (“people close to me very strongly approve of this behavior”).

Fourth, for each of the behaviors, please indicate whether the effect the behavior in question has on the natural environment is important for you when you decide to either perform that behavior or not to perform it. You can indicate this on a scale from 1 (“the effect the behavior has on the natural environment is completely irrelevant when I decide whether to perform that behavior or not”) to 7 (“the effect the behavior has on the natural environment is extremely important when I decide whether to perform that behavior or not”).

Fifth, for each of the behaviors, please indicate whether the difficulty or inconvenience of performing the behavior in question is important for you when you decide to either perform that behavior or not to perform it. You can indicate this on a scale from 1 (“the difficulty or inconvenience of performing the behavior is completely irrelevant when I decide whether to perform that behavior or not”) to 7 (“the difficulty or inconvenience of performing the behavior is extremely important when I decide whether to perform that behavior or not”).

Finally, for each of the behaviors, please indicate whether what most people in the society agree is the socially appropriate and “proper” thing to do (with respect to the behavior in question) and what most people expect that everyone ought to do is important for you when you decide to either perform that behavior or not to perform it. You can indicate this on a scale from 1 (“social appropriateness of the behavior is completely irrelevant when I decide whether to perform that behavior or not”) to 7 (“social appropriateness of the behavior is extremely important when I decide whether to perform that behavior or not”).

Block 1: Garbage

[Items:]

- Do you return dead batteries to collection points for dangerous waste?
- Do you bring unused medicine back to the pharmacy?
- Do you collect and recycle used paper?
- Do you use a compost bin?
- Do you collect and recycle used plastic?
- Do you collect and recycle used metal?
- Do you collect and recycle used glass?

[Questions for each item:]

- Do you perform this behavior?[130]
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

[130] Possible responses to the first question ("Behavior") are given in Appendix G.3. Possible responses to the other questions are indicated in the text above.

Block 2: Water and power consumption

[Items:]

- Do you rather shower than take a bath?
- Do you wait to have a full load before doing your laundry?
- Do you wash dirty clothes without prewashing?
- Is the heater in your house shut off late at night?
- Do you close the door to the refrigerator quickly when you take something out?
- Do you cook with a lid on the pots?
- Do you switch off or disconnect electric devices completely when not using them (not using standby)?
- Do you lower the room temperature when you are not at home for longer periods?
- Do you use energy saving light bulbs wherever possible?
- When buying new electronic devices, do you make sure to buy the ones with the lowest energy use?
- Are you maintaining electrical devices so that they are in good shape and do not use more energy than necessary?
- Are you de-icing the refrigerator regularly?
- Are you getting your electricity from a provider that guarantees 100% regenerative energy production?
- Do you use a tumble dryer to dry clothes?
- Do you use air conditioning during summer?
- Do you set the air conditioning temperature in the car below 24°C on hot days?

[Questions for each item:]

- Do you perform this behavior?
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

Block 3: Consumer behavior

[Items:]

- Do you use fabric softener with your laundry?
- If there are insects in your apartment, do you use chemical insecticides to kill them?
- Do you use chemical toilet cleaners?
- Do you use an all-purpose cleaner to clean the bathroom instead of cleaners made especially for the bathroom?
- Do you use phosphate-free laundry detergent?
- Do you buy organic vegetables?
- Do you buy organic dairy products?
- Do you buy organic meat products?
- Do you eat meat?
- Do you prefer to buy local food products?
- Do you prefer to buy things for the daily need at the nearest store rather than driving to a store where things are probably cheaper?

[Questions for each item:]

- Do you perform this behavior?
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

Block 4: Garbage inhibition

[Items:]

- Do you buy beverages in cans?
- In supermarkets, do you usually buy fruits and vegetables from the open bins (not prepacked)?
- Do you reuse your shopping bags?
- Do you bring your own coffee cup to school/work (instead of using cups from the coffee machine)?
- Do you order take-out from restaurants?
- Do you use rechargeable batteries?
- Do you usually buy beverages in reusable bottles?

