



Modeling other-regarding preferences and an experimental test *

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Abstract. Behavior inconsistent with self-interest has been observed in many contexts. We argue that models designed to cope with these anomalies are inadequate to deal with a variety of social values. Our extension of the Fehr & Schmidt ‘inequity aversion’ model is applied to results from dictator experiments in which the money to be divided is generated by the efforts of paired individuals in either one or two rooms. This production leads to sharing behavior qualitatively different from that found in other dictator experiments. The pattern of sharing can be explained by entitlements, equity, and the credibility of the experiment.

1. Introduction

The self-interest assumption, that choices depend only on one’s own interests, has done yeoman service in economics and the other social sciences. From Adam Smith to Gary Becker it has been the lynch-pin of many models of human behavior. The assumption simplifies and clarifies arguments wonderfully. But there has always been a problem. Everyday observation and common sense reveal countless acts of seemingly unselfish behavior among friends, family, and even casual strangers. Other-regarding behavior is often prevalent.

This paper is about other-regarding behavior. The study of other-regarding behavior has been advanced by the development of experimental techniques

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(especially the dictator experiment). Those experiments have helped us to get an understanding of its nature and some contextual variables which interact with behavior to make it more or less selfish. A number of models have been put forward to explain the resultant findings. Here, we present three things: the data from a set of dictator experiments, an extension of one of the current models being used to explain previous data and a more general discussion calling into question the class of models being developed to deal with the problem of other-regarding preferences.

Although there had been a continuous stream of theoretical work¹ dealing with the lack of empirical fit between laboratory results and self-interest, no serious accumulation of studies developed until experimental methods were applied to non-market behavior: especially the testing of *n*-person prisoner's dilemma games as models of public good problems (see Ledyard, 1995 for a good summary). The discrepancies between the theoretical prediction of zero contributions and observed contributions of between 20–40% created a substantial set of anomalous data points. Subsequently, a number of commentaries (see, for example, the early experiments by Frohlich and Oppenheimer, 1984 and commentaries by Dawes and Thaler, 1988) focused on this problem.

More recently, the results from increasingly sophisticated experiments have provided data and impetus for new models of preference. These models included elements to deal with such concerns as fairness or distributional issues (Bazerman, Lowenstein, and White, 1992; Bolton, 1991; Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999; and Cox, Sadiraj, and Sadiraj, 2001); reciprocity and conditional other-regardingness (Rabin, 1993; Cain, 1998); and reciprocity coupled with concern for disadvantaged individuals (Charness and Rabin, 2000). The various authors were eager to show the extent to which their models explained the growing set of laboratory results.

Here we use data from modified dictator experiments² to analyze how social, or other-regarding, preferences enter into individuals' decisions. Social preferences are shown to be context dependent. Aspects of laboratory experiments limit the generalizability of the results reached in the laboratory and hence also limit models based on those results. Without claiming to provide a "most" general model we expand upon the Fehr/Schmidt model to include 'just deserts:' a relevant factor when individuals are involved in production or work. But this element is only one of many possible components which can enter as a function of the decision context, and this leaves the more general problem of how to model preferences up in the air.

1.1. *The dictator experiment*

In the dictator experiment individuals arrive at one of two rooms (Room A and Room B). A monitor is randomly chosen from subjects in Room A and runs the experiment to increase the credibility of the instructions and to reduce experimenter effects. Other subjects in that room each receive an envelope with \$10 (and 10 blanks) in it. They are told they can keep any combination of the money and/or blanks so long as a combination of 10 blanks or bills are left in the envelope. They go to a privacy booth and choose the amount of money to take and leave the rest in the envelope for an individual who is in Room B and with whom they have been anonymously paired. They then drop off the sealed envelope in a box and leave Room A and the experiment. What is left in the envelope is delivered to the paired person in Room B and is recorded by the experimenter. This fire-wall of anonymity shields subjects' identities both from the experimenter and other subjects to insulate the subject from both reputational and other experimental effects.

A purely self-interested individual would leave no money. But the results of these experiments show that many people do not behave in a purely self-interested fashion. These "anomalies" are quite widespread and explanations tend to focus on situational variables and, more recently, doubts the subjects have regarding the decision situation. Two related situational variables have been identified: lack of social context and anonymity.

1.2. *Situational variables*

Anonymity: Higher levels of anonymity in dictator protocols lead to lower levels of seemingly other-regarding behavior (Hoffman, McCabe, and Smith, 1996). The authors provide a discussion conjecturing that variation in the amounts of money left is a function of the degrees of anonymity provided by the differing details of experimental designs. They introduce the concept of social distance, defining it as "the degree of reciprocity that subjects believe exist within a social interaction" (p. 654).³ They conclude that increased anonymity discourages belief in reciprocity within a social interaction and hence allows self-interest to be observed.

Lack of social context: Eckel and Grossman (1995 and 1996) showed that the relatively high level of money kept in dictator experiments is a result of the absence of any recognizable social context provided in the experiments. They argued that the lack of context leads to a psychological disconnect between the dictator and the potential recipient. When dictators leave money for others with identifiable characteristics the modal behavior changes and shows higher levels of other-regarding behavior. Ruffle (1998)

and Konow (2000) also designed experiments with social contexts to identify how fairness considerations might enter into dictator experiments and found further evidence for that hypothesis.⁴

Thus, lack of context can be viewed as contributing to possible Type I errors (incorrectly classifying subjects as having the characteristic being tested for: in this case self-interest). Indeed, without some social context, it is not clear which normative judgements and which values ‘ought’ to be applied by the experimental subjects. What, after all, *is* other-regarding behavior without an understandable social context defining a relationship between persons? What little social context exists in a traditional dictator experiment is one of ‘anonymous strangers’ without any information about claims, needs, and welfare. This calls into question the external validity of inferences about self-interested behavior from these experiments. Behavior towards what sorts of others, and in what sorts of contexts, do the actions in the traditional experiment reflect?

1.3. *Doubt*

As noted above, in traditional dictator experiments subjects are located in separate rooms and remain totally anonymous from one another and from the experimenter; observed behavior cannot be linked to any identifiable individual. These conditions can produce doubt regarding what is happening. Dictators may doubt the existence of the second room, the existence of individuals in that room, and whether the proposed transfer of money will actually take place. If subjects harbor such doubts the interpretation of their actions becomes problematic and the validity of inferences from their behavior is called into question.

Such doubts decrease the incentive to leave anything regardless of whether one does have concern for others’ welfare. In the presence of total scepticism everyone would leave nothing, and the behavior would test nothing more than ‘rationality’ or maximizing behavior. In the presence of sufficient scepticism one can’t differentiate between rationality and self-interest.⁵ And the greater the anonymity, the greater the doubt, and so by maximizing anonymity, the design again maximizes Type I errors. Withholding money from the other is viewed, perhaps improperly, as evidence for self-interest.

