How Noise Shapes Social Choice

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Abstract

This paper reports the results of an experimental study investigating the role of uncertainty for pro-social behavior. We compare settings in which the decision-maker is solely responsible for the outcome to settings in which his choice is only implemented with some probability while with the complementary probability a "default" is implemented. We find that a sufficiently high probability of the default is necessary to alter decisions. Sufficiently-high-probability defaults lead to less giving by pro-social decision-makers, not only when the default is generous to the receiver but also when it is selfish. This is neither consistent with expected utility theory nor with notions of ex ante fairness, and we discuss its fit with other explanations.

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1 Introduction

The fact that people give voluntarily to others has received substantial recognition in experimental economics – summed up, for example, in Engel (2011). It inspired a number of economic theories that embed concerns for fairness directly in the utility function (e.g., Fehr and Schmidt 1999, Bolton and Ockenfels 2000, Charness and Rabin 2002, Andreoni and Miller 2002). Such theories of social giving have traditionally been formulated in settings without uncertainty to explain why people give in games such as the dictator and the ultimatum game. Nevertheless, such theories are routinely applied to settings with uncertainty – either because the setting is inherently probabilistic, for example, when workers' incomes have a non-trivial stochastic component, or because the actions of other players are uncertain. Expected utility theory has been the standard method to extend the utility functions developed for deterministic settings to settings with stochastic noise or strategic uncertainty.

Such an approach to uncertainty has strong implications that might be particularly challenging in social settings. For example, expected utility theory predicts that a decider should not choose a non-degenerate lottery over all its deterministic outcomes except for the knife-edge case where he is completely indifferent amongst some of these deterministic outcomes. This is challenged in a handful of recent studies on probabilistic dictator games in which deciders do frequently choose interior probabilities and often choose equal probabilities of "winning" rather than one person winning with certainty (see, e.g., Karni, Salmon, and Sopher 2008, Bohnet et al. 2008, Bolton and Ockenfels 2010, Krawczyk and Le Lec 2010, and Kircher, Ludwig, and Sandroni 2013). This has given rise to the development of theories in which individuals care not only about (ex-post) fairness in final payoffs but also about (ex-ante) fairness in expected payoffs (c.f. Trautman 2009, Karni and Safra 2002, Krawczyk 2001, Fudenberg and Levine 2012, Borah 2012, Saito 2013, Takanashi 2021, Feldman and López Vargas 2023).

The aim of this paper is to consider social choices under uncertainty from a different angle, which is based on comparative statics across populations facing different social choices under uncertainty. While such a setup is substantially different in the experimental setup from the above lottery choices, it provides complementary evidence on the role of uncertainty for social decision making. As an example for the type of environment we are envisioning, consider a team leader who can propose how a bonus to his unit is split between himself and his subordinate, but knows that there is a chance that – independent of his proposal – his company might implement a "default" split. Would his proposal be different from the one he would have made in a (standard) dictator setting where the chance of a "default" implementation is zero? Would it matter for his proposal whether the "default" mainly rewards himself or his subordinate? We will see that the answer to these questions can discriminate between theories. In the conclusion we also discuss real world-applications such as compensation policies or voting for redistribution under the threat of revolution where the answers to these questions are directly relevant.

In terms of theories, it should be noted that the answer to both questions mentioned above is "no" if the decision maker adheres to expected utility theory, and it remains a "no" under extensions to non-expected utility that still satisfy the much weaker axiom of stochastic dominance.[1] This follows from the fact that he can neither change the probability nor the outcome of the default. To contrast this with concerns for ex-ante fairness, we find it useful to review Saito (2013) who axiomatizes a simple theory that allows for ex-ante fairness alongside the tradition of ex-post considerations. Consider a setting with a decider (he) and a receiver (she), let $\mathbf{x} = (x_1, x_2)$ denote their respective monetary payoffs, and let $U(\mathbf{x})$ be a utility function that incorporates fairness concerns by considering both of their payoffs, which Saito parameterizes according to the well-known Fehr-and-Schmidt (1999) utility function that penalizes inequality. When the decider faces a lottery, he evaluates it as a weighted average of ex-post fairness, i.e., $E(U(\mathbf{x}))$, and ex-ante fairness, i.e., $U(E(\mathbf{x}))$. If all the weight is on the former, the decider has standard expected utility preferences, and confronted with a choice of a probability distribution over (1,0) and (0,1) he would choose $(1, 0)$ for sure as it is equally (un)fair ex-post and offers more personal gain. If all the weight is on the latter, the decider evaluates fairness ex-ante by comparing the expected payoff that people obtain – as in Trautman (2009). Confronted with

[1] If neither the probability nor the outcome of the default can be affected, the independence axiom underlying expected utility theory guarantees that neither the probability nor the default outcome affect the decider's choice. It turns out that the much weaker axiom of stochastic dominance (sometimes also called "monotonicity") – which underlies even non-expected utility theories such as rank-dependent expected utility theory that is nowadays a building block of prospect theory – suffices to ensure this. This axiom entails that if two lotteries differ only in the payoff in a single state (and the odds of all states are the same in both lotteries), then the preferred lottery is the lottery that delivers the preferred outcome in that state in the absence of uncertainty. See the earlier working paper version of this paper (Höchtl et al, 2015) for elaboration.

the same choice over (1,0) and (0,1), now a highly inequality-averse decider would choose a "fair" lottery because in ex-ante terms this leads to equal expected outcomes $E(\mathbf{x}) = (1/2, 1/2)$. Such a theory can explain the choice of lotteries over deterministic outcomes and the focus on equal probabilities in the experimental work cited in the second paragraph.

Since this theory seems to offer an easy way to formalize ex-ante fairness, we use it to derive additional predictions for our experimental setting beyond simply a failure of expected utility theory (or stochastic dominance more generally). These predictions apply as long as at least some individuals are sufficiently motivated by ex-ante considerations and by social motives. It predicts that individuals in the standard dictator setting give more than those in a setting with a default that is very generous to the receiver, as the generous default already transfers sufficient resources in expected terms.[2] Similarly, it predicts that individuals in the standard dictator setting give less than those in a setting with a default that is rather selfish, as more resources are needed in this case to make the allocation fair in ex ante terms. Differences to the standard dictator game should vanish for a very small probability of the default, as in this case the ex-post and ex-ante outcomes become nearly identical. Finally, the theory predicts an asymmetry: a decider should become substantially more selfish if the default is too generous to the receiver, while the decider's choices will only become mildly more generous to the receiver when the default is too selfish in favor of the decider. The reason is easy to see. If the default is generous to the receiver, the decider is ex-ante behind his optimal payoff, which induces a large reduction in utility. He can correct this by reducing the generosity of his proposal, which lowers the receiver's expected payoff. If his proposal gets implemented, he suffers some reduction of utility ex-post as he is ahead of the receiver, but this effect is moderate. On the other hand, if the default is selfish, the decider gets more than his optimal split ex-ante, which he dislikes moderately. He can correct this by increasing the generosity of his proposal, but this places him behind the optimal split ex-post if his decision gets implemented, with a large penalty in terms of utility. So the

[2]Note that in our experiments the 'default' is the amount that is transferred if the decider cannot decide (there is no default in the case where he can decide) – that is, the default cannot be overruled by the decision maker. This is very different to settings where the 'default' is the amount that is transferred in a social setting if the decider does not take any decision – as, for instance, in the field experiments on charitable giving conducted by Altmann et al. (2019), where the decider can always overrule the default.

magnitude of the reaction is lower (than in the case of a very generous default to the receiver) or absent.

Our findings are derived from a large laboratory experiment involving more than 800 participants and a neutral frame. All participants take part in only one choice situation, and we compare average behavior across treatments. All our settings are variants of a dictator game, as this is a pure choice situation rather than a strategic game in the classical sense. Our basic treatment involves a 100% chance that the decider's choice is implemented, which corresponds to a standard dictator game, and our main comparisons involve settings in which the chance that the decider's choice is implemented is reduced to 50 percent. The 50 percent probability was chosen as the benchmark for comparison because it remains cognitively easy to comprehend through the common metaphor of a coin-flip. It is also a setting where previous literature did not observe differences between gambles and certain choices in classical (non-social) choice settings, which renders it a useful starting point to study the specific role of uncertainty for social preferences.[3] When the coin-flip is present, we implemented different treatments with either a selfish default that leaves all money to the decider, an equal-split default, or a default that transfers all money to the receiver.

We find that on aggregate standard dictators indeed behave differently from those who know that their choice might not be implemented: 45% of deciders transfer money in the standard dictator game setting, but only 30-39% (depending on the precise setting) do so when their choice is followed by a coin-flip that determines whether their choice or the default is implemented. In addition, the average transfer in the latter case is about 21-41% lower (depending on the precise setting). Predictions derived from Saito's (2013) theory indicate that only deciders who are pro-social (i.e., share a strictly positive amount) in a non-stochastic environment are affected by the default. This seems indeed to be the case. If the default is very generous to

[3]A simple axiom (called stochastic dominance) that underlies most theories of uncertainty is that an agent who prefers one outcome over another in the absence of uncertainty should also have clear preferences between two lotteries that are identical except for the outcome in one single state: the agent should pick the lottery with the (deterministically) preferred outcome. Violations of dominance are uncommon in individual consumption choices that do not involve a social component. When they do occur in non-social settings then, unlike our findings, they are linked to either compounded lotteries or to situations where the relevant probabilities are low. Most related to our setup are the studies by Birnbaum and Thompson (1996), Birnbaum et al. (1992), and Mellers, Weiss, and Birnbaum (1992), which also find violations of dominance, but only for a very low probability of payment changes. These studies do not find violations for 50-50 coin flips (or even close, such as a 60-40).

the receivers and leaves all the money with them, average transfers by non-selfish deciders are by about 40% lower than in the standard dictator game. This finding is well in line with notions of ex ante fairness as modelled, for instance, by Saito (2013). The reactions of non-selfish subjects to selfish defaults, however, are not: If the default is very selfish (giving all the money to the deciders), average transfers by non-selfish deciders are by more than 40% lower (and not higher) than in the standard dictator game. This is neither consistent with expected utility theory nor with notions of ex ante fairness. Behavior seems rather shaped by a self-serving bias in privileging norms: generous defaults are taken as an excuse for giving less as the default is already generous; and selfish defaults are interpreted as a reference point and therewith as an excuse for being more selfish.

