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**Working Paper**

## I win it's fair, you win it's not: Selective heeding of merit in ambiguous settings

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*I win it's fair, you win it's not.  
Selective heeding of merit in  
ambiguous settings*

Serhiy Kandul | Olexandr Nikolaychuk

JENA ECONOMIC RESEARCH PAPERS · # 2021-002

I win it's fair, you win it's not. 1  
Selective heeding of merit in ambiguous 2  
settings 3

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**Abstract** 8

One's willingness to accept an outcome or even to correct it de- 9  
pends on whether or not the underlying procedure is deemed legit- 10  
imate. We manipulate the role allocation procedure in the dictator 11  
game to illustrate that this belief is not independent of the outcome 12  
and is self-serving in its nature. Our findings suggest that there may 13  
be some positive level of dissatisfaction with virtually any social out- 14  
come in the populace without there being anything wrong as far as the 15

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underlying procedure. We also discuss the perceptions of fairness and merit as potential drivers of the observed behavioral phenomenon.

**Keywords:** fairness, entitlement, merit, redistribution, procedural preferences, dictator game.

**JEL codes:** D63, D91.

## Introduction

There is a plethora of resource allocation procedures governing our everyday interactions that are conventional or in other words, arbitrary in their nature. As far as final outcomes, often there is a winner and a loser. And quite often, the loser is suspicious of the allocation procedure while the winner is not and yet if the parties were to switch sides, so would their attitudes. Some high profile examples include legal disputes where the losing party requests another jury, sporting events where the losing party calls for an instant replay, and election results where the losing party demands a vote recount. In late 2016, Green Party's presidential candidate Jil Stein raised 4 million USD to recount more than 2.9 million votes in the states of Michigan, Pennsylvania and Wisconsin [28]. Four years later, the incumbent Donald Trump questioned the 2020 election results in six states, spending 3 million USD to recount the votes in Wisconsin alone [27]. What should we make of those reactions?

One obvious explanation of such behavior is that the losing party may have reasons to believe that there has been an error or that the procedure has been tempered with. An alternative explanation is that one's attitude

towards an allocation procedure depends on whether the final outcome is in 39  
their favor or not, which is brought about by the very fact that the procedure 40  
is arbitrary to begin with. Since it is often non-trivial to detect objective 41  
errors as such, some might even fool themselves into believing the former 42  
while being truly motivated by the latter. 43

This distinction in motivation has far reaching policy consequences since 44  
minimizing true errors calls for ancillary infrastructure (e.g., video assisted 45  
refereeing in sports) whereas questioning the procedure purely on account of 46  
one's dissatisfaction with the outcome calls for restriction of opportunity of 47  
such questioning (e.g., increasing the margins that cannot be disputed). 48

Here, we present an experiment where we introduce an extremely sim- 49  
ple and fair (in ex-ante sense [42]) allocation procedure while excluding the 50  
possibility of mistakes, computational errors or cheating of any kind (which 51  
could result in negative reciprocity etc.). The participants do not choose the 52  
procedure or agree to its implementation in any way but are free to incor- 53  
porate it into their decision process or ignore it completely. This makes the 54  
question of merit of the procedure an open one, and it is then up to the 55  
participants to judge. 56

## Related literature 57

In principle, people have a reasonably good intuitive understanding of merit 58  
since in everyday life, it tends to be associated with effort. In experimental 59  
literature, the concept is typically studied by manipulating the procedure be- 60  
hind the initial allocation in the dictator game. In a classic example [12], the 61

dictators share significantly less with their counterparts if they actually have 62  
to earn their endowments. The conclusion is that exerting effort changes the 63  
perception of merit (or relative merits of the parties involved) that justifies 64  
keeping more for oneself. 65

The intuition is tested further in the context of stealing [16] to reveal 66  
that the participants refrain from helping themselves to the money of those 67  
who have worked for it. When both players are allowed to compete over 68  
the final allocation by exerting effort [19], they are more than eager to use 69  
opportunities to earn merit for themselves. 70

A related line of research investigating the perceptions of fairness and 71  
responsibility shows that people tend to distinguish between factors within 72  
individual control and those beyond it [8–10,29] while potentially overweigh- 73  
ing the importance of the former [7]. More generally and perhaps even more 74  
importantly, perceptions of outcomes are separable from perceptions of pro- 75  
cedures that bring those about [17,25]. 76

