

EMPATHIC RESPONSIVENESS: EVIDENCE FROM A RANDOMIZED EXPERIMENT ON GIVING TO WELFARE RECIPIENTS*

Christina M. Fong
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Abstract

This paper reports a surprising finding from an experiment on giving to welfare recipients. The experiment tests how offers of money in n-donor dictator games are affected by 1) donors' humanitarian and egalitarian values and 2) direct information about the recipients' work-preferences. People who are self-reported humanitarians and egalitarians have giving that is highly elastic with respect to the apparent worthiness of the recipient. Among high scoring humanitarian-egalitarians, the median offer to a recipient who appeared industrious was \$5.00, while the median offer to a recipient who appeared lazy was only \$1.00. Among low scoring humanitarian-egalitarians, the median offer was \$1.00 in both conditions. I refer to this combination of altruism and equity/reciprocity as *empathic responsiveness*. This finding can be rationalized by a model of inequity aversion.

Keywords: Fairness, Social Preferences, Redistributive Politics, Empathy, Equity, Attitudinal Measures, Dictator Games, Public Goods Experiments

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Contact: Christina Fong, Department of Social and Decision Sciences, Carnegie Mellon University, Pittsburgh, PA 15213. Tel. 412-268-8168. email: fong2@andrew.cmu.edu

1. Introduction

The purpose of this paper is to report and interpret a surprising finding from an experiment on the determinants of giving to the poor. In the experiment, each donor received a sum of money (the pie) and then gave an amount of his or her choosing – from 0% to 100% of the pie – to a real-life welfare recipient. Prior to the experiment, I took an attitudinal measure of donors' self-reported altruistic motives. The measure is the Katz-Hass (1989) humanitarianism-egalitarianism scale (henceforth the HE scale) which increases in agreement with the following statements (scaled from 1, “disagree strongly”, to 5, “agree strongly”):

One should be kind to all people.

One should find ways to help others less fortunate than oneself.

A person should be concerned about the well being of others.

There should be equality for everyone – because we are all human beings.

Those who are unable to provide for their basic needs should be helped by others.

A good society is one in which people feel responsible for one another.

Everyone should have an equal chance and an equal say in most things.

Acting to protect the rights and interests of other members of the community is a major obligation for all persons.

Prosperous nations have a moral obligation to share some of their wealth with poor nations.

The donors were then matched with recipients who differed according to how industrious or lazy they appeared on the basis of their answers to a survey about their demographic characteristics, work preferences, and work histories.

The surprising finding of this paper is that donors who scored high on the HE scale gave significantly more to recipients who appeared industrious (and thus morally worthy of support) than to recipients who appeared lazy (and thus morally unworthy of support). 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 HE scale 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 (including this one) would have expected people who score high on the measure to give unconditionally, like Jesus, and people who score low to give conditionally. Instead, those who scored higher on the HE scale gave like Martin Luther who, in 1523, wrote a proposal for organized poor relief that was to benefit only those who were clearly needy and had “honourably labored at their craft or in agriculture.” (Martin Luther, 1523, cited in Piven and Cloward (1971) p. 9.)

I term the combined motive to help others but to help conditionally as *empathic responsiveness*. Below, I present a model of inequity aversion – an extended version of the model of inequity aversion by Fehr and Schmidt (1999) – that offers a formal interpretation of empathic responsiveness. The model shows that for a donor’s giving to respond to the perceived worthiness of recipients, she must have a high level of inequity aversion *and* be focused primarily on inequity between herself and the potential recipient (as opposed to caring about inequity between herself and all players in the game, which may include people who she does not interact with and is in no position to help). This definition of empathic responsiveness is similar to a definition of empathy by Preston and De Waal (2001). They define empathy as the ability to attend and respond to another being.¹ Empathy in this sense may result not only in positive responses to another person, such as sympathy followed by helping behavior, but also in negative responses such as anger followed by revenge. The common element of these two

¹ Another well-known interpretation of altruism is the empathy-altruism hypothesis of Daniel Batson and colleagues. The empathy-altruism hypothesis is that empathy is an emotion that can evoke altruistic motives (Batson, 1997b). This area of research focuses on the determinants of empathy-induced helping. For example, imagining how another feels can increase empathy for that person (Batson, 1997a). Adam Smith had similar insights in his *Theory of Moral Sentiments* (1759), where he wrote about the role that one’s imagination about the experiences of another may play in social behavior (Smith, 2002).

definitions is that they both require two things: i) a heightened focus on the potential recipient and ii) responsiveness to judgments about whether or not the potential recipient deserves help or punishment (which, broadly defined, may take the form of withholding of assistance).

