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Voting on a sharing norm in a dictator game

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Abstract

I conduct an experiment to assess whether majority voting on a non-binding sharing norm affects subsequent behavior in a dictator game. In a baseline treatment, subjects play a one shot dictator game. In a voting treatment, subjects are first placed behind a 'veil of ignorance' and vote on the amount that those chosen to be dictators 'should' give. The outcome of the vote is referred to as a 'non-binding agreement.' The results show that a norm established in this fashion does not induce more 'fairness' on the part of those subsequently chosen to be dictators. In fact, dictators were significantly more likely to offer nothing under the treatment. I outline a simple model to account for this 'crowding out' effect of a norm that may demand 'too much' of some subjects.

Keywords: Dictator game, communication, voting, promises, agreements, behavioral economics, guilt aversion, reciprocity, fairness, obligations
JEL codes: C91, C92, D63, D64, D70

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1 Introduction

The large and growing literature in experimental economics suggests that human behavior in social interaction is not well described by the 'standard' model assuming (narrowly) self interested motivations. In particular, many experimental subjects cooperate in social dilemmas and divide 'fairly' when faced with allocation tasks. Since such behaviors persist even when there are no reputation concerns and games are finitely repeated, they clearly violate the assumptions of self interest and rationality as conventionally interpreted.

Several alternative models have been proposed to account for these phenomena. These 'behavioral' models emphasize three types of factors which may enter an individual's decision calculus in a social interaction. The first is the *anticipated outcome* (i.e. the distribution of payoffs) resulting from an action. The second is a consideration of the *intentions* attributed to other players. The third is the perception of *obligations* that exist between the parties to an interaction (based, e.g., on norms or agreements).

Models of *interpersonal preferences* focus on the first of these factors, i.e. on the notion that players prefer certain types of *outcomes*. In particular, these models posit a direct concern for the *distribution* of payoffs achieved by the players involved in a given interaction (as opposed to a concern only for the choosing individual's payoff. See Andreoni 1990, Levine 1998, Fehr and Schmidt 1999, Bolton and Ockenfels 2000.)

Models of *reciprocity* also assume that players evaluate actions based on their anticipated outcomes, but they also emphasize the second factor, namely the importance of *intentions* attributed to other players. In these models, a player's preference over outcomes depends on the intentions she attributes to others. In particular, the weight that a player places on another individual's final payoff is assumed to depend on how 'kind' his or her intentions are perceived to be (Rabin 1993, Dufwenberg and Kirchsteiger 1999, Falk and Fischbacher 2001).

An important property shared by both of these approaches is that they make no explicit reference to the concept of "obligations." That is, the motivation to seek out "equitable" outcomes or to reciprocate "kind" behavior (defined as actions *intended* to achieve equitable outcomes) is assumed to exist independently of any social norms or contractual agreements.[1]

This feature makes it difficult to reconcile these theories with evidence indicating that pro-social motivations are influenced by pre-play cheap talk communication. A number of experiments, on social dilemmas (including trust games, public goods games, and common pool resource games) show that cheap talk communication has a strong impact on pro-social behavior (Bornstein et al 1989, Brosig et al 2001, Charness and Dufwenberg 2006, Dawes et al 1977, Ellingsen and Johannesson 2004, Kerr and Kaufmann-Gilliland 1991, Orbell et al 1988, Ostrom et al 1992). Indeed, as emphasized by Brosig et al (2001), "one of the few variables that is known to have a robust and strong positive effect on the level of cooperation is the opportunity to communicate" (see also: Sally 1995, Walker and Ostrom 2007).

As a typical example, Orbell et al (1988) conduct a public good experiment in which subjects were given the opportunity to engage in group discussion prior to interacting. One of their main results is that contributions were significantly higher in groups where promises had been exchanged between subjects. In an experiment involving a trust game, Charness and Dufwenberg (2006) allow "trustees" to send a message to "trusters" prior to interaction. Their results show that promises on the part of trustees increases trust by the first mover and trust-worthiness by the second mover.[2]

[1] Although the principle of reciprocity itself is sometimes referred to as a norm, an implicit assumption in all formalizations suggested to date is that the "kindness" one player attributes to another depends on whether her actions are perceived to be aimed at an equitable outcome, not on their compatibility with existing norms or contracts.

