Justice, What Money Can Buy: A Lab Experiment on Primary Social Goods and the Rawlsian Difference Principle

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Abstract: Many governments and charities adopt Rawlsian difference principle by maximizing the welfare of the least advantaged and giving priority to equality over efficiency. There are two views about which domain the principle should be applied to. The first applies it to the final distribution of income. Previous empirical studies have focused on this but found little evidence supporting it. The other view linked the

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principle with Rawlsian primary goods: Since the cost of losing primary social goods is huge, people will maximize the benefit of the least advantaged behind the veil of ignorance, such that everyone has access to necessary means. According to the latter reading of Rawls, we experimentally imposed a great cost for losing primary goods, and observed a salient majority of subjects obeying this principle, unlike previous studies finding a minority. Moreover, even if we lowered the cost for losing primary goods, more than one third of the subjects still adopted this principle.

Key words difference principle · primary social goods · fair equality of opportunity · veil of ignorance · John Rawls.

JEL codes**:** D63, D71, C91

1. Introduction

In 1968, Lena Maria Klingvall was born without arms in Sweden. Despite inborn disabilities, she accomplished amazing things, such as studying at the Royal College of Music in Stockholm, singing at the opening ceremony of the 1988 Paralympic Games, and touring around the world. She set two world records in swimming and won six golds at the world championships. These are achieved because western governments and charities provide plenty and expensive support for disabled people. For example, the Swedish government installs special elevators in severely disabled people's houses (Swedish Institute 2015). Blind Children UK (2013) spent £27,300 per guide dog, mounting to £50 million in 2012.

By paying special attention to the benefits of the least advantaged members of societies, disability policies aim to improve “equal opportunities” among all. Extra resources are allocated to those whose impairments cause various obstacles in life, so that they also have opportunities to realize their career goals. Such policies hence fulfill Rawls’s thought of justice, “fair equality of opportunity” (Rawls 1971, Sections 11 and 14).

Less developed countries also pay attention to the least advantaged. For example, Nussbaum (2000) described the following real world dilemma in India: parents were so poor that they could not afford to lose the help of their children in the fields. If they allowed their children to go to schools, families may not have a sufficient harvest to feed all their members. Some local governments aggressively subsidized the poor, so that children in both rich and poor families could receive primary education, a necessary means for their future success, although the policy may impose higher tax rates, which leads to efficiency loss.

Both modern and less developed countries implement some policies carrying out Rawlsian difference principle by giving priority to equality over efficiency (Rawls 1971, Section 13). Note that those institutions implemented the principle to the provision of Rawlsian primary social goods to realize equality of opportunity.

However, previous empirical studies have simply applied the difference principle to final income distributions and it has received little support. Experimentally, the difference principle has been tested behind the VOI using individual decisions between efficient and equal schemes (e.g., Johannesson and Gerdtham 1995; Johansson-Stenman, Carlsson and Daruvala 2002; Carlsson, Gupta and Johansson-Stenman 2003; Bosmans and Schokkaert 2004; Traub *et al*. 2005), or group decisions settled by a random dictator rule (e.g., Becker and Miller 2009; Schildberg-Hörisch 2010), or a majority rule (e.g., Beckman *et al*. 2002), or a unanimity rule (e.g., Frohlich, Oppenheimer and Eavey 1987a, 1987b; Frohlich and Oppenheimer 1990; Lissowski, Tyszka and Okrasa 1991; Bond and Park 1991; Jackson and Hill 1995; de la Cruz-Doña and Martina 2000). The VOI also has been extensively studied theoretically (see, for example, Laruelle and Valenciano 2003; Gajdos and Maurin 2004; Eusepi 2006; Kariv and Zame 2008; Satio 2013). Overall, these VOI empirical studies did not support the difference preference. For example, only 13.7% subjects in Schildberg-Hörisch (2010) and 9% subjects in Traub *et al.* (2005) exhibited behavior following the difference principle.

Non-VOI tests of the difference principle reported similar results. Charness and Rabin (2002) found that total welfare and reciprocity were more salient motives than inequality aversion. Only 20% subjects chose the maximin in Michelbach (2003).

Nevertheless, two non-VOI experimental studies supported the difference principle. Engelmann and Strobel (2004) found that subjects were slightly more likely to choose the maximin than efficiency, and concluded that efficiency, maximin preferences and selfishness combined could rationalize their data. Herne and Suojanen (2004) reported another result supporting the difference principle: 60% subjects followed this principle in the non-VOI treatment with a special design of a very high cost of not reaching a unanimity agreement, while few followed it in the VOI treatment. This suggests the difference principle was only valid in a specific environment other than VOI.

Some philosophers (e.g. Arneson 1999, 2015; Daniels 2003; Lamont 2013) have instead applied the difference principle to Rawlsian primary goods. They argued that the principle aims to attain Rawlsian justice of fair equality of opportunity by maximizing the interests of the least advantaged member, such that she has access to primary goods. Then she and advantaged others share equal chance to realize life goals.

In fact, in line with Daniels (2003), we find that for Rawls, following the difference principle is a rational choice of maximizing long-term expected utility (Rawls, 1974). Rawls (1971) first adopts Aristotle concept of good and defines welfare as achievable life plans (Section 15). He then assumes that people need primary goods to realize those plans (Section 25). Behind the VOI, long-run expected utility is maximized by maximizing the benefits of the least advantaged to ensure her availability of primary goods, because the loss of necessary means for future success is too great (Section 26).

This primary goods reading of Rawlsian difference principle has never been tested. In this paper, we revisit the principle experimentally with a primary goods design. In experiments with great cost of losing primary goods, we found much stronger support—the majority of our subjects acted according to the difference principle. Furthermore, when such cost is lowered, the proportion of subjects following the principle was still above one third, and could increase to 65.79% when a within-subjects design introduces potential order effects.

The rest of the paper is organized as follows. Section 2 presents the primary goods reading of Rawlsian difference principle. Section 3 introduces our experimental design and theoretical predictions. Section 4 reports the results, Section 5 reports additional robustness treatments and Section 6 concludes.

2. Rawlsian difference principle and primary social goods

In line with Rawls (1974) himself and many philosophers' understanding of Rawls (e.g. Arneson 1999, 2015; Daniels 2003; Lamont 2013), we adopt the following reading of Rawlsian difference principle (Rawls 1971): Rawls first discusses concepts of welfare. Specifically, Rawls follows Aristotle's definition of welfare: "*A person's good is determined by what is for him the most rational long-term plan of life given reasonably favorable circumstances.*" (pp. 92-93)

Postulating that people have this concept of welfare, Rawls reasons that they would "*prefer more primary social goods*" (p. 142) and "*they take an interest in primary social good"* (p. 230). Primary social goods are defined as goods that "*normally have a use whatever a person's rational plan of life,*" in which examples are "*rights, liberties, and opportunities, and income and wealth.*" (p. 62)

Rawls then believes that people, behind the VOI, would follow the difference principle to avoid losing primary social goods: "*It is not worthwhile* *for him to take a chance for the sake of a further advantage, especially when it may turn out that he loses much that is important to him.*" (p. 134). This fear of becoming the least advantaged and losing primary social goods could also be explained by loss aversion (Kahneman and Tversky 1979).[2] Because the loss of primary social goods is too huge, everyone behind the VOI, consistent with the expected utility maximization theory, would adopt the difference principle to secure primary social goods. This rational choice does not require infinite risk aversion postulated by his contemporary economists (Alexander 1974; Atkinson and Stiglitz 1980). Therefore Rawls argues that "*the aversion to risk plays no role at all.*"[3]

In fact, Rawls's rationality assumption cannot be clearer as he has a section titled "The Rationality of the Parties" (Section 25), where he defines rationality as the attempt "*to win for themselves the highest index of primary social good, since this enables them to promote their conception of the good most effectively…*" (p. 125).

