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COMPASSION OR CASH:
EVALUATING SURVEY RESPONSE INCENTIVES AND VALUING PUBLIC GOODS

V. Kerry Smith
Sharon L. Harlan
Michael McLaen
Jacob Fishman
Carlos Valcarcel
Marcia Nation

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ABSTRACT

This paper reports the results of an experiment evaluating the effects of incentives on individuals' willingness to participate in a survey. By pairing the assessment with a natural field experiment, the analysis considers private versus public goods as incentives, and estimates respondents' willingness to support the oldest food bank in the U.S.

V. Kerry Smith
Department of Economics
W.P. Carey School of Business
P.O. Box 879801
Arizona State University
Tempe, AZ 85287-9801
and NBER
kerry.smith@asu.edu

Sharon L. Harlan
School of Human Evolution and Social Change
Arizona State University
Tempe, AZ 85287-2402
Sharon.Harlan@asu.edu

Michael McLaen
PracticeMax
9382 E. Bahia Drive, Suite B202
Scottsdale AZ 85260
mikemclaen@cox.net

Jacob Fishman
Department of Economics
Arizona State University
Tempe, Arizona 85287
fishman.jk@gmail.com

Carlos Valcarcel
Department of Economics
Center for Environmental Economics and Sustainability Policy
Arizona State University
Tempe, AZ 85287-9801
Carlos.Valcarcel@asu.edu

Marcia Nation
Julie Ann Wrigley Global Institute of Sustainability
Arizona State University
Tempe, AZ 85287-5402
Marcia.Nation@asu.edu

A data appendix is available at:
http://www.nber.org/data-appendix/w21288

Compassion or Cash:

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I. Introduction

This paper reports a unique assessment of the effects of incentives on individuals' willingness to participate in a social survey.[1] We consider monetary incentives, contributions to a charity, and the option to choose either the cash or a contribution of the same amount. Both the monetary offer and the treatment offering the ability to select cash or a contribution were significantly more likely to increase participation when compared with the treatment offering to donate the same amount to charity. Respondents who received the option allowing a choice of cash or a contribution, and who participated, were less likely to donate as the size of the monetary incentive increased.

Our findings are relevant to three different sets of research: (1) we are the first, to our knowledge, to evaluate the effectiveness of a private good (cash) versus a public good (charity) in obtaining survey responses; (2) our findings are also relevant to the literature on charitable giving. By using a strategy relatively free of social pressure, respondents can give to a charity without being influenced by peer effects.[2] Neutralizing such peer effects is important because about 75 percent of giving has been estimated to be due to social pressure (DellaVigna, List and

[1] See Mercer et al. [2015] for a recent meta analysis of the evidence on the role of incentives for survey responses.
[2] Respondents are given the option to take the survey online, by phone, or in person. The majority (64%) who completed the survey during this experiment took it online.

Malmendier [2012]). As a result, past research does not offer the ability to estimate the private demand for charity in a way that is free of this social pressure.[3] And (3) our choice based treatment—cash versus a contribution—extends the logic of field experiments, combining them with surveys, and provides a new strategy for measuring the value of public goods.

Our choice option is a natural field experiment where subjects decide to give to the charity.[4] The charity is fixed. They must complete the survey to be able to "pay for" the services of the charity. They make their contribution by surrendering the monetary incentive. The subjects are a subset of the random sample of residents of the Phoenix metropolitan area. The potential respondents were randomly assigned to the different treatments associated with the experiment. The final sample was determined by the respondents' participation decisions within the fixed time interval at the outset of the field time of a survey associated with the Central Arizona Project Long Term Ecological Research Site (LTER).[5]

Section II outlines the design of our experiment, conducted during the first phase of the 2011 Phoenix Area Social Survey. Section III summarizes our results. Section IV we consider their implications for each of the research areas we highlighted. The last section summarizes the findings.

II. Experimental Design

[3] More specifically, this literature focuses on mechanisms to increase monetary gifts (Landry et al. [2006], List and Lucking-Reiley [2002]), the reasons for giving to charity (Croson and Shang [2009], DellaVigna, List, and Malmendier [2012], List [2011]) and the effects of changes in the price of charitable giving (Clotfelter [1990] and Karlan and List [2007]).

