Essays in Public Economics

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Submitted to the Graduate Faculty of

the Dietrich School of Arts and Sciences in partial fulfillment

of the requirements for the degree of

Doctor of Philosophy

University of Pittsburgh

2022

UNIVERSITY OF PITTSBURGH

DIETRICH SCHOOL OF ARTS AND SCIENCES

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March 14, 2022

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University of Pittsburgh, 2022

My dissertation is a collection of essays on topics in public economics. In particular, the essays examine questions pertaining to the economics of nonprofit organizations. The first two chapters of my dissertation address the financing of nonprofits, specifically through charitable giving. In these two chapters I pay particular attention to the efficacy of certain fundraising strategies. The third chapter of my dissertation explores the effectiveness of pay-for-performance incentive schemes in nonprofit institutions. This work identifies the impact of different incentive schemes on worker effort, as well as the types of workers who select into nonprofit sector versus for-profit sector jobs.

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Preface

I am extremely grateful to my advisors, Lise Vesterlund and Jason Cook. Their mentorship, encouragement, and guidance during my time at the University of Pittsburgh made this dissertation a reality. I am indebted to them for their constant support and for the opportunities they have opened for me, even during such challenging times. I would also like to thank David Huffman, Osea Giuntella, and Alistair Wilson for their help and support throughout these past few years, in addition to the rest of the faculty and administrators in the Economics Department.

Special thanks go out to the graduate students, particularly my cohort and officemates. Your immeasurable kindness and encouragement helped me navigate the ups-and-downs of graduate school. I am thankful to have spent these past six years with you all, in offices and meeting rooms both real and virtual, and to know I have made lifelong friends. It brings me so much joy to think about all of your impressive accomplishments, and I am excited to celebrate your many future successes.

Finally, I would like to thank my family. My parents taught me how to be curious, to persevere, and to always be decent and kind, values that have guided me through graduate school and will continue to guide me moving forward. My sister has always been my role model, and has shown me how to channel your passions toward purposeful work. And my last thank you is for Kathryn. Thank you for keeping things in perspective, for always lifting me up, and for sacrificing so much to let to me pursue my professional dreams.

1.0 The Gift of Giving: Recognizing Donors and Revealing Donation Amounts

Publicly announcing how much individuals donate on behalf of themselves is a common fundraising strategy. For tribute gifts made on behalf of others, however, charities only reveal donor identities to the honoree with few revealing the size of their contributions. As this simple form of donor recognition remains unexplored, this paper examines the fundraising consequences of recognizing donors with and without information about their donation. I do so both for giving on behalf of oneself and in the novel domain of tribute giving. I find that revealing contribution amounts in addition to recognizing donors benefits fundraisers, irrespective of the type of giving. For tribute donations in particular I find that both the likelihood of giving on behalf of others and contribution amounts increase when honorees learn how much donors give. The results either suggest that fundraisers are leaving tribute donations on the table, or that announcing the size of these gifts may be repugnant and constrains what practices fundraisers can implement.

1.1 Introduction

It is common practice for charities to recognize their donors publicly to thank them for their generosity. For example, organizations send honor-roll letters to their supporters that include donor names. Honor-roll letters also reveal information about how much individuals give, whether in specific donation amounts or in giving levels. These forms of communications have become a staple in the "best practices" of donor relations professionals (ADRP, 2021).

While practitioners note that these methods are a valuable means of reciprocating supporter engagement with their organization, donor recognition also has fundraising benefits. The practice of revealing donor names and their contribution amounts has been shown to be an effective tool in generating greater giving in the laboratory and in the field (e.g., Andreoni and Petrie, 2004; Rege and Telle, 2004; Karlan and McConnell, 2014; Samek and Sheremeta, 2017). Disclosing this information can motivate individuals who are concerned with their social image and prestige to increase their donations (Harbaugh, 1998; Benabou and Tirole, 2006).

Donor recognition is not only a practice implemented for contributions made on behalf of oneself but also for tribute donations made on behalf of others. Tribute donations are given to honor or celebrate joyous occasions, as well as to memorialize those who have recently passed. In lieu of traditional gifts, couples may request that wedding attendees give to their favorite charity; as an alternative to sending flowers, families may request contributions for a cause that was important to the deceased. To recognize individuals who make tribute donations, organizations notify honorees and stewards of memorial funds of tribute donations made on their behalf.[1]

Though donor recognition is an important practice irrespective of whether donations are made on behalf of oneself or others, charities approach donor recognition differently depending on the type of gift. For gifts made on behalf of oneself, it is common to see information revealed about the size of donations. In contrast, for tribute giving most organizations follow the "best practice that donation amounts are never disclosed unless otherwise specified by the donor" (Ibrisevic, 2019).

If publicly revealing donation amounts yields fundraising benefits for contributions made on behalf of oneself, one might posit that it would also benefit fundraising efforts when considering donations made to honor others. When only recognizing that a donation is made, individual donors receive the same recognition independent of gift size. Donors know that honorees will be notified of contributions made on their behalf without ever knowing how much donors give. As a result, they may decide to give less than they would have given had the size of their donation been revealed. However, only recognizing donors without revealing donation amounts may also increase the number of people who give. Absent other explanations, if the expected returns from larger-sized donations when revealing amounts outweigh the potential extensive margin benefits when only recognizing donors, it may suggest that charities are leaving money on the table by not disclosing tribute donation amounts.

Yet if we think about the broader context under which tribute donations are made, there may be more to consider when assessing how to recognize tribute gifts. Practitioners note that individuals who make tribute donations may be less familiar with the organizations receiving their support. It could be that fundraisers are more concerned with drawing in new donors who they can subsequently approach for future financial support, even if it means accepting smaller-sized contributions in the short-term. Only revealing that an individual donates, without revealing how

[1] I use the term "honoree" to represent both an honored person who is still living, and the stewards of memorial funds for honorees who are deceased.

much they donate, may provide organizations the best chance of achieving this objective.

In this paper, I explore the fundraising consequences of revealing contribution amounts in addition to recognizing donors. I examine this effect both for donations made on behalf of oneself and in the novel domain of tribute giving. When only recognizing donors, I evaluate both overall giving and whether individuals are more likely to give to charity. In complement, I document whether revealing donation amounts in addition to recognizing donors affects overall giving and the size of gifts among donors.

For donations made on behalf of oneself, this paper studies an alternative level of information revelation that could be used in donor recognition practices. Though disclosing both supporter names and contribution amounts is common, less is known about the effectiveness of only recognizing donors. Further, to the best of my knowledge this paper is the first to study tribute donations, which are fairly ubiquitous in practice but remain an unexplored topic in the literature. According to the *2018 Global Trends in Giving Report*, one third of surveyed donors worldwide had made a tribute donation on behalf of others, with over 40% doing so in the United States. A systematic examination of tribute giving is therefore a beneficial undertaking for multiple reasons. First, this paper documents the overall fundraising value of tribute donations. Second, this study offers evidence to evaluate whether existing donor recognition practices for tribute gifts generate the greatest fundraising benefit. Third, beyond donor recognition the results contribute to our broader understanding of actors in the charitable giving marketplace. Tribute donations are a unique form of giving that are influenced by a different set of decision factors, including distinct personal and social dynamics. This paper offers insights into the factors which shape the choice to give on behalf of others.

To study this question, I run a 2x3 between-subject laboratory experiment where participants can donate to charity. First, treatments vary along two donation-type dimensions: donations are either made on behalf of oneself (*Self*) or on behalf of others (*Other*). Second, treatments vary along three information dimensions that reveal different details about participant donations to others. These treatments are named *ID*, *ID&Amount*, and *Choose Info*.

I begin by describing the information treatments for donations made on behalf of oneself. In an *ID* treatment, participants who donate on behalf of themselves are told that their identities will be revealed to others in the same experimental session; however, how much they donate will not

be revealed. In contrast in an *ID&Amount* treatment, participants who donate not only have their identities revealed but also have their donation amounts disclosed to others. Finally in a *Choose Info* treatment, I gauge whether, conditional on donating, participants prefer to only reveal that they donate or if they also want to reveal how much they donate. All treatments conclude with a scheduled future donation, a novel assessment to evaluate whether recognition policies can affect later financial support for the charity.

When participants have the opportunity to donate on behalf of others, there are *ID*, *ID&Amount*, and *Choose Info* treatments that mirror the information environments when donations are made on behalf of oneself. However unlike *Self* treatments where donation information is revealed to others in the session, participants in *Other* treatments instead mail acknowledgment cards to individuals who they honor with their donations. In an *ID* treatment, participants who make a tribute donation mail cards to honorees that acknowledge their tribute gifts but do not include how much they donate. This treatment mirrors what honorees typically learn about tribute donations. In contrast in an *ID&Amount* treatment, participants who make a tribute donation also mail cards to honorees, but the cards now include contribution amounts. Finally in a *Choose Info* treatment, I gauge whether, conditional on donating on behalf of others, participants prefer to only reveal that they donate or if they also want to reveal how much they donate to honorees in the mailed cards. Across all *Other* treatments, individuals who decline to make tribute gifts can make donations on behalf of themselves or choose not to give at all. All treatments conclude with a scheduled future donation to evaluate whether recognition policies can affect later financial support for the charity, as is done in *Self* treatments.

The results highlight the advantage of standard fundraising procedures for gifts made on behalf of oneself, where donors identities and donation amounts are both revealed. Total giving increases by roughly 14% when donors are recognized in addition to how much they give. Individuals are also more than 7 percentage points more likely to give when the size of contributions are disclosed. At the individual level, I find that larger average contribution amounts are driven by the higher donation participation rate, rather than differences in average amounts given among donors. Finally, revealing donation amounts in addition to recognizing donors does not affect future giving behavior. The findings complement work that has shown the benefits of revealing donation amounts and donors relative to anonymous giving, for donations made on behalf of oneself.

For tribute donations, the results also illustrate that revealing the size of donations made on behalf of others is advantageous for the fundraiser. Overall giving is approximately 16% larger when contribution amounts and donors are revealed. While the overall likelihood of donating is not affected by revealing tribute contribution amounts relative to only recognizing donors, doing so increases the likelihood that individuals give on behalf of others by approximately 15 percentage points. The also results show that individual gifts are larger when they are told how much they donate in tribute will be revealed to honorees. Conditional on giving in tribute, participant donations are 16% larger when donation amounts are revealed compared to when they are not. The effect on tribute donations appears to drive the difference in the average size of all donations across treatments. This is what we would expect, given that tribute donations are the targeted type of giving.[2] Finally, revealing donation amounts above recognizing donors neither affects the likelihood, nor how much, individuals give in the future.

Altogether, the findings support existing fundraising practices for donations made on behalf of oneself where donation amounts are revealed. The results further suggest that fundraisers could benefit from changing donor recognition methods for tribute gifts.[3] However, the discordance between the study results and current fundraising practices invites a larger discussion about whether the choice to reveal tribute gift amounts involves a more complex set of factors.

For example, suppose someone buys a present for a family member – should they leave the price tag on their gift? Many of us would be inclined to say "no," as doing so would be inappropriate. Perhaps it is because we are concerned that the gift-giver will be embarrassed if they do not spend a lot of money on the present. This embarrassment could apply to revealing tribute donation amounts. When given the option, 53% of participants in this study choose not to reveal how much they give to honorees when making a tribute donation. Our reluctance to reveal tribute gift amounts, in part due to the potential for embarrassment, may more generally be because we are accustomed to following a set of gift-giving social norms. Tribute donations may be seen as a form of in-kind gift, and it is typically an unacceptable practice to leave the price tag on a present.

[2]The study was also run in an online setting following procedures outlined in Danz et al. (2021). In online sessions I did not find any impact of revealing tribute contribution amounts on giving behavior, relative to only recognizing donors. However I was unable to mail tribute donation acknowledgment cards to honorees in the online sessions, unlike in-person sessions. I believe the inability to enforce notifying honorees of tribute gifts led to this result.

[3]If anything, the findings suggest that the advantage to revealing donation amounts is greater for tribute donations than for donations made on behalf of oneself.

If social conventions of this kind are both widespread and widely-accepted among honorees and donors, revealing the size of tribute donations could be seen as repugnant even when engaging in this practice would otherwise be beneficial for fundraisers (Roth, 2007).

The rest of the paper is organized as follows. Section 1.2 summarizes prior work and provides an example decision-making framework to highlight how revealing donation amounts in addition to recognizing donors may affect giving on behalf of oneself. Using insights from the decision-making framework, the section then discusses how tribute donations may differ. Section 3 details the experimental design and reviews the findings for donations made on behalf of oneself. Section 4 describes the experimental design and reviews the findings for tribute donations. It then summarizes and compares the results for the two types of giving. Section 5 discusses the implications of the study results, and Section 6 concludes.

1.2 Background

1.2.1 Prior Work on Donations Made on Behalf of Oneself

While there is no literature on tribute donations, I begin by considering what prior work has shown about donor recognition for contributions made on behalf of oneself. First, individuals can signal desirable traits when donor identities and donation amounts are publicized. Charitable giving can highlight an individual's wealth, prestige, or generosity.[4] Donors who are concerned with their social image have been shown to take advantage of the opportunity to communicate their generosity. Laboratory studies have documented that revealing donor identities and contribution amounts can lead to greater giving.[5] In the field, similar fundraising benefits have been found from recognizing donors and the size of their donations.[6] In return, others have been shown to reward donors for

[4]See for example, Glazer and Konrad (1996a), Harbaugh (1998), Benabou and Tirole (2006), and Vesterlund (2016).

[5]Studies illustrating these effects include Andreoni and Petrie (2004), Rege and Telle (2004), Ariely et al. (2009), Duffy and Kornienko (2010a), Kumru and Vesterlund (2010), Samek and Sheremeta (2014), and Kessler et al. (2021). Information about peers' generosity can also affect one's contributions (see, e.g., Shang and Croson, 2009; Smith et al., 2015; Gee and Schreck, 2018; Kessler et al., 2021).

[6]Example field studies include Karlan and McConnell (2014) and Samek and Sheremeta (2017). Past work has also shown the benefits of publicizing other forms of donations, such as participation in blood drives (Lacetera and Macis, 2010).

their generosity.[7]

Across existing work for giving on behalf of oneself, two pieces of information are typically revealed together when donation information is publicized. That is, most studies reveal both who gives and how much they give.[8] Fundraisers could instead choose to only recognize donors without revealing how much they give, as is done commonly with tribute donations. Below I discuss the potential consequences of this alternative practice for donations made on behalf of oneself.

1.2.2 A Decision-Making Framework

I use a simple example to demonstrate how donation choices may depend on what information is revealed publicly. Suppose there is an agent whose payoff is given by:

$$u(c, d|I) = \ln(c) + \alpha \ln(1 + d) + s(E[\alpha|I]), \tag{1}$$

where c is consumption, d is the donation amount, α is the agent's private type, and $s(E[\alpha|I])$ is the utility increment - status - from the public belief about the agent's type. The agent's budget constraint with normalized prices is $c + d = w$.

The agent's type is discrete $\alpha \in \{\alpha_0, \alpha_1, \ldots, \alpha_N\}$, with $\alpha_0 < \alpha_1 < \ldots < \alpha_N$. I set $\alpha_0 = 0$ so that the lowest type - called selfish - does not have any intrinsic benefit from donating. I assume the status function $s(\cdot)$ is strictly increasing and concave, with normalization $s(0) = 0$.

Acquired status depends on the beliefs of the audience, $E[\alpha_i|I]$. The audience forms beliefs about the agent's type after receiving information, I, about the agent's action.

There are three information regimes: (1) the *benchmark* in which no information is revealed (anonymous donations); (2) *revealing donors* in which the public learns whether a donation has been made or not (there is a minimal donation amount $\underline{d}$); and (3) *revealing donors and amounts* in which the exact donation amount is revealed in addition to who donates. Proofs for the equilibria

[7] Elfenbein et al. (2012) note that eBay sellers, particularly newer or inexperienced ones, are rewarded from donating part of their proceeds to charity. Fehrler and Przepiorka (2016) document that donors are more likely to be selected as a partner in a trust game than non-donors. Recent work has highlighted additional complexity behind what the size of donations may signal, particularly when there is knowledge of the donor's income, past familiarity with the donor, or information about the type of solicitation that is used (Bracha and Vesterlund, 2017; Berman et al., 2015; Winichakul, 2021).

[8] In Andreoni and Petrie (2004) the researchers employ one treatment where amounts are revealed *without* revealing who the contributors are. Duffy and Kornienko (2010a) implicitly reveal dictator transfers amounts through rankings. Their control treatment reveals *all* dictator identities irrespective of whether anything is transferred.

considered in each information regime are included in Appendix Section A.3.

In the *benchmark* anonymous donations case, status does not respond to the donation. The selfish type donates \$0 and all other types choose their first-best donation amount (d_i^*) to maximize $\ln(c) + \alpha \ln(1+d)$, subject to $c + d = w$, which is given by:

$$d_i^* = \frac{\alpha_i \omega - 1}{1 + \alpha_i}, \quad \omega > \frac{1}{\alpha_i} \tag{2}$$

Proposition 1 (anonymous donations): If status does not respond to the donation, the agent donates its first-best.

In the *revealing donors* case, the public learns whether a donation has been made or not. The selfish type will choose between no donation and the minimal amount needed to warrant an announcement. So, if the minimal amount is \$0, this choice is substantively vacuous. At the same, in this paper I do not analyze optimal exclusion through setting optimally high minimal amounts. Thus, I assume that the minimal donation amount is positive but below the first-best amount of the lowest non-selfish type α_1 (d_1^*).

Assumption $*$: $\underline{d} < d_1^*$

In this environment, it is optimal for all non-selfish types to donate their first-best. By deviating to any other donation amount they will decrease their payoff from donating and consumption, and will not affect their status payoff. If they deviate to not donating, both the consumption-donation and the status payoff will strictly decrease. The selfish type chooses between his first-best choice of \$0 donation and the minimal donation amount. Not donating gives the selfish type his first-best consumption payoff but no status. Donating the minimal amount hurts his intrinsic payoff from consumption; it also rewards him with status from the public's belief that he is the average type ($\bar{\alpha}$), updated according to Bayes' Rule. Assumption $**$ describes the condition for when the payoff from the minimal donation is larger than not donating.

Assumption $**$: $s(\bar{\alpha}) \geq \ln(\omega) - \ln(\omega - \underline{d})$

Proposition 2 (revealing donors): If status is based on observing any donation, but not the amount, all non-selfish types donate their first-best. The selfish type donates the minimal amount if $**$ holds, and \$0 otherwise.

I now consider the *revealing donors and amounts* case. I construct a fully separating equilibrium where the public can discern exact agent types from the donation amount. In this equilibrium, the selfish type donates \$0 and all other types donate weakly above their first-best. If the types are not sparse, that is, the difference between the types is sufficiently small, all non-selfish types donate strictly above their first-best ($\hat{d}_i$).

The selfish type chooses between his first-best of donating \$0 and mimicking the donation of the lowest non-selfish type, $\hat{d}_1$. Not donating gives the selfish type his first-best consumption payoff but no status. Alternatively, mimicking α_1 hurts his intrinsic payoff from consumption but rewards the selfish type with α_1 status. However for the fully separating equilibrium, mimicking α_1 is too costly and the selfish type will donate his first-best.

Non-selfish types face similar choices and consider both types directly above and below them. Mimicking the type above them gives non-selfish types the status of the $i+1$ type and increases their intrinsic payoff from donating, but hurts their intrinsic payoff from consumption. Conversely, mimicking the type below them hurts their rewards from status and intrinsic payoff from donating, but increases their intrinsic payoff from consumption. Ultimately by donating more than their first-best, $\hat{d}_i$, non-selfish types make it too costly for types either directly above or below to mimic them.

Proposition 3 (revealing donors and amounts): If status is based on the donation amount, all non-selfish types donate more than their first-best. The selfish type donates his first-best.

To summarize, the decision-making framework characterizes the tradeoffs between donation information regimes. Prior work has documented the fundraising benefits of recognizing donors *and* revealing donation amounts (**Proposition 3**) over anonymous giving (**Proposition 1**). In my study for donations made on behalf of oneself, I compare the fundraising consequences of

recognizing donors *and* revealing donation amounts (**Proposition 3**) to only recognizing donors (**Proposition 2**). When evaluating these scenarios, the decision-making framework generates the following comparative static predictions. First, only revealing whether an agent gives may lead to greater donation rates compared to when gift amounts are also revealed. While there may be an extensive margin benefit to only recognizing donors, there may be an intensive margin benefit to revealing donation amounts above only recognizing donors. In particular, revealing how much people give in addition to who gives may lead to larger average contributions among donors.

1.2.3 Why Might Tribute Donations Be Different?

The example above describes the potential tradeoffs between only recognizing donors and also revealing tribute donation amounts. People may be more inclined to give when only donor identities are recognized, but donations may be larger in size when amounts are also revealed. For donations made on behalf of oneself, it could be that the benefits of larger-sized gifts when revealing donation amounts dominate the extensive margin benefits of greater participation rates when only revealing whether someone donates. Consistent with that expectation, most charities reveal both who donates and how much they give.

What might lead to the contrasting charity approach to donor recognition for tribute donations, where donors are recognized but how much they give is not revealed? For one, it may be that charities are more concerned with the extensive margin benefits that tribute donations could generate. That is, the extensive margin benefit is either large enough to compensate for a lack of benefit on the intensive margin, or this margin could matter more for other reasons. When deciding to give on behalf of others, individuals may be learning about an organization for the first time. Given the opportunity, charities may place additional emphasis on moving individuals from a "cold" to "warm" contributor list (Mixer, 1993; List and Lucking-Reiley, 2002; Landry et al., 2010; Karlan et al., 2011; Vesterlund, 2016). And to ensure that they acquire the donor, they may be willing to incur potential costs in the form of smaller-sized gifts in the short-term if it means that the individual is more likely to be involved with the charity and donate in the future.

Donors also face a unique set of factors when deciding whether, and how much, to give on behalf of others. Because tribute donations may be a form of personal, in-kind gift, they could be

governed by a set of social customs.[9] Norms regarding gift-giving would suggest that leaving the price-tag on a gift would be improper (Tugend, 2005; ASP, 2014).[10] In situations with particularly somber conditions, there also may not be many types of acceptable gifts. Offering gifts in these contexts requires additional sensitivity with how they are presented.

The above social conditions would necessitate that fundraisers consider how tribute donations may be viewed by honorees. Strong norms would also compel fundraisers to consider how donors feel about having the size of their tribute gifts revealed; and in cases where honorees can choose whether to learn individual contribution amounts, how donors perceive this choice. At a local level, revealing tribute donation amounts could be seen as a tacky and inelegant decision that donors and honorees both find inappropriate. At a larger scale, it may be that revealing the size of tribute donations is a strictly forbidden practice in a culture or community. Ultimately across many circumstances, revealing donation amounts may be a violation of unwritten rules. It could represent a repugnant transaction that practitioners cannot implement irrespective of the potential fundraising benefits (Roth, 2007).[11]

1.3 Donations Made on Behalf of Oneself

I begin by examining the effect of revealing donation amounts and donors when giving on behalf of *oneself*. The experimental design is summarized in Section 1.3.1 below. I then review the findings in Section 1.3.2. In Section 1.4, I examine how revealing donation amounts in addition to recognizing donors affects giving on behalf of *others*.

[9] Some literature has debated the efficiency implications of gifting in-kind presents instead of gifting the comparable value in cash (Waldfogel, 1993; Solnick and Hemeway, 1996; List and Shogren, 1998; Prendergast and Stole, 2001; Ellingsen and Johannesson, 2011). In marketing research, Samper et al. (2017) note that individuals who make a donation as a wedding gift contribute less than they would have otherwise spent on a traditional wedding present. Cavanaugh et al. (2015) also suggest that charitable donations are a more attractive choice for individuals who gift-givers are not close with.

[10] Multidisciplinary work on gift-giving further documents how price considerations can make the gift-giving process more difficult (Belk, 2005; Roth, 2007; Flynn and Adams, 2009)

[11] If "repugnance" is too strong of a designation for this setting, Roth (2007) notes that milder concepts such as *inappropriateness* or *unseemliness* may be more apt but still constitute real constraints on markets.

