

Schmidt, Robert J.; Trautmann, Stefan T.

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Implementing (un)fair procedures? Favoritism and process fairness when inequality is inevitable

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Process Fairness when Inequality is Inevitable

Robert J. Schmidt and Stefan T. Trautmann

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Implementing (Un)fair Procedures? Favoritism and Process Fairness when Inequality is Inevitable

Robert J. Schmidt¹ & Stefan T. Trautmann^{*}

University of Heidelberg, Germany

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Abstract

We study allocation behavior when outcome inequality is inevitable but a fair process is feasible, as in selecting one person from several candidates for a job or award. We show that allocators may be influenced by inappropriate criteria, impeding the implementation of a fair process. We study four interventions to induce process fairness without restricting the allocator's decisions: Increasing the transparency of the allocation process; providing a private randomization device; allowing the allocator to delegate to a public randomization device; and allowing the allocator to avoid information on inappropriate criteria. All interventions except transparency have positive effects, but differ substantially in their impact.

KEYWORDS: equality, procedural justice, discrimination

JEL CLASSIFICATIONS: D63, D73, J70

^{*}Corresponding author: Alfred-Weber-Institute for Economics, University of Heidelberg, Bergheimer Str. 58, 69115 Heidelberg, Germany, Phone: +49 6221 54 2952, Fax: +49 6221 54 3592; email: trautmann@uni-hd.de. We thank Emin Karagözoğlu as well as seminar audiences in Frankfurt, Heidelberg, Bremen, Vienna, Mannheim, Tilburg, Bocconi, Göttingen, Ulm, the Max-Planck Institute in Bonn, the ESA World Meeting in San Diego, the ESA European Meeting in Vienna, the HeiKaMax in Karlsruhe, the HeiKaMaxY in Mannheim, the Scandinavian Meeting in Gothenburg, and the Sabe/Iarep Meeting in London for very helpful suggestions.

1. Introduction

Consider a committee's choice between two candidates for a job. Both score equally good on various hard measures, thus there is little to guide the decision between the candidates. Other aspects can become influential at this stage, affecting the outcome of the decision. Such aspects may include the candidates' gender, race or political views. Importantly, the influence of such aspects may be undesirable to the committee itself, or to the organization for which the committee serves to select the candidate. They may still exert influence on the decision. Although the organization and the committee may thus aim to implement a fair selection process, biased outcomes can obtain and render the process unfair ex-post.

We show that when there are no clear economic criteria like performance or effort that could guide an allocation decision, softer criteria (in our case, overlap in political attitudes between allocator and recipients) have an overwhelming influence on how funds are allocated among two people. Despite its prevalence, we argue that the allocation on the basis of the political attitudes is not perceived as an appropriate criterion even by those who apply it. Moreover, irrespective of the decision makers' perception of the criterion, we take a prescriptive perspective and aim to reduce the influence of the political attitude information on the allocation outcome. To this end, we introduce a set of interventions in our setup and observe whether the adjusted decision environment is able to reduce the prevalence of allocation according to political views. In terms of the above example, even if some committee considers aspects like gender, race or politics acceptable criteria absent clear differences between the candidates, the organization behind it may not do so, and may wish to reduce the incidence of its use.

Our experimental setup puts decision makers in the position to allocate two outcomes, €8 and €2, among two recipients. Similar to the case of selecting candidates for a job or to allocating an indivisible good or service to people in need (e.g., a donor organ among to people in need of a donation), there will be ex-post or outcome inequality. Absent clear criteria why one recipient should receive the more favorable payment, outcome inequality is typically assumed to be aversive (e.g. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). However, if outcome fairness is not attainable, a fair process may substitute for fair outcomes (Bolton et al., 2005; Trautmann, 2009; Trautmann and Wakker, 2010; Krawczyk, 2011; Andreozzi et al., 2013; Saito, 2013; Trautmann and van de Kuilen, 2016). In particular, randomly allocating the high and the low outcome can be interpreted as a fair process in our setting (Broome, 1990; Konow, 2003). Empirically, random allocation has been shown to be an important mechanism employed by decision makers (Bastek et al., 2018; Brock et al., 2013; Miao and

Zhong, 2018; also Dwenger et al., 2016, in contexts other than fairness). In practical applications, an equal representation of candidates or recipients in terms of sex, race or political orientation in the absence of clear differences in any hard criteria can be interpreted as evidence of a fair process. Fairness perceptions become especially important from the perspective of the decision maker if recipients can react by rewarding or punishing fair or unfair processes (suing, filing complaint, recommending). Then, strategic and non-strategic reasons for implementing fair procedures become important to the decision maker. We implement the possibility for reward and punishment in our setup.

In our allocation decision, there are no easily justifiable criteria (such as effort etc.) for which recipient should receive the higher payment. At the same time, the decision maker has available a profile of the two recipients' political attitudes (while recipients are uninformed about others' political profiles). We find that in our baseline condition, about 90% of the high payoffs go to the recipient whose political profile is closer to that of the allocator. We then introduce four interventions with the aim to reduce the incidence of such favoritism. Each intervention condition deviates in one aspect from our baseline allocation condition: In *Transparency* we make all three political profiles in a group visible to each group member. Thus, favoritism becomes observable. In *Private Lottery*, the decision maker allocates probabilities of receiving the high payoff to the two recipients, instead of outcomes directly. Recipients are aware of the procedure and learn about the resulting allocation, but do not know the exact probabilities chosen by the decision maker. In *Public Delegation*, the decision maker can publicly delegate the decision to a 50-50 random device; in three subcases, delegation is either for free or costly. Finally, in *Information Avoidance*, the decision maker can decide not to see the information about the recipients' political attitudes. In two subcases, whether the decision maker avoided information or not before making her allocation decision is either observed or unobserved by the recipients.

An important feature of all four interventions is that a decision maker who wants to allocate on the basis of the recipients' political attitudes, has the freedom to do so. That is, the interventions are not trivial in the sense of simply restricting the use of the information or stipulating a decision, which would be unrealistic and undesirable in practice: decision makers will typically hold specialized information relevant to the selection process, which the organization wants them to be able to use in determining the best allocation. Instead, the interventions provide an institutional environment aimed at reducing favoritism.

In terms of the incidence of favoritism, we find that transparency has no effect. A private lottery works well, with the fair lottery being the modal choice to allocate the payments. Similarly, public

delegation is taken up by a majority of decision makers if for free; costly delegation is still taken up by a substantial share of decision makers. Information avoidance is taken up by 12.5% of the decision makers if privately known only, and by 52.3% when it is publicly known. Conditional on acquiring information, decision makers are equally prone to allocate to the politically closer recipient as in Baseline.

We present the Baseline experimental design, procedures and empirical measures in Section 2. Section 3 gives the results of the Baseline treatment. In Section 4 we define the interventions and report their effects on the decision makers' allocation behavior. Data on decision makers' beliefs are discussed where they help interpreting observed behavior. Data on fairness perceptions, the role of politics, and the behavior of the recipients aid our understanding of how favoritism affects the allocation decisions and how the interventions work. We present these results in the appendix, and refer to them in the main text where needed. Section 5 discusses our results in the context of the related literature, and concludes with prescriptions for practical settings.

