Other-regarding Preferences and Other-regarding Cheating

– Experimental Evidence from China, Italy, Japan and the Netherlands

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Abstract

This study examines whether other-regarding preferences (ORPs) can predict cheating for different beneficiaries: cheating for-self, and other-regarding cheating for an in-group or an out-group member. The results show that, on the one hand, more prosocial subjects cheat less for self compared to more proself subjects. On the other hand, they cheat more for others. Moreover, the extent of cheating varies across the countries sampled in the study. The four countries were chosen based on the dimensions of individualism and the perceived level of corruption. While China and Japan are more collectivistic than Italy and the Netherlands (Hofstede *et al*., 2010), China and Italy are ranked as more corrupt than Japan and the Netherlands according to the Corruption Perception Index (Transparency International, 2010). The extent of cheating is found to vary by the dimension of the perceived level of corruption, and not by the cultural dimension of individualism. Compared to subjects from Japan and the Netherlands, the two countries that are ranked as less corrupt, subjects from China and Italy not only cheat more for self, but also cheat more for others.

Keywords: other-regarding preferences; prosociality; rule-breaking; self-regarding cheating; other-regarding cheating; cross-cultural

JEL codes: C91, D63, H26, K42

1. Introduction

Cheating is commonly defined as a dishonest act to gain an advantage. Loosely speaking, cheating for monetary gain can constitute an act of *embezzlement* - the act of misappropriating money.[1] Both cheating and embezzlement involve the act of claiming unentitled benefits by breaking some explicit or implicit rules of what is permissible. The question posed in this paper is whether other-regarding preferences (ORPs), i.e., how much one cares about self vs. different others, can predict individual propensity to cheat, i.e., to break the designated experimental rules in the lab to misappropriate money, for themselves or others. I focus on the case in which there is no risk of being caught or any punishment associated with cheating.

Before considering the disutility of rule-breaking, the link between ORPs and cheating propensity is intuitive. The more weight one puts on his or her own monetary interests in his utility function (i.e., the more proself one is), the more one may be motivated to cheat to benefit self. Likewise, the more weight one puts on others' monetary interests (i.e., the more prosocial one is), the more one may be motivated to cheat to benefit others. This prediction, however, hinges on the assumption that the disutility of rule-breaking is independent of ORPs, which is not evident.

Rule-breaking, in real life, is rarely a *Pareto* improvement as it often hurts the interests of innocent others, either in a salient or non-salient way. Many rules are intended to be (or perceived to be) protecting the public interests or interests of some innocent others. Assuming that the harm of rule-breaking on others is salient,[2] then the more prosocial one is, the more disutility one would derive from rule-breaking, and consequently the less one is willing to break rules. In this case, when rule-breaking benefits others instead of oneself, more prosocial subjects, compared to less prosocial subjects, may face a conflict between two virtues: benefiting a salient counterpart, or following rules in order not to hurt a non-salient innocent other(s). Thus, I hypothesize that more prosocial subjects would be less prone to break rules for personal gain, but not necessarily less prone to break rules to benefit others. It remains to

[1] Embezzlement is defined as the crime of stealing the funds or property of an employer, company, or government or misappropriating money or assets held in trust (*West's Encyclopedia of American Law, edition 2, 2008*).

[2] Even when there is no salient harm of rule-breaking, since rules in real life may be followed without individual discrepancy, more prosocial subjects might have formed a stable propensity of not engaging in rule-breaking and act consistently even when there is no salient associated harm.

be seen how a prosocial subject would resolve the dilemma of helping a salient counterpart and resisting rule-breaking.

Besides the overall prosociality of how much one cares about others' material payoffs, I argue that how much one weighs the material payoffs of *different* others can matter for cheating propensity. An individual who is highly prosocial towards an ingroup member might be highly anti-social towards an outgroup member.[3] The combination of strong ingroup ties and weak outgroup ties has long been argued to be linked to more corruption in the sociology and political science literature. Edward Banfield, in *The Moral Basis of a Backward Society* (1958) explored the link between what he called "amoral familism"[4] and corruption, employing observational and survey data. Similarly, Fukuyama stated that pervasive public corruption is likely to result from the combination of stronger bonds of trust and reciprocity within the family and weaker trust outside the family (2000, p. 99). Such combination implies that the private obligations to help family members may be behaviorally more binding than the public obligations to follow formal rules made by untrusted parties outside the family. Yamagishi, in his book *The Structure of Trust* (1998), made a similar argument, but he shifted the focus from purely family ties to ingroup ties, which I adopt in this paper. The concepts of ties and bonds discussed in this strand of literature are broader than those of OPRs, since ties and bonds include trust and reciprocity that are also related to cultural values and norms. Here, I focus on an individual's particularistic ORP defined as how much one differentiates the respective weight of ingroup and outgroup members' payoff in the utility function, i.e., the relative difference between ingroup- and outgroup-prosociality. I hypothesize that particularism is positively linked to cheating for self, controlling for one's general prosociality.

I simulate embezzlement with a cheating game – the Mind Game - as in Jiang (2013) in a controlled lab environment: subjects are asked, first to pick mentally which face of a six-sided die would determine their payoffs, the side facing up or the side that facing down. Since they are asked to self-report the side after they see the die outcome, they can break the rule of the game and lie about the side originally chosen to maximize their earnings. Misreporting the side chosen mentally in order to earn more money simulates embezzlement as it is about misappropriation of monetary gain that one is not entitled to.

[3] On the evolution of out-group hostility, see Choi and Bowles, 2007; Bowles and Gintis, 2004.

[4] "…the inability of the villagers to act together for their common good or, indeed, for any end transcending the immediate, material interest of the nuclear family." (Banfield, 1958). Similar concepts also include particularized trust (Uslaner, 2002) and limited morality (Tabellini, 2008).

I collect data from four countries that differ by the dimensions of individualism and perceived level of corruption: China, Japan, Italy and the Netherlands. While China and Japan are more collectivistic than Italy and the Netherlands (Hofstede *et al*., 2010), China and Italy are ranked as more corrupt according to the Corruption Perception Index than the other two (Transparency International, 2010). The site differences in ORPs and cheating will be discussed in the additional analyses. I examine whether there are systematic cross-country differences, and whether these differences remain controlling for individual ORPs. Moreover, I explore if the cheating ranking of these four sites is consistent when cheating for different beneficiaries.

The results show that more prosocial subjects cheat less for self, but they also cheat more for others compared to more proself subjects. The extent of cheating varies significantly across countries. Moreover, it varies by the dimension of the perceived level of corruption and not by the cultural dimension of individualism. Interestingly, subjects from countries that are perceived to be more corrupt (China and Italy) do not just cheat more for self, they actually cheat relatively more for others compared to subjects from countries perceived to be less corrupt (Japan and the Netherlands), even controlling for the general propensity to cheat.

2. Methods

2.1 Experimental Design

The experiment consists of three parts: "Group Formation", "ORP Elicitation" and "Cheating Games".

(a) Group Formation

To capture particularistic preference, group identity needs to be incorporated, using either natural groups or artificial groups created in the lab. A natural ingroup can be a tribe, a village community or the army (see e.g., Bernhard, Fehr, & Fischbacher, 2006; Goette, Huffman, & Meier, 2012). Artificial ingroups, on the other hand, can be effectively formed by methods like "minimal group paradigm" that is as minimal as randomly categorizing people as a group (Tajfel *et al.,* 1971, see Yamagishi, Jin & Kiyonari, 1999 for an overview). While experiments with natural groups have the obvious advantage of staying closer to the real world, working with artificial groups can prevent decisions made in the lab being influenced by the extended future outside the lab and give the experimenter more control over the composition (size and membership of the various groups) across contexts and cultural groups (see Ben-Ner *et al*., 2009 for how ingroup favoritism differs in different contexts), and hence used in this study.

One main caveat of using the artificial group formation procedure is that it might not capture the true particularistic preference, even though it captures some baseline ingroup favoring sentiment. As Triandis *et al.* (1988) pointed out, people in individualistic cultures are better at forming new ingroups and getting along with new acquaintances whereas people in collectivist cultures have only a few ingroups which stay the same over time and they are not keen on forming new ingroups. Accordingly, the method of forming artificial groups is imperfect in the sense that individuals who are more universalistic in real life can be more successful in forming new ties with strangers in the lab and show stronger ingroup bias, compared to those who are more particularistic in real life. In that case, universalistic subjects may act particularistic and *vice versa,* in which case I will find the opposite result as hypothesized. In the worst case, however, employing the minimal group paradigm might not just result in type reversal, but that ingroup bias might fail to emerge at all among subjects from collectivistic cultures. For instance, Buchan, Johnson and Croson (2006) found that subjects from Asian countries (including Japanese, Chinese and Korean), other than subjects from the US, do not exhibit significant ingroup bias with artificial ingroups, even with communication that strengthens the overall ingroup bias among US subjects.

To alleviate the risk that keeping the group excessively minimal would lead to little ingroup bias, I construct the "group name task" which extends the minimal group paradigm devised by Tajfel *et al.* (1971). After subjects are randomly assigned into different groups, they are also given five minutes to discuss online and decide upon a name for the group that all group members like.[5]

Each group is instructed to discuss about their general "likes" and to decide upon a group name that belongs to one of the four very general categories as follows:

1 *activities/hobbies that you and your group members all like*

2 *places/cities that you and your group members all like*

3 *persons/animals that you and your group members all like*

4 *objects/food that you and your group members all like*

[5] Communication with group members over personal information, either online or in person, has been found to create a tighter bond than the traditional minimal group methods (Buchan *et. al*, 2006; Chen and Li, 2011)

These four general categories are chosen for the sake of cross-cultural comparisons, as they do not impose a culture-specific topic. The classical method of dividing subjects into two groups is for instance by asking them whether they prefer some Klee painting or some Kandinsky painting, something with which some (cultural) groups might be more familiar with than others.