[4] Harrison and List [2004] identified six factors to be considered in determining whether an experiment can be considered as taking place "in the field". Their factors include: the nature of the subject pool; the information subjects bring to the task; the nature of the community; the nature of the task or trading rules applied; the nature of the financial stakes; and the nature of the environment. (P. 1012)

[5] The primary objective of the survey required that adjustments be made in efforts to recruit respondents. As a result, the experiment had to stop when these changes were made.

The Phoenix Area Social Survey (PASS) is one of the activities of the NSF sponsored Central Arizona Project (CAP) LTER at Arizona State University (ASU). A pilot study for the first implementation of this survey was conducted in 2001-2002 and full administration of the social survey was completed in 2006. Our analysis was undertaken as part of the replication of the survey in 2011. It used the neighborhood definitions established in 2006 and added five new neighborhoods. The 2006 definition for the neighborhoods was based on two criteria: the network of monitoring sites for local ecosystems in the Phoenix metropolitan area and the identification of local communities based on demographic criteria including income, ethnicity, and retirement status. The 204 ecological monitoring sites maintained as part of the CAP LTER are used to study vegetation, soil, and other ecological variables on 30 x 30 meter sample plots distributed over all types of land uses in the study area (see Grimm and Redman [2004]). The sampling frame for neighborhoods was determined by examining aerial photographs of the areas surrounding 101 monitoring sites within residential neighborhoods (other sites were located in undeveloped desert or farmland). 94 sites (101 in residential areas less the 7 eliminated sites)[6] were aligned with Census Block groups to identify the socio-economic characteristics of the sampling units. Eight categories were specified in defining these groups of neighborhoods: low income urban core;[7] low income suburban; middle to high income urban core; middle income suburban; low to middle income fringe areas;[8] high income suburban; high income fringe; and retirement communities. Five neighborhoods were selected from each category to reflect variability in the demographic composition and the mix of home owners and renters. This

[6] Seven sites of the sixteen visited were eliminated because the residents were not close to the plot used for monitoring.
[7] Urban core neighborhoods are within 5 miles of downtown Phoenix or within 1.5 miles of the other 7 large city downtowns. The exact distances somewhat based on historical development patterns.
[8] Urban fringe areas are defined as having a moderate amount of undeveloped land within a mile of the neighborhood as of 2005.

process yielded a total of 40 neighborhoods in 2006. In 2011, five new neighborhoods from a set that were important sites for CAP LTER research (one monitoring and four others) were added to the sample following the same basic structure for identifying neighborhood characteristics.

The survey used a multi modal format and was administered by the ASU Institute for Social Science Research from May 26, 2011 to January 6, 2012. 2,127 potential respondents were selected as part of the sample design. Household selection had two dimensions in the 2011 survey. All addresses for the 2006 survey respondents were included in the sample. These were supplemented with other residential addresses in the sampled neighborhoods. These addresses were randomly sampled from an enumerated list of tax assessor parcels. The survey was announced to the sample with several initial mailers.[9] First, a postcard in English and Spanish was sent to the selected addresses notifying the potential respondents of the project and the specific, randomly assigned incentive for them to participate. Second, a letter in English and Spanish was sent explaining how to complete the survey along with the same assigned incentive. The letter included a brochure describing the project in both languages, a one dollar bill, and a magnet with a graphic design for the project. When the data collection was ended, 806 completed surveys were obtained with a response rate of 43.4% computed using the standard definition of the American Association for Public Opinion Research [2011].

The incentives varied in two dimensions based on: the amount of incentive and the way it was offered. Three different monetary values ($10, $20, and $30) were used in each of three different structures: (a) as a monetary incentive to be mailed to respondents after they completed the survey; (b) as a donation of one of the three amounts to the First Food Bank Alliance (also

[9] The Appendix A provides a brief summary of the details as well as copies of the materials sent to announce the survey.

known locally as St. Mary's Food Bank)[10] when the survey was completed; and (c) as either a check for one of the three monetary values or a donation of that amount to the food bank upon completion. The respondents in this treatment could select their preferred option. They could not modify the amount to be donated, it was all or nothing.