1.4. *Interaction of doubts and social context*

As noted, the lack of social context in dictator experiments presents a direct threat to the interpretation of seemingly self-interested behavior. That is what Eckel and Grossman (1995 and 1996) and Konow (2000) found. But none of those experiments systematically addressed the issue of subjects’ doubts

regarding the experiment and how that might impact behavior. There is another way in which the lack of social context can call experimental results into question. A thin to non-existent social context undermines the credibility of the experimenter and reinforces subjects' doubts and the problems those doubts raise for interpretation of results. Enriching the social context of the experiment, say by signaling 'relative need' or introducing some other aspects of the environment which define a basis for the relationship between the two individuals, should re-enforce precisely the credibility of the relationship between the dictator and her paired other in the experiments and hence increase seemingly other-regarding behavior. In summary, the apparent prevalence of other-regarding behavior is arguably a function of a variety of aspects of the decision environment.

1.5. *The conjectures and the current research*

We conjecture that the lack of recognizable social context in traditional dictator experiments interacts with anonymity to increase subjects' doubts, and hence reinforces behavior which can be interpreted as self-interested.⁶ To test this we introduce two additional elements into the traditional dictator experiment: First, we place dictators in the same room as the recipients to reduce doubts about the nature of the experiment. Second, we pay money for work performed by the dictator (hereafter referred to as *d*) and her paired other (hereafter referred to as *r*, as the recipient of any money *d* leaves) thereby creating a recognizable social context.⁷ The experiments are designed to allow both doubt and the social context to vary so that we can identify the relative effect of these variables as well as their interactions on behavior. Introducing a social context, such as work, brings other values into the dictator's choice problem. Specifically, it leads to consideration of 'just deserts' or pay for one's work. Taking that into account, we introduce an additional element into the Fehr/Schmidt model based on conjectures regarding subjects' behavior in our experiments.

The main issue addressed is the prevalence of self-interested behavior in subjects, how it varies, and how this variance can be explained. The specific propositions tested are the extent to which the subjects' doubts regarding the experimental design and the introduction and absence of a social context (production) affect behavior. We show how the results of the experiment cast some doubt on the adequacy of the Fehr/Schmidt model for this and other contexts and how our extension of their model into a more general model of rational moral choice overcomes these inadequacies.

2. Generalizing the inequity aversion model

In dictator games all decisions involve two individuals: d (the dictator) and r (the recipient). What is to be decided by the dictator? How much to take; how much to leave. Nothing more. And the context is without social fabric of any sort. What norms might come into play? Will d treat r fairly, taking into account differences d makes in r 's welfare as well as in d 's own? Will d , perhaps, strive for a degree of equity? "Yes" is the fruitful conjecture in Fehr/Schmidt.

They propose a model in which non-conformity to equity is costly in two ways to the dictator. It is costly not to take her fair share (half) but it is also costly not to give the recipient r her fair share (half). Following Fehr/Schmidt, the parametric weights of these two types of costs are: α which represents the cost of d not getting half and β which represents the cost of giving to r less than d takes for herself. If the model is strictly linear then the cost of keeping \$2 more than half, for example, is 2β . And the cost of keeping \$2 less than half is 2α . Fehr/Schmidt add some substantive assumptions to gain leverage over the problem. Most relevant, they note that in general $\alpha > \beta \geq 0$.⁸ In other words, d cares more for inequality when she has less than r , than she does when she has more. Since d makes the decision, this prevents d from giving more than half of the contents in the envelope to r .⁹

To define the model formally for our contexts, let the total money received by the dictator for distribution be \mathbf{x} . Then in any distribution, $\mathbf{x} = x_d + x_r$: the sum of the amount kept and the amount left. In the production experiments, the amount produced \mathbf{p} (in monetary terms) is the amount to be distributed, being the summed production of the paired individuals: $\mathbf{p} = p_d + p_r$. Hence, $p_d + p_r = x_d + x_r$.

In the simple dictator environment, the Fehr/Schmidt model characterizes the dictator's value of allocating x_d to herself, as:

$$U(x_d) = x_d - \alpha \text{Max}(x_r - x_d, 0) - \beta \text{Max}(x_d - x_r, 0) \quad (1)$$

Given that d is a unilateral decision maker and that $\beta < \alpha$, either β is sufficiently large to motivate a 50–50 split, or it doesn't affect behavior at all. The critical value is $\beta > .5$. As noted above, the dictator will never choose $x_r > x_d$. But with $\beta > .5$ she will always choose $x_r = x_d$. To see this, note that any extra penny d gets, r loses. This means that a one penny increase in d 's payout creates a 2 pennies increase in the difference between the payoffs of d and r . The monetary gain of the 1 cent is more than offset by the utility lost by deducting $\beta(2)$ as per the equation. Thus, with $\beta > .5$ we get a peak in d 's utility function at an even split of \mathbf{x} between d and r . An illustration of this is the solid line in Figure 1 for $\beta = .6$, $\alpha = .8$ and $\mathbf{x} = 1$.

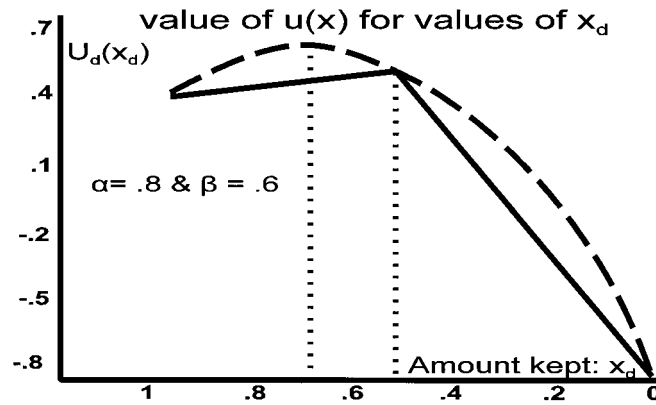


Figure 1. Two representations of $U(x)$ – linear (solid line) and non-linear (dashed line) with $\beta = .6$. The vertical dotted lines indicate individual maximization as a function of the choice of x_d .

Fehr/Schmidt note that the linear model doesn't fit the data compiled from dictator experiments over the years. Many of the dictators in these experiments leave more than nothing, but less than half. The linear model “yields too extreme predictions . . . in the dictator game.” (p. 847). Non-linearity can help us avoid this and can get results (i.e. predictions of money left) in the interior of $[0,0.5]$ (p. 848). We illustrate this with the dashed line in Figure 1. In other words, non linear versions can lead to predictions of the amount left between 0 and 50%.

