Truth in Giving:

1. Informed Giving

The willingness to redistribute income varies significantly across individuals and countries for many reasons, including differences in income, variation in the price of giving, and attitudes of donors and voters.[2] One such well-documented regularity is that individuals prefer to assist recipients who are not responsible for their predicament. A person who fell because he is sick, for instance, is more likely to receive support than a person who fell because he is drunk (Piliavin et al., 1969). Similarly, students are typically willing to help a classmate who was in an accident, but they often refuse to support one who needs help because he was partying (Betancourt, 1990). Variation in beliefs about why the poor need support can also help explain differences in

We are interested in the effects of information at the time when the donor is asked to give. (For this reason, figure 1 illustrates welfare conditional on being asked to give.) Information can also affect giving by influencing the likelihood that an individual would agree to play a dictator game.[5] Although deciding not to play and making a zero transfer both result in the recipient receiving nothing, prior evidence suggests that individuals treat these two decisions as quite distinct (Dana et al., 2006; DellaVigna et al., 2009). We leave the question of how endogenous costs of information might influence the willingness to enter a donation game as a subject for future research.

We have three major results. First, we find that a third of subjects are willing to sacrifice resources to obtain additional information, suggesting that a preference to give to specific groups is real. Second, subjects who buy information mostly use it to withhold resources from less-preferred recipients. Third, because we find that those who buy information are generous under uncertainty and far less giving when they learn they were paired with a less-preferred recipient, aggregate transfers decline drastically when dictators have the option to spend resources on information. Making information endogenous, we find that aggregate transfers fall by more than 25%.

The remainder of the paper is organized as follows. In section 2, we briefly discuss the relevant literature. Section 3 presents the experiment, and the following section reports our findings and robustness tests. We offer concluding remarks in section 5.

## 2. Background

There is ample evidence that donors are more generous when they have an opportunity to support a preferred group. For instance, subjects in laboratory dictator games give nearly three times more when the recipient is the American Red Cross than when it is an anonymous subject (Eckel and Grossman, 1996). Other experiments show

[5] Consider an individual who thinks about visiting an NGO fair. Information about the fair – how many organizations will be present, which ones have projects in Southeast Asia – can influence the individual's decision to visit the fair. A second point of influence occurs at the fair itself, when an NGO representative asks the individual to make a donation. We study the effect of information at this second point in time.

that a sense of entitlement influences transfers. In bargaining games, players who earn the right to play an advantageous role receive a larger share, both because entitled players choose to keep more of the pie and because recipients accept the less-equal division (Cherry et al., 2002; Hoffman and Spitzer, 1985). As we discussed in the introduction, donors are also more generous if they feel the needy are not responsible for their predicament. Consistent with this prediction, studies of social survey data show a robust association between beliefs that the poor are industrious rather than lazy and support for public redistribution (Alesina et al., 2001;

We recruited dictators from a campus-wide Carnegie Mellon subject pool that is managed by Carnegie Mellon's Center for Behavioral and Decision Research. The pool includes students at Carnegie Mellon University and the University of Pittsburgh as well as the general community in the university area. Subjects received written instructions at the outset of the experiment. (The complete instructions are reproduced in appendix A.)

In our CHOICE treatment, the instructions stated that subjects had been randomly paired with a "low-income public housing resident." Participants also knew that we recruited an equal number of disabled recipients and drug users. Subjects then chose between two envelopes. The instructions read:

- The small envelope labeled "Contains $10 and NO INFO about the person you are matched with" contains ten one dollar bills.
- The small envelope labeled "Contains $9 and INFO about why the person you are matched with has been held back in life" contains nine one dollar bills and one of the following two statements: "The person you are matched with said he has a physical disability that has prevented him from working," or "The person you are matched with said he does not have a physical disability but has been held back by drug use." The reduced dollar amount takes into account your $1 payment for the information.

