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## Article

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Games

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*Suggested Citation:* Karagözoğlu, Emin; Tosun, Elif (2022) : Endogenous game choice and giving behavior in distribution games, Games, ISSN 2073-4336, MDPI, Basel, Vol. 13, Iss. 6, pp. 1-32, <https://doi.org/10.3390/g13060074>

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## Article

# Endogenous Game Choice and Giving Behavior in Distribution Games

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**Abstract:** We experimentally investigated the effects of the possibility of taking in the dictator game and the choices of passive players between the dictator game and the taking game on the distribution decisions of active players. Our main findings support our hypothesis: when the dictator game is not exogenously given but chosen by the receivers (or passive players), this makes them accountable, which leads to less giving by dictators. We also conducted an online survey to gain further insights about our experimental results. Survey participants predicted most of the observed behavior in the experiment and explained the factors that might have driven the predicted behavior using reasoning similar to ours. Our results provide a new perspective for the dependence of giving in the dictator game on contextual factors.

**Keywords:** accountability; dictator game; endogenous game choice; experimental economics; taking behavior

**JEL Classification:** C83; C90; D64; D90



**Citation:** Karagözoğlu, E.; Tosun, E. Endogenous Game Choice and Giving Behavior in Distribution Games. *Games* **2022**, *13*, 74. <https://doi.org/10.3390/g13060074>

Academic Editors: Jan Stoop, Georg Granic and Ulrich Berger

Received: 29 September 2022

Accepted: 27 October 2022

Published: 3 November 2022

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## 1. Introduction

The dictator game is a simple yet useful tool that has been extensively used in the experimental economics literature to answer various questions on other-regarding behavior, fairness, social norms, image concerns, framing effects, property rights, gender differences, etc. The first version of the dictator game appeared in the work of Kahneman et al. [1]. Their game has a binary choice between an even versus uneven split. Forsythe et al. [2] later formalized the game to have a discrete choice set and tested it with real stakes to find that most of the players would give away significant proportions of their endowment. Since then, giving behavior in the dictator game has frequently been interpreted as a reflection of other-regarding preferences. More precisely, the observed behavior is explained using inequality aversion [3–5] and altruism [6,7].

However, further research on the topic studying different variations of the dictator game offers alternative explanations for the observed positive transfers, which are consistent with self-regarding behavior—in particular, playing with earned money instead of a windfall endowment [8–14], earned entitlements [9], higher stakes [15], increased anonymity [16,17], larger social distance between players [18–21], poorer recipients [22,23], and higher social status of the recipient [24,25], which all lead to less giving by dictators. Moreover, conducting the original dictator game experiment with the same subjects repeatedly also causes a decrease in giving over time. For example, Brosig et al. [26] reported that the behavior of participants became closer to the predictions of the mainstream models toward the final rounds.

Indeed, the results from all of these variations of the standard dictator game raise doubts about the interpretation of other-regarding behavior. That said, the giving in the dictator game is still positive in all of these experiments. Can we at least argue that this

residual giving is due to other-regarding behavior? Some researchers have come up with a brilliant variation of the game, which has the potential to uncover new insights to help us answer this question. What if the dictators could take money away from the passive player? Could the presence of this option lead to zero giving?

Cox et al. [27] were the first to introduce the possibility of taking in the dictator game, but their focus was actually on the study of the “moonlighting” game<sup>1</sup>. List [28] and Bardsley [29] more systematically studied whether the positive transfers were driven by other-regarding concerns or by the design of the game itself<sup>2</sup>. Their works focused on the differences in behavior between the dictator game and the “taking” game, in which the choice set of the dictator is expanded to allow for negative transfers. Based on their results, we know that dictators are less willing to make positive transfers to the receiver and that zero giving increases when the possibility of taking money is included in their choice set. As List [28] points out, giving in the dictator game is influenced by social norms, which are further influenced by the choice set. Cappelen et al. [30] also argue that “the choice-set effect captures a fundamental dimension of individual behavior in the dictator game”. As explained in greater detail below, our experiment revolves around extending this idea.

The present paper is inspired by two observations: the external validity critiques directed toward laboratory experiments, and the findings from various dictator game experiments [31,32], which offer different insights/interpretations for the observed giving behavior. First, in most economic experiments, the game to be played is exogenously assigned to the participants. However, this is rarely the case in real-life situations. More precisely, agents, in their daily routine, have some choice or discretion regarding the type of strategic interactions in which they will be involved<sup>3</sup>. Along these lines, a natural question arises: are the results from well-known experiments, where the participants are exogenously assigned to strategic interactions (or treatments), robust to the incorporation of endogenous game choice? To the best of our knowledge, there are only a few studies on endogenous selection in distribution games in the experimental literature. For example, in their working paper, Lambert and Tisserand [33] showed that individuals who are forced to bargain are significantly more aggressive than those who initially choose to bargain, implying that agents behave differently under enforcement of the procedure; this affects the process as well as the outcome. Moreover, in an experimental study by Smith and Wilson [34], participants behaved very differently in voluntary ultimatum games compared to exogenously assigned ultimatum games. More precisely, when responders were offered the opportunity to opt out instead of being forced to play the ultimatum game, the authors observed far higher rates of equilibrium play (including highly unequal splits) compared to binary-choice versions of the ultimatum game. In the present work, we incorporate the possibility of choosing the distribution game to be played into a framework that contains the dictator game and the taking game. As such, our paper is related to two strands of the experimental literature: (i) the interpretation of giving in the dictator game, and (ii) the external validity of results from dictator game experiments.

Some information about the experimental design is in order, although we present this in detail in Section 3. We ran a laboratory experiment with a between-subjects design and three treatments ( $N = 268$ ). The interaction was one-shot. In EX-D, randomly paired participants played the dictator game, to which they knew that they were exogenously assigned. In EX-T, randomly paired participants played the taking game, to which they knew that they were exogenously assigned. In the EN treatment, in each randomly matched pair, the passive participant (or the receiver) chose the game that they would play (i.e., the dictator game or the taking game). The fact that the game they played was the choice of this participant was common knowledge among them. Naturally, we have data on EN-D (endogenously chosen dictator games) and, rather surprisingly, we also have data on EN-T (endogenously chosen taking games) to make some comparisons. Therefore, EX-D and EX-T can be thought of as our control treatments, whereas EN (containing EN-D and EN-T) is our experimental treatment.

Our main hypothesis concerns the differences in giving behavior across EX-D and EN-D. We hypothesized that the mere fact that the dictator game in EN-D is the result of passive players' choice would bring the accountability/agency issues into the picture. Consequently, participants who chose to play the dictator game—despite that choice being very natural—would be held responsible/accountable by the dictators for imposing a restriction on their (i.e., the dictators') choice set (and, hence, the payoff set), and the dictators' giving would be smaller in EN-D than in EX-D. Some of our other hypotheses were that (i) giving would be higher in EX-D (EN-D) than in EX-T (EN-T), (ii) passive participants in EN would choose EN-D more frequently than EN-T, and (iii) there would be no difference in giving behavior between EX-T and EN-T. Our results from the lab experiment clearly confirm our hypotheses. To begin with, we replicated the corresponding results from the works of List [28] and Bardsley [29], i.e., dictator giving was higher in EX-D than in EX-T (Result 1). This result was also valid for the EN-D vs. EN-T comparison (Result 1). Secondly, as we predicted, passive players chose EN-D significantly more frequently than they chose EN-T in the EN treatment (Result 2). In line with our main hypothesis, dictator giving was higher in EX-D than in EN-D (Result 3). Finally, there was no difference in giving between EX-T and EN-T (Result 4). Our regression analyses, using various control variables, confirm these results. We report more detailed results and some auxiliary analyses in Section 4.

We also conducted an incentivized online survey study with a different set of participants ( $N = 296$ ) to gain more insight into the participants' behavior in the experiment. In the survey, after explaining the experimental design to the participants and testing their understanding, we asked them to predict the behavior that we observed in the experiment (via structured questions) and to explain the reasons behind their predictions (via open-ended questions). Our statistical analysis of their answers to the structured questions and text analysis of their answers to the open-ended questions show that not only were they good at predicting the results, but they also came up with arguments/reasons that were in line with the arguments that we used to support our hypotheses.

Our study contributes to two lines of experimental research by showing that a natural, empirically appealing extension of a well-known game (i.e., the dictator game), inspired by a valid external validity critique, leads to significantly different results, and that there is one more reason to be careful when associating dictator game giving with other-regarding behavior. To the best of our knowledge, this is the first paper to introduce endogenous game selection into a dictator game-taking game framework. Our results from the laboratory experiment clearly showed that allowing the game to be played to be endogenously determined (i.e., by the choice of one of the players), rather than exogenously assigning it, made a clear difference. When the dictator game was not the random/exogenous assignment of the experimenter but the choice of the opponent, the dictators in our experiment were significantly more selfish. Moreover, our survey study shows that this behavioral difference is well-predicted and the possible reasons behind it are well-understood, strengthening the message of the paper. In short, one may conclude that as much as involvement in the dictator-game-like interactions that we wanted to model/represent in our experiments was endogenous, we need to take the observations reported in this paper into consideration.

The remainder of this paper is organized as follows: In Section 2, we review the related literature, with a particular focus on endogenous game selection in distribution games. In Section 3, we present the experimental design, some details about implementation, and the hypotheses. In Section 4, we report (i) the main results from the laboratory experiment, (ii) some auxiliary results, and (iii) the results from our online survey. Section 5 ends the paper with concluding remarks and a discussion. In the Appendix, we present our instructions as translated from Turkish (Appendix A), as well as some robustness checks on our analyses (Appendix B).

## 2. Literature Review

As mentioned above, we restricted our attention to two lines of research: (i) papers that study the external validity of giving in the dictator game, and (ii) papers that study distribution games where there is—at least to some degree—an endogenous game selection. Hence, we do not cover papers such as those by Bolton and Zwick [35], Cherry et al. [12], Dana et al. [17], List [28], Bardsley [29], and Andreoni and Bernheim [36] in detail here, although they provided inspiration for the present work.

Levitt and List [37] were among the first to provide answers to some external validity questions related to the results from social preference games in a systematic way. A meta-analysis of the literature (following this paper) on the issue of external validity revealed that the lab–field correlation is quite poor [31]. Franzen and Pointner [38] and Stoop [39] showed that dictator game giving in the lab was related to the behavior in the field. However, according to Wang and Navarro-Martinez [32], laboratory experiments still fail to account for many contextual elements that shape behavior in real life. Related to our work, endogeneity in the choice to take part in a specific type of interaction is just another element that has been mostly absent in the dictator game experiments conducted so far.

There are only a few papers where the participants in the corresponding experiment are given a choice regarding which game they want to play in the dictator game framework. Oberholzer-Gee and Eichenberger [40] offered dictators a choice of playing an unattractive lottery instead of playing the dictator game. This option led to much smaller transfers (giving), with a median transfer of zero. Heinrich et al. [41] modified the dictator game by varying the productivity of taking and giving, where subjects chose the payoff-relevant game. They reported that dictators were more generous when the receiver had the right to choose. Finally, Korenok et al. [42] questioned the existence of *taking aversion* by giving the dictator a choice between a give-only game versus a take-only game and found out that the “moral cost of taking exceeds the moral cost of not giving”.

In these studies, only the dictators’ choice set was expanded, but no choice or decision power was given to the passive player (i.e., the receiver), except for in the work of Heinrich et al. [41]. These authors modified the dictator game in such a way that “the relative price of the payoff to others in terms of the payoff to self is varied”. Thus, there were different payoff distributions, where a low price indicated high productivity, and vice versa. In their setting, the passive players chose between multiple games in order to determine the payoff distribution; even after their decision, it was not certain whether this choice would be relevant in the game or not due to random factors.

The study of Smith and Wilson [34] is related to our study, despite the fact that their experiment was about the ultimatum game behavior and that the players have an option to play the ultimatum game or not, rather than choosing which game they want to play. These authors experimentally studied behavior in exogenously assigned ultimatum games and voluntarily chosen ultimatum games. More precisely, in their experiment, they allowed the responders to opt out, which would bring a payoff of USD 1 to each player. If they did not opt out, then an ultimatum game would be played. The authors reported far higher rates of (subgame perfect) equilibrium play in these voluntarily played ultimatum games. Since our design allows the receiver to choose the game to be played, and the two options have different choice (and payoff) sets available for the dictator, there is room for accountability or responsibility in the experiment, making it different from that of Smith and Wilson [34]. The fact that their second option is a fixed payment rather than a game to be played by the same players makes an important difference.

## 3. Experimental Design, Implementation, and Hypotheses

### 3.1. Experimental Design

As briefly explained in the introduction, we had three treatments in the experiment: (i) exogenously assigned dictator games (EX-D), (ii) exogenously assigned taking games (EX-T), and (iii) endogenously chosen game treatments where the passive subjects in each pair chose to play either the dictator game (EN-D) or the taking game (EN-T). The first

two were our control treatments, which were identical to the baseline and take (USD 5) treatments of List [28], whereas the third was our experimental treatment.

