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Manja Gärtner [a]
Anna Sandberg [b, c]

[a] Stockholm University
Department of Economics

[b] Stockholm University
Institute for International Economic Studies (IIES)

[c] Stockholm School of Economics
Department of Economics

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[a] Department of Economics, Stockholm University, SE-106 91 Stockholm, Sweden, Telephone: +46-8-674 71 23, E-mail: manja.gartner@ne.su.se

[b] Institute for International Economic Studies (IIES), Stockholm University, SE-106 91 Stockholm, Sweden, Telephone: +46-8-16 30 70, E-mail: anna.sandberg@iies.su.se

Abstract

We investigate whether individuals are more prone to act selfishly if they can passively allow for an outcome to be implemented (omission) rather than having to make an active choice (commission). In most settings, active and passive choice alternatives differ in terms of factors such as the presence of a suggested option, costs of taking an action, and awareness. We isolate the omission effect from confounding factors in two experiments, and find no evidence that the distinction between active and passive choices has an independent effect on the propensity to implement selfish outcomes. This suggests that increased selfishness through omission, as observed in various economic choice situations, is driven by other factors than a preference for selfish omissions.

Keywords: Fairness; Social preferences; Morals; Dictator game; Omission

JEL codes: D03

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1 Introduction

Are people more selfish if they can passively allow for a selfish allocation of resources, rather than actively having to implement it? This question speaks to a variety of different choice situations, as decisions about resource allocations often vary by whether they require active involvement or not. People may be more likely to walk by a charity solicitor on the street than to refuse to give to a solicitor who knocks on their door. An employer may passively allow for inflation to erode workers' real wages even if she would not be willing to actively cut nominal wages. Citizens face governmental policies about redistributive matters that vary by the involvement they require, such as whether being an organ donor requires active registration or not, or whether declarations of taxable income require more or less active statements. In this paper, we conduct two experiments to test whether individuals exhibit preferences for selfish omissions over selfish commissions in economic decision-making.

We define a choice as active, or as a *commission,* if the decision maker implements an outcome by taking an action, whereas we define a choice as passive, or as an *omission*, if the decision maker allows for an outcome to be implemented by being inactive. A body of research in moral philosophy and psychology suggests that individuals favor harmful omissions over harmful acts. Subjects faced with hypothetical choice scenarios often judge harmful acts as morally worse than equally harmful omissions (Spranca et al. 1991; Kordes-de Vaal 1996; Cushman et al. 2006; DeScioli, Bruening and Kurzban 2011; Cushman et al. 2012) and report to be more willing to do harm passively rather than actively (Ritov and Baron 1990; Baron and Ritov 1994; Cohen and Pauker 1994; Asch et al. 1994; Meszaros et al. 1996; Ritov and Baron 1999). However, while research in other disciplines and evidence from real-world examples suggest that passivity matters, it has not been systematically investigated whether the distinction between commissions and omissions *itself* affects economic decision-making. In most choice situations, it is, in fact, impossible to identify the effect of such a distinction, since active and passive choices differ systematically across other dimensions.

Most importantly, it can be difficult to empirically separate the omission effect from the status quo effect (i.e. the effect of an alternative having been implemented previously) and the default effect (i.e. the effect of presenting an alternative as the suggested option). However, since status quo and default options can be implemented either passively or actively, the omission effect differs conceptually from the status quo and the default effect. A default option is sometimes referred to as the outcome that will prevail if a decision maker is passive. We use a broader definition, recognizing that situations may differ in the degree of activity required to confirm the choice of a suggested option. Thus, we distinguish between "passive defaults" (implementing the suggested option requires no activity whatsoever) and "active defaults" (implementing the suggested option requires some activity). For example, public policies such as organ donation regulations often involve defaults that can be accepted passively, while market settings such as online purchases and acceptance of user agreements often require active verification of a pre-selected default option (e.g. regarding user conditions, shipping, or receiving future emails from a retailer). Such active verification can for instance consist of pressing an "I agree"-button on a computer screen or signing a piece of paper. Previous research on the default effect shows that individuals are more likely to choose an alternative if it is presented as the default option (e.g. Johnson et al. 1993; Carroll et al. 2009; Dhingra et al. 2012; Hayashi 2013). To determine the relevance of the distinction between commissions and omissions, it is crucial to disentangle the omission effect from the effect of introducing a suggested option. In order to test whether there exists an omission effect beyond the effect of a default option, we compare the effect of introducing an active default to the effect of introducing a passive default. The results of such an investigation should be of direct relevance for mechanism designers since if individuals react differently to active than to passive defaults, it might be possible to nudge behavior simply by changing whether a given default option needs to be actively confirmed or not.

Apart from the presence of a suggested option, commissions and omissions often differ in terms of the costs of taking an action and the decision maker's awareness. In many choice situations remaining passive is less costly than taking an action. Making an active choice may require more costs in terms of time and effort, such as filling out an organ donation form. It may also be cognitively more costly for individuals that are unsure about what to choose to resolve this uncertainty under active choices. In some settings, passive choices might be the result of unawareness of being in a choice situation or a lack of enough time to make an active choice, rather than the expression of a preference. In such settings, self-serving commissions may be judged more harshly than self-serving omissions, merely due to differences in the revealed intent. For example, one motivation for legally distinguishing between the act of killing and the failure to help someone who is dying is that the former provides stronger evidence of harmful intentions. Our contribution is to provide a test that isolates the effect of active and passive choices from these other confounding factors (i.e., suggested options, costs of taking an action, and awareness). The main question we ask is whether there is an omission effect in the sense that individuals have a preference for implementing selfish options passively rather than actively.

There are two main mechanisms that might underlie an omission effect in economic decision-making beyond any of these confounding motives. First, an omission effect may reflect the conviction that the distinction between selfish commissions and selfish omissions is morally significant in itself. For individuals that inherently care about behaving morally and that consider a selfish action to be more immoral than a selfish omission, holding everything else constant, this distinction will be reflected in their behavior. Second, an omission effect may enter the preferences of individuals that are motivated by social esteem or self-image concerns. It has been suggested that individuals care about their social- and/or self-image and are motivated by a concern for the value of reputation that is attached to each alternative in a particular choice set (Bénabou and Tirole 2006; Ellingsen and Johannesson 2008; Andreoni and Bernheim 2009; Bénabou and Tirole 2011). However, signaling values may also depend on how outcomes come about. Passive, as compared to active, norm violations may provide weaker signals about an agent's type, thus making them more likely to be forgotten or repressed by the agent herself or others.

Both these mechanisms suggest that the moral appropriateness of a choice may not just depend on the alternatives initially available to the individual and the selected outcome, but also on how an outcome comes about. This means that actions *themselves* may affect social norms and moral concerns. In line with this notion, Levitt and List (2007) and Krupka and Weber (2013) argue that a utility maximization framework that aims at explaining choices under social norms can benefit from including actions as an argument in the utility function. Whether an outcome follows from an active choice or from the decision maker merely allowing for the outcome to be implemented is one such difference in how outcomes come about. Accordingly, the question of whether the distinction between active and passive choices is an expression of preferences also relates to the broader issue of whether individual utility maximization can be modeled solely as a function of initial states and final outcomes.

We present two experiments aimed at isolating the distinction between omissions and commissions from confounding factors. In these experiments, we conduct a series of dictator games where subjects can choose between two different allocations of money between themselves and another participant: one selfish allocation and one fair allocation. In each game, one allocation is indicated by a pre-ticked box. We will refer to this allocation as the default option. Our treatments vary the relative stakes of the default, and whether the default allocation can be implemented by commission or by omission. We hypothesized that subjects facing a selfish default option, which implies violating a fairness norm, would be more likely to choose the default option by omission rather than by commission. Further, we hypothesized that the omission effect would be smaller for choices with a non-selfish, norm-compliant default option. However, our results show no statistically significant omission effect in the share of selfish choices, neither given a selfish nor given a non-selfish default option. Thus, we find no support for the hypothesis that there is an omission effect in prosocial behavior beyond the default effect and other confounding factors. All else equal, individuals do not prefer to implement a selfish default option passively rather than actively. We can show that this finding holds across a number of different allocation trade-offs with various properties as well as across settings with weak and strong default effects. Our result suggests that social preferences are not sensitive to an omission effect. Thus, increased selfishness through omission, as observed in various settings, is likely to be driven by other factors than a preference for selfish omissions. In particular, the presence of a suggested option, costs of taking an action and limited awareness are confounding factors which differ systematically across commissions and omissions in many settings and which may explain why passivity and selfish behavior often coincide. Each of these factors is discussed in the conclusion.

The remainder of the paper is organized as follows. Section 2 presents the design and results of the first experiment and section 3 presents the design and results of the second experiment. Section 4 concludes with a discussion of the implication of our results.

2 Experiment 1

2.1 Experimental design

The first experiment employs a repeated dictator game with 14 different binary allocation choices. In the first choice, all subjects face the same allocation trade-off between an allocation which is payoff-dominant for the dictator (the "*selfish"* allocation), giving 90 DKK (≈ 13.8 USD) to the dictator and 10 DKK (≈ 1.5 USD) to the recipient, and an allocation which is both fair and efficient (the "*fair"* allocation), giving 70 DKK (≈ 10.7 USD) each to the dictator and the recipient. Having an identical first choice across subjects allows for a between-subject analysis. The subsequent 13 choices have varying allocation trade-offs and follow in an order that is randomized at the individual level. These trade-offs differ in terms of the cost of giving and the size and direction of the payoff difference between the dictator and the recipient. Table A.1 in Appendix A lists all allocation trade-offs and their characteristics. For each new allocation choice, each subject is randomly rematched with an anonymous recipient in the same room. After the experiment, one choice is randomly chosen for payment.