[Questions for each item:]

- Do you perform this behavior?
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

Block 5: Automobile use

[Items:]

- When possible in nearby areas (around 30 km), do you use public transportation or ride a bike?
- Do you walk, ride a bicycle, or take public transportation to work or school?
- Do you drive a car with low fuel consumption?
- Do you drive an alternative fuel car (e.g., an electric vehicle)?
- Are you a member of a car sharing organization?
- Have you decided not to buy a car, even though you could afford it?
- Do you drive your car defensively to safe fuel?

[Questions for each item:]

- Do you perform this behavior?
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

Block 6: Volunteering in nature-protection activities/political support

[Items:]

- Do you often talk with friends about problems related to the environment?
- Are you a member of an environmental organization?
- Are you pointing out their unecological behavior to other people?
- Do you sometimes contribute financially to environmental organizations?
- Do you support higher taxes for environmentally damaging behavior?
- Do you support financial incentives for environmentally beneficial technology?
- Do you support strong measures against climate change?
- Do you vote for parties that have environmental topics high on their agenda?

[Questions for each item:]

- Do you perform this behavior?
- How many people you personally know perform this behavior?
- Do people close to you approve of this behavior?
- Is the effect this behavior has on the natural environment important for you when you decide to either perform that behavior or not to perform it?
- Is the difficulty or inconvenience of performing this behavior important for you when you decide to either perform that behavior or not to perform it?
- Is the degree of social appropriateness of this behavior important for you when you decide to either perform that behavior or not to perform it?

[Subsequently, participants made choices in nine short decisions task. Instructions for this part of the study are omitted here, because results from these tasks are not reported in my thesis.]

APPENDIX G.2: INSTRUCTIONS FOR PART 2

Welcome and thank you for participating in the experiment! For your participation, you will be paid a participation fee of 180 CZK. You may receive some additional money based on your choices and the choices of others during the experiment.

If you have any questions during the study, please raise your hand and wait for an experimenter to come to you. Please do not talk or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

General description of the experiment

On the following screens, you will read descriptions of a series of choices/behaviors divided into thematic blocks. After you read the description of possible choices/behaviors in each block, you will be asked to evaluate all the different possible choices/behaviors available to individual A.

First, for *each* of the possible choices/behaviors, you will indicate whether taking that action would be "socially appropriate" and "consistent with what most people expect individual A ought to do" or "socially inappropriate" and "inconsistent with what most people expect individual A ought to do." By socially appropriate, we mean behavior that most people agree is the "proper" thing to do and most people expect that person A ought to do it.

Second, for *each* of the possible choices/behaviors, you will indicate what effect will taking that action have on the environment, i.e. whether it would be "effective in protecting the environment" or "harmful towards the environment". When you rate the effect an action by Individual A has on natural environment, think about this effect against the backdrop of alternative actions Individual A could choose in a given situation. That is, say to yourself: "Compared to other actions Individual A can perform in the respective situation, does this action protect or harm the environment?"

Third, for *each* of the possible choices/behaviors, you will indicate whether taking that action would be "difficult to carry out". By difficult to carry out we mean "subjectively difficult for many people" or "inconvenient". There are things that are objectively quite easy to perform, but are subjectively difficult or inconvenient for many people (for example to exercise daily for 15 minutes). Please, rate actions you think are subjectively difficult for many people as "difficult". Of course, rate actions you think are objectively difficult as "difficult", also, because such actions are also subjectively difficult for many people (for example running 100 meters under 10 seconds).

Based on your ratings of social appropriateness, of the effect on the environment, and of difficulty you will be able to earn money today.

At the end of the experiment today, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. Thus, we will select both a thematic block and one possible choice at random. For the choice selected, we will determine what ratings of social appropriateness were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of social appropriateness for the randomly selected choice is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

Similarly, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine what ratings of effect on the environment were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of the selected choice's effect on the environment is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

Similarly, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine what ratings of difficulty were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of difficulty of performing the selected choice is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

In sum, you could earn as much as 450 CZK in today's experiment in addition to the 180 CZK participation fee.

To give you an idea of how the experiment will proceed, we will go through an example and show you how you will indicate your responses. On the next screen you will see an example situation.

