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EVIDENCE FROM AN EXPERIMENT ON CHARITY TO WELFARE RECIPIENTS: RECIPROCITY, ALTRUISM AND THE EMPATHIC RESPONSIVENESS HYPOTHESIS*

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This article investigates the determinants of generosity in an experiment on charity to real-life welfare recipients. It tests the effects of various measures of unconditional altruism and conditional or reciprocal altruism. The results show strong support for conditional or reciprocal altruism. However, people who are self-reported unconditional altruists make offers that are highly elastic with respect to the apparent worthiness of the recipient. One interpretation of this is that self-reported unconditional altruists have combined desires to help others and to reciprocate; unconditional altruism and reciprocal altruism may not be independent motives. I refer to this combination as *empathic responsiveness*.

Economists have long argued that social preferences may play an important role in determining generosity from the rich to the poor. Two broad classes of fairness motives have received much attention in the context of redistribution to the poor. The first is unconditional altruism broadly defined to include simple altruism (Becker, 1974), warm-glow altruism (Andreoni, 1989), and various forms of egalitarianism (Deutsch, 1985; Fehr and Schmidt, 1999; Loewenstein *et al.*, 1989). Unconditional altruism motivates people to help others at a cost to themselves, without regard for the characteristics or intentions of the recipients. There is a well-established tradition of assuming unconditional altruism to explain demands for redistribution.[1] The second is reciprocal or conditional altruism, broadly defined to include reciprocity to third parties and the principle of distributive justice known as equity.[2] The motives in this class are alike in that they all motivate costly rewards and punishments to others on the basis of what they deserve. For the purposes of this article, neither distinctions between different types of conditional altruism nor distinctions between different types of unconditional altruism are important. What matters is the difference between unconditional altruism and reciprocal or conditional altruism.

While a large number of economic experiments have investigated the roles of unconditional altruism and reciprocal altruism on redistributive behaviour, most of these experiments are abstract, with little realistic social context.[3] Further evidence that these motives matter in more realistic social contexts comes from attitudinal surveys

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[1] See, for instance, Hochman and Rogers (1969).

[2] According to the principle of equity, people should receive resources from a system that are proportional to their contributions to it (Walster *et al.*, 1978). One pertinent definition of reciprocity is *strong reciprocity*, which is a motive to reward kind people and punish unkind people at a cost to oneself (Bowles and Gintis, 2004; Fehr *et al.*, 2002).

[3] There are exceptions. See, for instance, Eckel and Grossman (1996).

and survey experiments. For example, social survey data show that beliefs that the poor are individually responsible for their poverty have large and very robust negative effects on attitudinal support for redistribution of income and wealth. This is also true in survey experiments, which show that randomly varied information about the moral worthiness of transfer recipients has large effects in the expected direction on support for government spending on them (Heclo, 1986).

Unfortunately, attitudinal data provide no incentives for carefully considered and truthful responses, and they do not measure real behaviour. This article helps bridge the gap between relatively abstract economic experiments and attitudinal data. It reports the effects of attitudinal measures of unconditional altruism and conditional altruism, as well as direct information about recipients, on actual charitable giving to real-life welfare recipients. In this experiment, donors were given money and decided how much, if any, of it to give to a welfare recipient. Donors were randomly matched with welfare recipients who differed according to their self-reported attachments to the labour force and preferences for full-time work. This information was given to the donors before they made their decisions.

The results show strong support for conditional altruism. Donors' beliefs about whether their recipients were poor because of bad luck, lack of effort, or both had large and robust effects on offers in the expected direction. These beliefs were measured after the experiment and – to rule out the possibility that subjects report beliefs to justify their decisions in the experiment – instrumented by

(1) prior beliefs about causes of poverty (measured during the week prior to the experiment) and
(2) the randomly provided direct information about the strength of the recipients' attachment to the labour force and desire to work.

In contrast, the effects of the attitudinal measure of unconditional altruism were surprising. Donors who scored high on this measure behaved like *conditional* altruists, giving significantly more to recipients who appeared industrious than to recipients who appeared lazy. Subjects who scored low on this measure gave little regardless of the characteristics of the recipient. Furthermore, when the recipient appeared lazy, there was virtually no difference between the offers from those who scored high on the measure and those who scored low. Only when the recipient appeared industrious did those who scored high on the measure give significantly more than those who scored low. The result is surprising because many researchers would have expected people who score high on the measure to give unconditionally, and people who score low to give conditionally.

One possible explanation is that the attitudinal measure of unconditional altruism may be correlated with missing variables that may explain the results. We cannot rule this possibility out. Another possibility is that there is a single motive that combines a desire to help with a desire to reciprocate. I term the combined motive to help others but to help conditionally *empathic responsiveness*. This concept blends two well-known concepts of empathy. The first is the idea that empathy is an emotion that can evoke altruistic behaviour (Batson, 1997). The second is the idea that empathy is the ability to attend and respond to another being (Preston and de Waal, 2002). Empathy in this sense may result not only in positive responses to another person, such as sympathy

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followed by helping behaviour, but also in negative responses such as anger followed by revenge.