Based on our understanding of these quotations, instead of requiring

[2] The other two reasons for following the maximin criterion are: First, people would reject utilitarian "*outcomes that one can hardly accept*". (Rawls argues that utilitarianism is unjust since it is willing to maximize total social welfare by sacrificing the welfare of few people.) Second, people may not even know the distribution of their social positions behind the VOI (ambiguity aversion, Ellsberg 1961) (Rawls 1971, p. 154). To test the latter, Gerber, Nicklisch and Voigt (2019) give subjects either full, partial or no information of their social positions (defined by differences in productivity), and find higher demand for redistribution under the no and partial information than under the full information.

[3] See the last paragraph of Section 13 in Rawls (1999, p. 72-73), compared to that in Rawls (1971, p. 83).

particular risk or other-regarding preferences, Rawls suggests that the difference principle chosen behind the VOI is in line with expected utility maximization, assuming that primary social goods are necessary means. As pointed out by Herne and Suojanen (2004), monetary payments in previous experiments are hardly primary social goods having a significant impact on one's future success.[4] Hence, previous studies likely fail to test Rawlsian original thought.

Many philosophers share our above reading of Rawls (e.g. Arneson 1999, 2015; Daniels 2003; Lamont 2013). For instance, Norman Daniels (2003), a Harvard philosopher, also comprehends Rawlsian difference principle as a rational choice. He, like us, first acknowledges that for Rawls, people's "*lifetime prospects*" are "*measured by the index of primary social goods.*" Then the difference principle is a rational choice because one cannot bear the risk of losing primary social goods: "*Rawls claims that the deep conditions of uncertainty in the original positions created by the veil of ignorance, as well as the fact that the stakes are so high, namely our lifetime prospects, mean that the appropriate principle of rational choice for contractors is a "maximin" principle."* (pp. 248-249)

One might wonder how Rawlsian original thought could be a theory of justice if it is only a rational choice without any moral concerns for equality. For Rawls (1971), this rational choice of adopting the difference principle will

[4] Herne and Suojanen (2004) writes: "*…participants' incomes during the rest of their lives do not depend on the principles of distributive justice that they happen to choose in the experiment. Therefore, ending up in the lowest income class is not such a serious outcome to anyone.*" (p. 179)

eventually result in “fair equality of opportunity” (Sections 12-14). He argues that it is unfair and “arbitrary from a moral perspective” (p.64) to determine one’s achievements in accordance with the natural lottery of “family and social class origins”, inborn abilities, chances throughout their lives (1974, p. 674). This injustice is supported by the study of Cappelen *et al.* (2013) where subjects prefer redistribution when income is decided by luck. To combat the injustice of the natural lottery, the difference principle maximizes the benefits of the least advantaged to ensure her availability of primary social goods, leading to fairer equality of opportunity (1971, Sections 12-14).[5]

3. Experimental design and theoretical predictions

Consider the VOI experiment of Schildberg-Hörisch (2010): Each subject must choose one distributional allocation from the 13 possibilities shown in Table 1. After making their decisions, all subjects are randomly divided into groups of two, and roles (either Player 1 or Player 2) are randomly assigned to each group member as “the veil of ignorance” would require. Then, one of the two subjects is chosen as the “random dictator”, whose allocation is implemented.

Under the pure monetary payoff design of Schildberg-Hörisch (2010), Alternative 1 is the most efficient allocation, from which Player 1 receives the largest amount of 240 ESC (Experimental Standard Currency) whereas Player 2 receives nothing. The next distributional allocation, Alternative 2, yielded

[5] When the inequality of opportunity does exist, Krawczyk (2010) find a preference for income transfer from the rich to the poor, since the rich enjoy a higher opportunity to win a prize.

220 for Player 1 and 10 for Player 2, indicating an efficiency loss of 50%, when transferring 20 ESC from Player 1 to Player 2. In fact, every time when we move to the next alternative, Player 1's payoff is reduced by 20 ESC but Player 2 only gains 10 ESC. Put differently, there exists a tradeoff between efficiency and equality in choosing different distributional allocations in Table 1. A subject with maximin preferences should choose Alternative 9. Note that choosing Alternatives 10-13 is not rational, in that it sacrifices efficiency without gaining equality. No subject chose any of these alternatives in our main experiment (as well as in Schildberg-Hörisch, 2010). However, three subjects in one of the Robustness treatments (PG+A) chose one of these, so a total of 0.8% (3/368) of our subjects violate dominance. Thus, we focus on Alternatives 1-9 for the rest of the paper.

Table 1 Possible distributional allocations in the allocation stage

Alternatives	1	2	3	4	5	6	7	8	9	10	11	12	13
Player 1	240	220	200	180	160	140	120	100	80	60	40	20	0
Player 2	0	10	20	30	40	50	60	70	80	90	100	110	120

We follow Schildberg-Hörisch (2010) and adopt the distributional choice of Table 1 in our "allocation stage." Complying with the primary goods reading of the difference principle, we add a "real effort stage" right after the allocation stage. Subjects need necessary means to complete a task in the real effort stage.

In the real effort stage, subjects perform the slider task devised by Gill and Prowse (2012). They earn 30 ESC for each successful task and, within 120 seconds, there are a total of 48 slider tasks to be completed. (None completed all tasks, so there were no censoring issues.) Each slider is initially positioned at 0. As shown in Figure 1, to accomplish a task successfully, subjects have to use the mouse to position the slider exactly at 50, the middle of the line.

There are two different working environments under which subjects could carry out the task. Under the first condition, the current position of a slider is displayed in a numerical scale, so subjects could adjust the slider accordingly (Panel A of Figure 1). The second is the environment without the numerical scale, so subjects could not easily adjust the slider to the correct position 50 (Panel B of Figure 1). (The English translation of the experimental instruction is in Appendix B.) It is obvious that numerical scales are necessary means or primary goods in the real effort stage.

Panel A. Scales displayed   Panel B. No scales displayed

Fig. 1 Two different working environments in the real effort stage

As discussed in Section 2, Rawls argues that one, behind the VOI, would maximize the benefits of the least-advantaged, to avoid that "*he losses much that is important to him*" (Rawls, 19714, p. 134). This implies a great cost for losing primary goods, when one does not adopt the difference principle. We can relate this concept to a child who is too poor to go to school since she has to work to survive. We therefore implement Rawlsian argument by setting up an income threshold in the allocation stage. In the 75-threshold (75) treatment, for a subject to see numerical scales in the real effort stage, her payoffs in the allocation stage must be at least 75. We use a between-subjects design with the imposition of three different thresholds in the allocation stage: 75, 35 and 0.

In the 75 treatment, Alternative 9 (where both players earned 80) is the only distribution that meet the threshold for both players. We use 75 instead of 80 as the threshold so that subjects would not choose Alternative 9 simply because 80 was mentioned so as to become the focal point. Subjects who choose Alternative 9 in this treatment exhibit their preferences for the difference principle in the allocation stage. Consistent with Rawlsian original thought of treating the difference principle as a rational choice, if a subject expects to achieve higher utility with numerical scales than without them in the real effort stage, she should follow the difference principle behind the VOI to secure primary goods, resulting in fair equality of opportunity between two players.

