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Adjustment Constraints in a Dictator Game
Experiment**

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The Sequencing of Voluntary Giving and Taxation: Donation Adjustment Constraints in a Dictator Game Experiment*

Tadakatsu Nakamura[†] and Tetsuo Yamamori[‡]

Abstract

Does allowing donation opportunities before rather than after taxation affect voluntary giving behavior? We investigate this question using a dictator game experiment with two conditions: a Rule-Based condition, in which all donations follow the tax revelation, and a Discretionary condition, in which the initial donation precedes it. This design allows us to examine how the sequencing of voluntary giving and taxation shapes redistribution outcomes. Participants donate even when future taxation for government transfer is anticipated, and total donations do not differ across timing conditions. However, participants partially offset taxation but do not similarly adjust for their own prior donations, revealing an asymmetric response to voluntary and mandatory transfers. Because pre-tax donations cannot be reversed, early giving creates adjustment constraints once taxation is realized. Pre-tax donation opportunities therefore increase the dispersion of final payoffs and the incidence of total transfers exceeding half of the endowment. These findings suggest that the sequencing of voluntary and mandatory transfers affects not the level but the stability of redistribution outcomes, and that partial crowding out may partly reflect adjustment constraints rather than warm-glow motivation alone.

Keywords: Dictator game, Voluntary giving, Taxation, Redistribution, Adjustment constraints

JEL Classification: C91, D64, H23, H41

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1. Introduction

Acts of goodwill constitute an essential component of modern society by complementing formal institutions and helping to address social needs that markets and governments alone cannot fully satisfy. In practice, various support systems that operate within regulatory and institutional frameworks are frequently sustained by individuals' altruistic behavior. Under such rule-based policy environments, taxation for public spending is predetermined, and individuals make voluntary giving decisions with full knowledge of their tax obligations. In contrast, major crises—such as large-scale natural disasters, pandemics, and armed conflicts—create settings in which taxation for public spending is determined on a discretionary basis, often well after voluntary giving has taken place.

Following the Great East Japan Earthquake in 2011, substantial donations were mobilized even though the future taxation required for reconstruction was widely anticipated; in some cases, the volume of donations far exceeded what recipient organizations could effectively allocate in the short term.⁴ A similar pattern emerged during the COVID-19 pandemic, when individuals and corporations made large donations before the scale of government fiscal responses had been determined. These episodes illustrate that voluntary giving under anticipated but uncertain taxation can lead not only complementary but also excessive, and that the sequencing of private and public transfers may shape redistribution outcomes in ways that standard models do not capture.

This paper addresses the question using a controlled laboratory experiment based on a dictator game. In the standard dictator game, the dictator's proposed allocation directly determines the final payoffs, and a substantial body of experimental research shows that many dictators choose more equitable allocations rather than keeping the entire endowment (Forsythe et al., 1994; Andreoni and Miller, 2002). Our design extends the dictator game framework by introducing taxation for government transfers: participants, acting as dictators, could make voluntary transfers to recipients, while the experimenter, representing the government, imposed a mandatory tax on dictators and transferred the proceeds to recipients. Crucially, we vary the timing of donation opportunities relative to the tax revelation. Two experimental conditions are implemented that mirror distinct policy environments: the Rule-Based frame, in which taxation is predetermined and fully observable before any donation decision, and the Discretionary frame, in which the initial donation precedes the tax revelation, analogous to settings in which public spending is determined on a discretionary basis.

⁴ Altruistic behavior has been shown to increase following extraordinary events, including large-scale natural disasters (Brown et al., 2012).

The design allows us to examine four aspects of donation behavior in relation to taxation. We assess first, whether participants make donations even before the tax amount is disclosed and how donation behavior varies across participants; second, how participants adjust their donations in response to actual tax, with particular attention to the asymmetry between voluntary and mandatory transfers; third, the heterogeneity of participants' fairness benchmarks and how crowding out varies across frames; and fourth, the determinants of total transfers exceeding half of the endowment and the role of sequential decision-making in limiting adjustment capacity.

Previous research on the relationship between individual altruistic behavior and public provision has developed along two main theoretical lines. Under the assumption that individuals maximize a utility function that includes others' welfare—so-called pure altruism—private donations and public spending are predicted to be neutral under certain tax conditions, implying complete crowding out of individual giving (Warr 1982, 1983; Roberts 1984; Bergstrom et al. 1986). If this neutrality result holds, individuals who anticipate future taxation would rationally postpone their donations until the exact level of taxation for public spending is revealed.

In contrast, when individuals derive utility directly from the act of giving itself—such as through warm-glow or impure altruism—the neutrality result no longer applies, and public spending need not fully crowd out donations (Andreoni 1989, 1990). Consistent with this view, a large empirical and experimental literature finds that public spending typically only partially crowds out private giving (Andreoni 1993; Payne 1998; Bolton and Katok 1998; Andreoni and Payne 2003). Experimental evidence further suggests that crowding out depends critically on whether individuals perceive public provision as financed through taxation, implying that the salience of fiscal costs shapes donation behavior (Eckel et al., 2005).

However, these studies typically observe donations at a single point in time, leaving open the possibility that partial crowding out reflects not only warm-glow motivation but also insufficient adjustment under sequential decision-making. Yet little is known about how individuals adjust the level and allocation of voluntary giving, and how these decisions relate to their fairness perceptions, when donation opportunities arise both before and after taxation becomes known.

Our main findings are as follows. First, participants donated even when future taxation was anticipated, and total donations did not differ across timing conditions, consistent with models of impure altruism or warm-glow giving (Andreoni, 1989, 1990). Second, participants partially offset taxation but did not similarly adjust for their own prior donations, revealing an asymmetric response to voluntary and mandatory transfers. Third, participants' fairness benchmarks were heterogeneous and did not generally coincide with the equal split, and the extent of crowding out differed across

frames, with roughly 60 percent crowding out in the Rule-Based frame versus 20 percent in the Discretionary frame. Fourth, in the Discretionary frame, pre-tax donations limited participants' adjustment capacity, increasing the dispersion of final payoffs and the incidence of total transfers exceeding half of the endowment even though mean total donations remained unchanged across frames. Together, these findings suggest that the sequencing of voluntary and mandatory transfers shapes not the level but the stability of redistribution outcomes, and that partial crowding out may reflect adjustment constraints rather than warm-glow motivation alone.

The remainder of the paper is organized as follows. Section 2 describes the experimental design and procedures. Section 3 presents the experimental results: we first examine donation behavior before and after taxation, then analyze how participants adjust their donations in response to actual tax, explore what voluntary giving reveals about participants' fairness benchmarks, and finally investigate the determinants of transfers exceeding half. Section 4 concludes.

2. Experimental Design and Procedures

This section describes the design and procedures of our laboratory experiment. The experiment was designed to allow observation of voluntary giving in response to taxation for government transfers.

2.1 Experimental design

We designed a dictator game in which dictators could make voluntary transfers to recipients, while the experimenter, acting as the government, imposed a mandatory tax on dictators and transferred the same amount to recipients. Most dictator game experiments examine voluntary giving after government transfers are realized (e.g., Bolton and Katok, 1998; Eckel et al., 2005). In contrast, our design introduces donation opportunities both before and after taxation. This structure allows us to observe how individuals adjust their donations once the tax realized, thereby providing a direct measure of the response of voluntary giving to government transfers.

The experimental design is as follows. Participants were randomly paired. Each pair was endowed with 100 tokens (experimental currency units), initially allocated entirely to the dictator, while the recipient received none. All participants made allocation decisions as dictators, knowing that their actual roles would be assigned by lottery with equal probability after all decisions had been made. The experiment consisted of two conditions: the Rule-Based frame (Frame R) and the Discretionary frame (Frame D), which differed in the timing of donation opportunities relative to taxation. Figure 1 illustrates the experimental framework for the two frames.