Subjects are randomized into either the *commission treatment* or the *omission treatment* and remain within one treatment throughout the experiment. In both treatments subjects have 40 seconds to make each allocation choice, as indicated by a timer on the screen. For each choice, we randomly select one of the two allocations to be presented as the default, i.e. subjects either face a *selfish default* or a *fair default*. The default is indicated by a pre-ticked rather than an empty box beside that allocation. The difference between the commission treatment and the omission treatment is whether implementing the pre-ticked default allocation requires an active or a passive choice. To choose the default allocation, subjects in the commission treatment must actively press a confirm button that restates the allocation choice, while subjects in the omission treatment simply let the timer run down. In order to implement the alternative option, subjects in both treatments must tick the box beside that option and press another confirm button. After a choice has been made, subjects in both treatments must wait for the timer to run down in order to proceed to the next stage. If a subject in the commission treatment fails to make an active choice before the timer runs down, the subject proceeds to the next stage without receiving any earnings from that choice.[1] We display screenshots of the decision interface in Figures A.1 and A.2 in Appendix A.

To ensure that we give subjects enough time to make a choice, we conducted a pilot study to elicit response times. Subjects in the pilot take at most 22 seconds to make a choice in the commission treatment without any time constraint.[2] By giving participants in the experiment 40 seconds to make a choice, i.e. almost twice as much time, we rule out that time constraints drive passive choices in the omission treatment. Note that subjects are not likely to incorrectly perceive 40 seconds to be too little time, since they participate in practice rounds prior to the experiment, with 40 seconds to make a practice choice. However, introducing a period length of 40 seconds implies that subjects who choose the default in the omission treatment have to face their passive choice for rather a long time. In order to make the omission situation more natural, we therefore introduce a second task. Throughout the experiment and in each treatment, subjects can work on a slider task (Gill and Prowse 2011). While the 14 allocation choices appear sequentially on the left-hand side of the screen at regular intervals, the slider task

[1] Participants in the commission treatment are informed that they need to press a button to confirm their allocation choice. They are also informed that they are only paid for the tasks that they complete according to the instructions. Thus, it should be clear to participants in the commission treatment that they are not paid for the choices in which they remain passive.

[2] The mean response time in the pilot is 12.34 seconds (*s.d.* = 4.98, $N = 16$). The data on response times from the experiment resembles the data from the pilot and confirms that participants had enough time to make a choice. Overall, among subjects who made an active choice in the commission treatment, the average response time is 10.32 seconds and 95% of the participants make their choice within 27 seconds. For the first choice, the equivalent numbers are 11.37 and 27 seconds.

constantly appears on the right-hand side of the screen. We show a screenshot of the slider task in Figure A.3 in Appendix A. In this task, subjects use their mouse to position sliders at a target location. The total number of sliders that can be solved is not restricted and for each correctly positioned slider, the subject earns 0.01 DKK (≈ 0.001 USD). The slider task is repetitive, tedious and pays very little as compared to the dictator games. Hence, it should not crowd out incentives for participating in the dictator games. Before the experiment, subjects play practice rounds and answer control questions about the decision situation and the slider task. After the experiment, subjects are asked to complete a questionnaire eliciting demographic characteristics. The written instructions are provided in Appendix E.

Table 1 illustrates our 2x2 design (selfish/fair default x commission/omission treatment), using the first allocation trade-off between (90,10) and (70,70) as an example. All choices in the experiment are structured accordingly.

Table 1 Overview of treatments in Experiment 1. Choice between (90,10) and (70,70).

		Default Allocation	Implementation of Default
I	Commission Treatment	(90,10)	Active
II	Omission Treatment	(90,10)	Passive
III	Commission Treatment	(70,70)	Active
IV	Omission Treatment	(70,70)	Passive

Our main hypothesis is that subjects prefer to be selfish by omission rather than by commission. In terms of the first allocation trade-off, we expect that subjects facing the selfish default (90,10) are more likely to choose (90,10) over (70,70), if the default is implemented passively (omission treatment) rather than actively (commission treatment).

Hypothesis 1: Given a selfish default, the share of default choices is higher in the omission treatment than in the commission treatment:

$$(\textit{Selfish choices} \mid \textit{II}) > (\textit{Selfish choices} \mid \textit{I}).$$

If individuals are uncertain about which allocation to choose and it is psychologically costly to resolve this uncertainty, they might remain passive to avoid costly contemplation. If so, any omission effect that we observe for selfish defaults may express a *general* omission effect, irrespective of the characteristics of the omission option, rather than a preference for selfish omissions over selfish commissions. To address this concern, we also elicit omission effects when the default is fair. We hypothesize that there is an omission effect under selfish defaults beyond what can be explained by preference uncertainty and, hence, that the omission effect under selfish defaults is larger than the omission effect under fair defaults. In terms of the first choice, we hypothesize that the increase in the propensity to choose the (90,10) default by omission rather than commission is larger than the increase in the propensity to choose the (70,70) default by omission rather than commission.

Hypothesis 2: The omission effect is larger for choices with a selfish default than for choices with a fair default:

(Selfish choices | II) – (Selfish choices | I) > (Fair choices | IV) – (Fair choices | III).

Our experimental design is well suited to isolate the distinction between omissions and commissions since the treatment conditions only differ in terms of activity, while relevant aspects other than this distinction are held constant. Most importantly, subjects face the same choice alternatives and the same default condition across the commission and the omission treatment. Second, any potential omission effect is unlikely to be driven by differences in the time invested or effort costs since subjects are given the same fixed decision time across treatments and since making an active choice only requires clicking a button to confirm the choice. Third, we calibrated decision time such that any omission effect in choices cannot be driven by differences in time pressure across treatments. Finally, since subjects in both treatments have enough time to make an active choice and are fully informed about the structure of the choice situation before the experiment starts, there is little reason to believe that unawareness of the choice situation or lack of time will drive any omission effect in our setting. Holding these confounding factors constant, we therefore argue that any observed behavioral difference across treatments must be caused by the distinction between omissions and commissions *itself*.

2.2 Results

400 subjects were recruited from the subject pool of the University of Copenhagen, using the online system ORSEE (Greiner 2004). The experiment took place at the laboratory at the local Center for Experimental Economics (CEE) in May 2013 and the experimental software z-Tree (Fischbacher 2007) was used. In total we ran 15 sessions, each session lasting roughly one hour.

At most, a subject solved 327 sliders in total, resulting in a payment of 3.27 DKK from the slider task. Subjects earned about 141 DKK (≈ 21.6 USD) on average.

The average participant age is 26. 48% of the participants are women, 26% are Danish, and 78% are full-time students. We show participant characteristics in Table A.2 in Appendix A. 200 subjects participated in the commission treatment and 200 in the omission treatment. Participants in the commission treatment largely managed to make an active choice by clicking the confirm button within the allotted time. In total, participants in the commission treatment failed to make an active choice in 2.6% of all choice occasions (72 out of 2800 choices). Looking only at the first choice, the equivalent number is 3% (6 out of 200 choices). We exclude observations for which this was not the case from the main analysis, but our main results are robust to including them under various assumptions.[3] Below, we first present the results from the first allocation choice, and then from all 14 allocation choices pooled.

2.2.1 First allocation choice

In the first choice, the subject can either allocate 90 DKK to herself and 10 DKK to the recipient, or 70 DKK each to herself and the recipient. Overall, the share of selfish choices is 43.4%, which is within the range found in previous studies using binary dictator games with a similar structure. As shown in Figure 1, when the selfish allocation (90,10) is presented as the default option, it is chosen by 43.8% of the subjects in the commission treatment and 46.7% of the subjects in the omission treatment. We cannot reject the null hypothesis that the probability of choosing the selfish option is the same across these two conditions ($\chi^2(1)$ = 0.16, p = 0.691). Thus, we do not find any support for our hypothesis that subjects are more likely to choose a selfish default in the omission treatment than in the commission treatment. Even though subjects display a tendency to be more selfish by omission than by commission, the point estimate is very small and not statistically significant.

The share of selfish choices, across treatments and for both selfish and fair defaults, is presented in Table 2. Since we find no evidence for an omission effect in selfish choices, there are no mechanisms to distinguish. Therefore, we leave the details of the analysis of the fair default and our second hypothesis to Appendix B. In short, when the fair allocation (70,70) is presented as the default option, there is no statistically significant difference in the share of fair choices between the two treatments. Moreover, testing our second hypothesis, we cannot reject

[3] We test the robustness of our results assuming that these observations would have been default choices, selfish choices, or non-selfish choices. The results from all tests presented in section 2 remain unchanged. The results from these robustness checks are presented in Appendix D.

the null-hypothesis of no difference in omission effects between the fair and the selfish default conditions.

Table 2 Share of participants choosing the selfish allocation in the first choice.

	Commission treatment	Omission treatment	Total
Default (90,10)	43.8%	46.7%	45.4%
	(*n*=89)	(*n*=105)	(*n*=194)
Default (70,70)	36.2%	47.4%	41.5%
	(*n*=105)	(*n*=95)	(*n*=200)
Total	39.7%	47.0%	43.4%
	(*n*=194)	(*n*=200)	(*n*=394)

Note: In each cell we show the share of participants choosing the selfish allocation (90,10). In parentheses we show the total number of participants in each cell.

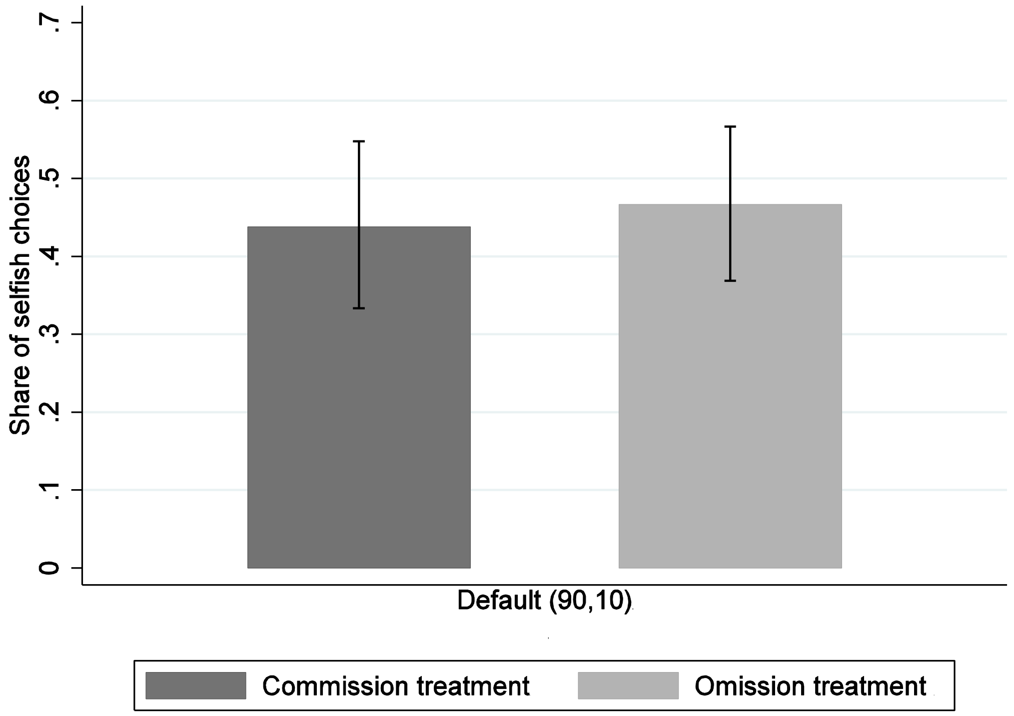

Figure 1 Average share of selfish choices in the first allocation choice with default (90,10).