In the rest of the experiment, the relevant group name and the identity code are shown on the upper left corner of every screen, reminding subjects of their own group name (see Appendix A2 for the list of group names came up by subjects). Moreover, subjects do not have access to the name of the other group. This is done to prevent them from associating themselves with the name of the other group in case the other group decides on a name that they might like more, or that happens to be the same.

(b) ORP Elicitation

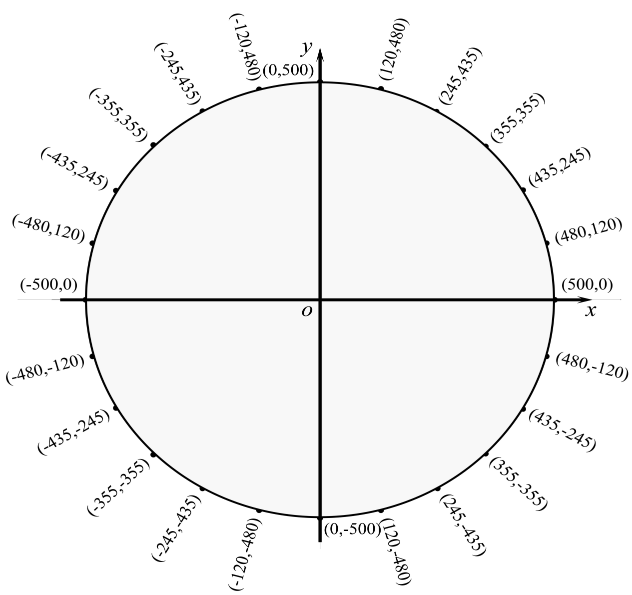
To elicit subjects' ORPs, I use a combination of the "Circle Test" and the "Ring Test". The Circle Test was developed by Sonnemans, Van Dijk and Van Winden (2006) as an adaptation of the Ring Test to capture continuous information of Social Value Orientation (SVO). SVO originates from the seminal work by Messick and McClintock (1968) to capture stable motivational bases of resource allocation choices in interpersonal context. The SVO measures predominantly used in psychology are categorical. The categories (Prosocial, Individualistic and Competitive) capture the three main motives proposed by Messick and McClintock to maximize own gain, joint gain or relative gain. The 24-items Ring test is a commonly used SVO measure to elicit the main motives categorized (Liebrand, 1984; Liebrand and McClintock, 1988). [6] In the variant I use here, subjects do not need to click on any point on the circle as used in Sonnemans *et al.* to know the relevant payoffs information. Instead, subjects choose on a partitioned circle any of the 24 points (with 6 points per quadrant), which allow subjects to have a quick grasp of the payoff structure (see Figure 1). x is the payoff that will be allocated to one, usually the subject him- or herself and y to some other individual such as an ingroup or an outgroup member. The angle between the line that connects the center of the circle with the payoff vector (x,y) and the horizontal axis is a measure of the extent the individual cares about the other (e.g., Prosocial: 45°, Individualistic: 0°, Competitive: - 45°). While a continuous circle gives exact information about

[6] According to the Ring test, subjects can also be categorized as Altruistic, Martyr, Masochistic, Sadomasochistic or Sadistic although these were considered as unusual and unlikely to be consistent with subjects' real choices (Liebrand, 1984).

subjects' preferences, a partitioned circle makes it easier to compare choices in case subjects do not take time to explore their options – a feature it shares with the decomposed ring test.

Note that the allocation decisions are not restricted to the first quadrant of the circle, so that negative payoffs can be allocated as well. The radius of the circle is 500 experimental points. The angle between two consecutive points on the circle is approximately 15° with some small deviations for making the number of points divisible by 5 so that the numbers appear more natural to the subjects. Unfortunately, one error was made when labeling the 24 points on the circle with 5 points off the number meant to be: for the 15° angle, it should have been (485, 120) instead of (480, 120), and reversely (120, 485) instead of (120, 480) for the 75° angle. Future research that uses this Circle Test variant should have it corrected before using it.

Figure 1: Partitioned Circle



Each subject had to make three decisions in circle tests with different beneficiaries. The first decision (Circle Test Self-In) was to allocate money between him- or herself and another randomly matched member of his or her group. The second decision (Circle Test Self-Out) was to allocate money between him- or herself and to a member randomly chosen from the other group. The third decision (Circle Test In-Out) was to allocate money between another member of his or her group and to a member of the other group. I elicit ORPs based on these three circle tests. Details will follow in the next subsection.

(c) Cheating Games

The cheating games capture the general cheating propensity for oneself or for another (i.e., for an ingroup member or for an outgroup person). I deploy several variants of the Mind Game (Jiang, 2013) in which participants lie purely in their minds so that it is more than evident that lying is only inferable statistically without any hard proof. The variants are: the Mind Game in which subjects can cheat to benefit themselves ("Cheat-for-Self"), the Mind Game in which subjects can cheat to benefit another ("Cheat-for-Another"). The other can be either an ingroup ("Cheat-for-In") or an outgroup member ("Cheat-for-Out").

i. One-player Mind Game: Cheat-for-Self

In *Cheat-for-Self* as in Jiang (2013), subjects can cheat for personal gain by breaking the prescribed rules. The three steps of Cheat-for-Self are as follows:

Step 1. *Subjects first choose purely in their mind which side of a six-sided die will count for their earnings: the side facing up or the side facing down.*

Step 2. *Then they throw a virtual die.*

Step 3. *After subjects see the outcome, they self-report the side initially chosen in Step 1.*

For each outcome of the die that turns up, the earned points can be either high or low, corresponding to the side chosen. Note that the number of dots on the opposite sides of a six-sided die always adds up to 7. For instance, if the outcome of the throw is "1" and the subject reports, truthfully or not, that he had "Down" in mind, he receives "6" experimental points in payoff. If he reports "Up", he will then receive "1" point. So subjects can cheat by reporting a side that will yield more points: 4, 5, 6 instead of 1, 2, 3 points, regardless of the side that they initially chose in their mind. In the experiment, people are asked (instructed) to report the side that they had chosen in their mind, hence, if they report the other side, they violate the instructions, that is break the rule of the experiment. Note also that the experimenter is not able to verify whether a rule violation occurred, but the extent of improbable luck here serves to elicit subject's individual cheating propensity in a controlled laboratory setting.

The Mind Game is utilized here, instead of the die-rolling game by Fischbacher and Föllmi-Heusi (2013) because subjects from different countries might differ systematically in how suspicious they are towards their cheating behavior being found out in the artificial lab environment that might add unnecessary noise. For instance, subjects from countries with more corruption might be more suspicious of hidden cameras if cheating is in the open, even if they complete the cheating task in a private space. Since the Mind Game cheating takes place merely in the mind, it alleviates the issue of the potential confound of the perceived probability of being physically caught in the lab. However, note that this method is not perfect, as subjects might still be suspicious about the statistical inference of cheating, given that subjects play multiple rounds. To alleviate this confound, one can choose to let subjects play only one round of Mind Game if one is only interested in aggregate level differences.

ii. Two-player Mind Game: Cheat-for-Another

Cheat-for-Another simulates the cheating context in which one party abuses the entrusted power to give another party undeserved benefits as a favor without any personal monetary gain. It allows us to examine the willingness to cheat for an ingroup or an outgroup member.

Two roles are thus created: "Scorers" and "Throwers". Scorers are entrusted the power of reporting the chosen sides in determining the payoffs of "Throwers", while receiving a fixed payoff themselves. Throwers only need to throw the virtual dice. For each matched pair of Scorer and Thrower, the three steps of the game are:

Step 1. *Scorer first chooses purely in his or her mind which side of a six-sided die will count for Thrower's earnings: the side facing up or the side facing down.*

Step 2. *After Scorer confirms having chosen a side, Thrower throws a virtual die.*

Step 3. *After seeing the outcome of the die thrown by Thrower, Scorer self-reports on the screen the side initially chosen in Step 1.*

Thrower's earning is then determined by the die outcome and Scorer's self-reported "chosen" side. If Scorer dishonestly reports the good side, it is Thrower who will benefit from it. The game is repeated for 15 rounds for each matched pair. Thrower earns the sum of the points from 15 rounds with the expected payoff of 52.5, while Scorer earns a fixed amount of 50 points.

2.2 Experimental Measurements

ORP measures. I elicit ORPs from the three Circle Tests in which subjects decide about payoff vectors (π_s^1, π_i^1), (π_s^2, π_o^2) and (π_i^3, π_o^3), where π is the amount allocated, the numbers 1, 2 and 3 refer to the first (Self-In), second (Self-Out) and third (In-Out) decision of the allocation, and *s*, *i* and *o* refer to the beneficiaries of the allocation, namely self, ingroup and outgroup. In line with the Social Value Orientation measure, the angle between the line that connects the center of the circle with the payoff vector (*x,y*) and the *horizontal* axis is a measure of the extent the individual cares about payoff *y, i.e., SVO°(y)=sin*$^{-1}$ (y/500).

