Truth in Giving:

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

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Abstract

It is often difficult for donors to predict the value of charitable giving because they know little about their recipients. This concern is particularly acute when making contributions to organizations that serve heterogeneous populations. Prior research shows that donors are more generous if they know their assistance benefits a group they like. But 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 dictator games. Our dictators can purchase signals about why the recipients are poor. We find that a third of the dictators is willing to pay money to learn more about their recipient. Dictators who acquire information mostly use it to withhold resources from less-preferred types, leading to a drastic decline in aggregate transfers. With endogenous information about recipients, we find that all types of poor recipients are worse off.

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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 out partying (Betancourt, 1990). Variation in beliefs about why the poor need support can also help explain differences in redistributive policy across democratic countries and between types of recipients.[3]

While there is substantial evidence that individuals use information about recipients to decide how generously to treat them,[4] 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 a preferred group and giving randomly. Figure 1 illustrates this point. The graph shows the utility of a subject in a dictator game experiment: if she is paired with a disabled person ($U_{\text{disability}}$); if she is paired with someone taking drugs (U_{drugs}); if the recipient's type is unknown (U_{expected}); and if she maximizes her own income. If the dictator does not know which type of recipient she is facing, she would choose the transfer T_2 that maximizes expected utility. Prior research (Eckel and Grossman 1996) shows that individuals give more generously when they are paired with recipients who belong to a preferred group, $T_3 > T_1$. 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

[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 (Forthcoming) and Fong et al. (2006).
[3] For a seminal paper on beliefs about causes of income and redistributive politics, see Piketty (1995).
[4] There is a large literature on this topic which we do not review here. For early experimental evidence, see Eckel and Grossman (1996).

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 nonetheless 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 selecting recipients and detecting fraud, this might increase donations and the political support for transfers if donors do in fact care about who receives assistance. However, resources spent on monitoring are no longer available as transfers, possibly reducing the welfare of those who deserve to be helped. Resolving the trade-off between providing information and transferring more resources to recipients requires administrators to understand whether donors demand information about recipient type and how those who give would adjust their transfers if they knew more. This trade-off is at the core of our experiments.

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). In focus groups, people expressed concern not about the cost of providing welfare but about making sure assistance went to the intended groups (Wax, 2005). Of course, claims such as 'I would be happy to give more, if only I knew that aid went to the right people' 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 et al., 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.[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 states 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; Fong 2001; Corneo and Gruner, 2002). The prior literature offers a rich set of reasons why giving is responsive to recipient type. Donations are likely to vary by recipient in our setting as well, reflecting both donors' preferences for specific groups and beliefs about the social productivity of giving.

In testing the effect of information on the willingness to give, previous research imposed additional information on subjects. An interesting experiment by Dana et al. (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 payoffs for the two choices are hidden but dictators have the option to reveal them free of charge. Dana et al. (2007) report that more than 40% of subjects choose not to learn the recipient payoffs. Strategic ignorance of this type allows dictators to give far less. Similarly, Oberholzer-Gee and Eichenberger (2008) offer subjects the choice of playing a dictator game or buying lottery tickets with a negative expected payoff. The lottery, in itself an unattractive option, again serves as a convenient excuse for many dictators not to give and play the lottery instead.[6]

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

[6]For related work, see Dana et al. (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.

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 dictators from the university community 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. In accordance with the prior literature, we expect dictators to be more generous when paired with a disabled person. During the experiment, dictators receive a $5 show-up fee and are randomly assigned to a recipient, with a 50/50 chance of being paired with a recipient who has a disability or one who abuses drugs and alcohol. Each dictator then plays a dictator game with his or her recipient.

3.1. Treatments

Table 1 summarizes our design. In our *CHOICE* treatment, subjects have the option of playing a $10 dictator game not knowing whether they are paired with a disabled recipient or with a drug user (cell B in Table 1) or paying $1 to learn their recipient type (cells D1, D2). Dictators who buy the information then allocate the remaining $9. In our *EXOG NO-INFO* treatment (cell A), subjects play a $10 dictator game not knowing whether they are paired with a disabled recipient or with a drug user. In our *EXOG INFO* treatment we tell dictators their randomly assigned recipient type (cells C1 and C2). We conduct C1 and C2 at two different stake sizes: $9 or $10. This enables us to econometrically identify the effect of having $9 instead of $10 at the time the transfer decision is made.