2. Baseline Allocation Game and Experimental Design

2.1. Experimental Paradigm

The experimental paradigm is a simple 3-person (=1 group) allocation game in which the decision maker (DM) allocates the two payments €8 and €2 to two recipients labelled A and B. Subsequently, after learning about the allocation, each recipient can costly reward or punish the DM by paying €0.10 for each €0.50 added or subtracted from the DM's endowment of €5 (which equals the average payoff of the recipients). Each recipient can reward or punish up to a maximum of €2 (at maximum cost of €0.40). Thus, the DM may end up with earnings between €1 where both recipients maximally punish, and €9 where they both maximally reward. Recipients may choose to neither punish or reward, and they make their decisions independently and simultaneously. Reward or punishment costs are deducted from the recipients' earnings. While the DM makes her allocation decision, recipients provide their non-incentivized beliefs about the allocation (high versus low payment); while the recipients make their decisions, the DM provides non-incentivized beliefs about each recipients' reward or punishment in terms of money destroyed/added. After all decisions are made, but before the DM is informed about recipients' choices, all three group members judge the fairness of the allocation process and the allocation outcome regarding the allocation of the €8 and €2 payments to recipients A and B (exact wording in Appendix A1). The DM is not informed about the recipients' actions until after the

questionnaire to prevent her fairness judgments reflect the recipients' views as revealed by their rewarding decisions.

At the very beginning of the experiment, before receiving any instructions for the allocation game and before knowing their role in the game, subjects are asked five questions regarding their political attitudes. We informed subjects that their answers might be shown to other participants during the experiment (while maintaining full anonymity). All questions had binary answer possibilities and we instructed subjects to choose the answer options that best describes their attitudes. The five questions concern (i) their general political orientation ("rather left wing" or "rather conservative"), and whether they support the (ii) unlimited inflow of refugees, (iii) female quotas in organizations, (iv) active euthanasia and the (v) exit from nuclear energy. The original wording is provided in Appendix A1. In the Baseline treatment, before making her allocation decision, the DM is shown the political profile of recipients A and B. In contrast, the recipients do not learn about the profile of any other player. All players in the group are aware of the information available to other players at each stage of the game.

In appendix A3, we show that there is substantial variation of political attitudes in our total sample and also at the level of the 3-payer groups: on average, the two recipients within a group disagree on 2.03 of the 5 items of the political attitude questionnaire. We are interested in how similarity in attitudes affects the DM's allocation in the absence of any other relevant information. We therefore define the following *similarity* or closeness measure. We compare the answers of recipient $j = \{A, B\}$ with the answers of the DM and denote the number of items on which recipient j agrees with the allocator as S_j . Thus, higher S_j indicates higher similarity between the allocator and recipient j . We then define the *relative similarity* $S_{AB} = S_A - S_B$. Thus, if $S_{AB} > 0$ recipient A is more similar to the DM, and if $S_{AB} < 0$ recipient B is more similar to the allocator. For $S_{AB} = 0$ the similarity of recipients with the allocator is identical according to our definition. Note that $S_{AB} = 0$ implies only that recipients agree with the allocator on the same number of items. It does not necessarily imply that they have identical political profiles, and the exact items they agree on may differ between recipients. The average of the absolute value of the relative similarity measure S_{AB} across all groups in our experiment is 1.15 (see Appendix A3).

2.2. Treatments and Hypotheses

The allocation game as described above forms our *Baseline* condition.¹ We hypothesized that allocation decisions are affected by the political attitude information provided to the DM. In Section 3 we show that this was indeed the case. In Section 4 we present four interventions for which we hypothesized that they reduce the incidence of favoritism. Importantly, in each of the intervention treatments, the DM in principle has the possibility to allocate according to favoritism just as in the Baseline treatment. Our focus in the current study is prescriptive. That is, we assume that either (i) the DM would like to implement a fair allocation process but does not succeed to do so, or (ii) that some other entity would like the DM to apply a fair process. We aim to design an environment in which the DM then makes choices that imply a fair process. To understand the individual and strategic motivations of the different players, especially the DM, we add a condition *Random*, in which the decision maker does not make a choice, but outcomes are allocated randomly with equal probability. The condition allows us to observe beliefs and rewards or punishments in the case a fair process is exogenously imposed, but outcomes are still unequal.

2.3. Laboratory Procedures

The experiment was programmed using z-Tree (Fischbacher, 2007) and recruitment was done via HROOT (Bock, Nicklisch, and Baetge 2012) and ORSEE (Greiner, 2015). Across all treatments, 876 subjects took part in the experiment. Subjects were mainly undergraduate students from a wide range of different majors, with 35% having an economics background. A typical session lasted about 35 minutes, and subjects earned on average about €8.10 including a show-up fee of €3. We conducted 48 experimental sessions at Heidelberg University (24 sessions) and Frankfurt University (24 sessions) between November 2016 and November 2017. Table 1 shows the number and location distribution of 3-person groups over all treatment conditions, and the number of groups with $S_{AB} \neq 0$ in each condition.

¹ An overview of all experimental conditions and a full set of instructions is available in the webappendix at https://www.dropbox.com/s/yig7lte2pnimcov/ST_2019_procfair_Webappendix_march10.pdf?dl=0.

Table 1. Number of 3-person Groups by Treatment and Location

	Heidelberg	Frankfurt	# of groups total	# of groups with $S_{AB} \neq 0$
Baseline	22	18	40	28
Random	24	14	38	26
Transparency	17	24	41	32
Private Lottery	18	23	41	27
Public Delegation	22	18	40	26
Information Avoidance (private)	18	30	48	41
Information Avoidance (public)	18	26	44	32

3. Baseline Results: Strong Evidence for Favoritism

In the baseline condition there were 28 out of 40 groups in which the two recipients had an unequal political distance to the DM, that is, $S_{AB} \neq 0$. In 25 of these 28 cases (89.3%), the DM allocated the higher payment to the more similar participant, significantly more often than we would expect under random allocation ($p < 0.001$, binomial test against 0.5).² Using relative similarity indicators based on individual dimensions (that is, defining S_j and S_{AB} for each dimension separately), Table 2 shows that all dimensions except female quotas significantly added to this preferential treatment.³

Table 2. Relative Similarity and Allocation Behavior in Baseline

Dimension of similarity	Assignment of high payoff
Left-wing political attitude	0.387** (2.16)
Support of unlimited refugee inflow	0.269* (1.76)
Support of mandatory female quota	0.221 (1.49)
Support of active euthanasia	0.791** (2.27)
Support of exit from nuclear energy	0.715*** (2.81)
Observations	38

Notes: Marginal effects of probit regression reported, z-values in parenthesis. The number of observations is 38 as in two groups recipients are exactly identical. The explanatory variables are dimension specific relative distance measures. *, **, *** indicates significance at the 10%, 5%, and 1% level.

² All test statistics reported in this paper regard two-sided tests.

³ Analyses of pooled data for all conditions of the experiment replicate this finding with all dimensions significantly adding to favoritism (Appendix A3).

To better understand the motivation of the DMs we consider their beliefs and fairness judgments. Table 3 shows the DMs' beliefs about the rewards and punishments. We see that DMs expect moderately positive rewards in case of the Random treatment that guarantees a fair procedure. In Baseline, they expect significantly higher reward from the high payoff recipient, and a significantly higher punishment from the low payoff recipient. On average they expect a total reward of €0.25 in Random and €0.00 in Baseline, a marginally significant difference ($p < 0.1$, Mann-Whitney-U test). From a strategic perspective based on their own beliefs, it may thus be desirable for DMs to signal a fair procedure. Appendix A2 discusses fairness perceptions in detail. There we show that DMs perceive the recipients' outcome allocation as rather unfair in both Baseline and Random (2.43 vs. 2.84 on a scale from 1 to 7). At the same time, they perceive the allocation based on their own decision as substantially less procedurally fair than the random allocation (2.45 vs. 4.03, $p < 0.001$, Mann-Whitney-U-test). This suggests, that despite the clear evidence for favoritism, DMs do not seem to consider allocation according to political preferences as appropriate from a fairness perspective. Moreover, in an ex-post questionnaire (Appendix A5), DMs indicate that they feel they were influenced by the information, but did not intentionally favor one person.