I thus derive the angles (normalized by the maximal angle 90°, i.e., *SVO(y) = SVO°(y)* /90°) of costly ingroup- and outgroup-prosociality Π_i^1, Π_o^2, as well as non-costly outgroup-prosociality Π_o^3:

$$SVO(\pi_i^1) = sin^{-1}\left(\frac{\pi_i^1}{500}\right) / 90°; \quad SVO(\pi_o^2) = sin^{-1}\left(\frac{\pi_o^2}{500}\right) / 90°; \quad SVO(\pi_o^3) = sin^{-1}\left(\frac{\pi_o^3}{500}\right) / 90°.$$

Similarly, the angle between the line that connects the center of the circle with the payoff vector (*x,y*) and the *vertical* axis is a measure of the extent the individual cares about his own payoff or the ingroup's payoff *x*, i.e, *SVO°(x)=sin*$^{-1}$ (x). The normalized angles of ingroup- and outgroup-proselfness Π_s^1, Π_s^2, and non-costly ingroup-prosociality Π_i^3 are:

$$SVO(\pi_s^1) = sin^{-1}\left(\frac{\pi_s^1}{500}\right) / 90°; \quad SVO(\pi_s^2) = sin^{-1}\left(\frac{\pi_s^2}{500}\right) / 90°; \quad SVO(\pi_i^3) = sin^{-1}\left(\frac{\pi_i^3}{500}\right) / 90°.$$

Because the maximum amount to allocate is 500 (90°), and the minimum is -500 (-90°), the measures are in between 1 and -1. The following measures based on the angles are used in the analyses to capture different dimensions of prosociality:

- Proselfness Π_{self}: average of ingroup-proselfness and outgroup-proselfness, i.e., $\frac{SVO(\pi_s^1)+SVO(\pi_s^2)}{2}$
- Prosociality $\Pi_{another}$: average of costly ingroup-prosociality and outgroup-prosociality, i.e., $\frac{SVO(\pi_i^1)+SVO(\pi_o^2)}{2}$

Although these two measures are derived from the same circle tests, they elicit different dimensions of the overall prosociality. The prosociality measure $\Pi_{another}$ contains richer information compared to Π_{self} as it differentiates positive and negative other-regarding preferences. For instance, suppose that in Circle Test Self-In one subject chooses (355, -355) and the other chooses (355, 355), though the two subjects would have the same level of ingroup-prosociality according to Π_{self}, the former would be negative in ingroup-prosociality and the later positive according to $\Pi_{another}$. By differentiating prosocial and anti-sociality, $\Pi_{another}$ can arguably increase model fit compared to Π_{self} as it is more refined. Nevertheless, Π_{self} captures more accurately how much one cares about his or her own payoffs.

For the particularism measure, I elicit both costly and non-costly particularism:

- Costly particularism PAR^{c}: the normalized difference of costly ingroup- and outgroup-prosociality, i.e., $\frac{SVO(\pi_i^1)-SVO(\pi_o^2)}{2}$
- Non-costly particularism PAR^{nc}: the normalized difference of non-costly ingroup- and outgroup-prosociality, i.e., $SVO(\pi_i^3) - SVO(\pi_o^3)$

Note that costly particularism measure is normalized by the denominator of 2, as the maximum difference between costly ingroup- and outgroup-prosociality is 2, e.g., when $SVO(\pi_i^1)$=1 and $SVO(\pi_o^2)$= -1, whereas the non-costly particularism is normalized by the denominator of 1, as the maximum difference between any two angles within each Circle Test is 1.

Cheating propensity. As for the extent of cheating, I use the foresight measure as in Jiang (2013) that captures the proportion of advantageous sides as if they had "foresight" of the die outcomes before the die is thrown. As the die is fair, the expected average foresight level would be 0.5. Thus, we can infer that subjects probably have cheated when the foresight significantly deviates from the theoretical level of 0.5 using a binomial test. Let f_j be the indicator of earnings higher than three for round $j;$ foresight F denotes the average of f_j over all the n rounds played in the game:

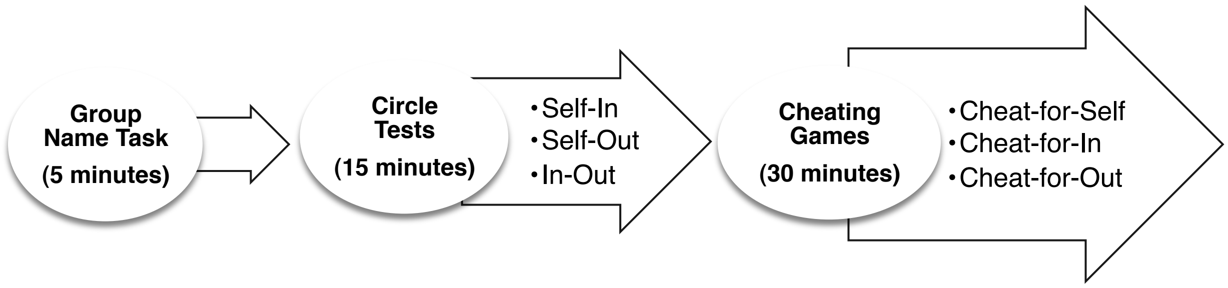
$$F = \frac{\Sigma f_j}{n}, \text{ where } f_j = 1 \; if \; earning > 3; 0 \; otherwise.$$

Accordingly, an individual's cheating propensity can be inferred. Nevertheless, even if foresight is one, it is only probabilistic that it occurs by cheating instead of by chance. There are three foresight measures at the individual level: $F^{CS}, F^{CI}, F^{CO} where$ *CS*, *CI* and *CO* refer to the games in which cheating takes place ("Cheat-for-Self", etc.). F^{CS} captures the individual cheating propensity, i.e., the propensity to break rules. F^{CA} is also used in the analyses when looking at the overall cheating for another (F^{CA} =F^{CI} in ingroup matching; $F^{CA} = F^{CO}$ in outgroup matching). At the individual level, subjects are probabilistically inferred to have cheated in Cheat-for-Self when F ≥ 0.70 or F ≤ 0.30 (14/20 or 6/20, binomial test, n = 20, p = 0.058) and when F ≥ 0.73 or F ≤ 0.27 in all other games (11/15 or 4/15, binomial test, n = 15, p = 0.059).

2.3. Experimental Procedure

The experiment was conducted in computer labs and programmed in z-Tree (Fischbacher, 2007). ***Figure 2*** sketches the experimental flow of the three main parts. For each part of the experiment, subjects followed the same procedure of first listening to the recorded instructions and then completing some comprehension questions for ensuring a clear understanding of the rules before making the actual decisions (see Appendix C for the comprehension questions).

Figure 2: Experimental flow chart



The experiment started with the introductory instructions of how subjects would receive their payments in a double-blind procedure at the end of the experiments, followed immediately by the instructions of the first part, the "Group Name Task". There were eight subjects in each session. Each subject was randomly assigned an identity code which sorted them into two groups. One group consists of the members "A", "B", "C" and "D", and the other "H", "I", "J" and "K". Each group then spent five minutes chatting virtually via the z-Tree chat windows among group members to decide upon a group name. After the five minutes were over, the virtual chat window automatically closed and every subject was asked to input privately the group name that was collectively agreed upon. A name was given to the group only if every member of the group inputted the same name. On the top left corner of the screen, the name collectively agreed upon would be shown throughout the experiment. If there were no consensus over the name, the experiment would then continue with the group labeled as "no name". In this experiment, all groups succeeded in agreeing upon the same group name. The Group Name Task was followed immediately by three Circle Tests that were design to elicit group-specific ORPs. The Circle Tests were given in a fixed order: Self-In, Self-Out and In-Out.

The last part of the experiment consisted of three cheating games to benefit self, ingroup and outgroup, with an ingroup and an outgroup counterpart respectively. Subjects first played 20 rounds of Cheat-for-Self. Afterwards, subjects were informed that two subjects from each group – A and B from one group, and H and I from another – were automatically assigned as "Scorers" and the rest "Throwers" (C, D, J and K). Scorers then proceeded to play Cheat-for-Another for a total of 30 rounds, 15 rounds with an ingroup Thrower followed by another 15 rounds with an outgroup Thrower. For simplicity, the order of ingroup and outgroup interactions was fixed (see the matching ***Table 1***). A matching table with neutral framing was presented in the instructions to subjects so that they only play 15 rounds against each matched partner and their roles as Scorer or Thrower never switch. The fixed order of interactions serves to study within-subject variations in different games and cross-country differences, but randomization of the ingroup and outgroup matching would be required to control for learning effects for other research questions such as comparing behavior in ingroup and outgroup matching at the aggregate level.

Table 1: matching of the two-person cheating games

Scorer	Thrower	
	Cheating Game	
	Round 1-15 (ingroup)	Round 16-30 (outgroup)
A	C	J
B	D	K
H	J	C
I	K	D

Afterwards, subjects also play the bribery game for which the results are not presented in this paper. At last, subjects filled in an online survey, during which the experimenter calculated and prepared their earnings.