Assuming that a subject is risk neutral, we now derive the theoretical predictions of each treatment. Let $task_{with}$ and $task_{without}$ denote the number of tasks completed with and without numerical scales, respectively. Behind the VOI, the long-term expected value, including payoff of both allocation and real effort stages, from choosing Alternative 1 is:

$$EV_1 = \frac{1}{2} \times (240 + 30 \times \mathrm{task}_{with}) + \frac{1}{2} \times (0 + 30 \times \mathrm{task}_{without}) \qquad \text{(Eq. 1)}$$

Given the same number of task completed by a subject when she becomes Player 1 and Player 2 (task_{with} and $\mathrm{task}_{without}$), EV_1 is greater than EV_2 to

EV_8, since there is efficiency loss in Alternatives 2-8. The expected value of choosing Alternative 9, where both players can see numerical scales, is:

$$EV_9 = \frac{1}{2} \times (80 + 30 \times \text{task}_{with}) + \frac{1}{2} \times (80 + 30 \times \text{task}_{with}) \qquad \text{(Eq. 2)}$$

Risk neutral subjects prefer Alternative 9 to Alternative 1 if $EV_9 > EV_1$, or

$$30 \times (\text{task}_{with} - \text{task}_{without}) > 80 \qquad \text{(Eq. 3)}$$

which states that a subject accomplishes at least 3 more tasks with numerical scales than without them.

The 0-threshold (0) treatment provides a benchmark and serves as a control. When the threshold is 0, choices in the allocation stage has no impact whatsoever on the real effort stage. For a risk neutral subject, Alternative 1 is the optimal choice, since there is efficiency loss in Alternatives 2-9.

In the 35-threshold (35) treatment, Alternatives 5-9 all meet the threshold. Following the same theoretical reasoning in the 75 treatment, a risk neutral subject should choose either Alternative 1 or Alternative 5. The condition to choose Alternative 5 is:

$$30 \times (\text{task}_{with} - \text{task}_{without}) > 40 \qquad \text{(Eq. 4)}$$

which states that a subject accomplishes at least 2 more tasks with numerical scales than without them. A risk averse subject would perform similar reasoning replacing expected payoffs (*EV*) with expected utility (*EU*).

The basic education in India described earlier provides a concrete scenario for our experimental design. First, a positive threshold represents the minimum income level to support a family in India so that parents can send their children to schools. Alternative 1 is the original state, in which only children of the rich family (Player 1) can be educated to pursue their future success, while children with poor parents (Player 2) must help in the fields. In our experiment, the threshold varies from 0, 35 to 75, representing countries with different levels of minimum income. Second, the government can levy tax and redistribute income (choose Alternative 2-9), although this could cause an efficiency loss in the allocation stage due to high income tax rates or bureaucracy. When the threshold is not too extreme, such redistribution policy could allow children from both rich and poor families to receive education, a necessary means to earn future income. In our experiment, 75 is among the highest thresholds such that equality of opportunity is achievable. When the threshold is above 80, no redistribution policy is capable of guaranteeing equality of opportunity.

In order to help subjects understand the impact of numerical scales, we conduct two rounds of the real effort task before the allocation stage—one with numerical scales displayed, and the other without. The order of the two rounds is randomly assigned to each subject. To mitigate wealth effects, subjects are paid only one of the two real effort tasks randomly determined at the end of the experiment. At the beginning of the entire experiment, we conduct the Holt and Laury (2002) binary lottery task to elicit subjects' risk attitudes. This elicitation measures risk attitudes, which would be used in our data analysis, since subjects are behind the VOI. Again, subjects are paid for this task only 50% of the time, randomly determined at the end of the experiment. By contrast, subjects are always paid for the allocation stage and the following real effort task. Figure 2 summarizes the procedure of our experiment.

We conducted 14 sessions of experiments using zTree (Fischbacher 2007) in May and June 2013, each lasting less than an hour. A total of 202 National Taiwan University students were recruited via the Taiwan Social Science Experimental Laboratory (TASSEL) online recruiting website, in which any undergraduate or graduate student of National Taiwan University could register and signup for experiments.[6] In the 0, 35 and 75 treatments, proportions of female participants were 58.57% (41/70), 45.71% (32/70) and 67.21% (41/61). 3 ESC in the experiment is equal to one NT dollar (NT$). The average payment was NT$483 (approximately US$16.32), ranging from NT$147 (US$ 4.97) to NT$925 (US$ 31.30). To compare with the 0 treatment and isolate the effect of risk, we also conducted a risk treatment of 38 subjects in which the other player's payoff was not paid out (all subjects were player 1 playing against the computer as player 2).

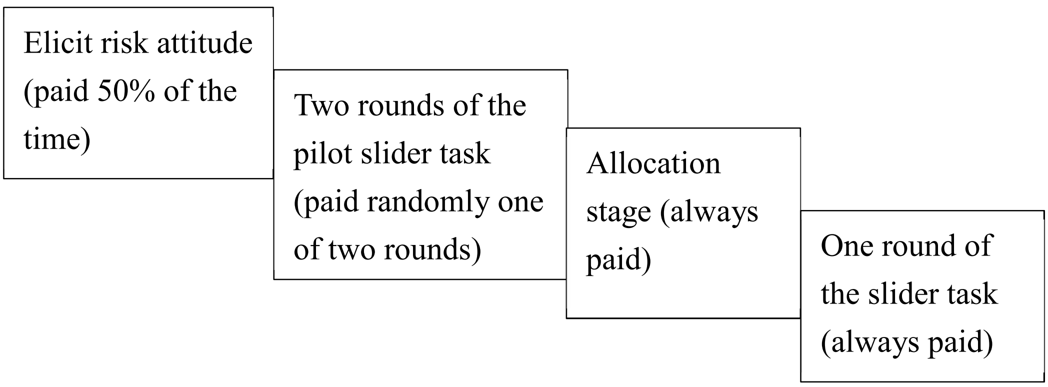

Fig. 2 Experimental procedure

[6] One subject was excluded in our data analysis because the subject accidentally participated twice. We dropped the data for his second attendance in a session of the 75 treatment.

4. Results

We first check whether numerical scales did serve as primary goods. Comparing performance in the two rounds of slider task, we find that subjects, on average, complete 7.35 (4.01) more tasks with the aid of numerical scales, improving from 5.63 (3.24) to 12.70 (3.82) tasks (standard deviations in parentheses), earning 220.5 more ESC. In the 0, 35 and 75 treatments, the average gain of having numerical scales are 7.09, 7.31 and 7.69 more tasks, respectively. Hence, numerical scales are indeed important means for many subjects' future success. Section 4.1 compares the results from the 35 and 75 treatments with those from the control (0) treatment. The econometric analysis of subjects' behaviors is in Section 4.2.

4.1 Treatments against control

Figure 3 plots the frequency distributions of subjects' choices. We start from the control treatment with a 0-threshold. As analyzed in Section 3, a risk neutral subject should choose Alternative 1, deviation could be explained by risk aversion behind the VOI, which would be accounted for in our econometric model. Since all Alternatives could secure primary goods for both players, only 14.29% (10/70) of subjects (who were probably extremely risk or inequality averse) chose Alternative 9, comparable to the result of 13.7% (18/131) in Schildberg-Hörisch (2010).[7]

[7] Schildberg-Hörisch (2010) found that females transferred more to Player 2. As shown in Table A1 in the appendix, we also find similar results in 0 and 35 treatments. However, this pattern disappears in the 75 treatment when the majority adopted the difference principle.

