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Reciprocal reactions to (in)transparent task assignments: An experimental investigation☆

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ABSTRACT

We investigate how the transparency of selecting an agent to perform a task affects the agent's reciprocal behavior. In a modified gift-exchange game, agents either receive information about having (not) been selected or receive no such information at all, which allows to analyze reciprocity of agents who were or were not the preferred choice of the principal. We do not find that transparency is harmful as agents' reciprocity is not reduced when learning that they have not been selected. Yet, we also do not find that reciprocity increases on average when agents learn they have been selected, although we find a positive effect on the extensive margin. This observation is driven by gender-specific reactions to transparency. While men react with increased reciprocity to the positive signal of having been selected (in line with intention-based reciprocity), women do not. Our observations could be explained by gender-specific attribution styles, i.e., whether the selection is attributed to own characteristics (internally) or good luck (externally).

1. Introduction

In this experimental study, we investigate how the transparency of employee selection affects the (reciprocal) behavior of employees. How to design and communicate work processes is a question that is particularly important for organizational design. As an instrument of corporate culture, the transparency of task assignment processes can be used to potentially motivate employees and foster cooperation and (positive) reciprocity.

When selecting an employee or delegating a task, employers typically choose one among several candidates. If no particular employee is obviously most suitable (e.g., due to her ability, experience, or availability), the selection may be influenced by the employer's personal preferences for characteristics of the candidates that are completely irrelevant for the job. For example, an employer (she) selects an employee (he) because she likes the city where he went to school or because of particular hobbies or lifestyle. Although the characteristics are irrelevant for the job, having been consciously selected may induce the employee to feel positively perceived,[1] to feel trusted,[2] or to feel the need of proving to be trustworthy by fulfilling the employer's expectations.[3] In such a situation, two aspects may drive the behavioral response of employees: first, the act of being selected for a task is typically a kind action and as such is likely to induce positive reciprocity. Second, knowing to have been selected among several candidates may induce an even stronger reciprocal response due to the 'gift' of being trusted with the task. More precisely, such knowledge may lead to higher efforts than in an intransparent situation, where employees do not know whether they have been consciously selected.

Yet, there are situations where the preferred employee is not available and another candidate is assigned to do the job. If the situation is transparent, this candidate knows that he has not been the first choice. This information may be unpleasant because it signals that the

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[1] Several studies show that people have a desire to be perceived positively by others (Andreoni & Bernheim, 2009; Ariely et al., 2009; Ellingsen & Johannesson, 2008; Grossman, 2015).

[2] For instance, Gambetta and Székely (2014) show that individuals like to signal their trustworthiness.

[3] People want to avoid the guilt from disappointing others' expectations, see, e.g., Battigalli and Dufwenberg (2007) for a theory of guilt aversion and Charness and Dufwenberg (2006) for experimental evidence.

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candidate is not perceived as positively or trustworthy as another one. In such a situation, the behavioral response of the candidate is not straightforward. On the one hand, it could be that he is disappointed or demotivated. Therefore, he may reciprocate less than the preferred employee, and also exert less effort than in an intransparent situation where he does not know that he has not been the first choice. Such a behavior would be predicted by models of intention-based reciprocity (e.g., Cox et al., 2007; Rabin, 1993). On the other hand, he could try to convince the employer that he would have deserved being selected by exerting higher effort, overcompensating the negative signal of the employer.

Obviously, the signal that is conveyed by (not) selecting an employee if the selection is based on task-irrelevant characteristics, is rather weak and may not suffice to trigger reciprocal responses. But of course, the selection is meaningful as it indicates that the employer wants the selected employee to perform a specific task and the task will typically (like in our experiment) be beneficial for the agent. The main goal of this study is to analyze whether the transparency of an employer's selection has an impact on employees' behavior, and in particular, to investigate how employees in a transparent situation react to the information of being (not) selected. To do so, we conduct a laboratory experiment, where we implement a modified gift-exchange game with a random move that represents the chance that a preferred agent is not available. Across treatments, we vary whether or not agents receive the information about the principal's selection decision and study the agents' reciprocal behavior. In our experiment, we do not aim at differentiating between principals' different motivations for selecting an agent. Instead, we focus on the agents' reaction to being selected or not, where being selected may be perceived as more kind than not being selected.

It is well-established that reciprocity depends on perceived intentions or kindness of actions (e.g., Charness & Levine, 2007; Englmaier & Leider, 2012; Nelson, 2002; Orhun, 2018). Given that people care about decision makers' intentions, also the decision making procedure is important as it affects people's beliefs about intentions (see, e.g. Aldashev et al., 2017; Bolton et al., 2005; Falk et al., 2008). In the context of personnel selection or when assigning agents to particular tasks, it may therefore matter for agents' behavior how they perceive the selection process. The organizational justice literature provides evidence from surveys and interviews suggesting that the perceived fairness of a selection process can induce positive reactions by agents, e.g., in the sense that agents state a more positive image of the organization or stronger intentions to work for it (Gilliland, 1995). Management research on applicant perceptions and reactions to application procedures provides similar insights (for an overview see Ryan & Ployhart, 2000). In both strands of the literature the focus is, however, not on reactions of employees *on* the job.[4]

More closely related to our research is the study by Brandts et al. (2006) focusing on whether the procedure by which an employee is selected matters for subsequent behavior in the employment relationship. Brandts et al. consider a pie-sharing game between one principal and two agents. The principal can gather information about personal characteristics of agents, which may serve as a signal of the agents' behavior, and then either choose an outside option or select one agent to share the pie between all three. To evaluate the effects of the personalized selection, they compare the agents' behavior to a setting where agents are randomly selected in case the outside option is renounced. They observe higher generosity by agents who are chosen according to their personal characteristics rather than randomly.[5] In contrast to our paper, Brandts et al. (2006) do not consider the motivational reactions of agents to the (in)transparency of the principal's selection. In their experiments, agents know they have been selected based on either personal characteristics or on a random draw.[6] In particular, the design of Brandts et al. (2006) does not allow to analyze how an agent who has *not* been selected reacts to this mere fact.