The self-serving norms story is obviously only one possible explanation for behavior when people are confronted with different defaults. Other motivations for social behavior have been proposed, and we design several treatments to address some of them. For instance, to see whether audience effects à la Andreoni and Bernheim (2009) shape behavior in our experiments, we have treatments where the choices of the decider are observable and other treatments in which the choices (and their payoff consequences) remain unobservable. The contributions of deciders in the standard dictator environment and in the various default settings do not differ much between revealed and concealed choices. We also have treatments where the default is implemented with a very small probability to address the question whether anchoring is responsible for some of our results. We do not observe a difference between the choices in those treatments and the choices in the standard dictator setting, independent of the default.

The rest of the paper is organized as follows: The next section describes our experimental setup. Section 3 introduces a simple theoretical framework and derives our hypotheses. Section 4 presents our main empirical findings. Section 5 expands on the related literature and Section 6 discusses applications of our results and concludes the paper.

2 Experimental Design

The computerized experiment was run with 828 participants ($N = 828$), mainly undergraduate students. The experiment was programmed and conducted with the software z-tree (Fischbacher 2007) and participants were recruited via ORSEE (Greiner 2004). The participants were randomly split into two equally sized groups, group 1 and group 2. The participants of group 1 were the "deciders" and their decisions were potentially payoff-relevant. Participants of group 2, the "receivers", did not take any payoff-relevant decision. All deciders completed two independent stages.

Our main focus is on the first stage: a one-shot dictator game. Across treatments, we vary the dictator game. In all treatments the decider faces exactly the same choice set. He has to decide how many of 10 points (8 Euros) in increments of 1 to transfer to a randomly determined anonymous receiver keeping the rest for himself. In the *Dictator* treatments (two treatments with $N = 208$ participants in total) the decider's choice is always pivotal and surely determines the earnings. In the *Default* treatments (five treatments with $N = 472$ participants – see Table 1 for details) the decision of the decider is pivotal with a probability of 50% only. Otherwise, an exogenously given default split of the 10 points is implemented. We use the equal probability lottery as a benchmark as it is particularly simple to explain to participants and avoids confusion. Within the *Default* treatments, we vary the generosity of the default to see whether it matters what happens in the case that the decider is not pivotal: all points go to the decider (*Default-0*, $N = 194$), all points go to the receiver (*Default-10*, $N = 192$), equal split of the 10 points (*Default-5*, $N = 86$).

Some of the literature on social preferences suggests that deciders may care about how they are perceived by the receiver, or more generally, by some 'audience' (Andreoni and Bernheim 2009, Dana, Cain, and Dawes 2006). To control for such 'audience effects', we conduct the described treatments (except for *Default-5*) in two variants: In condition R ($N = 212$, treatments: *Dictator*-R, *Default*-0-R and *Default*-10-R), the receiver knows the decider's decision problem and learns his decision, i.e. choices are revealed. In condition C ($N = 468$, treatments: *Dictator*-C, *Default*-0-C, *Default*-5-C and *Default*-10-C), the receiver does not learn the choice of the decider and cannot infer it from the payoff (payoffs from both parts of the experiment are combined such that the final payoff is no direct function of the decider's action), i.e. choices are concealed. Audience effects are, however, not our focus and the variation rather serves as a robustness check. Therefore, we do not implement all treatments under both conditions.

The two *Dictator* and the five *Default* treatments constitute our main treatments. Besides those, we conduct two additional treatments (*Low-10-R* with $N = 92$ and

Low-0-R with $N = 56$), in which the default is only implemented with the very low probability of 2%, to analyze whether anchoring drives behavior. Table 1 summarizes our experimental treatments.

Table 1: Experimental Treatments

Treatments	dictator game with default?	probability of default	points of receiver in the default	condition C/R*	# subjects (N)
Dictator-C	No	-	-	C	124
Default-0-*C*	Yes	50%	0	C	128
Default-5-*C*	Yes	50%	5	C	86
Default-10-*C*	Yes	50%	10	C	130
Dictator-R	No	-	-	R	84
Default-0-*R*	Yes	50%	0	R	66
Default-10-*R*	Yes	50%	10	R	62
Low-0-*R*	Yes	2%	0	R	56
Low-10-*R*	Yes	2%	10	R	92

* C stands for 'concealed choice' and R for 'revealed choice'
The number of subjects varies across treatments due to no-shows. In the concealed action treatments (besides *Default*-5-*C*) we have more observations as we varied the order in which the (modified) dictator game and the EET were presented.

In the second stage of the experiment we elicited the distributional preferences of deciders using the Equality Equivalence Test (EET) introduced by Kerschbamer (2015). This procedure exposes subjects to a series of incentivized binary choices between allocations that involve an own payoff for the decider and a payoff for a randomly matched anonymous passive participant. For each binary choice deciders were re-matched with another receiver, which is never the receiver from stage 1.[4] The EET systematically varies the price of giving (or taking). We use it to identify deciders who are mainly interested in the own material payoff as we do not expect any treatment variation from them.

[4]The experimental instructions (translated from German) are provided in Appendix I. For the concealed action condition of the *Dictator*- and *Default*-treatments, we also varied the order such that stage 1 was the EET and stage 2 the *Dictator*- or *Default*-treatment, respectively. The data shows no order effects.

3 A Simple Theoretical Framework and Hypotheses

Although our experiment was not designed to test a specific model of pro-social behavior, we find it useful to use a theoretical framework – the unifying model of ex-ante and ex-post fairness by Saito (2013) – as a lens to organize our thoughts on the outcome of dictator experiments in which an agent (the decider) has the power to decide on how to split ten units by transferring a share s to the other player (the receiver) and keeping the remainder $10 - s$ for himself, thereby generating the payoff vector $(10 - s, s)$. The twist is that the decider knows that his decision is implemented only with probability p, while with the complementary probability the exogenous default split $(10 - d, d)$ is implemented. Obviously we require both s and d to be feasible in the sense that both players receive weakly positive monetary payments.

For $p = 100\%$ we are in a standard dictator game setting where only the decision of the decider matters, and we denote the implemented transfer for this case by $s_{100\%}$. We are interested in how choices in such a standard dictator game differ from choices $s_{p,d}$ in a setting where with probability $1 - p$ the default split $(10 - d, d)$ is implemented, and how this depends on the level of the default and the probability of its implementation.

The transfer $s_{p,d}$ is the result of utility maximization. In the Saito model, a decider who is uncertain about which payoff x he will receive, which could be $10 - s$ or $10 - d$, chooses s to maximize

$$\begin{aligned} & \rho E(U(x, 10 - x)) + (1 - \rho)U(E(x), E(10 - x)) \\ = \; & \rho \left[pU(10 - s, s) + (1 - p)U(10 - d, d)\right] \\ + \; & (1 - \rho)U\left(10 - ps - (1 - p)d, ps + (1 - p)d\right), \end{aligned} \tag{1}$$

where E is the expectation operator and $U(.)$ is a utility function over the decider's and the recipient's consumption. Here, parameter $\rho \in [0, 1]$ gives the weight the decider puts on ex-post utility. For $\rho = 1$ the framework collapses to the well-known expected utility framework where the decider considers the fairness of each realized outcome and then averages across them. For $\rho < 1$, some weight is shifted to ex-ante utility, where the decider first computes the average payment to each individual and then assesses whether this expected division of the surplus is fair. Note that in the absence of uncertainty the expectations operator is superfluous, the payoff vector is simply the proposed split $(10-s, s)$, and (1) collapses to $U(10-s, s)$. The parameter ρ therefore only becomes relevant in the genuine presence of uncertainty. To complete the setup, we follow Saito by assuming that the utility function $U(.)$ has the Fehr and Schmidt (1999) form. Specifically, we assume that the function $U(.)$ has the form

$$U(x_1, x_2) = x_1 - \alpha \max\{x_2 - \gamma x_1, 0\} - \beta \max\{\gamma x_1 - x_2, 0\}. \tag{2}$$

In the original Fehr-Schmidt (1999) framework we have $\gamma = 1$, $\beta \in [0, 1)$ and $\alpha \geq \beta$. Here, β penalizes the decider for having a higher payoff than the receiver, and $\alpha \geq \beta$ penalizes him even more for having less than the receiver. Obviously, the theory can rationalize selfish behavior when β is sufficiently small. Strictly positive giving can also be rationalized, provided β is sufficiently large. However, the original theory with parameter restriction $\gamma = 1$ could rationalize only the equal split – except for the degenerate case where $\beta = 1/2$ implying that the decider is completely indifferent between all transfers in [0, 5]. We allow for $\gamma \in (0, 1]$ as a cheap way to rationalize any transfer in [0, 5] of a standard dictator. With $\gamma \in (0, 1]$ a transfer in (0, 5] can be rationalized when $\beta \geq 1/(1 + \gamma)$.[5]

We collect in the following a few observations that can be derived from this specification, where the proofs are relegated to Appendix II. In the derivation we requite $\beta \neq (1 + \gamma)^{-1}$ to avoid the necessity to discuss multiple optimal choices. First, individuals that are not ex-ante utility motivated but have standard expected utility preferences do not react to the probability or the level of the default. This result is independent of the exact form of the utility function, and follows directly from the independence axiom that underlies expected utility theory. It yields immediately:

Proposition 1a: *An individual with standard expected utility preferences does not react to the probability or the level of the default, for all feasible p and d. That is, for* ρ=1 *it holds that* $s_{p,d} = s_{100\%}$ *for all* $(p, d) \in$ [0,1]×[0,10].

Moreover, a person who keeps all money as a standard dictator will also keep all money in settings where his choice is not implemented for sure. The reason is that when his choice is not implemented the default is actually weakly more generous to

[5]With $\beta \geq (1 + \gamma)^{-1}$ the transfer s of a standard dictator can be rationalized with a γ such that $\gamma = s/(10 - s)$.

the receiver than he would like. Therefore:

Proposition 1b: *An individual who does not give as a standard dictator will not give when his choice is implemented only with some probability p, for all feasible p and d. That is, for $s_{100\%} = 0$ it holds that $s_{p,d} = s_{100\%} = 0$ for all $(p, d) \in$* [0,1]×[0,10].

