Social Dilemma Behavior

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To Julie and my parents.

Contents

Part I

Preface

Acknowledgments

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- Ulrik Haagen Nielsen

English summary

This thesis consists of five self-contained research contributions that all investigate how humans make decisions when they are confronted with a *social dilemma* in which, when being good to themselves (or someone else), they cannot avoid making others worse off. All work presented here use data from incentivized internet experiments that were conducted with adult participants recruited from the broad population in Denmark.

One chapter, *Give and Take in Dictator Games*, investigates to what extent generous acts are motivated by a desire to be good to others. By manipulating the menu of alternatives in a Dictator Game experiment, my co-authors and I find that maximally half of the people who make generous acts seem to have a genuine desire to be good to others. Our results generalize previous findings from laboratory experiments with university student samples.

Two chapters compare social dilemma choices to the subjects' response times in order to unearth whether it is more intuitive to people to act selfishly or prosocially in such situations. In one chapter, *Second Thoughts on Free Riding*, my co-authors and I find that it is intuitive to most people - possibly due to a social norm - to act cooperatively when others act cooperatively, too, whereas people who free ride on others' cooperative acts need to resolve a time-consuming moral dilemma before they decide to free ride. In another chapter, *Fairness is Intuitive*, my co-authors and I investigate if being fair or selfish is more intuitive in the standard Dictator Game which constitutes a clean and non-strategic social dilemma. We find that those who do what is most fair (i.e. share a money sum 50-50 with another person) are significantly faster than both the selfish people (i.e. share nothing) and those who give a positive share other than 50% which suggests that fairness is intuitive. This finding is robust across gender, age groups, and educational attainment in the broad Danish society.

In the chapter *Parents' Education and their Adult Offspring's Other-Regarding Behav-*

ior, I investigate to what extent adults' generosity is determined by their socioeconomic background as three recent studies have documented that *children's* other-regarding behavior is in fact related to their socioeconomic status. I find highly robust evidence, however, that there is no link. Since only 20% of the variation in adults' generosity is genetically inherited according to a Swedish twin study, there is thus a latent potential for future research to identify how the remaining 80% of the variation is shaped.

In the last chapter, *Does Shared Responsibility Breed Unfairness?*, my co-authors and I test if policy makers who are organized in committees are more likely to implement economically efficient, but at the same time distributionally unfair and thus unpopular policies than individual decision makers are? Whereas committee members in our setting collectively share the responsibility for the committee's decision, an individual decision-maker is left alone with that responsibility. We find that shared responsibility does in fact make committees more likely than single decision-makers to implement such necessary but unpopular policies, but only when the unfairness is *very* pronounced. We provide explanations for this. Our finding suggests that when multiple decision-makers (e.g. company directors) have to make an efficient, but highly unpopular decision (e.g. cut staff to improve profitability) they should organize themselves in a committee and collectively share the responsibility for their decision.

Dansk resumé

Denne afhandling indeholder fem selvstændige kapitler, der alle undersøger, hvordan mennesker træffer beslutninger, når de er konfronteret med et *socialt dilemma* i hvilket de, når de tilgodeser dem selv eller en anden, ikke kan undgå at stille andre dårligere. Alt arbejde præsenteret i afhandlingen anvender data fra incitamentsbaserede interneteksperimenter, der blev afholdt med voksne deltagere rekrutteret fra den danske population.

Et kapitel, *Give and Take in Dictator Games*, undersøger i hvilket omfang, generøse handlinger er motiveret af et ønske om at være god mod andre. Ved at manipulere deltagernes valgmuligheder i et "Dictator Game"-eksperiment har mine medforfattere og jeg fundet, at maksimalt halvdelen af de, som foretager generøse handlinger, i bund og grund gerne vil tilgodese andre. Vores resultater generaliserer tidligere observationer, der har været gjort i laboratorieeksperimenter med universitetsstuderende som deltagere.

To kapitler sammenholder beslutninger truffet i sociale dilemmaer med deltagernes responstider for at kunne afsløre, om det er intuitivt for folk at handle selvisk eller prosocialt i sådanne situationer. I det ene kapitel, *Second Thoughts on Free Riding*, finder mine medforfattere og jeg, at det er intuitivt for de fleste - måske på grund af en social norm - at handle kooperativt, når andre også handler kooperativt, mens mennesker, der nasser på andres kooperative handlinger, er nødt til at løse et tidskrævende moralsk dilemma, før de beslutter at nasse. I et andet kapitel, *Fairness is Intuitive*, ser mine medforfattere og jeg på, om det er mere intuitivt at være fair eller selvisk i et standardiseret "Dictator Game", som udgør et rent og ustrategisk socialt dilemma. Vi finder, at de, der gør, hvad der er mest fair (deler en pengesum 50-50 med en anden person), er signifikant hurtigere end de selviske personer (deler ingenting) og de, der giver en positiv andel forskellig fra 50%, hvilket indikerer, at det er intuitivt at handle retfærdigt. Dette resultat er robust på tværs af køn, aldersgrupper og uddannelseslængder i det danske samfund.

Jeg undersøger dernæst i kapitlet *Parents' Education and their Adult Offspring's*

Other-Regarding Behavior, i hvilket omfang voksnes generøsitet er bestemt af deres socioøkonomiske baggrund, da tre nylige studier har dokumenteret, at *børns* generøsitet er korreleret med deres socioøkonomiske status. Jeg finder dog særdeles robust evidens for, at der ikke er en sammenhæng. Da blot 20% af variationen i voksnes generøsitet er genetisk betinget ifølge et svensk tvillingestudie, er der dermed et latent potentiale for fremtidig forskning i at identificere, hvordan de resterende 80% af variationen formes.

I det afsluttende kapitel, *Does Shared Responsibility Breed Unfairness?*, tester mine medforfattere og jeg, om beslutningstagere, der er organiseret i udvalg, er mere tilbøjelige til at vedtage økonomisk efficiente, men samtidig fordelingsmæssigt uretfærdige og dermed upopulære beslutninger end enlige beslutningstagere er? Hvorimod udvalgsmedlemmer i vores scenarium kollektivt deler ansvaret for udvalgets beslutning, står en enlige beslutningstager alene med dette ansvar. Vi finder, at ansvarsdeling rent faktisk gør, at udvalg oftere end enlige beslutningstagere vedtager sådanne nødvendige men upopulære beslutninger, men kun når beslutningerne er *meget* uretfærdige. Vi giver forklaringer på dette. Vores resultater antyder, at når beslutningstagere (f.eks. virksomhedsledere) er tvunget til at træffe en efficient, men vældigt upopulær beslutning (f.eks. reducere medarbejderstaben), bør de organisere sig i et udvalg og kollektivt dele ansvaret for deres beslutning.

Part II

Thesis

Chapter 1

Introduction

People frequently engage in social interactions. This happens whenever they meet, communicate, or act with other people such as their friends, family members, co-workers, or pure strangers on the street, at work, on the internet etc. Once in a while, these interactions involve a social dilemma[1] in which one is confronted with a situation where, when being good to him- or herself (or someone else), it is unavoidable to be unfair towards others. To give a simple - and very hypothetical - example of such a situation, think of the dishwashing machine at your workplace and imagine that it needs to be emptied for its cleaned contents. You now have two options. You can either empty the dishwasher yourself which is tedious to you, but kind towards your co-workers, or you can instead wait for one of your co-workers to empty it which is pleasurable to you, but tedious to the one who empties it.

In this thesis, I present five self-contained research contributions in which I (and my co-authors) provide novel, empirical evidence on how people behave in social dilemma situations. I will attempt to answer, among other things, if people are naturally selfish or selfless and to what extent benevolent acts are motivated by a desire to be good to others. To do this, I use data from a number of incentivized economic experiments that were conducted on the Internet between 2008 and 2011. The experiments were organized by the Internet Laboratory for Experimental Economics (iLEE) at the University of

[1] According to psychologist Robyn M. Dawes, a social dilemma is a situation in which "(a) each individual receives a higher payoff for a socially defecting choice (...) than for a socially cooperative choice, no matter what the other individuals in society do, but (b) all individuals are better off if all cooperate than if all defect." (Dawes, 1980, p.169.) Whereas Dawes' definition only take into account situations in which other-regarding behavior is socially *efficient*, I will, however, also put situations that do not involve efficiency aspects (i.e. zero-sum interactions) under the umbrella of social dilemmas.

Copenhagen and funded by the Carlsberg Foundation. The experimental subjects were randomly recruited from the broad Danish adult population by Statistics Denmark.

In the remainder of this introduction, I will briefly introduce the idea behind each of the five research contributions as well as their main findings.

Chapter 2 investigates what motivates generous acts. Two previous studies by List (2007) and Bardsley (2008) have both shown that subjects in so-called Dictator Game experiments are less generous towards another anonymous subject if they are also given the opportunity to take money from this person than if they are not. Since changing the choice set should not deter people, who sincerely care about the other person's welfare, from generously giving money to this other person, the findings by List and Bardsley suggest that many generous acts are in fact not motivated by a desire to be good to others. In fact, a lot fewer subjects share positive amounts with the other person when they can also take money from this person. This indicates that, instead of caring about others, many subjects might just want to comply with a social norm about right behavior which is possibly choice-set dependent (e.g. Krupka and Weber, 2013). Another, and not mutually exclusive, explanation is that many people care about not appearing too selfish in the eyes of others. Hence, they make actions that are just slightly more generous than the most selfish alternative (e.g. Andreoni and Bernheim, 2009).

In our study, my co-authors and I investigate if the findings in List and Bardsley are caused by the subjects being confused about the entitlements to the money in the experiment. Also, we test the generalizability of their findings by conducting our experiment with a broad sample of the Danish adult population instead of college students.[2] We find, in line with List and Bardsley, that a significantly smaller proportion of the subjects give money to another subject when they are also allowed to take money from this person (34-36%) than when they are not (71-74%). This result is not due to uncertainty about entitlements and it is robust across the Danish adult population.

Chapters 3 and 4 test if it is more intuitive to people to act selfishly or selflessly by relating their decisions to how long time they spend on making these actions (i.e. their *response time*). Whereas short response times indicate that the decision-maker makes intuitive or impulsive decisions, long response times indicate that he or she carefully deliberates before making a decision. If there are systematic differences between fast

[2]It has typically been found that university students (Falk et al., 2013) and young people in general (Engel, 2011) are less generous than more representative adults.

and slow decisions, such findings may be of interest to broader audiences where they can advice, among others, charitable solicitors and business managers on when it is desirable to force people (e.g. potential contributors and consumers) to make fast and intuitive decisions and when it is desirable to force them to make slow and deliberate decisions.

In Chapter 3, my co-authors and I test a hypothesis from Gächter (2012) that "conditional cooperators are intuitively cooperative and selfish people take a reflected free ride".[3] It has previously been found that cooperation is intuitive to most, whereas free riding requires deliberation (Rand et al., 2012; Lotito et al., 2013). However, in these experiments the subjects were unable to condition their own cooperation on others', which means that it cannot be inferred from observing a selfish act whether the person who made this act is sincerely selfish (i.e. a free rider) or, alternatively, is a conditional cooperator with very pessimistic beliefs about how cooperative others are. We test Gächter's hypothesis in a Public Goods Game experiment, in which the subjects could in fact condition their own cooperation on others', by comparing response times of free riders to those of conditional cooperators. We control for the subjects' cognitive abilities and their general swiftness in order to isolate the relationship between response times and cooperator types. We find highly robust evidence that free riders make much slower decisions than conditional cooperators and conclude that conditional cooperation serves as a norm and that, in order to break this norm, one must resolve a time-consuming moral dilemma.

Chapter 4 investigates if acting selfishly or fairly is more intuitive to people in a Dictator Game experiment. The Dictator Game constitutes a simpler and cleaner social dilemma than the Public Goods Game by asking the subjects how much of a money sum X they would like to share with another person. My co-authors and I find that those who share 50-50 (52% of the subjects) generally make their decision faster than those who share nothing (25% of the subjects) which indicates that being fair is intuitive to most. This finding is robust to controlling for cognitive ability, swiftness, and other individual characteristics. There is, however, no link between response times and generosity among those subjects who give a positive share different from 50% to the other person. This indicates that as soon as people engage in an active and deliberate tradeoff between the alternatives at hand, the fairness of their decision cannot be predicted from how long

[3]Conditional cooperators are individuals who are willing to cooperate if others cooperate, too, whereas selfish people (also known as *free riders*) never cooperate independent of how cooperative others are. Typically, a majority of subjects in experiments are found to be conditional cooperators (e.g. Fischbacher et al., 2001; Kocher et al., 2008; Thöni et al., 2012.

time they spend on making it. We also provide evidence that fairness is intuitive across genders, age groups, and educational attainments in the broad Danish society.

In Chapter 5, I investigate what can explain the heterogeneity in how generous adults are. It has previously been documented that generosity is intergenerationally dependent (Wilhelm et al., 2008) and that 20% of the variation in adults' generosity is genetically inherited (Cesarini et al., 2009). It has, however, not been investigated through which mechanisms nurture shapes the remaining 80% of the variation. A recent literature has found that children's generosity is related to their socioeconomic status, but whereas Benenson et al. (2007) and Bauer et al. (2014) found British and Czech children of high socioeconomic status to more generous than others, Chen et al. (2013) found the opposite among Chinese children. In my study, I relate Danish adults' Dictator Game choices to their socioeconomic background which I measure as their parents' educational attainments. Educational attainment is known to positively correlate with income which is another frequently used measure for socioeconomic status. In addition, parental educational attainment is known to positively correlate with how much time the parents spend on (developing) child care (e.g. Guryan et al., 2008). According to my results, however, Danish adults' generosity is not correlated with their socioeconomic background. This finding is robust across age groups and genders. I provide two explanations for this. First, sociodemographic characteristics seem generally to be poor predictors for generosity. Second, correlations between preferences and socioeconomic backgrounds might very well be country- or sample-specific. I also provide more speculative explanations.

In Chapter 6, my co-authors and I ask if shared responsibility breeds unfairness? When multiple decision-makers have to make a decision that increases economic welfare but is distributionally unfair, are they then more likely to make the unfair decision if they organize themselves in a committee and share the responsibility than a representative who solely takes the blame? We test this in a variation of the Group Dictator Game (Cason and Mui, 1997; Luhan et al., 2009; Franzen and Pointner, 2013a; Duch et al., 2013) in which the recipients could hold the decision-makers responsible for their decisions by punishing them at a cost. We find that shared responsibility does in fact breed unfairness, but only when the unfairness is very pronounced. For fairer policies, there is no difference. We also find that both committees and individual decision-makers become increasingly unwilling to pass policies, the more unfair they are because they overestimate

the recipients' punishments. We find that more than 90% of the recipients are willing to punish at a cost. Their punishment pattern indicates an aversion towards disadvantageous inequality. We conclude that when decision-makers (e.g. politicians or corporate board members) are confronted with a decision problem that has an economically efficient but unpopular solution (e.g. reduce welfare benefits for some groups in society or cut staff), they should organize themselves in a committee and collectively take the blame. That seems to (weakly) increase the chance of implementing such a policy.

The chapters presented in this thesis have been written together with Alexander W. Cappelen (Norwegian School of Economics; Chapters 2 and 4), Raymond Duch (Nuffield College, University of Oxford; Chapter 6), Erik Ø. Sørensen (Norwegian School of Economics; Chapter 2), Bertil Tungodden (Norwegian School of Economics; Chapters 2 and 4), Jean-Robert Tyran (University of Vienna & University of Copenhagen; Chapters 2, 3, 4, and 6), and Erik Wengström (Lund University & University of Copenhagen; Chapters 3 and 4).

Chapter 2

Give and Take in Dictator Games

Abstract

It has been shown that participants in the dictator game are less willing to give money to the other participant when their choice set also includes the option to take money. We examine whether this effect is due to the choice set providing a signal about entitlements in a setting where entitlements initially may be considered unclear. We find that the share of positive transfers depends on the choice set even when there is no uncertainty about entitlements, and that this choice-set effect is robust across a heterogenous group of participants recruited from the general adult population in Denmark. The findings are consistent with dictator giving partly being motivated by a desire to signal that one is not entirely selfish or by a desire to follow a social norm that is choice-set dependent.

Note

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2.1 Introduction

It is of great importance to understand what motivates distributive behavior, and the dictator game has been an important workhorse for research in this field. Recent studies (List, 2007; Bardsley, 2008), however, show that a dictator's choice set matters in a manner that cannot easily be explained by standard distributional preference models (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Cappelen et al., 2007). In particular, it is shown that extending the choice set so that dictators also can take money from the recipient dramatically lowers the fraction of dictators giving away a positive amount. This choice-set effect is hard to reconcile with dictators being exclusively motivated by distributional concerns. In this chapter, we address the question of *why* the choice set matters in this way.

One possible explanation is that the choice set provides a signal about entitlements in a setting where entitlements initially may be considered unclear.[1] A standard dictator game is an artificial situation for most participants, where the dictator may be unsure about the extent to which she can think of the overall endowment, which is "manna from heaven", as her own money. In such a context, one can envision that when the dictator is given a choice set that allows her to take money from the other participant, this may strengthen the dictator's feeling that she is entitled to the overall endowment. To illustrate, a dictator only facing the option of giving money to the other participant may consider her equally entitled to the endowment and, consequently, may end up sharing equally, whereas the same dictator also having the option to take money away from the other participant may perceive this as a signal that she is entitled to the overall endowment and, consequently, may end up taking everything. This entitlement mechanism could potentially explain the role of the choice set observed in List (2007) and Bardsley (2008).

To test this mechanism, we designed a dictator game experiment with four treatments differing along two dimensions: the dictator faced either a give or take choice set and the entitlements were established either through experimental instructions only or by including a production phase. This design allows us to compare the effect of extending the choice set in the standard dictator game and in a dictator game with explicit entitlements created through a production phase, where a premise for the analysis is that the

[1] More generally, recent research has shown that entitlements are crucial in determining dictator giving behavior (Konow, 2000; Cherry et al., 2002; Cappelen et al., 2007; Almås et al., 2010; Cappelen et al., 2013).

production phase removes any uncertainty about entitlements.

We find that the effect of introducing a take option is equally strong in a dictator game with explicit entitlements as it is in the standard dictator game, in which there might be uncertainty about the entitlements. In both cases, the share of dictators transferring a positive amount falls from about 75% in the give treatment to about 35% in the take treatment. This result calls for alternative explanations of why the share of positive transfers drops dramatically when the choice set is extended.

The present experiment also provides a test of the robustness of the results presented in List (2007) and Bardsley (2008). They conducted laboratory experiments using student samples, which raises the question of whether the results would carry over to a more general sample and to an environment without the experimenter's visible presence. The present experiment was conducted via the internet and the participants were recruited from the general adult population in Denmark, thus representing a less intrusive environment than the classical lab setting and including a more heterogeneous group than a standard student sample. We show that the main result in in List (2007) and Bardsley (2008) indeed carries over to this environment, and that the choice-set effect is strong in all subgroups in our sample.

2.2 Design and sample

We implemented four dictator game treatments in a between-subjects design. Two of the treatments, Give and Take, were replications of the Baseline and Take ($5) treatments in List (2007), respectively. Each subject was told that she was matched with another participant and that they had received 300 DKK ($\approx$ 50$) in total. Furthermore, they were told that the dictator was tentatively allocated 200 DKK and that the recipient was tentatively allocated 100 DKK.[2] The two treatments differed only in the dictator's choice set. In Give, the dictator could transfer any amount $\tau^{Give} = \{0, 10, \ldots, 100\}$ from his tentative allocation to the recipient; in Take, the dictator could also take some or all of the recipient's tentative allocation, i.e. the dictator could transfer any amount $\tau^{Take} = \{-100, -90, \ldots, 100\}$.

The two additional treatments, Work-Give and Work-Take, were identical to Give

[2]A translated version of the instructions is available in Appendix A, Section A.3.

and Take in the distribution phase, only differing in including a production phase that preceded the distribution phase. In the production phase each subject had to count orange-colored cells in a 10 × 10 grid and earned 150 DKK if she submitted 12 correct answers within five minutes. The total earnings in the production phase constituted then the endowment to be distributed in the distribution phase. The key feature of Work-Give and Work-Take was that the subjects earned exactly the same amount of money for the same amount of work, which should remove any uncertainty about them having equal entitlements to the total earnings in the distribution phase.[3] In contrast, the participants in the Give and Take could potentially have been uncertain about their entitlements to the overall endowment, since in these treatments the money to be distributed was "manna from heaven".

The experiment was run at the Internet Laboratory for Experimental Economics (iLEE) in June-July 2011 with a heterogeneous subject pool that is close to being representative of the general adult population in Denmark on age, education, and gender.[4] Subjects were recruited in collaboration with Statistics Denmark (SD), which invited randomly selected adults aged 21-84 years from the general population in Denmark to participate by sending them a hard-copy letter. Subjects then logged into the internet platform using a code and the key to this code was only known to SD (and not to the experimenter). The data was later sent to SD and matched with register data on gender, age, and education, and payments were effected through electronic transfer.

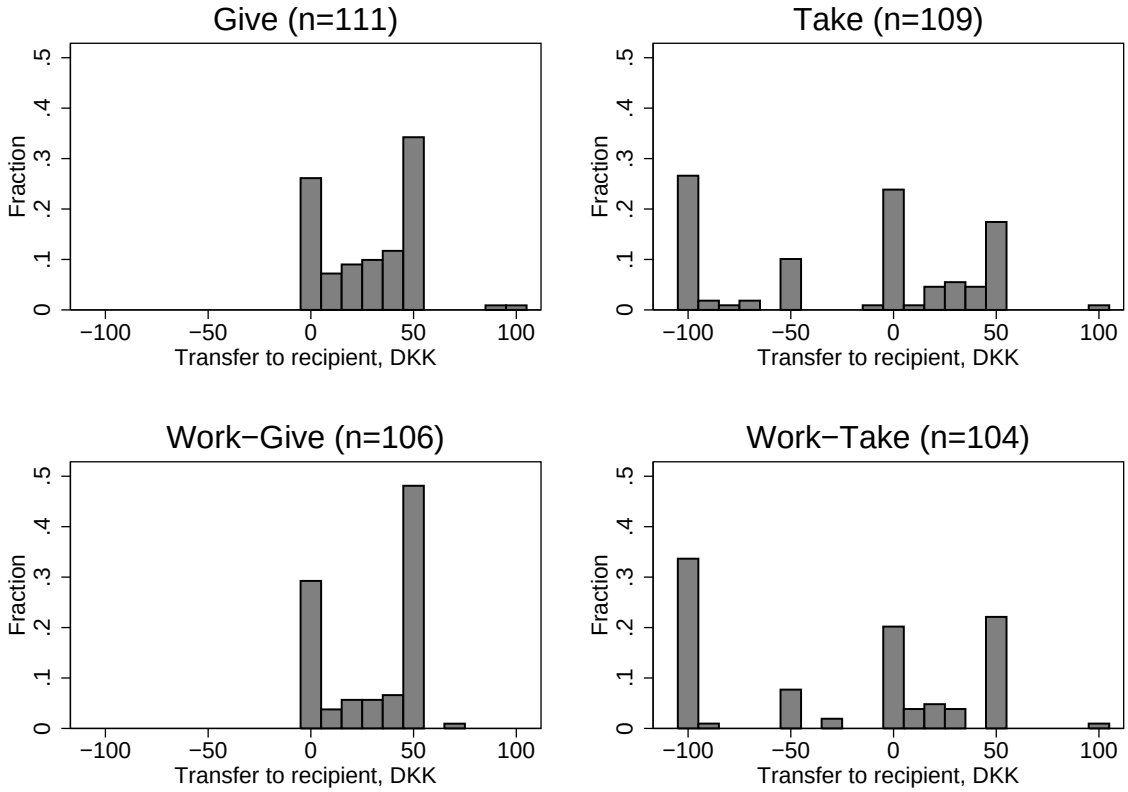
A total of 881 subjects participated in the experiment.[5] Subjects were assigned to one of four treatments as they logged into the experiment, and the role as dictator or recipient as they entered the distribution phase. The experiment was double-blind with subject-subject and subject-experimenter anonymity.

[3] In this respect, these treatments differ fundamentally from the Earnings treatment in List (2007), where the dictator earned twice as much as the recipient for the same job. In such a setting, people may have very different views about individual entitlements to the total earnings (Cappelen et al., 2007).

[4] On average, the participants are 49.3 years old (s.d. 15.2 years) and have 13.6 years of education (s.d. 2.4 years); the share of women is 48.2%. For further details, see Appendix A, Section A.1.

[5] 21 participants assigned to Work-Give and Work-Take did not complete the production task, and, therefore, did not take part in the distribution phase.

Figure 2.1: Distribution of transfers



Note: The panels show, by treatment, the distribution of transfers from the dictator to the recipient in DKK.

2.3 Results

The distribution of transfers from the dictator to the recipient is presented in Figure 2.1, whereas Table 2.1 reports the main aggregate statistics.

The two upper panels in Figure 2.1 provide a strikingly similar picture as reported in List (2007) and Bardsley (2008). The Give treatment replicates standard behavior in a dictator game, whereas the Take treatment shows that when the choice set is extended, there is a dramatic drop in the share of dictators transferring a positive amount to the recipient, from 73.9% to 33.9% ($p < 0.001$, Fisher's exact test).[6] This choice-set effect cannot be reconciled with standard distributional models, which predict that dictators transferring a positive share in the Give treatment would transfer the same amount in the Take treatment. The two lower panels show that exactly the same picture emerges

[6]List (2007) observes an even larger drop in transfers, the difference between the Baseline treatment and the "Take ($5)" treatment is 71 percent versus 10 percent.

Table 2.1: Aggregate summary statistics

Treatment	n	Share of positive transfers	Median transfer	Mean transfer
Give	111	0.739	30	29.0
		(0.042)	(5.5)	(2.1)
Take	109	0.339	0	−21.3
		(0.046)	(8.4)	(5.6)
Work-Give	106	0.708	40	30.6
		(0.044)	(6.8)	(2.2)
Work-Take	104	0.356	0	−24.4
		(0.047)	(13.1)	(6.0)

Note: Median and mean transfers are in DKK. Standard errors in parentheses. Median transfer standard errors are bootstrapped using 1,000 replications.

for the Work-Give and Work-Take treatments, where 70.8% and 35.6% of the dictators ($p < 0.001$, Fisher's exact test), respectively, transfer a positive share to the recipient.[7] Thus, uncertainty about entitlements cannot explain the choice-set effect.

The heterogenous sample of participants allows us to study whether the choice-set effect depends on subject characteristics. Table 2.2 reports regressions of whether a person has transferred a positive share to the recipient on dummies for whether the participant was allocated to a take treatment or a work treatment, background variables, and interaction variables.[8] The regressions confirm that the choice-set effect is statistically significant and independent of the introduction of a production phase. Furthermore, from the regressions including subgroup interaction variables, we observe that the choice-set effect is strong and not statistically significantly different across gender, age, and education.

2.4 Conclusion

The present study documents that the choice-set effect identified in List (2007) and Bardsley (2008) is highly robust. It is substantial also in a setting where any uncertainty

[7] Comparing Give and Work-Give, and Take and Work-Take, we observe that the distribution of transfers is not affected by the introduction of a production phase, which suggests that the dictators even in the Take and Give treatments perceived the two of them to have equal entitlements to the overall endowment.

[8] In Appendix A, Table A.2, we present a corresponding table where the dependent variable is the *amount* transferred by the dictator to the recipient.

Table 2.2: Regression: Positive transfer to recipient

	(1)	(2)	(3)	(4)	(5)
Take	−0.40***	−0.39***	−0.34***	−0.32***	−0.38***
	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)
Work	−0.03	−0.02	−0.02	−0.02	−0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Work × Take	0.05	0.02	0.01	0.01	0.02
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Female × Take			−0.10		
			(0.09)		
Age × Take				−0.13	
				(0.09)	
Education × Take					−0.03
					(0.09)
Female		−0.11*	−0.06	−0.11*	−0.11*
		(0.05)	(0.06)	(0.05)	(0.05)
Age		0.10*	0.11*	0.17**	0.10*
		(0.05)	(0.05)	(0.06)	(0.05)
Education		−0.10*	−0.10*	−0.10*	−0.08
		(0.05)	(0.05)	(0.05)	(0.06)
Constant	0.74***	0.78***	0.75***	0.74***	0.77***
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	430	415	415	415	415
R^2	0.143	0.171	0.173	0.175	0.171

Note: The table reports linear probability regressions where the dependent variable is a dummy that has value 1 if the dictator transferred a strictly positive amount to the recipient. "Take" is a dummy that has value 1 if the dictator was in treatment Take or Work-Take; "Female" is a dummy that has value 1 if the dictator is a female; "Age" is a dummy that has value 1 if the dictator is 50+ years old at 01.01.2011; "Education" is a dummy that has value 1 if the dictator has completed more than 13 years of education which is equivalent to a high school degree. In the regressions in columns (2)-(5), we have excluded 15 dictators for whom we do not have register data. Robust standard errors in parentheses (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

about the entitlements has been removed, and it is equally strong across subgroups in a heterogenous non-student sample. This suggests that the choice-set effect captures a fundamental dimension of individual behavior in the dictator game.

Our results are consistent with giving in the dictator game partly being motivated by a desire to signal that one is not entirely selfish (Andreoni and Bernheim, 2009) or by a desire to follow a social norm that is choice-set dependent (List, 2007). We also cannot rule out the artefact interpretation put forward by Bardsley (2008), in which dictator giving is driven by an experimenter demand effect. We do believe, however, that the environment of the present study is less intrusive than the classical lab setting, which should reduce the influence of the experimenter. But more research is certainly needed to fully understand the underlying mechanisms of the choice-set effect.

Chapter 3

Second Thoughts on Free Riding

Abstract

We use the strategy method to classify subjects into cooperator types in a large-scale online Public Goods Game and find that free riders spend more time on making their decisions than conditional cooperators and other cooperator types. This result is robust to reversing the framing of the game and is not driven by cognitive ability, confusion, or natural swiftness in responding. Our results suggest that conditional cooperation serves as a norm and that free riders need time to resolve a moral dilemma.

Note

This chapter has been written together with Jean-Robert Tyran and Erik Wengström. It has been published in the Economics Letters, volume 122, issue 2, pages 136-139 and is reprinted here with Elsevier's permission.

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3.1 Introduction

Is cooperation and prosocial behavior intuitive to people, or is their first instinct to free ride and behave selfishly? The answer to this question bears the promise of providing insights to the motives behind cooperative and prosocial behavior. As pointed out by Gächter (2012), finding out whether cooperation is instinctive or not is especially important for understanding behavior in novel situations.

One way to address this question is to study response times in economic experiments. Response times are measured as the time it takes to make a decision and are used to indicate whether a choice is made intuitively or after a deliberate thought process. They have been studied in psychology for decades (e.g. Luce, 1986) and have recently received more attention in economics, too (e.g. Wilcox, 1993 and Rubinstein, 2007). Empirical findings on the relationship between response times and prosociality are mixed. Rubinstein (2004, 2007) suggests that response times are relatively short for prosocial acts such as splitting the pie equally in Dictator and Ultimatum Games. However, in Dictator Games without the salient equal split option, Piovesan and Wengström (2009) found that selfish acts were made faster than more prosocial acts.

Concerning cooperation, Rand et al. (2012) argue that cooperation in Public Goods Games is intuitive as subjects who contribute more tend to spend less time on making their decision.[1] In this chapter, we add to this literature by testing the hypothesis raised by Gächter (2012) that "conditional cooperators are intuitively cooperative and selfish people take a reflected free ride". To the best of our knowledge, our chapter is the first study of response times which is able to distinguish between cooperator types such as free riders and conditional cooperators, i.e. subjects who are willing to cooperate provided that others cooperate, too.[2] Previous studies (Rand et al., 2012; Lotito et al., 2013; Tinghög et al., 2013) have focused on Public Goods Games in which subjects could not condition their choices on the choices of others. Using such methods, it is impossible to infer whether someone who does not contribute to a common pool is a free rider or a conditional cooperator with pessimistic beliefs about the others' contributions. To

[1] Lotito et al. (2013) corroborate their findings, whereas Tinghög et al. (2013) contest these results by forcing subjects to decide under time pressure.

[2] Conditional cooperators are found to be the most common type, typically with about half to two thirds of all individuals being classified as such (e.g. Fischbacher et al., 2001, Kocher et al., 2008, and Thöni et al., 2012).

circumvent this problem, we employ the strategy version of the Public Goods Game introduced by Fischbacher et al. (2001).

We use data from an online Public Goods Game conducted with a large and random sample of the Danish adult population. We find clear and significant evidence that free riders spend much more time on making a decision than conditional cooperators and other cooperator types. The median free rider spent more than 400 seconds, whereas the median conditional cooperator spent less than 90 seconds.

Our experimental procedure enables us to match choice data with, among other things, rich individual-level data from the Danish population registers. This technique provides us with a series of novel control variables such as the subjects' general swiftness in responding to surveys, their cognitive ability, and their educational attainment. We use these control variables to address the concern that free riders tend to be slower in making their (non-)cooperative choices, simply because they are generally slower in responding to questions ("swiftness"), which could be due to lacking cognitive ability or being less educated. None of these hypotheses can explain our findings, however. In fact, free riders scored significantly higher on a cognitive reflection test.

We also implement two treatments in which the game is framed in reverse. In the *Give* treatment, subjects choose how much to contribute to a common pool, whereas in the *Take* treatment subjects choose how much to withdraw from that pool. This treatment variation serves as a manipulation check. A given level of cooperation generally requires typing different numbers in the two treatments. For example, a free rider in our experiment must type 0 (i.e. no contribution) in the Give treatment but type 50 (i.e. withdraw everything) in the Take treatment. We use the treatment variation to control for a predisposition towards choosing high or low numbers or the fact that typing a one-digit number is faster than typing a two-digit number.

Our results support the hypothesis that conditional cooperators base their decision on intuition and that free riders base their decision on deliberation. We interpret our results as follows: Conditional cooperation serves a norm and breaking this norm by choosing to free ride leads to a moral dilemma which requires more deliberation to overcome. That is, the seemingly selfish free riders appear to have second thoughts as they are affected by the norm of conditional cooperation and only manage to free ride after a time-consuming justification process.

3.2 Sample and design

The Public Goods Game discussed here was part of the first wave of the Internet Laboratory for Experimental Economics (iLEE1) at the University of Copenhagen in May 2008 (see Thöni et al., 2012 for a description of iLEE1).[3] Statistics Denmark recruited a representative subject pool of the Danish adult population aged 18-80 years for us. In order to maintain subject-subject and subject-experimenter anonymity, Statistics Denmark sent hard-copy invitation letters to the subjects (see Appendix B, Section B.1) who participated remotely. Subjects were paid for their participation via electronic bank transfers.

Our study sample consists of 2,081 subjects who completed the Public Goods Game. These were randomly assigned to the Give and Take treatments in a between-subjects design in proportion 2:1 (1,391 vs. 690). 48.4% of our sample are women and the average participant was 45.6 years old ($\sigma = 14.5$), had 13.4 years of education ($\sigma = 2.5$), and had a gross income in 2008 of 343,864 DKK ($\approx$ 60,000 USD). See Appendix B, Section B.2 for descriptive statistics.

In the Give treatment, subjects were matched in groups of four. Each subject was endowed with 50 DKK. First, all four group members decided how much, if anything, they wanted to contribute to a common pool without knowing how much the others had contributed. Subjects could contribute any integer amount between 0 and 50 DKK. We used both written instructions and illustrations to explain that contributions to the common pool would be doubled and split evenly between the four group members independent of their individual contributions. Hence, a marginal contribution of 1 DKK was worth 2 DKK to the group, but only 0.5 DKK to the contributor. Subjects received no feedback before proceeding to the second stage in which all four group members indicated how much they wished to contribute *conditional* on how much the other group members had contributed on average. Specifically, subjects indicated their contribution given that the others had contributed 0 DKK, 5 DKK,..., 50 DKK on average. Hence, each subject submitted 11 conditional choices. We paid subjects by *ex-post* picking the unconditional choices of three group members at random and the corresponding conditional choice of the fourth group member.

The Take treatment was identical to Give, except that the endowments were now

[3]The choice data from this game has also been studied in Thöni et al. (2012) and Fosgaard et al. (2013).

initially placed in the common pool from which the subjects could withdraw up to 50 DKK each. See Appendix B, Section B.3 for details.

Response times were measured in seconds from opening the screen with conditional decision-making until submitting the 11 conditional choices and they ranged from 9 seconds to 4.7 days since no time restrictions applied. They were not significantly different across the two treatments ($p = 0.571$, Mann-Whitney test). The median response time was 102 seconds in the Give treatment and 105 seconds in the Take treatment.

3.3 Results

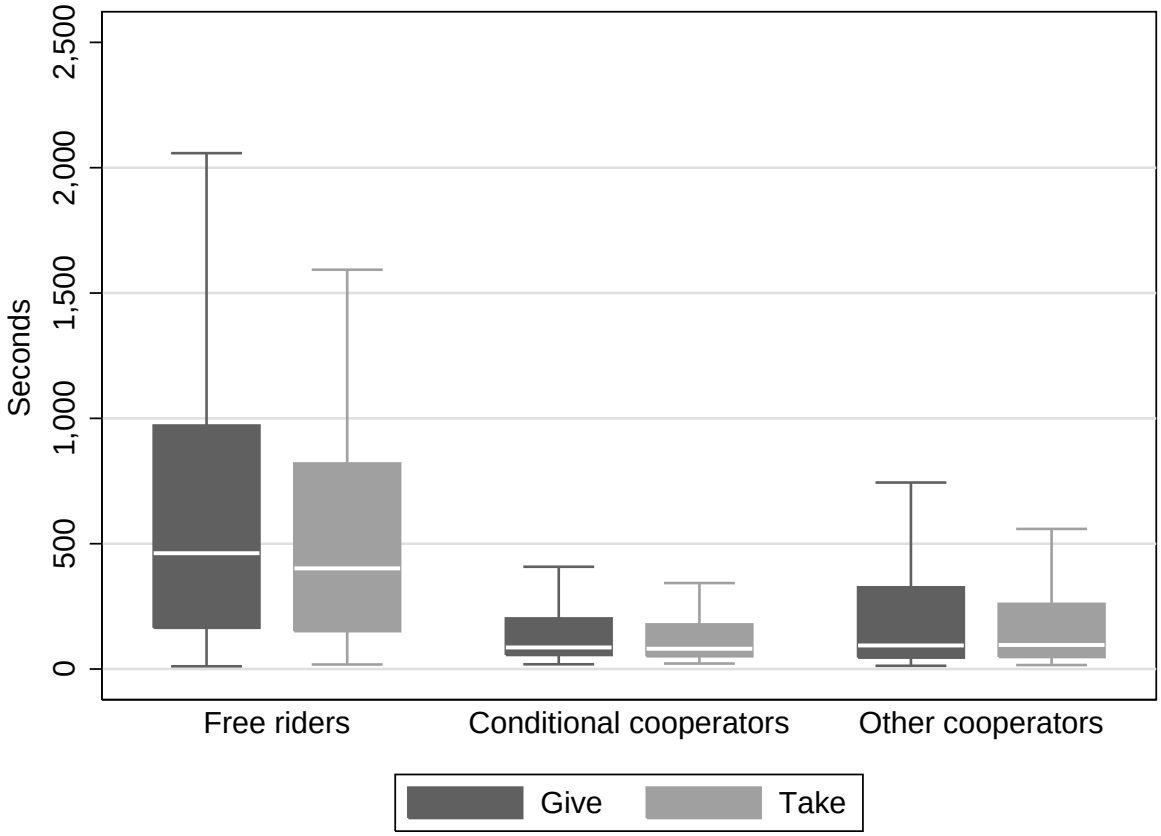
We identify three cooperator types for our analysis. "Free riders" are those subjects who contribute nothing to the common pool independent of the others' contributions. "Conditional cooperators" are those who weakly increase their own contribution in response to an increase by the others with at least one strict increase. "Other cooperators" is the residual category, which consists of subjects who make a positive contribution in at least one of the 11 instances. This category includes, for instance, unconditional cooperators who contribute the same positive amount independent of the others' contribution choices. A majority of the participants were conditional cooperators. In the Give treatment, 68% were conditional cooperators (Take: 55%), 15% were free riders (Take: 21%), and 17% were other cooperators (Take: 24%).