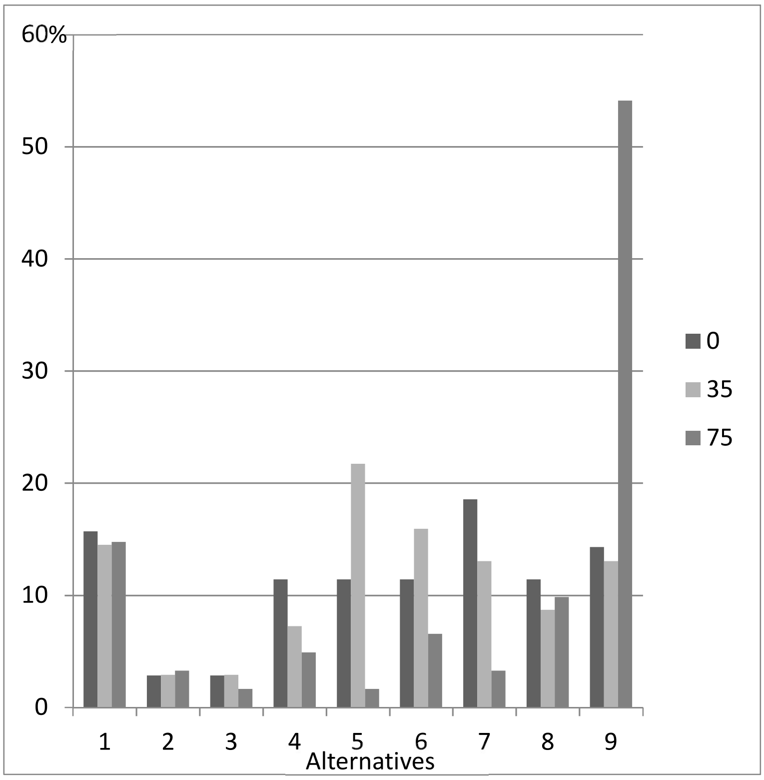

Fig. 3 Choice distributions for the 0, 35, and 75 thresholds treatments

By contrast, the percentage of subjects choosing Alternative 9 jumps to 54.10% (33/61) when the threshold was 75. This percentage is significantly different from that of 0 treatment (z = 4.84, $p < 0.0001$, two-tailed test). The theory predicts that a risk neutral subject would adopt Alternative 9 as long as 3 more tasks are accomplished with numerical scales than without. The majority of our subjects indeed choose Alternative 9.

We next compare the results of the 35 treatment with those of the 0 treatment. Alternative 5 nearly doubles from 11.43% to 21.43% as the threshold level increases from 0 to 35 (z = -1.5966, *p*=0.1104, two-tailed test).[8] The theory predicts that a risk neutral subject should choose Alternative 5 as long as she accomplished 2 more tasks with numerical scales than without.

4.2 Empirical models

We assume that a rational subject should choose an alternative bringing her the highest long-term expected utility. Since a subject' choice is conditional on utility of each alternative, we follow Engelmann and Strobel (2004) and estimate a conditional logit model. To account for uncertainty, we include expected utility, instead of expected payoffs, as explanatory variables.

As Rawls reasons the difference principle with long-term welfare maximization behind the VOI, we assume that subjects maximize constant relative risk aversion utility functions, which is estimated from their risk attitudes and payoffs in both the allocation and real effort stages. We denote

i: index of subjects;

j: Alternatives in the allocation stage with $j \in \{1,2,3,...,9\}$;

The conditional logit model specifies the probability of subject *i* choosing Alternative *j* as $p_{ij} = \dfrac{\exp(EU_{ij})}{\sum_{g\in\{1,\cdots,9\}}\exp(EU_{ig})}$, where EU_{ij} is the expected utility representation of subject *i*'s preference over Alternative *j*:d

$$EU_{ij} = \frac{1}{2}\cdot\frac{\left(P1_{ij} + 30\cdot Task_{P1_{ij}}\right)^{1-r_i}}{1-r_i} + \frac{1}{2}\cdot\frac{\left(P2_{ij} + 30\cdot Task_{P2_{ij}}\right)^{1-r_i}}{1-r_i},$$

[8] One subject in the 35-threshold treatment performed exceptionally well in the real effort tasks even without numerical scales. We suspect that the subject exploited a loophole of the zTree interface and performed the slider tasks with left and right buttons on the keyboard although we physically disabled the buttons with pins. This subject not surprisingly chose Alternative 1.

where r_i is subject i's coefficient of relative risk aversion elicited in the Holt and Laury (2002) task,[9] and $P1_{ij}$ and $P2_{ij}$ are subject i's payoffs in the allocation stage when she turns out to be Player 1 and Player 2, respectively.

$Task_{P1ij}$ and $Task_{P2ij}$ are the numbers of tasks that subject i expects to complete in the third round of slider task when one turns out to be Player 1 and Player 2, respectively. We assume that they are based on results of the two rounds of slider task conducted before the allocation stage, one with numerical scales and the other without. Player 1 could always see numerical scales under Alternatives 1-9, so $Task_{P1ij}$ is the number of tasks completed in the round with numerical scales. This is also true for Player 2 under Alternatives 1-9 in the 0 treatment, Alternatives 5-9 in the 35 treatment, and Alternative 9 in the 75 treatment.[10] In the other situations, $Task_{P2ij}$ is the number of tasks completed in the round without numerical scales. Both values are multiplied by 30 because a subject earned 30 ESC for each successful task. Note that the value

[9] The Holt and Laury (2002) task only provides a range of a subject's value of r. We take the average of the upper bound and the lower bound as each subject's r. However, we only have an upper bound for the most risk averse subjects and a lower bound for most risk loving subjects. We assume the difference of r between the highest and second highest risk averse level is equal to the difference between the second highest and third highest risk averse level. r of the most risk loving subjects is derived in a similar fashion.

[10] This explicitly assumes no learning effect. Gill and Prowse (2011) reported that subjects, on average, completed the fewest tasks (22.034) in round 1, and completed the most tasks (26.831) in round 8. The average increase per round was 0.6852. This increment is minor, compared to the 7.35 more tasks completed with the aid of numerical scales in our experiment. Hence, learning effect should not substantially change our results.

of EU becomes very large when r_i is close to 1. To avoid this problem, we normalize each subject's EU to the range of [0, 1].[11]

Table 2 Estimated odds ratio (conditional logit model)

	All Treatments		75 Treatment		35 Treatment		0 Treatment	
EU	2.56***	2.22***	9.07***	1.57	2.46**	2.48**	0.61	0.70
	(4.42)	(3.96)	(6.34)	(0.91)	(2.82)	(2.82)	(-1.26)	(-0.75)
Maximin		2.39***		6.25***		0.94		1.20
		(4.89)		(4.33)		(-0.16)		(0.41)
Log-L	-392.09	-381.87	-94.07	-86.91	-145.07	-145.06	-139.83	-139.75
Obs	1647	1647	459	459	612	612	576	576

Z-statistics in parentheses. ***, **, * denote significance at the 0.1, 1 and 5 percent level, respectively.

Note that this empirical model estimates the impact of expected utility on choices for an average subject. Although heterogeneity over individuals and options is all incorporated in the error term, the within-subject effect approximated by this model is unbiased.