In the EX-D treatment, we had the standard dictator game, where both players were allocated 10 Turkish Lira (TRY)<sup>4</sup>, whereas the dictator was endowed with an additional 10 TRY. The dictator could allocate from 0 TRY to 10 TRY, in 1 TRY increments, of this additional 10 TRY to the person with whom they were paired in the other room, and their decision determined the final allocation. The EX-T treatment was identical to the EX-D treatment except that the choice set of the dictators consisted of allocating an amount from  $-10$  TRY to 10 TRY so, effectively, the dictators could take up to 10 TRY from their partners<sup>5</sup>.

In the endogenous treatment, we provided the receivers (or passive players) with the necessary information regarding both games (i.e., the dictator game and the taking game) and allowed them to choose whichever one they wanted to play<sup>6</sup>. When the receivers made their decision, the dictators were informed about the choices that their partners made as well as the other alternative. Thus, the dictators knew which game they would play and which game they could have played but would not due to the selection of the receiver. The receivers were also aware of the fact that the dictators would know about both games together with the chosen alternative. Hence, the rules of the experiment were common knowledge<sup>7</sup>. This setting allowed us to study the effects of this endogenous selection on the transfers made by the dictators.

### 3.2. Experimental Implementation

We conducted our experimental sessions at Bilkent University with student subjects from various fields of study who were recruited on a voluntary basis. The subjects were at least 18 years old and native speakers; the latter was due to the fact that the experimental instructions were in Turkish. In total, we had 268 subjects in 11 sessions. The experiments were conducted between December 2019 and March 2020. For both the experiments and the online surveys, we received IRB approval from the Bilkent University Ethics Committee and we provided the participants with informed consent forms.

The call for participation was made through an invitation e-mail sent to all students of Bilkent University using the centralized e-mail system of the university. The participants registered to a session of their choice using the link provided in the invitation e-mail. We randomized the treatments across sessions, since performing within-session randomization would have been practically impossible. When the participants arrived at the laboratory, their roles were randomly assigned, and they were anonymously and randomly paired. Dictators and receivers who were placed in different rooms had no contact before, during, or after the session. In each room, the corresponding instructions (for either the dictators or the passive players) were read aloud to make the rules of the game common knowledge. The subjects could only talk to the administrators, and they could participate only once in the experiment (and, hence, in one of three treatments). After the game, the subjects answered some post-experimental questionnaires, which included (a short version of) the Big Five personality test, as well as some demographic questions on gender, age, field of study, monthly disposable income, and number of siblings. Finally, the subjects were paid their earnings in private (and in closed envelopes) by student helpers. An experimental session lasted about 20–30 min. For the full instructions of each treatment in the experiment, please see Appendix A.

For purposes of comparability, we deliberately used procedures and instructions similar to the ones used by List [28]. Consequently, we conducted the experiment on a paper-and-pencil basis. To preserve confidentiality/privacy, we utilized two different rooms for the sessions (as in the setting of List [28]): one for dictators and one for passive players. Therefore, it was necessary to convey the decision of each player to their partner in the other room. This was facilitated as follows: Subjects were provided with envelopes with a pair of numbers on them. After they made their decision by choosing a corresponding action on the piece of paper given to them, they placed it in the envelope and sealed

the envelope. Student helpers collected the envelopes, took them to the other room, and distributed them based on the numbers written on the envelopes.

In a setting where agents can take money from their partners, which might be seen as a rather socially undesirable action, confidentiality plays a crucial role. To preserve anonymity and privacy, (i) the pairing was made anonymously, (ii) separators were used so that the participants could make their choices in private and their decisions were carried inside sealed envelopes, (iii) the subjects were released from the two rooms at different times (separated by five minutes) at the end of the experiment, (iv) the subjects were paid not by the experimenters but by a student helper who had no stake in the research, and (v) the instructions emphasized that the researchers would not see the participants' earnings. All of this was made common knowledge to increase the participants' perceived privacy.

### 3.3. Hypotheses

We first hypothesized, in conformity with earlier papers on the taking game (e.g., List [28]; Bardsley [29]), that giving would be less when the dictator could take money from the pocket of the passive player. One difference is that we predicted that this behavior would be observed regardless of whether the game to be played was decided exogenously or endogenously. This is summarized in Hypothesis 1:

**Hypothesis 1.** *On average, the dictators in the taking game give less than the dictators in the dictator game; that is, (a) giving is less in EX-T than EX-D and (b) less in EN-T than EN-D.*

As we studied the effects of endogenous selection in distribution games, our second hypothesis concerned the choice of the passive players between the two distribution games. Note here that choosing the taking game means that the dictator can take money away from the corresponding subject's pocket. Hence, such a choice can only be rationalized if it is expected to trigger sympathy, friendliness, or similar feelings, which may lead to higher giving<sup>8</sup>. We did not expect to observe this systematically. Hence, we predicted that most of the passive players in the endogenous treatment would choose the dictator game rather than the taking game. This is summarized in Hypothesis 2:

**Hypothesis 2.** *Passive players choose the dictator game (EN-D) more frequently than they choose the taking game (EN-T) in the endogenous treatment.*

Thirdly, we predicted that that giving in the dictator game would be lower when the game to be played was determined by the choice of the receiver compared to when it was assigned exogenously. Our reasoning for this predicted behavior is that the receivers are restraining the choice set of the dictators by selecting the dictator game instead of the taking game. We expected that dictators would take this as a situational excuse<sup>9</sup> for more self-serving behavior and to give less, even though they—as with any other rational agent—would likely have done the same if they were to have been assigned to the receiver role by chance.

Experimental literature on the effects of self-serving biases on giving behavior in distribution games shows that the giving rates fall when agents are provided with situational excuses for selfish behavior (see, for instance, Dana et al. [17]; Rodriguez-Lara and Moreno-Garrido [43]; Regner [44]). Another important factor is accountability. When there is choice, then there is someone who can be held accountable for the negative effects that the choice produces. As Konow [45] argues, accountability has an important role in characterizing perceptions of fairness which, accordingly, affect the allocation decisions of agents. Dictators in our setting who played EN-D had an excuse that could be used to rationalize<sup>10</sup> the behavior of giving less, since the receivers could be held accountable for the playing of the dictator game, which is undesirable from the perspective of the dictators. This is summarized in Hypothesis 3:

**Hypothesis 3.** *On average, the dictators in EN-D give less than the dictators in EX-D.*

Finally, among the pairs playing the taking game in the endogenous treatment, we did not expect to see any significant differences in giving behavior compared to the giving in the exogenously assigned taking game. In line with Hypothesis 2, we did not even expect to see many data points for the case of EN-T, so these observations were expected to constitute only a small portion of our sample. Moreover, the choice of the taking game made by the receivers in this situation does not restrict the strategy space (or payoff space) of the dictators. Therefore, our reasoning for predicting less giving in Hypothesis 3 does not apply for this case.

At this point, the following clarification is worth mentioning: Both games in our setup have a fixed pie size and, as such, we did not have a trust game or a variant of it in our experiment. Hence, there is theoretically no room for reciprocity or trust motives in our experiment. Therefore, we also did not expect an effect in the opposite direction (i.e., the dictators in EN-T giving more than the dictators in EX-T). As a result, we did not expect to see any difference in the giving behavior whether the taking game was exogenously assigned or endogenously chosen. This is summarized in Hypothesis 4:

**Hypothesis 4.** *The average giving in EN-T and EX-T is the same.*

### 3.4. Survey Design and Implementation

As mentioned earlier, we also conducted an online survey between 30 June and 3 July 2020, with 296 students. We conducted it on a well-known survey platform: surveymonkey.com. The aim of this survey was to investigate the predictability of our results and to make a sanity check for our supporting arguments of the hypotheses and interpretations of the experimental results<sup>11</sup>. We explained the procedures of our experiment in detail to the survey participants and asked them to predict the behavior observed in the experiment (in relation to Hypotheses 1–4). In addition to these (binary) prediction questions, we also asked open-ended questions regarding the factors that might have driven the predicted behavior. More precisely, we first asked the participants whether they expected a difference in behavior across certain treatments. Then, to those who answered affirmatively, we asked the direction in which they expected a difference. Finally, we asked them to explain their reasoning behind these predictions. The survey also included some control questions to test the participants' understanding of experimental design, as well as some demographic questions. For the full survey, please see Appendix A. The survey took around 20 min to complete. One randomly chosen participant, among those who correctly predicted all four main results, was paid 200 TRY as an incentive to correctly predict the results of the experiment.

## 4. Analysis and Results

In this section, we present our results and analyses for both the laboratory experiment and the online survey. In presenting the results of the experiment, we first focus on the main results for which we had formulated hypotheses. Later, we present some interesting auxiliary observations. Finally, we present the results from the online survey.

We first present the statistical test results, and then the econometric analyses that incorporate control variables. In the statistical analyses, we conducted *t*-tests for mean comparisons. We also provide Mann–Whitney test results in the footnotes. To compare frequencies, we used Fisher's exact test. Before conducting the *t*-tests, we first checked whether or not the sample variances were equal<sup>12</sup>, and used the appropriate test specification—assuming equal variance or not. In the econometric analyses, we used OLS and probit regressions<sup>13</sup>. In reporting the regression results, we present various regression specifications, e.g., with(out) control variables and with(out) interaction variables. Finally, in the case of directional hypotheses, we report the one-sided test results and significance levels, whereas for the non-directional hypotheses (e.g., for Hypothesis 4) we use two-sided results.

Before moving on to our main experimental results, we should first describe our sample. As mentioned above, our laboratory experiment was conducted with undergraduate and graduate students at Bilkent University (Ankara, Turkey). In total, 268 students participated in the experiment. Our sample consisted of students from almost all fields of study. The percentage of economics majors in the whole sample was 12.4%.

Of our sample, 50.6% was female whereas 48.7% was male<sup>14</sup>. The age of the participants ranged from 19 to 31 years, with a mean of 21.9. The participants were asked to report how many siblings they had as well as to specify their monthly disposable income by selecting from four income categories. Most participants had one sibling (58.8%) and were in the third income category (46.21%), with 1000–2000 TRY disposable income per month. Participants also completed a short version of the Big Five personality test [46], which assigns scores of extraversion, agreeableness, conscientiousness, emotional stability, and openness; we used these as additional controls in the main experimental results.

#### 4.1. Main Experimental Results

##### 4.1.1. Statistical Tests

In Table 1, we report summary statistics on giving behavior in the different treatments<sup>15</sup>. There were 41 pairs in EX-D, 42 pairs in EX-T, 37 pairs in EN-D, and 13 pairs in EN-T.

**Table 1.** Dictators' giving across different treatments.

Treatment	N	Mean	Standard Deviation	Median	Min	Max	Proportion of Positive Offers	Mean Positive Offer
EX-D	41	3.756	2.835	5	0	10	0.756	4.968
EX-T	42	−1.167	6.484	0	−10	10	0.452	5.053
EN-D	37	2.729	2.341	3	0	5	0.622	4.391
EN-T	13	−2.231	7.270	−5	−10	8	0.461	5

Figures 1 and 2 provide the histograms of the transfer decisions in the dictator and taking games, respectively. First, we look at differences across the dictator game and the taking game. In line with earlier work on the taking game, we can see that, on average, the dictators in the taking game give less than those in the dictator game (for both the exogenous and endogenous treatments); this brings us to our first result:

**Result 1:** In line with Hypothesis 1, the average giving is less (i) in EX-T than in EX-D (*t*-test with unequal variances, one-sided,  $p < 0.0001$ ) and (ii) in EN-T than EN-D (*t*-test with unequal variances, one-sided,  $p = 0.0156$ )<sup>16</sup>.

It is fair to say that the differences in average giving between EX-T and EX-D (−1.17 vs. 3.76) and between EN-T and EN-D (−2.23 vs. 2.73) are not only statistically but also economically significant. Looking at the medians in these treatments also provides a clear picture: 0 vs. 5 for EX-T vs. EX-D and −5 vs. 3 for EN-T vs. EN-D.

While we did not have any hypotheses about the *proportion of positive offers* or the *average positive offers*, the results from those statistics were consistent with the literature and with our results<sup>17</sup>. Similar to the mean differences, the proportion of positive offers was also higher in the dictator game than in the taking game for both the exogenous (0.756 vs. 0.452, Fisher's exact test, two-sided,  $p = 0.007$ ) and endogenous treatments (0.622 vs. 0.461, Fisher's exact test, two-sided,  $p = 0.346$ ), while the latter was not statistically significant.

Similar to the results of List [28], we did not find a significant difference in the mean positive offers between EX-D and EX-T (4.968 vs. 5.053, *t*-test with unequal variances, two-sided,  $p = 0.864$ ). We found a slight increase in the mean positive offers between EN-D and EN-T (4.391 vs. 5), but the difference was not significant (*t*-test, two-sided with unequal variances,  $p = 0.482$ ). Thus, the inclusion of the possibility of taking seems to affect the type of dictators who give zero or close to zero.