Also Montinari et al. (2016) investigate how the selection procedure affects the reaction of the selected agent. In contrast to our paper, however, they focus on differences in agents' abilities and relate to the empathy wage notion by Baron (2013). In particular, they investigate how communication can affect the agents' response. In their three-player gift-exchange game with two agents who differ in their observable abilities, the principal's payoff increases in effort and ability of the selected agent. One of the two agents is either selected randomly or by the principal, who in one treatment can also send a free-form message to the selected agent. Without communication, the selected agents' effort choices do not differ between random and intentional selection. When communication is possible, low ability agents who have been intentionally selected reciprocate more than high ability agents and also more than in case they were randomly selected, even overcompensating the payoff disadvantage in ability. Montinari et al. (2016) argue that by communication principals can induce a feeling of gratitude in less qualified agents for having been selected into positions when better candidates are available. However, they do not vary the transparency of the principal's selection.

In contrast to previous research, we vary the transparency of the principal's selection, i.e., agents either receive information about having (not) been selected, or receive no such information at all. The design allows to analyze the reciprocal response of agents who were not the preferred choice of the principal. To the best of our knowledge, our paper is the first to study these issues. More precisely, our experiment consists of two stages. In the first stage, all subjects state their preferences over 15 pairs of alternatives (e.g., apartment or house). These preferences are not related to any kind of ability or social preference that might be relevant for the later task. In the second stage, subjects are assigned to groups of three where one subject is assigned to the role of the principal and the other two subjects to the role of agents. The principal learns the stated preferences of both agents (and the agents know this fact) and selects one of the agents to perform a task. The selection of the principal can only be based on the task-irrelevant stated preferences, but can already establish a sense of group membership which could serve as a basis for in-group favoritism or out-group discrimination (Tajfel et al., 1971). The task is captured by a gift-exchange game, where the principal pays the agent a fixed wage. After the principal's selection, a random move either implements the selection (in 60% of the cases) or not (in 40% of the cases). If the selection is implemented, the agent who was chosen by the principal enters the gift-exchange game, otherwise the agent who was *not* chosen does so. The agent who enters the gift-exchange game then chooses a costly effort that affects the principal's payoff.

By selecting a particular agent, the principal allocates a higher (expected) payoff to this agent than to the other one. Thus, it is likely that the action of having been selected by the principal is perceived as kind. Across treatments, we vary the information the agents receive about the selection. More precisely, in one treatment, the agents learn whether they have been selected or not selected, whereas in the other treatment,

[4] Nevertheless, there is some evidence that procedural justice fosters positive reactions of employees on the job, for example, when denied a bonus or a promotion, people seem to be less demotivated in case they had a fair chance (see, e.g., Konovsky, 2000; Lemons & Jones, 2001).

[5] Note that Brandts et al. use a strategy method for the agents' pie-sharing decisions. Therefore, results are not driven by the fact that the principal manages to select the more generous agent. Nevertheless, due to the opportunity for the principal to opt out, the results could still be confounded by this fact.

[6] Due to the strategy method in the study by Brandts et al. (2006), subjects are not aware whether they have been selected when making their choice, but they make their decision for the case that they have been selected — either randomly or intentionally.

they do not learn the principal's selection (aside from knowing that the principal has to select one agent). Thus, we can compare behavior (i) between a transparent and an intransparent setting, and (ii) between agents who learn that they have been selected and those who learn that they have not been selected. Differentiating between motivations to select an agent and reactions to specific motivations is beyond the scope of our experiment.

We find that – inconsistent with intention-based reciprocity – efforts are not lower when agents learn that they have not been selected. Also, efforts are not higher, which would be consistent with overcompensating the negative signal of not having been selected. A plausible reason for this result may be that individuals hold motivated beliefs (Bénabou & Tirole, 2002) that are more resistant to negative than positive information leading to asymmetric patterns of information processing: positive or re-enforcing self-related information is more readily incorporated into decision making than negative information (e.g., Eil & Rao, 2011; Grossman & Owens, 2012). Moreover, learning to have been selected does not increase the level of reciprocity on average, but only the likelihood to reciprocate.

Since we focus on reciprocal behavior, it suggests itself to take into account the established gender differences in the literature on social preferences. We do find, however, no evidence that women are more reciprocal than men as is typically observed in the literature (for an overview see Croson & Gneezy, 2009).[7] As the literature also suggests that women react stronger to social stimuli or experimental conditions (Eckel & Grossman, 1996; Espinosa & Kováří, 2015; Kamas & Preston, 2015), we examine gender-specific reactions to the information conditions. In fact, splitting the sample by gender reveals that neither men nor women react to the negative signal of not having been selected by the principal. Remarkably, only men (but not women) reciprocate more when learning that they have been selected than when learning that they have not been selected or when having no information at all. After a positive signal, behavior of men is thus consistent with intention-based reciprocity, while women's behavior is not.

We discuss how the different reactions to being selected can be explained by gender-specific attribution styles, i.e., whether the selection is attributed to own characteristics (internally) or good luck (externally). This attribution may interfere with perceiving the principal's selection as kind. In line with the literature (e.g., Sherman et al., 1997), we find gender differences in attribution style indicating that men attribute more internally and women more externally. Moreover, we find suggestive evidence that men rather expect to be chosen due to personal characteristics and women due to good luck.

Returning to the question of organizational design, our findings suggest that making the principal's selection decision transparent (after uncertainty about the candidate's availability is resolved) can motivate employees and foster reciprocity only in one case: when the selected (and available) candidate is male. The good news is, however, that transparency does not seem to have any negative effects.

