The perils of advocating transfers

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Abstract

This paper reports the results of an experimental study investigating unintended consequences of advocacy for wealth redistribution. Advocacy for transfers may polarize society and may, paradoxically, lead to less transfers. This reaction has a subtle logical structure that cannot be captured by any standard economic model. In fact, our results challenge economic principles that are crucial in many domains such as inferences of people's preferences based on their votes in democratic elections.

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1 Introduction

Wealth redistribution is arguably the most contentious issue in American politics. Key examples are the 16th Amendment to the Constitution in 1913, which made the income tax a permanent fixture in the U.S.; the withholding tax on wages of 1943, aimed at increasing the number of taxpayers and the tax revenue per taxpayer; and the Tax Reform Act of 1986, which lowered the top tax rate on individual income. None of these reforms were enacted without opposition. Typically, several policies are considered until one is implemented. In this paper, we are concerned with the effects of advocacy for redistribution policies which may or may not succeed.

Traditional models in economics rely on the assumption that actions are evaluated by their consequences and therefore, do not depend on events outside the control of the decision maker. In particular, whether or not an individual supports income redistribution should not depend, so the argument goes, on events that the decision maker cannot influence in any way (i.e., neither the content of the policy nor the probability of its implementation).

In this paper, we test and reject this basic tenet of economic behavior when applied to wealth redistribution. Instead, we put forward the idea that to properly capture how people react to proposed reforms on public policy requires a transformation of our understanding of decisions under risk. In order to demonstrate the difficulty and the absence of easy fixes, first consider the following abstract scenario. One outcome, out of several possibilities, must be implemented. Outcome A entails high taxation and high levels of transfers, while outcome B entails low taxation and low levels of transfers. This is a social choice and, as such, typically no one is completely decisive. An individual can only determine the social choice in the event that the individual is pivotal. An individual who prefers outcome A over outcome B (i.e., chooses A over B if deciding alone) would, as traditionally assumed, prefer to increase the odds of A as opposed to the odds of B. Equivalently, if default outcomes occur with fixed odds, then the choice must be for the implementation of A as opposed to B in the event that the individual is pivotal (see the appendix for a proof). This is so regardless of what the default and its odds might be. In an experiment intended to capture such stylized features, we find that, contrary to this assumption, a significant number of individuals change their support in response to changes in the default.

Consider first a default outcome of high transfers with exogenous odds, outside of decision makers' control. When the probability of high transfers is exogenously increased, more people support no transfers and also the average transfers are significantly lower. These results are a first indication of the perils of advocating transfers (i.e., creating an exogenous chance of transfers). Such advocacy might backfire and result in less transfers. We discuss below the consequences for general advocacy, for judges faced with disputes, for polarization of a split society, and for voting in elections.

In our experiment, a large number of participants make choices among alternatives that differ in the generosity to a receiver. A coin flip determines the state of the world. With equal chance, either the participant's choice or a default outcome D (i.e., determined by an advocate) is implemented. A fair coin flip is transparent to the participants and ensures that they understand that their "pivot" probability of being decisive is fixed and independent of their choice. A simplified version of the participants' choice under risk is summarized in Table 1. In the displayed decision situation there are only two alternatives, l_A over l_B, while in the experiment there are more. Now assume that a decider chooses outcome A over outcome B when only state 1 is present (i.e., in a standard dictator game decision under certainty). Then, standard theories predict that the decider must choose l_A over l_B when states 1 and 2 are possible, no matter which default D the advocate supports and no matter what the implementation probability of D is. This follows because the outcome of l_A is A in the only event where the decider's choice matters. A choice of l_B over l_A means, somewhat paradoxically, a choice of the worse outcome in the only event where the choice matters. This insight carries over to any number of possible outcomes available and is more technically known as the monotonicity axiom.

Table 1: Setup

	state 1	state 2
l_A	A	D
l_B	B	D

Our experimental design features seven main treatments and two control treatments. In each treatment there are two roles – decider and receiver. Each decider chooses how to split 8 Euro between himself/herself and a randomly assigned anonymous receiver. We analyze the following questions: Does the presence of an "advocate" change the average choice, compared to a standard dictator game setting? All standard theories suggest that it should not. If it does, does it lead individuals to support higher or lower transfers? And how are the results affected by the outcome D that the advocate supports? Compared to the benchmark of choice under certainty, we find that the fraction of individuals who transfer money and the average transfer decrease when the advocate supports high transfers. However, it is *not* the case that transfers increase when the advocate supports low (i.e., no) transfers. Advocating high transfers induces lower transfers, but reversing the advocacy (i.e., advocating low transfers) does not reverse the effect and induces higher transfers. This asymmetric effect of the advocate's presence is one of the reasons why we refer to our results as the perils of advocating transfers.

Our findings are derived from a large laboratory experiment involving more than 800 participants. We adopted a neutral frame and indicate this by calling outcome D the "default". 45% of deciders transfer money in the standard dictator game setting, but only 33% do so when the choice is followed by a lottery that determines whether their decision or the default D is implemented. In addition, the average transfer in the latter case is about 30% lower. A deeper analysis reveals that the default itself matters: If the default is generous to the receiver then, in contrast to the predictions of standard economic models, there is a pronounced and significant decrease in generosity compared to the standard dictator game (only about 30% of deciders choose positive transfers). If the default favors the deciders then there is only a small and insignificant reduction in generosity compared to the standard dictator game (40% of deciders choose positive transfers). Hence, these results show significant violations of the monotonicity axiom, but only when the default is of high transfers and not when the default is of low transfers. Finally, under a default that is generous to the receiver, the average transfer is by 25% lower compared to a setting where the default favors the decider.

