Conveniently Upset:
Avoiding Altruism by Distorting Beliefs about Others' Altruism

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Abstract

In this paper we present the results from a "corruption game" (a dictator game modified so that, the recipient has the possibility of taking a side payment in exchange for accepting a reduction in the overall size of the pie). Dictators (silently) treated to have the possibility of taking a larger proportion of the recipient's tokens, took more of them. They were also more likely to report believing that the recipient had accepted the side payment (reducing the size of the pie). They also selected larger numbers as their best guess of the likely proportion of recipients acting "unfairly". The results favor the hypothesis that people avoid altruistic actions by distorting beliefs about others.

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"He who wants to kill his dog, accuses it of rabies"[1]

I. Introduction

Beliefs are central in many economic models. To give just one example, consider political economy and the seminal paper of Piketty (1995), where he showed that economic systems can heavily depend on the beliefs that people hold concerning labor market parameters (such as whether effort pays or not).[2] This literature uses the finding that individual beliefs are a strong predictor of a person's demand for redistribution (even more than income and education; see, for example, Fong, 2001, Corneo and Gruner, 2002 and Alesina and La Ferrara, 2002). Of particular interest are papers in this literature where people who believe others misbehave (e.g., are corrupt) also want to regulate and tax them (see Di Tella and MacCulloch, 2009, and Aghion, *et al.,* 2010). Given their potential importance, a natural question involves identifying the factors that affect the formation of beliefs.

Standard economics emphasizes the possibility that beliefs reflect reality following a (perhaps incomplete) learning process.[3] Previous work, however, has emphasized the possibility that beliefs systematically deviate from fundamentals. A classic example is Lerner (1982), who discusses how people systematically tend to believe in a just world, even in the presence of contradictory evidence.[4] Others have pointed out the possibility of self-serving beliefs (see Hastorf and Cantril, 1954; see Babcock and Loewenstein, 1997, for an interesting discussion).[5] In this paper we emphasize a particular form of self-serving beliefs, namely

[1] French proverb: "Qui veut noyer son chien l'accuse de la rage", famously appearing in Moliere's *Les Femmes Savants*.

[2] He focused on how differences in beliefs about the role of luck vs. effort in generating income could give rise to two different political equilibria, one with low taxes and widespread belief that effort pays (an "American equilibrium") and another with higher taxes and a prevailing belief in the importance of luck. See also Denzau and North (1994); see Bowles (1998) and Alesina and Glaeser (2004) for discussions.

[3] As in Piketty (1995) and Buera, *et al,* (2010). For related evidence, see Giuliano and Spilimbergo, (2010). For the role of cultural factors in preserving differences in beliefs even in similar environments see, for example, Bisin and Verdier, (2000) and Giuliano (2007).

[4] This possibility has been applied in political economy by Benabou and Tirole (2006), who observe that such distortion in beliefs could be a useful strategy to compensate for incomplete will power. See also Benabou (2008) on collective distortions of beliefs. A related class of models has focused on the direct consumption value of overoptimistic beliefs are studied for example in Brunnermeier and Parker, (2005). See also Caplin and Leahy (2001) on anticipatory feelings.

[5] A striking example is Babcock *et al.* (1996), who report that teacher contract negotiators in the US select "comparable" districts in a biased fashion and that this is correlated with strike activity. A key finding is that the bias has real consequences (it causes bargaining impasse) and that the bias results from selective evaluation of

beliefs that facilitate adopting a course of action that would be disliked under undistorted beliefs (like killing a dog that does not have rabies). In particular, we test the hypothesis that people avoid altruistic actions by distorting beliefs about others' altruism. Thus, our paper is particularly close to Rabin (1995), who emphasizes the possibility of distorting beliefs by selectively gathering evidence so as to relax internal moral constraints, or reduce cognitive dissonance, and to the work of Konow (2000) on self-serving views about what is fair and what is not.[6]

Our Study

Our approach involves studying beliefs in a modified "dictator game" (Forsythe *et al.*, 1994) which we call a "corruption game". After completing a set of tasks (which give agents an equal number of tokens), agents were randomly assigned to the role of dictator or recipient as in the standard game. The main modification relative to the dictator game consisted in having the recipient take an action at the same time the dictator was deciding on the allocation of tokens between them. This action was simply to decide on the value at which tokens (*both* those of the recipient and those of the dictator) would be "sold" to the experimenter and hence determine the payoffs to each side. Recipients chose the selling price for the tokens from a list that included two options. The first option was simply a high price. The second option was the combination of a lower price for selling the tokens to the experimenter but it also included a side payment only to the recipient. This is meant to mirror the classic corruption mechanism of under-invoicing a sale (as when a privatized company is sold for less than the market value in exchange for a bribe to the official in charge).[7] After dictators made their choice, they were asked two questions about the recipients. First, they were asked whether they believe that it is more likely that the recipient chose the side payment. Secondly, they were asked to guess the proportion of the population

information (see Babcock and Loewenstein, 1997). On the other hand, the survey of tithing practices amongst Mormons studied in Dahl and Ransom (1999) finds little evidence of the use of self-serving definitions of what constitutes income for tithing purposes.

6 Early work on cognitive dissonance includes Festinger (1957) and Akerlof and Dickens (1982). Piketty (1995) and others in the political economy literature referenced above involve a channel going from beliefs to taxation, whereas in this paper we study a channel going from the capacity to tax to beliefs.

7 The other classic mechanism is over-invoicing purchases by procurement officers. Obviously the words "corruption" or "bribe" do not enter our actual instructions. There is a small experimental literature on corruption that studies different aspects of the problem. A prominent example is the work of Abink, *et al,* (2002) using variations of the trust game (see Berg, *et al,* 1995). See Dusek, *et al,* (2004) for a review.

of recipients that accepted side payments, with an opportunity to get a substantial reward if they give the right answer.