2.4. Subject Pools and Payment

For this study, 224 students in total participated in experiments that were conducted between October 2010 and July 2011 at 6 universities in 4 countries: China, Japan, Italy and the Netherlands. The six experimental sites were taken from two different continents, Asia and Europe, within which one is ranked higher by the Corruption Perception Index (CPI)[7] (China: 80th; Italy: 72th) and the other lower (Japan: 17th; The Netherlands: 9th). Students were recruited through the local university's experimental labs. Since China is much bigger in size than the rest, and Italy is commonly perceived by businessmen and analysts to differ in corruption level when comparing the northern part with the southern part, two cities geographically distant from each other were chosen in both Italy and China, one from the middle

[7] The Corruption Perceptions Index ranks countries/territories based on how uncorrupt their public sector is perceived to be by analysts, businessmen and experts (http://cpi.transparency.org/cpi2012/results/). It is a composite index combining surveys and assessments of corruption collected by different institutions that met the criteria set by Transparency International.

part and the other from the southern part of the country. Four sessions were run at Fudan University in Shanghai and three sessions at the Shenzhen campus (Guangdong) of Peking University. In the first session in China, computers collapsed after the Circle Tests. For this session, only the Circle Tests data is available and not included in the analyses in this paper. In addition, 3 sessions were run at Taranto University and 4 at University of Sienna, 8 at Waseda University in Tokyo and 6 at Tilburg University (see ***Table 2*** for the demographic data).

Table 2: Demographic data of all subjects

	Nr. Of subjects	**Male**	**Age**	**Nr. Of Siblings**	**Nr. Of Countries visited**	**Experience (Experiments participated)**	**Major:economics& business**
Fudan University (Shanghai); Peking University (Shenzhen)	CN (n = 48)	0.56 (32)	24.5 (35)	1.31 (36)	1.86 (36)	2.19 (36)	44% (36)
Waseda University (Tokyo)	JP (n = 64)	0.57 (63)	21.5 (64)	1.14 (63)	3.54 (63)	2.44 (32)	42% (64)
University of Siena (Siena); University of Taranto (Taranto)	IT (n = 56)	0.56 (55)	23.6 (55)	1.44 (54)	10.72 (54)	2.27 (55)	93% (55)
Tilburg University (Tilburg)	NL (n = 48)	0.85 (48)	21.3 (48)	1.73 (48)	11.49 (47)	3.15 (48)	96% (48)

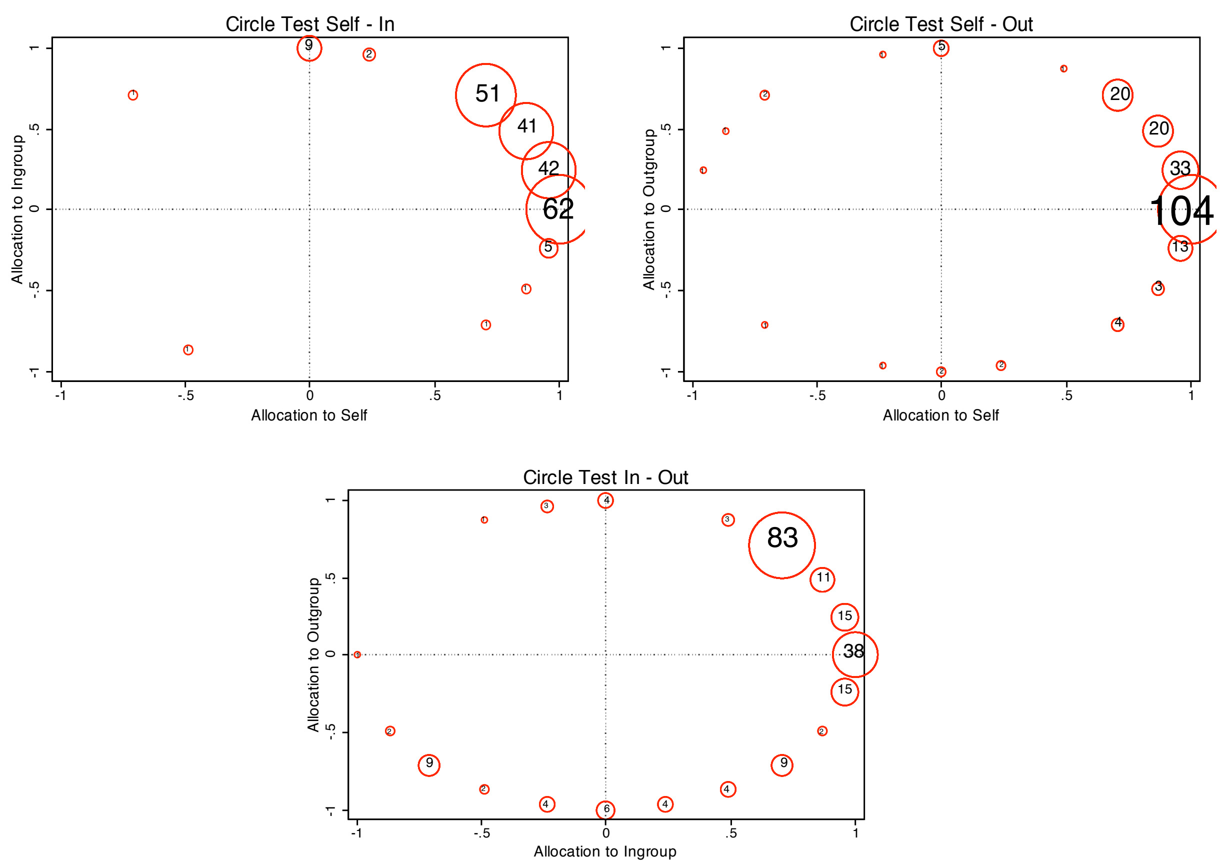
Note: Number of subjects with available demographic data in brackets. The internet link of the survey failed to work for the second session in CN and collapsed halfway for some of the subjects in the last session in CN, which resulted in less survey data of CN (n =36).

All instructions were translated into the native language and back-translated into English by different translators. To alleviate the experimenter specific effect, instructions were given by means of audio files pre-recorded by native speakers. Decisions were all incentivized. The average payment per hour was based on the local convention of the labs: €10 in both Italy and the Netherlands, 45 Yuan in China and ¥1000 in Japan. Each participant first received an initial endowment of 500 points in China (1 point = 2 *fen*), 500 *yen* in Japan and 500 *eurocent* in Italy and Netherlands. Then, out of the 24 Circle Test decisions made by the eight subjects (each making three), only one of the decisions was randomly chosen to be paid out. For instance, if decision Self-In made by participant A was chosen, both A and one of A's group members B, C or D would receive some payment on top of the 500 endowment, depending on the allocation chosen by A. Since the lowest payoff possible was -500, the endowment made sure that subjects would not receive negative payoffs. Subjects did not know what decisions others made since no feedback was given during the experiment. They also could not infer anything about the decisions since they were only informed about the total sum of payment at the end of the experiment. In the cheating games, subjects received 15 *fen* per point in China, 4 *yen* per point in Japan and 4 *eurocent* per point in Italy and Netherlands. The expected payoff was 70 points (3.5 per round x 20 rounds) in Cheat-for-Self. In Cheat-for-Another, Scorers earned a total fixed payoff of 100 points, whereas Throwers earned an expected payoff of 105 points (52.5 x 2 matchings).

2.5. Evidence for Ingroup Bias and Embezzlement

There is evidence of ingroup bias triggered by the Group Name Task. When self-interest is at stake as in Circle Tests Self-In and Self-Out, subjects costly allocate more to ingroup relative to outgroup (Wilcoxon Signed-rank tests, z = 8.01, $p < 0.001$, $n = 216$). Similarly, when self-interest is not at stake, subjects also allocate more to ingroup relative to outgroup (Wilcoxon Signed-rank tests, z = 8.58, $p < 0.001$). Figure 3 depicts the descriptive distribution of choices in these three tests.

Figure 3: Distribution of choices in Circle Tests



Note: The numbers in the bubbles are the counts of subjects who choose that payoff vector.

Subjects Excluded from the Analyses

As depicted in Figure 3, the patterns are intuitive as almost twice the number of subjects give nothing to the other when the other is an outgroup member than when the other is an ingroup member, and most of the choices are distributed on the right half of the circle which assign positive (instead of negative payoffs) to X which is either self or ingroup. Note, however, that 8 subjects out of 216 allocate a negative payoff to self, e.g., two chose (Self: 0; In: 500), (Self: -355; Out: 355), (In: -120; Out: 480) while there exists always an alternative that would give self a positive payoff with the same payoff outcome to the other. These subjects could be martyr, masochistic or sadomasochistic although these categories are rare, or they could simply be confused about which payoff is to self and which is to the other rather than their falling in the uncommon SVO categories. Confusion might have resulted from self, ingroup and outgroup being referred to as "X" and "Y" in the circle and the payoff vectors not being labeled which makes it likely that these subjects reversed the payoffs they would have liked to choose. This is in line with the observation that most subjects made this "weird" choice in Self-Out, but not in Self-In. I chose to exclude these subjects from the analyses of this chapter as including them runs the risk of making a type II error (a false negative) in not detecting the true underlying relations between ORPs and corruption and it is more important for my research purposes to study the behavior of subjects with more common social preferences. To further alleviate this risk, I also do not include the five altruists (i.e. subjects with $\Pi_{another} > 67.5°/90°$, a benchmark taken from the Ring Measure).[8] In line with the vast literature of the SVO, altruists were often not considered (see Balliet, Parks and Joireman, 2009 for a meta-analysis and Murphy and Ackermann, 2011 for a review). Hence, all in all, 13 subjects were excluded from the analyses related to ORP measures.

