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Truth in Giving: Experimental Evidence on the Welfare Effects of Informed Giving to the Poor

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Working Paper

09-133

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Experimental Evidence on the Welfare Effects of Informed Giving to the Poor[1]

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May 15, 2009

Abstract

It is often difficult for donors to predict the value of charitable giving because they know little about the persons who receive their help. This concern is particularly acute when making contributions to organizations that serve heterogeneous populations. While we have considerable evidence that donors are more generous if they know their assistance benefits a preferred group, we know little about the demand for such information. To start closing this gap, we study transfers of income to real-world poor people in the context of dictator games. Our dictators can purchase signals about why the recipients are poor. We find that a third of the dictators are willing to pay a dollar to learn more about their recipient. Dictators who devote resources to acquiring information are individuals whose giving is particularly responsive to recipient type. They use the information mainly to withhold resources from "undeserving" types, leading to a drastic decline in aggregate transfers. With endogenous information about recipients, we find that all types of poor subjects are worse off. Our results suggest that the effects of truth-in-giving policies are highly responsive to recipient heterogeneity and biased against more generous giving.

[1] We thank Julio Rotemberg and participants of the Middlebury Workshop on the Current State of Philanthropy for helpful comments and Dan Schultz for excellent research assistance.

1. Informed Giving

The willingness to redistribute income varies significantly across persons and countries for many reasons, including differences in income, variation in the price of giving, and donor attitudes.[2] One well-documented regularity in the literature on income redistribution 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, Rodin and Piliavin, 1969). Similarly, students are typically willing to help a classmate who was in an accident, but they often refuse to support a colleague who needs help because he was out partying (Betancourt, 1990). Variation in the beliefs about why the poor need support can help explain differences in redistributive policy across democratic countries (Piketty 1995; Alesina, Glaeser, Sacerdote, 2001; Alesina and Angeletos, 2005) and between types of recipients (Eckel and Grossman 1996, Fong 2007).

While there is substantial evidence that individuals use information about recipients to decide how generous a donation to make, we know surprisingly little about how much donors care to help their preferred types. The observation that donors adjust their transfers according to information and beliefs about recipients is only weak evidence that truth in giving matters because these observations are consistent with donors being almost indifferent between giving to the "right" persons and giving randomly. Figure 1 illustrates this point. The graph shows the utility of a dictator in four situations: paired with a disabled person ($U_{\text{disability}}$); paired with someone taking drugs (U_{drugs}); when the recipient's type is unknown (U_{expected}); and when the dictator maximizes his own income. Note that $U_{\text{disability}}$ and U_{drugs} cross. The idea is that our dictator, if forced to make a zero transfer, would prefer to give nothing to a drug addict. The graph also assumes that the dictator would not wish to make an "insultingly low transfer" to his preferred type, the disabled recipient. One can think of these transfers as the ones that would typically be rejected in an ultimatum game. (Alternatively, a dictator might want to direct any non-zero transfer to the disabled person, making the utility functions in figure 1 discontinuous

[2] See Andreoni (2006) and Vesterlund (2006) for reviews of the literature on private giving. For reviews of the literature on preferences for public redistribution, see Alesina and Giuliano (2009) and Fong, Bowles and Gintis (2006).

at zero. The figure is for illustrative purposes only. Our point is quite general and does not depend on these assumptions.)

If the dictator does not know which type of recipient he is facing, he would choose the transfer T_2 that maximizes expected utility. The previous literature (Eckel and Grossman 1996, Fong 2007) shows that individuals give more generously when they are paired with someone who appears to be more deserving, $T_3 > T_1$. But this difference tells us little about the ex ante value of information on types, which is given by $p\,(U(T_1) - U(T_2)) + (1-p)(U(T_3) - U(T_2))$, where p is the individual's prior belief about the probability of facing a drug user. As this expression shows, it is entirely possible that donors who are much more generous when they know a disabled person is the recipient would pay little to find out which of two unknown types is in fact disabled.

Knowing whether donors wish to learn about who they are assisting is important for the design of transfer programs. If governments and NGOs spend resources on monitoring recipients and detecting fraud, this will only increase donations and the political support for transfers if donors do in fact care to learn about the effectiveness of assistance. And even if some donors demand additional information, policy makers still face an interesting trade-off: resources spent on monitoring are no longer available as transfers, possibly reducing the welfare of those who deserve to be helped. Resolving this trade-off in an optimal manner requires administrators to understand whether donors demand information about recipient type and how those who give would adjust their transfers if they knew more.

History seems to suggest that uncertainty about the effectiveness of transfer programs can undermine the political support for income redistribution. For instance, the U.S. welfare debate of the 1980's was spurred by beliefs that welfare recipients took advantage of the former welfare program, Aid for Families with Dependent Children (Heclo 1986, Gilens 1999). The debate was not about cost, but about making sure assistance went to the "right" groups (Farkas et al.). Of course, claims such as 'I would be happy to give more, if only I knew that aid went to the right persons' are difficult to evaluate. These concerns might be real, indicating that improved information would

increase transfers, or they might mask a categorical unwillingness to give (Dana, Weber and Kuang, 2007).

In this paper, we provide a direct test of donors' willingness to spend their own resources to learn more about recipient type. We conduct a laboratory dictator game experiment in which subjects decide to allocate some of their endowment to real-life welfare recipients. The novel aspect of our experiment is that subjects can purchase additional information about their recipient. To our knowledge, ours is the first paper that studies giving decisions in the context of costly endogenous information.