Figure 3.1 shows that free riders were much slower than the cooperators. The median response time in the Give treatment was 459 seconds (Take: 410 seconds) for the free riders, 87 seconds (Take: 81 seconds) for the conditional cooperators, and 89 seconds (Take: 94 seconds) for the other cooperators. Hence, the median free rider was about *five times slower* than the median conditional cooperator in either treatment. Also, the 25th percentile of the Free Riders' response times was 180 seconds in the Give treatment (Take: 167 seconds) which was equivalent to the 72nd percentile of the Conditional Cooperators' response times (Take: 73rd percentile) and the 64th percentile of the Other Cooperators' (Take: 68th percentile).[4] Interestingly, response times varied more for free riders than for cooperators. Nonparametric tests show that free riders were significantly slower than both the conditional cooperators and the other cooperators. However, the response times

[4]See also plots of the cumulative distributions of response times across cooperator types and treatments in Appendix B, Figure B.4.

of the conditional cooperators and other cooperators were not significantly different.[5]

Figure 3.1: Boxplots of response times across cooperator types



Notes: The white line inside a box shows the median, the top and bottom of a box show the 25th and 75th percentile, respectively. The whiskers show the extreme observations within the 1.5 interquartile range.

Table 3.1 shows that these results are robust to controlling for other factors in a series of Tobit regressions of response times on cooperator types. We have set conditional cooperators as the baseline category and top-coded response times at 600 seconds to prevent our results from being driven by extreme outliers. We find that free riders were more than four minutes slower on average than the conditional cooperators (see columns 1 and 4, first line). This difference in speed is highly significant and robust to other estimation procedures (see Appendix B, Section B.4). The other columns of Table 3.1 serve to demonstrate that these pronounced differences in response times are not explained by differences in general swiftness, reading pace, cognitive ability, or other

[5]Mann-Whitney tests: Free riders *versus* conditional cooperators: $p < 0.001$ (Give), $p < 0.001$ (Take). Free riders *versus* other cooperators: $p < 0.001$ (Give), $p < 0.001$ (Take). Conditional cooperators *versus* other cooperators: $p = 0.964$ (Give), $p = 0.126$ (Take).

observable characteristics such as age, gender and education.

Table 3.1: Regressions of response times in seconds on cooperator types

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	268.5***	253.6***	241.6***	259.0***	250.8***	235.7***
	(16.9)	(17.0)	(16.6)	(20.4)	(20.1)	(19.9)
Other cooperator	47.4**	43.9**	40.9**	34.0	41.7*	50.5**
	(15.9)	(15.7)	(15.3)	(18.9)	(19.1)	(18.7)
Swiftness, fast[a]		-54.4**	-14.1		-74.3**	-42.9
		(18.9)	(19.1)		(24.3)	(24.5)
Swiftness, medium[b]		-50.7**	-29.5		-36.9	-22.3
		(16.1)	(15.8)		(21.1)	(20.7)
Reading, fast[c]			-134.5***			-112.9***
			(15.9)			(21.3)
Reading, medium[d]			-76.4***			-53.7**
			(14.5)			(19.6)
Age		1.28**	0.91		-0.046	-0.35
		(0.49)	(0.48)		(0.63)	(0.61)
Female		43.7***	41.8***		64.3***	59.3***
		(11.7)	(11.4)		(15.7)	(15.3)
Education		2.08	1.81		-0.22	-0.87
		(2.48)	(2.42)		(3.10)	(3.03)
Cognitive reflection test		26.9***	21.0***		16.1*	12.3
		(5.75)	(5.64)		(7.74)	(7.59)
Progressive matrices test		-5.10*	-4.21*		-1.85	-0.78
		(2.07)	(2.01)		(2.73)	(2.68)
Constant	173.4***	114.9**	191.8***	151.5***	159.0**	223.8***
	(6.99)	(44.5)	(44.8)	(10.4)	(56.6)	(57.0)
Observations	1391	1361	1361	690	669	669

Notes: Tobit regression with response times top-coded at 600 seconds as the dependent variable. Standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Swiftness is a dummy variable that measures whether the subject spent 0-30 seconds ("fast", 35% of the subjects), 31-60 seconds ("medium", 45%) or more than 60 seconds ("slow", 20%) on answering three simple survey questions about his or her own age, gender, and education.[6] The construction of the dummy variable *Reading* is similar. The

[6]The purpose of the questions was to validate the subjects' identities as we cannot be certain that an invited subject about whom we have access to register data also was the person who actually made choices in the experiment (and not, say, the teenage son of an invited subject). We have discarded 41 subjects from our original sample on this account. Note that the invited subjects were not informed that we know their characteristics.

variable indicates whether a subject spent 0-2 min ("fast", 33% of the subjects), 2-4 min ("medium", 41%), or more than 4 min ("slow", 26%) on reading the instructions for the game. The variable may capture both a person's ability/willingness to deliberate and his or her natural swiftness.

We also use two measures of cognitive ability as control variables: A three-item *Cognitive reflection test* which measures the ability to perform system 2-thinking (Frederick, 2005) and a 20-item *Progressive matrices test* which measures the ability to think logically in novel situations (Beauducel et al., 2010). On average, the subjects submitted 1.46 correct answers ($\sigma = 1.10$) in the cognitive reflection test and 8.64 correct answers ($\sigma = 3.24$) in the progressive matrices test.

We observe that women were generally slower than men and that subjects who spent relatively little time on reading the instructions also spent less time on submitting their contribution choices.

Table 3.2 shows that the only observed characteristics that robustly correlate with being a free rider are the time spent on reading the instructions and the score on the cognitive reflection test. Those who read the instructions more slowly (more carefully we presume) and those with a tendency to reflect more cognitively are more likely to be free riders.[7] In contrast, natural swiftness and education are uncorrelated with free riding.

Earlier studies have pointed out that subjects in Public Goods Game experiments are often confused about the incentive structure of the game (e.g. Andreoni, 1995, Houser and Kurzban, 2002, and Bayer et al., 2013). Ferraro and Vossler (2010) argue that many subjects erroneously believe conditional cooperation to be payoff maximizing. If this were true, confused conditional cooperators may experience less of a trade-off between maximizing own payoff and the payoff of the group. Observed differences in response times between conditional cooperators and free riders could thus be driven by a link between conditional cooperation and confusion rather than by a link between free riding and struggling to break a social norm.

To asses this concern, we asked six questions about the incentive structure of the

[7]The median free rider spent 180 seconds on reading the instructions in the Give treatment (Take: 211 seconds), the median conditional cooperator spent 147 seconds (Take: 152 seconds), and the median other cooperator spent 161 seconds (Take: 129 seconds). Mann-Whitney tests: Free riders *versus* conditional cooperators: $p < 0.001$ (Give), $p < 0.001$ (Take). Free riders *versus* other cooperators: $p = 0.015$ (Give), $p < 0.001$ (Take). Conditional cooperators *versus* other cooperators: $p = 0.216$ (Give), $p = 0.007$ (Take).

Table 3.2: Regressions of being a free rider on individual characteristics

	Give		Take	
	(1)	(2)	(3)	(4)
Swiftness, fast[a]	-0.020	0.002	-0.088*	-0.046
	(0.029)	(0.031)	(0.043)	(0.046)
Swiftness, medium[b]	-0.009	0.001	-0.069	-0.044
	(0.025)	(0.025)	(0.039)	(0.040)
Reading, fast[c]		-0.070**		-0.154***
		(0.022)		(0.033)
Reading, medium[d]		-0.024		-0.154***
		(0.021)		(0.032)
Age	0.001	0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.030	-0.031	-0.006	-0.015
	(0.019)	(0.018)	(0.032)	(0.031)
Education	0.010*	0.009*	0.004	0.003
	(0.004)	(0.004)	(0.006)	(0.006)
Cognitive reflection test	0.052***	0.048***	0.047**	0.039*
	(0.009)	(0.009)	(0.016)	(0.015)
Progressive matrices test	0.004	0.005	0.009	0.012*
	(0.003)	(0.003)	(0.006)	(0.006)
Observations	1361	1361	669	669

Notes: Logit regression. Marginal effects at means are reported with standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

game immediately after the Public Goods Game. Subjects were paid for providing correct answers to questions like: "A person who only cares about own income and expects the others to contribute 25 DKK on average would contribute __ DKK". In line with the literature, we find that only about half of the subjects answered all questions correctly. However, only 14% of the free riders in either treatment were confused. Importantly, our regressions in Table 3.1 (and to a lesser degree in Table 3.2) are robust to discarding the confused subjects (see Appendix B, Tables B.2 and B.7). We conclude that differences in confusion across cooperator types cannot account for our main finding.

In summary, we find that free riders are slow making their choices and conclude that this indicates that they have "second thoughts", i.e. need time to deliberate about whether they ought to break a social norm of (conditional) cooperation. We find no

evidence that free riders are generally slow in responding, less intelligent or less educated. Instead, they tend to be more cognitively able, read the instructions carefully, and tend to be less confused than cooperators.

Chapter 4

Fairness is Intuitive

Abstract

In this chapter we provide new evidence showing that fair behavior is intuitive to most people. We find a strong association between a short response time and fair behavior in the dictator game. This association is robust to controls that take account of the fact that response time might be affected by the decision-maker's cognitive ability and swiftness. The experiment was conducted with a large and heterogeneous sample recruited from the general population in Denmark. We find a striking similarity in the association between response time and fair behavior across groups in the society, which suggests that the predisposition to act fairly is a general human trait.

Note

This chapter has been written together with Alexander W. Cappelen, Bertil Tungodden, Jean-Robert Tyran, and Erik Wengström.

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4.1 Introduction

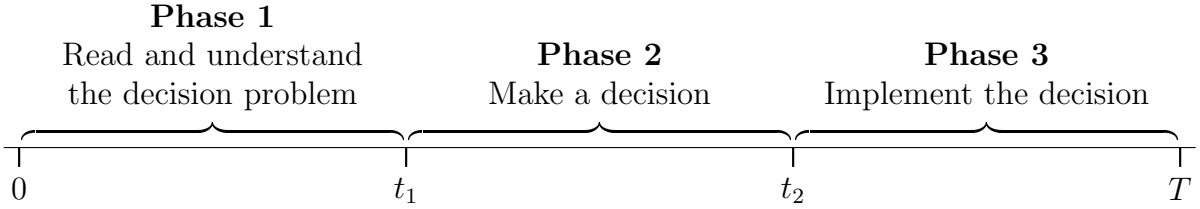
A key question in the social sciences is whether it is intuitive to behave in a fair manner or whether fair behavior requires active self-control. One way to approach this question is to study how long it takes a person to make a decision when choosing between alternatives that are more or less fair. Since a decision that relies on intuition is typically made faster than a decision that relies on deliberation, the response time of a fair decision relative to a selfish decision provides an important indication of the intuitiveness of fair behavior; if fair behavior is intuitive, we would expect a fair decision to be made faster than a selfish decision.

Recently, several experimental studies have used data on subjects' response time in economic games to argue that fair behavior is intuitive (Rubinstein, 2004, 2007; Rand et al., 2012; Lotito et al., 2013; Nielsen et al., 2014). In a series of public goods games, Rand et al. (2012) and Lotito et al. (2013) find that the contribution to the public good is decreasing in the participant's response time. A similar association has been documented in the ultimatum game where the response time of the proposer is negatively correlated with the share offered to the responder (Brañas-Garza et al., 2012). In line with these results, studies that exogenously manipulate the participant's response time show that people tend to contribute more to the public good under time pressure and less when they are forced to delay making their decision (Cappelletti et al., 2011; Grimm and Mengel, 2011; Rand et al., 2012). The negative association between response time and fair behavior in these experiments has been interpreted as showing that fair behavior is intuitive. A few studies have, however, challenged these findings. Tinghög et al. (2013) do not find that time pressure increases public good contributions and Piovesan and Wengström (2009) find that faster subjects more often than slower subjects make egoistic choices in distributive situations.

A key problem with the previous studies on response time and fairness, which could explain the conflicting results in the literature, is the fact that the overall response time in such experiments does not only depend on whether the decision is made intuitively. As illustrated in Figure 4.1, people can be seen as going through three phases when making a decision in an economic experiment. First, they have to read and understand the decision problem, then they have to make their decision (t_2), and, finally, they have to implement this decision on the computer screen (T). The response time T will thus

not only depend on whether the decision itself is based on intuition or deliberation, but also on the subject's cognitive ability and swiftness in implementing their decision. This introduces an important potential confound when a short response time is interpreted as indicating intuitive decision-making, since the short response time could also reflect that the participant easily grasps the decision problem (t_1) or is fast in implementing the decision ($T - t_2$). Hence, a negative association between the participant's response time (T) and the fairness of his or her behavior does not necessarily reflect that there is a negative association between decision time (t_1-t_2) and fair behavior; it might only reflect that there is a negative association between cognitive ability, swiftness, and the weight attached to fairness.

Figure 4.1: The components of response time



Note: The figure illustrates the three phases constituting a participant's response time.

In the present chapter, we employ an experimental design with two features that allow us to more clearly identify the association between decision time and fair behavior. The first feature is that we focus on the dictator game.[1] The advantage of the standard dictator game is that it requires little cognitive effort to understand the game. In particular, it is easy to identify the most selfish alternative as well as the most fair alternative. Thus, the time it takes to understand the decision task (t_1) is minimized, which reduces the potential confound created by heterogeneity in cognitive ability. In contrast, the instructions for a public good game are clearly more demanding and it is also non-trivial to identify the selfish and the fair alternative in this game. In the ultimatum game, most people easily identify the fair alternative as a 50-50 split, but it is inherently difficult to identify the selfish alternative since it depends on the participant's belief about how the other participant will respond. The second crucial feature of our design is that we collect independent measures of each participant's swiftness and cognitive ability. This enables

[1]Two previous studies of response time and fair behavior have employed the dictator game, but these conducted either a non-incentivized experiment (Rubinstein, 2004) or a non-standard dictator game with a fairly complex decision problem (Piovesan and Wengström, 2009).

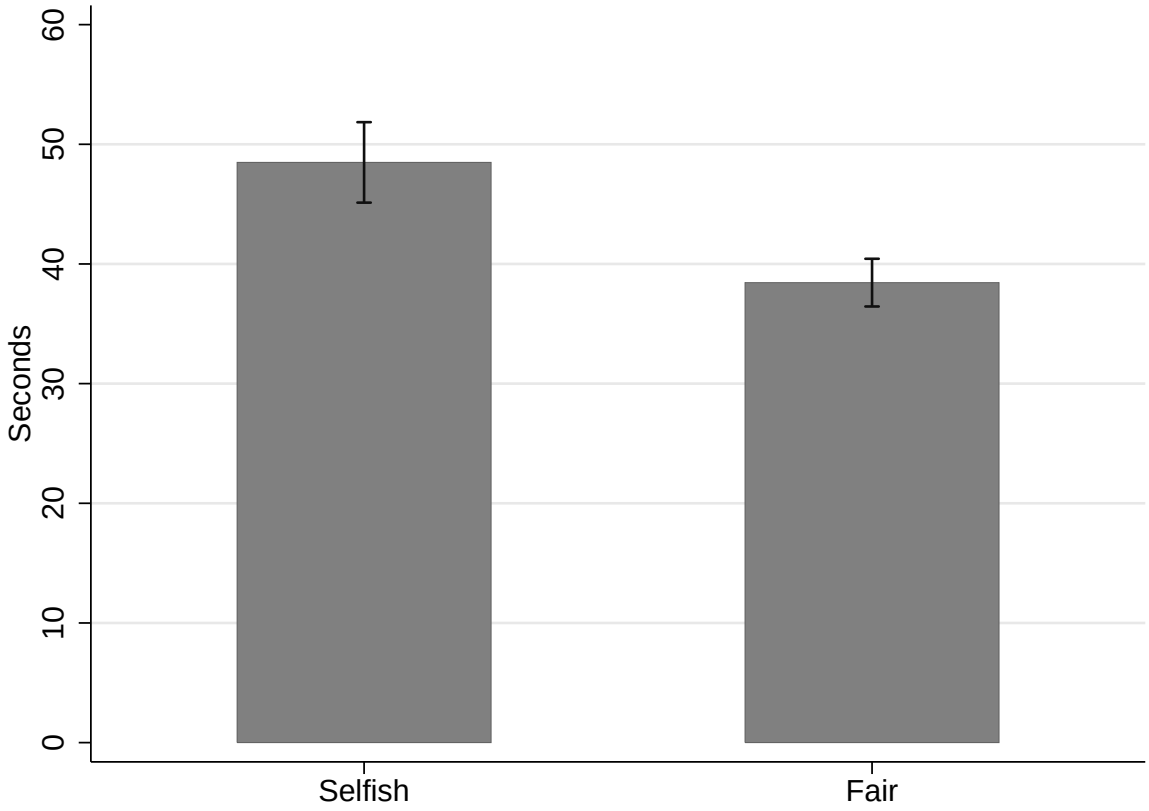
us to control for any remaining confound created by heterogeneity in subject's swiftness and cognitive ability.

Our experiment was carried out with a large and heterogenous sample of the Danish adult population recruited with the assistance of Statistics Denmark. This means that the participants in this experiment are much more diverse than a typical sample of college undergraduates. The collaboration with Statistics Denmark also allows us to match experimental data with data from the Danish population registers. This enables us to study whether there are systematic differences in the population with respect to what they find intuitive when making a distributional choice.

Our first main result, reported in Figure 4.2, is that there is indeed a close association between fair behavior and response time. The average response time among the selfish participants (i.e. those who shared nothing with the other participant) was 48.5 seconds, whereas it was only 38.4 seconds among the fair (i.e. those who split 50-50). We find considerable heterogeneity in both swiftness and cognitive ability among the participants in the experiment. In fact, we find that the observed variance in swiftness is as large as the observed variance in response time, and the differences in cognitive ability are also striking. The association between response time and fair behavior is, however, robust to controlling for these and other factors that could affect the subject's response time. We thus provide clean evidence of fairness being intuitive. Our second main result is that the association between fair behavior and short response time holds across groups in society when differentiating by age, gender, and length of education. Taken together, our two main results provide compelling evidence suggesting that the predisposition to act fairly is a general human trait.

The structure of the chapter is as follows: Section 4.2 presents the experimental design and the sample. Section 4.3 reports the results, while Section 4.4 provides some concluding remarks.

Figure 4.2: Average response time of the selfish and the fair



Note: The figure reports the average response time in seconds (top-coded at 120 seconds) for participants who shared nothing (the selfish, 25% of the 1,508 participants) or shared equally (the fair, 52% of the 1,508 participants) with the other participant. Standard errors are indicated.

4.2 The experiment

We here provide an overview of the sample and the experimental design.

4.2.1 The sample and administrative procedures

The experiment was conducted using the Internet Laboratory for Experimental Economics (iLEE) at the University of Copenhagen, which provides an online platform for running large-scale experiments. The participants were recruited from the general Danish adult population and were randomly selected for invitation by Statistics Denmark.

Statistics Denmark provided official register data which can be matched with the experimental data. By using the official register data, we can compare the background characteristics of our participants with a fully representative group of adults from the general population in Denmark. We observe from Table 4.1 that our sample of 1,508 participants is similar to the general population with respect to age, gender, and length of education.[2]

Table 4.1: Sample characteristics

	Participants		General population	
	Mean	*Std.dev.*	*Mean*	*Std.dev.*
Age	47.7	14.6	48.7	16.2
Male	0.515	0.500	0.495	0.500
Years of education	13.6	2.37	12.2	2.94

Notes: The table reports age, gender and years for education of the 1,508 participants in the experiment and for a representative sample of 40,000 individuals in the Danish adult population aged 18-80 years.

In order to ensure the participants' anonymity in the experiment, Statistics Denmark generated a unique and random six-digit id-number for each participant. The invitation letter, which was distributed to the participants by Statistics Denmark, included a URL to the experiment's website, and a unique login code which the invitee had to enter on

[2] 1,565 participants took part in the dictator game, but background information is lacking for 57 participants. Our main analysis is therefore conducted on the 1,508 participants for which we have both experimental data and background data. In Appendix C, Figure C.4, we show that the association between response time and fairness is robust to the inclusion of the 57 participants for whom background information is missing.

the website in order to access the experiment. The payments to the participants were made anonymously via electronic bank transfers to the subjects' bank accounts.

4.2.2 The design

The experiment was a standard one-shot dictator game with an endowment of 150 DKK (approximately 27 USD). Participants were matched in pairs and one of the participants, the dictator, was asked to decide how to split the money with the other participant, the receiver. The dictator could choose between 11 different amounts to give to the other participant: 0 DKK, 15 DKK,...., 75 DKK,..., 135 DKK, 150 DKK. Due to the simplicity of the experiment, it was not cognitively demanding to identify to identify the selfish alternative and the fair alternative. Each participant was involved in two situations, one as a dictator and one as a receiver, and was matched with a different participant in each situation.[3] After the experiment, one of the two situations was randomly drawn to determine payments to the participants.

In line with the existing literature, we measure the response time, T in Figure 4.1, as the time elapsed from opening the experiment's decision screen until closing it again by submitting a decision on the screen. A participant's response time, however, is likely to be affected by a wide range of personal characteristics unrelated to the participant's economic decision. In particular, a participant's cognitive ability and swiftness would affect the time used to read and understand the instructions as well as the time used to implement the decision. We therefore collect information that allows us to control for these factors.

We measure the participant's swiftness as his or her response time on a screen with three background questions about age, gender, and educational attainment. Since these questions are easy to understand and require no deliberation, we view the response time on this screen as an inverse measure of the participant's swiftness (i.e. a short response time means a high degree of swiftness). We also measure the participant's cognitive ability using a 20-item progressive matrices test (Beauducel et al., 2010) which is a general intelligence test measuring the participant's ability to think logically in unfamiliar situations.

The cumulative distributions of swiftness and cognitive ability are provided in Fig-

[3]The translated instructions to the experiment are provided in Appendix C, Section C.1.

ure 4.3. We observe from Panel A that there is a striking heterogeneity in the participants' swiftness; the fastest participants spent less than 20 seconds on answering the background questions, while the median response time is close to 40 seconds. As shown in Panel B, there is also considerable heterogeneity with respect to cognitive ability, with the average score of 8.77 being close to what is typically observed in samples with a similar age distribution (Beauducel et al., 2010). Taken together, the two panels in Figure 4.3 show that the potential confounds with swiftness and cognitive ability are serious when interpreting short response time as an indication of intuitive behavior.

4.3 Results

Figure 4.4 provides an overview of the choice frequencies and median response time of the different alternatives in the choice set.[4] The average share given to the receiver was 0.34, which is somewhat higher than what is typically found in dictator game experiments with student samples (Engel, 2011). We observe that the majority of the participants chose either the selfish alternative (the selfish participants, 25%) or the fair alternative (the fair participants, 52%). The median response time among the selfish was 37 seconds, whereas it was only 29 seconds among the fair.[5] The median response time among the 23% of subjects who chose neither the selfish nor the fair alternative (the trade-off participants) was 39 seconds.

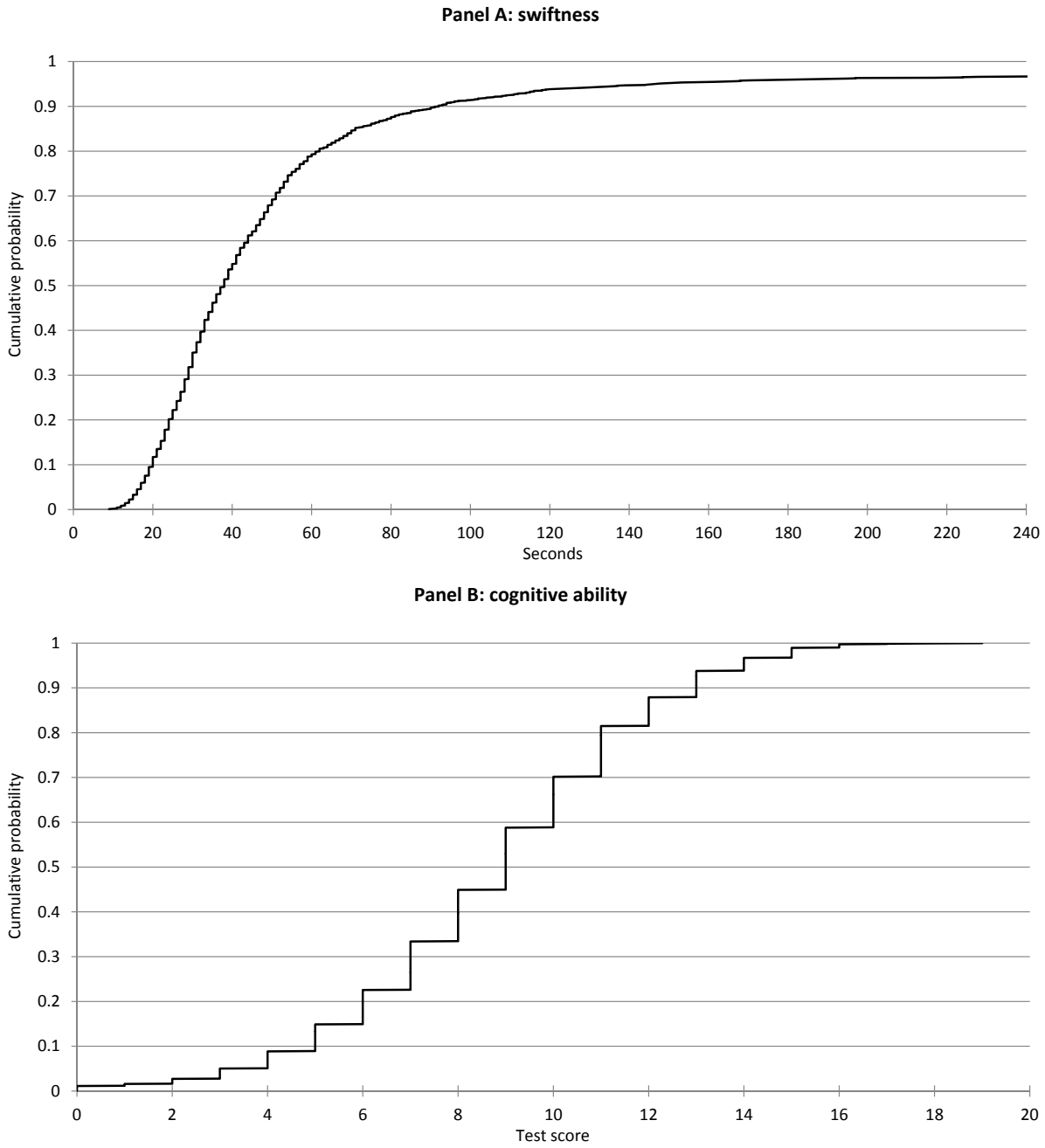
Our main focus is on whether the intuitive response to distributive behavior is to behave selfishly or fairly, and we thus start by comparing the response time of the two groups. In Figure 4.5 we report the cumulative distributions of response time of the selfish and the fair participants. We observe that the cumulative distribution of the fair participants strictly dominates the cumulative distribution of the selfish participants, and we can clearly reject that the two distributions are the same (Mann-Whitney test, $p < 0.001$).

Table 4.2, column (1), reports the corresponding OLS regression, where we again observe that the fair participants have significantly shorter response time than the selfish

[4]We did not enforce time restrictions in our experiment. This means that the distribution of response time in the experiment is heavily skewed to the right. Since more than 90% of the subjects submitted their decision within two minutes, however, we top-code the response time at 120 seconds. In Appendix C, Section C.2, we show that our results are robust to top-coding at 60 or 240 seconds.

[5]The median response times are lower than the average response times because the distribution of response time is skewed to the right.

Figure 4.3: Cumulative distributions of swiftness and cognitive ability

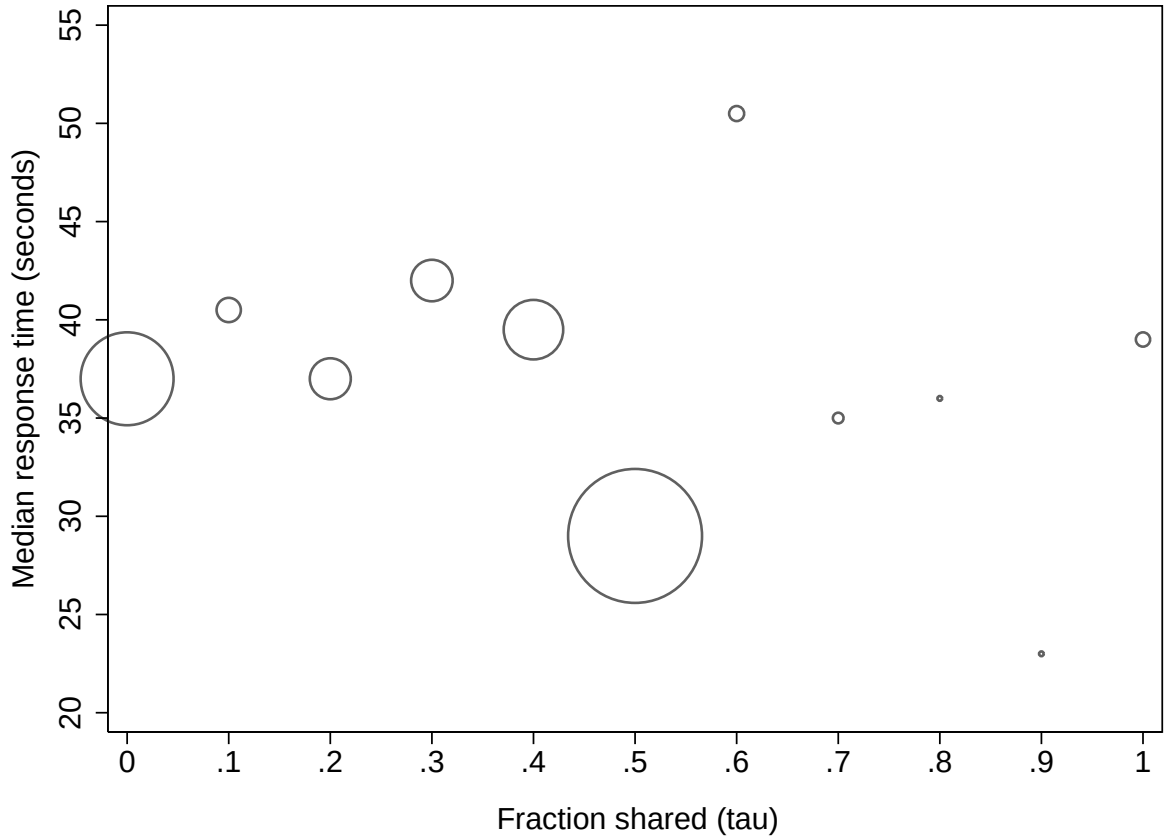


Note: The figure shows the cumulative distribution of swiftness (Panel A) and cognitive ability (Panel B) in the sample of participants (n = 1,508). Swiftness is measured as the response time on a three-item questionnaire on age, gender, and level of education. Cognitive ability is measured as the participant's score in a 20-item progressive matrices test (cognitive ability).

participants ($p < 0.001$).[6] In columns (2)-(5), we include different background variables as controls. From column (5) we observe that the association between fairness and response time holds when all controls are included. The estimated coefficient for being fair implies

[6]In Appendix C, Section C.2, we show that the results also hold for Tobit regressions.

Figure 4.4: Choice frequencies and median response time

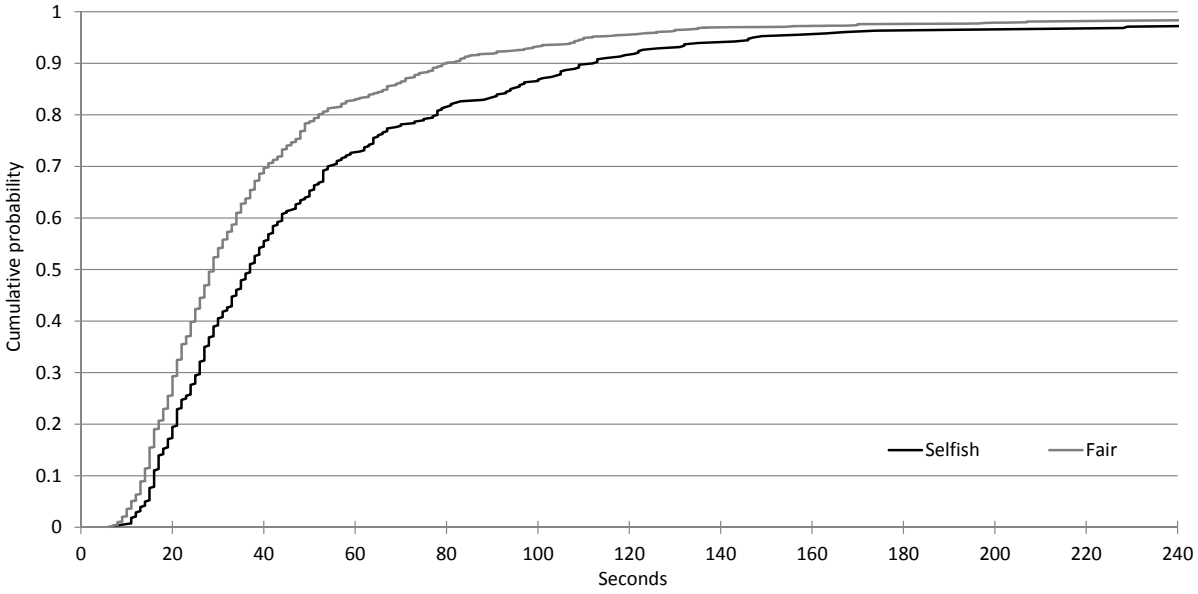


Note: The figure shows the median response time for each alternative in the choice set for the 1,508 participants. The circle sizes have been weighted by the choice frequencies.

that the average response time of the fair participants is 0.45 standard deviations lower than the average response time of the selfish participants. From column (5), we also observe that swifter participants respond significantly faster. This association highlights the danger of interpreting a short response time (T in Figure 4.1) as a short decision time ($t_2 - t_1$ in Figure 4.1). When including all controls we do not find any significant association between response time and cognitive ability. Finally, we find that older people tend to have a longer response time than younger people, while we do not find any association between response time and gender or education.

Participants who chose neither the fair nor the selfish alternative were engaged in an active trade-off between fairness and self-interest. From Table 4.3, column (1) we find that there is no significant association between response time and the share given for this group. This finding is robust to the inclusion of the full set of controls in column (5). We also observe that the estimated effect of cognitive ability is highly significant for the

Figure 4.5: Cumulative distribution of response time



Note: The figure shows the cumulative distribution of the response time in seconds for the selfish (25% of the 1,508 participants) and the fair participants (52% of the 1,508 participants). A selfish participant is defined as someone who gives nothing to the other participant; a fair participant is defined as someone who gives 50

trade-off group; the estimated difference between the response time of the participants in the bottom and top 10% of the cognitive ability distribution is more than 60 seconds. We interpret this result as showing that those who engage in an active trade-off between fairness and self-interest rely on deliberation and not on intuition when they make their decision. The estimated effect of swiftness is, however, in line with what we observe in Table 4.2, which is as expected since swiftness would primarily affect the implementation of the decision.

The fair participants also have a shorter response time than the trade-off participants ($p < 0.001$).[7] Thus, overall, our analysis provides evidence of fair behavior being intuitive and requiring a short decision-time, whereas any deviation from fair behavior seems to trigger deliberation and a longer decision-time.

[7]An OLS regression of response time for all participants is included in Appendix C, Table C.10.

Table 4.2: Regressions of response time, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)
Fair	−0.316***	−0.412***	−0.377***	−0.437***	−0.450***
	(0.059)	(0.056)	(0.059)	(0.060)	(0.058)
Swiftness		−0.012***			−0.010***
		(0.001)			(0.001)
Intelligence			−0.050***		−0.014
			(0.009)		(0.009)
Age				0.016***	0.005**
				(0.002)	(0.002)
Male				−0.016	0.000
				(0.054)	(0.052)
Education				−0.019*	0.003
				(0.011)	(0.011)
Constant	1.522***	2.441***	2.009***	1.126***	2.212***
	(0.049)	(0.086)	(0.099)	(0.183)	(0.222)
Observations	1,154	1,154	1,154	1,154	1,154
R^2	0.024	0.142	0.050	0.078	0.149

Notes: OLS regressions. The dependent variable is the response time (top-coded at 120 seconds) divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving half of the money to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.3: Regressions of response time, trade-off participants only

	(1)	(2)	(3)	(4)	(5)
Share given	−0.458	−0.270	−0.366	−0.381	−0.273
	(0.331)	(0.305)	(0.315)	(0.324)	(0.298)
Swiftness		−0.015***			−0.013***
		(0.002)			(0.002)
Cognitive ability			−0.096***		−0.063***
			(0.016)		(0.016)
Age				0.017***	0.001
				(0.004)	(0.004)
Male				−0.135	−0.077
				(0.107)	(0.099)
Education				0.024	0.050**
				(0.023)	(0.022)
Constant	1.751***	2.810***	2.529***	0.702*	2.510***
	(0.125)	(0.175)	(0.174)	(0.366)	(0.411)
Observations	354	354	354	354	354
R^2	0.005	0.161	0.101	0.066	0.215

Notes: OLS regressions. The dependent variable is the response time (top-coded at 120 seconds) divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included participants who chose neither the selfish nor the fair alternative (354 participants). "Share given" is the share of the money given to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.3.1 Heterogenous effects

We now turn to the question of whether there are systematic differences across society with respect to how people intuitively respond to a distributive problem. We address this question by examining how the association between fair behavior and response time interacts with the participant's characteristics.

In Table 4.4, we report OLS regressions of response time on interaction effects for those participants who chose either the selfish or the fair alternative. We observe no significant interaction effect between the fair behavior and swiftness or between fair behavior and cognitive ability. We also observe that the association between fair behavior and response time is strikingly similar for participants of different age, gender, and educational attainment. Taken together, these results show that the intuitive response to the dictator game is the same across society which suggests that fair behavior being intuitive is a general human trait.

4.4 Concluding remarks

We find that participants in a large and heterogenous sample use significantly less time to make a decision in the dictator game when they act fairly than when they act selfishly. This is robust to controlling for a rich set of background information about the participants, including independent measures of their swiftness and cognitive ability.

Our analysis sheds light on the conflicting results observed in the previous literature. We find significant heterogeneity in swiftness and cognitive ability among the participants, and we show that these characteristics matter when explaining response time. We argue that this, at least partly, reflects that response time consists of more than decision time; it also captures the time spent on reading and understanding the instructions as well as the time spent on implementing the decision. None of the previous studies on response time and fair behavior controlled for these personal characteristics, which means that the mixed results may reflect confounds related to associations between cognitive ability, swiftness, and the importance attached to fair behavior. Further, it follows from our analysis that an exogenous manipulation of response time does not necessarily map into an exogenous manipulation of decision time (Rand et al., 2012; Tinghög et al., 2013), it may as well affect the other components of response time, and thus does not cleanly

identify the effect of increased reliance on intuitive behavior.

We also find a striking similarity in the relationship between fair behavior and response time in the Danish society across gender, age groups, and educational attainment. Taken together our results provide compelling evidence suggesting that the predisposition to act fairly is a general human trait.

Table 4.4: Heterogeneity across age, gender, and education, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fair	−0.316***	−0.396***	−0.433***	−0.432***	−0.375***	−0.411***	−0.656***
	(0.059)	(0.086)	(0.097)	(0.091)	(0.089)	(0.086)	(0.145)
Swift		−0.531***					−0.442***
		(0.095)					(0.103)
Fair× Swift		0.015					−0.015
		(0.116)					(0.126)
High cognitive ability			−0.357***				−0.240**
			(0.103)				(0.104)
Fair× High cognitive ability			0.113				0.135
			(0.123)				(0.125)
Young				−0.382***			−0.157
				(0.100)			(0.107)
Fair× Young				0.078			0.076
				(0.121)			(0.130)
Male					−0.062		−0.046
					(0.098)		(0.095)
Fair× Male					0.110		0.074
					(0.119)		(0.115)
Low education						−0.097	−0.139
						(0.097)	(0.094)
Fair× Low education						0.180	0.150
						(0.119)	(0.115)
Constant	1.523***	1.842***	1.764***	1.769***	1.559***	1.571***	2.150***
	(0.049)	(0.074)	(0.084)	(0.080)	(0.075)	(0.068)	(0.124)
Observations	1,154	1,154	1,154	1,154	1,154	1,154	1,154
R^2	0.024	0.098	0.045	0.053	0.025	0.026	0.110

Note: OLS regressions. The dependent variable is the response time (top-coded at 120 seconds) divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving 50% of the money to the other participant, "Swift" is a dummy for being at or above the median of the swiftness distribution, "High cognitive ability" is dummy for scoring at or a dummy for being at or above the median of the 20-item progressive matrices test distribution, "Young" is a dummy for being at or below the median age distribution, "Male" is a dummy for being a male, and "Low education" is a dummy for being at our below the median of the educational attainment distribution (in years).