Cheating in Mind Games

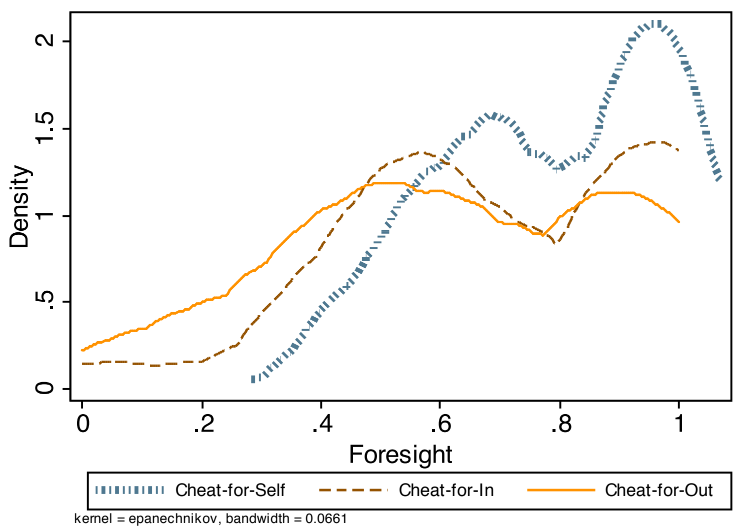
There is also strong evidence of cheating in the Mind Games. Cheating in Mind Games is captured by the foresight measure, i.e., the proportion of higher payoffs 4, 5, 6. Significant cheating can be

[8] Among the five of them, three of them choose (Self: 0; In: 500), (Self: 0; Out: 500), (In: 0; Out: 500); one chooses (Self: 0; In: 500), (Self: 0; Out: 500), (In: 500; Out: 0); and one chooses (Self: 120; In: 480), (Self: 245; Out: 435), (In: -120; Out: 480). Note that I do not exclude the altruists who are altruistic either to ingroup only (3 subjects) or to outgroup only (1 subject).

statistically inferred in all the five cheating games, as the average individual foresights are all significantly higher than 0.5 (Wilcoxon Signed-rank tests, all *p*-values are smaller than 0.001, *n* = 108).

Overall, 63.5% of the subjects are probabilistically inferred to have cheated for personal gain at the individual level (F ≥ 0.70), while no one is inferred to have cheated to hurt themselves (F ≤ 0.30). Interestingly, as depicted by the dotted line in Figure 4, there are two humps for foresights in Cheat-for-Self. The one centered at about F = 1 points to the presence of full cheaters, whereas the one centered at F = 0.7 suggests the possible presence of disguised cheaters who only cheat to the extent that is statistically not inferable.[9]

Figure 4: Kernel density distributions of foresights in the cheating games



Note: the kernel function used here is Epanechnikov with bandwidth of 0.066.

While the hump of full cheaters is present in both Cheat-for-In and Cheat-for-Out, the second hump in Cheat-for-In centers at 0.6 while it is around 0.5 in Cheat-for-Out. Almost half of the Scorers are inferred to have cheated to help their counterparts (F ≥ 0.73): 47.2% in ingroup matching and 41.7% in outgroup matching. In addition, a small proportion is inferred to have cheated to hurt their counterpart (F ≤ 0.27), especially towards outgroup: 4.6% in ingroup matching and 13% in outgroup matching.

[9] Of course, subjects did not know the significance level that I adopted, and it is not to be expected that these cheaters have actually calculated what would still be within my “region of honesty”.

3. Main Results

I examine whether more proself or more particularistic subjects cheat more when the beneficiary is self, and whether more prosocial subjects cheat more when the beneficiary is another.

Result 1.a: *In Cheat-for-Self, more proself subjects cheat more*.

As hypothesized, Π_{self} correlates positively with cheating foresight F^{CS} in Cheat-for-Self with statistical significance, as shown by the Tobit model estimates in column (1) in Table 3. The more proself subjects are, the more they cheat (p = 0.004).

Table 3: Tobit regressions of individual foresights in Cheat-for-Self (F^{CS}) on ORPs [10]

	Cheat-for-Self (F^{CS})		
	(1)	(2)	(3)
Π_{self}	0.204***	0.147*	0.205***
	(2.92)	(1.95)	(2.92)
PAR^{c}		-0.193*	
		(-1.96)	
PAR^{nc}			-0.003
			(-0.13)
Log-likelihood	-42.779	-40.882	-42.771
LR chi2 (1)	8.38	12.17	8.39
Prob > chi2	0.004	0.002	0.015

Note: number of observations = 203; 54 observations censored at $F^{CS} = 1$; marginal effects at means with z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Result 1.b: *Controlling for proselfness, more (costly) particularistic subjects cheat less for self at slightly above 5% significance level.*

In contrast to my hypothesis that more universalistic subjects are more likely to follow impartial rules, regression (2) in ***Table 3*** reveals a significant correlation between costly particularism and cheating for self in the opposite direction at slightly above 5% significance level: more costly-particularistic subjects cheat less controlling for proselfness (p = 0.051). Non-costly particularistic

[10] Since about a quarter of the subjects cheat to the full extent with their cheating foresights equal to 100%, censored regression models are used here for taking into account the clustering of upper or lower threshold values. Note that the results presented in this table are robust when excluding subjects who cheat to the full extent.

preference, on the other hand, adds little for predicting the individual cheating propensity with or without controlling for proselfness ($F_{1, 201} = 0.01$, $F_{1, 202} = 0.00$) as shown in (3).

One potential (and probably quite relevant) explanation for why there is an opposite correlation (in the case of costly particularism) is that particularism based on artificial ingroups is not generalizable to particularism based on real ingroups. As discussed in the ***Section 2.1***, Triandis (1972) hypothesized that subjects from individualistic countries, who in reality differentiate less between ingroup and outgroup members, may enter (and leave) new ingroups more easily. Accordingly, when an artificial ingroup formation procedure is used, subjects from individualistic countries might exhibit higher ingroup bias. The danger of eliciting ingroup bias by using the artificial ingroup formation therefore is that it elicits universalistic preference instead of particularistic preference. Additional analyses on cross-country differences in ORPs in ***Section 4*** will also shed light on this issue. As for little correlation in the case of non-costly particularism, more research is needed to find out the underlying causes. One may speculate that it is related to contextual saliency: while universalistic subjects may indeed show strong ingroup ties when allocating between self and ingroup and weak outgroup ties when allocating between self and outgroup, they prefer more equal payoffs when allocating between ingroup and outgroup.

The second main analysis explores how prosocial subjects resolve the virtues in dilemma: benefiting another whom they (may) care about or respecting the rule of no cheating. I control for the potential confounding factor of the baseline cheating propensity because more prosocial subjects might cheat less for others simply because they have a lower baseline cheating propensity.

Result 2: *In Cheat-for-Another, controlling for their individual cheating propensity, more prosocial Scorers cheat significantly more for outgroup Throwers, but not for ingroup Throwers.*

Overall, without separating ingroup or outgroup Throwers and controlling for the individual cheating propensity, as shown in ***Table 4 (2),*** more prosocial Scorers (measured by $\Pi_{another}$) do cheat significantly more for others at exactly 5% significance level. The correlation is still positive but only approaching marginal significance without controlling for individual cheating propensity, as shown in (1). The implication of the results shown in (1) and (2) taken together is that more prosocial subjects tend to cheat more for another, though the extent of cheating is weakened by their lower individual cheating propensity. As shown also in (2), cheating foresight in Cheat-for-Self F^{CS} does significantly and positively correlate with foresight in Cheat-for-Another F^{CA} ($p < 0.001$).

Table 4: Tobit regressions of individual foresights F^{CA} on ORPs

	Cheat-for-Another (F^{CA}) (1)	(2)	(3)	(4)
Π_{another}	0.182	0.231**	0.321***	0.373***
	(1.48)	(1.97)	(2.60)	(2.97)
Ingroup			0.134***	0.139***
(1: ingroup; 0: outgroup)			(4.24)	(4.28)
Ingroup x Π_{another}			-0.284***	-0.291***
			(-2.94)	(-2.93)
F^{CS}		0.625***		0.629***
		(5.16)		(5.18)
Log-likelihood	-108.053	-104.178	-104.178	-84.327
F	2.16	14.05	6.49	11.71
Prob > F	0.144	0.000	0.000	0.000

Note: number of observations = 202; since each Scorer has two related foresight measures in Cheat-for-Another, one in ingroup matching and the other in outgroup matching, standard errors adjusted for 101 individual clusters; 48 observations censored at $F^{CA} = 1$ and 5 observations censored at $F^{CA} = 0$; marginal effects with z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

The effect of prosociality Π_{another} significantly interacts with group identity with or without controlling for cheating propensity, as shown in (3) and (4). When controlling for cheating propensity, as shown in (4), while the coefficient of Π_{another} is positive and significant at 0.373 in outgroup matching (i.e., Ingroup = 0), it is insignificant at a negligible effect size of 0.082 in ingroup matching (i.e., the difference between 0.373 and 0.291, p = 0.464). As shown in (3), results are similar without controlling for cheating propensity F^{CS}. More prosocial subjects only cheat more for outgroup but not more for ingroup compared to less prosocial subjects. It is plausible that the more prosocial subjects are only able to self-justify cheating and be more free of the psychological moral cost of cheating when helping outgroup as helping ingroup may still be deemed as self-regarding behavior.

The results so far cover how different ORPs characteristics predict cheating for different beneficiaries. As expected, more proself subjects cheat more for self. Though (costly) particularism does have predictive power, the correlation is negative rather than positive, i.e., more (costly) particularistic subjects do not cheat more; on the contrary, they cheat less. One potential explanation is that the more ingroup-prosocial subjects in the artificial ingroup setting are the more universalistic in real life. In Cheat-for-Another, more prosocial subjects only cheat more for outgroup, but not more for ingroup. As net effects, more prosocial subjects cheat relatively more for others controlling for individual cheating propensity.