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.[3] 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, Cain and Dawes, 2006). We leave the question how endogenous states of information would 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 is willing to sacrifice resources to obtain additional information, suggesting a preference to give to the preferred group – or a distaste for giving to the "wrong" persons – is real. Second, subjects who devote resources to acquiring more information are a select group. As one would expect, their giving is particularly responsive to recipient type. Third, we find that subjects who buy information mainly use it to withhold resources from unpopular types. In contrast, the preferred group of recipients is no better off facing a donor with information than facing an ignorant donor. As a consequence, 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%. This

[3] 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.

finding stands in stark contrast to the findings in the previous literature on the exogenous provision of information.

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. 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 gave nearly three times more when the recipient was the American Red Cross than when it was an anonymous subject (Eckel and Grossman, 1996). Other experiments show that a sense of entitlement influences transfers. In bargaining games, players who earned 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, Glaeser, Sacerdote, 2001; Fong 2001).

In testing the effect of information on the willingness to give, previous research imposed additional information on subjects. An interesting experiment by Dana, Weber and Kuang (2007) is a noteworthy exception. Their study explores the possibility that strategically chosen ignorance affords individuals the "moral wiggle room" to pursue self-interested actions. In a dictator game, the authors ask dictators to choose between two distributions of income. At first, the recipient's payoffs for the two choices are hidden but dictators have the option to reveal the payoffs free of charge. Dana et al. report that more than 40% of subjects choose not to learn the recipient's payoffs. Strategic ignorance of this type allows dictators to give far less. Similarly, Oberholzer-

Gee and Eichenberger (2008) find that a lottery with a negative expected payoff can serve as a convenient excuse for many dictators not to give.[4]

We are aware of only one paper that attempts to measure the value of a more desirable distribution of income. Using social survey data, Corneo and Fong (2008) estimate that the value of justice in the U.S. economy is approximately 20% of GDP. The present paper adds to the literature on income distribution and information by combining endogenous states of information with monetary incentives, asking whether individuals are willing to pay for information that allows them to achieve a preferred distribution of income.

3. Experimental Design

Our experiment is a standard dictator game with student dictators and real-life welfare recipients living in public housing in Pittsburgh. Prior to the experiment, the recipients filled out a brief survey on their economic and personal circumstances. We asked if they felt that they had been held back economically. If this was the case, we asked why. Some recipients listed physical disability, others listed drug abuse and alcohol as important reasons. Consistent with the prior literature, we expect dictators to be more generous when paired with a disabled person, in large part because this group is seen as less responsible than drug users for its predicament.

3.1. Treatment and control conditions

Dictators receive a $5 show-up fee. In our main treatment, they have the option to play a $10 dictator game not knowing whether they are paired with a disabled recipient or with a drug user. The treatment also allows dictators to pay $1 to learn their recipient type. Having learned their type, dictators then allocate the remaining $9. To identify the effects of interest, we run three control conditions: one in which subjects have a $10

[4]. .For related work, see Dana, Cain and Dawes (2006) who find that a third of their subjects in a dictator game were willing to take $9 and not play the dictator game rather than play a $10 dictator game; and Linardi and McConnell (2009) who investigate the effect of providing an excuse not to volunteer on the time and effort volunteered to a charity.

endowment and receive information about their recipient type for free; one in which subjects have a $9 endowment and receive their information for free; and one in which subjects have $10 and no information.

3.2. Procedures

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 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 main 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 control condition with no information and a $10 pie, the envelope contained information about the dictator game, but students did not learn anything else about their recipient. In our conditions with free information and $9 or $10 endowments, we told subjects 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 main treatment.

Finally, we conducted an exit survey to collect demographic information (see appendix B.)

4. Results

We provide summary statistics in table 1 and information on mean transfers by treatment in table 2. In our main treatment, 32.8% of subjects chose to buy the information, indicating a good number of subjects have a positive willingness-to-pay to achieve an income distribution that better matches their preferences. The first column of table 2 presents offers from dictators who had information about their recipient type. The last row in table 2 presents results from Mann-Whitney hypotheses tests.

As in the previous literature, our data provide clear evidence that information about recipients influences giving. Subjects who knew their type were significantly more generous if they faced someone who suffered from a disability as compared to a drug user. The distribution of offers to disabled recipients is significantly different from the distribution of offers to drug users, both in the treatment with free information and when it was costly.

Already in the raw data in table 2, which does not control for variation in the size of the decision pie, it appears that the difference in transfers to disabled persons and drug users may be greater when subjects bought the information. However, if we look only at offers to disabled recipients, their transfers do not appear to increase when dictators choose to buy the information ($4.31 vs. $4.55). Similarly, if we look only at offers to drug users, these transfers decrease from $2.56 (free information) to $0.62 (purchased information), a change that is not statistically significant at conventional levels.

Figure 2 presents the distributions of offers among those who received the information for free and those who purchased the information, by recipient type. The figure shows that the entire distribution of transfers to drug using recipients seems to shift for the dictators who purchased information as compared to the treatment where information is free. Among those who purchased the information, there were no offers

above $4 (out of a $9 pie). Among those who received the information for free, roughly 20% gave over $5 (out of a $10 pie). The distribution of offers to disabled recipients does not differ noticeably across dictators who bought the information and dictators who received it for free.

The second column of table 2 presents mean offers, standard deviations, and a Mann-Whitney for dictators who had no information by assignment and by choice. It appears that dictators who declined to buy the information are less generous than the group who did not have an opportunity to learn more about their counterpart. The distribution of offers from those who were exogenously assigned no information is significantly different from the distribution of offers from those who endogenously chose to purchase the information.

The raw data in table 2, while interesting, need to be interpreted with care. Our comparisons do not hold constant demographics and, more importantly, there is variation in pie size across the different cells. Those who bought information have only $9 instead of $10 to distribute. In addition, these subjects might also feel poorer having spent $1 on information. To control for these factors, we estimate multivariate models in table 3.