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 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 envelope contained information about the dictator game, but subjects did not learn anything else about their recipient. In our *EXOGENOUS INFO* treatment, the envelope 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_i) in our experiment – those who buy information (t_1) and those who do not (t_2) – and two types of recipients (R_i) – those with a disability (r_1) and those who abuse drugs (r_2). Our setup yields six parameters to be estimated: each type's tendency to give when she is uncertain about her recipient's characteristics (ϕ_i) and the *change* in the donor's transfer when she knows she is facing a particular type of recipient (σ_{ir}).

As table 1 indicates, we observe giving (G_i) in six situations. For each of these, we can relate transfers to observed donor characteristics (X_i), estimating the following model:

$$(1)\quad G_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon$$

In comparing G_i across the cells in table 1, two difficulties are immediately obvious. In the treatments in which information is exogenous (A, C1, C2), we do not observe T_i and the transfers reflect the giving behavior of both types of donors, implying, for instance, that $\beta_{0A} \neq \phi_1$ when we estimate model (1) using the observations in cell A.[7] More importantly, when information is endogenous (B, D1, D2) our estimates will be biased if a dictator's unobserved type is correlated with giving. For example, if subjects with better-educated parents are more inquisitive (i.e., more likely to buy the information) and more generous we will overestimate the effect of information on giving, $\beta_{0D1} - \beta_{0B} > \sigma_{11}$.

To recover the parameters of interest, we make two identifying assumptions. We take T_i as distributed i.i.d. Furthermore, we assume that subjects' choices are independent of irrelevant alternatives (IIA), a commonly imposed principle of rationality (Arrow, 1951). In our context, the principle implies, for instance, that donor type 2 will choose the same transfer in the no-information-no-choice condition (cell A) and when she chooses not to buy information about her recipient (cell B). Because type 2 prefers not to buy the information, this option is irrelevant for her choice of an optimal transfer. In the

[7] To fix notation, let β_{0A} denote the estimated average transfer in cell A.

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 ($\beta_{0B}=\phi_2$). Comparing transfers in cells A and B then identifies $\phi_1 = \frac{\beta_{0A} - (1-\pi)\beta_{0B}}{\pi}$ where π denotes the fraction of t_1 in the population which we observe in our endogenous-information treatment. In A and B, dictators give without knowing which type of recipient they face. Transfers in A reflect the giving of both t_1 and t_2, while B reflects t_2's transfers only, giving us ϕ_1. Next, the difference between transfers in D1 and ϕ_1 identifies σ_{11}, the effect of t_1 learning that she faces a disabled recipient (and similar for σ_{12} using D2). Note that if t_1 and t_2 differ in their tendency to give, the true effect of information on giving, $\sigma_{11} = \beta_{0D1} - \phi_1$, differs from a simple reading of our data, which would suggest that this effect is the difference between transfers in cells B and D1 ($\sigma_{11} = \beta_{0D1} - \beta_{0B}$). Finally, we can use the observations in C1 and σ_{11} to infer σ_{21}, the change in t_2's transfer when she learns she faces a disabled recipient: $\sigma_{21} = \frac{\beta_{0C1} - \phi_2 + \pi(\phi_2 - \phi_1 - \sigma_{11})}{1-\pi}$. An equivalent logic identifies σ_{22}.

4. Results

We provide summary statistics in table 2 and information on mean transfers and sample sizes in table 3. When offered the opportunity, 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. Two broad patterns emerge from table 3. First, information about recipient type influences transfers. Subjects who know they face a disabled person are more generous than subjects who

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 the 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 appear less 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 and, more importantly, there is variation across cells in the size of the pie at the time of transfer. To control for these factors, we estimate multivariate models in table 4.

In a first step, we try to understand the demand for information. Specification (1) in table 4 is a probit model. The dependent variable is the decision to purchase information. The covariates are the subject characteristics from our exit survey. With the exception of gender, none of the available demographic information predicts which subjects spend resources to learn their recipient type. The decision to buy information is largely driven by unobserved heterogeneity. The point estimate for gender implies that male subjects are almost 20 percentage points less likely to become informed.