4. Additional Analyses on Culture and Cheating

The four countries sampled differ on different dimensions as shown in ***Table 5***: while China and Japan are more individualistic and less moral inclusive than the Netherlands and Italy (Hofstede*,* Hofstede, and Minkov, 2010; Schwarz, 2007), [11] China and Italy are perceived to be more corrupt based on the informed views of analysts and businessmen in these countries (CPI, 2011). They are also perceived to be worse than Japan and the Netherlands for both the sub-indicator of "control of corruption" as well as that of "rule of law" (World Governance Indicator, 2008).

Table 5: Country ranking based on previous survey studies

Country	Individualism (Hofstede *et al.*, 2010)	Moral inclusiveness (Schwarz, 2007)	World Governance Indicator (1st: control of corruption, 2nd: rule of law, 2008)	Corruption Perception Index (2011)
China	20	1	35, 45	80th
Italy	76	4	65, 63	72th
Japan	46	3.3	86, 89	17th
Neherlands	80	4	97, 95	9th

Note: the exact level of subjects' individualistic orientation can differ from that of the national level due to the subculture of the recruited subjects. The national level index data only serve as rough references.

There are two competing hypotheses regarding the propensity to break rules: on the one hand, it is intuitive that subjects from countries with more perceived corruption China and Italy would cheat more (see also Fisman and Miguel, 2007; Barr & Serra, 2010, Cameron *et al.,* 2009 for empirical evidence on culture and corruption). On the other hand, Mazar and Aggawal (2011) show that Hofstede's cultural dimension of collectivism correlates strongly with perceived corruption level. In line with that, subjects from more collectivistic countries China and Japan may cheat more, if at all.

Result 3: *Cheating levels differ across countries. The cluster of high CPI countries (China and Italy) exhibits significantly more cheating than that of low CPI countries (Japan and The Netherlands) in Cheat-for-Self, Cheat-for-In and Cheat-for-Out.*

As shown in Table 6, cheating levels differ significantly in Cheat-for-Self and Cheat-for-Out (Kruskal-Wallis Chi2 test, $p < 0.05$) and at marginal statistical significance in Cheat-for-In ($p < 0.1$).

[11] According to Hofstede et al., 2011, people from countries with high individualism index score have stronger preference for a loosely-knit social framework in which individuals are only expected to care for themselves and their immediate families, but not expected to care for ingroup members, and vice versa for countries which are highly collectivistic.

The results of the pairwise comparisons based on the Mann-Whitney U tests lend support to the hypothesis that the country cluster perceived to be more corrupt exhibits more cheating. No support is found for the alternative hypothesis that cheating differs according to individualism. In other words, the two more collectivistic countries do not cheat more. One can even observe that Japan, a country belonging to the more collectivistic cluster, cheats the least in all three games.

Table 6: Cheating in different embezzlement contexts

	means by culture (normalized)				Kruskal-Wallis Chi2 (3) with ties	*z-scores* of Mann-Whitney U tests		
All Scorers	JP (*N*=30)	CN (*N*=23)	NL (*N*=23)	IT (*N*=25)		(by corruption) JP+NL/ CN+IT	(by individualism) CN+JP/ IT+NL	IT+JP/ CN+NL
F^{CS}	0.713	0.813	0.761	0.886	*11.908****	-3.183***	-1.571	0.100
F^{CI}	0.611	0.788	0.728	0.784	*6.615**	-2.293**	-1.064	-0.914
F^{CO}	0.527	0.751	0.583	0.701	*10.565***	-3.98***	-0.318	-1.121

Note: z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1). I also use a control clustering as a baseline of the effect size: China and the Netherlands vs. Japan and Italy.

Given the significant correlations found between ORPs and cheating at the individual level, one might wonder if country of origin has any additional explanatory power to ORPs. Can the systematic cross-country differences be due to cross-country differences in ORPs, i.e., the cluster that cheats more for self is more proself or less particularistic (see Result 1.a, 1.b), or that the cluster that cheats more for others is more prosocial (see Result 2)? For identifying the presence of differences of ORPs at the group level, ***Table** 7* summarizes the results of the non-parametric tests: there was hardly any country that stochastically dominates any other in proselfness (Π_{self}), prosocilaity ($\Pi_{another}$) except for costly-particularism PAR^c. The cluster of more collectivistic countries (China and Japan) is less particularistic than that of the more individualistic ones (Italy and The Netherlands) at marginally significant level.

Table 7: Cultural Differences in ORPs

	means by culture (normalized)				Kruskal-Wallis	*z-scores* of Mann-Whitney U tests		
Scorers	JP (N=30)	CN (N=23)	NL (N=23)	IT (N=25)	Rank Test Chi2(3) with ties	(by corruption) JP+NL/ CN+IT	(by individualism) CN+JP/ IT+NL	IT+JP/ CN+NL
Π_{self}	0.809	0.786	0.786	0.753	*1.332*	0.670	0.987	-0.048
$\Pi_{another}$	0.176	0.193	0.186	0.121	*0.523*	0.416	0.162	-0.552
PAR^c	0.071	0.029	0.100	0.123	*3.629*	0.170	-1.703*	0.725

Note: z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1). This table presents results only based on Scorers' decisions. The results are robust when including also the Throwers.

Though particularism level does differ at the marginal significance level, it does not differ by the dimension of "corruption", but rather by the dimension of "individualism". All in all, the group level differences in the cheating games do not seem to corroborate the group level differences in particularism or other ORPs. Note, however, that the group level difference in particularism corroborates with Triandis *et al*. (1988) and Buchan *et al*. (2006) in that subjects from individualistic countries are actually more likely to exhibit ingroup bias towards new ingroup members. This result sheds light on the reverse correlation found in Result 1.b, confirming the possibility that the more particularistic subjects are those who are probably quicker in forming new ingroup ties.

For robustness, I also examine if cross-country cheating differences remain after controlling for the ORPs measures that are predictive of cheating at the individual level.

Table 8: Cross-country differences in Cheat-for-Self

Dependent variable: Cheat-for-Self (F^{CS})	Without controls (n = 203)	With controls (n = 203)	Without controls (n = 155)	With controls (n = 155)
Cluster Corrupt	0.083***	0.085***	0.093***	0.101***
(1: CN; IT)	(3.34)	(3.50)	(3.23)	(3.44)
Cluster Individualistic	0.035	0.032	0.031	0.009
(1: IT; NL)	(1.38)	(1.31)	(1.05)	(0.29)
Cluster Control	-0.004	-0.004	0.007	-0.002
(1: CN; NL)	(-0.17)	(-0.16)	(0.24)	(-0.06)
Π_{self}		0.209***		0.164**
		(3.06)		(2.17)
Nr. Of Experiments Participated				0.030*** (2.70)
Economic Major dummy				0.021 (0.62)
Male dummy				-0.001 (-0.02)
Log Likelihood	-40.322	-35.720	-31.167	-24.567
LR chi2	13.29	22.49	11.65	24.85
Prob > chi2	0.004	0.000	0.000	0.001

Note: number of observations = 203; 54 observations censored at for the full sample, 43 observations censored at for the partial sample, z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1)

As shown by Tobit regression results in ***Table 8***, country differences by the dimension of corruption is robust to including within-subject differences in proselfness. It is also robust to including control variables "Economic major", "Male" and "Experience" (number of experiments participated) with a reduced sample for which these survey data are available. For the full sample without demographic or ORP controls, the joint model of the country cluster dummies has significant explanatory power ($F_{3,200} = 4.53$, $p = 0.004$, n = 203), driven by the cluster dummy of corruption ($F_{1,200} = 10.86$, $p = 0.001$). When controlling for proselfness, the contribution of the model with the cluster dummy of corruption to the model fit is again significant ($F_{1,199} = 11.94$, $p = 0.001$), with comparable magnitude to that of within-subject differences in proselfness ($F_{1,200} = 9.49$, $p = 0.003$). For the partial sample with demographic controls, the contribution of the cluster dummies to the model fit is similar to that with the full sample ($F_{1,148} = 11.62$, $p = 0.001$), whereas the contribution of proselfness drops ($F_{1,148} = 4.77$, $p = 0.031$). This is in part because of a potential confounding covariate "nr. of experiments participated" which has taken up part of the explanatory power of proselfness ($F_{1,148} = 7.29$, $p = 0.001$).[12]

Likewise, ***Table 9*** shows the results of the robustness of the group level differences of cheating for others when including controls of within-subject differences in ORPs and cheating propensity.

Table 9: Cross-country differences and other-regarding cheating

Dependent variable	CG-in1 F^{CGI}	CG-in2 F^{CGI}	CG-out1 F^{CGO}	CG-out2 F^{CGO}
Cluster Corrupt	0.119**	0.087*	0.174***	0.158***
(1: CN; IT)	(2.40)	(1.80)	(3.22)	(3.02)
Cluster Individualistic	0.063	0.017	0.011	-0.013
(1: IT; NL)	(1.26)	(0.35)	(0.21)	(-0.24)
Cluster Control	0.057	0.060	0.052	0.041
(1: CN; NL)	(1.14)	(1.26)	(0.94)	(0.77)
F^{CS} dummy		0.215***		0.169***
(1: $F^{CS} \geq 0.73$)		(4.06)		(2.97)
$\Pi_{another}$		0.084		0.389***
		(0.74)		(3.14)
Bribery deal dummy				
Agreed X				
Log Likelihood	-49.920	-41.773	-47.299	-38.984
LR chi2	9.45	25.74	11.08	27.71
Prob > chi2	0.024	0.000	0.011	0.000
Censored at 0	2	2	3	3
Censored at 1	29	29	19	19

Note: number of observations = 101; 54 observations censored at for the full sample, 43 observations censored at for the partial sample, z-scores in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

[12] As a side note, the significant correlation between how experienced subjects are and how much they cheat for self might be due to more proself subjects being more likely to participate in an economic experiment, or behave more proself after having more experimental experience. It is a robust piece of evidence to justify eliciting subjects' previous experience with economic experiments.