Table 3 presents OLS estimates that control for experimental treatment, for example the presence of the option to buy information, and subject characteristics. We first test for a pure information effect to see whether subjects become more generous if they have more information, irrespective of what they learn. This is apparently not the case. The variable "Knows Recipient Type" is not significant in a model that controls for information alone (model 1) and when we take into account subject characteristics (model 2).

In model (3), we allow the effect of information to vary by recipient type and confirm what table 2 suggested: Relative to transfers to drug users, subjects are significantly more generous when they know they give to a disabled person. Note that model (3) is estimated without a main effect for disability, forcing the effect of disability to be zero when recipients do not know the recipient's type. Model (4) asks whether the difference between disability and drug use increases if the dictator bought the information. This

specification tests for the presence of a selection effect. While a random group received the information for free, only a subset of subjects decided to buy information. From specification (4), it appears that those who bought the information react more strongly to type. The coefficient on the three-way-interaction is positive and large ($2.53).

The difference between disability and drug use among those who purchased the information as reported in specification (4) is not a clean comparison yet because these subjects have a smaller endowment when they make their transfer decision. Having spent a dollar on information, one would expect the difference between disability and drug use to be compressed. We address this issue in two ways. A first is to estimate a model with the fraction of the endowment that is passed on as the dependent variable. These results, reported in column (5), indicate that those who purchased information increase the difference between disability and drug use by almost 30%. A second approach is to include an indicator ("Wealth = $9") that takes on the value of one if subjects had $9 when they made the transfer decision. This group includes subjects who were assigned to the treatment with a $9 endowment and subjects who purchased the information. Including this indicator allows us to separately identify information and income effects. The results for this model are reported in column (6) of table 3. To facilitate the interpretation of the model, we report predicted transfers in table 4. These calculations hold constant the influence of personal characteristics on donations. Table 4 also reports the results for Wald tests that examine the hypothesis that there is no difference between the baseline no-choice-no-information condition (top right cell) and the remaining treatments.

In model (6), we find our main results confirmed: The marginal effect of facing a disabled person is positive and significant when information is free, and it is even larger when subjects choose to buy information. An intuition about these marginal effects guided us in drawing figure 1, which shows a large increase in transfers as subjects learn that they face a disabled person. However, as the aggregate effects in table 4 show, our intuition was incorrect. Mean transfers do not increase significantly as we move from the baseline condition to the treatment where subjects bought information to learn that they face a disabled person. The income effect is only a small part of the explanation.

Although our model predicts that 85cents of the $1 information costs come out of the pocket of the welfare recipients, the marginal effect of endowment size on transfers is not statistically significant in column (6). Moreover, even if we simulate the change in transfers setting income effects to zero, there is still no statistically significant increase in transfers when dictators pay to learn that they give to a disabled person.

Rather than affecting the preferred group, the main effect of information is to punish drug users. They receive an expected $3.91 when dictators cannot know their type but only $1.10 when they acquire information. This punishment is largely responsible for the steep drop-off in aggregate transfers that we observe in the raw numbers in table 2. When we introduce the choice to buy information, transfers decline by 28% across the three choice conditions as compared to the three treatments without choice.[5] Of course, this is in part due to the cost of buying information. But even the fraction of their endowment that dictators choose to keep increases by 7% when we introduce the option to buy information. A key result in table 4 is that both types of recipients are worse off when dictators can choose to learn. Comparing the baseline no-information-no-choice treatment with the choice conditions, drug users are worse off irrespective of whether they face an informed or an ignorant dictator. Disabled recipients are no better off when they face an informed dictator and they are worse off when paired with an ignorant subject, which happens with probability 0.67.[6] Because we give dictators an extra choice when information is endogenous, they are at least as well off, and possibly better off, by construction.

Table 4 shows that the aggregate decline in transfers in the choice conditions is due to two effects: a significant decline in transfers to drug users by informed dictators and reduced transfers by ignorant donors. We test for the robustness of these results by estimating tobit and median regression models. The former take into account that

[5] The decline is 29% if we calculate the change using the predicted effects from table 4. If we assume that subjects would have given an extra dollar if information had been free, aggregate transfers still decline by 17%.

[6] We can speculate that the increase from the baseline to the informed choice would become statistically significant with an increase in the number of subjects who choose to buy information. However, even then, disabled recipients would not be better off in expectation due to the smaller transfers of dictators who choose to remain ignorant, a clear majority in our sample.

transfers are left- and right-censored. The latter predict median transfers which are less influenced by outliers. We report these results in table 5. Our findings remain largely unchanged. An interesting difference between the OLS results and the estimated coefficients in table 5 is that the reduction in transfers due to ignorant dictators is now significant at the five-percent level using tobit, and at the one-percent level using median regression.

It is instructive to explore both changes in table 4 – the decline in transfers due to informed dictators and the reduced donations coming from those who declined to buy the information. We start with a discussion of the change in behavior of the informed dictators. To keep the argument simple, assume for a moment that for students who did not buy the information, the presence of choice makes no difference and they would have donated $2.76 in the baseline no-information-no-choice condition. The transfers of the informed dictators reflect their preferences for giving to specific types. Ignoring wealth effects, we can use these transfers to see how much these subjects would have transferred in the baseline treatment if they believed that there was a 50% probability of being paired with a disabled recipient. Their optimal transfer is about $3, that is, $0.5\times5.07+0.5\times1.10$. But the observed mean baseline transfer ($3.91) is far higher than what is implied by this rough calculation. If the 66% of subjects who did not buy the information donated $2.76, the remaining subjects would have to have transferred $6.27 to yield the observed $3.91. And even if we add the estimated wealth effect to the contributions of informed dictators, the gap remains substantial. One interpretation is that subjects whose giving is particularly responsive to recipient type were careful not to punish the wrong low-income residents. Without information, subjects can make two kinds of mistakes: they can be too generous (relative to their preferences) with a drug user, and they can punish a disabled person by giving less than what is optimal. The data in table 4 suggest that it is this latter possibility that looms particularly large in subjects' minds. Subjects appear to be risk averse in the sense that they fear being too stingy with a recipient they would like to help. Once they know who they are paired with, this fear is gone and transfers to drug users decline drastically.