The previous propositions together highlight that the default can only matter for deciders who are ex-ante motivated (i.e., $\rho < 1$) and who are generous as standard dictators (i.e., $s_{100\%} > 0$). Let's focus on such a decider. Since the transfer is somewhat generous ($s_{100\%} > 0$) and maximizes (2), such an individual must be sufficiently fairness motivated, i.e., it must have $\beta > 0$ sufficiently large. More specifically, this requires $\beta \geq 1/(1+\gamma)$. Obviously, if the default implements exactly the desired split ($d = s_{100\%}$), the optimal decision stays the same irrespective of the probability of implementation. Otherwise, a sufficiently ex-ante motivated decider (ρ sufficiently below 1) counter-acts the generosity of the default, as summarized in the following.

Proposition 2: *Consider an individual that is not fully selfish as standard dictator ($s_{100\%} > 0$), and suppose this individual faces a probability $p > 0$ of a default d that is more generous to the receiver ($d > s_{100\%}$). This individual will now decide for a lower transfer: $s_{p,d} \leq s_{100\%}$. He transfers a strictly lower share if $\rho\beta - (1-\rho)\alpha > (1+\gamma)^{-1}$, which is assured if ex-ante fairness is at least as important as ex-post fairness ($\rho \leq 1/2$).*

Under the last condition, ex-ante motives are sufficiently important so that the decider wants to implement his desired transfer in ex-ante terms. He then counter-acts a too generous level of the default by decreasing his own transfer.

This bags the question whether we should also see that deciders get more generous when the default leaves very little to the receiver. Maybe surprisingly, the answer is no. This is due to the asymmetry between α and β in the utility function, which penalize when the decider is behind or ahead. If the default is too generous to the receiver, the disutility thereof is governed by α. If the default leaves too little to the receiver, the disutility of this is governed by β, which is lower.

We can show that a decider who faces a default that leaves him more money than he would choose as a dictator ($d' < s_{100\%}$) would become more generous in his choice only if $(1+\gamma)^{-1} \leq (1-\rho)\beta - \rho\alpha$, which is more demanding than the condition that generates more selfish behavior with a very high default (i.e., the condition in Proposition 2). Otherwise the decider does not adjust his behavior so that $s_{p,d} = s_{100\%}$. So it is less likely to observe individuals becoming more generous than to observe them becoming more selfish. This is summarized in the following:

Proposition 3: *The condition on α, β, γ and ρ that ensure that a decider becomes more generous when the default is more selfish than his dictator choice (i.e., for which $s_{p,d} > s_{100\%}$ when $d < s_{100\%}$ and $p > 0$) are strictly stricter than the conditions that imply that the decider becomes more selfish if the default is more generous than his dictator choice (i.e., for which $s_{p,d} < s_{100\%}$ when $d > s_{100\%}$ and $p > 0$).*

Finally, it is obvious from (1) that small probabilities of the default do not have a large bearing on decisions. Therefore, choices of deciders converge to those of the standard dictator as p approches 100%:

Proposition 4: *The choice of a decider does not substantially differ between a dictator setting and a setting with a small probability of the default, i.e., $s_{p,d} \to s_{100\%}$ for $p \to 100\%$.*

We now use these theoretical observations to state testable predictions for our experiment.[6] Since the theoretical framework implies that standard dictators transfer less than 5, we expect defaults that transfer 5 or 10 to the receiver to be more generous than individuals would give. In line with Proposition 2 we therefore predict:

Hypothesis 1: *Compared to the Dictator treatments, the average transfer of pro-social deciders is lower in the Default treatments with a generous default, i.e., in Default-5 and Default-10.*

For the selfish defaults, Proposition 3 highlights that fewer individuals will be responsive to the default:

Hypothesis 2: *Compared to the Dictator treatments, the average transfer of pro-social deciders is either the same or higher in the Default treatment with a selfish default, i.e., in Default-0.*

In general giving should decline as the default gets more generous to the receiver, which follows directly from the fact that the utility function (1) is submodular in s and d:

Hypothesis 3: *In the Default treatments with equal probabilities of the default the average transfer of pro-social deciders weakly decreases in the generosity of the default, i.e. giving weakly decreases from Default-0 to Default-5 to Default-10.*

Finally, according to Proposition 4 a very low probability of the default should

[6]We note that this simple theory of ex-ante and ex-post fairness does not distinguish between settings where the choices are revealed or concealed, and therefore reply to both settings.

have no substantial effect relative to a standard dictator setting:

Hypothesis 4: *Differences in average transfers of pro-social deciders between the Dictator and Default treatments vanish if the probability for the default is very low, i.e. giving in Low-10-R and Low-0-R does not differ significantly from giving in Dictator-R.*

4 Experimental Results

Table 2 summarizes for each treatment the average transfer, the share of positive transfers, and the average positive transfer (i.e., the average transfer given that the transfer is positive). As explained in the theory section (see Proposition 1b), only deciders who are generous as a standard dictator are predicted to change their behavior when their choice is pivotal with a probability lower than 100 % while a default split is implemented with the complementary probability. Our between-subjects design, however, does not allow us to identify those deciders in the *Default* treatments who would transfer nothing in a *Dictator* treatment.[7] To nevertheless have an indicator for whom we should and should not expect to behave as predicted, we use subjects' decisions in the EET (to which all deciders in the experiment are exposed) to classify them as either selfish or non-selfish.[8] We classify those subjects as selfish that in the 10 binary decisions in the EET never make a choice that does not maximize the own material payoff – with the rest classified as non-selfish.[9]

Being selfish according to this classification is highly correlated with giving nothing in the *Dictator* treatments. We observe that 89% of the deciders who transfer nothing are classified as selfish according to the EET, while only 36% of the deciders who transfer a positive amount are classified as selfish. The Spearman's correlation coefficient between giving and being selfish is -0.618 ($p = 0.000$).

Accounting for the classification according to the EET, the upper part of Table

[7]We have chosen a between-subjects design for our main research question to prevent issues of interdependence or consistency of decisions (see e.g., Falk and Zimmermann 2013, 2017).

[8]Obviously, issues of interdependence or consistency might still matter in our experiment. We believe, however, that the problem is less severe as the decision situations are less similar than would be a standard dictator decision and a decision in the modified dictator game with default. More importantly, the classification itself is not our main variable of interest.

[9]In total, 6.3% of deciders revealed inconsistencies in their choices in the EET. We classify those as non-selfish subjects in the following analyses. Dropping them instead does not qualitatively change our results.

Table 2: Summary statistics for non-selfish and selfish subjects

Non-selfish subjects	mean transfer (SD)	share of transfers > 0	mean transfer> 0	# of observations ($\frac{N}{2}$)
Pooled treatments				
Dictator[a]	3.29 (1.90)	82.9	3.97	35
Default-0[b]	1.68 (1.92)	53.7	3.14	41
Default-10[b]	1.94 (2.05)	53.1	3.65	32
Individual treatments				
Dictator-C	3.27 (1.83)	86.4	3.79	22
Default-0-C	1.74 (1.83)	59.3	2.94	27
Default-5-C	1.90 (2.05)	52.4	3.64	21
Default-10-C	1.72 (2.11)	44.4	3.88	18
Dictator-R[c]	3.31 (2.10)	76.9	4.30	13
Default-0-R	1.57 (2.14)	42.9	3.67	14
Default-10-R	2.20 (2.14)	54.6	3.67	11
Selfish subjects				
Pooled treatments				
Dictator[a]	0.68 (1.44)	25.0	2.71	68
Default-0[b]	0.95 (1.73)	28.6	3.31	56
Default-10[b]	0.44 (1.10)	18.8	2.33	64
Individual treatments				
Dictator-C	0.43 (1.08)	17.5	2.43	40
Default-0-C	0.78 (1.62)	24.3	3.22	37
Default-5-C	0.40 (1.15)	16.0	2.50	25
Default-10-C	0.36 (0.92)	18.2	2.00	44
Dictator-R[c]	1.04 (1.79)	35.7	2.90	28
Default-0-R	1.26 (1.94)	36.8	3.43	19
Default-10-R	0.60 (1.43)	20.0	3.00	20

[a]: revealed and concealed action treatments (*Dictator*-R and C)
[b]: revealed and concealed action treatments (*Default*-0-R and 0-C, resp. Default-10-R and 10-C)
[c]:Note that we have one missing observation for the EET in *Dictator*-R.

2 displays the results for the non-selfish subjects while the lower part focuses on the selfish ones.[10] Before displaying the results for the individual treatments, the first three rows of the table report pooled data for our main treatments (i.e., average data of the corresponding revealed and concealed action treatments between which we do not find significant differences as discussed later).[11]

[10]The aggregate for selfish and non-selfish subjects is shown in Appendix III.
[11]For sake of exposition, Table 2 does not list the statistics for the control treatments with a low