Frame R: Rule-Based

Frame D: Discretionary

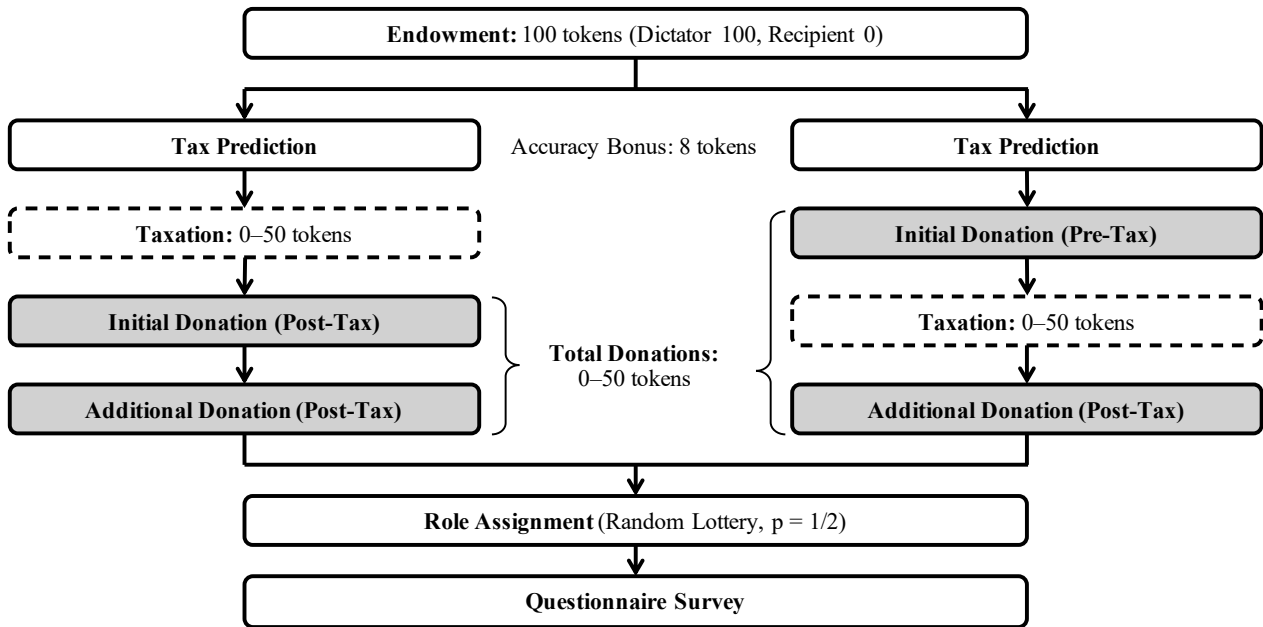


Figure 1. Experimental Framework

Notes: The dictator's final payoff equals 100 minus the tax and total donations, ranging from 0 to 100 tokens.

In Frame R, both donations were made after the tax was revealed (post-tax). In Frame D, the initial donation was made before the tax was revealed (pre-tax) and the additional donation after (post-tax). Importantly, the number of donation opportunities was held constant across frames; only their timing relative to the tax revelation differed. Frame R represents a rule-based policy environment in which taxation for government transfers is predetermined and fully observable at the time donation opportunities are provided. By contrast, Frame D captures a discretionary policy environment in which taxation for government transfers is not yet determined when participants make their initial donation decisions.

Across both frames, the total donation was constrained to lie between 0 and 50 tokens: the initial donation could take any value between 0 and 50 tokens, and the additional donation was limited to 50 minus the initial donation. The tax amount—randomly determined and representing a mandatory transfer from the dictator to the recipient—also ranged from 0 to 50 tokens. The dictator's final payoff was given by 100 minus the tax and both donation amounts, whereas the recipient's final

payoff consisted of the corresponding government transfer (i.e., the tax proceeds) and the two donations. These constraints ensure that the dictator's payoff remains non-negative and that the feasible set of total transfers is well-defined, focusing attention on deviations around the equal-split benchmark.

2.2 Procedures

The experiment was conducted at Takasaki City University of Economics and Dokkyo University, Japan, between December 2023 and January 2024. It comprised six sessions—four held at Takasaki City University of Economics and two at Dokkyo University—with 148 participants in total: 64 in Frame R (three sessions) and 84 in Frame D (three sessions). Participants were undergraduate and graduate students recruited through posters and fliers. None had previously participated in dictator game experiments, and enrollment was strictly controlled to prevent multiple participations.

Sessions took place in a laboratory using the z-Tree software package (Fischbacher 2007). Participants were randomly seated at partitioned computer terminals to ensure decision privacy. Each desk was equipped with a concealed ID card and an envelope containing the instructions and practice problems. IDs were randomly assigned to each seat in advance so that the participants could not predict which ID they would be assigned. To minimize potential experimenter effects, the experimental sessions were administered by research assistants. An assistant read the instructions aloud, and participants completed practice problems to confirm their understanding before the experiment commenced (see Appendix A for the instructions and practice problems).

After all decisions were completed, participants' actual roles were randomly determined by lottery. Participants received a fixed participation fee of 1,000 yen, in addition to a performance-based payoff. Participants also received a bonus of 8 tokens if their tax prediction exactly matched the actual tax amount. One token was converted into 25 yen, and the total reward was paid in cash at the end of the session. Each session lasted approximately 90 minutes, and participants earned an average of 2,255 yen.⁵

⁵ On December 1, 2023, 1 USD was approximately equal to 147 yen.

3. Experimental Results

This section presents the experimental results. We first examine whether participants make donations before and after the tax amount is disclosed, and characterize the patterns of donation behavior (Section 3.1). We then analyze how participants adjust their donations in response to actual tax (Section 3.2). Finally, we investigate participants' fairness benchmarks by examining the distributions of total transfers (Section 3.3) and the determinants of total transfers exceeding half of the endowment (Section 3.4). As a starting point, the two frames are well balanced on all observable demographic characteristics (see Appendix B for balance tests). At the aggregate level, mean total donations and actual taxes were similar across frames (donations: 15.97 tokens in Frame R and 15.90 tokens in Frame D, $p = 0.981$; taxes: 23.97 tokens in Frame R and 25.76 tokens in Frame D, $p = 0.437$).

3.1 Donation Behavior before and after Taxation

We begin by examining whether participants donate when the exact tax amount remains unknown, and whether they also donate after the tax is revealed. Under standard self-interest models, rational individuals would not make any voluntary giving at either stage. To examine this prediction, we first analyze donation behavior in Frame D, in which participants face a discretionary policy environment. Figure 2 displays the distributions of pre- and post-tax donations in Frame D. Both distributions exhibit a substantial mass at zero, yet a considerable proportion of participants made positive donations at each stage.

Pre-tax donations were significantly positive on average. A one-sample t-test rejects the null hypothesis that the mean pre-tax donation equals zero (mean = 8.10 tokens, SD = 12.533; $t(83) = 5.920$, $p < 0.001$). Participants donated before observing the actual tax: 40 out of 84 participants (47.6%) made a positive pre-tax donation, while 44 (52.4%) chose not to donate. Post-tax donations were also significantly positive on average. A one-sample t-test rejects the null hypothesis that the mean post-tax donation equals zero (mean = 7.81 tokens, SD = 11.90; $t(83) = 6.02$, $p < 0.001$). Participants donated after observing the actual tax: 44 out of 84 participants (52.4%) made a positive post-tax donation, while 40 (47.6%) chose not to donate. Among those who did, the mean amount was 14.9 tokens (SD = 12.84), representing approximately 30% of the remaining available tokens after the pre-tax donation.⁶

⁶ In Frame D, the remaining donation capacity for participants in the post-tax stage is defined as 50 tokens minus the

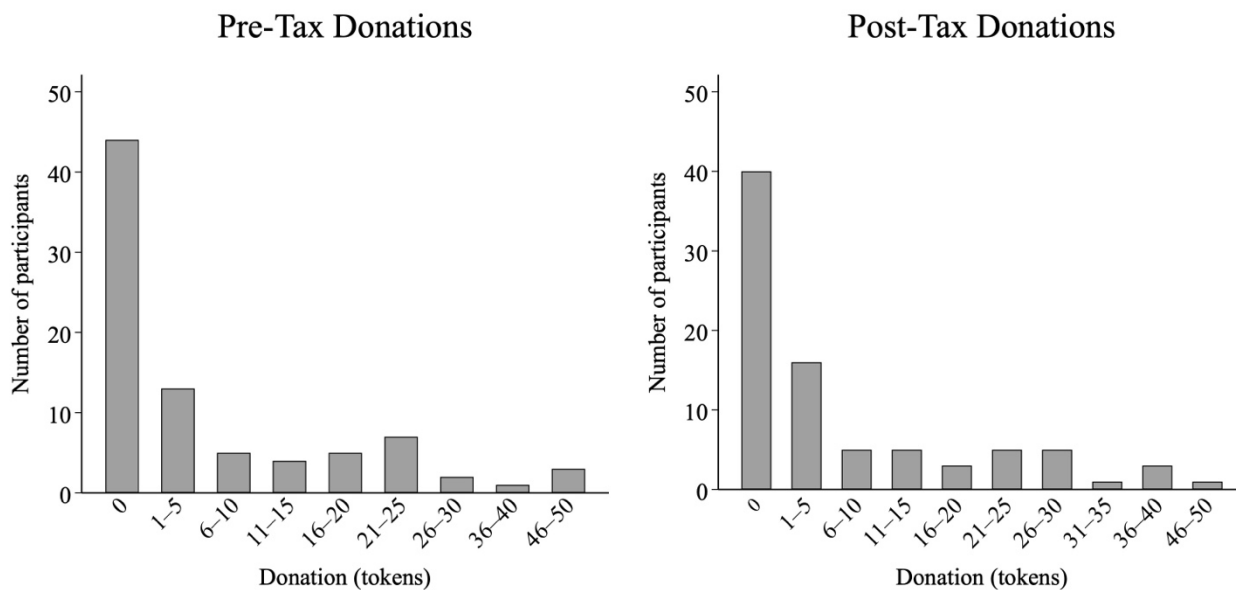


Figure 2. Distribution of Pre- and Post Tax Donations in Frame D

Notes: Donations are measured in experimental tokens and range from 0 to 50. The figure is based on observations in Frame D ($N = 84$).