[2] Since individual motivations in models of interpersonal preferences and reciprocity depend on (higher order) beliefs about behavior, one way to explain the effects of pre-play communication

Charness and Dufwenberg (2006) propose a theory of "guilt aversion" to account for these effects of pre-play communication. In Battigalli and Dufwenberg's (2007) formalization of the concept, the experience of guilt is assumed to results when one individual believes that she has "let down" another. They define letting down as follows: "player i lets player j down if as a result of i's choice j *gets a lower monetary payoff than j expected to get* before play started" (emphasis added).

Although this theory makes no explicit reference to the *source* of player j's expectations, it provides a simple and natural account for why player i might perceive an *obligation* to abide by social norms and contracts. As the authors put it, "there is a norm, it shapes [one subject's] expectations, and [another subject] lives up to this expectation because he would feel guilty if he did not." Likewise, If i makes a promise to j (say, to cooperate in a PD), i may believe that j expects her to keep that promise. This (second order) belief may cause her to experience guilt if she were to break her promise.

An interesting feature of this theory is that the effect of norms and contracts on behavior is *indirect*. That is, it is not the promise *per se* that motivates player i to cooperate with j, but rather her (second order) belief about the payoff that player j expects to receive. Thus, the impact of a promise on subsequent behavior is explained by a resulting change in second order beliefs. It follows from this that *for i to keep her promise to j, i must believe that j expects her to keep her promise.*

However, the theory of guilt aversion does not provide an answer to the question *why* a promise should affect beliefs in this way. I.e. why do players not regard a promise

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would be to assume that it merely changes these beliefs. However, this approach often leads to counter-intuitive and empirically false results. As an example, consider the trust game studied by Charness and Dufwenberg (2006). When the "truster" sends money to the "trustee," reciprocity predicts that the "trustee" will send back money if she perceives trust as "kind," i.e. as equality-seeking self-sacrifice. Now suppose that, prior to play, the trustee *promises* that she will send back money if trusted. Suddenly, "trust" becomes a *selfish* action which should be reciprocated by selfishness. We thus arrive at the counter-intuitive (and false) conclusion that pre-play communication should lead to less trustworthiness on the part of second movers.

as cheap talk, and expect others to think the same way? Charness and Dufwenberg explicitly state that their theory does not rely on a "fixed cost of lying." But unless we assume that i has a preference for not lying (or believes that j believes she has such a preference), guilt aversion alone cannot account for the observed effect of a promise.

As this discussion shows, the concept of guilt aversion provides a promising avenue for understanding the effects of pre-play communication and agreements on behavior. However, an open question remains. Namely, under what conditions are agreements "taken seriously" by the players involved? I.e. when will agreements actually affect the beliefs and behavior of players? In what way might this depend on properties of the game and on the method by which agreement is reached?

It would appear that these are essentially *empirical* questions. Future development of the theory should be based on empirical investigations into the effects of communication in different contexts, as well as different mechanisms for producing agreements. The current paper seeks to investigate these types of questions. In particular, I want to investigate the following two issues.

(1) Most of the evidence on pre-play communication comes from studies on social dilemmas and trust games. Communication effects in these contexts are open to several different interpretations. In a public goods game, communication may affect player i's behavior because it affects her first order beliefs about whether others intend to contribute, rather than her second order beliefs about what others expect of player i. In the trust game, the second mover receives a clear signal revealing that the other player trusts her to keep a promise. *Does the effectiveness of pre-play communication extend to a dictator game, where only one person is called upon to be "fair," and other players have no opportunity to demonstrate trust?*

(2) Existing studies on pre-play communication have usually allowed subjects to communicate verbally and explicitly exchange promises.[3] Do other methods of reaching agreement lead to similar affects on behavior in subsequent interaction? Under what conditions will players regard an agreement as binding, and believe that others do so as well? This question is particularly important in situations where agreement must be reached by more than two persons. In such contexts, decisions are often reached through formal processes such as majority voting. *Can a formal procedure such as majority voting create an effective perception of obligation?*

In order to address these issues, the experiment reported on here explores whether behavior in a one shot dictator game is affected when, prior to interaction, subjects vote on a non-binding sharing norm "behind a veil of ignorance."