2.1. Introducing production and just deserts

As indicated above, making the money in the envelope a function of joint production introduces at least one more value: how the dictator ought to compensate the recipient who is now a co-producer of value. We deal with this by adding a concept of ‘just deserts’ to the Fehr/ Schmidt model. Paralleling their approach, we note that non-conformity to just deserts is costly in 2 ways: not taking one's own just deserts (i.e. where $p_d > x_d$), and not giving another their just deserts $p_r > x_r$). In the dictator's utility function these would show up as two cost terms: $\gamma \text{Max}(p_d - x_d, 0)$ which represents the cost to d of not taking her own just deserts and $\psi \text{Max}(p_r - x_r, 0)$ which represents the cost to d of not giving to r her just deserts.

Hence, assuming linearity, in the dictator environment with production, the value of d's allocating x_d to herself, can be written as:

$$U(x_d) = x_d - \alpha \text{Max}(x_r - x_d, 0) - \beta \text{Max}(x_d - x_r, 0) - \gamma \text{Max}(p_d - x_d, 0) - \psi \text{Max}(p_r - x_r, 0) \quad (2)$$

The logic of the coefficients for the just deserts argument is a bit different than that for the coefficients having to do with equity. γ and ψ don't have the reciprocity that leads to the 2 for 1 multiplier that is in β . Rather, a penny short of just deserts is just that: a penny short. Hence, the just deserts effect won't be felt unless it can outweigh the benefit of a penny gained. This would require that γ or ψ be valued > 1 , if α and β are both zero. If α and β are not zero, then the minimum values for them to have an effect on behavior are a more complex function of α and β . In this setting, there are two different decision contexts for d based on the relative productivity of d and r : when $p_d > p_r$ and when $p_r > p_d$. In either case, when $2\beta + \psi < 1$ the dictator leaves nothing. But if d is more productive than r , or $p_d > p_r$, and if $2\beta + \psi > 1$ then d leaves just deserts or a 50/50 split. On the other hand, if d is less productive than r , or $p_r > p_d$, and if $2\beta + \psi > 1$ but $\psi - 1 < 2\alpha$ the dictator will leave 50%. In such cases but when $\psi - 1 > 2\alpha$ the dictator will leave just deserts.¹⁰ Hence, in cases with production, other regarding behavior can show up when both β and ψ are below .5.

This implies that for any level of the parameters there ought to be significantly fewer observations of self interested behavior in production dictator experiments than in the traditional ones. Paralleling the case of equity concerns, we will assume that the individual is more concerned about fairness to self than to others: hence $\gamma \geq \psi > 0$. Thus we assume $\alpha > \beta$ and $\gamma \geq \psi$. This does not imply that individuals think any injustice to them is as bad as any injustice they could inflict on others. For example, if d leaves just deserts then prefers just deserts for r to *taking everything* for herself.

Note that we can now consider the logical subsets of possible cases which we might observe in a dictator experiment with production. In general, the dictator, d is juggling a tradeoff between just deserts, equity, and material gain. Potential observations can be described as occurring in a space characterized as in Figure 2. Any data points would represent the proportion of the monies d leaves in relation to what proportion of production r performed. The location of each observation in this production/money-leaving space represents a particular level of conformity with a norm of behavior, given the relative production of d and r . When d is the more productive member of the pair, the observations will be on the left side of the vertical dashed line in the graph. When r is more productive they will be to the right of the line.

The placement of the points also shows the relationship between the behaviors and the norms. For example, all the points on the horizontal axis represent leaving nothing – very selfish (traditional self-interested economic) behavior. Just deserts consists of leaving the exact proportion of r 's contributions to income: the 45° line (labeled 'Just Deserts'). Finally, there is the other norm of behavior: "equity." It is highlighted and labeled in the graph as

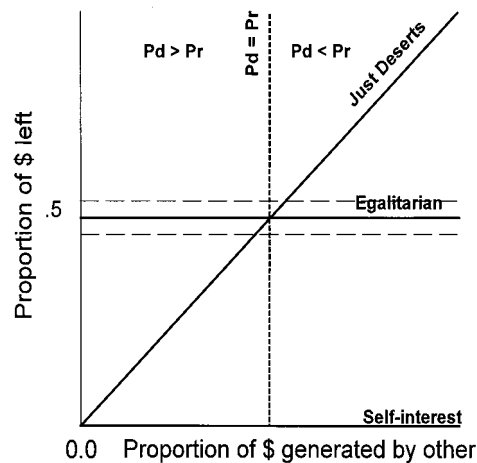


Figure 2. Schematic showing the principles being juggled in the observation space.

“egalitarian” and is the zone within one unit adjacent to the horizontal line. It represents a 50/50 split.¹¹

Taking the just deserts and the egalitarian lines as a benchmark, relative generosity (and selfishness) can be distinguished by whether the point representing the proportion left is above, (or below), one of the two other-regarding lines. Note that which region a data point falls in also shows a bit more about the dictator’s values. The sorts of costs which d is willing to incur is signaled by the quadrant in which the data point appears. If d leaves more than $1/2$ then d is willing to absorb α (inequity to self) costs. If d leaves less than $1/2$ the cost comes from the β term. If d gives more than just deserts (i.e., above the 45° line), then the costs are from γ and if d gives less than just deserts the costs are associated with ψ .

When only α and β are in d ’s preference structure as posited by Fehr/Schmidt (1) there is never a motive to give more than half (see note 9). The expanded model (2) makes a different prediction. If the appropriate conditions hold on the parameters (as specified above), the dictator is expected to leave just deserts – more than 50%. On the other hand, the two models agree that d is never expected to leave more money for r than needed to satisfy both the just deserts *and* equity. Both models predict no observations above both just dessert *and* equity lines. Linearity of the preference structures as in (2) generates knife edge properties: individuals are predicted to leave either enough to satisfy just deserts, or enough to satisfy equity, or they are predicted to leave nothing. In other words, all those with linear functions will be found directly in conformity with one of the principles. Non-linear

preferences would explain behaviors which are less extreme: compromises between principles.