Two treatments were used: dictators could take up to a large proportion of tokens from the recipient's pile (8 out of 10), or just a few tokens (2 out of 10). Dictators in one treatment were not made aware of the existence of another treatment. Recipients were not aware of the existence of either of those treatments. Our main hypothesis is that individuals who have the possibility of taking more in the dictator game will cause themselves to believe that recipients were more likely to take side payments. In other words, the paper's main hypothesis is that people can avoid altruistic actions by distorting beliefs about others' altruism.

Related Literature

A large experimental literature has studied games demonstrating how people fail to maximize their material payoffs. In a classic study of the dictator game, for example, Forsythe *et al.,* (1994) find that approximately 20 percent of proposers offered even splits. Significant sharing occurs even when full anonymity is ensured (see Hoffman *et al*, 1994 and Bolton, *et al*, 1998). The usual interpretation is that people have preferences that include the well-being of others, as in Kahneman, *et al.*, (1986).[8] One important finding in this literature is that decisions in dictator games are heavily influenced by the player's perceived "rights" to whatever sum is being distributed. In a classic demonstration of this effect, Hoffman and Spitzer (1985) and Hoffman *et al*, (1994) show that the distribution of payoffs is heavily affected by having players earn their roles. Similarly, Ruffle (1998), Cherry *et al,* (2002) and Oxoby and Spraggon (2008) find that players with legitimate property rights (having earned their positions by some means that is accepted by the other party) end up with larger shares. Beliefs also play an explicit role in theories of reciprocal altruism, where agents form a belief about other player's altruism so as to respond like with like (see Levine, 1998 and Rotemberg, 2005, 2008; see also the evidence in Ben-Ner, *et al,* 2004).[9]

[8] A vast literature studied different aspects of these preferences, including Rabin, (1993), Fehr and Schmidt, (1999), Bolton and Ockenfels, (2000), *inter alia.* Robustness across cultures is studied in Heinrich, *et al* (2001).

[9] A related result appears in the experimental study of the effect of democracy of Dal Bo *et al* (2010), who find that changing payoffs from those in a prisoner's dilemma game to those in a coordination game has a greater impact on behavior when the modification was implemented democratically than when it was imposed randomly.

Given the material cost of altruism, it is (perhaps) unsurprising that people sometimes develop strategies to avoid altruistic actions.[10] For example, Dana *et al,* (2007) present an experiment where people chose to remain ignorant of the consequences of their actions, even when this was costless, and went on to reduce other-regarding behavior.[11] In a similar spirit, Hamman *et al,* (2009) present results from sharing experiments where recipients receive significantly less when allocation decisions are made by agents rather than the principals themselves.[12] Our paper discusses another strategy for relaxing the fairness constraint on behavior: changing the beliefs about the recipient.[13]

Closer to our experiment in terms of the focus on self-serving biases is the work of Konow (2000), who shows that players can interpret fairness ideals in self-serving ways. He employs a clever double dictator setting, where dictators who are observed to make a certain favorable material allocation (and hence employ one fairness criterion) are observed to later use the same fairness criterion when asked to make an allocation decision in a similar setting but where his material utility plays no role. Our approach follows Konow (2000) in its focus on self-serving beliefs to reduce cognitive dissonance.[14] One difference from Konow (2000) is that we focus on beliefs about other player's actions instead of fairness ideals, making it straightforward to obtain a measure of beliefs by rewarding dictators for an accurate guess

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[10] The effectiveness of these strategies will obviously depend on the context and, in particular, the nature and ambiguity of the information available. The literature in psychology which has studied communication has showed how motivated reasoning is constrained by the extent to which reasonable justifications can be invoked (see, for example, Kunda, 1991), while work on elastic justification by Hsee (1996) showed that unjustifiable factors influenced decisions more when justifiable factors were more elastic. Schweitzer and Hsee (2002) present evidence suggesting that the reason private information constrains motivated communication is that people will eventually face excessive costs justifying (to themselves) extreme claims about inelastic information.

[11] For work where candidates for dictator prefer to opt out for a fixed fee that is lower than the dictator endowment see Dana, *et al,* 2006 and also Oberholzer-Gee and Eichenberger, 2008

[12] These findings emphasize some advantages of considering the expectation of fair behavior as a constraint on behavior (rather than a part of preferences), as in Rabin (1995) and Bolton, *et al* (1998).

[13] Our approach is similar to that in Dana *et al.,* (2007) in the use of ambiguity. A key difference of that paper from the usual dictator experiments in that it obfuscates the one to one mapping from dictator actions into outcomes (lack of "transparency", as denoted by Dana *et al.*, 2007). In our setting the recipient has an active role in the determination of active payoffs. We introduce a similar obfuscation, but concerning the recipient's actions (which could be termed as lack of "recipient-transparency" to distinguish it from the "moral wiggle room" scenario).

[14] When two cognitions (e.g. beliefs) are inconsistent, they are said to be "dissonant." In the case of the dictator game, the dissonant cognitions are the desire for material progress (keep the entire pie) and to be fair (with an "appropriate" split). People are naturally motivated to reduce dissonance and, according to social psychologists, often do so by distorting beliefs. In many settings people reduce dissonance by changing the way they learn. For example, in the studies of overconfidence of Köszegi (2006) and Benoit and Dubra (2009), agents stop collecting information, which contrasts to the approach we use here where the learning channel is closed.

concerning the actual actions of the players in the lab (see also the strategy in Babcock *et al.*, 1996).[15]

Section II introduces our experimental design in detail, while section III presents the results.

II. Experimental Design

Our conjecture is that people who can gain from taxing others will distort their belief in the direction of believing others are undeserving. This section presents a simple lab experiment designed to test this hypothesis. Intuitively, we will present a modified dictator game in which one player can take resources from the second player in a manner that is evocative of the practice of taxation. The key modification is that the second player can take an action that reduces the size of the overall pie in exchange for a side payment, in a manner evocative of the practice of hiding economic activity from the tax authorities in the shadow/underground economy (see, for example, Frey and Schneider, 2000). The first player is uncertain about which action was selected by the second player (i.e. if he was "corrupt" or not; obviously this terminology is used in the description in this paper but not in the experiment; the word corrupt does not appear anywhere in the experiment), which will make room for motivated beliefs (a strategic "offense" motive).