The objective of the survey was to collect information about the knowledge and environmental attitudes of the Phoenix area population with as high a response rate as possible. As a result, the experimental period was limited to May through the end of August of 2011. After that, the focus was changed to increased monetary incentives to encourage a high level of participation. Our analysis is limited to the respondents who agreed and completed the survey or who declined or terminated interviews during the experimental period. 557 interviews were completed and 187 were refused or terminated during the experimental period. Table 1 summarizes the distribution by experimental treatment and final interview status. The original assignment of treatments by neighborhood was intended to be random. The random assignment of treatments was made to the 2,127 identified as potential respondents. As indicated in the next to last row of Table 1, the final disposition does not appear consistent with a random assignment. Disproportionate assignments were made to the $20 or $30 cash offers and to the $30 choice treatment. The distribution relevant for the experimental period is the next row up where the $20 and $30 cash treatments are larger than any of the others. These distributions were assigned randomly for each of the 45 neighborhoods, rather than to the composite sample without regard to neighborhood. The cash treatment with the $30 incentive continues to be the largest group with the $20 cash also large. The remainder appear balanced considering the overall sample.

[10] There is no religious affiliation associated with the food bank and in order to prevent attitudes toward Catholicism from influencing responses, we did not refer to the food bank as "St. Mary's" in our communication with respondents.

The specific treatment each respondent received is random. This unequal distribution influences the precision of our estimates and the ability to recover measures of responses for all treatments in all 45 neighborhoods but not the randomization at the individual respondent level. Thus it should not bias our comparisons evaluating the effects of the treatments.

We evaluated the decisions to take the survey in several different ways. First, relying on the random assignment to each housing unit (and respondent) we estimate ordinary least squares (OLS) models with participation decision as a binary dependent variable and the features defining each treatment as independent variables.[11] These include the amount of the monetary offer and dummy variables for cash, charity, or the choice formats. The second model includes fixed effects for the neighborhoods along with the design related variables as determinants of each person's decision about whether to participate. The last approach is non-parametric. In this case, each of the nine possible treatments is interacted with dummy variables identifying each of the 45 neighborhoods. We use OLS, with the dummy variable for the participation decision, as the dependent variable and these interaction terms to measure the response rates for each combination where there is sufficient sample to recover an estimate. These estimated coefficients are then used as dependent variables in second stage models. In this case the framework relies on what can be detected using these estimates for average response rates across the treatment/neighborhood combinations for analyzing the experimental data cells. The second and third approaches provide different strategies to assess whether our conclusions would be affected by the disproportionate assignment of treatments to some neighborhoods.

Finally, for those respondents who received the treatment allowing a choice of cash or the charity, we also evaluate what they decided as the monetary incentive changed. This analysis

[11] While early research considering binary response, dependent variable models favored probit or logit, Angrist and Pischke [2009] and Wooldridge [2010, Chapter 15] conclude that the linear probability (OLS) model generally provides reliable estimates of the directions and magnitudes of the average effects of independent variables.

offers the ability to estimate their willingness to support the food bank. These estimates are free of the social pressure often associated with other efforts to evaluate the motives for charitable giving.[12]

III. Results

Table 2 provides the estimates for our linear probability models evaluating the factors that influenced the decision to participate in the survey. The first column is a model that includes the amount of the monetary incentive and dummy variables for each of the ways it was offered. We exclude the intercept to allow estimation of the separate effects for the cash, donation, and choice treatments. The size of the incentive is not a significant determinant of participation and this result holds regardless of the model specification considered, including non parametric estimates.

Our finding contrasts with the overall conclusion of the Mercer et al. [2015] meta-analysis. These authors found the best overall summary of their evaluation of financial incentives was that the past analyses were consistent with the size of the offer having a significant, nonlinear effect on response rates. Their analysis does have some important differences with our research. It involves survey-wide response rates so the meta-regression captures differences across different surveys. By contrast, our analysis involves the same survey instrument and evaluates differences across potential respondents at the individual level. The modes considered in Mercer et al.'s summary were mail, telephone, and in person interviews but were constant for each survey. The modes in our survey were the outcome of the respondents' decisions as part of

[12] As noted at the outset, the survey could be taken online, by telephone, or with a scheduled personal interview. A multinomial-logit model considers the demographic factors influencing the choice of online, telephone, or in person response rates indicated that whites were significantly more likely to use online or telephone, and education was a significant positive factor in the selection of the online mode.

the decision to take the survey. Finally, our analysis did not have a treatment with no incentive. As a result, it was not possible to consider the response patterns compared to this situation.