Together, these results indicate that donation occurs before and/or after taxation. Standard models of pure altruism predict that individuals should postpone giving until public provision is realized (Warr, 1982; Roberts, 1984). Contrary to this prediction, a substantial share of participants donated even before and/or after the tax was revealed, suggesting that government transfers did not fully displace voluntary giving. These findings are inconsistent with complete crowding out and are compatible with models of impure altruism or warm-glow giving, in which the act of giving itself yields utility (Andreoni, 1989, 1990).

To further characterize donation behavior, we classify participants in Frame D into four groups based on the timing of their donations. Out of 84 participants, 25 (29.8%) donated both before and after taxation, 15 (17.9%) donated only before taxation, 19 (22.6%) donated only after taxation, and 25 (29.8%) did not donate at all. Table 1 presents mean donations and tax variables by donation pattern in Frame D.

pre-tax donation.

Table 1. Mean Donations and Tax Variables by Donation Pattern in Frame D

Group	N	Pre-Tax Donation	Post-Tax Donation	Total Donation	Predicted Tax	Actual Tax	Tax Prediction Error
Both	25	15.52	11.04	26.56	22.04	22.64	0.60
Pre-only	15	19.47	0.00	19.47	26.80	33.33	6.53
Post-only	19	0.00	20.00	20.00	18.84	21.53	2.69
Neither	25	0.00	0.00	0.00	18.08	27.56	9.48
Total	84	8.10	7.81	15.90	20.99	25.76	4.77

Notes: "Both" = donated before and after tax; "Pre only" = donated only before tax; "Post only" = donated only after tax; "Neither" = did not donate. Tax Prediction Error = Actual Tax – Predicted Tax. All values are measured in experimental tokens.

In the Both group ("Both" in the table), the mean pre-tax donation was 15.52 tokens and the mean post-tax donation was 11.04 tokens. A Wilcoxon signed-rank test comparing pre- and post-tax donations indicates no significant difference ($z = -0.97$, $p = 0.332$). Nevertheless, the direction of adjustment varied across individuals: for 10 participants, post-tax donations were larger than their pre-tax donations, whereas for 14 participants, post-tax donations were smaller than their pre-tax donations, reflecting heterogeneous responses to the tax realization. Even so, the Both group recorded the highest mean total donations (26.56 tokens).

In contrast, the Pre-only group predicted relatively high taxes (26.80 tokens) but faced even higher actual taxes (33.33 tokens), which may help explain why they did not make additional post-tax donations: participants may have perceived themselves as having already transferred sufficiently or had little remaining capacity for additional donations. The Post-only group made relatively large post-tax donations after observing the actual tax, resulting in total donations comparable to those of the Pre-only group. Lastly, the Neither group exhibited the largest prediction errors (mean = 9.48 tokens), suggesting that inaccurate expectations may be associated with the absence of donations.

However, averaging across participants may obscure the underlying individual-level dynamics, as the mean values mask variation in tax predictions and post-tax donation adjustments. To address this, we employ regression analyses to examine how discrepancies between predicted and actual taxes, as well as initial giving behavior, influence post-tax donations. This approach allows us to quantify the extent to which individual expectations and prior donations shape adjustment behavior.

3.2 Donation Adjustment after Tax Realization

Having established that participants donate before and/or after taxation, we now examine how they adjust their donations in response to actual tax. As before, we focus on Frame D, the discretionary condition, in which participants made an initial donation under uncertainty and an additional after learning the tax. Table 2 reports OLS and Tobit estimates for post-tax donation in Frame D. Because post-tax donations represent voluntary adjustments made after both the pre-tax donation had been committed and the actual tax amount had been revealed, the estimates capture how participants recalibrate their donations in response to prior transfers. The Tobit specification accounts for substantial left-censoring in the dependent variable (40 of 84 participants made no post-tax donations).

In the baseline specifications without donation pattern dummies (columns (1) and (2)), higher actual taxes are associated with significantly lower post-tax donations (OLS: $\beta = -0.466$, $p = 0.006$; Tobit: $\beta = -0.751$, $p < 0.01$), consistent with a partial crowding-out effect. Each additional token of taxation is associated with a reduction in post-tax donations of approximately 0.47 tokens (OLS) or 0.39 tokens (Tobit marginal effect). In contrast, pre-tax donations do not significantly predict post-tax donations. A Wald test rejects the equality of the coefficients on actual tax and pre-tax donations in the Tobit specification ($p = 0.037$), indicating that participants responded differently to actual tax and their own prior donations when adjusting post-tax donations.

When donation pattern dummies are included (columns (3) and (4)), the crowding-out effect of actual tax remains significant but is attenuated (OLS: $\beta = -0.334$, $p = 0.014$; Tobit: $\beta = -0.454$, $p = 0.017$), as the pattern dummies absorb part of the tax response. The Wald test for coefficient equality is no longer significant in these specifications, which is expected given that the pattern dummies are structurally correlated with pre-tax donation behavior. In column (3), tax prediction errors show a weak positive association with post-tax donations, although this relationship is not robust in other specifications. The positive sign suggests that participants facing higher-than-expected taxes may donate more after taxation, potentially reflecting that some participants targeted a more equal distribution.

Notably, a two-sample t-test indicates that mean total donations—defined as the sum of pre- and post-tax donations—do not differ between Frame D and Frame R ($t(146) = -0.024$, $p = 0.981$), suggesting that the timing of donation opportunities does not affect the total amount of voluntary giving.⁷ However, identical total donations do not necessarily imply the same distributional

⁷ The full regression results for total donation (OLS and Tobit) and the probability of donating (logit) are reported in

outcomes, as the scope for adjustment may depend on prior voluntary and mandatory transfers. We next examine the fairness benchmarks guiding such adjustments.

Table 2. Estimates for Post-Tax Donation in Frame D

	Post-Tax Donation			
	(1) OLS	(2) Tobit	(3) OLS	(4) Tobit
Actual Tax	-0.466 *** (0.165)	-0.751 *** (0.251)	-0.334 ** (0.133)	-0.454 ** (0.186)
Pre-Tax Donation	-0.083 (0.081)	-0.058 (0.156)	-0.125 (0.084)	-0.167 (0.137)
Tax Prediction Error	0.161 (0.109)	0.221 (0.181)	0.146 * (0.076)	0.154 (0.124)
Both			12.531 *** (2.617)	70.138 *** (5.456)
Pre-only			5.682 ** (2.259)	5.768 (5.300)
Post-only			19.178 *** (2.957)	76.566 *** (4.931)
Female	3.260 (2.770)	7.227 * (4.276)	0.073 (2.055)	1.046 (2.900)
Age	-0.147 (1.155)	-0.907 (1.812)	-0.244 (1.002)	-1.513 (1.455)
Econ	3.715 (2.437)	4.498 (4.060)	4.605 ** (1.989)	9.239 ** (3.513)
Volunteer	0.965 (0.706)	1.432 (1.096)	0.834 (0.529)	1.760 ** (0.750)
Constant	18.421 (23.332)	31.576 (37.575)	9.212 (18.969)	-23.606 (29.231)
N	84	84	84	84
Adj. R ²	0.254		0.595	
Pseudo R ²		0.049		0.275
F statistic (robust Wald)	3.149 ***	3.576 ***	9.347 ***	28.069 ***

Notes: The dependent variable is post-tax donation, measured in experimental tokens. Post-tax donations represent voluntary adjustments made after both the pre-tax donation had been committed and the actual tax amount had been revealed. Pre-tax donation refers to the amount donated before the tax was revealed. Actual tax denotes the mandatory tax for government transfers imposed on participants. Tax prediction error is defined as actual tax minus predicted tax. Donation pattern dummies classify participants as donating both before and after tax ("Both"), only before tax ("Pre-only"), or only after tax ("Post-only"), with non-donors ("Neither") as the omitted category. Columns (1) and (2) exclude donation pattern dummies; columns (3) and (4) include them. Individual controls include female (1 = female), age, economics major (1 = yes), and volunteer experience (1–5 scale). Tobit models account for left-censoring at zero. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B.