The findings of this article open the possibility that the concept of unconditional altruism is a misinterpretation of pro-social behaviour. If one were to take a snapshot of a social situation in which everyone has the same information and beliefs about a recipient and then observe who gives more, one might previously have interpreted the high-givers as simple altruists. The problem is that perceptions of the worthiness of recipients may vary considerably from one situation to the next, so people who are empathically responsive will give a lot in some situations and little in others. If we know what beliefs people have about the worthiness of recipients, we may be able to predict who will give by knowing who is empathically responsive. This interpretation suggests that there may be a single pro-social trait that governs behaviour in many different settings (e.g., charity, incomplete labour contracts, and public redistribution) according to the information and beliefs that people have about others.

This article was originally motivated by the literature on redistributive politics, which has been paying increasing attention to the fact that people support more redistribution if they believe that the poor are industrious rather than lazy. An important question in this literature is where beliefs about causes of income come from. According to one view, the primary problem is to understand how beliefs about the causes of income determine left-wing or right-wing beliefs (Alesina and Angeletos, 2005; Piketty, 1995). A different view assumes that beliefs about the worthiness of recipients are highly dependent on context-specific information about the worthiness of recipients (Gilens, 1999; Heclo, 1986; Romer, 1994).

Following this literature, I designed this experiment to test whether or not prior beliefs and values are stable characteristics of donors with robust effects on redistribution or whether people use new information when it is available to form judgments of worthiness and make redistributive decisions. I originally predicted that prior beliefs and values would determine offers when information about the work preferences of recipients was low while direct information would determine offers when information was high. The experiment provided no evidence that new information overrides prior beliefs or values.

The remainder of this article is organised as follows: Section 1 presents the design and procedures of the experiment. Section 2 presents the survey design and constructed attitudinal measures. Section 3 presents the results. Section 4 concludes.

1. Experimental Design and Procedures

The experiment was an n-donor dictator game in which multiple donors were matched with one real-life welfare recipient.[4] Each donor was given a five dollar show-up fee and ten dollars to use in the experiment. Each donor read some information about the recipient, then privately and anonymously gave any amount of the ten dollar pie of his or her choosing – from zero to ten dollars – to the welfare recipient and kept the rest. Each recipient earned the sum of the donations from the donors she was matched with.

[4] Donors were primarily students from Carnegie Mellon University and the University of Pittsburgh, although a few non-students participated, including one homeless man. I recruited welfare recipients from a child care centre in Pittsburgh that serves low-income mothers.

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1.1. *Treatment Conditions*

Donors were randomly assigned to one of three treatment conditions. In each treatment condition, there were roughly 45 donors and one welfare recipient, with each donor and welfare recipient participating in only one treatment condition. The treatment conditions differed according to the information the welfare recipient provided about her attachment to the labour force.

Each recipient provided information about herself in a short survey containing the following questions:

1 Are you currently receiving any government assistance? Yes_____ No _____
2 What's your total household income from all sources? Less than $25,000 _____ Greater than $25,000_____
3 Do you have kids living at home? Yes_____ No _____
4 Are you single? Yes_____ No _____
5 What is your race? Caucasian_____ African American_____ Asian_____ Hispanic_____ Other _____
6 Are you: Male_____ Female_____
7 What is your age? (16-55)_____ Over 55_____
8 Are you employed full-time? Yes_____ No _____
9 If you don't work full-time, are you looking for more work?
_____Yes, I am looking for more work.
_____No, I am not looking for more work.
10 If it were up to you, would you like to work full-time? _____Yes, I would like to work full-time. _____No, I would not like to work full-time.
11 During the last five years, have you held one job for more than a one-year period? Yes_____ No_____

All three recipients were single African-American women with dependent children who did not have full-time jobs, received government assistance, had annual household incomes of less than $25,000, and were less than 55 years old.

However, they differed according to their answers to the three final questions on the survey. In one treatment condition, the welfare recipient answered 'yes' to all three of these questions ('industrious'-recipient condition), in another, the recipient answered 'no' to all three questions ('lazy'-recipient condition), and in a third, the last three questions were omitted (low-information condition).

1.2. *Procedures*

Donors visited a web site to register for the experiment and complete an attitudinal survey during the week prior to the experiment.[5] The web survey is described in detail in Section 2. When subjects arrived for the experiment, each was seated alone in a private room and handed instructions, a $5.00 bill for completing the web survey and $10.00 in one dollar bills for participating in the experiment. To increase subjects' sense of

[5] They had a three day window during which they could participate in the experiment. Subjects were not allowed to complete the survey and participate in the experiment on the same day. The number of elapsed days between completing the survey and participating on the experiment had no effect on the results.