2.2. *Doubts*

The effect of the subject's doubts in the experiments can now be added to the model. Given that *d* examines, handles, and can directly pocket the contents of the envelope while making her decision, doubts are not likely to modify the perceived total possible payout. Rather, in our context doubts affect the relationship between *d* and *r*. Doubts change the possibility that the total payoff is, in fact, a function of some other's behavior, and that leaving monies achieves anything with respect to either equity or just deserts. Thus, doubts modify the terms weighted by β and ψ in the above models.¹² We think of doubts as a probabilistic discount that the individual has; call it π where $1 \geq \pi \geq 0$. $\pi = 0$ means that *d* does not believe that there really is someone generating the income and receiving a benefit from anything *d* leaves. $\pi = 1$ means that *d* has no doubts whatsoever with regard to these matters. This is captured by the following linear model:

$$U(x_d) = x_d - \alpha \text{Max}(x_r - x_d, 0) - \pi \beta \text{Max}(x_d - x_r, 0) - \gamma \text{Max}(p_d - x_d, 0) - \pi \psi \text{Max}(p_r - x_r, 0) \quad (3)$$

To see what we can infer from this modification, again consider the parameters specified above. Doubts can change behavior from dividing the amount equally to keeping it all when $p_r > p_d$. For example, with $\alpha = .8$, $\beta = .4$, $\gamma = .7$, $\psi = .5$, when $\pi < .77$, *d* keeps all the money, and when $\pi > .77$, she splits it evenly. In other words, doubts lead to more self-interested appearing behavior.

But to get interior results, results that don't lead to complete conformity to one of the three normative principles articulated (material well being of self, concern for equity, and concern for just deserts) the linear model must be given up.

2.3. *A specification of the conjectures*

The experiments we have designed and the model we have specified allows us to identify two categories of conjectures. The first deals with the anticipated effects of the room and production treatments on the generation of doubts in subjects. The second deals with how the engendered doubts plus the contextual variables such as production with its induced norm of just deserts affect subjects' behavior in dictatorship experiments. Included in the latter category is the question of whether the observed behavior is interpretable via

the Fehr/Schmidt model or requires our extended model. By running experiments in one or two rooms, either with or without production we can test for the effects of context, doubts and their interactions.

Doubts are deemed to be a function of both the room and production treatments and their interactions. Thus a set of preliminary conjectures regarding the generation of doubts are:

Conjecture 1a: Doubts will be higher in the 2-room than in the 1-room treatment.

Conjecture 1b: Doubts will be higher in the No Production than in the Production treatment.

Conjecture 1c: The room treatment will have more impact on doubts in the presence of production.

Conjecture 1d: The production treatments will have more impact on doubts in one room than in two.

The second category of conjectures relate to the effects of doubt and production on behavior.

Conjecture 2: The amount of money left by dictators will increase in the production treatment and decrease with higher levels of doubts.

Conjecture 3: The amount left by dictators will increase as a function of the recipient's productivity.

But there are additional differences in the implications between the Fehr/Schmidt model and our expanded model. These are traceable to the introduction of the 'just deserts' term. As we pointed out above, in our model d 's choice is a function of the relative sizes of the parameters α , β , γ , and ψ and the relative productivity of the two parties. This leads to differences in predictions from our as opposed to the Fehr/Schmidt model. Specifically:

Conjecture 4: The Fehr/Schmidt model implies that a dictator will never leave more for her counterpart than she takes for herself.

But from the expanded model:

Conjecture 5: Some individuals will leave more than an equal division if the recipient is more productive than the dictator, but *none* will if the recipient is less productive.

The last two conjectures, together imply that

Conjecture 6: The amount left will never exceed the maximum of an equal split and just deserts.

The research design which introduces the social context of work and manipulates levels of doubts should allow us to observe whether behavior varies from the predictions of the Fehr/Schmidt model and whether it is explicable by the multiple concerns of material well being, equity, and just deserts, as modified by doubts in our expanded model.

3. Research design

As noted above we introduce two variations to the traditional experiment and hence have a 2×2 design. The first variation involves introducing the social context of production and the second involves conducting the experiment in one room. Production involves both dictators and their paired others proofreading a text to correct spelling errors. One dollar of credit is allocated for each error properly corrected (and a credit removed for errors introduced). Dictators receive an envelope with dollars corresponding to the net errors corrected by the pair (and an equal number of blank pieces of paper in the size of dollar bills), the actual work sheets, and a sheet indicating the proportion of errors corrected by the dictator and paired other. The dictator is free to allocate, anonymously, any number of dollars to the envelope to be given to the paired, but not identified, other. This procedure is done in both one and two room treatments. In the one room treatment, to insure anonymity, both dictators and recipients receive envelopes and must allocate between themselves and the paired other behind a privacy screen. But only the dictators' envelopes contain money; recipients get only blank pieces of paper to allocate. In all other respects, save for the administration of an anonymous post-experimental questionnaire, the experiments are conducted via a protocol as closely parallel as possible to that used in the standard double-blind dictator game.

Subjects for the production experiments were recruited at the University of Maryland via newspaper ads and the subjects for the standard dictator experiments were recruited both in Maryland and at the University of Manitoba.¹³ In all 131 usable dictators were distributed across the four treatments as depicted in Table 1.

To test for the effect of subjects' doubts and their understanding of the social context of the experiment we constructed a number of questions. Because the room treatments differed we had to vary the questionnaire slightly.¹⁴ The

Table 1. Experimental treatments (frequencies)

Condition	No production	Production	Total
1 Room	17	40	57
2 Room	41	33	74
Total	58	73	131

respondents were asked to express their degree of agreement on the following scale:

Agree Strongly 0 1 2 3 4 5 6 7 8 9 10 Disagree Strongly

The statements (which we use in the analysis in this paper) were:

1. I trusted that the experiment was being conducted in the way that it was described to me.
2. I am sure that there really are people in Room B. [**1R:**] I am sure that there really was a person paired with me.
3. I am sure that the money I left in the envelope will be given to the person in Room B with whom I have been paired. [**1R:**] I was sure that the money I left in the envelope was going to be given to the person with whom I had been paired.
4. Uncertainty about the existence of people in Room B affected the amount of money I left in the envelope. [**1R:**] Uncertainty about the existence of the person with whom I was paired affected the amount of money I left in the envelope.¹⁵

For purposes of analysis, the responses to the four questions were summed and a scale called “doubt” was constructed from the sum. The scale had a Chronbach’s α of .71. A higher score on the “doubt” scale is interpreted as a greater doubt that the experiment was conducted in accordance with the description provided by the experimenters.

4. Results

The first four conjectures are at the heart of the research design and their non-falsification are preconditions for testing the Fehr/Schmidt and extended models. We begin by testing these conjectures against the data and then turn our attention to the fit of the model to the data.

Table 2. Treatment effects on subjects' doubts

Conditions		Mean	SD	T-test	Prob*	N
<i>Without interaction effects</i>						
	One Room	9.14	8.49			57
	Two Room	14.76	10.68	3.36	.001	74
	Production	10.72	10.20			73
	No Production	14.33	9.80	2.06	.02	58
<i>With interaction effects</i>						
No Production	One Room	12.94	8.43			17
	Two Room	14.90	10.35	0.75	.457	41
Production	One Room	7.53	8.08			40
	Two Room	14.59	11.24	3.02	.002	33
One Room	Production	7.53	8.08			40
	No Production	12.94	8.43	2.25	.016	17
Two Room	Production	14.59	11.24			33
	No Production	14.90	10.35	0.12	.903	41

*one tailed.