The findings have implications for our understanding of two classes of social preferences: reciprocal altruism and unconditional altruism. By reciprocal altruism, I mean a class of motives that are alike in that they all motivate costly rewards and punishments to others on the basis of what they deserve. For example, it is widely accepted that people are less generous to the poor if they believe that the poor are responsible for their own poverty (e.g., because they are lazy), and that people are more generous if they think the poor are not responsible for their own poverty (e.g., if they are poor despite hard efforts).² This effect could be driven either by concerns about the equity principle of distributive justice or by various notions of reciprocity.³ In contrast,

² This idea has long been held outside of economics. See Williamson (1974) for early evidence, Miller (1992) for a review, and Weiner (1995) for more recent research in psychology. Recently, a growing number of economists have studied this and related ideas (Alesina and Angeletos, 2002; 2001; Benabou and Tirole, 2002; Besley and Coate, 1992; Bowles and Gintis, 2000; Fong, 2001; Fong et al., forthcoming; Konow, 2002; Lindbeck et al., 1999; Moffitt et al., 1998; Piketty, 1995).

³ According to the principle of equity, people should receive resources from a system that are proportional to their contributions to it (see Walster et al. (1978) for a review). Reciprocity, on the other hand, has been defined in a number of ways, at least two of which are pertinent here. It can be interpreted as one's reaction to the perceived good or bad intentions of others (Rabin, 1993). Individuals may withhold assistance to the poor if they believe that the poor intentionally rely on taxpayer generosity to consume leisure. They may support assistance to the poor if they believe that the poor intended to work hard and make it on their own but failed despite their efforts. Another definition of reciprocity that could explain the effects discussed here is *strong reciprocity*, which is a motive to reward people (at a cost to oneself) who generate benefits for a third party and punish people (at a cost to oneself) who impose costs on a third party (Bowles and Gintis, 2002; Fehr et al., 2002).

unconditional altruism motivates people to help others at a cost to themselves, without regard for the characteristics or intentions of the recipients. Examples of unconditional altruism include simple altruism (Becker, 1974), warm-glow altruism (Andreoni, 1989) and egalitarianism (see Deutsch (1985) for a review). For the purposes of this paper, neither distinctions between different types of reciprocal altruism nor distinctions between different types of unconditional altruism are important. What matters is the difference between reciprocal altruism and unconditional altruism.

The findings of this paper open the possibility that the concept of unconditional altruism is a misinterpretation of pro-social behavior. 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. In this case, unconditional altruism will not accurately predict an individual's behavior across situations. In contrast, the empathic responsiveness interpretation says that if we know what beliefs people have about the worthiness of recipients, we can predict who will give by knowing who is empathically responsive. This interpretation suggests that there may be a single pro-social trait that can predict behavior in many different settings (e.g., charity, incomplete labor contracts, public redistribution) as a result of the information and beliefs that people have about others.

The findings do not fundamentally change what we know about reciprocal altruism. They do, however, improve our understanding of how reciprocal altruism works and who the altruistic reciprocators are. The findings suggest that it is the people who want to help others – that is, the

people who are the most focused on inequity between themselves and people whom they can help – whose giving is the most likely to be elastic with respect to the perceived worthiness of the recipient. This is illuminating, because, as mentioned above, prior to this experiment, we might have expected those who say they want to help others to give unconditionally to everyone. We also might have expected those who give conditionally to be the ones who give less on average and are less willing to help others – but this is not the case. The results also identify a useful methodological tool for identifying who is likely to reciprocate. Measures such as the HE scale may enable future research to identify potential reciprocal altruists and test whether or not reciprocal altruism is a stable motive that drives behavior in predictable ways across very different settings.

This paper 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. Perhaps the most important question this literature faces is where beliefs about causes of income come from. In an influential paper, Thomas Piketty (1995) focuses on the role of personal economic experiences in learning about the causes of income. According to this view, the key problem is to understand the determinants of left-wing or right-wing beliefs, i.e. political orientation. A different view assumes that beliefs about the worthiness of recipients are highly dependent on direct information that people get from their economic environment about causes of income or worthiness of recipients. According to this view, understanding left-wing or right-wing political orientation is not sufficient to understand redistributive politics. Beliefs about causes of income or judgments about the worthiness of recipients are at least partially endogenous to choices about how to redistribute and

how much to redistribute (Alesina and Angeletos, 2002; Fong et al., forthcoming; Gilens, 1999; Heclo, 1986; Romer, 1994).

Following this literature, I designed this experiment to test the effects of direct information against the effects of prior beliefs and values. The idea was 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. Instead, the experiment produced the surprise finding of empathic responsiveness described above.

The remainder of this paper is organized as follows: Section 2 presents the design and procedures of the experiment. Section 3 presents the results. Section 4 interprets the findings by extending the Fehr-Schmidt model of inequity aversion to accommodate perceptions of the worthiness of others and the ability to focus on inequity between oneself and people whom one can help. Section 5 concludes.

2. Experimental Design and Procedures

The experiment was an n-donor dictator game. In each of three treatment conditions, about 45 donors were matched with a single real-life welfare recipient.⁴ Each donor was given a five

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

dollar show-up fee and ten dollars for participating 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. So, the donor earned the five dollar participation fee plus ten dollars minus the donation to the recipient. Each recipient earned the sum of the donations from the donors she was matched with.