The remainder of table 4 presents OLS and Tobit regressions of transfers, controlling for the treatment conditions and demographic characteristics. For the Tobit estimates, we report unconditional marginal effects in brackets below the standard errors. Specifications (2) and (3) in table 4 test the idea that donors are more generous if they are paired with a disabled recipient. This is clearly the case. Predicted transfers increase by about $2 when subjects know they give to a person with a physical disability. Specifications (4) and (5) show that the difference between transfers to drug users and the disabled further increases among those who chose to buy information. In this self-

[8] Subjects are significantly more generous with disabled recipients in the treatment with a $10 endowment and free information (Mann-Whitney two-sample test p=0.03), if they purchase information (p=0.00) but not with a $9 endowment and free information (p=0.16).

selected group, the disabled receive 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 there is no difference between the baseline no-choice-no-information condition (cell A) and the remaining cells.

As table 5 shows, the marginal effect of facing a disabled person (as opposed to a drug user) is positive and significant when information is free ($5.05 vs. $3.46, p=0.00), and it is even larger when subjects choose to buy information ($5.07 vs. $1.10). An intuition about these marginal effects guided us in drawing figure 1, which shows a large increase in transfers as subjects learn they face a disabled person. However, as the aggregate effects in table 5 show, our intuition was incorrect. The main effect of endogenous information is not to make the disabled better off; it is to reduce transfers to drug users. They receive an expected $3.91 when dictators cannot know their type but only $1.10 when donors bought information. This smaller transfer is largely responsible for the steep drop-off in aggregate transfers that we already observed in the raw numbers in table 3. When we introduce the choice to buy information, transfers decline by 28% in the three choice conditions (sum of transfers in cells B, D1 and D2) as compared to the three treatments without choice (sum of transfers in cells A, C1 and C2).

A key result in table 5 is then that both types of recipients are worse off when dictators can choose to learn. Comparing the baseline *EXOG NO INFO* treatment (cell A) with the *CHOICE* treatment (cells B, D1 and D2), drug users are worse off irrespective of whether they face a dictator who bought the information (comparing cells A and D2,

p=0.00) or one who did not (comparing cells A and B, p=0.10). The same is true for disabled recipients. There is a 0.67 probability that they are paired with a donor who did not buy information, which reduces expected transfers from $3.91 to $2.76 (comparing cells A and B, p=0.10). And there is a 0.33 probability that the donor bought information, which yields an insignificant increase to the disabled recipient (comparing cells A and D1, p=0.21).[9] Thus, in expectation, both types of recipients are worse off with endogenous information. Because we give dictators an extra choice when information is endogenous, they are at least as well off, and possibly better off, by construction.

To better understand these results, it is instructive to see how selection works in our context. The middle panel in table 5 reports our estimates of each type's tendency to give when she is uncertain about her recipient's characteristics (ϕ_i) and the change in transfer when she knows she is paired with a particular type (σ_{ir}). Type 1 donors, who buy information when it is available, are very generous when they do not know their recipient type (ϕ_1 =$6.25). In fact, they behave as if every recipient was a person with a physical disability.[10] One interpretation is that these subjects were careful not to punish their preferred recipients. Under uncertainty, subjects can make two kinds of mistakes: they can be too generous (relative to their preferences) with a drug user, or they can punish a disabled person by giving less than what is optimal. The data in table 5 suggest that it is this latter possibility that looms particularly large in the minds of type 1 subjects. They appear to be risk averse in the sense that they fear being too stingy with a recipient they would like to help. Once they know who they are paired with, this fear is gone and transfers to drug users decline drastically (σ_{12} =$-3.84). Because type 1 donors behave as if they face a disabled person under uncertainty, there is no significant change in donations when these donors learn they were in fact paired with a disabled person.

[9] We can speculate that the increase from cell A to cells D1 and D2 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.

[10] Type 1's optimal transfers to both types of recipients are in cells D1 and D2 in table 5. Adjusting these donations for the estimated "funds = $9" effect in table 4 and using our estimate of ϕ_1 ($6.25), the implied beliefs of type 1 are that 109% of recipients were disabled.

By contrast, type 2 donors give far less when they are uncertain (ϕ_2 =\$2.76), essentially behaving as if they expected their recipient to be a drug user. As a result, type 2 donors increase giving once they know their recipient is disabled (σ_{21} =\$1.86). There is no significant adjustment when they learn they are paired with a drug user. The overall effect of endogenous information on transfers reflects the change in giving by type 1 donors who use information to adjust downward their generous transfers under uncertainty.

