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Dictator game giving: altruism or artefact?

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Abstract Experimental dictator games have been used to explore unselfish behaviour. Evidence is presented here, however, that subjects' generosity can be reversed by allowing them to take a partner's money. Dictator game giving therefore does not reveal concern for consequences to others existing independently of the environment, as posited in rational choice theory. It may instead be an artefact of experimentation. Alternatively, evaluations of options depend on the composition of the choice set. Implications of these possibilities are explored for experimental methodology and charitable donations respectively. The data favour the artefact interpretation, suggesting that demand characteristics of experimental protocols merit investigation, and that economic analysis should not exclude context-specific social norms.

Keywords Altruism · Artificiality · Experiments · Methodology

JEL Classification C91 · C70 · D63 · D64

Experimental economists have investigated unselfish behaviour with a familiar set of simple games (Camerer 2003, pp. 43–100). The simplest is the dictator game, in which subjects have to decide how much, if any, of an endowment to give to another subject, typically in a one-shot anonymous setting. Usually transfers are plentiful; it is common for over 50% of subjects to give money away (Camerer 2003, pp. 57–58). Several models of unselfishness have been put forward within the parsimonious framework of rational choice theory. These theories compete over data from dictator

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games and other experiments, and include model of inequality aversion (Bolton and Ockenfels 1998; Fehr and Schmidt 1999), altruism (Andreoni and Miller 2002), egocentrism (Cox et al. 2002) and Rawlsian "social welfare" preferences (Charness and Rabin 2002). Such models continue to attract intensive research effort.

Whilst dictator giving has proven hard, though not impossible, to eradicate in the lab, some experimentalists still express unease about the results.[1] A common concern is that people could always make anonymous donations to random strangers in everyday life, for example by mailing cash to persons sampled from the telephone directory, but few if any choose to do so. Transfers are made instead to family members, specific organisations or face-to-face to people requesting money. Since we face the dictator game all day every day, it could be argued, the experimental design would appear to score highly on conventional standards of ex-ante external validity. It presents a familiar set of opportunities in a task which makes minimal cognitive demands. Yet the results appear to be misleading about the extent of faceless interpersonal altruism.

Giving is also notoriously sensitive to experimental treatments. This adds to the uncertainty about how the results from dictator games generalise to naturally-occurring contexts. See, for examples, Eckel and Grossman (1996) on anonymity and charities versus individuals as recipients, Hoffman et al. (1996) and Bohnet and Frey (1999) on social distance, Cherry et al. (2002) on earned versus unearned income, and Haley and Fessler (2005) on visual suggestions of observation. The evidence has tended to be that giving is sensitive to factors for which theory makes no prediction, or which can be explained by theory. Intriguingly, theoretically-motivated sensitivity tests reported by Andreoni and Miller (2002), found that rational choice theory fits dictator game data surprisingly well.

On the other hand, there is evidence from related games that behaviour is also sensitive to factors which are irrelevant according to consequentialist theories, such as intentions. For example, in ultimatum games Falk et al. (2003) find that respondents' behaviour depends on payoffs to the proposer in avoided subgames. Similar results are offered by Brandts and Solà (2001). In a variant on dictator games, Dana et al. (2005) find that introducing uncertainty over the recipient's outcomes reduces egalitarian choices even when the uncertainty can be costlessly resolved. This result is discussed further in Sect. 3 below and seems to indicate that much giving is not motivated by a desire for fairer outcomes for their own sake, but for a further motive. Because of this kind of data, orthodox accounts of social preferences might be better seen as attempts at reasonable approximations of specific motivations, for limited domains, than at literal general truths.