However, effort need not be ‘real’ in the aforementioned sense to affect 77  
the attribution of merit. The apparent distinction between commission and 78  
omission often observed in moral psychology illustrates that outcomes caused 79  
by action are judged harsher than the exact same outcomes caused by in- 80  
action [3, 14, 35, 36, 39]. It seems that the very act of applying oneself can 81  
already warrant some merit even though inaction need not be less effortful. 82

The perception of merit can also be affected by such innocuous factors 83  
as the result of a random draw. There is ample evidence in the literature 84  
[26,30,31] that refusing to share is perceived less harmful than taking despite 85  
the fact that the difference between the two actions in the context of the 86

dictator game is that of an arbitrary reference point resulting from an initial 87  
allocation brought about by chance. 88

Unfortunately it is not always possible to judge the true merit of one's 89  
action in practice. We posit that in the absence of clear causal links between 90  
actions and outcomes, such judgments are inherently self-serving in that 91  
people are willing to grant merit to those actions (more generally, procedures) 92  
that serve their personal interest but are not willing to do so otherwise. As 93  
a result of such *selective heeding*, the haves and have nots are likely to hold 94  
opposing views on the same outcome (and consequently, on the underlying 95  
procedure) and yet if the positions were to be flipped around, so would be 96  
the views. In the end, we expect selective heeding to affect one's preferences 97  
over the (re)distribution of outcomes. 98

To test this hypothesis, we construct a series of situations where both par- 99  
ties perform equally effortful actions that result in an asymmetric outcome 100  
but do not affect it in the causal sense. The benefactor is then free to assign 101  
(relative) merit to their action, which is elicited via their redistribution pref- 102  
erences. While keeping it ex-ante fair [1, 42], we manipulate the link between 103  
the actions and outcome to find differences in the participants' willingness to 104  
make monetary transfers that are likely to be mediated by their perceptions 105  
of fairness. 106

By considering distributive choices in situations where the interpretation 107  
of merit as the source of inequality is equivocal, our work adds to the lit- 108  
erature on moral 'wobble room' [13] as well as to the general discussion of 109  
social preferences [2, 11, 18]. Since we manipulate the role allocation proce- 110  
dure in the dictator game, it also falls under the rubric of procedural prefer- 111

ences [4, 22, 23, 41]. 112

## Experimental design 113

The experiment builds upon the dictator game where we introduce a role 114  
allocation procedure as an additional stage that precedes it. 115

In the classic dictator game [21], there is a monetary endowment (essen- 116  
tially, ‘manna from heaven’) to be shared between two anonymous players. 117  
One of the players (the dictator) decides how much of the endowment to 118  
*transfer* to the other, passive player (the recipient) while keeping the rest for 119  
themselves. The roles are assigned randomly and the game is played only 120  
once, which makes it a popular means of investigation into the human nature 121  
of self-interest, perceptions of fairness and merit [15]. 122

The aforementioned formulation of the game – i.e., with the random role 123  
allocation – constitutes our baseline treatment. In the other two treatment 124  
conditions, the participants are presented with what we refer to as a necklace 125  
consisting of 19 beads<sup>1</sup> (see Fig. 1). One of the beads is randomly selected 126  
by the computer and both participants are asked to guess it. In treatment 127  
WIN, the better guess (i.e., the one closer to the selected bead, in either 128  
direction) results in the assignment of the dictator role whereas in treatment 129  
LOSE, the better guess results in the assignment of the recipient role<sup>2</sup>. 130

In our opinion, it is self-evident that the design allows for no (extra) 131  
effort that either participant could exert in order to affect their chance of 132

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<sup>1</sup>We wanted to have a relatively large number of those while at the same time, mini-  
mizing scope for focal points implied by visual symmetry, familiarity etc.

<sup>2</sup>In case of a tie, the roles are assigned randomly. This happened only twice during the  
course of the experiment



Figure 1: Role allocation procedure in treatments WIN and LOSE. Asterisk (\*) denotes the bead randomly selected by the computer. Between the two participants, the *better* guess is assigned the role of the dictator (D) in treatment WIN (a), and the role of the recipient (R) in treatment LOSE (b). The better guess is the one closer to the selected bead, in either direction. There were 19 beads to choose from in the actual experiment.



being assigned the dictator role.