2.2.2 All allocation choices

In order to increase the precision of our estimates, we also test our hypotheses looking at all 14 allocation choices. Since we do not find any order effects in the likelihood to choose the default, we pool all individual choices.[4]

To test our first hypothesis, we look at all choices with a selfish default, i.e. all allocation trade-offs where the default option is strictly payoff-dominant for the dictator. Table 3 shows the results from OLS regressions, using an indicator for whether the participant chose the selfish default allocation, or not, as the dependent variable. The omission effect is given by the coefficient of the explanatory variable *Omission treatment*. Model 1 restricts the sample to only the first choice, and verifies the result from the previous section (β = 0.028, p = 0.69). Model 2 pools all allocation choices with a selfish default, i.e. all allocation trade-offs where the default option is strictly payoff-dominant for the dictator. There is no significant omission effect in the likelihood to choose a selfish default even when pooling all choices (β = 0.004, p = 0.90). Pooling all choices substantially increases the precision of the estimated coefficient. The 95% confidence interval narrows from [-0.114,0.171] in Model 1 to [-0.056,0.064] in Model 2.

Next, to see whether the omission effect varies across allocation trade-offs with different properties, we split our data into different subsets of choices. Model 3 restricts the analysis to allocations where the dictator's payoff is at least as high as the recipient's payoff. Model 4 only includes choices that trade-off selfishness and behindness, i.e. choices between one option which is weakly payoff-dominant for the dictator and another option giving the dictator less than the recipient. Model 5 is restricted to the choices where the selfish option is less efficient than the non-selfish option. Also for these subsets of choices, there is no significant omission effect. The 95% confidence intervals for the estimated coefficients are [-0.035,0.127], [-0.079,0.040] and [-0.079,0.040], respectively. Hence, even when increasing the precision by pooling all choices, we find no evidence that individuals are more likely to be selfish passively rather than actively. As in the previous section, we leave the analysis of the fair default to Appendix B.

[4] Table A.3 in Appendix A tests our first hypothesis for each choice separately. The difference in default choices between the omission and the commission treatment is significant at the 5 percent level for only one of the 14 choices. In that case, the difference is not in the hypothesized direction; in the choice between (50,50) and (30,110), subjects are *less* likely to choose the (50,50) default by omission than by commission. When correcting the significance level for multiple testing (a Bonferroni correction with n = 14 and α = 0.05 yields an adjusted required significance level of $0.05/14 \approx 0.004$), no individual treatment effect is statistically significant.

Table 3 Treatment effect on the propensity to choose the selfish default.

	Model 1	Model 2	Model 3	Model 4	Model 5
Omission treatment	0.028	0.004	0.046	-0.019	0.020
	(0.072)	(0.030)	(0.041)	(0.030)	(0.039)
Constant	0.438***	0.666***	0.515***	0.758***	0.554***
		(0.020)	(0.029)	(0.020)	(0.028)
R^2	0.00	0.00	0.00	0.00	0.00
N (choices)	194	2,576	1,347	1,227	998
Choices included:	First choice	Selfish vs. non-selfish	Selfish vs. non-selfish (not behind)	Selfish vs. behind	Selfish vs. efficient

Note: OLS regressions. The sample is restricted to choices where the default option is selfish (strictly payoff dominant for the dictator). Dependent variable: = 1 if default chosen, = 0 otherwise. *Model 1* only includes the first choice between (90,10) and (70,70). *Model 2* includes all allocation choices except choice number 13 which has no strictly payoff dominant option for the dictator. *Model 3* includes choices 1, 2, 3, 4, 5, 6 and 10. *Model 4* includes choices 7, 8, 9, 11, 12 and 13. *Model 5* includes choices 1, 6, 9, 10 and 12. See Table A.1 in Appendix A for a list of all choices. Standard errors are clustered on participant in all models except *Model 1*. * p<0.1; ** p<0.05; *** p<0.01.

In Appendix C we also test our first hypothesis by estimating a latent class model, classifying subjects into different social preference types. This allows us to take inequity aversion and social welfare preferences into account in the same model. In line with our previous results, we find no significant difference in the distribution of types between the omission and the commission treatment under selfish default options.

2.2.3 Default effects

To evaluate the size of the default effect in Experiment 1, we compare the share of selfish choices when the selfish allocation is the default to the share of selfish choices when the fair allocation is the default. In the first allocation choice, the share of (90,10) choices is not significantly larger when (90,10) is the default as compared to when (70,70) is the default, neither in the commission treatment (43.8% vs. 36.2%, $\chi^2(1)$ = 1.17, p = 0.28), nor in the omission treatment (46.7% vs. 47.4%, $\chi^2(1)$ = 0.01, p = 0.92). Similarly, we find little evidence of default effects when pooling all choices. Using the sample restriction from Model 2 in Table 3, there is a small but significant default effect in the commission treatment (β = 0.053, p = 0.01) but not in the omission treatment (β = 0.07, p = 0.80). Using the sample restrictions from Models 3-5, there are no significant default effects, neither in the commission treatment nor in the omission treatment.[5] Assuming that the default effects for selfish and fair defaults point in the same direction, this means that we find no strong evidence of default effects in Experiment 1.

3 Experiment 2

In Experiment 1, we find no evidence of an omission effect on prosocial behavior. However, nor do we find any strong evidence of a default effect. Thus, from Experiment 1, we can conclude that there is no evidence of an omission effect independent of the default effect. However, given that default and omission effects have been suggested to be based on similar mechanisms (see e.g. Anderson 2003), it cannot be ruled out that an omission effect only occurs if these mechanisms are clearly at work, i.e. in the presence of a positive default effect. To test for the existence of an omission effect in the presence of a positive default effect, Experiment 2 introduces a setting where we expect the default effect to be stronger. For this purpose, we present the default as an entitlement, rather than introducing it as a randomly selected pre-ticked box.

3.1 Experimental Design

The second experiment employs a one-shot dictator game with one binary allocation choice. The relative payoff between the two allocations resembles that used in the first choice in Experiment 1. Subjects choose between an allocation which is payoff-dominant for the dictator (the *selfish* allocation), giving \$1.05 to the dictator and \$0.05 to the recipient, and an allocation which is both fair and efficient (the *fair* allocation), giving \$0.70 each to the dictator and the recipient. After the experiment, subjects are randomly matched and randomly assigned to either the role of the dictator or the role of the recipient and paid accordingly.

Subjects are randomized into the *commission treatment*, the *omission treatment* or the *no-default treatment*. As in Experiment 1, subjects have 40 seconds to make their choice, as indicated by a timer on the screen. In the commission treatment and the omission treatment, the selfish allocation is presented as the default. To create a stronger default effect than in Experiment 1, we present the default allocation as an initial distribution rather than a pre-ticked box. Before making the choice, participants in the commission and omission treatments read

[5] We estimate default effects for the pooled choices by running separate regressions for each treatment, using an indicator variable for selfish choice as the dependent variable, and an indicator variable for selfish default as the explanatory variable. Using the sample restrictions from Models 3, 4 and 5, the estimated coefficients of *Selfish default* are $\beta = 0.045$ ($p = 0.14$), $\beta = 0.031$ ($p = 0.23$), and $\beta = 0.043$ (p = 0.21) in the commission treatment and $\beta = 0.08$ ($p = 0.80$), $\beta = -0.002$ ($p = 0.96$), and $\beta = 0.04$ ($p = 0.91$) in the omission treatment.

instructions stating that "You and the other worker receive the following bonus payments for this task: You: $1.05, Other: $0.05". Participants are then informed that they can either confirm this bonus payment, or select an alternative option. Subjects in both treatments can choose either the selfish default allocation or the alternative, fair allocation by ticking an empty box next to it. The only difference between the commission treatment and the omission treatment is that subjects in the omission treatment have the additional option of implementing the selfish allocation by remaining passive and letting the timer run down. Thus, as in Experiment 1, the difference in the share of selfish choices between the commission treatment and the omission treatment measures the omission effect.[6]

We introduce the no-default treatment to measure the size of the default effect. The no-default treatment has no default option, but equals the commission treatment in all other respects. The difference in the share of selfish choices between the no-default treatment and the commission treatment allows us to measure the default effect. Table 4 illustrates the differences between treatments. The experimental instructions can be found in Appendix F.

Table 4 Overview of treatments in Experiment 2. Choice between (1.05,0.05) and (0.70,0.70).

	Default Allocation	Implementation of Default
Commission Treatment	(1.05,0.05)	Active
Omission Treatment	(1.05,0.05)	Passive or Active
No-default Treatment	No Default	-

We hypothesize that there is a default effect in this setting, i.e. that subjects are more likely to choose the selfish allocation in the commission treatment than in the no-default treatment. If there is an omission effect, subjects will be more likely to choose the selfish default in the omission treatment than in the commission treatment. If there is no omission effect, even in the presence of a default effect, there will be no difference between the commission and the omission treatment.

3.2 Results

453 subjects were recruited through a work task posted on Amazon Mechanical Turk in August 2014. Subjects received a fixed payment of $0.50 for participation on top of their earnings from

[6] To simplify the design of Experiment 2, we do not include the slider task that was used in Experiment 1.

the experiment, which were paid as bonuses. To avoid non-random attrition from treatments and to select only sufficiently motivated subjects, the experiment started with a short transcription task that asked subjects to transcribe a text from a picture into a text box. The experiment was part of a larger study, but the dictator game choices reported here were elicited first. After the dictator game, subjects participated in an additional task and answered a short questionnaire.