Example situation (part 1 of the description)

Individual A reads newspaper in a park. After he is finished reading and is about to leave, he must decide what to do. Individual A has four possible choices: leave the newspaper on the park bench, take the newspaper with him/her home and later throw it in a recycling container for used paper near his/her house, throw the newspaper in a nearby waste basket, go to a distant recycling container for used paper and throw the newspaper in there. Individual A can choose one of these four options.

A list of the four possible choices available to individual A is presented below. For each of the choices, you will be asked to indicate whether you believe choosing that option is:

- very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, very socially appropriate;
- very harmful towards the environment, harmful towards the environment, somewhat harmful towards the environment, somewhat effective in protecting the environment effective in protecting the environment, very effective in protecting the environment;
- very difficult to carry out, somewhat difficult to carry out, somewhat easy to carry out, very easy to carry out.

You will rate *each* action on social appropriateness, its effect on the environment, and difficulty of carrying that choice out.

Example situation (part 2 of the description)

If this example situation were one of the situations for this study, you would consider each of the possible choices in the table below and, for that choice, indicate the extent to which you believe taking that action would be 1) "socially appropriate" and "consistent with what most people expect individual A ought to do", or "socially inappropriate" and "inconsistent with what most people expect that individual A ought to do", 2) "effective in protecting the environment", or "harmful towards the environment", 3) "difficult" or "inconvenient" to carry out, or "easy" and "convenient" to carry out. REMEMBER, YOU SHOULD PROVIDE WHAT YOU THINK WOULD BE A "COMMON" RATING, RATHER THAN YOUR "PERSONAL" RATING.

For example, suppose you believe that most people think that:
- leaving the newspaper on the park bench is "very socially inappropriate", "harmful towards the environment" and "very easy to carry out";
- taking the newspaper home and later throwing it in a recycling container for used paper near his/her house is "very socially appropriate", "very effective in protecting the environment" and "somewhat difficult to carry out";
- throwing the newspaper in a nearby waste basket is "somewhat socially appropriate", "somewhat harmful towards the environment" and "very easy to carry out";
- going to a distant recycling container for used paper and throwing the newspaper in there is "very socially appropriate", "very effective in protecting the environment" and "very difficult to carry out".

Then you would have rated the actions in the following way:

Behavior	Appropriateness	Effect on natural environment	Difficulty
Leave the newspaper on the park bench	- Very socially inappropriate	- Harmful towards the environment	- Very easy to carry out
Take the newspaper with him/her home and later throw it in a recycling container for used paper near his/her house	- Very socially appropriate	- Very effective in protecting the environment	- Somewhat difficult to carry out
Throw the newspaper in a nearby waste basket	- Somewhat socially appropriate	- Somewhat harmful towards the environment	- Very easy to carry out
Go to a distant recycling container for used paper and throw the newspaper in there	- Very socially appropriate	- Very effective in protecting the environment	- Very difficult to carry out

Monetary bonus

As mentioned before, at the end of the experiment today, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. Thus, we will select both a thematic block and one possible choice at random. For the choice selected, we will determine what ratings of social appropriateness were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of social appropriateness for the randomly selected choice is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

Similarly, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine what ratings of effect on the environment were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of the selected choice's effect on the environment is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

Similarly, we will randomly select one of the thematic blocks. For this thematic block, we will also randomly select one of the possible choices that Individual A could make. For the choice selected, we will determine what ratings of difficulty were selected BY THE MOST PEOPLE HERE TODAY.

If your rating of difficulty of performing the selected choice is the same as the rating most frequently given by participants in today's session, then you will receive an additional 150 CZK.

In sum, you could earn as much as 450 CZK in today's experiment in addition to the 180 CZK participation fee.

For instance, if we were to select the "newspaper" example situation and the possible choice "leave the newspaper on the park bench," and if your response had been "very socially inappropriate," then you would receive 150 CZK, in addition to the 180 CZK participation fee, if this was the response selected by most participants in today's session.

Are there any questions or are you ready to start rating the choices/behaviors?

On the following screens you will read descriptions of different possible choices/behaviors available to individual A. For *each* of the choices/behaviors, please indicate whether you believe choosing that option is:

- very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, very socially appropriate;
- very effective in protecting the environment, effective in protecting the environment, somewhat effective in protecting the environment, somewhat harmful towards the environment, harmful towards the environment, very harmful towards the environment;
- very difficult to carry out, somewhat difficult to carry out, somewhat easy to carry out, very easy to carry out.