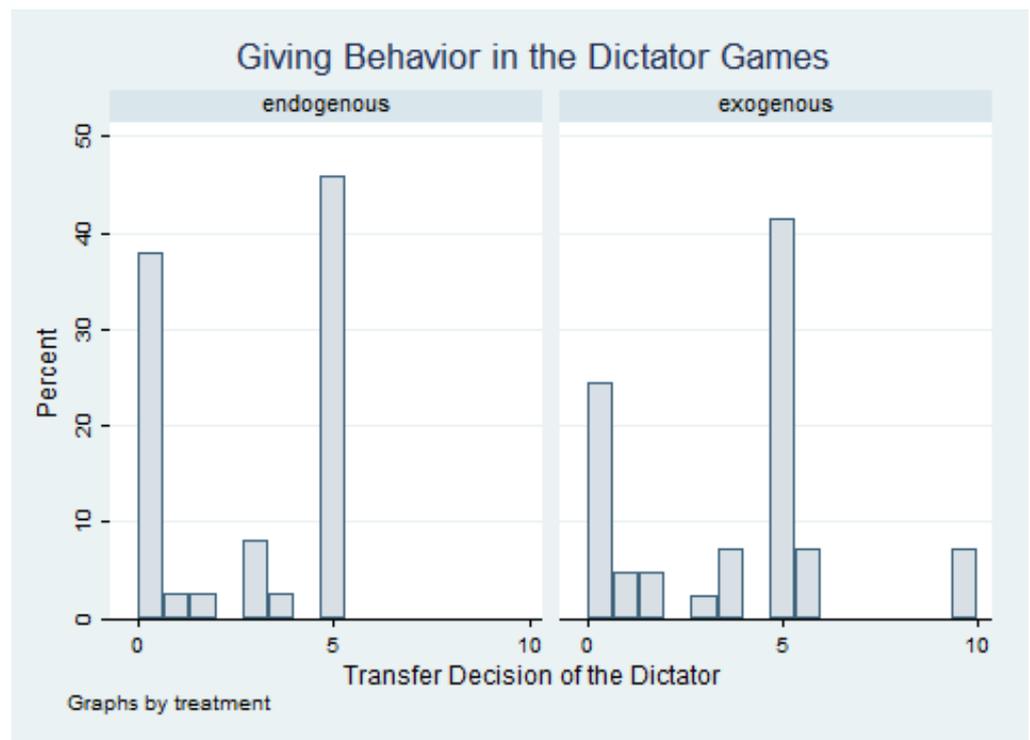


Figure 1. Giving behavior in exogenous and endogenous dictator games.

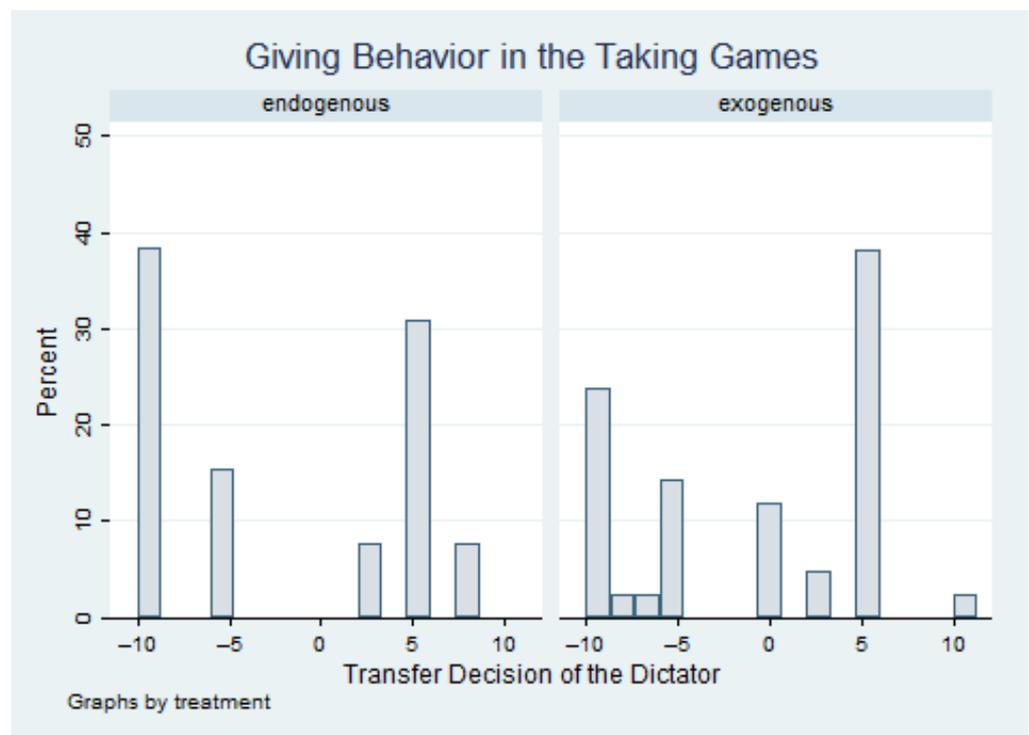


Figure 2. Giving behavior in exogenous and endogenous taking games.

Second, it should be noted that we did not have any control over the distribution of pairs between EN-D and EN-T, since the decision to play those games was made by the passive players in the experiment. This is why the number of pairs was smaller in EN-T than in the other treatment–game combinations. This brings us to our second result:

**Result 2:** In line with Hypothesis 2, EN-D was more frequently chosen (37 times) than EN-T (13 times) in the endogenous treatment (Fisher’s exact test, one-sided,  $p = 0.0004$ )<sup>18</sup>.

Third, we hypothesized that the dictators in EN-D would give less than the dictators in EX-D because of the accountability/responsibility of the passive players in the former. To test this claim, we first checked the variance of the giving data in EN-D and EX-D, which turned out to be statistically no different. Thus, using a two-sample  $t$ -test with equal variances, we can conclude that the mean of dictator giving in EX-D is more than that of EN-D at a 5% significance level, with a  $p$ -value of 0.0436. This leads us to our third result:

**Result 3:** In line with Hypothesis 3, the dictators in EN-D gave less than the dictators in EX-D ( $t$ -test, one-sided,  $p = 0.0436$ )<sup>19</sup>.

Finally, we checked the statistical validity of our fourth hypothesis. We first checked the variance of the giving data in EN-T and EX-T. As the variances were statistically no different from one another, we again used a two-sample  $t$ -test with equal variances. This leads us to our fourth result.

**Result 4:** In line with Hypothesis 4, giving in EN-T and EX-T was not different ( $t$ -test, one-sided,  $p = 0.6173$ )<sup>20</sup>.

Moreover, in line with Result 3 and Result 4, the proportion of positive offers was higher in EX-D than in EN-D—although the difference is not significant (0.756 vs. 0.622, Fisher’s exact test, two-sided,  $p = 0.227$ )—while there was almost no difference between EX-T and EN-T (0.452 vs. 0.461, Fisher’s exact test, two-sided,  $p = 1.000$ ). Similarly, the mean positive offer was higher in EX-D than in EN-D—although the difference was not significant (4.968 vs. 4.391,  $t$ -test with unequal variances, two-sided,  $p = 0.207$ )—while there was no difference between EX-T and EN-T (5.053 vs. 5,  $t$ -test, two-sided,  $p = 0.940$ ).

#### 4.1.2. Regression Analyses

In this part, we check the robustness of the results that we reported in the previous part, by considering various control variables and conducting regression analyses. First, in Table 2, we regress the transfer decision of the dictator on the take dummy (*Take*), which takes the value of one if the game played is a taking game (EX-T or EN-T) and zero otherwise (EX-D or EN-D), as well as the *Male* dummy, which equals one if the dictator is male. In the first two columns, we pool the data from exogenous and endogenous treatments and include a treatment dummy (*Endogenous*) variable that takes the value of one if the game played is chosen endogenously (EN-D or EN-T) and zero otherwise (EX-D or EX-T)<sup>21</sup>. Since the earlier literature provides solid evidence for gender effects in similar settings, we are more interested in the *Male* dummy variable than the other control variables.

In the regressions with other controls (given in columns 2, 4, and 6), we added the dictators’ birth year (*Byear*), number of siblings (*Sibling*), monthly disposable income (*Income*), extraversion (*Extravert*), agreeableness (*Agree*), conscientiousness (*Consc*), emotional stability (*Stable*), and openness to experiences (*Open*) scores, as well as two more dummy variables (*Monday*, which takes the value of one if the session was conducted on a Monday; and *Econ*, which takes the value of one if the dictator’s major is economics). This table provides additional support for Result 1, since the coefficient of *Take* is negative and statistically significant in all specifications.

For the analysis provided in Table 3, we have four specifications. We first regress the dictators’ decisions in the exogenous and endogenous dictator games on the treatment dummy (*Endogenous*) and the *Male* dummy. The results from this analysis are given in the first column of Table 3<sup>22</sup>. Next, in column 2, we add the controls. The third column presents results from a specification without control variables but with an interaction variable between *Endogenous* and *Male*. Finally, the fourth column presents results from the most inclusive specification, which contains both the control variables and the interaction variable.

**Table 2.** OLS results for giving in dictator and taking games.

	Dependent Variable: Dictator Decision					
	Pooled		Exogenous Treatment		Endogenous Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Take	−5.098 *** (0.000)	−5.216 *** (0.000)	−5.127 *** (0.000)	−5.514 *** (0.000)	−5.043 *** (0.001)	−4.498 *** (0.009)
Endogenous	−1.139 (0.197)	−1.035 (0.286)				
Male	−0.702 (0.411)	−0.493 (0.588)	−0.806 (0.484)	−0.252 (0.842)	−0.528 (0.674)	−0.908 (0.509)
Byear		−0.022 (0.908)		0.162 (0.524)		−0.354 (0.242)
Sibling		0.816 (0.115)		0.913 (0.141)		0.537 (0.645)
Income		1.235 ** (0.018)		1.085 (0.144)		0.851 (0.280)
Extravert		0.088 (0.593)		0.285 (0.230)		−0.155 (0.544)
Agree		−0.005 (0.979)		0.028 (0.934)		−0.144 (0.601)
Consc		0.098 (0.559)		−0.192 (0.450)		0.400 * (0.089)
Stable		0.196 (0.195)		0.173 (0.405)		0.170 (0.486)
Open		−0.264 (0.225)		−0.624 ** (0.035)		0.312 (0.372)
Econ		−1.531 (0.205)		−2.126 (0.151)		0.978 (0.697)
Monday		−1.091 (0.365)		−1.088 (0.405)		0.000 (.)
Constant	4.217 *** (0.000)	42.92 (0.908)	4.287 *** (0.000)	−317.6 (0.531)	2.974 *** (0.005)	700.0 (0.248)
<i>N</i>	132	131	83	83	49	48
<i>R</i> <sup>2</sup>	0.207	0.290	0.202	0.333	0.220	0.370
<i>adj. R</i> <sup>2</sup>	0.189	0.211	0.182	0.218	0.186	0.178
<i>prob(F-stat)</i>	0.0000	0.0001	0.0001	0.0026	0.0033	0.0686

Notes: Statistical significance is indicated by stars (\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ );  $p$ -values are also reported in parentheses.

As we can see from Table 3, the coefficient of *Endogenous* is significant in all four specifications (more strongly when the interaction variable and/or control variables are added) with a negative sign, so the dictators give less in EN-D than in EX-D. Moreover, in the third and fourth columns we repeat the same analysis by adding an interaction term for the treatment and gender dummies. The evidence from all four specifications strongly supports Result 3 (see above).

We can also see that dictators' giving is (i) positively correlated with their number of siblings, which is understandable [47], and (ii) negatively affected when the session is conducted on a Monday, although this effect is marginal. Finally, moving to our last result, in Table 4, we replicate the analysis in Table 3 using exogenous and endogenous taking games. As one can clearly see, *Endogenous* does not have a significant effect in any specification, supporting Result 4. A further analysis to support this result can be found in Appendix B.

**Table 3.** OLS results for dictator games.

	Dependent Variable: Dictator Decision			
	(1)	(2)	(3)	(4)
Endogenous	−1.137 *	−1.520 **	−2.595 ***	−3.212 ***
	(0.062)	(0.026)	(0.008)	(0.004)
Male	−0.635	−0.370	−1.780 **	−1.670 *
	(0.307)	(0.574)	(0.039)	(0.070)
Endogenous x Male			2.352 *	2.556 **
			(0.056)	(0.047)
Byear		0.219		0.201
		(0.112)		(0.135)
Sibling		0.993 **		0.840 **
		(0.017)		(0.041)
Income		0.443		0.291
		(0.242)		(0.438)
Extravert		−0.047		−0.056
		(0.687)		(0.626)
Agree		−0.162		−0.226
		(0.285)		(0.137)
Consc		0.141		0.116
		(0.261)		(0.348)
Stable		−0.017		−0.057
		(0.878)		(0.599)
Open		0.018		0.036
		(0.918)		(0.833)
Econ		−0.362		−0.021
		(0.682)		(0.981)
Monday		−1.661 *		−1.775 *
		(0.082)		(0.058)
Constant	4.174 ***	−434.4	4.929 ***	−396.9
	(0.000)	(0.114)	(0.000)	(0.140)
<i>N</i>	77	77	77	77
<i>R</i> <sup>2</sup>	0.056	0.240	0.102	0.286
<i>adj. R</i> <sup>2</sup>	0.030	0.097	0.065	0.139
<i>p-value</i>	0.1188	0.0919	0.0476	0.0415

Notes: Statistical significance is indicated by stars (\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ); *p*-values are also reported in parentheses.