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In each treatment condition, the dictator is given 50 indivisible experimental currency units (ECU)<sup>3</sup> to distribute between themselves and the recipient. We employ what is known as the strategy method [37] to elicit the transfer decisions from both players in the role of the dictator and then use the role allocation procedure to determine the final payments for a given pair (if anything, this makes the design more conservative [6]).

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From the participant perspective, the general flow is as follows: (i) learn the rules of the dictator game and role allocation procedure; (ii) guess the bead selected by the computer (unless in the baseline condition); (iii) make the transfer decision in the role of the dictator; and (iv) find out the role assignment and the resulting final payment. Immediately after the transfer decision (i.e., before the roles and consequently, payments are revealed), the participants are asked to evaluate the role allocation procedure in terms of

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<sup>3</sup>We used an exchange rate of 10 ECU to 1 EUR.

fairness and merit (see What drives selective heeding?). 147

From our conjecture of selective heeding, it follows that the role allocation 148  
procedure is taken into account by the dictators when it is in their favor and 149  
disregarded otherwise. This leads to the following two hypotheses: 150

(i) average transfer in treatment WIN is lower than the baseline; 151

(ii) average transfer in treatment LOSE is not different from the baseline. 152

Findings in line with these hypotheses would support our conjecture that 153  
people tend to attribute merit to their irrelevant actions but not to irrelevant 154  
actions of others. 155

## Results 156

The experiment was conducted with 130 participants at the economics lab- 157  
oratory of the Friedrich Schiller University Jena. It was programmed in 158  
z-Tree [20] and the recruitment was done with the help of ORSEE [24]. 159

The participants were allocated to the treatment conditions randomly 160  
and interacted with each other using computer terminals to preserve their 161  
anonymity. The game was played once and no repeat participation was 162  
allowed. All treatment conditions were run concurrently over 9 sessions. 163  
Each session concluded within 30 minutes, and the average payment was 164  
5.0 EUR (including a show-up fee of 2.5 EUR). 165

One participant was excluded from the analysis, which had no qualita- 166  
tive effect on the results<sup>4</sup>. The final sample of 129 observations includes 81 167

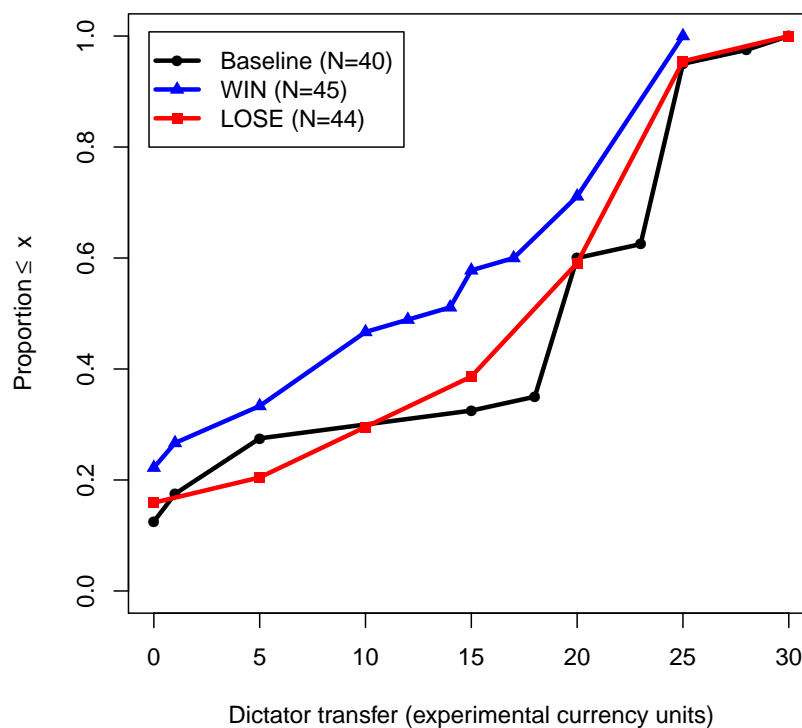
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<sup>4</sup>We suspect that they were not properly motivated by our incentive scheme since they

females, 74 undergraduates, and 32 Business Administration and Economics 168  
 majors. The average participant age is 24.6 years (SD 3.6) and the average 169  
 laboratory experience is 7.6 experiments (SD 6.1). 170

In total, we have 40, 44 and 45 observations in the baseline, LOSE and 171  
 WIN conditions, respectively. The empirical distribution functions of the 172  
 dictator transfers are presented in Fig. 2. 173

Figure 2: Empirical distribution function of the dictator transfer by treat-  
 ment condition.