4.1. Competing Explanations and Robustness

A key identifying assumption in our analysis is that choices are independent of irrelevant alternatives. There are both theoretical reasons and hints in our data that make it less than obvious that this assumption holds in our context. Specifically, we are concerned that the mere presence of the choice option might have caused t_2 to give less. One explanation for the suspected decline in transfers is a weakening audience effect. Andreoni and Bernheim (2009) provide evidence that dictators give in order to signal to their audience that they are fair. In circumstances in which selfish behavior has plausible explanations other than the selfishness of the donor (the signal is more difficult to interpret), subjects tend to keep a larger share of the pie for themselves. In the endogenous information treatment, we not only ask dictators how much they want to give to a poor person, we also encourage them to think about *why* the recipients are poor, possibly highlighting reasons not to give. Plausible reasons to be more selfish, we suspect, might contribute to a decline in transfers among type 2 donors when they have the option to buy information (cell B). Note that in our experiments "the audience" is neither the recipients – dictators are unlikely to ever meet the persons living in low-income housing – nor the experimenters because our experimental set-up is double-blind. More plausibly, in our context dictators signal fairness to themselves, pointing to the importance of perceived fair behavior for self-respect (Bénabou and Tirole, 2006).

We have some empirical evidence that weakening audience effects are at play in our setting. If type 2 donors become more selfish in the presence of choice (cell B), we underestimate ϕ_2 and overestimate ϕ_1. One consequence of a bias of this sort is extreme

implied beliefs about the distribution of recipient types under uncertainty (cell A). Our estimates do in fact suggest that under uncertainty, 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 ϕ_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 ϕ_1 and ϕ_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 changes in transfers when these donors learn they face a disabled person increase. 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 ϕ_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 ϕ_2 are given in the bottom panel of table 5.

As the simulated values in table 5 and the more general analysis in figure 3 show, weakening audience effects are important for the magnitude of our estimates. However, the key insights from this experiment are little affected by the possibility that the choice treatment may have provided an excuse to give less. Irrespective of audience effects, subjects who buy information are the ones whose giving is more responsive to recipient type. Moreover, the effect of information on giving continues to vary substantially by type, with type 1 donors using the information to give far less to drug users. This effect is absent for type 2 donors. Finally, the welfare effects of endogenous information continue to be negative for all types of recipients.[12] The disabled, who stand to gain the

[11] With no difference in giving under uncertainty between the two types ($\phi_1=\phi_2$=\$3.91), type 1 behaves as if there was a 42.7% chance of being paired with a disabled person, a value that is reasonably close to our instructions.

[12] For recipients paired with a type 2 donor, expected transfers decline more sharply in the presence of audience effects, from \$3.78 to \$2.76 in the bottom panel of table 5. For recipients paired with type 1, the

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

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change in transfers is a gain of \$0.9 for the disabled (=\$1.75-\$0.85, σ_{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, σ_{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.