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entitlement to the money, we paid them in cash right away and told them they had 'earned' the money for their participation in the study. The procedures also provided as much anonymity as possible. In order to assure subjects that there was no deception in the experiment, I obtained a letter from an Associate Provost of Carnegie Mellon University stating that I have certified that there is no deception in our experiment and gave each subject a copy. I gave each subject verbal instructions according to a script (see Appendix B) and then told him or her to read the written instructions carefully.[6]

Next, subjects randomly matched themselves to one of three welfare recipients by drawing a copy of a recipient's survey from a box. At this point, dictators were instructed that the recipient was a welfare recipient. The only additional information subjects received about their recipients was that contained in the recipients' surveys. Then, the donor read the recipients' survey, sealed his or her donation in an envelope and deposited it into a box. Finally, the donors completed exit surveys before leaving the experiment.

1.3. *Design Considerations*

There are two institutions in the real world that are most pertinent to this article. These are private charities – which are *n*-donor dictator games – and voting over taxes and transfers. Simple dictator games are not well suited for this study, which seeks to increase external validity by increasing the social content in and realism of the decisions subjects are asked to make. In a traditional dictator game, a gift may express concern about inequality in the earnings that two players make from the game itself. In contrast, in this experiment, the recipient is likely to earn *more* than the donor from the game, but to be perceived as poorer than the donor in real life. If subjects give to a recipient when 44 other subjects may also give, then they are more likely to be acting on beliefs that this person is, in real life, poorer than they are. Charity does have one well-known complication, however: an individual's donation may be affected by beliefs about the total amount donated by others.

Voting is an important topic for future research, but is beyond the scope of this study. The probability that a vote wins is inversely related to the number of voters. This introduces issues that have poorly understood behavioural consequences. If a subject wishes to appear or feel generous when she is not, or if she has a point to express with her vote, it will be cheaper for her to do so under voting. She will also have less control over how much she ultimately gives.

2. Survey Design and Constructed Measures

In the attitudinal survey, there is a trade-off between collecting more information from respondents and reducing the quality of the data by exhausting their attention. I imposed a page limit on the survey and only included questions that were most pertinent to the study. Readers interested in other potential determinants of actual giving – including religiosity, and detailed socioeconomic data – may consult the large social surveys and the associated literature as well as several papers that have tested the effects

[6] I made minor changes to the procedures over the course of the first day (the first 20 subjects) to reduce noise and encourage subjects to read their recipient's survey. I gave no verbal instructions to the first 20 subjects. The results presented below are even stronger when I drop the first 20 observations.

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of various personality and attitudinal measures on behaviour in bargaining experiments.[7]

The questions from the web survey are presented in the first column of the Table in Appendix A. The second column of the Table states whether each question had a significant relationship with offers in the pooled sample. A 'yes' in this column means that both the ordinary least squares coefficient (with robust standard errors) and the Spearman rank correlation were significant, a 'no' means neither was significant, and 'Spearman' indicates that only the Spearman rank correlation was significant.

The survey contains attitudinal measures recommended by psychologist Linda Skitka (in correspondence, 2002) as likely to predict giving to welfare recipients. These measures include: scales designed by Katz and Hass (1988) on humanitarianism and egalitarianism (questions 4-13) and the Protestant work ethic (questions 14–23); and a battery of questions about welfare recipients from the General Social Survey (questions 24–29). The survey also contains questions (1, 30, and 31) that are strong predictors of attitudes to redistribution and inequality in nationally representative data from the 1998 Gallup Organization Social Audit, 'Haves and Have-nots' (Fong, 2001). I also included questions on general beliefs about the fairness of the income distribution and efficiency of redistribution (questions 2 and 3).

Katz and Hass (1988) designed the Humanitarianism-Egalitarianism scale to measure communalism which, according to sociologists, embraces humanitarianism and egalitarianism and is one of two core American values (Lipset, 1967). The other core value is the Protestant work ethic. In subsequent research, the humanitarianism scale has been used in combination with other measures, including whether the respondent is liberal or conservative, to create a single measure of 'conservative-liberal ideology' (Skitka and Tetlock, 1993).

2.1. *Constructed Measures of Prior Beliefs and Humanitarianism-Egalitarianism*

I constructed two measures from the web survey questions. All of the individual questions used to construct the two measures used in this article had significant correlations with offers in the pooled sample.[8] The first measure combines the questions about the causes of poverty and failure (17, 19, and 30) and increases in beliefs that circumstances beyond control rather than lack of effort cause poverty. These questions do not all have the same response scales, so I used their first principle component, rather than their sum, and then standardised it to a mean of zero and standard deviation of one.[9] I refer to this as the measure of prior beliefs about the causes of poverty and failure, or just *prior beliefs* for short.

[7] See, for instance, Ben-Ner *et al.* (2004), Carpenter (2003), Glaeser *et al.* (2000), Gunnthorsdottir *et al.* (2002), and van Dijk *et al.* (2002).

[8] There was a noteworthy null finding in the Table in Appendix A: questions about the causes of wealth and success and about the prevalence of opportunity to get ahead had no significant effects on offers (except for one that had a significant effect in one of the tests). Elsewhere I have found analogous results using nationally representative survey data (Fong, 2004). Beliefs about specific social categories (e.g., 'rich people' or 'poor people') have larger effects on attitudes to redistribution to or from those social categories than general beliefs or beliefs about other social categories.