3.3 Fairness Benchmarks and Total Transfers

As a first-order approximation, we examine deviations from the equal-split benchmark—defined as each participant's final payoff minus 50. A negative deviation indicates that the dictator's total transfers exceeded half of the endowment (hereafter, "transfers exceeding half"), leaving the dictator with less than the recipient.

Figure 3 presents the distributions of final payoff deviations in Frames R and D. While the central tendency is similar across frames (Mann–Whitney: $z = -0.457$, $p = 0.648$), the variance is marginally larger in Frame D than in Frame R (Levene's test: $W_0 = 3.720$, $p = 0.056$), with a greater mass of negative deviations in Frame D. As noted above, mean total donations do not differ significantly across frames, implying that the greater dispersion and higher frequency of negative deviation in Frame D cannot be attributed to differences in overall giving levels. Rather, these findings suggest that under the discretionary condition, committing to a donation before observing the tax realization limits the scope for subsequent adjustment, leading participants' transfers exceeding half.

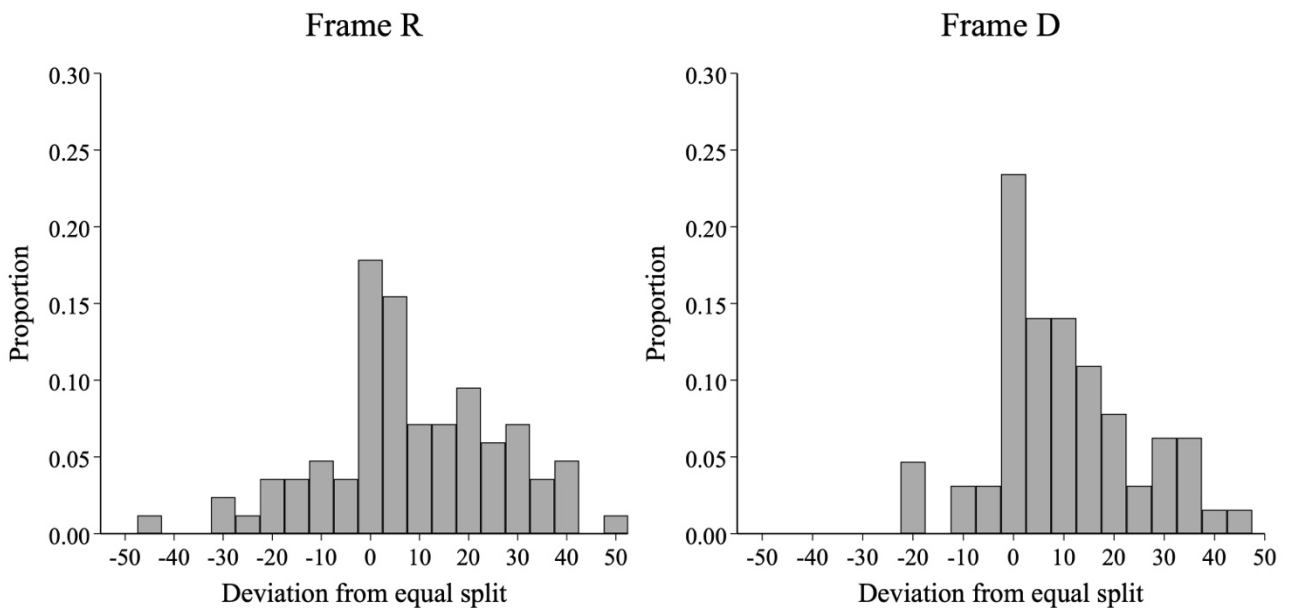


Figure 3. Distribution of Final Payoff Deviations in Frames R and D

Notes: Deviation from equal split equals the dictator's (i.e., each participant's) final payoff minus 50. The bin width is 5 tokens. The vertical axis reports the proportion of participants. Frame R: $N = 64$; Frame D: $N = 84$.

This greater dispersion in Frame D appears to be partly driven by a higher incidence of transfers exceeding half. In Frame D, for 21.4% of participants (18 of 84), transfers exceeded half, compared with 10.9% (7 of 64) in Frame R (Pearson $\chi^2(1) = 2.85$, $p = 0.091$). When participants commit to a donation before observing the actual tax, they face a risk of overshooting their intended allocation. The pre-tax donation may serve as a behavioral anchor, and subsequent adjustment to the actual tax may be insufficient—potentially consistent with anchoring and insufficient adjustment (Tversky and Kahneman, 1974). Alternatively, transfers exceeding half may reflect strong inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

The analysis thus far adopted the equal split as a first-order approximation of participants' fairness benchmarks. However, equality need not coincide with participants' subjective fairness standards. If participants regard a division other than 50:50 as fair, deviations from the equal split may reflect heterogeneous fairness perceptions rather than systematic over- or under-transfers relative to a universal norm. We examine this possibility using Frame R, the rule-based condition, in which participants observe the actual tax before making either donation decision, so that both the initial and additional donations are made with full knowledge of the tax amount. Under this condition, the total transfer—defined as the sum of the government transfer and donations—can be interpreted as a revealed measure of each participant's intended redistribution.

Figure 4 plots total transfer against actual tax in Frames R and D. In Frame R (left panel), where participants observe the tax before making any donation decision, the scatter provides a benchmark for participants' fairness preferences. Several patterns emerge. First, a non-trivial cluster of observations falls exactly on the equal-split line: 14 of 64 participants (21.9%) deliberately target a 50:50 division. Second, approximately one-fifth of observations lie on the 45-degree line, representing participants who made no donation beyond the government transfer: 15 of 64 participants (23.4%). Third, most observations fall between these two benchmarks, with the mean total transfer of approximately 40 tokens—well below the equal split of 50. These patterns suggest substantial heterogeneity in fairness standards: some participants strictly pursue equality, others provide partial donations, and a sizable group leaves redistribution entirely to taxation.

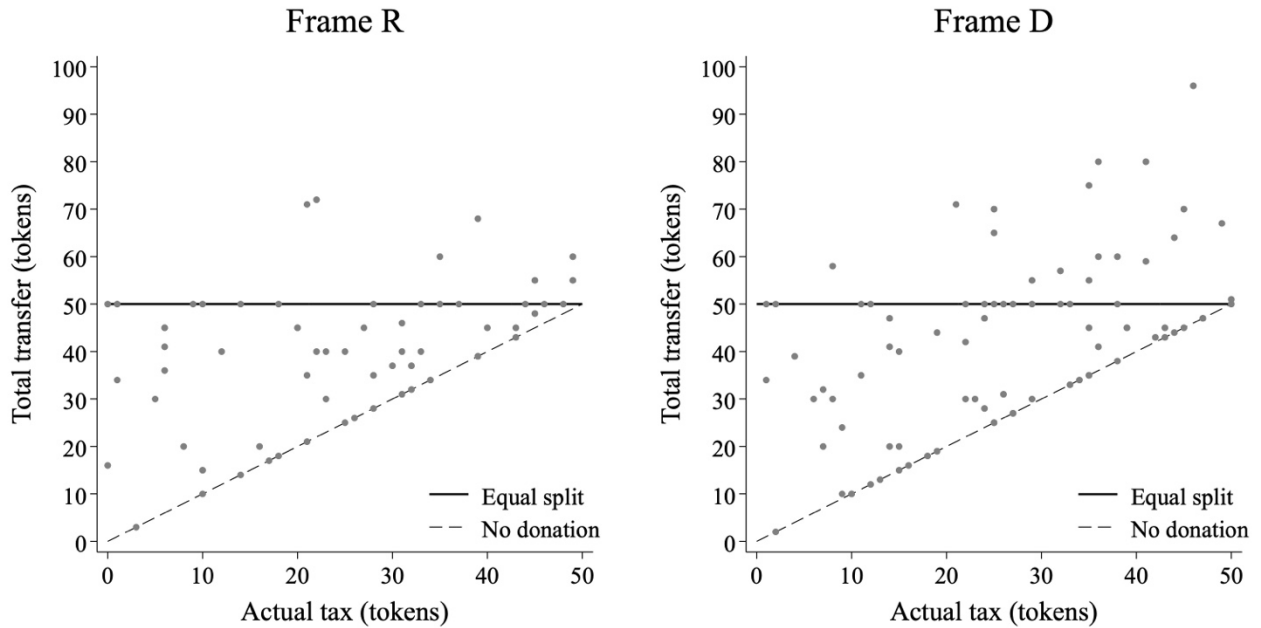


Figure 4. Total Transfer against Actual Tax in Frames R and D

Notes: Each point represents one participant (left panel: Frame R, $N = 64$; right panel: Frame D, $N = 84$). Total transfer equals the sum of the government transfer (i.e., actual tax) and donations. The solid line at 50 indicates the equal-split benchmark. The dashed line at 45 degrees indicates zero donation. Some observations overlap due to identical values.