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FIGURE 1 – OPTIMAL TRANSFERS BY RECIPIENT TYPE

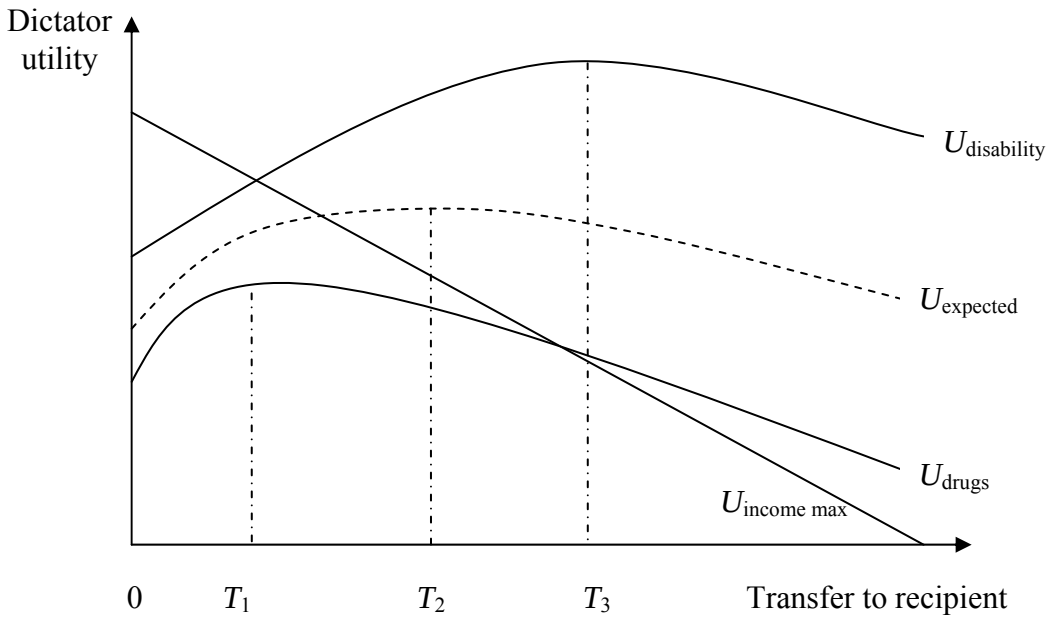
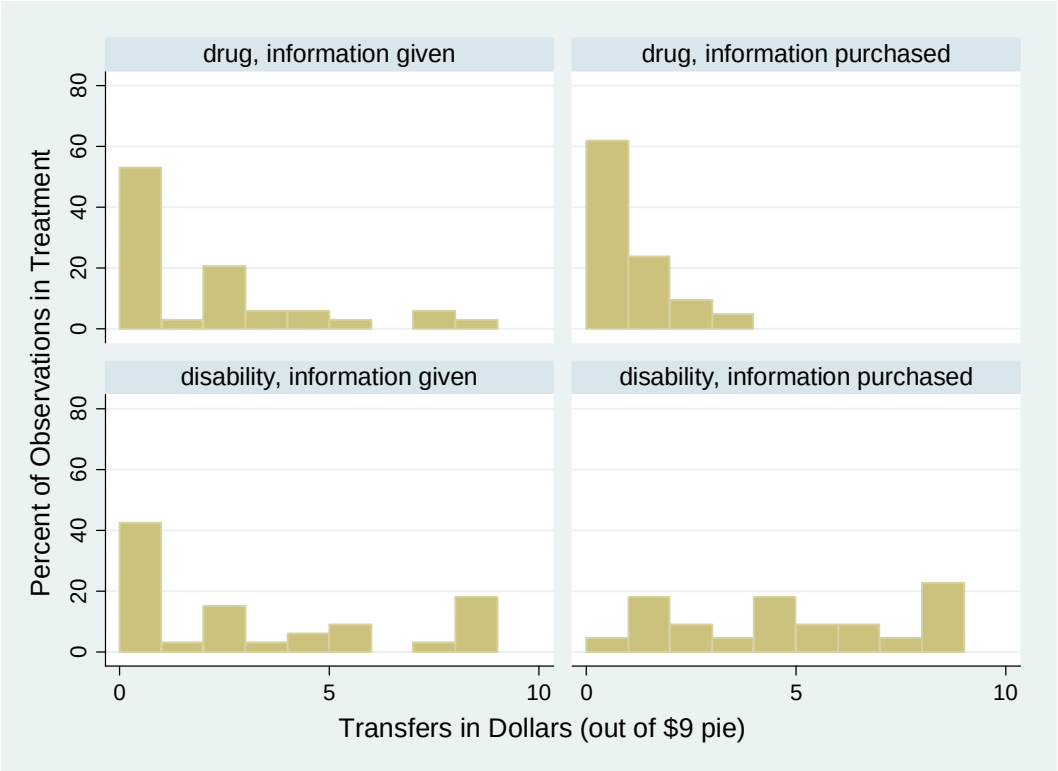
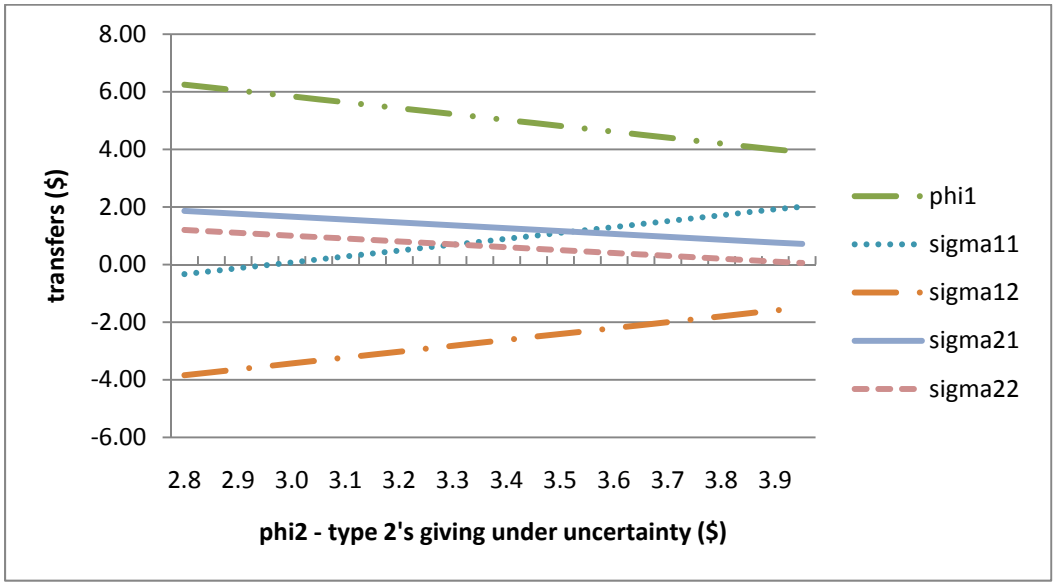