1.3.1 Experimental Design

For sessions where donations are made on behalf of oneself (*Self*), participants are randomly assigned to one of three treatments: *ID*, *ID&Amount*, or *Choose Info*. The treatments vary the donation information that is revealed. In *ID*, donors are identified by whether they donate, but the size of their donations are not revealed. In *ID&Amount*, donors and the size of their donations are revealed, and represent common conditions for gifts made on behalf of oneself. Finally in *Choose Info*, individuals choose whether they would like to reveal how much they donate.

Table 1: Experimental Design – *Self*

Part	**Tasks**
A: Work	Summation Problems
B: Charitable Giving	Private Donation (D1)
	Public Donation (D2)
C: Continuing Support	Future Donation

Notes: Participants are also randomly assigned to either *ID*, *ID&Amount*, or *Choose Info* treatments. *ID*, *ID&Amount*, and *Choose Info* vary the information that is revealed to others in the same experimental session about the Public Donation (D2). For the Public Donation, no donation information is revealed if participants do not donate, or if their Private Donation (D1) is randomly selected to be implemented.

Table 1 summarizes the parts of the experiment for *Self* sessions.[12] Instructions for the three parts are provided sequentially throughout the experiment. Example instructions are included in Appendix Section A.1. At the beginning of each experimental session, participants are seated at individual computer stations. At the top of the computer stations are numbered flags. Participants are told that the number on the flag is their Participant Number, and that the number may be used to reveal some of their decisions to others in the session. They are told that they will be informed prior to any decision whether their Participant Number will be used to identify them and their decision.

Once participants are seated, they begin Part A of the experiment which involves a work task. In the task participants are asked to correctly calculate the sum of a series of six one-digit numbers.

[12] A summary of the treatment differences between *ID*, *ID&Amount* and *Choose Info* is provided in Appendix Section A.2.1.

Once they correctly solve ten problems, the work task ends and participants earn $18 that they can use later in the experiment for their charitable giving decisions.

Participants then move to Part B of the experiment. In this part they are told that they will have the opportunity to donate to a local charity using the $18 they earned from the work task. Participants face two decision tasks in Part B. Each task involves three donation decisions, one to each of three different charities. Participants are told that one of the three charities will be randomly selected to receive donations at the end of their session. Half of participants will have their task-1 donation implemented, while the other half will have their task-2 donation implemented to the charity. Therefore participants know that only one of their six decisions will be randomly chosen to be implemented. Participants are told that they should make each decision considering the full $18 they earned from the work task, as only one decision is implemented. All donation decisions are made in $2 increments. To present a unique giving opportunity, donations are also matched one-for-one by University of Pittsburgh research foundation funds.

Task 1 begins with information about the three charities and their missions.[13] Local charities are selected to generate a greater sense of connection between participants and the organizations. The three charities are also selected to support different causes. Participants are then asked to make their task-1 donation decisions, which they are told will remain private. Task-1 decisions are denoted as "Donation 1" or "D1." Because D1 are kept private, these decisions are intended to capture underlying differences in participant generosity and preferences toward the three charities.

In task 2 participants are again asked to donate to each of the three charities. Unlike task 1, they are told that if they donate and their task-2 decision is implemented, other participants who have their task-2 decision implemented will learn that they donated. That is, participants will learn the Participant Numbers of others in the session who donate in task 2. They are provided an example of the donation information that they could receive about other participants. This information is provided in a table that appears on their computer screens once task 2 is finished. If participants decide not to donate for their task-2 decision or if their task-1 decision is implemented, then their Participant Number is not revealed.[14] Task-2 decisions are denoted as "Donation 2" or "D2."

[13]The charities are Animal Friends, the Greater Pittsburgh Community Food Bank, and the Women's Center and Shelter of Greater Pittsburgh.

[14]This implementation makes the inference on who donates noisy, mirroring how donations are seen in the field.

Beyond Participant Numbers, individuals may learn additional information about others' donations depending on their randomly assigned treatment. In *ID*, participants are told that only their Participant Number will be revealed. Individuals in *ID&Amount* learn both who gives and also how much others give. Donation amounts are revealed next to Participant Numbers on their computer screens once task 2 is finished. In *Choose Info*, participants who donate choose whether they also would like to reveal how much they donate, in addition to revealing whether they give. Therefore, participants in *Choose Info* learn Participant Numbers and how much others donate, but only for individuals who choose to reveal the latter information.

1.3.1.1 Part C

Participants are asked a series of additional questions. First they are asked to state how likely they are to donate to each of the charities in the future. Responses are recorded on a five-point scale, with responses ranging from "Very Unlikely" to "Very Likely." Participants are then asked whether they would like to learn more about the organization. If they answer "Yes" to this question, participants submit their email address to be added to the charity's mailing list.

Participants then learn which of their decisions is implemented. Finally to assess the potential impact on future giving, after all participants receive information about the implemented decision they have an opportunity to make one additional donation. In a novel experimental design, for this decision participants are told that they can choose to schedule a future donation to the selected charity in one-month's time. They are given an additional $18 to make this decision, and told there is a 10% chance the decision will be implemented. If their future donation is implemented, the researchers make a subsequent donation to the selected charity. Individuals schedule a time to return to the laboratory in the future to receive payment for any amount they do not donate out of the additional $18 provided.

After the future donation decision, participants complete a set of supplementary questions that are drawn from the psychology literature, namely from the Interpersonal Relativity Index (Davis, 1983). Participants are asked two questions each from the, "Principle of Care," " Empathic Concern," and "Perspective Taking" domains. Prior work has used similar questions to explore the associations between psychological concepts and constructs more commonly used in economics when describing motives behind charitable giving. This research has shown that greater Principle

of Care scores are correlated with warm-glow motives, while greater Empathic Concern scores are correlated with individual estimates of altruism (Ottoni-Wilhelm and Vesterlund, 2020).

Following the supplementary questions and a demographic survey, participants receive a summary of experimental results.[15] The results include a reminder about the implemented donation, and whether their future donation is randomly chosen to be implemented. Participants also receive summary information about their earnings from the experiment. Final earnings consist of a $6 show-up fee, the money that participants do not donate out of the $18 they received from Part A, and the money they do not donate out of the additional $18 they are provided for their future donation, if it is implemented.

1.3.2 Results

The experiment was programmed in oTree and run at the Pittsburgh Experimental Economics Laboratory (PEEL).[16] One hundred twenty participants participated in *Self* sessions across six sessions.[17] Across all sessions participants earned an average of $16.88, and each session lasted approximately one hour. The experiment raised a total of $3,652 for the three charities.

For this analysis I focus on results from the two donation environments of interest, when only donors are recognized (*ID*) and where donors *and* donation amounts (*ID&Amount*) are revealed. To begin, I compare the likelihood of making a donation across treatments. Figure 1 reports the share of donations (D2) in *ID* and *ID&Amount*. When only donors are recognized, participants donate 76% of the time. In contrast, participants donate at a significantly higher rate when amounts are revealed, approximately 90% of the time ($p<0.01$). This contrasts the comparative

[15] The demographic survey includes questions about age, year in school, gender, existing prosocial tendencies, political ideology, and religiosity. Participant prosocial tendencies are defined across two variables, labeled as "Volunteer" and "ExistingCharity." Each of these variables is defined across five categorical intervals. For the Volunteer variable, participants are asked, "On average, how often do you volunteer for a good cause?" Participants choose from one of the following categories: "Never," "Once a year," "Once a month," "Every week," "Several times a week." For the ExistingCharity variable, participants are asked, "On average, how much do you donate to charitable organizations per year?" Participants choose from one of the following categories: "$0-$20," "$20-$50," "$50-$100," "$100-$500," and "Over $500." For the political ideology variable, participants choose from one of five categories, ranging from "Very Liberal" to "Very Conservative." Finally for the religiosity variable, participants respond to the question, "On average, how often do you attend religious service?" They choose from one of the following five categories: "Never," "Once a year," "Once a month," "Every week," and "Several times a week."

[16] For a review of the oTree platform and its features, see Chen et al. (2016).

[17] A total of two hundred forty participants completed the study across twelve sessions. A full summary of participants and sessions is provided in Appendix Table A.2.1. Participant demographics for all sessions are provided in Appendix Table A.4.1.

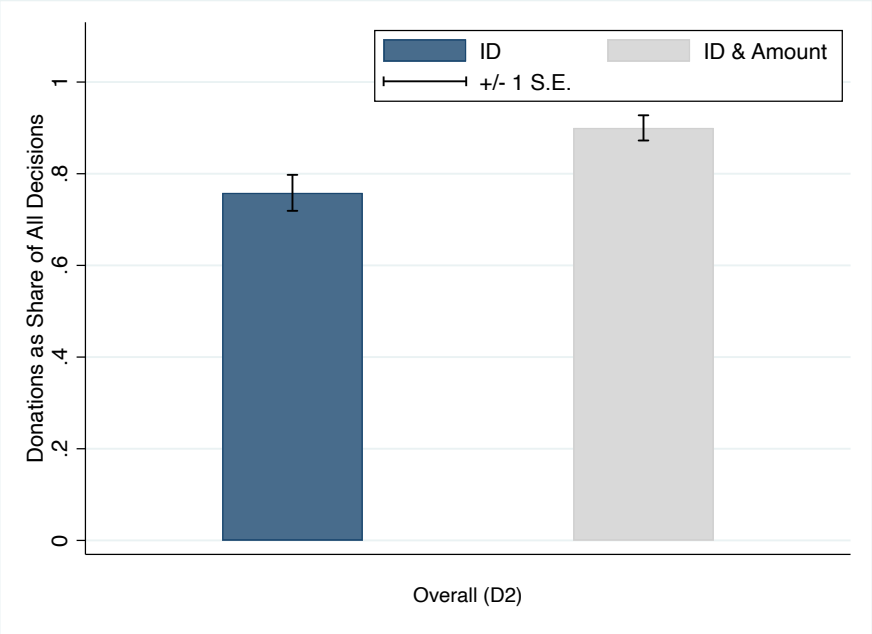

Figure 1: Donation Rate – (*Self*)

static predictions discussed in Section 1.2.2.[18] I return to discuss this result at the end of Section 1.3.2.

I complement the results shown in Figure 1 with estimates from a series of regressions in Table 2. I regress a binary measure for whether participants donate (D2) on a treatment indicator for *ID&Amount*. To account for potential individual differences, I control for private giving rates (D1), individual demographics, and charity fixed effects.

Table 2 reaffirms the difference observed in Figure 1, where revealing the size of donations to others induces greater rates of giving. Table 2 suggests that individuals are approximately 8 percentage points more likely to give when they know that others will not only learn that they donate but also learn how much they give (p=0.02). Not surprisingly, the results show that participants who donate in private (coefficient on $\mathbb{1}$(Donation 1)) also are significantly more likely to donate when their donations are revealed to others (p<0.01).

[18]Note that by not donating, it is not that case that individuals are indirectly revealing who they are. The list of donors is a noisy signal of who gives in task 2. It could be that the participants not on the list are among those randomly selected to have their task-1 decision implemented.

Next, I move from the extensive margin effects to explore how revealing donation amounts affects the size of contributions. In Figure 2, I plot the distribution of donation amounts (D2) ranging from no donation to the maximum possible out-of-pocket contribution ($18). I plot the distributions separately for *ID* and *ID&Amount* to compare the relative frequency of each donation amount. The share of *ID* donations at each amount are represented by the left dark bars in each cluster, and *ID&Amount* donations are represented by the right gray bars.

Figure 2 shows first that there is a relatively larger frequency of individuals who do not donate ($0) in *ID*, the same extensive margin effect previously discussed. Second, when only donors are recognized there is a relatively larger frequency of $2 donations in *ID*, i.e., the smallest possible donation amount that triggers an announcement. By donating the minimal amount, participants reveal to others that they donated while limiting the out-of pocket cost of making the gift. Meanwhile donations of $10-$18 are relatively more common in *ID&Amount* than in *ID*. Non-parametric tests reveal a statistically significant difference between the two distributions (Mann-Whitney U-test, $p<0.01$).

To complement the analysis of aggregate changes to giving, I explore whether individual donation decisions differ depending on whether amounts are or are not revealed. Table 3 reports results from this analysis where I regress donation amounts (D2) on a treatment indicator for *ID&Amount*.[19] Across model specifications I control for private giving decisions (D1). I also control for demographics and charity fixed effects in columns 2-3.

Table 3 shows similar results to those visualized in Figure 2. With the full sample of participants (columns 1-3) the results illustrate that contributions are larger in *ID&Amount* after controlling for private giving decisions and demographics. Average giving is between $0.74 and $1.03 larger when amounts are revealed compared to when only donors are disclosed, increases of approximately 14.4-20.0% relative to the *ID* donation mean. However in the tobit specification that accounts for censored observations, the estimated effect is no longer significant in a two-sided test (p=0.18). Finally, private giving behavior (D1) is a strong predictor of how much individuals give in a public setting.

In columns 4-6, I look at the average size of donations after restricting the sample to participants who make positive donations. The comparative statics outlined in Section 1.2.2 predict larger

[19]Columns 1-3 report results from the full sample, while columns 4-6 restrict the sample to those who give a positive amount.

average giving among donors in response to revealing both the size of donations and donors. The results are consistent with this prediction – average donations are \$0.20-\$0.57 (3.9-11.1%) larger when donation amounts are revealed in addition to recognizing donors. However, the magnitude of the difference in donation amounts between treatments is not statistically significant across both OLS and tobit specifications. The lack of difference in the average size of contributions among donors suggests that the effect on overall giving is primarily driven by the difference in participation levels, i.e., the likelihood of donating, and not from an increase in average giving.

Altogether, the results illustrate the fundraising benefits of not only recognizing who gives but also revealing how much they give. Some interesting patterns emerge, however, when evaluating the underlying mechanisms that drive the increase in overall giving. From Section 1.2.2, the extensive margin results suggest that Assumption $**$ does not hold; that is, for some individuals the status acquired from donating when amounts are not revealed is less than the cost of making a donation. As a result, the likelihood of giving is not higher when only donor identities are revealed; in fact, the likelihood of giving is significantly *lower*. This further suggests that there could be additional benefits to giving for donors when detailed information about their contribution amounts is revealed compared to when only donor identities are recognized, that are not captured in the simple example from Section 1.2.2. There is also suggestive evidence that the average size of contributions among donors is larger when donation amounts are revealed, but the difference is not significantly different. The direction of the result is consistent with the prediction from Section 1.2.2. The results add further support to current fundraising practices where donations amounts are revealed for gifts made on behalf of oneself. Next, I examine whether revealing contribution amounts affects tribute giving behavior differently than its impact on donations made on behalf of oneself.

1.4 Donations Made on Behalf of Others

1.4.1 Experimental Design

For *Other* sessions, participants are also randomly assigned to one of three treatments that vary the information that is revealed about donations: *ID*, *ID&Amount*, or *Choose Info*. These treatments are intended to mirror *ID*, *ID&Amount*, and *Choose Info* in *Self* sessions. Table 4

summarizes the design for *Other* sessions.

Unlike *Self* sessions, at the end of task 1 of Part B participants are asked to think of a family member or friend who they believe would be most likely to support each of the three charities. They are told they will have an opportunity to give on behalf of those individuals in task 2. Once participants have decided on these individuals, participants submit the name of the family member or friend for each charity.[20]

In task 2, participants are again asked to donate to each of the three charities as they did in task 1. For their task 2 decisions participants have the opportunity to make tribute donations on behalf of the family members and friends they name. If they choose to make a tribute donation and the donation is implemented, participants are told that the researchers will mail an acknowledgment card to the honoree. Participants are provided an example of the card. They are told that they will receive an envelope (with postage) at the end of the experiment to address to their honorees and place the card in. The researchers will then take the cards to the post office to mail. If participants decide not to make a tribute donation they can still donate on behalf of themselves or not donate at all; in which case, a card will not be mailed on their behalf.

In all treatments individuals learn that the cards will note that participants made donations on the honoree's behalf and include charity information. Additional details that are included in the mailed cards depend on whether participants are in *ID*, *ID&Amount*, or *Choose Info*. In *ID* individuals are told that the honoree learns that a donation was made but does not learn the size of the donation.[21] In contrast in *ID&Amount*, participants learn the donation amount *will* be included in the card. Specifically, the card will list the amount that participants contribute out of their $18 and highlight the total amount the charity receives after the one-for-one research foundation match.[22] Finally in *Choose Info*, participants can choose whether to only inform the honoree that a donation was made on their behalf, or also reveal the amount donated. That is, participants choose whether or not they want to mail the *ID* or *ID&Amount* card, if they make a tribute donation.

[20] Names are limited to first names to preserve participant anonymity.

[21] An example of the *ID* card is included in Appendix Section A.2 using The Greater Pittsburgh Community Food Bank as the example charity.

[22] An example of this card is included in Appendix Section A.2 next to the *ID* card. The example *ID&Amount* card includes a placeholder for where the out-of-pocket donation amount is listed and for where the total donation is listed.

The remaining decisions for *Other* sessions mirror the decisions described in Section 1.3.1.1 when summarizing the design for *Self* sessions.

1.4.2 Results

One hundred twenty participants took part in six *Other* sessions. I focus my subsequent analysis on the two primary treatments of interest, *ID* and *ID&Amount*.

To begin, in Figure 3 I compare donation rates across treatments. In addition to the overall donation rate (D2), I also examine differences in the likelihood of making a tribute donation. Donation rates are shown for each treatment in Figure 3. The left cluster of Figure 3 describes the donation rate for all gifts, while the right cluster shows the donation rate for tribute gifts. The left dark bars represent the respective donation rate for *ID*, while the right gray bars represents the same outcome but for *ID&Amount*.

Figure 3 shows no difference in the overall likelihood of giving. Participants are no less likely to make a donation of any type when tribute donation amounts are revealed than participants who do not have tribute donation amount information revealed to honorees (84% vs. 86%, p=0.72). While overall donation rates are not affected by revealing tribute contribution amounts, the likelihood of donating on behalf of someone else changes. In particular, tribute donations are far more likely to occur when the cards not only reveal that a donation is made but also how much is given. Those who are told that tribute gift amounts will be disclosed to their honorees are 17 percentage points more likely to make a tribute donation (p<0.01). Therefore while revealing tribute donation amounts does not appear to attract new donors, it does affect what kind of donation individuals make. I will return to discuss the implication of this result at the end of this section.

In Table 5 I evaluate whether revealing tribute contribution amounts affects donation rates (D2) using a series of regression models. Table 5 considers the same two extensive margin outcomes illustrated in Figure 3: the likelihood of making any donation when tribute donations are feasible (columns 1-3), and the likelihood of making a tribute donation (columns 4-6).[23]

The results in Table 5 reflect the visual comparisons in Figure 3. First, revealing tribute donation amounts does not affect the overall likelihood that individuals give to charity. While revealing tribute donation amounts has no effect on overall donation rates, it does affect the willingness of

[23]In these regressions I control for private giving decisions (D1) as well as demographic and charity controls.

participants to make tribute donations. Columns 4-6 of Table 5 show that revealing how much people give on behalf of others increases the likelihood that individuals make a tribute donation, on the order of over 15 percentage points (p=0.01). Therefore, while revealing the size of tribute contributions does not affect overall participation, it does lead to greater participation specifically in tribute giving.

Beyond looking at the likelihood of making a donation, I also examine how revealing the size of tribute donations in addition to recognizing donors to honorees affects overall giving. That is, the subsequent results explore whether participants change how much they give, knowing that tribute donation amounts are revealed to their loved ones.

In Figure 4, I plot the distribution of donation amounts ranging from no donation to the maximum possible out-of-pocket contribution ($18). I plot the distributions separately for *ID* and *ID&Amount* in order to make comparisons regarding the frequency of different-sized donations between the two treatments. The share of *ID* donations are represented by the left dark bars, and *ID&Amount* donations are represented by the right gray bars.

Figure 4, to begin, shows the same extensive margin results with respect to overall donation rates. The frequency of $0 donations is comparable across treatments. Next, I note that there is a relatively larger frequency of $2 donations, i.e., the smallest possible donation amount when only the identity of who makes a tribute donation is revealed to honorees. This implies that participants are minimizing the out-of-pocket cost of making a donation and taking advantage of the opportunity to show honorees that they donated. Further, there is a substantially larger share of maximum out-of-pocket contributions when amounts are revealed to honorees. This suggests that participants may be taking the opportunity to show their honoree that they are being as generous as possible given their earnings from the work task. Non-parametric tests reveal a statistically significant difference between the two distributions (Mann-Whitney U-test, $p<0.01$).

Next, I analyze whether the size of individual donations differ depending on whether tribute contribution amounts are or are not revealed. Table 6 reports results from this analysis where I regress donation amounts (D2) on a treatment indicator for *ID&Amount*. Columns 1-3 report results from the full sample, while columns 4-6 show the average donation size by restricting the sample to those who give a positive amount.[24]

[24]Model specifications include controls for private giving decisions (D1), demographics and charity fixed effects.

The results in Table 6 highlight how revealing tribute donation amounts increases individual giving. With the full sample, model estimates show that average giving is roughly \$1.11 to \$1.64 larger when tribute donation amounts are revealed. This suggests that charities can raise giving by 15.6%-23.1% if they reveal tribute donation amounts in addition to recognizing donors (p=0.06).[25] When further exploring *who* is giving more, I find that the effect appears to come from tribute donors who have how much they give revealed to their honoree. The results reported in Appendix Table A.4.2 show that tribute donations where the amount is revealed are more than \$2 (25%) larger than tribute donations where only the donor identity is recognized; however, this effect is not statistically significant in a two-sided test (p=0.11). This mechanism is consistent with what we would expect, as this is the population directly affected by the tribute donor recognition practices.

In columns 4-6 I examine the average size of contributions by conditioning the sample to donors, i.e., only those who make positive donations (D2). Revealing tribute donation amounts in addition to recognizing donors may involve the similar features to those outlined in Section 1.2.2 for donations made on behalf of oneself. That is, there may be marginal returns to status by giving more when the size of tribute gifts is revealed. In this case the prediction that the average size of contributions among donors will increase may also hold for tribute giving. Columns 4-6 of Table 6 show that average donations are significantly larger when tribute contribution amounts are revealed, consistent with the comparative statics discussed in Section 1.2.2 for donations made on behalf of oneself. Donations are between \$1.29 and \$1.64 larger when amounts are revealed, equivalent to a 15.6-19.8% increase compared to when only donors are recognized (p=0.06). In Appendix Table A.4.2 I again look at *who* is giving more. The results show that tribute donations where the amount is revealed are about \$2.25 (25%) larger than tribute donations where only the donor identity is disclosed; as with the results using the full sample, these estimates are not significant in a two-sided test (p=0.18).

Altogether, the findings suggest that revealing tribute donation amounts can benefit charity fundraising efforts, as overall giving is significantly larger compared to when only donor identities are disclosed. Yet in contrast to the arguments discussed in Section 1.2, not revealing the size of gifts does not appear to generate the predicted benefits on the extensive margin, i.e., draw in new support and move donors from a cold-list to a warm-list. That is, the overall donation rate

[25]The column 1 estimate without controls is not statistically significant (p=0.10).

is not higher when the size of tribute donations are revealed compared to when only donors are recognized. Instead, a greater share of individuals choose to make a tribute donation when they know how much they give will be disclosed to their honorees. Tribute donations are also larger in size when amounts are revealed, similar to the comparative statics outlined in Section 1.2.2 for donations made on behalf of oneself. Taken together, the fundraising returns to revealing tribute donation amounts in this study result from a greater number of larger-sized donations.

1.4.3 Revealing Donation Amounts and Future Support

Beyond immediate giving, practitioners emphasize that tribute donations offer a unique opportunity to generate subsequent support from individuals who were previously less familiar with their organization. Though I do not observe the predicted extensive margin response in short-term giving, I nonetheless explore whether only recognizing donors and not how much they give impacts future giving. It is important to observe whether the implemented methods are more or less effective in generating continuing support from individuals, in order to understand the full fundraising impact of tribute donation recognition practices. While future giving is cited by practitioners as a potential benefit for giving on behalf of others, I also explore whether subsequent donations are affected by recognition practices for gifts made on behalf of oneself.

Table 7 reports the results of this analysis, with results for *Self* sessions in columns 1-3 and *Other* sessions in columns 4-6. The model specifications in Table 7 mirror those used in Table 6. However the dependent variable is future donation amounts instead of donations implemented during the session.[26]

The results in Table 7 show that future donation amounts are not significantly larger when short-term contribution amounts are revealed, though the direction of the estimates are consistent with an increase in future financial support. This is true both for individuals who make donations on behalf of themselves and others during the experimental sessions.[27] A test of differences in

[26]Because participants make only one future donation decision and only one charity is selected per session, I do not have sufficient treatment variation within charity. Therefore unlike regressions on the likelihood of giving and size of short-term donations, I do not include charity fixed effects in these specifications. I continue to control for private donations (D1) in order to account for individual differences in preferences toward the selected charities.