It should also be noted that our results imply important gender effects for the dictator games<sup>23</sup>. The main reason that we are interested in this variable is due to the well-established effects of gender in experimental distributional settings. Engel [48], in his meta-study on dictator games, found that women give significantly more compared to men in the dictator game. Moreover, in their recent paper, Chowdhury, Jeon, and Saha [49] studied the effects of gender on giving behavior using both of the exogenously assigned games in our experimental setup. Similar to our setting, they ran “between-subject dictator games with exogenously specified ‘give’ or ‘take’ frames involving a balanced pool of male and female dictators and constant payoff possibilities” and found that, in the taking framework, females were more generous compared to males. In brief, the experimental literature so far shows that gender plays an important role in determining the giving behavior in the dictator game and its variants. This is why we pay special attention to it in our analyses throughout this paper and, indeed (in conformity with the existing literature), gender was a crucial factor affecting the overall giving behavior as well as the specific treatment effect for the case of the dictator game in our experiment.

To clarify the role of gender in our results and provide further support for our use of the interaction term, in Tables 5 and 6 we examine gender differences both within and between treatments in detail<sup>24</sup>. Table 5 presents differences in the transfer decisions of men and women, in the dictator role, within EX-D and EN-D. Firstly, we can see that women

gave substantially more in EX-D compared to men (4.928 vs. 3.148), and this difference was statistically significant ( $p = 0.0554$ )<sup>25</sup>. On the other hand, women gave less than men did in EN-D (2.333 vs. 2.904), although this difference within the EN-D treatment was not statistically significant<sup>26</sup>. Therefore, if we sum these two opposing effects up in a single gender variable, we are unable to capture these different gender effects in different treatments. The interaction takes care of this issue in our regressions.

**Table 4.** OLS results for taking games.

	Dependent Variable: Dictator Decision			
	(1)	(2)	(3)	(4)
Endogenous	−1.141 (0.596)	−3.058 (0.247)	0.200 (0.939)	−1.610 (0.618)
Male	−0.795 (0.673)	−0.540 (0.797)	0.0824 (0.969)	0.400 (0.870)
Endogenous x Male			−4.082 (0.376)	−3.713 (0.439)
Byear		−0.502 (0.303)		−0.536 (0.276)
Sibling		0.118 (0.914)		0.123 (0.911)
Income		2.846 ** (0.035)		2.672 * (0.051)
Extravert		0.338 (0.397)		0.331 (0.409)
Agree		0.282 (0.569)		0.221 (0.661)
Consc		−0.032 (0.934)		0.012 (0.975)
Stable		0.279 (0.435)		0.301 (0.404)
Open		−0.315 (0.496)		−0.346 (0.459)
Econ		−2.596 (0.383)		−2.743 (0.360)
Monday		−0.685 (0.789)		−0.434 (0.867)
Constant	−0.845 (0.514)	991.8 (0.309)	−1.200 (0.377)	1059.3 (0.282)
<i>N</i>	55	54	55	54
<i>R</i> <sup>2</sup>	0.008	0.250	0.023	0.261
<i>adj. R</i> <sup>2</sup>	−0.030	0.030	−0.034	0.021
<i>p-value</i>	0.8077	0.3583	0.7475	0.3969

Notes: Statistical significance is indicated by stars (\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ); *p*-values are also reported in parentheses.

**Table 5.** Gender differences in dictator games.

Treatment	Gender	N	Mean	Standard Deviation	Median	<i>p</i> -Value
EX-D	Female	14	4.928	2.731	5	0.0554
	Male	27	3.148	2.741	5	
EN-D	Female	15	2.333	2.469	1	0.4785
	Male	21	2.904	2.278	3	

Notes: This table presents the data of the dictator transfer decisions for exogenous and endogenous dictator games. The last column presents the *p*-value of a two-sample *t*-test with equal variances for the gender differences within each treatment.

**Table 6.** Gender differences in taking games.

Treatment	Gender	N	Mean	Standard Deviation	Median	<i>p</i> -Value
EX-T	Female	25	−1.2	6.621	0	0.9684
	Male	17	−1.117	6.480	0	
EN-T	Female	9	−1	7.416	2	0.3829
	Male	4	−5	7.071	−7.5	

Notes: This table presents the data of the dictator transfer decisions for exogenous and endogenous taking games. The last column presents the *p*-value of a two-sample *t*-test with equal variances across genders.

Recall that *Male* equals one when the dictator is male. Thus, the *Endogenous x Male* coefficient in our regressions represents the differential treatment effect (i.e., the difference between EX-D and EN-D) for males<sup>27</sup>, While the *Endogenous* coefficient represents the treatment effect for females. We know from column 3 of Table 3 that the treatment effect for the dictator game is significant for both genders, since both of these variables are significant. Thus, we can say that the treatment effect is more pronounced for women than for men (mean difference of −2.595 vs. −0.244), since women gave more in EX-D but less in EN-D compared to men as dictators. It is not possible to capture this important between-treatments gender difference by including only a single gender variable in the regressions.

When we include the interaction term, only then does the *Male* coefficient imply the mean gender difference in the exogenous treatment, which is significant. Indeed, it is also supportive of our results that the *Male* coefficient is not significant in the first two columns of Table 3, because that variable captures two opposing gender effects at the same time.

This result of a pronounced treatment effect for women compared to men is also consistent with the earlier literature on the effects of gender in altruistic behavior. It shows that the differences in giving behavior between men and women depend crucially on the setting [50–52] and are consistent with women being more sensitive to framing effects than men [53]. However, unlike the earlier studies, here the differences in framing result from endogenous selection.

Constructing the same table for EX-T and EN-T, as shown in Table 6, shows that there was no significant gender difference in giving behavior for the taking games. This is also consistent with our results in Table 4. Moreover, men and women did not differ in terms of the frequency of choosing EN-T compared to EN-D according to the results of Fisher’s exact test. Thus, we cannot explain the receivers’ behavior of choosing the taking game by gender differences either.

#### 4.2. Results from the Online Survey

In this last part of our results section, we present our observations from the online survey that we conducted. First, some simple descriptive statistics: 55.74% of our survey participants were female, whereas 44.26% were male, and their average age was 20.9 years. Most of our participants did not participate in any session of our experiment (85.47%) or take any classes related to game theory (93.92%).

In reporting the results from the survey, we present both the answers the participants gave to the prediction questions and the answers they gave to the open-ended questions. Regarding the former, we used standard statistical tests to reach conclusions. The figures provide the answers to each survey question in percentages, with the corresponding *p*-value for the one-sample *t*-test of difference between the answers. This analysis shows that most of our survey participants predicted all of our results (from the experiment) except for the fourth one.

As for the latter, we conducted a content analysis on the participants’ written answers. For the content analysis, we followed the usual procedure of having two research assistants (RAs) classify the texts. We provided the RAs with the necessary information on the experiment and the survey, together with which concepts to look for in the explanations. They individually analyzed the texts and then worked together to reconcile the mismatches in their categorizations, if possible. In these figures, we only provide the results that

the RAs could agree upon. Again, the figures that we present in this part also contain information about the most frequently appearing categories. The classifications of these explanations show that participants' explanations for the predicted behavior were mostly in line with ours.

When we look at the survey results in detail, in Figures 3 and 4, we can first see that a significant majority of the respondents correctly predicted that the giving behavior would differ between EX-D and EX-T (81.76% in Figure 3) and between EN-D and EN-T (72.97% in Figure 4). Second, again, a significant majority of respondents (89.26% in Figure 3 and 58.33% in Figure 4) correctly predicted that giving would be less in taking games than in dictator games.

Furthermore, we can see that not only do the participants' predictions match our first result, but also their reasoning for the decrease in giving when the possibility of taking is introduced specifically focuses on the differences in the choice set. On the other hand, the explanations for their incorrect prediction (i.e., more giving in EX-T compared to EX-D) are mostly inconsistent with the given answer, or there is a misinterpretation of the situation. Their explanations for "more giving in EN-T compared to EN-D" are meaningful but somewhat too optimistic in terms of expecting reciprocal behavior from the dictators when the taking game is chosen by the receiver rather than the dictator game. As shown earlier in Result 1, this optimistic scenario does not match with what happened in our experiment.

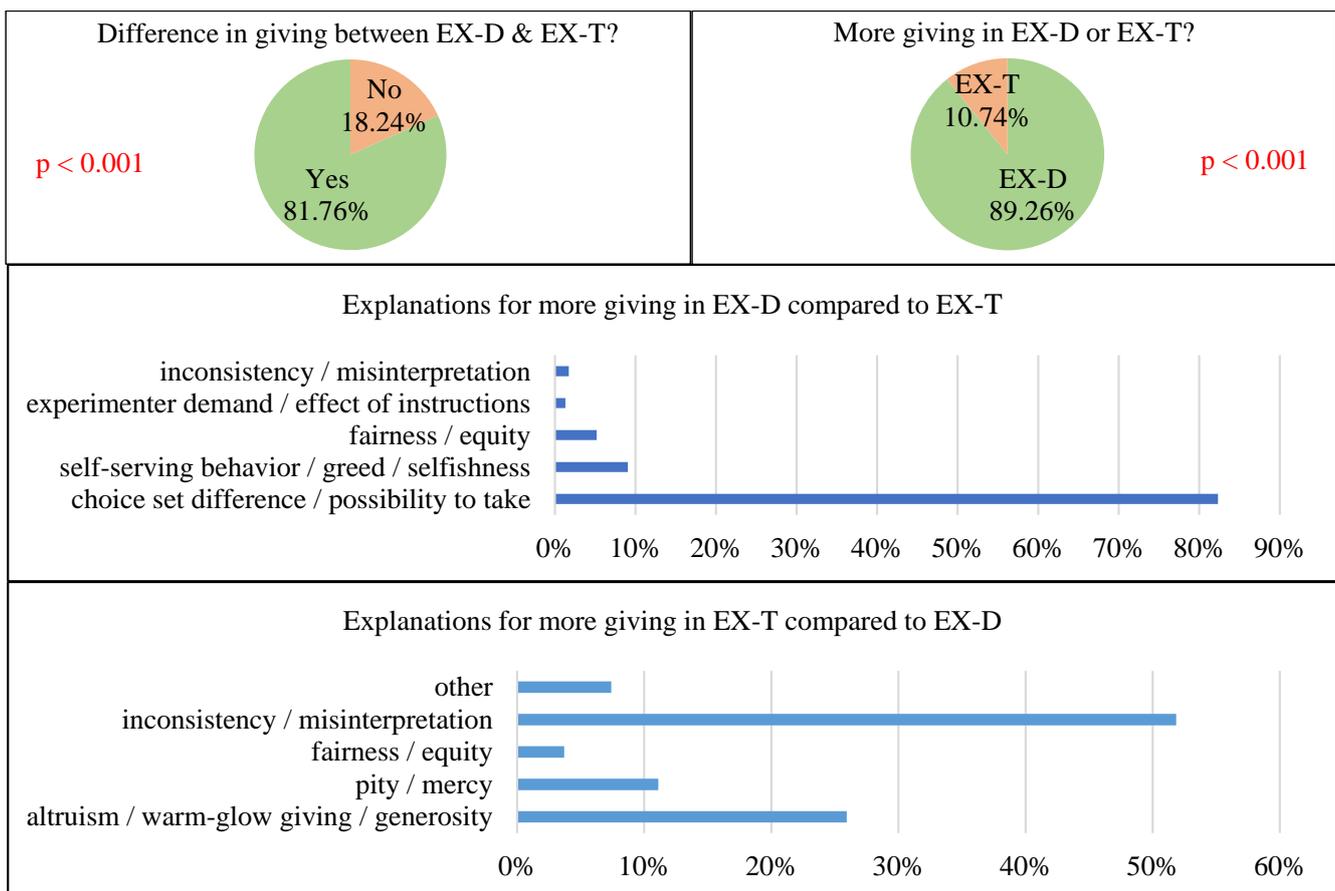


Figure 3. Survey results for Hypothesis 1 (exogenous games).