The above synopsis of the existing evidence suggests that dictators' giving is probably more volatile than a literal interpretation of social preference theories might suggest, and does not translate straightforwardly into insights about behaviour in the field. However, the possibility still appears open that dictator game data is explained by subjects' underlying tastes, values, goals and so on, imparting value, whether directly or indirectly, to the other's payoff. The experiments reported below investigate

[1] I refer to remarks made by Arthur Schram at the 2003 University of Nottingham workshop on "The Role of Experimental Methods in Economics" and discussion with Robert Sugden.

directly whether dictator games measure social attitudes, in this broader sense. Much, perhaps even most, of the dictator games literature supposes that subjects arrive at the lab with such dispositions, which then determine their satisfaction with the initial allocations. If this process explains giving, subjects should also want to give if taking is possible. The experiments compare dictator games to "taking" games which introduce taking options; the proportion of subjects willing to give should be unchanged. This prediction is quite intuitive, but is also an implication of rational choice theory, as shown in Sect. 1 below.

In contrast with many studies of dictator games, but in common with Andreoni and Miller (2002), the present study is a theoretically-driven sensitivity test. It focuses on a subject's willingness to give, in abstraction from the amount given. The treatment manipulation is ineffective assuming that the endowment is consistently regarded as either too selfish, too generous or just right along budget lines which implement the same price of transfers. In contrast to previous studies, therefore, it tests a basic qualitative proposition implied by common readings of the design. The bold claim that a given social preference theory is confirmed by the data requires this postulate and more, for there subjects' attitudes determine a well-behaved indifference map over outcomes. The test conducted is lenient towards weaker social preferences explanations of giving, since it allows gifts to vary in size, as predictably occurs with any change in the environment, without rejection of the null hypothesis. The null is only rejected if there is evidence that some subjects' willingness to give can be completely eliminated by taking options.

1 Design

Three experiments were conducted, with two treatments in each. The design compares a dictator game (treatment 1; T1) to a "taking game" (treatment 2; T2), which introduces taking options. The treatments are represented in Fig. 1 below. Let (a, b) represent an allocation of £a to a dictator and £b to her partner, B denote a dictator

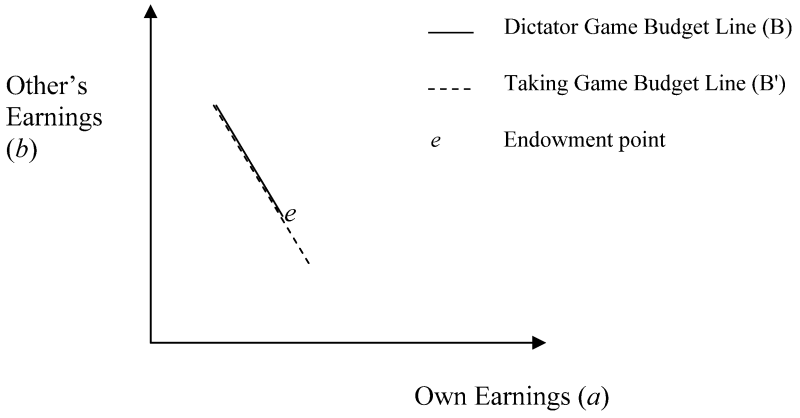

Fig. 1 Dictator and taking game budget lines

game budget line and e the endowment point. B has the same gradient and endowment point as the budget line in T2, denoted B′, but B′ extends to the south east of B.[2]

Social preference theories posit convex indifference curves in (a, b) space. Points on a budget line further away from the optimum in a given direction lie on progressively lower indifference curves. Suppose a subject starting at e, facing B, gives money because her optimum is to the northeast of e. Then she should also like to give facing B′, since an indifference curve can be tangential with a budget line, of arbitrary length, at one point only. In experiments 1 and 2 the treatments provide identical giving options, that is, $B \cap B' = B$. The population proportions giving should therefore be equal. In experiment 3, in contrast, subjects cannot give in T2 so $B \cap B' = e$. So there, if the optimum represents a positive transfer, e should be chosen from B′. The proportion of givers in T1 should therefore not exceed that choosing e in T2. In each case the null hypothesis is that of equality of the relevant proportions, with a two-sided alternative hypothesis.