40% of the participants are women and 75% are located in the US. 151 subjects participated in each treatment. No subject in the commission treatment and only 2 subjects (1.3% of subjects) in the no-default treatment failed to make a choice in the allotted time. These shares are not significantly different across the two treatments ($\chi^2(1) = 2.01$, $p = 0.16$). We include these two subjects in the analysis presented below, but our results are robust to excluding them. Table 5 presents the share of subjects choosing the selfish option across treatment conditions. Figure 2 illustrates the findings.

First, we test whether we succeeded in introducing a default effect into the decision. In the no-default treatment, 33.8% of the subjects choose the selfish option. Presenting the selfish option as the default in the commission treatment significantly increases the share of selfish choices by 23.8 percentage points ($\chi^2(1) = 17.29$, $p = 0.00$). This corresponds to an increase in selfish choices of 70.4% through the introduction of a selfish default. Thus, as hypothesized, subjects are more prone to choose the selfish option when it is presented as the default.

Second, we test whether an omission effect occurs in the presence of this default effect. The difference in the share of selfish choices in the commission treatment and the omission treatment is small with 1.3 percentage points and statistically insignificant ($\chi2(1) = 0.054$, $p = 0.816$). Thus, even in a context where the default effect is strong and the mechanisms behind a potential omission effect are at work, we find no evidence that the possibility of choosing a selfish default passively rather than actively increases the share of selfish choices.

Subjects in the omission treatment could implement the selfish default allocation either actively (by ticking the box) or passively (by letting the timer run down). Among the subjects that choose the selfish option in the omission treatment, 10.6 % use the possibility to remain passive. This corresponds to 6% of all subjects in the omission treatment, which is a significantly larger share than the subjects failing to make an active choice in the no-default treatment and the commission treatment (omission vs. commission: $\chi^2(1) = 9.30$, $p = 0.00$; omission vs. no-default: $\chi^2(1) = 4.62$, $p = 0.03$). Thus, a significant, but small, fraction of subjects in the omission treatment consciously use the option to be selfish by omission.

However, since the share of selfish choices is the same across the omission and the commission treatments, choosing the selfish option passively only seems to serve as a substitute to being selfish by commission.

Table 5 Share of participants choosing the selfish allocation in Experiment 2.

	No-default treatment	Commission treatment	Omission treatment	Total
Default (1.05,0.05)	33.8% (*n*=151)	57.6% (*n*=151)	56.3% (*n*=151)	49.2% (*n*=453)

Note: In each cell we show the share of participants choosing the selfish allocation (1.05,0.05). In parentheses we show the total number of participants in each cell.

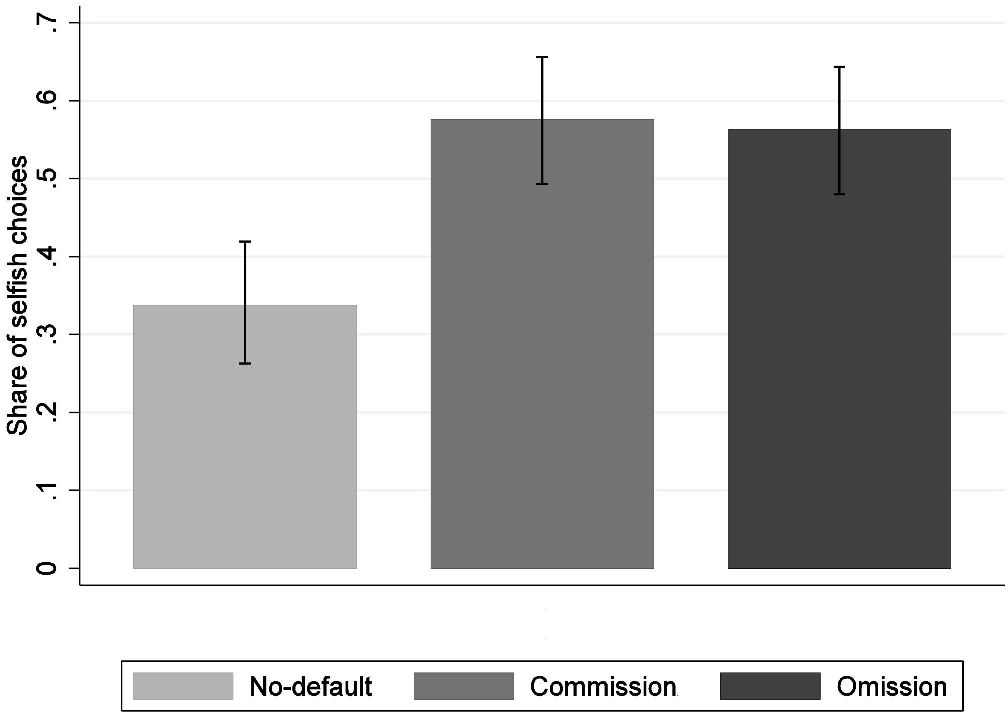

Figure 2 Average share of selfish choices across treatments

4 General Discussion

We find no support for the hypothesis that there is an omission effect in prosocial behavior beyond the default effect. All else equal, individuals do not prefer to implement a selfish default option passively rather than actively. This holds true both in a setting with weak default effects (Experiment 1) and a setting with strong default effects (Experiment 2). This indicates that prosocial preferences do not depend on the difference between commissions and omissions as attributes of the action taken by a decision maker. It has been suggested that a utility maximization framework in the presence of social norms may benefit from including actions as an argument of the utility function (see, for example, Levitt and List 2007 and Krupka and Weber 2013). Our results show that prosocial preferences are stable with respect to whether the choice is active or passive. This finding relates broadly to previous research on the sensitivity of dictator game behavior to contextual changes. Several studies show that seemingly small manipulations of the choice structure, such as relaxing the transparency of the one-to-one mapping between the dictator's actions and the final outcomes (Dana et al. 2007) or varying the choice set (List 2007; Bardsley 2008), can have strong effects on levels of giving. But not all manipulations affect giving behavior. For instance, Dreber et al. (2013) find no significant social framing effect when changing the name of the dictator game or the labeling of strategies. We complement previous research on giving in dictator games by providing a manipulation targeting the nature of the action taken by the dictator that has *no* effect on levels of giving.

Our result suggests that increased selfishness through omission, as observed in various settings, is not an omission effect in the sense that it expresses an individual's preference for selfish omissions. Rather, observed behavioral differences between passive and active choices are likely to be driven by other factors than the difference between active and passive choices itself. This leads to the question of which other situational factors could explain why passivity and selfish behavior often coincide. Below, we discuss the role of default and status quo effects, costs of taking an action, time constraints and awareness as potentially important confounding factors that commonly also differ across commissions and omissions in previous economics literature. We further discuss the potential role of the presence of punishment in facilitating an omission effect.

First, the omission option is a passive default option. Default effects have been identified in a variety of consequential real-world decisions, such as whether to become an organ donor (Abadie and Gay 2006; Johnson and Goldstein 2003), the choice of retirement savings plan (Carroll et al. 2009), auto insurance (Johnson et al. 1993) and energy provider (Pichert and Katsikopoulos 2008). Presenting a choice alternative as the default has also been found to affect dictator game giving, either through a self-serving interpretation of entitlements (Hayashi 2013), or through resolving preference uncertainty (Dhingra et al. 2012). Further, Grossman (2014) finds that passive defaults can affect strategic ignorance in a dictator game. He lets dictators choose whether to remain ignorant about the payoff consequences of their choice for the recipient, thereby allowing them to choose a higher payoff for themselves without knowingly violating a fairness norm. The share of dictators choosing to remain ignorant increases from 25% when the choice is active and has no default, to 54% when the passive default option is to remain ignorant. However, since Grossman's (2014) study is restricted to comparing a setting with a passive default option to a setting with no default option, we cannot know if the observed effect is solely driven by the effect of introducing a suggested option, or if the possibility to passively implement the default plays an additional role. In fact, given the current state of evidence, it is generally hard to disentangle omission effects from default effects. While some default options are chosen passively, such as through setting a passive policy default or maintaining a contract, while others require active verification, such as through signing a document or renewing a contract, to our knowledge, no previous study systematically investigates the distinction between active and passive defaults. Our study provides a first test, indicating that, all else equal, individuals do not prefer passive over active defaults. Thus, it is likely that observed increases in selfish behavior in settings with passive default options are driven by the default effect rather than a preference for selfish omissions.

Second, commission and omission options can differ substantially in terms of action costs. In our experimental setting action costs are reduced to a minimum, since making an active choice only requires clicking a button. An example of a field setting with minimal action costs is online purchases where choosing to add options, such as environmentally friendly delivery or a charity donation, only requires checking a box. However, in some real life situations, active choices are associated with considerable investments of effort and time. For instance, actively choosing to become an organ donor may require both time and effort, such as filling out a form and reflecting on the ethicality of one's choice. Similarly, making an informed decision about a new retirement plan requires a careful evaluation of all alternatives. Thus, in some settings, an increased propensity to stay passive may simply reflect the costs associated with making an active choice.

Third, it can be the case that passive choices, but not active choices, result from unawareness of the choice situation. In such settings, an increased willingness to be selfish by omission rather than commission can arise because agents are genuinely unaware of the possibility to commit a non-selfish act. Or, it can arise because they use this potential unawareness as an argument when justifying their choice to remain passive. For instance, individuals may be more likely to passively walk by a silent charity solicitor, allowing for potential unawareness, as compared to actively refraining from contributing if the solicitor explicitly asks for a contribution. Along these lines, several studies find that when given the possibility to opt-out of a situation in which a fairness norm would suggest a monetary contribution, some individuals choose this opt-out option (Dana et al. 2006; Andreoni et al. 2011; DellaVigna et al. 2012; Lazear et al. 2012). For instance, Andreoni et al. (2011) find that when placing a silent solicitor, rather than a solicitor who approaches customers, at one of two doors of a supermarket, the share of costumers using this door increases from 35.6% to 52.8%. Our result suggests that an omission effect itself cannot explain this type of avoidance behavior. Rather, it is plausible that a silent solicitor allows for some ambiguity about whether a customer was actually aware of the presence of the solicitor, and that customers use this potential unawareness as an argument when justifying their choice not to contribute.