To indicate your response, please click on the corresponding radio button with the computer mouse.

There will be 15 blocks of choices/behaviors for you to rate in total. Please note that you will not be able to review your ratings on a previous screen after you proceed to rate items on the next screen.

Block 1: Garbage

[Items being rated:]

- Person A returns dead batteries to collection points for dangerous waste.
- Person A does not return dead batteries to collection points for dangerous waste.
- Person A does not use batteries.
- Person A brings unused medicine back to the pharmacy.
- Person A does not bring unused medicine back to the pharmacy.
- Person A was never left with any unused medicine.
- Person A collects and recycles used paper.
- Person A does not collect and recycle used paper.
- Person A uses a compost bin.
- Person A does not use a compost bin.
- Person A collects and recycles used plastic.
- Person A does not collect and recycle used plastic.
- Person A collects and recycles used metal.
- Person A does not collect and recycle used metal.
- Person A collects and recycles used glass.
- Person A does not collect and recycle used glass.

Block 2: Water and power consumption

[Items being rated:]

- Person A showers rather than takes a bath.
- Person A takes a bath rather a shower.
- Person A waits to have a full load before doing his/her laundry.
- Person A does not wait to have a full load before doing his/her laundry.
- Person A washes dirty clothes without prewashing.
- Person A washes dirty clothes with prewashing.
- The heater for person A's house is shut off late at night.
- The heater for person A's house is not shut off late at night.
- Person A closes the door to the refrigerator quickly when he/she takes something out.
- Person A does not close the door to the refrigerator quickly when he/she takes something out.
- Person A cooks with a lid on the pots.
- Person A does not cook with a lid on the pots.
- Person A does not cook.
- Person A switches off or disconnects electric devices completely when not using them (not using standby).
- Person A does not switch off or disconnect electric devices completely when not using them.
- Person A lowers the room temperature when he/she is not home for longer periods.
- Person A does not lower the room temperature when he/she is not home for longer periods.
- Person A uses energy saving light bulbs wherever possible.
- Person A does not use energy saving light bulbs wherever possible.
- When buying new electronic devices, person A makes sure to buy the ones with the lowest energy use.
- When buying new electronic devices, person A does not make sure to buy the ones with the lowest energy use.
- Person A is maintaining electrical devices so that they are in good shape and do not use more energy than necessary.
- Person A is not maintaining electrical devices so that they are in good shape and do not use more energy than necessary.
- Person A is de-icing the refrigerator regularly.
- Person A is not de-icing the refrigerator regularly.
- Person A shares the refrigerator with other people and someone else is responsible for de-icing it.
- Person A is getting his/her electricity from a provider that guarantees 100% regenerative energy production.
- Person A is not getting his/her electricity from a provider that guarantees 100% regenerative energy production.
- Person A can not influence at all from which provider to get electricity.
- Person A does not use a tumble dryer to dry clothes.
- Person A uses a tumble dryer to dry clothes.
- Person A does not have a tumble dryer.
- Person A does not use air conditioning during summer.
- Person A uses air conditioning during summer.
- Person A does not have air conditioning in his/her apartment.

- Person A does not set the air conditioning temperature in the car below 24°C even on hot days.
- Person A sets the air conditioning temperature in the car below 24°C on hot days.
- Person A does not have a car.
- Person A does not have a car with air conditioning.