The dictator game setting removes all coordination aspects of agreement. Since only one person makes a decision, there is no strategic uncertainty. As a consequence, the anticipated *outcome* generated by a given choice cannot be affected by an agreement. Therefore, the only reason to live up to such an agreement is that one feels *obligated* to do so. Therefore, this setting is capable of testing whether the communication procedure applied influences players' perceived obligations, and (if so) whether those obligations then affect behavior.

Perhaps the main innovation of the present study is to study the effectiveness of a formal procedure by which agreement is to be reached. One of the most common mechanisms for collective agreement in real-world situations is majority voting. Therefore, this seems like a natural starting point for an empirical investigation into different techniques for creating agreements.

[3]Verbal communication comes in many different variants, including face-to-face discussions, anonymous chatting, one-way messages, and audio or video conferences. Other studies remove the opportunity to communicate and instead allow only for visual identification. See Brosig et al (2003) for a study comparing the associated effects in the context of a VCM.

Finally, the design of the experiment is based on the idea that participants may agree on a norm of sharing when placed behind a "veil of ignorance." Thus, voting takes place before subjects are told whether they will be the dictator or the recipient in the subsequent interaction.

The hypothesis to be tested is clear: given majority agreement on a norm of sharing (say, 50-50), would dictators in the subsequent interaction be more likely to distribute in the norm-prescribed fashion than under a no-vote baseline?

Surprisingly, the results of the experiment point in the *opposite* direction. Specifically, offers in the voting treatment were somewhat lower, and a significantly higher proportion of dictators offered zero, as predicted by the standard benchmark of (narrowly) selfish behavior.

In order to explain this result, I construct a simple model that predicts a 'crowding out' effect that might arise among subjects normally willing to abide by a 'modest' sharing norm. The intuition is that a more demanding norm, established by the group, will eliminate the 'warm glow' payoff that such a subject experiences from giving a moderate amount. Given this, sharing is no longer worth is, causing the subject to give nothing.

Naturally, the explanation offered is 'ad hoc,' since it was constructed to account for the observations obtained after the fact. However, I believe that the suggested effect may be worth testing in future experiments. If verified by other experiments, it would have important implications for the effects of explicit rules on behavior. Specifically, it would imply that relatively 'demanding' rules may affect individuals differently: those who would normally follow a slightly less demanding rule may increase their level of pro-social behavior, while those normally willing to abide by significantly less demanding rules may actually reduce their level of pro-social behavior.

The next section presents the experimental design. Section 3 describes the results. Section 4 discusses and offers an interpretation of the results in terms of a "crowding out" effect. Section 5 presents a simple formalization of this interpretation. Section 6 concludes. The Appendix contains a translation of instructions.

2 Experimental design

Baseline treatment

Subjects entered the laboratory and were randomly assigned to an isolated terminal. Instructions (reproduced in the appendix) were handed out and read aloud by the experimenter (myself). The instructions were the same for all subjects and informed them that they would be randomly matched with another subject, that one of them would then be randomly chosen as to be 'subject A,' and that this subject would then have the opportunity to choose an integer-valued distribution of 10 EUR between the two subjects. They were informed that their payment at the end of the session would consist of a 5 EUR show-up fee plus the payoff from the experiment. Questions were answered privately at the subjects' seat (again, by myself).

Subjects A then saw a screen displaying a table of all possible integer distributions of 10 EUR. While they made their choices, subjects B saw the same table and were asked to report a hypothetical choice, had they been subject A. After both had made their choices, subjects A were asked what they thought B probably expected to receive. Subjects B were asked what they expected to receive. After all choices had been submitted, all subjects were paid privately at their seats, with cash placed in envelopes. This procedure guaranteed that no subject would witness other subjects being paid. Subjects were informed of this procedure in the instructions.

Voting treatment

The voting treatment was exactly like the baseline treatment except for the following features. Instructions for the dictator game were marked 'Instructions for phase 2.' These were passed out first and read out loud, as before. In contrast to the baseline treatment, the instructions contained a sentence asking subjects to think about 'what subject A should do' in the situation described. Questions were answered

before the instructions for phase 1 were handed out. These described the voting procedure. Subjects were told that they would have the opportunity to vote on a 'non-binding rule' prescribing how subjects A 'should' behave in phase 2. Subjects did not know at this time which role they would ultimately be assigned to. Thus, voting on the social norm took place behind a 'veil of ignorance.'