Table 3. Decision Makers' Beliefs about Reward and Punishment in Baseline and Random

	High payoff recipient	Low payoff recipient
Baseline	€0.83 ^{###}	- €0.83 ^{###}
Random	€0.39 [#]	0.11
Difference	- €0.44 ^{**}	€0.94 ^{***}

Notes: Entries are average beliefs about additions to / reductions from the DM endowment (at cost €0.10 per €0.50 addition / reduction). *, **, *** indicates significant difference between Baseline and Random conditions at the 10%, 5% and 1% level, Mann-Whitney-U test. #, #, ### indicates significant difference from zero, two-sided t-test.

4. Interventions to Reduce Favoritism

Given the absence of any strong difference in deservingness or merit, we argue that an allocation that gives equal chances to the two recipients is preferable in the current setting. This also seems to be supported by the DMs' own fairness judgments, and from the perspective of their strategic incentives (beliefs). Still, we found clear evidence that the DMs use the information on political attitudes in their allocation decision. In almost 90% of the groups with differences in political attitudes, we find

favoritism. We therefore introduce interventions that aim to reduce the incidence of favoritism, without directly imposing a fair procedure as in Random. The reason is that a direct obligation to use a fair (random) procedure will often not be helpful in practice. In the current setting we know that there are no hard facts guiding the allocation decision, and all deviation from equal chances can be attributed to the effects of political attitudes. However, in practice it will often be less clear whether hard facts are absent, and when to impose a fair procedure on the DM. Rather, we would like the DM to freely choose to implement a fair procedure. These considerations guided the design of the following four interventions.

4.1. Transparency

4.1.1. Design and Hypotheses

In this condition, we establish full transparency with respect to the political profiles between group members. That is, in contrast to Baseline, where only the decision maker learns the recipients' profiles, all three political profiles within a group are known by each group member. We hypothesized that the extent of favoritism will be reduced compared to Baseline. Recipients will now be able to assess the potential effect role of politics on the allocation (albeit with only 1 data point). We thus conjectured that some DMs will use a contrarian strategy by choosing the less close recipient in the sense of positive discrimination, and that others may explicitly randomize "in their mind." These two effects would lead to a more balanced allocation in terms of the similarity measure.

4.1.2. Results

In Transparency, we have 32 groups in which the recipients have different degrees of similarity with the allocator. The DM chooses the more similar recipient in 30 cases (93.8%), significantly more often than a random choice ($p < 0.001$, binomial test against 0.5). We thus do not find reduced favoritism in an environment where recipients are able to identify it. Recipients anticipate the allocator's choice behavior, as 89.1% of recipients expect the allocator to choose the more similar recipient (recipients are aware of the political profiles when providing their beliefs).

DMs fairness judgments in Transparency are indistinguishable from the Baseline condition (see Appendix A2): they consider both the outcomes and the process as unfair. DMs' beliefs are slightly (insignificantly) less optimistic about the high-payoff recipient and slightly (marginally significantly) less pessimistic about the low-payoff recipient (Appendix A4). That is, it seems to be neither the case that DM consider it very appropriate to allocate to the more similar participant (which should have led to

substantially more optimistic beliefs), nor do they expect stronger repercussions under transparent allocation towards a more similar recipient. Transparency does not work as an intervention to reduce favoritism.

4.2. Private Lottery

4.2.1. Design and Hypotheses

In this condition, we allow allocators to distribute chances to receive the high outcome between recipients, rather than the outcome themselves. The DM is equipped with a randomization device allowing her to allocate a probability p of 100%, 90%, 75%, 60%, 50%, 40%, 25%, 10%, 0% for the high payoff to one recipient, with probability $1-p$ allocated to the other recipient. Given the selected probabilities, the computer then determines the (still unequal) final outcome allocation. While in Baseline and Transparency only an implicit randomization by “coin flip in their mind” was possible for the DMs, the lottery procedure allows them to explicitly select a fair equal-chance procedure. However, asymmetric allocations of chances are also possible, in particular the allocation of a 100% chance to the politically closer recipient is still available (as in Baseline, only the DM knows the political profiles, and does so before making her allocation decision). Importantly, recipients do not learn which lottery the allocator selected, only the final outcome.

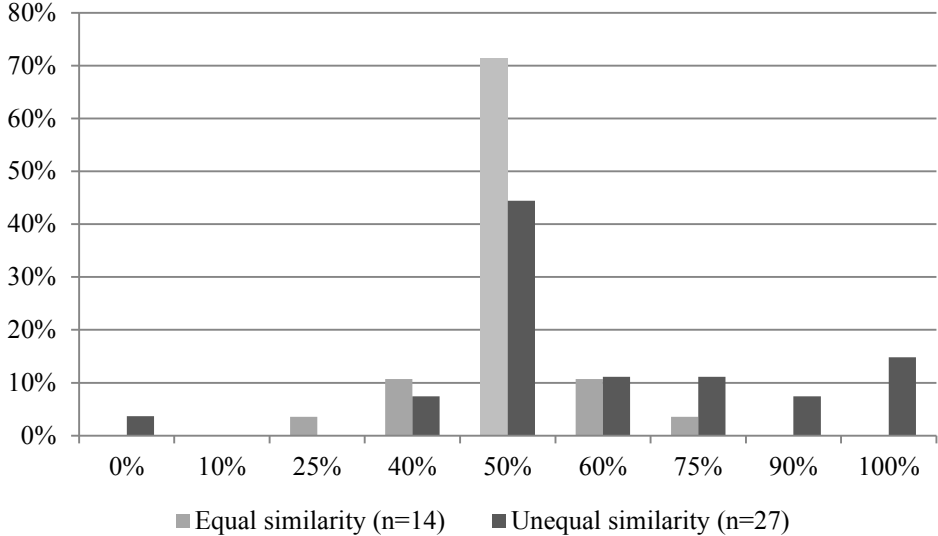
We hypothesized that the extent of favoritism will be reduced compared to Baseline, because a fair procedure (allocation of 50% chance to either participant) is directly available. Implicit randomization in their mind may be more difficult to implement for participants than a direct choice of the fair procedure. Moreover, the availability of the explicit randomization device may make the fair procedure more salient, even in the presence of other unfair allocations ($p \neq 50\%$). Moreover, for a modest degree of favoritism, a more nuanced way to allocate chances (90%, 75%, or 60%) is now available. That is, although only intrinsic motivations can guide to the DM allocation decisions absent public information about her allocation of chances, we predict that the incidence of “100%” allocation is substantially reduced in comparison to Baseline or Transparency.

4.2.2. Results

In Private Lottery, we have 27 groups in which the recipients have different degrees of similarity with the allocator. In these groups, the DM allocates a chance of 100% 4 times (14.8%) to the more similar recipient, and 1 time (3.7%) to the less similar recipient. Clearly handing out the higher €8-payment to

either candidate is not a compelling option for DMs in this treatment. Figure 1 shows the full distribution of chance allocations for the 27 groups with unequal similarity measure, and for those 14 groups with equal similarity, i.e., $S_{AB} = 0$.

Figure 1. Allocation of Chance to Closer Recipient



Notes: X-axis shows the probability assigned by the DM to the more similar recipient (or one of the equally close recipients in case of equal similarity). Y- axis gives percentage of choices of each of these chance allocations over groups with unequal similarity measures, or groups with equal similarity measures, respectively

In groups where recipients have different similarities, the modal allocation is the equal-chance allocation, which accounts for almost half of all decisions. Still, the politically closer recipient receives on average a probability weight of 61.7%, which is significantly larger than the equal-chance allocation of 50% ($p < 0.05$, t-test); moreover, a recipients’ similarity score S_j and her assigned probability of the €8-payoff are positively correlated ($\rho = 0.3317$, spearman rank correlation, $p < 0.05$). However, favoritism is significantly reduced compared to Baseline and Transparency if we take the realized allocation probabilities of 89.3% and 93.8% in these conditions as a benchmark ($p < 0.001$ against Baseline and $p < 0.001$ against Transparency, two-sided t-tests). That is, there is only a very modest degree of favoritism. This also shows up in the data for the groups with equal similarity measure. Despite the equal similarity measure, in almost 30% of the cases the DM allocates modestly different chances to the two recipients. These deviations from the equal-chance allocation may either be random, or may derive

from differences on individual items of the political attitude questionnaire. The finding suggests that some modest degree of favoritism seems desirable to the DM. On balance, however, providing a private randomization device successfully reduces the incidence of favoritism in our setting by a substantial margin.