As one can see, the empirical distribution function of the dictator transfer 174  
 in the WIN condition is stochastically dominated by the other two whereas 175  
 transferred 80% of their endowment as the dictator. Among other things, they were 42  
 years old and were not a student.

no such claim can be made as far as the comparison between the baseline 176  
and LOSE conditions. The resulting average transfers are equal to 16.9, 17.1, 177  
and 13.1 (ECU) in the baseline, LOSE, and WIN conditions, respectively. 178

To test for the statistical significance of the observed differences between 179  
the treatment conditions, we estimate the specification given below using 180  
ordinary least squares and subsequently perform the t-test on the coefficient 181  
estimates at the respective treatment variables: 182

$$y_i = \alpha + \beta \times WIN_i + \gamma \times LOSE_i + \sum_j \{\delta^j \times CNTRL_i^j\} + \epsilon_i,$$

where  $i$  indexes the participant;  $WIN_i$  and  $LOSE_i$  equal one if the participant 183  
is assigned to the respective treatment condition and zero otherwise; 184  
 $\{CNTRL_i^j\}$  represents a set of control variables capturing the effects of age, 185  
gender, educational background and previous experience in laboratory ex- 186  
periments. 187

The regression results are summarized in Table 1. At the significance 188  
level of 5%, we reject the null hypothesis that the average transfer in the 189  
WIN condition is not larger than the baseline (one-tailed  $p$ -value of 0.031 or 190  
0.039, with or without controls)<sup>5</sup> and at the same time, cannot reject the 191  
null hypothesis that the average transfer in the LOSE condition is equal to 192  
the baseline (two-tailed  $p$ -value of 0.582 or 0.946, with or without controls). 193

As additional evidence, consider the effect sizes of the WIN and LOSE 194  
manipulations. Using Cohen's  $d$  as a quantitative measure results in the ac- 195

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<sup>5</sup>According to our directional hypothesis. See Experimental design.

Table 1: Regression results (s.e. in parentheses). Dictator transfer measured in experimental currency units, perceptions of fairness and merit measured on a 1–7 scale. Also, estimated effect sizes relative to the baseline.

|                         | Transfer  |          | Fairness |         | Merit    |          |
|-------------------------|-----------|----------|----------|---------|----------|----------|
| Intercept               | 16.900*** | −27.483  | 4.650*** | −1.393  | 3.725*** | 7.501*** |
|                         | (1.551)   | (28.337) | (0.324)  | (6.118) | (0.142)  | (2.666)  |
| WIN                     | −3.789*   | −3.943*  | 0.817*   | 0.818*  | −0.369*  | −0.358*  |
|                         | (2.132)   | (2.094)  | (0.445)  | (0.452) | (0.195)  | (0.197)  |
| LOSE                    | 0.145     | −1.175   | 0.191    | 0.046   | 0.548*** | 0.568*** |
|                         | (2.143)   | (2.132)  | (0.447)  | (0.460) | (0.196)  | (0.201)  |
| Controls                | −         | +        | −        | +       | −        | +        |
| R-squared (N=129)       | 0.035     | 0.140    | 0.029    | 0.074   | 0.158    | 0.202    |
| Cohen’s <i>d</i> : WIN  | 0.382     |          | 0.389    |         | 0.385    |          |
| Cohen’s <i>d</i> : LOSE | 0.015     |          | 0.092    |         | 0.697    |          |

Significance (two-tailed): \*\*\*  $\equiv p < 0.01$ ; \*\*  $\equiv p < 0.05$ ; \*  $\equiv p < 0.1$ .

Controls: age (quadratic), gender, undergraduate, Business Administration or Economics major, laboratory experience (quadratic).

tual estimates of 0.382 and 0.015, which we interpret as practically important 196  
and practically unimportant, respectively. 197

Therefore, we find empirical support for both of our research hypotheses 198  
– i.e., the participants exhibit *selective heeding* as far as judging the merit of 199  
one’s action. If they are assigned the role of the dictator through an arbitrary 200  
procedure that favors them, they tend to make lower transfers than those 201  
who are assigned the role in a completely random manner. However, if an 202  
arbitrary procedure favors the other player instead, the dictator transfers do 203  
not reflect that. 204

## What drives selective heeding?

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In anticipation of the above findings, we measured the participants' perceptions of fairness of the role allocation procedure they were facing (hereafter, *fairness*) as well as their perceptions of how much the designated person deserved to determine the payoff allocation relative to the not designated person (hereafter, *merit*; see Supplementary information).