However, risk aversion of this type is not sufficient to explain the decline in aggregate transfers across the choice and no-choice groups of treatments. To generate the transfers in table 4, informed dictators needed to believe there is a 130% probability of facing a disabled person in the baseline treatment. To see this, consider the set of beliefs that would yield the observed $3.91 in the baseline treatment. The beliefs of students who did not buy the information will not change in the presence of the choice option, implying they would donate $2.76 in the baseline condition. If the group who bought the information believed they faced a disabled with probability one, they would donate $5.07, resulting in an average transfer of only $3.24.

The results in table 4 identify the other significant change: the decline in transfers by ignorant dictators to $2.76 appears to be more than a mere selection effect. Rather, the presence of the choice option seems to have had a negative impact on those who choose to remain ignorant. One explanation is that ignorant dictators anticipate that informed subjects will punish drug users. The expectation that some players will give little may have allowed ignorant dictators to feel it is ok to be selfish. There is a large literature in psychology that documents a positive relationship between norm-guided actions and expectations about what others are doing (Jones, 1984; Cialdini, et al., 1990; Bardsley and Sausgruber, 2005; Krupka and Weber, 2008). If ignorant dictators expected drug users to be punished, being cheap themselves may have carried less of a stigma.

A limitation of our experimental design is that we cannot determine the relative merit of risk aversion – the idea that informed dictators were generous when they had no information because they feared hurting disabled recipients – and destigmatization – the idea that ignorant dictators felt free to give less because they expected the informed dictators to reduce their transfers also. However, we can simulate combinations of risk aversion and destigmatization that are consistent with the data in table 4. Figure 3 shows the result of this exercise. If informed dictators behaved as if there was a 50% probability of facing a disabled person (they are not risk averse), the destigmatization effect needs to be $1.55 to fit the observed data. Using the estimates in table 6, figure 3 also shows the wealth effect of informed subjects being poorer. For every probability of facing a preferred recipient, the destigmatization effect increases considerably. As figure

3 shows, destigmatization alone is sufficient to explain the observed data. However, the reverse is not true. Even if informed dictators behaved as if they were paired with a disabled recipient for sure, there still needs to be moderate destigmatization on the order of 11% of ignorant dictators' endowment to explain our data. Determining the relative importance of risk aversion and destigmatization is an interesting question we leave for future research.

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 achieve a distribution of income that better matches its 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, when information is imposed on donors, as is the case in the previous literature, recipients from the preferred group receive more transfers compared to the no-information baseline. In contrast, all types of recipients are worse off in our endogenous information setting. The aggregate decline in transfers is due to donors using information mainly to punish types they deem undeserving of their help. Our data also show that patrons who choose to remain ignorant reduce their transfers, an observation that is consistent with studies arguing that donors become less generous when they expect others to give less. Third, the marginal effect of knowing that one was paired with a disabled person is positive and large in our setting, irrespective of whether dictators receive information for free or not. This marginal effect – facing a deserving person without much "moral wiggle room" to justify self-interested decisions – appears to be what is captured in standard dictator games. The results presented in this paper caution against relying on these effects 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 real-world settings, there are two challenges to coming close to this state. For one, the production and dissemination of information is costly. In addition, when recipient heterogeneity is significant and not every potential donor is willing to invest resources in identifying the "right" types, heterogeneity appears to lower the stigma of being selfish. From a government and NGO perspective, the trick then is to produce credible signals about deservedness that are hard to ignore. Interestingly, one of the most important transfer programs, the U.S. Social Security program, appears to come close to this ideal. U.S. Social Security is an entitlement program in which transfers are tied (albeit loosely) to prior earnings. If prior earnings signal past effort and thus deservedness, linking earnings to transfers is an elegant way to credibly but cheaply signal the worthiness of recipients. This aspect of Social Security was chosen consciously and staunchly defended by President Roosevelt, who believed that an entitlement program would generate and sustain much stronger political support than a means-tested program (Romer 1994). This insight is certainly consistent with the results presented in this paper.