A Simple Model

We begin the model without motivated beliefs. There are two players: the "allocator" and the "seller" (denoted by subscripts 1 and 2). Their endowments are denoted x_i, and their consumptions c_i. The allocator can transfer an amount t from the seller, although $(1-\rho)\,t$ gets lost in the transference, where $\rho \in [0,1]$ represents the efficiency of the redistribution. "Intrinsic" utility from consumption is given by the function $U(c_1)$, with the standard properties $U'(\cdot) > 0$ and $U''(\cdot) < 0$. The allocator also cares about the utility of the seller. We

15 The approach is to elicit fairness ideals by having a proposer that is not a stake-holder, and therefore not susceptible to self-serving bias (see Konow, 2003). It should be noted that Cappelen *et al* (2010) present similar findings (involving the preponderance of the liberal egalitarian principle) in a study that studies the role of other fairness ideals and puts special attention on framing.

model fairness concerns in line with the theories on reciprocal altruism (e.g., Levine, 1998; Rotemberg, 2005). In the Appendix we provide a specification where fairness concerns are a function of outcomes (e.g. Fehr and Schmidt, 1999).

The total utility of the allocator is a weighted average of his own intrinsic utility and the intrinsic utility of the seller: $U_1 + \lambda(\lambda_2)\cdot U_2$. The weight $\lambda(.)$ is a function of the allocator's evaluation of the seller's unconditional altruism, denoted λ_2.[16] We assume $\lambda'(\cdot) > 0$, so there is an element of reciprocal altruism, so agents respond like with like. Also, assume that $\lambda(\cdot)$ is positive and concave. The problem of the allocator is then:

$$\max_{t\in[0,x_2]} U(x_1 + \rho t) + \lambda(\lambda_2) U(x_2 - t)$$

The comparative statics are straightforward. If the allocator believe that the seller is more altruistic (i.e. greater λ_2) then he will take less. A marginal increase in efficiency, ρ, will increase the net amount transferred from the seller, ρt, but it may lead to an increase or a decrease in t.

We can now introduce motivated beliefs. The allocator knows that the seller was randomly chosen from a population of sellers of two types: $\lambda_2 \in \{\lambda_2^L, \lambda_2^H\}$. The actual proportion of low type sellers (i.e. the least altruistic) is given by $p_0 \in [0,1]$. The allocator's belief about that proportion is given by $p \in [0,1]$. The allocator may hold a biased belief about p, but for that he/she has to pay a cognitive dissonance cost that is increasing in the size of the bias: $\mu \cdot C(p - p_0)$, with $\mu > 0$, and $C(\cdot)$ strictly convex and strictly increasing in $|p - p_0|$. If we let the parameter $\mu \to \infty$, that limiting case would represent the absence of motivated beliefs. There are many costs bundled in the last term. First, the more distorted the view that the individual holds, the more likely it is to find counterfactual evidence, which makes him uncomfortable. Second, in order to generate biases people have to systematically ignore information, which takes effort and time, and may eventually lead to sub-optimal choices in

[16] Given the dictator setting, an asymmetric formulation is sufficient to capture the relevant effects. In more general settings, reciprocal altruism involves $\lambda_i(\lambda_j)$ for $i=1,2$. (see, Levine 1998 and Rotemberg, 2005).

other aspects of life. The more someone wants to "deviate" from his prior belief (e.g. the objective estimate of the truth), the higher the psychological cost that he will have to pay.

The problem of the allocator becomes:

$$\max_{t\in[0,x_2],\,p\in[0,1]} U\left(x_1+\rho t\right)+\lambda\left(\lambda_2^L+p\left(\lambda_2^H-\lambda_2^L\right)\right)U\left(x_2-t\right)-\mu\cdot C\left(p-p_0\right)$$

Proposition 1: If the solution is interior and $\mu<\infty$, then an increase in ρ changes p^* and t^* in opposite directions.

<u>*Proof:*</u> take the FOC w.r.t. p for the interior solution: $\left(\lambda_2^H-\lambda_2^L\right)\lambda'\left(\lambda_2^L+p^*\left(\lambda_2^H-\lambda_2^L\right)\right)U\left(x_2-t^*\right)-\mu\cdot C'\left(p^*-p_0\right)=0$. And then differentiate w.r.t. ρ:

$$\frac{dp^*}{d\rho}=\frac{\left(\lambda_2^H-\lambda_2^L\right)\lambda'\left(\lambda_2^L+p^*\left(\lambda_2^H-\lambda_2^L\right)\right)U'\left(x_2-t^*\right)}{\left(\lambda_2^H-\lambda_2^L\right)^2\lambda''\left(\lambda_2^L+p^*\left(\lambda_2^H-\lambda_2^L\right)\right)U\left(x_2-t^*\right)-\mu\cdot C''\left(p^*-p_0\right)}\frac{dt^*}{d\rho},$$

where the denominator in the RHS is positive if the solution is interior, and the numerator is positive given the assumptions.

If the allocator decided to increase t^* after a reduction in ρ, then he must have also increased p^*. Intuitively, given that the allocator is taking more resources, he is willing to pay a cognitive dissonance cost to have more negative beliefs about the seller and therefore alleviate the "cost" of taking extra resources.

<u>Implementation</u>

The experiment was programmed and conducted in z-Tree (Fischbacher 1999). At the very beginning of the game each subject is asked to complete 5 tasks. Each task consists in finding a sequence of binary numbers in a larger sequence, as shown in Figure I. The average time to complete each task was around 1 minute. After completing the 5 tasks, each player is told that he has earned 10 tokens, and only then is allowed to continue with the rest of the experiment. The fact that subjects enter the game with the same number of tokens is not crucial for our purposes, but it does help making the interpretation of the results straightforward given that "entitlements" across subjects are similar. After working for the tokens, the subjects go through detailed instructions about how the game works and then face a set of questions about it that ensures that they have understood them, as well as the implications (for example, concerning anonymity).