4.1. Rooms treatment, doubts, and behavior

The data in Table 2 support both conjectures 1a and 1b. Dictators in the one room experiments had significantly lower levels of doubt than dictators in 2-room experiments and their doubts were lower in the work contexts than in the standard dictator experiment. But as anticipated in Conjectures 1c and 1d, the effects of the two treatments interact. The lower section of Table 2 shows the interactive effects of the two treatments.

The work context effect on doubts varies as the number of rooms is varied. Conducting the standard dictator experiments, i.e. without work, in one (vs. two) rooms has minimal additional impact on subjects' doubts. The mean levels of doubts are 14.90 and 12.94 in two and one rooms and the difference is not significant. However, when subjects are in a work context, the difference made by their placement in one or two rooms increases dramatically. Mean doubts are 14.59 and 7.53 in two and one room respectively ($t = 3.02$, $p = .002$): Without social context placement does not make a major difference; with it, it does.

Similarly, there is an interactive effect of the number of rooms with the work context in its effect on doubts. When subjects are in one room, the presence or absence of the social context of production makes a considerable difference in their expressed levels of doubt. With production the average level is 7.53, without, 12.94, ($t = 2.25$, $p = .016$). But in two rooms, the absence or presence of a social context seems not to affect doubt levels. There is virtually no difference between the two groups. The means are 14.59 and 14.90, with and without production ($t = .012$, $p = .903$). These differences point to an interaction of the two treatments on subjects' levels of doubts.

Incidentally, the fact that doubt levels differ in this consistent pattern across different treatments is evidence against the interpretation that dictators' expressed doubts are simply a rationalization for leaving less money.¹⁶

The Fehr/Schmidt model is mute on the issue of doubts and makes no provision for changes in context. The expanded model makes explicit predictions in both regards. Our Conjecture 2 predicts the amount of money left by dictators would increase in the production treatment and decrease with higher levels of doubts. To test this and also our Conjecture 3 we used regressions to estimate the amount of money left by dictators in all treatments.¹⁷ The results of the tests are displayed in Table 3.¹⁸

Model 1 in Table 3, shows that the relationship of the two treatment variables (the number of rooms, the existence of production; and their interaction) on how much money the dictator has left for the recipient. The two (binary) treatment variables yield an adjusted R^2 of .26; only production is significant in the regression. As noted above, however, the two treatment variables' main effects are conjectured to be indirect. The variables were shown (Table 2) to have an interactive effect on doubts and it is this effect, and the productivity of the recipient which we believe affects how much the dictator leaves.

One of our main arguments has been that the production environment provides subjects with a social context in which self-interest both is clearly defined (keep the money rather than give it to a partner who has played an identifiable role in "earning" that income) and in which it may or may not predominate. The particular context provided may invoke specifiable norms of behavior in opposition to self-interest.¹⁹ After all, the dictator receives money that has been generated, explicitly, with the efforts of someone else. This treatment was chosen because there are strongly held norms regarding individuals' entitlements to the fruits of their labor: commonly called 'just deserts' (Frohlich and Oppenheimer, 1992). Ruffle (1998) and Konow (2000) also have some confirmatory evidence showing that individuals are willing to bear costs to satisfy this norm. The data from the experiments lets us examine how doubt and production intervene to affect behavior. By including others' corrections, we can also test the extent to which "just deserts" trumps self-

Table 3. Regressions estimating proportion of money left

Variables	Model 1	Model 2	Model 3	Model 4
Constant	2.471 (.921)**	4.055 (.792)***	4.195 (1.250)***	2.698 (.614)***
Room	-.650 (1.103)	-.411 (.852)	-1.421 (1.478)	6.94E_02 (.560)
Production	4.954 (1.099)***	-.645 (1.059)	-3.310 (1.787)*	
Room*Production	-2.017 (1.419)	-1.239 (1.102)	2.948 (2.508)	
Doubt		-.122 (.027)***	-.133 (0.82)	_.645E_02 (.028)*
Other's productivity (= Other's corrections)		.535 (.069)***	.774 (.118)***	.593 (.052)***
Doubt*Production			.124 (.133)	
Doubt*Room			6.9E-02 (0.93)	
Doubt*Room*Production			-8.4E-02 (.163)	
Other's productivity*Doubt			-8.1E-03 (.011)	
Other's productivity*Room			-.173 (.191)	
Other's productivity*			-1.30E-02	_.187E_02
Room*Doubt			(.013)	(.004)***
Adj R ²	.260	.561	.611	.618

one-tailed p: * < .05; ** < .01; *** < .001

interest in a manner consistent with our expanded model of other-regarding behavior.

We test these ideas in Models 2 through 4 where we model the amount left by dictators as a function of doubt, and the paired 'other's corrections.' The 'other's corrections' is the amount of money the other's efforts contributed to the envelope the dictator received. It was anticipated in Conjecture 3 that there would be a direct relationship between that amount and the amount left due to the other's entitlement. Model 2 presents a regression examining the relationship between others' corrections and how much is left in the en-

velope, given the interaction effects among the variables. Here we see that supplementing the treatment variables with the conjectured variables through which the treatments were operating (doubts and other's productivity) leads to a jump in the explained variance to about 56 percent.²⁰ Now neither treatment variable is significant, although both doubt and other's productivity are highly significant ($p < .001$). A dollar increase in productivity of the recipient leads to a 53 cent increase in what is left. But also recall, from the discussion of equation 3 above, that doubts were conjectured to modify what is left mainly via interactions with other terms concerning the 'reality' behind the experimental environment. To test for these multiple interactions, we put forward Model 3. But here the effects of doubt and its interactions are not significant, possibly because of their being dissipated across so many different interactions. We used a step wise procedure to see if we could identify a reduced model and to find if there was a significant interaction of doubts with one or more of the variables in such a reduced model. That is reported as Model 4. As in Model 3, we are able to explain above 61% of the variance and the two most important variables are other's productivity and doubt (along with the complex interaction term).

It strongly appears from these regressions that the work context invokes a different preference structure on the part of the dictator. For every dollar's correction made by the dictator's counterpart, 64 cents was left. 'Just deserts' is a strong explanatory factor. Altogether, roughly 61% of the variance in the money left in production and non-production experiments is explained by the behavior of one's counterpart, doubts, and the interaction of those two factors and the experimental treatment of production. This provides what we consider to be strong evidence in support of the expanded model.

4.2. *Specific effects of just deserts and doubts in modeling social preferences*

Production is conjectured to introduce notions of 'just deserts' or entitlements in the dictators. The evidence presented so far strongly supports that conjecture but, as demonstrated below, it also shows that more than the simple inequity aversion conjectured in Fehr/Schmidt is required to explain subjects' behavior.