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FIGURE 1 – OPTIMAL TRANSFERS BY RECIPIENT TYPE

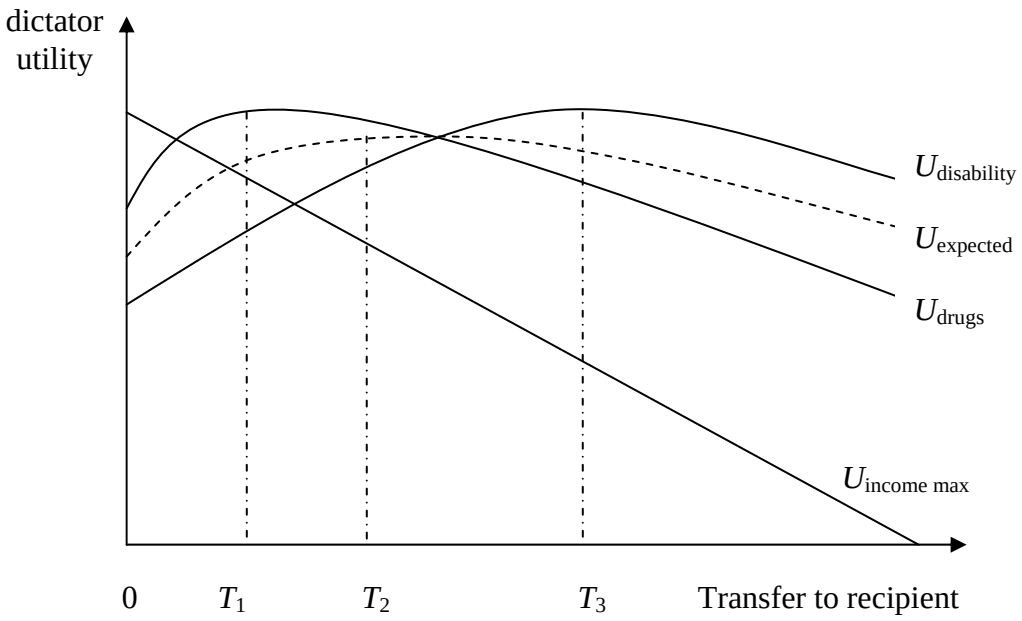
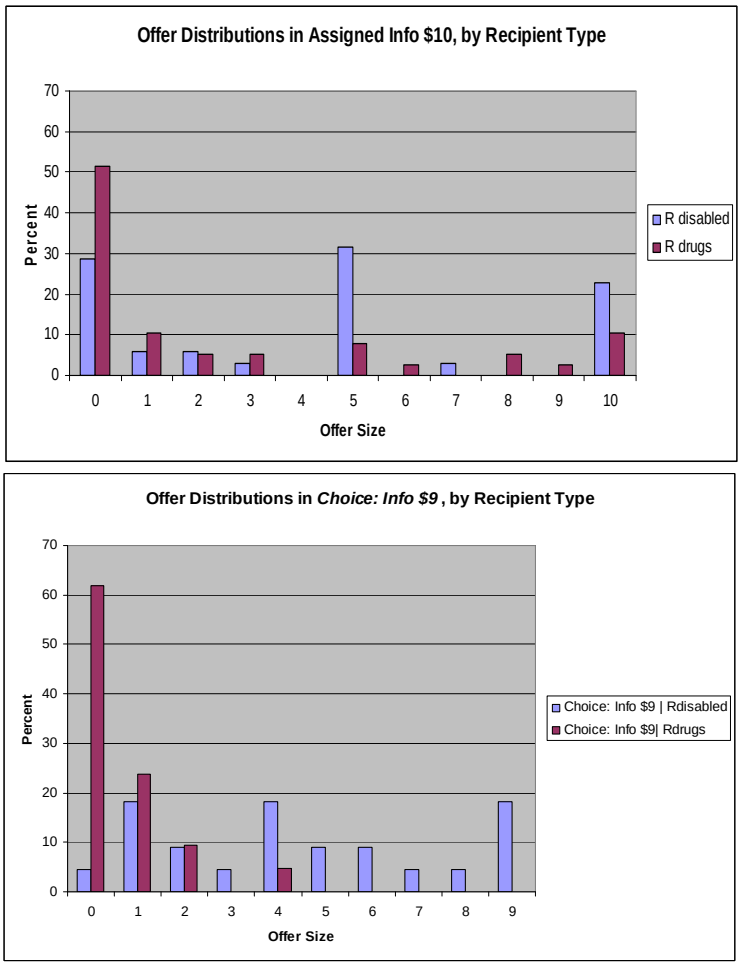
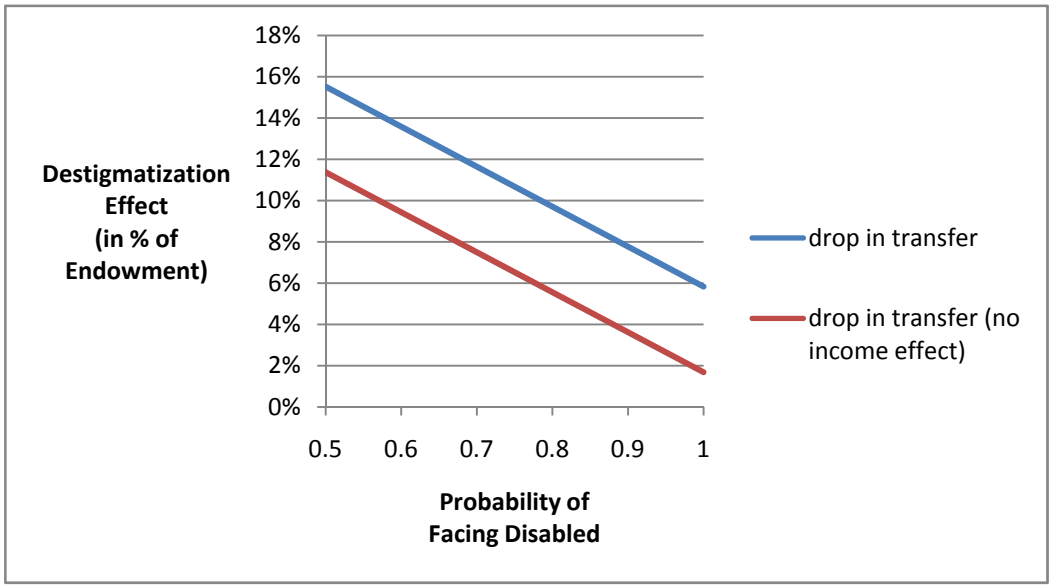


FIGURE 2 – TRANSFERS BY RECIPIENT TYPE



Notes – The upper graph shows the distribution of transfers when dictators received information about their recipient for free. In the lower graph, we show the distribution of transfers for dictators who paid $1 to learn their recipient type.

FIGURE 3 – RISK AVERSION AND DESTIGMATIZATION – BOUNDING EXERCISE



Note: This graph is drawn using the estimated effects reported in table 4.