Common procedures were as follows. The experiments were run as pen-and-paper exercises, with dictators choosing payoffs from a table. Subjects were mixed-gender undergraduates. Separate samples were used for each treatment to eliminate deliberate responses to experimental manipulation. T1 and T2 were run at different times of day for organisational reasons, with the order varied across experiments. Subjects in one room were randomly matched with a partner in another. Subjects in the receiver role did not have any task to perform but were fully informed; they simply awaited the outcome. To preserve anonymity, dictators were seated apart and made their decision privately. The experimenter issued instructions, available on the journal website, which were also read aloud, but was not present when decisions were made.[3] Assistants then administered and made payments, who were uninformed about the design, so the experimenter and assistants were blind to individual behaviour. The resulting double anonymity between subjects and experimenters was emphasised in the instructions. T1's instructions abridged T2's, making only the changes required to alter the choice set. As conventionally conceived, this holds the framing constant across treatments. Basic details of each experiment are given in Sects. 2.1–2.3; for full results and statistical significance see Fig. 2 and Table 1 of Sect. 2 respectively.

2 Experiments 1–3

2.1 Experiment 1

Experiment 1 implemented equal endowments (£6 each) and scaled-up transfers, following Cox et al. (2002) who provided initial evidence of dictator taking. In T1 dictators could transfer £(0–4), in increments of £0.33p. Transfers were doubled. In T2

[2] Cox et al. (2002) report that when giving involves a higher transfer rate than taking, kinking the budget line of the taking game, dictators generally take. The authors show that in that environment orthodox indifference curves may license both taking game taking and dictator game giving, a possibility excluded here.

[3] There were minor differences in script between experiments, but never across treatments within an experiment. The principal difference is that in experiment 1 subjects had to circle a number on a decision table, but in the other experiments they had to circle a letter. This was to strengthen the inscrutability of the design to the assistant paying subjects.