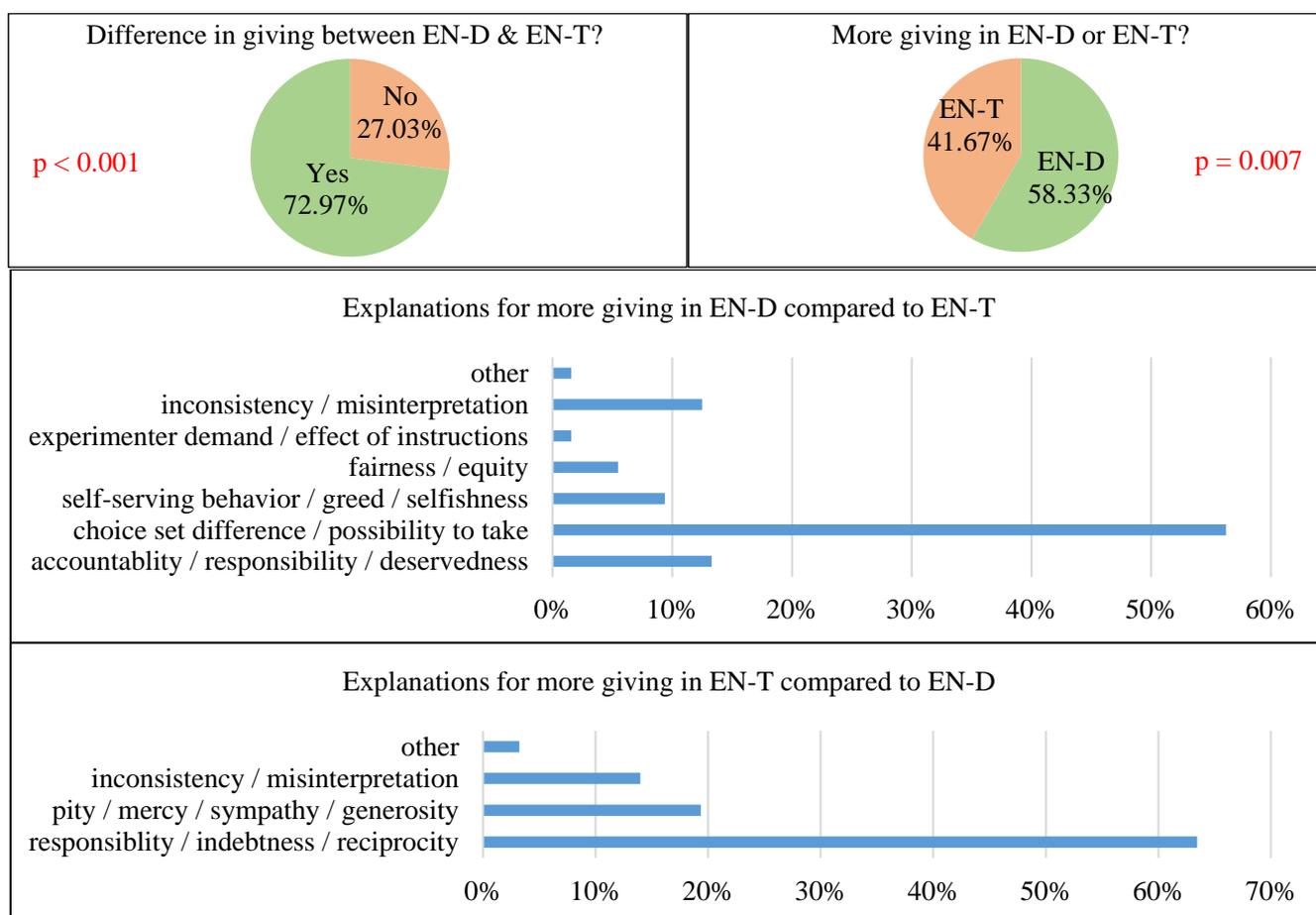


Figure 4. Survey results for Hypothesis 1 (endogenous games).

The questions regarding our second result had the highest correct prediction rates (90.20% and 91.01%), and the survey participants meaningfully attributed the behavior of choosing EN-D rather than EN-T to the rationality of the receivers (due to the impossibility of taking in EN-D) or risk aversion (see Figure 5). Again, explanations for the incorrect prediction were either inconsistent or misinterpreted.

In relation to our main hypothesis, 59.46% of participants predicted that there would be a difference in mean giving between EN-D and EX-D; 63.64% of them correctly predicted the giving to be higher in EX-D than in EN-D (see Figure 6). The explanations for this prediction mainly focused on the accountability/responsibility of the receivers in the restriction of the choice set of dictators or distrust/insincerity resulting from selecting the dictator game in the endogenous treatment<sup>28</sup>.

While explaining the reasoning behind less giving, the survey participants held the receivers responsible for such a natural (and rational) action, which is also consistent with our reasoning. The survey participants themselves explained in the previous question that EN-D should be more frequently chosen due to the differences in choice set between the taking and dictator games, or simply due to the risk aversion of the agents, but in this question some of these same participants (somewhat oddly) found this behavior to be insincere. Some of these explanations also had components related to distrust but, as explained above, the games in our experiment had a fixed pie size, so the participants were not playing a trust game or a variant of it. These explanations do not necessarily fit into the models of fairness with an intentionality notion (e.g., Rabin [54]), because the receivers do not aim at hurting the dictator when they choose the dictator game instead of the taking game. We can see from Figure 5 that they are only trying to protect themselves from being hurt. Even if they are held accountable for this choice by the dictators, it

is hard to explain the change in the giving behavior in EN-D based on intentionality reasoning. Thus, we interpreted these explanations and our results as being more in line with self-motivated reasoning.

It should be noted that neither the rational agent models nor the simple models for other-regarding behavior—such as an equality-seeking agent model—can accommodate the differences in giving behavior shown in this paper between the dictator and taking games as well as between the exogenous and endogenous treatments. A rational agent would simply pick the lowest possible amount of transfer (meaning 0 TRY for the dictator game and −10 TRY for the taking game), regardless of how the played game was assigned. Similarly, an equality-seeking agent would simply pick the transfer of 5 TRY, no matter which game was played and how that particular game was chosen. Our results point towards a reasoning behind the documented differences in giving behavior in which a combination of factors plays a role simultaneously, which is hard to fit into one simple model.

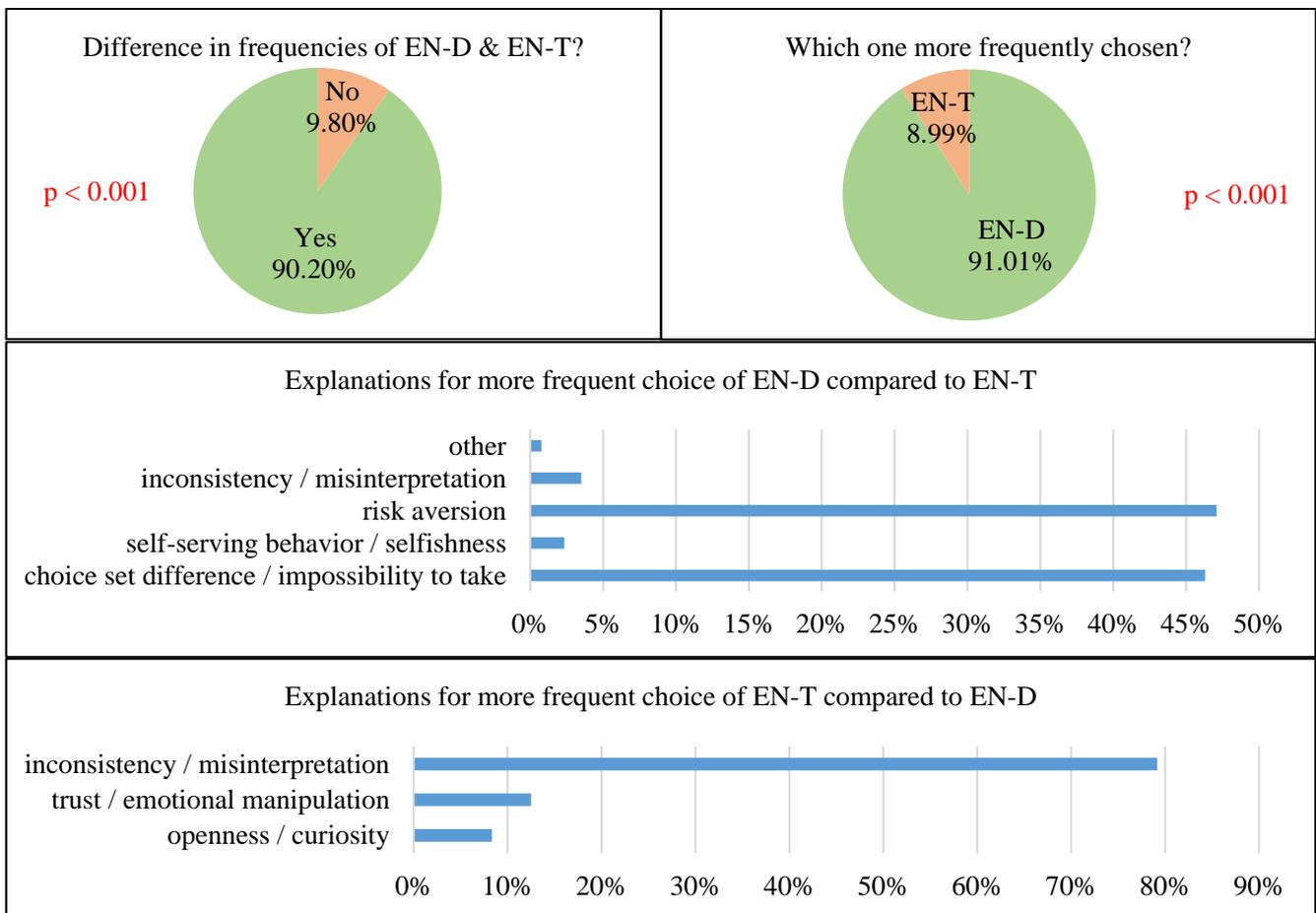
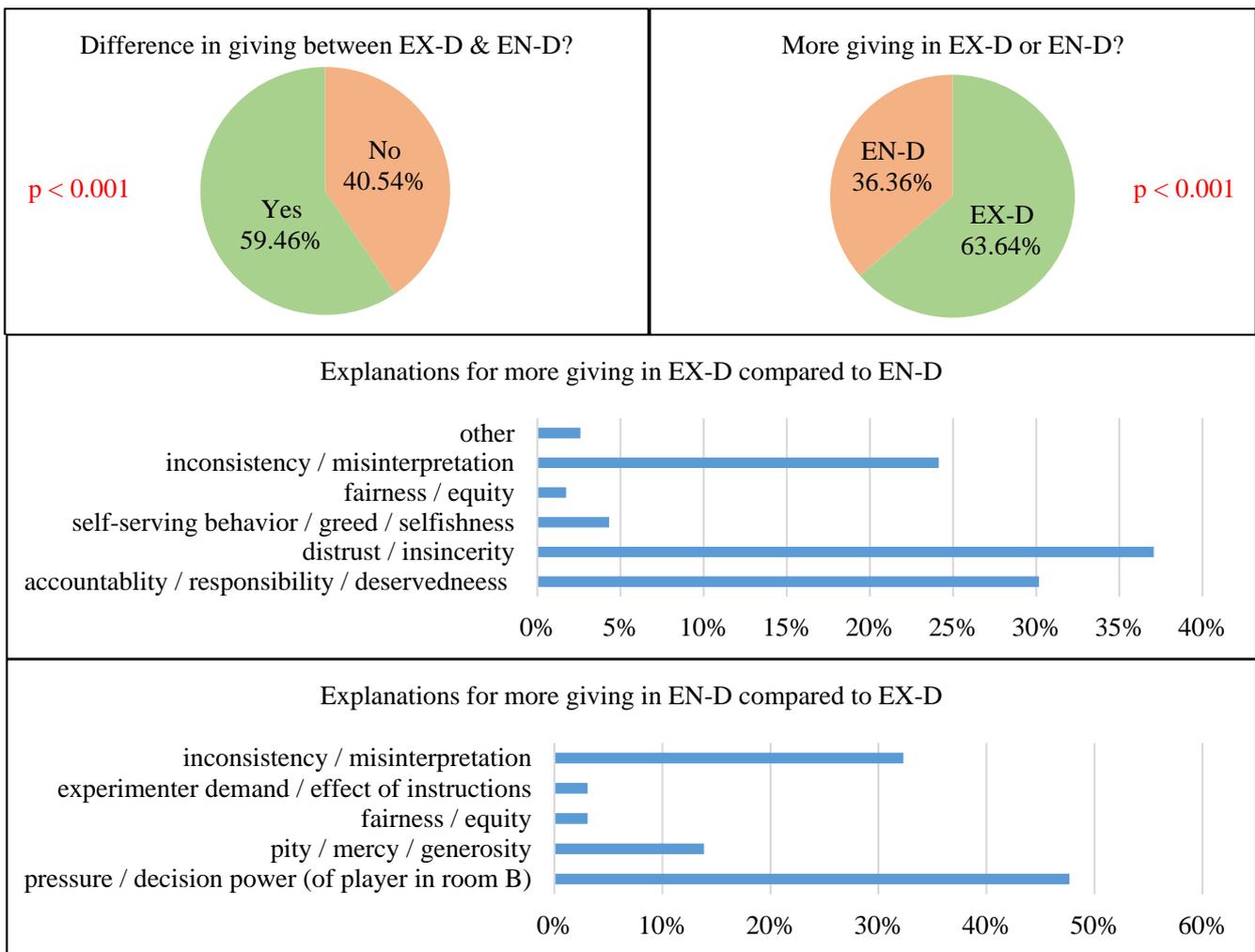


Figure 5. Survey results for Hypothesis 2.



**Figure 6.** Survey results for Hypothesis 3.

Finally, 65.88% of the participants predicted that there would also be a difference in the mean giving of EN-T and EX-T; 75.9% of them expected a higher rate of giving in EN-T, in a reciprocal manner (see Figure 7). However, this was not the case in our experiment. In fact, the mean giving fell in EN-T compared to EX-T, as shown in Table 1, although this difference was not statistically significant. As we can see from Figure 7, their reasoning for more giving in EN-T compared to EX-T focused heavily on explanations related to responsibility, indebtedness, and reciprocity in the case when the passive player chooses the taking game in the endogenous treatment.