TABLE 1 – SUMMARY STATISTICS

	Observations	Mean	Standard deviation	Min	Max
Transfer	302	2.59	3.31	0	10
Disabled	302	0.50	0.50	0	1
Knows type	302	0.61	0.49	0	1
Bought information	302	0.14	0.35	0	1
Endowment = $9	302	0.22	0.42	0	1
Male	295	0.53	0.50	0	1
Age	294	24.20	8.36	18	62
Years in school	292	4.30	2.46	1	9
Race Black	302	0.13	0.33	0	1
Race Asian	302	0.22	0.41	0	1
Race Hispanic	302	0.04	0.20	0	1
Republican	302	0.10	0.30	0	1
Democratic	302	0.39	0.49	0	1
Independent	302	0.17	0.38	0	1

TABLE 2 – MEAN TRANSFERS

		Information	
		Yes	No
No choice	Disability	4.31 (3.80) *N*=35	3.03 (3.29) *N*=30
	drug use	2.56 (3.60) *N*=39	
Choice	Disability	4.55 (3.00) *N*=22	1.97 (3.18) *N*=88
	drug use	0.62 (1.02) *N*=21	
Hypotheses tests		H_0 (disability=drug | no choice): p=0.03 H_0 (disability=drug | choice): p=0.00 H_0 (no choice=choice | disability): p=0.75 H_0 (no choice=choice | drug): p=0.12	H_0 (no choice=choice | no info): p=0.04

Notes – Standard deviations are in parentheses. The hypotheses tests report the results of Mann-Whitney two-sample tests.

TABLE 3 – THE EFFECT OF INFORMATION ON TRANSFERS

	(1) Transfer	(2) Transfer	(3) Transfer	(4) Transfer	(5) Transfer (percent)	(6) Transfer
Knows Recipient Type	0.533	0.355	-0.803	-0.331	-0.0326	-0.452
	(0.473)	(0.480)	(0.546)	(0.791)	(0.0799)	(-0.738)
Knows × Disabled			2.420***	1.511**	0.151**	1.595***
			(0.600)	(0.747)	(0.0755)	(-0.548)
Knows × Bought Info				-1.314	-0.126	-0.355
				(1.140)	(0.115)	(1.097)
Knows × Bought Info × Disabled				2.532**	0.297**	2.366**
				(1.248)	(0.126)	(-1.119)
Wealth = $9						-0.853
						(-0.567)
Male		-1.220***	-1.134**	-1.159**	-0.119***	-1.061***
		(0.463)	(0.448)	(0.448)	(0.0452)	(-0.388)
Age		-0.000822	0.00365	0.00178	5.08e-05	0.00513
		(0.0299)	(0.0290)	(0.0290)	(0.00293)	(-0.0273)
Years in School		0.0461	0.0147	0.0190	0.00194	-0.0641
		(0.111)	(0.108)	(0.108)	(0.0109)	(-0.0917)
Race Black		-1.361**	-1.286**	-1.280**	-0.128*	-1.291**
		(0.664)	(0.643)	(0.640)	(0.0647)	(-0.572)
Race Asian		-0.738	-0.649	-0.558	-0.0555	-0.383
		(0.569)	(0.551)	(0.557)	(0.0563)	(-0.483)
Race Hispanic		-0.622	-0.121	0.0852	0.0137	-0.902
		(1.309)	(1.273)	(1.272)	(0.129)	(-0.96)
Republican		-0.821	-0.740	-0.810	-0.0756	-1.071
		(0.842)	(0.815)	(0.813)	(0.0821)	(-0.689)
Democrat		0.262	0.295	0.319	0.0327	0.707
		(0.549)	(0.531)	(0.529)	(0.0534)	(-0.466)
Independent		-0.521	-0.525	-0.635	-0.0620	0.205
		(0.690)	(0.667)	(0.667)	(0.0674)	(-0.567)
Choice of Information	-0.901*	-1.117**	-1.148**	-1.099	-0.110	-1.144*
	(0.477)	(0.500)	(0.484)	(0.708)	(0.0716)	(-0.689)
Constant	2.909***	3.980***	3.911***	3.903***	0.394***	3.908***
	(0.471)	(0.927)	(0.897)	(0.946)	(0.0956)	(-0.888)
	235	233	233	233	233	291
	0.032	0.102	0.164	0.179	0.188	0.175

***, ** and * denote significance at 1%, 5% and 10% level, respectively.

TABLE 4 – PREDICTED TRANSFERS, CONTROLLING FOR DEMOGRAPHICS

		Information	
		Yes	No
No choice	Disability	5.05	3.91
		(0.13)	
	drug use	3.46	
		(0.54)	
Choice	Disability	5.07	2.76
		(0.21)	(0.10)
	drug use	1.10	
		(0.00)	

Note: The effects are calculated from model 6 in table 3. The results for a Wald test are reported in parentheses. We test the hypothesis that the effect of a particular treatment is equal to the effect in the no-choice-no-information condition.