The voting procedure worked as follows. A table displaying all integer allocations of the available surplus was displayed on the subjects' screens. Next to the table, a green arrow initially pointed at a randomly chosen allocation. Each subject could then vote to move the arrow up or down, or to keep it where it was. The instructions asked subjects to vote sincerely. I.e. they said 'If you think that A should choose a distribution further down in the table, please vote *down*,' etc. and 'If you agree with the distribution indicated, please vote *stay*.' If a majority of subjects voted to move the arrow in the same direction, it was moved and another vote was taken. This procedure was repeated until the arrow was not moved for two consecutive rounds.[4]

After voting had ended, subjects were informed verbally that phase 1 was now over and phase 2 would begin. The rest of the experiment worked exactly as in the baseline, except that the table of distributions displayed on the screen was accompanied by a red arrow pointing at the 'norm,' and all on-screen instructions contained the sentence 'The red arrow indicates the rule agreed upon in the previous phase.'

Laboratory, subject pool, show-up fee

Experiments were conducted at the Max Planck Institute's experimental computer laboratory on the campus of Jena University. Subjects were students from a variety

[4]The latter rule was meant to ensure that subjects would not agree on something by accident. It also allowed subjects to 'ratify' an agreement that had been reached, thus enhancing the feeling that the norm had been agreed to.

of disciplines. (The largest groups were: business administration (37%) and social sciences, including economics (21%).) Half of the subjects were female. In addition to the earnings from the experiment, all subjects received a 5 EUR participation fee.

3 Results

We ran a total of ten sessions, four baseline sessions (B1 through B4), and six treatment sessions (V1 to V6). All sessions except V1 and B3 involved 16 participants, i.e. 8 pairs. Due to subjects failing to show up, session V1 involved only 12 participants (6 pairs), and session B3 involved only 14 participants (7 pairs). Thus, the data collected include 31 offers made under the baseline and 46 offers made under the voting treatment.

Voting on the norm

Table 1 summarizes the voting stages in each of the treatment sessions. Initially, I had expected that subjects would always agree on a norm to give 5 (i.e. 50% of the surplus. In 4 of the 6 voting sessions (V1, V2, V5, and V6) this is indeed what happened. In sessions V3 and V4, however, the collective agreement was to give only 4.

Table 1 also shows the initial position of the arrow when voting began as well as the number of subjects voting to 'stay' in the final round of voting. According to this measure, the norm of 4 was associated with a lower level of agreement in the groups. (Recall that this round is essentially about confirming the decision that the group has arrived at.) Also note that in both of the sessions in which a norm of 4 was agreed to, the random initial position of the arrow was at zero. This indicates that there may have been some anchoring effect. However, there are too few observations to test this possibility.

Table 1. Voting on the norm

Session	Initial Position	Norm	Stay votes
V1	3	5	10 (of 12)
V2	8	5	13 (of 16)
V3	0	4	6 (of 16)
V4	0	4	7 (of 16)
V5	0	5	10 (of 16)
V6	9	5	14 (of 16)

Distribution of offers

Table 2 displays the distribution of offers made under the baseline and treatment, as well as detailed results for each of the sharing norms. A first look at these data immediately reveals that the voting treatment did not affect dictator behavior as expected. Indeed, the most striking feature of these data is that a very large number of subjects (43%) decided to give nothing after a norm of 5 had been agreed to (as compared to 16% under the baseline).

Table 2. Dictator offers (out of 10 EUR)

Offer	Baseline	Treatment	Norm of 4	Norm of 5
0	5 (16%)	17(37%)	4 (25%)	13 (43%)
1	4 (13%)	3(7%)	3 (18%)	0 (0%)
2	4 (13%)	2(4%)	1 (6%)	1 (3%)
3	6 (19%)	6(13%)	1 (6%)	5 (17%)
4	3 (10%)	9(20%)	4 (25%)	5 (17%)
5	9 (29%)	9(20%)	3 (19%)	6 (20%)
	(N=31)	(N=46)	(N=16)	(N=30)

Before statistically evaluating the effect of the treatment, note that the norm agreed to by each group is endogenous and likely correlated with the behavioral dispositions