In our EXOGENOUS NO INFO treatment, the envelopes contained information about the dictator game, but subjects did not learn anything else about their recipient. In our EXOGENOUS INFO treatment, the envelopes contained information about which type of recipient they faced.

Our procedures are double blind in the sense that we have no way of linking dictator decisions to subject identities, a fact that was obvious to our subjects because they picked their own instructions (and hence recipient type) out of a large box. At the same time, we were able to make sure that no participant opened both envelopes in the CHOICE treatment. Finally, we conducted an exit survey to collect demographic information (see Appendix B.)

### 3.3. Identification

Before we turn to our results, it is important to discuss how the experimental treatments shown in table 1 allow the identification of the effects of interest. We observe two types of dictators ($T_1$) in our experiment – those who buy information ($t_1$) and those who do not ($t_2$) – and two types of recipients ($R_i$):

robustness section below, we will discuss how our results change if we relax the IIA assumption.

If IIA holds and $T_i$ is distributed i.i.d., we can identify the effect of information on transfers as follows: to start, cell B identifies type 2's giving under uncertainty ($\theta_{cB} = \theta_2$). Comparing transfers in cells A and B then identifies

know they face a drug user.[8] This impression from table 3 is consistent with the data in figure 2 which suggest that the entire offer distribution shifts right when our donors are paired with disabled recipients. Second, mean transfers in table 3 indicate that a non-random sample of subjects chooses to buy information. Recipients who use drugs receive an average of $2.56 and $1.68 in the non-selected samples (cell C2) but only $0.62 from those who spent a dollar to learn their recipient type (cell D2). Similarly, those who decide not to buy information are more generous ($1.97, cell B) than a non-selected sample of dictators who do not know their recipient type ($3.03, cell A).

The raw data in table 3, while interesting, need to be interpreted with care. These comparisons do not hold constant demographics

selected group, the disabled received an additional $3.93 (=$1.53+$2.39), according to the OLS estimates, or an additional $3.47 (=$1.33+$2.13) in the Tobit model. Specifications (6) and (7) show that these basic results are confirmed once we control for donor characteristics. The coefficient on "Were Offered to Buy Information" is negative, indicating that subjects who choose not to purchase information are less generous. The effect of having only $9 at the time of the transfer decision is predicted to reduce donations by more than 80 cents, an effect that is not statistically significant.

To facilitate the interpretation of table 4 with its many interaction effects, we report predicted transfers (using specification 6) in Table 5. These calculations hold constant the influence of personal characteristics and the size of the endowment. Table 5 also reports the results for Wald tests that examine the hypothesis that

By contrast, type 2 donors give far less when they are uncertain (

implied beliefs about the distribution of recipient types under uncertainty (cell A). Our estimates do in fact suggest that under uncertainty, type $t_1$ behaved as if all recipients were disabled, while $t_2$ gave as if they faced a drug user with certainty. These beliefs appear extreme because we were clear in our instructions that dictators were equally likely to be paired with either type of recipient.

In figure 3, we simulate weakening audience effects – the idea that a focus on the reasons why a recipient is poor provides a convenient excuse to give less. We let $t_2$, type 2's transfer under uncertainty, vary from \$2.76, the predicted transfer in cell B, to \$3.91, the predicted transfer in cell A and the point at which there is no difference between $t_1$ and $t_2$. As the figure shows, the underlying parameters change substantially with weakening audience effects. For example, donors who buy information become less generous under uncertainty. And the change in transfers when these donors learn they face a disabled person increases. As a result, the implied beliefs about the likelihood of being paired with a disabled recipient look far more reasonable.[11] An intriguing possibility is to calibrate the model by choosing $t_2$ so as to have type 1 believe she faces a drug user with a probability of 50%. The simulated parameters for this value of $t_2$ are given in the bottom panel of table 5.

most from improved information, lose an expected $0.88 when audience effects are absent and $0.47 if the audience effect is $1.02 as in the bottom panel of table 5. The aggregate decline in transfers caused by endogenous information (-28%) is invariant to audience effects.