TABLE 5 – THE EFFECT OF INFORMATION ON TRANSFERS – ROBUSTNESS TESTS

	(1) Transfer (OLS)	(2) Transfer (Tobit)	(3) Transfer (Median)	(4) Transfer (OLS)	(5) Transfer (Tobit)	(6) Transfer (Median)
Knows Recipient Type	-0.331	-0.851	-1.769***	-0.452	-1.177	-1.641**
	(0.791)	(1.598)	(0.484)	(0.738)	(1.440)	(0.659)
Knows × Disabled	1.511**	2.981*	3.628***	1.595***	3.067***	2.722***
	(0.747)	(1.516)	(0.459)	(0.548)	(1.087)	(0.489)
Knows × Bought Info	-1.314	-2.326	1.200*	-0.355	-0.138	1.331
	(1.140)	(2.444)	(0.678)	(1.097)	(2.267)	(0.937)
Knows × Bought Info × Disabled	2.532**	5.190**	-0.0368	2.366**	4.652**	0.683
	(1.248)	(2.595)	(0.749)	(1.119)	(2.249)	(0.957)
Wealth = $9				-0.853	-1.795	-0.440
				(0.567)	(1.127)	(0.505)
Male	-1.159**	-2.357**	-0.849***	-1.061***	-2.345***	-1.085***
	(0.448)	(0.932)	(0.273)	(0.388)	(0.780)	(0.344)
Age	0.00178	-0.0336	-0.0204	0.00513	-0.0226	-0.0106
	(0.0290)	(0.0615)	(0.0175)	(0.0273)	(0.0565)	(0.0236)
Years in School	0.0190	0.204	0.121*	-0.0641	-0.0213	0.0352
	(0.108)	(0.226)	(0.0655)	(0.0917)	(0.185)	(0.0796)
Race Black	-1.280**	-3.539**	-0.440	-1.291**	-2.593**	-0.489
	(0.640)	(1.460)	(0.384)	(0.572)	(1.202)	(0.491)
Race Asian	-0.558	-0.526	0.401	-0.383	-0.256	0.165
	(0.557)	(1.126)	(0.340)	(0.483)	(0.949)	(0.422)
Race Hispanic	0.0852	0.530	0.282	-0.902	-1.480	-0.0352
	(1.272)	(2.763)	(0.747)	(0.960)	(2.015)	(0.797)
Republican	-0.810	-2.634	-0.360	-1.071	-3.396**	-0.366
	(0.813)	(1.804)	(0.491)	(0.689)	(1.532)	(0.611)
Democrat	0.319	0.912	0.170	0.707	1.608*	0.644
	(0.529)	(1.090)	(0.323)	(0.466)	(0.928)	(0.407)
Independent	-0.635	-1.770	-0.278	0.205	0.225	0.0563
	(0.667)	(1.426)	(0.404)	(0.567)	(1.143)	(0.498)
Choice of Information	-1.099	-2.539*	-1.830***	-1.144*	-2.752**	-2.035***
	(0.708)	(1.438)	(0.438)	(0.689)	(1.356)	(0.609)
Constant	3.903***	3.516*	3.025***	3.908***	3.909**	3.306***
	(0.946)	(1.927)	(0.585)	(0.888)	(1.761)	(0.790)
	233	233	233	291	291	291
	0.179	.	.	0.175	.	.

Models (1) and (4) reproduce models (4) and (6) from table 3, respectively.
***, ** and * denote significance at 1%, 5% and 10% level, respectively

Appendix A: Instructions for the Treatment Condition ($10.00 endowment and choice to buy information for $1.00)

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 the 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 said 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 in 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.

Take a moment to reread these instructions on your own. Raise your hand if you have any questions.

Verbal instructions: Now we will pass the box of manila envelopes around the room. Draw one envelope and wait for further instructions.

Inside your manila envelope are two smaller white envelopes and Part B of the instructions. Now, open your manila envelope and remove the contents. Do not open the white envelopes unless instructed. Now, we are going to read through Part B of the instructions together before continuing.

Instructions – Part B

You have removed two small white envelopes from your manila envelope. You may keep one of the white envelopes. You must return the other white envelope **without opening it**.

- 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 a $1.00 payment for the information.

In other words, you will choose to make your decision in one of two ways:

- You can make your decision without information about why the subject you are matched with has been held back economically.
- Alternatively, for a $1.00 fee, you can make your decision with information about why the subject you are matched with has been held back economically.

When you have decided which white envelope you want to keep, raise your hand. We will pass around a deposit box to collect the white envelope that you do not want to use.

After you have returned one of the white envelopes, open the white envelope that you decided to keep. Remove and examine the contents. Pocket the amount of money that you want to keep for yourself. Put the rest of the money – which will be sent to the low-income subject with whom you are matched – in the manila envelope and seal the envelope.

When you are finished, raise your hand. An experimenter will collect your sealed manila envelope in a box and will give you an exit survey. Please **write your ID number on the exit survey** and complete the survey. When you are done, gather your belongings and deposit the exit survey in the box in the front of the room. At this point, you will be free to leave the experiment.

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

Appendix B: Exit Survey for Main Treatment Condition

1. We would like to know how important it was to you to know whether your recipient was held back by a disability or drug abuse. If you chose 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 course, 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. Where did you grow up? City and country (if it was multiple places, just tell us the one that you identify most strongly with, or the one that feels most like home)._____

11. How long have you been living in the United States? _____

12. What is your political identification, if any? Republican _____ Democrat _____ Independent _____ Other_____ None of the above _____ Don't know _____

13. What was the total annual household income of your parents or legal guardians when you were a senior in **high school**? If you can, give us the household income before taxes and government transfers (e.g., Social Security). Otherwise, give us your household's take-home income.

    Less than $30,000_____, $30,000 to $49,999_____, $50,000 to $74,999_____, $75,000 to $99,999_____, $100,000 to $149,999_____, $150,000 to $199,999_____, $200,000 to $299,999_____, $300,000 to $400,000 _____, Over $400,000 _____

14. Was this your household's income before taxes and transfers or after? Before_____After _____

15. Please explain what considerations you made when making your decisions in the experiment. What thoughts or considerations did you have?