Some of the participants’ explanations for why they predicted more giving in EN-T compared to EX-T make it clear that those survey participants have very different reasoning when thinking of the situation in the endogenous treatment. Some of those explanations are as follows (where Treatment 3-2, Treatment 2, Room A participants, and Room B participants refer to EN-T, EX-T, dictators, and passive players, respectively):

*“Choice of Treatment 3-2 shows that the participant in Room B trusts the participant in Room A. The participant in Room A tends to transfer more money so as not to waste this trust”.*

*“ . . . because B-participants might not have chosen Treatment 3-2, but they did. With this in mind, A-participants may be more sympathetic and generous to them, and A-participants may also feel somewhat responsible for sharing money”.*

“... because in Treatment 2, the experimenter determined the path of the experiment. A-participants chose to transfer more money in Treatment 3-2 as B-participants made a sacrifice by putting themselves in a completely vulnerable situation”.

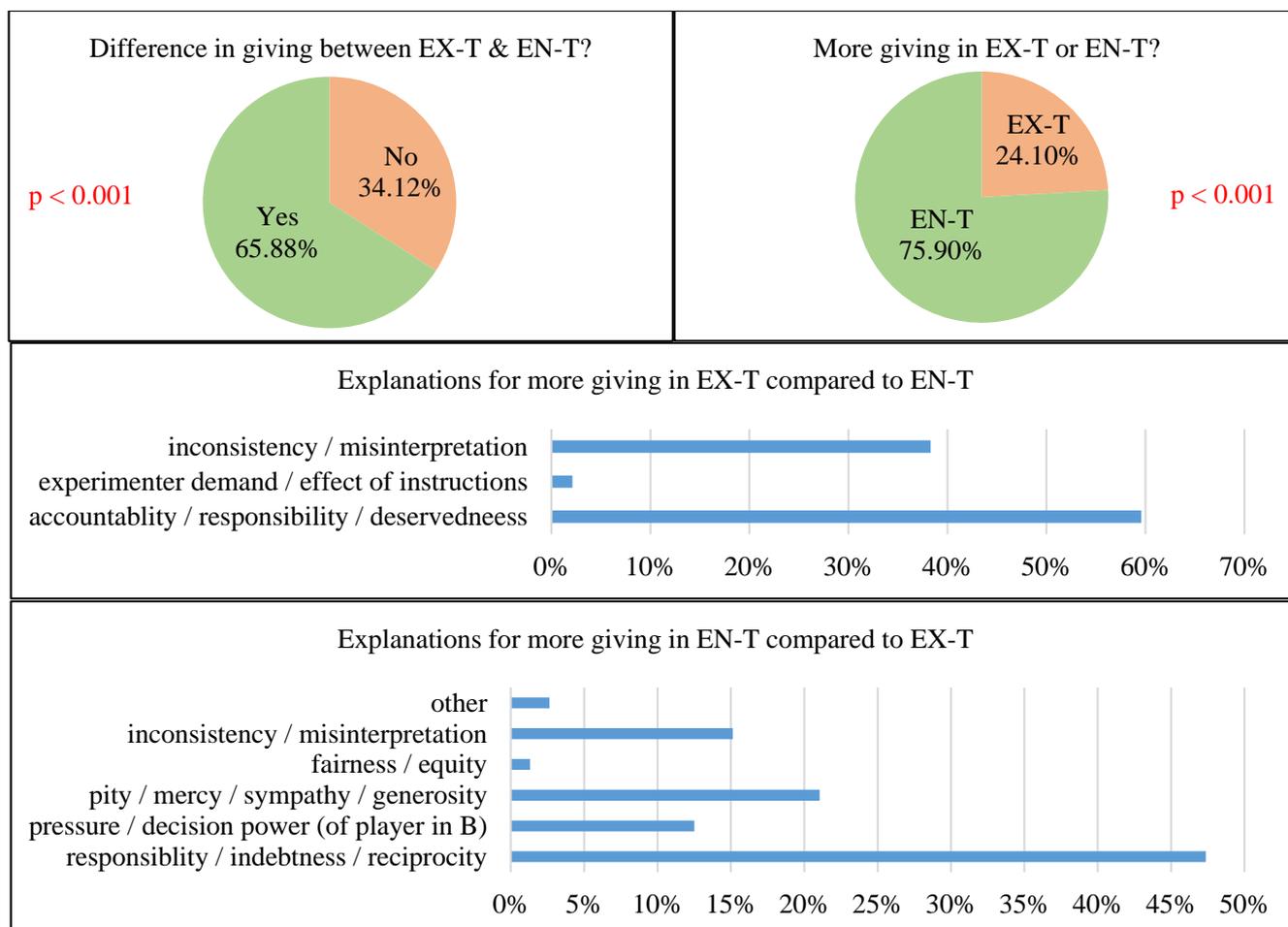


Figure 7. Survey results for Hypothesis 4.

## 5. Concluding Remarks

We experimentally examined the behavior of agents in some variations of the well-known dictator game. Specifically, we investigated the effects of the possibility of taking in the dictator game and the choice of passive players between the dictator game and the taking game on the distribution decisions of active players. In doing so, we utilized a between-subject design with three treatments: (i) exogenously assigned dictator game (EX-D), (ii) exogenously assigned taking game (EX-T), and (iii) endogenous treatment where passive subjects choose to play either the dictator game (EN-D) or the taking game (EN-T). This setting allowed us to study the differences between the dictator and taking games as well as the differences between exogenously and endogenously assigned games within dictator or taking game setups. This work, to the best of our knowledge, is the first to study endogenous game selection and its impacts on giving behavior in a dictator game vs. taking game setting by allowing passive players to have a say in the game they want to play.

We obtained three main results, which provide new insights about the interpretation of giving behavior in distributive settings by highlighting the importance of endogenous selection into games: First, like List [28] and Bardsley [29], we found that dictators give less in the taking game than in the dictator game; that is, giving was less in EX-T than in EX-D. Moreover, we found that the same result holds when the game is endogenously chosen (i.e.,

giving was less in EN-T than in EN-D), adding robustness to the former result. Second, as expected, we found that the dictator game was chosen significantly more frequently than the taking game in the endogenous treatment. That said, the taking game was chosen by a non-negligible number of participants. We thought that this was something worthy of study. However, our analysis did not lead to any conclusions about “who is more likely to choose the taking game”. Some survey respondents who predicted that the taking game would be chosen more frequently argued that the passive players must have chosen this game to emotionally manipulate their opponents or to show trust and expect reciprocal behavior. Third, the dictators in EN-D gave significantly less than the dictators in EX-D. Even though the same dictator game was played in EX-D and in EN-D, the fact that in EN-D the game played was chosen by the passive players had an impact on the behavior of the dictators. Our survey results support the interpretation that the dictators hold the receiver responsible for playing the dictator game instead of the taking game, thereby leading to a fall in giving behavior. Essentially, the dictators used this choice of the receivers as a justification for giving less, even if choosing the dictator game was the dominant action that anyone would do.

The accountability of the receivers for playing the dictator game in EN-D affects dictators’ transfers negatively, as this choice is undesirable for their self-interest. This result supports the observation that transfers fall significantly when agents have the opportunity to create situational excuses for their self-serving behavior by using a setting novel to the existing literature. On the other hand, it should be noted that this result is in contrast with the findings of Heinrich et al. [41], where passive players in the dictator game had a say in the procedure.

Our results emphasize the importance of accountability, which inspired this paper; as much as people can choose the types of interactions that they will take part in, our experimental paradigms or models must take this into account. Our results also add another layer of doubt to the altruistic interpretation for giving in the dictator game by showing that it is influenced by whether the game is exogenously assigned or the receivers’ choice.

Future work may study (i) the factors that led some subjects to choose the taking game, (ii) the discrepancy between our findings for the EX-T vs. EN-T comparison and the survey participants’ predictions regarding the differences between giving behavior in EX-T and EN-T, and (iii) endogenously chosen game settings in the field: The first is important because we observed more choices of EN-T than we expected. The second is important because a non-negligible number of survey participants referred to trust and reciprocity, whereas we believe that there is no straightforward room for these notions to play a role in our experimental setup. The third is important to test the robustness of our findings in a more natural setting.

**Author Contributions:** Conceptualization, E.K. and E.T.; Formal analysis, E.T.; Funding acquisition, E.T.; Methodology, E.K. and E.T.; Software, E.T.; Supervision, E.K.; Writing—original draft, E.K. and E.T.; Writing—review & editing, E.K. and E.T. All authors have read and agreed to the published version of the manuscript.

**Funding:** This research was funded by the Department of Economics at Bilkent University. The APC was discounted (thanks to the IOAP participation of the University of Pompeu Fabra) and the remainder was waived by the journal.

**Data Availability Statement:** The data is available on request.

**Conflicts of Interest:** The authors declare no conflict of interest.

## Appendix A. Experimental Instructions and Survey

In this section, we provide the instructions used in our experiment and the survey (all of the questions as well as the explanation in the beginning), both translated from Turkish. The instructions for the dictator game and the taking game in the exogenous and endogenous treatments are quite similar. Therefore, we present them together, where

different parts are specified inside square brackets with the related game name noted beforehand. In the following text we use the labels “dictator” and “passive player” for readers’ convenience. In the actual instructions, we did not use those labels.

### **Instructions: Exogenous Treatments (EX-D and EX-T)**

#### **Passive Player:**

In this experiment, each participant in this room (Room B) is paired with a participant who is in another room (Room A). You will not be told who you are matched with during or after the experiment, and similarly he or she will not be told who you are either during or after the experiment.

You will notice that there are other people in the same room with you who are also participating in the experiment. You are not paired with any of these people. The decisions that they make will have absolutely no effect on you, nor will any of your decisions affect them. Most importantly, your decisions will be strictly anonymous and cannot be linked to you in any way in the data collected from this experiment.

The experiment will be conducted as follows:

- You and the person you are paired with each have been allocated 10 TRY.
- Additionally, the person you are paired with has been provisionally allocated an additional 10 TRY. You have not been allocated this additional 10 TRY.
- EX-D: [The person you are paired with will decide what portion, if any, of this 10 TRY to transfer to you. The choice of that person can be anywhere from 0 TRY to 10 TRY, in 1 TRY increments.]
- EX-T: [The person you are paired with will decide what portion, if any, of this 10 TRY to transfer to you. That person can also transfer a negative amount (i.e., can take up to 10 TRY from you). Thus, the choice of that person can be anywhere from –10 TRY to 10 TRY, in 1 TRY increments.]
- As a result, your take-home earnings from this experiment will be the summation of your initial 10 TRY allocation and the amount the person you are paired with decided to transfer to you in this exercise. Likewise, the earnings of that person will be their initial 10 TRY allocation added to the amount left to them from this choice exercise.

As the decisions made in the experiment are transferred via sealed envelopes, they cannot be observed by the experimenters. Moreover, at the end of the experiment, your payments will be made by an assistant who is not related with this experiment and the receipts will be collected by the assistant (to be given to the financial affairs of the university).

You will see the decision of the person you are paired with in a moment. When you see the decision, you can fill in the questionnaire that will be distributed. Please do not talk to the other people in this room until your session is completed and everyone has left.

If you have any questions, please raise your hand and wait until an administrator comes.

#### **Dictator:**

In this experiment, each participant in this room (Room A) is paired with a participant who is in another room (Room B). You will not be told who you are matched with during or after the experiment, and similarly he or she will not be told who you are either during or after the experiment.

You will notice that there are other people in the same room with you who are also participating in the experiment. You are not paired with any of these people. The decisions that they make will have absolutely no effect on you, nor will any of your decisions affect them. Most importantly, your decisions will be strictly anonymous and cannot be linked to you in any way in the data collected from this experiment.

The experiment will be conducted as follows:

- You and the person you are paired with each have been allocated 10 TRY.
- Additionally, you have been provisionally allocated an additional 10 TRY. The person you are paired with has not been allocated this additional 10 TRY.

- EX-D: [The only thing you need to do is to decide what portion, if any, of this 10 TRY to transfer to the person you are paired with. Your choice can be anywhere from 0 TRY to 10 TRY, in 1 TRY increments.]
- EX-T: [The only thing you need to do is to decide what portion, if any, of this 10 TRY to transfer to the person you are paired with. You can also transfer a negative amount (i.e., you can take up to 10 TRY from the person you are paired with). Thus, your transfer choice can be anywhere from  $-10$  TRY to 10 TRY, in 1 TRY increments.]
- As a result, your take-home earnings from this experiment will be the summation of your initial 10 TRY allocation and the amount left to you from this choice exercise. Likewise, the earnings of the person you are paired with will be their initial 10 TRY allocation added to the amount you decided to transfer them in this exercise.

When you come to a decision about your choice, you can mark the form in front of you with the pen on the table. When you mark the form, the other form beneath it will be marked as well due to the copy paper in between. You need to keep one of these copies for your payment and put the other copy in an envelope to be given to the person you are paired with. It does not matter which copy you keep and which one you put in envelope.