Taking this distribution as a benchmark for participants' underlying fairness preferences, Frame D (right panel) exhibits a noticeably wider spread. While 14 of 84 observations (16.7%) fall on the equal-split line—somewhat fewer than in Frame R—a larger share falls on the 45-degree line (25 of 84, 29.8%), indicating outcomes in which total transfers equal the government transfer and no donation is made. Moreover, Frame D contains more observations well above the equal-split line than Frame R, suggesting that more participants in the discretionary condition made transfers exceeding half. These patterns indicate that when the initial donation is chosen before the tax is realized, participants may be unable to perfectly adjust their donations *ex post*, leading to more dispersed redistribution outcomes. We next quantify these frame differences.

Table 3 reports OLS estimates for total transfers from dictators to recipients, separately by frame and for the pooled sample. We begin with Frame R, the rule-based condition, in which participants observe the tax before making any donation decision (column (1)). The coefficient on actual tax is 0.382 ($p < 0.01$), significantly different from both zero ($p < 0.01$) and one ($F = 28.835$, $p < 0.001$). Because total transfers are the sum of actual tax and donations, a coefficient below one indicates that donations partially decrease as taxation increases. This coefficient implies that each additional token of taxation is associated with a reduction in donations of 0.62 tokens—roughly 60

percent crowding out. The active calibration, together with the firm rejection of one-for-one pass-through, suggests that participants in the rule-based condition adjusted their donations in response to taxation, consistent with partial crowding out driven by warm-glow motivation relatively free from adjustment constraints. Predicted tax is also positively associated with total transfers ($p < 0.05$). Because participants in Frame R observe the actual tax before donating, this association should not be interpreted as reflecting a causal effect of expectations on behavior. We therefore report this result for completeness without further interpretation.

For comparison, we turn to Frame D, the discretionary condition, in which the initial donation is made before the tax is revealed (column (2)). The coefficient on actual tax is 0.787 ($p < 0.001$), which cannot be statistically distinguished from unity ($F = 2.404$, $p = 0.125$). This coefficient implies that each additional token of taxation is associated with a reduction in donations of 0.21 tokens—roughly 20 percent crowding out, substantially less adjustment than in Frame R. This suggests that participants' adjustment capacity was limited in the discretionary condition, where pre-tax donations had already been committed. Predicted tax is not significantly associated with total transfers in Frame D, suggesting that forward-looking expectations may play a smaller role when the initial donation has already been made.

To formally compare responsiveness across frames, we also report a pooled model with interactions between Frame D and both tax variables (column (3)). The interaction between Frame D and actual tax is positive and marginally significant ($p = 0.087$), confirming that donations respond less to taxation in the discretionary condition than in the rule-based condition. These findings suggest that the timing of donation opportunities shapes not the overall level of voluntary giving but the degree to which participants calibrate their donations toward an internal benchmark.

Table 3. Estimates for Total Transfer from Dictators to Recipients

	Total Transfer		
	(1)	(2)	(3)
	OLS Frame R	OLS Frame D	OLS Pooled
Actual Tax	0.382 *** (0.115)	0.787 *** (0.137)	0.443 *** (0.115)
Predicted Tax	0.337 ** (0.155)	0.123 (0.162)	0.311 ** (0.149)
Frame D			-1.976 (7.949)
Frame D × Actual Tax			0.303 * (0.176)
Frame D × Predicted Tax			-0.194 (0.215)
Female	0.394 (3.458)	2.655 (3.471)	1.542 (2.514)
Age	1.190 (1.532)	-1.499 (1.759)	-0.034 (1.156)
Econ	-5.725 * (3.179)	3.868 (3.560)	-0.357 (2.459)
Volunteer	0.516 (0.943)	1.226 (0.981)	0.754 (0.703)
Constant	0.155 (33.074)	44.855 (37.327)	21.124 (25.406)
N	64	84	148
Adj. R ²	0.188	0.276	0.234
F	3.358 ***	5.981 ***	5.509 ***
F (Actual Tax = 1)	28.835 *** [0.000]	2.404 [0.125]	

Notes: The dependent variable is total transfer from dictators to recipients, defined as the sum of actual tax and donations. Frame D is a dummy variable equal to one for participants in Frame D. Columns (1) and (2) report separate OLS estimates for each frame. Column (3) reports a pooled OLS model with frame interactions. F (Actual Tax = 1) tests the null hypothesis that the coefficient on actual tax equals one, with p-values in brackets. Individual controls include female (1 = female), age, economics major (1 = yes), and volunteer experience (1–5 scale). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.4 Determinants of Transfers Exceeding half

The preceding analysis showed that participants' fairness benchmarks are heterogeneous, with the mean total transfer in Frame R around 40 tokens—below the equal split of 50. Against this backdrop, we examine cases in which participants' final payoffs fall below the equal-split benchmark. Although the equal split may not represent participants' subjective fairness standard, it provides a clear and

conservative reference: a dictator whose final payoff fall below the equal split end up with less than the recipient.

Table 4 reports logit estimates and average marginal effects for the probability of transfers exceeding half, alongside OLS estimates for deviations from the equal-split benchmark. The logit average marginal effects (column (2)) indicate that each additional token of actual tax increases the probability of transfers exceeding half by roughly 1 percentage point on average ($p < 0.001$). The Frame D dummy indicates an increase of about 10.3 percentage points in the probability of transfers exceeding half, though this effect is only marginally significant ($p = 0.073$). This is consistent with the sequencing effect documented in Section 3.2: when the initial donation precedes the taxation, participants have less scope to adjust donations in light of the actual tax, increasing the likelihood of overshooting. The predicted tax shows a marginal positive association with transfer exceeding half ($p = 0.099$), conditional on actual tax.⁸

In the OLS specification for final payoff deviation (column (3)), the actual tax has a coefficient of -0.611 ($p < 0.001$). Under complete crowding out, this coefficient would equal zero, whereas in the absence of crowding out it would equal -1 . The coefficient indicates partial crowding out, consistent with the frame-specific estimates reported in Section 3.3. The Frame D dummy is not statistically significant ($p = 0.602$), confirming that mean deviations do not differ across frames. Predicted tax shows a marginally negative association with deviation ($p = 0.080$).

Taken together, these results indicate that the timing of donation opportunities does not affect the average level of redistribution but does influence its stability. Pre-tax donation opportunities create a two-stage decision environment that increases the likelihood of transfer exceeding half, even though mean total donations remain unchanged. The higher incidence of overshooting in Frame D reflects not stronger egalitarian norms but rather limited adjustment capacity under sequential decision-making, consistent with anchoring and insufficient adjustment (Tversky and Kahneman, 1974).⁹

⁸ We also estimated specifications replacing predicted tax with tax prediction error (defined as actual tax minus predicted tax). When both tax prediction error and actual tax are included, the coefficient on tax prediction error equals the negative of the predicted tax coefficient, yielding no additional information. When tax prediction error is entered as the sole tax variable, larger prediction errors are associated with a higher probability of transfers exceeding half (Logit AME = 0.004, $p = 0.039$) and lower final payoffs (OLS: $\beta = -0.250$, $p < 0.001$). However, this association largely reflects the level of actual tax embedded in the prediction error rather than forecast inaccuracy per se.

⁹ We also estimated models including interactions between the Frame D dummy and both the predicted tax and actual Tax. Neither interaction term is statistically significant in either specification, likely reflecting limited statistical power given the small number of transfers exceeding half ($N = 25$). The main effects remain stable across all specifications.