Chapter 5

Parents' Education and their Adult Offspring's Other-Regarding Behavior

Abstract

Does socioeconomic background when measured by parental educational attainment explain the heterogeneity in adults' other-regarding preferences? I test this by using data from two online experiments - a Dictator Game and a Trust Game - that were conducted with a broad sample of the Danish adult population. I match the experimental data with high-quality data from the Danish population registers about my subjects and their parents. Whereas previous studies have found socioeconomic status, including parental educational attainment, to be predictive for children's generosity, I find no such evidence among adults. This result is robust across age groups and genders. I provide two explanations for this. First, sociodemographic characteristics in general appear to be poor predictors of adults' other-regarding behavior. Second, by using Danish survey data, I find that Danish parents' educational attainment appears to be uncorrelated with how important they find it to teach their children to "think of others". More speculative explanations are also provided.

Acknowledgments

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5.1 Introduction

Empirical research has consistently documented that humans are vastly different in how generously they treat each other. In the experimental literature, for instance, hundreds of Dictator Game experiments have shown that a majority of people in that social context appear to be willing to voluntarily share a positive amount of money with a stranger.[1] However, little is known about how other-regarding preferences are shaped and where the heterogeneity in them originates from.

A few studies have investigated the intergenerational transmission of these preferences. Wilhelm et al. (2008) compared charitable giving in the United States of parents and their offspring and found a high degree of intergenerational dependency in giving to religious causes, but a less significant correlation in giving to other causes. This is in line with Bisin and Verdier (2000) who observed that smaller ethnic or religious minorities tend to invest more in transmitting their attitudes to future generations than larger minorities or majorities do, because the smaller minorities arguably worry more about the survival of these attitudes. Also, Okumura and Usui (forthcoming) used American survey data to find that children of sociable parents appear to be more sociable themselves. A natural question to ask is then whether such transmission of attitudes and behavior is caused by nature or nurture. In a Swedish twin-study, Cesarini et al. (2009) compared Dictator Game choices (as well as risky choices) made by each of two twins and found that choices made by monozygotic twins are more correlated than those made by same-gender dizygotic twins. They concluded that 20% of the variation in Dictator Game choices can be explained genetically, i.e. by nature. It has also been found that other types of preferences and behavior in social interactions such as political orientation and voting behavior are partly genetically inheritable (e.g. Alford et al., 2005; Fowler et al., 2008).

With 80% of the variation remaining unexplained by nature, however, there is an unearthed potential in identifying what facets of nurture matter for the development of other-regarding preferences. Three studies by Benenson et al. (2007), Chen et al. (2013), and Bauer et al. (2014) all related children's choices in the Dictator Game to measures

[1] In a meta-study of Dictator Game experiments, Engel (2011) used 20,813 observations from 328 treatments to find that about one third (36%) of subjects selfishly keep as much to themselves as possible, while 64% reveal a willingness to share a positive amount with another individual. Further, it has been documented that giving in the Dictator Game is positively correlated with generous behavior outside the lab (e.g. Benz and Meier, 2008; Franzen and Pointner, 2013b).

of their socioeconomic status (SES). Benenson et al. used British school children coming from six different schools which the authors categorized as either low- or high-SES schools based on the fraction of pupils in each school who received free lunches. Chen et al. used Chinese children and compared their choices in the Dictator Game with a survey measure of their family income. Bauer et al. instead conducted their experiment with Czech children who were categorized into SES individually by using survey information about the parents' educational attainments. Children of two less-educated parents were categorized as being of low SES. Whereas Benenson et al. and Bauer et al. found low-SES children to be less generous (and more spiteful) than high-SES children, Chen et al. found the opposite.[2]

In this chapter, I build on these children studies and ask whether *adults'* other-regarding preferences are also related to their socioeconomic background which I measure by their parents' educational attainments. Since practically all economic decisions in the marketplace, in politics, and in other social interactions are made by adults, it is relevant to understand how adults make their decisions and the determinants of these.

I use data from two online experiments - a Dictator Game and a Trust Game - that were conducted with more than 1,500 subjects in each. The experiments were carried out at the Internet Laboratory for Experimental Economics (iLEE) at the University of Copenhagen in collaboration with Statistics Denmark who recruited the subjects from the broad Danish adult population. Statistics Denmark provide data that allows me to match choice data from the experiments with high-quality individual-level data from the Danish population registers. This means that I can match experimental choices with characteristics about the subjects and their parents, too.

I measure the subjects' socioeconomic backgrounds by their parents' educational attainments and adopt the definition of low socioeconomic status from Bauer et al. (2014) which means that I define low-SES subjects as those who were born to two low-educated parents, i.e. parents who had not graduated from high school. By using this definition, 19% of the Dictator Game subjects and 18% of the Trust Game subjects can be categorized as low-SES subjects. There are multiple reasons why I use parental educational attainment as an indicator for SES. First, income which is another frequently applied

[2]The conclusion in Chen et al. (2013) is similar to the conclusion drawn in Knight and Kagan (1977). However, the study by Knight and Kagan was based on a two-player simultaneous-move experiment, which resembled a Prisoner's Dilemma more than a Dictator Game.

measure of SES is highly correlated with educational attainment.[3] Also, educational attainment has the advantage of being more stable (i.e. non-decreasing in time and almost constant from one year to another) than income. Second, because the Danish population registers were not formalized until 1968 and because the registers from before 1980 are not available to researchers from, I have many more observations about my subjects' parents' highest level of education than for their incomes at the time when my subjects were children. Third, parental educational attainment is known to positively correlate with how much time they spend on (developing) child care (e.g. Guryan et al., 2008).[4]

I find highly robust evidence that adults' other-regarding behavior is unrelated to their parents' educational background. Since my subjects come from 39 different birth-year cohorts, I can check if this finding is robust across age groups. This appears to be the case. I also check if there are gender differences in the link between generosity and parental education, since Almås et al. (2012) have found this to be the case when relating adolescents' willingness to compete to their socioeconomic status. I do not find such gender differences, however.

I can think of many possible reasons why adults' other-regarding behavior should not be linked to their socioeconomic background. First of all, by relating my subjects' own sociodemographic characteristics such as their own educational attainment or income to their generosity, I generally find such characteristics to be poor predictors of generosity. This finding is in line with a recent study by Birkeland et al. (forthcoming) who used both Norwegian data and choice data from the exact same Dictator Game experiment as I do to find that criminal records are poor predictors of generosity, too. Second, I use Danish survey data to relate the respondents' educational attainments to how important they think it is to teach children to "think of others". I find no correlation, however. Bauer et al. (2014), who found a positive link between children's generosity and their parents' educational attainments, conducted a similar analysis and found that better-educated Czech parents appear to put more effort in teaching their children to be generous. This observation suggests that the relationship between other-regarding preferences and socioeconomic background might be country-specific, which could also

[3]For instance, among 40,000 representative Danish adults about whom I have access to register data, I find that an extra year of education significantly increases annual gross income by 17,000-25,000 DKK ($\approx$ 3,000-4,500 USD). See Appendix D, Table D.1.

[4]See also Bonke (2009), Bonke and Esping-Andersen (2011), and Rasmussen (2009) for Danish evidence on the link between parental educational attainment and the time they use with their children.

explain why the findings in Chen et al. (2013) oppose those in Benenson et al. (2007) and Bauer et al. (2014).

There are also more speculative explanations. For instance, if a child's endowment of other-regarding preferences is positively correlated with his or her maturity (e.g. Fehr et al., 2008; Almås et al., 2010; Fehr et al., 2013), say, and if maturity for a given age is positively related to the child's SES (e.g. Boehm, 1962) then one could expect to see that the SES gap in other-regarding preferences is decreasing in age and maturity and possibly closes in late adolescence (also known as the "impressionable years"). At least, the SES gap in verbal and math achievements appears to diminish in age (see White, 1982 for a review). Another speculative explanation is that peer effects become increasingly important in age such as when the son or daughter leaves the parents' household. Whereas Bauer et al. (2014) found peers to be unimportant for children's other-regarding preferences, Dohmen et al. (2012) found peers to be very important for German adults' endowment of risk preferences and trust attitudes, even when controlling for their parents' preferences and attitudes, too.

With socioeconomic background - at least when measured by parental educational attainment - being unable to explain the heterogeneity in adults' other-regarding preferences, there is a potential for future research to identify other channels through which nurture influences the formation of such preferences.

5.2 Recruitment

In 2007, Statistics Denmark created a random sample of the Danish adult population aged 18-80 years for the Internet Laboratory for Experimental Economics (iLEE) at the University of Copenhagen. The sample consisted of 40,000 individuals, of whom 22,027 were invited to participate in a first wave of online experiments (iLEE1) in 2008.[5] A total of 4,290 individuals responded by logging into the iLEE website and 2,291 completed the entire iLEE1.

In this chapter, I use data from two experiments that were part of later waves of experiments. In these waves, only the 2,291 subjects who had completed iLEE1 were invited. One experiment - a Dictator Game - was part of the second wave of experiments

[5]See invitation letters in Appendix D, Section D.1. See also Thöni et al. (2012) for a full description of the recruitment procedure.

(iLEE2) in 2009 and the other experiment - a Trust Game - was part of the third wave (iLEE3) in 2010.

Because the experiments were conducted online and because Statistics Denmark handled all correspondence with the subjects, both subject-subject and subject-experimenter anonymity have been maintained. This was carefully explained to the subjects, too. Statistics Denmark has ensured the subjects' anonymity by creating a unique and random ID number for each subject. Only Statistics Denmark have the key to this ID number. The ID number serves two purposes. I can use it to match data from one experiment with data from another and I can use it to match experimental data with data from the Danish population registers about my subjects. These contain high-quality individual-level data collected from governmental and private institutions (e.g. banks and employers). By using the registers, I am also able to match experimental data with my subjects' parents' characteristics.

Because the subjects participated remotely at the virtual lab, I cannot be sure that the invitees, about whom I have access to register data, were in fact those who participated in the experiment. It could be that an invited mother asked her teenage son, say, to participate instead. To avoid such confounds, I validate the subjects' identities by comparing their self-reported age and gender to the invitee's age and gender according to the registers.

5.3 Experimental Design and empirical approach

5.3.1 Study 1: Dictator Game

The Dictator Game experiment was the first experiment conducted in iLEE2.[6] It involves two persons: One of them ("Dictator") must decide how to divide a money sum between him- or herself and another anonymous person ("Recipient") who remains passive. All subjects were randomly paired twice such that they were both in the role of the Dictator and the Recipient. They were paired with a different subject in each role and there was no feedback during the experiment. In order to determine payments, the decision made in one pair was picked at random *ex post*. The instructions were written in simple language and they contained graphics to easily explain the experiment to the subjects.

[6]The choice data from this experiment has also been used in Birkeland et al. (forthcoming).

The Dictator's task was to decide how much of 150 DKK ($\approx$ 27 USD) to share with the Recipient. He or she could share any fraction $\tau \in \{0, 0.1, \ldots, 1\}$ of the money sum. See the translated instructions in Appendix D, Section D.2.

A total of 2,291 subjects were invited for participation in iLEE2 and 1,567 of them made a decision in the Dictator Game. Of these, 1,544 could be validated based on their self-reported age and gender. Subjects were paid via electronic bank transfers if they completed the entire wave.

5.3.2 Study 2: Trust Game

The Trust Game was the first experiment conducted in iLEE3 a year later. The Trust Game is a sequential-move two-person game. In the present version of the game, one person ("First-Mover") had to choose between IN and OUT. If the First-Mover chose IN, then he or she trusted the Second-Mover and the game proceeded to a second stage in which the other person ("Second-Mover") had to choose whether or not to reciprocate this trust by choosing between LEFT and RIGHT. By choosing RIGHT, the Second-Mover generously reciprocated the First-Mover's trust. If the Second-Mover chose LEFT, however, then the First-Mover was worse off as compared to not trusting the Second-Mover in the first place. Like the Dictator's decision problem in the Dictator Game, the Second-Mover's decision problem in the Trust Game was non-strategic and was simply a question of making a generous or, alternatively, a selfish act.

There was no feedback in the experiment. Instead, the strategy method was used to elicit the Second-Mover's choice.[7] With the strategy method, the two-stage game was collapsed into a simultaneous-move game with strategies and payoffs in DKK as shown below:

	LEFT	RIGHT
IN	20,90	80,40
OUT	50,20	50,20

The Nash equilibrium of the game is {OUT,LEFT}, i.e. the Second-Mover does not reciprocate the First-Mover's trust in the second stage and with this knowledge the First-

[7]Most experiments have shown no difference in behavior between applying the strategy method or direct responses (see Brandts and Charness, 2011 for a review), but in a Trust Game experiment Casari and Cox (2009) found Second-Movers to be less reciprocating under the strategy method than under direct responses.

Mover will not trust the Second-Mover in the first stage. However, playing {IN,RIGHT} is a Pareto improvement, i.e. the First-Mover trusts the Second-Mover who reciprocates this trust. As in the Dictator Game, all subjects were in the role of both players. They were paired with a different subject in each role and the outcome from one pair was randomly picked to determine payments.

1,531 subjects made a Second-Mover choice in the Trust Game, of whom 1,494 could be validated. Subjects were paid via electronic bank transfers if they completed the entire iLEE3 wave.

5.3.3 Empirical approach

In order to test for a correlation between generosity and parental educational attainment, I regress the choices made in the two experiments on a dummy variable that takes value 1 if both the subjects' parents were less-educated. I follow the strategy from Bauer et al. (2014) and define low-educated parents as those who had not completed high school, i.e. had less than 12 years of schooling. In order to check that my results are not particular to that definition, however, I employ other measures of parental educational attainment in Appendix D, Section D.3.2.

Since my results will rely heavily on the educational attainment data from the Danish population registers, I check for an intergenerational dependency in education, i.e. how parents' educational attainment relates to that of their children. By performing OLS regressions, I find that an extra year of paternal (maternal) schooling increases the offspring's educational attainment by 0.11 (0.09-0.14) years on average. See Appendix D, Table D.2. This is comparable to Swedish estimates (Björklund et al., 2004, 2006), but smaller than American estimates (see Holmlund et al., 2011 for a review).

Because the Danish population registers were not formalized until 1968, I am only able to match about two thirds (or 1,009) of the validated Dictator Game subjects with their mother and 978 with their father. A total of 969 subjects can be matched with both their parents. It is mainly the youngest subjects in my sample whom I can match with their parents. Further, I know both parents' educational attainments for 741 subjects. Similarly, 931 Second-Movers from the Trust Game can be matched with both their parents and I know both parents' educational attainments for 719 of these. In the remainder of this chapter, I will focus solely on these 741 and 719 subjects, respectively.

It is important to note that my subjects come from 39 different birth-year cohorts and that they grew up in all parts of Denmark. This has several advantages, but it also means that I cannot check for peer effects in their other-regarding behavior. Furthermore, I cannot separate age effects from cohort effects because of the cross-sectional nature of my data.

5.3.4 Descriptive statistics

In Table 5.1, I provide descriptive statistics of my study samples. 48% of the Dictator Game subjects were women, the average age was 37.1 years at the beginning of 2009, and the average educational attainment measured by the length of schooling was 13.6 years. 89% of the Dictator Game subjects were employed, 16% lived in a single-person household, and 31% resided in one of Denmark's four largest cities (Copenhagen, Aarhus, Odense, or Aalborg). These shares are similar for the Trust Game sample.

A total of 19% (18%) of the Dictator Game (Trust Game) subjects had two low-educated parents given my definition. The average father was 29.2 years old when the typical subject in my sample was born and he had 12.4 years of schooling. In comparison, the average mother was 26.5 years old and had 11.6 years of schooling. I present distributions of the parents' educational attainments in Appendix D, Figure D.8.

5.4 Results

5.4.1 Study 1: Dictator Game

In the Dictator Game, the average subject shared 31.3% (or 47 DKK) of the money sum with the Recipient. 30.6% of the subjects shared nothing and 46.3% split the money evenly with the other subject. In Figure 5.1, I compare the share given to the Recipient across parental educations. Those with low-educated parents shared 34.3% on average, whereas the others shared 30.7%. This effect is opposite the one found in Benenson et al. (2007) and Bauer et al. (2014), but in line with Chen et al. (2013). However, neither the means (t-test, $p = 0.091$) nor distributions of the fraction of the money sum shared with the Recipient (Mann-Whitney test, $p = 0.088$) are significantly different at the 5%-level.

In Table 5.2, I essentially show that this finding is robust in a regression analy-

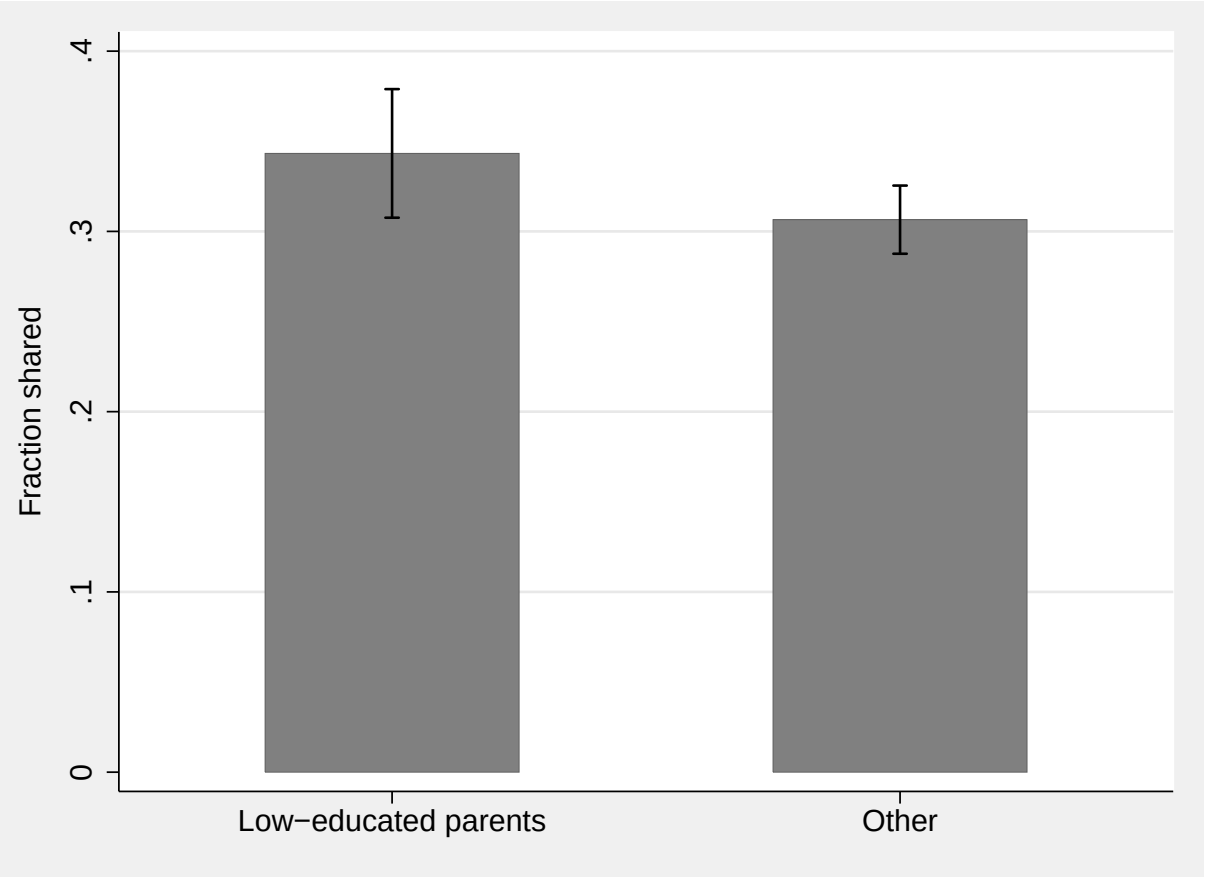
Table 5.1: Description of the subjects and their parents

	Dictator Game		Trust Game	
	Mean	Std. dev.	Mean	Std. dev.
Individual characteristics				
Age[a]	37.1	10.4	38.1	10.4
Female	0.483	0.500	0.480	0.500
Years of education	13.6	2.16	13.8	2.20
Employed	0.888	0.316	0.822	0.383
Single-person household	0.159	0.366	0.143	0.351
Urban residence	0.306	0.461	0.317	0.466
Parental characteristics				
Low-educated parents	0.190	0.393	0.184	0.387
Father's age	29.2	5.22	29.2	5.24
Father's education	12.4	3.23	12.5	3.16
Mother's age	26.5	4.79	26.5	4.75
Mother's education	11.6	3.25	11.6	3.23

Notes: [a] Note that subjects' ages differ because the Trust Game was conducted one year after the Dictator Game.

sis. Column 1 is a univariate regression comparing means across parental educational attainments. In column 2, I control for individual characteristics and find that the non-correlation between generosity and parental educational attainment is robust to these. In line with the literature, I observe that age is positively correlated with generosity (Engel, 2011). In my data, subjects increase the share given by 0.4 percentage points per year of age. However, I find no correlation between gender and generosity. I also control for each subject's current employment status and household characteristics since others have found these to be predictive of behavior in social dilemma experiments. For instance, Rand et al. (2012) documented that individuals who naturally rely on other people in their daily life (e.g. on the job) are more cooperative than others. Also, Borgloh et al. (2010) found that unmarried individuals share more in Dictator Games and donate more to charities, too. Gächter and Herrmann (2011) found (young) people living in urban areas to be less cooperative than those living in rural areas. Whereas I find weak evidence that employed subjects are less generous than unemployed, I do not find any correlation between household characteristics and generosity. In column 3, I add controls for the

Figure 5.1: Comparison of Dictator Game sharing across parental educations



Note: Lines show the 95% confidence intervals.

parents' ages at the beginning of the subject's birth year. This allows me to control for the trend growth in educational attainment that has taken place over time, i.e. the fact that younger cohorts are better educated than older cohorts. Thus, when controlling for the subject's own age as well as the parents' ages when he or she was born, I implicitly control for the parents' current age. My result is robust to this control, too.

In Appendix D, Section D.3.2, I present robustness checks by employing other model specifications including other functional forms of parental education. My results are generally robust to these checks (see Appendix D, Tables 5.6-5.8).

I then check if my finding is robust across age groups. I do this by defining "young" subjects as those who were 19-30 years old at the beginning of 2009 and "old" as those who were 31 years or older. By splitting up the regressions on young and old, I find no significant relationship between generosity and parental educational attainment for either age group. See Table 5.3. The effect of low parental education on generosity appears to be negative, but insignificant among the younger subjects and positive, but

Table 5.2: Regressions of Dictator Game sharing on parental educational attainment

	(1)	(2)	(3)
Low-educated parents	0.037	0.010	0.005
	(0.022)	(0.022)	(0.023)
Age		0.004***	0.004***
		(0.001)	(0.001)
Gender		0.025	0.024
		(0.017)	(0.017)
Employed		-0.054*	-0.055*
		(0.027)	(0.027)
Single-person household		0.038	0.039
		(0.024)	(0.024)
Urban residence		0.004	0.005
		(0.019)	(0.019)
Father's age			0.002
			(0.003)
Mother's age			-0.005
			(0.003)
Constant	0.31***	0.18***	0.26***
	(0.010)	(0.040)	(0.073)
N	741	741	741
R^2	0.004	0.041	0.046

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

insignificant among the older subjects. I use the *seemingly unrelated regression* method (Zellner, 1962) to pairwise compare coefficients across age groups and conclude that these are not significantly different.[8] This is robust for both genders.

I also test if the correlation between generosity and parental educational attainment is gender-specific. There are several reasons why this might be the case. In a survey study, Okumura and Usui (forthcoming) found a stronger correlation in sociability between boys and their parents than between girls and parents. Others have found that fathers tend to involve themselves more in child care if they have a son than if they do not, which could plausibly affect boys' as compared to girls' development of (other-regarding) preferences

[8]Column 1 vs. column 4: $\chi^2 = 0.76, p = 0.384$. Column 2 vs. column 5: $\chi^2 = 1.18, p = 0.463$. Column 3 vs. column 6: $\chi^2 = 0.92, p = 0.338$.

Table 5.3: Regressions of Dictator Game sharing on parental educational attainment, by age

	19-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.019	-0.030	-0.051	0.032	0.013	0.010
	(0.064)	(0.065)	(0.065)	(0.023)	(0.023)	(0.024)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	223	223	223	518	518	518
R^2	0.000	0.036	0.059	0.004	0.058	0.061

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

(e.g. Harris et al., 1998; Bonke and Esping-Andersen, 2011). A third line of research have found gender differences in preferences including other-regarding preferences (see Croson and Gneezy, 2009 for a review). In my data, however, I find generosity and parental education to be uncorrelated for both genders. See Table 5.4.

Table 5.4: Regressions of Dictator Game sharing on parental educational attainment, by gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.060	0.022	0.011	0.013	-0.002	-0.0001
	(0.032)	(0.033)	(0.034)	(0.029)	(0.030)	(0.030)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	383	383	383	358	358	358
R^2	0.009	0.067	0.074	0.001	0.020	0.027

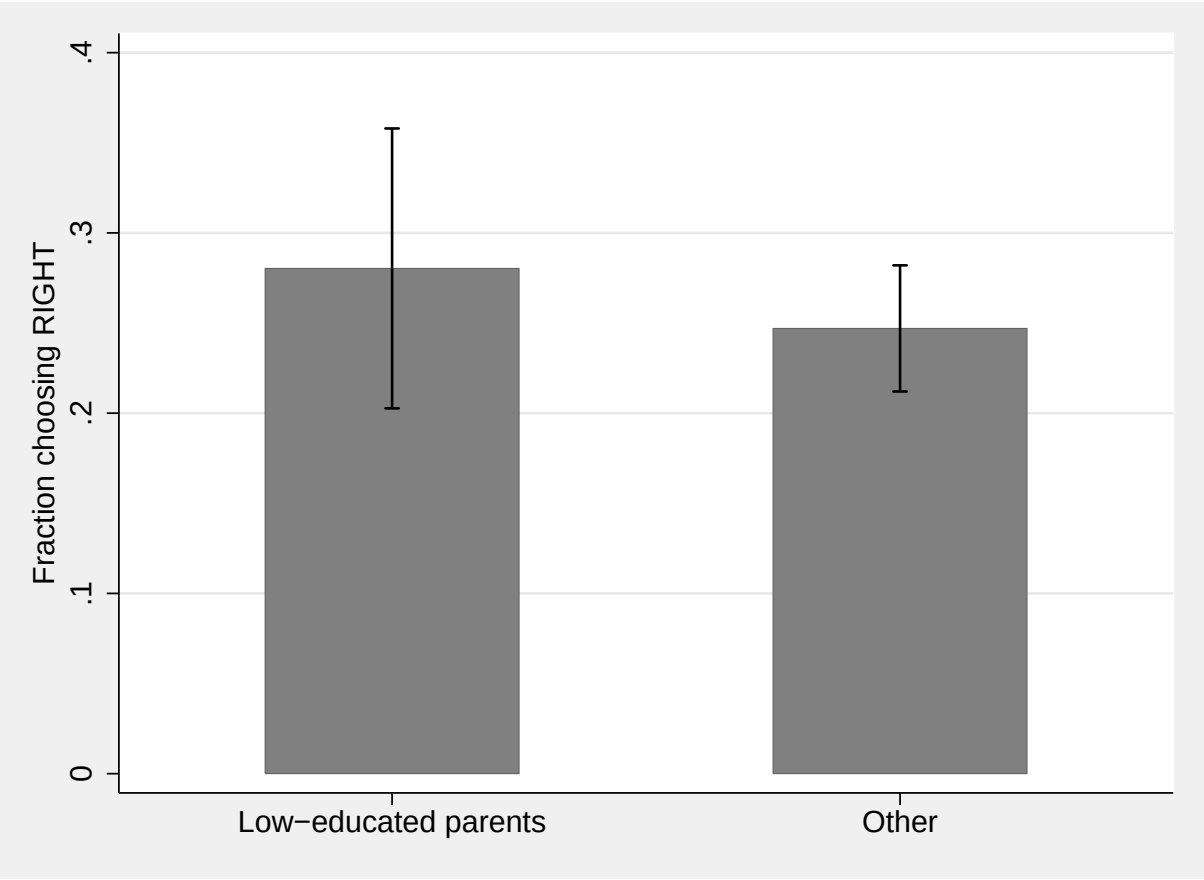
Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.4.2 Study 2: Trust Game

One could speculate that my findings are specific to the particular Dictator Game experiment I study. To approach such concerns, I redo the analysis using Second-Mover

choices from a Trust Game experiment conducted with an overlapping subject sample a year later. In the Trust Game, the subjects had only two options when they were in the role of the Second-Mover: Choose LEFT (i.e. be selfish) or RIGHT (i.e. be generous). 25.3% of the subjects generously chose RIGHT. In Figure 5.2, I compare the fraction of subjects who chose RIGHT across parental educational attainments. 28.0% of the subjects born to two low-educated parents chose RIGHT, while 24.7% of the others did so. This difference is not significant, however (Fisher's exact test, $p = 0.439$).

Figure 5.2: Generosity in the Trust Game across parental educations



Note: Lines show the 95% confidence intervals. Choosing RIGHT was generous in the Trust Game.

In Table 5.5, I check the robustness of this finding in a series of logit regressions in which the dependent variable is a dummy that takes value 1 if the Second-Mover chose RIGHT. Again, I cannot detect any significant link between adults' generosity and their parents' educational attainments. My results are robust to a alternative functional forms of parental education, too (see Appendix D, Tables D.16-D.18).

I then check, as before, if the result is also robust across age groups. In Table 5.6,

Table 5.5: Generosity in the Trust Game and parental educational attainment

	(1)	(2)	(3)
Low-educated parents	0.033	0.023	0.040
	(0.043)	(0.044)	(0.046)
Age		0.003	0.004*
		(0.002)	(0.002)
Gender		-0.071*	-0.072*
		(0.032)	(0.032)
Employed		-0.010	-0.007
		(0.043)	(0.043)
Single-person household		0.049	0.044
		(0.050)	(0.050)
Urban residence		0.018	0.015
		(0.038)	(0.037)
Father's age			-0.010*
			(0.005)
Mother's age			0.012*
			(0.006)
N	719	719	719

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject generously chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

I observe a negative, but insignificant correlation between generosity and low parental education for the youngest subjects and a positive, but insignificant correlation among the oldest. This pattern is similar to what I observed in the Dictator Game. Since I cannot easily test for differences in coefficients across logit regressions, I apply the seemingly unrelated regressions method using estimates from linear probability model regressions that are very similar to the logit regression estimates presented here (see Appendix D, TableD.21). I pairwise compare coefficients on parental educational attainment and find that the link between generosity and parental education is not significantly different across age groups.[9] This conclusion holds for both genders.

I also check if men's endowment of generosity is more correlated with their parents' educational characteristics than women's or *vice versa*. The estimates presented in Ta-

[9]Column 1 vs. column 4: $\chi^2 = 2.31, p = 0.129$. Column 2 vs. column 5: $\chi^2 = 1.54, p = 0.318$. Column 3 vs. column 6: $\chi^2 = 0.35, p = 0.503$.

ble 5.7 indicate that this is not the case.

Table 5.6: Generosity in the Trust Game and parental educational attainment, by age

	20-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.106	-0.072	-0.063	0.047	0.037	0.046
	(0.088)	(0.102)	(0.103)	(0.048)	(0.049)	(0.050)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	211	211	211	508	508	508

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject generously chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5.7: Generosity in the Trust Game and parental educational attainment, by gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.080	0.061	0.081	0.006	-0.007	0.006
	(0.067)	(0.069)	(0.072)	(0.054)	(0.054)	(0.056)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	374	374	374	345	345	345

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject generously chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.5 Discussion

In this section, I first show that the behavior in the two experiments is related by performing a within-subject analysis in which I exploit the fact that a total of 583 validated individuals, about whom I know their parents' educational attainments, participated in both the Dictator Game in 2009 and the Trust Game in 2010. I regress the fraction of the money sum shared with the Recipient in the Dictator Game on a dummy variable

that takes value 1 if the subject generously chose RIGHT in the Trust Game (see Table 5.8). I observe that subjects choosing RIGHT in the Trust Game on average shared 7-13 percentage points more of the money sum in the Dictator Game. This is highly robust for both young and old as well as men and women and it suggests that the non-link between adults' other-regarding behavior and their parents' educational attainment is generalizable across different social dilemmas.

Table 5.8: Regressions of Dictator Game choice on Trust Game choice

	Full sample (1)	Full sample (2)	$\leqslant 30$ years (3)	> 30 years (4)	Men (5)	Women (6)
Chose RIGHT	0.11***	0.10***	0.12**	0.098***	0.13***	0.072*
	(0.023)	(0.023)	(0.046)	(0.025)	(0.030)	(0.034)
Age		0.004***	0.002	0.009***	0.005***	0.003*
		(0.001)	(0.005)	(0.002)	(0.001)	(0.001)
Female		0.027	0.047	0.023		
		(0.019)	(0.036)	(0.022)		
Employed		-0.031	-0.070	0.003	-0.010	-0.055
		(0.025)	(0.042)	(0.030)	(0.034)	(0.036)
Single-person household		0.014	-0.034	0.051	0.057	-0.045
		(0.027)	(0.041)	(0.036)	(0.037)	(0.040)
Metropolitan residence		0.016	-0.013	0.015	-0.003	0.043
		(0.022)	(0.037)	(0.027)	(0.030)	(0.031)
Constant	0.28***	0.12**	0.25*	-0.10	0.062	0.21***
	(0.011)	(0.044)	(0.12)	(0.080)	(0.061)	(0.059)
N	583	583	180	403	301	282
R^2	0.040	0.076	0.066	0.105	0.114	0.055

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses. "Chose RIGHT" is a dummy variable that takes value 1 if the subject chose RIGHT in the Trust Game and 0 otherwise.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The question is then why I do not find a link when children studies have done so? One possible explanation is that sociodemographic characteristics are generally poor predictors of adults' other-regarding behavior. I check this by regressing generosity in the Dictator Game and Trust Game experiments separately on the subjects' own sociodemographic characteristics, i.e. their educational attainment and gross income. I find such sociodemographic characteristics to be rather uncorrelated with generosity in both exper-

iments (see Appendix D, Tables D.3 and D.4). If anything, less-educated Danish adults appear to be more generous than better-educated which is in line with the negative, but insignificant effect from parental educational attainment on adults' generosity that was observed in Figures 5.1 and 5.2.

Another possible explanation is more sample-specific. Bauer et al. (2014) studied children and parents who were recruited from Prague, Czech Republic. In order to justify that there exists a positive (negative) relationship between generosity (spitefulness) and parental educational attainment as found in their experiment, they used Czech data from the European Values Survey to document that low-educated Czech respondents think it is less important to teach children to be selfless than better-educated respondents do. I am able to perform a similar analysis by using Danish value data.[10] In the Danish survey, the respondents were asked to choose up to five traits which they thought were most important to teach children. They could choose from a menu of 11 options. One option was to teach children to "think of others". In a series of Fisher's exact tests and logit regressions, I check whether low-educated respondents, who were parents themselves, were more or less likely than better-educated parents to choose this option. I find no significant relationship, though.[11] This is robust across the interviews conducted in 1990, 1999, and 2008.[12] Hence, the results reported in both this chapter and previous children studies could possibly be sample- or country-specific. This could also explain why the conclusion drawn in the Chinese study by Chen et al. (2013) differs from those drawn in European studies by Benenson et al. (2007) and Bauer et al. (2014).

Other possible explanations are more speculative. One such explanation is that the SES gap in other-regarding preferences closes in late adolescence (also commonly known as the "impressionable years" in the psychology, sociology, and political science literatures), which is an age group that has neither been investigated in this chapter nor in the before-mentioned children studies. Research contributions by Fehr et al. (2008), Almås et al.

[10] I use data from the Danish Value Survey, cross-section 1981-2008, which was collected by Peter Gundelach, Department of Sociology, University of Copenhagen. This data was made available to me by Dansk Data Arkiv (archive no. DDA-23923). All results and interpretations reported in this chapter are my own.

[11] I use Fisher's exact tests to test whether the share of low-educated parents who thought it was important to teach children to think of others was different from the fraction of better-educated parents. I can reject that these fractions are different across educational attainments in any interview year. 1990: $p = 0.229$, 1999: $p = 0.863$, 2008: $p = 0.942$. See also a logit regression analysis in Appendix D, Table D.5.

[12] Subjects were not asked about their own educational attainment in the first interview-year, 1981.

(2010), Fehr et al. (2013), and Bauer et al. (2014) have all documented that there exists a positive link between children's generosity and their age, thus concluding that other-regarding preferences develop in childhood. In addition, Benenson et al. (2007) found the SES gap in generosity to be increasing in young childhood (ages 4-9). By combining these observations with findings that maturity is positively linked to SES (e.g. Boehm, 1962) and that the SES gap in other domains such as school performance closes in adolescence (e.g. White, 1982), is it then possible that the apparent SES gap in other-regarding preferences is closing in maturity and in age later in childhood, too? Future research is necessary to answer this question.

Yet another speculative explanation is that peers, as compared to parents, become increasingly important in age. In a German survey study, Dohmen et al. (2012) found peer effects to be important for the endowment of risk preferences and trust attitudes in young adults. For children's other-regarding preferences, however, Bauer et al. (2014) did not find peers to be important. Future research could also investigate the importance of peers for adults' preferences and behavior.

5.6 Conclusion

In this chapter, I have made three important contributions to the ongoing research that attempts to identify where the heterogeneity in (other-regarding) preferences and behavior originates from. First, I use a comparably large and heterogenous adult sample to find that adults' other-regarding behavior is not related to their socioeconomic background, which I measure by their parents' educational attainments. Second, I find this result to be robust across two different social contexts; a Dictator Game and a Trust Game. Third, my findings are robust across age groups and genders, too.

I provide two possible explanations for why I cannot identify a relationship when other researchers have found children's other-regarding preferences to be related to their socioeconomic status (Benenson et al., 2007; Chen et al., 2013; Bauer et al., 2014). One is that sociodemographic characteristics in general are poor predictors of adults' other-regarding preferences. The other is that relationships between other-regarding behavior and socioeconomic background might be country-specific.

With 80% of the heterogeneity in adults' other-regarding preferences being unex-

plained by nature (Cesarini et al. (2009)), there exists an important and latent potential in understanding how nurture intervenes in individuals' development and formation of such preferences. In this chapter I conclude that socioeconomic background - at least when measured by parental educational attainment - cannot explain this heterogeneity.

Chapter 6

Does Shared Responsibility Breed Unfairness?

Abstract

Are economically efficient, but unpopular policies more likely to be implemented by decision-makers who are organized in a committee than by a single decision-maker? We experimentally study this question in a Group Dictator Game in which decision-makers can be held responsible and punished by the recipients for implementing distributionally unfair and thus unpopular policies. When organized in a committee, the decision-makers share the blame for their decision, whereas they are held fully responsible when not. We find that shared responsibility does in fact lead to more unfair outcomes, but only when the unfairness is very pronounced. We provide explanations for this. We also find that the recipients' punishment pattern is consistent with aversion towards disadvantageous inequality and that most decision-makers maximize their own payoffs given their beliefs, but tend to overestimate the recipients' punishments.

Note

This chapter has been written together with Raymond Duch and Jean-Robert Tyran.

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6.1 Introduction

Many important decisions in the economic and political sphere are made in committees. In politics, for instance, politicians, political parties, and coalition members collectively decide whether or not to implement some policy. Likewise in companies where boards of directors often collectively decide policies of strategic importance. It is sometimes unavoidable for these decision-makers to make decisions that, in order to improve economic efficiency, are deemed unfair and thus unpopular by those affected by the decisions. Such examples include taking welfare benefits from certain groups in society to balance the government's budget or cutting employee benefits to increase company profits.