All of the coefficients for the dummy variables for the mode effects are statistically significant. Model 1's results confirm that providing cash yields significantly higher participation than when the same amount is offered as a donation. The same is true for the treatment offering a choice of cash or the contribution when compared to a donation. These test results are reported in the two rows below the number of observations for each model. Finally, a test for differences in the coefficients for the cash treatment and a choice to donate or keep the cash fails to reject the null hypothesis of equality.

The second column includes fixed effects for each neighborhood serving as a sampling unit in the model along with the dummy variables for the form used in offering incentives. None of the conclusions summarized based on model (1) are altered. The last two columns in the table ask a related question, namely, does the comparison of cash versus the choice treatment change if we account for each respondent's decision to donate or keep the incentive? To address this question, it is important to acknowledge the decision of whether or not to keep the cash is likely correlated with the decision to participate. As a result, the models in columns 3 and 4 are based on instrumental variable (IV) estimates using the randomly assigned monetary incentive as the instrument for the choice to donate. As the models in the first two columns indicate, the monetary incentive was not a significant determinant in potential respondent's decision to participate. However, it was a significant determinant (as we discuss in more detail below) in their decisions to donate the money they received for completing the survey.[13]

[13] A simple regression indicates a significant negative relationship with a F =10.89 (*p*-value = 0.001) for the joint null hypothesis of no association.

Using the IV estimator based on the predicted donation choice from a model with the size of the monetary incentive offered, we find the test results for the effects of cash incentives versus donation do not change. Cash and choice are not significantly different in their incentive effects. Moreover, these conclusions are also not affected if fixed effects for the neighborhoods are included in the models. One contrasting finding with these estimates arises with the difference between coefficients associated with choice and donation. These coefficients would not be judged as significantly different. However, the magnitudes of the estimated parameters are comparable to what we found in OLS models omitting the endogenous decision to donate effect. Thus, this change seems likely to be due to the larger standard errors for the IV estimates.

Our last comparison of the effects of different types of incentives is given in Table 3. Here we report the results for the two step non-parametric test of the effects of the types of incentives. The analysis is now more comparable to Mercer et al. in that we are comparing the estimated average response rates for cells defined by the neighborhood and treatment received. With our small sample and disproportionate assignments, this strategy is a more demanding approach for testing these hypotheses. As the sample size at the bottom of Table 3 suggests, we are not able to estimate response rates for all of the possible combinations of neighborhood and treatment alternatives. The sample includes 271 versus 405 (9 x 45) of the possible alternatives. All of the estimates use the estimated standard errors from the OLS first stage estimates of the coefficients for the dummy variables for each neighborhood/treatment alternative to construct feasible generalized least squares (FGLS) estimates for assessing the incentive effects. Model 1 considers the monetary incentive alone. Model 2 includes the dummy variables for the format in which the incentive was offered. The findings are consistent with the analysis at the individual level. We report the estimated difference between the response rates and the p-value for tests of

equality. Cash incentives and a choice of cash or donation lead to significantly greater participation than offering to donate, regardless of whether the model takes account of the monetary incentives.

Table 4 reports the influence of the size of the monetary incentive on the decision to donate. The sample is restricted to the individuals who agreed to participate and received the choice treatment. Several aspects of these results are notable. First, the likelihood of donating declines as the size of the monetary incentive increases. Model 1 is based on the choices of these respondents receiving the choice treatment.[14] Model 2 restricts the sample to those who received the choice treatment and completed the survey online. It reflects the choice of individuals who experienced the most limited social pressure to donate. The results are comparable.

It is possible to use these models to estimate the average respondents' economic value for the public good services provided by the food bank. This last element stems from the ability to adapt the participation incentive so it provides a natural field experiment. To develop this estimate, consider a simple model for each survey participant's decision to donate to the food bank. Equation (1) specifies a linear, indirect utility function for the utility realized if a respondent donates his financial incentive to the food bank (V_C) and (2) if he does not (V_{NC})

$$(1)\ V_C = \alpha_C + \alpha_1 m + \alpha_2 T + \epsilon_C$$

$$(2)\ V_{NC} = \alpha_{NC} + \alpha_1 (m + a) + \alpha_2 T + \epsilon_{NC}$$

This formulation is similar to the logic Hanemann [1984] originally outlined to derive the welfare properties of discrete choice models. We assume the marginal utility of income (m),