Block 3: Consumer behavior

[Items being rated:]

- Person A does not use fabric softener with his/her laundry.
- Person A uses fabric softener with his/her laundry.
- If there are insects in Person A's apartment, he/she does not use chemical insecticides to kill them.
- If there are insects in Person A's apartment, he/she uses chemical insecticides to kill them.
- Person A does not use chemical toilet cleaners.
- Person A uses chemical toilet cleaners.
- Person A uses an all-purpose cleaner to clean the bathroom instead of cleaners made especially for the bathroom.
- To clean the bathroom Person A uses cleaners made especially for the bathroom instead of an all-purpose cleaner.
- Person A uses phosphate-free laundry detergent.
- Person A uses laundry detergent with phosphate.
- Person A buys organic vegetables.
- Person A does not buy organic vegetables.
- Person A buys organic dairy products.
- Person A does not buy organic dairy products.
- Person A does not eat dairy products.
- Person A buys organic meat products.
- Person A does not buy organic meat products.
- Person A does not eat meat.
- Person A eats meat.
- Person A prefers to buy local food products.
- Person A does not prefer to buy local food products.
- Person A prefers to buy things for the daily need at the nearest store rather than driving to a store where things are probably cheaper.
- Person A does not prefer to buy things for the daily need at the nearest store rather than driving to a store where things are probably cheaper.

Block 4: Garbage inhibition

[Items being rated:]

- Person A does not buy beverages in cans.
- Person A buys beverages in cans.
- In supermarkets, person A usually buys fruits and vegetables from the open bins (not prepacked).
- In supermarkets, person A usually buys prepacked fruits and vegetables (not from open bins).
- Person A reuses his/her shopping bags.
- Person A does not reuse his/her shopping bags.
- Person A brings his/her own coffee cup to school/work (instead of using cups from the coffee machine).
- Person A does not bring his/her own coffee cup to school/work (instead of using cups from the coffee machine).
- Person A does not drink coffee at school/work.
- Person A orders take-out from restaurants.
- Person A does not order take-out from restaurants.
- Person A uses rechargeable batteries.
- Person A does not use rechargeable batteries.
- Person A usually buys beverages in reusable bottles.
- Person A usually does not buy beverages in reusable bottles.

Block 5: Automobile use

[Items being rated:]

- When possible in nearby areas (around 30 km), person A uses public transportation or rides a bike.
- Even when possible in nearby areas (around 30 km), person A does not use public transportation or ride a bike.
- Person A walks, rides a bicycle, or takes public transportation to work or school.
- Person A does not walk, ride a bicycle, or take public transportation to work or school.
- Person A drives a car with low fuel consumption.
- Person A does not drive a car with low fuel consumption.
- Person A does not drive.
- Person A drives an alternative fuel car (e.g., an electric vehicle).
- Person A does not drive an alternative fuel car (e.g., an electric vehicle).
- Person A is member of a car sharing organization.
- Person A is not member of a car sharing organization.
- Person A decided not to buy a car, even though he/she can afford it.
- Person A decided to buy a car.
- Person A could not afford to buy a car.
- Person A drives his/her car defensively to safe fuel.
- Person A does not drive his/her car defensively to safe fuel.

Block 6: Volunteering in nature-protection activities/political support

[Items being rated:]

- Person A often talks with friends about problems related to the environment.
- Person A usually does not talk with friends about problems related to the environment.
- Person A is a member of an environmental organization.
- Person A is not a member of an environmental organization.
- Person A is pointing out their unecological behavior to other people.
- Person A is not pointing out their unecological behavior to other people.
- Person A sometimes contributes financially to environmental organizations.
- Person A does not contribute financially to environmental organizations.
- Person A supports higher taxes for environmentally damaging behavior.
- Person A does not support higher taxes for environmentally damaging behavior.
- Person A supports financial incentives for environmentally beneficial technology.
- Person A does not support financial incentives for environmentally beneficial technology.
- Person A supports strong measures against climate change.
- Person A does not support strong measures against climate change.
- Person A votes for parties that have environmental topics high on their agenda.
- Person A does not vote for parties that have environmental topics high on their agenda.

[Subsequently, participants rated choices in nine other blocks (these blocks were shorter, including mostly just 2 behaviors). Instructions for this part of the study are omitted here, because results from these tasks are not reported in my thesis.]