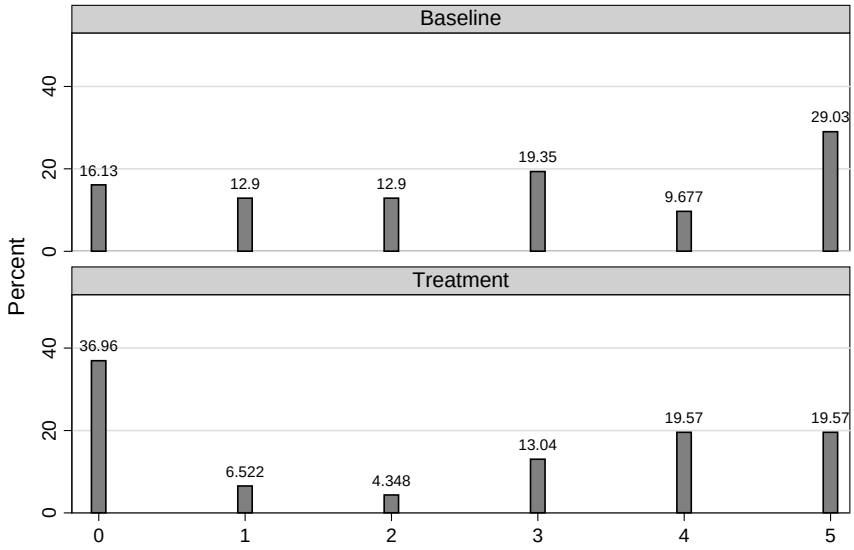

Figure 1: Distribution of offers by treatment

of those assigned the role of dictator. Therefore, we cannot consider each norm as a separate treatment condition. For the statistical tests, I consider only the pooled treatment data (See column 2 of Table 2 and Figure 1).

In order to test the hypothesis that the offers under the treatment are the same as under the baseline, I compare the distribution of offers using rank-sum (RS) and Kolmogorov-Smirnov (KS) tests.[5] Both fail to reject the hypothesis that offers under the treatment and baseline are drawn from the same underlying distribution (RS: $p = 0.25$, KS: $p = 0.31$).

However, a closer look at Figure 1 suggests that the number of subjects keeping the entire amount for themselves was significantly larger after voting on a norm. This conjecture can be tested using Fisher's exact (FE) test or a Chi-squared test

[5]The RS test is sometimes referred to as the Mann-Whitney test. Both test are based on the assumption that the data are drawn from a continuous distribution. Forsythe et al (1994) propose a method to apply the tests to discreet distributions by adding small random perturbations to the data. Applying this method to my data does not alter the results reported here. I therefore follow the more standard procedure of applying the tests directly to the untransformed data.

(CHI2). Using these tests, it indeed appears that a significantly larger number of dictators offered nothing under the treatment (both tests: $p = 0.04$).

Another look at Figure 1 suggests a more detailed way to look at the data. Specifically, it appears that the treatment may have affected the distribution of *small* offers, while the distribution of large offers remained similar. This conjecture can be tested by comparing the distributions of offers below three under the baseline and treatment. This comparison yields a significant difference (RS: p=0.02, KS: p=0.11). A comparison of the distributions above three yields no difference (RS: p=0.8, KS: p=0.99).

To summarize, the evidence suggests that the treatment did not have the anticipated effect on dictator offers. Surprisingly, the opposite appears to be the case. Specifically, it appears that the distribution of small offers was shifted to the left, with a significantly larger number of subjects choosing to offer nothing.[6]

4 Discussion

How are we to interpret these results? The first conclusion to be drawn is clear: majority voting did not produce an effective feeling of obligation on the part of dictators. This non-effect is confirmed by answers given in the post-experiment questionnaire. Figure 2 shows dictator subjects' reported agreement with the statement that 'I felt an obligation to share the money with B.' The difference is not statistically significant (RS p=0.3, KS p=0.62). If anything, it appears that dictators were *less* likely to agree with this statement under the treatment.

According to the theory of guilt aversion, agreement on a sharing norm should have affected behavior if it had an impact on dictator subjects' second order beliefs.

[6]More detailed tests at the individual level do not reveal a systematic relationship between voting and subsequent dictator behavior. That is, it is not the case that subjects who agreed to a norm behaved differently from those who did not.