The experiment used three welfare recipients, one for each treatment. Each recipient provided information about herself in a short survey. All three 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.⁵ However, they differed according to their answers to the three final questions on our recipient recruitment survey, which were the following:

- 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.
- 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.
- During the last five years, have you held one job for more than a one-year period? Yes _____ No _____

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).

⁵ I chose these broad demographic categories in order to facilitate finding a suitable recipient for each condition.

Donors visited a web site to register for the experiment and complete the attitudinal survey during the week prior to the experiment.⁶ When subjects arrived for the experiment, experimenters seated each of them alone in a private room and handed them instructions, ID stickers, a \$5.00 bill for completing the web survey and \$10.00 in one dollar bills for participating in the experiment. Subjects received their payment in cash right away in order to increase their sense of entitlement to the money. The procedures also provided as much anonymity as possible, allowing subjects to make anonymous and private decisions, but requiring them to place ID stickers on their offer envelope and exit survey.⁷ 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. Each subject received a copy of this letter with the instructions. 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.⁸

Next, subjects randomly matched themselves to one of three welfare recipients by drawing a copy of the recipient's survey from a box. Dictators received no information about the recipients,

⁶ They had a three day window during which they could stop by anytime during certain hours to participate in the experiment. To prevent survey responses from affecting behavior 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.

⁷ The ID number was assigned to them by the webpage and was automatically entered along with their web survey responses.

⁸ 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 twenty observations.

except that they were real people, until they went over to the box to draw a survey.⁹ The fact that the recipient was a welfare recipient was stated both on a sign near the box of envelopes and in the recipient's survey. The only additional information subjects received about their recipients was that contained in the recipients' surveys.

Finally, the donor read the recipients' survey, chose how much of the ten dollar pie to give to the recipient, then sealed his or her donation together with the recipient's survey into an envelope and deposited it into a box. The donors completed exit surveys before leaving the experiment.

2.1. Design Considerations

There are two institutions in the real world that are most pertinent to this paper. 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 the external validity of research on social preferences 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 *n*-donor games, 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 are also giving, then either they are masochistic or they are acting on beliefs that this person is, in real life, poorer than they are. Charity does have one well-known complication,

⁹ The box always contained several envelopes in order to reinforce the fact that the decision was anonymous. Experimenters placed dummy envelopes in the box for the first arrivals each day. Subjects affixed ID stickers to the seal of the envelope so that I could verify that no money was stolen.

however: an individual's donation may decrease in the total amount donated by others. I discuss this issue in more detail below.

Voting is an important institution to study in future research, but is beyond the scope of this study. Under voting, the probability that a vote wins is inversely related to the number of voters. This introduces a number of issues that have unknown behavioral consequences. If a subject wishes to appear generous when she is not, either to herself or to the experimenter, 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 and this may have unknown behavioral consequences.

2.2. Web Survey Design and Constructed Measures

The two main design challenges for the attitudinal survey were to choose the appropriate questions and to keep the survey as short as possible. There is a tradeoff between collecting more information from respondents and reducing the quality of the data by exhausting their attention. To maintain high-quality data, I imposed a page limit on the survey and only included the questions that were most pertinent to the study. Readers interested in other potential determinants of actual giving – including religiosity, and detailed socioeconomic data – may begin by consulting the large social surveys and the literature that uses them and then develop and test customized surveys for use in experiments like the one presented in this paper. Readers may also consult several other papers that have tested the effects of various personality and attitudinal measures on behavior in bargaining experiments.¹⁰

¹⁰ It is beyond the scope of this paper to review all of the personality and attitudinal measures that have been developed and used to attempt to predict behavior. However, for some recent research using various measures in

The questions from the web survey are presented in the first column of the table in Appendix A. 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 (1989) 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) to compare their effects of those of the variables that I was most interested in (humanitarianism and prior beliefs about the causes of income). Questions 2 and 3 had no significant effects on offers, which is consistent with my predictions.

Katz and Hass (1989) designed the HE 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 according to this body of literature is the Protestant work ethic. Katz and Hass predicted that humanitarians would respond with more empathy to blacks. They found that their humanitarianism scale correlated with a measure of “pro-black attitudes” and their Protestant work ethic scale correlated with “anti-black attitudes”. In subsequent research, the humanitarianism scale and various beliefs about the causes of income have been used in combination with other measures, including whether the respondent is liberal

bargaining experiments, see Ben-Ner et al. (2001), Carpenter (2003), Glaeser et al. (2000), Gunnthorsdottir et al. (2002), and van Dijk and van Winden (2002).

or conservative, to create a single measure of “conservative-liberal ideology” (Skitka and Tetlock, 1993). However, “liberalism” and “conservatism” are part of what I seek to explain, so results from that literature combine concepts that I treat as explanatory variables with concepts that I treat as dependent variables. The goals of that literature are sufficiently different from mine that the results that have been published using the humanitarianism scale are not pertinent to this paper.