Our findings arise under a fair coin-flip lottery that is cognitively easy to understand. This is an indication that our results are due to the social aspects of the choice. Violations of monotonicity are uncommon in individual consumption choices. When they do occur in such choices then, unlike our findings, they are linked to either compounded lotteries or to situations where the relevant probabilities are low. Most related to or setup is work by (1–3) which also find some non-monotonic behavior, but only for a very low probability of payment changes. These studies do not find non-monotonicity for 50-50 coin flips (or even close, such as a 60-40).

Investigations of support reversals in social settings are also rare. (4) highlight a concern for procedures in social settings. This does not entail a reversal in support as we document it here. $(5, 6)$ show that individuals split lottery tickets and $(7, 8)$ highlight a concern for expected payoffs. Here, however, we provide a systematic analysis of how choices vary with the level of the default and the probability of its implementation.

The "crowding-out" effect that we find here is also fundamentally different from a standard crowding-out effect where people give less if someone else gives more, as for example documented in (9). This phenomenon can be captured with either expected utility theory or with a relatively simple extension of it.

Our findings point to a critical aspect of policy proposals. Consider a divided society. The richer part may be willing to voluntarily transfer some resources to the poorer part. However, if there is fear of civil unrest leading to significant non-voluntary transfers, then such willingness to voluntarily transfers may disappear. So, the fear of change might keep society unchanged. Similarly, a judge's verdict may be affected if the judge fears that the ruling will be overturned on appeal. The same logic applies to decisions in arbitration which are often annulled if the dispute goes to trial. Finally, the support of independent swing voters on redistributive policies may diminish if there is an increase in partisan support for these policies. This effect is above and beyond any standard strategic considerations that such increase may bring about.

Our results also pose a challenge for economic theory. It is hard enough to determine the precise psychological underpinnings of our findings. However, the main difficulty is that whichever the root cause of these findings might be, they cast doubt on the traditional link between decision under risk and decision under certainty. Once this link is weakened, it becomes unclear how to conduct revealed preference exercises. We conclude this introduction with some candidate explanations for our results, followed by a detailed description of the main difficulty.

A concern for ex-ante fairness could be a candidate explanation for our results. (10) show that any concern for expected transfers violates expected utility theory. (11–15) deliver representation results. A difficulty with this explanation is that we do not observe an increase in transfers when the default favors deciders (compared to the decision under certainty) as a desire for a fixed expected transfer would imply. Hence, our experiment shows a broader adverse effect of advocacy.

Naturally, our results cannot be accommodated within expected utility theory nor by extensions of it that relax, say, the independence axiom, while keeping monotonicity. So, the majority of currently axiomatized theories in economics can be ruled out. However, even some theories that can accommodate violations of monotonicity are hard to reconcile with our findings. Consider warm-glow theory. The most popular version of it postulates that people receive a warm-glow payoff (i.e., above and beyond any consequence of their actions) if they act generously (16). This theory can accommodate violations of monotonicity, but cannot accommodate the decrease in generosity that we observe (see Appendix I for details).[1] Theories of low-cost expressive voting (17–19) predict more benevolent choices when the probability of being decisive is lower – which is contrary to our findings. Theories postulating a desire to be perceived positively by the receiver (20–22) are unlikely to explain our findings. They predict that the size of the effect depends on whether the decider's choice is concealed from the receiver or is saliently revealed. Our basic patterns, however, persist irrespective of whether we reveal or conceal the choices from the receiver. Finally, one might be worried about "anchoring": the mere presence of the default D might change the setup relative to a standard dictator game. Yet, when we introduce the default only with a very small probability (2%), we do not observe a move towards less generosity. This also rules out explanations similar to (23) where risk aversion depends on the social reference point that might be manipulated through D.

As mentioned, there are, however, good candidate theories for our results. For example, in Appendix I we show that a sufficiently extended theory of warm-glow can accommodate our results and so can a properly extended theory based on concerns for ex-ante fairness. However, there are difficulties beyond the search for the right explanation for our findings.

Assume that policy A (no transfers) is the victor in an election against B (some transfers). Now consider a politician who wants to implement "the will of the people." This politician intends to act on behalf of her constituents and to select the policy that

[1] One of the motivating factors for the development of warm glow theory was the observation that philanthropy and contributions to public goods did not respond as much to increased taxation as expected utility theory would predict. These observations are based on measurements after taxation is implemented. The idea advanced in this paper that generosity may be strongly reduced given high risks of future taxation indicates how psychologically complex the phenomena of reactions to wealth redistribution may be.

most would choose if each could decide. Under expected utility theory, the politician should select A because this is what the majority would have chosen. Under a theory that subsumes expected utility theory and can accommodate our results, there is no clear answer. This follows because no voter actually chooses A over B. A vote for A just increases the odds that A wins the election. From this choice one cannot infer that this voter would choose A if given the opportunity to decide. This choice may be A or B.

The predicament of inferring preferences from actions, like votes, is common. Almost any decision involves some risk and a preference of A over B is typically understood as a choice of A over B under certainty. Monotonicity is the crucial link between decisions under risk and under certainty. Hence, revealed preference becomes problematic once monotonicity is violated. To make proper inferences about preferences one may need enough empirical evidence to know precisely when and how to replace standard theory and not just how to generalize it. This paper takes some steps in this direction, but a full answer is beyond its scope. As far as we know, there is currently no theory that can simultaneously accommodate our results and has parameters sufficiently identified to always reveal basic preferences from decisions under risk.

The paper is organized as follows: The next section briefly expands on the related literature, before introducing the experimental setup in Section 3. Section 4 presents our findings. Section 5 shows applications of our results and concludes the paper. The applications include a novel rationale for abstention in elections. This motivates new areas of research in economics and political science.