[9] Principal components analysis organises the data into orthogonal linear combinations. The first principal component is the linear combination of the data that has the maximal variance.

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The second measure is the standardised sum of the questions in the Katz-Hass Humanitarianism-Egalitarianism scale, except question 12. I excluded question 12 because it asks about the causes of criminality and is more related to the measure of causes of poverty and failure than to the importance of helping others. I refer to this measure as the *HE scale*.

2.2. *Posterior Beliefs about Recipient*

The exit survey included the following question: 'Which if the following explains why your recipient is poor: a) lack of effort on his or her part, b) circumstances beyond his or her control, c) both.' I refer to this question as posterior beliefs about why the donor's recipient is poor, or just *posterior beliefs* for short. I use this measure in various ways, as described below.

3. Results

We conducted the web survey and experiment in June of 2002. Three subjects participated as recipients and 144 participated as dictators. Twenty dictators participated on the first day of the experiment, 31 on the second day, and 93 on the third day. There were 44 subjects each in the low-information and lazy-recipient conditions and 56 in the industrious-recipient condition.

One of the donors in the industrious-recipient condition was a homeless man. I include him in the analysis and report when he has noteworthy effects on the results. Another donor in the low-information condition gave $15.00 (by donating his show-up fee along with the whole pie). Aside from increasing the average offer in the low-information condition, this did not have noteworthy effects on the results.

3.1. *Effects of Prior Beliefs and Direct Information on Posterior Beliefs*

There was a very strong effect of the treatment conditions on posterior beliefs about why the donor's recipient was poor. Table 1*a* presents the frequencies of the posterior belief responses for each treatment condition. Beliefs that lack of effort matters were much stronger in the lazy-recipient condition and beliefs that circumstances matter were much stronger in the industrious-recipient condition. According to the Kruskal-Wallis test, the distribution of responses was significantly different across treatments at the 1% level.

Table 2 presents an ordinary least squares regression predicting the posterior belief, where it is coded so that zero is lack of effort, 0.5 is both lack of effort and bad luck, and 1 is bad luck. Prior beliefs about causes of poverty and failure have a highly significant effect in the expected direction. The interactions between prior beliefs and treatment conditions show that the effect of prior beliefs on posterior beliefs is virtually the same in all treatment conditions. Direct information also has the expected effects. The effects of being in the low-information condition and the industrious-recipient condition as opposed to the lazy-recipient condition are both positive and highly significant. For someone with average prior beliefs, the predicted posterior belief is 0.341 in the lazy-recipient condition, 0.516 in the low-information condition, and 0.706 in the industrious-recipient condition.

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Table 1

Selected Summary Statistics in Each Treatment.

	Lazy-Recipient	Low-Info	Industrious-Recipient
(*a*)		Frequencies (%)	
Posterior belief: lack of effort.* (dummy)	16	6	1
	(36.4)	(13.6)	(1.85)
Posterior belief: both (dummy)	25	30	31
	(56.8)	(68.2)	(57.41)
Posterior belief: circumstances (dummy)	3	8	22
	(6.8)	(18.2)	(40.74)
N	44	44	54
(*b*)		Mean (s.d.)	
Mean offer	1.841	3.205	2.786
	(2.261)	(3.310)	(2.728)
N	44	44	56
(*c*)		Mean (s.d.)	
Mean offer omitting homeless man	1.841	3.091	2.836
and top-coding offers to $10.00†	(2.261)	(2.963)	(2.727)
	44	44	55

*Posterior beliefs are responses to the exit survey question: 'Which of the following explains why your recipient is poor: Lack of effort on his or her part, circumstances beyond his or her control, or both?'
†A homeless man participated in the industrious-recipient condition and gave $0.00. One subject in the low-information condition broke the rules of the game by giving his $5.00 show-up fee along with the whole $10.00 pie.

Table 2

OLS Regressions Predicting Posterior Belief That Recipient is Poor Because of Circumstances as Opposed to Lack Of Effort.†

	Posterior belief
Low info (treatment dummy)	0.175***
	(0.056)
Prior belief in luck‡ (std. dev. units, zero mean)	0.123***
	(0.041)
Low info × prior belief	−0.012
	(0.059)
Industrious recipient (treatment dummy)	0.365***
	(0.053)
Industrious recipient × prior belief	−0.016
	(0.053)
Constant	0.341***
	(0.039)
Observations	142
R-squared	0.34

Standard errors in parentheses. *significant at the 10% level; **significant at the 5% level; ***significant at the 1% level.
†Posterior beliefs are responses to the exit survey question: 'Which of the following explains why your recipient is poor: Lack of effort on his or her part, circumstances beyond his or her control, or both?' In this regression, these beliefs are coded into a single measure where 'lack of effort' is zero, 'both' is 0.5, and 'circumstances beyond his or her control' is 1.
‡'Prior belief' is a measure, constructed from questions taken prior to the experiment, of the belief that poverty and failure are caused by circumstances beyond control rather than lack of effort.