Table 4. Estimates for Transfers Exceeding half and Final Payoff Deviations

	Exceeds Half		Deviation
	(1) Logit	(2) Logit AME	(3) OLS
Frame D	0.908 *	0.103 *	-1.234
	(0.508)	(0.057)	(2.364)
Predicted Tax	0.032 *	0.004 *	-0.203 *
	(0.019)	(0.002)	(0.115)
Actual Tax	0.086 ***	0.010 ***	-0.611 ***
	(0.021)	(0.002)	(0.093)
Female	0.086	0.010	-1.295
	(0.535)	(0.060)	(2.517)
Age	0.206	0.023	0.048
	(0.239)	(0.027)	(1.139)
Econ	-0.302	-0.034	-0.078
	(0.500)	(0.056)	(2.522)
Volunteer	0.036	0.004	-0.884
	(0.140)	(0.016)	(0.699)
Constant	-9.659 *		30.434
	(5.015)		(24.554)
N	148	148	148
Pseudo R ²	0.198	0.198	
Adj. R ²			0.222
Wald χ^2	26.881 ***	26.881 ***	
F			6.623 ***

Notes: Columns (1) and (2) report logit coefficient estimates and average marginal effects, respectively, for the probability of total transfers exceeding half of the endowment. Exceeds Half is a dummy variable equal to one if total transfers exceed half of the endowment (50 tokens). Column (3) reports OLS estimates for deviation from the equal-split benchmark, defined as the dictator's (i.e., each participant's) final payoff minus 50. Individual controls include female (1 = female), age, economics major (1 = yes), and volunteer experience (1–5 scale). Results are robust to a parsimonious specification excluding individual controls (Logit AME of Actual Tax = 0.010, $p < 0.01$; Logit AME of Frame D = 0.099, $p = 0.093$). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4. Conclusion

This paper investigated how the timing of donation opportunities relative to taxation for government transfer affects altruistic behavior, using a laboratory experiment based on a dictator game. We implemented two conditions reflecting distinct policy environments: a rule-based frame (Frame R), in which taxation is fully observable before any donation decision, and a discretionary frame (Frame D), in which the initial donation precedes the tax revelation. The experimental design allows us to examine not only whether individuals donate under anticipated taxation, but also how they adjust

their donations once the tax is realized, and how the sequencing of these decisions shapes distributional outcomes. Our findings can be summarized as follows.

First, participants donated even when future taxation was anticipated, and total donations did not differ across the two conditions. This result is inconsistent with standard models of pure altruism that predict complete crowding out of private giving by public transfers. Instead, it is more consistent with models of impure altruism or warm-glow giving in which the act of giving itself yields utility (Andreoni, 1989, 1990).

Second, participants responded asymmetrically to donations and government transfers (i.e., actual tax) when adjusting their post-tax donations. Mandatory taxation significantly reduced subsequent donations, whereas prior donations did not. This asymmetry suggests that government transfers were incorporated more directly into post-tax decisions than earlier donations. One possible explanation is that the sequential structure of the decision limited the extent to which participants revised earlier donations when making post-tax decisions. Another possibility is that donations and government transfers were processed in separate psychological accounts, leading participants to respond more strongly to taxation than to their own earlier donations, consistent with mental accounting (Thaler, 1999).

Third, participants' fairness benchmarks were heterogeneous and did not generally coincide with the equal split. In Frame R, where both donation decisions were made after observing the tax, the average total transfer was approximately 40 tokens—well below the equal split of 50. This pattern is broadly consistent with models of inequity aversion (Fehr and Schmidt, 1999) and equity preferences (Bolton and Ockenfels, 2000), in which individuals dislike unequal outcomes but do not necessarily target full equality. The coefficient on actual tax in Frame R (0.382) indicates that participants actively calibrated their donations in response to government transfers, offsetting roughly 60 percent of each additional token of taxation. By contrast, the corresponding coefficient (0.787) in Frame D implies that donations decreased by only about 20 percent of each additional token of taxation, consistent with limited adjustment capacity when pre-tax donations had already been committed.

Fourth, this asymmetry in adjustment capacity had consequences for distributional outcomes. Pre-tax donation opportunities increased the dispersion of final payoffs and the incidence of transfers exceeding half, even though mean total donations remained unchanged across frames. The higher incidence of overshooting in Frame D reflects not stronger egalitarian norms but rather limited adjustment capacity under sequential decision-making.

These findings carry implications for the design of tax-and-transfer systems. Under discretionary policy environments, where voluntary giving precedes mandatory taxation, individuals face a risk of overshooting their intended allocation, potentially transferring more than intended. Under rule-based policy environments, by contrast, individuals can calibrate their giving in light of known tax obligations, yielding more stable redistribution outcomes. This suggests that policymakers should consider how the sequencing of public and private transfers affects not only the level but also the stability of redistribution outcomes. In contexts where voluntary giving and taxation coexist—such as disaster relief, charitable giving with tax deductions, or community-based public goods provision—the order in which individuals encounter giving opportunities and tax obligations may shape the distributional consequences of the overall transfer system.

Several limitations should be noted. Our experiment employed a one-shot dictator game with a single tax realization, which abstracts from repeated interactions, learning, and strategic considerations that may arise in real-world settings. The tax amount was randomly assigned rather than determined by a deliberative political process, which may limit the external validity of the findings regarding fiscal perceptions. Additionally, our participant pool consisted of university students, and the extent to which our results generalize to broader populations remains an open question.

Future research could extend our framework in several directions. Examining whether repeated interaction or experience with taxation alters the adjustment patterns documented here would be valuable. Investigating the role of information about others' donations or about the intended use of tax revenues could shed light on the mechanisms underlying fairness benchmarks. Finally, exploring how the sequencing of voluntary and mandatory transfers operates in field settings—where stakes, social norms, and institutional complexity are substantially greater—would provide an important complement to the laboratory evidence presented here.

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Appendix A. Experimental Instructions and Practice Problems

Appendix A contains the English translations of the experimental instructions and the practice questions for the Frame D treatment, which were originally written in Japanese.

A.1 Instructions

This experiment is designed to study how individuals make decisions in a particular environment. It is financially supported by Takasaki City University of Economics. By carefully reading these instructions, you can increase your earnings depending on the choices you make.

The procedures of the experiment are simple. The payment you earn in this experiment (hereafter, "experimental earnings") depends on your decisions and the decisions of other participants, according to the rules determined by the experimenter. Experimental earnings are calculated in an experimental currency called "tokens". At the end of the experiment, you will receive your experimental earnings in addition to a participation fee of 1,000 yen. Tokens will be converted into cash at the rate of 1 token = 25 yen and paid in cash.

Please read these instructions carefully to ensure that you fully understand the experiment before proceeding. After the explanation, you will be asked to complete practice questions to confirm your understanding. The experiment will begin only after all participants have answered all practice questions correctly. Please do not operate your computer until instructed by the staff. (You may freely use a pen, a calculator, and memo paper during the experiment.)

Overview of the Experiment

In this experiment, participants are paired in advance according to the identification numbers (IDs). Within each pair, one participant is referred to as Player A and the other as Player B. As of now, the roles of Player A and Player B have not yet been assigned.

At the beginning of the experiment, Player A is endowed with 100 tokens, and Player B is endowed with 0 tokens. Player A chooses how many tokens to transfer voluntarily to Player B. The transfer must be an integer between 0 and 50 (inclusive). However, in addition to this voluntary transfer, the experimenter will impose a mandatory transfer of tokens from Player A to Player B. The number of tokens collected by the experimenter is determined randomly and independently of Player A's voluntary transfer. The mandatory transfer is an integer between 0 and 50 (inclusive), and each integer in this range is equally likely.

Let, X denote the number of tokens Player A voluntarily transfers to Player B, and Y denote the number of tokens collected by the experimenter from Player A and transferred to Player B. Final token allocations are determined as follows:

$$\text{Player A's earnings: } 100 - X - Y$$

$$\text{Player B's earnings: } X + Y$$

Note that no participant's final earnings can be negative because $X + Y$ will never exceed 100.

Determination of Pairs and Roles

Two assignment patterns, labeled Pattern α and Pattern β , determine pairings and roles (Player A or Player B) based on the IDs. In each pattern, you are paired with a different participant. Your role also differs across patterns. If you are assigned the role of Player A under Pattern α , you will be assigned the role of Player B under Pattern β . Conversely, if you are Player B under Pattern α , you will be Player A under Pattern β .

You will be able to confirm your role for each pattern on your computer screen once the experiment begins. After the experiment is completed, a lottery will determine the pairing and your role (Player A or Player B) that will be implemented for payment. Therefore, because you may be assigned to either role, you will make your decisions in the experiment under the assumption that you are Player A.