Fourth, time constraints may be used by agents as a credible excuse for being selfish by omission. Dana et al. (2007) conduct a binary dictator game in which dictators can either make an active choice or passively let a timer cut them off at a random point within a 10 second interval, allowing for a random mechanism to make the choice. They find that 24 percent of the dictators passively allow for being cut off by the timer. Our finding indicates that, rather than being driven by an omission effect, the dictators in Dana et al. (2007) are driven by other motivations. For instance, they may be using the shortage of time allowed for making an active choice as a credible excuse to remain passive.

Finally, while we find no omission effect in choices, it is possible that there is nevertheless an omission effect in the moral judgment of others' choices. Previous vignette studies in psychology (Spranca et al. 1991; Kordes-de Vaal 1996; Cushman et al. 2006; DeScioli, Bruening and Kurzban 2011; Cushman et al. 2012) suggest that observers judge harmful acts as morally worse than equally harmful omissions. Moreover, previous incentivized experiments on fairness that look at the punishment of unfair behavior find results relating to the omission effect. Coffman (2011) studies third party punishment in a dictator game and finds that dictators who take money directly from the recipient are punished more than dictators who delegate the implementation of the same outcome to an intermediary with no other option than to implement the outcome. This suggests that third party judgment is affected by whether the action taken by the dictator is directly associated with the selfish outcome. DeScioli, Christner and Kurzban (2011) also study third party punishment, conducting a dictator game in which a selfish but dominated allocation is indicated as the passive default option. They find that dictators choose this passive default more often when a punisher is present as compared to when there is no punisher. Cox et al. (2013) study second party punishment in an ultimatum game, varying which of two allocations is the status quo option. They find that selfish choices are punished less often by the recipient when the selfish option is the status quo, as compared to when the fair option is the status quo. These studies suggest that when punishing the behavior of others, individuals take into account whether an unfair allocation is implemented as a passive default (DeScioli, Christner and Kurzban 2011) and whether it is the initial endowment that needs to be implemented actively (Cox et al. 2013). However, at least two important questions still need to be addressed. First, is there an omission effect in the punishment of selfish behavior, independent of the effect of defaults on punishment? Second, is there an omission effect in prosocial behavior, independent of the default effect, in the presence of punishment opportunities?

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Appendix A: Additional tables and figures

Table A.1 All allocation choices.

Choice nr.	π_i	π_j	$\hat{\pi}_i$	$\hat{\pi}_j$	$\frac{\hat{\pi}_j - \pi_j}{(\pi_i - \hat{\pi}_i)}$	$\pi_i - \pi_j$	$\hat{\pi}_i - \hat{\pi}_j$
1	90	10	70	70	3	80	0
2	75	75	90	60	1	0	30
3	140	10	75	75	1	130	0
4	125	25	150	0	1	100	150
5	150	0	90	60	1	150	30
6	125	40	135	0	4	85	135
7	110	40	70	80	1	70	-10
8	70	80	140	10	1	-10	130
9	100	25	75	100	3	75	-25
10	90	90	110	10	4	0	100
11	40	80	60	60	1	-40	0
12	50	50	30	110	3	0	-80
13	50	100	50	50	-	-50	0
14	25	125	0	0	-5	-100	0

Note: π_i denotes payoff to the dictator and π_j denotes payoff to the recipient. Each allocation involves a tradeoff between the two payoff vectors (π_i, π_j) and $(\hat{\pi}_i, \hat{\pi}_j)$. The ratio $(\hat{\pi}_j - \pi_j)/(\pi_i - \hat{\pi}_i)$ indicates the "relative price" of giving, i.e. the amount of DKK the recipient gains (loses) for every DKK the dictator loses (gains). $\pi_i - \pi_j$ and $\hat{\pi}_i - \hat{\pi}_j$ indicates the inequality of payoffs between the dictator and the recipient in the two payoff vectors (a positive number indicates that the dictator is ahead and a negative number indicates that the dictator is behind). All subjects face choice 1 as their first choice. The order of choices 2-14 is randomized.

Table A.2 Participant characteristics.

	All	Commission treatment	Omission treatment	Difference	*p*-value
Age	25.595	25.320	25.870	-0.550	0.196
	(4.253)	(4.025)	(4.463)		
Female	0.480	0.465	0.495	-0.030	0.550
	(0.500)	(0.500)	(0.501)		
Danish	0.258	0.240	0.276	-0.036	0.419
	(0.438)	(0.428)	(0.448)		
Full time student	0.778	0.758	0.798	-0.040	0.334
	(0.416)	(0.430)	(0.403)		
Economics course(s)	0.610	0.635	0.585	0.050	0.305
taken	(0.488)	(0.483)	(0.494)		

Note: Standard deviations in parentheses. *N*=400. All variables except for *Age* are binary. The *p*-value for the age difference across treatments is obtained using a two-sided, two-sample t-test. All other p-values are obtained using chi-squared tests.

Table A.3 Treatment effects in all 14 allocation choices

Choice no.	π_i	π_j	$\hat{\pi}_i$	$\hat{\pi}_j$	Share of default choices: Default (π_i, π_j) Commission	Omission	Omission – Commission Effect size	p	Default $(\hat{\pi}_i, \hat{\pi}_j)$ Commission	Omission	Omission – Commission Effect size	p
1	90	10	70	70	0.438	0.467	0.028	(0.691)	0.638	0.526	-0.112	(0.109)
2	75	75	90	60	0.390	0.330	-0.060	(0.380)	0.653	0.670	0.017	(0.797)
3	140	10	75	75	0.684	0.648	-0.035	(0.607)	0.449	0.339	-0.110	(0.107)
4	125	25	150	0	0.535	0.467	-0.068	(0.334)	0.543	0.583	0.040	(0.582)
5	150	0	90	60	0.538	0.656	0.119	(0.096)	0.442	0.404	-0.038	(0.575)
6	125	40	135	0	0.651	0.558	-0.093	(0.198)	0.333	0.448	0.115	(0.091)
7	110	40	70	80	0.769	0.768	-0.002	(0.979)	0.222	0.257	0.035	(0.570)
8	70	80	140	10	0.378	0.258	-0.120	(0.082)	0.733	0.692	-0.042	(0.502)
9	100	25	75	100	0.618	0.690	0.073	(0.263)	0.366	0.391	0.025	(0.727)
10	90	90	110	10	0.623	0.514	-0.109	(0.110)	0.432	0.452	0.020	(0.789)
11	40	80	60	60	0.043	0.061	0.019	(0.561)	0.990	0.941	-0.049	(0.054)
12	50	50	30	110	0.917	0.806	-0.110	(0.022)	0.116	0.168	0.052	(0.308)
13	50	100	50	50	0.592	0.632	0.040	(0.556)	0.500	0.532	0.032	(0.658)
14	25	125	0	0	0.930	0.894	-0.036	(0.370)	0.075	0.123	0.047	(0.267)

Note: π_i denotes payoff to the dictator and π_j denotes payoff to the recipient. Each allocation involves a tradeoff between the two payoff vectors (π_i, π_j) and $(\hat{\pi}_i, \hat{\pi}_j)$. All subjects face choice 1 as their first choice. The order of choices 2-14 is randomized within subject.