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The perception of fairness was measured on a 1–7 scale where 1 implies an absolutely unfair procedure and 7 implies an absolutely fair procedure as far as the chances of both participants to determine the payoff allocation. The perception of merit was measured on a 1–7 scale where 1 implies that the designated player deserves to determine the payoff allocation more than the other player, 7 implies the opposite, and 4 implies that the two deserve it equally.

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Our conjecture was that those perceptions could be responsible for driving the differences in the transfers.

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To interpret the data, we follow the same protocol as before only switching out the dependent variable as necessary. The empirical distributions are presented in Fig. 3–4 and the regression results are summarized in Table 1.

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As far as the perception of fairness, the average evaluation is equal to 4.65, 4.84 and 5.47 in the baseline, LOSE, and WIN conditions, respectively. At the significance level of 5%, the participants in the LOSE condition do not perceive the role allocation procedure to be any different from the baseline (two-tailed  $p$ -value of 0.920 or 0.670, with or without controls). In the WIN condition, however, the role allocation procedure is perceived to be signifi-

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Figure 3: Perception of fairness of the role allocation procedure by treatment condition. 1 implies an absolutely unfair procedure while 7 implies an absolutely fair procedure as far as the chances of both participants to determine the payoff allocation.

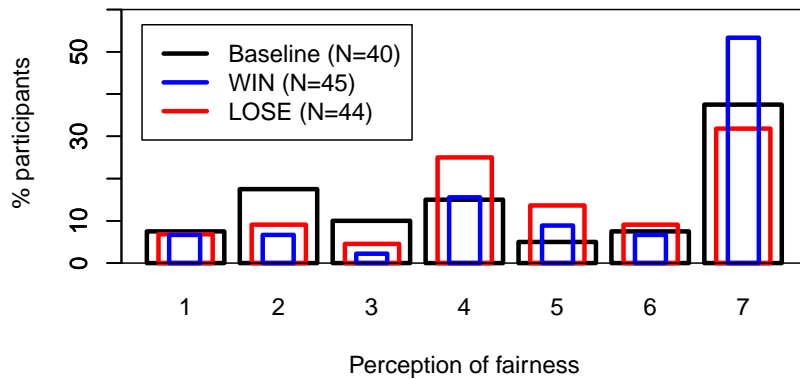
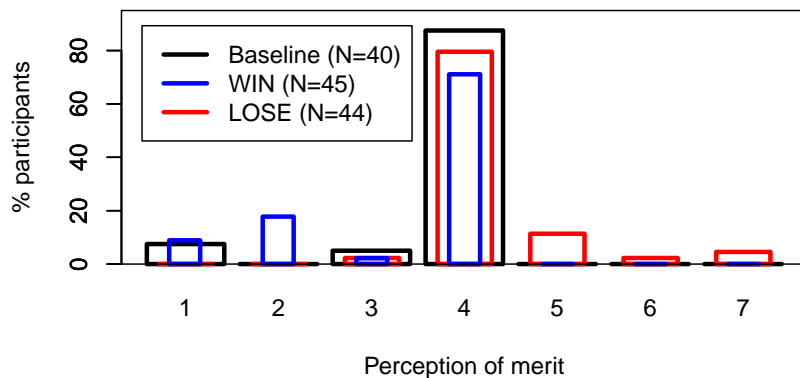


Figure 4: Perception of merit as far as the role allocation procedure by treatment condition. 1 implies that the designated person deserves to determine the payoff allocation more than the other, 7 implies the opposite, and 4 implies that the two deserve it equally.



cantly fairer (one-tailed  $p$ -value of 0.037 or 0.034, with or without controls). 229

These findings are in line with the effect our manipulations have on the actual monetary transfers, which suggests that selective heeding may be driven 230  
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by one's perception of fairness. The Cohen's  $d$  estimates of 0.092 and 0.389, 232  
which we interpret as practically unimportant and practically important, 233  
respectively, provide further support. 234