## 5. Conclusion

Our simple experiment shows a rich array of effects of making information about recipients endogenous. We emphasize three. First, we find clear evidence that a significant group of donors is willing to invest resources to learn their recipient type and achieve a distribution of income that better matches their preferences. This finding is consistent with Corneo and Fong (2008) who use survey data to estimate that achieving a more just distribution of income carries significant value. Second, subjects who buy information use it to withhold resources from less-preferred recipients. Third, with endogenous information aggregate transfers fall by more than 25%, in part because information is costly, leaving less money for transfers, in part because dictators who buy information reduce their giving substantially. When information is endogenous, all types of recipients are worse off in expectation. This finding stands in stark contrast to the results of previous literature on the exogenous provision of information.

Our findings add to our understanding of transfers in dictator games and real-world giving. Most obviously, our results caution against relying on findings from studies with exogenous changes in information to predict transfers in richer decision-making environments. Both recipient heterogeneity and endogenous information states appear to have a significant negative impact on overall transfers to the poor. Our findings also have implications for governments and NGOs that seek to increase the financial and political support for transfer programs. Not surprisingly, our subjects were most generous when they received free information indicating their recipient was disabled. In

change in transfers is a gain of $0.90 for the disabled (=$1.75-$0.85, $\gamma_{11}$ minus the estimated effect of having $9 at the time of transfer) and a loss of $2.60 for drug users (=-$1.75-$0.85, $\gamma_{12}$ minus the estimated effect of having $9 at the time of transfer).

real-world settings, there are two challenges to coming close to this state. For one, the production, dissemination, and consumption of information are costly. In addition, when recipient heterogeneity is significant and not every potential donor is willing to invest resources to find a preferred type of recipient, heterogeneity appears to provide a convenient excuse to be more selfish. From a government and NGO perspective, the trick then is to produce credible signals about recipients belonging to a preferred group that are hard to ignore.

References

Alesina, A., Giuliano, P., Preferences for Redistribution. Handbook of Social Economics. Forthcoming.

Alesina, A., Glaeser, E., Sacerdote, B., 2001. Why doesn't the United States have a European-style welfare state? Brookings Papers on Economic Activity, 2, 187-277.

Andreoni, J., 2006. Philanthropy. In: Kolm, S-C., Ythier, J.M. (Eds.), Handbook of Giving, Reciprocity and Altruism, volume 2. Elsevier Science North Holland, Amsterdam, pp. 1201-1269.

Andreoni, J., Bernheim, B.D., 2009. Social image and the 50-50 norm: A theoretical and experimental analysis of audience effects. Econometrica 77, 1607-1636.

Arrow, K.J., 1951. Social Choice and Individual Values. Wiley, New York.

Bénabou, R., Tirole, J., 2006. Incentives and prosocial behavior. American Economic Review 96, 1652-1678.

Betancourt, H., 1990. An attribution-empathy model of helping behavior. Personality and Social Psychology Bulletin 16, 573-591.

Cherry, T.L., Frykblom, P., Shogren, J.F., 2002. Hardnose the dictator. American Economic Review 92, 1218-1221.

Corneo, G., Fong, C.M., 2008. What's the monetary value of justice? Journal of Public Economics 92, 289-308.

Corneo, G., Grüner, H.-P., 2002, Individual preferences for political redistribution,

Heclo, H., 1986. The political foundations of anti-poverty policy. In: Danziger, S.H., Weinberger, D.H. (Eds.), Fighting Poverty. Harvard University Press, Cambridge, pp. 312-340.

Hoffman, E., Spitzer, M.L., 1985. Entitlements, rights, and fairness: An experimental test of subjects' concepts of distributive justice. Journal of Legal Studies 14, 259-297.

Linardi, S., McConnell, M., 2009. No excuses for good behavior. Mimeo.