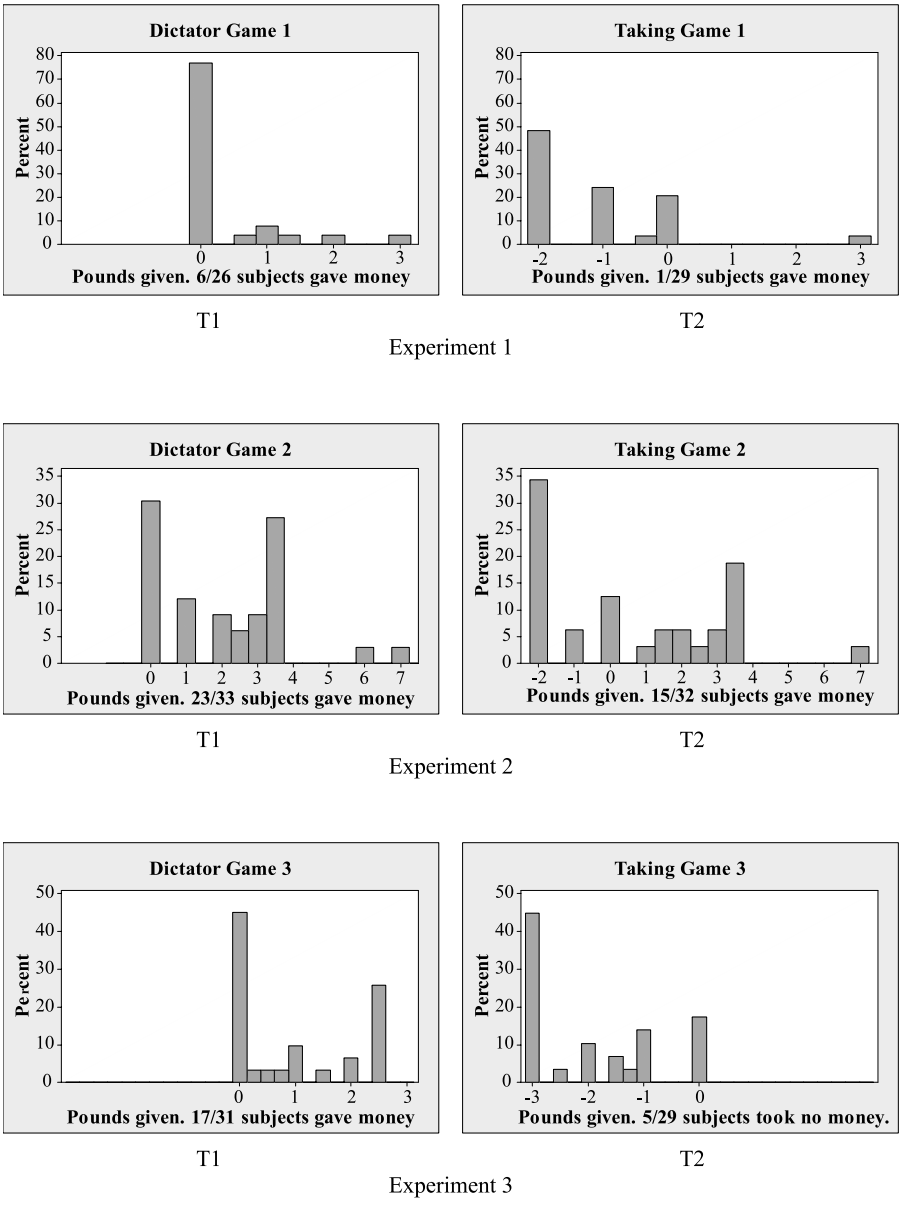

Fig. 2 Frequency distributions of transfers in experiments 1–3

dictators could make the same gifts, or take up to £2 at the same rate: £1 received cost their partner £2. Subjects also received £2 show-up fee. Experiment 1 was conducted in June 2004, with 110 subjects. T2 was implemented before T1.

In T1 the proportion of subjects giving, p_1, was 6/26. In T2 this proportion, p_2, was just 1/29. The difference in proportions, $p_1 - p_2 \equiv d_1$, is significantly different

Table 1 Statistical significance

Experiment	Bootstrap confidence interval for difference in proportions
1	$0.0384 \leq d_1 \leq 0.385$ (95%)
2	$0.041 \leq d_1 \leq 0.415$ (90%)
3	$0.079 \leq d_2 \leq 0.667$ (99%)

from zero ($p < .05$).[4] The extent of the inconsistency is unclear, however, since few subjects gave in T1. This problem motivated experiment 2, using more normal dictator game parameters. Also, experiment 1 suggested that giving might be extremely fragile; experiment 2 made taking options a smaller proportion of the choice set to test this.

2.2 Experiment 2

Experiment 2 used transfers at a rate of £1:1 and unequal endowments. Subjects were informed they had a show-up fee of £4 each. Dictators in both treatments were given an additional £7. In T1 they could give £(0–7). In T2 they could give £(0–7) or take £(0–2) of their partner's show-up fee. Transfers were made in increments of £0.50p. The experiment was conducted over two dates, in November and December 2004, with 130 subjects. T1 was implemented before T2.

Results were in the same direction as for experiment 1, with 23/33 subjects giving in T1 and 15/32 giving in T2, but only significant at the 10% level. However, taking options in T2 constitute a smaller proportion of the choice set. A sterner test of dictator giving was therefore conducted next, removing strictly positive transfers from T2.

2.3 Experiment 3

Experiment 3 also implemented £1:1 transfers and unequal endowments. Dictators were informed they had an endowment of £10 and their partner £5. In T1, dictators could give £(0–3). In T2, dictators could take £(0–3), but could not give. Transfers were in increments of £0.25p. Experiment 3 was conducted in February 2005 with 120 subjects. T2 was implemented before T1.

In T1, the proportion of subjects giving was 17/31, exceeding that choosing the status-quo in T2, p_3, of 5/29. The difference in proportions, $p_1 - p_3 \equiv d_2$, is highly significant ($p < .01$). The point estimate of the proportion of generous choices that disappears across treatments, $s \equiv d_2/p_1$, is 0.69, indicating that most dictator game givers would take, were the endowment the least selfish option. The bootstrap 95% confidence interval for this estimate is $0.342 \leq s \leq 0.929$.