[14] We also considered a sub-sample of the respondents receiving this treatment who agreed to provide their household income. The choice to donate model was:

Donate = +.398 - .012 Dollar Incentive + .0018 income
(2.99) (-2.26) (2.28)

Income = household income in thousands of dollars, Number of observations = 99, R^2 = .11
As other literature has suggested, the likelihood of donating the monetary incentive to charity increases with the income level of the respondent.

designated by α_1, remains constant across the two decisions. a represents the monetary incentive. Since the model describes the choice for an individual who has agreed to participate, it recognizes that taking the survey implies the time commitment, represented here by T. With this treatment, a decision to donate the incentive implies the respondent's "out of pocket" cost is the time for the survey. By contrast, a decision to keep the incentive offsets the time cost of doing the survey. The α_C and α_{NC} terms capture the difference in well-being a person experiences with the decision to donate versus not donate to the food bank.

It is not possible to unpack whether different respondents interpret the "amount" of public good services provided by the food bank in relation to the warm glow they might experience from giving to any cause. More specifically, if we re-formulate the two state-specific preference functions to include the total amount given to charity by others, say Q, and allow for the possibility of a warm glow effect, then it is possible to illustrate why testing this hypothesis is unfeasible. When the individual contributes a, V_C would then be given as equation (3):

$$(3)\ V_C = \alpha \cdot f(Q + a, a) + \alpha_1 m + \alpha_2 T + \epsilon_C$$

and V_{NC}would be equation (4):

$$(4)\ V_{NC} = \alpha \cdot f(Q, O) + \alpha_1 (m + a) + \alpha_2 T + \epsilon_{NC}$$

$$\text{where } \alpha_C = \alpha \cdot f(Q + a, a) \text{ and } \alpha_{NC} = \alpha \cdot f(Q, O)$$

Using these expressions for $V_C$and V_{NC} we see more information on respondents' beliefs about Q and assumptions about the form of $f(\cdot)$ would be needed to test warm glow.

In principle, such analyses could be undertaken in future research. This pairing of field experiments and response incentive offers a strategy for addressing some of the limitations that List [2011] identified as being associated with the existing research on the demand for charitable services. For example, a revised incentive structure could distinguish these effects using the

results from Eckel et al.'s [2005] laboratory experiments with undergraduates. More specifically, these authors found that explaining exogenous contributions to a charity as the result of a tax on subjects' endowment resulted in a crowding out of their subsequent contributions and thus reduced or eliminated warm glow effects. Using this logic it would be possible to explain to a subset of the potential survey respondents that a portion of the incentive they *would have earned* was already earmarked as a "tax" for the food bank and ask if they wished to contribute the balance of their proposed financial incentive. Finally, to derive our measure for the economic value of donating, assume ϵ_C and ϵ_{NC} represent unobserved heterogeneity associated with the two states. A person donates if $V_C - V_{NC} > 0$, and we can use the estimated parameters for our choice model to recover a measure for the maximum donation, a^*, participants in the survey would make on this "giving occasion". Equation (5) solves for a^* consistent with $E(V_C) = E(V_{NC})$ given

$E(\epsilon_C - \epsilon_{NC}) = 0.$

$$(5)\ a^* = \frac{\alpha_C - \alpha_{NC}}{\alpha_1}$$

The time commitment is the same regardless of whether the individual donates or not and thus drops from the relationship.

Our estimates for the maximum contribution can be interpreted using Kotchen's [2015] analysis.[15] He demonstrates that the maximum take it or leave it donation can exceed the amount a person would contribute if he had discretion in selecting the amount. Our experiment recovers a partial measure for the concept he envisioned as the take it or leave it value of donating. In our case, the value is for contributions to the food bank on a given occasion. These estimates are computed for two samples, one with all respondents receiving the choice treatment and for the

[15] While Kotchen's analysis focuses on the donation vehicle as part of a contingent valuation survey, his model is directly relevant to the choice implied by our field experiment.

subset who answered using the online survey model and might be expected to experience the lowest amount of social pressure. Both estimates are significantly different from zero. They are $38 and $39 respectively. As we discuss in the next section, there are other elements to include in the full monetary measure of the take it or leave it donation value. These additional components would include the opportunity cost of the time to take the survey. Our design precluded recovering a measure for this added value because the sample used to estimate a^* had to agree to take the survey in order for our analysis to observe their donation decisions.