2. Experimental design and procedure

Our experiment consists of a modified gift-exchange game.[8] The first modification is that we implement a three-player game with one principal and two agents. The principal selects one of the two agents to make the subsequent effort choice in the gift-exchange game. To do so, the principal only receives information about the agents' preferences over alternatives (explained below). Second, we include a random move which either implements the principal's selection or not, as detailed below.[9]

Table 1
Effort choice and effort costs.

e	0	1	2	3	4	5	6	7	8	9	10
c(e)	0	1	2	4	6	8	10	12	15	18	22

Stage 1 – Pairs of alternatives: In the first stage, all subjects are asked to choose for 15 pairs of alternatives (which are displayed on the screen) the alternative they personally prefer (e.g., apartment or house). The alternatives are empty of meaning with respect to any kind of ability or social preferences. This is important in two respects: first, it prevents the later agents from thinking that they deserve being selected due to particular attitudes. Second, it ensures that the later principals cannot screen agents according to their supposed social preferences but only based on subjective motives. Subjects are informed that the chosen alternatives of some subjects will be used in the second stage, but have no knowledge about the task in stage 2 and how the information from stage 1 will be used. Subjects receive a fixed payoff of 10 points for stage 1.[10] Once all subjects have completed stage 1, they receive the instructions for stage 2.

Stage 2 – Gift-exchange game: In the second stage, subjects are randomly assigned to groups of three. In each group, one subject takes on the role of the principal and receives an initial endowment of 50 points. The other two subjects are the agents and receive no initial endowment. Each subject is informed about the own role on the screen. The principal learns the alternatives that the two agents in her group have chosen in stage 1. By informing the principal about the choices over the 15 alternatives, we try to give the agent a feeling of being deliberately selected instead of being randomly selected by the principal. She then selects one of the two agents to enter the gift-exchange game. After the principal's selection, a random draw determines whether the selection is implemented (with probability 0.6) or not (with probability 0.4). We do not implement a strategy-method for the effort choice but consider decisions in the realized situations only: If the principal's selection is implemented, the agent who has been selected (A_s) chooses the effort. If the principal's selection is not implemented, the agent who has not been selected ($A_{\neg s}$) chooses the effort. The agent who eventually chooses the effort automatically receives the endowment of 50 points from the principal[11] and has to make a costly effort choice $e \in \{0, 1, \ldots, 10\}$. We denote this as the payoff-relevant effort choice. The higher the effort, the higher the associated costs $c(e)$, as shown in Table 1. The agent who does not make the payoff-relevant effort choice receives a fixed compensation of 20 points and states a hypothetical, non-incentivized effort.[12]

The payoff-relevant effort choice e affects the respective agent's own payoff and the payoff of the principal but not the payoff of the other agent. The resulting monetary payoff m_P for the principal is given by:

$$m_P = 5 \cdot e,$$

where 5 may be interpreted as a revenue factor, i.e., the principal is able to convert one effort unit into five monetary units. The respective

[7] There are only a few exceptions that find no or opposite gender differences in reciprocity (e.g., Bellemare & Kröger, 2007; Dittrich, 2015).

[8] The setting is completely anonymous and we use a neutral language in the experiment, where we refer to the principal and agent as type A and B, respectively, and to the effort choice as number choice. In the text, however, we use the terms principal, agent, and effort choice.

[9] Original instructions were in German. For a translated version including a complete list of the alternatives from stage 1 as well as further material used in the experiment see online Appendices A and B.

[10] The exchange rate of points to Euros is 5:1.

[11] Note that the principal makes no choice on the agent's wage and cannot opt out from interacting at all like in typical gift-exchange games as our focus is on the effect of the selection decision. Variation in wage offers would render agents' effort choices inconclusive.

[12] This is to avoid revealing roles by idle subjects in the lab. Likewise, the principal simultaneously states a non-incentivized guess about the hypothetical effort of this agent.

agent's monetary payoff m_A is the difference between the transfer of 50 points from the principal (which can be interpreted as fixed wage) and the corresponding effort cost $c(e)$:

$$m_A = 50 - c(e).$$

We chose not to implement a real effort task to keep the design as simple as possible and avoid possible task dependent confounding effects. The random move enables us to analyze the impact of being the selected or not selected agent on the effort choice. In order to do so, we conduct two treatments, *NoInfo* and *Info*, which differ in the information subjects receive after the random move and prior to the effort choice. In *NoInfo*, the agent only learns that he makes the payoff-relevant effort choice but not whether he has been selected by the principal. In *Info*, agents additionally learn whether they have been selected or not selected. Furthermore, in *Info* (but not in *NoInfo*), the agent who makes the hypothetical effort choice learns whether or not he has been selected and the principal learns whether or not her selection was implemented.

At the end of stage 2, we elicit additional information from each subject. We ask the principal to guess the effort of the agent who makes the payoff-relevant effort choice ($Belief_P$). If $Belief_P$ is identical to the chosen effort of the agent, the principal receives 5 points, and 0 points otherwise. At the same time, we ask the respective agent to state a second-order belief ($Belief_A$), i.e., to guess which effort the principal guessed in $Belief_P$. If the agent's second-order belief is identical to $Belief_P$, the agent receives 5 points, and 0 points otherwise. In *NoInfo (Info)*, $Belief_A$ is made without (with) the knowledge of having been selected and $Belief_P$ without (with) the knowledge of whether the selection was implemented.[13] We elicited the agent's second-order belief to analyze whether it is related to his effort choice. The agent who makes the payoff-relevant effort choice may want to try to fulfill (or even exceed) the principal's expectation $Belief_P$ (see, e.g., Bernheim, 1994), suggesting a positive correlation between $Belief_A$ and effort. Finally, subjects complete a questionnaire that covers socio-demographic characteristics, image concerns as well as measures of attitudes towards trust, risk, reciprocity, and (for a subsample of participants) social preferences and locus of control.