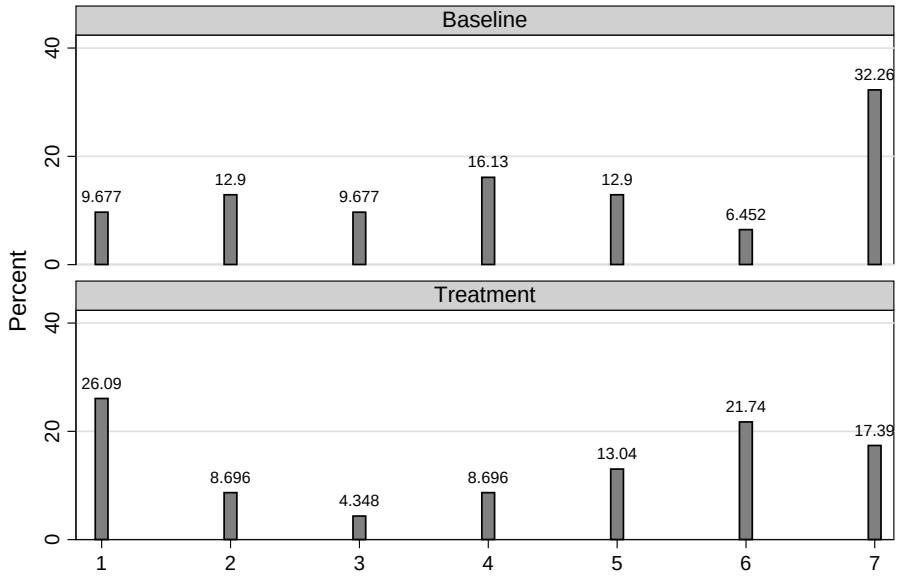

Figure 2: Dictator subjects' agreement with the statement 'I felt an obligation to share the money with subject B.'

Figure 3 displays the distributions of these second order beliefs by treatment.[7] They are virtually identical.

The absence of a positive effect of the agreement on subsequent offers thus appears to be consistent with the theory of guilt aversion. That is, voting on a norm does not appear to have affected dictator subjects' second order beliefs or feelings of obligation. This result highlights the importance of empirically investigating the precise conditions under which agreements do or do not affect beliefs and behavior.

The more interesting result, however, is that dictators did not only *ignore* the norm that was established, but instead appeared to actively *violate* it by sharing less than those participating in the baseline treatment. How can we understand this "crowding out" effect of agreement on a sharing norm?

[7]Belief elicitation was not incentivised. Dictator subjects were simply asked what amount they believed the other subject expected to receive.

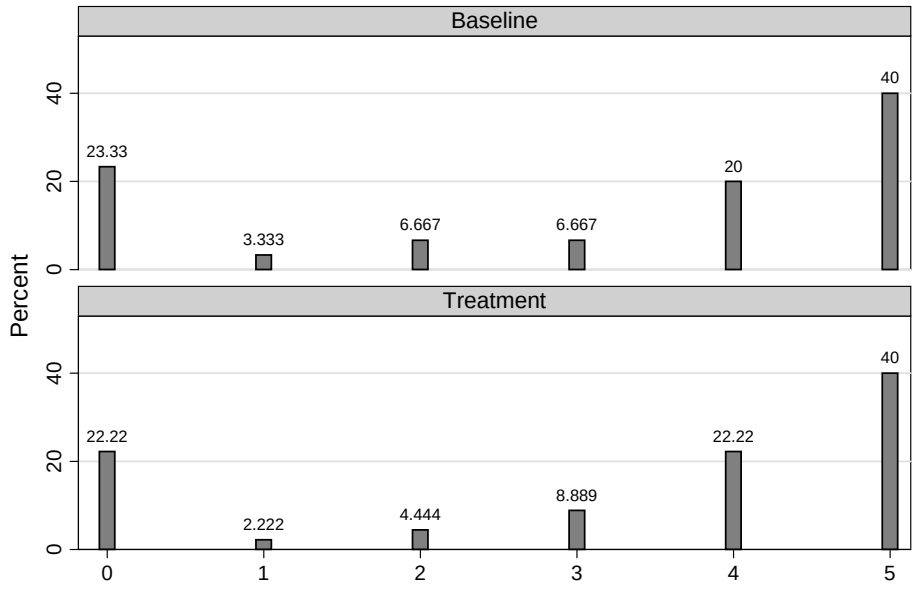

Figure 3: Dictator subjects' second order beliefs

A possible hint is provided by the results of a study by Rankin (2006). In that study, receivers were given the opportunity to send 'requests' to dictators prior to their distribution decision. The results show that such requests actually had a negative effect on the amounts given. Rankin speculates that requests may crowd out dictator generosity because 'an individual may give less when solicited to do so than when the gift is voluntary.'