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3.2. *Conditional Altruism*

There is a highly significant and very robust effect of posterior beliefs on offers. To address the possibility of reverse causality from offers to the posterior belief, I instrument the posterior belief with prior beliefs and the randomly assigned treatment conditions.

Table 3 presents instrumental variables regressions of offers on predicted values of posterior beliefs. In column 1, posterior beliefs are instrumented with prior beliefs and the treatment conditions. In column 2, dummies for male, homeless, and US born are added to the model. That is, the three demographic dummies are regressors in the model and additional instruments for beliefs. In column 3, the HE scale is added to the model. In all three columns, the donor's belief that his or her recipient is poor because of circumstances rather than lack of effort has large and significant positive effects on offers. The effect ranges from \$3.22 to \$4.09.

Together, Tables 1–3 show that there was a very strong combined effect of direct information and prior beliefs operating on offers via posterior beliefs. Taken separately, the effects of prior beliefs and direct information on offers were not as strong. The following two sub-sections summarise these weaker effects.

3.2.1. *Effects of direct information: the main treatment effects*

Table 1*b* shows that the mean offers in the lazy-recipient, low-information, and industrious-recipient conditions were \$1.84, \$3.21, and \$2.79, respectively.[10] According to a Mann-Whitney test, the distributions of offers in the lazy and industrious-recipient

Table 3

Instrumental Variables Regressions Predicting Offers With Predicted Values of Posterior Belief About Why Recipient Is Poor[†]

	Offer		
	(1)	(2)	(3)
Posterior belief (IV)	3.676***	4.087***	3.224**
	(1.269)	(1.303)	(1.357)
Male (dummy)		−0.766	−0.441
		(0.468)	(0.467)
US born (dummy)		0.117	0.171
		(0.465)	(0.447)
Humanitarianism-Egalitarianism scale			0.729***
(std. dev. units, zero mean)			(0.256)
Constant	0.652	0.745	1.011
	(0.717)	(0.770)	(0.763)
Observations	142	142	142
R-squared	0.07	0.09	0.17

IV standard errors in parentheses. *significant at the 10% level; **significant at the 5% level; *** significant at the 1% level. Columns 2 and 3 also include a dummy for homeless.

[†]Posterior beliefs are responses to the exit survey question: 'Which of the following explains why your recipient is poor: Lack of effort on his or her part, circumstances beyond his or her control, or both?' In this regression, these beliefs are coded into a single measure where 'lack of effort' is zero, 'both' is 0.5, and 'circumstances beyond his or her control' is 1.

[10] Panel (*c*) presents the mean offers when offers are top-coded to \$10.00 and the homeless man is omitted.

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conditions were significantly different at the 10% level ($p = 0.053$) and offers in the low-information condition and lazy-recipient condition were significantly different at the 5% level ($p = 0.031$). Note that the mean offer was higher in the low-information condition than the industrious-recipient condition, which was unexpected. However, none of the tests conducted in the analysis showed a significant difference between offers in the low-information and industrious-recipient treatment conditions. In fact, none of the treatment effects on offers were robust. For example, in median regressions predicting offers, there were no significant effects of the treatment dummies.

As Table 1*a* and Table 2 clearly show, the failure to find robust significant treatment effects is not due to a manipulation failure. However, there are several other reasons for the fairly weak effects of the treatment conditions. First, when the homeless man is excluded, offers in the lazy-recipient condition and the industrious-recipient condition are significantly different at the 5% level, according to the Mann-Whitney test. Second, there was a floor effect which may have reduced variation in the offers (29% of the donors gave \$0.00). Third, the treatment conditions are fairly subtle, all involving recipients who belong to the same social category: single African-American mothers on welfare.

Finally, it is worth noting that offers in the lazy-recipient treatment condition were higher than the mean offers in double-blind single-donor dictator games, but lower than offers in many other single-donor dictator games that have been reported.[11] The design of the experiment encourages low offers in a number of ways. First, following Hoffman *et al.* (1996), we offered as much anonymity to donors as possible. Second, we used language and procedures intended to make subjects feel entitled to the ten dollar pie. Third, we used an *n*-donor dictator game rather than a two-person game, which may cause an individual's offer to decrease in the total amount given by others. Fourth, people do not like to support welfare recipients, probably because they tend to be seen as a morally unworthy social category and because welfare is seen to promote morally undesirable behaviour (Gilens, 1999; Heclo, 1986; Luttmer, 2001). There are, however, some effects that may operate in the other direction. People exhibit more generosity to recipients who have been identified and when the social distance between players has been reduced by providing information about the identity of those who play against one another (Bohnet and Frey, 1999; Small and Loewenstein, 2003).