When an unpopular decision is made in a committee, those affected by the decision (henceforth, recipients) are often uninformed, and thus unsure, about exactly which individual committee members were for or against the decision. In such cases, the individual committee members share the responsibility of the committee's decision since the recipients do not know who to blame. This stands in contrast to situations in which a single person is appointed to represent the committee and make a decision on their behalf. For instance, a labor union often appoints a single representative to engage in collective bargaining and a government is also often represented by a single politician in international negotiations. In these situations, the recipients (e.g. union members and citizens, respectively) can more clearly assign responsibility for the decision-making.

A number of studies have found that if decision-makers are solely responsible for their decisions, they are less willing to make unfair decisions (i.e. implement unequal allocations of money) when they can be held responsible and punished by the recipients than when they cannot (Coffman, 2011; Bartling and Fischbacher, 2012).[1] Because many recipients do in fact punish the responsible decision-makers for making unpopular decisions[2], the individual decision-makers may often prefer to delegate such decision-making to a third-party who will then make the unfair decisions and take the blame on their

[1] According to the literature, the more responsible a decision-maker is for others' welfare, the more prosocial and less self-interested decisions does he or she make. This seems to some extent to hold even when the decision-maker cannot be held responsible and punished. See Charness (2000) and Charness et al. (2012) for evidence from employer-employee relations as well as Gneezy et al. (2010) and Cryder and Loewenstein (2012) for giving to strangers and charities.

[2] See Coffman (2011), Bartling and Fischbacher (2012), and Duch et al. (2013) for punishments in the responsibility attribution context. See also evidence of punishments of unfair behavior from other contexts such as Ultimatum Games (e.g. Oosterbeek et al., 2004), Public Goods Games (e.g. Fehr and Gächter, 2000, 2002; Herrmann et al., 2008), and Dictator Games (e.g. Houser and Xiao, 2010).

behalf (see also Hamman et al., 2010). In companies, for instance, boards of directors often hire third-party consultants when profitable, but unpopular decisions such as cutting staff size are necessary in order to alleviate the board members' responsibility for these decisions. And in politics, third-party expert panels are frequently appointed to make the seemingly unavoidable and unpopular policy recommendations. To the best of our knowledge, however, it has not been studied how committees' decision-making compare to to representatives' when responsibility can be attributed. Whereas delegation concerns pushing unpopular decision-making on to a third-party, the question of decision-making by committees *versus* representatives of committees concerns how the committee organizes itself when it needs to deal with unpopular decision-making.

A recent literature has compared decisions made in committees (see Charness and Sutter, 2012 and Kugler et al., 2012 for meta-studies) and by representatives (Song, 2008) to decisions made by single individuals, who only represent themselves. These studies have generally found that both committees and representatives make more rational and self-interested decisions than individuals do.[3] It has, however, not been studied to what extent committees make more or less self-interested decisions than representatives.

In this chapter, we ask: Does the shared responsibility among committee members as compared to representatives breed unfairness? We test this issue in a Group Dictator Game experiment in which we vary the decision-making process in two treatments and allow the recipients to hold the decision-makers responsible by punishing them at a constant marginal cost. The experiment was conducted online in 2011 using a large and heterogenous sample of the Danish adult population which, as compared to standard university student samples which are most commonly used in economic experiments, adds generalizability to our study since young people including university students appear to be more selfish than the average adult (Engel, 2011; Falk et al., 2013).

We randomly matched our subjects into groups of six with three of them being decision-makers and the other three being recipients. The task of the decision-makers was to pass or fail each of four exogenously given proposals about the distribution of a fixed money sum between themselves and the recipients. The proposals varied in their

[3]The results from the literature on less strategic games such as the Group Dictator Game are mixed, however. Cason and Mui (1997) found groups to be more generous than individuals, Franzen and Pointner (2013a) found no difference (see also Bischoff and Krauskopf, 2013 for similar results when comparing individuals' and groups' giving to charities), and Luhan et al. (2009) found groups to be more selfish than individuals.

distributional fairness, but they were all unpopular in the sense that they were unfair towards the recipients by giving each recipient less than each decision-maker.[4] In one treatment ("Committee"), the three decision-makers constituted a committee that made decisions by majority rule. Each individual committee member had to vote yes or no to each proposal and the proposal would pass if it received at least two yes-votes. Otherwise, the proposal failed in which case both decision-makers and recipients earned nothing. In the other treatment ("Representative"), one of the decision-makers was randomly chosen to represent the three decision-makers and solely decide whether or not to pass the proposals.

The recipients could punish the decision-makers individually, both if a proposal was passed and if it was failed. Importantly for our study, we did not inform the recipients about which committee members had voted yes or no to the proposals in the Committee treatment and the non-transparent decision process thus made the recipients unaware of who were responsible for the committee's decision. In the Representative treatment, however, the decision process was fully transparent as the recipients were informed about exactly which decision-maker had been appointed representative and, hence, was responsible for whether or not a proposal was passed.

We find that committees in our experiment were more likely to pass the most unfair proposal than representatives were, but for the three fairer proposals we cannot detect a significant difference. Moreover, we find that both committees and representatives became increasingly unwilling to pass the proposals, the more unfair they were. Among the recipients, we find that more than 90% in either treatment were willing to punish even though it was costly. The observed punishment pattern indicates that the recipients punished in order to avoid disadvantageous inequality (e.g. Fehr and Schmidt, 1999). Also, we find no difference in the proportions of yes-votes across the two treatments which suggests that committees were likely to pass the most unfair proposal than representatives because of the change in the decision rule across the two treatments.[5] By using data about the decision-makers' expected punishments, we find that most decision-makers in

[4]We denote unequal money splits unfair as there seems to exist a norm of splitting equally in situations where no involved party is more entitled to the money than another (e.g. Andreoni and Bernheim, 2009).

[5]When the majority rule is applied and when a committee member votes in favor of a proposal with probability x, then the probability that the committee will pass the proposal is greater (less) than x for $0.5 < x < 1$ ($0 < x < 0.5$). The reason is that only a majority, and not all, of the committee members need to vote yes (no) to pass (fail) a proposal. This follows from Condorcet's jury theorem.

both treatments voted in order to maximize their own payoffs, but overestimated the punishments from the recipients. They thus often failed the proposals by mistake when they should have passed them.

We generally find that the decision-makers' voting behavior is unrelated to their political orientation[6] as well as how generous they have proven to be in a standard Dictator Game. Similarly, the recipients' punishments appear to be unrelated to their political orientation which indicates that recipients of any political conviction feel equally offended when being unfairly treated.

Our findings suggest that when an efficient but unpopular decision must be made and when the decision-makers can be held responsible for their decisions, appointing a committee, in which the individual members share the responsibility, instead of a representative will lead to more efficient, but also unfair outcomes.

6.2 Recruitment, design, and procedure

6.2.1 Recruitment

Our experiment was conducted online in June and July 2011. It was organized by the Internet Laboratory for Experimental Economics (iLEE) at the University of Copenhagen and it was part of the fourth wave of iLEE experiments (henceforth, iLEE4). Our subjects were recruited by Statistics Denmark from the broad Danish adult population. In 2007, Statistics Denmark had created a random sample of the Danish adult population which consisted of 40,000 individuals who were between 18 and 80 years old at the time. In 2008, a random selection of 22,027 of these were invited to participate in a first wave of experiments (iLEE1), which 2,291 of them completed.[7] In 2011, we asked Statistics Denmark to invite all these 2,291 individuals to participate in the fourth wave of internet experiments (iLEE4) which included ours.[8] Printed invitation letters were sent on June 14 and reminder letters to non-respondents were sent on June 28 (see Appendix E, Section E.1). The wave was open for participation until July 31.

By conducting the experiment on the internet and by collaborating with Statis-

[6] A study by Dawes et al. (2012) has found that right-wing voters are less generous in a Dictator Game experiment in which efficiency, as compared to our study, was not a concern.
[7] See Thöni et al. (2012) for a detailed description of this wave and its recruitment procedure.
[8] A second and a third wave had been conducted in 2009 and 2010, respectively.

tics Denmark, we were able to ensure both subject-subject and subject-experimenter anonymity. Subject-subject anonymity has been ensured as the subjects participated remotely and thus never met. Subject-experimenter anonymity has been ensured as Statistics Denmark handled all formal correspondence between us as experimenters and the subjects. We do not know the name or identity of the subjects. Instead, we identify them by a unique and random six-digit ID number which was created by Statistics Denmark. Only Statistics Denmark have the key to this number. We can use the ID number to, among other things, match the subjects' choices in the experiments with high-quality, individual-level data from the Danish population registers and match their choices in one experiment and wave with choices made in another experiment and/or wave.

A total of 942 subjects responded to our invitation mails by logging into the iLEE4 website. 689 of these completed the entire wave which consisted of six incentivized experiments, one non-incentivized experiment, and a questionnaire. The median subject spent 59.5 minutes on completing the wave and the average subject was paid 366 DKK.[9] Payments were carried out via electronic bank transfers about two weeks after the end of the wave.

6.2.2 Design

In this chapter, we study a novel Group Dictator Game experiment[10] which we designed to test if shared responsibility breeds unfairness. We matched our subjects into groups of six with three of them being decision-makers and the other three being recipients. The three decision-makers had to pass or fail each of four exogenously given proposals (see Table 6.1) that concerned the distribution of a fixed surplus of 180 DKK between themselves and the three recipients. If a proposal was passed, the money sum would be distributed as stated in the proposal. If it was failed, both decision-makers and recipients earned 0 DKK. The proposals varied in their distributional fairness, but they were all unfair towards the recipients in the sense that each recipient received a smaller share of the 180 DKK than each decision-maker if the proposals were passed. Whereas all four proposals were Kaldor-Hicks efficient if they were passed, only Proposals 1 and 2 were Pareto efficient. We made Proposals 3 and 4 violate Pareto efficiency in order to make the

[9] 1 USD $\approx$ 5.5 DKK.

[10] See Cason and Mui (1997), Luhan et al. (2009), Franzen and Pointner (2013a), and Duch et al. (2013) for other variations of the Group Dictator Game.

unfairness even more salient. The inequality between a decision-maker and a recipient increased from 6 DKK if the least unfair proposal, Proposal 1, was passed to 114 DKK for the most unfair proposal, Proposal 4.

We gave the recipients the opportunity to punish the decision-makers at a cost, both if a proposal was passed and if it was failed. This allowed the recipients to hold the decision-makers responsible for their decisions. For practical reasons, there was no feedback during our experiment. We thus elicited punishments via the strategy method.[11] Also, the subjects could not communicate with each other.

Table 6.1: Proposals and payoffs in DKK per subject

	If passed		If failed	
Proposal	Decision-maker	Recipient	Decision-maker	Recipient
1	33	27	0	0
2	57	3	0	0
3	63	-3	0	0
4	87	-27	0	0

Decision-makers' task

To answer if shared responsibility results in more unfair outcomes, we implemented two treatments: "Committee" and "Representative". In the Committee treatment, the three decision-makers constituted a committee that had to pass or fail each of the four proposals. The three committee members had to individually vote yes or no to each proposal and a proposal would pass if it had received at least two yes-votes. The three decision-makers in a group were identified by a number (DM1, DM2, or DM3) which remained the same throughout the experiment, but they were otherwise anonymous to each other, to the recipients, and to us as experimenters. Importantly for our study, the recipients were not informed about which committee members had voted yes or no. They were only informed, via the strategy method, whether the proposal was passed or not. Hence, the three committee members shared the responsibility of the committee's decision to pass or

[11] Even though there is usually not found a difference in choices elicited using the strategy method as compared to direct responses (see Brandts and Charness, 2011 for a review), two studies by Brandts and Charness (2003) and Brosig et al. (2003) found punishments to be harsher with direct responses than when elicited using the strategy method.

fail the proposals independent of their individual votes. Also, the decision-makers could not observe each others' votes.

In the Representative treatment, a single decision-maker (DM1) represented the three decision-makers and the representative's vote alone dictated whether a proposal would pass or fail. Both the represented decision-makers (DM2 and DM3) and the recipients were informed that DM1 was solely responsible for passing or failing a proposal.

Recipients' task

The recipients were allowed to punish the decision-makers individually at a cost, both if a proposal was passed and if it was failed. We informed the recipients in detail about each proposal before they submitted their punishments to that proposal. Each recipient could punish the three decision-makers by up to 40 DKK in total for every outcome (i.e. pass or fail) to every proposal, but the recipient was free to punish some decision-maker more than another. The recipients faced a constant marginal cost of 0.1 DKK per 1 DKK of punishment assigned such that the recipient's maximal cost of punishing was 4 DKK.

Since they punished the decision-makers individually, a decision-maker could be punished by up to 120 DKK if he or she received the maximal punishment from all three recipients. In that case, the two other decision-makers were punished by 0 DKK.

6.2.3 Procedure

When the subjects entered the experiment, they went through a sequence of six screens with written instructions, before they were asked five control questions about the experiment (see translated screens in Appendix E, Section E.2). All subjects were required to submit correct answers to the control questions before they could proceed to the decision phase of the experiment. They were allowed as many attempts as they wanted and they could always - both on the screen with control questions and on the later decision screens - click a button to re-read the instructions.

When the subjects passed the screen with control questions, we sequentially allocated them into treatments (i.e. Committee or Representative), roles (i.e. decision-maker or recipient), and groups. The subjects who were assigned the role of a decision-maker were also informed whether they were DM1, DM2, or DM3. We informed both decision-makers and recipients about whether the three decision-maker in their group made collective

decisions (Committee treatment) or if one decision-maker was appointed representative (Representative treatment).[12]

The subjects then proceeded to the decision screens. The recipients saw one decision screen per proposal, while the decision-makers saw two. On the first screen, we informed the decision-makers about the proposal and asked them if they expected the proposal to pass and how much punishment they expected to receive, both if the proposal passed and if it failed. The elicitation of expectations was not incentivized. On the second screen, they had to submit their vote to the proposal. The four proposals appeared in randomized order in the experiment and there were no time limitations enforced on any screen during the experiment.

To determine the subjects' payments, we randomly picked one proposal for each group. Every decision-maker in the group was paid according to the outcome of that proposal (see Table 6.1) minus the punishment he or she received from the recipients. Each recipient was also paid according to the outcome of that proposal minus the cost of punishing the decision-makers. If some groups were incomplete when we calculated earnings[13], we replaced the missing subjects by fictitious, rational players. Missing recipients were thus replaced by fictitious recipients, who never punished since punishing was costly, and missing decision-makers were replaced by fictitious decision-makers, who thus voted yes to all proposals. We did not inform the subjects about this and their decisions were thus unaffected by this way of handling incomplete groups.

A total of 737 subjects completed our experiment. Since the subjects participated remotely at our virtual lab, we cannot be certain that the invited adults from the broad Danish population were those who in fact participated in the experiment. We therefore validate the participants by comparing each participant's self-reported age and gender to the invitee's age and gender according to the Danish population registers. By doing this, we can validate 717 of the 737 subjects. These 717 subjects will constitute our study

[12]When conducting the experiment, we framed it in a political context by designating the decision-makers "parties" and the recipients "voters" in order to make it more easily interpretable to the subjects who were recruited from the broad Danish adult population. In the Committee treatment, we told the subjects that each party had 20 seats in a parliament that consisted of 60 seats. In the Representative treatment, however, we told them that one party ("party 1") had 40 seats, whereas the others had 10 seats each. A proposal would pass if it was backed by at least 31 seats. We enforced strict party discipline which means that if a party voted yes to a proposal then it would get backed by all of the party's (10, 20, or 40) seats.

[13]This would be the case if the group had not been complete from the beginning or if one or more group members had exited the iLEE4 wave after being assigned a role, treatment, and group in our experiment, but before completing the entire wave.

sample.

Table 6.2 shows how our study sample was distributed across treatments and roles. As is seen, the study sample is almost perfectly balanced. The average subject was 48.6 years old on January 1, 2011 (σ = 15.5) and 47.7% of them were women. See more descriptive statistics in Appendix E, Section E.3.

Eventually, 689 subjects completed the entire iLEE4 wave and were eligible for payments. These subjects earned 16 DKK on average (min. -60 DKK; max. 87 DKK) in our experiment and the median subject spent 14 minutes on completing it.

Table 6.2: Distributions of subjects across treatments and roles

	Treatment	
Role	Committee	Representative
Decision-maker	180	174
- *DM1*	*60*	*56*
- *DM2*	*60*	*58*
- *DM3*	*60*	*60*
Recipient	175	185
Total	355	359

Note: The table presents the distribution of the 717 validated subjects who completed the experiment.

6.3 Hypothesis

Because punishing was costly and because there was no feedback in the experiment, rational and self-interested recipients should not punish. And if the decision-makers correctly anticipate this, then rational and self-interested decision-makers should vote yes to all four proposals in which case all proposals would pass in both treatments.

However, many people tend not to act in a self-interested or rational manner when they feel unfairly treated. Instead, numerous experiments have documented that many people are willing to give up money to punish those who treat them unfairly (e.g. Fehr and Gächter, 2000, 2002; Herrmann et al., 2008).[14] In our experiment, all proposals

[14]In their study, Bartling and Fischbacher (2012) found that recipients' punishment behavior in fact indicates that they punish to assign responsibility more than because of unfair outcomes or selfish intentions among decision-makers.

were unfair towards the recipients if passed, because they created inequality that was disadvantageous to these. Hence, we should expect a positive fraction of the recipients to punish the decision-makers if the proposals were passed.

We will assume that a fraction $\alpha_{y,k} \in [0,1]$ of the recipients are willing to punish if proposal k is passed ($y = 1$) or failed ($y = 0$), where $k \in \{1,2,3,4\}$. This means that the average group has $3 \cdot \alpha_{y,k}$ punishing recipients for proposal k and outcome y. The average punishing recipient punishes by $0 < p_{y,k} \leqslant 40$ independently of the other recipients' punishments such that the total punishment assigned in the average group is $P_{y,k} = 3 \cdot \alpha_{y,k} \cdot p_{y,k}$ and the average punishment given per recipient is $\bar{p}_{y,k} = P_{y,k}/3 = \alpha_{y,k} \cdot p_{y,k}$. Moreover, we will assume that the decision-makers hold correct beliefs about $\alpha_{y,k}$, $p_{y,k}$, $P_{y,k}$, and $\bar{p}_{y,k}$ and that the magnitudes of these are the same across the two treatments.

Since the recipients cannot target their punishments in the Committee treatment, we assume that they will punish each committee member equally much which means that the average committee member receives a punishment of $\rho^C_{y,k} = P_{y,k}/3 = \bar{p}_{y,k}$. In the Representative treatment, however, the recipients are able to target their punishments at the responsible representative (DM1) who will then receive a three times greater punishment, $\rho^R_{y,k} = P_{y,k} = 3 \cdot \rho^C_{y,k}$ (see Bartling and Fischbacher, 2012 and Duch et al., 2013 for suggestive evidence).

Now, a self-interested representative should vote yes to a proposal if he or she believes that the payoff from passing that proposal is higher than from failing it, $X_{1,k} - \rho^R_{1,k} \geqslant X_{0,k} - \rho^R_{0,k}$, where $X_{y,k}$ is each decision-maker's allocation of the money sum according to proposal k and outcome y. Since $X_{0,k} = 0$ for all k, we can rewrite the inequality such that the representative should pass the proposal whenever $X_{1,k} \geqslant \rho^R_{1,k} - \rho^R_{0,k}$. When the representative follows this strategy, he or she *best-responds*.

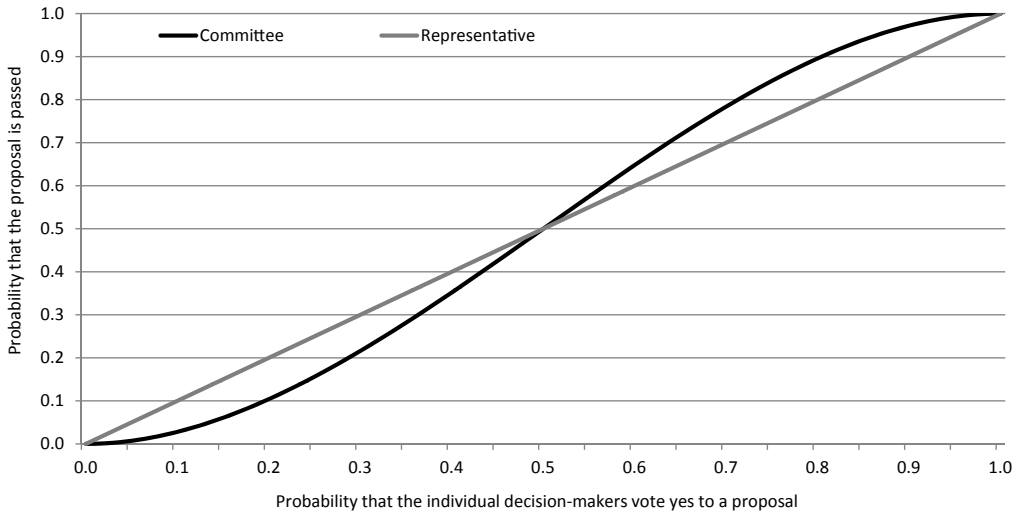
In the Committee treatment, a committee member should vote yes whenever $X_{1,k} \geqslant (\rho^C_{1,k} - \rho^C_{0,k})$. Because the difference $\rho^C_{1,k} - \rho^C_{0,k}$ is just one third of the difference $\rho^R_{1,k} - \rho^R_{0,k}$ given that $\rho^C_{y,k} = \rho^R_{y,k}/3$, committee members should vote yes more often than representatives because the individual incentives for passing the proposals are higher. This should lead to the unfair policies being implemented more often in the Committee treatment than in the Representative treatment. We can test this hypothesis.

Hypothesis: Unfair proposals are passed more often by committees than by

representatives.

There are, however, factors that could affect the chance of committee's implementing the unfair policies. First, recall that the probability that a proposal will pass in the Representative treatment is simply x if the probability that representative will vote yes is x. If the probability that a committee member votes yes is also x, however, the probability that the proposal in the Committee treatment will pass is then greater (less) than x for $0.5 < x < 1$ ($0 < x < 0.5$), because it required only two yes-votes out of three. This is an *aggregation effect* that follows from Condorcet's jury theorem.[15] See Figure 6.1 for a visualization of the aggregation effect when the committee, as in our case, has three members.

Figure 6.1: How individual voting corresponds to the decision to pass a proposal



Second, since it required only two yes-votes to pass a proposal, not all committee members could expect to be pivotal in their group. If two committee members had both voted yes (no), then the proposal would pass (fail) independently of what the third

[15]It is important to note here that our treatments vary both the level of transparency of the decision-making and the decision rule. Our virtual lab setting did not allow us change one without changing the other, too. For instance, increasing transparency without changing the decision rule would require us to provide information about the committee members' votes to the recipients which would require real-time interaction in the experiment. This was not applicable, however. Also, we could not avoid the aggregation effect in the committee's decision-making. Other attempts to do so include allowing communication between the committee members to force them to arrive at a common decision (Cason and Mui, 1997; Luhan et al., 2009; Franzen and Pointner, 2013a) which, again, would require real-time interaction. Bischoff and Krauskopf (2013) allowed their committee members to make endogenous offers on how to divide a monetary surplus and then let the median offer among the committee members determine the committee's decision. This would, however, mean that we could not easily investigate decision-making on (exogenously given) efficient and unfair policies as we do in this chapter.

committee member had voted. If a committee member expects to be pivotal for the committee's decision to proposal k with some probability $\gamma_k < 1$, then his or her incentives to vote yes (or best-respond) are lower than if $\gamma_k = 1$, because it is of lower cost to make errors or vote expressively in favor of a fairer, but less efficient outcome (e.g. Tyran, 2004; Feddersen et al., 2009; Shayo and Harel, 2012). This is a *decision-maker incentive effect* and it might plausibly reduce the probability that the individual committee members vote yes and, hence, also reduce the probability that a committee, relative to a representative, passes a proposal.

Third, the recipients might condition their own punishments on how much they expect the other recipients to punish. If a recipient, for instance, believes that the two other recipients will punish the representative or the committee members enough to deter them from passing some unfair policy, then he or she can simply just free ride on their punishments. Increasing the punishment is then wasteful as it will not have an effect on the outcome, anyway. Further, if a recipient expects the other two recipients not to punish, then he or she alone would not necessarily be able to punish enough to deter the decision-maker(s) from passing a proposal, in which case punishing will also be wasteful. A third possibility is that the recipient thinks that increasing the punishment just a little bit will be enough to deter the decision-makers from passing some unfair policy. As can be seen, these *recipient incentive effects* can have a mixed impact on the recipients' punishments and on the decision-makers' behavior, too, dependent on their anticipation of the effects.

We assumed in the beginning of this section that the average committee member receives exactly one-third of the punishment that each representative receives which gives relatively higher incentives for the committee members to pass the proposals. However, if the recipients punish in order to deter the decision-makers from passing certain policies, and not just to hold them responsible for their decisions, then they should punish more in the Committee treatment as compared to the Representative treatment which should, in the first place, reduce the probability that the committees pass the proposals. This is also a recipient incentive effect and, if anticipated by the committee members, it could reduce the likelihood that committee members vote in favor of the policies and also reduce the probability that committees will ensure more unfair outcomes.

To sum up, the aggregation effect enhances (reduces) the anticipated treatment effect

when the probability that the representatives and committee members vote yes is high (low) enough. However, decision-makers and recipients might react to incentives which can have mixed effects on both votes, punishments, and outcomes in the two treatments. In what follows, we will concentrate mostly on outcomes and then discuss individual behavior and motives to the extent feasible in our setting.

6.4 Results

This section is organized as follows. In Section 6.4.1, we show that committees were more likely than representatives to implement the most unfair policy, whereas there is no difference for the fairer policies. We also show that the proposals in both treatments were less likely to pass, the more unfair they were.

Section 6.4.2 shows that more than 90% of the recipients punished at least once in the experiment. The punishment pattern is most consistent with the recipients punishing in order to avoid disadvantageous inequality. It is also found that punishments are not related to political orientation. In Section 6.4.3, we find that the individual committee members and representatives were equally likely to vote yes to the proposals which indicates that committees were more likely than representatives to implement the most unfair policy due to an aggregation effect. We also find suggestive evidence of the decision-maker incentive effect as committee members were (weakly) less likely to best-respond than representatives. We find that neither committee members' nor representatives' voting behavior appear to be related to their political conviction or their generosity expressed in a Dictator Game. Finally, the reason why both committees and representatives were more likely to block proposals, the more unfair they were is that they had exaggerated beliefs of the recipients' punishments. Especially the committees overestimated their actual punishments which can also explain why we do not observe a difference in voting across the two treatments.

6.4.1 Does shared responsibility breed unfairness?

In order to answer if shared responsibility breeds unfairness, we compare the fractions of committees and representatives that passed each of the four proposals. To calculate this fraction of committees, we randomly form committees *ex post* consisting of three

decision-makers (one DM1, one DM2, and one DM3) and then calculate the number of yes-votes to determine if the proposal was passed or not. We can do this *ex post* matching, as there was no feedback to influence the decision-makers' votes during the experiment. The *ex ante* group assignment which was mentioned in Section 6.2.3 only served payment purposes. Since the 180 decision-makers in the Committee treatment were evenly distributed across roles (i.e. 60 DM1's, 60 DM2's, and 60 DM3's), we are able to match them into 60 random committees *ex post*. We then simply calculate the fraction of these 60 committees that passed each proposal. In order to avoid that our conclusions rely on some particular, unlikely *ex post* assignment of decision-makers into committees, we use bootstrapping methods to repeat this procedure 500 times in order to calculate a mean fraction of committees that passed each proposal. The standard errors of the means are of magnitude 0.0007-0.0015.

In Figure 6.2, we show the fractions of committees and representatives that passed the proposals. The pass rates in the Committee (Representative) treatment were 97.5% (98.2%) for Proposal 1, 91.9% (85.7%) for Proposal 2, 75.6% (73.2%) for Proposal 3, and 72.7% (55.4%) for Proposal 4. We use two-sided Fisher's exact tests to pairwise test if the pass rates for every proposal were equivalent across treatments. We cannot reject that this is the case for Proposals 1-3[16], but for the most unfair proposal, Proposal 4, we find a significant difference suggesting that committees passed the proposal more often than representatives.[17] Hence, shared responsibility breeds unfairness, but only when the unfairness is very pronounced.

The figure also suggests that both committees and representatives became increasingly likely to fail the proposals, the more unfair they were. We use two-sided Fisher's exact tests to check this observation by comparing pass rates across proposals. In the Committee treatment, we find significant differences between all neighboring proposals.[18] In the Representative treatment, we find a significant difference in pass rates for Proposals 1 and 2, but due to the weaker statistical power in the Representative treatment we find no significant difference for the other neighboring proposals.[19] If we instead compare

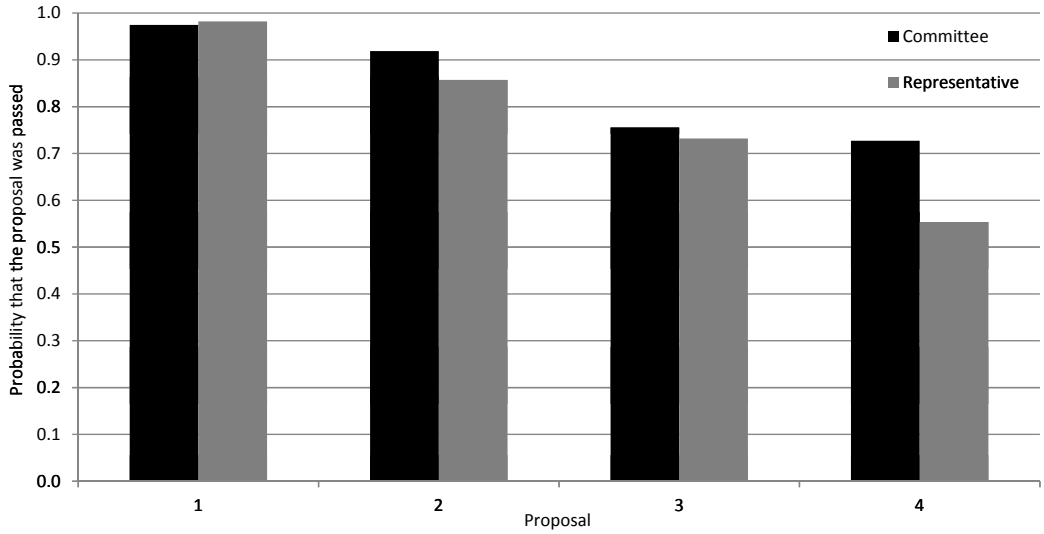
[16]This is in line with Bischoff and Krauskopf (2013) and Franzen and Pointner (2013a) who found that committees on average are neither more nor less selfish than individuals.

[17]Two-sided Fisher's exact tests. Proposal 1: $p = 1.000$. Proposal 2: $p = 0.133$. Proposal 3: $p = 0.644$. Proposal 4: $p = 0.006$.

[18]Two-sided Fisher's exact tests for the Committee treatment. Proposal 1 vs. 2: $p < 0.001$. Proposal 2 vs. 3: $p < 0.001$. Proposal 3 vs. 4: $p < 0.001$.

[19]Two-sided Fisher's exact tests for the Representative treatment. Proposal 1 vs. 2: $p = 0.032$. Proposal

Figure 6.2: Fraction of committees and representatives that passed each proposal



Note: For the Committee treatment, we report outcomes from 30,000 committees we have created at random. For the Representative treatment, we report the fraction of representatives who voted yes.

pass rates of second nearest-neighbors, we find significant differences with $p < 0.001$.

6.4.2 Recipients' punishment behavior

The vast majority of recipients were willing to punish the decision-makers. In fact, almost all the recipients in the experiment chose to punish at least once, even though it was costly. In Committee, 90.3% of the recipients punished and in Representative, 93.0%. These two proportions are not significantly different according to a two-sided Fisher's exact test ($p = 0.446$).

Figures 6.3 and 6.4 show that punishments were systematically harsher the more unfair the passed proposals were. In Representative, the punishments tripled from the least to the most unfair proposal and in Committee, the increase was even greater.

On average, the recipients punished the three committee members by 5.6 DKK in total if Proposal 1 was passed and by 16.2 DKK if it was failed. For the three other proposals, the recipients instead punished the decision-makers more if the proposals were passed than if they were failed. All these differences are statistically significant.[20] Figure 6.4 shows that the punishment pattern was very similar in the Representative treatment:

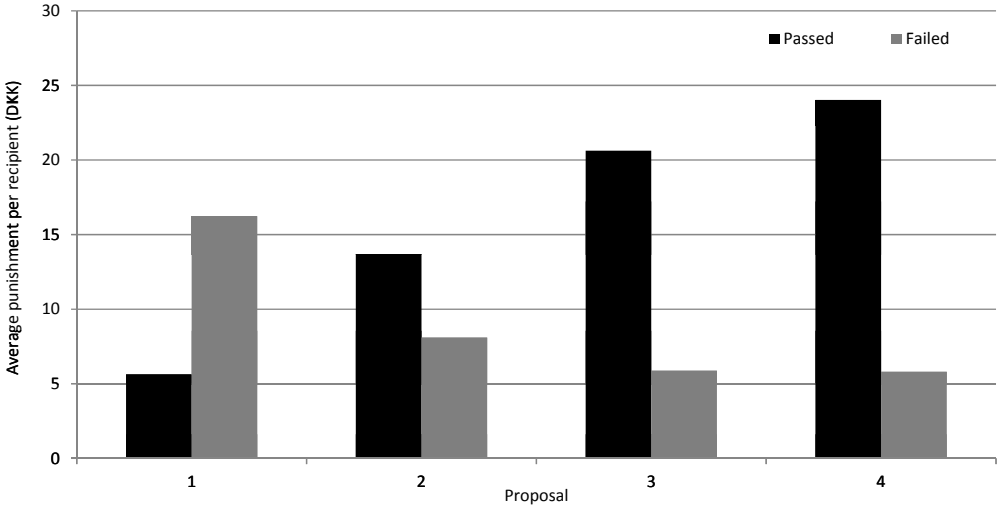
2 vs. 3: $p = 0.160$. Proposal 3 vs. 4: $p = 0.075$.

[20]Two-sided t-tests for Committee treatment. Proposal 1: $p < 0.001$. Proposal 2: $p = 0.004$. Proposal 3: $p < 0.001$. Proposal 4: $p < 0.001$.

The recipients punished more if Proposal 1 was failed than if it was passed, but for the three other proposals they punished relatively more if the proposals were passed than if they were failed.[21]

We also note, by comparing the height of the bars across the two figures, that the recipients appear to have punished more in the Representative treatment than in the Committee treatment (Appendix E, Table E.2). Some of the punishments in the Representative treatment were, however, targeted the represented decision-makers (DM2 and DM3). If we discard punishments targeted those, we find no difference in how much committees and representatives were punished in six out of eight situations (i.e. four proposals times two outcomes). In the remaining two situations, the representatives were punished significantly less than the three committee members were in total (Appendix E, Table E.3). For instance, the average committee member was punished significantly more than the average representative if Proposal 4 was passed which provides a weak indication that the recipients punished to deter the committee members from passing the unfair proposals (see the discussion in Section 6.3). Across all eight situations, the average representative was punished approximately 2-3 times more than the average committee member.

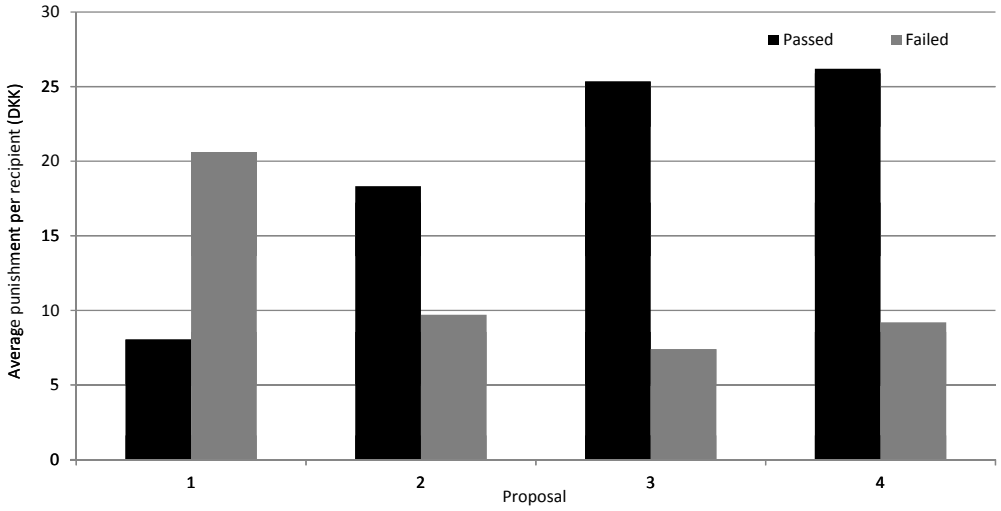
Figure 6.3: Recipients' average total punishments, Committee treatment



In order to rationalize the recipients' punishments, we make use of the fact that their payoffs were symmetric around zero (27, 3, −3, and −27 DKK) when the proposals were

[21] Two-sided t-tests for Representative treatment. Proposal 1: $p < 0.001$. Proposal 2: $p < 0.001$. Proposal 3: $p < 0.001$. Proposal 4: $p < 0.001$.

Figure 6.4: Recipients' average total punishments, Representative treatment



passed. If the recipients cared about their own monetary payoffs, we should expect them to have punished equally hard if Proposal 1 was failed as if Proposal 4 was passed, because these outcomes would result in the same forgone payoff[22] to the recipients, namely 27 DKK. If they rather cared about avoiding disadvantageous inequality (e.g. Fehr and Schmidt, 1999), say, we would instead expect them to punish harder if Proposal 4 was passed than if Proposal 1 was failed, since passing Proposal 4 would create great inequalities which failing Proposal 1 would not.

Another possibility is that they were loss averse (e.g. Kahneman and Tversky, 1979). In that case, they would dislike receiving a loss of 27 DKK (i.e. if Proposal 4 passed) relatively more than not receiving a gain of 27 DKK (i.e. if Proposal 1 failed), because a loss of 27 DKK hurts more than a gain of 27 DKK is pleasurable when comparing these two to the status quo of receiving 0 DKK. They should thus punish more if Proposal 4 passed than if Proposal 1 failed. Likewise, loss averse recipients should punish more if Proposal 3 passed than if Proposal 2 failed. Because all loss averse recipients should enjoy being given a positive amount of money as compared to the status quo of 0 DKK and also enjoy the status quo more than losing money, loss averse recipients should not punish if Proposals 1 and 2 passed or if Proposals 3 and 4 failed.[23]

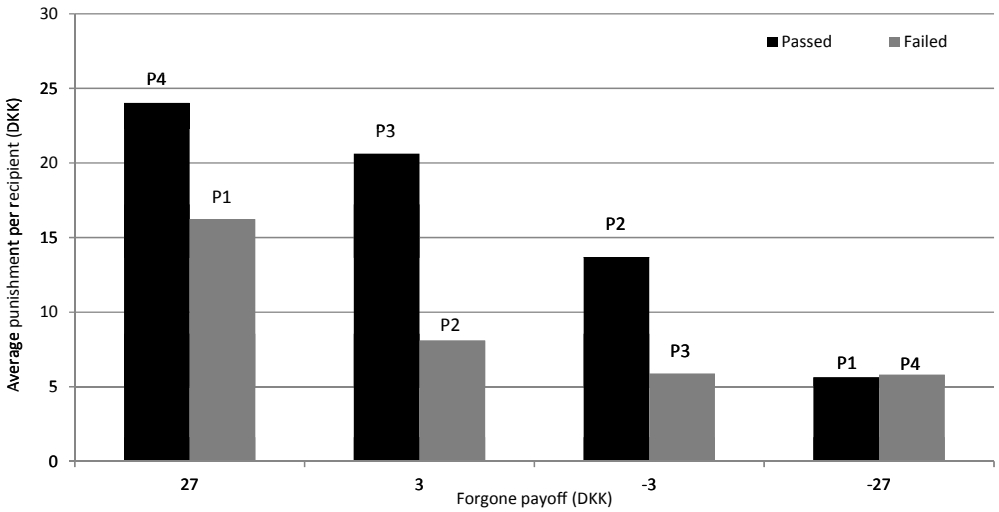
[22]By a forgone payoff, we mean the payoff that each recipient would have had if the decision-makers had failed (passed) a proposal minus the payoff they receive when the proposal is passed (failed). For instance, if Proposal 1 is failed, then the recipient has a forgone payoff of $27 - 0 = 27$ DKK, because the recipient receives 27 DKK if the proposal is passed, but 0 DKK if it is failed.