Figure A.1 Screenshot of commission treatment

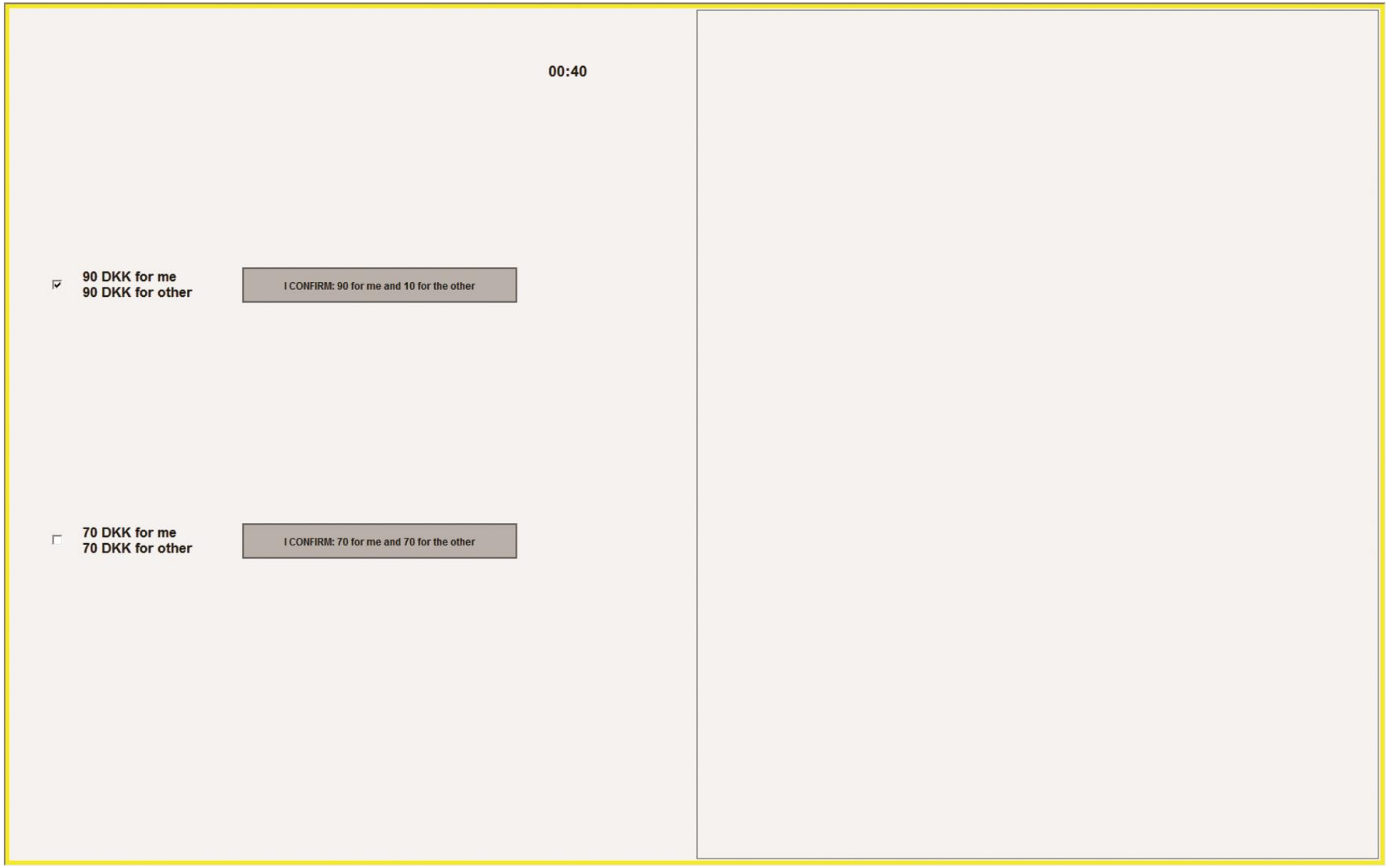

Figure A.2 Screenshot of omission treatment

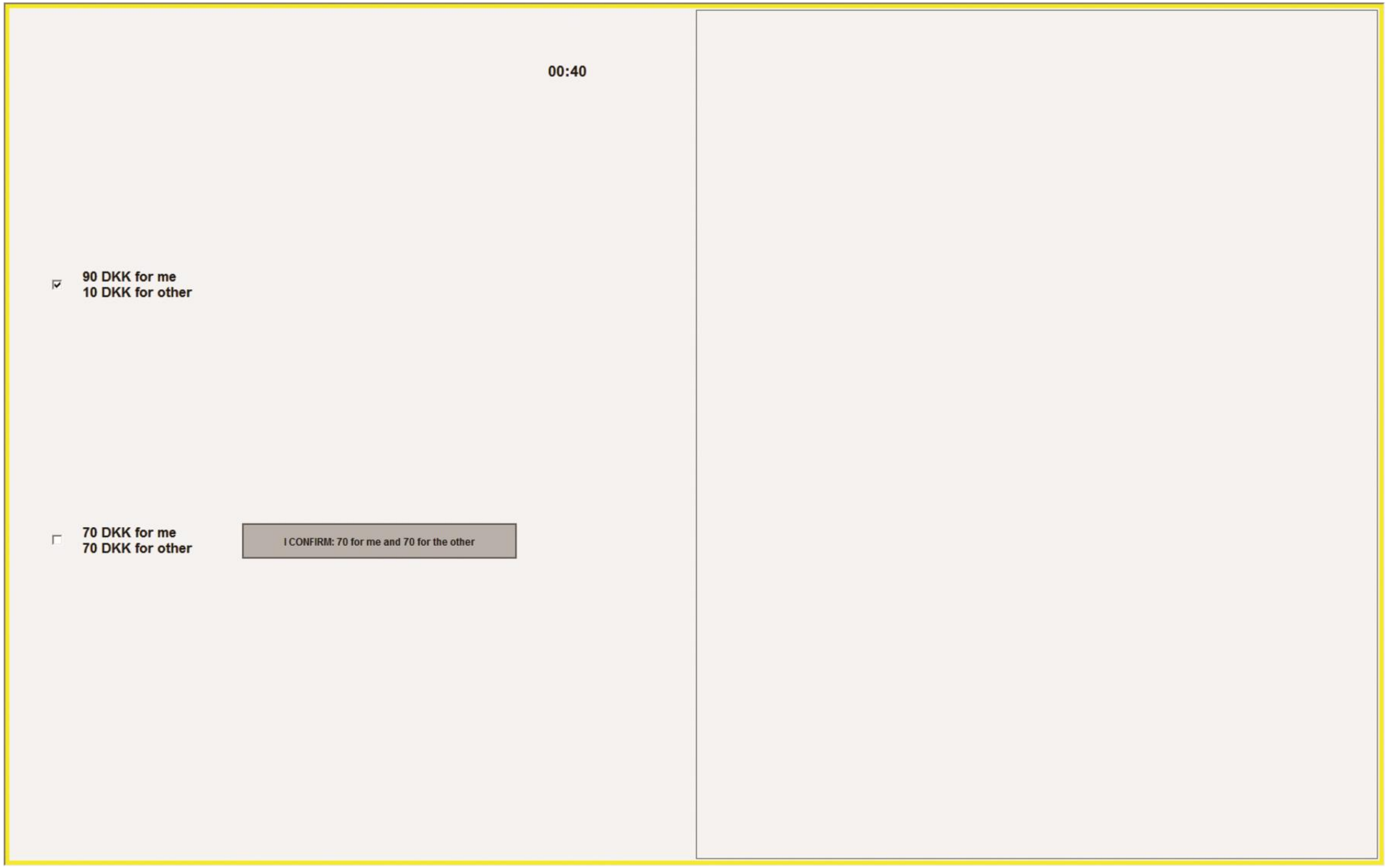

Figure A.3 Screenshot of slider task

Appendix B: Results for fair default and Hypothesis 2 (Experiment 1)

First allocation choice

Our second hypothesis addresses the concern that an observed omission effect under selfish defaults may reflect a more general omission effect. This would imply a similar omission effect also under fair defaults. When the fair allocation (70,70) is presented as the default option, the share of selfish choices is 36.2% in the commission treatment and 47.4% in the omission treatment. Thus, rather than showing an omission effect, subjects display a tendency, albeit not statistically significant ($\chi^2(1) = 2.57$, $p = 0.109$), to be *less* prone to implement the fair default allocation by omission rather than by commission. If anything, this could indicate that prosocial choices contribute less to a subject's positive self- or social image if they are implemented by omission rather than by commission. Given this result, and given that we find no significant omission effect under selfish defaults, we can rule out the existence of a general omission effect. Nevertheless, it may be interesting to investigate whether omissions have a significantly different effect on fair choices as compared to selfish choices. The first regression in Table B.1 provides such a test. The coefficient of the interaction term indicates that the absolute difference in omission effects between the (90,10) default condition and the (70,70) default condition is 14 percentage points, but not statistically significant. Hence, we cannot reject the null hypothesis of no difference between the two default conditions.

All allocation choices

The omission effect under non-selfish defaults, pooling all allocation choices, is given by the coefficient of the indicator variable *Omission treatment* in Models 2-5 in Table B.1. This effect tends to be negative and is statistically not significant at the 5 percent level in all models. In Model 2, when we exclude choices that involve behindness, the effect is marginally significant at the 10 percent level. Thus, in line with the results from the first allocation choice, we find no tendency for a general omission effect when looking at all choices pooled. Further, the difference in omission effects between choices with a selfish and a non-selfish default, as indicated by the interaction term, is not significant at the 5 percent level in all models. This difference is significant at the 10 percent level only in Model 3, showing a difference of 12.9 percentage points.[7]

[7] When including the observations that have been dropped due to a failure to make an active choice in the commission treatment, some significance levels in Model 3 of Table B.1 change slightly. Under the assumption that these choices would have been non-selfish, the interaction term and the coefficient of *Omission treatment*

Table B.1 Treatment and default effects on the propensity to choose default, all choices pooled

	Model 1	Model 2	Model 3	Model 4	Model 5
Omission treatment	-0.112 (0.070)	-0.050 (0.033)	-0.083* (0.044)	0.005 (0.032)	-0.058 (0.041)
Selfish default	-0.200*** (0.071)	0.279*** (0.038)	-0.016 (0.052)	0.467*** (0.031)	0.066 (0.045)
Omission treatment X Selfish default	0.140 (0.100)	0.054 (0.054)	0.129* (0.072)	-0.024 (0.046)	0.078 (0.065)
Constant	0.638*** (0.048)	0.387*** (0.023)	0.531*** (0.031)	0.290*** (0.022)	0.488*** (0.029)
R^2	0.02	0.10	0.01	0.21	0.01
N (choices)	394	5,132	2,765	2,370	1,971
Choices included	First choice	Selfish vs. non-selfish	Selfish vs. non-selfish (not behind)	Selfish vs. behind	Selfish vs. efficient

Note: OLS regressions. The sample is restricted to choices where the default option is non-selfish (not strictly payoff dominant for the dictator). Dependent variable: = 1 if default chosen, = 0 otherwise. *Model 1* only includes the first choice between (90,10) and (70,70). *Model 2* includes all allocation choices except choice number 13 which has no strictly payoff dominant option for the dictator. *Model 3* includes choices 1, 2, 3, 4, 5, 6 and 10. *Model 4* includes choices 7, 8, 9, 11, 12 and 13. *Model 5* includes choices 1, 6, 9, 10 and 12. See Table A1 in Appendix A for a list of all choices. Standard errors are clustered on participant in all models except for *Model 1*. * p<0.1; ** p<0.05; *** p<0.01

become significant at the 5 percent level. Under the assumption that these choices would have been default choices, the coefficient of *Omission treatment* becomes significant at the 5 percent level.

Appendix C: Structural estimation

In this section, we exploit the panel data nature of our data set to test our first hypothesis by classifying subjects into different social preference types. In particular, we investigate if the distribution of types differs between the omission and the commission treatment, given a selfish default. Thus, we restrict the sample to all choices with a default that is payoff-dominant for the dictator. The structural estimation provides a useful addition to our other estimates as it takes both inequity aversion and social welfare preferences into account in the same model. While our previous analyses are restricted to comparing average selfishness rates across treatments, the structural estimation allows for a more multifaceted distribution of individual social preferences.

We assume that individual *i*'s preferences take the following theoretical form, similar to the specification in Fehr and Schmidt (1999) and Charness and Rabin (2002):

$$U_i(\pi_i,\ \pi_j) = \beta_i \pi_i + \rho_i r(\pi_j - \pi_i) + \sigma_i s(\pi_j - \pi_i),$$

where

$$r = 1\ if\ \pi_i > \pi_j, r = 0\ otherwise,$$

$$s = 1\ if\ \pi_i < \pi_j, s = 0\ otherwise,$$

denoting individual *i*'s payoff by π_i and the other individual's payoff by π_j. The parameter β_i captures the weight individual *i* places on her own income, and the parameter ρ_i [σ_i] captures the weight that *i* places on relative income (π_j - π_i) while *i* earns more [less] than *j*. Thus, given β_i>0, for a completely *selfish* individual, who only cares about maximizing her own payoff, we would observe $\rho_i = \sigma_i$ =0. For an *inequality averse* individual, who prefers to minimize differences in income between herself and others, we would observe ρ_i>0 and σ_i<0. This utility function is simple and parsimonious while still allowing for identifying a range of potentially important social motivations, such as inequity aversion and social welfare preferences, through a few binary allocation choices. Note that our set of binary choices varies these motives in a way that allows us to calibrate the function. This utility function is also commonly used in previous literature, and thus well suited for comparison purposes.