The social norm in the experiment discussed here differs from the requests studied by Rankin in that the norm is established by the group and not the person with whom one interacts. However, the effect of this 'group request' may be similar. Specifically, dictators who normally would have been willing to share small amounts (say, less than 30% of the pie) may have felt pressured to share more, and this may have reduced the 'warm glow' utility they might have derived from sharing.

5 A model to explain the "crowding out" effect

In this section, I provide a sketch of a model that formalizes the intuitive explanation outlined in the previous section. Naturally, this model is being constructed after the fact, in an *ad hoc* manner. It should therefore be regarded as a suggested starting point for further empirical investigations in other contexts.

Specifically, suppose that a decision maker compares his planned behavior to what he perceives to be a prescribed *norm*, and suppose that he gets a fixed payoff ('warm glow') from abiding by (or surpassing) this norm. Then, if the norm is too demanding, the 'warm glow' may become too 'expensive,' leading the decision maker not to follow it.

Concretely, suppose that a decision maker is asked to divide a pie of size X. Suppose that she compares her action (the share s_i that she gives away) to what she perceives as the *norm* (the share n that she 'should' give away), and that she receives a *fixed* 'warm glow' payoff $\alpha_i > 0$ from abiding by the norm. Assume that this 'psychological' reward is simply added to her material payoff. Then, her utility from offering a share s_i of the pie is equal to

$$U_i(s_i, n) = \begin{cases} (1 - s_i) \cdot X & if \quad s_i < n \\ (1 - s_i) \cdot X + \alpha_i & if \quad s_i \geq n \end{cases}$$

Then, subject i's optimal choice is given by

$$s_i^*(n) = \begin{cases} 0 & if \quad \alpha_i < n \cdot X \\ n & if \quad \alpha_i \geq n \cdot X \end{cases}$$

In other words, the subject will compare the *cost* of norm compliance $n \cdot X$ with the subjective benefit α_i. It follows that this subject will follow a norm of sharing n only as long as $n \leq \overline{n}_i = \frac{\alpha_i}{X}$.

This simple model thus implies that the dictator may be more likely to offer *zero* the *higher* is the norm that he perceives. Intuitively, the reason is that the higher norm makes it more costly to receive a smaller 'warm glow' payoff. Once an expectation to share is established, the dictator experiences no joy from sharing. This may explain why dictators were more likely to offer zero in the treatment of the experiment.

Further consideration of the model reveals that it may account for the fact that the treatment in this experiment affected the *lower* end of the offer distribution. Specifically, consider a subject who believes (prior to voting) that the appropriate amount to share is positive but small (say, 3), and suppose that she is willing, given her personal parameter α_i, to follow this norm. We can conclude that $\alpha_i \geq 3$. Now suppose that a norm of giving 50% is established. Then, if $\alpha_i \in [3, 5)$, she will choose to offer nothing.

As noted above, this "toy" model only represents a sketch of an *ad hoc* explanation of the results obtained in this experiment. It should therefore be interpreted as a first step in guiding future modeling efforts that must then be subjected to further testing.

6 Conclusion

The experiment reported on in this paper sought to test whether collective agreement behind a veil of ignorance can affect fairness concerns in a dictator game. The ex ante hypothesis was that such an agreement would always establish a norm of sharing equally, and that dictator offers would be larger than under a baseline treatment in which no norm is established. Surprisingly, the results appear to support the opposite conclusion. In particular, it appears that the number and distribution of large offers (above 30% of the available amount) was unaffected by the treatment. In contrast, the distribution of small offers was shifted to the left, and the number of subjects keeping the entire amount was significantly higher.

A possible explanation for this result is that dictators experience a fixed 'warm glow' payoff from abiding by a given norm. When an excessively demanding norm is established by the group, it eliminates the incentive to comply, especially for those who previously perceived (and would have abided by) a less demanding norm. As a consequence, dictators who are otherwise inclined to share moderate amounts (less than %40) may have felt overburdened and therefore kept the whole amount for themselves.

The experiment raises questions concerning the usefulness of establishing *explicit*, shared norms in areas where pro-social behavior is motivated by heterogeneous 'personal norms.' In particular, explicit norms that are 'too demanding' may crowd out incentives to abide by less demanding personal norms. This may cause some individuals to reduce their level of pro-social behavior in response to such norms. Further experimental studies should aim at exploring these effects.