2 Related Experimental Work

Recent experimental evidence suggests that people are pro-social to varying degrees and that they like to be perceived as pro-social. For instance, in (22) people give up money to avoid a giving decision known to the recipient. In (24) people behave more selfishly, when they can avoid learning how their decisions affect others. (20) consider dictator games with uncertainty where the dictator or nature determines final payoffs and where the recipient cannot directly observe whether nature of another subject is responsible for a given outcome. The authors find that (egoistic) dictators capitalize on the recipients' lack of information in order to behave more selfishly. To abstract from hiding motives, we analyze giving decisions that cannot be hidden and also compare them with those that one can hide.

Other studies argue that risk and ambiguity provide an excuse not to give: In (25), dictators give less when they are uncertain about how their transfers translate into payoffs for others. In (26), participants choose between a certain or uncertain payoff for themselves and for a recipient. Participants are more averse to the risk the recipient faces than to the risk they face themselves. In (27) dictators behave more self-interestedly when the risky payoffs for others involve ambiguous probabilities than when the probabilities are known. In these studies the dictators' decisions are always implemented for sure.

Some studies show concerns for fair procedures or ex-ante payoff comparisons. (4) document in ultimatum and battle-of-the-sexes games that it matters for the fairness perceptions of recipients whether an outcome is determined by a fair or unfair procedure. (5, 6) find evidence that people care about both ex-post and ex-ante fairness considerations. Similarly, (7,8,28) provide evidence for a concern for expected payoffs in giving decisions. Yet, these studies neither consider any situation where the dictators' choices might not be implemented, nor do they ask whether the default matters for revealed or concealed choices, nor do they vary the implementation probability. In our study, we systematically analyze how choices vary with the level of the default and the probability of implementation.

3 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 (29) and participants were recruited via ORSEE (30). The subjects 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 the first stage: a one-shot dictator game. In the second stage, deciders completed a distributional preference test.

Across treatments, we vary the dictator game. In all of them the deciders face exactly the same choice set. They have 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 themselves. In the *Dictator* treatments (two treatments with $N = 208$ participants in total) the decider's choice is always pivotal and surely determines the earnings. Broadly speaking this choice can be seen as one between giving zero (choice A in the introduction) and giving some positive amount (choice B in the introduction), and some of our analysis will focus on this dichotomy. In the *Default* treatments (five treatments with $N = 472$ participants) 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 (which corresponds to the default D in the introduction). 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 receiver (*Default-10*, $N = 192$), all points go to the decider (*Default-0*, $N = 194$), 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' (20,22). To investigate the importance of such 'audience effects' for behavior, we conduct each of the described treatments (except for *Default-5*) in two variants: In condition R ($N = 212$, treatments: *Dictator-R*, *Default*-10-R and *Default*-0-R), the receiver knows the decider's decision problem and learns her decision, i.e. choices are revealed. In condition C ($N = 468$, treatments: *Dictator-C*, *Default*-10-C, *Default*-5-C and *Default*-0-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.

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.[2]

In the second part of the experiment we elicited the distributional preferences of deciders using the Equality Equivalence Test proposed by (31). This procedure exposes subjects to a series of incentivized binary choices between allocations that

[2] The experimental instructions and an overview of the treatments are provided in Appendix II.

involve an own payoff for the decider and a payoff for a randomly matched anonymous passive subject. Similar to (32) the test systematically varies the price of giving (or taking). We use it to identify individuals who are mainly interested in the own material payoff as we do not expect any treatment variation from them.

4 Experimental Results

Table 2 summarizes for each treatment the average transfer, the minimum and maximum transfer, the fraction of positive transfers and the average positive transfer (i.e. the average transfer given that the transfer is positive). Before displaying the results for the individual treatments, Table 2 reports pooled data in the first four rows: For both, the *Dictator* and the *Default* treatments, we pool the revealed and concealed action treatments (between which we do not find significant differences as reported below), and for the *Default* treatments, we additionally pool the treatments with different defaults. To highlight the main results, we first report results for this pooled data before we separate treatments.

In the *Dictator* treatments, where the decider's choice is implemented for sure and there is no default, we observe that deciders transfer, on average, 1.6 of the 10 points to the paired receiver. Roughly 45% of the deciders choose to transfer a positive amount. Deciders in the *Default* treatments, whose choice is implemented with a probability of 50% only, give significantly less: On average, they transfer 1.1 points and 33% give a positive amount; the Mann-Whitney U (MWU) test comparing the sizes of the transfers yields $p = 0.022$; the Chi^2 test comparing the fractions of positive transfers yields $p = 0.039$.[3] Hence, subjects get less generous if there is a substantial chance that their choice is not pivotal for the final outcome.

Is the result that subjects are less generous in the *Default* treatments independent of the default? For *Default-0* the average transfer is 1.26, while it is only 0.95 for *Default-5* and only 0.94 for *Default-10*. Similarly, 39% of the transfers are positive for *Default-0*, whereas only 28% and 30% of the transfers are positive for *Default-5* and *Default-10*, respectively. Compared to the *Dictator* treatments, deciders are significantly less generous when the default is rather generous to the receiver (*Dictator*

[3] When comparing giving behavior in the following, we only report the results from the MWU tests comparing the sizes of transfers since the results from the Chi^2 tests comparing the fractions of positive transfers do not qualitatively differ.

vs. *Default-10* / *Default-5*: MWU tests of transfers, p =0.008/0.028), but not if the default is favorable for themselves (*Dictator* vs. *Default-0*, MWU test of transfers, $p = 0.142$).[4]