FIGURE 2 – TRANSFERS BY RECIPIENT TYPE AND INFORMATION CONDITION



Note: These graphs show data for subjects who had $9 at the time they made their transfer decision, either because they started with a $9 endowment (in graphs labeled "information given") or because they spent $1 on information (in graphs labeled "information purchased").

FIGURE 3 – WEAKENING AUDIENCE EFFECTS – ROBUSTNESS TEST



Note: This graph is drawn using the estimated effects reported in table 5.

TABLE 1 – EXPERIMENTAL TREATMENTS

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

Notes – In the Information = yes column, subjects know which type of recipient they face. In the no-choice row, the information is exogenous. In the choice row, subjects can choose to learn which type of recipient they face.

TABLE 2 – SUMMARY STATISTICS

	Full Sample					**Subjects Who Buy Information**				
	Obs.	Mean	Standard deviation	Min	Max	Obs.	Mean	Standard deviation	Min	Max
Transfer	302	2.59	3.31	0	10	43	2.63	2.99	0	9
Disabled	302	0.50	0.50	0	1	43	0.51	0.51	0	1
Knows type	302	0.61	0.49	0	1	43	1.00	0.00	1	1
Bought information	131	0.33	0.47	0	1	43	1.00	0.00	1	1
Funds = $9	302	0.22	0.42	0	1	43	1.00	0.00	1	1
Male	295	0.53	0.50	0	1	43	0.30	0.46	0	1
Age	294	24.20	8.36	18	62	43	23.47	6.87	18	50
Years in school	292	4.30	2.46	1	9	42	4.17	2.20	1	9
Race Black	302	0.13	0.33	0	1	43	0.14	0.35	0	1
Race Asian	302	0.22	0.41	0	1	43	0.26	0.44	0	1
Race Hispanic	302	0.04	0.20	0	1	43	0.05	0.21	0	1
Republican	302	0.10	0.30	0	1	43	0.07	0.26	0	1
Democratic	302	0.39	0.49	0	1	43	0.44	0.50	0	1
Independent	302	0.17	0.38	0	1	43	0.23	0.43	0	1

Notes – "Bought information" is an indicator that equals one for subjects who were offered information and bought it (N=49) and zero for subjects who were offered information and declined it (N=82). "Funds = $9" is an indicator that equals one for subjects who had $9 at the time they made their transfer decision - either because they started with a $9 endowment or because they spent $1 on information – and zero for all other subjects in the experiment. "Years in school" is the number of years in college (see appendix B for details).

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	3.03 (3.29) *N*=30
	Paired with drug user	2.56 (3.60) *N*=39	1.68 (2.38) *N*=34	
Can buy information	Paired with disabled		4.55 (3.00) *N*=22	1.97 (3.18) *N*=88
	Paired with drug user		0.62 (1.02) *N*=21	

Notes – We report mean transfers, standard deviations (in parentheses) and sample sizes. In the \$10 column, subjects had \$10 when they decided to make the transfer. In the \$9 column, subjects had \$9.