We conducted the original experiment in the Ulm Laboratory for Economics and Social Sciences (ULESS) at Ulm University (Germany). Data collection took place in 2015 (first wave) and 2017 (second wave). Overall, 345 subjects participated (165/180 in the first/second wave). We replicated the experiment in the WU lab in Vienna (Austria) in 2023 with 288 subjects. The experiment was programmed and conducted with z-Tree (Fischbacher, 2007) and recruitment was organized with hroot (Bock et al., 2014) in Ulm and ORSEE (Greiner, 2015) in Vienna. Participants could take part in one session only. Each session lasted about 45 min. Average earnings were 7.69 euros in Ulm (7.64/7.74 euros in the first/second wave) and 7.61 euros in Vienna.[14] In addition, subjects received a show-up fee of 4/6 euros in the first/second wave in Ulm, and 6 euros in Vienna.[15]

[13] Again, to avoid revealing roles by idle subjects in the lab, we ask the agent who makes the hypothetical effort decision to submit the second-order belief about the principal's guess about his hypothetical effort choice (cf. Footnote 12). However, we abstain from using the hypothetical first and second-order beliefs in our further analysis.

[14] In the second wave in Ulm, the experiment took about 15 min longer as we additionally elicited social preferences at the end of the experiment (coming as a surprise) by using the Equality Equivalence Test (Kerschbamer, 2015). In this test, subject earned an additional amount of 4.09 euros on average.

[15] The different show-up fees in the first and second wave in Ulm are due to a change in lab policy.

3. Behavioral predictions and hypotheses

If agents were purely self-interested, the information about the selection should not matter for them and we should neither observe treatment effects nor an impact of the selection when agents are informed about it. Self-interested agents would always choose the smallest possible effort level. Note also that social preferences like, e.g., altruism (e.g., Andreoni & Miller, 2002) or outcome-based fairness (e.g., Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999) that do not consider intentions behind individuals' actions cannot explain behavioral differences when receiving (no) information about the principal's selection. Considering (perceived) intentions is crucial as otherwise an agent's action would only depend on entering the gift-exchange game but not on (not) knowing to have been deliberately selected (or not).

To make behavioral predictions and derive our hypotheses, we assume that agents care about the principal's kindness. We do, however, take a simplistic view, as we abstract from different motivations of selecting an agent but merely focus on the selection being a kind act in itself. Models of reciprocity often consider reciprocal behavior by incorporating intentions in form of higher order beliefs (e.g., Dufwenberg & Kirchsteiger, 2004; Falk & Fischbacher, 2006; Rabin, 1993; Sebald, 2010). In contrast to these equilibrium models, Cox et al. (2007) propose a more tractable preference model which avoids many issues emerging from higher order beliefs. In particular, this model incorporates the agent's resulting emotional state – conditional on the previous behavior of the principal – into the agent's choice. In online Appendix C, we analytically derive our hypotheses based on a simplified version of Cox et al. (2007).

In our case, the only action by the principal is her selection of an agent. More precisely, we have to consider perceived intentions when the agent who enters the gift-exchange game has been deliberately selected by the principal or not. Agents may perceive (not) being selected as a (un)kind action because the principal thereby allocates a higher (lower) expected payoff to the agent:

Agent's expected payoff if selected $\geq$ *Agent's expected payoff it not selected*

$$\iff 0.6 \cdot (50 - c(e)) + 0.4 \cdot 20 \geq 0.6 \cdot 20 + 0.4 \cdot (50 - c(e))$$

$$\iff 30 \geq c(e).$$

This inequality holds for any effort level $e \in \{0, 1, \ldots, 10\}$. Selecting an agent and therewith demonstrating the intention to allocate a higher payoff to him should be perceived as the kinder action and be rewarded by higher effort. Thus, we state the following first hypothesis for the case that the information about the principal's selection is available (*Info* treatment):

Hypothesis 1. *Efforts are higher when agents know they have been selected (A_s) than if they know they have not been selected ($A_{\neg s}$).*

In *NoInfo*, an agent never learns whether he has been selected or not, i.e., the effort choice should obviously not differ between these two cases as they cannot be differentiated. Nevertheless, an agent may form a belief about whether he has been selected. As long as he is uncertain about whether or not he has been selected, he will perceive the action of the principal (in expected terms) as less (more) kind than when he knows he has (not) been selected in *Info*. Therefore, we expect higher (lower) efforts when agents know they have (not) been selected than if they have no information about the selection:

Hypothesis 2. *Efforts are higher when agents know that they have been selected in Info than efforts in NoInfo where agents have no knowledge about having been selected or not.*

Hypothesis 3. *Efforts are lower when agents know that they have not been selected in Info than efforts in NoInfo where agents have no knowledge about having been selected or not.*

The basis of all hypotheses on behavior in the different information conditions is agents' reciprocal inclination. The literature on social preferences prominently establishes gender differences in reciprocity. Predominantly, women behave more reciprocally than men (e.g., Chaudhuri & Gangadharan, 2003; Croson & Gneezy, 2009). This literature also finds that women are more trustworthy than men (e.g., Chaudhuri & Gangadharan, 2007; Rau, 2011). Indeed, in the gift-exchange game, reciprocity and trustworthiness cannot be clearly separated as an agent's response in excess of providing minimum effort can indicate either. Building on the evidence of gender differences in both, reciprocity and trustworthiness, we hypothesize that in our experiment, efforts of female agents will be higher than efforts of male agents.

Hypothesis 4. *Female agents choose higher efforts than male agents.*

4. Results

We start our analysis by having a brief look at the principal's selection of one of the two agents to choose the effort. Prior to the selection, the principal learns which of the alternatives the two agents have chosen in stage 1. Note that in our experiment the principal's selection should not be based on task-related characteristics – which would mean being based on characteristics that signal an agent's propensity to reciprocate. Nevertheless, agents should not think the selection is purely random.