Across defaults, we find that deciders are weakly-significantly more generous when the default gives nothing to the receiver (*Default*-0) than if it gives all or half the money to the receiver. It makes no difference whether all or half the money goes to the receiver.[5]

Table 2: Overview

	average transfer	standard deviation	min/max transfer	fractions of positive transfers	average transfer> 0	no. of observations ($\frac{N}{2}$)
Pooled treatments						
Dictator[a]	1.60	2.05	0/7	45.2	3.53	104
Default[b]	1.07	1.74	0/5	33.5	3.20	236
Default-0[c]	1.26	1.84	0/5	39.2	3.21	97
Default-10[c]	0.94	1.63	0/5	30.2	3.10	96
Individual treatments						
Dictator-R	1.83	2.19	0/7	50.0	3.67	42
Default-0-*R*	1.39	2.00	0/5	39.4	3.54	33
Default-10-*R*	1.10	1.81	0/5	32.3	3.40	31
Dictator-C	1.44	1.95	0/5	42.0	3.42	62
Default-0-*C*	1.19	1.76	0/5	39.1	3.04	64
Default-10-*C*	0.86	1.55	0/5	29.2	2.95	65
Default-5-*C*	0.95	1.73	0/5	27.9	3.42	43
Low-0-*R*	1.82	2.04	0/5	53.6	3.40	28
Low-10-*R*	2.13	1.96	0/5	63.0	3.38	46

[a]: revealed and concealed action treatments (*Dictator-R* and *C*)
[b]: *Default*-0/5/10 treatments with revealed and concealed action (*Default*-0-R, 0-*C*, 10-*R*, 10-*C*, 5-*C*)
[c]: revealed and concealed action treatments (*Default*-0-*R* and 0-*C*; resp. Default-10-*R* and 10-*C*)

Comparing revealed and concealed action treatments, we observe that transfers are slightly, though insignificantly, higher if they are revealed to the receiver (cf. Table 2). This holds true for all games conducted in both visibility conditions (for *Dictator* R vs. C, *Default*-0 R vs. C, *Default*-10 R vs. C the MWU test results are

[4]As a robustness check for the finding that transfers do not differ significantly from those in a standard dictator game when the default is favorable for the decider, we conducted another *Default*-0 treatment, in which the default is implemented with a high probability of 80% ($N = 66$). We conducted this treatment with concealed action only. Results (which we do not report in detail here) do not differ from those in the *Default*-0-*C* and *Dictator-C* treatments.

[5]Comparing *Default*-0 with *Default*-10, *Default*-0 with *Default*-5, and *Default*-10 with *Default*-5, the results of the MWU test of transfers are $p = 0.089$, 0.0996, and 0.392, respectively.

$p = 0.180$, $p = 0.378$, and $p = 0.318$, respectively). The insignificant effect of visibility on transfers is confirmed by a regression analysis as explained below. Moreover, and more importantly, the observed choice pattern across treatments hardly changes with the visibility of the decider's action. Irrespective of whether transfers are revealed or concealed, deciders are weakly-significantly less generous when their decision is implemented only with 50% probability.[6] As observed for the pooled data, the decrease in generosity depends on the default; it is large and significant when the default is generous to the receiver and becomes small and insignificant when the default leaves no money for the receiver. Thus, signaling to the receiver does not appear to be the driving force behind our observations.

Could it be that the mere knowledge of the default anchors people's decisions in a way that can explain why people give less if there is a substantial chance that their choice is not pivotal for the final outcome – in particular, when the default is rather generous to the receiver? To analyze this issue we consider the treatments *Low*-0-R and *Low*-10-R, in which the default is implemented with a *low* probability of only 2%. If the default indeed anchors people's decisions, it should do so even if the probability of its implementation is very low. Thus, if anchoring drives the observed differences between the *Dictator* and 50% *Default* treatments, transfers in the low probability *Default* treatments should show deviations in the same direction from transfers in the *Dictator*-R treatment. Average transfers in the *Low*-0-R and *Low*-10-R treatment are 1.82 and 2.13, respectively and do not significantly differ from transfers in the *Dictator*-R treatment (MWU test *Dictator*-R vs. *Low*-0-R/*Low*-10-R: $p = 0.452/0.202$). If anything, transfers are higher in *Low*-10-R than in *Dictator*-R. Yet, even if the generous default that gives all the money to the receiver anchored decisions such that deciders get more generous, it could not explain our previous observation that deciders in the *Default*-10 treatments are *less* generous than in the *Dictator* treatments.

The regression analysis reported in Table 3 confirms our findings on the impact of the default, the implementation probability and the visibility of the decider's choice on transfers. Columns 2 and 3 in Table 3 present the results of Tobit regressions, in which the dependent variable is the size of the transfer and which account for the fact that transfers have to lie between zero and ten. Columns 4 and 5 report the results

[6] Comparing *Dictator* with *Default* under conditions R and C, respectively, the MWU test yields $p = 0.063$ and $p = 0.071$.