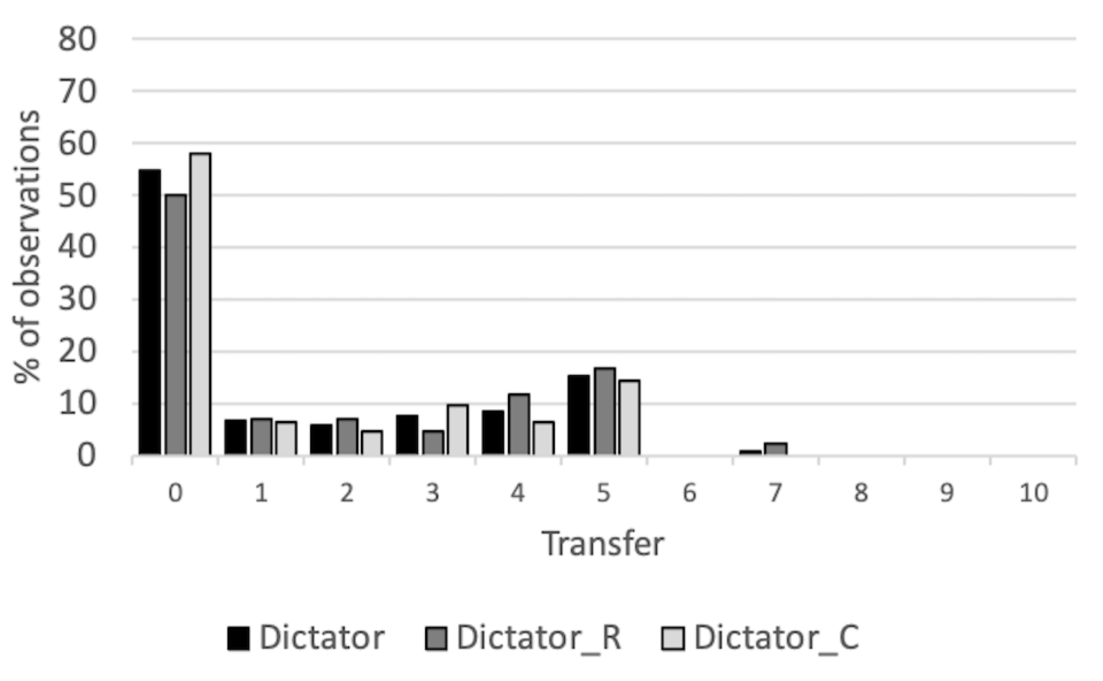

Figure 1: Distribution of transfers in *Dictator* treatments

Before analyzing the effects of the defaults on transfers, let us evaluate how generous the different defaults are to the receiver by considering transfers in the *Dictator* treatments. The distribution of transfers in the *Dictator* treatments is shown in Figure 1 for the pooled data as well as disaggregated by the visibility condition. The average transfer is 1.6 points, the minimum transfer is 0 and the maximum transfer is 7 in *Dictator-R* and 5 in *Dictator-C*. Also, about 55% of transfers are zero. Thus, transferring 10 points to the receiver is more generous than what any dictator would transfer in our sample. Similarly, a transfer of 5 is (weakly) more generous than what (at least) 99.04% percent of dictators would give. A default that transfers no points, however, is less generous – about 45% of deciders transfer more in the *Dictator* treatments.

We now turn to comparing transfers in the *Dictator* and more or less generous *Default* treatments. Figure 2 illustrates the average transfers in the *Dictator* and *Default* treatments (for both visibility conditions) separately for selfish and non-selfish subjects.[12] In the generous default treatments, *Default-5* and *Default-10*, the

probability of the default. In Appendix III Table 6, we show the main summary statistics for those treatments.

[12]See Appendix IV, for illustrations of the distribution of transfers of selfish and non-selfish sub-

average transfer of selfish subjects is rather similar to the *Dictator* treatment in the concealed action treatments, ranging from 0.36 to 0.43 (*Dictator-C* vs. *Default-10-C* / *Default-5-C*: MWU tests of transfers, p = 0.989 /0.877). In the revealed action treatments, transfers are less generous in *Default-10-R* (0.6) than in *Dictator-R* (1.04), yet insignificantly so (MWU tests of transfers, p = 0.274, two-sided). The average transfer of non-selfish subjects, however, drops significantly by more than 40% from the *Dictator* treatments to the treatments with generous defaults for each visibility condition (*Dictator-C* vs. *Default-5-C* / *Default-10-C*: p =0.01/0.015; *Dictator-R* vs. *Default-10-R*: p = 0.004, MWU tests of transfers one-sided). The share of positive transfers (cf. Table 2) shows the same picture for both – it drops significantly by at least 22% for non-selfish deciders.[13]

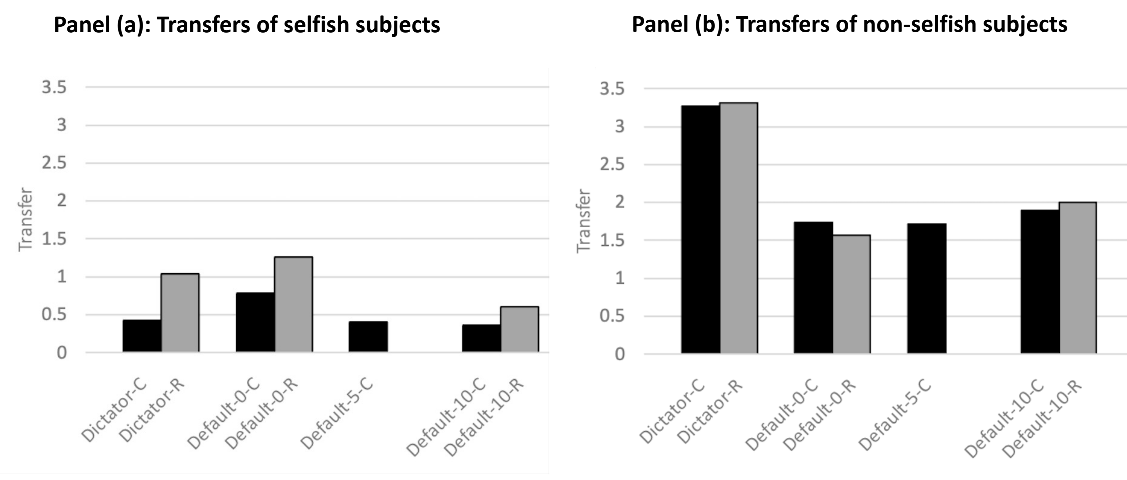

Figure 2: Average transfers across treatments

These observations support Hypothesis 1 for non-selfish subjects and it does not seem to be the case that they are driven by audience effects. We thus state as a first result:

Result 1: *In line with Hypothesis 1, compared to the Dictator treatments, transfers of non-selfish subjects are lower in the Default treatments with a generous default,*

jects separated by visibility condition.

[13]When comparing giving behavior, we only report the results from the MWU tests comparing the average transfers since the results from the Chi^2 tests comparing the shares of positive transfers do not qualitatively differ.

i.e., in Default-5 and Default-10. We do not observe a significant difference in behavior of selfish subjects between the Dictator treatments and the treatments with a generous default.

In the selfish default treatments, *Default-0*, the average transfers of selfish subjects, for whom we do not expect a change in behavior, tend to be higher than in the corresponding *Dictator* treatments, though insignificantly so. In the concealed action treatments average transfers are 0.78 versus 0.43 in *Dictator-C* (*Default-0-C* vs. *Dictator-C*: MWU tests of transfers, $p = 0.772$, two-sided) and in the revealed action treatments average average transfers are 1.26 versus 1.04 in *Dictator-R* (*Default-0-R* vs. *Dictator-R*: MWU tests of transfers, $p = 0.222$, two-sided).

In stark contrast, for non-selfish subjects, the average transfers drop by more than 46% in the less generous default treatments, *Default-0*, compared to the *Dictator* treatments for both, revealed and concealed action: from 3.27 in *Dictator-C* to 1.74 and from 3.31 in *Dictator-R* to 1.57 (*Dictator* vs. *Default-0*, MWU test of transfers, one sided: $p = 0.004$ and $p = 0.029$ for concealed and revealed action, respectively). Similarly, the share of positive transfers drops by at least 26%.

Thus, contrary to the prediction in Hypothesis 2, non-selfish deciders do not become more generous if the default is rather selfish but rather less generous.[14] As before, audience effects do not appear to be a driving force behind the observations as the pattern we observe is the same in the revealed and in the concealed action condition. Thus, we state as a second result:

Result 2: *In sharp contrast to Hypothesis 2, compared to the Dictator treatments, the average transfer of non-selfish subjects is lower in the Default treatment with a selfish default, i.e., in Default-0. This is true for the revealed and for the concealed action condition. As expected we do not observe a significant difference in behavior of selfish subjects between the Dictator and the selfish default condition*

Comparing transfers across defaults, Figure 2 suggests that there is no difference in transfers between the two generous defaults (*Default*-10 with *Default*-5). This is confirmed by statistical test for both, the selfish and the non-selfish subjects (MWU

[14]Further support for this result comes from another *Default-0* treatment that we conducted in which the default is implemented with a high probability of 80% ($N = 66$). We conducted this treatment with concealed action only, instructions and payoffs are otherwise identical to the *Default* treatments. We do not report this treatment in detail here as it differs in the probability of the default. Nevertheless, we can take the observations as a robustness check for the previous finding. We observe that the average transfer is 2.23 for non-selfish subjects (0.55 for selfish subjects) while in *Dictator-C* it is 3.27 for non-selfish subjects (0.43 for selfish subjects).

test of transfers for selfish/non-selfish subjects $p = 0.850/0.761$, two-sided). The figure also suggests that for non-selfish subjects there is no difference in transfers when the default gives nothing to the receiver (*Default*-0) compared to the treatments where the default gives all or half of the points to the receiver (*Default*-10 with *Default*-5). Again this is confirmed by statistical tests (MWU tests of transfers: all $p > 0.318$ for revealed and for concealed actions, one-sided). For selfish subjects the figure suggests the presence of a difference – it turns out to be insignificant, however (MWU test of transfers *Default*-0-C vs. *Default*-10-C/*Default*-5-C: $p = 0.379/0.386$ and *Default*-0-R vs. *Default*-10-R $p = 0.222$).[15] Again, the observations do not seem to be affected by the visibility condition. We therefore conclude:

Result 3: *In the Default treatments Default-10, Default-5, and Default-0 average transfers of selfish and non-selfish subjects do not change significantly in the generosity of the default.*

One could argue that the mere presence of a default could affect transfers in the *Default* treatments. A simple form of anchoring could imply that deciders choose more often a transfer equal to the respective default (e.g., due to a feeling of the default being the 'desired' or 'appropriate' transfer). From the results presented up to now (especially, from Result 1) it seems apparent that such simple anchoring does not drive behavior. Could it be that the mere knowledge of the default anchors deciders' choices in a more complex way that can explain why they give less if the default is generous to the receiver but not more if the default is rather selfish (compared to a situation where their choice determines the final outcome for sure)? If the default indeed anchors deciders' choices, it should do so even if the probability of its implementation is very low. This means that for a low probability of the default, transfers should show deviations in the same direction from transfers in the *Dictator* treatment than if the default is implemented with 50%. If, however, anchoring does not play a major role, differences between the *Default* treatments and *Dictator* treatment should vanish if the default is only implemented with a very low probability. To analyze the issue of anchoring in more detail, we consider the treatments *Low*-0-R and *Low*-10-R, in which the default is implemented with a *low* probability of only 2%. We conducted those treatments with revealed actions only. Note that anchoring might not necessarily affect selfish and non-selfish subjects differently, therefore, we also consider overall