3.2.2. *Effects of prior beliefs*

In the pooled sample, the Spearman rank correlation between offers and prior beliefs that luck matters is 0.212 ($p = 0.011$). Within each treatment condition, there are significant Spearman rank correlations in the lazy-recipient condition (correlation = 0.393, p-value = 0.008) and the low-information condition (correlation = 0.355, p-value = 0.018). However, all of these associations are sensitive to specification and the choice of estimator.

Thus, the separate effects of prior beliefs and the treatment conditions are much weaker than the combined effect on offers via posterior beliefs that is reported in Section 3.2 above.

[11] See Camerer (2003) for a review of bargaining game results.

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3.3. *Attitudinal Measure of Unconditional Altruism*

In the pooled sample, the Spearman rank correlation between the HE scale and offers is 0.332 ($p < 0.001$). Unreported analyses show that this association is very robust. Within treatment conditions, the Spearman rank correlations between the HE scale and offers were 0.478 ($p = 0.0002$) in the industrious-recipient condition, 0.382 ($p = 0.011$) in the low-information condition and 0.075 ($p = 0.629$) in the lazy-recipient condition. The association between the HE scale and offers in the industrious-recipient condition is very robust. The effect of the HE scale is highly significant using a variety of estimators (e.g., OLS, Tobit and median regressions) and specifications (e.g., including prior beliefs and dummies for male, homeless and US born).

The combination of the strong effect of the HE scale in the industrious-recipient condition and the weak effect in the lazy-recipient condition is surprising. This pattern is robust enough to warrant further analysis. Table 4 presents regressions predicting offers for each treatment condition with dummy variables for having an HE score in the upper third and middle third of the sample. The omitted category is having an HE score in the lowest third of the sample. The Table presents both OLS and median regressions for each treatment condition. First, a very important point is that in *all* columns, the estimated constants are very similar in magnitude. This means that the mean and median offers from those in the lowest third of the distribution of the HE score do not vary with the treatment conditions. Second, it shows that the effect of scoring in the middle of the distribution of the HE scale is weak at best.

The interesting effects are driven by those who score in the upper third of the distribution of the HE scale. These effects are highlighted with bold type in Table 4. In the industrious-recipient condition, those who scored high on the HE scale gave sig-

Table 4

Median and OLS Regressions Predicting Offers in Each Treatment Condition

	Offers in lazy-recipient condition		Offers in low information condition		Offers in industrious-recipient condition	
	Median Reg.	OLS	Median Reg.	OLS	Median Reg.	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Mid. HE score[†]	1.000	0.950	3.000	2.217	−0.000	0.474
(dummy)	(1.446)	(0.884)	(1.385)**	(1.251)*	(1.362)	(0.634)
High HE score[‡]	−0.000	0.691	2.000	2.240	4.000	3.263
(dummy)	(1.514)	(0.861)	(1.382)	(1.218)*	(1.303)***	(0.799)***
Constant	1.000	1.250	1.000	1.583	1.000	1.526
	(1.129)	(0.659)*	(1.010)	(0.932)*	(0.954)	(0.400)***
Observations	44	44	44	44	56	56
Pseudo R^2	0.07		0.10		0.16	
R^2		0.03		0.09		0.29

Standard errors in parentheses. *** significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level. The omitted category is those who scored in the lowest third on the Humanitarianism-Egalitarianism scale.

[†]'Mid HE score' is a dummy variable that equals 1 if the respondent scored in the middle third of the distribution of the Humanitarianism-Egalitarianism scale.

[‡]'High HE score' is a dummy variable that equals 1 if the respondent scored in the top third of the distribution of the Humanitarianism-Egalitarianism scale.

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nificantly more than those who scored low, but in the lazy-recipient condition, they gave virtually the same amount. In the industrious-recipient condition, the response from high HE scorers was dramatic. The median offer from high HE scorers was \$5.00, or 50% of the pie, which was \$4.00 more than the median offer from those who scored low on the HE scale. The mean offer from those who scored high on the HE scale was \$4.79, which was \$3.26 more than the mean offer from those who scored low on the HE scale.

Looking across the rows of Table 4 shows that those who scored low on the HE scale gave virtually the same amount in all three treatment conditions while those who scored high on the HE scale gave substantially more in the industrious-recipient condition than in the lazy-recipient condition. The behaviour of those who scored in the middle of the HE scale appears somewhat erratic. A simple way to present the significance levels of these differences is to estimate the interaction effects the other way: to estimate treatment effects within the sub-samples of subjects who had low, middle and high scores on the HE scale. Table 5 presents these estimates. The estimated means and medians for each treatment condition in each sub-group can be read from either Table 4 or Table 5. What Table 5 shows is that the treatment effects of those who score high on the HE scale are highly significant using both estimators (columns 5 and 6). In contrast, none of the treatment effects were significant among those who scored in the bottom or the middle of the distribution of the HE scale (columns 1–4).