Remarkably, both in Cheat-for-In and in Cheat-for-Out, as shown by the regressions results in columns CG-in2 and CG-out2 in ***Table 9***, differences remain significant in Cheat-for-Out and marginally significant in Cheat-for-In. Notwithstanding that cheating propensity seems to account substantially for the cheating variance, countries which are perceived to be more corrupt actually cheat more for ingroup or outgroup relative to self, compared to countries which are perceived to be less corrupt. This seems to suggest that though subjects from countries that are perceived to be more corrupt do engage in more self-regarding cheating, they actually engage in relatively more other-regarding cheating. Note that I also use the dummy variable of cheating propensity here to ease the comparison of the effect size: in Cheat-for-In, the marginal effect of the dummy variable of cheating-for-self is more than twice that of the dummy variable of "cluster corrupt", but comparable marginal effects are found in Cheat-for-Out). Countries seem to differ by the level of baseline cheating propensity in this experimental set-up. It is possible that people from societies with different modes of governance, either ruled by informal rules and social relationships or by impartial formal rules, may differ in their moral judgment of what to do when there are conflicting virtues.

All in all, this section shows systematic cross-country differences in cheating for different beneficiaries, even though there is little cross-country differences in ORPs. Comparing the four countries sampled by the ranking on the corruption index and the cultural dimension of individualism, I found that the differences in overall cheating are more in line with the corruption index ranking rather than with the cultural dimension of individualism. Compared to subjects from Japan and the Netherlands, the two countries that are ranked as less corrupt, subjects from China and Italy not only cheat more for self, but also cheat more for others. Moreover, these differences are also robust to the inclusion of individual level ORPs and demographics covariates.

5. Discussion

This paper inquires whether we can understand more about embezzlement if we relax the assumption of narrow self-interest maximization and incorporate ORPs, especially when formal rule enforcement mechanisms are absent.

From the perspective of governance, it is important to have rules that protect the social welfare and that curb self-interested acts in a social dilemma situation, i.e., when the personal material payoff is in conflict with the public interest. More importantly, such rules need to be successfully enforced.

Enforcement of rules can be costly and ineffective through formal deterrence mechanisms like legal sanctions, especially given that illegal acts tend to be secretive and hard to detect. Ideally, one would hope that individuals prefer to follow rules that are beneficial to society as a whole. One possibility for some individuals being relatively more likely to follow rules is that they are more impartial towards others' and their own interests, and less motivated to break rules for private benefit at the cost of the interests of some innocent others. Several results in this paper shed light on the relationship between ORPs and cheating for different beneficiaries. On the one hand, more prosocial individuals, i.e., those who derive more positive utility from benefiting others compared to the less prosocial, were more motivated to follow rules and less likely to cheat for themselves. On the other hand, they are more likely to break rules to benefit those from outgroup.

Following the sociological literature that suggests that it is the combination of ingroup love and outgroup hostility that leads to more corruption in society, I also hypothesized that more particularistic subjects should be more likely to cheat. The result turned out to be opposite. It is unclear why more particularistic subjects cheat less. One possibility is that this result only applies in the artificial group formation context. Despite of the Group Name Task being more than minimal, compared to other minimal group paradigms, an "ingroup" member in this experiment is still a stranger in real life: just one with whom one has had a minimal interaction in the form of a 5 minute group chat. Subjects who give more to an ingroup member can be those who are more universalistic in real life and open to make new ties with strangers, which is in line with the theoretical insights offered by Triandis *et al*., (1988). The cross-country analyses provide some preliminary evidence for this possibility as well, since subjects from the more individualistic countries are more particularistic in the Circle Tests than those from more collectivistic countries. Thus, one needs to be careful with interpreting or generalizing the related ingroup bias results reported here. More evidence using real ingroup could be gathered to shed light on this. Nevertheless, particularistic preference over an artificial ingroup can perhaps be used as a proxy for universalistic preference in a real ingroup rather than particularistic preference. In a sense, we do find corroborative evidence that a negative correlation exists between cheating propensity and particularism elicited using the group formation procedure. This result urges future research to provide systematic evidence on the type of ingroup bias induced by the artificial group formation procedure, as well as the external validity of this specific type of ingroup bias.

One of the potential caveats in the experimental design is that the harm of rule-breaking is on the experimenter and probably is not salient to the subjects since subjects in economic experiments arguably do not take into account the experimenter's payoff, or might not perceive the specific rule given by the experimenter as legitimate. And even though the rules are clearly specified that subjects should report truthfully about the side chosen, subjects might infer that there is no negative externality to break the rules despite the fact that they cannot be certain that breaking the rules in this experiment is really harm-free. The findings that most subjects do not cheat to the full extent and cheat to different extents in different contexts, however, seem to suggest that subjects do care about following rules in this experiment. The significant negative correlation found between prosociality and rule-breaking suggests that prosocial subjects indeed hesitate more than proself subjects to break rules for own benefit, even when the harm is not salient. Moreover, since I use within-subject treatment variation as a way of identifying the effects of ORPs, this drawback would only cause difficulty in finding any correlation between prosociality and rule-breaking propensity if everyone cheats to the full extent. Should I still find a significant correlation in this set-up, the results would probably be stronger if the harm of rule-breaking is more salient. Nonetheless, it would be a fruitful direction of a future inquiry to check if these results still hold when the harm of cheating is saliently imposed on an identifiable subject or group instead of a research fund.

Last but not least, the cross-country differences in cheating, despite of the cross-country similarities in ORPs, call for future research to further examine the factors behind group-level differences on moral and social norms related to rule-breaking.

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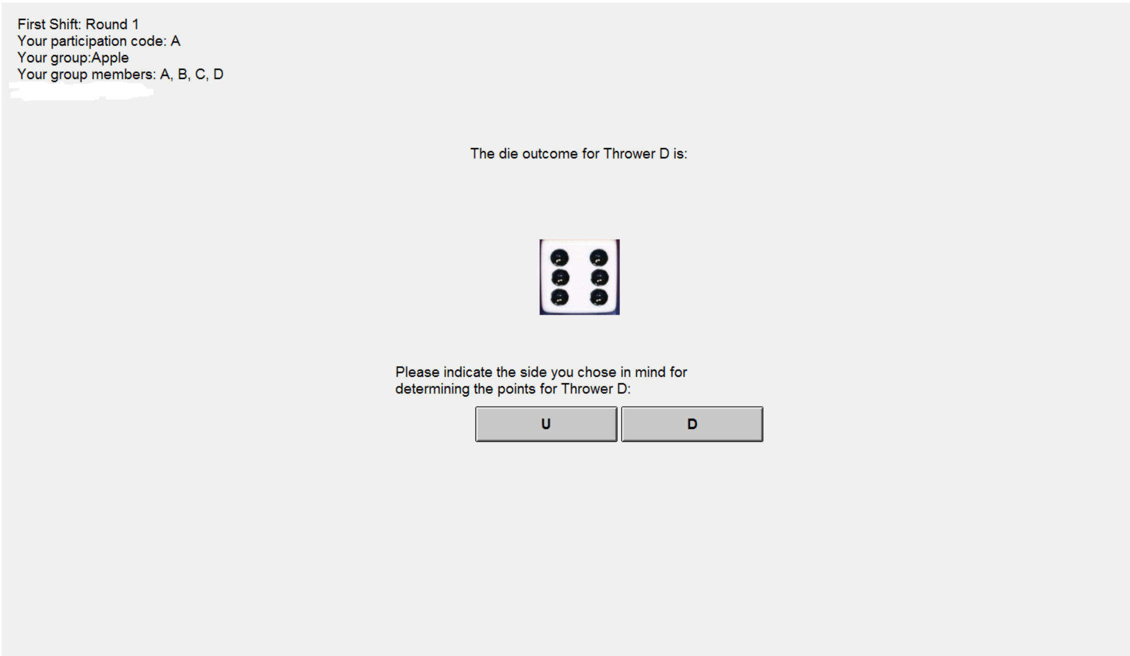
Appendix A: List of Group Names

Japan	China	Netherlands	Italy
Books(3)[13]	Sanguosha (2) [game]	De gemoedelijke Frikadellen vereniging van Tilburg	taranto (2)
Dog(2)	icecream	Tilburg013	LE CAVIE
music	running	Tilburg (3)	lasagna (2)
Waseda (2)	shanghai	Pizza	i fantastici quattro
Tokyo (2)	Pear	Frizza	giacinto facchetti
soba	F1	frietjemet	palmi
tomato	shenyanyuan	Tilburgs Tuig Tript Tijdens Tijgerjacht	Siena (2)
basketball	shenzhen	PSV	bruco
curry rice	MJ	Barca	new york
fish	FD (3)	Cultuur Snuiven	mare
sappora	panda team		tigri
			rosa rossa

[13] The number in brackets refers to the number of times the name is chosen by a group.