From these questions, I constructed two measures for the analysis. One is the standardized (to a mean of zero and standard deviation of one) sum of all of the questions in the HE scale except question 12.¹¹ I excluded question 12 from the analysis 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. The second 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 the sum of the responses and then standardized it (to a mean of zero and standard deviation of one).¹² The second column of the table in Appendix A 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

¹¹ Combining several questions into a single scale increases the measurement reliability.

¹² Principal components analysis organizes the data into orthogonal linear combinations. The first principal component is the linear combination of the data that has the maximal variance. None of the results are sensitive to the choice of the sum or the first principle component of responses to form these measures.

significant. All of the individual questions used to construct the two measures used in this paper had significant correlations with offers in the pooled sample.¹³

3. Results

We conducted the web survey and the experiment in June of 2002. One hundred and forty-four subjects participated as dictators and three as recipients. 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.

3.1. Treatment effects

Table 1 presents summary statistics for selected variables. The mean offers in the lazy-recipient, low-information, and industrious-recipient conditions were \$1.84, \$3.21, and \$2.79, respectively.¹⁴ According to a Mann-Whitney test, the distributions of offers in the lazy- and

¹³ 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 replicated this finding using nationally representative survey data from a Gallup Organization Social Audit (Gallup Organization, 1998) and the General Social Survey. Beliefs about the specific category of person who is the recipient of redistribution have much larger effects on support for governmental redistribution than general beliefs that the economy is a meritocracy (Fong, 2004).

¹⁴ One subject in the low-information condition gave \$15.00 even though the maximum allowed offer was \$10.00. If this observation is top-coded to \$10.00, then the mean offer in the low-information condition falls to \$3.09. However, rather than top-code that observation to \$10.00, I present median regression results along with ordinary least squares results so that readers may judge the sensitivity or lack thereof of the results to this and other potential outliers.

industrious-recipient conditions were significantly different at the ten percent level ($p = 0.053$) and offers in the low-information condition and lazy-recipient condition were significantly different at the five-percent 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 used in the analysis for this paper showed a significant difference between offers in these two 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.

The failure to find significant treatment effects is not due to a manipulation failure, because the treatment conditions had highly significant effects on the donors' posterior beliefs about why their recipients were poor. Table 1 presents, for each treatment condition, the frequencies of responses to the following exit survey question: "Which of 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." Table 1 clearly shows that beliefs that the recipient was poor because of lack of effort were much stronger in the lazy-recipient condition and beliefs that the recipient was poor because of circumstances 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 one-percent level. Ordered probit analyses predicting ordered responses to the exit survey question also show highly significant treatment effects in the expected direction.

One possible reason for the small treatment effects on the offers is that this experiment is tightly controlled; all three recipients belong to the same social category (single, poor, black mothers on public assistance). Other studies that compare offers to recipients from extremely different social categories show much larger effects. For example, Eckel and Grossman (1996)

found that subjects gave roughly three times more to the American Red Cross than to anonymous subjects.

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.¹⁵ The design of the experiment encourages low offers in a number of ways. First, following Hoffman, McCabe, and Smith (1996), we offered as much anonymity to donors as possible, except that ID numbers were attached to responses. Second, we used language intended to make subjects feel entitled to the ten dollar pie.¹⁶ Third, this experiment is 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.¹⁷ Fourth, people do not like to support welfare recipients. Nationally representative survey experiments that randomly vary descriptions of recipients and ask about attitudinal support for governmental assistance to them show dramatically less support for government spending on "welfare" than on other types of means-tested aid (Gilens, 1999; Heclo, 1986). It has also been found that there is a negative exposure effect on support for welfare. People register less support for welfare if they live in Census tracts where a higher fraction of the residents are welfare recipients (Luttmer, 2001). There is, however, at least one effect that may operate in the other direction. People exhibit

¹⁵ See Camerer (2003) for a review of bargaining game results.

¹⁶ We told subjects that they had "earned" the ten dollars that was to be divided for their participation in the experiment. Subjects who have earned their money (e.g., by performing well on a quiz) as opposed to simply receiving it from an experimenter earn more in bargaining games; they offer less to other subjects and other subjects demand less from them (Cherry et al., 2002; Hoffman and Spitzer, 1985).

¹⁷ See Andreoni (1989) for a discussion of this problem when the motive is pure altruism. See also the inequity aversion model below.

more generosity to recipients who 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; Jenni and Loewenstein, 1997; Small and Loewenstein, 2003).