Roles ("allocator" and "seller") are randomly assigned and each allocator is matched with a seller. Each pair of players has 20 tokens total. The allocator has to decide how to split the 20 tokens between himself and the corresponding seller. The seller has to "sell" the tokens to the experimenter. If the seller chooses "Option A" then the price is $2 (so *both* the seller and the allocator are paid $2 per token). If he chooses "Option B" then the price of the tokens is $1 (so *both* the seller and the allocator are paid $1 per token), but the seller gets an additional payment of $10 only for him. The game is simultaneous, so the seller does not know how the allocator split the 20 tokens when choosing A or B. And the allocator does not know whether the seller chose A or B when splitting the tokens. Note that a purely selfish seller without fairness concerns (and anticipating that allocators would not give away tokens) would always choose Option B, and a purely selfish allocator would always choose to take the 20 tokens for him.

We took many precautions to ensure that the subjects understood the rules of the game clearly. The subjects must complete a true-false questionnaire about the rules of the game. In order to give them incentives to pay close attention to the rules, they are told in advance that the questionnaire will pay extra money for each correct answer. There are four questions. In the first two, subjects were given hypothetical decisions for both allocator and seller, and they had to calculate the resulting payments for both players. In the first hypothetical situation the allocator keeps 10 tokens for himself and the seller chooses B. In the second situation the allocator keeps 19 tokens for himself and the seller chooses A. The answers of the subjects were correct over 70% of the time. The last two questions of the questionnaire were: i. The other players or the experimenter will be able to identify your decisions in the game; ii. Even though they do not know your name, the seller (allocator) knows how you split the tokens (whether you chose A or B) at the time of choosing A or B (splitting the tokens). The correct answer is False in both cases. The subjects were right 90% of the time.

After providing each answer in the questionnaire, all subjects were shown a screen indicating whether they had selected the right answer and a detailed explanation on how to get to the right answer (even if they had entered the right answer).

After they finished answering the questionnaire, allocators reached the stage where they have to split the 20 tokens. It is a screen with 20 circular yellow tokens, 10 on each side, as shown in Figure II. The allocator can transfer tokens among the two players with a click-drag-and-drop of the mouse. We introduced the efficiency of redistribution (i.e. ρ) as a step-function: when transferring a token from one player to another it conserves its entire value ($\rho = 1$), but a given allocator cannot move the first K tokens of each player (colored in green), which in practice is equivalent to $\rho = 0$. We randomized two treatments: $K \in \{2,8\}$.

The key aspect of our design is that the sellers are kept blind about *K*, which is clearly shown to the allocators. The allocators are told that the sellers do not know how the tokens are distributed before making its decision. Moreover, we showed to the allocators *all* the instructions given to the sellers, where there is no mention whatsoever about how the allocator can split the 20 tokens. As far as they are concerned, the sellers are expected to believe that $K = 0$. In the case of allocators, each one has knowledge of the treatment. In other words, those treated with a particular K (e.g., $K = 2$) are of course aware that *K* exists (i.e., those with $K = 2$ are aware they can move/take up to eight tokens), but they are not told whether other allocators in the lab face different values of *K*.[17]

Since the allocator is shown that the sellers expect $K = 0$, the "rational learning" hypothesis predicts that the allocator's belief concerning whether the sellers choose Option B should not depend on *K*. On the contrary, under the alternative hypothesis of motivated beliefs we should observe that those with $K = 2$ think that the sellers are relatively more likely to choose B. Intuitively, allocators with $K = 8$ can take at most a couple of tokens from the

[17] The setup could be viewed as mildly deceptive only in as far as it does not provide information that might be considered relevant to some participants (concerning the existence of the two treatments). Alternatively, we could have told everyone (i.e. both allocators and sellers) that *K* was going to be randomly chosen to be either 2 or 4. We saw two serious limitations in that setup. First of all, it would make the game significantly more complicated to understand for the players. Secondly, the random determination of *K* may change the conception of what is fair for a given *K* (e.g. Dana *et al.*, 2007), which can have an ambiguous impact on the predictions of the model.

seller, so they do not have much incentives to hold negative beliefs about the sellers. But allocators with $K = 2$ can take many tokens from the seller, which gives them greater incentives to hold negative beliefs about the sellers and then justify (from a "fairness" perspective) their behavior. This is exactly what the main Proposition of our simplified model predicts. And given that subjects are randomly assigned to the two treatments, we can use a simple test for the equality of means.

We retrieve two measures of the allocators' beliefs (about how the sellers behaved). First, before making the payment (an instance when the allocator can infer with certainty the seller's actions), we ask the allocator whether he thinks that the seller to which he was matched chose Option A or B. We denote this variable *Is Corrupt*. There is no monetary reward for guessing correctly. The allocator is asked to briefly explain his answer anonymously with paper and pencil. By asking him to explain his choice we ensure that he has the opportunity to think in more detail about the topic and about the seller. In the following screen, the allocator is given a bonus question. The allocator is told that he will be awarded 5 dollars if his answer is correct, which is meant to elicit truth-telling. The size of the reward is substantial, slightly over 60% of the average final payments received during the experiment. The question asks: "What percentage of sellers playing today in the lab chose Option B?" The possible answers are the ten categories: "0-10% (0.1); 10-20% (0.2); …; 90-100% (1)" (note in parenthesis the number that value that we assign to each option). We denote this variable *Are Corrupt*.[18]

III. Experimental Results

The experiment took place in *Universidad de San Andres*, a private university in Argentina. Participants were randomly drawn from a database of hundreds of college students who declared to be interested in participating in experiments. Students were not informed of the content or purpose of the experiment, only that the experiment took place in front of a computer, that the participants were asked to perform simple tasks, that the decisions in the

[18] We believe the reward (5 dollars) is large enough to ensure that subjects report honestly. One alternative could be to introduce a later stage where allocators are given the opportunity to punish the sellers by "burning" some of their tokens (at a cost).

experiment were anonymous, and that they could earn some money in return. Most of the students belong to families in the highest decile of the income distribution of Argentina. They are presumably to the center-right of the political spectrum in Argentina, although we do not have direct data on this.