[15] Even pooling the visibility conditions, transfers between *Default*-0 and *Default*-10 do not differ significantly.

results here. Average transfers in the *Low*-10-*R* and *Low*-0-*R* treatment are 2.13 and 1.82, respectively, and do not significantly differ from transfers in the *Dictator-R* treatment, which are 1.82 on average (MWU test *Dictator-R* vs. *Low*-0-*R*/*Low*-10-*R*: $p = 0.403/0.903$, two-sided). Separating, selfish and non-selfish types (cf. Table 6 in Appendix III), we do not find any indication that non-selfish types are affected by the default: average offers in *Low*-10-*R* (2.55) and in *Low*-0-*R* (3.11) do not differ significantly from giving in *Dictator-R* treatment (3.31), $p = 0.311$ and $p = 0.832$ respectively (MWU tests, two sided). For selfish types we also find no significant differences. Average offers in *Low*-10-*R* (1.81) and in *Low*-0-*R* (1.06) do not differ from offers of selfish subjects in the *Dictator-R* treatment (1.04), $p = 0.0825$ and $p = 0.899$ respectively (MWU tests, two sided). These observations give support to Hypothesis 4. We thus state:

Result 4: *In line with Hypothesis 4, differences between the Dictator and the Default treatments vanish if the probability for the default is very low, i.e. giving in Low-10-R and Low-0-R does not differ significantly from giving in Dictator-R.*

In the following we substantiate our findings by regression analyses (see Table 3). We run Tobit regressions, in which the dependent variable is the size of the transfer (left-censored at transfer=0 and right-censored at transfer=10, see columns 2 and 4 in Table 3) and logistic regressions, in which the dependent variable "giver" indicates whether or not a transfer is positive (see columns 3 and 5 in Table 3). The independent variables are treatment dummies for the *Default*-10, *Default*-0, and *Default*-5 treatments, and a dummy for the revealed action condition. The reference treatment is thus the *Dictator-C* treatment taken up by the constant. To account for the different expected behavior of selfish and non-selfish subjects, we run separate regressions for both.

The regression results support our previous observations. They show that the size of the transfer as well as the likelihood to make a positive transfer is significantly reduced for non-selfish – but not for selfish – deciders if there is a 50 percent chance that the decider is not pivotal. This holds true in those settings where the default is generous to the receiver (*Default*-10 and *Default*-5) but also in those settings where the default gives all the money to the decider. Comparing transfers across defaults reveales no significant differences, neither for selfish nor for non-selfish deciders (for non-selfish subjects all $p > 0.578$, for selfish subjects: -0.76 vs. -0.60, p=0.910; -0.60

vs. 0.83, p=0.325; -0.76 vs. 0.83, p=0.121).[16] Moreover, the estimations show that revealing the decider's action does not have a significant impact overall for non-selfish deciders. For selfish deciders there seems to be a positive effect on transfers, though.

Table 3: Regression results on the size of the transfer and on the probability of making a positive transfer for non-selfish and selfish subjects

	Non-selfish		Selfish	
Dependent variable:	**Transfer** (Tobit)	**Giver** (Logistic)	**Transfer** (Tobit)	**Giver** (Logistic)
Default-0	- 2.34***	-1.45***	+0.83	+0.23
	(0.722)	(0.550)	(0.968)	(0.412)
Default-5	- 2.60***	-1.96***	-0.60	-0.29
	(0.965)	(0.680)	(1.444)	(0.637)
Default-10	- 2.05***	-1.47***	-0.76	-0.31
	(0.768)	(0.575)	(0.997)	(0.430)
Revealed action	-0.22	-0.40	+1.58*	+0.60
	(0.638)	(0.435)	(0.834)	(0.351)
Constant	+3.10***	+1.74***	-3.50***	-1.37***
	(0.563)	(0.488)	(0.939)	(0.330)
Number of observations	127	127	213	213
Number of left-/right-censored observations	50/0		164/0	
(Pseudo) R-squared	0.0299	0.0765	0.0173	0.0237

Numbers in parentheses indicate standard errors. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.
We have one missing observations in *Dictator-R* treatment for the EET.

By conducting our treatments (besides the *Default-5* treatment) with two visibility conditions, i.e., transfers are either revealed to or concealed from the receiver, we can control for whether audience effect as theoretically and experimentally analyzed in Andreoni and Bernheim (2009) drive our results. In sum, we do not find any indication that audience effects are a driving force behind the reaction of non-selfish subjects to the different defaults – all previously discussed results are true for revealed as well as for concealed actions and moreover, there does not seem to be a level effect in transfers for non-selfish subjects. For selfish subjects, we also do not find systematic

[16]For selfish subjects, for whom we do not expect a reaction across defaults, the latter p-value when comparing transfers between *Default*-10 and *Default*-0, is rather low. It could be that some kind of anchoring plays a role – as we discuss later in more detail – such that they are inclined to be more generous if the default is very generous.

differences in the reactions to the defaults, yet, there seems to be some level effect in transfers (cf. Figure 2 and Table 3). Possibly, for selfish subjects image concerns have some effect on transfers but the effect is weak and only marginally significant.

We also substantiate our result on anchoring by regression analyses. As aforementioned, anchoring might apply to selfish and non-selfish types similarly. Here, we therefore present regression results, where we do not separate between selfish and non-selfish types.[17] Table 4 presents the results of Tobit (columns 2 and 3) as well as logistic regressions (columns 4 and 5), in which the dependent variable is again either the size of the transfer (Tobit, left-censored at transfer=0 and right-censored at transfer=10) or whether or not the decider is a "giver" (Logistic). The independent variables are dummies for the *Low*-10-*R* and *Low*-0-*R* treatment; we omit the *Dictator-R* treatment. In one specification of the Tobit and one of the logistic regression, we include a dummy indicating whether the decider is classified as selfish to control for the expected level differences in giving by selfish and non-selfish subjects. The estimations do not indicate any significant effect of the default that is implemented with the low probability of only 2% on transfers compared to the *Dictator-R* treatment. This again does not give any support to the idea that anchoring drives our findings.

5 Discussion

We have seen that for generous defaults concerns for ex-ante fairness account well for the observed patterns in our data. This is not the case for the selfish default to which non-selfish deciders react with a decrease in transfer – compared to a situation where their choice is implemented for sure. Why do non-selfish deciders not become more (but rather less) generous in case of a selfish default? One possible explanation is that the default of 0 lowers deciders' reference point for what is considered an appropriate amount to give and thereby induces them to give less. Taken together this amounts to a story about the self-serving interpretation of which fairness norm to apply: If the default is already generous, subjects lower their transfers as the expected transfer is rather high in any case; that is, in this case they apply the notion of ex ante fairness. And if the default is selfish, they lower transfers because they interpret the selfish

[17]In Appendix V, we show the regression results for selfish and non-selfish types separately with similar results.

Table 4: Regression results on the low probability of default treatments

Dependent variable:	**Transfer** (Tobit)	**Transfer** (Tobit)	**Giver** (Logistic)	**Giver** (Logistic)
Low-10-*R*	+ 0.69	+ 0.55	+0.53	+0.48
	(0.761)	(0.720)	(0.434)	(0.455)
Low-0-*R*	+ 0.09	+0.03	+0.14	+0.09
	(0.878)	(0.859)	(0.489)	(0.530)
Selfish		-2.45***		-1.23***
		(0.650)		(0.428)
Constant	+0.62	+2.18***	+0.00	+0.80**
	(0.579)	(0.674)	(0.309)	(0.444)
Number of observations	116	112	116	112
Number of left-/right-censored observations	51/0	50/0		
(Pseudo) R-squared	0.0023	0.0375	0.0102	0.0701

Numbers in parentheses indicate standard errors. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.
The number of observations is lower when we control for selfish deciders as we have four missing observations for the EET.

default as a reference point for what is a fair allocation; that is, here they interpret the default as an indication of what they deserve to receive.

Besides this self-serving interpretation of defaults story, are there different explanations for our findings? Here we discuss some alternative explanations and thereby also briefly expand on the related literature.

Foremost, our experimental setup is closely related to the work by Andreoni and Bernheim (2009) who propose a signalling theory where people share their income to avoid being viewed negatively by the recipient (see also Charness and Dufwenberg 2006, and Dana, Cain, and Dawes 2006 for similar ideas). In contrast to our experimental setup, Andreoni and Bernheim (2009) analyze settings in which the coin is flipped first and afterwards – if the default is not implemented – the decider chooses how much to give. Importantly, the decider observes the outcome of the coin flip (implying that here is no risk involved in his decision), while the receiver does not – the latter observes only her payoff. If the default is a random draw between $(10, 0)$ and $(0, 10)$, people give significantly less than in a standard dictator game and often choose to give zero, which is an outcome where the recipient cannot tell if it was the default or the decider and therefore cannot attribute blame. If the default is randomly drawn from $(1,9)$ and $(9,1)$, deciders again give less than in a dictator setting but mostly choose to give 1, where again the recipient cannot tell the origin. As we have seen, in our experiments, contributions of deciders in the standard dictator environment and in the various default settings do not differ much between revealed and concealed choices. This suggests that signalling is not the main driver behind our core findings – in particular for the observation that the donations by non-selfish subjects go down when the default is generous and also go down when it is selfish independent of observability. Obviously this does not rule out other psychological motivations such as self-signalling – which is notoriously difficult to distinguish from deep underlying preferences – or signalling to the experimenter, for which we have not controlled.

Alternatively, it is well-known that the presentation of a choice problem can alter the resulting choice, where anchoring is one leading concern. Our standard dictator treatments do not mention a default anywhere, and one could speculate that the mentioning of the default alone might alter choices. To rule this out we have examined treatments where the default is present but is only implemented with a very small probability (2%). We did not observe a difference between choices here and in the standard dictator setting, independent of the default. This also rules out explanations similar to Linde and Sonnemans (2012) where risk aversion depends on the social reference point that might be manipulated through a default, but (for the generous default) stays in line with ex-ante fairness concerns which vanish at very low probabilities.