3.2. Overview of attitudinal effects

There is no evidence to support the prediction that prior beliefs and values would have stronger effects on offers in the low-information treatment condition than in the two high-information conditions. Instead, beliefs about the causes of poverty and failure had their strongest effect in the lazy-recipient condition and the HE scale had its strongest effect in the industrious-recipient condition. The Spearman rank correlations between offers and beliefs about the causes of poverty and failure were 0.393 ($p = 0.008$) in the lazy-recipient condition, 0.355 ($p = .018$) in the low-information condition, and 0.016 ($p = 0.905$) in the industrious-recipient condition. 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.

In unreported analysis, I found that of the correlation between the beliefs about the causes of poverty and failure in the lazy-recipient condition is sensitive to specification. For example, in median regressions predicting offers in the lazy-recipient condition, the effect of the beliefs about causes of poverty and failure on offers becomes insignificant after including either a dummy for male or a dummy for U.S. born, even though neither of these demographic characteristics have robust significant effects on offers in any treatment condition.

In contrast, the correlation between the HE scale and offers in the industrious-recipient condition is very robust. In median regressions predicting offers in the industrious-recipient

condition with the HE scale and various other variables, the effect of the HE scale remains highly significant when other variables – including dummies for male and US born and the prior beliefs about causes of poverty and failure – are included. This strong effect of the HE scale in the industrious-recipient condition and its weak effect in the lazy-recipient condition reflects the empathic responsiveness effect. The empathic responsiveness effect, which is the only robust finding from this experiment, is presented in more detail in the following section.

3.3. The empathic responsiveness effect

Table 2 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 2. In the industrious-recipient condition, those who scored high on the HE scale gave significantly more than those who scored low, but in the lazy-recipient condition, they gave virtually the same amount.¹⁸ In the industrious-recipient condition, the response from high HE scorers was dramatic. The

¹⁸ In the low-information condition, there is no robust evidence that high HE scorers give more (the OLS and median regressions fail to produce consistent findings).

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 2 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 behavior of those who scored in the middle of the HE score 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 3 presents these estimates. The estimated means and medians for each treatment condition in each sub-group can be read from either Table 2 or Table 3. What Table 3 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. Model

This section extends the Fehr-Schmidt (1999) model of inequity aversion – which is one of the more tractable fairness models – to show how judgments of worthiness may matter and how

empathic responsiveness may occur.¹⁹ Let us specify the utility of individual i as follows:

$$U_i = c_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} \left(\max \left\{ \frac{c_j}{\theta_i^j} - c_i, 0 \right\} \right) - \beta_i \frac{1}{n-1} \sum_{j \neq i} \left(\max \left\{ c_i - \frac{c_j}{\theta_i^j}, 0 \right\} \right) \quad (1)$$

where c_i and c_j are consumption of individuals i and j , respectively, n is the total number of players with whom i compares her consumption, $0 \leq \beta_i < 1$ is a parameter capturing aversion to advantageous inequity and $\alpha_i \geq \beta_i$ is a parameter capturing aversion to disadvantageous inequity. The assumption that $\alpha_i \geq \beta_i$ captures the finding that the distaste for having less than other people is stronger than the distaste for having more (Loewenstein et al., 1989).

There are two differences between Equation 1 and the Fehr-Schmidt model. The major difference is that I introduce the term θ_i^j , where $\theta_i^j > 0$ is a parameter capturing individual i 's judgment about the amount of consumption that j deserves per dollar of consumption by i . For example, if $\theta_i^j = 10$, then individual i judges that j deserves to consume ten times more than he or she does. The Fehr-Schmidt model is a special case of (1) in which $\theta_i^j = 1$. I introduce θ_i^j because it is necessary to accommodate the fact that the equitable distribution of consumption may involve unequal consumption levels. A second difference is that Fehr and Schmidt (1999) focused on cases where n equals the total number of players in the game. I will highlight the

¹⁹ For other recently developed fairness models see Rabin (1993), Falk and Fischbacher (1998), Levine (1998), Dufwenberg and Kirchsteiger (1999), and Bolton and Ockenfels (2000). See Sobel (2001), Camerer (2003), and Fehr and Schmidt (forthcoming) for reviews of the extensive literature on fairness.

effects of focusing on inequity between oneself and those one can potentially help (so that n may be less than the total number of players in the game). I will refer to the $n-1$ people to whom individual i compares her consumption level as i 's *focus* of inequity. The second term of (1) says that the individual bears a utility cost from the average amount of disadvantageous inequity between herself and the people who are the focus of her inequity aversion. Similarly, the third term of (1) measures aversion to the average amount of advantageous inequity that i experiences between herself and those who are the focus of her inequity aversion.

In the experiment described above, the consumption levels of the players are:

$$\begin{aligned} c_i &= \varpi_i + x(1 - s_i) \\ c_r &= \varpi_r + xs_i + x \sum_{d \neq i} s_d \\ c_d &= \varpi_d + x(1 - s_d) \end{aligned} \tag{2}$$

where the subscripts i , r , and d refer to donor i , the recipient, and all other donors, respectively, ϖ is an exogenously given consumption level that an individual would have without participating in the experiment, x is the amount of money that can be divided between the donor and recipient (referred to as the pie), and s is the share of the pie that a donor decides to give to the recipient. Let us assume that $\varpi_i > \varpi_r$, and $\varpi_d > \varpi_r \forall d$, which is the natural situation the model is intended to capture.