We can shed more light on the desirability of favoritism using the fine-grained chance allocations in the current treatment together with the political attitude measures of the DMs. In the 27 groups with $S_{AB} \neq 0$, DMs who are classified as left-wing⁴ on average assign a probability weight of 65.8% for the high payoff to the closer participant, while subjects categorized as right-wing on average assign a probability weight of 50% to both recipients. The difference between the two groups is marginally statistically significant ($p < 0.1$, Mann-Whitney-U test).

4.3. Public Delegation

4.3.1. Design and Hypotheses

In this condition, we give the DM the opportunity to publicly delegate the allocation decision to a random device that assigns the payoffs with equal chances to the two recipients. Delegation may be costly for the allocator with the possible prices being €0.00, €0.50 and €1.00. We apply the strategy method in order to have allocators indicate for each price whether they want to delegate, or whether they want to make the decision themselves (knowing the recipients' political profiles). If they choose to decide themselves for some price, they are also directly asked to indicate their allocation decision for that case. These decisions are all shown on one screen. Subsequently, one of the three prices is randomly selected and the DM's decision for that price is implemented. Recipients learn only about the payoff relevant decision. That is, for the price that has been selected, they learn the price of delegation, whether the DM delegated or not, and what their payoffs is.

We hypothesized that public delegation allows (i) intrinsically motivated DMs to implement the fair procedure similar to Private Lottery, and (ii) strategically oriented DM to signal fair procedure (see comparison of DMs' beliefs between Random and Baseline). We thus expected a strong reduction in favoritism (indicated by high take up of delegation). However, we also expect DMs to trade-off the costs of delegation against its benefits (strategic or intrinsic). We thus expect a lower degree of delegation for

⁴ Based on the answers to our 6 political questions, answering more than 50% of the questions in a typical left-leaning way. For the exact definition of this measure, see Appendix A.4.1.

higher prices. This effect is potentially moderated by the stronger signal to oneself and others of choosing delegation given a higher price.

4.3.2. Results

If delegation is costless, 70.0% of the DMs delegate the decision to the random device, a significantly larger share than we would expect if DM randomized ($p < 0.05$, binomial test against 0.5). As the price increases to €0.50, 32.5% of the DM delegate, a significant reduction ($p < 0.001$, sign-test). A further increase to a price of €1.00 leads to 20% of the DM selecting delegation, a significant further reduction ($p < 0.05$, sign-test). However, for 22.5% of the DMs we observe that they delegate if delegation is costless, and in the two costly cases they choose each recipient exactly once for the high payoff. That is, the strong price sensitivity may to some degree be driven by the fact that an alternative (but unobserved by recipients) “implied randomization device” was available through the strategy method.

To better understand the motivation to delegate, for the costless delegation case we asked the DMs’ about their expected rewards or punishments in case they delegate and in case they do not. We find that DMs expect significantly lower reward from the high payoff recipient and lower punishment from the low payoff recipient, with a positive but insignificant overall effect on the sum of rewards and punishments. These patterns of beliefs are consistent with those in Baseline versus Random. Regressing political attitudes (a left-wing index) on the delegation decision in the costless case does not suggest any political preference for public delegation (see Appendix A.3. for details).

Overall, the opportunity for public delegation successfully reduces the incidence of favoritism. Counting delegation as non-favoritism (“50%”), we find overall 65.4% (for price €0), 73.1% (for price €0.50) and 78.8% (for price €1.00) of allocations of high payoffs towards the politically closer recipient (calculations include only groups with $S_{AB} \neq 0$). That is, even costly delegation can have a substantial effect in reducing favoritism. Interestingly, for costless delegation, the effect is comparable to the effect of Private Lottery, which led to a 61.7% likelihood of the closer recipient being allocated the high payoff.

4.4. Information Avoidance

4.4.1. Design and Hypotheses

In this condition, before the allocation decision takes place, the DM has to decide whether she wants to be informed about the political profiles of the recipients before making her decision, or not. If the DM

decides to learn the profiles of the recipients, the decision environment is the same as in Baseline. If she decides to stay uninformed, she does not learn the political profile of recipients and takes the allocation decision without having any knowledge about the recipients (i.e., de-facto allocating randomly)

We conduct two variants of this condition: *private* and *public* avoidance of information. In the first variant, the avoidance decision happens privately, i.e. recipients do not learn whether the DM wanted to remain uninformed about recipients' politics. In the second variant, recipients are informed about the DM's self-selected information status. Recipients always know that information avoidance was possible.

We hypothesized that it may be difficult for DMs to disregard information once they obtained it, but that they may prefer not to receive information that they do not want their decision to be influenced by. Moreover, public delegation or private randomization may sometimes be difficult to implement in practice, possibly because it is at odds with the idea that there exists an optimal allocation and the DM is tasked to identify and implement it. Withholding information that is considered irrelevant or inappropriate to use is potentially easier to justify. We further hypothesized that publicly observable information avoidance is more attractive to DM as it combines intrinsic and strategic benefits.

4.4.2. Results

For private Information Avoidance, we find that 6 of the 48 DMs (12.5%) avoid information about recipients' politics. Conditional on obtaining such information, we find a similar degree of favoritism as in baseline. Of the 42 groups where DMs obtained information, $S_{AB} \neq 0$ in 36 groups. In these groups, 32 DMs (88.9%) allocated the higher payoff to the closer recipient. In public Information Avoidance, we find that 23 of the 44 DMs (52.3%) avoid information about recipients' politics. Conditional on obtaining such information, we find a similar degree of favoritism as in baseline. Of the 21 groups where DMs obtained information, $S_{AB} \neq 0$ in 17 groups. In these groups, 16 DMs (94.1%) allocated the higher payoff to the closer recipient.

The difference in information avoidance between private and public conditions is substantial and statistically significant ($p < 0.001$, Mann-Whitney-U test). Counting information avoidance as non-favoritism ("50%"), we find overall 83.3% allocations of high payoffs towards the politically closer recipient in the private and 68.8% in the public condition (calculations include all DMs who avoid information and DMs who obtain information and observe $S_{AB} \neq 0$). That is, public information avoidance has a somewhat lower but comparable effect to public Delegation and Private Lottery. The private information avoidance opportunity does not work well. It seems that DMs have a strong

preference for information, even if they later end up finding it unfortunate making use of it in the binary allocation decision. It seems more compelling for DMs to later resort to random allocation if given the opportunity, and irrespective of its private or public nature, than to avoid information in the first place. We find no link between the political attitude of the DM and the decision to avoid information (Appendix A3).

5. Discussion & Conclusion

This paper studies a setup common to many allocation problems: (1) a resource is indivisible; (2) objective criteria to guide the allocate decision are unavailable (in practice they may not help distinguish between people after an initial preselection of candidates); and (3) softer subjective criteria may be available to the allocator. We hypothesized that such softer criteria play an important role affecting the allocation. Importantly, we assume that often either the decision maker or the institution employing or controlling the decision maker, consider a decision on the basis of such soft criteria inappropriate (Shaw and Olson, 2014; Choshen-Hillel et al., 2015).