[27]This is not surprising given the smaller sample and less precise estimation. The results are still insignificant if I further restrict the sample to those who have their task-2 decision implemented. In Appendix Table A.4.3, I also review potential differences in the future giving behavior of individuals who choose to make a tribute donation and those who decline, across treatments.

the effect magnitudes across *Self* and *Other* sessions reveals no significant difference (p=0.50).[28] However, private giving (D1) during the experiment predicts the size of future donations. Every additional private dollar given predicts an increase in the size of future donations of approximately \$0.69 (9.3%) in sessions where individuals donate on behalf of themselves, and an increase of \$0.59 (7.9%) in sessions where individuals could donate on behalf of others (both p<0.01).

In total, the results suggest that revealing how much individuals give in addition to recognizing donors has important fundraising benefits. Compared to only recognizing donors, overall giving when donors and donation amounts are disclosed is greater irrespective of whether donations are made on behalf of oneself or others. When looking at where the benefits come from, however, interesting differences emerge. The fundraising benefits for donations made on behalf of oneself largely come from the extensive margin. Individuals are more likely to donate when the size of their contributions is revealed to others. The increase in the total number of donations leads to greater overall giving, even though the average size of donations among donors is not significantly larger.

In contrast, revealing how much individuals give on behalf of others does not increase overall donation rates, but doing so improves the chances that individuals make tribute donations. Increasing the rate of tribute giving increases overall giving as these donations tend to be larger in size, especially when donation amounts are revealed to honorees. Finally, revealing the size of contributions for both types of giving does not appear to impact future financial support.[29]

The different mechanisms through which revealing donation amounts benefits fundraising efforts could be due to the contrasting audiences that receive information about gifts made on behalf of oneself and others. For contributions made on behalf of oneself, donations were revealed to peers in the same experimental session. This feature was intended to mimic how this type of donation is often publicized in the field to the broader community. Because donors may not be well-known to one another in a general populace, donors who would not have otherwise given can use the

[28] In pooled sample specifications reported in Appendix Table A.4.4, I show that the likelihood of making any future donation does not differ between *ID* and *ID&Amount* for *Other* sessions. I also explore whether revealing tribute donation amounts affects participants' likelihood of adding their email address to charity mailing lists in order to receive updates from the organizations. In Table A.4.5 of Appendix Section A.4 I find no impact of revealing donation amounts on this outcome. I analyze the same future giving outcomes for *Self* sessions in Appendix Tables A.4.6-A.4.7, and similarly find no impact.

[29] For a direct test of differences in the effects of revealing amounts between giving on behalf of self and others found in this study, see Appendix Tables A.4.13-A.4.15.

donation (and particularly how much they give) as an opportunity to introduce themselves and their generosity to their community. But for those who would have given in private, they may not feel the need to donate larger amounts if their previously planned donation is sufficient for their introduction to others.

For donations made on behalf of others, the primary recipient of the gift information is the honoree. Honorees have established relationships with donors. Donors who would not have otherwise given do not need to introduce themselves, nor their generosity, to honorees. Among those who would have given in private, however, the opportunity to reveal the magnitude of their generosity in the form of donation amounts could boost their current status with a loved one.

1.5 Discussion

The study results point to a potential opportunity for organizations to improve their fundraising efficacy by revealing tribute gift amounts, contradicting the current practices of most charities today. Yet, the fact that many charities do not reveal tribute contribution amounts in their current practices warrants additional review and discussion of factors that could shape this choice. In Section 1.2.3, I summarized other reasons why tribute giving may differ from giving on behalf of oneself, and thus lead fundraisers to not reveal the size of tribute contributions. I return to these arguments and discuss whether gifts made on behalf of others may be subject to different constraints than those placed on gifts made on behalf of oneself. This discussion has greatly benefited from conversations with practitioners and other philanthropy experts.

1.5.1 Is Revealing Tribute Donation Amounts Repugnant?

Practitioners may avoid disclosing tribute donation amounts not because they believe it will yield the greatest fundraising benefit, but rather because doing so would be repugnant (Roth, 2007). Since tribute donations can be seen as a form of in-kind gift, it could be that gift-giving norms dictate acceptable practices regarding donations made on behalf of others. For example, it is widely-accepted that the price tag should be removed from any in-kind gift before it is presented to the recipient (Tugend, 2005; ASP, 2014). Revealing how much is given in tribute could be see as

analogous to leaving the price tag on the donation, which could objectify an otherwise personal gift (Roth, 2007). Repugnance could then arise from both the gift-givers' (donors) and gift-recipients' (honorees) perspectives. Donors may not feel comfortable revealing how much they give. This may be especially true of donors who give small amounts. In complement, honorees may not feel comfortable with having tribute donation amounts revealed, either because they do not want to embarrass donors or because of the potential gap between the degree of support they receive from donors and the gravity of the cause.

To begin, some donors may be embarrassed when their tribute donation amounts are revealed to family or friends, especially if they only planned to give a small amount. If donors are embarrassed, we might expect to see this expressed in participant preferences toward revealing or not revealing contribution amounts in the *Choose Info* treatment. The results suggest that a significant portion of participants do not feel comfortable revealing how much they give; of the 74 tribute donations in *Choose Info*, participants choose not to reveal the amount for 53% of these gifts. I cannot reject the null hypothesis that an equal proportion of participants prefer revealing to not revealing tribute donation amounts (z=0.47, p=0.64).

At the same time, individuals who we might expect to be more embarrassed to reveal how much they give are more likely to choose to keep donation amount information concealed. In Appendix Table A.4.9, I show that the choice to not reveal tribute donation amounts is associated with smaller-sized gifts.[30] Therefore, while the aggregate preferences of individuals appear to be mixed, the decision to not reveal donation amounts likely satisfies donors who may be most embarrassed by the size of their gifts. But in complement, by not revealing amounts practitioners are not appealing to the portion of their supporter base that typically gives more and prefers to reveal

[30]In Appendix Section A.4, I document additional associations between the choice to reveal contribution amounts and donation characteristics, demographics, and participant motives (as measured by responses to Interpersonal Relativity Index questions (Davis, 1983)). These correlations are summarized in Appendix Table A.4.9 and are only intended to be descriptive and exploratory. I also provide further analysis of the giving behavior of individuals who can choose whether they would like to reveal how much they give to honorees. Overall donation rates (D2) in *Choose Info* are comparable to the other treatments. However, tribute donation rates in *Choose Info* are significantly higher compared to *ID* and comparable to rates found in *ID&Amount*. See Appendix Table A.4.10 and Appendix Figure A.4.1. Further, the average size of donations is larger in *Choose Info* but not significantly different than *ID*. See Appendix Table A.4.11. While tribute donors in *Choose Info* give larger amounts than their counterparts in *ID*, those who decline to make a tribute donation give less than their counterparts who decline in *ID*. Combining these two outcomes, the results suggest that while *Choose Info* may lead to greater tribute giving rates, these benefits are not coupled with larger contribution amounts. Finally, future donation amounts are not significantly larger when individuals can choose to reveal donation amounts, relative to when only donors are revealed in the study. See Appendix Table A.4.12.

tribute donation amounts.

Beyond donors, it could be that honorees or stewards of a memorial fund prefer that contribution amounts are not revealed to them. For instance, honorees may not want to embarrass low contributors nor be seen as rewarding high contributors. This suggests that part of the repugnance underlying the transaction may be a consequence of the fundraising benefit that revealing donation amounts could offer. In other words, the offensive characteristics of revealing the size of tribute gifts may be strongest among individuals who prefer to reveal how much they give *and* who typically give larger amounts. The donor preference to reveal donation amounts may be associated with their desire to self-promote their generosity, even when the gifts are supposedly made to honor others.

Honorees may also be dissatisfied with the potential gap between the degree of donor support and the gravity of the charitable cause. Nonprofit missions are often large in scale, both in terms of geography and importance. If honorees consider the magnitude of the charitable mission, the size of tribute donations may feel underwhelming in comparison. For example, learning someone contributes $5 to support those struggling with food insecurity, or contributing a similar amount to support a refugee family who has been resettled due to war and conflict, may leave honorees disheartened. Honorees may want to instead focus on learning *who* is willing to provide support, regardless of how much, toward a cause that is important to them. Each of these sources of potential repugnance can create constraints on acceptable donor recognition practices, and in consequence the availability of effective fundraising tools for this type of giving.

1.5.2 When Could Revealing Tribute Donation Amounts be Less Repugnant?

While most organizations do not reveal tribute donation amounts to honorees, there are certain contexts where repugnance may not be a market constraint and it may be acceptable to reveal information about the size of gifts. For instance, it may admissible to reveal the size of tribute donations for particular types of tribute gifts or for giving in response to specific events. Certain in-kind tribute donations made during the holidays may fall into this category. Heifer International, for example, allows donors to give animals such as a goat to support farmers in developing countries (Heifer, 2021). The value of donating a goat is easy to find online; at the same time, donating a goat on behalf of someone else is likely to be saved for more cheerful occasions such as the holidays.

Generally, revealing donation amounts could be more agreeable in circumstances that are more

celebratory in nature. One setting that may be informative for our understanding of when the size of tribute donations could be disclosed are wedding registries. Couples add items to these lists that they would like invited guests to purchase as presents. Items include material presents, but can also include "experiences" that the couple is planning for their honeymoon. It is clear from the registry how much each wedding present costs, to both the honorees requesting the gifts and the wedding invitees purchasing the gifts. Tribute donations made in response to congratulatory events could be seen in a similar regard as these wedding presents where contribution amounts are easily discernible.

Additional evidence from Facebook supports the argument that revealing tribute donation amounts may be more acceptable in response to joyous occasions. Facebook has a feature where its users can set up birthday fundraisers on their page to raise money for a charity. The company states that donation amounts collected from birthday fundraisers are revealed to the fundraiser creator (Facebook, 2021).[31] The widespread use of this feature suggests that this practice is tolerable to both the fundraiser creator and Facebook users who donate through this feature.

1.5.3 Other Considerations

When honorees receive information about donors who have given on their behalf, they may have expectations about how much family and friends will give to their cause. Honorees could be left dissatisfied in cases where loved ones do not give as much as they had anticipated, and they do not raise as much money for the cause as they had planned. Practitioners may prefer to avoid the possibility of letting honorees down if they are not as successful at fundraising as they would like. Avoiding this information could also allow honorees to approach each individual supporter with equal appreciation. Introducing information about the size of tribute contributions would lift the veil over the degree of support that each family member or friend provides, and make it more difficult to see each supporter with equal regard. This type of behavior would be similar to bequest gifts where estates are often equally divided among children (Bernheim and Severinov, 2003).

The above factors are important to consider when determining why many organizations currently do not reveal the amounts donated on behalf of others. They may also be important to review when thinking about *when* it may be acceptable to reveal the size of tribute gifts. Ulti-

[31] Donors can also specify if they want to reveal the amount they give to other users.

mately, the results from this study offer initial evidence to open the discussion of whether there may be potential fundraising benefits from implementing this practice. Beginning this dialogue can also move the conversation beyond any "initial yuck factor" that may exist when considering the practice of revealing tribute donation amounts (Roth, 2007). Additional exploration of the social dynamics present with tribute giving can illustrate when, where, and why revealing tribute donation amounts could be worthwhile.

1.6 Conclusion

Recognizing donors and how much they give is a standard practice for gifts made on behalf of oneself. This practice is further supported by research that demonstrates the fundraising benefits from doing so for this type of giving. Yet, organizations follow a contrasting practice for tribute giving where donations are made on behalf of others. For tribute giving, those who are honored receive acknowledgments of who donated on their behalf but not of how much they donated.

In this paper I explore how revealing contribution amounts in addition to recognizing donors affects charitable giving decisions. I examine the impact of disclosing amounts both for donations made on behalf of oneself and on behalf of others. Neither tribute giving nor the strategy of only revealing donor names has previously been explored in the literature. To study this question, I use a laboratory experiment where individuals are randomly assigned to between-subject treatments that first vary whether participants make donations on behalf of themselves or on behalf of others. Second, the treatments vary the information that is revealed to others. Before making a donation, participants in an *ID* treatment are told that only their identities will be revealed to others, and not how much they give. In contrast, individuals in an *ID&Amount* treatment know that how much they give will be revealed to others, in addition to who they are. The *ID* treatment reflects the common practice used for tribute donations, while *ID&Amount* is intended to capture the conditions most commonly observed for donations made on behalf of oneself.

The results highlight the fundraising benefits to revealing amounts for donations made on behalf of oneself. Overall giving increases, as individuals are more likely to give knowing that others will learn how much they give, compared to when others only learn who gives. These results complement

past work that documents the benefits of revealing both donors and their contribution amounts, relative to anonymous giving. In the unexplored domain of tribute giving, the findings also suggest that there are benefits to revealing contribution amounts for gifts made on behalf of others, contrary to the predominant practice followed by fundraisers today. Total giving increases, but not because individuals are more likely to donate. Instead, revealing tribute donation amounts in addition to recognizing donors shifts individuals toward making tribute donations. The greater share of tribute donations benefits fundraisers, as these contributions are larger in size when honorees learn how much is given on their behalf.

Altogether, the results add to evidence supporting current practices implemented for gifts made on behalf of oneself. For giving on behalf of others, the study opens an important conversation about the best practices regarding donor recognition of tribute gifts. The findings suggest that there could be an opportunity for fundraisers to increase revenue if tribute gift amounts are revealed, which would contrast how the majority of organizations treat these donations today. Yet, revealing how much individuals give on behalf of others could come with different constraints than those applied to gifts made on behalf of oneself. Revealing tribute donation amounts could be viewed as a repugnant transaction, if it violates well-established and widely-accepted gift-giving social norms. It is important moving forward to assess the accuracy of these perceptions of repugnance and the accompanying constraints, in order to clarify our recommendations for when to reveal donation amounts and when to avoid disclosing this information.

More broadly, this paper provides a foundation to build upon and expand our insights into tribute giving, a common type of donation in practice but one that has not been studied previously. The experimental laboratory offers a controlled environment to isolate the impact of different types of donor recognition on tribute giving behavior. Subsequent research can further our understanding of tribute donations and the interesting features that are present with this form of contribution, beyond questions about donor recognition. These future insights can deepen our knowledge of tribute giving characteristics, to the benefit of practitioners and academics alike.

Table 2: Effect of Revealing Amount on Donation Rate (*Self*)

	(1) OLS	(2) OLS	(3) Probit
Dep. Var.:		$\mathbb{1}$(*Donation 2*)	
$\mathbb{1}$(Donation 1)	0.711***	0.713***	0.329***
	(0.073)	(0.074)	(0.030)
ID&Amount	0.076**	0.075*	0.080**
	(0.036)	(0.040)	(0.033)
Observations	240	240	240
R-squared	0.494	0.514	
Controls	No	Yes	Yes

Notes: Columns 1-3 report estimates using an indicator variable for whether participants make any donation (D2) as the dependent variable. Columns 1-2 report results from ordinary least squares specifications, while column 3 reports the marginal effects from a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. All specifications control for whether participants donate in private (D1). MacKinnon-White HC3 robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

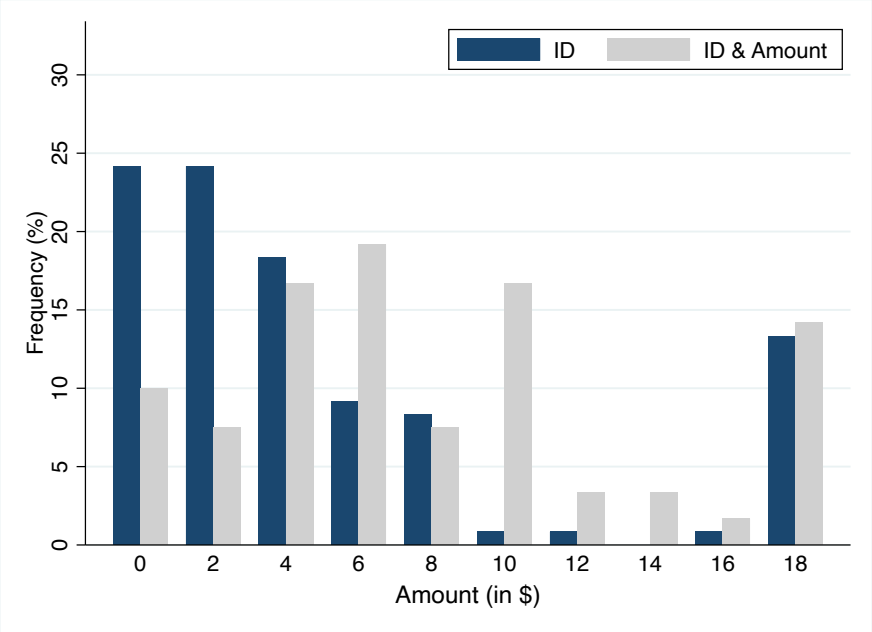

Figure 2: Distributions of Donation Amounts (*Self*)

Table 3: Effect of Revealing Amount on Donation Size (*Self*)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Tobit	OLS	OLS	Tobit
Dep. Var.:	*Donation 2 Amount*					
	Unconditional			*Conditional*		
Donation 1	0.859***	0.877***	1.090***	0.783***	0.786***	1.026***
	(0.037)	(0.039)	(0.056)	(0.045)	(0.049)	(0.065)
ID&Amount	0.963*	1.025**	0.741	0.539	0.568	0.198
	(0.526)	(0.489)	(0.545)	(0.585)	(0.554)	(0.637)
Observations	240	240	240	199	199	199
R-squared	0.536	0.626		0.465	0.589	
Controls	No	Yes	Yes	No	Yes	Yes

Notes: Columns 1-2 and 4-5 report ordinary least squares (OLS) estimates with the donation amount (D2) as the dependent variable. Columns 3 and 6 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Columns 1-3 include all observations, while columns 4-6 condition on observations where participants donate a positive amount. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

Table 4: Experimental Design – *Other*

Part	**Tasks**
A: Work	Summation Problems
B: Charitable Giving	Private Donation (D1)
	Name Honorees
	Choose Donation Type
	Public Donation (D2)
C: Continuing Support	Future Donation

Notes: Participants are also randomly assigned to either *ID*, *ID&Amount*, or *Choose Info* treatments. *ID*, *ID&Amount*, and *Choose Info* vary the information that is revealed to honorees who participants may donate on behalf of for their Public Donation (D2). For the Public Donation, no donation information is revealed if participants do not donate on behalf of others, or if their Private Donation (D1) is randomly selected to be implemented.

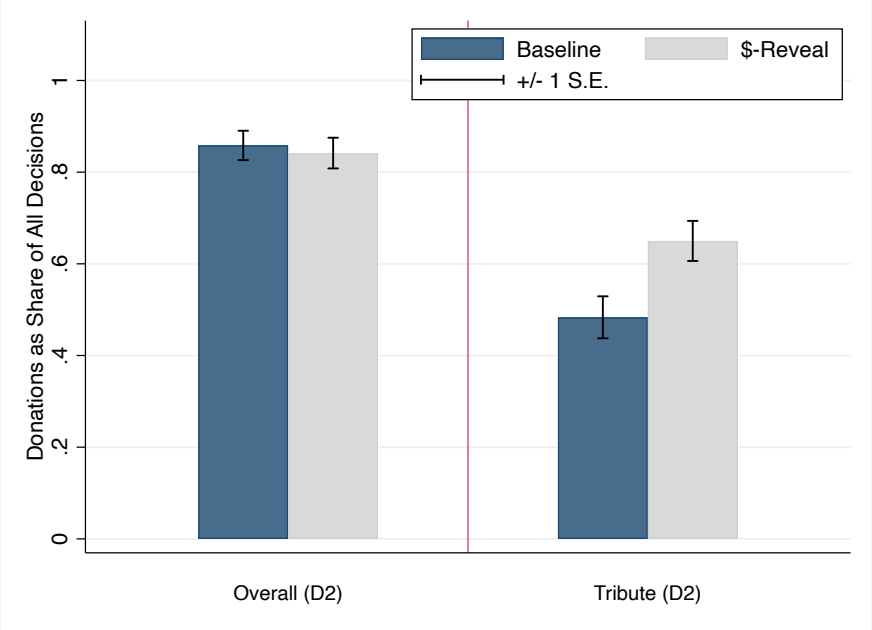

Figure 3: Donation Rates (*Other*)

Table 5: Effect of Revealing Amount on Donation Rates (*Other*)

	(1) OLS	(2) OLS	(3) Probit	(4) OLS	(5) OLS	(6) Probit
Dep. Var.:	$\mathbb{1}$(*Donation 2)*			*Make Tribute Donation*		
$\mathbb{1}$(Donation 1)	0.439***	0.375***	0.224***	0.346***	0.244***	0.249**
	(0.101)	(0.108)	(0.052)	(0.089)	(0.098)	(0.099)
ID&Amount	-0.031	-0.026	-0.033	0.155**	0.169***	0.171***
	(0.043)	(0.045)	(0.043)	(0.063)	(0.067)	(0.063)
Observations	240	240	240	240	240	240
R-squared	0.156	0.239		0.078	0.142	
Controls	No	Yes	Yes	No	Yes	Yes

Notes: Columns 1-3 report estimates using an indicator variable for whether participants make any donation (D2) as the dependent variable. Columns 4-6 report estimates using an indicator variable for whether participants make a tribute donation as the dependent variable. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. All specifications control for whether participants donate in private (D1). MacKinnon-White HC3 robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

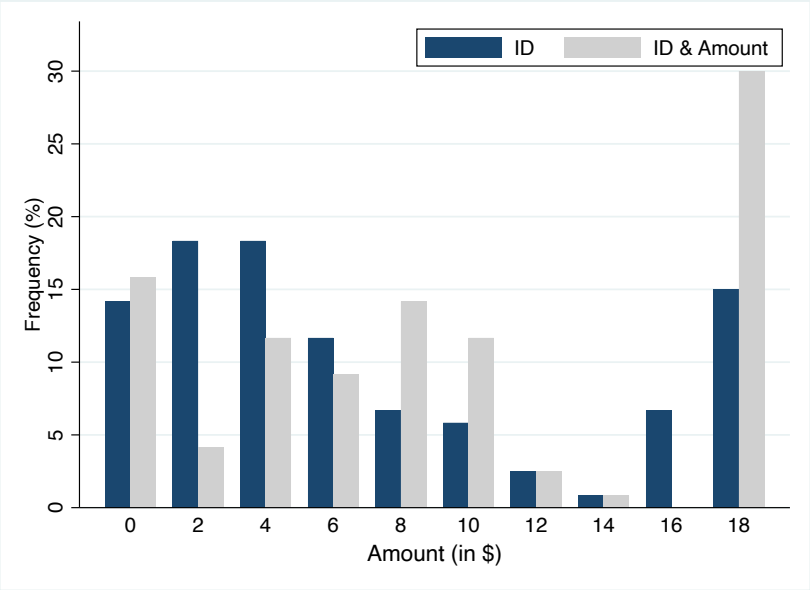

Figure 4: Distributions of Donation Amounts (*Other*)

Table 6: Effect of Revealing Amount on Donation Size (*Other*)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Tobit	OLS	OLS	Tobit
Dep. Var.:	*Donation 2 Amount*					
	Unconditional			*Conditional*		
Donation 1	0.746***	0.735***	0.966***	0.753***	0.766***	1.076***
	(0.064)	(0.065)	(0.091)	(0.046)	(0.045)	(0.076)
ID&Amount	1.111	1.243*	1.643*	1.179*	1.289*	1.641*
	(0.668)	(0.674)	(0.860)	(0.624)	(0.700)	(0.873)
Observations	240	240	240	204	204	204
R-squared	0.449	0.495		0.513	0.533	
Controls	No	Yes	Yes	No	Yes	Yes

Notes: Columns 1-2 and 4-5 report ordinary least squares (OLS) estimates with the donation amount (D2) as the dependent variable. Columns 3 and 6 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Columns 1-3 include all observations, while columns 4-6 condition on observations where participants donate a positive amount. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

Table 7: Effect of Revealing Amount on Future Donation Size (*Self* and *Other*)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Tobit	OLS	OLS	Tobit
Dep. Var.:	*Future Donation Amount*					
	Self			*Other*		
Donation 1	0.693***	0.694***	0.871***	0.606***	0.590***	0.714***
	(0.111)	(0.124)	(0.157)	(0.118)	(0.127)	(0.146)
ID&Amount	0.580	0.266	0.198	1.336	1.430	1.841
	(1.061)	(1.201)	(1.236)	(1.134)	(1.381)	(1.376)
Observations	80	80	80	80	80	80
R-squared	0.360	0.434		0.342	0.395	
Controls	No	Yes	Yes	No	Yes	Yes

Notes: Columns 1-2 and 4-5 report OLS estimates using future giving amounts as the dependent variable. Columns 3 and 6 reports results using a tobit specification, accounting for right-censored observations of the dependent variable. Columns 1-3 report results for future donations among individuals who make donation decisions on behalf of oneself during the session, while columns 4-6 report results for future donations among individuals who have an opportunity to donate on behalf of others during the session. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

2.0 Do Actions Speak Louder than Motives? Evaluating the Effectiveness of Image-Fundraising

Charitable giving can boost an individual's image, and organizations can capitalize on this by engaging in "image-fundraising." Public announcements of donations give individuals the opportunity to demonstrate their generosity and are found to increase giving. This paper evaluates whether generosity inferred from charitable giving is discounted when donations are made in response to image-fundraising. I show in an experimental study that others reward larger donations, and that image-fundraising increases giving. However, others account for the conditions under which donations are made and reduce rewards for giving in an image-fundraising environment. While image-fundraising benefits charitable organizations, individuals are not recompensed for donating more in this setting.