To focus on inequity between a donor and a recipient, let us abstract from differences between the donors. Let us assume that $\varpi_i = \varpi_d \forall d$, and that when donor i chooses s_i , she assumes that all other donors will give the same amount that she does, regardless of how much

she gives. That is, for any s_i , $s_i = s_d \forall d$.²⁰ Finally, let us assume that from i 's perspective, $\theta_i^d = 1 \forall d$. This is the assumption that Fehr and Schmidt make, namely that in traditional laboratory experiments using traditional subjects who are similar to each other (e.g., university students), the equitable distribution of payoffs is equality.

Under these assumptions, there is no inequity between the donors. The only inequity that can arise is between a donor and a welfare recipient. If i experiences advantageous inequity before

offering money, i.e., if $\varpi_i + x > \frac{\varpi_r + x \sum_{d \neq i} s_d}{\theta_i^r}$, then the utility function is:²¹

$$U_i^{Segment1} = \varpi_i + x(1 - s_i) - \frac{\beta_i}{n-1} \left((\varpi_i + x(1 - s_i)) - \left(\frac{\varpi_r + x s_i + x \sum_{d \neq i} s_d}{\theta_i^r} \right) \right) \quad (3)$$

$$\text{if } \varpi_i + x(1 - s_i) > \frac{\varpi_r + x s_i + x \sum_{d \neq i} s_d}{\theta_i^r}$$

²⁰ This assumption is consistent with experimental evidence. Subjects' own actions tend to be associated with their beliefs that other players will take that same action, and experimental evidence suggests that the causality goes in both directions (Dawes et al., 1977). Behavior may have a causal effect on beliefs because: "Individuals may decide to use their own behavior as information about what other people would do; after all ... if I do this, it follows that my peers may do so also. In addition, there is the possibility as I make up my mind to defect or cooperate, the reasons leading up to the choice I finally make become more salient, while those leading up to the other choice become less so." (Dawes et al., 1977, p. 10).

²¹ If $\varpi_i + x < \frac{\varpi_r + x \sum_{d \neq i} s_d}{\theta_i^r}$, then i will offer zero, because only the second segment of the utility function exists and it is decreasing in s_i .

$$U_i^{Segment2} = \varpi_i + x(1-s_i) - \frac{\alpha_i}{n-1} \left(\left(\frac{\varpi_r + xs_i + x \sum_{d \neq i} s_d}{\theta_i^r} \right) - (\varpi_i + x(1-s_i)) \right) \quad (4)$$

$$\text{if } \varpi_i + x(1-s_i) < \frac{\varpi_r + xs_i + x \sum_{d \neq i} s_d}{\theta_i^r}$$

Donor i will either offer zero or the amount that achieves zero inequity. In the internal solution, the optimal offer increases in the perceived worthiness of the recipient and decreases in the perceived total amount given by others.

Proposition 2: Let s_i^* be the level of s_i that maximizes utility. Either $s_i^* = 0$ or

$$s_i^* = \frac{\varpi_i + x - \left(\frac{\varpi_r + x \sum_{d \neq i} s_d}{\theta_i^r} \right)}{x + \frac{x}{\theta_i^r}}, \text{ which is the value of } s_i \text{ that achieves zero inequity. Furthermore,}$$

$$\frac{\partial s_i^*}{\partial \theta_i^r} > 0 \text{ and } \frac{\partial s_i^*}{\partial \sum_{d \neq i} s_d} < 0.$$

Proof. Segment 1 of the utility function exists if $\varpi_i + x(1-s_i) > \frac{\varpi_r + xs_i + x \sum_{d \neq i} s_d}{\theta_i^r}$, Segment 2

exists if $\varpi_i + x(1-s_i) < \frac{\varpi_r + xs_i + x \sum_{d \neq i} s_d}{\theta_i^r}$, and both segments of the utility function are linear

functions of s_i . The second segment of the utility function decreases in s_i . If the first segment is also decreasing in s_i , then the donor will choose the corner solution $s_i^* = 0$. If the first segment is

increasing in s_i , the donor will choose an interior solution. This occurs at the value of s_i where the first segment ends and the second segment begins, which is where

$$\varpi_i + x(1 - s_i) = \frac{\varpi_r + xs_i + x \sum_{d \neq i} s_d}{\theta_i^r}. \quad \text{This is the point of zero inequity. The partial derivatives}$$

follow directly. ■

Next, let us consider the conditions under which the interior solution will occur. It occurs when the first segment of the utility function is increasing in s_i . This condition can be expressed as a threshold condition on β_i , which says that β_i must be greater than the ratio of the marginal pecuniary cost of the offer to the marginal effect of the offer on advantageous inequity. This condition can be simplified to:

$$\beta_i > \frac{n-1}{1 + \frac{1}{\theta_i^r}} \quad (5)$$

This condition is more likely to be satisfied the lower $n-1$ is. If donor i is only concerned about inequity between herself and the recipient, then $n-1 = 1$. In this case, if we assume that $0 < \theta_i^r \leq 1$, there are two extremes. As θ_i^r approaches zero, the threshold value that β_i must exceed approaches zero and if $\theta_i^r = 1$ the donor will choose the internal solution if $\beta_i > 1/2$. Now, if the donor is also concerned about inequity between herself and other donors in the game, then $n-1$ becomes large and condition (5) becomes difficult or impossible to satisfy, unless θ_i^r is small. Note that θ_i^R must be sufficiently *low* for there to be an internal solution. The intuition of this is that offering money becomes a less effective tool for alleviating inequity as θ_i^r increases, while the marginal effect of the offer on consumption does not depend on θ_i^r .

To summarize, the model suggests a definition of empathic responsiveness and shows why conditional giving increases in empathic responsiveness. Empathic responsiveness can be interpreted as a combination of i) a high value of β_i and ii) a focus on inequity between oneself and the potential recipient rather than a focus on inequity between oneself and everyone in the game (including those whom donor i does not interact with and cannot help). By this definition, empathic responsiveness makes people more likely to choose the interior solution where offers depend on the perceived worthiness of the recipient. Those who are low in empathic responsiveness will choose the corner solution where offers are independent of the perceived worthiness of the recipient.

5. Conclusion

This paper presented and interpreted a finding that people who score high on a measure of humanitarian and egalitarian values have giving that is highly responsive to the perceived worthiness of recipients. This is surprising because we might have expected people who score high on this measure to behave like unconditional altruists, giving equally to all recipients, and people who score low on the measure giving only when the recipient appears worthy. In fact, subjects who scored high on this measure were the *only* subjects whose giving depended on 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.

As I have shown, this phenomenon can be interpreted by extending the Fehr-Schmidt model of inequity aversion to accommodate perceptions of worthiness of recipients and the extent to which inequity aversion is focused on potential recipients rather than also being applied to other players in the game. That is, empathic responsiveness is a combination of a heightened concern for those whom one can help and a stronger desire for equity.

The results of this paper suggest that empathic responsiveness can predict behavior across situations that differ according to individuals' perceptions of the worthiness of the recipients. Unconditional altruism cannot do this. For example, inter-generational transfers have often been modeled as motivated by unconditional altruism. Yet, are those who behave altruistically to their aging parents or their children necessarily altruistic to the poor? The empathic responsiveness interpretation suggests not; a single trait could predict large transfers to one's parents but little generosity to the poor if one believes that one's parents deserve help but the poor do not.

Appendix A. The Web Survey

	Signif. in pooled sample? ^a
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 recognize 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
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.

^a“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: Instructions

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 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.

Table 1. Selected summary statistics in each treatment.

	Lazy-R n = 44	Low-Info n = 44	Industrious-R n = 56 ^b
Offer mean (s.d.)	1.841 (2.261)	3.205 ^a (3.310)	2.786 (2.728)
Posterior: lack of effort freq (percent) ^c	16 (36.4)	6 (13.6)	1 (1.9)
Posterior: both freq (percent) ^c	25 (56.8)	30 (68.2)	31 (55.4)
Posterior: circumstances freq (percent) ^c	3 (6.8)	8 (18.2)	21 (38.9)

^a One individual gave \$15.00 even though \$10.00 was the maximum allowed. When this data point is top-coded to \$10.00 the mean offer in this condition is \$3.091.

^b Two observations are missing for the posterior beliefs in the industrious-recipient condition.

^c Rows 2-5 tabulate responses to the exit survey question: “Which of the following explains why your recipient is poor?”

Table 2. Median and OLS regressions predicting offers in each treatment condition.

	Lazy-recipient condition		Low information condition		Industrious-recipient condition	
	Median Reg. (1)	OLS (2)	Median Reg. (3)	OLS (4)	Median Reg. (5)	OLS (6)
Mid. HE score	1.000 (1.446)	0.950 (0.884)	3.000 (1.385)**	2.217 (1.251)*	-0.000 (1.362)	0.474 (0.634)
High HE score	-0.000 (1.514)	0.691 (0.861)	2.000 (1.382)	2.240 (1.218)*	4.000 (1.303)***	3.263 (0.799)***
Constant	1.000 (1.129)	1.250 (0.659)*	1.000 (1.010)	1.583 (0.932)*	1.000 (0.954)	1.526 (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. The omitted category is those with low scores on the HE scale.

* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 3. Median and OLS regressions prediction offers in low, middle, and high HE score sub-samples.