As far as the perception of merit, the average evaluation is equal to 3.73, 235  
4.27 and 3.36 in the baseline, LOSE, and WIN conditions, respectively. At 236  
the significance level of 5%, the participants believe that the designated per- 237  
son deserves to determine the allocation significantly less in the LOSE condi- 238  
tion (two-tailed  $p$ -value of 0.005 or 0.006, with or without controls) and more 239  
so in the WIN condition (one-tailed  $p$ -value of 0.036 or 0.030, with or without 240  
controls). With the respective Cohen's  $d$  estimates of 0.697 and 0.385, both 241  
effects are also practically important. 242

Finding that the participants perceive the designated person to be less 243  
deserving of the dictator role in the LOSE condition is particularly interesting 244  
here. Also, note that a value of 4.27 implies that the not designated person 245  
deserves the dictator role more than the designated person. This suggests 246  
that the average transfer not only should be larger than the baseline but also 247  
exceed 50% of the endowment to allocate. 248

Overall, we conclude that the observed differences in the dictator transfers 249  
(i.e., selective heeding) are likely to be driven by changes in one's percep- 250  
tion of fairness of the role allocation procedure and not by changes in one's 251  
perception of the relative merits of the parties involved. 252



## Discussion

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We examine a modified version of the dictator game where the player roles 254  
are assigned by an ex-ante fair procedure that is linked to the participant ac- 255  
tions but in effect is completely random. This enables the eventual dictator 256  
to entertain arbitrary beliefs as far as the relative entitlements to the en- 257  
dowment. Subtle modifications to the procedure illustrate that these beliefs 258  
are not only dependent on the outcome but are also inherently self-serving 259  
in that the participants tend to merit their own actions if the result of the 260  
random draw serves their interest but refuse to merit the actions of others if 261  
it doesn't. 262

We refer to this asymmetry as selective heeding and investigate two po- 263  
tential driving forces behind it such as the perceptions of fairness and merit. 264  
It appears that the observed differences in the dictator transfers are consis- 265  
tent with the changes in the perception of fairness but not consistent with 266  
the changes in the perception of merit even though the latter are in line with 267  
the moral imperatives dictated by our manipulations. 268

Our findings add to the literature on self-centered behavior. As long as 269  
people can find situational excuses they tend to relax their moral standards 270  
and consequently, behave more selfishly [13, 32, 38]. Apparently, 'winning' 271  
a game of pure chance is as good of a reason as one could possibly have. 272  
Moreover, there seems to be some inherent asymmetry to the logic: I win it's 273  
fair, you win it's not. 274

The self-centered phenomenon that we document here goes beyond the 275  
notions of 'self-serving bias' [5,34] or 'attribution bias' [33,40]. In the absence 276

of clear causal links between actions and outcomes it may be all too easy 277  
to overestimate one's own merit or underestimate somebody else's. One 278  
corollary of this is that there may be some positive level of dissatisfaction with 279  
virtually any social outcome in the populace without there being anything 280  
wrong as far as the underlying procedure. 281

When the vote recount of the 2016 U.S. presidential election revealed no 282  
major issues, numerous debates started on whether or not the very possibil- 283  
ity of a recount needs a revision. Proponents of more stringent recount rules 284  
suggest, among other things, to limit the possibility of initiating a recount to 285  
those candidates who fall behind by a rather small margin. Their opponents 286  
claim that a democratic society should not seek to restrict the possibility 287  
at all, advocating that the currently observed practices remain intact. In 288  
2020, Trump's lawyers attempted to contest some of the very same election 289  
practices that had been used in 2016. Our findings provide a valuable con- 290  
tribution to the discussion demonstrating that losing candidates may indeed 291  
seek to revise election outcomes on false pretenses. 292

## **Competing interests statement** 293

Both authors declare no competing interests. 294

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## Supplementary information 414

### Fairness and merit elicitation 415

To elicit the participants' perceptions of the fairness of the role allocation 416  
procedure as well as their perceptions of how much the selected person de- 417  
served to determine the final allocation, the following two questions were 418  
posed. 419

As far as the chances of both participants to determine the actual 420  
transfer, is the procedure fair? 421

- Disagree completely
- 
- 
- 
- 
- 
- Agree completely

422

Do you think that the selected person deserves to determine the actual 423  
transfer as much as the non-selected person? 424

- The selected person deserves it more
- 
- 
- The two deserve it equally
- 
- 
- The non-selected person deserves it more<sup>6</sup>

---

<sup>6</sup>Due to a typing error, this option was identical to the other extreme in the baseline. However, we believe that the participants were able to see through that due to the following observations: (i) the very way the question is phrased implies a particular ordering; (ii) there is no considerable increase in the variance; and (iii) there wasn't once a question raised to clarify the issue.