We first discuss results of Rawlsian EU model including samples of all three treatments. As shown in Table 2, 2.56 reported in the row of EU is the odds ratio of choosing Alternative j, $p_{ij}/(1-p_{ij})$. Hence, an odds ratio greater than 1 means that the alternatives with higher expected utility are more likely to be chosen. Similar results of the 75 and 35 treatments suggest that

[11] Specifically, each subject i's expected utility for Alternative j is scaled by the following formula: $\frac{EU_{ij}-EU_{i,min}}{EU_{i,max}-EU_{i,min}}$, where $EU_{i,min}$ and $EU_{i,max}$ are the minimum and maximum values of subject i's expected utility among Alternatives 1-9.

long-term expected utility maximization can explain subjects' behaviors behind the VOI, when they need primary goods. Rawls's argument that following the difference principle is a rational choice is supported by the econometric analysis. However, incorporating risk attitudes is insufficient to account for subject behavior in the 0 treatment.

The *EU* model explains behaviors in the 75 treatment quite well, in which the majority choose Alternative 9. This demonstrates that, when we follow the Rawlsian thought to impose a great cost for losing primary goods, adopting the difference principle need not require infinite risk aversion. In fact, only 3.92% (2/51) subjects exhibit extreme risk aversion and do not bear any risk in the Holt-Laury binary lottery task. Furthermore, subjects adopting the difference principle consist of those with all sorts of risk attitudes, ranging from infinitely risk averse, risk neutral to risk loving.

As shown in the last column of Table 2, long-term expected utility is not significant in the 0 treatment. One possibility is that subjects are maximizing expected utility in the allocation stage $\left(EU_{ij} = \frac{1}{2} \cdot \frac{P1_{ij}^{1-r_i}}{1-r_i} + \frac{1}{2} \cdot \frac{P2_{ij}^{1-r_i}}{1-r_i} \right)$ only, since all alternatives secured primary goods for both players. Nevertheless, including it does not result in a significant coefficient. Another explanation is that decisions are affected by social preferences. The social preferences for equality behind the VOI have been shown by Schildberg-Hörisch (2010), who conducted an additional risk treatment, in which a subject made a decision behind the VOI without being coupled to another player. She found that female subjects choose the more equal distribution behind the VOI.

Therefore, we include the social preference *Maximin*, the dummy variable for Alternative 9, in conditional logit models. As shown in Table 2, it is a significant variable explaining data from all treatments, mainly due to its effect in the 75 treatment where *EU* becomes insignificant. However, choosing Alternative 9 in the 75 treatment could be driven by either utility maximization or *Maximin* preference, since among those selecting Alternative 9, two thirds (22/33) of them were also maximizing their *EU*. By contrast, none of those picking Alternative 9 in the 35 treatment were maximizing *EU*. Subjects having the maximin preference would choose Alternative 9, while *EU* maximizers would select other alternatives. The result suggests that Rawlsian long-run utility maximization model accounts for the 35 treatment data, as *EU* is a significant independent variable, while *Maximin* is not.

In addition, we also conducted an additional risk treatment as in Schildberg-Hörisch (2010) to compare with the 0 (VOI) treatment. The Epps-Singleton test cannot reject the null that the two distributions are identical (Wald Chi squared = 5.627, p = 0.2288, two-tailed test). Table 3 reports least-squares regression results similar to Schildberg-Hörisch (2010), with the amount transferred from player 1 to player 2 as the dependent variable, and the treatment dummy (*VOI)*, gender, and risk preference as explanatory variables. The coefficients of *VOI* and *Female×VOI* are not significant, so social preference seems to play a minor role in the 0 treatment.

Table 3 OLS results for the 0 (VOI) and risk treatments

Treatment	0 (VOI) and risk	
Female	22.0344* (2.09)	9.6576 (0.55)
VOI	16.1086 (1.49)	-14.5624 (-0.40)
Female×VOI		19.3010 (0.88)
Risk coefficient	28.0988* (2.47)	26.7842* (2.33)
Constant	24.5078* (1.29)	45.0217 (1.49)
Obs	108	108
Adjusted R^2	0.1071	0.1051

t-statistics in parentheses. ***, **, * denote significance at the 0.1, 1 and 5 percent level, respectively.

5. Robustness treatment

5.1 Error margin treatments

In the above experiments, we follow Rawlsian original thought by implementing a serious consequence of losing primary goods. However, there could be other causes for following the difference principle under the threshold design. For instance, people could exhibit maximin preferences if they are paid nothing when the threshold is not satisfied, which is unrelated to primary goods. Also, in the 35 and 75 treatments, subjects are motivated by both securing primary goods and earning monetary payoffs of the allocation stage.

Therefore, we conduct a new set of treatments with the “error margin” as primary goods to avoid thresholds and focus on the tradeoff between efficiency and equality among various levels of primary goods. Specifically, subjects do not see any numerical scales but are allowed a margin of error near the target 50. As shown in Table 4, possible error margins range from 0 to 6. When a 0 error margin is allowed, subjects must adjust the slider to exactly 50. For error margins of 1, 2, 3, 4, 5 and 6, subjects only have to position the slider within ranges of [50, 51], [49, 51], [49, 52], [48, 52], [48, 53] and [47, 53], respectively. Hence, it is easier for a subject to complete a slider task with greater error margins, making them primary goods in the real effort stage.

Alternative 1 in Table 4 is the most “efficient” distribution of error margins that maximizes the sum of two players’ error margins. When Player 1’s error margin falls by 2 in Alternatives 5 and 9, Player 2’s error margin only increases by 1. Note that the efficiency loss of primary goods here is 50%, the same as that of monetary payoffs in previous treatments.

Table 4 Possible set of error margins in the allocation stage

Alternatives	1	2	3	4	5	6	7	8	9	10	11	12	13
P1’s monetary payoffs	240	220	200	180	160	140	120	100	80	60	40	20	0
P1’s error margins	6	5	5	4	4	3	3	2	2	1	1	0	0
P2’s monetary payoffs	0	10	20	30	40	50	60	70	80	90	100	110	120
P2’s error margins	0	0	0	0	1	1	1	1	2	2	2	2	3

In order to let subjects correctly perceive the benefit of having a greater margin of error, subjects first perform seven rounds of slider task for each level of error margin before the allocation stage.[12] To incentivize them, one random round is paid at the end of the experiment. Since Gill and Prowse (2012) found subjects reducing their effort after several rounds, we set the maximum error margin at 6 to avoid fatigue. Moreover, we shorten each round from two minutes to one minute, but increase the payoff for each successful task from 30 ESC to 60 ESC.

When allocation payoffs affect choices in primary goods experiments, treatments that separate the effect of primary goods from allocation payoffs are more desirable. Hence, in addition to a combined treatment (PG+A) where the allocation stage is paid out and affects the distribution of primary goods, we consider a primary goods (PG) treatment, in which the allocation stage is not paid out. We also consider an allocation (A) treatment as a control, in which one's decision only affects monetary payoffs in the allocation stage and everyone receives the same fixed amount of primary goods, namely an error margin of two.

We conducted the first set of experiments in February 2018 where 50 subjects faced the primary goods and combined treatments simultaneously. After both decisions, the computer randomly drew one of the two scenarios to implement, making the treatment effect within-subjects. These are the PG-sim

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[12] To counter-balance possible learning effects, error margins from the first to the seventh round are: 1, 3, 5, 6, 4, 2 and 0.

and PG+A treatments. We conducted the second set of between-subjects experiments for primary goods (PG) and allocation (A) treatments during March to May 2019. 40 subjects faced the PG treatment and 38 subjects faced the A treatment.[13] A total of 128 National Taiwan University students were recruited. The average payment was NT$751 (approximately US$24.23), ranging from NT$183 (US$5.9) to NT$1640 (US$52.9).