The subjects entered the laboratory in groups of 16. They were randomly assigned to the role of allocator and the role of seller, and matched in pairs. Before starting play, subjects had to read and sign a standard agreement. They were informed that the experimenters were not going to deceive them in any way, that their actions were anonymous, and that their decisions were actually going to affect their payments and those of their circumstantial partners. They were again reminded about these rules at the start of the experiment through a set of on-screen instructions. The subjects earned on average a little fewer than 10 dollars, and the time spent since they entered the laboratory until they left was around 30 minutes. The stakes were reasonable: e.g. with 10 dollars you can buy 3 lunches at the university cafeteria. All the subjects reported that they would like to be called for a future experiment, and most of them gave a relatively high rating to the experience on a scale from 1 (unpleasant) to 5 (pleasant), with an average of 3.5.

We employed a total of 64 subjects (i.e. 32 allocators and 32 sellers). Even though the sample size is modest, it proved to be large enough to statistically identify the effect of interest. Unfortunately, we had to discard two observations. At the end of the game the subjects were asked on the computer screen whether they understood the rules of the game, and one of the subjects declared not to have understood the rules. That is the first of the two observations discarded. As a confirmation, we note that this subject spent over 15 minutes solving the last of the initial five tasks (we recorded the time that each subject spent on each screen of the experiment). Therefore, we conclude that he did not understand the rules not because they were difficult to understand, but merely because he had to rush over the rest of the game to compensate for the time lost. We discarded the second observation because in the questionnaire he declared that he was not a student from the university. Including these two observations does not alter any of the results below.

After finishing the game all the subjects were invited to participate in an on-screen paid survey. Every subject accepted to participate. In that survey we obtained information about some basic demographic and socio-economic characteristics, values and beliefs. The choices made by the sellers are not relevant for the test that we are interested in. Note simply that 75% of sellers choose Option B (low price + side payment) over Option A.

The data definitions of the variables used in the paper appear in Table A, and their corresponding descriptive statistics in Table B. Figure 1 shows the distribution of tokens taken by the allocator, by treatment group. Figure 2 shows the distribution of beliefs about the seller, also by treatment group. As a mild consistency check on the interpretations offered in this paper (based on the concept of fairness), after taking its decision, but prior to learning the decision of its partner, we asked each subject: "How morally satisfied are you with your choices in the experiment?" The possible answers range from very satisfied (1) to very unsatisfied (5). Consistent with the view giving importance to fairness concerns, taking more tokens is negatively correlated with the self-reported "moral satisfaction" of the allocators, and choosing Option B over Option A is negatively correlated to the declared moral satisfaction of the sellers.[19]

In Table I we compare the mean values of key variables across allocators randomly treated with *K=2* and with *K=8*. Note that the differences are not statistically significant for variables like gender, age and father's education, so the treatment is balanced across those dimensions. The allocators with *K=8* took on average 1.5 tokens from the sellers, while the allocators with *K=2* took on average 6 tokens. As can be seen from Table I, the difference is statistically significant at the 1% level. We can now test the main Proposition, which predicts that the allocators treated with *K=2* should have more negative beliefs about the sellers. Under the null hypothesis of no self-serving biases, the beliefs *Is Corrupt* and *Are Corrupt* should be independent from *K*. As can be seen from Table I, the standard mean difference tests reject the null hypotheses that beliefs are independent from *K* with p-values of 0.019 and 0.078, respectively. As predicted by the Proposition, the allocators with *K=2* have a belief *Is Corrupt* that is 0.4 (i.e. 40 percentage points) greater, and a belief *Are Corrupt* that is

[19] See Konow (2010) who combines experimental outcomes with reports on emotions to test theories of giving.

greater by 0.2 (which would be roughly equivalent to 20 percentage points) than those allocators with *K=8*.

We present the regression-equivalent of the mean difference tests in columns (1) and (3) of Table III, where we regress the beliefs about the sellers on a dummy variable that takes the value one if $K = 2$ and zero otherwise. For the belief *Is Corrupt* we use a probit model, and we report directly the marginal effects at the mean of the independent variables. For the dependent variable *Are Corrupt* we use OLS. Once again, we find that that the allocators with *K=2* have worse beliefs about the sellers, where the difference is both statistically and economically significant. In order to assess the robustness of the results, in columns (2) and (4) we add a basic set of control variables: gender, age, socio-economic class and father's education. The results are unaltered, which is always true if we use different sets of control variables.

Finally, there is an alternative (more direct) way to test the Proposition. Note that the Proposition predicts that a treatment (i.e. *K*) is expected to affect the allocator's beliefs about the seller if and only if it affects the number of tokens taken by the allocator. In other words, an allocator will not bother in creating biased views about the sellers if there is no bad behavior to justify. As can be seen from Table II, a 20% of the allocators with $K = 2$ took 2 or less tokens. If they had been treated with K=8 instead, by construction they would also have had to take 2 or less tokens. Since those allocators do not need to justify further redistribution when treated with *K=2* rather than *K=8*, we should not expect K to affect their beliefs. Indeed, we cannot reject the hypothesis that the average beliefs of the allocators with *K=2* that stole 2 or less tokens are equal to the beliefs of the allocators with *K=8*. But we do see significant differences between the beliefs of the allocators with *K=2* that stole more than 2 tokens and those allocators with *K=8*.