We find that the agents' choices in stage 1 are not correlated with their efforts in stage 2 (rank correlations with Bonferroni adjustment). Thus, principals cannot identify (and therefore select) more reciprocal agents on the basis of the chosen alternatives. Indeed, being chosen by the principal cannot be predicted by any of the agents' choices in stage 1. Principals may, however, select the agent who is more similar to themselves, which could in turn establish a sense of group membership between the chosen agent and the principal (Tajfel et al., 1971). To investigate whether the principal's selection of an agent is based on the similarity of choices between principal and agent in stage 1, we calculate the congruence of choices, i.e., the proportion of identical answers for each principal–agent pair in each group. Selected agents have a significantly higher level of congruence than non-selected agents (60.8% vs. 54.0%, $p < 0.01$, Mann–Whitney-U Test, henceforth: MWU).[16] We take this as an indication that the selection is deliberately made.

For the Vienna subsample, we additionally asked subjects in the concluding questionnaire whether they think that the principal's selection was (rather) random or deliberate (on a 4-item Likert scale). Here, only 20.3% of all agents and 19.8% of agents who make the payoff-relevant effort choice state the belief that the principal's selection was purely random. The result is similar for principals who state their belief about the other principals' selections (22.9%). Agents and principals do not significantly differ in their answers ($p = 0.61/0.60$, two-sided tests of proportions, henceforth: prtest). Thus, the vast majority of subjects does not think the selection is purely random.

4.1. Effort choice

We restrict our analysis to the payoff-relevant effort choices, i.e., there is only one observation within each group of one principal and two agents. Prior to our main analysis, we examine whether there are systematic differences between the subsamples, i.e., the first and second wave in Ulm and Vienna.[17] As the distributions of efforts do not differ between the subsamples (median test, exact $p = 0.22$),[18] we subsequently pool the data.

Overall, we observe that 72.0% of the effort choices are strictly positive, i.e., agents show non-selfish behavior similar to previous studies (see, e.g., Charness, 2004; Fehr et al., 1998). To analyze whether the information on having been selected impacts behavior, we differentiate in *Info* between the two possible information conditions: the agent either learns that he has been selected, *Info*(A_s), or that he has not been selected, *Info*($A_{\neg s}$). In Table 2, we report summary statistics of the effort choice in both treatments, *NoInfo* and *Info*, disaggregated by information condition in *Info*. We illustrate the corresponding distribution of effort choices in Fig. 1.[19]

In *Info*, the mean effort is similar regardless of whether the agent learns that he has been selected or not (3.9 vs. 3.3, $p = 0.13$, MWU[20]). The share of zero efforts, however, significantly differs between information conditions (32.3% in *Info*($A_{\neg s}$), 16.7% in *Info*(A_s), $p = 0.05$, prtest).

Similarly, efforts in *Info*(A_s) and *NoInfo* do not significantly differ (3.9 vs. 3.3, $p = 0.10$, MWU). But again there are significant differences at the extensive margin as the shares of zero efforts differ (16.7% vs. 36.6%, $p < 0.01$, prtest). When we compare efforts in *Info*($A_{\neg s}$) with *NoInfo*, we do not observe significant differences, neither overall (3.3 vs. 3.3, $p = 0.47$, MWU) nor at the extensive margin (32.3% vs. 36.6%, $p = 0.60$, prtest).

To analyze reciprocal reactions to (in)transparency of the principal's selection more deeply, we run Tobit regressions (left censored at $e = 0$ and right censored at $e = 10$), where we regress the effort choice on a treatment dummy *Info* (= 1 for *Info*) and an interaction term for the dummies *Info* and having been *selected* by the principal (*selected* = 1, if agent has been selected by the principal). Note that we do not include the variable *selected* as a main dummy as this would imply that we distinguish between selected and not-selected agents also in *NoInfo*. In specification (2), we include a gender dummy *Male* (= 1 for men). In specification (3), we add the agent's second-order belief (*Belief*$_A$) as control and in specification (4), we include additional controls: measures of image concerns, attitude towards trust and reciprocity.[21] The regression results are shown in Table 3.

The regression confirms that overall there is no major effect of the information condition on effort levels. The results show at most a marginally significant effect of being selected compared to not-selected in *Info* (captured by the coefficient *Info*selected*). Efforts are higher when having been selected in *Info* than in the *NoInfo* treatment (*Info+Info*selected=0*, $p = 0.05/0.03$ in specifications (3)/(4)), but not lower when not having been selected in *Info* than in the *NoInfo* treatment (captured by *Info*). Taking into account agents' second-order beliefs, i.e., that the agent's belief about the principal's effort expectation may affect the effort choice, in specifications (3) and (4) reveals

[16] This also holds true for each subsample (first and second wave Ulm and Vienna), separately.

[17] We report and discuss a balancing table with corresponding tests for the incentivized choice variables and individual characteristics that are available across all subsamples in online Appendix D.

[18] We use the extension of the median test for more than two independent samples as it is most applicable in cases where observations cannot be completely ranked, like with censored data (Siegel, 1956). Conducting the tests separately for treatments or using Kruskal–Wallis tests instead does not indicate significant differences either.

[19] In *NoInfo*, we do not differentiate between having been selected by the principal or not, because here, agents are not aware of having been selected or not, i.e., their effort choice cannot be influenced by any information about the principal's selection. Nevertheless, the mean effort choices in *NoInfo* differ between agents who have been or have not been selected (4.09 vs. 1.96, $p < 0.01$, two-sided MWU). This is indeed driven by the Ulm sample and may be regarded as difference by chance.

[20] As the hypotheses are directional, we report p-values from one-sided tests in the following unless stated otherwise.

[21] For the explanations and calculations of the control variables see online Appendix B. Moreover, we show in online Appendix F that all effects are unchanged when including dummies for the second wave in Ulm and for Vienna.