2.1 Introduction

Consider an individual who works with a local community organization. The group announces a fundraising drive and recognizes donors by mailing an "honor-roll" letter that acknowledges how much each member gave. The individual expects many members of his community to receive the organization's letter. While he had already intended to donate, he may give more than he originally planned in an attempt to appear generous to his neighbors.

Though this type of "image-fundraising" (IMG) environment may lead to larger donations that benefit fundraisers, donors may not appear any more generous. In the prior example, those who receive the honor-roll letter may discount donations that are motivated by social image concerns. As a result, they may not perceive larger donations in this context to be a signal of greater generosity. Image-fundraising could ultimately impose a cost on some donors by pressuring them to give more, without improving their social image nor the rewards that come with such improvement.

While past research has shown that image-fundraising can increase giving and thus be useful for organizations, there is less evidence about how it affects donors themselves (e.g., Andreoni and

Petrie, 2004; Rege and Telle, 2004; Ariely et al., 2009). In this study, I evaluate the effectiveness of image-fundraising by considering its influence on donations and on the social returns donors receive from giving. To do this, I consider how the solicitation impacts the behavior of both donors and observers of charitable giving. On the one hand, this type of fundraising appeal could benefit donors as it makes it possible for them to signal their generosity (Elfenbein et al., 2012; Fehrler and Przepiorka, 2016). On the other hand, it may be costly for donors if they are forced to give more than they would otherwise prefer, without getting any rewards for doing so. Prior work has suggested that some fundraising methods can be welfare-reducing for donors due to social pressure or producing feelings of shame (DellaVigna et al., 2012; Andreoni et al., 2017; Butera et al., 2021). This paper complements the literature by considering whether donor social benefits decline because observers discount contributions made in response to IMG.

To test this question, I run an experiment where donors may give to charity, and where their donations are seen by observers who they subsequently interact with. I conduct a between-subject design where donations are solicited under two treatments. In an image-fundraising treatment, "Donors" are ranked by their donations and ranking information is disclosed to all Donors. It is contrasted with a baseline treatment, where donations are not compared across Donors. In both settings, half of the participants are defined as "Observers," and do not donate. However, Observers are aware of how donations are solicited, with potential image concerns being greater when giving in response to image-fundraising than baseline. Each Observer is then matched with a Donor, learns of the matched Donor's gift, and plays as the dictator and trustor in a dictator and trust game, respectively. Dictator and trustor transfers capture the social benefits that Donors may subsequently receive from others for giving.

The results show that image-fundraising increases giving – total donations are 20% greater in the IMG treatment than in the baseline treatment. The results also indicate that Observers reward Donors who contribute larger amounts by transferring more to them in the dictator and trust games. However, Observers discount donations solicited using image-fundraising. For a fixed donation, Observers transfer roughly 20% less to Donors in the IMG treatment than to Donors in the baseline treatment. Ultimately, while Donors give more under image-fundraising compared to baseline, they are not rewarded for their increased donations. Observer transfers in the dictator and trust games are no greater in the former treatment than in the latter. Altogether, the findings

highlight how image-fundraising is an effective tool for increasing donations, but also illustrate how it may not yield benefits for donors. The findings further demonstrate why charities utilize this type of solicitation, but also why donors may prefer to give in response to non-IMG. Given the popularity of these methods, the results have important implications for charities who want to utilize productive solicitations to contend for private contributions.

The rest of the paper proceeds as follows. Section 2.2 places this paper within the existing literature. Section 2.3 describes an example decision-making framework that yields predictions for how image-fundraising affects both donors and observers of charitable giving. Section 2.4 outlines the experimental design. Section 2.5 reviews the experimental findings, and Section 2.6 concludes.

2.2 Literature Review

This paper adds to several different strands of recent literature on charitable giving. First, this study builds on work evaluating social image motives in charitable giving (Glazer and Konrad, 1996b; Harbaugh, 1998; Andreoni and Petrie, 2004; Rege and Telle, 2004; Benabou and Tirole, 2006; Andreoni and Bernheim, 2009; Ariely et al., 2009; Samek and Sheremeta, 2014; Vesterlund, 2016).[1] For instance, laboratory studies have tested how reputational concerns and extrinsic motivations interact to affect giving (Ariely et al., 2009), how visibility into donations and income independently and interactively affect decisions (Bracha and Vesterlund, 2017), how competition for social standing affects gifts (Duffy and Kornienko, 2010b), how people are averse to "standing out" (Jones and Linardi, 2014), and how donors are sensitive to framing (Krupka and Weber, 2013).[2] Field experiments have complemented laboratory findings to show that individuals avoid charitable giving altogether when given the opportunity. Avoidance can be due to the physical presence of solicitors and to the fundraising appeal (DellaVigna et al., 2012; Andreoni et al., 2017).[3] Avoidance may

[1] Social image has also been identified as a motive for economic choices beyond public goods provision. See for example, Bursztyn and Jensen (2017).

[2] In Ariely et al. (2009), the authors note how social image motives affect participant effort, which subsequently affects the size of donations. Krupka and Weber (2013) find that all selfish allocations in a dictator game (e.g., 10/0, 9/1, 8/2, 7/3, 6/4) are seen as more socially inappropriate in a "taking" treatment than in a standard "giving" treatment.

[3] DellaVigna et al. (2012) examine the effectiveness of a door-to-door fundraising campaign, while Andreoni et al. (2017) explore how giving to the Salvation Army at a grocery store depends on how many entrances are staffed with bell ringers and the particular fundraising "ask."

also result independent of social factors, and instead be a method of maintaining self-image (Adena and Huck, 2020).

Second, this paper relates to literature on the effectiveness of donations as signals of generosity. Social image concerns should only motivate individuals to give more if donations serve as a valid proxy for their generosity. In this work, individuals receive information about others' charitable histories and decide whether or not to reward them (Engelmann and Fischbacher, 2009; Elfenbein et al., 2012; Fehrler and Przepiorka, 2013, 2016). Rewards vary by context but include monetary transfers, the purchase of goods, and partner selection in a cooperative task. Economists have also evaluated donations as incentives to motivate workers (Imas, 2014; Charness et al., 2016; Cassar and Meier, 2017). Donations signal a firm's commitment to corporate social responsibility — in turn, workers may exert more effort if they are convinced of the firm's status and value similar objectives. This research finds that the effectiveness of prosocial incentives depends on whether the incentive scheme is viewed as an authentic representation of a firm's values. The presence and size of complementary material incentives also matter.

Third, this study contributes to research modeling perceptions of other-regarding behaviors.[4] Many papers test how much individuals consider the intentions behind actions relative to the actions themselves. Theoretical models in this literature range from outcome-based to intentions-based (Rabin, 1993; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006). In laboratory experiments, researchers measure intentions by introducing moves of nature (Charness and Levine, 2007; Stanca, 2010), or by altering a player's strategy space (Fehr et al., 1998; Falk and Fischbacher, 2006; Falk et al., 2008). Beyond intentions, recent papers consider the role of strategic motives in judgments of other-regarding choices. These papers test the role of motives by altering expectations of a second experimental stage or repetition of a game (Stanca et al., 2009; Strassmair, 2009; Johnsen and Kvaloy, 2016; Zhurakhovska, 2017), while others vary future punishment and reward instruments (Orhun, 2018). Most of this research evaluates how material motives affect reciprocal exchanges.[5] This study is concerned with whether

[4] In the terminology of this literature, this paper examines a form of indirect reciprocal exchange (Schram and Seinen, 2006).

[5] One exception is Simpson and Willer (2008), who use a within-subject design to examine the role of reputational incentives in reciprocal exchanges. The researchers ask participants to respond to two fictitious partners who (they are told) made dictator transfers in either a private or public setting. Other research by Berman et al. (2015) notes how perceptions of an individual who advertises his other-regarding behaviors depend on prior knowledge about the "braggart's" prosocial activities.

perceptions of donor motives, particularly social image concerns, prove consequential for charitable fundraising efforts.

2.3 A Simple Decision-Making Framework

I present a simple example to highlight the mechanisms behind image-fundraising. Consider two parties: Donor and Observer. The following equations represent the utility functions for each party. As an example, I assume the utility functions are log-linear.

$$\textbf{Donor: } \psi_D = \ln(w_D - d + \gamma s(d) + m t_R) + \alpha_D \ln(d) \tag{3}$$

$$\textbf{Observer: } \psi_R = \ln(w_R - t_R) + \alpha_R[\ln(d) + \ln(w_D - d + \gamma s(d) + p m t_R)] \tag{4}$$

A Donor may donate $d \in \mathbb{R}_0^+$, from endowment w_D. How much he gives is influenced by his altruistic type, α_D. The Donor can be one of two types, either a low altruistic type, α_D^L, or a high altruistic type, α_D^H. The Donor can also give to signal his generosity when image-fundraising is used, $\gamma = 1$. In particular, he earns status utility $s(d)$ when he donates in response to image-fundraising and can disseminate information about his charitable gifts to a larger audience. I assume status utility can be written in consumption units and is non-negative. Non-negativity implies that the opportunity to promote positive charitable contributions solicited using IMG may satisfy image-concerned Donors. Note, however, that status utility does not capture the social returns that the Donor may receive from others. That is, any impact image-fundraising has on the social returns that the Donor receives from giving is independently captured by the Observer transfer, t_R, which is discussed below.

After the Donor gives, an Observer learns of his donation amount. The Observer, with altruistic type α_R, cares about the value of the donation for the charity recipient and about the Donor.[6] The Observer can choose to commend the Donor for his donation by transferring t_R to him from her endowment w_R. However, she only wants to celebrate a Donor whom she perceives as generous. The Observer's commendation depends on her posterior about the Donor's type, $\mu(\theta|d)$, where

[6]The subscript "R" is used for Observers instead of "O" to avoid confusion with Donor "D" subscripts.

$\theta = L, H$, and where her belief is formed based on the donation. The Observer's posterior that the Donor is a high-type is defined by the parameter, $p = \mu(H|d)$. The Observer transfer is augmented by an efficiency multiplier represented by the parameter, $m > 1$. The multiplier proxies for efficiency gains from the Donor's gift, which for example, could include tax deductions for the Donor.

To consider the impact of image-fundraising, I focus on Donor and Observer behavior in the Perfect Bayesian Equilibrium (PBE) of this framework that satisfies the intuitive criterion (Cho and Kreps, 1987). This refinement results in an equilibrium that separates donations made by high-type Donors and low-type Donors given Observer beliefs, and Observer beliefs are updated after seeing equilibrium donations using Bayes' rule. Further, high-type Donors choose a donation that satisfies their first-order condition. COROLLARY 1 and COROLLARY 2 provide a compact explanation of the comparative statics of this example with respect to image-fundraising. A complete characterization of this equilibrium is included in Appendix Section B.3.

COROLLARY 1: Image-fundraising increases the total amount donated relative to no image-fundraising.

With image-fundraising ($\gamma = 1$), Donor giving is observable to a broader set of people, i.e., Donor neighbors and communities. High-type Donors increase the size of their donations in order to promote their giving. Meanwhile, low-type Donors do not change their (lack of) giving in response to the fundraising method. Low-type Donors give nothing as they do not have other-regarding preferences. In aggregate, an increase in high-type Donor contributions will boost the total amount donated in response to IMG relative to no IMG.

COROLLARY 2: For fixed donation d, image-fundraising decreases Observer transfers to Donors relative to no image-fundraising.

Irrespective of whether image-fundraising is used, Observers send positive transfers to high-type Donors and nothing to low-type Donors in equilibrium. However, when making transfers to high-type Donors when image-fundraising is used, Observers are aware that image-concerned Donors

could be giving in part to promote their generosity. Taking this factor into account, Observers reduce the social benefits they confer to high-type Donors. This reduction can be interpreted as Observers' hesitation to attribute donations solely to Donor generosity when image motives are present.

As a consequence, COROLLARY 1 and COROLLARY 2 suggest that Donors may not be rewarded for giving more in response to image-fundraising. Instead, some Donors may be left worse off if they make larger contributions but Observers discount donations made in response to IMG. I test the comparative statics characterized in COROLLARY 1 and COROLLARY 2 in the laboratory experiment described in Section 2.4.

2.4 Experimental Design

To consider both Donor and Observer behavior when evaluating the effectiveness of image-fundraising, I run a between-subject laboratory experiment with two conditions, an Image- Fundraising (IMG) treatment and a Baseline treatment.[7] Participants are randomly assigned to either the role of Donor or Observer. In the experiment, Donors are labeled "Blue Players" while Observers are labeled "Green Players." Participants stay in these roles throughout the experiment. The experiment consists of two rounds, and instructions are provided sequentially before each decision. The experimental design, roles, decisions, and decision order are summarized in Table 8. Experiment instructions are provided in Appendix Section B.1. To begin, all participants are seated at individual computer stations. At the top of each station is a card holder with an assigned participant number. Participants receive initial instructions that describe the two possible roles they may be assigned to, and how participant numbers may be used during the experiment.

2.4.1 Charitable Giving Round

At the beginning of this round, participants receive their role assignment. Participants assigned as Donors are asked to make two donation decisions, in $2 increments. Each decision is made from

[7] I leverage design elements from Fehrler and Przepiorka (2016), where the researchers evaluate whether the act of donating enhances a person's reputation and affects partner selection in cooperative tasks. I complement their research by examining whether the solicitation method alters the signal that donations send about a person's generosity.

Table 8: Experimental Design

Round	Order	Decisions	
	Within Round	Donor	Observer
Charitable Giving	Decision 1	D1 (Altruistic Type)	—
	Decision 2	D2 (Donation)	—
Social Exchange	Decision 1	—	Dictator
	Decision 2	Trustee	Trustor

an initial $10 endowment, with one of the two decisions implemented at random to count toward payment. Implemented donations are made to the UPMC Children's Hospital of Pittsburgh.[8] Donations are matched one-for-one by University of Pittsburgh research foundation funds.

The first decision is denoted as D1. For D1, Donors are asked how much they are willing to donate to the Children's Hospital when their decision is private (i.e., not disclosed to any other participant). D1 proxies for the underlying altruistic type of Donors. After choosing D1, Donors are asked to make a second donation from their initial $10 endowment. The second decision is denoted as D2. Unlike D1, D2 is public (i.e., disclosed to other participants), but varies in the degree of public disclosure across treatments. Subsequent discussion in this paper refers to D1 as participant altruistic types, and D2 as participant donations.

In both treatments, Donors are told that one matched Observer in the session will see their donation (D2), and that they will interact with the matched Observer in the next round. However, Donors are told that the matched Observer will not be able to identify them in any way. In the IMG treatment, Donors are told their donations will also be ranked against all other Donors in the session, with rankings determined by donation amounts (Duffy and Kornienko, 2010b). They

[8]Participants are told that donations will be used to purchase art materials for the Creative and Expressive Arts Therapy Program at the Children's Hospital. This program helps children cope with symptoms of their illness, stress, and traumatic experiences. Donors can also request a donation receipt by submitting a form to the Children's Hospital at the end of the experiment. By submitting the donation form, Donors are informed that they will relinquish their anonymity, as names will be linked to their donations.

are told that rankings will be shown to all Donors alongside their donation amounts. Rankings will also appear alongside their participant number, which other Donors can use to identify them. The ranking information appears on Donor computer screens at the end of the Charitable Giving Round and before the start of the Social Exchange Round. The ranking of donations in the IMG treatment represents an image-fundraising appeal intended to elicit social image concerns.

Importantly, only Donors in the IMG treatment receive ranking information. Observers in the IMG treatment only learn about their matched Donor's donation amount, and cannot compare information about their matched Donor to other Donors. The intent of the treatment manipulation is to alter how Observers perceive identical charitable donations, but some that are made in response to image-fundraising and others in response to a baseline solicitation. By only disclosing the matched Donor's donation amount, my primary objective is to keep all other characteristics of the donations identical across treatments, and therefore isolate the impact of image-fundraising. Then, for the Social Exchange Round decisions described in greater detail below, Observers focus on two pieces of information – how much their matched Donor gave, and the fundraising conditions under which their matched Donor gave. In particular, Observers in the IMG treatment know about the image-fundraising behind their matched Donor's donation. Observers in this treatment wait while donations are ranked, are notified when rankings are in progress, and the experiment does not continue until all Donors have had time to review the rankings. In contrast, Observers in the Baseline treatment are aware of how their matched Donor's donation is solicited, which does not involve image-fundraising. If instead I use an experiment design where Observers also learn the full distribution of donations in a session, Observers could determine their matched Donor's rank before making their decisions. As a result, other characteristics of the donations may not be identical across treatments. This would impede my ability to cleanly identify the image-fundraising treatment effect.

The inclusion of both D1 and D2 is important for two reasons. First, because D1 proxies for the underlying altruistic type of Donors, I can test whether participants are effectively randomized across treatments. Assuming effective randomization, there will be no differences in the average Donor altruistic type across treatments. Second, I can measure the fundraising impact of the different solicitations by comparing the difference between the two decision values across treatments. That is, the treatment effect of interest identifies whether donations are larger for Donors in the IMG

treatment relative to Donors in the Baseline treatment, after controlling for individual altruistic types.

2.4.2 Social Exchange Round

In the Social Exchange Round, each Observer is randomly matched with one Donor, and sees their matched Donor's donation. As mentioned above, Observers in both treatments cannot identify their matched Donor in any way. However, Observers in the IMG treatment know their matched Donor gave after receiving an image-fundraising appeal. Each pairing then plays two games, a dictator game and a trust game, with one of the two games implemented at random to count toward payment. All transfers in the games are made in $2 increments. Observers play as the dictator and trustor, and make each transfer from an initial $10 endowment. Observer dictator and trustor transfers are multiplied by three. Donors play as the trustee and can choose to send money back to the Observer. The maximum amount Donors can transfer back is the tripled amount they receive from Observers. Donor trustee transfers are collected using the strategy method, where Donors make choices for every potential Observer trustor transfer.

The dictator and trust games represent different social interactions Donors and Observers can have with one another. The dictator game describes a limited interaction where Observer transfers are the last decision, while the trust game describes an expanded exchange where Donors can transfer money back to Observers. Observers cannot materially benefit from the dictator game exchange. In contrast, Observers can materially benefit from the trust game. However, this benefit is conditional on their trust of Donors, and if Donors are indeed trustworthy. Finally, the trust game provides a measure of Donor "trustworthiness," the return transfer to Observers.

After the Social Exchange Round, Observers complete an incentivized belief elicitation regarding their matched Donor's trustworthiness. Observers are asked how much they believe their matched Donor transfers as the trustee for every possible trustor transfer.[9] This elicitation captures possible differences in Observer beliefs about Donor trustworthiness due to their partner's donation and the fundraising method. Next, all participants complete an incentivized multiple price list risk preference elicitation (Holt and Laury, 2002). Prizes correspond to comparable stakes present in

[9]To incentivize this decision, one randomly chosen trustor transfer is selected for each Observer, and the Observer receives an additional $5 if their beliefs are ±$2 from the Donor's associated trustee transfer.

the trust game.[10] Finally, participants complete a survey that collects demographics studied in past work.[11] Characteristics include participant age, gender, year in school, prosocial tendencies, and political leanings.[12] Once participants complete the demographic survey, they learn which decisions were randomly chosen to count toward payment, and their final earnings.

To summarize, participant earnings are calculated from the following components. First, one of the two Charitable Giving Round decisions (D1 or D2) is chosen at random.[13] Next, one of the two games (dictator or trust) played in the Social Exchange Round is chosen at random. Finally, two participants in each session are randomly chosen to be paid for their risk preference elicitation responses, and Observers are paid depending on the accuracy of their belief elicitation responses. Payment from all rounds are combined with a $6 show-up fee for final earnings.

2.5 Experimental Results

The experiment was programmed in both z-Tree and oTree, and run with the Pittsburgh Experimental Economics Laboratory (PEEL) participant pool at the University of Pittsburgh (Fischbacher, 2007; Chen et al., 2016).[14] Two hundred thirty-eight participants completed the study over twelve sessions. Six sessions were run in-person, and six were run online in a virtual laboratory setting due to the COVID-19 pandemic.[15] Results reported in this section include controls for

[10]Two randomly chosen participants in each session have one randomly drawn row implemented from their multiple price list choices.

[11]Eckel and Wilson (2004) find no relationship between risk attitudes and trust behaviors. Cassar and Meier (2017) find that non-prosocially-motivated individuals decrease effort in response to an instrumental non-monetary incentive in a principal-agent setting. Butler et al. (2017) find that participants with left-leaning political views are more likely to be influenced by social approval when deciding to "blow the whistle" on fraudulent behavior. These characteristics could be associated with Donor and Observer choices, and are included as control variables in subsequent analyses. As shown in Section 2.5, the results are not sensitive to the inclusion of these controls.

[12]Participant prosocial tendencies are defined across two variables, labeled as "Volunteer" and "ExistingCharity." Participants' political leanings are defined by an "Ideology" variable. Each of these variables is defined across five categorical intervals. For the Volunteer variable, participants are asked, "On average, how often do you volunteer for a good cause?" Answers are scaled 0-4 corresponding to categorical answers of "Never," "Once a year," "Once a month," "Every week," and "Several times a week." For the ExistingCharity variable, participants are asked, "On average, how much do you donate to charitable organizations per year?" Answers are scaled 0-4 corresponding to categorical answers of "$0-$20," "$20-$50," "$50-$100," "$100-$500," and "Over $500." The Ideology variable is scaled 0-4 from "Very Liberal" to "Very Conservative."

[13]Consequently, only one of the two donation decisions is made to the Children's Hospital for each Donor.

[14]The oTree implementation of the risk preference elicitation uses code from Holzmeister (2017).

[15]Online sessions were also run with the PEEL participant pool. Experiment procedures in the virtual laboratory mirrored those used in the in-person laboratory, following Danz et al. (2021). However, online sessions were hosted

online sessions. Eleven sessions were run with twenty participants, and one session was run with eighteen participants. Subjects earned an average of $19.38. The experiment raised $1,192 for the UPMC Children's Hospital of Pittsburgh.

I first discuss the effect of image-fundraising on donations. Next, I report Observer responses to Donors, evaluating the impact of image-fundraising on dictator and trustor transfers. I then review Donor trustworthiness and Observer beliefs about trustworthiness. Finally, I consider the net effect of image-fundraising in terms of donations and the social returns to Donors.

2.5.1 Donor Contributions

Table 9 shows the average values for Donor choices across treatments.[16] As evidence of random assignment, neither the average altruistic type (D1) nor the distribution of altruistic types significantly differ across treatments.[17] Table 9 also provides initial evidence that image-fundraising leads Donors to give more. Average donations (D2) are $1.27 larger than the average altruistic type in the IMG treatment, while average donations in the Baseline treatment are only $0.10 larger (difference of $1.17; t=3.65, $p< 0.01$). These results suggest that the rankings used in the IMG treatment are effective in priming Donors' social image concerns.