Please seal the envelope once you put one of the copies inside. As the decisions made in the experiment are transferred via sealed envelopes, they cannot be observed by the experimenters. Moreover, at the end of the experiment, your payments will be made by an assistant who is not related with this experiment and the receipts will be collected by the assistant in order to be given to the financial affairs of the university.

You will have five minutes to come to a decision about your choice. When you come up with your choice, you can fill in the questionnaire that will be distributed. Please do not talk to the other people in this room until your session is completed and everyone has left. Do not be concerned if other people make their decisions before you; we will not collect envelopes until after five minutes.

If you have any questions, please raise your hand and wait until an administrator comes.

#### Example Form:

Please circle the TRY amount you want to transfer to the person you are paired with:

EX-D: [0 1 2 3 4 5 6 7 8 9 10]

EX-T: [-10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10]

#### Instructions: Endogenous Treatments (EN-D and EN-T)

##### Passive Player:

In this experiment, each participant in this room (Room B) is paired with a participant who is in another room (Room A). You will not be told who you are matched with during or after the experiment, and similarly he or she will not be told who you are either during or after the experiment.

You will notice that there are other people in the same room with you who are also participating in the experiment. You are not paired with any of these people. The decisions that they make will have absolutely no effect on you nor will any of your decisions affect them. Most importantly, your decisions will be strictly anonymous and cannot be linked to you in any way in the data collected from this experiment.

The experiment will be conducted as follows:

- You and the person you are paired with each have been allocated 10 TRY.
- Additionally, the person you are paired with has been provisionally allocated an additional 10 TRY. You have not been allocated this additional 10 TRY.
- **The only thing you need to do is to decide which of the following experiments you want to continue with:**
  - **Experiment 1:** In this experiment, the person you are paired with will decide what portion, if any, of this 10 TRY to transfer to you. The choice of that person can be anywhere from 0 TRY to 10 TRY, in 1 TRY increments.

- **Experiment 2:** In this experiment, as well, the person you are paired with will decide what portion, if any, of this 10 TRY to transfer to you. However, different from Experiment 1, in this experiment that person can also transfer a negative amount (i.e., can take up to 10 TRY from you). Thus, the choice of that person can be anywhere from  $-10$  TRY to 10 TRY, in 1 TRY increments.
- **As a result, your take-home earnings from this experiment will be the summation of your initial 10 TRY allocation and the amount that the person you are paired with decided to transfer you in this exercise.** Likewise, the earnings of that person will be their initial 10 TRY allocation added to the amount left to them from this choice exercise.
- In front of you, there are two envelopes with “Experiment 1” or “Experiment 2” written on. Inside these envelopes there are the related experimental instructions to be given to the person you are paired with. You will have five minutes to come to a decision on which experiment you want to continue with. After five minutes, you need to give the envelope of the experiment you have chosen to the experimenter.
- **There will not be any other decision you need to take throughout the experiment. You do not need to mark anything with pen to make a choice; it is enough to hand in the envelope of the chosen experiment.**

If you want to take a look at the instructions and the decision forms of these two experiments, you can find examples of those in the next pages. After the experimenter hands in the envelope of the experiment you have chosen to the person you are paired with, the transfer decision of that person will be brought to you again in another envelope after five minutes.

As the decisions made in the experiment are transferred via sealed envelopes, they cannot be observed by the experimenters. Moreover, at the end of the experiment, your payments will be made by an assistant who is not related with this experiment and the receipts will be collected by the assistant in order to be given to the financial affairs of the university.

When you see the decision of the person you are paired with, you can fill in the questionnaire that will be distributed. Do not be concerned if other people make their choice between experiments before you; we will not collect envelopes until after five minutes. Please do not talk to the other people in this room until your session is completed and everyone has left.

If you have any questions, please raise your hand and wait until an administrator comes.

#### **Dictator:**

In this experiment, each participant in this room (Room A) is paired with a participant who is in another room (Room B). You will not be told who you are matched with during or after the experiment, and he or she will not be told who you are either during or after the experiment.

You will notice that there are other people in the same room with you who are also participating in the experiment. You are not paired with any of these people. The decisions that they make will have absolutely no effect on you nor will any of your decisions affect them. Most importantly, your decisions will be strictly anonymous and cannot be linked to you in any way in the data collected from this experiment.

The experiment will be conducted as follows:

- You and the person you are paired with each have been allocated 10 TRY.
- Additionally, you have been provisionally allocated an additional 10 TRY. The person you are paired with has not been allocated this additional 10 TRY.
- The person you are paired with is presented with two different experiments to choose from. In both of these experiments you are expected to decide what portion, if any, of this 10 TRY to transfer to the person you are paired with. In one of them (Experiment 1) your choice of transfer can be any integer from 0 TRY to 10 TRY. In the other one,

however, you can also transfer a negative amount (i.e., you can take up to 10 TRY from the person you are paired with, so your choice of transfer can be any integer from  $-10$  TRY to 10 TRY).

- **EN-D:** [The person you are paired with has chosen the experiment without the possibility of negative transfers (Experiment 1). In this case, the only thing you need to do is to decide what portion, if any, of this 10 TRY to transfer to the person you are paired with. Thus, your choice can be any integer from 0 TRY to 10 TRY]
- **EN-T:** [The person you are paired with has chosen the experiment with the possibility of negative transfers (Experiment 2). In this case, the only thing you need to do is to decide what portion, if any, of this 10 TRY to transfer to the person you are paired with. You can also transfer a negative amount (i.e., you can take up to 10 TRY from the person you are paired with). Thus, your choice can be any integer from  $-10$  TRY to 10 TRY.]
- As a result, your take-home earnings from this experiment will be the summation of your initial 10 TRY allocation and the amount left to you from this choice exercise. Likewise, the earnings of the person you are paired with will be their initial 10 TRY allocation added to the amount you decided to transfer to them in this exercise.

When you come to a decision about your choice, you can mark the form that came inside the envelope with the pen on the table. When you mark the form, the other form beneath it will be marked as well due to the copy paper in between. You need to keep one of these copies for your payment and put the other copy in the envelope with “decision” written on it to be given to the person you are paired with. It does not matter which copy you keep and which one you put in envelope.

Please seal the envelope once you put one of the copies inside. As the decisions made in the experiment are transferred via sealed envelopes, they cannot be observed by the experimenters. Moreover, at the end of the experiment, your payments will be made by an assistant who is not related with this experiment and the receipts will be collected by the assistant in order to be given to the financial affairs of the university.

You will have five minutes to come to a decision about your choice. When you come up with your choice, you can fill in the questionnaire that will be distributed. Please do not talk to the other people in this room until your session is completed and everyone has left. Do not be concerned if other people make their decisions before you; we will not collect envelopes until after five minutes.

If you have any questions, please raise your hand and wait until an administrator comes.

#### **Example Form:**

Please circle the TRY amount you want to transfer to the person you are paired with:

EN-D: [0 1 2 3 4 5 6 7 8 9 10]

EN-T: [-10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10]

#### **Instructions—Survey**

##### **Introduction:**

Participation in this survey is completely voluntary, answering the survey questions does not carry any foreseeable risk, and the identity information of the participants (if provided by the participant) in the collected data will be anonymized and will not be used in the data analysis nor in the reporting of the results.

In this survey, you are asked some questions and expected to make predictions about the decisions participants made in the three treatments of our experiment, which are described below. The experiment was carried out at Bilkent University between December 2019 and March 2020. It will take around 10–15 min for you to complete the survey. A randomly selected person among the participants whose predictions are correct (among those who complete the survey) will earn 200 TRY.

If you want to participate in this lottery and have the chance to win 200 TRY or to be informed about the results of the study, write your e-mail address with the extension

“bilkent.edu.tr” in the field below and click the “Confirm” button. Attention: Those who write an e-mail other than “bilkent.edu.tr” extension e-mail will not be included in the draw. If you do not want to participate in the draw, you can click the “I do not want to participate in the lottery” button and continue without entering an e-mail address.

Your e-mail address with bilkent.edu.tr extension: \_\_\_\_\_ CONFIRM  
I DO NOT WANT TO PARTICIPATE IN THE LOTTERY

#### Explanation of the Experiment:

All of the treatments of the experiment were conducted in two separate rooms. Each participant in Room A was randomly matched with another participant in Room B. Before or after the experiment, the participants were not informed about who they were matched with, and all of the decisions made by the participants were kept confidential. In all three treatments, all participants in Room A and B were given 10 TRY each. In addition, 10 TRY was provisionally given to those in Room A. This extra 10 TRY was not given to those in Room B.

In all treatments, the earnings of the individuals in Room A were the sum of the initially given 10 TRY and the amount left to them as a result of their transfer choice. Likewise, the earnings of the individuals in Room B were the sum of the initially given 10 TRY and the amount transferred to them from the extra 10 TRY by the person with whom they were matched in Room A. Finally, each participant participated in only one of the above treatments.

#### TREATMENT 1

In this treatment, those in Room A (if they wish) were asked how much of this extra 10 TRY they would like to transfer to the person they matched with in Room B. Therefore, the transfer choice of a person in Room A can be any integer from 0 to 10.

#### TREATMENT 2

In this treatment, those in Room A (if they wish) were asked how much of this extra 10 TRY they would like to transfer to the person they matched with in Room B. However, unlike TREATMENT 1, in this treatment, a participant in Room A can transfer a negative amount to the person he matched in Room B; in other words, he can take up to 10 TRY from him. Therefore, the transfer choice of a person in Room A can be any integer from -10 to 10.

#### TREATMENT 3

In this treatment, TREATMENT 1 and TREATMENT 2 were explained to each participant in Room B, and they were asked which treatment they would like to participate in. Both of the treatments were told to the people in Room A as well, and the choice of the person they matched with in Room B between these two treatments was conveyed. According to the choice of the person in Room B, each participant in Room A continued the relevant treatment and made a transfer choice in the relevant treatment.

Therefore, while the participants in TREATMENT 1 and TREATMENT 2 were assigned randomly by the researcher to a treatment, the participants in TREATMENT 3 were assigned endogenously to one of the two treatments by the person they matched with in Room B.

**Control Questions:**

Within TREATMENT 3 (as a result of the selection of the person in Room B), if TREATMENT 1 was chosen, it was called TREATMENT 3-1; if TREATMENT 2 was chosen, it was named TREATMENT 3-2.

We will start with a few control questions that will test your understanding of the experimental design.

1. In TREATMENT 1, if the transfer choice of a person in Room A is 0, what will be the earnings of this person and his matched person in Room B from the experiment? The earnings of the decision maker in Room A: \_\_\_\_TRY  
The earnings of the person in Room B whom the decision maker in Room A matched with: \_\_\_\_TRY
2. In TREATMENT 2, if the transfer choice of a person in Room A is  $-10$ , what will be the earnings of this person and his matched person in Room B from the experiment? The earnings of the decision maker in Room A: \_\_\_\_TRY  
The earnings of the person in Room B whom the decision maker in Room A matched with: \_\_\_\_TRY
3. In TREATMENT 3, participation in TREATMENT 1 or TREATMENT 2 is determined by the participant in Room B.  
True \_\_\_\_  
False \_\_\_\_
4. In TREATMENT 1, a person in Room A may choose to transfer an amount from 10 TRY given to the person he matched with in Room B to himself.  
True \_\_\_\_  
False \_\_\_\_
5. In what interval can the transfer choice of the person in Room A be in TREATMENT 3-2?  
Any integer between \_\_\_\_TRY and \_\_\_\_TRY
6. Each participant participated in all three treatments.  
True \_\_\_\_  
False \_\_\_\_

**Survey Questions:**

1. Our current question is about the decisions taken by the participants in Room A who participated in TREATMENT 1 or TREATMENT 2. Do you think there is a difference between TREATMENT 1 and TREATMENT 2 in terms of the average amount that Room A participants chose to transfer to the matched person in Room B? No Difference \_\_\_\_ There is a Difference \_\_\_\_
  - i. If there is a difference, in which treatment do you think the participants in Room A chose to transfer more money to the matched person in Room B? In TREATMENT 1 or in TREATMENT 2?  
They chose to transfer more money in TREATMENT 1 \_\_\_\_  
They chose to transfer more money in TREATMENT 2 \_\_\_\_
  - ii. Can you briefly explain why you think that way? \_\_\_\_\_
2. Our current question is about the decisions taken by the participants in Room A who participated in TREATMENT 3-1 or TREATMENT 3-2. Do you think there is a difference between TREATMENT 3-1 and TREATMENT 3-2 in terms of the average amount that Room A participants chose to transfer to the matched person in Room B? No Difference \_\_\_\_  
There is a Difference \_\_\_\_
  - i. If there is a difference, in which treatment do you think the participants in Room A chose to transfer more money to the matched person in Room B? In TREATMENT 3-1 or in TREATMENT 3-2?  
They chose to transfer more money in TREATMENT 3-1 \_\_\_\_