Because a fair allocation of outcomes is unattainable, establishing process fairness may be desirable (Moorman, 1991; Organ and Ryan, 1995). Decision makers or organizations may have an intrinsic preference for process fairness (Trautmann, 2009; Krawczyk, 2010; Saito, 2013). They may also fear repercussions (legal, sabotage) from the affected agents if the allocation process is not perceived as fair (Grosch and Rau, 2017; Dickinson et al., 2017; see also Appendix A2), or vice versa, expect higher acceptance of unfavorable outcomes if fairness is signaled (e.g., by delegation, Bartling and Fischbacher, 2012; Coffman and Real, 2018). More generally, process fairness can make undesirable organizational outcomes and procedures more acceptable (Adler, 1983; Skarlicki and Folger, 1997; Garonzik et al., 2000; Brockner, 2002; Kessler and Leider, 2016). An organization may therefore want to induce the allocator to employ a fair process. However, any mechanism inducing process fairness should be such that the allocator can freely choose to allocate fairly: in most settings, only the decision maker will be able to judge which criteria can be used as hard facts, and whether they help to distinguish among candidates.

In a Baseline condition, we replicate empirical evidence that favoritism is strong in situations where an indivisible resource has to be allocated among multiple parties (Robin et al., 2012; Hong et al., 2016). About 90% of the decision makers allocate according to the soft criterion provided by the political attitude measures. Given that political similarity is the only available information, and that an unfair

outcome allocation cannot be prevented, it is not surprising that the attitudes determine the allocation. However, evidence on fairness judgments and expected rewards in comparison to a controlled Random allocation condition suggests that allocation according to political attitudes is not perceived as appropriate. That is, in Baseline, decision makers do not prefer to allocate according to politics, but there is little to prevent such a result to emerge. Irrespective of whether the allocators find the use of politics to guide their decision appropriate, we emphasize that the entity designing the allocation process may find it undesirable. We therefore design four interventions with the goal to reduce favoritism, that is, aiming for a 50% benchmark for the allocation of the better outcome to the politically closer participant, but without enforcing random choice. Moreover, given the above considerations regarding repercussions if processes are perceived as unfair, the intervention should also improve fairness judgments.

A simple measure to compare the interventions is provided by the implied share of favoritism. The politically closer recipient is allocated the better outcome in the following share of groups: 93.8% when full transparency is provided; 61.7% if a private random lottery can be employed to distribute chances rather than outcomes directly; between 65.4% and 78.8% if public delegation is available (depending on the cost of signaling the fair procedure); and 68.8% respectively 83.3% if information can be avoided before making a decision and this is either communicated to recipients or not.

We make a few observations. All interventions except Transparency have some effect reducing the incidence of favoritism, but none of the interventions fully eliminates favoritism. The possibility to signal the intention to apply a fair procedure is important for its success. Higher cost of implementing a fair procedure reduce allocators willingness to apply a fair procedure. Making an indivisible resource flexibly “divisible” by using allocation according to probability shares has a stronger effect than any of the process-fairness signaling interventions, despite the private nature of the mechanism. This is consistent with the result that none of the interventions attains a clearly positive judgment of process fairness: unfair outcomes matter for the assessment of the process (Baron and Hershey, 1988; König-Kersting et al., 2018). That is, although there is a process-fairness effect on outcome fairness evaluations (which we may dub an *inverse outcome bias*, see Appendix A2), the effect is too modest to make a compelling case for equal-chance random allocation. These findings replicate previous work showing that random allocation of indivisible outcomes may not be as convincing empirically as the theoretical process fairness perspective predicts (Keren and Teigen, 2010; Elster, 1989, p. 55). The success of the Private Lottery condition suggests that equal-chance random allocation (as in Public Delegation or Information Avoidance) may be too strong. Allowing allocators a modest degree of favoritism (by

allocating unequal probabilities) works better than the more stringent interventions. As a final observation, we note that the take-up of Information Avoidance, either private or public, seems too modest in comparison to the observed preference for process fairness in Private Lottery and Public Delegation. We interpret the finding as a preference for knowing (e.g., Loewenstein, 1994, but see Hertwig and Engel, 2016), with allocators not anticipating the possibly undesirable degree of influence on their decisions.

Apart from Private Lottery, the interventions discussed here have close counterparts in practical settings. Transparency and delegation to a neutral entity are classic solutions in many domains. Information Avoidance has been proposed in public discourses as a remedy for biased selection in labor or rental markets. Our results suggest that transparency may not work when each decision maker makes only one decision, and coordination across decision makers is not easily attainable. How would companies coordinate to attain a close to equal share of male and female CEOs? The possibility to publicly delegate or avoid certain types of information is supported by our results as an effective tool. Interestingly, the most successful intervention, namely a weighted random selection if candidates are very close, has been proposed as a tool to implement justice in distribution processes in an early contribution by Edgeworth (1890). He suggested using *graduated* lotteries in the context of permission to the civil service, i.e., lotteries calibrated with regard to merit, for example, examination scores. Merit would be accounted for, and feelings of injustice by the candidates, stemming from the possibly arbitrary character of the examination, would be mitigated. Boyle (1998) argues that such lotteries help implement fairness in selection processes in organizations, incorporating both efficiency and equity considerations, and avoiding biases and corruption. Despite these theoretical foundations, weighted lotteries are, to the best of our knowledge, not a tool used in practice. We believe that this is due to the possibly questionable normative status of the mechanism. If one candidate is preferable, she should be allocated the better outcome. If there is no clear ordering of candidates, an equal share allocation seems most compelling. A weighted random allocation allowing for inclusion of soft criteria is not convincing from this perspective. However, if the mechanism works best for imperfect, worldly decision makers, it may be the right intervention to achieve our empirical goal: procedural fairness.

Appendix

A.1. Questionnaire Details

A.1.1. Original German wording in political-attitudes questionnaire

The original wording of the political attitudes questionnaire taken at the beginning of the experiment was as follows:

Bitte beantworten Sie zunächst den folgenden Fragebogen zu verschiedenen politischen Themen, indem Sie die Antwortmöglichkeit auswählen, die am ehesten auf Sie zutrifft (die Fragen beziehen sich jeweils auf Deutschland).

Ihre Antworten werden möglicherweise an andere Teilnehmer bei diesem Experiment weitergeleitet. Ihre Anonymität bleibt jedoch selbstverständlich während des gesamten Experimentes und auch bei der Auswertung der Daten gewahrt.

- 1. Wie ist Ihre politische Ausrichtung? [„Eher Links“ / „Eher rechts“]*
- 2. Sind Sie für oder gegen die unbegrenzte Aufnahme von Flüchtlingen? [“Dafür” / “Dagegen”]*
- 3. Wie stehen Sie der Einführung von Frauenquoten in Unternehmen gegenüber? [“Positiv” / “Negativ”]*
- 4. Sind Sie für oder gegen die Einführung der aktiven Sterbehilfe? [“Dafür” / “Dagegen”]*
- 5. Wie beurteilen Sie den Ausstieg aus der Atomenergie? [“Sinnvoll” / “Nicht sinnvoll”]*

A.1.2. Wording of the fairness-perception questionnaire

The wording of the outcome and process fairness evaluations regarding the allocation of payments to player A and B was follows:

Outcome Fairness [German wording] / [translation]

[„Als wie fair beurteilen Sie die Auszahlungen, die für Spieler A und Spieler B aus dem Verteilungsprozess resultieren?“] / [„How fair do you evaluate the payments that result for Player A and Player B?“]

Process Fairness [German wording] / [translation]

[„Als wie fair beurteilen Sie den Sie den Verteilungsprozess?“] / [„How fair do you evaluate the allocation process?“]

Participants answered the questions on a 7-point-Likert scale, with point 1 labeled as “very unfair” and point 7 labeled as “very fair”, and point 4 representing a neutral judgment).

A.2. Fairness Perceptions

This appendix provides details on the players' perceptions of process fairness and outcome fairness. Table A1 shows fairness perceptions in the Baseline condition, and compares them to the perceptions in the forcedly fair Random condition, the Transparency condition, the Private Lottery condition, and the Private Information Avoidance condition. Results are shown by player type and pooled over all player types.