IV. Discussion

Perhaps the most interesting component of this analysis arises from embedding a natural field experiment within our assessment of response incentives. By allowing respondents to decide whether to keep or donate the financial incentive, it is possible to estimate a part of the economic value of the public good services provided by First Food Bank Alliance. A take it or leave it opportunity to contribute a fixed amount implements a variant of the logic envisioned in Kotchen's [2015] analysis of the donation vehicle for contingent valuation surveys. In our case, the choice is a "real one" that fits the structure of his model. Using the model to evaluate the economic value of the food bank's services would require an expansion in his framework that addresses how people consider opportunities to donate within a setting that links what might be described as planned overall giving in a well-defined time horizon with the choices made in each specific opportunity to donate. To our knowledge this issue has not been addressed in the literature on charitable giving. List [2011] discusses annual giving, he describes who makes these donations, and how they are distributed among different charitable groups. The time horizons discussed are annual and the potential distribution over a lifetime, not in terms of each specific opportunity to give.

There are several ways of conceptualizing this task. We consider two possibilities. Unfortunately both require more information than we collected in this initial experiment. It is nonetheless worth sketching the logic of each because it would be possible to use the basic framework treating survey incentives as a source for natural field experiments to investigate the interrelationship between the mix, timing, and total amount given to charity over a household's budgeting cycle. The first possible interpretation of the timing of charitable contributions would be to treat them as akin to repeated "use" or consumption, where the frequency of purchases is endogenous. Liu, Rettenmaier and Saving [2011] consider a simple static model that distinguishes a fixed cost of consumption from the unit price of the commodity or service experiencing repeated consumption. In their analysis this fixed component or "setup cost" could be a travel cost for a recreation trip and the price could be the unit cost of the activity such as ski lift costs or lodging.[16]

The analogy to the setup cost with charitable contributions would be the time and costs associated with investigating the reliability of a charitable organization. This assessment could involve evaluating the share of contributions used for administration and management, performance in meeting stated objectives, and so forth. Costs arise in obtaining reliable information to address these issues. The Liu et al framework suggests that as the setup costs increase, the frequency of giving to otherwise completely equivalent charities is likely to decline and the intensity of giving to just one of them increases. The implications for total giving in the time period used to characterize overall consumption expenditures (across all of the equivalent charities in their analysis) would be an empirical issue. Food banks are widely recognized as having among the lowest administrative costs and having reliable and predictable outcomes. This

[16] These types of examples have a long history in economic modeling of recreation demand. See Phaneuf and Smith [2005] for a review.

is certainly true of the First Food Bank Alliance. As a result, this logic does not provide predictions for the frequency of giving or the total amount in a given budgeting period. As a result their model provides a framework for specifying hypotheses for testing and a clear motivation for further empirical research

Using a parallel to second literature associated with consumer responses to sales and holding inventories of storable goods (see Hendel and Nevo [2004]) leads to a similar general conclusion. This literature would require more assumptions about how people conceptualize the time distribution of giving. Resources available for meals and warm clothing, for example, may be more important in the winter than in the fall and spring. Needs in the summer would likely be different and depend on the location of the food bank. To use this literature to gauge inter-temporal substitution of giving would require more assumptions about how people envision what different distributions for a given amount of contributions to charity over time will accomplish.

We might be able to learn about inter-temporal substitution by using events that motivate individuals to alter a planned pattern of giving such as natural disasters. These events may well trigger the substitution of an immediate opportunity for other longer term or more permanent needs. Field experiments require advance planning, navigating internal review board requirements and often significant resources. As a result, it is difficult to take advantage of strategic opportunities caused by natural events. By using field experiments as part of survey incentives it should be possible to design a research question associated with disasters to both consider an important question—inter-temporal displacements of donations and "do good" by helping those in need as a result of the disaster providing the immediate need for help.