Theories of low-cost expressive voting (see Brennan and Lomansky 1993, Feddersen, Gailmard, and Sandroni 2009, Tyran 2004) predict more benevolent choices when the probability of being decisive is lower – which is contrary to our findings. It is more difficult to compare our results with predictions of theories of warm glow along the lines of Andreoni (1990), since those were traditionally formalized for settings without uncertainty. Under the strong assumption that individuals obtain warm glow from the specific proposal that they make rather than from its actual implementation, one obtains counter-factual predictions similar to those under expressive voting: as the probability of the default goes up the material consequences of a generous proposal decrease since it is less often implemented, and proposals should become more generous to reap the benefits of warm glow. This is not what we observe in our experiments. Less stark assumptions might render our findings within the realm of warm-glow theory, though.

The "crowding-out" effect that we find here for the generous default is also fundamentally different from a standard crowding-out effect where people give less if someone else gives more, as for example documented in Payne (1998). In particular, such phenomena can be captured with either expected utility theory or with a relatively simple extension of it.

Overall, despite a very different setup from previous work, the comparative statics of defaults on giving behavior seem to be partly in line with theories of ex-ante fairness (for generous defaults) and partly with a self-serving interpretation of defaults (for selfish defaults). As is evident from the above discussion, we are not claiming that other models cannot rationalize it, but some of the obvious alternatives that we have explored seem less suitable to explain the findings. Most obviously, our findings contradict standard expected utility theory. Our results depend on excluding individuals that reveal themselves as always selfish in standard (non-risk) choice situations in the line of Andreoni and Miller (2002), as these individuals show no variation in their social behavior relative to any treatment.

6 Conclusion

In this paper we have compared choices about the division of a pie in settings where individuals make the final decision with settings in which individuals face a probability that their choice gets overruled in favor of a default. We found that individuals who behave non-selfishly in standard dictator settings cut back in terms of generosity of their own decision when the default is more generous to the receiver and also when the default is selfish and leaves all money to the decider. We did not observe a significant change in decisions for subjects who show no generosity in a standard dictator setting. Effects are only present if the probability of the default is high (50%) while they are absent when the probability is negligible (2%). Our findings are inconsistent with classical expected utility preferences, as they violate first order stochastic dominance. The decrease in generosity for non-selfish deciders can be rationalized on the one hand by ex-ante fairness preferences (in case of a generous default) and on the other hand by a self-serving interpretation of the default (in case of a selfish default).

Apart from the general relevance for our deeper understanding of social preferences in settings with uncertainty, our findings make it tempting to speculate about several applied areas where they might become important if they turn out to be a robust phenomenon. In particular, in the example discussed in the introduction it becomes difficult to interpret the bonus awarded by a team leader to his subordinate as just compensation for previous actions or as a sign of his generosity if it is in part motivated by the forward-looking chance that the compensation might be overruled by the central management. Similarly, a labor union might demand a bigger share of surplus if there is a chance their their agreement will later be rescinded in courts or legislative directives.

Our results are also relevant for the literature on governing under the threat of revolution, which has received a lot of attention since recent contributions by Acemoglu and Robinson (2000, 2006). Envision a ruling class that contemplates social transfers to the remaining population, but faces the threat of being overthrown and expropriated. Classic motivations for sharing some of their wealth include standard social preferences as well as a desire to reduce the pressure to be overthrown. Motivations such as those highlighted in this paper would counteract these tendencies, though. The threat of subsequent expropriation resembles a default outcome that shifts resources to others, and if it triggers similar negative consequences as found in our simple experimental setup it would inhibit social transfers and might in fact make conflict more likely.

We do not find a moderating element, though. Proposals do not seem to become more generous if there is a chance that in the future the whole surplus is transferred to the decider. This opens up the possibility that any form of uncertainty lowers the willingness to be generous to the other side. For political negotiations where two opposing parties A and B formulate optimal policies for some initial negotiation, the threat that negotiations break down and either side is equally likely to determine the final outcome might shift initially optimal points in more selfish directions, making it harder to find an agreement. And even if break-down of negotiations clearly favors party A, our result indicates that the bliss point for both parties will shift to more selfish outcomes initially: For party B this is because breakdown triggers a very unfavorable allocation, and for party A since breakdown confirms a very favorable allocation.

This discussion indicates that our experimental findings might be of substantial relevance in practical settings. Most current models in applied theory do not incorporate such insights as they rely on expected utility theory. The fact that individuals seem to systematically respond to default events beyond their control that affect their sense of fair allocations is currently not incorporated in applied models, even though the basic idea is quite plausible. Our results show that such behavior is not only plausible but also readily observed, and might therefore warrant further investigation in theoretical and empirical work.

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Appendix

Appendix I – Instructions

An experimental session consisted of two stages. In the experiment, participants first got some general instructions, then the instructions for stage 1 'experiment 1´) and after stage 1 is finished, they got the instructions for stage 2 ('experiment 2´). In the following we give the (translated) instructions for the revealed action condition (concerning stage 1, we give the instructions for the *Dictator*- as well as the *Default*-treatment). The instructions for the concealed action condition differ in the information that is provided at the end of stage 1 and 2. Under the concealed information condition, participants are not told (and also do not get this information in the experiment) that they will be informed about their payoffs in stage 1 and 2 separately and neither that they are informed about the decisions of the respective decision maker.

General instructions

Welcome and thank you for participating in this experiment! From now on please do not communicate with other participants. Today you are taking part in two independent decision experiments. In these experiments you can earn a considerable amount of money. The following instructions explain the structure of the two experiments and how you can earn money. First you will be given the instructions for experiment 1. After experiment 1 is finished, instructions for experiment 2 will be handed out. The payment of both experiments will be made at the end of the session.

Anonymity:
Your decisions in both experiments remain anonymous. Neither the experimenters nor other participants will get to know which decisions you took. Your decisions are saved as data that cannot be linked to your person. Your name will appear only on the receipt for your total earnings in the experiment. Your total earnings will be composed of both experiments and will possibly be determined by other participants' decisions and random draws respectively such that inference from your total earning on the decisions you have taken is not

possible. In order not to jeopardize anonymity in the experiment please do not communicate any of your decisions.

Two groups:
The participants will be split in two groups. To do so, every participant will draw a numbered card. The cards have been shuffled and the draws will be made concealed out of an in-transparent bag. For organizational reasons there is no number one. Participants drawing a number from the lower half (e.g. in the case of 22 participants the numbers from 2 to 12) are assigned to **group A** while those with numbers from the upper half are assigned to **group B**. Members of group A are asked to leave for the room next door. The decisions of the members of group A will be relevant for the earnings of the members of group B. Members of group B cannot influence the earnings of group A members.

After you have drawn your card, please keep it concealed such that no one can see the number.

We will now proceed to the drawing of the cards. Participants with numbers of the lower half are asked to move to the room indicated by the experimenters.

Instructions Experiment 1 [Instructions for *Dictator-R*-treatment]

Please follow the instructions for Experiment 1 carefully. If there are any questions please raise your hand and an experimenter will answer your question.

Decision in experiment 1. This decision is payoff-relevant.
You are a member of group A (an A-person) and you are assigned exactly one member of group B (a B-person in the other room). In this experiment you are asked to take one decision, which is payoff-relevant for you as well as for the B-person you are paired with.

Random pairing
The pairing of A-person to B-person is randomly done by the computer and

remains anonymous. This means you will not get to know which B-person you have been assigned to for experiment 1. In what follows we will refer to the B-person assigned to you as your B-person. Your B person will be given these instructions and is therefore informed about this experiment.

Payoffs resp. conversion rate token/Euro

During the entire experiment we will refer to the payoffs in tokens. The rate of conversion into Euro for experiment 1 is

10 Tokens = 8 Euro
resp. 1 Token = 80 Cent

As indicated above, your decision in this experiment determines the payment to your B-person. This person receives a payment exclusively based on your decision.

Your decision in detail:

As A-person in experiment 1 you are asked to choose one of eleven alternatives. Each alternative has a consequence for you and your B-person. This decision problem is presented in a table. The rows in the table display the alternatives. Your task is to choose one of the rows.

On the screen the decision problem will look as follows (in this example there are only 4 rows while on the screen there will be eleven rows):

Please click which alternative you want to choose (check one row only)	you receive (in tokens)	your B-person receives (in tokens)
	a	b
	c	d
	e	f
	g	h

The letters a, b, c, d, etc. are just for illustration. In the experiment there will be numbers instead of letters.

If you choose the second alternative in this example you receive c tokens while your B-person receives d tokens. The sum of the tokens in each row will always be ten.

If you choose the fourth alternative in this example you receive g tokens while your B-person receives h tokens.

All payments will be made at the end of the entire experiment.

Information for A- and B-persons:
Both persons are informed at the end of both experiments about their exact payoff from experiment 1. The identities of the persons will be kept undisclosed as lined out before.

Instructions Experiment 1 [Instructions for *Default-R*-treatments]

Please follow the instructions for Experiment 1 carefully. If there are any questions please raise your hand and an experimenter will answer your question.

Decision in experiment 1. This decision is potentially payoff-relevant.
You are a member of group A (an A-person) and you are assigned exactly one member of group B (a B-person in the other room). In this experiment you are asked to take one decision, which is potentially payoff-relevant for you as well as for the B-person you are paired with.

Random pairing
The pairing of A-person to B-person is randomly done by the computer and remains anonymous. This means you will not get to know which B-person you have been assigned to for experiment 1. In what follows we will refer to the B-person assigned to you as your B-person. Your B person will be given these instructions and is therefore informed about this experiment.

Payoffs resp. conversion rate token/Euro

During the entire experiment we will refer to the payoffs in tokens. The rate of conversion into Euro for experiment 1 is

10 Tokens = 8 Euro
resp. 1 Token = 80 Cent

As indicated above, your decision in this experiment potentially determines the payment for your B-person. In experiment 1, this person will only receive a payment based on your decision.

Your decision in detail:

As A-person in experiment 1 you are asked to choose one of eleven alternatives. Each alternative has a consequence for you and your B-person. This decision problem is presented in a table. The rows in the table display the alternatives. Your task is to choose one of the rows.

Decider and computer proposal

We refer to the alternative (= the row) you choose a **decider proposal**. Beside the decider proposal for each pair of participants there is an alternative proposed by the computer. We refer to this alternative as **computer proposal**. As A-person you see the computer proposal before you take your decision.