Oberholzer-Gee, F., Eichenberger, R., 2008. Fairness in extended dictator game experiments. B.E. Journal of Economic Analysis & Policy 8, Article 16.

Piketty, T., 1995, Social mobility and redistributive politics, Quarterly Journal of Economics 110, 551-585.

FIGURE

FIGURE 3 – W

TABLE 1 – EXPERIMENTAL TREATMENTS

| | | Information | |
|---|---|---|---|
| | | Yes | No |
| No choice | Disability | C1 | A |
| | drug use | C2 | |
| Choice | Disability | D1 | B |
| | drug use | D2 | |

TABLE 3 – MEAN TRANSFERS

| | | Information Does the Dictator Know His Recipient Type? | | | | | |
|---|---|---|---|---|---|---|---|
| | | Yes | | | No | | |
| | | $10 | $9 | | | | |
| Cannot buy information | Paired with disabled | 4.31 (3.80) N=35 | 2.97 (3.45) N=33 | Paired with disabled | 3.03 (3.29) N=30 | 4.31 (3.55) N=35 | 2.97 |
| | Paired with drug user | 2.56 (3.60) N=39 | | | | | |

TABLE 5 – PREDICTED TRANSFERS, CONTROLLING FOR DEMOGRAPHICS

| | | Information<br>Does the Dictator Know His Recipient Type?<br>Yes | No |
|---|---|---|---|
| Cannot buy information | Paired with disabled | 5.05<br>(0.13) | 3.91 |
| | Paired with drug user | 3.46<br>(0.54) | |
| Can buy information | Paired with disabled | 5.07<br>(0.21) | 2.76<br>(0.10) |
| | Paired with drug user | 1.10<br>(0.00) | |
| Structural parameters[13] | $\lambda_1$: t1's giving when she is uncertain | | 6.25*** |
| | $\lambda_2$: t2's giving when she is uncertain | | 2.76*** |
| | $\lambda_{11}$: effect of t1 learning she faces a disabled recipient | | -0.33 |
| | $\lambda_{12}$ effect of t1 learning she faces a drug user | | -3.84** |
| | $\lambda_{21}$ effect of t2 learning she faces a disabled recipient | | 1.86* |
| | $\lambda_{22}$ effect of t2 learning she faces a drug user | | 1.20 |
| Simulated parameters with imposed beliefs on type 1 | $\lambda_1$: t1's giving when she is uncertain | | 4.16 |
| | $\lambda_2$: t2's giving when she is uncertain (assumed) | | 3.78 |
| | $\lambda_{11}$: effect of t1 learning she faces a disabled recipient | | 1.75 |
| | $\lambda_{12}$ effect of t1 learning she faces a drug user | | -1.75 |
| | $\lambda_{21}$ effect of t2 learning she faces a disabled recipient | | 0.84 |
| | $\lambda_{22}$ effect of t2 learning she faces a drug user | | 0.18 |

Notes: The effects are calculated from specification 6 in table 4. In the top panel, we test the hypothesis that transfers in the no-information-no-choice condition (cell A) are not different from the transfers in the other cells. We report the results for a Wald test in parentheses below the predicted transfers. For the structural parameters in the middle panel, we report tests of the hypothesis that the parameters are not different from zero. ***, ** and * denote significance at 1%, 5% and 10% level, respectively. In the bottom panel, we simulate the parameters of interest choosing the value of $\lambda_2$ which has type 1 dictators behave as if there was a 50% chance of being paired with a disabled recipient.

[13] In these calculations, we use our estimate of "Funds = $9 at time of transfer" to purge observed giving from this effect. Specifically, we observe dictators who bought information and learnt they face a disabled recipient to give $5.07. This transfer reflects a learning and an endowment effect. In the absence of the latter, these subjects would have given $5.92, which is the basis for calculating the structural parameters. For estimates of transfers in cells C1 and C2 of Table 1, we use $5.05 and $3.46 respectively, namely the predicted transfers in treatments with free information and a $10 pie. Thus, the structural parameters are estimated as if subjects have $10 to divide in all cells of Table 1.