Obviously, not all dictators weigh others' entitlements by exactly 59.3% (as in the coefficient of "Others' Production" in Model 4 would indicate). There are different classes of dictators: some who left all that the other was entitled to, some who left none, and some who left a proportion. Many combinations of such behaviors would be consistent with the analysis of variance, and with our model of rational normative behavior. They would simply

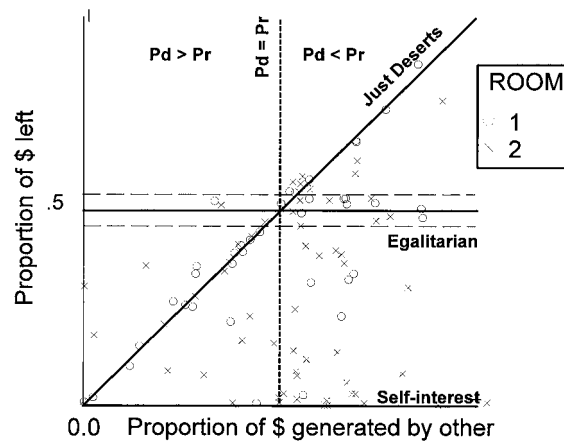


Figure 3. Proportion left and 3 notions of entitlement.

be different samples of individuals with differing weights placed on others' entitlements.

To display these different modes of rational normative behavior we examine scatter plots showing what proportion of the money each dictator left for her counterpart against the proportion of the production contributed by that counterpart. In Figure 3 (as in Figure 2) the three sorts of normative behavior are highlighted and labeled. Leaving the 'earnings' of one's counterpart (just deserts) yields a point on the diagonal line; splitting the amount 'equally' falls in the horizontal region half way up the graph (see note 11); naked self-interest (or leaving nothing) results in an observation lying on the horizontal axis.

The results in Figure 3 show 3 sizeable clusters of individuals: one cluster in each of the of the 3 identified zones.²¹ However, the Fehr/Schmidt model and our extended model make different predictions regarding the distribution of observations over the graph. In particular Conjecture 4 notes that the Fehr/Schmidt model implies that a dictator will never leave more for her counterpart than she takes for herself. In other words, there should be no data points above the horizontal zone. By contrast, the expanded model implies in Conjecture 5 that individuals can leave more than an equal division when the recipient is more productive than the dictator, but *not* when the recipient is less productive: any points above the horizontal zone should appear to the right of the vertical dotted line. Further, our model implies in Conjecture 6 that the amount left will never exceed the maximum of an equal split and just deserts. So no points should fall in the upper left-hand quadrant or above the diagonal in the upper NE portion of the graph.

Table 4. Motivational categories of dictators by room

Pattern of behavior	1 Room	2 Room
Just deserts	21	9
Egalitarian	11	8
Self-interested	4	10
Other (e.g. non linear)	9	9
Actual total	40	33

Totals aren't column totals because 8 'fair persons' are double counted. In the 1-room 5 are double counted: 2 left none and 3 left 'equal'. In the 2-room 3 are double counted: 1 left none, 2 left 'equal'.

The data do not support the Fehr/Schmidt model but do *fully* support the conjectures based on the extended model. It is clear from Figure 3 that there are a number of individuals who leave more than the even division of money. Indeed, 8 dictators who were less productive than their counterpart left more than 50% of the revenue, providing evidence counter to the Fehr/Schmidt model (Conjecture 4). But it also provides evidence in favor of our model (Conjecture 5). Moreover, we have noted earlier that when the dictator is more productive than her counterpart, no-one leaves more than one-half of the earnings and so Conjecture 5, as a whole, is supported by the data. In addition, one of the most striking findings in the graph is that *no one* gives more than the maximum of egalitarian line and the just dessert line. That is, no one is more generous than the more generous of those principles (the points along the edge which are shown as within that area are due to jittering (see note 20). The absence of any such points in Figure 3 is obvious support for Conjecture 6.

As is apparent from the figure, a substantial number of dictators, precisely 30 (see Table 4) left exactly the amount of money corresponding to the number of errors corrected by their paired others (i.e. the points appear on the diagonal). Indeed, the just deserts response is modal. This respect for entitlements is a rule of fairness recognized in other experiments (Frohlich and Oppenheimer, 1992; Ruffle, 1998; Konow, 2000). But there are two other clear patterns of behavior among dictators. There is a string of points along the x-axis representing all those dictators who left nothing (14 of them). Moreover, there are 19 dictators who split the proceeds equally among themselves and their paired other and who show up clustered in the horizontal region in the middle of the graph. Adding these up (and correcting for those who are 'double counted' as sitting on two lines at the intersection points)

leaves only 18 of 73 dictators whose behavior cannot be explained by one of the simple rules which can be predicted with our expanded linear model (see equation (2)).

The production environment provides a social context which seems to evoke a pattern of normative responses from subjects. A majority (44 out of 73) give money strictly in accordance with one of two well established other-regarding norms: entitlement and equal distribution. But we should consider those 14 dictators who left nothing: cases which could clearly be interpreted as representing self-interest. We have conjectured that doubt can lead to seemingly self-interested behavior and we can check to see whether that behavior might be attributable to their doubts about the experiment. After all, one of the consequences of introducing doubts is conjectured to be the deterioration of morally oriented behavior (see equation (3)). This conjecture is already borne out by the regression analysis presented above. But we can directly compare the levels of doubt of seemingly self-interested dictators to the levels of doubt manifest by the other dictators. We find that dictators who left nothing exhibited a mean doubt level of 17.86 (S.D. = 10.62) while those who left something exhibited mean doubt levels of 9.03 (S.D. = 9.42). This constitutes a significant difference ($t = 2.86$, p [one tailed] = .005). And finally, we ought to note the difference between the proportion of dictators who left nothing in the 1-room (8.9%) and in the 2-room (27.7%) treatments.²² So doubts appear to play precisely the role the model predicts in the seemingly mean-spirited behavior of those who leave nothing, and that behavior is not unequivocal evidence of self-interested motivations.

We should also point out the existence of the “cloud” of points in the trapezoid to the lower right (below the two lines representing the other-regarding principles): individuals who left less than might be justified but more than nothing. The behaviors of these individuals can’t be explained without reference to non-linear models.

At the same time note that a high proportion of the points are on or below the *lower* of the two lines representing the norms of other regarding behavior. That is in the right half of the space, most are around and below the horizontal egalitarian line and to the left they are around just deserts diagonal line. This supports a view that the individuals are prone to trading off one norm with another in a self-serving fashion. That is, with two salient norms of behavior available, the dictators take the cost of conforming to the alternatives in to account. From the discussion in the multivalued models above this is to be expected.