We can provide an alternative, more direct, test of the Proposition. Re-arrange the expression in the Proof of the Proposition*:*

$$\frac{\frac{dp^*}{d\rho}}{\frac{dt^*}{d\rho}} = \frac{\left(\lambda_2^H - \lambda_2^L\right)\lambda'\left(\lambda_2^L + p^*\left(\lambda_2^H - \lambda_2^L\right)\right)U'\left(x_2 - t^*\right)}{\left(\lambda_2^H - \lambda_2^L\right)^2 \lambda''\left(\lambda_2^L + p^*\left(\lambda_2^H - \lambda_2^L\right)\right)U\left(x_2 - t^*\right) - \mu \cdot C''\left(p^* - p_0\right)} = \delta$$

Where δ is zero if $\mu \to \infty$ (i.e. absence of self-serving biases) and positive otherwise. The null hypothesis $\delta = 0$ gives a more direct test of the Proposition. It is straightforward to implement this test, exploiting the exogenous variation in ρ that we generated in the experiment. Note that the ratio in the LHS resembles the well-known Wald estimator. We can recover δ by simply running a regression of the beliefs about the sellers on the number of tokens taken from the seller, using the dummy $K = 2$ as instrumental variable. The results are shown in columns (1) and (3) of Table IV for the beliefs *Is Corrupt* and *Are Corrupt*. As expected, we reject the null hypothesis of $\delta = 0$ at the 1% and 10% levels of significance, respectively. In columns (2) and (4) we reproduce the regressions but adding control variables, and the results remain the same.

IV. Conclusions

A natural interpretation of positive offers in dictator games is that people have "fairness" concerns. This is reinforced by variations in sharing induced by having some players "earn" their endowments. Recent work has identified circumstances in which agents try to avoid information that would allow them to know if they are being fair (Dana, et al, 2007) and distort their beliefs about what fairness ideals to apply (Konow, 2000). In this paper we introduce a "corruption game" which is just a modified dictator game where recipients take an action that can reduce the size of the overall pie in exchange for a side payment. We then use it to explore another strategy that would allow dictators to keep a higher share of the pie: avoiding altruism by distorting beliefs about others' altruism.

Our set up is a standard anonymous dictator game with three modifications. The first modification just involved an initial stage where the players had to earn 10 tokens each. Secondly, we randomized the number of tokens that the dictator was able to control (blind to the sellers). The last modification is significative: simultaneously with the dictator's allocation decision, we allowed the recipient to set the unique "selling price" for the tokens of both players. The recipient was offered to accept a lower price in exchange for a side payment just for himself. We designed this feature with the hope of mirroring the action of "under invoicing" that is typical in many corruption settings. We then measured the dictators' beliefs about whether his recipient and other recipients in the lab took the side payments (offering rewards for accurate answers as to elicit truth-telling).

As expected, dictators that take lots of tokens from the recipient report believing that sellers accept side payments. The question is whether some variation in those beliefs responds to a self-serving nature: i.e. whether the dictators are conveniently upset in order to keep a greater share of the pie. To test this hypothesis, we randomized the number of tokens that the dictator is allowed to take from the recipient's pile: 2 out of 10 tokens (i.e. low stakes), or 8 out of 10 tokens (i.e. high stakes). The recipients were unaware of the treatments. Our main finding is that dictators in the high stakes scenario, who had more incentives to hold negative beliefs about the sellers, were indeed significantly more likely to believe that the sellers would accept side payments.

Appendix: A Simple Model of Fairness over Outcomes

We start without motivated beliefs. The players are the allocator and the seller, just like before (denoted by subscripts 1 and 2). Their "actual" endowments, "deserved" endowments and consumptions are given by x_i, e_i and c_i, respectively. Intuitively, the "deserved" endowments are those that would arise in a perfectly fair world. The allocator can transfer an amount t from the seller, although $(1-\rho)t$ gets lost in the transference, where $\rho \in [0,1]$ represents the efficiency of the redistribution. The first component of the allocator's utility function is given by his consumption utility, $U(c_1)$, which we assume to be linear and whose coefficient we normalize to one: $U(c_1) = c_1$. The second component of the allocator's utility is given by his moral sentiments, $\Omega(c,e)$:

$$\Omega(c,e) = -\frac{\alpha}{2}\left((c_1 - e_1)^2 + (c_2 - e_2)^2\right)$$

We assume $\alpha > 0$. Note that the moral cost for the allocator is higher whenever someone consumes something different from what he deserves. A higher α would represent that the allocator is more concerned about justice. For the sake of simplicity we assume $x_1 = e_1$ and $x_2 = e_2$. If this was the whole story, the allocator would solve a trade-off between the marginal utility from consumption and the marginal moral cost from doing something unfair:

$$\max_{t \in [0, x_2]} x_1 + \rho t - \frac{\alpha}{2}\left(\rho^2 t^2 + t^2\right)$$

From the FOC we obtain the interior solution:

$$t^* = \frac{\rho}{\alpha(1+\rho^2)}$$

The comparative statics for α and ρ are very intuitive: i.e. if the individual cares more about justice then he will take less, and if redistribution is more efficient then he will take more.

Now we can introduce motivated beliefs. In a first stage the seller has to choose whether to "betray" the allocator or not. However, the allocator does not know whether the seller betrayed him or not, at least not until after he chose t. A given allocator may be in one of two states of the world, denoted by superscripts 1 and 2, but he does not know in which state he is. In both states the allocator got what he deserved: $x_1^2 = e_1^2$. In the first state ("just world") the seller got exactly what he deserved, $c_2^1 = e_2^1$, and in the second state (is corrupt or "robber baron") he got more than he deserved: $x_2^2 - e_2^2 = \theta > 0$. The perceived probability of being in the latter (former) state is p $(1-p)$. The actual probability is $p_0 \in [0,1]$. The allocator can change his perception of that probability by incurring a "cognitive dissonance cost": $\mu \cdot (p - p_0)^2$, with $\mu > 0$.