# Appendix A

## A.1. Instructions for the CHOICE treatment

Note: the words that differ across treatments are in brackets.

Written instructions – Part A

You are about to participate in an economics experiment. You have been paid $5.00 for showing up. You will have the opportunity to earn additional cash during the experiment. The amount of additional cash you earn will depend on the decisions you make during the experiment and could range from $0.00 to $10.00. Your decisions will be completely anonymous; nobody will be able to match the decisions you make to your name or face. No talking is allowed during this experiment. If you have a question, please raise your hand.

In this experiment, you will be paired with a low-income black man recruited from public housing in Pittsburgh. You will be allocated $10.00 and will have an opportunity to give any portion of it, from $0.00 to $10.00, to this low-income public housing resident. He has been given a brief description of the experiment but will receive no further information. In particular, he will receive no information about you. If you allocate money to him, we will match his ID number to his mailing address and mail him all of the money you decided to give.

The low-income public housing residents who participate in this experiment completed a short survey prior to the experiment. Some indicated they have a physical disability that has kept them from working. Others said they do not have a physical disability but have been held back economically by drug use. We recruited an equal number of each. Thus, half of you will be matched with a low-income subject who said he has a physical disability, and half of you will be matched with a low-income subject who said he does not have a physical disability but has been held back economically by drug use.

When the time comes, we will pass around a blue box containing manila envelopes. Each envelope lists an ID number of a different low-income public housing resident. When it is your turn, draw one envelope from the blue box and wait for further instructions. This will match you with a low-income subject. Each low-income subject is matched with exactly one participant in this room. The envelope will also list a second ID number. This is your ID number.

Finally, you may be aware that at some studies, subjects are not always told the truth. This study is an exception. To assure you 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 that will be made in this experiment will be paid in exactly the amounts chosen by the subjects. A copy of this certification is posted at the front of the room.

Now go ahead and reread the instructions and complete the experiment on your own. Raise your hand if you have questions or are instructed (e.g. when you are ready to turn in materials).

A.2. Instructions for the EXOG NO INFO treatment

Written instructions – Part A. No change from Part A instructions of the CHOICE treatment.

Verbal instructions. The bracketed words that differ from the CHOICE treatment verbal instructions are: [a][envelope] and [envelope].

Written instructions – Part B.
[a small white envelope from your manila envelope. It contains ten one dollar bills. Open the white envelope.]

A.3. Instructions for the EXOG INFO treatment

Written instructions – Part A. No change from Part A instructions of the CHOICE treatment.

Verbal instructions. The bracketed words that differ from the CHOICE treatment verbal instructions are: [a][envelope] and [envelope].

Written instructions – Part B.
[a small white envelope from your manila envelope. It contains ten one dollar bills and one of the following two statements: "The person you are matched with said he has a physical disability that has prevented hi

Appendix B: Exit Survey for Main Treatment Condition

1. We would like to know how important it was for you to know whether your recipient was held back by a disability or drug abuse. If you had to buy the information, what is the maximum amount of money you would have been willing to pay for it? _____

2. If you did not buy the information, at what price, if any, would you have been willing to purchase it?_____

3. Are you: male_____ or female_____?

4. How old are you? _____

5. What is you year in school? (Please check the appropriate option.) Undergraduate: 1st yr _____2nd yr _____3rd yr _____ 4th yr_____ 5th yr or beyond _____Graduate: Master's student ____Doctoral student _____Professional degree student (e.g., law student, med student) _____ Other: Please specify _____

6. What is your major and/or degree program? (e.g., business, public policy, computer science, etc.)

7. What classes are you taking this semester? For each one, list course number, title, and when it is offered:

8. What is your race? White_____ Black _____Asian_____ Hispanic _____ Other _____

9. Were you born in the United States? Yes_____ No _____

10.