Whether the final payoffs of the A- and B-persons are as specified in the decider or the computer proposal is determined at the end of both experiments by drawing a number for each pair out of a lottery drum. The drum is filled with 10 balls numbered from 1 to 10. If an even number is drawn, the decider proposal is implemented; if an odd number is drawn, the computer proposal is implemented.

On the screen the decision problem will look as follows (in this example there are only 4 rows while on the screen there will be eleven rows):

The letters a, b, c, d, etc. are just for illustration. In the experiment there will be numbers instead of letters.

Please click which alternative you want to choose (check one row only)	*decider proposal*		*computer proposal*	
	you receive (in tokens)	your B-person receives (in tokens)	you receive (in tokens)	your B-person receives (in tokens)
	a	b	4*i	4*j
	c	d		
	e	f		
	g	h		

If you choose the second alternative in this example and the decider proposal is implemented, you receive c tokens while your B-person receives d tokens. The sum of the tokens in each row will always be 10.

If you choose the fourth alternative in this example and the decider proposal is implemented, you receive g tokens while your B-person receives h tokens.

If in this example the computer proposal is implemented, you receive i and your B-person receives j tokens. On the screen there will be numbers for i and j which add up to 10.

The random draw to determine whether decider or computer proposal will be implemented will be made at the end of the entire experiment prior to payment.

Information for A- and B-persons:
At the end of the experiment both persons are informed about their exact payoff from experiment 1 and about whether the decider or computer proposal has been implemented. The identities of the persons will be left undisclosed as lined out before.

Instructions Experiment 2 [Instructions for distributional preference test]

Please follow the instructions for Experiment 2 carefully. If there are any questions please raise your hand and an experimenter will answer your question.

10 decisions in experiment 2, only one is payoff-relevant.
In experiment 2 you are asked to take 10 decisions in total. Please note that of these 10 decisions only one will be paid out. Which decision is going to be paid out will be determined by a random draw out of a lottery drum at the end of the experiment. The drum is filled with 10 balls numbered from 1 to 10 each representing one of your decisions. Each ball has the same probability of being drawn.

Random pairing
Each of the 10 decisions has consequences for you and a person from group B (B-person). Please note that you are randomly assigned to another B-person for each decision and that the computer assures that none of these is the same person you have been paired with in experiment 1. You will not be informed during or after the experiment who the B-person with which you were paired for a certain decision is. The same is true also for the B-persons. In what follows we refer to the B-person paired with you simply as your B-person. The assignment of A-person and B-person to form a pair is randomly done by computer and remains anonymous. This means you will not get to know with which B-person you have been paired with for experiment 1. In what follows we will refer to the B-person assigned to you as your B-person. Your B-person also gets these instructions and is therefore informed about this experiment.

Payoffs resp. conversion rate token/Euro
During the entire experiment we will refer to the payoffs in tokens. The rate of conversion into Euro for experiment 1 is

10 Tokens = 5 Euro
resp. 1 Token = 50 Cent

Only 1 of the 10 decisions will be paid out in experiment 2. You and your B-person will be paid the amount determined by your decision. In experiment 2, your B-person will only receive the payment according to your decision in experiment 2.

Details of the 10 decisions:

Each of your decisions is a choice between alternatives left and right. Each alternative is an allocation of tokens for you and your B-person. The 10 decisions are presented in rows looking as follows

Alternative: **Left**			Alternative: **Right**		
Click here if you choose Alternative **Left**	you receive (in tokens)	your B-person receives (in tokens)	you receive (in tokens)	your B-person receives (in tokens)	Click here if you choose Alternative **Right**
	a	b	c	d	

The letters a, b, c, d, etc. are just for illustration. In the experiment there will be numbers instead of letters.

If you choose alternative left in this example, you receive a tokens while your B-person receives b tokens.

If you choose alternative right in this example, you receive c tokens while your B-person receives d tokens.

On the screen the 10 decisions are presented in a table. Please choose one of the alternatives in each row by checking one of the boxes.

Altogether you have to check 10 boxes in experiment 2, one in each of the 10 rows.

After all participants in the room have made their 10 decisions, we will draw the random numbers for each participant and both experiments as lined out before.

Information for your B-person:

Both persons will be informed what the payoffs for both are in the decision situation that has been drawn as payoff-relevant and it will be revealed which alternative person A has chosen in this situation. The identities of the persons will be left undisclosed as lined out before.

Appendix II – Proofs

Proof of Proposition 1a

For $\rho = 1$ the decider has expected utility preferences, and the independence axiom underlying expected utility theory ensures Proposition 1a. Q.E.D.

Proof of Proposition 1b

It is easy to see that $U(10 - s_{100\%}, s_{100\%})$ is weakly larger than any utility achievable in (1). Moreover, a standard dictator with $s_{100\%} = 0$ obtains utility $U(10, 0)$. When $d = 0$ and $s = 0$ this person again obtains $U(10, 0)$ independent of p. So the optimal choice is $s_{p,0} = s_{100\%} = 0$. Finally, utility function (1) is submodular in (s, d), so the optimal choice is decreasing in d, implying that $s_{p,d} \leq s_{p,0}$, which requires $s_{p,d} = 0$ for all s, d since 0 is the feasible lower bound of its domain.

Proof of Proposition 2

We first notice that a standard dictator maximizes

$$U(10 - s, s) = \begin{cases} 10 - s - \alpha(s + \gamma s - 10\gamma) & \text{for } s(1+\gamma) > 10\gamma \Leftrightarrow s > \frac{10\gamma}{1+y} \\ 10 - s - \beta(10\gamma - \gamma s - s) & \text{otherwise} \end{cases}$$

with $\gamma \in (0, 1]$, $\beta \in [0, 1)$ and $\alpha \geq \beta$. From $\alpha \geq \beta \geq 0$ it follows that s^* satisfies $s^* \leq \frac{10\gamma}{1+\gamma}$. We can then easily see that

$$\begin{aligned} s^* &= 0 && \text{for } \beta < \frac{1}{1+\gamma} \\ s^* &\in \left[0, \frac{10\gamma}{1+\gamma}\right] && \text{for } \beta = \frac{1}{1+\gamma} \\ s^* &= \frac{10\gamma}{1+\gamma} && \text{for } \beta > \frac{1}{1+\gamma} \end{aligned}$$

Note our requirement in the main text that $\beta \neq 1/(1+\gamma)$, so we will not discuss this knife edge case further.

A decider only interested in ex post fairness ($\rho = 1$) maximizes:

$$pU(10 - s, s) + (1 - p)U(10 - d, d)$$

Since p and d are exogenously given parameters, maximization of this expression reduces to maximizing $U(10-s, s)$, as asserted in Proposition 1a. Therefore, this decider chooses exactly as the standard dictator above.

A decider only interested in ex ante fairness ($\rho = 0$) maximizes

$$U(10 - ps - (1-p)d, ps + (1-p)d)$$
$$= \begin{cases} 10 - ps - (1-p)d - \alpha[(1+\gamma)(ps + (1-p)d) - 10\gamma] & \text{for } s > \frac{10\gamma-(1+\gamma)(1-p)d}{(1+\gamma)p} \\ 10 - ps - (1-p)d - \beta[10\gamma - (1+\gamma)(ps + (1-p)d)] & \text{otherwise.} \end{cases}$$

Thus, the solution satisfies

$$\begin{aligned} &s^* = 0 && \text{for } \beta < \frac{1}{1+\gamma} \\ &s \in \left[0, \frac{10\gamma - (1+\gamma)(1-p)d}{(1+\gamma)p}\right] && \text{for } \beta = \frac{1}{1+\gamma} \\ &s^* = \frac{10\gamma - (1+\gamma)(1-p)d}{(1+\gamma)p} && \text{for } \beta > \frac{1}{1+\gamma} \end{aligned}$$

Consider now a decider who is interested in ex ante and ex post fairness ($\rho \in (0,1)$). Suppose the dictator chooses $s_{100\%} > 0$ (implying $\beta > \frac{1}{1+\gamma}$, given that we ignore the indifference case $\beta = \frac{1}{1+\gamma}$). Further suppose that the decider faces a probability $p > 0$ of a default $d > s_{100\%}$.

From $\beta > \frac{1}{1+\gamma}$ we know that $s_{100\%} = \frac{10\gamma}{1+\gamma}$. Now, for $s_{p,d}$ there are only two candidates for the solution: $s_{p,d} = 10\gamma/(1+\gamma)$ and $s_{p,d} = [10\gamma - (1+\gamma)(1-p)d]/[(1+\gamma)p]$. To see this, first notice that the utility function is a weighted average of the ex-ante utility and the ex-post utility. The ex-ante-utility is concave in s, with maximum at $\frac{10\gamma}{1+\gamma}$, while the ex-post-utility is concave with maximum at $s_{p,d} = \frac{10\gamma-(1+\gamma)(1-p)d}{(1+\gamma)p}$. So their weighted sum is concave, and obtains a maximum somewhere between the two candidates. We can now see from the utility function that within this range the utility function is linear. So, the optimum has to include one of the corners. If the utilities at the two corners are equal, all intermediate levels are also optimal.

For $s_{p,d} = \frac{10\gamma}{1+\gamma}$, the decider's utility is

$$A = \rho\left[p\frac{10}{1+\gamma} + (1-p)[10 - d - \alpha(d + \gamma d - 10\gamma)]\right] + $$
$$(1-\rho)\left[10 - p\frac{10\gamma}{1+\gamma} - (1-p)d - \alpha(1-p)\left[(1+\gamma)d - 10\gamma\right]\right]$$

For $s_{p,d} = \frac{10\gamma-(1+\gamma)(1-p)d}{(1+\gamma)p}$ the decider's utility is

$$\begin{aligned} B =& \rho\left[p\left[10 - \frac{10\gamma-(1+\gamma)(1-p)d}{(1+\gamma)p} - \beta[10\gamma - \frac{10\gamma-(1+\gamma)(1-p)d}{p}]\right] + \right. \\ & \left. + (1-p)[10 - d - \alpha(d + \gamma d - 10\gamma)] \quad \right] + \\ & + (1-\rho)\frac{10}{1+\gamma} \end{aligned}$$

Now, $A - B$ yields

$$\begin{aligned} & \rho(1-p)\left[\frac{10\gamma-(1+\gamma)d}{1+\gamma} + \beta[(1+\gamma)d - 10\gamma]\right] + \\ & + (1-\rho)(1-p)\left[\frac{10\gamma-(1+\gamma)d}{1+\gamma} - \alpha[(1+\gamma)d - 10\gamma]\right] = \\ & = (1-p)[10\gamma - (1+\gamma)d]\left[\frac{1}{1+\gamma} - \rho\beta + (1-\rho)\alpha\right] \end{aligned}$$

For B to be preferred to A we need

$$(1-p)[10\gamma - (1+\gamma)d]\left[\frac{1}{1+\gamma} - \rho\beta + (1-\rho)\alpha\right] \leq 0$$

Since $d > s_{100\%}$ implies $10\gamma - (1+\gamma)d < 0$, the above inequality is satisfied iff

$$\begin{aligned} \rho\beta - (1-\rho)\alpha - \frac{1}{1+\gamma} \quad &\leq \quad 0 \Leftrightarrow \\ (1-\rho)\alpha + \frac{1}{1+\gamma} \quad &\geq \quad \rho\beta \qquad\qquad \text{(X)} \end{aligned}$$

Since (by assumption) $\alpha \geq \beta$, a sufficient condition for inequality (X) to hold is $\rho \leq \frac{1}{2}$. Q.E.D.