How do changes at the individual level affect aggregate fundraising efforts? Figure 5 visualizes the effect of image-fundraising on the total amount donated. Figure 5 shows the cumulative distribution functions for participant altruistic types and donations, by treatment. The comparison of interest is the difference-in-difference across treatments. In the IMG treatment, the distribution of donations shifts rightward relative to the distribution of Donor altruistic types, suggesting that total giving increases in response to image-fundraising. A paired-sample sign test reveals a statistically significant difference between the two distributions in the IMG treatment ($p< 0.01$). In contrast, a similar shift does not occur between the distribution of altruistic types and donations in the Baseline treatment, suggesting that total giving does not change in response to the Baseline

over Zoom. Subjects joined a Zoom meeting and participants were randomly assigned participant numbers that were shown in the corner of each person's video feed.

[16] Decision values refer to the out-of-pocket, unmatched amounts.

[17] The median altruistic type is $4 for both Baseline and IMG treatments (Mann-Whitney rank-sum, z=0.15, p=0.88). Participant demographic characteristics (i.e., age, year in school, gender, prosocial tendencies, political leanings) also do not significantly differ across treatments. Summary statistics regarding participant demographics are reported in Appendix Table B.2.1, by experimental role.

solicitation. A paired-sample sign test evaluating differences between the two distributions in the Baseline treatment reveals no statistically significant difference (p= 0.66).[18]

The above analysis is summarized in Table 10, in a series of regressions evaluating the effect of image-fundraising and participant altruistic types on donation amounts. The results are consistent across all specifications. As an example, the results in column 2 indicate that after controlling for altruistic types, image-fundraising increases donations by nearly \$1.13 (t= 3.57, p< 0.01). This accounts for more than a 24% increase over the average Baseline donation.[19] The findings also illustrate that Donors who are more privately altruistic give more, independent of the fundraising mechanism. A \$1 increase in a Donor's altruistic type is associated with an \$0.89 increase in the donation amount (t= 18.44, p< 0.01). The estimates in column 3 from a Tobit regression accounting for censored observations are similar – IMG increases donations, and participant altruistic types are positively associated with donation amounts.[20] Overall, the results imply that image-fundraising has the effect of increasing total giving from Donors and benefits fundraisers, consistent with COROLLARY 1 from Section 2.3.

RESULT 1: Image-fundraising increases total giving compared to no image-fundraising.

2.5.2 Observer Responses

Dictator and trustor decisions measure whether Observers discount donations made in response to image-fundraising. In addition to the fundraising method, I evaluate the impact of partner donation amounts on Observer choices.[21] Table 11 summarizes estimates of the effect of image-fundraising and partner donations on Observer transfers.

Columns 1-3 of Table 11 report results regarding the effect of image-fundraising and donation amounts on Observer transfers, pooled across dictator and trust games. The specifications also

[18]Results are reported from a two-sided test of the null hypothesis of finding positive and negative differences with equal probability between matched donations and altruistic types.

[19]A Probit regression also demonstrates that Donors in the IMG treatment are significantly more likely than Donors in the Baseline treatment to donate an amount that is larger than their altruistic type. Results from the Probit regression are included in Appendix Section B.2, Table B.2.2.

[20]Twenty four participants gave the maximum amount (\$10) while twelve gave no money.

[21]Moving forward, the reported Observer dictator and trustor transfers refer to the non-tripled amounts.

include an indicator for whether the transfer is a dictator or trustor transfer. The estimates show that conditional on the donation amount, Observers transfer less to Donors in the IMG treatment relative to Donors in the Baseline treatment. As an example, the results in column 2 suggest that for a fixed donation amount, image-fundraising reduces Observer transfers by an average of \$0.77 (t= −1.98, p= 0.05).

When exploring dictator transfers in particular, similar patterns emerge. Columns 4-6 report results regarding the effect of image-fundraising and donation amounts on Observer dictator transfers. The results show that conditional on the donation amount, Observers transfer less to Donors in the IMG treatment relative to their counterparts in the Baseline treatment. As an example, the results in column 5 suggest that for a fixed donation amount, image-fundraising reduces Observer dictator transfers by an average of \$0.67. This effect is equivalent to a 20% decrease over the average Observer dictator transfer in the Baseline treatment. However, the coefficient estimates for the treatment effect are not statistically significant (col. 4 – t= −1.24, p= 0.22; col. 5 – t= −1.40, p= 0.17; col. 6 – t= −1.60, p= 0.11).

Finally, in parallel with the dictator game, columns 7-9 illustrate that for a fixed donation amount, Observers transfer less to Donors in the trust game due to image-fundraising. As an example, the results in column 8 suggest that this solicitation method reduces Observer trustor transfers by an average of \$0.87. This effect is equivalent to a 17% decrease over the average Observer trustor transfer in the Baseline treatment. Yet, the only statistically significant IMG coefficient estimate is in the Tobit specification that accounts for censored observations (col. 9 – t= −1.66, p= 0.10). Meanwhile, the OLS estimates, while qualitatively consistent with the pooled estimates and dictator game findings, are not statistically significant (col. (7) – t= −1.22, p= 0.23; col. (8) – t= −1.44, p= 0.15). In aggregate, the results in Table 11 support the implications of COROLLARY 2 from Section 2.3 – Observers appear to discount donations made in response to image-fundraising.[22]

[22]In Section B.2 of the Appendix I conduct a non-parametric test where I match Observer observations across treatments by their partner Donor's donation, and estimate differences in dictator and trustor transfers. The non-parametric analysis is intended to proxy for a within-subject setting where one Observer encounters two Donors, one whose donation is solicited using image-fundraising and another whose donation is not. Given the set of possible matches, the exercise is iterated 1,000 times to create a distribution of average transfer differences across treatments. The results of this non-parametric exercise suggest IMG decreases both dictator and trustor transfers, and provides further evidence to support RESULT 2. Details of this analysis are included in Section B.2 of the Appendix.

RESULT 2: For a fixed donation, image-fundraising decreases Observer transfers to Donors relative to no image-fundraising.

Although Observers appear to discount donations made in response to image-fundraising, Observers respond positively to partner donation amounts when deciding how much to transfer to Donors. The results reported in columns 1-3 show that Observers transfer an average of $\$0.39-\$0.56$ more to their partner Donors for every \$1 increase in the donation amount. When looking at specific types of transfers, the positive rewards are reflected in both dictator and trust games. As a dictator, the results suggest that Observers transfer $\$0.38 - \$0.53$ more for every \$1 increase in their partner's donation amount. Likewise in the trust game, observing a larger donation significantly increases trustor transfers. Observers transfer $\$0.40 - \$0.60$ more as a trustor for every \$1 increase in their partner's donation amount. The estimates are all statistically significant and are not sensitive to the model specification.

The numerical results from Table 11 are illustrated in Figure 6. Figure 6 shows average dictator and trustor transfers at each partner donation amount. The number of observations are reported in parentheses underneath each donation amount. The figure illustrates the treatment estimates on dictator and trustor responses reported in Table 11 after conditioning on the amount donated. Observer transfers in the Baseline treatment (left bar in each cluster) are weakly greater than Observer transfers in the IMG treatment (right bar in each cluster) at nearly every donation amount above \$0, for both dictator and trust games.[23] Further, both dictator and trustor transfers increase as partner donation amounts increase, mirroring Table 11 results.

The results above provide insights into how Observers assess the signaling value of donations. The findings highlight how Observers reward Donors for giving more to charity. At the same time, when assessing Donor generosity, Observers appear to consider the fundraising appeal that prompts Donors to give. Specifically, Observers discount contributions motivated by social image concerns. Taken together, the results suggest that larger donations do not necessarily signal greater generosity if Observers also consider the fundraising context in which donations are made.

[23] One exception is for \$6 dictator transfers, where Observers in the IMG treatment send an average of \$0.09 more than Observers in the Baseline treatment (\$3.17 vs. \$3.08).

2.5.3 Donor Trustworthiness

Beyond Observer dictator and trustor transfers, I also collect Observer beliefs about Donor trustworthiness to further evaluate how Observer may perceive Donors differently across the two treatments. The first three columns of Table 12 report results examining the effect of image-fundraising, trustor transfers, and partner donations on Observer beliefs about Donor trustworthiness.[24]

Consistent with results regarding Observer transfers in the dictator and trust games, the beliefs analysis shows that Observers perceive Donors in the IMG treatment to be less trustworthy than Donors in the Baseline treatment. After controlling for the donation amount and the trustor transfer, Observers believe that Donors who give in response to image-fundraising will send less back to them in the trust game, compared to Donors in the Baseline treatment. The estimates are statistically significant regardless of the model specification. In particular, for a fixed donation amount and trustor transfer, the results highlight that Observers expect Donors in the IMG treatment to send back $\$0.62 - \$0.78$ less as the trustee than their Donor counterparts in the Baseline treatment. These findings lend further support for the argument that Observers discount donations made in response to image-fundraising.

In addition to differences in Observer beliefs about Donor trustworthiness across treatments, I can examine whether Donor trustworthiness actually differs across treatments. Columns 4-6 of Table 12 report the results from this analysis. Interestingly, the results regarding Observer beliefs contrast the estimates regarding Donor trustworthiness. For a fixed donation and trustor transfer, Donors in the IMG treatment appear more trustworthy that Donors in the Baseline treatment. In particular, Donors in the IMG treatment send back between $\$0.73 - \$1.06$ more as the trustee than their Donor counterparts in the Baseline treatment. The estimates are all statistically significant, and are not sensitive to the model specification (col. 4 – $t= 2.22$, $p= 0.03$; col. 5 – $t= 2.66$, $p< 0.01$; col. 6 – $t= 2.61$, $p< 0.01$).

Finally, beyond treatment differences, the results from the beliefs analysis also indicate that Observers view Donors who give more as more trustworthy. Conditional on the trustor transfer amount, Observers believe that Donors will send back $\$0.29 - \$0.31$ more for every additional dollar that they observe their matched Donor contributing. In complement, Donors who give

[24]Because beliefs about Donor trustworthiness are collected using the strategy method, the estimates are pooled across hypothetical trustor transfer amounts.

more are more trustworthy. For a fixed trustor transfer, every additional dollar that individuals donate in the Charitable Giving Round is associated with $\$0.13 - \$0.16$ greater trustworthiness. These results further validate why capitalizing on donor social image concerns can be effective in charitable fundraising. In particular, the association between donations and trustworthiness shows that charitable giving is correlated with other-regarding behaviors more broadly, and that others interpret the size of donations as an effective signal of individual generosity (Elfenbein et al., 2012; Fehrler and Przepiorka, 2013, 2016).

2.5.4 Evaluating the Net Effect of Image-Fundraising

If image-fundraising increases donations but also affects Observer responses to Donors, what is the net effect of employing these solicitation methods? To answer this question I first consider the impact on average donations. I also evaluate the social returns that Donors receive from Observers by comparing the unconditional average dictator and trust transfer across treatments. The purpose of the discussion that follows is to describe the qualitative effects of using image-fundraising, as the magnitude of each effect is sensitive to the matching mechanism used in this experiment. This exercise reviews where this type of solicitation generates value for fundraisers and donors, but is not intended to describe the precise returns it will generate for each of these parties. The findings are summarized in Table 13 below.

For fundraisers, donations increase by over 26% in the IMG treatment relative to the Baseline treatment.[25] While Observers respond positively to Donors who give more, Observers also discount donations solicited using image-fundraising. The former rewarding response is more than offset by the latter reaction. Donors in the IMG treatment receive about 3% less in dictator transfers relative to Donors in the Baseline treatment. Similarly, Donors in the IMG treatment receive about 4% less in trustor transfers relative to Donors in the Baseline treatment. Altogether, the results suggest that on average, Donors are not rewarded for their larger contributions made following image-fundraising.

[25]The percentage differs from the 24% estimated in Section 2.5.1, as it is derived from comparing *average* donations across treatments. The percentage reported in Section 2.5.1 is calculated using the coefficient from regressing donations on IMG, as a percentage of the mean Baseline donation.

2.6 Conclusion

Donor-honor-roll letters and other forms of image-fundraising methods are popular with charitable organizations. Organizations can use these solicitations to capitalize on donors' social image concerns, who increase their donations in an effort to appear generous. However, if others expect individuals to be motivated by image, they may not perceive larger donations solicited using image-fundraising as signals of greater generosity. Donors could feel pressure to give more, but may not be rewarded for their greater giving.

Using a laboratory experiment, I test the efficacy of image-fundraising by considering its effects on both donations and the subsequent social rewards that donors receive from giving. The results suggest that image-fundraising increases total donations, supporting prior research showing the effectiveness of this form of solicitation (e.g., Andreoni and Petrie, 2004; Rege and Telle, 2004; Ariely et al., 2009). However, the social benefits that individuals would have otherwise received for greater giving are not realized in an image-fundraising environment. That is, the social rewards for the larger donations that image-fundraising generates are offset by the marking down of donations produced using this type of appeal. Ultimately, some individuals may be left worse off for donating more than they otherwise would, while receiving no improved social returns. The results highlight the ramifications of employing image-fundraising methods, not just for charities but also for individual donors.

Though the study calls attention to the potential unfavorable consequences of employing image-fundraising for donors, this type of solicitation remains ubiquitous in practice. One possibility is that organizations, especially those of high quality, may have inadvertantly synchronized their use of image-fundraising given its profitability for charities. Without alternatives, individuals may be pressured to donate more knowing their giving will be made public. However, as the market for private contributions grows increasingly crowded, this work also presents the prospect that image-fundraising may not be a sustainable strategy for some organizations. An open question that remains is what types of institutions may need to alter their fundraising strategies to remain competitive if individuals can seek out, and give to, other charities.

More broadly, the results contribute to the recent body of work that examines the use of public recognition as a policy tool (e.g., Butera et al., 2021). This paper adopts a similar analytical ap-

proach, as it explores the impact of public recognition on outcomes beyond the targeted behavior. In particular, this study adds quantitative estimates of the social costs and benefits associated with these policy instruments. Given the frequent adoption of image-fundraising by charitable organizations, the results are derived from a useful context for analyzing the effects of public recognition methods. To that end, this paper highlights how further exploration of public recognition schemes may be fruitful, in order to deepen our understanding of how stable or varied the effects of these policies are across economic settings.

Table 9: Donor Characteristics

	(1)	(2)	(3)	(4)
	Full Sample	Baseline	IMG	p-value
Altruistic Type (D1)	4.62	4.58	4.67	0.87
	(2.93)	(2.93)	(2.96)	
Donation (D2)	5.31	4.68	5.93	0.03
	(3.18)	(3.01)	(3.25)	
D2-D1	0.69	0.10	1.27	<0.01
	(1.38)	(1.51)	(1.95)	
Observations	119	59	60	

Notes: Reported numbers are means for the specified sample in each column. Standard deviations are reported in parentheses. The p-values are reported from two-sided t-tests comparing means of Donors in the Baseline and IMG treatments. Observations correspond to Donors only.

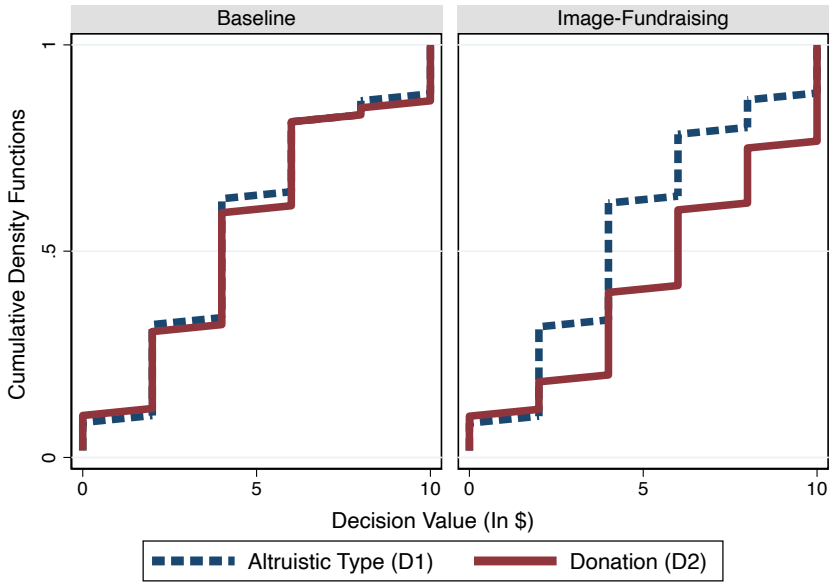

Figure 5: CDFs for Donations and Altruistic Types, by Treatment

Table 10: Effect of Image-Fundraising on Donations

	(1) OLS	(2) OLS	(3) Tobit
	Dependent Variable: Donations (D2)		
IMG	1.18***	1.13***	1.34***
	(0.32)	(0.32)	(0.42)
Altruistic Type (D1)	0.89***	0.89***	1.24***
	(0.05)	(0.06)	(0.09)
Constant	0.60**	5.50*	5.89*
	(0.30)	(2.79)	(3.54)
Controls	No	Yes	Yes
R^2	0.71	0.73	—
N	119	119	119

Notes: Robust standard errors are reported in parentheses. The dependent variable in all models are participant donations (D2). Regressions in columns 1 and 2 are ordinary least squares, where column 2 adds controls for demographic characteristics. Tobit regression results in column 3 account for censored donations. The control variables are gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, an estimate of risk preferences, and an indicator for online sessions. Observations correspond to Donors only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effect of Image-Fundraising on Observer Responses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	Tobit	OLS	OLS	Tobit	OLS	OLS	Tobit
	Dependent Variable:			*Dependent Variable:*			*Dependent Variable:*		
	Pooled Transfers			*Dictator*			*Trustor*		
IMG	-0.67*	-0.77**	-1.24**	-0.60	-0.67	-1.05	-0.74	-0.87	-1.45*
	(0.38)	(0.39)	(0.58)	(0.48)	(0.48)	(0.66)	(0.60)	(0.60)	(0.87)
Donation (D2)	0.41***	0.39***	0.56***	0.40***	0.38***	0.53***	0.41***	0.40***	0.60***
	(0.06)	(0.07)	(0.10)	(0.07)	(0.08)	(0.11)	(0.10)	(0.11)	(0.16)
Constant	1.45***	-5.38	-6.89	1.43***	-5.42	-9.28	3.12***	-3.68	-3.68
	(0.38)	(6.08)	(8.71)	(0.43)	(9.06)	(12.16)	(0.55)	(7.98)	(10.74)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
R^2	0.23	0.25		0.20	0.28		0.15	0.19	
N	238	238	238	119	119	119	119	119	119

Notes: Robust standard errors are reported in parentheses. Regressions in columns 1-3 report results with dictator and trustor transfers pooled as the dependent variable. Regressions in columns 4-6 use dictator transfers as the dependent variable. Regressions in columns 7-9 use trustor transfers as the dependent variable. Regressions in columns 1-2, columns 4-5, and columns 7-8 are ordinary least squares specifications. Columns 2, 5, and 8 add demographic controls, relative to columns 1, 4, and 7. Tobit model results that account for censored Observer dictator and trustor transfers are reported in columns 3, 6, and 9. The control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, an estimate of risk preferences, and an indicator for online sessions. Observations correspond to Observers only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

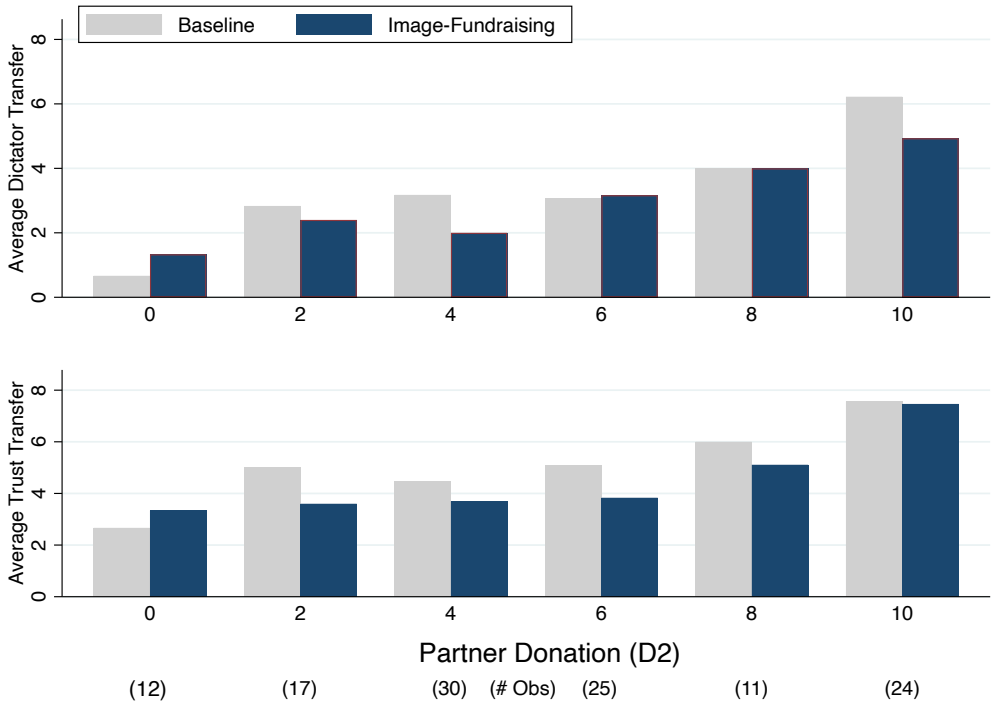

Figure 6: Average Observer Transfers, by Treatment & Partner Donation

Table 12: Beliefs about Trustworthiness and Trustworthiness, by Treatment

	(1) OLS	(2) OLS	(3) Tobit	(4) OLS	(5) OLS	(6) Tobit
	Dependent Variable: Beliefs			*Dependent Variable: Trustworthiness*		
IMG	-0.62***	-0.74***	-0.78***	0.73**	0.90***	1.06***
	(0.232)	(0.232)	(0.245)	(0.327)	(0.340)	(0.405)
Donation (D2)	0.29***	0.30***	0.31***	0.16***	0.13**	0.14**
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)
Trustor	0.39***	0.39***	0.40***	0.37***	0.37***	0.40***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Constant	-1.47***	0.93	0.93	-1.59***	2.81	4.10
	(0.24)	(2.06)	(2.20)	(0.39)	(2.45)	(2.97)
Controls	No	Yes	Yes	No	Yes	Yes
R^2	0.63	0.64		0.41	0.45	
N	595	595	595	595	595	595

Notes: Robust standard errors are reported in parentheses. Regressions in columns 1-3 use Observer beliefs about Donor trustworthiness as the dependent variable. Regressions in columns 4-6 use Donor trustworthiness as the dependent variable. Regressions in columns 1-2 and columns 4-5 are ordinary least squares. Columns 2 and 5 add demographic controls, relative to columns 1 and 4. Tobit specifications in columns 3 and 6 account for censored Observer trustworthiness beliefs and censored Donor trustworthiness transfers, respectively. The control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, an estimate of risk preferences, and an indicator for online sessions. Results in columns 1-3 correspond to Observers only, while results in columns 4-6 are for Donors only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Net Effect of Image-Fundraising

	For Fundraisers		For Donors			
	Donation (D2)	Change	Dictator Transfer	Change	Trustor Transfer	Change
Baseline	\$4.68	**+26.71%**	\$3.32	**−2.79%**	\$5.05	**−4.36%**
IMG	\$5.93		\$3.23		\$4.83	

Notes: Values represent averages for donations (D2), dictator, and trustor transfers.

3.0 Paying for What Kind of Performance? Performance Pay, Multitasking, and Mission-Oriented Jobs

(joint with Daniel B. Jones, Mirco Tonin, and Michael Vlassopoulos)

How does pay-for-performance (P4P) impact productivity and the composition of workers in mission-oriented jobs when output has multiple dimensions? This is a central issue in the public sector, particularly in areas such as education and health care. We conduct an experiment, manipulating compensation and mission, to answer these questions. We find that P4P has significantly smaller positive effects on productivity on the incentivized (quantity) dimension in the mission-oriented setting relative to the non-mission-oriented setting. On the other hand, P4P generates no loss in performance on the non-incentivized (quality) dimension of effort in the mission-oriented setting, whereas it does so in the non-mission-oriented setting. In both mission and non-mission settings, P4P attracts higher ability workers, but it does so at the expense of attracting prosocially-motivated workers in the mission setting.