5.2 Results of the error margin treatments

Figure 4 shows results of the first set of experiments. When the allocation payoffs are paid out, fewer subjects adopt the difference principle: The percentage of subjects choosing Alternative 9 drops significantly from 30% (15/50) to 14% (7/50), according to a proportion test ($z = 1.93$, $p = 0.0268$, one-tailed test). The allocation payoffs clearly have an impact on decisions.

[13] To maintain the 50% chance of implementation, after the PG and A treatments, subject also made a second decision. If she faced the PG treatment first, she would make the second decision under the A treatment, and vice versa. However, in the first decision subjects were only told their choices would be implemented with a 50% chance and the instructions for the second decision were not revealed. Hence, the first decision was not affect by the second, while the second might be prone to order effects. We refer to the second decision as the PG-after and A-after treatments and report their results in footnote 14.

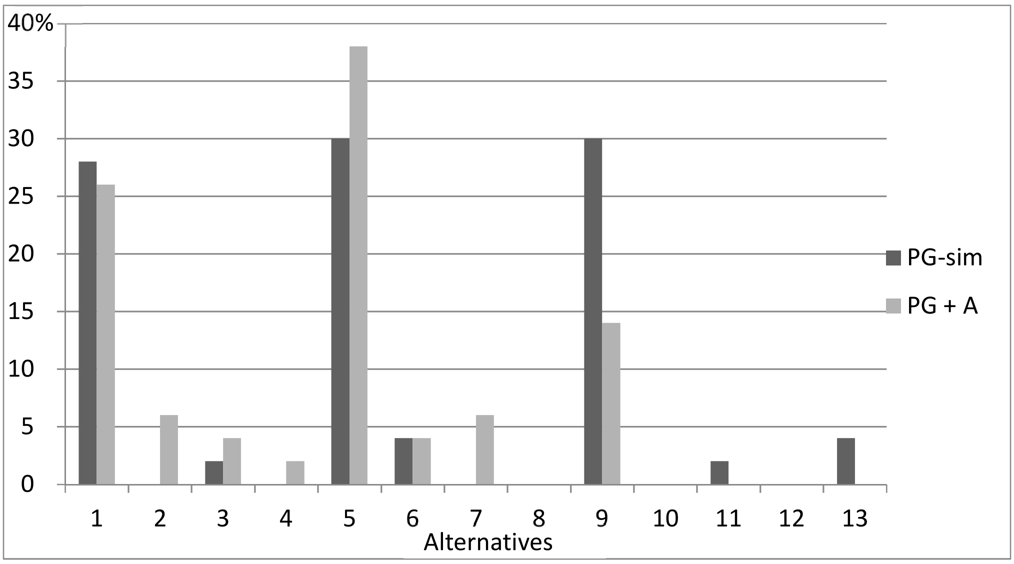

Fig. 4 Choice distributions of PG-sim and PG+A treatments

Figure 5 shows results of the second set of experiments. In the PG treatment, 35% (14/40) choose Alternative 9, which is the mode. The proportion is higher than that of the A treatment (21.05% or 8/38), but the difference is only marginally significant ($z = 1.37$, $p = 0.0856$, one-tailed test). This suggests that more subjects adopt the difference principle when allocating primary goods.[14]

[14] Results of the PG-after treatment are quite different from those of the PG treatment. (Figure A1 in Appendix A reports the choice distributions of PG-after and A-after treatments.) In particular, 65.79% subjects adopt the difference principle, significantly higher than 35% in the PG treatment ($z = 2.7184$, $p = 0.0066$, two-tailed test). The Epps-Singleton test also suggests that the two distributions are not identical (Wald Chi squared = 13.262, $p = 0.01$, two-tailed test). In contrast, Results of the A-after treatment are not significantly different from that of the A treatment (Wald Chi squared = 1.959, $p = 0.74$, two-tailed test).

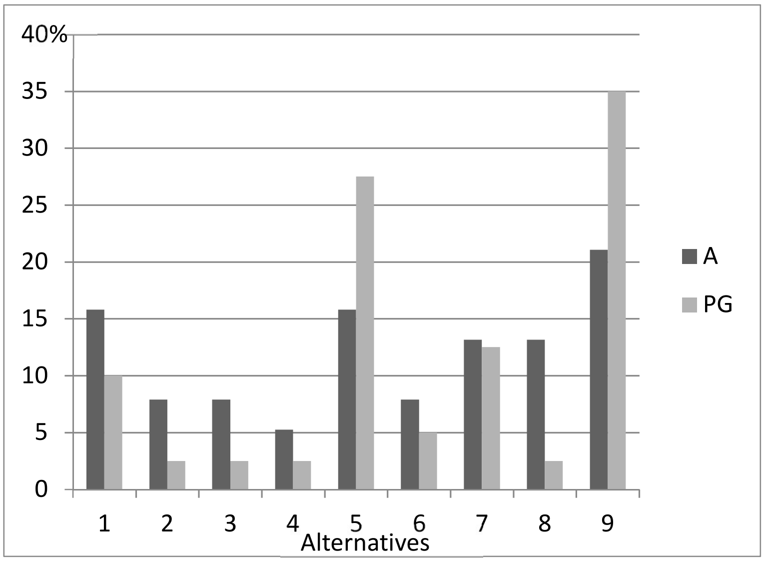

Fig. 5 Choice distributions of PG and A treatments

Using the same conditional logit model approach in Section 4.2, we analyze choices from all the robustness treatments and report the results in Table 5.[15] The dummy "*Sim*" denotes the PG-sim treatment. Results indicate that expected utility maximization alone cannot explain subjects' choices, except in the PG+A treatment. This suggests that many subjects, when adopting the difference principle, are not solely motivated by utility maximization. Indeed, Table 6 shows that, among those selecting Alternative 9 in the PG & PG-sim treatments, 43% (9/21) of them would receive higher expected utility from other alternatives, while all 8 subjects choosing Alternative 9 in the A treatment were not maximizing their *EU*. Hence, we need other motives to explain adherence to the difference principle, such as social preferences. In fact, *Maximin*, the

[15] We omitted one risk-loving subject who earned 0 when error margin was 0 (since a payoff of 0 in the denominator yields infinite utility). We also omitted three subjects who chose dominated options (Allocations 10-13) to conduct the same analysis only on Allocation 1-9.

dummy variable for Alternative 9, is a significant explanatory variable except in the PG+A treatment.

Table 5 Estimated odds ratio for Robustness treatments

Treatment	PG & PG-sim		A		PG+A	
EU	2.4368 (1.82)	1.3149 (0.53)	1.4187 (0.63)	2.4447 (1.40)	14.2031*** (5.38)	10.0473*** (4.27)
EU × *Sim*	1.2979 (0.40)	2.8438 (1.45)				
Maximin		4.1442*** (3.91)		2.9920* (2.40)		1.7087 (1.35)
Maximin × *Sim*		0.1665*** (-2.95)				
Log-L	-188.2135	-181.2211	-81.0934	-78.5510	-85.0027	-84.1539
Obs	792	792	333	333	414	414

Z-statistics in parentheses. ***, **, * denote significance at the 0.1, 1 and 5 percent level, respectively.