As we noted with our specific application, additional information would be needed to pursue the hypotheses implied by either of these models. It is also not impossible to implement

fully a variant of Kotchen's model. We do not know what the respondents were already giving to the food bank. Moreover, the range of incentives offered to them limit what we can say about donations when people were provided a wider range of incentive values. Nonetheless, our results do demonstrate that this strategy offers a viable basis for estimating people's willingness to pay for increases in the amounts of these types of public goods. We can go a bit further in considering the maximum contribution on a gift occasion by including an estimate of the opportunity cost of time and use the two sets of results to gauge the implied value of the food bank's services. Smith and Mansfield's [1998] estimates of the value of time, based on the compensation offered to North Carolina households in 1995-96 for their time spent in a telephone interview, provide the closest parallel to the current situation. Based on these results, we estimate that the average respondent gave up 30 minutes that would be worth between $12 and $24 (in 2011 dollars).[17] This decision was made regardless of whether they kept or donated the money. By keeping the incentive, they would cover the opportunity costs of time on average. One might argue that those who donated the incentive would also have been willing to contribute an additional $12 to $24 if they did not have to take the survey.[18]

A further question one might ask when evaluating the economic value of charity to a food bank concerns the public services that respondents believe they are receiving when they donate. While the analytical models for charitable contributions assume perfect substitution between individual gifts in defining the amount of the public good and generally represent them as the

[17] The Smith-Mansfield estimates were based on an offer to complete a second telephone survey. Depending on the specific statistical model selected from this study, the estimate for the per hour opportunity cost of time ranged from $19.65 to $32.74 in 1995 dollars. Using the consumer price index to convert from 1995 to 2011 dollars implies these should be scaled by 1.476. The PASS survey took ½ hour, adjusting for the time and rounding the resulting estimates yields these results.

[18] Of course, this logic accepts the fungibility of time and money implicit by the use of the Smith-Mansfield estimates to value these respondents' time. Other research has suggested that the opportunity cost of time in different uses will be different and the people would not necessarily make this type of exchange in all contexts. See Palmquist, Phaneuf, and Smith [2010] for related discussion.

sum of the cash contributions (see Andreoni [1989], List [2011]), the charities tend to describe the effects of contributions by translating into outcomes.[19] The First Food Bank Alliance, for example, routinely translates dollar contributions into meals (at a fixed ratio) such as "$30 to provide 210 meals". This logic is consistent with the strategy used for framing contingent valuation surveys since the Exxon Valdez survey (Carson et al. [1992]). That is, these surveys described the object of choice offered to survey respondents as a plan to avoid specific injuries, rather than a specific improvement in the environment. Survey respondents in these cases make choices about the plan. In principle, the natural field experiment could be designed to include details of how survey incentives would be used to "produce" different types of public or impure public goods.

V. Summary and Implications

To our knowledge this study is the first effort to compare public goods with private goods (cash) as response incentives for a household survey. We found that our public good—a contribution to a food bank—was not as effective as either a cash incentive or a format that allows each respondent to decide to keep or donate the cash[20]. Our findings on the effects of the amount of the incentive contrast with the existing literature, that found ". . . a strong, nonlinear effect of incentives across all three models of data collection included in the analysis." (Mercer et al. [2005], p. 124). While the results reported here focused on models specified to be linear in the incentive, we also considered non-parametric, nonlinear (log transformed), and interaction variables for the amount of the incentive with the form of the incentive (i.e. cash, donation or choice). None of these alternatives changed our overall conclusions.

[19] We are grateful to Kelly Bishop for suggesting consideration of this strategy.
[20] St. Mary's Food Bank Alliance was established in 1967 and is recognized as the first food bank in the world and one of the largest in the U.S. See www.firstfoodbank.org/learn-more/our-history.

Our approach does contrast with past assessments of response rates that tend to involve comparisons across surveys which used a single mode for their responses. Our respondents could choose the mode for their response – from online, telephone, or in person interviews. Incentives, whether cash or donation, were paid after the survey was completed.[21] While past research on survey incentives found that prepaid incentives were more effective than promises with delayed payment, Mercer et al. note the effects are not uniform for all models. Our research argues that the use of incentives to enhance participation in surveys provides an opportunity for conducting field experiments. It is possible to reduce the front-end negotiation for access to different groups for experiments and broaden the types of subject pools studied. Of course, for this linkage to be cost effective the front-end field experiments cannot compromise the objectives of the surveys that offer the mechanisms for undertaking the experiments.

[21] A total of $2,570 was contributed to the food bank as part of this research.