3.1 Introduction

Performance-related compensation has become an important personnel tool in a large segment of the labor market across many countries.[1] This type of compensation has been recognized to offer two benefits for firms that make use of it: incentive effects on productivity that result from aligning the interest of workers with those of the firm, and, sorting effects that arise from attracting more productive workers to firms offering performance-related pay relative to fixed pay (Lazear, 1986). While bonuses, piece rates and other forms of pay-for-performance (P4P) are more widespread in the private sector, the public and broader nonprofit sector is often characterized by relatively rigid pay policies in which seniority-based rules prevail over schemes rewarding performance. In recent years, there has been lively debate on whether this should change, with disagreement over the desirability of the pay of teachers, nurses, law enforcement officers and other frontline public

[1] See, for instance, Lemieux et al. (2009) and Bloom and Van Reenan (2011).

service providers becoming more conditional on performance (Burgess and Ratto, 2003). Those who advocate for the adoption of pay-for-performance schemes extoll the potential efficiency gains, while those who are more reluctant usually highlight two aspects of the public sector as being particularly problematic in regard to implementing performance-related compensation.[2]

The first concern is that performance is more difficult to measure in the public than in the private sector (Dixit, 2002). This is partly due to technological aspects, as it is easier to measure output in an area like manufacturing (e.g., a car plant) than in services (e.g., a school), and public-sector activity is concentrated in the (advanced) service sector. There is also a governance aspect to this, in that private, for-profit firms have a clear prominent objective, the bottom-line, to which every activity can be potentially benchmarked against. Public sector organizations are instead often characterized by a multiplicity of stakeholders with differing objectives. This makes the jobs of workers in the sector more multidimensional and their performance more difficult to define (e.g., how much did the teacher increase test scores, as opposed to employability, as opposed to civic virtues?).

A second aspect making P4P potentially problematic in the public sector is that prosocial motivation plays a particularly prominent role in some occupations, e.g., teachers or health care providers, in which helping beneficiaries is an important characteristic of the job (Francois and Vlassopoulos, 2008; Besley and Ghatak, 2018). In these settings, P4P may have adverse effects for two reasons. First, performance may decline due to crowding out of prosocial motivation by higher-powered financial incentives (Gneezy et al., 2011). Second, P4P might lead to the attraction of less prosocially motivated workers and cause the dilution of prosocial motivation in the workforce (Jones, 2015; Finan et al., 2017).

This paper contributes to the debate about the desirability of pay-for-performance in sectors with a mission, where prosocial motivation among employees is particularly important (for a recent survey on the role of mission in the labor market, see Cassar and Meier, 2018). We do so by providing experimental evidence, from both an in-person and a virtual laboratory setting, of incentive and sorting effects of performance-related compensation in a work context characterized by prosocial elements and multitasking. To this end, we design a real-effort experiment with a novel task – a word formation task inspired by the board game Scrabble – characterized by two dimensions

[2]See Neal (2011) and Imberman (2015) for surveys of the literature on the effects of performance pay in education.

that we refer to as quantity (the number of words) and quality (the complexity of each word, as measured by points).[3] We first observe how performance in the two dimensions differs between a treatment in which compensation is a fixed wage and one in which it is a piece rate on quantity. We also implement a treatment that induces prosocial motivation by linking quality to a charitable donation and, again, observe performance under a fixed wage and a piece rate on quantity. To assess sorting effects, in a second phase we allow subjects to choose whether to keep the compensation structure that was imposed upon them in the prior phase, or switch to a (higher) fixed wage without a prosocial mission instead. This latter analysis sheds light into any potential dilution in the underlying prosocial motivation of the workforce on the extensive margin as a consequence of P4P, as noted above.

We find the following main results. In the absence of a mission, i.e., induced prosocial motivation, our experiment reproduces the classical results of the literature on incentives, with the piece rate increasing performance in the incentivized dimension while reducing effort along the unincentivized one. We also find that higher ability workers self-select into the piece rate compensation scheme, therefore generating an additional positive effect of P4P on productivity due to sorting (Lazear, 2000). These results establish a baseline against which we compare results from the mission-oriented setting, which is the primary contribution of our paper. In the environment with a mission, we instead find a much smaller incentive effect due to P4P while sorting of higher ability workers into P4P also takes place. Further, we find that people with high prosocial motivation are willing to give up financial gains to stay in the mission sector with fixed pay. Our results imply that P4P is less effective in increasing performance on the incentivized dimension when there is a motivational component. On the other hand, the negative consequences on the unincentivized dimension are also not present.

Returning to the discussion about the merit of introducing pay-for-performance in mission-oriented workplaces, our study suggests that performance pay is less successful in increasing effort on the incentivized dimension when workers are motivated, but also does less harm to other dimensions of effort. With respect to sorting effects, performance pay seems to attract higher ability workers when a mission is present, as it does in the more typically studied non-mission setting. However, we do not see that prosocial motivation predicts choice of P4P in mission-oriented workplaces.

[3]For a review of alternative methods of measuring effort in economic experiments see Charness et al. (2018).

Since we see some sorting of motivated workers in the mission setting under the flat wage, we may conclude that performance pay does pose a trade-off in terms of the type of workers it attracts in the mission sector.

This paper explores the interaction between financial incentives, prosocial motivation, and performance in an environment with multitasking. As such, it intersects with different strands of literature. We outline our contribution relative to existing work below.

First, we build on the literature on incentives that has long recognized the effect of incentive pay in multitasking environments (Holmstrom and Milgrom, 1991). The empirical evidence is mixed, with some studies reporting findings in line with the standard neoclassical theoretical prediction (e.g. Al-Ubaydli et al., 2015; Hong et al., 2018), while others finding no adverse effect due to piece rate compensation on the unincentivized dimension (Shearer, 2004; Copeland and Monnet, 2009). The issue has attracted considerable attention with regard to health care contracting, where multitask agent problems are indeed ubiquitous (Chalkley and Malcomson, 1998; see Scott et al., 2011, for a review). This literature has also underlined the importance of sorting effects for incentive pay (Lazear, 1986), with empirical evidence supporting the theoretical findings (Lazear, 2000). Dohmen and Falk (2011), in particular, show with a lab experiment the importance of multidimensional sorting of workers along the risk preferences and self-assessment dimensions.[4] We contribute to this literature by providing evidence on the impact of the introduction of pay-for-performance on the allocation of effort in an environment with both multitasking and mission.

Second, we connect to the growing experimental literature that documents the importance of prosocial motivation and incentives for performance (Tonin and Vlassopoulos, 2010, 2015; Imas, 2014; Charness et al., 2016; Carpenter and Gong, 2016; DellaVigna and Pope, 2018; Kajackaite and Sliwka, 2017; Cassar, 2019; Bricese et al., 2021). We contribute to this literature by studying how prosocial motivation and pay-for-performance interact to affect the quality and quantity produced when piece rate contracts provide incentives to produce quantity but not quality. Finally, a related strand has been concerned with the effect that financial incentives may have on the selection of motivated workers (Francois, 2007; Delfgaauw and Dur, 2007, 2010; Prendergast, 2007; Dal Bo et al., 2013; Banuri and Keefer, 2016; Ashraf et al., 2020; Barigozzi et al., 2018). We add to the limited evidence of sorting effects by ability and prosocial motivation in the mission sector. In

[4]For example, Douven et al. (2019) studies a situation of multitasking in the provision of mental health care and finds that more altruistic providers provide higher quality care.

particular, we are able to examine whether sorting in this sector is affected by the presence of performance pay, which to the best of our knowledge has not been empirically investigated before.

In short, relative to the existing literature our paper makes three main contributions: first, we provide evidence on the incentive and sorting effects of introducing pay-for-performance in an environment characterized by multitasking and a mission. Second, we examine the incentive effect of performance pay on the incentivized and non-incentivized dimensions of *the same job*, both in a context where prosocial motivation may be active and when it is not, allowing us to isolate the differential effects of pay-for-performance in these two settings. Third, we add to the evidence on how performance pay influences who sorts into mission-oriented jobs in terms of ability and prosocial motivation, contrasting the case in which the job pays for performance to the one when it does not. Taking this all together, our paper is the first with the ability to directly compare the impacts of performance pay across a prosocial/mission-oriented setting and a standard setting, while holding job/task characteristics constant – while also allowing for an analysis of sorting and multitasking. This setting yields the novel insights that performance pay does not pull motivated workers from an unincentivized task to the same degree that it does non-motivated workers, but that performance pay may attract fewer motivated workers into the mission-oriented sector than would occur under flat pay.

The rest of this paper is organized as follows: the next section describes the experimental design, while section 3 lays out a simple theoretical framework and derives behavioral predictions. In section 4 we present the experimental results. Section 5 offers some concluding remarks, and the Appendix includes additional tables and the experimental instructions.

3.2 Experimental Design

The experiment involves a real effort task performed in three phases followed by a questionnaire administered at the end. Participants were informed about the nature of the task and the structure of the experiment at the beginning but received detailed instructions about each phase only at the beginning of the respective phase. The end-of-experiment questionnaire (in the Appendix) included incentivized questions eliciting risk preferences based on the instrument developed by

Holt and Laury (2002), a dictator game with a chosen local charity as the recipient, demographic characteristics and a set of questions proposed by Perry (1996) measuring public service motivation (PSM). In what follows, we describe the novel word-formation task, the phases of the experiment, the treatments, and some procedural details.

3.2.1 Real Effort Task

We employ a novel word-formation real effort task. Participants are presented with a set of seven letters, which they must use to spell a word using all or a subset of the letters (at least two). When a participant spells and submits a word a new set of letters appears on the screen. Letters have points attached to them that are indicated in the lower right-hand corner of each letter square, like the real-life word game Scrabble (see Figure 7 for an example screen). The points reflect the relative rarity of each letter and difficulty of constructing words using those letters. This opens the possibility to measure participants' performance on the task along two dimensions, that we will label as quantity (number of words completed) and quality (points per word). Only words that are grammatically correct are permissible. If the submitted word is incorrectly spelled, the chosen letters are cleared and the participant is asked to try again. Participants receive feedback about the number of words spelled, the total points earned and the time remaining at the top of the screen.

The main features of the task that are crucial for our purposes is that it allows us to measure multiple dimensions of production, quantity and quality, and that the number of words that can be formed in a given period of time is reduced by searching for higher quality ones; that is, words formed using rarer (and often more) letters hence bear more points.[5] Depending on the compensation and motivational structure, workers may face a trade-off between these two dimensions. Note that to ensure that participants face a meaningful trade-off between spelling a large number of words and accumulating points, we place a restriction where the points associated with a certain word are credited only if the total points of the word are at least five.

[5]This task allows for a much simpler and more precise measure of quality compared to other tasks used in the experimental multitasking literature, such as packing envelopes (Al-Ubaydli et al., 2015), planting trees (Shearer, 2004), or various manufacturing tasks (Hong et al., 2018).

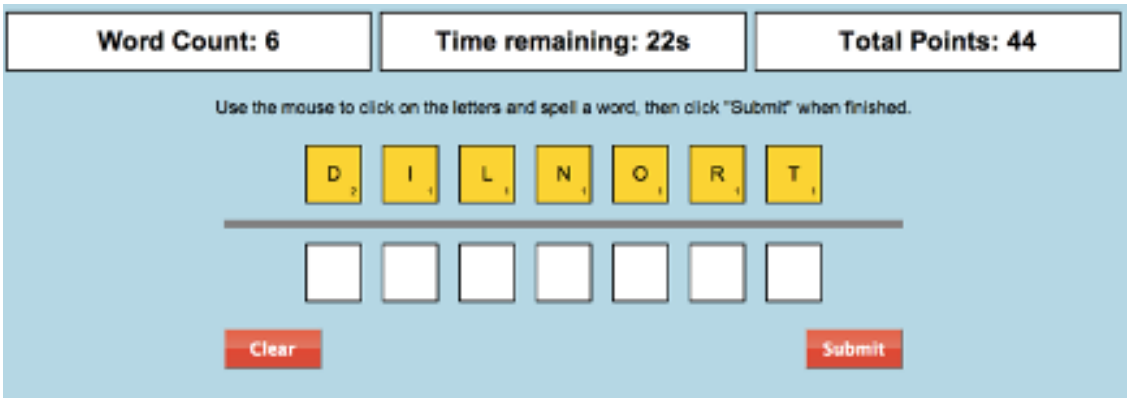

Figure 7: Screenshot of Work Formation Task

3.2.2 Phases

The experiment involves three phases aimed at measuring ability, the incentive effect, and the sorting effect of the incentive schemes.

- Phase 1 (Ability measurement): The purpose of this phase is to familiarize participants with the task and to elicit measures of ability on the task. We use a series of five steps to capture different aspects of ability on the task, described in more detail below.
- Phase 2 (Incentive effect measurement): Participants complete the word-spelling task for eight minutes. Payment differs by treatment (described below).
- Phase 3 (Sorting effect measurement): Participants spend another eight minutes completing the word task. Before starting, they can choose to continue with the treatment-assigned payment scheme (as they saw in Phase 2) or move to an outside option (common across treatments) in which they would receive a flat payment of $9. In this phase, we are primarily interested in the choice of payment scheme that participants make.

Final payment for participants consisted of a show-up fee, plus their payoff from the first phase and the payoff of a randomly chosen of the two other phases, plus any payoff arising from the additional decisions at the end (risk preference measurement and dictator game).[6]

[6] As mentioned previously, sessions were run both in an in-person and a virtual laboratory setting, drawing from two different subject pools. In sessions run in-person participants received a $5 show-up fee, while in sessions run

3.2.3 Ability Measurement

The first phase of the experiment consisted of five short tasks aimed at measuring participants' task-relevant ability. The tasks faced in this phase – as well as in the other phases – were constant across treatments for the sessions conducted in the lab, while for the online sessions they were constant only for tasks 2 and 4 of Phase 1, due to technical issues. Treatment differences in the incentives participants would face in the following phases were not yet described. It is therefore a pure measure of ability, free of any potential treatment effects, which is directly comparable across all participants.

The five tasks were as follows: (i) complete five word formation tasks in two minutes (no compensation), (ii) complete five tasks within 60 seconds (participants start with an endowment of experimental currency units and are penalized the longer it takes to complete the five tasks), (iii) complete as many word formation tasks as possible in three minutes (receive piece rate for each word), (iv) complete five word formation tasks (compensation based on score accumulated for words worth five or more points), (v) complete as many word formation tasks as possible in three minutes (compensation based on score accumulated for words worth five or more points).

The first of the five tasks in Phase 1 was non-incentivized and was primarily meant to provide participants with practice using the interface. Tasks 2 and 3 incentivized participants to spell many words in a short period of time, thereby measuring participants' abilities in the quantity dimension. The remaining two tasks incentivized participants to accumulate as many points as possible in a short period of time, thereby measuring participants' abilities in the quality dimension. Based on earnings on tasks 2-5 we construct a composite ability measure. Specifically, we first standard-normalize participants' earnings in each task (as they are not otherwise measured in the same units), sum standard-normalized earnings across tasks 2-5, and standard-normalize again.[7]

3.2.4 Treatments

We implement a 2*2 between-subject design, depicted in Table 14, in which we manipulate two aspects of Phase 2: the incentive scheme (flat or piece rate) and whether there is an opportunity

virtually with the Pittsburgh Experimental Economics Lab participant pool, participants received a $6 show-up fee in accordance with the lab rules.

[7]To account for the difference in experimental environments, we standard-normalize *within* experiment type (in-person lab vs. virtual lab), though results are similar if we do not.

to raise money for a charity (we refer to the presence of a charity as *mission* and absence as *non-mission*). For the treatments involving a charity, participants select a recipient from a list of local charities with diverse missions (see the Experimental Instructions in Appendix C.3 for the list). In particular, the treatments are as follows:

- F-NM (flat-rate, non-mission or non-motivated): participants are paid \$7 for working during Phase 2 regardless of the number of words spelled (or points accumulated). Together with the compensation associated with the other phases, this amount ensures that participants receive a fair compensation given the duration of the experiment.
- P-NM (piece-rate, non-mission or non-motivated): participants receive \$0.10 per word spelled. We chose this piece rate to have, on average, a compensation similar to the one received in the flat-rate treatment. Indeed, in this treatment on average participants accumulated earnings of \$5.84 (see Table 3).
- F-M (flat-rate, mission or motivated): participants are paid \$7 for working during Phase 2 regardless of the number of words spelled (or points accumulated). In addition, participants generate \$0.02 for a local charity of their choosing for each *point* that they accumulate, with points associated with each word credited only if they are five or greater. We chose this rate to have, on average, a donation that is substantial, but smaller than own compensation. Indeed, in this treatment the charitable donation is on average \$4.15 (see Table 3). Notice that to contribute to charity in a significant manner, a participant needs to produce many words of sufficiently high quality; therefore, the motivation we add to the task does not apply exclusively to the quality dimension. Excluding from the count words with less than five points strengthens the trade-off between quantity and quality.
- P-M (piece-rate, mission or motivated): participants receive \$0.10 per word spelled. In addition, participants generate \$0.02 for a local charity of their choosing for each point that they accumulate, with points associated with each word credited only if they are five or greater.[8] Considering that on average participants accumulate 5.62 points per word, the two incentives have roughly similar power.

[8]It is possible that participants could maximize their monetary earnings in the experiment and donate to a charity outside the lab. This would reduce the difference between P-M and P-NM. Factors like transaction costs or mental accounting, however, would lead subjects to prefer donations made by the experimenter.

In light of this parametrization, the $9 flat payment we offer as an outside option in Phase 3 implies a concrete trade-off for F-M and also, on average, for P-NM and P-M, while it of course dominates F-NM. It is noteworthy that to reduce potential confusion about the purpose of having points assigned to letters in the two non-mission treatments, we opted to tell participants in all treatments: "We are hoping to learn how many points people can accumulate in a fixed period of time."[9] This aligns expectations about the objective of the principal across treatments and aims to capture real workplace conditions involving quantity-quality trade-offs where, while quantity may be directly incentivized because of easier measurement, it is usually understood by workers that the employer is also concerned about quality. An important qualifier here is that quality is, by design, easy to measure and the multitasking problem arises because of the payment structure and not because of a measurement problem.

Table 14: Treatment Conditions

		Sector	
		Non-mission	Mission
Incentive Scheme	Flat rate	F-NM flat-rate, non-mission ($N = 61$)	F-M flat-rate, mission ($N = 61$)
	Piece-rate	P-NM piece-rate, non-mission ($N = 61$)	P-M piece-rate, mission ($N = 61$)

3.2.5 Procedures

Half of the sample was collected in-person in experimental sessions conducted in the Behavioral Lab at the University of South Carolina, while the other half was collected online through the Pittsburgh Experimental Economics Lab (PEEL) at the University of Pittsburgh. Payments were

[9] A full set of Experimental Instructions is reproduced in Appendix C.3.

expressed in terms of experimental currency units (ECUs), which had an exchange rate of 10 ECUs to $1. We conducted 15 in-person sessions in total, with roughly 8 participants per session on average.[10]

The mean earnings across all participants were roughly $16. The typical session lasted approximately 75 minutes. In addition, we conducted 8 online sessions with the PEEL participant pool, with an average of 15 participants per session. Typical online sessions also lasted 75 minutes, and participants earned roughly $20. To best mirror the conditions of the in-person laboratory in the online sessions, we followed procedures outlined in Danz et al. (2021). In the in-person sessions, participants were recruited largely from introductory (freshman/sophomore-level) economics courses from the business school at the University of South Carolina. Participation was voluntary and was not linked to grades in their introductory courses. For the online sessions, participants were recruited through the Pittsburgh Experimental Economics Laboratory recruiting database. Students in this participant pool represent a wider array of fields of study as they are recruited from introductory courses outside of economics, in addition to introductory economics courses.

3.2.6 Motivation for Design Features

In sum, our experiment is aimed at testing whether performance pay operates differently in mission-oriented settings, both with regards to incentive effects and sorting effects. Many researchers have noted that performance pay may operate quite differently in the public and nonprofit sectors due to the prevalence of multitasking amongst workers. Borrowing examples from Prenteic et al. (2007), teachers in schools might aim to improve students' scores on standardized tests *and* improve cultural awareness; hospitals may aim to minimize re-admission of patients *and* improve the long-term health of patients. Importantly in each of these two examples, there is a trade-off between the two objectives that workers are expected to pursue, where one objective is harder to measure and therefore harder to incentivize. This raises questions around how incentives on an easily measurable dimension impacts effort on other dimensions. Our experiment captures these elements by presenting participants with a task that involves a quantity-quality trade-off, captured by the number of words spelled (quantity) and the points per word (quality). Indeed, a

[10]Because there was no strategic interaction between participants, the sessions did not require a particular number of participants to run. The number of participants varied across sessions, but the average number of participants per session is relatively constant across treatments.

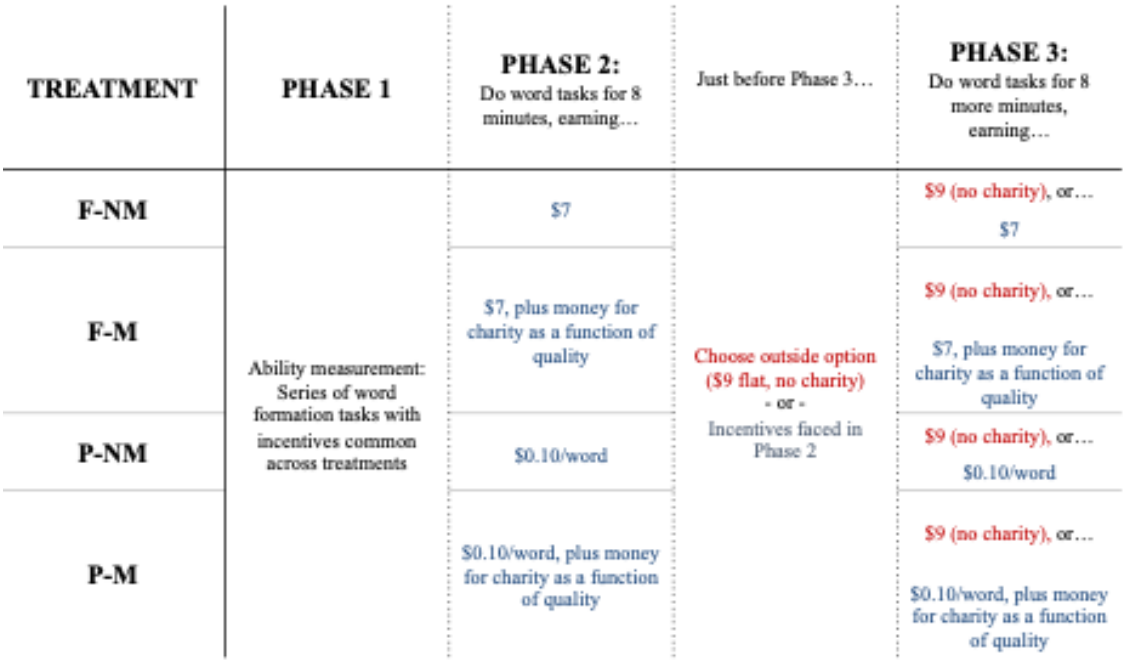

Figure 8: Summary of the Timeline of the Experiment

feature of the task is that it is easier/faster to form, for instance, two-letter words (out of the seven available letters) with more common letters than to form more difficult words that involve rarer letters and/or words that could be longer and would bear more points. The parallel here would be a short and superficial consultation with a doctor versus a longer and deeper consultation. Obviously, for the sake of providing evidence on these questions, we choose a task where the "quality" dimension *is* measurable.

In our mission treatments, to contribute to charity in a significant manner a participant needs to produce many words of sufficiently high quality. This is related to the sense of mission of, for instance, a doctor who cares about the well-being of patients and, therefore, tries to see as many patients as possible, compatible with having enough time to provide a high-quality treatment to each of them. Linking effort to charitable donations is a well-established methodology to elicit prosocial motivation (e.g., Tonin and Vlassopoulos, 2010, 2015; Imas, 2014; Charness et al., 2016; DellaVigna and Pope, 2018; Cassar, 2019) and has the advantage of keeping the task itself constant between the different treatments. It is, however, an indirect way to do so and it may be less salient than having own effort directly contributing to a socially valuable cause.