[23]Not receiving a loss of 27 DKK (3 DKK) is, however, more pleasurable than receiving a gain of 27 DKK (3 DKK) to loss averse individuals, but this does not alter our qualitative prediction.

By comparing punishments to the same forgone payoffs in the Committee treatment (see Figure 6.5), we find that the recipients punished more aggressively if the Proposals 2, 3, and 4 were passed than if Proposals 3, 2, and 1 were failed, respectively, even though the forgone payoffs were of identical magnitude.[24] Figure 6.6 shows the same picture for the Representative treatment.[25] This observation combined with the observation that the Recipients punished more if Proposal 2 was passed which would give each Recipient 3 DKK than if it was failed which would give each Recipient 0 DKK (Figures 6.3 and 6.4), is consistent with the recipients being averse towards disadvantageous inequality. Loss aversion, however, cannot explain why the recipients punished so severely if Proposal 2 was passed, i.e. almost as much as if Proposal 1 was failed.

In Appendix E, Section E.4.3 we compare the recipients' punishments to their political orientation which they revealed in a survey at the end of the wave. We do not identify a significant link, however.

Figure 6.5: Recipients' average punishment across forgone payoffs, Committee treatment

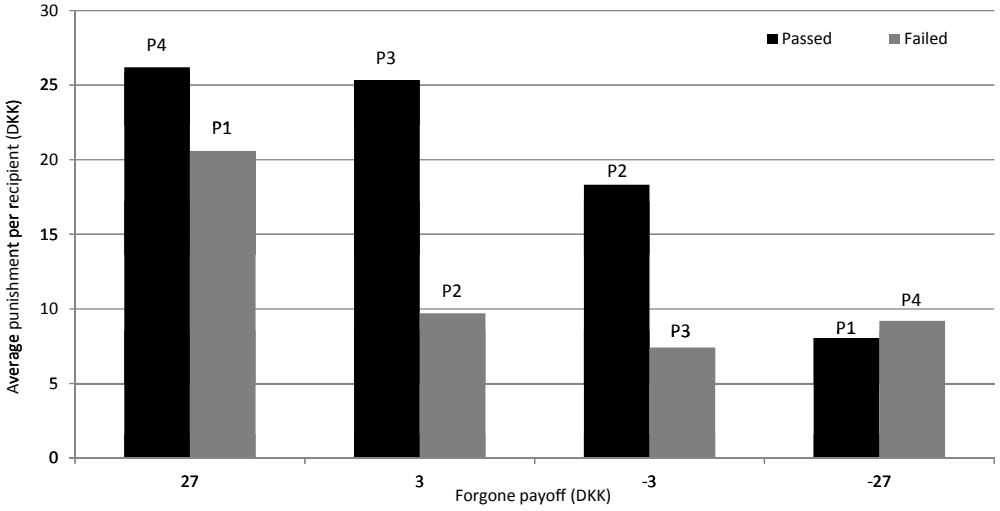


Note: "Pk" is an abbreviation for Proposal number k.

[24]Two-sided t-tests for Majority treatment. Forgone payoff of 27 DKK: $p < 0.001$. 3 DKK: $p < 0.001$. -3 DKK: $p < 0.001$. -27 DKK: $p = 0.891$.

[25]Two-sided t-tests for Representative treatment. 27 DKK: $p = 0.019$. 3 DKK: $p < 0.001$. -3 DKK: $p < 0.001$. -27 DKK: $p = 0.452$.

Figure 6.6: Recipients' average punishment across forgone payoffs, Representative treatment



Note: "Pk" is an abbreviation for Proposal number k.

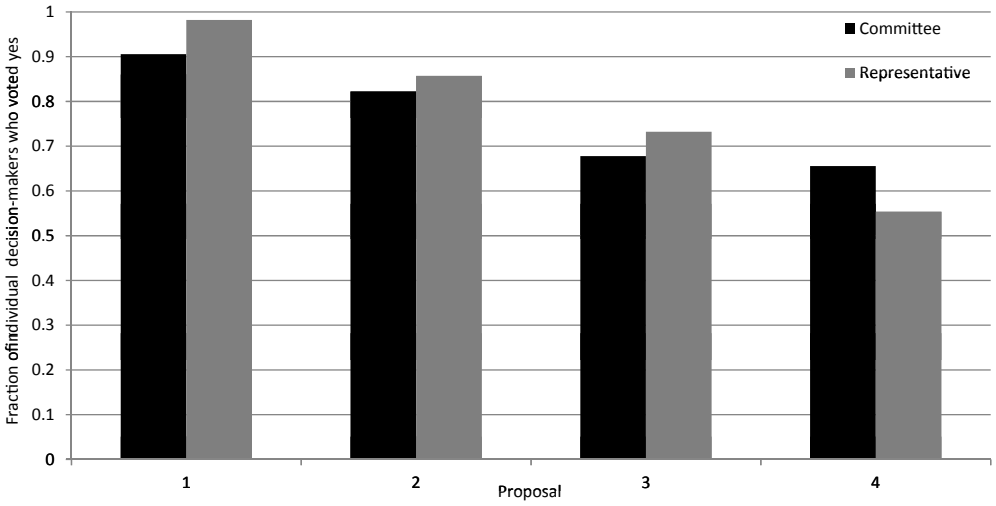
6.4.3 Understanding the Decision-Makers' voting behavior

To answer why committees more often than representatives passed the most unfair proposal, we compare the fractions of individual committee members and representatives who voted yes to each proposal (see Figure 6.7). We find that there are no significant differences in these fractions.[26] This indicates that the most unfair proposal, Proposal 4, was passed more often by committees than representatives due to the before-mentioned aggregation effect which, for the given voting probabilities, had an enhancing effect on the probability that the committees passed the proposals. As discussed in Section 6.3, however, it is possible that committee members were less likely to vote in favor of the policies, if they thought that they were not necessarily pivotal in their committee or if they expected to receive very harsh punishments.

To investigate the latter, we look at the decision-makers' expected punishments. In Figures 6.8 and 6.9, we present the average committee member's and representative's expected punishments, respectively. These figures reveal two main messages. The decision-makers in both treatments quantitatively exaggerated the recipients' punishments, but on the other hand we find that the patterns of expected punishments look very similar to the actual punishment patterns (Figures 6.3 and 6.4). The decision-makers rightfully

[26]Two-sided Fisher's exact tests. Proposal 1: $p = 0.081$. Proposal 2: $p = 0.684$. Proposal 3: $p = 0.510$. Proposal 4: $p = 0.205$.

Figure 6.7: Fraction of individual decision-makers who voted yes to each proposal



Note: For the Committee treatment, we report individual decisions for all decision-makers. For the Representative treatment, we report decisions made by DM1's only.

expected to receive greater punishments if they failed Proposal 1 than if they passed it, whereas they, also rightfully, expected greater punishments if they passed the other proposals than if they failed them.[27] Furthermore, we find that they expected higher punishments if they passed Proposals 2, 3, and 4 than if they failed Proposals 3, 2, and 1, respectively, even though the recipients' forgone payoffs were the same. The combination of these observations suggest that the decision-makers correctly anticipated the recipients to punish in a manner that is consistent with aversion towards disadvantageous inequality.

Interestingly, by comparing the height of the bars across Figures 6.8 and 6.9, we find no difference in how hard the average committee members and representatives expected to be punished in seven out of eight situations, while in the eighth situation - if Proposal 3 failed - the average committee member expected a greater punishment than the average representative (Appendix E, Table E.4). Especially the committee members' exaggerated punishments could possibly be due an expectation of the recipients punishing them relatively harsh in order to deter every one of them from voting in favor of the unfair proposals (see the discussion in Section 6.3). This could explain why we do not observe a

[27]Two-sided t-tests for expected punishments in the Committee treatment. Proposal 1: $p < 0.001$. Proposal 2: $p < 0.001$. Proposal 3: $p < 0.001$. Proposal 4: $p < 0.001$. Two-sided t-tests for expected punishments in the Representative treatment. Proposal 1: $p < 0.001$. Proposal 2: $p = 0.474$. Proposal 3: $p < 0.001$. Proposal 4: $p < 0.001$.

Figure 6.8: Committee members' average expected punishments

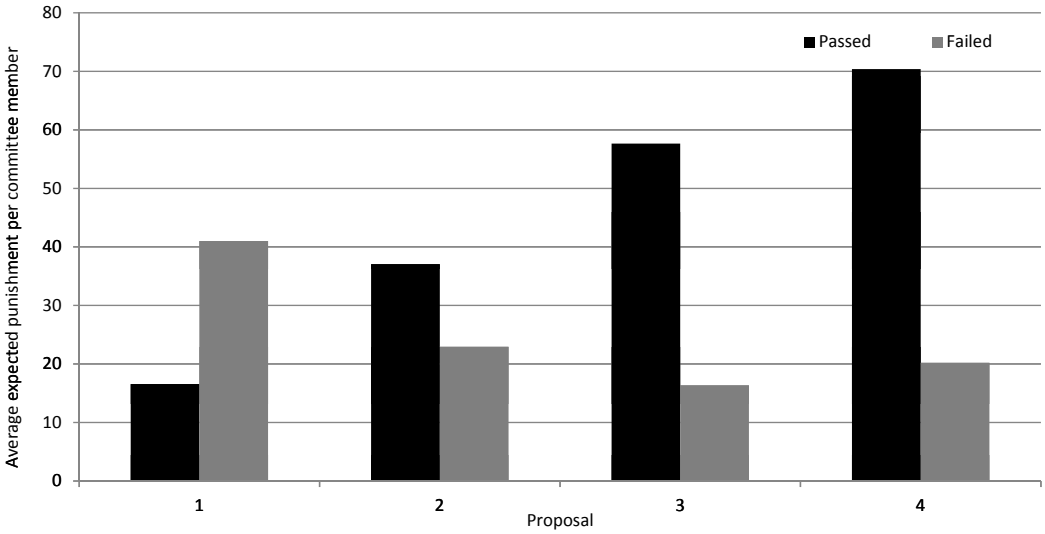
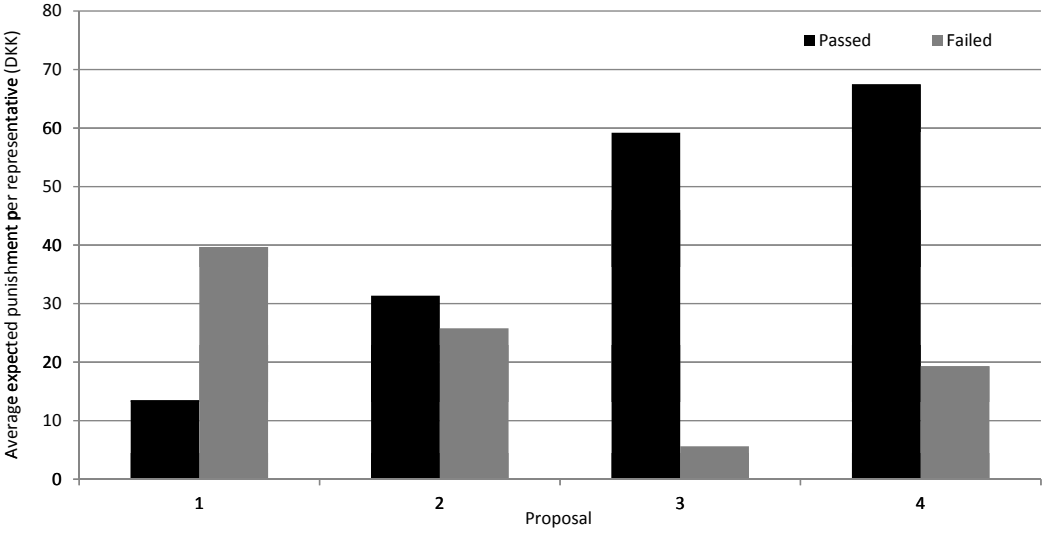


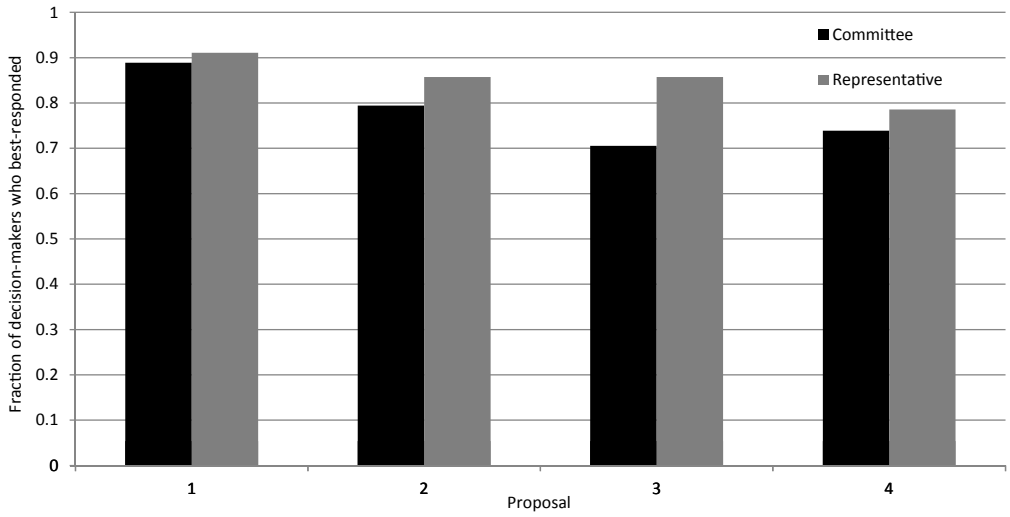
Figure 6.9: Representatives' average expected punishments



difference in the individual votes across the two treatments since the committee members and representatives, given their stated beliefs, had almost identical incentives to pass the proposals (if the committee members in fact expected to be pivotal).

Next, we investigate what motivated the decision-makers' votes. We compare the fractions of committee members and representatives who used their votes to best-respond to their expected punishments (Figure 6.10). We find that 71-91% of committee members and representatives best-responded. Also, relatively more representatives than committee members best-responded which is in line with the decision-maker incentive effect. The difference was, however, only significant for Proposal 3.[28]

Figure 6.10: Fractions of Decision-Makers who best-responded



We can think of three possible explanations of why a non-negligible fraction of the decision-makers in both treatments decided to deviate from their best-response. First, this minority of the decision-makers might have had other-regarding motives to deviate. If they were averse to advantageous inequality, say, we would expect them to vote no to the proposals independent of their expected punishments. In fact, relatively fewer of the no-voters than yes-voters best-responded (see Appendix E, Table E.5). However, aversion towards advantageous inequality cannot explain why, among the decision-makers who did not best-respond, 54-66% of the committee members and 33-87% of the representatives had voted yes and, hence, promoted inequalities. A second possible explanation is thus that many of those who failed to best-respond did not pay proper attention to the belief-

[28] Two-sided Fisher's exact tests. Proposal 1: $p = 0.805$. Proposal 2: $p = 0.336$. Proposal 3: $p = 0.024$. Proposal 4: $p = 0.597$.

elicitation part, possibly because of the lacking incentives, in which case their stated beliefs were different from their actual beliefs. If this were true, we cannot rule out that these decision-makers were in fact selfish and voted selfishly given their actual beliefs, even though they, according to our expectations data, did not best-respond. A third explanation is that these decision-makers were confused about the workings of the game. This is, however, not a very plausible explanation since all subjects who entered the decision phase of the experiment had answered five control questions correct.

We also relate the decision-makers' voting behavior to their political orientation as well as how generous they have proven to be in a standard Dictator Game, which is a simple and non-strategic setting. However, we find no robust evidence of a link between their votes on the efficient, but unpopular policies in our experiment or whether or not they best-responded on the one side, and their general political conviction or how generous they are on the other side (Appendix E, Sections E.4.4 and E.4.5).

6.4.4 Conclusion

The main result in this chapter is that committees, in which the committee members collectively share the responsibility of the committee's decision, are more likely than representatives to implement economically efficient but *highly* unfair policies. The reason for this seems to be an aggregation effect of the committee members' votes, when they decide via majority voting. When the unfairness is less pronounced, however, there is no difference. This is likely a result of relatively more exaggerated beliefs among the committee members as compared to the representatives about how severely they will be punished for implementing unfair policies. We also find that both committees and representatives frequently fail more unfair policies due to overestimated punishments. Further, in our experiment we find that more than 90% of those affected by the policies (i.e. recipients) are willing to give up a small amount of money to hold the committee members and representatives responsible for making unfair decisions and punish them accordingly, because, as the observed punishment pattern indicates, the recipients are averse to disadvantageous inequality.

When multiple decision-makers need to make a very unpopular decision to increase economic efficiency, they are weakly more likely to implement such a decision in a committee than by appointing one of them as a representative to make the decision. However,

there is a potential to investigate this matter further. For instance, how robust is our finding to changing the cost of punishing? To give an example, it seems fair to think that employees' revolts against their employers are relatively more costly than voters' "revolts" against politicians and political parties in the polling booth. Also, what if the committee's decision-making process was in fact transparent? This would serve as an intermediate between our two treatments, but it would also require feedback in the experiment which was not applicable in our experiment. Third what would happen, if the policies did not involve efficiency concerns and, hence, only fairness concerns? Or what would happen if the committee did not decide by majority rule or if the committee members did not have equally much voting power? We encourage future research to pick up these questions.

Part III

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Part IV

Appendices

Appendix A

Give and Take in Dictator Games

A.1 Sample

As referred to in footnote 4 in Chapter 2, Table A.1 presents descriptive statistics on the socioeconomics variables used in the analysis, and a comparison to a random sample of the Danish population.

Table A.1: Description and representativeness of the sample

Variable	25th percentile	Median	75th percentile	Mean
Age	40	50	61	49.3
	(1.2)	(0.7)	(0.9)	(0.5)
Age (pop.)	38	50	63	50.5
	(—)	(0.4)	(0.1)	(0.1)
Education	12.0	13.0	15.2	13.6
	(0.0)	(0.2)	(0.2)	(0.1)
Education (pop.)	10.0	13.0	14.0	12.3
	(—)	(—)	(—)	(0.1)
Female				0.48
				(0.02)
Female (pop.)				0.51
				(0.00)

Note: "Age" is measured in years at 01.01.2011 and "Education" is measured in years from 1st grade to highest level of completed or enrolled education. Standard errors of means in parentheses. The reference sample for the population statistics is a random sample of 38,581 Danes aged 21-84 provided by Statistics Denmark. The percentile standard errors are calculated using 1,000 bootstrap replications.

A.2 Regression results

In Table A.2, we present the regressions referred to in footnote 8 in the main text.

Table A.2: Regression: Transfer from dictator to recipient

	(1)	(2)	(3)	(4)	(5)
Take	−50.3***	−48.6***	−49.5***	−47.0***	−44.8***
	(6.02)	(6.19)	(8.07)	(7.82)	(6.96)
Work	1.56	2.55	2.54	2.59	2.20
	(3.04)	(3.07)	(3.08)	(3.08)	(3.06)
Work × Take	−4.70	−8.19	−8.04	−8.23	−7.32**
	(8.80)	(8.95)	(9.04)	(8.96)	(9.09)
Female × Take			1.70		
			(8.85)		
Age × Take				−3.10	
				(8.97)	
Education × Take					−8.47
					(9.16)
Female		−3.84	−4.65	−3.78	−3.69
		(4.31)	(3.16)	(4.30)	(4.31)
Age		8.20	8.16	9.72**	8.00
		(4.42)	(4.43)	(3.12)	(4.45)
Education		−5.10	−5.12	−5.16	−0.93
		(4.51)	(4.53)	(4.55)	(3.14)
Constant	29.0***	28.4***	28.9***	27.6***	26.5***
	(2.13)	(4.86)	(4.65)	(4.40)	(4.48)
Observations	430	415	415	415	415
R^2	0.254	0.266	0.266	0.266	0.268

Note: The table reports OLS regressions where the dependent variable is the amount (in DKK) that the dictator transferred to the recipient. "Take" is a dummy that has value 1 if the dictator was in treatment Take or Work-Take; "Female" is a dummy that has value 1 if the dictator is a female; "Age" is a dummy that has value 1 if the dictator is 50+ years old at 01.01.2011; "Education" is a dummy that has value 1 if the dictator has completed more than 13 years of education which equivalent to a high school degree. In the regressions in columns (2)-(5), we have excluded 15 dictators for whom we do not have register data. Robust standard errors in parentheses (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

A.3 Instructions (translated)

A.3.1 Treatment Give: Dictator

[**Screen 1**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, you have been provisionally allocated an additional **100 DKK**. The other participant has **not** been allocated these additional 100 DKK.

Your decision is a simple one: Decide what portion, if any, of these **100 DKK** to transfer to the other person. You can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given your decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from your decision.

The other person will not make any decision, but he or she has the opportunity to read the instructions we have given to you. Similarly, you can read the instructions given to the other person by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.2]

[**Screen 2**] You now have to choose how much you would like to transfer to the other participant.

How much would you like to transfer to the other participant?

- 0 DKK
- 10 DKK
- 20 DKK
- 30 DKK
- 40 DKK
- 50 DKK
- 60 DKK
- 70 DKK
- 80 DKK
- 90 DKK
- 100 DKK

A.3.2 Treatment Give: Recipient

[**Screen 1**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, the other participant has been provisionally allocated an additional **100 DKK**. You have **not** been allocated these additional 100 DKK.

The other participant has been asked to make a simple decision: Decide what portion, if any, of these **100 DKK** to transfer to you. He or she can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given the other participant's decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from his or her decision.

You will not make any decision, but you have the opportunity to read the instructions we have given to the other participant by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.1] Similarly, the other participant can read the instructions given to you.

A.3.3 Treatment Take: Dictator

[**Screen 1**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, you have been provisionally allocated an additional **100 DKK**. The other participant has **not** been allocated these additional 100 DKK.

Your decision is a simple one: Decide what portion, if any, of these **100 DKK** to transfer to the other person. You can also transfer a negative amount. This means that you can take up to **100 DKK** from the other participant. You can choose any amount from -100 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given your decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from your decision.

The other person will not make any decision, but he or she has the opportunity to read the instructions we have given to you. Similarly, you can read the instructions given to the other person by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.4]

[**Screen 2**] You now have to choose how much you would like to transfer to the other participant. You can also transfer a negative amount. This means that can take up to **100 DKK** from the other participant.

How much would you like to transfer to the other participant?

- -100 DKK
- -90 DKK
- -80 DKK
- -70 DKK
- -60 DKK
- -50 DKK
- -40 DKK
- -30 DKK
- -20 DKK
- -10 DKK
- 0 DKK
- 10 DKK
- 20 DKK
- 30 DKK
- 40 DKK
- 50 DKK
- 60 DKK
- 70 DKK
- 80 DKK
- 90 DKK
- 100 DKK

A.3.4 Treatment Take: Recipient

[**Screen 1**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, the other participant has been provisionally allocated an additional **100 DKK**. You have **not** been allocated these additional 100 DKK.

The other participant has been asked to make a simple decision: Decide what portion, if any, of these **100 DKK** to transfer to you. He or she can also transfer a negative amount. This means that the other participant can take up to **100 DKK** from you. He or she can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given the other participant's decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from his or her decision.

You will not make any decision, but you have the opportunity to read the instructions we have given to the other participant by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.3] Similarly, the other participant can read the instructions given to you.

A.3.5 Treatment Work-Give: Dictator

[**Screen 1**] In this part of the experiment, we ask you to do a task. **The task is to count orange cells.** [See Figure A.1]

You earn 1 point per correct answer. **In order to pass the task, you need 12 points within 5 minutes.** If you pass the task, you earn **150 DKK** and is directed to a **distribution phase**. If you fail the task, you earn **0 DKK** and is directed to the next part of the experiment.

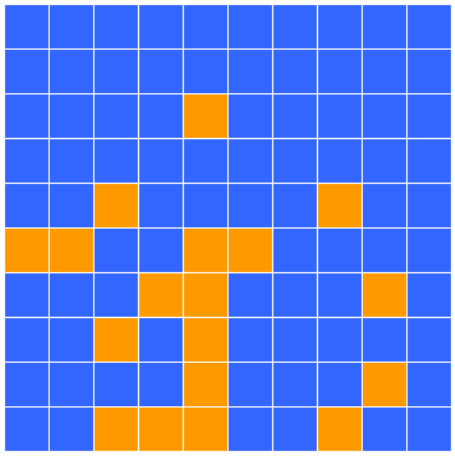
In the distribution phase you will paired with another participant, who has also passed the task and earned 150 DKK. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment. One of you will **randomly** be chosen to decide how your total income of **300 DKK** should be shared among you.

You can read more about the distribution phase if you pass the task.

When you press "Continue", your 5 minutes start. You **cannot** stop the time - not even by logging out.

[**Screen 2**] *See Section A.3.9.*

Figure A.1: An example of a 10 × 10 grid for the work task



[**Screen 3**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, you have been provisionally allocated an additional **100 DKK**. The other participant has **not** been allocated these additional 100 DKK.

Your decision is a simple one: Decide what portion, if any, of these **100 DKK** to transfer to the other person. You can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given your decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from your decision.

The other person will not make any decision, but he or she has the opportunity to read the instructions we have given to you. Similarly, you can read the instructions given to the other person by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.6]

[**Screen 4**] You now have to choose, how much you would like to transfer to the other participant.

How much would you like to transfer to the other participant?

- 0 DKK
- 10 DKK
- 20 DKK
- 30 DKK
- 40 DKK
- 50 DKK

- 60 DKK
- 70 DKK
- 80 DKK
- 90 DKK
- 100 DKK

A.3.6 Treatment Work-Give: Recipient

[**Screen 1**] In this part of the experiment, we ask you to do a task. **The task is to count orange cells.** [See Figure A.1] You earn 1 point per correct answer. **In order to pass the task, you need 12 points within 5 minutes.** If you pass the task, you earn **150 DKK** and is directed to a **distribution phase**. If you fail the task, you earn **0 DKK** and is directed to the next part of the experiment.

In the distribution phase you will paired with another participant, who has also passed the task and earned 150 DKK. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment. One of you will **randomly** be chosen to decide how your total income of **300 DKK** should be shared among you.

You can read more about the distribution phase if you pass the task.

When you press "Continue", your 5 minutes start. You **cannot** stop the time - not even by logging out.

[**Screen 2**] *See Section A.3.9.*

[**Screen 3**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, the other participant has been provisionally allocated an additional **100 DKK**. You have **not** been allocated these additional 100 DKK.

The other participant has been asked to make a simple decision: Decide what portion, if any, of these **100 DKK** to transfer to you. He or she can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given the other participant's decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from his or her decision.

You will not make any decision, but you have the opportunity to read the instructions we have given to the other participant by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.5] Similarly, the other participant can read the instructions given to you.

A.3.7 Treatment Work-Take: Dictator

[**Screen 1**] In this part of the experiment, we ask you to do a task. **The task is to count orange cells.** [See Figure A.1] You earn 1 point per correct answer. **In order to pass the task, you need 12 points within 5 minutes.** If you pass the task, you earn **150 DKK** and is directed to a **distribution phase**. If you fail the task, you earn **0 DKK** and is directed to the next part of the experiment.

In the distribution phase you will paired with another participant, who has also passed the task and earned 150 DKK. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment. One of you will **randomly** be chosen to decide how your total income of **300 DKK** should be shared among you.

You can read more about the distribution phase if you pass the task.

When you press "Continue", your 5 minutes start. You **cannot** stop the time - not even by logging out.

[**Screen 2**] *See Section A.3.9.*

[**Screen 3**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, you have been provisionally allocated an additional **100 DKK**. The other participant has **not** been allocated these additional 100 DKK.

Your decision is a simple one: Decide what portion, if any, of these **100 DKK** to transfer to the other person. You can also transfer a negative amount. This means that you can take up to **100 DKK** from the other participant. You can choose any amount from -100 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given your decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from your decision.

The other person will not make any decision, but he or she has the opportunity to read the instructions we have given to you. Similarly, you can read the instructions given to the other person by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.8]

[**Screen 4**] You now have to choose, how much you would like to transfer to the other participant. You can also transfer a negative amount. This means that can take up to **100 DKK** from the other participant.

How much would you like to transfer to the other participant?

- -100 DKK
- -90 DKK
- -80 DKK
- -70 DKK
- -60 DKK
- -50 DKK
- -40 DKK
- -30 DKK
- -20 DKK
- -10 DKK
- 0 DKK
- 10 DKK
- 20 DKK
- 30 DKK
- 40 DKK
- 50 DKK
- 60 DKK
- 70 DKK
- 80 DKK
- 90 DKK
- 100 DKK

A.3.8 Treatment Work-Take: Recipient

[**Screen 1**] In this part of the experiment, we ask you to do a task. **The task is to count orange cells.** [See Figure A.1] You earn 1 point per correct answer. **In order to pass the task, you need 12 points within 5 minutes.** If you pass the task, you earn **150 DKK** and is directed to a **distribution phase**. If you fail the task, you earn **0 DKK** and is directed to the next part of the experiment.

In the distribution phase you will paired with another participant, who has also passed the task and earned 150 DKK. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment. One of you will **randomly** be chosen to decide how your total income of **300 DKK** should be shared among you.

You can read more about the distribution phase if you pass the task.

When you press "Continue", your 5 minutes start. You **cannot** stop the time - not even by logging out.

[**Screen 2**] *See Section A.3.9.*

[**Screen 3**] You are now paired with another participant. You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

Both of you have been allocated **100 DKK** in this part of the experiment. In addition, the other participant has been provisionally allocated an additional **100 DKK**. You have **not** been allocated these additional 100 DKK.

The other participant has been asked to make a simple decision: Decide what portion, if any, of these **100 DKK** to transfer to you. He or she can also transfer a negative amount. This means that the other participant can take up to **100 DKK** from you. He or she can choose any amount from 0 DKK to 100 DKK that can be divided by 10 DKK. **Your payment** is your initial 100 DKK allocation plus the amount that is allocated to you given the other participant's decision. **The other participant's payment** is his or her initial 100 DKK plus the amount that follows from his or her decision.

You will not make any decision, but you have the opportunity to read the instructions we have given to the other participant by clicking the button **"The other's instructions"** in the top-right corner. [See Section A.3.7] Similarly, the other participant can read the instructions given to you.

A.3.9 Work task

Figure A.1 shows the 10×10 grid in which subjects had to count orange-colored cells. A new grid appeared every time a correct answer was submitted. The grids were shown in fixed order and the numbers of orange-colored cells were (in chronological order): 15, 11, 21, 18, 9, 11, 18, 16, 15, 15, 11, 20.

Appendix B

Second Thoughts on Free Riding

B.1 Invitation letter

In Figure B.1, we present the invitation letter. The English translation of the letter is as follows:

"Dear [name],

Statistics Denmark and the Internet Laboratory for Experimental Economics (iLEE) at the Department of Economics, University of Copenhagen hereby invite you to participate in an experiment concerning economic decision processes.

Experiments are an important tool in economic research as they are used to improve the understanding of how people make economic decisions. Ultimately, such insights can contribute to improving economic policies. An economic experiment can take many forms. For example, it can involve that participants buy and sell goods in a fictitious market or make investment decisions.

In order to get a representative picture, Statistics Denmark has selected a large number of individuals from all over Denmark who now get the opportunity to participate in the experiment. You are among the randomly selected. Your participation is, of course, voluntary, but we very much hope that you would like to participate. No special knowledge about economics or computers is required to participate in the experiment and your decisions in the experiment will be handled with strict confidentiality and anonymity.

You have the opportunity to earn money in the experiment. We cannot guarantee that you will earn a specific amount since your earnings will depend on your own as well as others' decisions. The rules are described in more detail on the website.
In order to ensure the participants' perfect anonymity, all participants log in with a randomly determined number. We conduct a range of different experiments which means that not everyone participates in the same experiment. To see the details of your experiment such as the task, duration etc. we kindly ask you to log in to our website as soon as possible:

www.econ.ku.dk/ilee with your log in number: **28.826-6**

In case you encounter problems logging in or if you have any questions, you are welcome to contact the Department of Economics by email ilee@econ.ku.dk or by phone 35 32 44 09.

Kind regards and thank you in advance for your help.

Isak Isaksen
Head of Office, Statistics Denmark

Jean-Robert Tyran
Professor, Department of Economics"

B.2 Representativeness of sample

Statistics Denmark created a random sample of the Danish adult population aged 18-80 years in late 2007 consisting of 40,000 adults. We randomly invited 22,027 persons for participation in the first wave of the iLEE panel in 2008. In Table B.1, we compare our study sample with the non-respondents from the sample of 40,000 adults. We see that due to self-selection, our study sample is younger, better educated, and has a higher income than the representative population.

Table B.1: Representativeness of sample

	Respondents	Non-respondents	t-test
Age	45.6 (14.5)	48.0 (16.4)	$p < 0.001$
Female	0.484 (0.500)	0.505 (0.500)	$p = 0.069$
Years of education	13.4 (2.49)	12.1 (2.94)	$p < 0.001$
Gross income (DKK)	343,864 (247,839)	279,794 (249,630)	$p < 0.001$
Observations	2,081	37,829	

Notes: Means are reported with standard deviations in parentheses. p-values come from two-sided tests.

B.3 Instructions

In this section, we show translated instructions. Original instructions were in Danish and presented on four screens per treatment. We show instructions for treatment Give below. Treatment Take used the same instructions except for text indicated in brackets.

Before the subjects proceeded to the game described below, they played a simple Public Goods Game with the same parameters as the main game discussed here and in the respective framing (referred to as part 1 below). In part 1, the rules of the game were carefully explained, including the use of graphical illustrations shown in Figure B.2 (Give treatment) and Figure B.3 (Take treatment). Subjects were able to review the

Figure B.1: Invitation letter to iLEE1 in 2008

DANMARKS STATISTIK

Name
Address

Kære ***Name***

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterer dig hermed til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser.

Eksperimenter er et vigtigt redskab inden for økonomisk forskning, idet de er med til at skabe en bedre forståelse for, hvordan mennesker træffer økonomiske beslutninger. I sidste ende kan dette være med til at forbedre den førte økonomiske politik. Et økonomisk eksperiment kan tage mange forskellige former – eksempelvis kan det gå ud på, at deltagerne skal købe og sælge varer på et fiktivt marked eller træffe beslutninger om at investere.

For at opnå et repræsentativt billede har Danmarks Statistik udvalgt et stort antal personer fra hele Danmark, som nu får mulighed for at deltage i eksperimentet. Du er blandt de tilfældigt udtrukne. Din deltagelse er naturligvis frivillig, men vi håber meget, at du vil deltage. Der kræves ingen særlig kendskab til hverken økonomi eller computere for at kunne deltage i eksperimentet, og dine beslutninger i eksperimentet bliver behandlet strengt fortroligt og anonymt.

Ved at deltage i eksperimentet får du mulighed for at tjene penge. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. De nærmere regler er beskrevet på hjemmesiden.
For at sikre deltagerne fuld anonymitet logger alle deltagere ind med et tilfældigt udvalgt nummer. Vi laver en række forskellige eksperimenter, og alle deltager derfor ikke i det samme eksperiment. For at se detaljerne i netop dit eksperiment, herunder opgaven, tidsforbrug mv., bedes du snarest muligt logge ind på vores hjemmeside:

www.econ.ku.dk/ilee med dit login nummer: **28.826-6**

Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte Økonomisk Institut på e-mail **ilee@econ.ku.dk** eller telefon 35 32 44 09.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen
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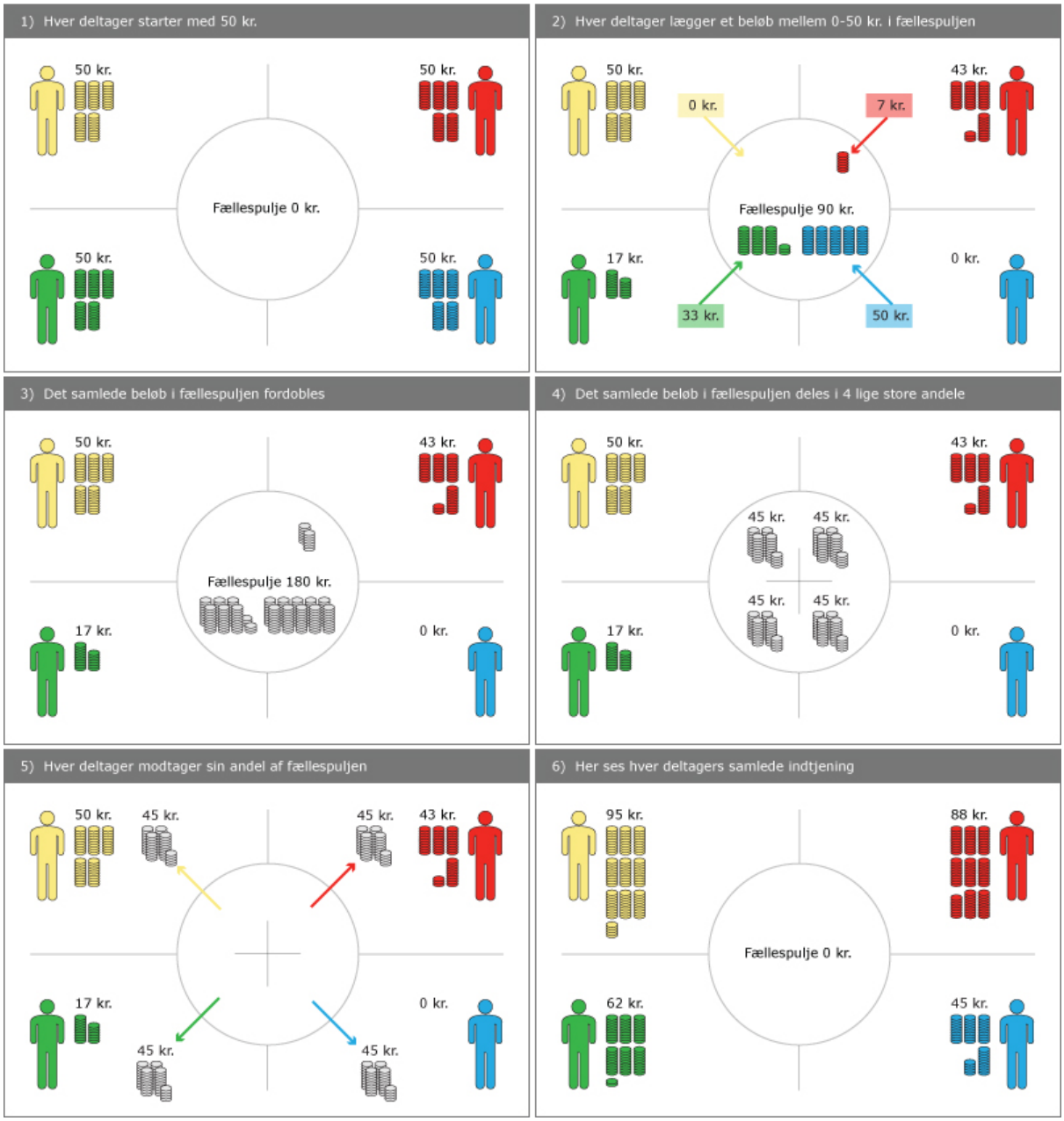
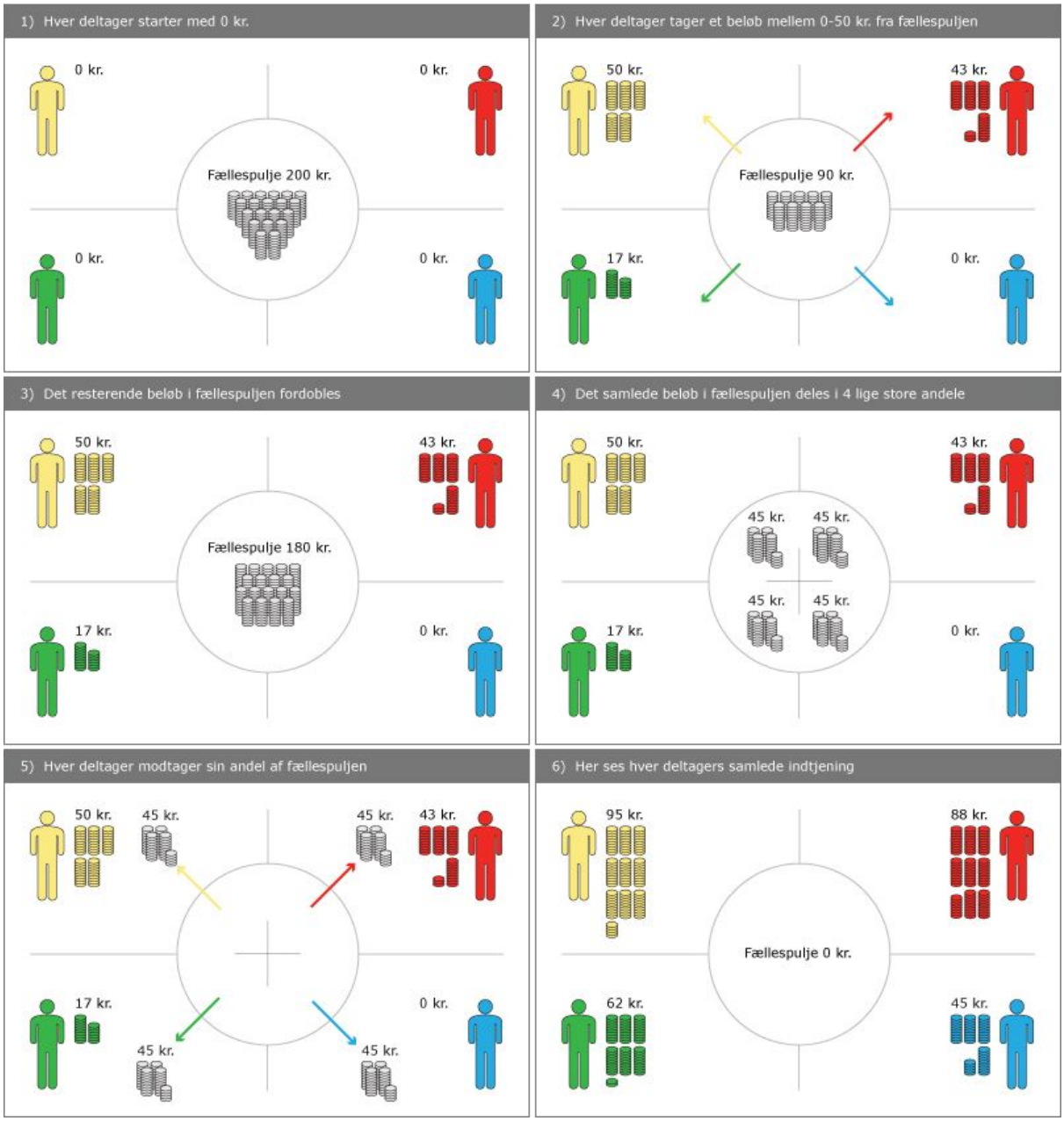
instructions for part 1 during part 2 by clicking the respective button on the screen. They could also click a button to use a built-in calculator.