In this new situation, apart from choosing how much to take (t) the allocator has to choose simultaneously how much to distort its perception of p:

$$\max_{t\in[0,x_2],\, p\in[0,1]} c_1 - \frac{\alpha}{2}\left((c_1 - e_1)^2 + (1-p)(c_2 - e_2^1)^2 + p(c_2 - e_2^2)^2\right) - \mu(p - p_0)^2$$
$$s.t. \quad c_1 = x_1 + \rho t \quad and \quad c_2 = x_2 - t$$

We are interested in the following comparative static:

Proposition 1a: If the solution is interior and $\mu < \infty$, then an increase in ρ changes p^* and t^* in the same direction.

<u>Proof</u>: take the FOC w.r.t. p *and differentiate w.r.t.* ρ : $\frac{dp^*}{d\rho} = \frac{\alpha\theta}{2\mu}\frac{dt^*}{d\rho}$.

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Figure I: A sample task (one of five)

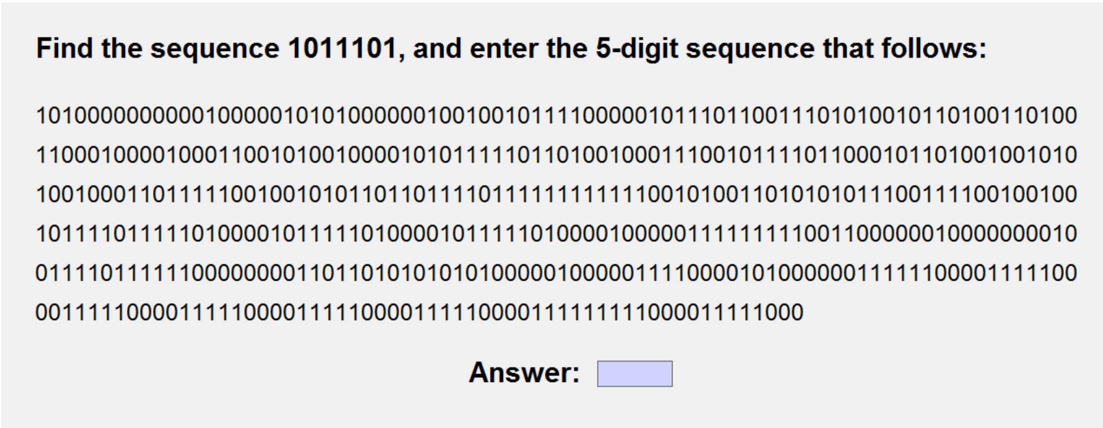

Figure II: Screen faced by an allocator when asked to decide on the split (for *K=2*)