APPENDIX G.3: VARIABLE "BEHAVIOR" (FROM PART 1 OF THE STUDY)

Table 1: Variable "Behavior": A list of 56 questionnaire items and all 130 alternative responses

Item	Pro-environmental response	Environmentally unfriendly response	Other response(s)
Do you return dead batteries to collection points for dangerous waste?	Yes	No	I do not use batteries
Do you bring unused medicine back to the pharmacy?	Yes	No	I was never left with any unused medicine
Do you collect and recycle used paper?	Yes	No	
Do you use a compost bin?	Yes	No	
Do you collect and recycle used plastic?	Yes	No	
Do you collect and recycle used metal?	Yes	No	
Do you collect and recycle used glass?	Yes	No	
Do you rather shower than take a bath?	Yes	No	
Do you wait to have a full load before doing your laundry?	Yes	No	
Do you wash dirty clothes without prewashing?	Yes	No	
Is the heater in your house shut off late at night?	Yes	No	
Do you close the door to the refrigerator quickly when you take something out?	Yes	No	
Do you cook with a lid on the pots?	Yes	No	I do not cook
Do you switch off or disconnect electric devices completely when not using them (not using standby)?	Yes	No	
Do you lower the room temperature when you are not home for longer periods?	Yes	No	
Do you use energy saving light bulbs wherever possible?	Yes	No	
When buying new electronic devices, do you make sure to buy the ones with the lowest energy use?	Yes	No	
Are you maintaining electrical devices so that they are in good shape and do not use more energy than necessary?	Yes	No	
Are you de-icing the refrigerator	Yes	No	I share the refrigerator

regularly?			with other people and someone else is responsible for this	
Are you getting your electricity from a provider that guarantees 100% regenerative energy production?	Yes	No	I can not influence at all from which provider to get electricity	
Do you use a tumble dryer to dry clothes?	No	Yes	I do not have a tumble dryer	
Do you use air conditioning during summer?	No	Yes	I do not have air conditioning in my apartment	I do not have a car with air conditioning
Do you set the air conditioning temperature in the car below 24°C on hot days?	Yes	No	I do not have a car	
Do you use fabric softener with your laundry?	No	Yes		
If there are insects in your apartment, do you use chemical insecticides to kill them?	No	Yes		
Do you use chemical toilet cleaners?	No	Yes		
Do you use an all-purpose cleaner to clean the bathroom instead of cleaners made especially for the bathroom?	Yes	No		
Do you use phosphate-free laundry detergent?	Yes	No		
Do you buy organic vegetables?	Yes	No		
Do you buy organic dairy products?	Yes	No	I do not eat dairy products	
Do you buy organic meat products?	Yes	No	I do not eat meat products	
Do you eat meat?	No	Yes		
Do you prefer to buy local food products?	Yes	No		
Do you prefer to buy things for the daily need at the nearest store rather than driving to a store where things are probably cheaper?	Yes	No		
Do you buy beverages in cans?	No	Yes		
In supermarkets, do you usually buy fruits and vegetables from the open bins (not prepacked)?	Yes	No		

Do you reuse your shopping bags?	Yes	No		
Do you bring your own coffee cup to school/work (instead of using cups from the coffee machine)?	Yes	No	I do not drink coffee at school/work	
Do you order take-out from restaurants?	No	Yes		
Do you use rechargeable batteries?	Yes	No		
Do you usually buy beverages in reusable bottles?	Yes	No		
When possible in nearby areas (around 30 km), do you use public transportation or ride a bike?	Yes	No		
Do you walk, ride a bicycle, or take public transportation to work or school?	Yes	No		
Do you drive a car with low fuel consumption?	Yes	No	I do not drive	
Do you drive an alternative fuel car (e.g., an electric vehicle)?	Yes	No	I do not drive	
Are you a member of a car sharing organization?	Yes	No	I do not drive	I could not afford to buy a car
Have you decided not to buy a car, even though you could afford it?	Yes	No	I do not drive	
Do you drive your car defensively to safe fuel?	Yes	No	I do not drive	
Do you often talk with friends about problems related to the environment?	Yes	No		
Are you a member of an environmental organization?	Yes	No		
Are you pointing out their unecological behavior to other people?	Yes	No		
Do you sometimes contribute financially to environmental organizations?	Yes	No		
Do you support higher taxes for environmentally damaging behavior?	Yes	No		
Do you support financial incentives for environmentally beneficial technology?	Yes	No		
Do you support strong measures against climate change?	Yes	No		
Do you vote for parties that have environmental topics high on their agenda?	Yes	No		