Table 3: Regression results on the size of the transfer and on and making a positive transfer

Dependent variable:	**Transfer** (Tobit)	**Transfer** (Tobit)	**Giver** (Logistic)	**Giver** (Logistic)
Default-10 dummy	- 1.59**	- 1.43**	-0.63**	-0.72**
	(0.678)	(0.608)	(0.298)	(0.323)
Default-5 dummy	- 1.52*	- 1.64**	-0.69*	-0.95**
	(0.920)	(0.826)	(0.406)	(0.441)
Default-0 dummy	- 0.67	- 0.83	-0.24	-0.43
	(0.654)	(0.531)	(0.287)	(0.314)
Revealed action dummy	+0.57	+0.65	+0.17	+0.20
	(0.568)	(0.489)	(0.250)	(0.251)
Selfish dummy		-3.65***		-1.71***
		(0.506)		(0.271)
Constant	-0.46	+1.90***	-0.26	+0.84***
	(0.534)	(0.522)	(0.222)	(0.291)
Number of observations	340	340	340	340
Number of left-/right-censored observations	214/0	214/0		
(Pseudo) R-squared	0.0089	0.0707	0.0159	0.1275

Numbers in parentheses indicate standard errors. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

of logistic regressions, in which the dependent variable "giver" indicates whether or not a transfer is positive. The independent variables are treatment dummies for the *Default*-10, *Default*-0, and *Default*-5 treatments (i.e. the results are relative to the *Dictator* treatment), and a dummy for the revealed action condition. In one specification of the Tobit and logistic regression we also include a dummy indicating whether or not the dictator is "selfish" in the sense of never making an allocation decision that does not maximize the own payoff in the distributional preference test in the second part of the experiment. We control for selfish subjects because those are the ones for whom we do not expect any treatment variation. 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 if deciders are likely not to be pivotal precisely in those settings where the default is generous to the receiver (*Default*-10 and *Default*-5). When the default, however, gives all the money to the decider, transfers do not change relative to the *Dictator* treatment. Moreover, the estimations show that revealing the decider's action does not have a significant impact. Further regressions (see Appendix III) confirm our findings on anchoring. The estimations show that in the treatments *Low*-0-R and *Low*-10-R, where the default is implemented with the low probability of only 2%, the default does not significantly affect transfers compared to the *Dictator-R* treatment. This as an indication that anchoring does not explain our findings.

5 Conclusion

Advocating transfers involves a cost. If, in a default event, wealth is to be redistributed, then people's propensity to act generously diminishes. By contrast, a default with a low level of transfers does not increase generosity. Together, these two tendencies create an important asymmetry in the reaction to suggested transfers.

Our results motivate an economic analysis of phenomena that cannot be captured by standard expected utility theory. For example, in a society divided by wealth, the richer part may support some transfer to the poorer part, but their support may vanish once there is strong advocacy for higher levels of transfer. This resistance to wealth transfers may polarize society and lead to less income redistribution.

Similarly, consider a judge who believes that a plaintiff is entitled to some compensation. If the plaintiff threatens to go to a higher court to receive more money then the judge may cut back on the level of transfer in his ruling to account for the chance of excessive compensation in the higher court. The same logic holds in a settlement case where the defendant proposes the settlement.

As a further application, consider the case of an election concerning a proposal of redistributive taxation. One segment of the population favors this transfer and another segment opposes it. A third segment of swing voters cares about ex-ante fairness and favors some expected transfers, but less than the proposed policy. For these swing voters, the ideal probability that the proposal passes is p. Also assume, as customary in voting models, that the relative sizes of the groups are uncertain. It is then relatively straightforward to construct an equilibrium where some swing voters vote against the proposal and some swing voters abstain if they expect that absent any swing voters the proposal would pass with high probability (i.e., greater than p). By doing so they can bring the probability that the proposal wins as close to p as possible. Analogously, if, absent any swing voters, the proposal passes with a low probability, then some swing voters vote in favor of the proposal and some abstain so that the proposal wins with the ideal odds of the swing voters.

The idea that some independent voters offset partisanship just enough to obtain their ideal outcome is analogous to the successful information aggregation results of (33), but there is one crucial difference. In standard theory, voters only strictly prefer to abstain when they are uninformed (so that informed voters with the same preferences can decide the election). Here some voters may strictly prefer to abstain even if they are fully informed and there are no costs to vote. Our results suggest that this alternative reason for abstention and the counter-balancing of partisanship may play a significant role in actual elections (see (34) for evidence of reaction to partisanship).

The implications of our experimental findings might be of substantial relevance in other 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 offset probabilities and magnitudes of transfers 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.

Supplementary Material

Appendix I – Basic Concepts

This appendix formally defines the concept of monotonicity and the monotonicity axiom. It shows that it is equivalence to a preference for lotteries that first-order stochastically dominate other lotteries - with a particular choice of order for the underlying outcomes. This gives monotonicity its meaning, as it implies a preferences for higher lotteries (in the sense of stochastic dominance). A key difference to the rest of the literature is that instead of considering monotonicity in terms of outcomes that are naturally ranked (such as monetary outcomes where more money is more preferred), we consider settings where the outcomes are not naturally ranked (such as colors, or in our application allocations involving own money and another person's money) and where the ranking only arises because of the decider's preferences (in the absence of lotteries). Starting from broad generality, it then links the setting back to the one of the main body of our paper, and to alternative explanations of the violations that we find in our experiments, in particular to explanations based on simple theories of warm glow.

Let $\mathcal{O}$ be a finite set of alternatives. An element $o \in \mathcal{O}$ is called an *outcome*. Let $\Delta(\mathcal{O})$ be the set of probability measures over $\mathcal{O}$. An element $l \in \Delta(\mathcal{O})$ is called a *lottery*, as it assigns probabilities to each of the different outcomes. With slight abuse of notation, let $l(o)$ denote the probability that l assigns to outcome o. An outcome is identified with a degenerate lottery that assigns probability one to this outcome. As usual, a person's preference is formally a complete binary relation on $\Delta(\mathcal{O})$, where $l_1 \succcurlyeq l_2$ means that he/she considers l_1 at least as good as l_2, $l_1 \succ l_2$ that he/she strictly prefers l_1 over l_2, and $l_1 \sim l_2$ denotes his/her indifference between l_1 and l_2.