4. Conclusion

This article tested the effects of context-rich attitudinal measures of conditional or reciprocal altruism and unconditional altruism on offers in a charity game involving giving to real-life welfare recipients. There was strong evidence of conditional altruism. Attitudinal measures of beliefs that the recipient in the experiment was poor because of

Table 5

Median and OLS Regressions Predicting Offers In Sub-Samples of Subjects Who Scored In The Lowest Third, Middle Third, and Highest Third of The Distribution of The Humanitarianism-Egalitarianism Scale (HE scale)

	Low score on HE scale		Mid. score on HE scale		High score on HE scale	
	Median Reg	OLS	Median Reg	OLS	Median Reg	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Low info (treatment dummy)	0.000	0.333	2.000	1.600	2.000	1.882
	(1.503)	(0.788)	(1.351)	(1.170)	(1.477)	(0.994)*
Industrious recipient (treatment dummy)	0.000	0.276	−1.000	−0.200	4.000	2.848
	(1.416)	(0.635)	(1.309)	(0.715)	(1.386)***	(0.968)***
Constant	1.000	1.250	2.000	2.200	1.000	1.941
	(1.120)	(0.491)**	(0.932)**	(0.517)***	(1.079)	(0.677)***
Observations	43	43	48	48	53	53
Pseudo R^2	0.00		0.04		0.11	
R^2		0.01		0.08		0.15

Standard errors in parentheses. The omitted category is the lazy-recipient condition.
* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

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bad luck rather than laziness – instrumented by both prior beliefs that bad luck rather than laziness causes poverty and randomly provided direct information about the recipient's attachment to the labour force – have large and very robust positive effects on offers.

The attitudinal measure of unconditional altruism also had significant and robust positive effects on offers in the pooled sample. However, results across treatment conditions exhibited a surprising and very robust pattern that is inconsistent with unconditional altruism. Those who scored high on the humanitarianism measure were highly responsive to the perceived worthiness of recipients. Their median offer to the recipient who appeared industrious was $5.00 out of a $10.00 pie, while the median offer of those who scored low on the measure was only $1.00. In contrast, when the recipient appeared lazy, the median offer from those who scored high on the measure was only $1.00 and did not differ from the median offer of those who scored low on the measure.

How might we interpret the surprising effects of the measure of unconditional altruism? One possibility is that it may be correlated with unknown missing variables that drive the results. Another possibility is that instead of two separate motives – namely unconditional and conditional altruism – there is a single pro-social trait that combines a desire to help and a desire to reciprocate. I refer to this trait as empathic responsiveness.

Appendix A: The Web Survey

	Signif. in pooled sample?*
1 There is plenty of opportunity in America today. Anyone who works hard can go as far as he or she wants.	No
2 The distribution of income in Pennsylvania is basically fair.	No
3 It is possible to redistribute income in this country without hurting the economy very much.	No
4 One should be kind to all people.	Yes
5 One should find ways to help others less fortunate than oneself.	Yes
6 A person should be concerned about the well being of others.	Yes
7 There should be equality for everyone – because we are all human beings.	Yes
8 Those who are unable to provide for their basic needs should be helped by others.	Yes
9 A good society is one in which people feel responsible for one another.	Yes
10 Everyone should have an equal chance and an equal say in most things.	Spearman
11 Acting to protect the rights and interests of other members of the community is a major obligation for all persons.	Yes
12 In dealing with criminals the courts should recognise that many are victims of circumstance.	No
13 Prosperous nations have a moral obligation to share some of their wealth with poor nations.	Yes
14 Most people spend too much time in unprofitable amusements.	No
15 Our society would have fewer problems if people had less leisure time.	No
16 Money acquired easily is usually spent unwisely.	Yes
17 Most people who don't succeed in life are just plain lazy.	Yes
18 Anyone who is willing and able to work hard has a good chance of succeeding.	No
19 People who fail at a job have usually not tried hard enough.	Yes

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	Signif. in pooled sample?*
20 The person who can approach an unpleasant task with enthusiasm is the person who gets ahead.	No
21 If people work hard enough they are likely to make a good life for themselves.	No
22 I feel uneasy when there is little work for me to do.	No
23 A distaste for hard work usually reflects a weakness of character.	No
24 Welfare makes people work less than they would if there wasn't a welfare system.	Yes
25 Welfare helps people get on their feet when facing difficult situations such as unemployment, a divorce or a death in the family.	Yes
26 Welfare encourages young women to have babies before marriage.	Spearman
27 Welfare helps keep people's marriage together in times of financial problems.	No
28 Welfare helps to prevent hunger and starvation.	No
29 Welfare discourages young women who get pregnant from marrying the father of the child.	No
30 Which of the following more often explains why a person is poor: a) lack of effort on his or her part b) circumstances beyond his or her control c) both	Yes
31 Which of the following more often explains why a person is rich: a) strong effort on his or her part b) circumstances beyond his or her control c) both	No

Note: Questions 1–29 are scaled from 1 (disagree strongly) to 5 (agree strongly). The survey also asked for the respondent's race, sex, if any friends or family ever received welfare, and the total household income during the senior year of high school.