[4] See Sect. 2, Table 1. The Z-test for equality of proportions and chi-sq test are inappropriate when frequencies approach zero. Since alternative methods exist for calculating two-tailed p-values for Fisher's exact test (Agresti 1992) and it is unclear here which is preferable the bootstrap technique is used (Efron 1979). A number of draws equal to the sample size was taken, with replacement, 10,000 times from T1 data. The procedure was repeated with T2 data, simulating the alternative hypothesis. Two-sided confidence intervals were calculated with the percentile method, which provide the p-values in the text.

To summarise, the three experiments show similar indications of inconsistency between treatments 1 and 2. In experiment 1 the difference in unselfish behaviour is significant at the 5% level, in experiment 2 at the 10% level, and in experiment 3 at the 1% level. The most decisive results, of experiment 3, indicate that most dictator game givers would take presented with sufficient taking options, despite being better off initially.

3 Discussion

The reversing of generosity between treatments is inconsistent with any theory of dictator game giving which regards underlying other-regarding motivation as causing a desire to share the endowment, and therefore with any orthodox social preference account.[5] The evidence here is actually consistent with that presented by Andreoni and Miller (2002), who find in favour of rational choice altruism, since they only examine behaviour in the giving domain. It rules out their interpretation, though, since that is inconsistent with the observed sensitivity of giving to taking options. Further, modifications of theory to incorporate intentions à la Rabin (1993) cannot explain these results since they concern exchanges of harms and benefits rather than their unilateral infliction studied here.

It is interesting to compare the results to those of Dana et al. (2005) who also cast doubt on the other-regarding interpretation of dictator behaviour. The authors find, relative to a dictator game in which dictators choose between monetary payoffs of \$(6, 1) and \$(5, 5), that fair choices atrophy when the dictator does not have full responsibility for the other's payoff. For example, in one variant the other's payoff is \$1 or \$5 with equal probability, but the uncertainty can be costlessly resolved before choosing. Few subjects chose to resolve the uncertainty and most chose \$6 for themselves. The authors conclude that subjects are more concerned to seem than to be fair, since by choosing not to know the other's payoff they can attribute the inferior outcome to chance; it is the transparency of responsibility in the dictator game that causes generosity. However, the taking observed in experiments 1–3 above occurs where dictators are transparently responsible, so this interpretation does not seem tenable here.

One explanation of the present results is that dictator game giving is a "Hawthorne" effect (Roethlisberger and Dickson 1939; Adair 1984; Jones 1992), that is, an artefact of behavioural experimentation. Call this reading 1. The Hawthorne effect might plausibly be interpreted as subjects' reacting to experimental "demand characteristics" (Orne 1962, 1973), meaning the cues the protocol supplies about appropriate behaviour.[6] Here, subjects might feel that the dictator game is about giving, since they can either do nothing or give, whilst taking options could introduce a conflicting message. The attitude causing the treatment effect may therefore be one towards

[5] List (2007) has subsequently replicated the result that willingness to give erodes with taking options.

[6] Given the proliferation of meanings of the term "Hawthorne effect" it is probably best avoided. The "demand characteristics" terminology is already established in psychology and conveys better the structural nature of the purported confound than "demand effect", which may connote influence exerted by the experimenter in person.

the experiment, such as a desire to be a good subject. Demand characteristics do not appear to have been much researched by economists but psychologists sometimes claim to explore them empirically. Examples are Fernandez and Turk (1994), Davies and Best (1996), Faith et al. (1998), Lampinen et al. (1999), and MacLeod (1999). Reading 1 suggests that it may not be transparency alone that accounts for Dana et al.'s (2005) data. For the introduction of actions which less directly harm the other may alter the message the environment gives off about normal behaviour.