A probability space is a triple $(S, \Im, P)$ where S is a finite state space, $\Im$ is the algebra of all subsets of S, and P is a probability measure on S. Let $X : S \longrightarrow \mathcal{O}$ be a random variable. The following definition says that one lottery dominates another if we can find some random variables such that the probability of each lottery outcome is the same under each random variable, and in each state of the world the actual realized outcome of one random variable is preferred over the outcome of the other.

Definition 1 *Given a preference $\succcurlyeq$, lottery l_1 monotonically dominates lottery l_2 if there exists a probability space $(S, \Im, P)$ and random variables X_1 and X_2 such that*

$P(s \in S \mid X_1(s) = o) = l_1(o)$ and $P(s \in S \mid X_2(s) = o) = l_2(o)$ for every outcome $o \in \mathcal{O}$,

$$X_1(s) \succcurlyeq X_2(s) \text{ for every state } s \in S \text{ such that } P(s) > 0; \tag{1}$$

and $X_1(\bar{s}) \succ X_2(\bar{s})$ for some state $\bar{s}$, $P(\bar{s}) > 0$.

Note that if outcome o_1 is preferred to outcome o_2 (i.e., $o_1 \succ o_2$), then a lottery l_1 that assigns equal odds to o_1 and an outcome o monotonically dominates a lottery l_2 that assigns equal odds to o_2 and o. Next, we can formally state the monotonicity axiom.

Definition 2 *A preference $\succcurlyeq$ satisfies the monotonicity axiom if $l_1 \succ l_2$ whenever lottery l_1 monotonically dominates lottery l_2 (and $l_1 \succcurlyeq l_2$ when (1) holds).*

Finally, a lottery first-order stochastically dominates another lottery if relative to any particular outcome it places higher weights on outcomes that are more preferred:

Definition 3 *Given a preference $\succcurlyeq$, lottery l_1 first-order stochastically dominates lottery l_2 if for every outcome $x \in \mathcal{O}$,*

$$l_1(o \in \mathcal{O} \mid o \succcurlyeq x) \geq l_2(o \in \mathcal{O} \mid o \succcurlyeq x); \tag{2}$$

and $l_1(o \in \mathcal{O} \mid o \succcurlyeq x) > l_2(o \in \mathcal{O} \mid o \succcurlyeq x)$ for some outcome $x \in \mathcal{O}$.

If an outcome o_1 is preferred to outcome o_2 (i.e., $o_1 \succ o_2$) then a lottery l_1 that assigns equal odds to o_1 and o_2 first-order stochastically dominates a lottery l_2 that assigns probability p less than $1/2$ to o_1 and $(1-p)$ to o_2. A preference $\succcurlyeq$ satisfies the *first-order stochastic dominance axiom* if $l_1 \succ l_2$ whenever lottery l_1 first-order stochastically dominates lottery l_2 (and $l_1 \succcurlyeq l_2$ when (2) holds). The following proposition links first-order stochastic dominance and monotonicity:

Proposition 1 *Given a preference $\succcurlyeq$, lottery l_1 first-order stochastically dominates lottery l_2 if and only if l_1 monotonically dominates l_2. A preference $\succcurlyeq$ satisfies first-order stochastic dominance if and only if it satisfies monotonicity.*

Proof : Assume that l_1 monotonically dominates l_2. Then,

$$l_1(o \in \mathcal{O} \mid o \succcurlyeq x) = P(s \in S \mid X_1(s) \succcurlyeq x) \geq P(s \in S \mid X_2(s) \succcurlyeq x) = l_2(o \in \mathcal{O} \mid o \succcurlyeq x).$$

Moreover, if $x = X_1(\bar{s})$ then

$$P(s \in S|\ X_1(s) \succcurlyeq x) > P(s \in S|\ X_2(s) \succcurlyeq x).$$

Now assume that l_1 first-order stochastically dominates l_2. Let $\bar{\mathcal{O}} = \{o^1, o^2, ..., o^n\} \subseteq \mathcal{O}$ be the joint support of l_1 and l_2 (i.e., the outcomes in $\bar{\mathcal{O}}$ are those that either l_1 or l_2, or both, assign strictly positive probability to). Assume, without loss of generality, that $o^i \neq o^{i+1}$ and $o^i \succcurlyeq o^{i+1}$, $i = 1, ..., n-1$. Let $\{p_1^1, p_1^2, ..., p_1^n\}$ and $\{p_2^1, p_2^2, ..., p_2^n\}$ be the probabilities that l_1 and l_2, respectively, assign to $\{o^1, o^2, ..., o^n\}$.

Let $\bar{\imath}$ be such that $p_1^1 + ... + p_1^{\bar{\imath}} = 1$ and $p_1^{\bar{\imath}} > 0$. Let $k(0) = 0$ and $k(i)$, $i = 1, ..., \bar{\imath}$ be such that

$$p_2^1 + ... + \ p_2^{k(i)} \leq p_1^1 + ... + \ p_1^i \text{ and } p_2^1 + ... + \ p_2^{k(i)+1} > p_1^1 + ... + \ p_1^i. \qquad (3)$$