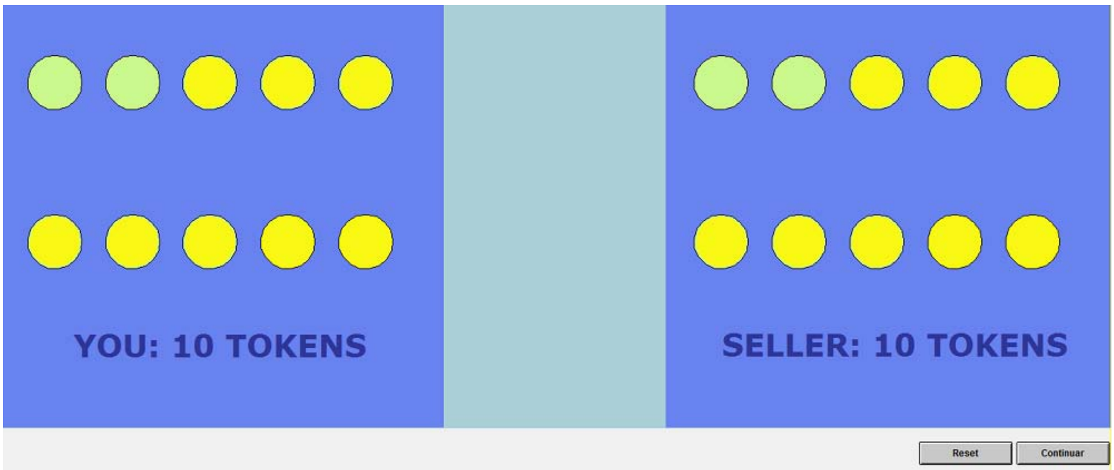

Figure III: Distribution of tokens taken by the allocator

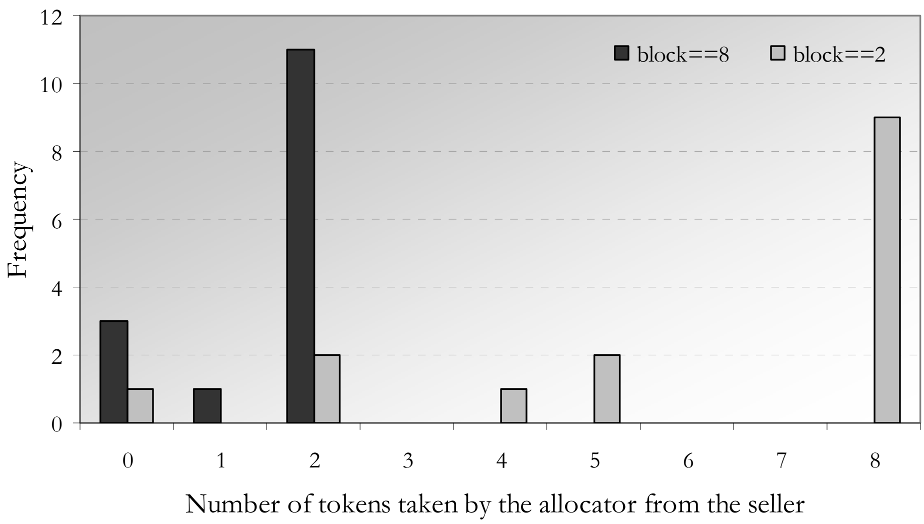

Figure IV: Allocators' beliefs about the sellers

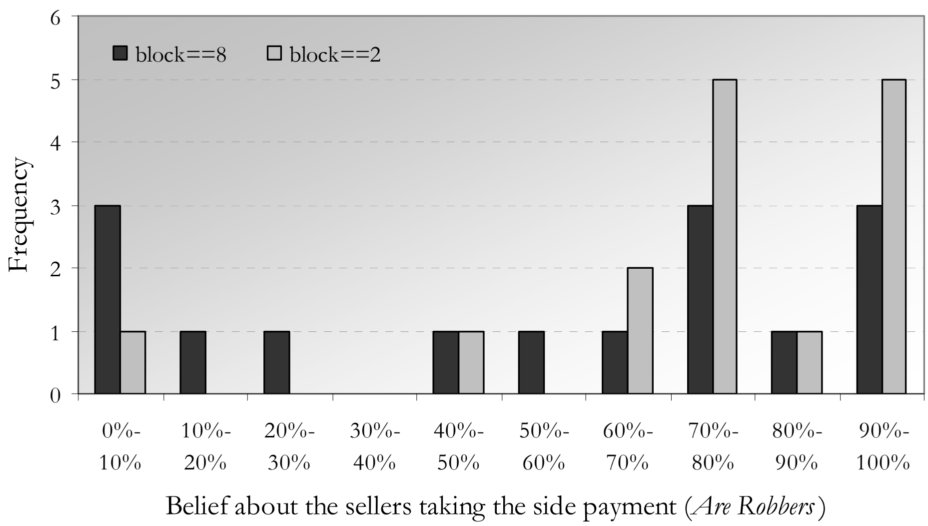

Table A: Data definitions

Name	Definition
Is Corrupt	Dummy variable that takes value 0 if the allocator guessed that his corresponding seller chose Option A, and 1 for Option B.
Are Corrupt	"What percentage of sellers playing today in the lab chose Option B? 0-10% (0.1); 10-20% (0.2); …; 90-100% (1)"
Block == 2	Dummy variable that takes value 1 if the allocator faced 2 blocked tokens, and 0 if he faced 8 blocked tokens.
Tokens Taken	Number of tokens taken from the seller by the allocator.
Socio-economic class	"What is the socio-economic class of your family? Lower class (1); Middle-lower class (2); Middle class (3); Middle-higher class (4); Higher class (5)"
Father's education	"What is the educational level of your father? Primary incomplete (1); Primary complete (2); Secondary incomplete (3); Secondary complete (4); Undergraduate incomplete (5); Undergraduate complete (6); Graduate (7)"

Table B: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Is Corrupt	30	0.67	0.48	0	1
Are Corrupt	30	0.69	0.31	0.1	1
Block == 2	30	0.50	0.51	0	1
Tokens Taken	30	3.77	3.05	0	8
Socio-economic class	30	3.50	0.63	2	5
Father's education	30	5.73	0.94	3	7

Notes: observations corresponding to the allocators.

Table I

	block==8		block==2		p-value difference*
	mean	std	mean	std	
Tokens Taken	1.53	0.83	6.00	2.80	<0.01
Is Robber	0.47	0.52	0.87	0.35	0.02
Are Robbers	0.59	0.35	0.79	0.24	0.08
Gender	0.47	0.52	0.60	0.51	0.48
Age	21.07	1.28	21.07	2.89	1.00
Father's education	5.93	0.88	5.53	0.99	0.25
Observations	15		15		30

* P-value of the standard mean difference test whose null hypothesis is that the mean of the distributions under block==2 and block==8 are equal. Observations corresponding to the allocators only.

Table II

Belief: *Are Robber**

		Took more than two tokens?		
		No	**Yes**	**All**
Block	**2**	0.67 (0.15) [3]	0.83 (0.25) [12]	0.79 (0.24) [15]
	8	0.59 (0.35) [15]		0.59 (0.35) [15]
	All	0.61 0.322622 [18]	0.83 (0.25) [12]	0.69 (0.31) [30]

Belief: *Is Robber**

		Took more than two tokens?		
		No	**Yes**	**All**
Block	**2**	0.33 (0.58) [3]	1.00 (0) [12]	0.87 (0.35) [15]
	8	0.47 (0.52) [15]		0.47 (0.52) [15]
	All	0.44 (0.51) [18]	1.00 (0) [12]	0.67 (0.48) [30]

* The cells contain the mean beliefs in each subgroup, with the corresponding standard errors (number of observations) in parenthesis (brackets). Observations corresponding to the allocators only.

Table III

	(1)	(2)	(3)	(4)
	Probit[1]		OLS	
Dep. Var.:	*Is Corrupt*		*Are Corrupt*	
Block == 2	0.400**	0.428***	0.200*	0.217*
	(0.159)	(0.156)	(0.110)	(0.121)
Controls[2]	no	yes	no	yes
Observations	30	30	30	30

Notes: Heteroskedastic-robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%; [1] We report the marginal effect at the mean independent variables instead of the raw coefficients; [2] The control variables are: gender, age, socio-economic class and father's education. Observations corresponding to the allocators only.

Table IV

	(1)	(2)	(3)	(4)
	IV Probit[1]		2SLS	
Dep. Var.:	*Is Corrupt*		*Are Corrupt*	
Second Stage:				
Tokens Taken	0.586***	0.970**	0.045*	0.052*
	(0.200)	(0.480)	(0.024)	(0.028)
First Stage:				
Block == 2	4.467***	4.194***	4.467***	4.194***
	(0.742)	(0.530)	(0.755)	(0.627)
Controls[2]	no	yes	no	yes
Observations	30	30	30	30

Notes: Heteroskedastic-robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%; [1] We report the marginal effect at the mean independent variables instead of the raw coefficients; [2] The control variables are: gender, age, socio-economic class and father's education. Observations corresponding to the allocators only.