The number of decisions per subject in our data set is too small to estimate β_i, ρ_i and σ_i individually for each subject. Instead, we estimate a latent class conditional logit model, allowing for a finite number *C* of classes (or "types") in the population. Each subject is assumed to belong to a class *c*, and we allow preference parameters β_c, ρ_c, and σ_c to vary across, but not within, classes. If an individual *i* belongs to class *c* and faces *T* different choices with two alternatives in each choice, the probability of observing a particular sequence of choices is

$$P_i\left(\beta_{c,}\,\rho_{c,}\,\sigma_c\right) = \prod_{t=1}^{T}\prod_{k=1}^{2}\left(\frac{\exp\left(U_{ikt}\left(\pi_{ikt},\,\pi_{jkt};\;\beta_{c,}\,\rho_{c,}\,\sigma_c\right)\right)}{\sum_{m=1}^{2}\exp\left(U_{i\,mt}\left(\pi_{imt},\,\pi_{jmt};\;\beta_{m,}\,\rho_{m,}\,\sigma_m\right)\right)}\right)^{y_{ikt}},$$

where y_{ikt} is our dependent variable that takes the value of 1 if agent *i* chooses choice alternative *k* in choice *t*, and 0 otherwise.

H_c denotes the probability of belonging to a class *c*, and is specified as

$$H_c(\boldsymbol{\theta}) = \frac{\exp(\boldsymbol{\theta_c z_i})}{1+\sum_{l}^{C-1}\exp(\boldsymbol{\theta_l z_i})},$$

where $\boldsymbol{\theta} = (\boldsymbol{\theta_1}, \boldsymbol{\theta_2}, \dots, \boldsymbol{\theta_{C-1}})$ are class membership parameters that are estimated along with the preference parameters and $\boldsymbol{\theta_C}$ is normalized to zero. The individual-specific characteristics $\boldsymbol{z_i}$ include an indicator for the treatment group (omission or commission) and a constant. Thus, class membership varies with treatment status.

The log-likelihood for this model is

$$\ln L\,(\boldsymbol{\beta}, \boldsymbol{\rho}, \boldsymbol{\sigma}, \boldsymbol{\theta}) = \sum_{i=1}^{N} ln \sum_{c=1}^{C} H_c(\boldsymbol{\theta}) P_i\left(\beta_{c,}\,\rho_{c,}\,\sigma_c\right),$$

where $\boldsymbol{\beta} = (\beta_1, \beta_2, \dots, \beta_C)$, $\boldsymbol{\rho} = (\rho_1, \rho_2, \dots, \rho_C)$ and $\boldsymbol{\sigma} = (\sigma_1, \sigma_2, \dots, \sigma_C)$. The model is estimated using the *lclogit* package and the *glamm* package for Stata (Pacifico and Yoo 2013; Skrondal and Rabe-Hesketh 2004).

To determine the optimal number of classes *C*, we estimate the model using 2-10 numbers of classes and compute the Bayesian information criterion (BIC) and the Akaike information criterion (AIC) for each specification. Both information criteria suggest a specification with three classes. Using three classes, the model estimates the nine preference parameters, $(\beta_{1,}\ \rho_{1,}\ \sigma_1)$, $(\beta_{2,}\ \rho_{2,}\ \sigma_2)$, $(\beta_{3,}\ \rho_{3,}\ \sigma_3)$.

In Table C.1, we report the estimated preference parameters for each of the three types, and the share of subjects that is estimated to be of each type, both overall and for each treatment. Following the above description of the preference parameters, we can roughly characterize the first type as inequality averse and the second type as selfish. The third type is best described as having maxi-min social preferences, only caring about the income of the other person when ahead and only about her own income when behind. Overall, the share of inequality averse types is 43.8%, the share of selfish types is 39.5% and the share of maxi-min types is 16.7%. A joint test of whether the shares of types differ between the omission and the commission treatment shows that there is no significant treatment effect ($\chi^2(2)$ = 1.40, p = 0.4975). Hence, given a selfish default option, we find no evidence that the distinction between active and passive choices has an independent effect on an individual's prosocial behavior. Consequently, even when taking into account that there is a distribution of individual social preferences, and that individual choices might be classified in other ways than merely selfish vs. non-selfish, we can confirm the findings from our main analyses.

Table C.1 Latent class model estimates for defaults with $\pi_i > \hat{\pi}_i$

	Type 1	Type 2	Type 3
β	0.086***	0.192***	0.060***
	(0.006)	(0.038)	(0.014)
ρ	0.039***	0.018*	0.061***
	(0.003)	(0.009)	(0.010)
σ	-0.007**	-0.013	0.005
	(0.003)	(0.013)	(0.004)
N	2576 choices by 397 participants		
Proportion of types:			
Overall	43.8%	39.5%	16.7%
Commission treatment	47.0%	36.3%	16.7%
Omission treatment	40.7%	42.5%	16.8%

Dependent variable: Binary variable indicating whether allocation was chosen or not. Standard errors in parentheses. Significance levels are denoted by * p < 0.05, ** p < 0.01, *** p < 0.001. Parameters are estimated using the expectation maximization algorithm. The model includes all allocation trade-offs except choice 13 which has no strictly payoff dominant option for the dictator.

Appendix D: Robustness checks

Participants in the commission treatment failed to make an active choice in 2.6% of all choice occasions. In our main analyses, we exclude these observations. In this section, we test the robustness of our main results by reproducing Table 3 including these observations. In Table D.1, we assume that these observations would have been selfish choices (i.e. default choices), and in Table D.2 we assume that they would have been non-selfish choices (i.e. not default choices).

Table D.1 Treatment effect on the propensity to choose the selfish default, interpreting failure to choose in the commission treatment as a selfish choice.

	Model 1	Model 2	Model 3	Model 4	Model 5
Omission treatment	0.010	-0.004	0.037	-0.025	0.010
	(0.072)	(0.030)	(0.041)	(0.030)	(0.039)
Constant	0.457***	0.674***	0.523***	0.763***	0.564***
	(0.052)	(0.020)	(0.029)	(0.019)	(0.028)
R^2	0.00	0.00	0.00	0.00	0.00
N (choices)	197	2,605	1,359	1,242	1,009
Choices included:	First choice	Selfish vs. non-selfish	Selfish vs. non-selfish (not behind)	Selfish vs. behind	Selfish vs. efficient

Note: OLS regressions. The sample is restricted to choices where the default option is selfish (strictly payoff dominant for the dictator). Dependent variable: = 1 if default chosen, = 0 otherwise. *Model 1* only includes the first choice between (90,10) and (70,70). *Model 2* includes all allocation choices except choice number 13 which has no strictly payoff dominant option for the dictator. *Model 3* includes choices 1, 2, 3, 4, 5, 6 and 10. *Model 4* includes choices 7, 8, 9, 11, 12 and 13. *Model 5* includes choices 1, 6, 9, 10 and 12. See Table A.1 in Appendix A for a list of all choices. Standard errors are clustered on participant in all models except for *Model 1*. * p<0.1; ** p<0.05; *** p<0.01.

Table D.2 Treatment effect on the propensity to choose the selfish default, interpreting failure to choose in the commission treatment as a non-selfish choice.

	Model 1	Model 2	Model 3	Model 4	Model 5
Omission treatment	0.043	0.019	0.055	-0.001	0.032
	(0.071)	(0.031)	(0.041)	(0.031)	(0.039)
Constant	0.424***	0.651***	0.505***	0.740***	0.542***
	(0.052)	(0.021)	(0.029)	(0.021)	(0.028)
R^2	0.00	0.00	0.00	0.00	0.00
N (choices)	197	2,605	1,359	1,242	1,009
Choices included:	First choice	Selfish vs. non-selfish	Selfish vs. non-selfish (not behind)	Selfish vs. behind	Selfish vs. efficient

Note: OLS regressions. The sample is restricted to choices where the default option is selfish (strictly payoff dominant for the dictator). Dependent variable: = 1 if default chosen, = 0 otherwise. *Model 1* only includes the first choice between (90,10) and (70,70). *Model 2* includes all allocation choices except choice number 13 which has no strictly payoff dominant option for the dictator. *Model 3* includes choices 1, 2, 3, 4, 5, 6 and 10. *Model 4* includes choices 7, 8, 9, 11, 12 and 13. *Model 5* includes choices 1, 6, 9, 10 and 12. See Table A.1 in Appendix A for a list of all choices. Standard errors are clustered on participant in all models except for *Model 1*. * p<0.1; ** p<0.05; *** p<0.01.

Appendix E: Experimental instructions for experiment 1

The following are the instructions used in the experiment reported in the paper.

E.1 General instructions

Welcome to this experiment! Please read these instructions, and the instructions on your screen, thoroughly. Do not talk to other participants during the experiment. Whenever you have any questions, please raise your hand and wait for one of us to come to you.

The experiment consists of two parts: *Part 1* and *Part 2*. These instructions describe Part 1. Information about Part 2 will follow once you have completed Part 1.

You can earn money in this experiment. All amounts stated in the experiment are in Danish kroner (DKK) and your earnings will be paid privately in cash at the end of the experiment. You are only paid for tasks that you completed according to the instructions given to you. **Your decisions and the decisions of other participants will remain anonymous.**

Part 1 of this experiment consists of two different types of tasks: the *slider task* and the *distribution task*. It will take about 20 minutes. The size of your reward from this part will depend solely on your decisions.

On the following pages, we describe both tasks, and give an overview of the structure of the experiment. After reading the instructions, you will have time to practice both tasks on the screen. We will also ask a number of control questions on the screen to make sure you understand the instructions.

E.2 Instructions for slider task

The slider task provides a set of sliders on the right side of your screen.