Above we demonstrated that doubt is increased by running the experiment in two rooms rather than in one. Thus, the “Room” treatment can serve as a crude proxy for doubt. We have divided the data into one and two room

treatments and one can examine, indirectly, the effect of increased doubts on the pattern of dictators' behavior. The obvious question is, "How does the experimental treatment of putting everyone in one room, rather than two, affect the behavior of dictators as it pertains to the models?"

Let us point to the obvious differences observable in Table 4. There is a great difference in the proportion of dictators who leave nothing in the two treatments (less than 9% in 1 room and a bit over 27% in the 2 room case). But this is just the tip of the iceberg. In the 1-room treatment, the just deserts behavioral response is stronger than in the Two Room treatment (46% vs 25%) while about the same proportion of the dictators split the money equally (27% vs 24%). A Chi-Square analysis of the prevalence of these norms of leaving behavior (and the residual category of leaving an amount that does not correspond to any of the three norms) yields a statistic of 9.041, significant at the .029 level. So the separation into two rooms affects the nature of the norms invoked and applied by dictators. It also appears that one and two room treatments affect behavior and do this in part via inducing doubts differentially.

5. Conclusions

The simple model of naive self-interest at the core of much of micro-economics has, for some time now, been roundly (and justifiably) criticized by many, but perhaps by none so pithily as Sen (1977) via his title "Rational Fools". Our results indicate that, given a context, it is quite feasible to model behavior that goes beyond the traditional simple model. What we see is that individuals can – nay – do adopt a moral point of view as a function of the cues in their environment when those cues are credible. One component of a preliminary model, inequity aversion, was developed in Fehr/Schmidt as noted above. Fehr/Schmidt provide one of many possible starting points for expanding a model of social preferences. Cox, Sadiraj, and Sadiraj (2001) provides yet another and also gives instances in which individuals are willing to give amounts which result in their paired other receiving a higher payoff than their own. However, all of the models are deficient in a number of ways.²³ When the context includes a more complicated (e.g., a work) environment and the context engenders doubts regarding the true nature of the interactions, the models need amending. With production, some individuals seem to respond to a norm of 'just deserts' and when individuals are overcome by doubt they appear to revert back to the narrow self-interested calculations of the traditional model. Something like the more complex models of equations (2) and (3) seem to be needed.

In sum, in social contexts with non-negligible norms (such as with just deserts) there are massive deviations from the levels of self-interest found in standard dictator experiments. This demonstrates that other-regarding behavior is very prevalent even in totally anonymous (non-competitive) situations much as in the world external to the laboratory: situations which always provide a social context. In our experiments this other-regarding behavior falls into two (at least) identifiable categories. One norm is just deserts – in which dictators offer up to their counterpart the full fruits of the latter’s labor. The other is an even split, or equity. There are also dictators who behave in a strictly self-interested fashion. But they evidence the highest levels of doubt regarding the veracity of the experimenters regarding the design, and so even then some of that behavior may be mis-attributed to self-interest. The Fehr/Schmidt inequity aversion model and other models provide starting points for characterizing the behavior observed, but it requires significant revision. The revised model may provide a framework for dealing with production and other normatively laden contexts.

5.1. *External validity and experimental design*

But there is another implication of our findings. Our findings speak not only to the potential validity of certain models, but also to a possible problem in the experimental methodology used to test the models. It speaks to the problems of external validity of some previous experiments in the field saying: “Your results may not be what you think they are!” What was interpreted as self-interested behavior may be simply a manifestation of doubt and mistrust of the experimental environment – in part engendered by the lack of social context. It follows that it may not be sensible to try measuring self-interested motivations without a social context. More generally, it raises the specter that experiments attempting to identify motivation (such as, but not restricted to, self-interest) without a social context are prone to serious threats to external validity. A striking example is the standard dictator experiment, with the thinnest of contexts. It is simply not credible to many of its subjects (Frohlich, Oppenheimer, and Moore, 2001). Consequently, inferences about the prevalence of self-interest in such experiments are questionable.

This raises questions regarding Hoffman, McCabe, and Smith’s (1996) notion that social distance is the variable which makes all the difference. We have shown that social distance is not, in itself, enough to make a difference unless it affects subjects’ doubts. A regular dictator game lacks social context and so varying social distance by being in the same room, or another room, doesn’t matter, *per se*. However, when an interpretable relationship between the subjects is introduced (as in a production experiment) relevant norms appear to be invoked more strongly, and doubts are assuaged more fully when

there is less separation. Some of the differences in behavior may be directly attributable to social distance but it would appear that the effects have deeper roots. The behavior is influenced by the nature of the social context. The social context may act as a cue to evoke responses that compete with self-interest. If this is replicated in subsequent research, we must be sensitive to our modeling of social contexts when we wish to use laboratory findings to predict outcomes in external everyday contexts.

5.2. Modeling normatively oriented behavior: The problem of finding a general solution

Recent experimental results have provided a basis for modeling social preferences. Indeed, it offers a near paradigmatic example of the progress of scientific explanation as envisioned by the Vienna School philosophers of science. In the normal course of inquiry, data has emerged which appeared to falsify the notion of self-interest as traditionally formulated. Reasonably simple, manipulable, and testable models have been put forward to explain emerging anomalies. The models build on a variety of motivations: inequity aversion, reciprocity and possibly other values.

We agree that reciprocity and equity are basic values which, under appropriate conditions, enter into the decision calculi of many individuals. The data unearthed in the experimental environments cannot be ignored. Cox, Sadiraj, and Sadiraj (2001) also note that some results may require the inclusion of intentions as a determinant of responses. But, the models which have been developed have been primarily responsive to laboratory data. In life's everyday choices, many other values beyond equity and reciprocity are evoked. Ultimately, models must be designed to explain behavior in the broader world. Here, we have attempted to replicate, in the laboratory, the conditions which might evoke one value which may be an important component of a large number of economic interactions: respect for just deserts. We conjectured that it can be modeled in a fashion similar to the modeling of inequity aversion and have shown how one such model could act as a template for a generalizable structure to deal with the multiple values individuals might evoke when trying to do 'the right thing.' The structure of the model we use allows for continuously variable treatment of others in adjustment to their specific behavior. This is most likely to be the general case, rather than treating all others symmetrically (or retaliating uniformly). The data fit the generalized model quite well.

But the general problem of other regarding behavior is more complicated. Different moral values are evoked in different contexts. Such values form a part of a more complex moral point of view which may be under-

stood as aspects of multi-dimensional preferences differentially evocable by environmental cues.