Table A1. Fairness Perceptions

Condition	DM		High Payoff Recipient		Low Payoff recipient		All Players	
	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness
Baseline	2.45 ^{###}	2.43 ^{###}	3.15 ^{###}	2.55 ^{###}	2.10 ^{###}	1.85 ^{###}	2.57 ^{###}	2.28 ^{###}
Random	4.03	2.84 ^{###}	3.53	2.66 ^{###}	4.32	2.50 ^{###}	3.96	2.67 ^{###}
Δ Random	1.58 ^{***}	0.41	0.38	0.11	2.22 ^{***}	0.65 ^{**}	1.39 ^{***}	0.39 ^{**}
Transparency	2.44 ^{###}	2.20 ^{###}	2.85 ^{###}	2.37 ^{###}	2.56 ^{###}	2.37 ^{###}	2.62 ^{###}	2.31 ^{###}
Δ Transparency	-0.01	-0.23	-0.30	-0.18	0.46	0.52 ^{**}	0.05	0.03
Private Lottery	3.05 ^{###}	2.63 ^{###}	3.37 [#]	2.93 ^{###}	2.71 ^{###}	2.20 ^{###}	3.04 ^{###}	2.59 ^{###}
Δ Private Lottery	0.60 ^{**}	0.20	0.22	0.38	0.61 ^{**}	0.35	0.47 ^{***}	0.31 ^{**}
Private Information Avoidance	2.88 ^{###}	2.33 ^{###}	2.98 ^{###}	2.48 ^{###}	2.40 ^{###}	2.17 ^{###}	2.75 ^{###}	2.33 ^{###}
Δ Private Information Avoidance	0.43	-0.10	-0.17	-0.07	0.30	0.32	0.18	0.05

Notes: The table contains evaluations about process fairness and outcome fairness measured on a 7-point-Likert scale (1="very unfair"; 7="very fair"). Δ *Name* indicates differences between condition *Name* and Baseline. #, #, ### indicates significant difference from the value of 4 (which indicates neutrality on the fairness scale), two-sided t-test, and *, **, *** indicates significant difference between Baseline and *Name* condition, Mann-Whitney-U test, at the 10%, 5% and 1% level.

Table A2 considers the role of public delegation and public information avoidance for fairness perceptions. We find that delegation and information avoidance lead to higher perceptions of process fairness. Note that only for the DM the situation is self-selected; the recipients are exposed to the situation as determined by the DM.

Table A2. Fairness perceptions in Delegation and Public Information Avoidance conditions

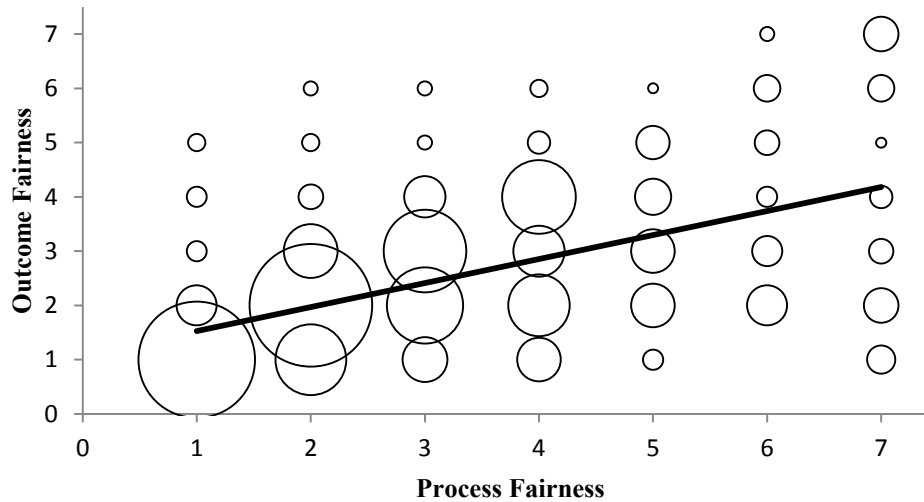
Condition	DM		High Payoff Recipient		Low Payoff recipient		All Players	
	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness	Process Fairness	Outcome Fairness
No Delegation (n=23)	3.39 [#]	2.39 ^{###}	3.74	3.04 [#]	2.17 ^{###}	1.91 ^{###}	3.10 ^{###}	2.45 ^{###}
Delegation (n=17)	4.41	2.47 ^{###}	4.65	3.18 [#]	4.65	3.24	4.57 [#]	2.96 ^{###}
Δ Delegation	1.02	0.08	0.91	0.14	2.48 ^{***}	1.33 ^{***}	1.47 ^{***}	0.51 ^{**}
Information publicly not avoided (n=21)	2.57 ^{###}	2.33 ^{###}	2.81 ^{###}	2.29 ^{###}	2.10 ^{###}	2.00 ^{###}	2.49 ^{###}	2.21 ^{###}
Information publicly avoided (n=23)	3.52	2.00 ^{###}	3.04 ^{###}	2.17 ^{###}	3.17 [#]	2.83 ^{###}	3.25 ^{###}	2.33 ^{###}
Δ Information publicly avoided	0.95	-0.33	0.23	-0.12	1.07 [*]	0.83	0.76 ^{**}	0.12

Notes: The table contains evaluations about process fairness and outcome fairness measured on a 7-point-Likert scale (1="very unfair"; 7="very fair"). Δ Delegation and Δ Information publicly avoided indicates differences between groups in which the DM delegated / avoided information, and those where she did not. #, #, ### indicates significant difference from the value of 4 (which indicates neutrality on the fairness scale), two-sided t-test, and *, **, *** indicates significant difference between delegation and non-delegation, respectively information avoidance and non-avoidance, Mann-Whitney-U test, at the 10%, 5% and 1% level.

Table A1 and A2 show that there is some tendency for self-serving process and outcome fairness evaluations. High payoff recipients report higher process fairness judgments than low payoff recipients in conditions Baseline ($p < 0.01$, Mann-Whitney-U test), Private Lottery ($p < 0.1$, Mann-Whitney-U test), Private Information Avoidance ($p < 0.05$), and in Delegation (but only when the DM does not delegate, $p < 0.01$, Mann-Whitney-U test). Low payoff recipients perceive higher process fairness in Random ($p < 0.1$, Mann-Whitney-U test). Likewise, high payoff recipients report higher outcome fairness judgments than low payoff recipients in conditions Baseline ($p < 0.01$, Mann-Whitney-U test), Private Lottery ($p < 0.01$, Mann-Whitney-U test) and Delegation (but only if the DM does not delegate, $p < 0.05$, Mann-Whitney-U test). Overall the evidence for self-serving fairness perceptions suggests the existence of an *outcome bias* in fairness judgments. Table A1 and A2 also show that increased perception of process fairness often coincides with higher perceptions of outcome fairness, despite the constant degree of outcome inequality in payoffs across all conditions of the experiment. Pooling data of all treatments and all players, we find a significant positive correlation between outcome and process fairness evaluations at the individual level ($\rho = 0.5716$, spearman rank correlation, $p < 0.001$). Figure A1 shows the relationship graphically for all players pooled (effects are very similar for all three player types).

With process fairness judgments affected by treatment conditions and outcome inequality fixed, we observe what we may term a *process bias* in outcome fairness judgments.

Figure A1. Relationship Between Process and Outcome Fairness Perceptions (all subjects pooled)



We finally assess whether fairness perceptions correlate with recipients’ punishment or retaliation behavior. Table A3 shows the results of simple regression analysis of the rewarding behavior across all conditions, as a function of the recipients’ process and outcome fairness judgments. We find no significant relationship of process or outcome fairness judgments of high payoff recipient with their behavior. For low payoff recipients, rewarding behavior correlates strongly with their process fairness judgments, but not with outcome fairness judgments.