Table 1: Survey Outcomes by Experimental Design and Sample

	Incentive Option That Respondent Was Originally Offered									
Final Outcome	**Cash 10**	**Cash 20**	**Cash 30**	**Donate 10**	**Donate 20**	**Donate 30**	**Choice 10**	**Choice 20**	**Choice 30**	**Total**
Asian Language	0	0	0	0	1	0	0	0	0	1
Bad Mail	6	32	36	8	7	9	14	14	9	135
Field Complete	0	12	26	3	2	1	3	0	1	48
No Mail Receptacle	1	0	0	0	0	0	0	1	0	2
Non-Contact	0	1	84	0	0	0	0	0	6	91
Non Resident	0	1	0	0	0	0	0	0	0	1
Online Complete	45	70	131	28	26	25	35	38	41	439
Partial Interview	0	0	1	0	0	0	0	0	0	1
Refusal	17	21	48	16	25	18	11	19	10	185
Telephone Complete	8	16	21	1	4	1	5	9	5	70
Terminate	1	0	0	0	0	0	1	0	0	2
Undeliverable	0	0	0	1	0	0	0	1	1	3
Vacant	15	57	61	22	14	15	13	17	13	227
Distribution for Possible Sample During the Experimental Period	93	210	408	79	79	69	82	99	86	1,205
Distribution for Full Sample	179	327	435	166	164	157	160	166	373	2,127
Distribution for Analysis of the Experiment	71	119	226	48	57	45	55	66	57	744

Table 2: OLS and IV Estimates for Experimental Treatments [a]

	Model 1	Model 2	Model 3	Model 4
Dollar Incentive	0.00038	0.00149	—	—
	(0.19)	(0.74)	—	—
Money / Cash	0.782	0.820	0.791	0.744
	(14.84)	(5.60)	(37.24)	4.45
Donation	0.599	0.680	0.607	0.592
	(11.22)	(4.53)	(17.15)	3.47
Choice	0.762	0.813	0.797	0.841
	(14.64)	(5.52)	(5.23)	3.88
Give to Charity	—	—	-0.117	-0.471
	—	—	(-0.19)	(-0.70)
Fixed Effects	No	Yes	No	Yes
R^2	0.756	0.788	—	—
No. of observations	744	744	744	744
Test: Cash = Donate	16.06	9.73	19.93	11.16
p-value	0.0001	0.002	0.000	0.001
Test: Choice = Donate	10.19	7.43	1.49	2.09
p-value	0.002	0.007	0.223	0.148
Test: Cash = Choice	0.27	0.03	0.00	0.37
p-value	0.603	0.862	0.966	0.545

[a] The numbers in parentheses below the estimated coefficients are the t-ratios for the null hypothesis of no association. In the case of models 3 and 4, these are asymptotic Z-statistics for the IV estimates.

Table 3: Second State FGLS Estimates for Treatment Affects [a]

	Model 1	Model 2	Model 3
Dollar Incentive	0.0002	-0.0003	—
	(0.11)	(-0.15)	—
Money / Cash		0.8209	0.8144
		(17.65)	(45.06)
Donation		0.7392	0.7339
		(15.44)	(22.50)
Choice		0.8408	0.8354
		(18.31)	(29.45)
Intercept	0.8006	—	—
	(19.39)	—	—
R^2	0.925	0.927	0.927
No. of observations	271	271	271
Test: Cash = Donate		0.082	0.080
p-value		0.033	0.032
Test: Choice = Donate		0.102	0.101
p-value		0.020	0.020
Test: Cash = Choice		-0.020	-0.021
p-value		0.561	0.533

[a] The numbers in parentheses are asymptotic Z statistics for the null hypothesis of no association.

Table 4: OLS Estimates for Choice to Donate Incentive to Charity [a]

	Model 1	Model 2
Dollar Incentive	-0.01292	-0.01558
	(-3.19)	(-3.02)
Intercept	0.49588	0.61799
	(5.24)	(5.06)
No. of observations	178	114
R^2	0.058	0.077
WTP for Food Bank	38.37	39.67
	(6.88)	(6.30)

[a] The numbers in parentheses for parameter estimate are t statistics for the hypothesis of no association. Those below the WTP estimate are asymptotic Z statistics for hypothesis WTP = 0.

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