One plausible research agenda would be the exploration of the full range of such values and situations. This would require careful excavation, testing and modeling, just as did reciprocity and equity. Our data underscores the difficulty which such a ‘value specifying’ approach will have. It will be difficult, if not impossible, to talk about specific alternatives to non self-interested behavior without specifying a well-defined and easily interpretable social context. And outside the laboratory, the individual’s perception and definition of her social environment will determine the values which compete for her behavioral response. As such, how are models developed from the laboratory contexts to be justified?

An alternative research agenda would start assuming individuals usually *do* have other-regarding values that enter into their choices, and then tries to discover ways to deal with modeling the problems of value orientation more generally in a manner giving leverage over choices. Perhaps a fruitful path is to consider the issues involved in adopting a moral point of view (Frohlich and Oppenheimer, 2001): one which takes the interests of relevant others into account in one’s decisions. This raises immediate questions. What, for starters, threatens the adoption of a moral point of view (doubt and competition are two factors already known)? What is sacrificed by institutions which depress revelation of preferences which reflect such social values? What sorts of institutions are more or less likely to make certain values enter into a decision maker’s calculations? In other words, if other-regarding behavior exists and is context-dependent, what social, political and economic contexts matter, how do they matter, and how can we do something about it?

Notes

1. A few of the earlier theoretical studies include: Valavanis (1958); Preston (1961); and Frohlich (1974).
2. The dictator experiment have become one of the standard tests of an individual’s motivation (Hoffman, McCabe, and Smith, 1996).
3. Although their descriptive language doesn’t show it, their tests stress variation in the anonymity in the subject – experimenter relationship.
4. On the other side, Roth, (1995: 282) showed that laboratory experiments framed in a market contexts generate choices which are more conforming with self-interest than ‘dictator’ or other non-market experiments. The two properties are clearly related: after all, zero social context means maximum anonymity.
5. Of course one could still have deontological values which would prevent one from taking everything: ‘Since I received pay for showing up, I am not entitled to the money in the envelope, so I will return it,’ etc.

6. Konow (2000: 1086) notes that even in 2-room experiments with a production context, subjects still spontaneously report doubts regarding the veracity of the experiment (specifically the existence of the other room and other paired subjects)
7. The experiments are based on pilots which we designed and ran originally in Canada and Japan (Frohlich, Oppenheimer, Saijo and Turnbull, 1997). The pilot experiments resemble some treatments in Konow (2000).
8. They note there are people who prefer to have inequality: (i.e., for whom $\beta < 0$). We identified a class of such subjects in our earlier experiments (1984) and called them 'difference maximizers.' But for the purposes of explaining the bulk of the data, they restrict themselves to $\beta \geq 0$. As long as $\beta_d > 0$ d cares somewhat (Fehr and Schmidt, 1999: 822) about the unequal treatment of r.
9. Note that there are some dictators who, contrary to the model, give more than half (indeed some give all) of the monies to their paired other. Although the model could be 'tweaked' to cover some of these cases (e.g. allowing $\alpha < 0$ while β is permitted to be larger than α), the message should be different. Rather, it seems to us that some individuals process the values differently (we say more on this in Frohlich, Oppenheimer, and Moore, 2001: 276–278).
10. Note that this would often imply that ψ be significantly greater than 1.
11. The line has a band surrounding it because the proceeds are whole dollars and thus, uneven amounts of production cannot be divided equally.
12. Other interpretations are surely possible. We also entertain the possibility that doubts should modify all the interactive terms. Such shifts in the structure of the equation don't make for substantial shifts in the argument as we present it, but they may be worth pursuing were one to pursue the issue in depth.
13. These subject are those reported in Frohlich, Oppenheimer, and Moore (2001). No significant differences were found between the amounts left by the Manitoba and Maryland students in the same treatment.
14. The questions from the 1-room experiment (in the cases when they were modified) are located after the original questions and are preceded by a "[1R:]" after the original question.
15. This question was scaled in the opposite direction and so the response was subtracted from 10 in construction the doubt scale.
16. A fuller discussion of why 'rationalization' does not appear to be associated with dictator behavior is contained in Frohlich, Oppenheimer, and Moore (2001: 280–282). The main burden of the argument is that both dictators and recipients express indistinguishable levels of doubt within each treatment – which is inconsistent with dictator rationalization.
17. A few points of comparison are called for here. Our results for the two room, non-production experiments replicated the experiments reported in Hoffman, McCabe, and Smith (1996) as DB1 and in Eckel and Grossman (1996). Our dictators were somewhat more generous than theirs. Hoffman et al report 64% as giving nothing, and Eckel and Grossman report 63% but in our experiments 40% gave nothing. Further, we found about 35% gave \$4 or more and they found 8% and 0% respectively. In no subset did we have such dire outcomes: neither in the U.S. or Canada (where the subjects were from the Business School). It might be noted that our higher baseline giving makes the strong treatment effects noted below even more credible because the higher levels of giving in the one room and production experiments are contrasted with higher initial one room and non-production results.
18. Two outliers who both left all the money in the no production treatment are excluded from the analysis in all the regressions. Hence, the N is 128. A full discussion of out-

liers from the no-production treatment is contained in Frohlich, Oppenheimer, and Moore (2001: 276–278). To combine the production and the non-production experiments in this regression we ascribe to the non-production experiments a ‘zero’ as the value of others’ corrections. This is perhaps not quite right in terms of what is going on cognitively. If we instead just look at the subset of dictators in the production treatments, we get similar equations, for that subset. Given the similarity, we choose to report the analysis for the group as a whole as it is of some intrinsic interest.

19. This is similar to the tradition of defining altruism as manifest only when something is given to others at *some cost to self*.
20. As R^2 overstates the relationships, we here talk of adjusted R^2 as variance explained, even though the concept is really tied to R^2 alone.
21. The actual count, as in Table 4, is a bit more complicated. Recall, from note 11, that the bands showing equity are just a guide. The numbers here refer to the apparent conformity to a norm of behavior rather than to the actual count in the scatter plots, since some interpretation of the intersections is necessary. Overlapping data points are distinguished by the introduction of ‘random displacements of the points’. Hence some points may be slightly displaced from their actual position.
22. To see this, note that some points are double counted and the percentages are calculated on the simple sum of observations (including the double counts) in each column.
23. Cox, Sadiraj, and Sadiraj (2001) note that their model fails to accurately reflect the data from some experiments which are explicable using the Fehr/Schmidt model and vice versa. This conclusion is compatible with the notion that the form that other regarding behavior takes may be substantially context dependent. No “single” model which explicates a particular set of values is likely to be adequate to capture behavior in all contexts. There are various research alternatives: the enterprise can be refocused to the level of ‘moral point of view,’ one can leave the substance of the norm out of the theory, and consider the structure of trade-offs with self interest that make sense, or one can develop multiple models for multiple social contexts.

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