You can adjust each slider to any position between 0 and 100 by pressing the slider with your mouse and dragging it to the desired position. There is one number at each end of a slider. The black number to the right tells you the current position of the slider. The red number to the left tells you a target position.

A slider is correctly adjusted, when the current position is equal to the target position. For example, the upper slider in the picture below has the current position of zero and a target position of 50. The lower slider in the picture shows the same slider when it is adjusted correctly – that is, when the current position is equal to the target position.

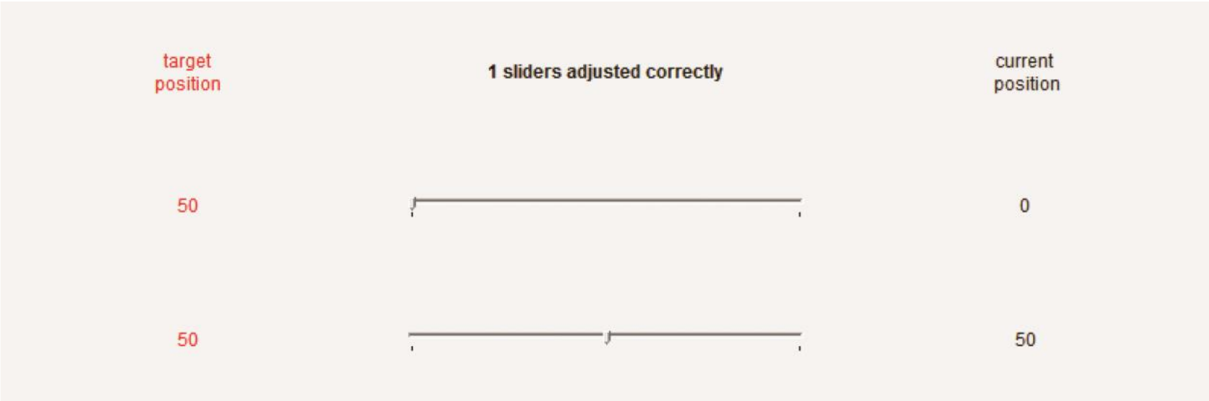

Figure E.1 Screenshot of correctly adjusted slider

The sliders will be on your screen throughout part 1 of the experiment. As soon as one set of sliders is correctly adjusted, a new set of sliders will appear. Thus, you can adjust the sliders at any time and you can solve as many sliders as you like for the duration of the experiment. A counter on top of the sliders tells you how many sliders you adjusted correctly.

You earn 0.01 DKK (1 øre) for each slider that is correctly adjusted. At the end of the experiment, you will be paid for all the sliders you have correctly adjusted throughout the experiment.

E.3 Instructions for distribution task

a. For participants in the commission treatment

The distribution task consists of several decisions. **Each decision will appear for 40 seconds on the left side of the screen, as indicated by a timer**. When the time on the timer has run out, the decision will disappear.

For each decision, you will be randomly paired with another participant, who is in this room and who participates in the slider task. You can choose one of two different distributions of money between you and the other participant.

When a decision appears on the screen, you will see two alternatives with checkboxes next to them. Each alternative states an amount of crowns that will be given to you and an amount that will be given to the other participant. One of the two checkboxes has already been checked at random.

For example, a decision between an alternative that gives 100 DKK to you and 100 DKK to the other, and an alternative that gives 200 DKK to you and 200 DKK to the other, where the first alternative has already been checked, would look like this:

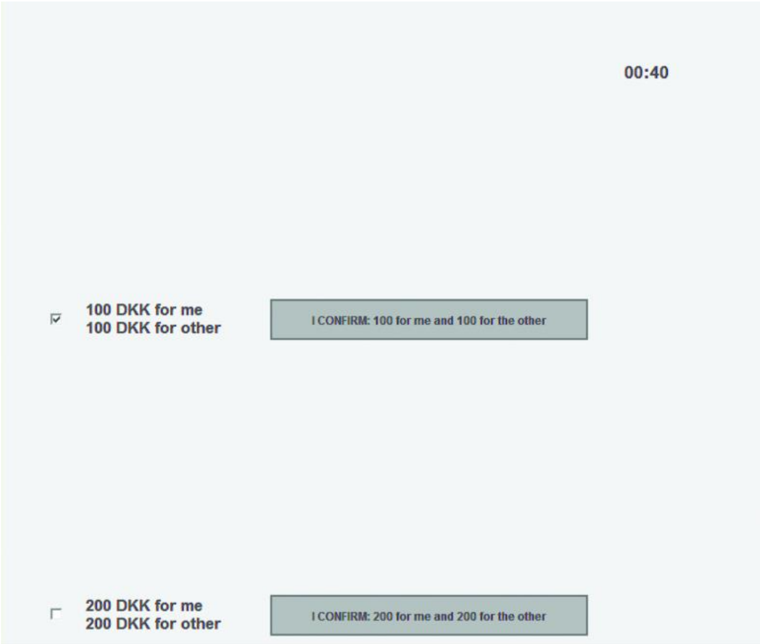

Figure E.2 Screenshot of decision in commission treatment

To choose one of the two alternatives, the checkbox next to the alternative needs to be checked and the button stating the alternative needs to be pressed for confirmation. If this is done correctly, the button will turn red, and you can no longer change your decision.

Note that the distribution task will not disappear before the time has run down, even if you have pressed a button.

For each new decision you face in the distribution task, you will be paired with a new participant. **At the end of the experiment, one of the decisions will be selected at random, and you and the other participant will be paid the amounts stated in the alternative that you chose.**

b. For participants in the omission treatment

The distribution task consists of several decisions. **Each decision will appear for 40 seconds on the left side of the screen, as indicated by a timer**. When the time on the timer has run out, the decision will disappear.

For each decision, you will be randomly paired with another participant, who is in this room and who participates in the slider task. You can choose one of two different distributions of money between you and the other participant.

When a decision appears on the screen, you will see two alternatives with checkboxes next to them. Each alternative states an amount of crowns that will be given to you and an amount that will be given to the other participant. One of the two checkboxes has already been checked at random.

For example, a decision between an alternative that gives 100 DKK to you and 100 DKK to the other, and an alternative that gives 200 DKK to you and 200 DKK to the other, where the first alternative has already been checked, would look like this:

Figure E.3 Screenshot of decision in omission treatment

The alternative that is already checked will be selected automatically when time has run out. To choose the other alternative, the checkbox next to that alternative needs to be checked and the button stating that alternative needs to be pressed for confirmation. If this is done correctly, the button will turn red, and you can no longer change your decision.

Note that the distribution task will not disappear before the time has run down, even if you have pressed a button.

For each new decision you face in the distribution task, you will be paired with a new participant. **At the end of the experiment, one of the decisions will be selected at random, and you and the other participant will be paid the amounts stated in the alternative that you chose.**

E.4 Overview of the experiment (example from commission treatment)

Below is a sketch of how Part 1 of the experiment evolves over time.

Throughout the experiment, there will always be sliders you can solve on the right side of the screen. From time to time, distribution task decisions will appear on the left side of the screen. When time has run down on the timer of a distribution task decision, that decision will disappear. In between the different decisions, the left side of the screen will be blank.

Please note: The slider task will always be present and the sliders can be adjusted at any time. Your adjustments to the sliders remain even when the distribution task appears or disappears. How many sliders you or other participants solve does *not* have an influence on the amounts that you will face in the distribution task.

Figure E.4 Structure of experiment (commission treatment)

Appendix F: Experimental instructions for Experiment 2

This HIT is part of a research project. First, we present to you a scanned text paragraph. You are asked to enter the paragraph word for word into a text box. Make sure to enter the text exactly as it appears in the scanned image. Disregard any line breaks. You can see an example of such a text below. You must complete the paragraph to have your work accepted.

[transcription task]

You and another MTurk worker are matched in this HIT. Note that the other worker is an actual worker and not virtual. Each of you receives a fixed payment of $0.50, but in the following task you can earn an additional bonus.
After you have completed this HIT, you or the other worker that you have been matched with will be chosen at random and both of you will be paid a bonus according to that worker's decisions.
Note that your bonus will be paid through MTurk on top of the fixed payment that you receive. We will make sure to pay the bonuses as soon as the work is completed or the HIT expires.
Press OK to proceed.

F.1 No-default treatment

You can select one of two allocations of bonus payments for you ("You") and the other worker ("Other") in this task.
On the next screen, a timer will count down 40 seconds and you will proceed automatically after these 40 seconds. During this time you can select one option. Note that only completed tasks are paid.
The other worker will receive a message that states the details of the choice situation and which bonus payment you selected. You will both be paid accordingly.
Press OK to start the timer.

Please select the allocation you prefer. Note that only completed tasks are paid.
- ❑ You: $1.05, Other: $0.05
- ❑ You: $0.70, Other: $0.70

F.2 Commission treatment

You and the other worker receive the following bonus payments for this task:
You: $1.05, Other: $0.05
On the next screen, a timer will count down 40 seconds and you will proceed automatically after these 40 seconds. During this time you can either confirm the bonus payment above or select an alternative option. Note that only completed tasks are paid.
The other worker will receive a message that states the details of the choice situation and which bonus payment you selected. You will both be paid accordingly.

Press OK to start the timer.

Please select the allocation you prefer. Note that only completed tasks are paid.
- ❑ You: $1.05, Other: $0.05
- ❑ You: $0.70, Other: $0.70

F.3 Omission treatment

You and the other worker receive the following bonus payments for this task:
You: $1.05, Other: $0.05
On the next screen, a timer will count down 40 seconds and you will proceed automatically after these 40 seconds. During this time you can either confirm the bonus payment above or select an alternative option.
If no choice is made within 40 seconds, we will automatically transfer $1.05 to you and $0.05 to the other worker as bonus payments for this task.
The other worker will receive a message that states the details of the choice situation and which bonus payment you selected, or whether the timer ran down before a choice was made. You will both be paid accordingly.
Press OK to start the timer.

Please select the allocation you prefer. If no choice is made within 40 seconds, we will automatically transfer $1.05 to you and $0.05 to the other worker as bonus payments for this task.
- ❑ You: $1.05, Other: $0.05
- ❑ You: $0.70, Other: $0.70