As with any experiment, however, alternative interpretations of results should be considered. A second interpretation of the present data, reading 2, is that dictator giving is subject to a "range" effect, in the sense of Parducci and Wedell's (1986) "range-frequency" model, according to which the rate of selection of an option is affected by its location and frequency in the choice set. Here options have equal frequency on subjects' decision sheets, but the perceived kindness of an action may depend on its location in the range available. Consider experiment 3, for example. In treatment 2, taking £1 might be seen as kind since a subject *could* take up to £2. Whilst in treatment 1, giving nothing is the least kind action available, so the same desire to be kind, judged relative to the meanest action, would result in a positive gift. Note that the distribution of choices in experiment 3, treatment 1, resembles a translation to the right of the distribution in treatment 2 (Fig. 2, Experiment 3). If perception of the kindness of an action is anchored to the available alternatives, this is inconsistent with orthodox social preferences. It is consistent, though, with the way kindness is incorporated into Rabin's (1993) reciprocity model, where it is judged relative to a reference point. In the model this point is left open, but in application it has been specified as follows: an action is neutral if it allocates to a partner a payoff mid-way between the highest and lowest payoff the agent could allocate them. If it allocates more (less) than this, it is judged to be kind (unkind).

If the perceived kindness of an action is subject to a range effect generally, this should have further economic consequences. Consider one example, closely related to the dictator game. Many charities nowadays issue requests for regular direct debit contributions, and/or specify a menu of donations at many different levels.[7] One rationale for this may be to exploit an interaction of the range effect and "mental accounting" (Thaler 1999). For an extended gift menu, relative to simple open-ended requests for one-off donations, may expand individuals' perceptions of the choice set pertaining to the donation decision. People may thereby come to perceive smaller donations as more selfish. A second possible consequence would be a solution to the crowding-out puzzle of public goods theory. There, changes in others' contributions to a public good and changes in income produce the same outcome. That implies strong crowding-out between individuals' contributions given that private consumption is a normal good, an extreme result generating puzzling comparative-static "neutrality" results (Bergstrom et al. 1986). For example, equilibrium public good provision is invariant to the distribution of income and the number of contributors.

[7] See, for example, the range of giving options the World Wildlife Organisation presents on its UK website: http://wwf.org.uk. At the time of writing, April 2007, this site gives many options for giving, including regular subscriptions and legacies. The one-off donation option presents the viewer with a range of donations from £5 to £1000 with a default gift of £5.

However, if the desirability of a donation depends on its position within the choice set, the consequentialism driving these results is broken.[8]

Finally, one might invoke stochastic choice models to explain the data, reading 3. There are two approaches that currently predominate in the literature, models based on Fechner's (1966 [1860]) measurement error theory, such as that incorporated into Quantal Response Equilibrium analysis (McKelvey and Palfrey 1995), and the 'random preferences' model (Loomes and Sugden 1995). Consider first utility measurement error. This posits a random disturbance to an option's underlying utility. In the dictator game a utility function should select a dominant option depending on its form: the lower bound of the choice set for a selfish agent, the upper bound for a perfect altruist, the equal split for a pure egalitarian and so on. The error would have to be such that it regularly obscures the underlying difference in desirability between giving, taking and maintaining the status quo. But payoffs here are completely transparent, so substantial measurement error is just implausible.[9] Alternatively, if the uncertainty concerns underlying preferences themselves, we have random preferences theory, according to which the utility function of each agent is randomly drawn from a set of functions prior to a choice. However, by hypothesis, the same population of utility functions is sampled in both treatments. Therefore the expectation must be that of parity of distribution of realised utility functions. It follows that we should expect the same frequency distributions of choices in T1 and T2, excepting the implications of censoring and sampling variation. This is the equivalent to the hypothesis tested, so the random preferences model makes no relevant difference in this context.

To distinguish conclusively between readings 1 and 2 further empirical work is required. This may involve exploring subjects' perceptions of the experiment, since on reading 1 these drive the result. On either reading, the results confirm that dictator game giving provides no evidence of context-free pro-social behaviour or, therefore, orthodox social preferences. The present data favour reading 1, however, since in experiment 1 the proportion exercising no restraint appears to differ across treatments: 77% give nothing in treatment 1 whilst 48% take maximally in treatment 2 (Fig. 2, Experiment 1). Also, the equal-split allocation is non-modal in experiment 1, treatment 2, where it is the endowment. In contrast it is a pronounced modal choice in experiments 2 and 3 where the endowment favours the dictator, suggesting it is not focal when it constitutes the do-nothing allocation.