The non-utilitarian motivation for adopting the difference principle in the PG and PG-sim treatments are quite different from those in the threshold treatments, likely because there is no great loss from deviating from the difference principle. Actually, following the difference principle may not be particularly beneficial in these treatments, since, on average, a subject expected to complete 12.06 and 11.61 tasks under Alternatives 1 and 9, respectively.[16]

[16] The average number of tasks completed by a subject with marginal errors 0, 2 and 6 are 2.76, 11.61 and 21.36, respectively, and (2.76+21.36)/2 = 12.06.

Table 6. Expected utility maximization for those choosing Alternative 9

Treatment	PG & PG-sim	A
Alternative 9 yields the highest *EU*	12	0
Other alternatives yield the highest *EU*	9	8

Contrary to the results of PG, PG-sim and A treatments, the social preference *Maximin* becomes an insignificant variable and *EU* can rationale behaviors in the PG+A treatment. It is not clear why a treatment combining primary goods and allocation payoffs could produce outcomes very different from pure primary goods or allocation treatment. The mixed effect of social preference and utility maximization under different settings requires further research to investigate behaviors in the presence of primary goods.

To sum up, threshold and error margin experiments yield two main results:

1. When we follow Rawls by implementing a severe consequence of losing primary goods from not adopting the difference principle in the 75 treatment, 54.1% subjects act according to the principle. This is significantly higher than the 13.7% reported by Schildberg-Hörisch (2010). In the 35 treatment, where we can distinguish between utility maximization and maximin preferences, Rawlsian long-term expected utility maximization account for behaviors behind the VOI.
2. Even when the primary goods design involves less severe consequences when the difference principle is not followed, we still find 35% of our

subjects adopting the difference principle for pure primary goods,[17] and less subjects act according to it when allocating monetary payoffs. Moreover, expected utility maximization alone could not rationalize subject behavior, suggesting that other motives are in play, such as social preferences.

6. Discussion and conclusion

Since Rawls published his influential book, *A Theory of Justice*, in 1971, it has become one of the most popular research topics. Searching "John Rawls" in the google scholar finds it being cited 137,662 times.[18]

The main contribution of our paper is bridging the gap between theory and evidence of Rawlsian difference principle. Previous empirical studies have almost exclusively applied the principle to final income distributions. Schildberg-Hörisch (2010) found only a minority choosing it, and Engelmann and Strobel (2004) reported that efficiency, maximin preferences and selfishness were all significant motives. Following Rawls by imposing a huge cost from losing primary goods, we find significantly more subjects adopting the difference principle.

One may want to compare our 0 treatment results with previous studies, other than that of Schildberg-Hörisch (2010). This comparison should be conducted with caution. For instance, Engelmann and Strobel (2004) found

[17] When subjects also make decisions in the PG treatment, either after the A treatment or simultaneously, results vary from 65.79% (PG-after) to 14% (PG-sim).

[18] The google scholar search was conducted on the 7th, November, 2016.

that 40% subjects chose maximin in some non-VOI treatments (when 41% choose efficiency), but comparison is difficult since their design does not involve uncertainty. Charness and Rabin (2002) introduced reciprocity in their experiments, which makes comparisons even more difficult.

When we implement a huge cost for losing primary goods, the majority of subjects choose according to the difference principle in experiments. Hence, facing great costs of losing it, people, behind the VOI, are willing to maximize the benefit of the least advantaged member to ensure the availability of her primary goods. In fact, they sacrifice a lot to carry out Rawlsian justice of fair equality of opportunity.

When losing primary goods is not catastrophic, more than one third of the subjects still follow the difference principle, and fewer subjects act according it when allocating monetary payoffs. Interestingly, the Rawlsian model of expected utility maximization is insufficient to explain the data. Hence, preference for the difference principle in the primary goods treatment has to be explained by other factors, such as social preferences. Therefore, there are also non-utilitarian effects of primary goods, not covered by Rawls's idea of maximizing long-run expected utility. However, the effect of the *Maximin* social preference disappeared in a treatment combining both primary goods and allocating monetary payoffs. Hence, we found an ambiguous result across treatments. In addition, when subjects face the same decision after allocating only monetary payoffs, the majority adhere to the difference principle, exhibiting an order effect. These findings await future investigation.

APPENDIX A: ADDITIONAL FIGURES AND TABLES

Table A1. **Comparing Our Results with Schildberg-Hörisch (2010)**

	Treatment					
	0		75		35	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Female	33.032*	0.014	-1.315	0.943	28.154*	0.015
Risk Coefficient	29.860*	0.018	8.445	0.133	11.148**	0.005
Constant	58.971***	<0.001	56.910	0.147	5.244	0.830
Obs	64		51			68
Adjusted R^2	0.195		0.0068			0.160

***, **, * denote significance at 0.1, 1, and 5 percent level, respectively.

To compare our results with those in Schildberg-Hörisch (2010), we conduct OLS regressions shown in Table A1. The dependent variable, as in Schildberg-Hörisch's model, is the amount transferred from Player 1 to Player 2. We introduce two explanatory variables. The first variable is the dummy variable for gender: the value is 1 for female and 0 for male. The second one is subject's coefficient of relative risk aversion elicited in accordance with the experiment of Holt and Laury (2002).

The results are similar to the results of Schildberg-Hörisch: We also find that female subjects transfer more to Player 2 in the 0 and 35 treatments.

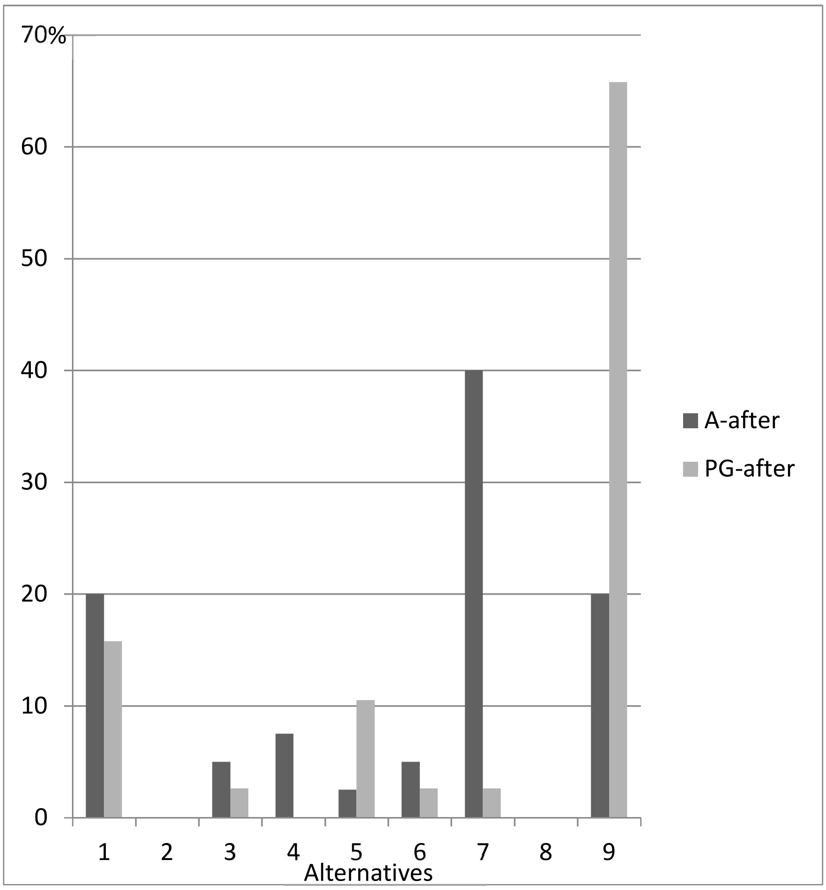

Fig. A1 Choice distributions of PG-after and A-after treatments

APPENDIX B

English Translation of Experiment Instruction

Payoff from the Experiment

At the end of this experiment, a show-up fee of NT$ 100 and the NT$ you have earned during the course of the experiment will be paid. During the experiment, the earnings are denominated as ESC (Experimental Standard Currency). Your ESC earnings in the experiment are dependent on your decisions, your effort, others' decisions, and some random processes. Each of you will be paid privately. You are not obligated to tell others your payoff. Note that in this experiment, the ESC/NT$ exchange rate is: 3 ESC = NT$ 1