Figure B.2: Illustration explaining the Give treatment



Translated text: "1) Every participant starts with 50 DKK. 2) Every participant puts an amount between 0-50 DKK in the common pool. 3) The total amount in the common pool is doubled. 4) The total amount in the common pool is divided in 4 equal shares. 5) Every participant receives his or her share from the common pool. 6) Here is every participant's total earnings."

Figure B.3: Illustration explaining the Take treatment



Translated text: "1) Every participant starts with 0 DKK. 2) Every participant withdraws an amount between 0-50 DKK from the common pool. 3) The total amount in the common pool is doubled. 4) The total amount in the common pool is divided in 4 equal shares. 5) Every participant receives his or her share from the common pool. 6) Here is every participant's total earnings."

B.3.1 Instructions for the Give treatment

[Screen 1: Instructions]
Instructions - part 2

You are in a **new group** in part 2. You and every group member again receive **an initial endowment of 50 DKK from us** [Your group again starts with an **endowment of 200 DKK from us**]. You are, however, about to be in two different situations now.

Situation 1 resembles the first part of the experiment. You must decide how much you would like to give to [withdraw from] the common pool **without knowing** how much the others contribute [withdraw].

In **Situation 2** you must decide how much you would like to give [withdraw], **if you know** how much the other group members on average give to [withdraw from] the common pool. You must fill a decision table as the one you see here:

Example - Situation 2:

If the others on average put [withdraw] ... DKK in [from] the common pool	then I put [withdraw] ... DKK in [from] the common pool
0	[ ]
5	[ ]
.	.
.	.
.	.
45	[ ]
50	[ ]

When everyone in the group has made their decisions in Situation 1 and Situation 2, one of the four members is randomly picked.

For the picked group member, the decision table from Situation 2 will count. For the three other group members, who have not been picked, the decision from Situation 1 will count. When you make your decisions in Situation 1 and Situation 2, you will not be informed whether you have been picked. **You are therefore requested to carefully consider all decisions as they might turn out to be relevant for you.**

Example 1
Assume that you have been picked. This means that it is your decision table which counts. For the three other group members it is the decision from Situation 1 that counts. Assume that they have chosen to put 0, 10, and 20 DKK in [withdraw 50, 40, and 30 DKK from] the common pool, i.e. 10 DKK [40 DKK] on average. If you have decided in your decision table to contribute 8 DKK [withdraw 42 DKK] in case the others contribute 10 DKK [withdraw 40 DKK] on average, then the total amount in the common pool is 0 + 10 + 20 + 8 = 38 DKK. This amount is doubled to 76 DKK and shared equally such that each group member receives 19 DKK from the common pool plus the amount they have decided to keep [withdraw].

Example 2
Assume that you have not been picked. This means that for you and two other group members, it is the decision in Situation 1 which counts. Assume that your decision in Situation 1 was to contribute 40 DKK [withdraw 10 DKK] and that the two others contribute 20 and 30 DKK, respectively. On average, you and the two other group members put 30 DKK [withdraw 20 DKK]. If the picked group member has chosen to put 10 DKK in [withdraw 40 DKK from] the common pool, when the others on average put 30 DKK [withdraw 20 DKK], then the total amount in the common pool will be 40 + 30 + 20 + 10 = 100 DKK. This amount is doubled to 200 DKK and shared equally such that every group member receives 50 DKK from the common pool plus the amount they have decided to keep [withdraw].

Note that averages will be rounded to nearest 5 DKK. For example, 13.5 DKK will be rounded to 15 DKK.

[Continue]

[Screen 2: Unconditional decision making]
Situation 1

Once again, you must decide how much to put into [withdraw from] the common pool. You can enter an integer between 0 and 50.

I would like to put [withdraw] __ DKK in [from] the common pool.

[Confirm your decision]

[Screen 3: Conditional decision making]
Situation 2

Please enter how much you would like to contribute [withdraw] if you knew how much the others on average contributed to [withdrew from] the common pool. Please fill out all 11 cells in the decision table. In each of the cells, you can enter integers between 0 and 50.

If the others on average put [withdraw] ... DKK in [from] the common pool	then I put [withdraw] ... DKK in [from] the common pool
0	[ ]
5	[ ]
10	[ ]
15	[ ]
20	[ ]
25	[ ]
30	[ ]
35	[ ]
40	[ ]
45	[ ]
50	[ ]

[Confirm your decisions]

[Screen 4: Confusion questions]

What would different kinds of people do?

You are now requested to fill out the two tables below. The tables are similar to the decision table, which you have just filled out. Now, however, you must fill out the first table as if you were only interested in your own income, and the other table as if you only cared about others' income.

You receive 5 DKK for every correct answer, i.e. up to 30 DKK in total.

Imagine that you are a person, who only cares about **own** income.

A person, who only cares about own income and expects the others to contribute [withdraw] 0 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

A person, who only cares about own income and expects the others to contribute [withdraw] 25 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

A person, who only cares about own income and expects the others to contribute [withdraw] 50 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

Imagine now that you are a person, who only cares about **others'** income.

A person, who only cares about others' income and expects the others to contribute [withdraw] 0 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

A person, who only cares about others' income and expects the others to contribute [withdraw] 25 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

A person, who only cares about others' income and expects the others to contribute [withdraw] 50 DKK on average, would contribute [withdraw] __ DKK to [from] the common pool

[Confirm your answers]

B.4 Robustness checks

In this section, we provide robustness checks of the regressions presented in Chapter 3, Tables 3.1 and 3.2. We make the following robustness checks:

Figure B.4: Cdf's of response times across cooperator types and treatments.

Table B.2: Regressions from Chapter 3, Table 3.1 using only subjects that answered all of the incentivized post-experiment control questions correctly (see Screen 4 in the instructions).

Table B.3: Regressions from Chapter 3, Table 3.1, but with top-coding at 300 seconds.

Table B.4: Regressions from Chapter 3, Table 3.1, but with OLS regressions and response times observations above 600 discarded.

Table B.5: Regressions from Chapter 3, Table 3.1, but with OLS regressions and response times observations above 300 discarded.

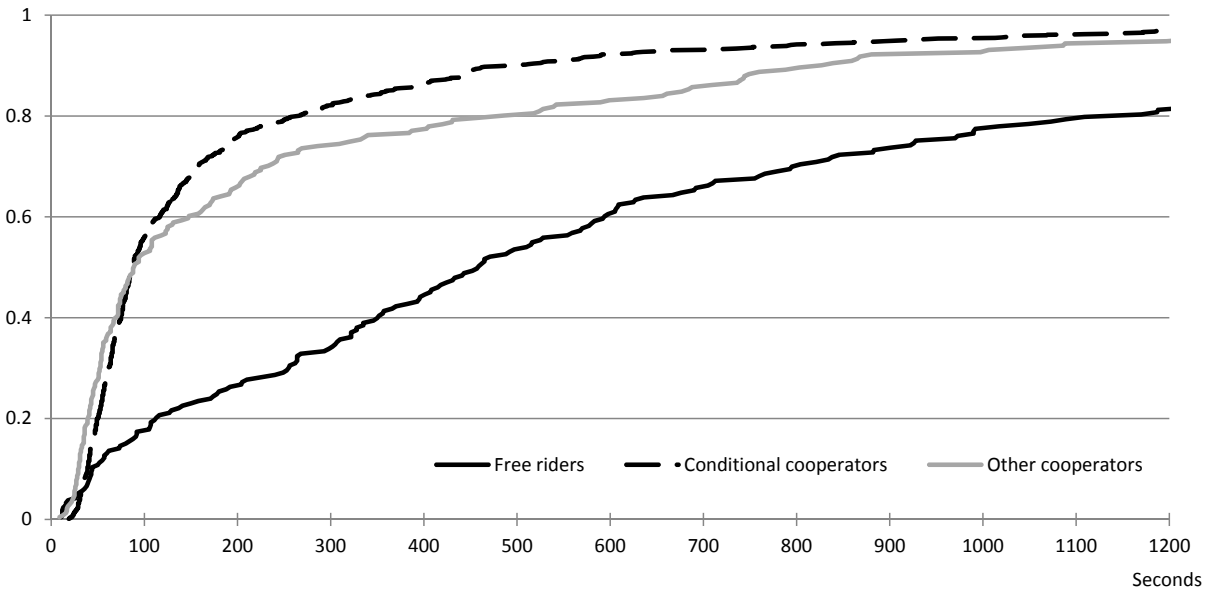
Table B.6: Regressions from Chapter 3, Table 3.1, but with median regressions.

Table B.7: Regressions from Chapter 3, Table 3.2 using only subjects that answered all of the incentivized post-experiment control questions correctly (see Screen 4 in the instructions).

Table B.8: Regressions from Chapter 3, Table 3.2 with linear probability model (LPM) regressions instead of logit regressions.

Figure B.4: CDFs of response times across cooperator types and treatments

(a) Give



(b) Take

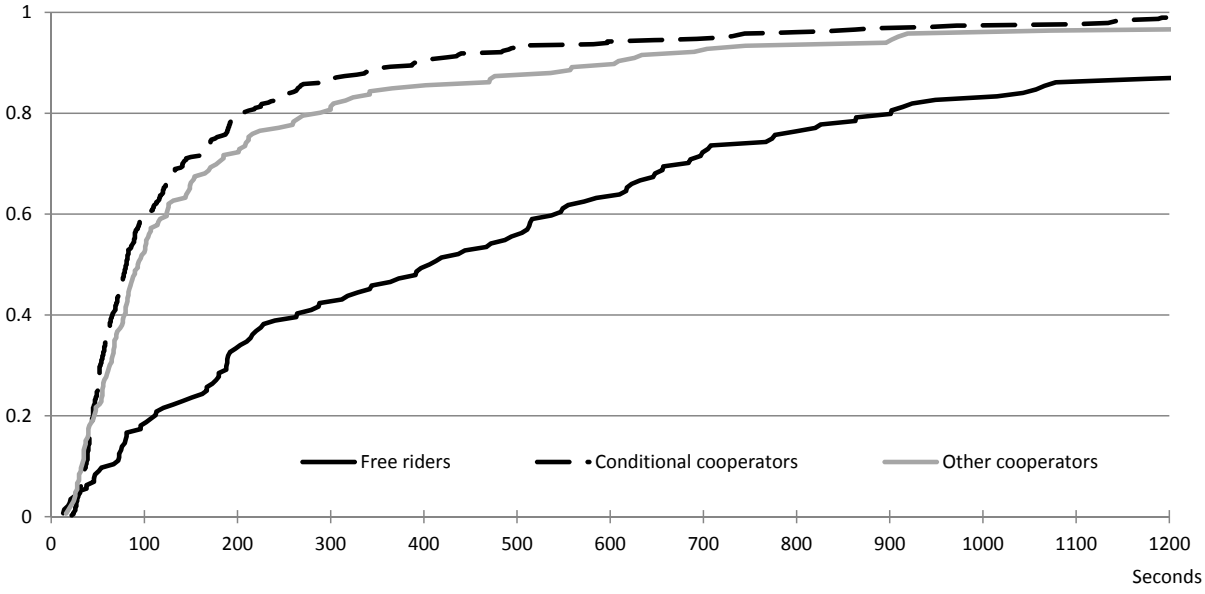


Table B.2: Regressions of response times in seconds on different cooperator types with non-confused subjects only

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	250.5***	244.4***	241.6***	263.3***	255.7***	249.6***
	(22.5)	(22.5)	(22.2)	(25.7)	(24.8)	(25.0)
Other cooperator	82.2**	75.2**	72.6**	90.7**	93.6**	97.0**
	(28.4)	(28.3)	(28.0)	(31.8)	(31.2)	(31.0)
Swiftness, fast[a]		-37.1	-8.05		-62.7	-46.6
		(32.6)	(33.0)		(34.4)	(34.7)
Swiftness, medium[b]		-42.2	-30.7		-18.7	-10.2
		(28.2)	(28.0)		(30.1)	(30.1)
Reading, fast[c]			-111.0***			-66.4*
			(26.6)			(30.6)
Reading, medium[d]			-61.4**			-11.1
			(23.1)			(26.8)
Age		0.61	0.47		-0.13	-0.30
		(0.83)	(0.82)		(0.89)	(0.89)
Female		69.7***	67.0***		71.8**	69.4**
		(19.2)	(19.0)		(22.2)	(22.1)
Education		3.74	3.49		-3.79	-4.47
		(4.14)	(4.10)		(4.42)	(4.39)
Cognitive reflection test		21.1*	15.4		0.61	-0.77
		(9.57)	(9.55)		(11.1)	(11.0)
Progressive matrices test		-10.6**	-8.16*		-5.40	-4.63
		(3.61)	(3.60)		(3.85)	(3.84)
Constant	200.0***	187.0*	226.4**	155.8***	257.5**	286.2***
	(12.3)	(76.6)	(77.5)	(15.2)	(80.6)	(81.8)
Observations	667	659	659	401	392	392

Notes: Tobit regressions with response times top-coded at 600 seconds as the dependent variable. Standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: Regressions of response times in seconds on different cooperator types with top-coding at 300 seconds

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	166.0***	156.4***	147.5***	152.0***	147.4***	136.5***
	(11.2)	(11.3)	(10.9)	(12.9)	(12.8)	(12.4)
Other cooperator	17.5	16.5	14.4	18.7	22.4	29.1**
	(9.70)	(9.63)	(9.32)	(11.4)	(11.5)	(11.1)
Swiftness, fast [a]		-39.6***	-13.5		-59.3***	-35.0*
		(11.7)	(11.7)		(14.9)	(14.7)
Swiftness, medium [b]		-32.8**	-19.5*		-30.4*	-19.6
		(9.98)	(9.78)		(12.9)	(12.6)
Reading, fast[c]			-86.9***			-83.2***
			(9.82)			(12.9)
Reading, medium[d]			-46.0***			-32.0**
			(9.00)			(11.9)
Age		0.65*	0.42		0.0094	-0.21
		(0.30)	(0.29)		(0.38)	(0.37)
Female		27.5***	26.3***		34.2***	30.3**
		(7.25)	(7.02)		(9.57)	(9.23)
Education		1.29	1.20		-0.44	-0.98
		(1.53)	(1.48)		(1.90)	(1.83)
Cognitive reflection test		16.6***	12.5***		8.72	5.76
		(3.55)	(3.46)		(4.72)	(4.57)
Progressive matrices test		-2.74*	-2.19		-0.82	-0.13
		(1.28)	(1.24)		(1.66)	(1.60)
Constant	142.3***	111.4***	158.6***	128.2***	145.8***	191.3***
	(4.28)	(27.5)	(27.6)	(6.24)	(34.5)	(34.4)
Observations	1391	1361	1361	690	669	669

Notes: Tobit regressions with response times top-coded at 300 seconds as the dependent variable. Standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: Regressions of response times in seconds on different cooperator types with cutoff at 600 seconds

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	127.5***	121.4***	116.8***	117.4***	117.8***	106.9***
	(12.3)	(12.6)	(12.4)	(14.4)	(14.5)	(14.3)
Other cooperator	-4.11	-0.90	-1.27	6.47	6.42	15.0
	(10.4)	(10.5)	(10.4)	(12.0)	(12.4)	(12.1)
Swiftness, fast [a]		-24.1	-5.88		-53.0**	-29.5
		(12.7)	(12.9)		(16.2)	(16.2)
Swiftness, medium [b]		-18.1	-9.96		-26.5	-16.2
		(10.9)	(10.8)		(14.1)	(13.8)
Reading, fast[c]			-61.3***			-82.2***
			(10.8)			(14.2)
Reading, medium[d]			-29.2**			-29.3*
			(10.0)			(13.2)
Age		0.47	0.37		0.22	0.0086
		(0.32)	(0.32)		(0.42)	(0.41)
Female		22.9**	22.9**		13.1	10.9
		(7.80)	(7.70)		(10.4)	(10.1)
Education		0.94	0.85		-1.29	-1.74
		(1.64)	(1.62)		(2.07)	(2.02)
Cognitive reflection test		15.4***	12.5**		5.95	3.42
		(3.84)	(3.83)		(5.12)	(4.99)
Progressive matrices test		-1.62	-1.16		0.28	0.97
		(1.36)	(1.35)		(1.78)	(1.74)
Constant	130.5***	95.0**	125.4***	119.6***	141.0***	183.5***
	(4.43)	(29.4)	(30.0)	(6.48)	(37.4)	(37.5)
Observations	1194	1169	1169	597	581	581
R^2	0.085	0.112	0.136	0.104	0.139	0.192

Notes: OLS regressions with response times as the dependent variable. Standard errors in parentheses. We have excluded response times exceeding 600 seconds. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.5: Regressions of response times in seconds on different cooperator types with cutoff at 300 seconds

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	22.6**	20.5*	17.1*	39.0***	38.4***	36.2***
	(7.86)	(7.97)	(7.76)	(9.28)	(9.43)	(9.39)
Other cooperator	-6.51	-5.88	-6.41	4.57	5.52	9.51
	(5.39)	(5.42)	(5.28)	(6.81)	(7.04)	(6.92)
Swiftness, fast [a]		-26.2***	-14.5*		-36.6***	-25.0**
		(6.62)	(6.63)		(9.40)	(9.45)
Swiftness, medium [b]		-18.1**	-13.2*		-21.3*	-16.4*
		(5.72)	(5.61)		(8.37)	(8.22)
Reading, fast[c]			-42.2***			-34.7***
			(5.66)			(8.44)
Reading, medium[d]			-19.9***			-3.91
			(5.34)			(8.06)
Age		0.19	0.11		0.044	-0.043
		(0.17)	(0.17)		(0.24)	(0.23)
Female		8.41*	9.04*		7.69	6.31
		(4.11)	(4.00)		(6.11)	(5.97)
Education		-0.47	-0.38		0.015	-0.21
		(0.86)	(0.83)		(1.24)	(1.21)
Cognitive reflection test		4.10*	2.11		4.08	2.83
		(2.01)	(1.97)		(3.03)	(2.97)
Progressive matrices test		-0.31	-0.060		0.040	0.16
		(0.71)	(0.70)		(1.03)	(1.01)
Constant	94.1***	102.6***	123.3***	93.2***	104.6***	120.8***
	(2.29)	(15.5)	(15.7)	(3.67)	(21.8)	(22.3)
Observations	1020	998	998	525	510	510
R^2	0.010	0.044	0.098	0.033	0.076	0.123

Notes: OLS regressions with response times as the dependent variable. Standard errors in parentheses. We have excluded response times exceeding 300 seconds. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.6: Regressions of response times in seconds on different cooperator types

	Give			Take		
	(1)	(2)	(3)	(4)	(5)	(6)
Free rider	372***	380.9***	351.4***	332***	316.3***	284.6***
	(14.3)	(15.0)	(16.2)	(21.4)	(22.1)	(24.1)
Other cooperator	2	2.47	8.45	13	18.3	14.6
	(13.8)	(14.3)	(15.4)	(20.4)	(21.5)	(23.2)
Swiftness, fast [a]		-38.2*	-25.3		-63.6*	-41.8
		(17.1)	(19.1)		(27.3)	(30.1)
Swiftness, medium [b]		-29.6*	-26.0		-48.7*	-28.9
		(14.5)	(15.8)		(23.6)	(25.5)
Reading, fast[c]			-99.4***			-85.4**
			(16.0)			(26.2)
Reading, medium[d]			-76.3***			-47.2*
			(14.5)			(24.0)
Age		0.60	0.44		0.48	-0.011
		(0.44)	(0.48)		(0.70)	(0.76)
Female		22.3*	20.4		22.3	21.9
		(10.6)	(11.4)		(17.6)	(18.9)
Education		0.96	0.11		-0.99	-1.18
		(2.25)	(2.42)		(3.48)	(3.73)
Cognitive reflection test		10.5*	7.33		6.25	4.90
		(5.21)	(5.65)		(8.70)	(9.36)
Progressive matrices test		-2.05	-1.02		0.71	0.0092
		(1.88)	(2.02)		(3.08)	(3.31)
Constant	87***	73.6	156.0***	81***	94.4	171.9*
	(6.11)	(40.4)	(45.0)	(11.2)	(63.6)	(70.2)
Observations	1391	1361	1361	690	669	669

Notes: Median regressions with response times as the dependent variable. Standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.7: Regressions of being a free rider on individual characteristics with non-confused subjects only

	Give		Take	
	(1)	(2)	(3)	(4)
Swiftness, fast [a]	-0.006	0.002	-0.086	-0.040
	(0.060)	(0.062)	(0.068)	(0.073)
Swiftness, medium [b]	-0.016	-0.013	-0.105	-0.073
	(0.051)	(0.052)	(0.061)	(0.063)
Reading, fast[c]		-0.028		-0.198***
		(0.048)		(0.052)
Reading, medium[d]		-0.009		-0.201***
		(0.042)		(0.051)
Age	0.003	0.003	-0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Female	-0.054	-0.055	0.031	0.019
	(0.035)	(0.035)	(0.049)	(0.049)
Education	0.007	0.007	0.007	0.007
	(0.008)	(0.008)	(0.010)	(0.010)
Cognitive reflection test	0.059**	0.057**	0.037	0.032*
	(0.018)	(0.018)	(0.024)	(0.024)
Progressive matrices test	0.005	0.006	0.002	0.006*
	(0.007)	(0.007)	(0.008)	(0.009)
Observations	659	659	392	392

Notes: Logit regression. Marginal effects at means are reported with standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.8: Regressions of being a free rider on individual characteristics

	Give		Take	
	(1)	(2)	(3)	(4)
Swiftness, fast [a]	-0.018	0.0072	-0.098*	-0.049
	(0.032)	(0.033)	(0.049)	(0.050)
Swiftness, medium [b]	-0.0082	0.0038	-0.075	-0.044
	(0.027)	(0.027)	(0.043)	(0.042)
Reading, fast[c]		-0.080**		-0.19***
		(0.027)		(0.043)
Reading, medium[d]		-0.032		-0.19***
		(0.025)		(0.039)
Age	0.0013	0.0011	0.00023	-0.00037
	(0.00081)	(0.00082)	(0.0013)	(0.0013)
Female	-0.031	-0.032	-0.0065	-0.013
	(0.020)	(0.020)	(0.032)	(0.031)
Education	0.010*	0.010*	0.0037	0.0029
	(0.0041)	(0.0041)	(0.0063)	(0.0062)
Cognitive reflection test	0.054***	0.050***	0.047**	0.039*
	(0.0095)	(0.0095)	(0.016)	(0.015)
Progressive matrices test	0.0040	0.0045	0.0092	0.011*
	(0.0035)	(0.0035)	(0.0056)	(0.0055)
Constant	-0.13	-0.094	0.071	0.22
	(0.074)	(0.077)	(0.11)	(0.12)
Observations	1361	1361	669	669
R^2	0.050	0.057	0.030	0.068

Notes: LPM regressions. Standard errors in parentheses. [a] 0-30s, [b] 31-60s, [c] 0-120s, [d] 121-240s.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C

Fairness is Intuitive

C.1 Instructions

In this section, we present translated instructions from the experiment. The instructions were originally written in Danish. Subjects were able to review the instructions on later screens by clicking the respective button on the screen.

[Screen 1: Instructions for part 1]
Instructions for the experiment's first part

All participants in the experiment initially receive 75 DKK.

You are now involved in **2** decision situations.

In each situation, you will be randomly matched with another participant. (It will not be the same participant.)

Your decision

In one situation, you are the decision maker. **You must decide how you wish to divide the total amount that you and the other participant have been given (75 + 75 = 150 DKK) between the two of you.**

The initial situation is shown below.

[Continue]

[Screen 2: Instructions for part 2]
The other situation

In the other situation, he or she is the decision maker. He or she must make a similar decision about how he or she wants to divide the total amount that you have initially been given.

Outcome

Figure C.1: Illustration explaining the initial situation in the Dictator Game

Translated text: "Udgangspunkt"=Initial situation. "Dig"=You. "Den anden"=The other one.

Only one of the situations will be selected for payments. Both you and that other participant will be paid according to the decision made in that situation. It is equally likely that you will be paid according to the situation in which you are the decision maker as it is that you will be paid according to the situation in which the other participant is the decision maker.

[Go back] [Continue]

[Screen 3: Decision screen]
Your decision

Pick one of the options below and click **Submit decision**.

[See the instructions again] [Submit decision]

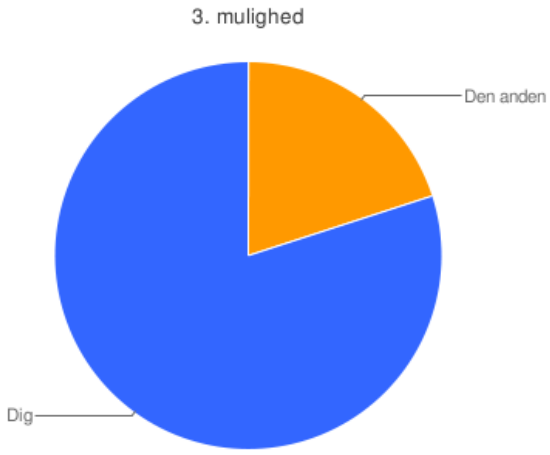
Figure C.2: Screenshot of the decision screen in the Dictator Game

	Fordeling (dig - den anden)	Du får	Den anden får	Din beslutning
1	100% - 0%	150 kr.	0 kr.	○
2	90% - 10%	135 kr.	15 kr.	○
3	80% - 20%	120 kr.	30 kr.	○
4	70% - 30%	105 kr.	45 kr.	○
5	60% - 40%	90 kr.	60 kr.	○
6	50% - 50%	75 kr.	75 kr.	○
7	40% - 60%	60 kr.	90 kr.	○
8	30% - 70%	45 kr.	105 kr.	○
9	20% - 80%	30 kr.	120 kr.	○
10	10% - 90%	15 kr.	135 kr.	○
11	0% - 100%	0 kr.	150 kr.	○

Translated text: "Fordeling (dig - den anden)"=Division (you - the other). "Du får"=You get. "Den anden får"=The other gets. "Din beslutning"=Your decision

Figure C.3: Screenshot of the decision screen in the Dictator Game after clicking 80-20

	Fordeling (dig - den anden)	Du får	Den anden får	Din beslutning
1	100% - 0%	150 kr.	0 kr.	○
2	90% - 10%	135 kr.	15 kr.	○
3	80% - 20%	120 kr.	30 kr.	◉
4	70% - 30%	105 kr.	45 kr.	○
5	60% - 40%	90 kr.	60 kr.	○
6	50% - 50%	75 kr.	75 kr.	○
7	40% - 60%	60 kr.	90 kr.	○
8	30% - 70%	45 kr.	105 kr.	○
9	20% - 80%	30 kr.	120 kr.	○
10	10% - 90%	15 kr.	135 kr.	○
11	0% - 100%	0 kr.	150 kr.	○



Translated text: "Fordeling (dig - den anden)"=Division (you - the other). "Du får"=You get. "Den anden får"=The other gets. "Din beslutning"=Your decision. "3. mulighed"=3rd option. "Dig"=You. "Den anden"=The other one.

C.2 Robustness checks

In this section, we provide robustness checks of the results presented in the chapter. We make the following robustness checks:

Figure C.4: Figure 4.2, but with participants about whom we do not have background information included, too.

Table C.1: Regressions from Table 4.2, but with top-coding at 60 seconds.

Table C.2: Regressions from Table 4.2, but with top-coding at 240 seconds.

Table C.3: Regressions from Table 4.2, but with top-coding at 120 seconds and Tobit regression methods.

Table C.4: Regressions from Table 4.3, but with top-coding at 60 seconds.

Table C.5: Regressions from Table 4.3, but with top-coding at 240 seconds.

Table C.6: Regressions from Table 4.3, but with top-coding at 120 seconds and Tobit regression methods.

Table C.7: Regressions from Table 4.4, but with top-coding at 60 seconds.

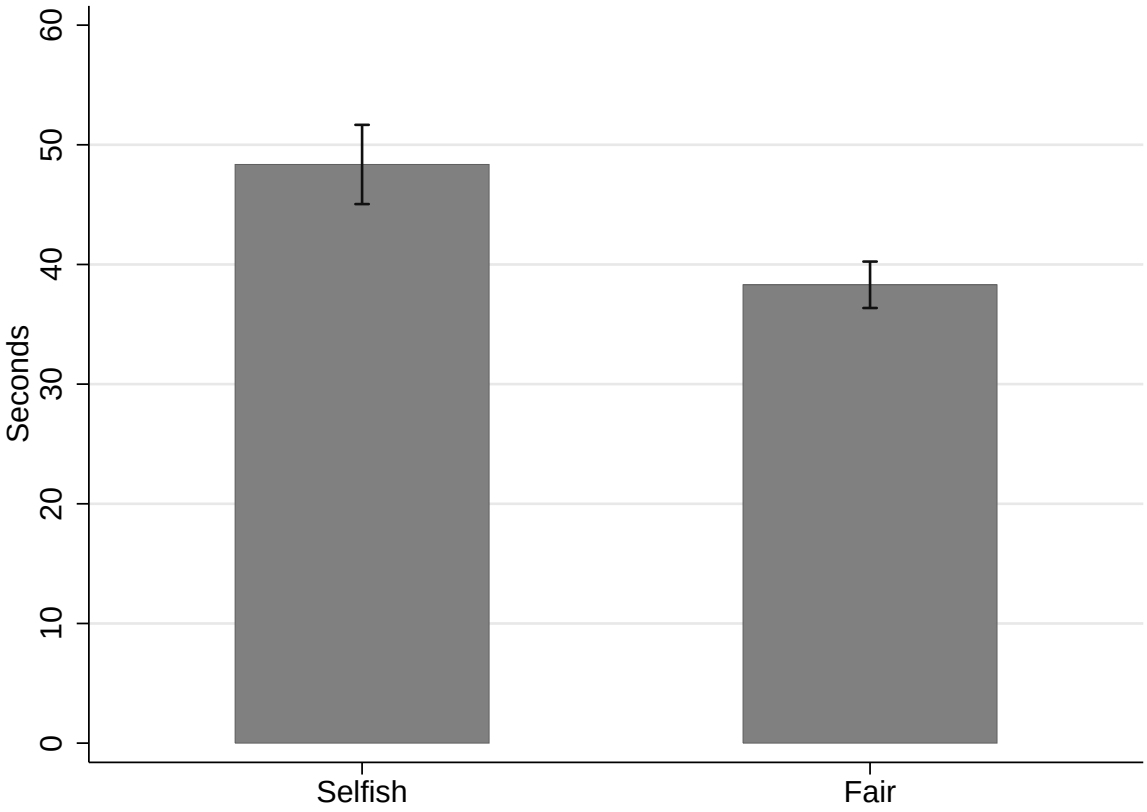
Table C.8: Regressions from Table 4.4, but with top-coding at 240 seconds.

Table C.9: Regressions from Table 4.4, but with top-coding at 120 seconds and Tobit regression methods.

For Tables C.1, C.4, and C.7 we note that the mean response time when top-coding at 60 seconds was 36.2 seconds ($\sigma = 16.9$). Likewise, for Tables C.2, C.5, and C.8 we note that the mean response time when top-coding at 240 seconds was 48.1 seconds ($\sigma = 44.7$). Tables C.3, C.6, and C.9 use the actual response time as the dependent variable, and not the normalized response time used in the chapter and in the other tables presented in this appendix.

In Table C.10, we compare the response time of fair participants to all the others' response time, i.e. both the selfish participants and the trade-off participants. Similar to what was found in Table 4.2, we find that the average response time of the fair participants is 0.45 standard deviations lower than the average response time of the other participants.

Figure C.4: Average response time of the selfish and the fair



Note: The figure reports the average response time in seconds (top-coded at 120 seconds) for participants who shared nothing (selfish) or shared equally (fair) with the other participant. Standard errors are indicated. We have included all 1,565 participants' choices and response times for this figure. 25% of these shared nothing, while 52% shared half.