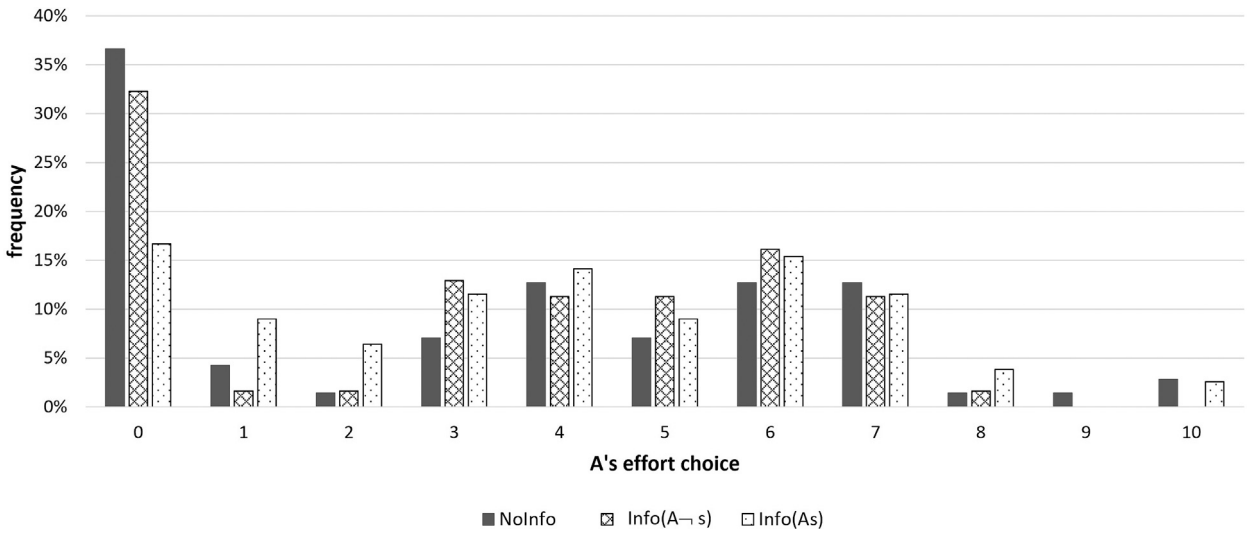

Fig. 1. Distribution of effort choices.

Table 2
Effort choice by treatment and information condition.

Treatment	Mean effort (Std.dev.)	Share of zero efforts (in %)	Mean effort > 0 (Std.dev.)	Observations	
				Overall	effort> 0
NoInfo	3.3 (3.1)	36.6	5.2 (2.2)	71	45
Info	3.6 (2.7)	23.6	4.7 (2.0)	140	107
$Info(A_s)$	3.9 (2.7)	16.7	4.6 (2.2)	78	65
$Info(A_{\neg s})$	3.3 (2.7)	32.3	4.9 (1.6)	62	42

Table 3
Tobit estimations of effort choice e (censored at 0 and 10).

Variables	(1)	(2)	(3)	(4)
Constant	2.516***	2.593***	0.261	−3.234
	(0.550)	(0.597)	(0.606)	(2.103)
Info	0.095	0.098	0.132	0.151
	(0.730)	(0.728)	(0.615)	(0.615)
Info*selected	0.960	0.963	0.938*	1.001*
	(0.640)	(0.641)	(0.564)	(0.558)
Male		−0.168	−0.068	−0.007
		(0.546)	(0.455)	(0.461)
$Belief_A$			0.687***	0.655***
			(0.087)	(0.087)
Additional controls	No	No	No	Yes
Observations	211	211	211	211
Left-/Right-/Un-censored:		59/4/148		
Pseudo-R^2	0.004	0.004	0.076	0.079

Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4
Probit estimations of effort choice.

Variables	(1)	(2)	(3)	(4)
Constant	0.342**	0.387**	−0.350	−1.617
	(0.152)	(0.174)	(0.216)	(1.032)
Info	0.119	0.121	0.113	0.116
	(0.225)	(0.225)	(0.251)	(0.253)
Info*selected	0.507**	0.504**	0.658***	0.667***
	(0.237)	(0.237)	(0.254)	(0.258)
Male		−0.095	−0.025	−0.018
		(0.186)	(0.204)	(0.216)
$Belief_A$			0.238***	0.236***
			(0.048)	(0.049)
Additional controls	No	No	No	Yes
Observations	211	211	211	211
Pseudo-R^2	0.034	0.035	0.214	0.221

Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Dependent variable: 1 - positive effort, 0 - zero effort.

indeed a significantly positive correlation between the belief and the chosen effort.[22]

We additionally analyze the effects of transparency on the extensive margin by conducting Probit regressions where the dependent variable is an indicator for positive effort.[23] The results are shown in Table 4.

The regression confirms that knowing to have been selected increases the likelihood of choosing a positive effort compared to knowing to have not been selected (*Info*selected*) as well as to *NoInfo* (*Info+Info*selected=0*, $p < 0.01$ for all specifications). Similar to before, the agents' second-order beliefs are significantly positively correlated with the likelihood to choose a positive effort.

In sum, we find support for Hypothesis 1 only on the extensive margin, but not considering effort levels overall: knowing to have been selected increases the likelihood of providing positive effort relative to $Info(A_{\neg s})$, but it does not translate to higher effort in general. Knowledge of the selection, however, increases effort (overall as well as on the extensive margin) relative to *NoInfo*, which supports Hypothesis 2. We find no support for Hypothesis 3, though, as efforts in $Info(A_{\neg s})$ are not lower than in *NoInfo*, neither overall nor at the extensive margin. Also, efforts are not higher in $Info(A_{\neg s})$ than in *NoInfo* which would be consistent with overcompensating the negative signal. Note that

[22] One could argue that the belief is influenced by the previous effort choice, but since we incentivized the belief (as described above), we assume this to be unlikely.

[23] We restrict our analysis in the main text to overall efforts and the extensive margin and refer to online Appendix E for a regression analysis on the intensive margin, i.e., effort levels contingent on being positive. On the intensive margin, we do not find any significant effects. Note that the analysis of the intensive margin suffers from an endogeneity problem as the share of subjects who choose positive efforts significantly differs across information conditions.

Table 5
Effort choice by treatment, information condition, and gender.