[8] Suppose, for example, that one decides to donate to a homeless person on the street. One might frame the decision problem as how much of one's loose change, £c, to give. Suppose one decides on the mid-point between the most and least generous action, giving £$c/2$ (initial hypothesis). Now suppose that one had £$(c+1)$ in coins initially (counterfactual 1). This should cause one to make a larger donation; the mid-point gift becomes £$(c+1)/2$. Now suppose instead, with initial change £c, that another person unexpectedly donates £1 before one arrives (counterfactual 2). Donating £$c/2$ is still the mid-point action, so the beneficiary should receive £$c/2+1$ in total, whereas in standard theory the recipient should receive the same total contributions in both counterfactuals.

[9] There is also evidence that the Fechner model predicts poorly when options' relative payoffs are transparent. For example, Loomes and Sugden (1998) study choices over simple gambles, including pairs close in expected value where one is dominant. There, the model predicts that noise eclipses preferences, but subjects overwhelmingly choose the dominant gamble. Dictator game decisions are even simpler since there is no uncertainty.

4 Conclusions

Most subjects' generosity appears to be reversible by allowing sufficient opportunity to take. Dictator game giving is therefore not explained by unselfishness towards others that exists independently of the experimental context. By implication it is not explained by orthodox microeconomic models of social preferences. Two interpretations are offered involving different ways attitudes may depend on the experimental choice set. On reading 1, dictator giving is a product of experimental demand characteristics and attitudes to the experiment, with the choice set contributing to the cues which indicate appropriate behaviour. This has methodological implications but also potential theoretical consequences, since standard accounts of such effects invoke a more sociological model of action than economists have traditionally employed. On reading 2, dictator game giving stems from the sensitivity of decisions to an option's location in the choice set. Reading 2 points to a psychologically-enriched model of choice. Reading 1 accommodates better the experimental data and can account for the apparent external invalidity of the dictator game. It is therefore important to consider its implications both for economic theory and for experimental methodology.

Regarding theory, responsiveness to demand characteristics of experiments may indicate the role of context-specific social norms in behaviour. If so, whilst 'one man's artefact is another man's phenomenon', the phenomenon in question would appear to be sociological in character. For example, the concept of role obligation seems relevant if volunteers are motivated to behave as 'good subjects'. This coheres well with certain facts about gift-giving which are opaque to orthodox microeconomics. For example, for all their apparent efficiency compared to specific goods, cash gifts are often regarded as inappropriate outside the family, excepting specific occasions such as weddings (Douglas and Isherwood 1979). And where cash is given at weddings this is only appropriate either at the behest of the couple or as part of a specific cultural tradition—it is customary to give cash at Greek weddings, for example, but not at British ones.

Regarding experimental methodology, scepticism about laboratory social science sometimes centres on the supposedly ineliminable nature of demand characteristics. Greenwood (1982) in particular offers a philosophical critique of the lab on this basis, and similar arguments are given in Harré and Secord (1972, Chap. 3) and Dilman (1996) for examples. This literature focuses on the older experimental traditions of psychology. It seems that as a purely a priori argument the critique claims too much. For if the premise holds that consensual experiments necessarily have demand characteristics, it is still empirical whether they differ across treatments in a given case and also empirical whether this causes a treatment effect.[10] That implies a role for empirical checks such as the current exercise. It is quite possible that the dictator game is the most sensitive commonly-used game, but in the absence of checks this is a matter of speculation. Orne's (1962, 1973) studies contain suggestions for the exploration of demand characteristics using independent empirical exercises. These may merit adoption or adaptation for economics experiments, to provide insight into where there are, and are not, likely to be problems.

[10] I owe this point to Robert Sugden. The implications of Greenwood's critique for experimental economics are discussed in Bardsley (2005).

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