Experimental Procedure

When the experiment begins, your computer screen will display the Mandatory Transfer Prediction screen shown below; the numbers (1)–(5) are for explanatory purposes only and do not appear on the actual screen, the font sizes and other formatting may differ from the actual display, and "***" represents one- to three- digit numerical values; these conventions apply to all subsequent figures.

Mandatory Transfer Prediction Screen

(1) 1 / 1

(2) Time Remaining (seconds) **

(3) Your Participant ID: **

(4) You and your partner's role (Player A or Player B) will be determined by whether the lottery result is Pattern α or Pattern β . During the experiment, you will make decisions as Player A, but the lottery may assign you to be Player B.

Pattern α :

Player A ID: **

Player B ID: **

Pattern β :

Player A ID: **

Player B ID: **

(5) Player A Initial Tokens: 100

Player B Initial Tokens: 0

Please predict the number of tokens that will be collected mandatorily from you (Player A) by the experimenter and transferred to your paired participant (Player B).

You may enter any integer between 0 and 50, inclusive.

OK

In Area (1), the number of rounds is displayed. Because this experiment consists of only one round, the screen will always display "1/1". In Area (2), the remaining decision time (hereafter, "the time limit") is displayed in seconds. In Area (3), your participant ID is displayed. In Area (4), the IDs of you and your paired participant are displayed for both Pattern α and Pattern β . As the implemented pattern is determined by lottery after the experiment is completed, you will make decisions under the assumption that you are Player A. In Area (5), the initial token endowments of you (Player A) and your paired participant (Player B) are displayed. Below this, a box is provided in which you enter your prediction of the number of tokens that the experimenter will collect mandatorily from you (Player A) and transfer to your paired participant (Player B).

Step 1: Prediction Decision

Please predict the number of tokens the experimenter will mandatorily collect from you (Player A) and transfer to your paired participant (Player B), and enter your prediction in the box in Area (5). You may enter any integer between 0 and 50, inclusive. After entering your prediction, click the OK button. The time limit for this decision is 60 seconds. Once you click the OK button, you cannot revise your choice.

Clicking the OK button without entering a value will trigger a warning message in German. If the warning message displayed, please raise your hand and call an experimental staff (the same applies to other decision screens). Failure to click the OK button before the time limit expires will cause the message "Please decide" to appear in Area (2). The screen will not automatically proceed to the next stage. If the time limit is exceeded, please promptly enter a number and click the OK button. After you click the OK button, the screen will change to the Waiting Screen. The Waiting Screen will remain until all participants in the laboratory have clicked the OK button.

Please keep the following in mind:

- The number of tokens the experimenter will collect from you (Player A) and transfer to your paired participant (Player B) is determined independently of your prediction, with one integer between 0 and 50, inclusive, randomly selected for each participant, each integer being equally likely.
- If your prediction of mandatory transfer is correct, you will receive an additional 8 tokens as a prediction bonus at the end of the experiment. The bonus is determined based on the number of tokens that would be collected from you if you were assigned the role of Player A and is paid regardless of whether you are ultimately assigned that role.

Once all participants in the laboratory click the OK button, the screen will proceed to the Voluntary Transfer Decision Screen. The information displayed in Areas (1) through (4), as well as the initial token endowments shown in Area (5) are the same as those shown on the Prediction of Mandatory Transfer Screen.

Voluntary Transfer Decision Screen

(1) 1 / 1

(2) Time Remaining (seconds) **

(3) Your Participant ID:
**

(4) You and your partner's role (Player A or Player B) will be determined by whether the lottery result is Pattern α or Pattern β . During the experiment, you will make decisions as Player A, but the lottery may assign you to be Player B.

Pattern α :

Player A ID: **
Player B ID: **

Pattern β :

Player A ID: **
Player B ID: **

(5) Player A Initial Tokens: 100
Player B Initial Tokens: 0

In addition to the tokens collected by the experimenter, please enter the number of tokens you (Player A) voluntarily transfer to your paired participant (Player B).
You may enter any integer between 0 and 50, inclusive.

OK

Step 2: Voluntary Transfer Decision

In addition to the tokens collected from you (Player A) by the experimenter, please decide how many tokens you would like to transfer voluntarily to your paired participant (Player B), and enter your decision in the box in Area (5). You may enter any integer between 0 and 50, inclusive. After entering your decision, click the OK button. The time limit for this decision is 120 seconds. Once you click the OK button, you cannot revise your decision.

After you click the OK button, the screen will change to the Waiting Screen. The Waiting Screen will remain until all participants in the laboratory have clicked the OK button or the time limit has expired.

Please remember the following:

- At this point, you do not know the number of tokens the experimenter will collect from you (Player A) and transfer to your paired participant (Player B).

- The number of tokens the experimenter will collect from you (Player A) and transfer to your paired participant (Player B) is determined independently of your voluntary transfer decision and is randomly selected for each participant.
- If the time limit expires in this step, the screen will automatically proceed to the next stage, and your voluntary transfer will be set to 0 unless you have clicked OK.

Once all participants in the laboratory click the OK button or the time limit expires, the screen will proceed to the Mandatory Transfer Confirmation Screen.

Mandatory Transfer Confirmation Screen

(1) 1 / 1

(2) Time Remaining (seconds) **

(3) Your Participant ID: **

(4) You and your partner's role (Player A or Player B) will be determined by whether the lottery result is Pattern α or Pattern β . During the experiment, you will make decisions as Player A, but the lottery may assign you to be Player B.

Pattern α :

Player A ID: **

Player B ID: **

Pattern β :

Player A ID: **

Player B ID: **

(5)

Player A Current Tokens: **

Player B Current Tokens: **

The number of tokens that will be collected from you (Player A) by the experimenter is as follows.

**

Please confirm the information and press the OK button.

OK

In Area (5), the number of tokens currently held by you (Player A) and your paired participant (Player B) are displayed. (These amounts reflect the token holdings after your voluntary transfer in Step 2.) Below this information, the number of tokens the experimenter will mandatorily collect from you is displayed.

Step 3: Confirmation of Mandatory Collection

Please review the number of tokens currently held by you (Player A) and your paired participant (Player B), as well as the number of tokens that will be collected from you by the experimenter, and then click OK button. The time limit for this step is 30 seconds. Once you click OK button, you cannot return to the previous screen to review this information again.

After you click the OK button, the screen will change to the Waiting Screen. The Waiting Screen will remain until all participants in the laboratory have clicked the OK button or the time limit has expired. Once all participants have clicked the OK button or the time limit has expired, the screen will proceed to the Additional Transfer Decision Screen.

Additional Transfer Decision Screen

(1) 1 / 1

(2) Time Remaining (seconds) **

(3) Your Participant ID: **

(4) You and your partner's role (Player A or Player B) will be determined by whether the lottery result is Pattern α or Pattern β . During the experiment, you will make decisions as Player A, but the lottery may assign you to be Player B.

Pattern α :

Player A	ID: **
Player B	ID: **

Pattern β :

Player A	ID: **
Player B	ID: **

(5) Player A Current Tokens: **

Player B Current Tokens: **

Will you (Player A) add any tokens to transfer to your paired participant (Player B)? Please Enter the number of additional tokens if you want to add any; otherwise, enter 0.

You may enter any integer between 0 and 50, inclusive.

OK

In Area (5), the screen displays the number of tokens currently held by you (Player A) and your paired participant (Player B). (These amounts reflect the token holdings after the mandatory transfer by the experimenter in Step 3 has been deducted.) Upon reviewing the current token holdings of both participants, you may add to the number of tokens you voluntarily transfer to your paired participant.

Step 4: Additional Voluntary Transfer Decision

If you (Player A) wish to transfer additional tokens to your paired participant (Player B), enter the number of additional tokens in the box in Area (5); otherwise, enter 0. You may enter any integer from 0 up to 50 minus the number of tokens you transferred in Step 2. After entering your decision, click the OK button. The time limit for this decision is 120 seconds. Once you click the OK button, you cannot revise your decision.

After you click the OK button, the screen will change to the Waiting Screen. The Waiting Screen will remain until all participants in the laboratory have clicked the OK button or the time limit has expired.

Please remember the following:

- If you transferred 50 tokens in Step 2, you cannot make any additional voluntary transfer to your paired participant. In this case, enter 0 and click the OK button.
- Negative numbers cannot be entered; in other words, you cannot reclaim any tokens transferred in Step 2.
- If the time limit expires in this step, the screen will automatically proceed to the next stage, and your voluntary transfer will be set to 0 unless you have clicked OK.