*'Yes' denotes that the variable had a significant effect on offers in the pooled sample ($n = 144$) using both the Spearman Rank correlation coefficient and ordinary least squares with robust standard errors. 'No' denotes that neither test showed a significant effect of the question on offers. 'Spearman' denotes that only the Spearman rank correlation coefficient was significant.

Appendix B: Raw Data of Offers

	Lazy-recipient		Low-info		Industrious-recipient		Total	
Offer Size	Freq.	%	Freq.	%	Freq.	%	Freq.	%
0	19	43.18	10	22.73	13	23.21	42	29.17
1	3	6.82	7	15.91	10	17.86	20	13.89
2	10	22.73	6	13.64	9	16.07	25	17.36
3	4	9.09	3	6.82	7	12.5	14	9.72
4	1	2.27	5	11.36	0	0	6	4.17
5	5	11.36	7	15.91	10	17.86	22	15.28
6	0	0	1	2.27	2	3.57	3	2.08
7	1	2.27	1	2.27	1	1.79	3	2.08
8	0	0	0	0	1	1.79	1	0.69
9	0	0	0	0	0	0	0	0
10	1	2.27	3	6.82	3	5.36	7	4.86
15	0	0	1	2.27	0	0	1	0.69
Total	44	100	44	100	56	100	144	100

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Appendix C: Instructions and Raw Data on Offers

Verbal instructions:

This is an economics experiment and you will be paired with a REAL person in a drawing which works like this. In the blue box [pointed to it] there are copies of surveys completed by *other* people. Draw *one* envelope, take the survey out of the envelope, and read the survey *CAREFULLY*. You will then have a chance to decide if you would like to allocate any of the money for participating today to the other person. That is, you will decide if you would like to give the other person any amount from zero to ten dollars. After you have made your decision, put the person's survey back in the envelope, put any money you may choose to allocate in the envelope, close the envelope, seal it carefully, and put one ID sticker on the outside. It's a good idea to put the sticker on the seal of the envelope so we can verify that no one tampers with it.

Now, there's one more thing. Each person who has a copy of their survey in that box will be paired with about 45 people in this experiment. That means that the total amount of money they will earn in this experiment is the *total* that you and about 44 other people choose to allocate that person.

Now I'd like you to read three sets of instructions carefully. First, carefully read this set [point to set on desk]. Second carefully read the short paragraph outside of the box. Third, draw an envelope, and *CAREFULLY* read the survey. Then, after that there is a short set of instructions. Finally, when you are done, take your second ID sticker and see one of us. Thanks a lot. Let me know if you have any questions.

Written instructions

You have completed an Internet survey and are about to participate in an economics experiment. You have been paid $5.00 for the survey and $10.00 for participating in an experiment. You have also received stickers which state your experiment ID number. This number matches the number that was generated for you on-line when you registered for the experiment. Please do not lose these stickers because you will need them to complete the experiment.

There are two boxes in this room. The blue one holds envelopes surveys filled out by real people about their income sources and work activity. When the time comes, you will go to the blue box and randomly draw exactly one of these envelopes. You will have an opportunity to reallocate any portion of the $10.00 you received for participating in this experiment to the person whose survey you draw. You will place the survey, along with any money you may wish to reallocate, back into the envelope and then deposit the envelope into the red box.

You may be aware that in some studies, subjects are not always told the truth. This study is an exception. To assure you that all procedures have been and will be carried out exactly as stated in the instructions and that there is no deception in this experiment, we have asked the Associate Provost of Carnegie Mellon University, Dr Susan Burkett, to attest to the fact that there is no deception in this experiment, that all procedures have been and will be carried out exactly as stated in the instructions, and that all allocations of money made by subjects will be paid in exactly the amounts chosen by the subjects. A copy of this certification with Associate Provost Susan Burkett's signature is attached and the original is posted by the blue box.

Feel free to read these instructions as often as you like. If you have any questions, please raise your hand and an experimenter will assist you. If you feel you understand these instructions, please go to the blue box and follow the instructions posted there.

Text of sign placed on box that contained recipient surveys:

This box contains copies of surveys completed by welfare recipients (e.g., recipients of government Temporary Assistance to Needy Families and/or Food Stamps). In this drawing, each

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welfare recipient will be paired with roughly 45 subjects. Therefore, the welfare recipient you draw will be paired with 44 other subjects. Please draw a survey and read it carefully.

Instructions included with recipient survey in the envelope drawn from the box:

If you would like to reallocate any of the $10.00 that you received for participating in this experiment to the welfare recipient whose survey you drew, please place the money in the large yellow envelope that contained the survey.

Regardless of the amount you allocate please put the welfare recipient's survey back into the large yellow envelope, seal it, and affix one ID sticker to the outside. Now deposit this envelope in the red box on your right. At the end of the day we will record all reallocations by ID number and then we will write a check to each recipient in the amount of her earnings from the experiment. We will send the check by certified mail.

When you have made your decision and deposited the envelope containing the survey and any money you wish to reallocate, please see an experimenter. Be sure to take your remaining ID sticker with you.

Carnegie Mellon University

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