Appendix

A.1 Translation of Instructions

A.1.1 General instructions (all treatments)

The following instructions were printed on a single page placed at each seat before subjects entered the laboratory.

Instructions

Welcome. Please carefully read the following instructions.

General Rules

The experiment will last for approximately 30 minutes. During this time, we ask you to abide by the following rules:

- Do not speak to the other participants.
- Turn off and stow away your cellular phone.
- Stow away any reading or writing materials. Starting now, your table should contain only these instructions.
- In case you should have questions at any time, please raise your hand and wait until an assistant comes to your table.

Payment at the end of the Experiment

Regardless of the outcome of the experiment, each participant will receive a minimum payment of 5 EUR. Your total earnings may depend on your own decisions and those of other participants.

At the end of the experiment, please remain quietly at your seat. Payment is conducted at your seat. The following procedure ensures that no other participant will learn what you have earned:

- An assistant will bring you an envelope and a receipt.
- Please verify *immediately* that the content of the envelope corresponds to the amount indicated on the receipt.
- Sign the receipt and return it to the assistant.
- Quietly leave the room.

Participants who do not abide by these rules will be excluded from the experiment and payment.

In a few moments, you will receive additional instructions regarding the specifics of the experiment.

A.1.2 Baseline Treatment

The following instructions were printed on a single sheet and handed out after subjects were given time to read the general instructions. After giving subjects time to read, these instructions were read out loud by the experimenter. After this, subjects were again given time to read the instructions and ask questions. Questions were answered privately at the subjects' tables.

Information regarding the experiment

The 16 participants present will be randomly divided into 8 pairs. From each pair, one participant will be (once again, randomly) determined to be participant 'A,' and the other to be participant 'B.' Participant A will then decide how 10 EUR will be divided between himself and participant B. Only integer numbers will be allowed. That is, A will choose a distribution from the following table:

Possible distributions of the 10 EUR

A receives	**B receives**
10 EUR	0 EUR
9 EUR	1 EUR
8 EUR	2 EUR
7 EUR	3 EUR
6 EUR	4 EUR
5 EUR	5 EUR
4 EUR	6 EUR
3 EUR	7 EUR
2 EUR	8 EUR
1 EUR	9 EUR
0 EUR	10 EUR

The amounts assigned by A (plus the 5 EUR mentioned above) will be payed out to the participants at the end of the experiment.

A.1.3 Voting Treatment

In the voting treatment, subjects first received the same instructions as above, with the following modifications:

- A sentence was added at the top of the page that read, "The experiment consists of two phases. For reasons that will become clear in a moment, we first describe the second phase."
- Beneath this sentence, an additional heading in bold read, "Second phase:"
- At the bottom of the page, a sentence was added that read, "Before instructions for the second phase are passed out, we ask you to think about the following question: In your opinion, how should a participant in role A behave?"

As in the baseline treatment, these instructions were read out loud and subjects were encouraged to ask questions before proceeding. After questions had been answered, the following instructions for phase 1 were passed out. Again, subjects were given time to read, then instructions were read aloud, after which again subjects could read and ask questions.

First phase:
In the first phase, the participants present will have the opportunity to agree, by way of voting, on a non-binding rule according to which those chosen as participant 'A' in phase 2 should behave. At the time of the vote, no participant will know which role (A or B) he himself will occupy.

The vote will proceed according to the following rules:

All participants will see the table printed on the preceding page on their screen. An arrow next to the table will initially point at a randomly chosen allocation. If it is your opinion that A should choose an allocation further down in the table, please choose 'down.' If it is your opinion that A should choose an allocation further up in the table, please choose 'up.' If you agree with the allocation indicated, please choose 'agree.'

If a majority of the participants present (at least 9 people) vote for a movement in the same direction, the arrow will be shifted accordingly. If no such majority exists, the arrow will remain in place.

This procedure will be repeated until the arrow is not shifted in two consecutive rounds.

After each round of voting, you will learn the number of participants who have voted for each of the options.

The allocation rule agreed upon in this fashion will continue to be indicated by an arrow in phase 2. This rule is non-binding. That is, those persons chosen as participant A will still be able to choose any allocation in the table.

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