Table A3. Regression analysis of fairness perception on reward/retaliation behavior

	Impact on DM’s payoff from high payoff recipient	Impact on DM’s payoff from low payoff recipient
Process Fairness Judgment	0.002 (0.042)	0.211*** (0.044)
Outcome Fairness Judgment	-0.061 (0.050)	0.001 (0.056)
Constant	1.000*** (0.130)	-0.844*** (0.131)
N	292	292

Notes: Process Fairness and Outcome Fairness are judged on a 7-point-Likert scale, with the value of 1 being labeled as “very unfair” and the value of 7 being labeled as “very fair.” The impact on DM’s payoff ranges from –€2 to +€2, with incremental steps of €0.50. OLS regressions; all conditions pooled; * p < 0.1; ** p < 0.05; *** p < 0.01, standard errors in parenthesis.

A.3. The role of politics

This appendix sheds more light on the role of political attitudes in the current allocation setting. Pooling the data from all treatments (876 subjects), Table A4 shows the distribution of answers for each of the 5 political attitude questions. The data shows a large degree of variation over recipients, as 92.5% of recipients within a group differ on at least one item. On average, recipients within one group chose different answers in 2.03 items.⁵

Table A4. Summary Statistics on Political Attitudes

Item	Distribution among respondents (n=876)				Correlation
	Left wing:	61.5%	Conservative:	38.5%	
General political attitude	Left wing:	61.5%	Conservative:	38.5%	1
Support of unlimited inflow of refugees	Yes:	50.3%	No:	49.7%	0.26***
Support of female quota in organizations	Yes:	63.6%	No:	36.4%	0.15***
Support of active euthanasia	Yes:	77.7%	No:	22.3%	0.14***
Support of exit from nuclear energy	Yes:	86.6%	No:	13.4%	0.09***

Notes: The last column shows the correlation with the general political attitude question. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The exact wording of the questionnaire is provided in Appendix A1.

Table A5 shows the distribution of the similarity score S_j between recipients and DM, and the distribution of $|S_{AB}|$ over all groups in the experiment. The distribution of $|S_{AB}|$ provides a direct measure for the potential for favoritism. The larger $|S_{AB}|$, the more recipients differ in their similarity to the DM.

Table A5. Distribution of S_j and $|S_{AB}|$

Distribution of similarity score S_j						
S_j	0	1	2	3	4	5
#	6	61	116	207	144	50
Distribution of absolute value of relative similarity score S_{AB}						
$ S_{AB} $	0	1	2	3	4	5
#	80	121	64	22	4	1

⁵ We note that in the post-experimental questionnaire, 96.0% of the respondents indicate having answered the questions in the political questionnaire truthfully.

We next look at the role of political similarity for allocation decisions. Table A6 replicates Table 2 for the whole sample of decisions for all conditions with an active allocation and known attitudes (i.e., DM does not delegate or avoid information; probabilities in Private Lottery larger 50% coded as assigning higher payoff). We find that all dimensions contribute to the preferential treatment of more similar recipients by the DM.

Table A6. Relative Similarity and Allocation Behavior (all Conditions with Active Allocation and Known Attitudes)

Dimension of similarity	Assignment of high payoff
Left-wing political attitude	0.461*** (5.51)
Support of unlimited refugee inflow	0.389*** (5.13)
Support of mandatory female quota	0.272*** (3.62)
Support of active euthanasia	0.584*** (5.15)
Support of exit from nuclear energy	0.543*** (4.08)
Observations	167

Notes: Marginal effects of probit regression reported, z-values in parenthesis. The explanatory variables are dimension specific relative distance measures. *, **, *** indicates significance at the 10%, 5%, and 1% level.

We next study how the DM’s political attitudes correlate with her allocation behavior. For this purpose, we define a *left-wing index* as the share of left-wing stereotype answers in the political attitudes questionnaire (left answers in Table A4). The index can take the values 0, 0.2, 0.4, 0.6, 0.8, and 1. Its median in the whole sample is 0.8. Subjects with index 0, 0.2, 0.4 or 0.6 are classified as below median left-wing (n=441 in the total sample), and subjects with index 0.8 or 1 are classified as equal/above median left-wing (n=435 in the total sample).

Table A7 shows that there is tendency for above-median left wing DMs to show more pronounced favoritism. This effect is insignificant for the Baseline condition on the basis of only 28 groups, but highly significant in the whole sample with 195 groups considered.

Table A7. Left Wing Index and Favoritism

	Favoritism	No favoritism	
<i>Panel a: Baseline</i>			
Left-wing index below median	12	3	15
Left-wing index equal/above median	13	0	13
Fisher exact test, p=0.226	25	3	28
<i>Panel b: All conditions</i>			
Left-wing index below median	54	48	102
Left-wing index equal/above median	69	24	93
Fisher exact test, p<0.01	123	72	195

Notes: Contingency table shows the number of DMs by political attitude choosing the closer recipient in Baseline and Transparency, not delegating and then choosing the closer recipient in Delegation, not avoiding information and then choosing the closer recipient in Information Avoidance, for the higher payment; assigning higher probability for the higher payment to the closer participant in Private Lottery; only groups with $S_{AB} \neq 0$.

We finally study how the DM's political attitude affects her decision to delegate (if delegation is costless) and to avoid information (Table A8). We find that there is no effect of DM's politics on the decision to delegate or to avoid information (irrespective of whether avoidance is publicly revealed).

Table A8. Left Wing Index and Delegation and Information Avoidance

	Delegation (if costless)	Avoid information (if private)	Avoid information (if public)	All three conditions pooled
Left-wing index	-0.134 (-0.33)	-0.221 (-1.49)	0.194 (0.57)	-0.162 (-0.83)
Female	-0.139 (-0.86)	0.086 (1.14)	-0.202 (-1.29)	-0.088 (-0.97)
Age	0.001 (0.04)	0.011 (0.74)	-0.007 (-0.38)	0.011 (0.81)
Economics	-0.131 (-0.73)	0.322* (1.72)	-0.223 (-1.29)	0.051 (0.49)
# Observations	40	48	44	132

Notes: Marginal effects of probit regressions reported, z-values in parenthesis. The left-wing index indicates the share of left-wing stereotype answers in the questionnaire. *, **, *** indicates significance at the 10%, 5%, and 1% level.

A.4. Recipients' behavior and DM' beliefs

This appendix summarizes recipients' rewarding and punishment behavior, and the DM' beliefs regarding the recipients' behavior. We first report recipients' behavior in the different conditions, and

then the DM's beliefs regarding the recipients' behavior. All analyses report the (belief in the) monetary impact on DM's payoffs caused by each recipient (i.e., not the costs involved by the recipient for punishing or rewarding).

Table A9. Recipients' Impact on DM's Payoff

Condition	High payoff recipient	Low payoff recipient
Baseline	1.41 ^{####}	- 0.30 [#]
Random	0.43 ^{####}	0.43 ^{##}
Δ <i>Random</i>	- 0.98 ^{***}	0.73 ^{***}
Transparency	1.00 ^{####}	- 0.60 ^{####}
Δ <i>Transparency</i>	- 0.41 [*]	- 0.30
Private Lottery	0.56 ^{####}	- 0.13
Δ <i>Private Lottery</i>	- 0.85 ^{***}	0.17
Private Information Avoidance	0.88 ^{####}	- 0.49 ^{####}
Δ <i>Private Information Avoidance</i>	- 0.53 ^{***}	- 0.19

Notes: Entries are average additions to / reductions from the DM endowment (at cost €0.10 per €0.50 addition / reduction). *, **, *** indicates significant difference between Baseline and the respective intervention at the 10%, 5% and 1% level, Mann-Whitney-U test. #, #, #### indicates significant difference to the value of zero, two-sided t-test.