Table C.1: Regressions of response time, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)
Fair	-0.336***	-0.445***	-0.413***	-0.482***	-0.499***
	(0.062)	(0.058)	(0.062)	(0.062)	(0.060)
Swiftness		-0.013***			-0.011***
		(0.001)			(0.001)
Cognitive ability			-0.063***		-0.021**
			(0.009)		(0.010)
Age				0.019***	0.006***
				(0.002)	(0.002)
Male				-0.014	0.004
				(0.056)	(0.054)
Education				-0.026**	-0.002
				(0.012)	(0.012)
Constant	2.272***	3.298***	2.880***	1.848***	3.081***
	(0.051)	(0.089)	(0.103)	(0.190)	(0.230)
Observations	1,154	1,154	1,154	1,154	1,154
R^2	0.025	0.159	0.062	0.095	0.172

Notes: OLS regressions. The dependent variable is the response time top-coded at 60 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving half of the money to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.2: Regressions of response time, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)
Fair	-0.245***	-0.324***	-0.292***	-0.337***	-0.347***
	(0.055)	(0.053)	(0.056)	(0.056)	(0.055)
Swiftness		-0.010***			-0.009***
		(0.001)			(0.001)
Cognitive ability			-0.038***		-0.010
			(0.008)		(0.009)
Age				0.012***	0.003
				(0.002)	(0.002)
Male				-0.032	-0.019
				(0.051)	(0.050)
Education				-0.013	0.006
				(0.011)	(0.011)
Constant	1.098***	1.845***	1.470***	0.785***	1.691***
	(0.045)	(0.082)	(0.093)	(0.173)	(0.212)
Observations	1,154	1,154	1,154	1,154	1,154
R^2	0.017	0.107	0.034	0.051	0.110

Notes: OLS regressions. The dependent variable is the response time top-coded at 240 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving half of the money to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.3: Regressions of response time, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)
Fair	-10.49***	-13.67***	-12.50***	-14.54***	-14.90***
	(2.00)	(1.89)	(2.00)	(2.01)	(1.94)
Swiftness		-0.39***			-0.34***
		(0.03)			(0.04)
Cognitive ability			-1.66***		-0.45
			(0.30)		(0.31)
Age				0.52***	0.16**
				(0.07)	(0.07)
Male				-0.74	-0.20
				(1.83)	(1.76)
Education				-0.61	0.13
				(0.38)	(0.38)
Constant	49.40***	79.96***	65.46***	35.94***	72.03***
	(1.64)	(2.91)	(3.35)	(6.16)	(7.48)
Observations	1,154	1,154	1,154	1,154	1,154

Notes: Tobit regressions. The dependent variable is the response time in seconds top-coded at 120 seconds. Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving half of the money to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.4: Regressions of response time, trade-off participants only

	(1)	(2)	(3)	(4)	(5)
Share given	-0.179	0.003	-0.100	-0.056	0.033
	(0.306)	(0.280)	(0.294)	(0.296)	(0.276)
Swiftness		-0.015***			-0.012***
		(0.002)			(0.002)
Cognitive ability			-0.083***		-0.044***
			(0.015)		(0.015)
Age				0.018***	0.006*
				(0.003)	(0.004)
Male				-0.091	-0.037
				(0.098)	(0.092)
Education				-0.012	0.011
				(0.021)	(0.020)
Constant	2.452***	3.477***	3.123***	1.766***	3.232***
	(0.116)	(0.160)	(0.163)	(0.334)	(0.381)
Observations	354	354	354	354	354
R^2	0.001	0.172	0.085	0.086	0.209

Notes: OLS regressions. The dependent variable is the response time top-coded at 60 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included participants who did not choose either the selfish or the fair alternative (354 participants). "Share given" is the share of the money given to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.5: Regressions of response time, trade-off participants only

	(1)	(2)	(3)	(4)	(5)
Share given	-0.435	-0.261	-0.345	-0.390	-0.282
	(0.326)	(0.304)	(0.311)	(0.322)	(0.295)
Swiftness		-0.014***			-0.013***
		(0.002)			(0.002)
Cognitive ability			-0.095***		-0.070***
			(0.015)		(0.016)
Age				0.013***	-0.002
				(0.004)	(0.004)
Male				-0.129	-0.075
				(0.107)	(0.099)
Education				0.036	0.061***
				(0.023)	(0.021)
Constant	1.309***	2.290***	2.077***	0.271	2.136***
	(0.124)	(0.174)	(0.172)	(0.364)	(0.407)
Observations	354	354	354	354	354
R^2	0.005	0.143	0.101	0.048	0.207

Notes: OLS regressions. The dependent variable is the response time top-coded at 240 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included participants who did not choose either the selfish or the fair alternative (354 participants). "Share given" is the share of the money given to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.6: Regressions of response time, trade-off participants only

	(1)	(2)	(3)	(4)	(5)
Share given	-15.68	-9.13	-12.54	-13.17	-9.28
	(11.44)	(10.52)	(10.88)	(11.15)	(10.21)
Swiftness		-0.52***			-0.46***
		(0.07)			(0.07)
Cognitive ability			-3.35***		-2.24***
			(0.55)		(0.56)
Age				0.56***	0.03
				(0.12)	(0.13)
Male				-4.89	-2.87
				(3.71)	(3.42)
Education				0.90	1.81**
				(0.80)	(0.74)
Constant	57.30***	94.22***	84.43***	20.81*	84.59***
	(4.34)	(6.11)	(6.07)	(12.61)	(14.14)
Observations	354	354	354	354	354

Notes: Tobit regressions. The dependent variable is the response time in seconds top-coded at 120 seconds. Standard errors in parentheses. We have only included participants who did not choose either the selfish or the fair alternative (354 participants). "Share given" is the share of the money given to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.7: Heterogeneity across age, gender, and education, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fair	-0.336***	-0.361***	-0.436***	-0.396***	-0.402***	-0.403***	-0.557***
	(0.062)	(0.089)	(0.101)	(0.095)	(0.093)	(0.090)	(0.149)
Swift		-0.559***					-0.466***
		(0.098)					(0.106)
Fair× Swift		-0.117					-0.118
		(0.119)					(0.130)
High cognitive ability			-0.398***				-0.277***
			(0.107)				(0.107)
Fair× High cognitive ability			0.062				0.128
			(0.128)				(0.128)
Young				-0.378***			-0.138
				(0.104)			(0.110)
Fair× Young				-0.053			-0.005
				(0.125)			(0.134)
Male					-0.062		-0.046
					(0.103)		(0.098)
Fair× Male					0.122		0.077
					(0.125)		(0.118)
Low education						-0.033	-0.085
						(0.102)	(0.097)
Fair× Low education						0.121	0.059
						(0.124)	(0.119)
Constant	2.272***	2.608***	2.541***	2.515***	2.308***	2.289***	2.896***
	(0.051)	(0.076)	(0.088)	(0.083)	(0.078)	(0.072)	(0.128)
Observations	1,154	1,154	1,154	1,154	1,154	1,154	1,154
R^2	0.025	0.126	0.055	0.066	0.026	0.026	0.143

Notes: OLS regressions. The dependent variable is the response time top-coded at 60 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving 50% of the money to the other participant, "Swift" is a dummy for being at or above the median of the swiftness distribution, "High cognitive ability" is dummy for scoring at or a dummy for being at or above the median of the 20-item progressive matrices test distribution, "Young" is a dummy for being at or below the median age distribution, "Male" is a dummy for being a male, and "Low education" is a dummy for being at our below the median of the educational attainment distribution (in years).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.8: Heterogeneity across age, gender, and education, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fair	-0.245***	-0.343***	-0.352***	-0.373***	-0.312***	-0.340***	-0.611***
	(0.055)	(0.082)	(0.091)	(0.086)	(0.083)	(0.080)	(0.138)
Swift		-0.460***					-0.382***
		(0.090)					(0.098)
Fair× Swift		0.074					0.034
		(0.109)					(0.120)
High cognitive ability			-0.300***				-0.200**
			(0.096)				(0.098)
Fair× High cognitive ability			0.112				0.109
			(0.115)				(0.118)
Young				-0.338***			-0.147
				(0.094)			(0.102)
Fair× Young				0.128			0.109
				(0.113)			(0.124)
Male					-0.092		-0.078
					(0.092)		(0.090)
Fair× Male					0.121		0.093
					(0.112)		(0.109)
Low education						-0.119	-0.157*
						(0.091)	(0.089)
Fair× Low education						0.183*	0.154
						(0.111)	(0.109)
Constant	1.098***	1.374***	1.301***	1.315***	1.151***	1.157***	1.680***
	(0.045)	(0.070)	(0.079)	(0.075)	(0.070)	(0.064)	(0.118)
Observations	1,154	1,154	1,154	1,154	1,154	1,154	1,154
R^2	0.017	0.069	0.032	0.037	0.018	0.019	0.081

Notes: OLS regressions. The dependent variable is the response time top-coded at 240 seconds divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving 50% of the money to the other participant, "Swift" is a dummy for being at or above the median of the swiftness distribution, "High cognitive ability" is dummy for scoring at or a dummy for being at or above the median of the 20-item progressive matrices test distribution, "Young" is a dummy for being at or below the median age distribution, "Male" is a dummy for being a male, and "Low education" is a dummy for being at our below the median of the educational attainment distribution (in years).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.9: Heterogeneity across age, gender, and education, selfish and fair participants only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fair	-10.49***	-13.35***	-14.43***	-14.66***	-12.55***	-13.87***	-22.37***
	(2.00)	(2.91)	(3.27)	(3.09)	(2.99)	(2.89)	(4.89)
Swift		-17.72***					-14.71***
		(3.22)					(3.47)
Fair× Swift		0.85					-0.20
		(3.90)					(4.25)
High cognitive ability			-11.86***				-7.92**
			(3.47)				(3.50)
Fair× High cognitive ability			3.80				4.40
			(4.13)				(4.19)
Young				-13.03***			-5.48
				(3.38)			(3.61)
Fair× Young				3.11			2.86
				(4.07)			(4.38)
Male					-2.34		-1.79
					(3.32)		(3.19)
Fair× Male					3.75		2.56
					(4.03)		(3.86)
Low education						-3.62	-5.15
						(3.28)	(3.17)
Fair× Low education						6.37	5.07
						(4.00)	(3.87)
Constant	49.40***	60.02***	57.40***	57.79***	50.76***	51.20***	70.70***
	(1.64)	(2.50)	(2.85)	(2.71)	(2.53)	(2.31)	(4.19)
Observations	1,154	1,154	1,154	1,154	1,154	1,154	1,154

Notes: Tobit regressions. The dependent variable is the response time in seconds top-coded at 120 seconds. Standard errors in parentheses. We have only included the selfish and the fair participants (1,154 participants). "Fair" is a dummy for giving 50% of the money to the other participant, "Swift" is a dummy for being at or above the median of the swiftness distribution, "High cognitive ability" is dummy for scoring at or a dummy for being at or above the median of the 20-item progressive matrices test distribution, "Young" is a dummy for being at or below the median age distribution, "Male" is a dummy for being a male, and "Low education" is a dummy for being at our below the median of the educational attainment distribution (in years).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.10: Regressions of response time, all participants

	(1)	(2)	(3)	(4)	(5)
Fair	-0.350***	-0.428***	-0.388***	-0.439***	-0.454***
	(0.049)	(0.046)	(0.049)	(0.049)	(0.047)
Swiftness		-0.012***			-0.011***
		(0.001)			(0.001)
Cognitive ability			-0.062***		-0.027***
			(0.008)		(0.008)
Age				0.016***	0.004*
				(0.002)	(0.002)
Male				-0.039	-0.011
				(0.048)	(0.046)
Education				-0.011	0.014
				(0.010)	(0.010)
Constant	1.558***	2.511***	2.123***	1.020***	2.291***
	(0.036)	(0.072)	(0.078)	(0.160)	(0.192)
Observations	1,508	1,508	1,508	1,508	1,508
R^2	0.032	0.158	0.072	0.086	0.171

Notes: OLS regressions. The dependent variable is the response time (top-coded at 120 seconds) divided by the standard deviation of the response time (31.0 seconds). Standard errors in parentheses. We have included all the 1,508 participants. "Fair" is a dummy for giving 50% of the money to the other participant, "Swiftness" is measured as 120 seconds minus the time used (top-coded at 120 seconds) to answer a three-item questionnaire about age, gender, and educational attainment, "Cognitive ability" is the number of correct answers in a 20-item progressive matrices test, "Age" is the participant's age in years, "Male" is a dummy for the participant being a male, and "Education" is the length of the participant's education in years.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix D

Parents' Education and their Adult Offspring's Other-Regarding Behavior

D.1 Mailings

Statistics Denmark sent out invitations and reminders to the waves. In Figure D.1 and D.2, I show the invitation and reminder letters, respectively, for iLEE2. The English translation of the invitation letter is:

"Dear [name],

Statistics Denmark and the Internet Laboratory for Experimental Economics (iLEE) at the Department of Economics, University of Copenhagen invited you to participate in an experiment concerning economic decision processes in May 2008. You completed that experiment and we therefore invite you to participate in a follow up experiment.

Your participation is, of course, voluntary, but we very much hope that you would like to participate again as it is interesting for us to see how decisions made in the two experiments are related. Your decisions in the experiment will be handled with strict confidentiality and anonymity.

By participating in the experiment you have the opportunity to earn money. We cannot guarantee that you will earn a specific amount since your earnings will depend on your own as well as others' decisions. The rules are described in more detail on the website. In order to ensure full anonymity, all participants log in with a randomly determined number. To see the details of the experiment such as the task, duration etc. we kindly ask you to log in to our website as soon as possible:

www.econ.ku.dk/ilee with your log in number: **finalidnumber**

n In case you encounter problems logging in or if you have any questions, you are welcome to contact the Department of Economics by email **ilee@econ.ku.dk** or by phone 35 32 44 09.

Kind regards and thank you in advance for your help.

Isak Isaksen
Head of Office, Statistics Denmark

Jean-Robert Tyran
Professor, Department of Economics"

The English translation of the reminder letter is:

"Dear [name],

Statistics Denmark and the Internet Laboratory for Experimental Economics (iLEE) at the Department of Economics, University of Copenhagen invited you about two weeks ago to participate in an experiment concerning economic decision processes. You were invited, because you completed a similar experiment in May 2008. Your participation is valuable to us as it is interesting for us to see how decisions made in the two experiments are related.

The experiment is open until Sunday July 19 so everyone has the opportunity to complete it. If you have already initiated the experiment you will continue where you let go when you log in again.

The website will be inaccessible some days during the period, because we move our server. In that case, please try again at some other point in time.

To see the details of the experiment such as the task, duration etc. we kindly ask you to log in to our website as soon as possible:

www.econ.ku.dk/ilee with your log in number: **finalidnumber**n

w In case you encounter problems logging in or if you have any questions, you are welcome to contact the Department of Economics by email **ilee@econ.ku.dk** or by phone 35 32 44 09 on Wednesdays between 2pm and 3pm.

Kind regards and thank you in advance for your help.

Isak Isaksen
Head of Office, Statistics Denmark

Jean-Robert Tyran
Professor, Department of Economics"

Figure D.1: Invitation letter to iLEE2 in 2009

«Navn»
«Coadr»
«Adresse»
«By»
«Post» «Postdist»

Kære «Navn»

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterede dig i maj måned 2008 til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser. Dette eksperiment gennemførte du, og vi inviterer dig derfor hermed til at deltage i et opfølgende eksperiment.

Din deltagelse er naturligvis frivillig, men vi håber meget, at du igen vil deltage, da det er interessant for os at se, hvordan beslutninger i de to forskellige eksperimenter hænger sammen. Dine beslutninger i eksperimentet bliver behandlet strengt fortroligt og anonymt.

Ved at deltage i eksperimentet får du mulighed for at tjene penge. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. De nærmere regler er beskrevet på hjemmesiden.

For at sikre deltagerne fuld anonymitet logger alle deltagere ind med et tilfældigt udvalgt nummer. For at se detaljerne om eksperimentet, herunder opgaven, tidsforbrug mv., bedes du snarest muligt logge ind på vores hjemmeside:

www.econ.ku.dk/ilee med dit login-nummer: **«finalid_number»**

Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte os enten ved at sende en email til **ilee@econ.ku.dk** eller ved at ringe til os på telefon 35 32 44 09.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen
Kontorchef, Danmarks Statistik

Jean-Robert Tyran
Professor, Økonomisk Institut

Danmarks Statistik
Sejrøgade 11
2100 København Ø

Tlf. 3917 3917
Fax. 3917 3999
CVR 17-1504-13

dst@dst.dk
www.dst.dk

Figure D.2: Reminder letter to iLEE2 in 2009

«Navn»
«Coadr»
«Adresse»
«By»
«Post» «Postdist»

Kære «Navn»

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterede dig for godt to uger siden til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser. Du blev inviteret, fordi du i maj måned 2008 gennemførte et lignende eksperiment. Din deltagelse er værdifuld for os, da det er interessant for os at se, hvordan beslutninger i de to forskellige eksperimenter hænger sammen.

Eksperimentet er åbent til og med søndag d. 19. juli, så alle får mulighed for at gennemføre. Hvis du ikke har logget ind endnu, håber vi, at du vil vælge at gøre det nu. Hvis du allerede har påbegyndt eksperimentet, vil du fortsætte, hvor du slap, når du logger ind igen.

Enkelte dage i perioden vil hjemmesiden være utilgængelig pga. flytning af vores server. Prøv i givet fald igen på et andet tidspunkt.

For at se detaljerne om eksperimentet, herunder tidsforbrug, indtjeningsvilkår mv., bedes du snarest muligt logge ind på vores hjemmeside:

www.econ.ku.dk/ilee med dit login-nummer: **«finalid_number»**

Vi håber, at du vælger at gennemføre eksperimentet. Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte os enten ved at sende en email til **ilee@econ.ku.dk** eller ved at ringe til os på telefon 35 32 44 09 på onsdage mellem 14 og 15.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen
Kontorchef, Danmarks Statistik

Jean-Robert Tyran
Professor, Økonomisk Institut

Danmarks Statistik
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Tlf. 3917 3917
Fax. 3917 3999
CVR 17-1504-13

dst@dst.dk
www.dst.dk

D.2 Instructions

D.2.1 Instructions for the Dictator Game

[Screen 1: Instructions for part 1]
Instructions for the experiment's first part

All participants in the experiment initially receive 75 DKK.

You are now involved in **2** decision situations.

In each situation, you will be randomly matched with another participant. (It will not be the same participant.)

Your decision

In one situation, you are the decision maker. **You must decide how you wish to divide the total amount that you and the other participant have been given (75 + 75 = 150 DKK) between the two of you.**

The initial situation is shown below.

Figure D.3: Illustration explaining the initial situation in the Dictator Game

Translated text: "Udgangspunkt"=Initial situation. "Dig"=You. "Den anden"=The other one.

[Continue]

[Screen 2: Instructions for part 2]
The other situation

In the other situation, the other participant is the decision maker. He or she must make a similar decision about how he or she wants to divide the total amount that you have initially been given.

Outcome

Only one of the situations will be selected for payments. Both you and that other participant will be paid according to the decision made in that situation. It is equally likely that you will be paid according to the situation in which you are the decision maker as it is that you will be paid according to the situation in which the other participant is the decision maker.

[Go back] [Continue]

[Screen 3: Decision screen]
Your decision

Pick one of the options below and click **Submit decision**.

Figure D.4: Screenshot of the decision screen in the Dictator Game

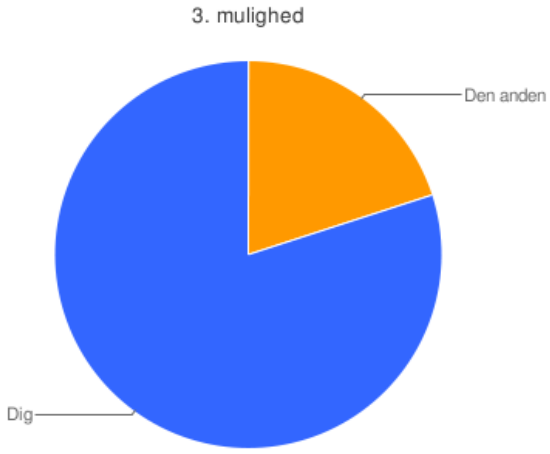
	Fordeling (dig - den anden)	Du får	Den anden får	Din beslutning
1	100% - 0%	150 kr.	0 kr.	○
2	90% - 10%	135 kr.	15 kr.	○
3	80% - 20%	120 kr.	30 kr.	○
4	70% - 30%	105 kr.	45 kr.	○
5	60% - 40%	90 kr.	60 kr.	○
6	50% - 50%	75 kr.	75 kr.	○
7	40% - 60%	60 kr.	90 kr.	○
8	30% - 70%	45 kr.	105 kr.	○
9	20% - 80%	30 kr.	120 kr.	○
10	10% - 90%	15 kr.	135 kr.	○
11	0% - 100%	0 kr.	150 kr.	○

Translated text: "Fordeling (dig - den anden)"=Division (you - the other). "Du får"=You get. "Den anden får"=The other gets. "Din beslutning"=Your decision

[See the instructions again] [Submit decision]

Figure D.5: Screenshot of the decision screen in the Dictator Game after clicking 80-20

	Fordeling (dig - den anden)	Du får	Den anden får	Din beslutning
1	100% - 0%	150 kr.	0 kr.	○
2	90% - 10%	135 kr.	15 kr.	○
3	80% - 20%	120 kr.	30 kr.	◉
4	70% - 30%	105 kr.	45 kr.	○
5	60% - 40%	90 kr.	60 kr.	○
6	50% - 50%	75 kr.	75 kr.	○
7	40% - 60%	60 kr.	90 kr.	○
8	30% - 70%	45 kr.	105 kr.	○
9	20% - 80%	30 kr.	120 kr.	○
10	10% - 90%	15 kr.	135 kr.	○
11	0% - 100%	0 kr.	150 kr.	○



Translated text: "Fordeling (dig - den anden)"=Division (you - the other). "Du får"=You get. "Den anden får"=The other gets. "Din beslutning"=Your decision. "3. mulighed"=3rd option. "Dig"=You. "Den anden"=The other one.

D.2.2 Instructions for the Trust Game

[Screen 1: Instructions for part 1]
Instructions for first part of the experiment

You are involved in two decision situations.

In each situation you are randomly matched with another participant. It is not the same participant in the two situations.

Only one of the situations will matter for payments. Both you and the other participant will be paid according to this situation. It is randomly determined which situation will matter for payments. It is equally likely that you will be paid according to one situation or the other.

[Continue >>]

[Screen 2: Instructions for the first situation]
First situation

In this situation you are the **first decision maker** and another participant is the **second decision maker**.

You have to choose between IN and OUT.

If you choose OUT, then the second decision maker will not affect the outcome of the situation.

If you choose IN, then the outcome will depend on the choice of the second decision maker. The second decision maker chooses between RIGHT and LEFT **without** knowing whether you have chosen IN or OUT.

The table shows the three possible outcomes:

.	**You receive**	**The first decision maker receives**
If you choose **OUT**	50 DKK	20 DKK
If you choose **IN** and the second decision maker chooses **LEFT**	20 DKK	90 DKK
If you choose **IN** and the second decision maker chooses **RIGHT**	80 DKK	40 DKK

The situation is illustrated here:

[See Figure D.6.]

What do you choose?

[ ] OUT: You receive 50 DKK and the other decision maker receives 20 DKK.
[ ] IN: The other decision maker's decision between LEFT and RIGHT will determine the outcome.

[Submit decision]

[Screen 3: Instructions for the second situation]
Second situation

In this situation you are the second decision maker while the other participant is the first decision maker. It is not the same participant as in the first situation.

If the first decision maker chooses OUT then your decision, which is illustrated below, will not affect the outcome of the situation.

If the first decision maker instead chooses IN then the outcome will depend on your choice.

You have to choose between LEFT and RIGHT without knowing whether the first decision maker has chosen IN or OUT.

The table shows the three possible outcomes:

.	**The first decision maker receives**	**You receive**
If the first decision maker chooses **OUT**	50 DKK	20 DKK
If the first decision maker chooses **IN** and and you choose **LEFT**	20 DKK	90 DKK
If the first decision maker chooses **IN** and		

and you choose **RIGHT**	80 DKK	40 DKK

The situation is illustrated here:

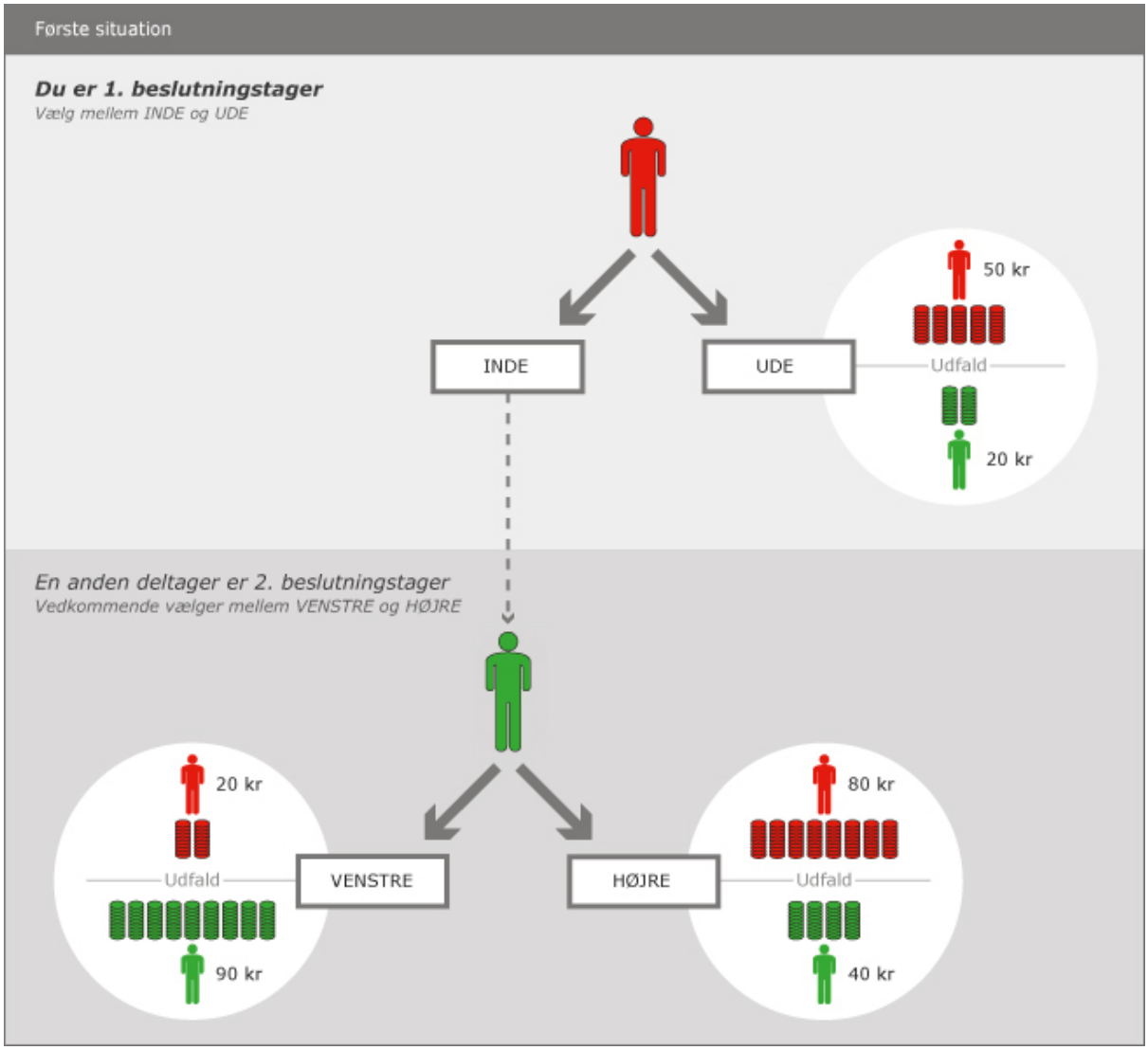
[See Figure D.7.]

Given that the first decision maker chooses IN, what do you choose?

[ ] LEFT: You receive 90 DKK and the other decision maker receives 20 DKK.
[ ] RIGHT: You receive 40 DKK and the other decision maker receives 80 DKK.

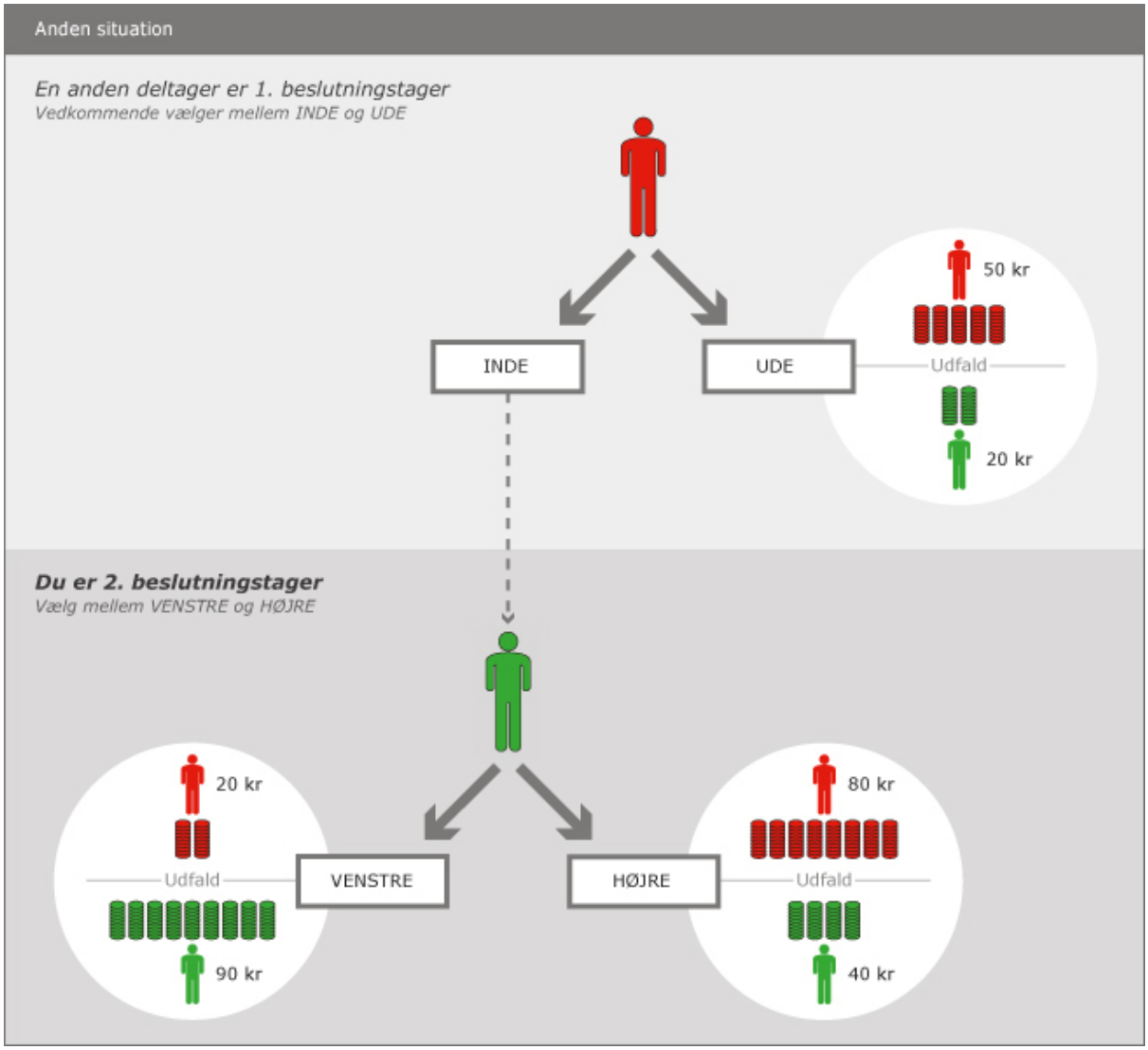
[Submit decision]

Figure D.6: Illustration of the First-Mover's decision problem (Screen 2)



Translated text: "INDE" = IN. "UDE" = OUT. "VENSTRE" = LEFT. "HØJRE" = RIGHT. "kr." = DKK. "UDFALD" = OUTCOME.

Figure D.7: Illustration of the Second-Mover's decision problem (Screen 3)



Translated text: "INDE" = IN. "UDE" = OUT. "VENSTRE" = LEFT. "HØJRE" = RIGHT. "kr." = DKK. "UDFALD" = OUTCOME.

D.3 Results

I provide additional descriptive statistics and results in Section D.3.1 and robustness checks of the results presented in the chapter in Section D.3.2.

D.3.1 Additional results

In this section, I present the following additions to the chapter:

Figure D.8: Histograms of both paternal and maternal educational attainment.

Table D.1: Regressions of gross income on educational attainment.

Table D.2: Regression of subject's educational attainment on parental educational attainment.

Table D.3: Regressions of Dictator Game choice on own characteristics.

Table D.4: Regressions of Trust Game choice on own characteristics.

Table D.5: Regressions of Danish parents' values on their educational attainment.

Figure D.8: Distribution of parental educational attainment

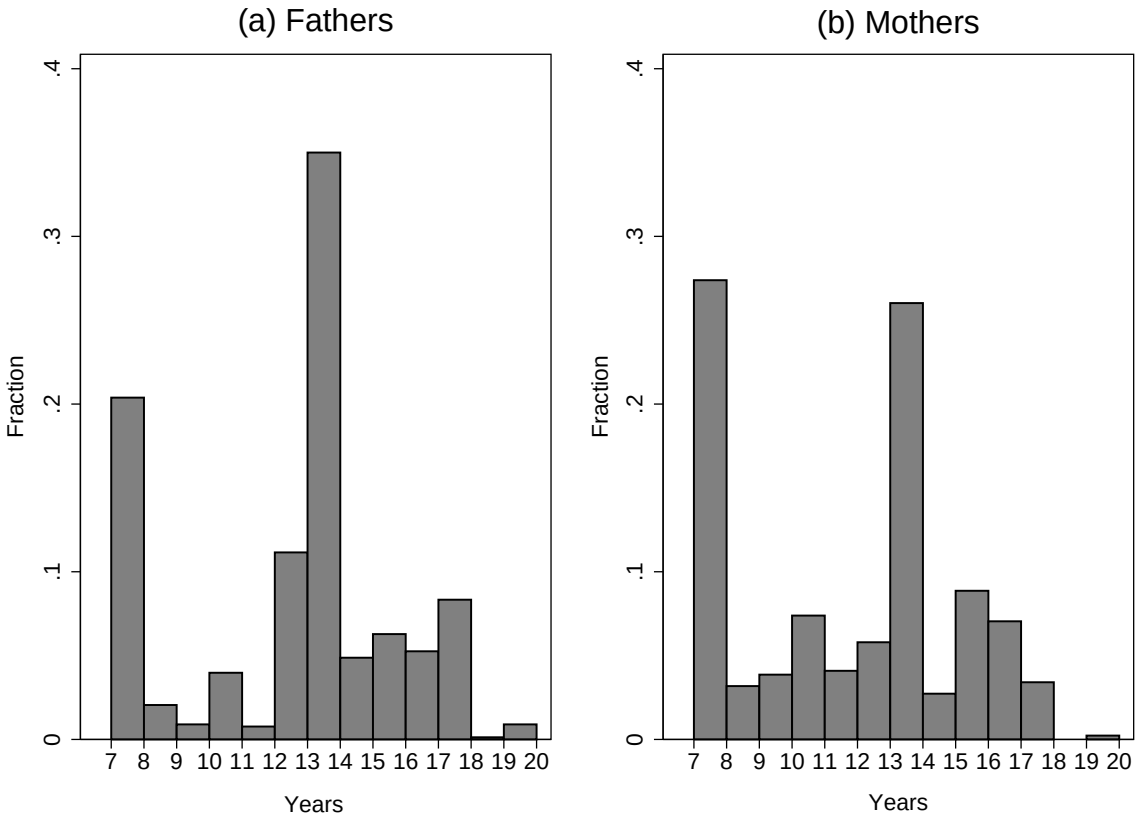


Table D.1: Income and educational attainment

	(1)	(2)
Years of education	0.26***	0.17***
	(0.0037)	(0.0036)
Age		0.027***
		(0.00070)
Female		-0.58***
		(0.020)
Employed		2.07***
		(0.024)
Single-person household		0.017
		(0.025)
Metropolitan residence		-0.065**
		(0.024)
Constant	-0.28***	-1.51***
	(0.046)	(0.062)
N	38706	38706
R^2	0.112	0.271

Notes: OLS regressions with own gross income in 2009 in 00'000 DKK as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.2: Intergenerational dependency in educational attainment

	(1)	(2)	(3)
Father's years of education	0.11***	0.11***	0.11***
	(0.0062)	(0.0057)	(0.0057)
Mother's years of education	0.088***	0.14***	0.14***
	(0.0063)	(0.0061)	(0.0061)
Age		0.065***	0.068***
		(0.0018)	(0.0019)
Female		0.25***	0.25***
		(0.033)	(0.033)
Employed		1.48***	1.47***
		(0.046)	(0.046)
Single-person household		-0.20***	-0.21***
		(0.044)	(0.044)
Urban residence		0.63***	0.61***
		(0.038)	(0.038)
Father's age			0.0061
			(0.0045)
Mother's age			0.032***
			(0.0051)
Constant	10.7***	6.22***	5.13***
	(0.074)	(0.12)	(0.16)
N	16566	16566	16566
R^2	0.056	0.200	0.206

Notes: OLS regressions with own education in years as the dependent variable. Standard errors in parentheses. Father's/Mother's age is measured at the parents' ages on January 1 in the subject's birth year.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.3: Regressions of Dictator Game choice on subject's own characteristics

	(1)	(2)	(3)	(4)
Years of education	-0.0059	-0.0081*		
	(0.0040)	(0.0041)		
Log(gross income)			0.00036	-0.028
			(0.012)	(0.017)
Age		0.0047***		0.0052***
		(0.00086)		(0.00096)
Female		0.027		0.022
		(0.017)		(0.017)
Employed		-0.044		-0.033
		(0.028)		(0.031)
Single-person household		0.033		0.033
		(0.024)		(0.024)
Metropolitan residence		0.0091		0.0025
		(0.019)		(0.019)
Constant	0.39***	0.27***	0.31***	0.16***
	(0.055)	(0.060)	(0.016)	(0.041)
N	740	740	739	739
R^2	0.003	0.046	0.000	0.045

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.4: Regressions of Trust Game choice on subject's own characteristics

	(1)	(2)	(3)	(4)
Years of education	-0.018*	-0.021**		
	(0.007)	(0.008)		
Log(gross income)			0.011	-0.017
			(0.022)	(0.024)
Age		0.004*		0.004*
		(0.002)		(0.002)
Female		-0.071*		-0.074*
		(0.032)		(0.033)
Employed		-0.000		-0.004
		(0.043)		(0.044)
Single-person household		0.040		0.046
		(0.050)		(0.050)
Metropolitan residence		0.032		0.018
		(0.038)		(0.038)
N	718	718	718	718

Notes: Logit regressions with the dependent variable a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.5: Regressions of Danish parents' values on their educational attainment across interview years

	1990	1999	2008
Low education	-0.015	0.018	0.044
	(0.043)	(0.046)	(0.036)
Age	-0.002	-0.004**	-0.002
	(0.0015)	(0.001)	(0.001)
Female	0.082*	-0.003	0.038
	(0.040)	(0.038)	(0.029)
Employed	0.060	-0.021	0.049
	(0.050)	(0.048)	(0.041)
Categories chosen	0.170***	0.105**	0.083**
	(0.041)	(0.036)	(0.029)
N	687	742	1148

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose "think of others" as an important trait to teach children. Coefficients are dy/dx at mean with standard errors in parentheses. The data presented in this table is from the Danish Values Survey, cross-section 1981-2008.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.3.2 Robustness checks

In this section, I present a range of robustness checks of the regressions presented in the chapter. First, I correct for trend growth in parental educational attainment, i.e. the fact that younger cohorts on average are better-educated than older cohorts. I do this by identifying the 1st quartile of both the father's and the mother's schooling separately for each birth-year cohort in the full and representative sample of the Danish adult population which contains 40,000 individuals (see Section 5.2 in the chapter). I find, for instance, that the 1st quartile paternal and maternal schooling for those born in 1989 is 12 and 11 years, respectively. This is more than the 1st quartile of paternal and maternal schooling for those born in 1973 which amounts to 7 years for both fathers and mothers. I then create a dummy variable that takes value 1 if both the paternal and maternal educational attainment for each birth-year cohort is within the 1st quartile. This means that the variable, for those born in 1989, takes value 1 if the father's educational attainment is maximally 12 years *and* if, at the same time, the mother's educational attainment is maximally 11 years.

As a second robustness check, I create a linear variable of parental educational attainment by taking the sum of the two parents' educational attainments. Third, I replace the dependent variable (τ) by a binary variable that takes value 1 if the subject was selfish, i.e. shared the subject shared nothing with the Recipient. For the Trust Game, I present similar robustness checks.

My robustness checks are listed below:

Table D.6: OLS regressions of generosity in **Dictator Game** on parental educational attainment corrected for trend growth.

Table D.7: OLS regressions of generosity in **Dictator Game** on sum of parental educational attainment.

Table D.8: LPM regressions of sharing nothing with the Recipient in the **Dictator Game** on parental educational attainment.

Table D.9: OLS regressions of generosity in **Dictator Game** on parental educational attainment corrected for trend growth, by subjects' age.

Table D.10: OLS regressions of generosity in **Dictator Game** on sum of parental educational attainment, by subjects' age.

Table D.11: LPM regressions of sharing nothing with the Recipient in the **Dictator Game** on parental educational attainment, by subjects' age.

Table D.12: OLS regressions of generosity in **Dictator Game** on parental educational attainment, by subjects' age (alternative age categorization).

Table D.13: OLS regressions of generosity in **Dictator Game** on parental educational attainment corrected for trend growth, by subjects' gender.

Table D.14: OLS regressions of generosity in **Dictator Game** on sum of parental educational attainment, by subjects' gender.

Table D.15: LPM regressions of sharing nothing with the Recipient in the **Dictator Game** on parental educational attainment, by subjects' gender.

Table D.16: Logit regressions of generosity in **Trust Game** on parental educational attainment corrected for trend growth.

Table D.17: Logit regressions of generosity in **Trust Game** on sum of parental educational attainment.

Table D.18: LPM regressions of generosity in **Trust Game** on parental educational attainment.

Table D.19: Logit regressions of generosity in **Trust Game** on parental educational attainment corrected for trend growth, by subjects' age.

Table D.20: Logit regressions of generosity in **Trust Game** on sum of parental educational attainment, by subjects' age.

Table D.21: LPM regressions of generosity in **Trust Game** on parental educational attainment, by subjects' age.

Table D.22: Logit regressions of generosity in **Trust Game** on parental educational attainment, by subjects' age (alternative categorization).

Table D.23: Logit regressions of generosity in **Trust Game** on parental educational attainment corrected for trend growth, by subjects' gender.

Table D.24: Logit regressions of generosity in **Trust Game** on sum of parental educational attainment, by subjects' gender.

Table D.25: LPM regressions of generosity in **Trust Game** on parental educational attainment, by subjects' gender.