Furthermore, pay-for-performance may have different implications in terms of the type of workers it attracts. We address this issue in Phase 3 of our experiment by offering participants a choice between the payment scheme they experienced in Phase 2 or an outside option (flat wage, non-mission), which we chose to have a common anchor to gauge the sorting patterns of the various treatments. Besides, the features of the chosen outside option arguably characterize most of the employment opportunities in the private sector. In practice, of course, workers choose among jobs that are mission- or non-mission-oriented and offer fixed or variable payment schemes. However, we opted for a simpler pairwise choice to keep the experimental design manageable.

3.2.7 Prediction

This section presents a simple theoretical framework to generate theoretical predictions. Consider an agent who exerts effort on two dimensions $e = (e_1, e_2)$, where e_1 can be thought of producing quantity, while e_2 can be thought of producing quality. For simplicity, we assume that effort leads to outcomes linearly. Let us further assume that the cost of effort for the agent $c(e_1, e_2)$ is increas-

ing and strictly convex with $\frac{\partial^2 c(e_1,e_2)}{\partial e_1 \partial e_2} > 0$, that is, the two efforts are substitutes. Furthermore, we assume that $c(e_1, e_2)$ attains a local minimum at an interior point $e^* = (e_1^*, e_2^*)$, so that, even without incentives the agent will exert some positive effort in the two tasks.

Agents are heterogeneous in two dimensions: ability on the task, denoted by $\gamma \geq 0$, and prosociality, denoted by $\theta \geq 0$. We assume that ability enters the cost function multiplicatively, $\frac{1}{\gamma} c(e_1, e_2)$.

Agents facing flat-rate compensation therefore maximize:

$$w + m\theta e_2 - \tfrac{1}{\gamma} c(e_1, e_2)$$

where w is the flat monetary rate received regardless of effort and m is an indicator variable equal to 1 if the worker is mission-oriented.

Similarly, agents facing piece-rate compensation maximize:

$$pe_1 + m\theta e_2 - \tfrac{1}{\gamma} c(e_1, e_2)$$

where p is the piece-rate that dictates how effort on the quantity dimension is translated into payment.

We can use this simple framework to derive some predictions as to the relative levels of effort exerted in the various treatments, summarized in Table 16.

3.2.8 Phase 1

3.2.8.1 Flat-rate Non-Motivated (F-NM)

In the absence of any pay-for-performance in the non-motivated condition, the agent will choose $e^* = (e_1^*, e_2^*)$, given by $\frac{\partial c(e_1,e_2)}{\partial e_1} = \frac{\partial c(e_1,e_2)}{\partial e_2} = 0$. Effort on either dimension is not expected to vary with ability or prosociality. This treatment forms a baseline against which we can compare the other treatments below to form predictions.

3.2.8.2 Piece-rate Non-Motivated (P-NM)

In this treatment, the agent is offered a piece rate p, in the quantity dimension. The first-order conditions are given by $\frac{\partial c(e_1,e_2)}{\partial e_1} = \gamma p$ and $\frac{\partial c(e_1,e_2)}{\partial e_2} = 0$. Given that e_1 and e_2 are substitutes, it

follows that the agent will increase effort in the quantity dimension at the expense of quality and that effort in the quantity dimension will be increasing in ability. We thus predict that (proofs in the Appendix):

$$\textbf{Prediction 1a: } e_1^{FNM} < e_1^{PNM} \text{ and } e_2^{FNM} > e_2^{PNM}$$
$$\textbf{Prediction 1b: } \frac{de_1^{PNM}}{d\gamma} > 0$$

3.2.8.3 Flat-rate Motivated (F-M)

In this treatment, we assume that a prosocial agent will derive an intrinsic benefit from producing quality, which we denote by θe_2. The agent thus maximizes $\theta e_2 - \frac{1}{\gamma}c(e_1, e_2)$, with first-order conditions given by $\frac{\partial c(e_1,e_2)}{\partial e_1} = 0$ and $\frac{\partial c(e_1,e_2)}{\partial e_2} = \gamma\theta$. Given the substitutability of effort we thus predict that the agent will increase effort in the quality dimension at the expense of quantity:

$$\textbf{Prediction 2a: } e_1^{FM} < e_1^{FNM} \text{ and } e_2^{FM} > e_2^{FNM}$$
$$\textbf{Prediction 2b: } \frac{de_2^{FM}}{d\gamma} > 0$$

3.2.8.4 Piece-rate Motivated (P-M)

In this treatment, the agent receives a piece-rate (p) for quantity and derives an intrinsic benefit θ from producing quality. The agent thus maximizes $pe_1 + \theta e_2 - \frac{1}{\gamma}c(e_1, e_2)$, with first-order conditions given by $\frac{\partial c(e_1,e_2)}{\partial e_1} = \gamma p$ and $\frac{\partial c(e_1,e_2)}{\partial e_2} = \gamma\theta$. We thus predict that:

$$\textbf{Prediction 3a: } e_1^{PM} > e_1^{FM} \text{ and } e_2^{PM} < e_2^{FM}$$
$$\textbf{Prediction 3b: } \frac{de_1^{PM}}{d\gamma} > 0 \text{ and } \frac{de_2^{PM}}{d\gamma} > 0$$

We can also show that in the two piece-rate treatments effort in the quantity dimension is higher in the non-motivated condition than in the motivated one, and vice-versa for the quality dimension:

$$\textbf{Prediction 4: } e_1^{PNM} > e_1^{PM} \text{ and } e_2^{PNM} < e_2^{PM}$$

To summarize, we predict that effort in quantity will be highest under P-NM and lowest in F-M, while the reverse order will be true for effort in the quality dimension.

Finally, one additional object of interest is the *difference* in the impacts of performance pay across the motivated and non-motivated conditions, or the direction of the inequality in $[e_d^{PNM} - e_d^{FNM}]$ vs. $[e_d^{PM} - e_d^{FM}]$, in either efforts in quantity or quality (i.e., $d \in \{1, 2\}$). Our theoretical

model does not provide a firm prediction on the direction of this inequality without additional assumptions and structure; this is therefore left as an empirical question, further motivating the need for an experiment.

3.2.9 Phase 3

We next generate predictions about choice of payment scheme in Phase 3 (All proofs are in the Appendix).

Flat-rate Motivated (F-M): For a given level of ability γ sorting on prosociality type will take place, as agents with sufficiently high levels of prosocial motivation would choose F-M.

Piece-rate Non-Motivated (P-NM): There will be sorting on ability, that is, the proposed scheme is more likely to be chosen by high ability types.

Piece-rate Motivated (P-M): The proposed scheme is more likely to be chosen by high θ and/or γ agents. Note also that ability and motivation are substitutes for sorting, that is, a higher ability individual requires less motivation to sort and similarly a higher motivated individual requires less ability to find sorting in optimal.

3.3 Results

This section reports results from our experiment. We start by providing simple summary statistics. Table 15 provides a summary of participant characteristics (Table C.1.1 in Appendix C.1 presents a summary of these characteristics by treatment). We also report other survey and decision task responses in the table. Importantly for the sake of our real effort task, which hinges on spelling words, almost all participants are native English speakers. Females are also slightly overrepresented in our sample and almost half of the sample declared a business field as their major.

As we test for heterogeneity in participants' responses along both the ability dimension (using the composite ability measure described in the previous section) and along the prosocial motivation dimension (using the public service motivation – PSM – survey score), it is worth documenting the relationship between these two measures in our setting. Figure 9 graphically depicts the relationship between the measures, with Ability on the x-axis and the PSM Score on the y-axis.

The average score on the PSM survey, 0.69, is reported in Table 15; as noted there, higher scores indicate more public service motivation. The average of the ability measure is, by construction, zero. Figure 9 shows that there is substantial variation in both the PSM Score and the ability measure across participants. Importantly, there does not appear to be any clear relationship between the PSM Score and ability in our setting. Any potential correlation between ability and motivation obviously impacts the degree to which we may expect, and the degree to which it is desirable to observe, high ability workers to sort into the mission-oriented job with performance pay. The fact that these measures are independent in our setting helps us separately identify sorting of workers along these two dimensions.

The remainder of the section assesses differences in participant behavior across treatments. First, we will present the basic averages and the distributions of outcomes. Second, we will use regression analyses to statistically test for treatment effects in Phase 2 of the experiment in response to randomly assigned payment schemes; that is, we will study the "incentive effects" of performance pay in mission-oriented vs. non-mission-oriented tasks. Finally, we will turn to exploring the types of participants that opt for the randomly assigned payment scheme over a common outside option, assessing "sorting effects" of performance pay in our setting.

3.3.1 Averages of Outcomes Across Treatments

Before presenting the experimental results, for convenience Table 16 below summarizes the main theoretical predictions concerning Phase 2.

Table 17 reports the averages of our main outcome variables, total words spelled and points per word, across the four treatments, while Figure 10 depicts the full distribution.[11] As a point of reference, we note that the maximum number of words recorded is 136, the highest average points

[11]Table 17 reports outcomes from Phase 2, where participants are randomly assigned to a treatment. We report outcomes from Phase 3 in Appendix Table C.1.2.

per word is 9.27, and the maximum total points is 517. Three broad findings are readily observable from Table 3 and Figure 10.

First, effort on the incentivized dimension (quantity of words spelled) is much higher in the piece-rate non-motivated treatment (P-NM) than any other treatment. The number of words spelled is somewhat higher in the piece-rate motivated (P-M) treatment than the comparable mission setting with flat-rate (F-M), but the difference is much smaller than the difference between the two non-motivated treatments. We explore the statistical significance of these differences in the next subsection, but it is already clear that performance pay has a larger positive impact on the incentivized dimension of effort in the non-mission-oriented setting than in the mission-oriented environment.

Second, the quality of words spelled (as measured by the average number of points associated with each word) is lower in the P-NM treatment than in any other treatment and is relatively similar across the remaining three treatments, as can be seen in the second row of Table 16. In particular, "Points per word" is the simplest method of measuring average quality: we divide total points accumulated (even for words worth less than five points, which are worth zero points from the participant's perspective) by total words spelled. We also consider three alternative measures of quality: "Counted points per word", "Counted points per counted word", and "Share of words that are counted". Because patterns of results are similar across these different measures of quality, we report the simplest measure ("Points per word") in the main text moving forward and present the other measures in the Appendix (see Table C.1.4).

Recall that in the "motivated" treatments, more money for charity is generated by spelling words worth more points, while individual payment is fixed or depends solely on the number of words. This non-incentivized quality dimension of effort appears to be lowest in P-NM, where effort on the incentivized quantity dimension is highest, indicating that participants indeed faced a trade-off between quantity and quality. They acted on this trade-off to a greater degree in the P-NM treatment than in the P-M treatment, where quantity is higher than in F-M, but with no large drop in quality.

Finally, the last row of Table 3 reports the choice of payment scheme that participants opted for in Phase 3. In particular, we report the share of participants who opt for the treatment-assigned payment scheme (e.g., piece-rate with money raised for charity) over the common outside option

(flat-rate $9, with no opportunity to raise money for charity). Note that the $9 flat rate from the outside option is higher than the $7 flat rate from the F-M and F-NM treatment-assigned payment schemes. It is not surprising then to see that all but one of the participants in the F-NM treatment-assigned payment scheme preferred the outside option. More interesting is the fact that a large share of participants (54%) in the F-M treatment opted for the treatment-assigned payment scheme in Phase 3; that is, more than half of participants in that treatment were willing to accept a lower flat rate for the opportunity to raise money for charity. Lower rates of participants opted for the treatment-assigned payment schemes in the two piece-rate treatments. The rate of participants staying in the treatment-assigned scheme is relatively higher in P-M than in P-NM, which may not seem surprising given the differences in behavior, discussed above, in Phase 2 across the two treatments. In a later subsection, we explore *who* is opting for the treatment-assigned option and see that there are differences across the two treatments that run deeper than simply the number of participants choosing each option.

We further summarize our two key measures from Table 3 in distribution plots. In Figure 10 we provide kernel density plots of the distribution of words spelled and points per word, respectively. A central point we will return throughout the paper is evident in these figures: consistent with previous research, the distribution of effort and output in the non-motivated treatments is substantially different when performance pay is in place (P-NM vs. F-NM). The same cannot be said of the difference between performance pay and flat-rate treatments in the motivated setting (P-M vs. F-M). We formally test our theoretical predictions in the following section.

3.3.2 Assessing Incentive Effects in Phase 2

Table 18 presents regressions that allow us to test for statistical differences in effort on the incentivized (quantity) and non-incentivized (quality) dimensions across treatments in Phase 2. We simply regress an outcome (e.g., total words spelled in Column 1) on treatment indicator variables. F-NM is the omitted treatment, so all tests are relative to that treatment. The bottom of the table reports noteworthy tests of differences between other coefficients in the table (e.g., whether the P-M coefficient is significantly different than the F-M coefficient). To account for differences across the online and in-person experiment settings, we also include (but do not report) a dummy variable indicating whether the session was run online or not in all regressions in this

table and those that follow.[12] In Appendix Table C.1.3, we report similar regressions that include participant-level controls (gender, the Phase 1 ability measure, etc.). Appendix Table C.1.5 reports results from nonparametric tests of differences across treatments for our two main outcome variables (total words spelled and points per word). Results in both of these tables are consistent with the results reported in Table 18.

We frame this discussion around the theoretical predictions. Theoretical Prediction 1a speaks to the comparison between piece-rate and flat-rate payment in a non-mission-oriented setting. In our experiment, that is the comparison between treatments P-NM and F-NM. The theoretical prediction suggests that quantity should be higher in P-NM than F-NM, while quality should be lower as it is sacrificed to increase quantity. We find clear evidence in line with both parts of this prediction. Column 1 shows that participants in the P-NM treatment spell an additional 22.32 words relative to the F-NM treatment. Recall that the mean of "words spelled" in the F-NM treatment was 36.23, so piece-rate payment substantially increases the quantity of words spelled. The simple comparison of averages suggested that this increase in quantity comes at the expense of quality, and the statistical tests for differences in Table 18 bear that out: quality is significantly lower in P-NM.

Theoretical Prediction 2a focuses on the difference between flat-rate payment in mission-oriented vs. non-mission-oriented settings, or F-M and F-NM. We predicted higher quantity in F-NM, but higher quality in F-M. The basic intuition was that, in order to increase effort on quality, workers in F-M would have to reduce effort on quantity. Directionally, the estimated differences are consistent with these predictions, but the differences are statistically significant only in the case of the measure of quality.

Finally, Theoretical Prediction 3a focuses on the impact of performance pay in a mission-oriented setting. There, as in the non-mission-oriented setting, we predicted that performance pay would positively impact quantity but not quality. Here, the relevant test is the difference between the P-M and F-M coefficients, the p-value of which is reported in the bottom portion of the table. We do find evidence that quantity (Column 1) is higher in P-M than F-M, with P-M participants spelling 6.95 additional words on average (p-value = 0.01). While this is consistent with our theoretical prediction, it is worth noting that this response is substantially more muted

[12]We pool observations across settings as participants are randomly allocated to treatments within each sample and differential behaviour across samples does not affect treatment effects.

than in the non-mission-oriented treatments. There, as we noted above, performance pay led to an additional 22.32 words. We observe a similar phenomenon with respect to the quality of the words: P-M participants spell words worth 0.34 fewer points than F-M participants. Although this difference is significant (p-value= 0.02), the magnitude of the effect of performance pay is much smaller than the parallel comparison between P-NM and F-NM, where points per word were 0.72 points lower with performance pay. Thus, while there is evidence in favor of the theoretical prediction, the more notable finding is that the effects of performance pay are much smaller in the mission-oriented setting than in the non-mission-oriented setting.[13]

To put our main results differently: our experiment tests whether the impacts of performance pay are different in a mission-oriented setting relative to a non-mission-oriented setting. The key takeaways from Table 18 could therefore instead be drawn from interpreting the analysis in a difference-in-differences framework, assessing the *differential* impact that performance pay has on workers in mission-oriented setting, or the difference between "PM-FM" and "PNM-FNM". In that framework, our findings can be summarized as follows: performance pay increases output on "Total words" by 15.4 *fewer* words when workers are exposed to the mission treatment relative to those who are not (see bottom of Table 18). Likewise, the negative effect of performance pay on "points per word" (-0.72 for P-NM participants) is 0.39 points *less* negative when workers are motivated. The diff-in-diff coefficient is statistically significant for Total words (p-value$<$ 0.01) but not for Points per word (p-value= 0.11). In short, performance pay has a less positive effect on the incentivized dimension when workers are motivated, but also a less negative effect on unincentivized dimensions.

Table 19 reports heterogeneity in response to performance pay by worker ability and prosocial motivation, drawing on the ability measure we construct from participants' Phase 1 performances and on their PSM scores.[14] We modify the specification reported in Table 18, interacting treatment indicators with our constructed ability measure and the PSM score. We are mainly interested in assessing predictions related to how ability and prosociality impact performance in the two dimensions.

Column 1 shows that higher ability workers spell more words: a one standard deviation increase

[13]We note that the incentive effect we measure on mission-oriented job is for a randomly chosen worker and not for one that has already sorted into the sector.

[14]In Appendix Table C.1.6 we present heterogeneity analysis by Ability and Prosociality, using Dictator Donation Share as measure of prosociality instead of PSM score.

in ability leads to 9.56 additional words in the F-NM treatment. We do not find any differential impact of ability in any of the other treatments, as none of the interaction terms are statistically significant. That performance increases in ability in the piece rate treatments is consistent with Predictions 1b and 3b and echoes Lazear (2000) who notes that "a piece rate allows the more able to work harder and receive more from the job". That performance also increases in ability in the flat wage treatments can perhaps be attributed to the fact that our measure of ability also captures some intrinsic interest/joy in performing the task. With respect to prosociality, we also see in column 1 that it does not play a significant role in terms of quantity produced. Note that our model does not predict an influence of prosociality on quantity produced.

Turning attention to predictions concerning performance on the quality dimension for treatments involving a mission, column 2 shows no effect of ability and prosociality in F-M (Prediction 2b), and contrary to Prediction 3b a negative impact of ability and prosociality in performance in P-M, with the interaction terms being significant only at the 10% level.

To summarize, we have documented that – at least with respect to incentive effects – performance pay has a substantially smaller positive effect on the incentivized dimension of effort (quantity) when workers are in our experimental mission-oriented sector compared to the case without mission. We also document that the large positive effect of performance pay on the incentivized dimension in the non-mission-oriented treatments comes at the expense of quality (a non-incentivized dimension of effort). While a similar pattern is found in the mission-oriented treatments, the negative effect on quality is much smaller. Thus, performance pay is less successful in increasing effort on the incentivized dimension when workers are motivated, but also does less harm to other dimensions of effort.

3.3.3 Assessing Sorting Effects in Phase 3

We now turn to assessing the types of participants who opt for performance pay when given the choice and how these sorting decisions differ depending on whether the work is in the mission-oriented or non-mission-oriented sector. Recall that Phase 3 consists of an additional eight minutes of the real effort task, but unlike Phase 2 we give participants the option to either: continue to work under the treatment-assigned payment scheme, or switch to a common outside option ($9 flat-rate payment with no funds raised for charity).

Table 20 reports results from regressions where we test for differences across treatments in the characteristics of workers who prefer the treatment-assigned payment scheme over the common outside option. The simplest regression (Column 1) regresses a dummy variable indicating that the participant chose the treatment-assigned payment scheme on a set of treatment indicator variables with F-NM (where only one participant opted for the clearly inferior treatment-assigned scheme) again serving as the omitted category and we also include our measures of ability and prosociality as controls. In the non-mission-oriented treatments (P-NM vs. F-NM), the regression reveals that the share of participants opting for the treatment-assigned payment scheme is significantly higher when the treatment-assigned scheme is a piece-rate. The opposite is true in mission-oriented treatments (P-M vs. F-M) where a relatively smaller share of participants opt for the treatment-assigned scheme when it pays a piece-rate, although this difference is insignificant. In other words, the results suggest that performance pay increases the attractiveness of jobs in non-mission-oriented settings but does not affect the attractiveness of jobs in mission-oriented settings, relative to an outside option that pays a high fixed wage with no mission.

Because we are interested in the types of workers who sort into performance pay, our focus in the remainder of the table is in understanding how ability and prosocial motivation (as measured by the Phase 1 ability score and PSM score respectively, both standard-normalized) dictate the choice of the treatment-assigned option, especially in the P-M and P-NM treatments. In Column 2, both of these measures are fully interacted with treatment indicators to test how ability and prosociality differentially impact the decision to opt for the treatment-assigned payment scheme across treatments.

Notably, the decision to opt for the treatment-assigned scheme is only meaningfully impacted by ability when the treatment-assigned scheme is P-NM or P-M, that is, piece-rate with or without charity. In P-NM, workers who are one standard deviation higher in ability are 19 percentage points more likely to choose to work under the piece-rate scheme, while in P-M this figure is smaller, at 11 percentage points. Aside from the multitasking element, P-NM most closely mirrors the type of setting where performance pay has typically been studied, as in Lazear (2000) for instance. Lazear (2000) theoretically predicts and empirically documents that the most productive workers will be attracted to performance pay, which provides the beneficial "sorting effect" of performance pay. Our theoretical model makes the same prediction (predicting that higher ability workers are more

likely to choose the performance pay scheme if given the choice) and the results reported in the table support that prediction. There is no evidence of sorting on ability in F-M or F-NM, but there is also no reason to expect that there should be.

Turning attention to prosociality, PSM is only a significant determinant of choosing the treatment -assigned option in the F-M treatment. Workers who are one standard deviation higher in their PSM score are 12 percentage points more likely to opt for the $7 flat-rate with charity than the $9 flat-rate without. While the P-M X PSM coefficient is positive, it is not significant, thus, there is little evidence of sorting on the basis of public service motivation in this treatment. On the other hand, there is sorting based on motivation when the choice is between a fixed pay without mission and a (lower) fixed pay with mission.

To summarize, we find that high ability workers self-select into the piece rate compensation scheme in both mission and non-mission settings, therefore generating an additional positive effect of P4P on productivity due to sorting (Lazear, 2000). Also, in the environment with a mission, we find that with fixed pay there is sorting on prosociality, whereas with P4P this is not the case.

3.4 Conclusion

We carry out a real effort lab experiment to address the following two questions: (i) How does performance pay impact productivity on incentivized and non-incentivized dimensions when workers are in a sector with a mission? (ii) How does performance pay impact the composition of workers in jobs with a prosocial dimension? We first show that, in absence of a mission, performance pay has strong positive effects on productivity on the incentivized dimension (quantity) and negative effects on the non-incentivized dimension (quality). When a mission is present, however, the effect of performance pay is much more subdued, with much smaller changes on either dimension. Further, when workers can choose to remain in the experimentally-assigned payment scheme or opt for an outside option with flat payment and no mission, we find that workers sort on ability, with lower ability workers opting out of the P4P scheme and this takes place in both sectors. In the sector with a mission, sorting also takes place along the motivation dimension when the choice is between a fixed wage without mission and a (lower) fixed wage with mission. That is, people with high public

service motivation are willing to give up financial gains to be able to contribute to the mission.

How does this inform the debate on whether or not to introduce performance pay in sectors with a strong mission, like education or health care? On one side, our results suggest that it may be misleading to extrapolate the experience about high-powered incentives coming from standard for-profit sectors of the economy to sectors where motivation plays a more important role. For instance, those pushing for the adoption of P4P for teachers on the basis of its beneficial effects on productivity in the manufacturing sector may miss their target. On the other hand, our results also counter the argument that introducing pay-for-performance may have ruinous effects in a mission environment characterized by multitasking. Returning to the example of the educational sector, our findings do not support arguments that with bonuses based on test scores teachers will be teaching solely to the test. We find that, when mission is present, the reallocation of effort away from the unincentivized dimension into the incentivized one is subdued. Regarding selection, by introducing pay-for-performance a mission sector may attract more skilled workers but give up the selection on motivation we observe with fixed pay. These results are of course related to the fact that, as shown in Figure 9, motivation and ability are uncorrelated in our sample and this may not be the case in all contexts.

More generally, what we have shown is that the effects of pay-for-performance are not independent of mission. Therefore, whenever prosocial motivation matters, these two types of incentives should be studied together. While the lab environment allows for clean measurement and identification, it has clear limitations when addressing issues relevant to complex workplaces like schools or hospitals. Before drawing firm policy implications, it is therefore important to build a broader knowledge base, including field studies whose design can be informed by the results presented here.