Let $y(i)$, $i = 1, ..., \bar{\imath}$, be given by $y(i) = k(i) - k(i-1) + 1$. The state space S is given by $S = \{s_{(i,j)},\ \ j = 1, ..., y(i) \text{ and } i = 1, ..., \bar{\imath}\}$. The probability P is such that

$$P(s_{(1,j)}) = p_2^j,\ j = 1, ...k(1);\ P(s_{(1,y(1))}) = p_1^1 - (p_2^1 + ... + \ p_2^{k(i)}); \qquad (4)$$

for $i = 2,...,\bar{\imath}$

$$\begin{aligned} \text{if } y(i) &= 1 \text{ then } P(s_{(i,1)}) = p_1^i; \text{ if } y(i) \geq 2 \text{ then} \\ P(s_{(i,1)}) &= p_2^1 + ... + \ p_2^{k(i-1)+1} - (p_1^1 + ... + \ p_1^{i-1}), \\ P(s_{(i,j)}) &= p_2^{k(i-1)+j},\ \ j = 2, ..., y(i) - 1, \\ P(s_{(i,y(i))}) &= \ p_1^1 + ... + \ p_1^i - (p_2^1 + ... + \ p_2^{k(i)}). \end{aligned}$$

By (3), the probabilities of all states are non-negative. By construction,

$$\sum_{j=1}^{y(i)} P(s_{(i,j)}) = p_1^i \text{ and so, } \sum_{i=1}^{\bar{\imath}} \sum_{j=1}^{y(i)} P(s_{(1,j)}) = 1.$$

Finally, define $X_1(s_{(i,j)}) = o^i$ and $X_2(s_{(i,j)}) = o^{k(i-1)+j}$. By (3), $P(s \in S \mid X_1(s) = o) = l_1(o)$ for all outcomes. By (4), $P(s \in S \mid X_2(s) = o^n) = l_2(o^n)$ if $n = 1, ..., k(1)$. Assume, by induction hypothesis, that $P(s \in S \mid X_1(s) = o^n) = l_2(o^n)$ for all $n \leq$

$k(i-1)$. Let i^* be smallest integer such that $k(i^*) > k(i-1)$

$$P(s \in S|\ X_2(s) = o^{k(i-1)+1}) = P(s_{(i-1,y(i-1))}) + \sum_{i',\ k(i'-1)=k(i-1)} P(s_{(i'-1,1)}) + P(s_{(i,1)}) =$$

$$\Big(p_1^1 + \ldots + p_1^{i-1} - (p_2^1 + \ldots + p_2^{k(i-1)})\Big) + \sum_{i'=\ i,\ldots,i^*-1} p_1^i + \Big(p_2^1 + \ldots + p_2^{k(i-1)+1} - (p_1^1 + \ldots + p_1^{i^*-1})\Big) =$$

$$p_2^{k(i-1)+1}.$$

By construction, $P(s_{(i,j)}) = p_2^{k(i-1)+j}$, $j = 2, \ldots, y(i)-1$. Thus, $P(s \in S|\ X_2(s) = o^{k(i-1)+1}) = l_2(o^n)$ for all $n \leq k(i)$. Therefore, $P(s \in S|\ X_2(s) = o) = l_2(o)$ for all outcomes.

By (3), $k(i) \geq i$ (otherwise $k(i)+1 \leq i \Longrightarrow p_2^1+\ldots+p_2^i \geq p_2^1+\ldots+p_2^{k(i)+1} > p_1^1+\ldots+p_1^i$). Thus, $X_1(s) \succcurlyeq X_2(s)$ for every $s \in S$. For some $\bar{s} \in S$, $X_1(s) \succ X_2(s)$ and $P(\bar{s}) > 0$. Otherwise, $X_1(s) \sim X_2(s)$ in every state with positive probability and, thus, $l_1(o \in \mathcal{O}|\ o \succcurlyeq x) = l_2(o \in \mathcal{O}|\ o \succcurlyeq x)$ for every outcome x.■

Given this connection, we can return to the basic example in our paper. In the setup (see Table 1) there are three outcomes A, B, D. Assume that the decision-maker has strict preferences over them. If the decision maker prefers A over B, then by Definition 1, l_A monotonically dominates l_B (no matter how D is ranked). Similarly, l_A first-order-stochastically dominates l_B. The monotonicity axiom requires the choice of l_A over l_B.

This concept can also be applied to an election, only that for elections it is easier to evaluate lotteries directly by first-order stochastic dominance. Consider an election between policies A and B. A voter, called Dee, believes that if she abstains, then both policies have a chance of being implemented. If she votes for policy A, then she increases the chances of policy A winning the election. Thus, voting for a policy is equivalent to choosing a lottery over another. The outcomes of both lotteries are policies A and B but the probabilities may differ. Lottery l_A assigns higher probability to A than lottery l_B and, therefore, voting for A over B may mean choosing a lottery l_A over a lottery l_B. If Dee were given the actual choice between the two policies, then she chooses the policy she voted for if and only if first-order stochastic dominance is satisfied. By Proposition 1, Dee chooses the policy she votes for if and only if monotonicity is satisfied.

Several decision-theoretic models satisfy first-order stochastic dominance and, therefore, cannot accommodate violations of monotonicity. This includes several models that can accommodate violations of the independence axiom. Even models that can accommodate some violations of monotonicity may fail to accommodate the specific violation of monotonicity that we found in this paper. An example is the theory of warm-glow giving. To see how this works, it is necessary to review warm-glow theory briefly.

Suppose that Dee must choose from a set of lotteries. In warm-glow theory of giving Dee's overall payoff associated with each choice is the usual expected utility plus a warm-glow payoff W that comes from making a choice that Dee finds ethical. Dee gets this payoff if *she* chooses what she finds ethical. If someone else makes the exact same choice for her then she does not get the warm-glow payoff. Usually, it is also assumed that the choice that produces a warm-glow payoff is the option that gives others higher material benefits and, thus, in the theory of warm-glow giving, Dee receives a warm-glow payoff W when she elects to benefit others.