Once all participants in the laboratory have clicked the OK button or the time limit has expired, the screen will proceed to the Results Confirmation Screen. In Area (5), the experimental results are displayed for both Pattern α and Pattern β . For the pattern in which you are assigned the role of Player A: (a) is the total number of tokens you (Player A) transferred to your paired participant (Player B), that is, the sum of the tokens you selected in Steps 2 and 4. (b) is the number of tokens the experimenter collected from you (Player A) and transferred to your paired participant (Player B). (c) and (d) indicate the final token holdings of you (Player A) and your paired participant (Player B), respectively. In contrast, for the pattern in which you are assigned the role of Player B: (a) is the total number of tokens your paired participant (Player A) transferred to you (Player B). (b) is the number of tokens the experimenter collected from your paired participant (Player A) and transferred to you (Player B). (c) and (d) indicate the final token holdings of your paired participant (Player A) and you (Player B), respectively.

Results Confirmation Screen

(1) 1 / 1

(2) Time Remaining (seconds) **

(3)

Your Participant ID:
**

(4)

You and your partner's role (Player A or Player B) will be determined by whether the lottery result is Pattern α or Pattern β . During the experiment, you will make decisions as Player A, but the lottery may assign you to be Player B.

Pattern α :

Player A ID: **

Player B ID: **

Pattern β :

Player A ID: **

Player B ID: **

(5)

Pattern	α	β
Player A	**	**
Player B	**	**
(a) Total Tokens Transferred by Player A to Player B	**	**
(b) Tokens Collected by Experimenter	**	**
(c) Final Tokens of Player A	**	**
(d) Final Tokens of Player B	**	**

Prediction Accuracy Bonus **

Step 5: Confirmation of Experimental Results

The experimental results for each pattern, in which you may be assigned as Player A or Player B, are displayed in Area (5) on your computer screen. Please review the information and click the OK button. The time limit for this step is 60 seconds.

Once all participants in the laboratory have clicked the OK button or the time limit has expired, the screen will proceed to the Waiting Screen.

Determination of Experimental Payment

After all participants' screens have moved to the Waiting Screen, one of the two patterns (α or β) will be selected to determine your paired participant, your role, and your final token holdings. First, the experiment staff will prepare two cards, one labeled " α " and the other labeled " β " on the front. In full view of all participants, the cards will be turned face down and shuffled. Next, one participant will be selected as a representative and asked to randomly draw one of the two face-down cards. If the selected card is " α ," your paired participant, your role, and your final token holdings will be

determined according to Pattern α . If the selected card is " β ," they will be determined according to Pattern β .

Notes

1. During the experiment, your decisions may be known only to your paired participant under each pattern (α and β). They will not be disclosed to any other participant. Your prediction will not be disclosed to any other participant, including your paired participant.
2. Your experimental earnings are equal to the sum of your final token holdings and any prediction bonus tokens, multiplied by 25 yen, plus a fixed participation fee of 1,000 yen.
3. No personal information of any participant—including your paired participant—such as name, student ID, gender, or contact information, will be disclosed to you during or after the experiment. To prevent any inference about seating assignments, participant IDs have been randomly assigned to seats.
4. During the experiment, please follow only the instructions provided in the experimental instructions and on the computer screen. Performing any actions not specified in the instructions may result in the failure to save the experimental data. Even if such failure is unintentional, no payment can be made if the data loss is caused by your actions.
5. If the experimental data cannot be saved due to another participant's inattention, or if the experiment cannot be continued for any reason, you will receive only the participation fee.
6. Your decisions and payment in this experiment will never be publicly disclosed together with your name. The correspondence between participants' names and IDs will not be disclosed to anyone other than the accounting staff, even after the experiment has concluded.
7. If you have any questions during the experiment, please raise your hand and call a experimental staff.

A.2 Practice Problems

These questions are designed to ensure that you understand the procedures of the experiment. Please answer all questions while referring to the experimental instructions. After you finish, please raise your hand and call a staff member. The experiment will begin after all participants have answered every question correctly. Your answers to these questions do not affect your experimental earnings.

Question 1

For each statement below, select the most appropriate answer and check the corresponding box.

- (1) Whether a participant in a pair will be Player A or Player B is [] in advance.
 - predetermined
 - not predetermined
- (2) Participants make their decisions [] whether they are Player A or Player B.
 - without knowing
 - after knowing
- (3) After the experiment ends, the probability that you are Player A is:
 - 0%
 - 50%
 - 100%
- (4) After the experiment ends, the probability that you are Player B is:
 - 0%
 - 50%
 - 100%
- (5) The participant paired with you is [] across Pattern α and Pattern β .
 - the same
 - different
- (6) Suppose that in Pattern α you are Player A and your partner is Player B. In Pattern β , you will be. Player:
 - A
 - B

Question 2

Assume that you are Player A and answer the following questions.

- (1) In Step 2, you choose to give 10 points to Player B. In Step 3, the experimenter determines that 5 points are taxed. In Step 4, you choose to give an additional 5 points to Player B.

Your final payoff = _____

Your partner's final payoff = _____

- (2) In Step 2, you choose to give 45 points to Player B. In Step 3, the experimenter determines that 30 points are taxed. In Step 4, you choose to give an additional 5 points to Player B.

Your final payoff = _____

Your partner's final payoff = _____

- (3) In Step 2, you choose to give 0 points to Player B. In Step 3, the experimenter determines that 5 points are taxed. In Step 4, you choose to give an additional 0 points to Player B.

Your final payoff = _____

Your partner's final payoff = _____

- (4) In Step 2, you choose to give 10 points to Player B. In Step 3, the experimenter determines that 30 points are taxed. In Step 4, you choose to give an additional 15 points to Player B.

Your final payoff = _____

Your partner's final payoff = _____

Appendix B. Supplementary Tables

Appendix B provides sample characteristics, as well as the regression tables related to the t-test reported at the end of Section 3.3. While the text reports only the t-test results showing no difference between frames, the regression tables present the corresponding OLS, Tobit, and Logit analyses for further reference.

B.1 Sample Characteristics

Table B.1 reports demographic and background characteristics by frame. None of these characteristics differ significantly between frames (all $p > 0.27$), confirming successful randomization.

Table B.1. Sample Characteristics and Balance Tests

Variables	Full Sample (N = 148)	Frame R (N = 64)	Frame D (N = 84)	p-value
Female (1 = Female)	0.37 (0.49)	0.42 (0.50)	0.33 (0.47)	0.27
Age (years)	20.48 (1.23)	20.55 (1.36)	20.43 (1.12)	0.56
Econ (1 = Economics Major)	0.45 (0.50)	0.45 (0.50)	0.45 (0.50)	0.99
Volunteer Experience (0–5)	1.62 (1.82)	1.69 (1.83)	1.57 (1.83)	0.70

Notes: Each cell shows mean with standard deviation in parentheses. P-values from two-sample t-tests. Volunteer Experience is measured on a 0–5 scale based on self-reported frequency of volunteer activities (0 = none, 5 = five or more times).

B.2 Supplementary Regression Results

Table B.2 reports the full regression results for total donations (OLS and Tobit) and the probability of donating (logit) related to the t-test reported at the end of Section 3.2.

Table B.2. Estimates for Total Donations and Donation Incidence (Full Results)

	Total Donation		Donation dummy
	(1) OLS	(2) Tobit	(3) Logit
Frame D	-1.234 (2.364)	-0.827 (3.068)	0.202 (0.393)
Actual Tax	-0.389 *** (0.093)	-0.439 *** (0.116)	-0.009 (0.014)
Predicted Tax	0.203 * (0.115)	0.297 ** (0.150)	0.029 * (0.017)
Female	1.295 (2.517)	2.704 (3.195)	0.462 (0.434)
Age	-0.048 (1.139)	0.199 (1.437)	0.089 (0.160)
Econ	0.078 (2.522)	-0.909 (3.294)	-0.442 (0.392)
Volunteer	0.884 (0.699)	1.344 (0.883)	0.174 (0.116)
Constant	20.800 (24.574)	10.731 (30.882)	-1.489 (3.295)
N	148	148	148
Adj. R ²	0.133		
Pseudo R ²		0.023	0.058
F	5.498 ***	4.475 ***	
Wald χ^2			8.403

Notes: The dependent variable in columns (1) and (2) is total donation, measured in experimental tokens and defined as the sum of pre-tax and post-tax donations. The dependent variable in column (3) is a dummy variable equal to one if total donations are strictly positive. Frame D is a dummy variable equal to one for participants in Frame D. Individual controls include Female (1 = female), Age, Econ (1 = economics major), and Volunteer Experience (0–5 scale). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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