Appendix B: Sample Screen



Appendix C. Experimental Instructions

Introduction:

In the next 90 minutes, you will participate in two different experiments:

Experiment 1: You will be asked to make three decisions on payment allocations to you and others.

Experiment 2: You will play three die-throwing games.

In each of the experiments, you can earn money. When the experiments are over, you will be asked to fill in a short questionnaire during which your total payment will be calculated by the experimenters. You will receive your total cash payment from the two experiments anonymously and privately. In order to prevent others, including the experimenter, from inferring your decisions from the payment, you will receive your payment without any explanation of the details. We will now explain how you have to pick up your payment in steps:

Payment Procedure in steps:

1. We will put your payment in an envelope like the one in my hand. Each envelope corresponds to one participation code (which will be explained later) with the corresponding payment inside.
2. As soon as you finish the questionnaire online, you can walk to the payment table on your way to the door and find your envelope according to your participation code.
3. You will also find inside the envelope a receipt with the total payment amount written on it. You take out your payment and make sure it is the exact amount as written on the receipt.
4. You write down your student number and your name or signature on the receipt and put the receipt back into the envelope. Afterwards, you leave the envelope on the table and you are free to leave.

Group Name Task:

Participants will be randomly divided into two groups: the members of one group are coded as A, B, C and D; the members of the other group are coded as H, I, J and K. You will be informed about your participation code A, B, C, D, H, I, J or K before the experiment starts. Note that the code will be the same throughout the experiments.

Before the experiment, you and your group members will have about five minutes to come up with a name for your group. You can discuss about the name in the chat window that is opened on the computer. The name for the group has to belong to one of the categories described below:

1 activities/hobbies that you and your group members all like

2 places/cities that you and your group members all like

3 persons/animals that you and your group members all like

4 objects/food that you and your group members all like

Any questions?

INSTRUCTIONS FOR EXPERIMENT 1

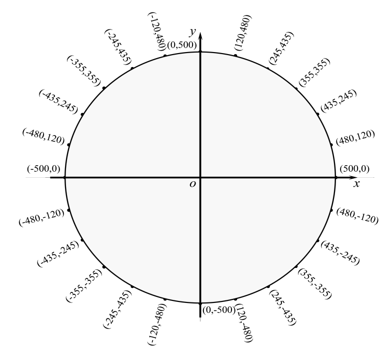
In this experiment, you will be asked to make three decisions on how to allocate money between two participants:

Decision 1: you allocate money to yourself and to another member of your group

Decision 2: you allocate money to yourself and to a member of the other group

Decision 3: you allocate money to another member of your group and to a member of the other group

The allocations you can choose from for each of these allocation decisions are depicted in the figure below: you see a circle with two axes: an x-axis and a y-axis. The two axes represent respectively the payment for the two participants:



for Decision 1, **x** represents yourself, **y** represents another member of your group;

for Decision 2, **x** represents yourself, **y** represents a member of the other group;

for Decision 3, **x** represents another member of your group, **y** represents a member of the other group.

The different points on the circle correspond to different payment allocations in Euro Cents. In all allocations, the first amount in brackets corresponds to payment for the participant represented by **x** and the second amount corresponds to payment for the participant represented by **y**. Thus, for the participant represented by **x**, the points on the right half of the circle corresponds to positive amounts of payment and the points on the left part of the circle corresponds to negative amounts of payment; for the participant represented by **y**, the points on the upper half of the circle corresponds to positive amount of payment and the points on the lower half of the circle corresponds to negative amounts of payment.

Payment:

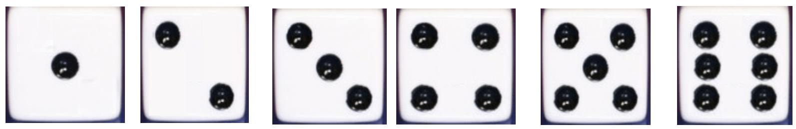
First, every participant will be endowed with 500 Euro cents. Second, as each of the 8 participants will make 3 decisions, there will be 24 decisions in total made by all participants. 1 out of 24 decisions will be randomly chosen to be implemented. For instance, if Decision 1 made by A is chosen, both A and one of A's group members B, C or D will receive the extra payment according to A's allocation decision; if Decision 2 made by B is chosen, both B and one of the other group's members H, I, J or K will receive the extra payment accordingly; if Decision 3 made by C is chosen, one of C's group members A, B or D and one of H, I, J or K will receive the extra payment accordingly. **You will not be informed about any decision made by others as well as the decision chosen to be implemented.** *Any questions?*

INSTRUCTIONS FOR EXPERIMENT 2

The second experiment consists of three different die-throwing games. In all the three games, you can earn points. All resulting points will be exchanged to Euros. Every point you earn is equivalent to 4 Eurocents.

Instructions for Die-throwing game (1):

We will explain the main idea of the experiment with a real die; in the actual experiment, however, the real die is replaced with a virtual die. Recall that a standard die has six sides and each side has a different number of dots. The pairs of numbers add up to 7 on the opposite sides: 1 vs. 6, 2 vs. 5 and 3 vs. 4, and vice versa. We call the visible side facing up the up side "U" and the opposite invisible side the down side "D". For instance, if the up side is 1, the down side of the die is 6 (See the table below for all the sides.)



U	1	2	3	4	5	6
D	6	5	4	3	2	1

There are 20 rounds in Game (1). Each round consists of one throw. Before throwing, you have to choose – in your mind - the relevant side for that round. In each round, the number of points that you earn depends on the throw of the die as well as on the side that you have chosen in that round. Note that the die outcomes are random and the outcome you see on the screen corresponds to the upside of the virtual die. In each of the 20 rounds, you are asked to **follow these steps one by one**:

Step 1: make a choice of side in your mind between "U" (Up) and "D" (Down).

Step 2: throw the virtual die by clicking on the "Throw Die" button on the screen.

Step 3: click on the side ("U" or "D") you had chosen in your mind on the screen.

On the basis of the die outcomes and the side you indicate in Step 3, your final earning points will be calculated automatically and shown to you after each round. For instance, if you have indicated "D" in Step 3 to be the side that you chose in mind and the die outcome turns up to be "4", you will earn 3 points for that throw, whereas if you have indicated "U" in Step 3 to be the side that you chose in mind, you will earn 4 points.

Instructions for Die-throwing game (2):

The second die-throwing game is similar to the previous die-throwing game except that there are two roles in this game: a scorer and a thrower.

- Thrower: someone who throws the die.
- Scorer: someone who decides the side of the die in his/her mind before the thrower throws the die and records the side for the resulting points that the thrower earns.

There are 2 scorers and 2 throwers in each group. There are 30 rounds in Game (2). The roles will remain fixed throughout the 30 rounds. Every scorer is first matched with a thrower from his own group for one shift of 15 rounds and then with a thrower from the other group for another shift of 15 rounds. (See the matching table below.)

Scorer	Thrower (First 15 rounds)	Thrower (Second 15 rounds)
A	C	J
B	D	K
H	J	C
I	K	D

In each of the 15 rounds, you are asked to follow these steps one by one:

Step 1: before the thrower throws the die, the scorer chooses in his/her mind between "U" (Up) and "D" (Down) and clicks on the "OK" button on the screen.

Step 2: the thrower throws the virtual die by clicking on the "throw die" button on the screen.

Step 3: both the thrower and the scorer see the die outcome. The scorer clicks on the side ("U" or "D") he/she had chosen in his/her mind on the screen for determining the thrower's points earned for that round.

On the basis of the die outcomes and the side the scorer indicated in Step 3, the thrower's earning points for that round will be calculated automatically and shown to both the scorer and the thrower.

Payment:

- Each thrower receives the total number of points of 30 rounds of throws according to the side came up by the scorers.

- The scorer receives 100 points for scoring the two throwers, i.e., 50 points for scoring each thrower.

Comprehension Test Questions (given right after the instructions of each part):

Experiment 1:

Suppose that the followings happen: (note that the choices in the example are chosen randomly and should not be seen as suggestions.)

- A chooses allocation (120, 480) for Decision 1, allocation (-480, -120) for Decision 2, allocation (-480, 120) for Decision 3.
- A's Decision 3 is chosen to be implemented and C and J are randomly chosen for receiving the extra payments.

Please calculate accordingly the total payment from Experiment 1 for each of the participants in this hypothetical case taking into account the 500 Euro cents endowment:

Total payment to A (in Euro cents):__500___

Total payment to B (in Euro cents):__500___

Total payment to C (in Euro cents):__20___

Total payment to D (in Euro cents):__500___

Total payment to H (in Euro cents):__500___

Total payment to I (in Euro cents):__500___

Total payment to J (in Euro cents):__620___

Total payment to K (in Euro cents):__500___

Experiment 2-DG1:

1. If you indicated side "D" to be the side chosen in your mind and the die outcome turns up to be "5", how many points do you earn?______2_______

2. If you indicated side "U" to be the side chosen in your mind and the die outcome turns up to be "6", how many points do you earn?______6_______

Experiment 2-DG2:

Note that the numbers in the examples below are meant to illustrate payment calculations and should not be interpreted as suggested or expected outcomes.

1. If you are assigned as a thrower, is it true that your score depends not only on your own luck through the die outcomes you throw, but also depends on the sides your scorers indicate in Step 3?___True_____

2. If you are assigned as a scorer, how many points will you earn in total if one thrower has earned 55 points for 15 rounds and the other thrower has earned 50 points for the other 15 rounds?____100____