TABLE 4 – THE EFFECT OF INFORMATION ON TRANSFERS

Dependent variable	(1) Bought Info	(2) Transfer	(3) Transfer	(4) Transfer	(5) Transfer	(6) Transfer	(7) Transfer
Model	Probit	OLS	Tobit	OLS	Tobit	OLS	Tobit
Knows Recipient		-0.095	-0.176	-0.366	-0.978	-0.452	-1.177
Type		(0.626)	(1.283)	(0.731)	(1.461)	(0.738)	(1.440)
			[-0.082]		[-0.453]		[-0.546]
Knows × Disabled		2.099***	4.032***	1.533***	2.870***	1.595***	3.067***
		(0.471)	(0.969)	(0.534)	(1.083)	(0.548)	(1.087)
			[1.869]		[1.331]		[1.423]
Knows × Bought				0.128	0.842	-0.355	-0.138
Info				(1.070)	(2.259)	(1.097)	(2.267)
					[0.390]		[-0.064]
Knows × Bought				2.394**	4.604**	2.366**	4.652**
Info × Disabled				(1.104)	(2.259)	(1.119)	(2.249)
					[2.134]		[2.158]
Were Offered to		-0.344	-0.675	-1.067	-2.558*	-1.144*	-2.752**
Buy Information		(0.458)	(0.930)	(0.670)	(1.354)	(0.689)	(1.356)
			[-0.313]		[-1.186]		[-1.277]
Funds = $9 at time		-0.878*	-1.600	-1.108**	-2.196**	-0.853	-1.795
of transfer		(0.513)	(1.044)	(0.534)	(1.084)	(0.567)	(1.127)
			[-0.742]		[-1.018]		[-0.833]
Male	-0.559**					-1.061***	-2.345***
	(0.264)					(0.388)	(0.780)
Age	-0.026					0.005	-0.023
	(0.016)					(0.027)	(0.057)
Years in School	0.004					-0.064	-0.021
	(0.062)					(0.092)	(0.185)
Race Black	-0.118					-1.291**	-2.593**
	(0.347)					(0.572)	(1.202)
Race Asian	0.320					-0.383	-0.256
	(0.329)					(0.483)	(0.949)
Race Hispanic	0.344					-0.902	-1.480
	(0.633)					(0.960)	(2.015)
Republican	-0.300					-1.071	-3.396**
	(0.494)					(0.689)	(1.532)
Democrat	-0.091					0.707	1.608*
	(0.326)					(0.466)	(0.928)
Independent	0.121					0.205	0.225
	(0.370)					(0.567)	(1.143)
Constant	0.405	2.494***	0.408	3.033***	1.808	3.908***	3.909**
	(0.465)	(0.451)	(0.928)	(0.578)	(1.147)	(0.888)	(1.761)
Observations	129	302	302	302	302	291	291
R-Squared	.	0.083	.	0.103	.	0.175	

The dependent variable in specification (1) indicates whether the subject bought the information when given the opportunity. We report the coefficients of a probit model. In specifications (2) - (7), the dependent variable is the transfer to the recipient. We report the results for OLS and Tobit models. ***, ** and * denote significance at 1%, 5% and 10% level, respectively. Standard errors are in parentheses. For the Tobit models, we also report unconditional expected marginal effects at the observed censoring rate in brackets below the standard errors.

TABLE 5 – PREDICTED TRANSFERS, CONTROLLING FOR DEMOGRAPHICS

		Information: Does the Dictator Know His Recipient Type? Yes	No
Cannot buy information	Paired with disabled	5.05 (0.13)	3.91
	Paired with drug user	3.46 (0.54)	
Can buy information	Paired with disabled	5.07 (0.21)	2.76 (0.10)
	Paired with drug user	1.10 (0.00)	
Structural parameters[13]	ϕ_1 : t1's giving when she is uncertain		6.25***
	ϕ_2 : t2's giving when she is uncertain		2.76***
	σ_{11} : effect of t1 learning she faces a disabled recipient		-0.33
	σ_{12} effect of t1 learning she faces a drug user		-3.84**
	σ_{21} effect of t2 learning she faces a disabled recipient		1.86*
	σ_{22} effect of t2 learning she faces a drug user		1.20
Simulated parameters with imposed beliefs on type 1	ϕ_1 : t1's giving when she is uncertain		4.16
	ϕ_2 : t2's giving when she is uncertain (assumed)		3.78
	σ_{11} : effect of t1 learning she faces a disabled recipient		1.75
	σ_{12} effect of t1 learning she faces a drug user		-1.75
	σ_{21} effect of t2 learning she faces a disabled recipient		0.84
	σ_{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 F-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 ϕ_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 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.

Written 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).

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 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."

Open the white envelope.]

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.