Treatment	Mean effort (Std.dev.)	Share of zero efforts (in %)	Mean effort > 0 (Std.dev.)	Observations overall	Observations effort> 0
NoInfo					
Men	3.0 (3.1)	42.4	5.2 (2.1)	33	19
Women	3.6 (3.1)	31.6	5.3 (2.2)	38	26
Info					
Men	3.8 (2.9)	23.5	4.9 (2.2)	68	52
Women	3.5 (2.6)	23.6	4.6 (1.8)	72	55
Info(A_s)					
Men	4.3 (2.7)	10.5	4.8 (2.3)	38	34
Women	3.5 (2.7)	22.5	4.5 (2.2)	40	31
Info($A_{\neg s}$)					
Men	3.0 (3.0)	40.0	5.06 (2.0)	32	18
Women	3.6 (2.4)	25.0	4.83 (1.3)	30	24

it is unlikely that heterogeneity in reactions drives the null result as the variance of efforts in *Info($A_{\neg s}$)* is similar to *NoInfo* and *Info(A_s)* (see Table 2 and Fig. 1). The lack of reaction in response to not being selected could be due to individuals holding motivated beliefs à la Bénabou and Tirole (2002) that are more resistant to negative as compared to positive information. Positive signals may then be more readily incorporated into decision making leading to stronger reactions than negative signals (e.g., Eil & Rao, 2011; Grossman & Owens, 2012).

We also find no support for Hypothesis 4 as we do not observe a significant coefficient of the male dummy, neither in the Tobit nor in the Probit estimations. Thus, there is no indication that women choose higher efforts or are more likely to choose a positive effort than men.

4.2. Gender-specific behavior

Beyond suggesting that women are more reciprocal then men, the literature on gender differences in social preferences also suggests that women react stronger to social stimuli than men (Espinosa & Kováří, 2015), are more affected by the experimental conditions (e.g., framing, see Kamas & Preston, 2015), and are more responsive to changes in parameters of the experimental setting than men (Eckel & Grossman, 1996). The information about having been selected or not could be interpreted as a type of social stimuli or can change the framing of the decision situation and thus induce gender-specific behavior. If men's and women's reactions to transparency go in the same direction, but women react to a stronger extent, then the gender difference in effort may be more pronounced within each information condition than in the aggregate. This could also explain why we do not find support for Hypothesis 4. Therefore, we complement our analysis by examining gender-specific reactions to the information conditions. In online Appendix C, we suggest how a gender-specific response to transparency can be derived in the simplified framework of Cox et al. (2007).

Table 5 reports gender-specific summary statistics of effort choices. Note that we find no significant gender differences in efforts in any treatment or information condition. Nevertheless, men and women do indeed react differently to the information conditions as re-running our regressions separately for men and women (see Tables 6 and 7) reveals.

For men, the results are consistent with the previous findings: the knowledge of having been selected increases the effort of men, both overall and at the extensive margin, as opposed to the knowledge of not having been selected (*Info*selected*). Men's effort is also higher when knowing to have been selected in *Info* than in *No-Info* (*Info+Info*selected=0*, $p < 0.03$ throughout specifications). For women, we see no such effects. Moreover, we observe that the second-order belief ($Belief_A$) is positively correlated with the (likelihood of choosing a positive) effort for both, men and women. Testing for gender differences in the level or distribution of second order beliefs does not reveal any significant results. Hence, second order beliefs do not help explaining the observed gender differences.

In sum, it seems that the observed effect of transparency of the selection decision between equally suitable agents is driven by men only. Thus, the behavioral reactions of men are at least partially in line with the predictions of intention-based reciprocity (supporting Hypotheses 1 and 2, but not 3), while the behavioral response of women cannot be reconciled with it. In stark contrast to men and to the literature on gender specific-reactions to social stimuli, women do not seem to react when receiving the positive signal of having been selected.

What could drive the observed heterogeneous treatment effects with respect to gender? In our experiment, the principal chooses the agent who is more like herself 74.9% of the time, which is in line with in-group favoritism/out-group discrimination (e.g., Tajfel, 1982). If subjects expect this selection behavior, the observed reactions to (not) being selected may also be driven by gender-specific responses to in-group favoritism. We do not think, however, that in-group favoritism is a mechanism to explain our findings: only men but not women exert higher effort when having being selected, while evidence in the literature suggests that, if anything, women show higher in-group favoritism (Rudman & Goodwin, 2004).

One possible explanation of the different reactions of women and men to having been selected by the principal rests on gender-specific attribution styles. Remember that our hypotheses rely on the agent's perception of being selected by the principal as kind. Attribution styles, however, could influence this perception. On the one hand, when the selection is attributed internally (i.e., to own characteristics rather than to good luck), the principal's selection can be perceived as a kind act. On the other hand, when the selection is attributed externally (i.e., to good luck), the kind intentions of the principal become diffused.

To measure the attribution style, we elicit the subjects' external and internal locus of control in the Vienna sample by taking the same measure as used in the German socio-economic panel (see online Appendix B.1, 8 questions with 7-point Likert answer scale, e.g., Specht et al., 2013). Locus of control is seen as a relatively stable personality trait that refers to the extent to which people believe that their own ability or personality is a decisive factor for their life (internal locus of control) or that luck or exogenous forces are mostly at work (external locus of control) (Rotter, 1966).

We indeed find that men are more internal in their locus of control than women (5.3 vs. 5.0, $p = 0.03$, two-sided MWU), while women are more external than men (3.9 vs. 3.4, $p < 0.01$, two-sided MWU), which is well in line with psychological evidence of a gender gap in locus of control (e.g., Sherman et al., 1997). In turn, this may induce gender-specific reactions to being selected.

In addition to demonstrating gender differences in attribution styles, we want to corroborate the link between agents' attribution style and the perception of the principal's selection decision. To do so, we exploit answers to the (unincentivized) question on whether agents expect that they will be selected by the principal (0=no, 1=yes), which was posed before agents learn whether they will make the payoff-relevant effort choice in stage 2. We regress this expectation on internal and external locus of control (*ILoC* and *ELoC*, resp.) and find substantially different results for men and women (see Table 8).