16. Finally, please write down any other comments, questions, or thoughts you have about this experiment.

Felix: FYI, raw output for columns 4-6 of the proposed Table 3b.

```
. xi: reg offer Tchoice knowstype knowstypebyrdisable boughtinfo boughtxdis
decis
> ionpie9 Tchoice  male age yearinschool black asian hispanic republican
democrat
>  independent

      Source |       SS       df       MS              Number of obs =     291
-------------+------------------------------           F( 15,   275) =    3.88
       Model |   568.21938    15   37.881292           Prob > F      =  0.0000
    Residual |  2686.89402   275  9.77052372           R-squared     =  0.1746
-------------+------------------------------           Adj R-squared =  0.1295
       Total |   3255.1134   290   11.224529           Root MSE      =  3.1258

------------------------------------------------------------------------------
       offer |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
     Tchoice |  -1.143502   .6893411    -1.66   0.098    -2.500558    .2135544
   knowstype |  -.4515673   .7382815    -0.61   0.541    -1.904969    1.001834
knowstypeb~e |   1.594824   .5480322     2.91   0.004     .5159527    2.673696
  boughtinfo |  -.3545709   1.097138    -0.32   0.747    -2.514426    1.805285
  boughtxdis |   2.366096   1.118501     2.12   0.035     .1641851    4.568008
decisionpie9 |  -.8527581   .5668393    -1.50   0.134    -1.968654    .2631376
     Tchoice |  (dropped)
        male |  -1.060936   .3880501    -2.73   0.007    -1.824862   -.2970101
         age |   .0051328   .0273098     0.19   0.851    -.0486301    .0588956
yearinschool |  -.0640718   .0917119    -0.70   0.485    -.2446183    .1164748
       black |  -1.290628   .5717931    -2.26   0.025    -2.416275   -.1649797
       asian |  -.3834613    .483429    -0.79   0.428    -1.335153    .5682304
    hispanic |  -.9018465   .9598353    -0.94   0.348    -2.791405     .987712
  republican |  -1.071216   .6887193    -1.56   0.121    -2.427048    .2846157
    democrat |   .7069816   .4655987     1.52   0.130     -.209609    1.623572
 independent |   .2048967   .5669087     0.36   0.718    -.9111356    1.320929
       _cons |   3.908402   .8877059     4.40   0.000      2.16084    5.655965
------------------------------------------------------------------------------

. xi: tobit offer Tchoice knowstype knowstypebyrdisable boughtinfo boughtxdis
dec
> isionpie9 male age yearinschool black asian hispanic republican democrat
indepe
> ndent, ll ul

Tobit regression                                  Number of obs   =        291
                                                  LR chi2(15)     =      59.67
                                                  Prob > chi2     =     0.0000
Log likelihood = -537.28852                       Pseudo R2       =     0.0526

------------------------------------------------------------------------------
       offer |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
     Tchoice |  -2.752039   1.356235    -2.03   0.043     -5.42192   -.0821592
   knowstype |  -1.177117    1.43955    -0.82   0.414     -4.01101    1.656775
knowstypeb~e |   3.067356   1.087151     2.82   0.005     .9271954    5.207517
  boughtinfo |  -.1376988   2.266717    -0.06   0.952    -4.599949    4.324551
  boughtxdis |   4.652035   2.249164     2.07   0.040     .2243385    9.079731
decisionpie9 |  -1.795422   1.127294    -1.59   0.112    -4.014608    .4237646
        male |  -2.345332   .7795865    -3.01   0.003    -3.880023    -.810641
         age |  -.0226419   .0564518    -0.40   0.689    -.1337727     .088489
yearinschool |  -.0212635   .1848556    -0.12   0.909    -.3851696    .3426425
       black |  -2.592562   1.202231    -2.16   0.032    -4.959271   -.2258539
       asian |   -.255941   .9494741    -0.27   0.788    -2.125072     1.61319
```

```
    hispanic |  -1.479931   2.015096    -0.73   0.463    -5.446842     2.48698
  republican |   -3.39604   1.531637    -2.22   0.027    -6.411216   -.3808641
    democrat |    1.60788   .9284633     1.73   0.084    -.2198896    3.435649
 independent |   .2248574   1.143015     0.20   0.844    -2.025278    2.474993
       _cons |   3.909035   1.760979     2.22   0.027     .4423784    7.375692
-------------+----------------------------------------------------------------
      /sigma |   5.591532   .3881943                      4.827334     6.35573
------------------------------------------------------------------------------
  Obs. summary:        133  left-censored observations at offer<=0
                       135     uncensored observations
                        23 right-censored observations at offer>=10

. xi: qreg offer Tchoice knowstype knowstypebyrdisable boughtinfo boughtxdis
deci
> sionpie9 male age yearinschool black asian hispanic republican democrat
indepen
> dent

Median regression                                 Number of obs =       291
  Raw sum of deviations      743 (about 1)
  Min sum of deviations 649.9577                  Pseudo R2     =    0.1252

------------------------------------------------------------------------------
       offer |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
     Tchoice |  -2.035211   .6094248    -3.34   0.001    -3.234942   -.8354807
   knowstype |  -1.640845   .6586796    -2.49   0.013     -2.93754   -.3441502
knowstypeb~e |   2.721831   .4888644     5.57   0.000     1.759439    3.684223
  boughtinfo |   1.330986    .936737     1.42   0.156    -.5131007    3.175073
  boughtxdis |   .6830986   .9571358     0.71   0.476    -1.201146    2.567343
decisionpie9 |  -.4401408   .5051546    -0.87   0.384    -1.434602    .5543206
        male |  -1.084507    .343627    -3.16   0.002    -1.760981   -.4080334
         age |  -.0105634   .0235928    -0.45   0.655    -.0570089    .0358821
yearinschool |   .0352113   .0795749     0.44   0.658    -.1214421    .1918647
       black |  -.4894366   .4905422    -1.00   0.319    -1.455132    .4762585
       asian |    .165493     .42158     0.39   0.695    -.6644412    .9954272
    hispanic |  -.0352113   .7968153    -0.04   0.965    -1.603844    1.533421
  republican |  -.3661972   .6114342    -0.60   0.550    -1.569884    .8374892
    democrat |   .6443662   .4074429     1.58   0.115    -.1577373     1.44647
 independent |    .056338   .4978738     0.11   0.910    -.9237901    1.036466
       _cons |   3.306338    .789954     4.19   0.000     1.751213    4.861464
------------------------------------------------------------------------------

.
```