	Low HE score		Middle HE score		High HE score	
	Median Reg (1)	OLS (2)	Median Reg (3)	OLS (4)	Median Reg (5)	OLS (6)
Low info	0.000 (1.503)	0.333 (0.788)	2.000 (1.351)	1.600 (1.170)	2.000 (1.477)	1.882 (0.994)*
Industrious-R	0.000 (1.416)	0.276 (0.635)	-1.000 (1.309)	-0.200 (0.715)	4.000 (1.386)***	2.848 (0.968)***
Constant	1.000 (1.120)	1.250 (0.491)**	2.000 (0.932)**	2.200 (0.517)***	1.000 (1.079)	1.941 (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.

Supplementary Materials

Table S1. Raw data.

Offer Size	Lazy-recipient		Low-info		Industrious-recipient		Total	
	Freq.	Percent	Freq.	Percent	Freq.	Percent	Freq.	Percent
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	10	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

Recipient Survey

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 _____

Exit Survey

- 1) How sorry do you feel for the welfare recipient that you were paired with? [Scored from 1 to 5 where 1 is, "I feel no sorrow for this person," and 5 is, "I feel extreme sorrow for this person."]
- 2) If you gave money, how do you think your recipient will spend it? [Responses are open ended.]
- 3) Does it matter to you how your recipient might spend the money you gave, or might have given? [Scored from 1 to 5 where 1 is, "It does not matter at all," and 5 is, "It matters a great deal."]
- 4) Do you think the person who receives your money will put it to good use? [Scored from 1 to 5 where 1 is, "Strongly believe it WILL NOT be put to good use," and 5 is, "Strongly believe it WILL be put to good use."]
- 5) In your opinion, was the person you were paired with in this experiment more or less deserving of financial assistance than the typical welfare recipient? [Scored from 1 to 5 where 1 is, "Much less deserving," 3 is, "About the same," and 5 is, "Much more deserving."]
- 6) Do you support government welfare transfers to the person you were paired with? ["No, I do not support government transfers to the person I was paired with." or "Yes, I support government transfers to the person I was paired with."]
- 7) Which of 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."]

[Questions 8-10 are scored from 1 "Disagree strongly" to 5 "Agree strongly".]

- 8) Welfare will make your recipient work less than she or he would if there wasn't a welfare system.
- 9) Welfare helps your recipient get on his or her feet when facing difficult situations such as unemployment, a divorce or a death in the family.
- 10) Welfare helps your recipient avoid hunger and starvation.

Finally, we would like to ask just a few demographic questions.

- 11) Were you born in the United States? [Yes/No]
- 12) If not, what is your country of citizenship? _____
- 13) How many years in total have you lived in the United States? _____

Table S2. Mean (standard deviation) of exit survey questions.

Exit Survey Question (See Exit Survey for wording)	Lazy Recipient	Low Information	Industrious Recipient
Exit Survey Q1	2.534 (0.918)	3.223 (0.869)	2.935 (1.051)
Exit Survey Q3	3.545 (1.355)	3.309 (1.392)	3.211 (1.566)
Exit Survey Q4	3.326 (0.865)	3.325 (0.869)	3.396 (0.823)
Exit Survey Q5	2.430 (0.750)	3.191 (0.681)	3.157 (0.464)
Exit Survey Q6 (0 if does not support transfers to recipient, 1 if does support)	0.682 (0.471)	0.818 (0.390)	0.839 (0.371)
Exit Survey Q7 (0 if circumstances, 1 if both, 2 if lack of effort)	1.295 (0.594)	0.955 (0.569)	0.623 (0.527)
Exit Survey Q8	3.580 (1.205)	3.174 (1.057)	2.960 (1.129)
Exit Survey Q9	3.273 (1.086)	3.853 (0.895)	3.776 (0.969)
Exit Survey Q10	3.986 (1.025)	3.949 (0.823)	3.900 (1.069)

Table S3. P-values from Mann-Whitney tests for differences in exit survey responses across treatments. H_0 : Distribution of responses is the same in the two conditions named in the top row of the column.

Exit Survey Question	Lazy/Industrious	Lazy/Low Info	Low Info/Industrious
Exit Survey Q1	0.0753	0.0005	0.1003
Exit Survey Q3	0.3359	0.3451	0.9819
Exit Survey Q4	0.6164	0.9676	0.6808
Exit Survey Q5	0.0000	0.0000	0.8767
Exit Survey Q6 (0 if does not support transfers to recipient, 1 if does support)	0.0000	0.0000	0.0000
Exit Survey Q7 (0 if circumstances beyond control, 1 if both, 2 if lack of effort)	0.0000	0.0076	0.0052
Exit Survey Q8	0.0121	0.0943	0.3299
Exit Survey Q9	0.0266	0.0138	0.7709
Exit Survey Q10	0.7200	0.4506	0.7472

Note: Question 2 was open ended. The most frequent responses were “food,” “children,” and “necessities.” Very few subjects listed negative uses of the money.

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