425

## Experiment instructions

426

{*All treatments*} 427

### Experiment Instructions

428

Welcome and thank you for participating in this experiment. Please 429  
 switch off your mobile phones and remain silent. If you have any questions, 430  
 please raise your hand and experimenter will answer you privately. 431  
 For your participation you will receive a show-up fee of 2.50 EUR. During 432  
 the experiment you can earn additional money. Your additional earnings 433  
 depend on your own decisions, decisions of other participants as well as on 434  
 chance. During the whole experiment your anonymity is guaranteed. Your 435  
 additional earnings will be expressed in ECU (experimental currency unit) 436  
 that will be converted into EUR at the end of the experiment using the 437  
 following exchange rate: 438

1 ECU = 0.10 EUR. 439

### Structure of the Experiment

440

At the beginning of the experiment all participants will be matched into 441  
 pairs. You stay within the same pair throughout the experiment. 442

{*Baseline only*} 443

The experiment will continue as follows. 444

The participants in the pair will be randomly assigned one of the two 445  
 roles: **Participant A** or **Participant B**. Each role is equally likely to be 446  
 selected. 447

{*Treatments WIN and LOSE only*} 448

The experiment consists of two parts. 449

In the first part of the experiment, you and the other person in your pair 450  
 will be presented with a 'necklace' consisting of several beads. One of these 451  
 beads will be randomly selected by the computer. 452

Each of you will be asked to guess which bead it was. We will then com- 453  
 pare your guesses and assign the role of **Participant A** to the person whose 454  
 guess was closer to the bead selected by the computer (in any direction). 455  
 The other person will be assigned the role of **Participant B**<sup>7</sup> 456

Consider the following examples, where the computer randomly selects 457  
 the bead labeled as \*, one person selects the bead labeled as X, and the 458  
 other person selects the bead labeled as Y: 459



The roles will then be assigned in the following way: 460

- (a) Person X will be assigned the role of Participant A, and person Y will 461  
 be assigned the role of Participant B. 462

---

<sup>7</sup>In case of a tie, the roles will be assigned randomly (each role is then equally likely to be selected).

(b) Person X will be assigned the role of Participant B, and person Y will  
 be assigned the role of Participant A.

{ All treatments;

'I' = 'A' in the baseline and WIN, and 'B' in LOSE;

'J' = 'B' in the baseline and WIN, and 'A' in LOSE; }

[In the second part of the experiment,]<sup>{WIN & LOSE}</sup> **Participant I** receives  
 an endowment of 50 ECUs and decides how much from this amount he would  
 like to send to Participant J. Participant I can specify any amount between  
 0 and 50 ECU in steps of 1 ECU.

The amount specified by Participant I will be deducted from this endow-  
 ment and transferred to Participant J, and the rest will remain for Participant  
 I.

Participant J does not make any decisions in this [situation<sup>{Baseline}</sup>; part  
 of the experiment<sup>{WIN & LOSE}</sup>].

Before you know the result of assigning the roles, you will be asked to  
 decide as Participant I. After you and the other person in your pair have made  
 your decisions in the role of Participant I, the computer will [randomly select  
 one of you to be the actual Participant I<sup>{Baseline}</sup>; assign the roles according  
 to your guesses in the first part of the experiment<sup>{WIN & LOSE}</sup>] and then  
 implement the respective decision for your pair.

At the end of [this decision situation<sup>{Baseline}</sup>; the second part<sup>{WIN & LOSE}</sup>],  
 you will be informed about the following: the role you have been assigned  
 to, the transfer implemented for your pair, as well as your final payment for  
 participating in the experiment.

We will conclude the experiment by asking you to fill out a short ques- 487  
tionnaire. 488

If you have read these instructions carefully and do not have any further 489  
questions, please click the 'Ready' button on your screen. We will then test 490  
your understanding of the procedure with a few basic questions. As soon as 491  
everybody has answered those questions correctly, the experiment will begin. 492

Good luck! 493

## IMPRESSUM

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