Table D.6: Regressions of Dictator Game sharing on parental educational attainment corrected for trend growth

	(1)	(2)	(3)
Low-educated parents	0.047	0.021	0.018
	(0.024)	(0.025)	(0.025)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	741	741	741
R^2	0.005	0.041	0.046

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.7: Regressions of Dictator Game sharing on sum of parental educational attainment

	(1)	(2)	(3)
Sum of parents' education	-0.0033*	-0.00069	-0.00026
	(0.0015)	(0.0016)	(0.0017)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	741	741	741
R^2	0.006	0.041	0.046

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.8: Regressions of sharing nothing in Dictator Game on parental educational attainment

	(1)	(2)	(3)
Low-educated parents	-0.089*	-0.041	-0.035
	(0.043)	(0.045)	(0.045)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	741	741	741
R^2	0.006	0.030	0.035

Notes: LPM regressions with a dummy that takes value 1 if the subject shared nothing with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.9: Regressions of Dictator Game sharing on parental educational attainment corrected for trend growth, by subjects' age

	19-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.014	0.0010	-0.012	0.040	0.019	0.016
	(0.071)	(0.073)	(0.073)	(0.026)	(0.026)	(0.026)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	223	223	223	518	518	518
R^2	0.000	0.035	0.056	0.005	0.059	0.061

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.10: Regressions of Dictator Game sharing on on sum of parental educational attainment, by subjects' age

	19-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of parents' education	0.0013	0.0025	0.0053	-0.0030	-0.0014	-0.0012
	(0.0038)	(0.0040)	(0.0041)	(0.0018)	(0.0018)	(0.0018)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	223	223	223	518	518	518
R^2	0.000	0.037	0.063	0.006	0.059	0.061

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.11: Regressions of sharing nothing in Dictator Game on parental educational attainment, by subjects' age

	19-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.10	-0.052	-0.0027	-0.068	-0.033	-0.032
	(0.13)	(0.13)	(0.13)	(0.046)	(0.046)	(0.047)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	223	223	223	518	518	518
R^2	0.003	0.043	0.073	0.004	0.050	0.051

Notes: LPM regressions with a dummy that takes value 1 if the subject shared nothing with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.12: Regressions of Dictator Game sharing on parental educational attainment, by subjects' age (alternative categorization)

	19-25 years			26+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.018	-0.045	-0.053	0.037	0.0082	0.0033
	(0.074)	(0.078)	(0.079)	(0.023)	(0.023)	(0.024)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	141	141	141	600	600	600
R^2	0.000	0.028	0.035	0.004	0.052	0.058

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.13: Regressions of Dictator Game sharing on parental educational attainment corrected for trend growth, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.051	0.011	0.004	0.041	0.027	0.030
	(0.037)	(0.037)	(0.037)	(0.032)	(0.033)	(0.033)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	383	383	383	358	358	358
R^2	0.005	0.066	0.074	0.004	0.022	0.030

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.14: Regressions of Dictator Game sharing on sum of parental educational attainment, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of parents' education	-0.004	0.001	0.001	-0.003	-0.002	-0.002
	(0.0022)	(0.0024)	(0.0024)	(0.0021)	(0.0023)	(0.0023)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	383	383	383	358	358	358
R^2	0.007	0.066	0.075	0.005	0.021	0.028

Notes: OLS regressions with the fraction shared with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.15: Regressions of Dictator Game sharing on parental educational attainment, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.13*	-0.047	-0.031	-0.043	-0.031	-0.036
	(0.064)	(0.065)	(0.067)	(0.058)	(0.060)	(0.061)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	383	383	383	358	358	358
R^2	0.011	0.065	0.071	0.002	0.008	0.016

Notes: LPM regressions with a dummy that takes value 1 if the subject shared nothing with the Recipient as the dependent variable. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.16: Regressions of generosity in the Trust Game on parental educational attainment corrected for trend growth

	(1)	(2)	(3)
Low-educated parents	0.046	0.037	0.052
	(0.049)	(0.050)	(0.051)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	719	719	719

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.17: Regressions of generosity in the Trust Game on sum of parental educational attainment

	(1)	(2)	(3)
Sum of parents' education	-0.003	-0.001	-0.003
	(0.003)	(0.003)	(0.003)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	719	719	719

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.18: LPM regressions of generosity in the Trust Game on parental educational attainment

	(1)	(2)	(3)
Low-educated parents	0.033	0.024	0.040
	(0.042)	(0.043)	(0.044)
Controls			
Own characteristics	No	Yes	Yes
Parents' ages	No	No	Yes
N	719	719	719
R^2	0.001	0.014	0.021

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.19: Regressions of generosity in the Trust Game on parental educational attainment corrected for trend growth, by subjects' age

	20-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.148	-0.114	-0.124	0.068	0.059	0.067
	(0.085)	(0.105)	(0.087)	(0.054)	(0.055)	(0.057)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	211	211	211	508	508	508

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.20: Regressions of generosity in the Trust Game on sum of parental educational attainment, by subjects' age

	20-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of parents' education	0.013*	0.011	0.010	-0.005	-0.005	-0.005
	(0.007)	(0.007)	(0.007)	(0.003)	(0.004)	(0.004)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	211	211	211	508	508	508

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.21: LPM regressions of generosity in the Trust Game on parental educational attainment, by subjects' age

	20-30 years			31+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.11	-0.062	-0.023	0.047	0.038	0.046
	(0.11)	(0.11)	(0.11)	(0.047)	(0.048)	(0.048)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	211	211	211	508	508	508
R^2	0.005	0.034	0.071	0.002	0.013	0.016

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.22: Regressions of generosity in the Trust Game on parental educational attainment, by subjects' age (alternative categorization)

	20-25 years			26+ years		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	-0.142	-0.112	-0.098	0.047	0.034	0.044
	(0.102)	(0.118)	(0.108)	(0.046)	(0.046)	(0.048)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	138	138	138	581	581	581

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.23: Regressions of generosity in the Trust Game on parental educational attainment corrected for trend growth, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.136	0.119	0.136	-0.017	-0.030	-0.016
	(0.078)	(0.081)	(0.082)	(0.059)	(0.058)	(0.061)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	374	374	374	345	345	345

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.24: Regressions of generosity in the Trust Game on sum of parental educational attainment, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of parents' education	-0.004	-0.003	-0.004	-0.001	0.000	-0.001
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	374	374	374	345	345	345

Notes: Logit regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Coefficients are dy/dx at mean with standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.25: LPM regressions of generosity in the Trust Game and parental educational attainment, by subjects' gender

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Low-educated parents	0.080	0.062	0.080	0.0059	-0.0072	0.0070
	(0.064)	(0.067)	(0.068)	(0.054)	(0.056)	(0.057)
Controls						
Own characteristics	No	Yes	Yes	No	Yes	Yes
Parents' ages	No	No	Yes	No	No	Yes
N	374	374	374	345	345	345
R^2	0.004	0.014	0.020	0.000	0.004	0.014

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject chose RIGHT as the Second-Mover in Trust Game. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix E

Does Shared Responsibility Breed Unfairness?

E.1 Mailings

Statistics Denmark sent out invitations and reminders on our behalf. Figure E.1 shows the invitation letter sent out on June 14, 2011 and Figure E.2 shows the reminder letter sent out to irresponsive invitees on June 28, 2011.

E.1.1 Translation of invitation letter

"**Dear [First name]**

Statistics Denmark and the Internet Laboratory for Experimental Economics (iLEE) at the Department of Economics, University of Copenhagen hereby invite you to participate in an experiment concerning economic decision processes as you have previously completed one or more of our experiments.

Your participation is, of course, voluntary, but we very much hope that you want to participate again as it is interesting for us to see how decisions made in different experiments are related. **This experiment is open for participation until Sunday July 24, 2011.**

By participating in the experiment, you will have the opportunity to earn money. We cannot guarantee that you will earn a specific amount since your payment will depend on your own and other participants' decisions. The more precise rules are specified on the website.

Your decisions in the experiment will be handled with strict confidentiality and anonymity. In order to ensure the participants' perfect anonymity, all participants log in with a randomly determined number. To see the details of the experiment such as the task, duration etc. we kindly ask you to log in to our website as soon as possible:

ilee.econ.ku.dk with your login number: <<**login code**>>

If you encounter problems logging in or if you have questions, then you are welcome to contact us by sending an email to ilee econ.ku.dk or by calling us on telephone 35 32 39 97.

Kind regards and thank you in advance for your help.

Isak Isaksen
Head of Office, Statistics Denmark

Jean-Robert Tyran
Professor, Department of Economics"

E.1.2 Translation of reminder letter

"**Dear [First name]**

Statistics Denmark and the Internet Laboratory for Experimental Economics (iLEE) at the Department of Economics, University of Copenhagen hereby invite you to participate in an experiment concerning economic decision processes as you have previously completed one or more of our experiments.

We have previously sent you an invitation to this experiment, but since you had not completed the experiment by June 28, 2011 at 14.00 o'clock we allow ourselves to send you a kind reminder.

Your participation is, of course, voluntary, but we very much hope that you want to participate again as it is interesting for us to see how decisions made in different experiments are related. **This experiment is open for participation until Sunday July 24, 2011.**

By participating in the experiment, you will have the opportunity to earn money. We cannot guarantee that you will earn a specific amount since your payment will depend on your own and other participants' decisions. The more precise rules are specified on the website.

Your decisions in the experiment will be handled with strict confidentiality and anonymity. In order to ensure the participants' perfect anonymity, all participants log in with a randomly determined number. To see the details of the experiment such as the task, duration etc. we kindly ask you to log in to our website as soon as possible:

ilee.econ.ku.dk with your login number: <<**login code**>>

If you encounter problems logging in or if you have questions, then you are welcome to contact us by sending an email to ilee econ.ku.dk or by calling us on telephone 35 32 39 97.

Kind regards and thank you in advance for your help.

Isak Isaksen
Head of Office, Statistics Denmark

Jean-Robert Tyran
Professor, Department of Economics"

Figure E.1: Invitation letter to iLEE4 in 2011

«Navn»
«Adresselabel»
«Postnr» «Postnavn»

Referencenr.: «Respnr»

Kære [Fornavn]

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterer dig hermed til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser, da du tidligere har gennemført et eller flere af vores eksperimenter.

Din deltagelse er naturligvis frivillig, men vi håber meget, at du igen vil deltage, da det er interessant for os at se, hvordan beslutninger i de forskellige eksperimenter hænger sammen. **Dette eksperiment er åbent for deltagelse til og med søndag d. 24. juli 2011.**

Ved at deltage i eksperimentet får du mulighed for at tjene penge. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. De nærmere regler er beskrevet på hjemmesiden.

Dine beslutninger i eksperimentet bliver behandlet strengt fortroligt og anonymt. For at sikre deltagerne fuld anonymitet logger alle deltagere ind med et tilfældigt udvalgt nummer. For at se detaljerne om eksperimentet, herunder opgaven, tidsforbrug mv., bedes du snarest muligt logge ind på vores hjemmeside:

ilee.econ.ku.dk med dit login-nummer: **«logincode»**

Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte os enten ved at sende en e-mail til **ilee@econ.ku.dk** eller ved at ringe til os på telefon 35 32 39 97.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen
Kontorchef, Danmarks Statistik

Jean-Robert Tyran
Professor, Økonomisk Institut

Figure E.2: Reminder letter to iLEE4 in 2011

«Navn»
«Adresselabel»
«Postnr» «Postnavn»

Referencenr.: «Respnr»

Kære «navn»

Danmarks Statistik og Internet Laboratoriet for Eksperimentel Økonomi (iLEE) ved Økonomisk Institut på Københavns Universitet inviterer dig hermed til at deltage i et eksperiment vedrørende økonomiske beslutningsprocesser, da du tidligere har gennemført et eller flere af vores eksperimenter.

Vi har tidligere sendt dig en invitation til dette eksperiment, men da du d. 28. juni 2011 klokken 14.00 endnu ikke havde gennemført eksperimentet, tillader vi os at sende dig en høflig påmindelse.

Din deltagelse er naturligvis frivillig, men vi håber meget, at du igen vil deltage, da det er interessant for os at se, hvordan beslutninger i de forskellige eksperimenter hænger sammen. **Dette eksperiment er åbent for deltagelse til og med søndag d. 24. juli 2011.**

Ved at deltage i eksperimentet får du mulighed for at tjene penge. Vi kan ikke garantere dig, at du vil tjene et bestemt beløb, idet din indtjening vil afhænge af dine egne samt andre deltageres beslutninger. De nærmere regler er beskrevet på hjemmesiden.

Dine beslutninger i eksperimentet bliver behandlet strengt fortroligt og anonymt. For at sikre deltagerne fuld anonymitet logger alle deltagere ind med et tilfældigt udvalgt nummer. For at se detaljerne om eksperimentet, herunder opgaven, tidsforbrug mv., bedes du snarest muligt logge ind på vores hjemmeside:

ilee.econ.ku.dk med dit login-nummer: **«logincode»**

Hvis du har problemer med at logge ind eller har yderligere spørgsmål, er du velkommen til at kontakte os enten ved at sende en e-mail til **ilee@econ.ku.dk** eller ved at ringe til os på telefon 35 32 39 97.

Med venlig hilsen og på forhånd tak for din hjælp.

Isak Isaksen
Kontorchef, Danmarks Statistik

Jean-Robert Tyran
Professor, Økonomisk Institut

E.2 Translation of screens

In this section, we present translated text from the experiment's computer screens. Original text was in Danish. We show text for the Committee treatment. The Representative treatment used the same instructions except for text indicated in brackets. In Section E.2.1, we present the text from the screens with instructions and control questions. In Section E.2.2, we present the text from the decision-makers' decision screens and in Section E.2.3, we present the text from the recipients' decision screens. These decision screens were presented to the subjects in randomized order as they participated in the experiment.

Subjects were able to review the instructions at any time by clicking the respective button in the top-right corner of their screen.

E.2.1 Instructions and control questions

[Screen 1: Introduction to experiment]
Instructions

You will now take part in four political decision situations together with 5 other participants in the experiment.

3 of you are parties while the other 3 are voters. You are now asked to read the instructions for both the parties and the voters. You will later be informed whether you are a party or a voter.

[Continue >>]

[Screen 2: Instructions for parties]
Instructions (continued)

Parties

The parties must decide how a money sum should be distributed between the parties themselves and the voters. The parties can pass or fail a given distribution.

The 3 parties have a total of 60 seats in the parliament, but not necessarily equally many seats. Therefore, please pay attention to how many seats each party has.

If a proposal receives support from at least 31 seats among the parties, then there is a majority in favor of the proposal and it is thus passed. Each party and each voter receives the corresponding payment. **If a proposal receives support from 30 seats or less**, then the proposal is failed and each party and each voter receives 0 DKK. All parties receive the same payment independent of whether they individually voted for or against the proposal and how many seats they have.

Press "Continue" to see an example.

[Continue >>]

[Screen 3: Example for parties]
Instructions (continued)

Example

A proposal could be: Each party receives **40 DKK** and each voter receives **20 DKK**. If the proposal is failed, then both parties and voters receive **0 DKK**.

The parties must decide whether to pass or fail the proposal. They do this by voting on it.

In this example, **Party 1** has 30 seats, **Party 2** has 20 seats, and **Party 3** has 10 seats. Together, they have 60 seats.

Assume now that Party 1 votes **yes** while Party 2 and Party 3 vote **no**. The proposal receives support from 30 seats and is failed since there is not a majority in favor of it. Both parties and voters thus receive **0 DKK**.

Assume instead that Party 1 and Party 3 vote **yes** while Party 2 votes **no**. The proposal now receives support from 40 seats and is passed since a majority vote for it. Every party thus receives **40 DKK** and each voter receives **20 DKK**.

[Continue >>]

[Screen 4: Instructions for voters]
Instructions (continued)

Voters

The voters have the opportunity to **punish** the individual parties, both if the proposal is passed and if it is failed.

The voters are informed about each proposal and how many seats each party has. But the voters are not told what the parties individually have voted. It is therefore not clear which parties decided the outcome of the parties' voting.

It is costly to punish the parties. **For each 1 DKK that a voter punishes a party**, it costs the party **1 DKK** and the voter **0.10 DKK**. Every voter can maximally punish the parties by **40 DKK** in total which means that the voter's maximal cost is **4 DKK**.

Press "Continue" to see an example.

[Continue >>]

[Screen 5: Example for voters]
Instructions (continued)

Example

A proposal could be: Each party receives **40 DKK** and each voter receives **20 DKK**. If the proposal is failed, then both parties and voters receive **0 DKK**.

The parties must decide whether to pass or fail the proposal. They do this by voting on it.

In this example, **Party 1** has 30 seats, **Party 2** has 20 seats, and **Party 3** has 10 seats. Together, they have 60 seats.

Every voter now has the opportunity to punish the parties. The voter can punish the parties if the proposal is passed and if it is failed. The voters must fill out a table as the one you see below.

	Party 1 30 seats	**Party 2** 20 seats	**Party 3** 10 seats	**Sum**
If the proposal is **passed**	**0 DKK**	**5 DKK**	**5 DKK**	**10 DKK**
If the proposal is **failed**	**25 DKK**	**5 DKK**	**5 DKK**	**35 DKK**

If the proposal is passed (top row) then the voter punishes Party 2 and Party 3 by 5 DKK each (**10 DKK in total**). Party 1 is not punished. This costs the voter **1 DKK** in total.

If the proposal is failed (bottom row) then the voter punishes Party 1 by 25 DKK while Party 2 and Party 3 are punished by 5 DKK each (**35 DKK in total**). This costs the voter **3.50 DKK** in total.

[Continue >>]

[Screen 6: Explanation of payments]
Payment

There are four proposals in total and both parties and voters are paid the outcome of exactly one randomly determined decision situation. You do not know which decision situation will be picked for payments. It is therefore in all situations best to make a decision as if this one would determine your payments.

Note that the parties' payments also depend on the voters' decisions and likewise that the voters' payments also depend on the parties' decisions. You can see below how the parties' and voters' payments are calculated.

Parties

If the proposal is **passed**, then each party receives the according payment minus the punishment which the voters might put on them. If the proposal is **failed**, then each party receives **0 DKK** minus the punishment which the voters might put on them. Each party can receive a punishment between **0** and **120 DKK** in total.

Voters

If the proposal is **passed**, then each voter receives the according payment minus the cost of the voter's punishment. If the proposal is **failed**, then each voter receives **0 DKK** minus the cost of the voter's punishment. Each voter can maximally punish the parties by **40 DKK** in total and each voter's maximal cost is therefore **4 DKK**.

Note that it is possible to lose money in this part of the experiment.

[Continue >>]

[Screen 7: Summary of instructions]
Recall that...

- The 3 parties must vote yes or no to four proposals about how a money sum should be distributed between parties and voters.
- The 3 parties have 60 seats together, but not necessarily equally many seats.
- If a proposal has at least 31 seats in support, then it is passed.
- If a proposal has 30 seats or less in support, then it is failed.
- The 3 voters can punish the parties, both if the proposal is passed and if it is failed.
- Each voter can punish the parties by between 0 and 40 DKK in total. A party's maximal punishment is therefore 120 DKK.
- It costs the voter 0.10 DKK to punish a party by 1 DKK.
- Each party's payment is the outcome of the parties' decision minus the punishment of the voters.
- Each voter's payment is the outcome of the parties' decision minus the cost from punishing the parties.
- Losses are possible and losses will be deducted from your earnings in other parts of the experiment.

[Continue >>]

[Screen 8: Control questions]
Have you understood the instructions?

You are kindly asked to answer these questions before you can proceed.

1. How large a punishment can the parties maximally receive in total? (in DKK)

[ ]

2. How small a punishment can the parties minimally receive in total? (in DKK)

[ ]

3. The voters can punish the parties if the proposal is:

( ) Passed

( ) Failed

( ) In both cases

4. What is the least number of seats in support that is necessary to pass a proposal?

[ ]

5. If a voter punishes the parties by 10 DKK in total, how large is then the voter's cost?

( ) 0 DKK

( ) 1 DKK

( ) 10 DKK

[Continue >>]

E.2.2 Decision screens for parties

[Screen 1: Role assignment]
Your role

You are randomly chosen to be a **party**. Recall that there are 3 parties and 3 voters in total. You must make four decisions in total. One of the four decisions will randomly be chosen for payments.

You are **Party 1** and you have **20 seats** [**40 seats**] out of 60 in total.

[Continue >>]

[Screen 2: Introduction to Proposal 1]
Proposal 1

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 1: **Each party receives 33 DKK and each voter receives 27 DKK.** If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Expectation

Before you vote, we ask you to tell us what you expect the outcome to be.

Do you expect the proposal to get passed?

( ) YES
( ) NO

How big a punishment do you expect to receives from the voters in total? You can choose between 0 and 120 DKK.

If the proposal is passed:
[ ] DKK
If the proposal is failed:
[ ] DKK

[Continue >>]

[Screen 3: Decision screen for Proposal 1]
Proposal 1

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 1: **Each party receives 33 DKK and each voter receives 27 DKK.** If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Voting

Vote "YES" if you wish the proposal to get passed or "NO" if you wish the proposal to get failed.

Your vote

( ) YES
( ) NO

[Continue >>]

[Screen 4: Introduction to Proposal 2]
Proposal 2

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 2: **Each party receives 57 DKK and each voter receives 3 DKK.** If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Expectation

Before you vote, we ask you to tell us what you expect the outcome to be.

Do you expect the proposal to get passed?

( ) YES
( ) NO

How big a punishment do you expect to receives from the voters in total? You can choose between 0 and 120 DKK.

If the proposal is passed:
[ ] DKK
If the proposal is failed:
[ ] DKK

[Continue >>]

[Screen 5: Decision screen for Proposal 2]
Proposal 2

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 2: **Each party receives 57 DKK and each voter receives 3 DKK.** If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Voting

Vote "YES" if you wish the proposal to get passed or "NO" if you wish the proposal to get failed.

Your vote

( ) YES
( ) NO

[Continue >>]

[Screen 6: Introduction to Proposal 3]
Proposal 3

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 3: **Each party receives 63 DKK and each voter receives -3 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Expectation

Before you vote, we ask you to tell us what you expect the outcome to be.

Do you expect the proposal to get passed?

( ) YES
( ) NO

How big a punishment do you expect to receives from the voters in total? You can choose between 0 and 120 DKK.

If the proposal is passed:
[ ] DKK
If the proposal is failed:
[ ] DKK

[Continue >>]

[Screen 7: Decision screen for Proposal 3]
Proposal 3

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 3: **Each party receives 63 DKK and each voter receives -3 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Voting

Vote "YES" if you wish the proposal to get passed or "NO" if you wish the proposal to get failed.

Your vote

( ) YES
( ) NO

[Continue >>]

[Screen 8: Introduction to Proposal 4]
Proposal 4

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 4: **Each party receives 87 DKK and each voter receives -27 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Expectation

Before you vote, we ask you to tell us what you expect the outcome to be.

Do you expect the proposal to get passed?

( ) YES
( ) NO

How big a punishment do you expect to receives from the voters in total? You can choose between 0 and 120 DKK.

If the proposal is passed:
[ ] DKK
If the proposal is failed:
[ ] DKK

[Continue >>]

[Screen 9: Decision screen for Proposal 4]
Proposal 4

You are **Party 1** and you have **20 seats**.

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

Proposal 4: **Each party receives 87 DKK and each voter receives -27 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Voting

Vote "YES" if you wish the proposal to get passed or "NO" if you wish the proposal to get failed.

Your vote

( ) YES
( ) NO

[Continue >>]

E.2.3 Decision screens for voters

[Screen 1: Role assignment]
Your role

You are randomly chosen to be a **voter**. Recall that there are 3 parties and 3 voters in total. You must make four decisions in total. One of the four decisions will randomly be chosen for payments.

[Continue >>]

[Screen 2: Proposal 1]
Proposal 1

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

The 3 parties must decide whether to pass or fail the proposal below.

Proposal 1: **Each party receives 33 DKK and each voter receives 27 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Do you want to punish one or more of the parties?

In the row "Passed" (top row) you must indicate how much you want to punish each party if the proposal is passed. In the row "Failed" (bottom row) you must indicate how much you want to punish each party if the proposal is failed.

Proposal		**Party 1** 20 seats	**Party 2** 20 seats	**Party 3** 20 seats	**Sum**
1	If the proposal is **passed**	[ ]	[ ]	[ ]	**0 DKK**
	If the proposal is **failed**	[ ]	[ ]	[ ]	**0 DKK**

When you punish the parties by 1 DKK, it costs you 0.10 DKK. You can maximally punish the parties by 40 DKK in total given each outcome of the parties' voting.

[Continue >>]

[Screen 3: Proposal 2]
Proposal 2

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

The 3 parties must decide whether to pass or fail the proposal below. **Note that the proposal has changed.**

Proposal 2: **Each party receives 57 DKK and each voter receives 3 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Do you want to punish one or more of the parties?

In the row "Passed" (top row) you must indicate how much you want to punish each party if the proposal is passed. In the row "Failed" (bottom row) you must indicate how much you want to punish each party if the proposal is failed.

Proposal		**Party 1** 20 seats	**Party 2** 20 seats	**Party 3** 20 seats	**Sum**
2	If the proposal is **passed**	[ ]	[ ]	[ ]	**0 DKK**
	If the proposal is **failed**	[ ]	[ ]	[ ]	**0 DKK**

When you punish the parties by 1 DKK, it costs you 0.10 DKK. You can maximally punish the parties by 40 DKK in total given each outcome of the parties' voting.

[Continue >>]

[Screen 4: Proposal 3]
Proposal 3

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

The 3 parties must decide whether to pass or fail the proposal below. **Note that the proposal has changed.**

Proposal 3: **Each party receives 63 DKK and each voter receives -3 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Do you want to punish one or more of the parties?

In the row "Passed" (top row) you must indicate how much you want to punish each party if the proposal is passed. In the row "Failed" (bottom row) you must indicate how much you want to punish each party if the proposal is failed.

Proposal		**Party 1** 20 seats	**Party 2** 20 seats	**Party 3** 20 seats	**Sum**
3	If the proposal is **passed**	[ ]	[ ]	[ ]	**0 DKK**
	If the proposal is **failed**	[ ]	[ ]	[ ]	**0 DKK**

When you punish the parties by 1 DKK, it costs you 0.10 DKK. You can maximally punish the parties by 40 DKK in total given each outcome of the parties' voting.

[Continue >>]

[Screen 5: Proposal 4]
Proposal 4

The distribution of seats in the parliament is as follows: **Party 1** has 20 seats [40 seats], **Party 2** has 20 seats [10 seats], and **Party 3** has 20 seats [10 seats].

The 3 parties must decide whether to pass or fail the proposal below. **Note that the proposal has changed.**

Proposal 4: **Each party receives 87 DKK and each voter receives -27 DKK.** This means that the voters lose money. If the proposal is failed, then both parties and voters receive **0 DKK**. The proposal is passed if it has at least 31 seats in favor of it.

Do you want to punish one or more of the parties?

In the row "Passed" (top row) you must indicate how much you want to punish each party if the proposal is passed. In the row "Failed" (bottom row) you must indicate how much you want to punish each party if the proposal is failed.

Proposal		**Party 1** 20 seats	**Party 2** 20 seats	**Party 3** 20 seats	**Sum**
4	If the proposal is **passed**	[ ]	[ ]	[ ]	**0 DKK**
	If the proposal is **failed**	[ ]	[ ]	[ ]	**0 DKK**

When you punish the parties by 1 DKK, it costs you 0.10 DKK. You can maximally punish the parties by 40 DKK in total given each outcome of the parties' voting.

[Continue >>]

E.3 Representativeness of sample

Statistics Denmark created a random sample of the Danish adult population aged 18-80 years in late 2007 consisting of 40,000 adults. In Table E.1, we compare the 717 subjects from our study sample with the random sample. We see that our participants are younger, better-educated and have higher incomes than the representative population.

Table E.1: Representativeness of sample

	Participants	Full sample	t-test
Age	48.6 (15.5)	50.7 (16.2)	$p < 0.001$
Female	0.477 (0.500)	0.505 (0.500)	$p = 0.142$[a]
Years of education	13.6 (2.31)	12.3 (2.95)	$p < 0.001$
Gross income ('000 DKK)	358 (245)	300 (252)	$p < 0.001$
Observations	717	39,404	

Notes: Age and Female are measured on January 1, 2011. Years of Education is measured on October 1, 2010. Gross income ('000 DKK) is is a flow variable measured for 2011. Means are reported with standard deviations in parentheses. p-values come from two-sided tests. [a] Fisher's exact test.

E.4 Additional results

E.4.1 Recipients

Table E.2: Average punishments across treatments

	If passed			If failed		
	Com.	Rep.	t-test	Com.	Rep.	t-test
Proposal 1	5.63	8.05	$p = 0.079$	16.2	20.6	$p = 0.014$
	(0.89)	(1.04)		(1.22)	(1.28)	
Proposal 2	13.7	18.3	$p = 0.010$	8.10	9.71	$p = 0.279$
	(1.22)	(1.31)		(1.00)	(1.09)	
Proposal 3	20.6	25.3	$p = 0.007$	5.87	7.40	$p = 0.255$
	(1.26)	(1.21)		(0.88)	(1.00)	
Proposal 4	24.0	26.2	$p = 0.224$	5.81	9.20	$p = 0.022$
	(1.28)	(1.25)		(0.94)	(1.13)	
Observations	175	185		175	185	

Notes: Means are reported with standard errors in parentheses. p-values come from two-sided tests.

Table E.3: Average punishments across treatments, ex. DM2 and DM3 in Representative treatment

	If passed			If failed		
	Com.	Rep.	t-test	Com.	Rep.	t-test
Proposal 1	5.63	3.96	$p = 0.125$	16.2	14.6	$p = 0.326$
	(0.89)	(0.64)		(1.22)	(1.08)	
Proposal 2	13.7	12.3	$p = 0.386$	8.10	5.79	$p = 0.074$
	(1.22)	(1.06)		(1.00)	(0.82)	
Proposal 3	20.6	18.3	$p = 0.167$	5.87	3.30	$p = 0.013$
	(1.26)	(1.09)		(0.88)	(0.56)	
Proposal 4	24.0	19.5	$p = 0.010$	5.81	5.07	$p = 0.543$
	(1.28)	(1.17)		(0.94)	(0.78)	
Observations	175	185		175	185	

Notes: Means are reported with standard errors in parentheses. p-values come from two-sided tests.

E.4.2 Decision-makers

Table E.4: Average expected punishments across treatments, ex. DM2 and DM3 in Representative treatment

	If passed			If failed		
	Com.	Rep.	t-test	Com.	Rep.	t-test
Proposal 1	16.6 (2.35)	13.5 (3.49)	$p = 0.509$	41.0 (3.32)	39.7 (6.10)	$p = 0.976$
Proposal 2	37.1 (3.16)	31.4 (5.47)	$p = 0.377$	23.0 (2.62)	25.8 (5.50)	$p = 0.616$
Proposal 3	57.7 (3.61)	59.2 (6.25)	$p = 0.835$	16.4 (2.28)	5.63 (2.15)	$p = 0.012$
Proposal 4	70.3 (3.70)	67.5 (6.59)	$p = 0.700$	20.2 (2.82)	19.3 (4.76)	$p = 0.875$
Observations	180	56		180	56	

Notes: Means are reported with standard errors in parentheses. p-values come from two-sided tests.

Table E.5: Fractions of decision-makers who best-responded, ex. DM2 and DM3 in Representative treatment

	Committee treatment			Representative treatment		
	Vote			Vote		
Proposal	Yes	No	Test	Yes	No	Test
1	0.933	0.471	$p < 0.001$	0.927	0.000[a]	$p = 0.089$
2	0.865	0.469	$p < 0.001$	0.896	0.625[b]	$p = 0.078$
3	0.762	0.586	$p = 0.022$	0.829	0.933	$p = 0.428$
4	0.737	0.742	$p = 1.000$	0.871	0.680	$p = 0.108$
Observations	180			56		

Note: p-values come from two-sided Fisher's exact tests. [a] One observation. [b] Eight observations.

E.4.3 Relating recipients' behavior to their political orientation

In this section, we compare the recipients' punishments to their political orientation. Subjects were asked about their political orientation in a questionnaire at the end of the wave. Attrition after our experiment, but before the political questions thus causes a small drop in the number of observations.

Table E.6: Relating the recipients' total punishment to their political orientation

	(1)	(2)	(3)	(4)	(5)
Left-wing (WVS)	-11.2				
	(7.96)				
Left-wing voting		-7.82			
		(8.47)			
Abstain or blank voting		14.9			
		(15.8)			
Extreme left-wing (WVS)			-28.2		
			(18.0)		
Extreme right-wing (WVS)				15.2	
				(28.0)	
Extremist (WVS)					-15.9
					(15.3)
Constant	144.2***	116.7***	127.8***	127.3***	126.9***
	(14.3)	(17.1)	(16.6)	(16.6)	(16.6)
Controls					
Subject was an eligible voter	No	Yes	No	No	No
Treatment	Yes	Yes	Yes	Yes	Yes
Order of proposals	Yes	Yes	Yes	Yes	Yes
N	342	341	342	342	342
R^2	0.065	0.039	0.067	0.060	0.063

Notes: OLS regressions with total punishment in the experiment measured in DKK as the dependent variable. Standard errors in parentheses. "Left-wing (WVS)" is a dummy that takes value 1 if the subject stated a left-wing orientation (value 1-5) on a 1-10 scale from the World Value Survey where 1 is most left-wing and 10 is most right-wing. "Left-wing voting" is a dummy that takes value 1 if the subject would vote for a left-wing party if there "were a Danish parliamentary election tomorrow". "Abstain or blank voting" is a dummy that takes value 1 if the subject would abstain or vote blank if there "were a parliamentary election tomorrow". "Extreme left-wing (WVS)" and "Extreme right-wing (WVS)" are dummies that take value 1 if the subject stated an extreme left-wing or right-wing orientation (value 1 or 10), respectively, on a 1-10 scale from the World Value Survey. "Extremist (WVS)" is a dummy that takes value 1 if the subject stated either an extreme left- or right-wing orientation (value 1 or 10) on a 1-10 scale from the World Value Survey.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

E.4.4 Relating decision-makers' behavior to their political orientation

In this section, we compare the decision-makers' punishments to their political orientation. Subjects were asked about their political orientation in a questionnaire at the end of the wave. Attrition after our experiment, but before the political questions thus causes a small drop in the number of observations.

Table E.7: Relating the committee members' votes to their political orientation

	Proposal 1		Proposal 2		Proposal 3		Proposal 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Left-wing (WVS)	-0.042		0.012		0.090		-0.14	
	(0.047)		(0.059)		(0.075)		(0.075)	
Left-wing voting		0.0052		0.096		0.066		-0.16*
		(0.050)		(0.061)		(0.079)		(0.079)
Abstain or blank voting		0.12		-0.0098		-0.0054		-0.062
		(0.086)		(0.11)		(0.14)		(0.14)
Constant	0.98***	0.95***	0.54***	0.49***	0.64***	0.66***	0.76***	0.78***
	(0.067)	(0.067)	(0.082)	(0.084)	(0.091)	(0.096)	(0.10)	(0.10)
Controls								
Subject was an eligible voter	No	Yes	No	Yes	No	Yes	No	Yes
Decision-maker number	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	167	167	167	167	167	167	167	167
R^2	0.015	0.023	0.105	0.121	0.029	0.025	0.034	0.037

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject voted yes to the proposal. Standard errors in parentheses. "Left-wing (WVS)" is a dummy that takes value 1 if the subject stated a left-wing orientation (value 1-5) on a 1-10 scale from the World Value Survey where 1 is most left-wing and 10 is most right-wing. "Left-wing voting" is a dummy that takes value 1 if the subject would vote for a left-wing party if there "were a Danish parliamentary election tomorrow". "Abstain or blank voting" is a dummy that takes value 1 if the subject would abstain or vote blank if there "were a parliamentary election tomorrow".
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.8: Relating the representatives' votes to their political orientation

	Proposal 1		Proposal 2		Proposal 3		Proposal 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Left-wing (WVS)	0.047		-0.056		-0.10		0.30*	
	(0.038)		(0.10)		(0.14)		(0.14)	
Left-wing voting		-0.0046		-0.098		-0.16		0.098
		(0.032)		(0.098)		(0.13)		(0.15)
Abstain or blank voting		-0.34***		-0.30		-0.53		-0.36
		(0.069)		(0.22)		(0.28)		(0.31)
Constant	0.97***	1.00***	0.83***	0.86***	0.81***	0.88***	0.50**	0.66***
	(0.048)	(0.040)	(0.12)	(0.11)	(0.13)	(0.13)	(0.16)	(0.15)
Controls								
Subject was an eligible voter	No	Yes	No	Yes	No	Yes	No	Yes
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	55	55	55	55	55	55	55	55
R^2	0.069	0.365	0.077	0.113	0.017	0.087	0.197	0.162

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject voted yes to the proposal. Standard errors in parentheses. "Left-wing (WVS)" is a dummy that takes value 1 if the subject stated a left-wing orientation (value 1-5) on a 1-10 scale from the World Value Survey where 1 is most left-wing and 10 is most right-wing. "Left-wing voting" is a dummy that takes value 1 if the subject would vote for a left-wing party if there "were a Danish parliamentary election tomorrow". "Abstain or blank voting" is a dummy that takes value 1 if the subject would abstain or vote blank if there "were a parliamentary election tomorrow".
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.9: Relating whether or not the committee members best-responded to their political orientation

	Proposal 1		Proposal 2		Proposal 3		Proposal 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Left-wing (WVS)	-0.050		-0.0069		0.060		0.00090	
	(0.051)		(0.064)		(0.072)		(0.069)	
Left-wing voting		-0.020		0.11		0.062		0.039
		(0.054)		(0.067)		(0.076)		(0.072)
Abstain or blank voting		0.055		-0.0054		0.063		-0.067
		(0.093)		(0.12)		(0.13)		(0.13)
Constant	0.93***	0.91***	0.72***	0.65***	0.76***	0.75***	0.72***	0.70***
	(0.072)	(0.072)	(0.089)	(0.092)	(0.088)	(0.093)	(0.094)	(0.096)
Controls								
Subject was an eligible voter	No	Yes	No	Yes	No	Yes	No	Yes
Decision-maker number	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	167	167	167	167	167	167	167	167
R^2	0.019	0.017	0.023	0.042	0.031	0.031	0.015	0.020

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the decision-maker best-responded to his or her expectations. Standard errors in parentheses. "Left-wing (WVS)" is a dummy that takes value 1 if the subject stated a left-wing orientation (value 1-5) on a 1-10 scale from the World Value Survey where 1 is most left-wing and 10 is most right-wing. "Left-wing voting" is a dummy that takes value 1 if the subject would vote for a left-wing party if there "were a Danish parliamentary election tomorrow". "Abstain or blank voting" is a dummy that takes value 1 if the subject would abstain or vote blank if there "were a parliamentary election tomorrow".

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.10: Relating whether or not the representatives best-responded to their political orientation

	Proposal 1		Proposal 2		Proposal 3		Proposal 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Left-wing (WVS)	0.0030		-0.20*		0.024		0.32*	
	(0.081)		(0.094)		(0.11)		(0.12)	
Left-wing voting		-0.014		-0.13		-0.071		0.24
		(0.079)		(0.095)		(0.11)		(0.13)
Abstain or blank voting		-0.24		0.035		0.092		0.021
		(0.17)		(0.21)		(0.23)		(0.27)
Constant	1.00***	1.01***	0.87***	0.78***	0.93***	0.96***	0.56***	0.65***
	(0.10)	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.14)	(0.13)
Controls								
Subject was an eligible voter	No	Yes	No	Yes	No	Yes	No	Yes
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	55	55	55	55	55	55	55	55
R^2	0.096	0.129	0.209	0.173	0.037	0.051	0.122	0.072

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the decision-maker best-responded to his or her expectations. Standard errors in parentheses. "Left-wing (WVS)" is a dummy that takes value 1 if the subject stated a left-wing orientation (value 1-5) on a 1-10 scale from the World Value Survey where 1 is most left-wing and 10 is most right-wing. "Left-wing voting" is a dummy that takes value 1 if the subject would vote for a left-wing party if there "were a Danish parliamentary election tomorrow". "Abstain or blank voting" is a dummy that takes value 1 if the subject would abstain or vote blank if there "were a parliamentary election tomorrow".

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

E.4.5 Relating decision-makers' behavior to their Dictator Game behavior

In this section, we compare the decision-makers' punishments to whether or not they selfishly shared nothing with the recipient in a Dictator Game conducted two years before as part of iLEE2 (see Chapter 5 for details). We use information about the subjects' reported age and gender as well as the invitees' age and gender to validate the subjects' identities in both our experiment and in the Dictator Game. A total of 158 of the 180 committee members from our experiment had participated in the Dictator Game in 2009 and all of these subjects' identities can be validated. Likewise, 49 of the 56 representatives had participated in the Dictator Game and all of these subjects' identities can also be validated.

Table E.11: Relating the representatives' votes to a selfishness indicator measured in a Dictator Game

	Committee members				Representatives			
	Prop. 1	Prop. 2	Prop. 3	Prop. 4	Prop. 1	Prop. 2	Prop. 3	Prop. 4
Shared nothing in Dictator Game	-0.086	-0.0092	0.0022	-0.0089	n/a	0.13	0.16	-0.34
	(0.054)	(0.064)	(0.083)	(0.084)	(—)	(0.12)	(0.16)	(0.17)
Constant	0.97***	0.60***	0.71***	0.65***	n/a	0.78***	0.76***	0.89***
	(0.065)	(0.080)	(0.091)	(0.097)	(—)	(0.098)	(0.13)	(0.18)
Controls								
Decision-maker number	Yes	Yes	Yes	Yes	No	No	No	No
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	158	158	158	158	49	49	49	49
R^2	0.024	0.095	0.025	0.013	—	0.080	0.031	0.148

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the subject voted yes to the proposal. Standard errors in parentheses. The explanatory variable is a dummy that takes value 1 if the subject had shared nothing with the recipient in a Dictator Game conducted in 2009. There was no variation in the 49 representatives' votes to proposal 1.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.12: Relating whether or not the decision-makers best-responded to a selfishness indicator measured in a Dictator Game

	Committee members				Representatives			
	Prop. 1	Prop. 2	Prop. 3	Prop. 4	Prop. 1	Prop. 2	Prop. 3	Prop. 4
Shared nothing in Dictator Game	0.0066	-0.011	-0.029	0.042	-0.027	0.22	-0.10	-0.18
	(0.058)	(0.074)	(0.083)	(0.083)	(0.089)	(0.15)	(0.15)	(0.16)
Constant	0.93***	0.61***	0.67***	0.69***	0.72***	0.91***	0.81**	1.02***
	(0.10)	(0.13)	(0.14)	(0.14)	(0.15)	(0.24)	(0.24)	(0.26)
Controls								
Decision-maker number	Yes	Yes	Yes	Yes	No	No	No	No
Order of proposal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	158	158	158	158	49	49	49	49
R^2	0.035	0.082	0.073	0.049	0.340	0.264	0.169	0.128

Notes: LPM regressions with the dependent variable being a dummy that takes value 1 if the decision-maker best-responded to his or her expectations. Standard errors in parentheses. The explanatory variable is a dummy that takes value 1 if the subject had shared nothing with the recipient in a Dictator Game conducted in 2009.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$