Proof of Proposition 3

For Proposition 3, consider a particular decider and a setting with a default that is more generous to the receiver than his desired transfer. Specifically, consider a default $d = s_{100\%} + \Delta$, with $\Delta > 0$. We compare this default to a default $d' = s_{100\%} - \Delta$ that is more selfish.

The reaction to the default that gives Δ units more to the receiver than the decider would have chosen as a standard dictator is obtained by replacing in the above derivation the d by $s_{100\%} + \Delta$. Assuming $\beta > \frac{1}{1+\gamma}$ (thereby ignoring the knife edge case) yields:

$$s_{p,d} \begin{cases} = \frac{10\gamma}{1+\gamma} \\ \text{if condition (X) is violated} \\ \in \left[\frac{10\gamma}{1+\gamma} - \Delta\frac{1-p}{p}, \frac{10\gamma}{1+\gamma}\right] \\ \text{if condition (X) holds as an equality} \\ = \frac{10\gamma}{1+\gamma} - \Delta\frac{(1-p)}{p} \\ \text{if condition (X) holds as a strict inequality} \end{cases}$$

For the reaction to the default d' that leaves Δ more to the decider than he would have chosen as a standard dictator there are again generically only two candidates for the solution: $s_{p,d'} = \frac{10\gamma}{1+\gamma}$ and $s_{p,d'} = \frac{10\gamma}{1+\gamma} + \Delta\frac{1-p}{p}$. Following similar lines as in the proof of Proposition 2 we find that for $s_{p,d'} = \frac{10\gamma}{1+\gamma} + \Delta\frac{1-p}{p}$ to be preferred over $s_{p,d'} = \frac{10\gamma}{1+\gamma}$ we need

$$\rho\alpha + \frac{1}{1+\gamma} \leq (1-\rho)\beta \quad \text{(Y)}$$

Condition (X) from the proof of Proposition 2 is equivalent to

$$\frac{1}{1+\gamma} \geq \rho\beta - (1-\rho)\alpha \quad \text{(X')}$$

Condition (Y) is equivalent to

$$\frac{1}{1+\gamma} \leq (1-\rho)\beta - \rho\alpha \quad \text{(Y')}$$

We now show that (Y') is always more demanding than (X'). For $\rho \leq \frac{1}{2}$, the

RHS of (X') is negative (as $\alpha \geq \beta$) implying that (X') is satisfied. Condition (Y') might be violated (if β is close to $\frac{1}{1+\gamma}$ as α is larger than β). Therefore, for $\rho \leq 1/2$, (Y') is more demanding than (X').

For $\rho > 1/2$, we proceed by contradiction. Assume (X') is violated while (Y') holds. Then we have

$$\frac{1}{1+\gamma} < \rho\beta - (1-\rho)\alpha \quad \text{(X")}.$$

Adding the left sides of (X") and (Y') and adding the right sides, this implies

$$\frac{2}{1+\gamma} < \beta - \alpha$$

The left hand side is strictly positive, but the right hand side is weakly negative since $\alpha \geq \beta$. Q.E.D.

Proof of Proposition 4

Consider a decider with given $s_{100\%}$, and a default d such that $\Delta = s_{100\%} - d$. Now let p converge to 1. As shown in the proof of Proposition 3, the dictator chooses $s_{p,d} \in [s_{100\%} - |\Delta|\frac{1-p}{p}], s_{100\%} + |\Delta|\frac{1-p}{p}]]$. Clearly this converges. Q.E.D.

Appendix III – Summary statistics

Table 5: Summary statistics – aggregate data

	mean transfer (SD)	share of transfers > 0	mean transfer > 0
Pooled treatments			
Dictator[a]	1.60 (2.05)	45.2	3.53
Default-0[b]	1.26 (1.84)	39.2	3.21
Default-10[b]	0.94 (1.63)	30.2	3.10
Individual treatments			
Dictator-C	1.44 (1.95)	42.0	3.42
Default-0-C	1.19 (1.76)	39.1	3.04
Default-5-C	0.95 (1.73)	27.9	3.42
Default-10-C	0.86 (1.55)	29.2	2.95
Dictator-R	1.83 (2.19)	50.0	3.67
Default-0-R	1.39 (2.00)	39.4	3.54
Default-10-R	1.10 (1.81)	32.3	3.40
Low-0-R	1.82 (2.04)	53.6	3.40
Low-10-R	2.13 (1.96)	63.0	3.38

[a]: revealed and concealed action treatments (*Dictator*-R and C)
[b]: revealed and concealed action treatments (*Default*-0-R and 0-C, resp. Default-10-R and 10-C)

Table 6: Summary statistics for *Low*-0-R and *Low*-10-R for selfish and non-selfish subjects

	Selfish			Non-selfish		
Treatments	mean transfer (SD)	share of transfers > 0	# of observations ($\frac{N}{2}$)	mean transfer (SD)	share of transfers > 0	# of observations ($\frac{N}{2}$)
Low-0-R	1.06 (1.77)	37.5	16	3.11 (2.09)	77.8	9
Low-10-R	1.81 (1.83)	57.7	26	2.55 (2.09)	70.0	20

Observations in *Low*-0-R are only 25 overall and not 28 due to three missing observations for the EET.

Appendix IV – Distributions of transfers

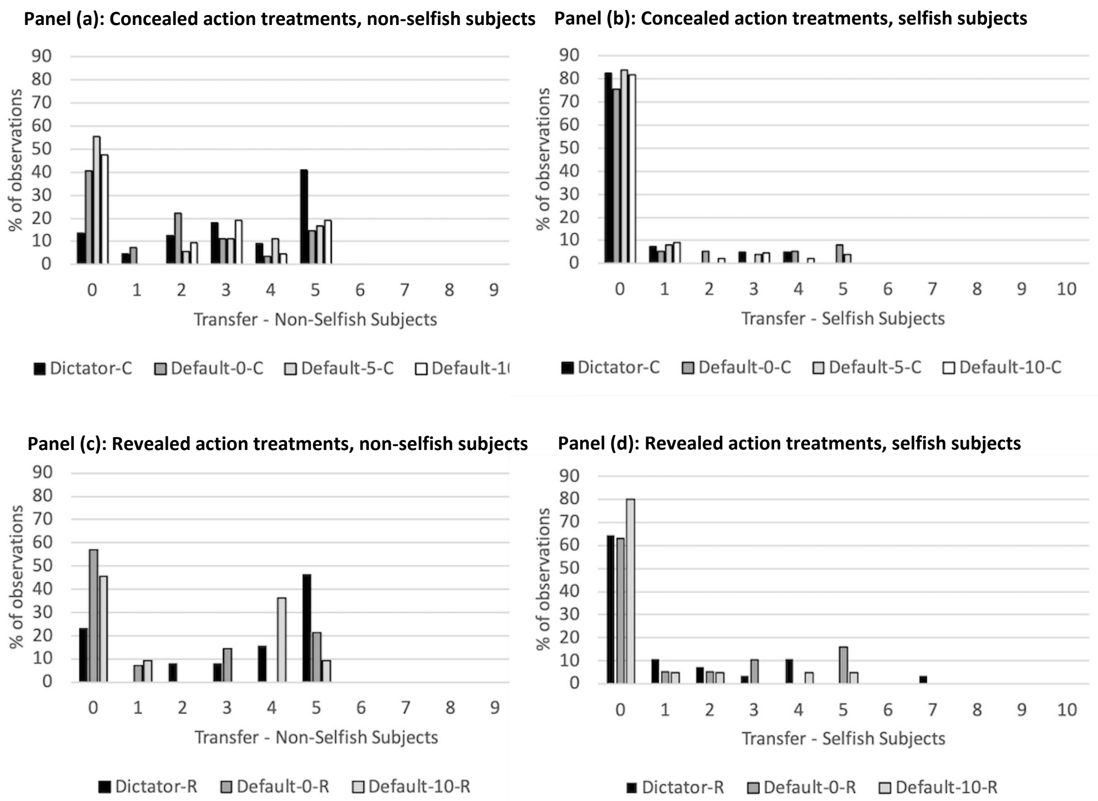

Figure 3: Distribution of transfers

Appendix V – Regression results anchoring

Table 7: Regression results on the low probability of default treatments for non-selfish and selfish subjects

	Non-Selfish		Selfish	
Dependent variable:	**Transfer** (Tobit)	**Giver** (Logistic)	**Transfer** (Tobit)	**Giver** (Logistic)
Low-10-*R*	- 0.90	- 0.36	+1.72	+0.90
	(0.964)	(0.819)	(1.049)	(0.556)
Low-0-*R*	-1.93	+0.05	+0.11	+0.08
	(1.93)	(1.037)	(1.241)	(0.650)
Constant	+3.01***	+0.66*	-0.94	-0.59
	(0.749)	(0.658)	(0.828)	(0.394)
Number of observations	42	42	70	70
Number of left-/right-censored observations	11/0		39/0	
(Pseudo) R-squared	0.0057	0.0060	0.0148	0.0315

Numbers in parentheses indicate standard errors. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.
The number of observations is lower when we control for selfish deciders as we have four missing observations for the EET.