To see how the theory of warm-glow giving can accommodate violations of first-order stochastic dominance consider the following example. Assume that A is a generous outcome that mostly benefits others and that B is an outcome that mostly benefits Dee. Let's say that Dee must choose between lottery l_A where A and B occur with probability p and $1-p$, respectively and lottery B where A and B occur with probability p' and $1-p'$, respectively. Let's assume that $p > p'$ so that the of choice l_A can be interpreted as voting for A. In warm-glow theory the payoffs are as follows:

$$\begin{aligned} l_A: &\quad W + pu(A) + (1-p)u(B) \\ l_B: &\quad p'u(A) + (1-p')u(B) \end{aligned}$$

where $u(A)$ and $u(B)$ are the payoffs of outcomes A and B, respectively. So, if $W + u(A) < u(B)$ then Dee chooses B over A. However, if p and p' are sufficiently close, then Dee votes for A (i.e., chooses l_A over l_B). These choices violate first-order stochastic dominance and, therefore, they also violate the monotonicity axiom.

In our experiment Nature realizes state 1 or state 2 with equal chance. In state 2, a default outcome D is realized for both lotteries. In state 1, outcome A is realized in lottery l_A and outcome B is realized in lottery l_B

Assume that option A is more generous than option B and so lottery l_A is more generous than lottery l_B. Also assume that Dee receives a warm-glow payoff $W > 0$

	state 1	state 2
l_A	A	D
l_B	B	D

if she selects the most generous feasible alternative. Then, her payoffs are

$$\begin{aligned} l_A: &\quad W + 0.5u(A) + 0.5u(D) \\ l_B: &\quad 0.5u(B) + 0.5u(D) \end{aligned}$$

and

$$\begin{aligned} A: &\quad W + u(A) \\ B: &\quad u(B). \end{aligned}$$

With these payoffs, a violation of monotonicity does not occur because if $W + u(A) > u(B)$, then $W+0.5u(A)+0.5u(D) > 0.5u(B)+0.5u(D)$. To obtain a violation of monotonicity where A is chosen over B and l_B is chosen over l_A, it is necessary to assume that Dee's warm-glow payoff for choosing l_A over l_B is less than W. For example, these choices can be accommodated as follows. Assume that Dee's utility for the generous option A is smaller than her utility for B. So, $u(A) < u(B)$. However, she receives a warm-glow payoff W for selecting A such that $W + u(A) > u(B)$. So, she chooses A over B. However, if it is announced that with probability 0.5 outcome D is implemented where part of Dee's wealth is transferred away, then she no longer has a warm-glow payoff for generous giving and her payoffs are just the standard ones

$$\begin{aligned} l_A: &\quad 0.5u(A) + 0.5u(D) \\ l_B: &\quad 0.5u(B) + 0.5u(D). \end{aligned}$$

Thus, she chooses l_B over l_A.

Appendix II – Experimental Procedure and Instructions

The computerized experiment was conducted at the Innsbruck EconLab of the university of Innsbruck. The experiment was programmed and conducted with the software z-tree (29) and participants were recruited via ORSEE (30). An experimental session consisted of two parts, experiment 1 and experiment 2, where experiment 1 was either a *Dictator*- or a *Default*-treatment and experiment 2 was the distributional preference test.[7] In the experiment, participants first got some general instructions, then the instructions for part 1 and after part 1 is finished, they got the instructions for part 2. In the following we give the (translated) instructions for the revealed action condition (concerning part 1, we give the instructions for the *Dictator*- as well as the *Default*-treatment). The instructions for the concealed action differ in the information that is provided at the end of part 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 part 1 and 2 separately and neither that they are informed about the decisions of the respective decision maker. Table 4 summarizes our experimental treatments.

Table 4: Experimental Treatments

Treatment	payoff of receiver in the default	probability of default	number of observations ($\frac{N}{2}$)
Dictator-C	n.a.[1]	n.a.[1]	42
Dictator-R	n.a.[1]	n.a.[1]	62
Default-10-*C*	10	50%	65
Default-10-*R*	10	50%	31
Default-0-*C*	0	50%	64
Default-0-*R*	0	50%	33
Default-5-*C*	5	50%	43
Low-10-*R*	10	2%	46
Low-0-*R*	0	2%	28

[1] n.a. stands for 'not available': There is no default in the *Dictator* treatments.

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

[7] For the concealed action condition of the *Dictator*- and *Default*-treatments, we also varied the order such that experiment 1 was the distributional preference test and experiment 2 the *Dictator*- or *Default*-treatment, respectively. The data shows no order effects.

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 intransparent 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):
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.

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

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):

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		

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 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 III – Regression Results on Anchoring

Table 5 presents the results of Tobit as well as logistic regressions, in which the dependent variable is either the size of the transfer (Tobit) 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* treatment. In one specification of the Tobit and one of the logistic regression, we again include a dummy indicating whether the dictator is “selfish".

Table 5: Regression results on “anchoring”

Dependent variable:	**Transfer** (Tobit)	**Transfer** (Tobit)	**Giver** (Logistic)	**Giver** (Logistic)
Low-10-*R* treatment dummy	+ 0.69	+ 0.47	+0.53	+0.45
	(0.761)	(0.704)	(0.434)	(0.453)
Low-0-*R* treatment dummy	+ 0.09	- 0.16	+0.14	+0.03
	(0.878)	(0.815)	(0.489)	(0.512)
Selfish dummy		-2.46***		-1.25***
		(0.626)		(0.416)
Constant	+0.62	+2.28***	+0.00	+0.85**
	(0.579)	(0.646)	(0.309)	(0.433)
Number of observations	116	116	116	116
Number of left-/right-censored observations	51/0	51/0		
(Pseudo) R-squared	0.0023	0.0381	0.0102	0.0715

Numbers in parentheses indicate standard errors. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

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