Procedures of the Experiment

There are three rounds slider tasks in this experiment. In each round, you have 120 seconds to conduct 48 slider tasks at most. From each successful task, you earn 30 ESC. As shown in Figure B. 1(a), a single slider is composed with a line and a positional point, whose location is displayed by the 'numerical scale' to the right of the slider. The positional point is originally located at the farthest left of the line, with a 0 numerical scale. The point can be adjusted along the line an unlimited number of times. When it is located at the farthest right of line, the numerical scale shows that its location is 100. Your task, as shown in Figure B. 1(b), is to use the mouse to adjust the positional point to the location of 50. As shown in Figure B. 2, there are 48 slider tasks in each round. The remaining time is shown at the top-right corner of the screen. Below it, there is a banner showing how many tasks you have successfully completed so far.

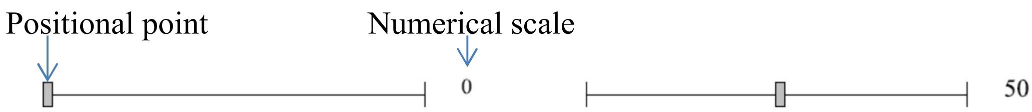

FIGURE B. 1(a) FIGURE B. 1(b)

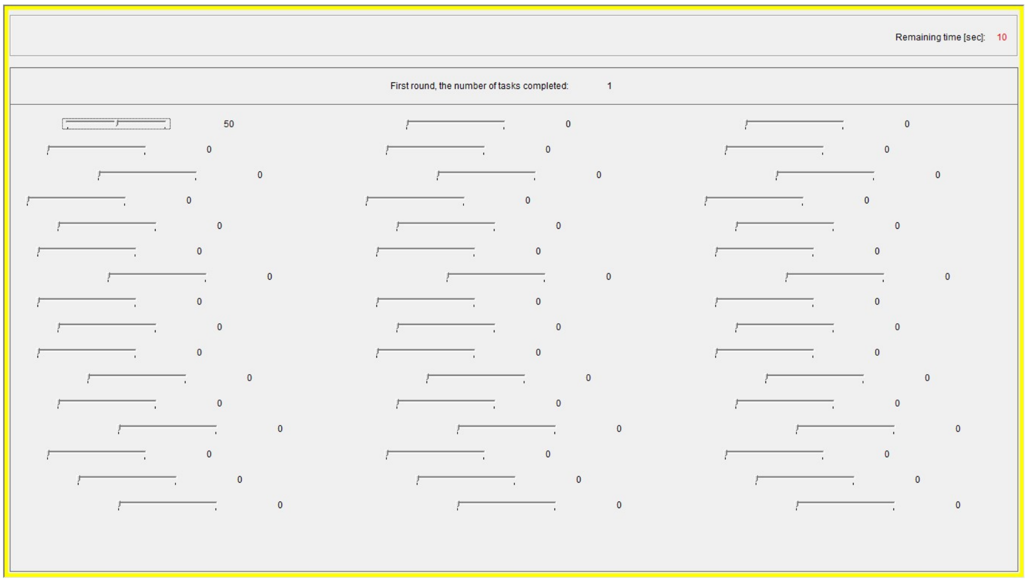

FIGURE B. 2

In each round, when you conduct slider tasks, you are not guaranteed to see the numerical scale and how many tasks you have completed. (The two information are called ‘adjustment information’ thereafter.) Whether you can see adjustment information is dependent on the following specified conditions:

The first two rounds slider tasks: The computer will randomly choose one round where adjustment information is visible, while in the other round the information is invisible.

The third round slider tasks: Before the third round slider task, there is an ‘allocation stage’. **In order to see adjustment information in the third round, your payoff in the allocation stage must be above or equal to 0 ESC.** (For 35- and 75-threshold treatments, 0 ESC is replaced by 35 ESC and 75 ESC, respectively.)

In the allocation stage, the computer will randomly divide all subjects into groups of two. Each group includes Player 1 and Player 2. There are 13 possible payoffs for Player 1 and Player 2, as shown by the 13 distributions in Table B. 2. In Distribution 1, Player 1 receives 240 ESC, and Player 2 receives 0 ESC. Every time when you move towards right to the next distribution, Player 1’s payoffs will reduce by 20, but player 2’s payoffs will only increase by 10. Therefore, the total payoffs of Player 1 and Player 2 will reduce by 10.

Table B. 2 Possible 13 distributions in the allocation stage

Distributions	1	2	3	4	5	6	7	8	9	10	11	12	13
Player 1	240	220	200	180	160	140	120	100	80	60	40	20	0
Player 2	0	10	20	30	40	50	60	70	80	90	100	110	120

The step by step procedure of the allocation stage is illustrated as follows:

Step 1: The computer will randomly choose another subject as ‘the other participant’ in your group. During and after this experiment, no one is able to know whom the other participant in her/his group is. You have to choose one distribution from the 13 possible distributions. **Note that when you choose, you do not know if you will become Player 1 or Player 2.**

Step 2: **The computer will randomly arrange you as Player 1 or Player 2.** If you are Player 1, the other participant is Player 2. If you are Player 2, the other participant is Player 1.

Step 3: The computer will randomly choose one from your group as 'the decisive person', and her/his chosen distribution will determine your payoffs in the allocation stage. Note that you and the other participant are both likely to be chosen by the random process to determine your payoffs in the allocation stage. So, please make your decision carefully, just as your decision is going to be realized.

For example, under the circumstance without knowing whom will be Player 1 or Player 2, you choose Distribution 1 (Player 1 will receive 240; Player 2 will receive 0), and the other participant chooses Distribution 2 (Player 1 will receive 220; Player 2 will receive 10). Next, the computer randomly arranges you as Player 1, and the other participant as Player 2. Finally, the computer randomly chooses one from you two as the 'decisive person', and you are chosen. Therefore, your chosen Distribution 1 will determine your payoffs of the allocation stage. You receive 240 and the other participant receives 0. Thus, in the third round slider tasks, both you and the other participant will be able to see 'adjustment information'. (In the 35- and 75-threshold treatments, the last sentence is replaced by: Thus, in the third round slider tasks, you will be able to see 'adjustment information', and the other participant will not able to see it.)

At the end of the third round slider tasks, the payoffs in the allocation stage and the third round slider tasks will be definitely realized. Besides, the computer will randomly choose one of the first two rounds slider tasks. And the payoffs of the chosen round will be realized. Note that any of the first two rounds is likely to be chosen by the random process to determine your payoffs in this experiment. So, please conduct each round slider tasks as that round is going to be chosen!

The total ESC payoffs in this part of experiment is:

Payoffs in the allocation stage + payoffs in the third round slider tasks + payoffs in one of the first two rounds slider tasks

If you have any questions, please raise your hand. We will come to you to explain to you.

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