The results show that the less externally attributing men are, the more likely they expect that the principal will choose them (*ELoC+ELoC*Male=0*, $p \leq 0.01$, both specifications). This supports the interpretation that men expect to be chosen due to personal characteristics rather than due to chance and perceive the principal's selection accordingly as kind. For women, however, the relation is different: the less internally attributing women are, the more likely they expect that the principal will choose them (*ILoC*, $p = 0.04$, both specifications). This may indicate that women expect to be chosen due to lucky circumstances rather than intentionally (based on their characteristics) thus diluting the perception that the principal's selection is a kind act.

Table 6
Tobit estimations of effort choice e for men and women (censored at 0 and 10).

Variables	men			women		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	1.944**	−0.364	−3.332	3.000***	0.901	−1.988
	(0.857)	(0.716)	(2.996)	(0.708)	(0.865)	(3.023)
Info	0.091	0.175	0.417	0.124	0.121	−0.079
	(1.170)	(0.854)	(0.853)	(0.908)	(0.850)	(0.813)
Info*selected	2.114**	1.717**	1.385*	−0.095	0.123	0.298
	(0.991)	(0.821)	(0.761)	(0.822)	(0.764)	(0.760)
$Belief_A$		0.756***	0.748***		0.592***	0.533***
		(0.117)	(0.111)		(0.131)	(0.130)
Additional controls	No	No	Yes	No	No	Yes
Observations	101	101	101	110	110	110
Left-/Right-/Un-censored		30/2/69			29/2/79	
Pseudo-R^2	0.015	0.165	0.111	0.001	0.051	0.064

Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7
Probit estimations of effort choice for men and women.

Variables	men			women		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.191	−0.550**	−1.370	0.480**	−0.178	−1.918
	(0.221)	(0.275)	(1.597)	(0.213)	(0.294)	(1.654)
Info	0.062	0.064	0.180	0.195	0.166	0.025
	(0.321)	(0.369)	(0.385)	(0.322)	(0.354)	(0.350)
Info*selected	0.999***	1.200***	1.044***	0.081	0.240	0.314
	(0.360)	(0.385)	(0.404)	(0.328)	(0.349)	(0.357)
$Belief_A$		0.266***	0.307***		0.212***	0.200***
		(0.075)	(0.087)		(0.065)	(0.065)
Additional controls	No	No	Yes	No	No	Yes
Observations	101	101	101	110	110	110
Pseudo-R^2	0.097	0.312	0.383	0.007	0.149	0.206

Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Dependent variable: 1 - positive effort, 0 - zero effort.

Table 8
Probit estimations of the belief to be selected by the principal.

Variables	(1)	(2)
Constant	2.532***	2.809**
	(0.809)	(1.261)
ELoC	−0.152	−0.103
	(0.149)	(0.158)
ILoC	−0.267**	−0.265**
	(0.129)	(0.128)
ELoC*Male	−0.225	−0.317*
	(0.180)	(0.183)
ILoC*Male	0.139	0.175
	(0.131)	(0.133)
Additional controls	No	Yes
Observations	191	191
$Pseudo - R^2$	0.034	0.067

Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Dependent variable: 1 - belief to be selected, 0 - belief to be not selected.

5. Conclusion

In our experimental study, we investigate whether the transparency of a principal's selection of one agent (out of two equally suitable agents) to perform a task affects the agent's reciprocal behavior. To do so, we conduct a modified gift-exchange game with a random move at an interim stage, which either implements or overrules the principal's selection. This allows us to examine how agents who know that they have been selected by the principal react as well as how agents who know they have not been selected react. We also compare behavior to an intransparent setting where agents do not know whether or not they have been selected.

Our results suggest that, if anything, transparency about the selection increases reciprocity. On the one hand, this is good news, as transparency does not seem to be harmful, meaning that efforts do not decline after learning to have not been chosen. It seems that subjects do not react to this negative signal as intention-based reciprocity would predict. One reason may be that individuals are less responsive to negative than to positive information (Eil & Rao, 2011; Grossman & Owens, 2012) in case they hold motivated beliefs (Bénabou & Tirole, 2002). On the other hand, however, the reaction to the information of having been selected is gender-specific in that only men react more reciprocally when they know that they have been selected compared to the case of not having been selected or receiving no information at all. In this domain, men's behavior is well in line with the predictions of intention-based reciprocity, while women's is not.

While we did not expect the transparency of the principal's selection to have a differential effect on men and women, we try to offer an explanation for our results. One possible explanation for our observations may be gender-specific attribution styles (e.g., Deaux & Farris, 1977; Manger & Eikeland, 2000). These imply that women may attribute the fact of having been selected rather to (good) luck, while men attribute it to personal characteristics. The attribution to luck may offset the kind intention behind having been selected and thus women show no increase in reciprocity. The attribution to personal characteristics, in contrast, may amplify the kind intention and thus, men become more reciprocal. While we can corroborate gender-specific attribution styles and their relation to the belief of being selected for a subsample of participants only, we believe that these findings can contribute to an understanding of the differential behavioral effects of transparency on men and women, which are difficult to reconcile with the theoretical predictions of intention-based reciprocity. Future research, however, needs to address the relation between the attribution of selection decisions and reciprocal behavior more explicitly.

To conclude, we find that transparency of selection decisions between agents that are equally suitable for the job induces gender-specific reactions to the information of having been selected. Surprisingly, transparency does not reduce the motivation of employees when they learn that they have not been selected. Consequently, transparency is not harmful. Informing employees (especially male employees) about the principal's decision may even be a good instrument to motivate employees. For instance, when hiring a male employee who was the first choice, it may be a good idea to inform him about this fact to induce (higher) reciprocity, while it does not decrease reciprocity when informing employees that they have not been the first choice.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.socec.2023.102073.

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