- They chose to transfer more money in TREATMENT 3-2 \_\_\_\_
- ii. Can you briefly explain why you think that way? \_\_\_\_\_
3. Our current question is about the decisions taken by the participants in Room B who participated in TREATMENT 3. Do you think there is a difference between the frequency of choosing TREATMENT 3-1 and TREATMENT 3-2 in TREATMENT 3 (by the participants in Room B)?  
No Difference \_\_\_\_  
There is a Difference \_\_\_\_
- i. If there is a difference, do you think TREATMENT 3-1 or TREATMENT 3-2 was chosen more often?  
TREATMENT 3-1 was chosen more often \_\_\_\_  
TREATMENT 3-2 was chosen more often \_\_\_\_
- ii. Can you briefly explain why you think that way? \_\_\_\_\_
4. Our current question is about the decisions taken by the participants in Room A who participated in TREATMENT 1 or TREATMENT 3. Do you think there is a difference between TREATMENT 1 and TREATMENT 3-1 in terms of the average amount that Room A participants chose to transfer to the matched person in Room B?  
No Difference \_\_\_\_  
There is a Difference \_\_\_\_
- i. If there is a difference, in which treatment do you think the participants in Room A chose to transfer more money to the matched person in Room B? In TREATMENT 1 or in TREATMENT 3-1?  
They chose to transfer more money in TREATMENT 1 \_\_\_\_  
They chose to transfer more money in TREATMENT 3-1 \_\_\_\_
- ii. Can you briefly explain why you think that way? \_\_\_\_\_
5. Our current question is about the decisions taken by the participants in Room A who participated in TREATMENT 2 or TREATMENT 3. Do you think there is a difference between TREATMENT 2 and TREATMENT 3-2 in terms of the average amount that Room A participants chose to transfer to the matched person in Room B?  
No Difference \_\_\_\_  
There is a Difference \_\_\_\_
- i. If there is a difference, in which treatment do you think the participants in Room A chose to transfer more money to the matched person in Room B? In TREATMENT 2 or in TREATMENT 3-2?  
They chose to transfer more money in TREATMENT 2 \_\_\_\_  
They chose to transfer more money in TREATMENT 3-2 \_\_\_\_
- ii. Can you briefly explain why you think that way? \_\_\_\_\_

Finally, we have a few more short questions for you:

1. Gender: Male \_\_\_\_ Female \_\_\_\_
2. Age: \_\_\_\_\_
3. Department: \_\_\_\_\_
4. Grade: \_\_\_\_\_
5. Have you taken a course on Game Theory, Experimental Economics, Behavioral Economics (e.g., ECON 204, ECON439, ECON 444)?  
Yes \_\_\_\_ No \_\_\_\_
6. Between December 2019 and March 2020, as mentioned in the survey, Res. See. Have you participated in one of the Decision Making Experiments conducted by Elif Tosun?  
Yes \_\_\_\_ No \_\_\_\_

Thank you for your participation.  
Assoc. Prof. Dr. Emin Karagözoğlu  
Res. Asst. Elif Tosun

## Appendix B. Additional Analyses

In this section, we present some additional analyses that further support our claims. We run probit regressions to understand the effect of the endogenous treatment on dictators' probability of giving zero.

### Probit Regressions

Recall from Figure 1 that the frequency of giving 0 and giving 5 are both higher in EN-D than in EX-T. Similarly, from Figure 2 we can see that dictators give  $-10$  (i.e., take 10) more frequently in EN-T than in EX-T. To achieve a better understanding of this, we defined new dummy variables as *Zerogiving*, *Fivegiving*, and *Minustengiving* to be used in probit regressions. We regressed these dummy variables over *Endogenous*, *Male*, and other covariates to understand whether the probability of giving 0, 5, or  $-10$  was affected by our treatment variation.

From Table A1 (columns 1 and 2), we can see that the endogenous selection of the game played increases the probability of dictators giving zero when the game played is a dictator game. This effect is significant at the 5% level, but our results for *Fivegiving* (columns 3 and 4) are not statistically significant.

**Table A1.** Probit regression results for dictator games.

	Dependent Variable: <i>Zerogiving</i>		Dependent Variable: <i>Fivegiving</i>	
	(1)	(2)	(3)	(4)
Endogenous	1.382 ** (0.021)	1.929 ** (0.011)	0.113 (0.812)	0.0124 (0.983)
Male	1.035 * (0.066)	1.321 ** (0.047)	0.226 (0.590)	0.0911 (0.852)
Endogenous x Male	$-1.382$ * (0.051)	$-1.822$ ** (0.032)	$-0.0328$ (0.956)	0.124 (0.858)
Constant	$-1.465$ *** (0.004)	247.0 (0.120)	$-0.366$ (0.286)	$-54.44$ (0.693)
<i>N</i>	77	77	77	77
<i>Pseudo R</i> <sup>2</sup>	0.068	0.166	0.005	0.077
<i>p-Value</i>	0.0892	0.2584	0.9038	0.8378
<i>Controls</i>	No	Yes	No	Yes

Notes: Statistical significance is indicated by stars (\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ ); *p*-values are also reported in parentheses. Columns 2 and 4 include the following control variables: *Byear*, *Sibling*, *Income*, *Extravert*, *Agree*, *Consc*, *Stable*, *Open*, *Econ*, *Monday*.

Running the same regressions for the taking games (EX-T and EN-T) did not yield any significant results, even with the *Minustengiving* dependent variable (see Table A2). One possible explanation for the observation that the treatment variation influences only the frequency of *Zerogiving*, but not *Fivegiving* or *Minustengiving*, is that people who are willing to give 5 or  $-10$  are possibly less likely to be affected because they have somewhat strong standpoints (e.g., clearly fair-minded or clearly selfish).

It should be noted that these probit regression results are compatible with our Result 3 and Result 4. For example, recall that in Table 3 the coefficient estimate for the *Male* dummy is negative, whereas the coefficient estimate for the interaction variable is positive; both are statistically significant. In Table A1, we can observe the same pattern for the probability of giving zero.

We also wondered why any passive player would choose the taking game rather than the dictator game in the endogenous treatment. Therefore, we defined a dummy variable (*passive dummy*) that takes the value of one when the receiver chooses to play the taking game and zero otherwise. By regressing the *passive dummy* over our control variables, we aimed to gain insights into possible factors behind this behavior. It should be noted that

we could not use *Endogenous* in these regressions, since the analysis was naturally limited to EN-T and EN-D only.

**Table A2.** Probit regression results for taking games.

	Dependent Variable: <i>Zerogiving</i>		Dependent Variable: <i>Fivegiving</i>		Dependent Variable: <i>Minustengiving</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Endogenous	(Omitted)	(Omitted)	−0.381 (0.446)	−0.994 (0.193)	−0.0584 (0.914)	0.915 (0.248)
Male	0.476 (0.351)	6.272 (0.265)	−0.671 (0.108)	−0.912 * (0.083)	−0.0152 (0.972)	0.473 (0.421)
Endogenous x Male	(Omitted)	(Omitted)	0.428 (0.638)	0.574 (0.613)	0.780 (0.382)	0.143 (0.897)
Constant	−1.405 *** (0.000)	1284.2 (0.223)	−0.0502 (0.841)	379.9 * (0.093)	−0.706 ** (0.010)	−202.0 (0.467)
N	42	27	55	54	55	54
Pseudo R <sup>2</sup>	0.029	0.452	0.041	0.269	0.019	0.218
p-Value	0.3483	0.2303	0.3948	0.1181	0.7492	0.4111
Controls	No	Yes	No	Yes	No	Yes

Notes: Statistical significance is indicated by stars (\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ );  $p$ -values are also reported in parentheses. Columns 2, 4, and 6 include the following control variables: Byear, Sibling, Income, Extravert, Agree, Consc, Stable, Open, Econ, Monday.

In Table A3, the probit analysis where the dependent variable is *Passive Dummy* (equal to one if the passive player has chosen EN-T) shows us that none of our control variables is able to explain the choice of the taking game in the endogenous treatment. As can be seen from the instructions in Appendix A, we made the rules of this simple game obvious to the participants. Thus, it is hard to attribute this behavior to any systematic issues about (mis)understanding either.

**Table A3.** Probit regression results for the endogenous treatment.

	Dependent Variable: <i>Passive Dummy</i>	
	(1)	(2)
Male	−0.0107 (0.978)	−0.0286 (0.953)
Constant	−0.623 ** (0.011)	−237.7 (0.302)
N	49	47
Pseudo R <sup>2</sup>	0.000	0.133
p-Value	0.6873	0.6873
Controls	No	Yes

Notes: Statistical significance is indicated by stars (\*\*  $p < 0.05$ );  $p$ -values are also reported in parentheses. Column 2 includes the following control variables: Byear, Sibling, Income, Extravert, Agree, Consc, Stable, Open, Econ, Monday.

## Notes

<sup>1</sup> It is fair to say that Eichenberger and Oberholzer-Gee [55] tested the taste for fairness earlier by comparing the standard dictator game with their “gangster game”, where the player can take money from an anonymous student. However, in this game, the player has no chance of giving any money, so overall it is a different game and not a dictator game with a taking option. Nevertheless, this experiment produced some striking results, where the property rights were clearly assigned via a pre-experimental test yet the gangsters took more than three-quarters of the earned endowment of the better-graded students, pointing at why the game is named as such.

<sup>2</sup> Brosig et al. [26] also used taking games as a part of their experiments, but this was not the focus of their paper.

3 Examples abound. For instance, if one does not want to haggle, then one may choose to buy goods from outlets where there are fixed prices and no room for bargaining. If one does not want to compete, then one may choose to work under a per-piece-payment contract rather than under a rank-order tournament contract. If one is not good at saying “no”, then one may choose not to receive calls from donation campaigns. Our setup is not designed to accommodate a specific real-life instance, but the question that we tackle in this paper is inspired from such examples.

4 At the time we ran our experiments, 10 TRY was approximately equal to USD 2.

5 Thus, in both games, the payoff to the dictator was 20 TRY minus the allocation amount, and the payoff to the receiver was 10 TRY plus the allocation amount, where the allocation amount differed between the games as explained.

6 Throughout this paper, the dictator and taking games played due to the endogenous selection of the receiver are referred to as EN-D and EN-T, respectively.

7 In particular, the receivers also had the instructions given to the dictators at hand.

8 Needless to say, no theoretical model with standard preferences would predict that an agent would choose the taking game.

9 For another use of this phrase, see Regner and Matthey [56].

10 We refer to the rationalization in psychology, meaning the defense mechanisms in which apparent logical reasons are given to justify behavior that is motivated by unconscious instinctual impulses.

11 We chose to conduct a standalone survey rather than asking open-ended questions to the participants of the experiment because we expected the answers to the latter to be biased and subject to rationalization/justification biases.

12 We used Levene’s robust test statistic for the equality of variances between groups.

13 Running tobit regressions instead of OLS does not qualitatively change the results.

14 Two subjects chose not to report their gender.

15 In reporting these results, we omitted one pair because the subject in the dictator role in this pair clearly did not understand the instructions, leaving us with 133 pairs. The subject was a dictator in the EX-T treatment, but he did not understand what to do in the experiment even after we answered all of his questions about the instructions. When we were collecting the envelopes, his decision form was still not ready, and we had to explain what he had to do one more time. Finally, all of our results are robust to the inclusion of this pair.

16 This is the only situation where we found unequal variances between the compared samples, and using the non-parametric Mann–Whitney test as a robustness check did not change our result (i.e., (i)  $p = 0.006$  and (ii)  $p = 0.0425$ ).

17 We would like to thank an anonymous reviewer for suggesting that we have a look at the proportion of positive offers and the mean positive offer across treatments.

18 Using binomial tests did not change our result (i.e.,  $p < 0.0005$ ).

19 Using the Mann–Whitney test as a robustness check yielded the same conclusion, but with a significance level of 10% instead of the conventional 5% (i.e.,  $p = 0.0509$ ).

20 Using the Mann–Whitney test did not change our result (i.e.,  $p = 0.7372$ ).

21 The *Endogenous* variable is present only in specifications (1) and (2), since only those specifications use the pooled data.

22 Note that the number of observations is not 78 due to the missing values in the gender variable, as explained earlier.

23 For the results without the gender effects, one can refer to the statistical tests section, where we provide the overall differences between treatments without controlling for any other variable.

24 The gender differences in mean giving calculated from Tables 5 and 6 directly matched with the corresponding coefficients presented in column 3 of Tables 3 and 4, respectively, since we did not have the controls in that specification while having the interaction term necessary to differentiate the effects of gender in the exogenous and endogenous treatments.

25 This difference directly corresponds to the *Male* coefficient in column 3 of Table 3 ( $-1.78$ ), meaning that male dictators give 1.78 TRY less on average compared to female dictators in EX-D.

26 This difference can be found from the sum of the *Male* and *Endogenous x Male* coefficients in column 3 of Table 3.

27 This is the reason why its coefficient is 2.352 in column 3 of Table 3; it is the differential treatment effect for males because the coefficient corresponds to the difference between the treatment effects for men ( $-0.244$ ) and for women ( $-2.595$ ).

28 While interpreting these results, it should be noted that each explanation from the survey participants can fall into multiple categories, so the sum of these percentages does not add up to 100. Moreover, we combined similar keywords/explanations into a single category for the ease of both categorizing and reading the results, but this does not imply that each explanation that falls into a specific category includes all of the keywords written in the heading of that category.

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