Table A10. Recipients' Impact on DM's Payoff by Delegation and by Information Avoidance

Condition	High payoff recipient	Low payoff recipient
No Delegation (n=23)	1.02 ^{####}	- 0.67 ^{####}
Delegation (n=17)	0.26	0.26
Δ <i>Delegation</i>	- 0.76 ^{***}	0.93 ^{***}
Information publicly not avoided (n=21)	0.90 ^{####}	-0.38 [#]
Information publicly avoided (n=23)	0.93 ^{####}	-0.17
Δ <i>Information publicly avoided</i>	0.03	0.21

Notes: Entries are average additions to / reductions from the DM endowment (at cost €0.10 per €0.50 addition / reduction). *, **, *** indicates significant difference between no delegation and delegation, respectively information non-avoidance and avoidance, at the 10%, 5% and 1% level, Mann-Whitney-U test. #, #, #### indicates significant difference to the value of zero, two-sided t-test.

Table A9 shows rewarding and punishment by recipient and condition, and differences with Baseline. In all conditions, high payoff recipients reward less than in Baseline. For low payoff recipients there are few statistically significant differences; the only effect observed is lower punishment in Random than in Baseline. Table 10 shows rewarding and punishing depending on the publicly known delegation and information avoidance. We observe that high payoff recipients reward less, but low payoff recipients

punish less (send insignificantly positive rewards) when DM's delegate. At the end of the experiment, we ask recipients, about their maximum willingness-to-pay for delegation, if they were to be in the role of DM and find that low outcome recipients state a significantly higher WTP ($p < 0.05$, Mann-Whitney-U test). Finally, Information avoidance has no effect on rewarding behavior.

Table A11. DM's Beliefs about Recipients' Impact on Their Payoff by Delegation and by Information Avoidance

Condition	High payoff recipient	Low payoff recipient
Baseline	0.83 ^{###}	- 0.83 ^{###}
Random	0.39 [#]	0.11
Δ <i>Random</i>	- 0.44 ^{***}	0.94 ^{***}
Transparency	0.73 ^{###}	- 0.54 ^{###}
Δ <i>Transparency</i>	- 0.10	0.29 [*]
Lottery	0.49 ^{###}	- 0.50 ^{###}
Δ <i>Lottery</i>	-0.34	0.33 [*]
Avoidance (private)	0.66 ^{###}	- 0.41 ^{###}
Δ <i>Avoidance</i>	- 0.17	0.42 ^{**}

Notes: Entries are average beliefs about additions to / reductions from the DM endowment (at cost €0.10 per €0.50 addition / reduction). *, **, *** indicates significant difference between Baseline and the respective intervention at the 10%, 5% and 1% level, Mann-Whitney-U test. #, #, ### indicates significant difference to the value of zero, two-sided t-test.

Table A12. DM's Beliefs about Recipients' Impact on Their Payoff

Condition	High payoff recipient	Low payoff recipient
No Delegation	0.50 ^{###}	- 0.83 ^{###}
Delegation	0.08	- 0.22 [#]
Δ <i>Delegation</i>	- 0.42 ^{***}	0.61 ^{***}
Information publicly not avoided	0.57 ^{###}	- 0.68 ^{###}
Information publicly avoided	0.31 ^{###}	- 0.31 ^{###}
Δ <i>Information publicly avoided</i>	- 0.26 [*]	0.37 ^{**}

Notes: Entries are average beliefs about additions to / reductions from the DM endowment (at cost €0.10 per €0.50 addition / reduction). *, **, *** indicates significant difference between no delegation and delegation, respectively information non-avoidance and avoidance, at the 10%, 5% and 1% level, two-sided t-test. #, #, ### indicates significant difference to the value of zero, two-sided t-test. All DM were asked about both scenarios, i.e. delegation *and* no delegation as well as information avoidance *and* no information avoidance.

Tables A11 and A12 show the DM's beliefs for the behavior shown in tables A9 and A10. DM's anticipate the general pattern that high payoff recipients become less rewarding and low payoff recipients become less punishing. However, they are too pessimistic in their judgment, expecting too strong a decline in rewards and too weak a decline in punishment. Table A12 shows a similar pattern. DM's believe that both delegation and information avoidance will have an effect on rewards and punishment. They do not fully anticipate the strength of the effects for the low payoff recipients. Moreover, they seem to be less well calibrated for the case of no delegation and no avoidance. We report the following additional result not shown in the tables. We test whether beliefs are different for those DMs who delegate and those who do not. We do not find substantial qualitative or quantitative differences. That is, expected monetary awards cannot explain differences between these two groups of players.

At the end of the experiment, we also ask DMs whether they would appreciate delegation if they were to be in the role of a recipient. We find that stating that they would appreciate delegation in that case strongly correlates with opting for costly delegation ($p < 0.001$ for both prices). This further supports the conclusion that the motivation to pay for delegation is rather intrinsic than extrinsic.

A.5. Post-Experimental Questionnaire

This appendix provides the post-experimental questionnaire questions in Table A13, and the results in Table A14.

Table A13. Post-Experimental Questionnaire – Questions

	Question	Answer options
(1)	Do you think that the DM's decision was influenced by the information about the recipient? <i>(Recipient version of the question)</i> Was your allocation decision influenced by the information about the recipient? <i>(DM version of the question)</i>	Yes/No
(2)	Do you think that the DM intentionally favored one of the two recipients? <i>(Recipient version of the question)</i> Did you intentionally favor one of the two recipients? <i>(DM version of the question)</i>	Yes/No
(3)	Do you think that the [recipient with the high payoff] feels intentionally favored or disadvantaged? <i>(Only DM)</i>	Favored/ Disadvantaged/ Neither nor
(4)	Do you think that the [recipient with the low payoff] feels intentionally favored or disadvantaged? <i>(Only DM)</i>	Favored/ Disadvantaged/ Neither nor
(5)	How important is it to implement a fair process, even when this is costly? <i>(All)</i>	Likert-Scale from 1-7 (1=unimportant; 7=important)

Table A14. Post-Experimental Questionnaire – Results

	Type	(1): Yes/No	(2): Yes/No	(3): Favored/ Disadvantaged/ Neither	(4): Favored/ Disadvantaged/ Neither	(5)
Baseline	DM	36/4	21/19***	31/3/6	2/33/5	5.7
	High	35/5	19/21***			5.7
	Low	38/2	29/11***			5.8
Transparency	DM	37/4	11/30***	25/1/15	0/34/7	5.2
	High	37/4	14/27***			5.5
	Low	38/3	20/21***			5.8
Private Lottery	DM	22/19	13/28***	14/3/24	5/15/21	5.3
	High	34/7	22/19***			5.5
	Low	31/10	27/14			5.6
Private Info Avoidance	DM	36/6	17/25***	27/0/21	0/32/16	5.3
	High	47/1	21/27***			5.4
	Low	46/2	28/20***			5.6
Delegation	DM	9/8	5/12	1/1/15	1/1/15	5.1
	High	10/7	5/12**			5.5
	Low	10/7	4/13***			6.2
No Delegation	DM	17/6	12/11*	12/0/11	0/11/12	5.4
	High	20/3	14/9**			5.6
	Low	21/2	17/6**			5.6
Public Info Avoidance	DM	-	-	3/1/19	0/2/21	5.3
	High	-	-			5.9
	Low	-	-			5.2
No Public Info Avoidance	DM	19/2	12/9***	17/0/4	0/17/4	6.0
	High	21/0	8/13***			5.5
	Low	21/0	13/8***			5.4

Notes: *, **, *** indicates significant difference between (1) and (2), at the 10%, 5% and 1% level, two-sided t-test.

We test whether items 1 and 2 differ from one another and find that in almost all cases, both the recipients as well as DMs do more strongly agree to the statement that the DM is influenced by the information about recipients, than to the statement that the DM explicitly and intentionally favors one of the recipients. This corroborates the idea that it is not intentional nepotism that drives the strong degree of favoritism, but the inability to disregard available information even if the DM does not intend to use it.

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