Table 15: Summary Statistics – Participant Characteristics

Female	0.57	Any monetary donations?	0.69
	(0.50)	(past year)	(0.46)
Age	19.67	Any goods donations?	0.77
	(1.76)	(past year)	(0.42)
Year in College	2.30	Any volunteering?	0.68
	(1.05)	(past year)	(0.47)
Business Major	0.46	Any blood donations?	0.27
	(0.50)	(past year)	(0.45)
Native English?	0.92	Holt-Laury: Share of	0.50
	(0.27)	risky choices	(0.19)
Currently Works	0.65	Dictator: Share contributed	0.45
	(0.48)		(0.31)
Current Wage	8.50	PSM Survey score	0.69
(cond'l on current work)	(12.28)	(0-1, higher=more PSM)	(0.09)
Observations: 244			

Notes: Standard deviations in parentheses.

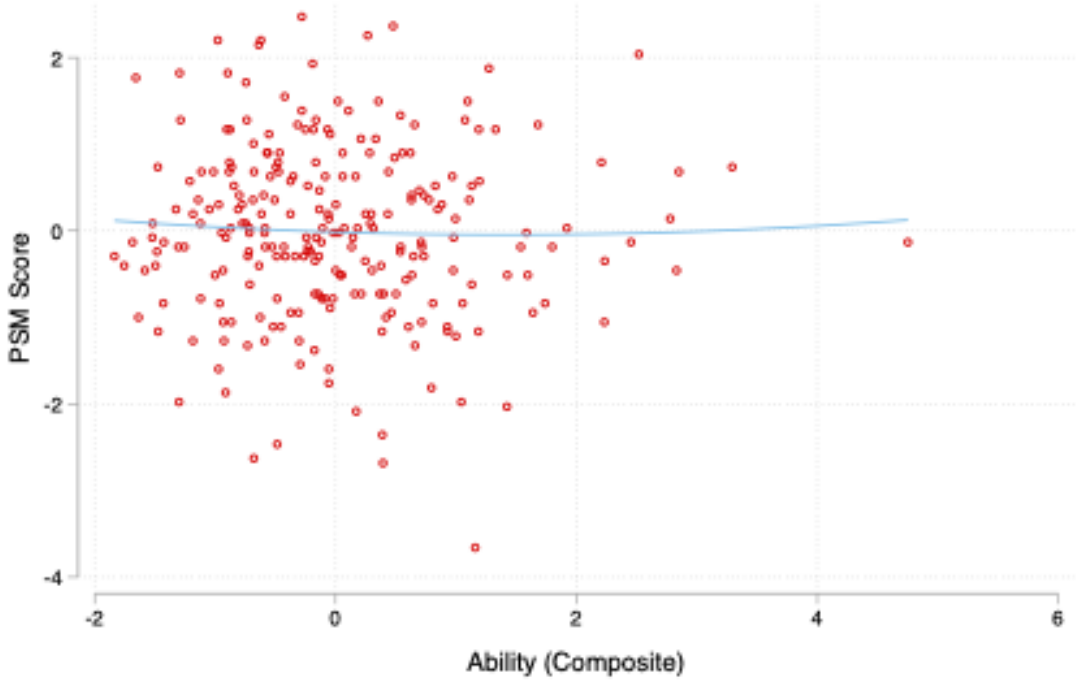

Figure 9: Scatterplot Depicting Relationship Between Ability and Public Service Motivation

Note: The solid blue line is a quadratic fit.

Table 16: Summary of Effort Predictions in Phase 2

Prediction 1a, Prediction 3a	$e_1^{PNM} > e_1^{FNM}$, $e_1^{PM} > e_1^{FM}$
Prediction 2a, Prediction 4	$e_2^{FM} > e_2^{FNM}$, $e_2^{PM} > e_2^{PNM}$
Prediction 1b, Prediction 3b	$\frac{de_1^{PNM}}{d\gamma} > 0$ and $\frac{de_1^{PM}}{d\gamma} > 0$
Prediction 2b, Prediction 3b	$\frac{de_2^{FM}}{d\gamma} > 0$ and $\frac{de_2^{PM}}{d\gamma} > 0$
Unnumbered – immediate from framework	$\frac{de_2^{FM}}{d\theta} > 0$ and $\frac{de_1^{PM}}{d\theta} > 0$

Notes: The comma separating two quantities indicates that they cannot be unambiuously signed.

Table 17: Averages of Outcome Variables by Treatment

Treatment:	P-NM	F-NM	P-M	F-M
Phase 2: Words Spelled	58.44	36.23	40.84	33.89
	(3.51)	(1.92)	(2.42)	(1.41)
Phase 2: Points per Word	5.70	6.42	6.43	6.77
	(0.15)	(0.14)	(0.10)	(0.10)
Phase 2: Earnings	5.84	7.00	4.08	7.00
	(0.35)	(0.00)	(0.24)	(0.00)
Phase 2: Charitable contribution	0.00	0.00	4.49	4.15
	(0.00)	(0.00)	(0.21)	(0.16)
Phase 3: Chose treatment-	0.38	0.02	0.49	0.54
assigned payment?	(0.06)	(0.02)	(0.06)	(0.06)
Observations	61	61	61	61

Notes: Standard deviations in parentheses.

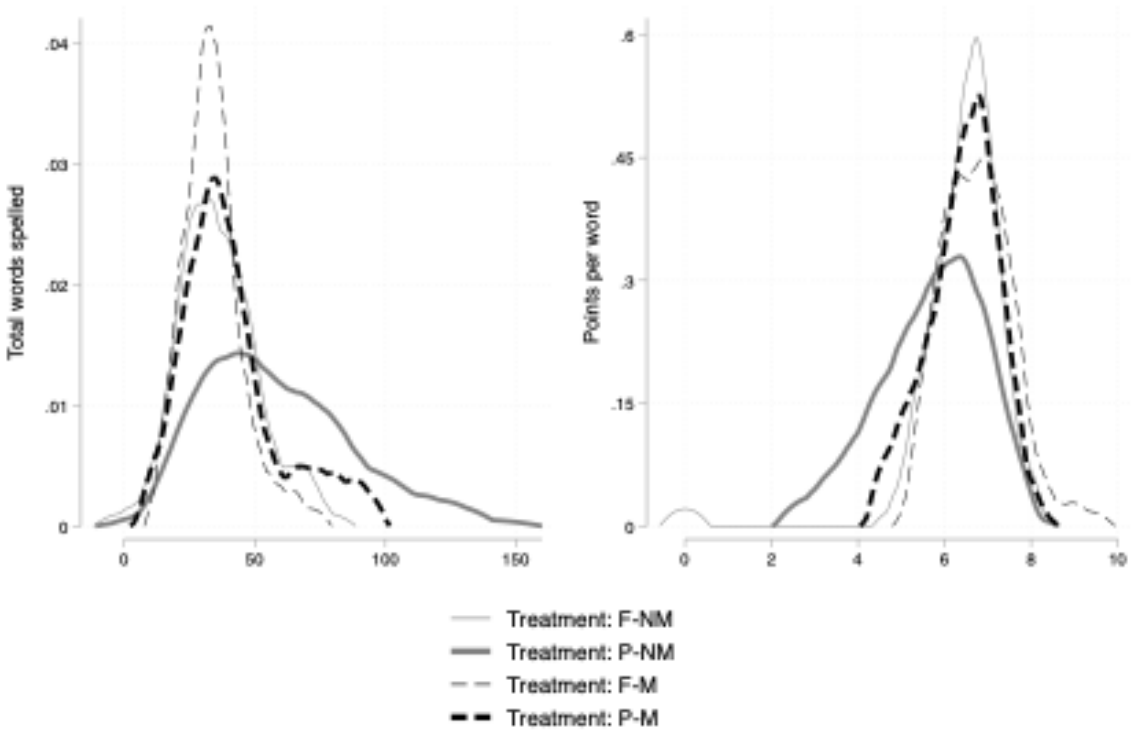

Figure 10: Kernel Density Distribution Plots of Phase 2 Outcomes

Table 18: Testing Differences in Phase 2 Outcomes Across Treatments

Variables:	(1) Total Words	(2) Points per Word
Treatment: F-NM	(omitted)	(omitted)
Treatment: F-M	-2.24	0.34**
	(2.35)	(0.17)
Treatment: P-NM	22.32***	-0.72***
	(4.00)	(0.20)
Treatment: P-M	4.71	-0.00
	(3.06)	(0.17)
Observations	244	244
R-squared	0.21	0.16
P-M vs. F-M p-val.	0.01	0.02
P-M vs. P-NM p-val.	0.00	0.00
DinD est. (PM-FM)-(PNM-FNM)	-15.37	0.39
DinD p-val.	0.00	0.11

All columns include an indicator variable for whether the data was collected online. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 19: Heterogeneity in Phase 2 Outcomes by Ability and Prosocial Motivation

	(1)	(2)
Variables:	Total Words	Points per Word
Treatment: F-NM	(omitted)	(omitted)
Treatment: F-M	-3.14*	0.33**
	(1.78)	(0.16)
Treatment: P-NM	21.68***	-0.74***
	(3.41)	(0.19)
Treatment: P-M	4.96**	-0.00
	(2.21)	(0.15)
Pub. Srvc. Motivation (PSM)	2.79*	0.14
(mean 0, std. dev. 1)	(1.56)	(0.13)
Treatment: F-M X PSM	-3.52*	-0.05
	(1.81)	(0.14)
Treatment: P-NM X PSM	-6.57	0.01
	(4.23)	(0.21)
Treatment: P-M X PSM	-1.36	-0.30*
	(2.46)	(0.16)
Phase 1 Ability Measure	9.56***	0.16
(mean 0, std. dev. 1)	(1.50)	(0.10)
Treatment: F-M X Ability	-1.52	-0.21
	(2.01)	(0.15)
Treatment: P-NM X Ability	4.65	-0.40*
	(3.35)	(0.21)
Treatment: P-M X Ability	3.32	-0.21*
	(2.11)	(0.13)
Observations	244	244
R-squared	0.51	0.19

All columns include an indicator variable for whether the data was collected online. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 20: Heterogeneity in Phase 3 Outcomes by Ability and Prosocial Motivation

	(1)	(2)
Variables:	Inside Option	Inside Option
Treatment: F-NM	(omitted)	(omitted)
Treatment: F-M	0.52***	0.53***
	(0.07)	(0.07)
Treatment: P-NM	0.35***	0.36***
	(0.06)	(0.06)
Treatment: P-M	0.47***	0.48***
	(0.07)	(0.07)
Pub. Srvc. Motivation (PSM)	0.04	-0.00
(mean 0, std. dev. 1)	(0.03)	(0.01)
Treatment: F-M X PSM		0.12***
		(0.05)
Treatment: P-NM X PSM		-0.05
		(0.06)
Treatment: P-M X PSM		0.03
		(0.07)
Phase 1 Ability Measure	0.09***	0.01
(mean 0, std. dev. 1)	(0.03)	(0.01)
Treatment: F-M X Ability		0.03
		(0.07)
Treatment: P-NM X Ability		0.19***
		(0.06)
Treatment: P-M X Ability		0.11**
		(0.05)
Observations	244	244
R-squared	0.22	0.27
P-M vs. F-M p-val.	0.60	
P-M vs. P-NM p-val.	0.17	
DinD est. (PM-FM)-(PNM-FNM)	-0.40	
DinD p-val.	0.00	

All columns include an indicator variable for whether the data was collected online. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix A The Gift of Giving: Recognizing Donors and Revealing Donation Amounts

A.1 Experiment Instructions

A.1.1 Other Sessions

Welcome to the Experiment

Thank you for participating in our study! This is an experiment about decision-making. The other people in today's session are also participating in the experiment. You must not talk to or communicate with them in any way. If you have a question, please raise your hand and a researcher will answer your question in private.

This experiment will consist of two parts, Part A and Part B. Instructions will be provided before each part. All participants will receive a $6 show-up fee. You can earn additional amounts depending on the decisions you make in the experiment and luck.

At the top of your station you will see a card holder with a number. This number is your Participant Number. Your Participant Number will be used to pay you at the end of this session.

[*break*]

Part A Instructions

In Part A of the experiment, you will be asked to calculate the sum of 6 randomly chosen one-digit numbers. You will be asked to complete 10 of these problems. You should find the sums without using a calculator. You submit an answer by clicking the submit button with your mouse. When you enter an answer, the computer will immediately tell you whether your answer is correct or not. For completing this task, you will earn $18 that you may use during the rest of the experiment. When you correctly solve 10 of these problems, you will automatically proceed to the next stage of the experiment.

[*break*]

Part B Instructions

In Part B you will have the opportunity to use your $18 from Part A to donate to charity. Part B will consist of two rounds. In each round, you will make three donation decisions, one to each of three different charities. For each decision, you will decide how much of your $18 to donate to that charity and how much to keep. We will implement only one of your decisions.

At the end of the experiment we will randomly decide which of the three charities will receive donations from today's session. Half of the participants in today's session will be randomly selected to have their Round-1 decision implemented for the selected charity. The other half of participants will have their Round-2 decision implemented for the selected charity. You will keep what remains of your $18 after we implement your selected decision.

Round 1

On the next page you will learn about three local charities and their missions. The charities are, in alphabetical order: Animal Friends, the Greater Pittsburgh Community Food Bank, and the Women's Center and Shelter of Greater Pittsburgh. The three charities are providing critical resources to the Pittsburgh community during these challenging times. After you have read about each charity, you will be asked to make your decisions. For each charity, you will be asked how much of your $18 from Part A you wish to keep and how much you wish to donate. If one of your Round-1 decisions is randomly selected to be implemented, your decision will be kept private. It will not be shown to any other participant in today's session. Since donations to any charity may be selected to count for payment, you should treat each decision as if it is the one that will be implemented. You can use your full $18 on any given decision since only one counts for payment.

Donations must be made in increments of $2. Every donation will be matched "one-for-one" by a research foundation, i.e., a $2 donation will be matched with an additional $2 to make a $4 donation. If you would like to see a receipt of the total donation made to the three charities, you may email the researcher at kpw18@pitt.edu. If you would like an individual receipt for your

donation, we will provide instructions on how to do so at the end of the session.

[*break*]

End of Part B, Round 1

Before we begin round 2, we would like you to think of three individuals in your life who you believe would be most likely to support each of the three charities you learned about in round 1. On the next page, once you have thought of those three individuals, you will be asked to enter their names in a table next to each charity. Please only write their first name in order to preserve your anonymity during the experiment. In round 2, you may have the opportunity to donate on behalf of the people you name.

[*break*]

Round 2

In round 2, you will again make donation decisions to the three charities you learned about in round 1. As mentioned before, at the end of the experiment we will randomly select one charity to receive donations made during this session and will implement one of your decisions made to that charity. Half of the participants in today's session will have their Round-1 decision implemented while the other half will have their Round-2 decision implemented to that charity.

For each charity, you will again decide how much of your $18 from Part A you wish to keep and how much you wish to donate. Donations must be made in increments of $2, and you can use up to the full $18 for each decision. Donations will again be matched "one-for-one" by a research foundation. Because each decision is equally likely to be selected, you should treat each decision as if it is the one that will be implemented.

For your Round-2 decisions, for each charity you may choose to make a donation on behalf of the person you named earlier as most likely to support the organization. If you choose to do so and your Round-2 decision is implemented, the researcher will provide you a card that you will

mail to notify them about your donation. The card will tell them that you donated on their behalf, describe the charity that you donated to, and include researcher contact information.

[*Subsequent text depends on experimental treatment*]

[*ID*:] The card will not include how much you donated.

[*ID&Amount*:] The card will also include how much you donated, and that the donation was made out of $18 from Part A. Finally the card will include the total amount the charity receives after the "one-for-one" match by a research foundation.

[*Choose Info*:] You may also choose whether the card will include how much you donated. If you choose to include how much you donated, the card will state that the donation was made out of $18 from Part A and include the total amount the charity receives after the "one-for-one" match by a research foundation.

[*Instructions return to standard language across treatments.*]

The researchers will provide the card at the end of the experiment, along with an envelope with postage that you will place the card in and address to the person you named. The researchers will then take the envelopes to the post office. No card is mailed if you do not make a positive donation or if you decide not to donate on behalf of the person you named.

An example of the card that will be sent to the person you donate on behalf of is provided below. In the example card, the randomly selected charity is the Greater Pittsburgh Community Food Bank. The example card includes a placeholder for where you can personalize the card by including the person's name.

[*Subsequent text depends on experimental treatment*]

[*ID*:] An example of the card that will be sent to the person you donate on behalf of is provided below. In the example card, the randomly selected charity is the Greater Pittsburgh Community Food Bank. The example card includes a placeholder for where you can personalize the card by including the person's name.

[*ID&Amount*:] An example of the card that will be sent to the person you donate on behalf of is provided below. In the example card, the randomly selected charity is the Greater Pittsburgh Community Food Bank. The example card includes a placeholder for where you can personalize the card by including the person's name. The example card also marks where the out-of-pocket donation amount will be listed, and where the total donation amount after the one-for-one match will be listed.

[*Choose Info*:] Examples of the cards that will be sent to the person you donate on behalf of are provided below. The first is an example of the card that will be sent if you choose not to include how much you donated. The second is an example of the card that will be sent if you choose to include how much you donated. In the example cards, the randomly selected charity is the Greater Pittsburgh Community Food Bank. The example cards include a placeholder for where you can personalize the card by including the person's name. The second example also marks where the out-of-pocket donation amount will be listed, and where the total donation amount after the one-for-one match will be listed, if you choose to include how much you donated.

[*Example cards shown*]

[*Instructions return to standard language across treatments.*]

You will not mail a card to someone if your Round-1 decision is implemented, or if in Round 2 you opted not to donate on behalf of someone else.

A.1.2 Self Sessions

Welcome to the Experiment

Thank you for participating in our study! This is an experiment about decision-making. The other people in today's session are also participating in the experiment. You must not talk to or communicate with them in any way. If you have a question, please raise your hand and a researcher will answer your question in private.

This experiment will consist of two parts, Part A and Part B. Instructions will be provided before each part. All participants will receive a $6 show-up fee. You can earn additional amounts depending on the decisions you make in the experiment and luck.

At the top of your station you will see a card holder with a number. This number is your Participant Number. Your Participant Number will be used to pay you at the end of this session.

We may use your Participant Number to reveal some of your decisions to others in today's session. Prior to making a decision, you will be informed whether you and your decision will be revealed and, if so, to whom.

[*break*]

Part A Instructions

In Part A of the experiment, you will be asked to calculate the sum of 6 randomly chosen one-digit numbers. You will be asked to complete 10 of these problems. You should find the sums without using a calculator. You submit an answer by clicking the submit button with your mouse. When you enter an answer, the computer will immediately tell you whether your answer is correct or not. For completing this task, you will earn $18 that you may use during the rest of the experiment. When you correctly solve 10 of these problems, you will automatically proceed to the next stage of the experiment.

[*break*]

Part B Instructions

In Part B you will have the opportunity to use your $18 from Part A to donate to charity. Part B

will consist of two rounds. In each round, you will make three donation decisions, one to each of three different charities. For each decision, you will decide how much of your $18 to donate to that charity and how much to keep. We will implement only one of your decisions.

At the end of the experiment we will randomly decide which of the three charities will receive donations from today's session. Half of the participants in today's session will be randomly selected to have their Round-1 decision implemented for the selected charity. The other half of participants will have their Round-2 decision implemented for the selected charity. You will keep what remains of your $18 after we implement your selected decision.

Round 1

On the next page you will learn about three local charities and their missions. The charities are, in alphabetical order: Animal Friends, the Greater Pittsburgh Community Food Bank, and the Women's Center and Shelter of Greater Pittsburgh. The three charities are providing critical resources to the Pittsburgh community during these challenging times. After you have read about each charity, you will be asked to make your decisions. For each charity, you will be asked how much of your $18 from Part A you wish to keep and how much you wish to donate. If one of your Round-1 decisions is randomly selected to be implemented, your decision will be kept private. It will not be shown to any other participant in today's session. Since donations to any charity may be selected to count for payment, you should treat each decision as if it is the one that will be implemented. You can use your full $18 on any given decision since only one counts for payment.

Donations must be made in increments of $2. Every donation will be matched "one-for-one" by a research foundation, i.e., a $2 donation will be matched with an additional $2 to make a $4 donation. If you would like to see a receipt of the total donation made to the three charities, you may email the researcher at kpw18@pitt.edu. If you would like an individual receipt for your donation, we will provide instructions on how to do so at the end of the session.

[*break*]

Round 2

In round 2, you will again make donation decisions to the three charities you learned about in round 1. As mentioned before, at the end of the experiment we will randomly select one charity to receive donations made during this session and will implement one of your decisions made to that charity. Half of the participants in today's session will have their Round-1 decision implemented while the other half will have their Round-2 decision implemented to that charity.

For each charity, you will again decide how much of your $18 from Part A you wish to keep and how much you wish to donate. Donations must be made in increments of $2, and you can use up to the full $18 for each decision. Donations will again be matched "one-for-one" by a research foundation. Because each decision is equally likely to be selected, you should treat each decision as if it is the one that will be implemented.

If your Round-2 decision is implemented, you will learn who donated in Round 2.

[*Subsequent text depends on experimental treatment*]

That is, you will see the Participant Numbers of those who donated *[*ID*: but not how much they donated.] [*ID&Amount*: and how much they donated.] [*Choose Info*: If you make a donation in Round 2 that is implemented, you may also choose whether others will learn how much you donated in Round 2. That is, you may also see the amount that others donated if they choose to reveal this information.]* Similarly, other participants who had their Round-2 decision implemented *[*ID*: will learn if you donated but not how much you donated.] [*Reveal*: will learn if you donated and how much you donated.] [*Choose Info*: may also learn the amount you donated if you choose to reveal this information.]* We will do this by showing a list of the Participant Numbers *[*ID&Amount*: along with donation amounts] [*Choose Info*: (and donation amounts for those who choose to reveal this information)]* for participants who made a donation in Round 2 to all participants who had their Round-2 decision implemented.

If you did not donate in Round 2 or if your Round-2 decision was not implemented, then your Participant Number *[*ID&Amount*: and donation amounts]* will not be revealed to others.

An example of the donor information that will be shown is provided below. This information will be shown at the end of the session to the participants who had their Round-2 decision implemented. The leftmost column lists Participant Numbers, which match the numbers on the cards on top of your computer stations. *[*ID&Amount*: The middle column shows donation amounts before the one-for-one research foundation match. The rightmost column shows the total donation amount after the one-for-one match.] [*Choose Info*: The middle column shows donation amounts before the one-for-one research foundation match (if participants chose to reveal the amount). The rightmost column shows the total donation amount after the one-for-one match (if participants chose to reveal the amount).* The participants included in this table are those who had their Round-2 decision implemented and donated a positive amount to the selected charity.

[*Example table shown*]

[*Instructions return to standard language across treatments.*]

The Participant Numbers *[*ID&Amount*: and donation amounts]* of those who did not donate in Round 2 or who had their Round-1 decision implemented are not included in this table.

A.2 Experiment Design & Materials

A.3 Additional Discussion of Decision-Making Framework

The proofs for **Proposition 1** and **Proposition 2** are immediate from the discussion in the main text of Section 1.2 preceding each proposition.

Proposition 3 Proof: The individual rationality and incentive compatibility constraints for arbi-

Table A.2.1: Description of Experimental Treatments

Treatment	Participants	Features
ID	80	Do Not Reveal Donation Amount (D2)
ID&Amount	80	Reveal Donation Amount (D2)
Choose Info	80	Choice to Reveal Donation Amount (D2)
Total	240	

Notes: In each of *ID*, *ID&Amount*, and *Choose Info*, half of participants were in *Other* sessions while the other half were in *Self* sessions. Therefore for each of *ID*, *ID&Amount*, and *Choose Info*, 40 participants were in *Other* sessions and 40 were in *Self* sessions.

trary type α_i are given by:

$$\ln(\omega - \hat{d}_i) + \alpha_i \ln(1 + \hat{d}_i) + s(\alpha_i) \geq \ln(\omega), \quad (IR) \tag{5}$$

$$\ln(\omega - \hat{d}_i) + \alpha_i \ln(1 + \hat{d}_i) + s(\alpha_i) \geq \ln(\omega - \hat{d}_{i+1}) + \alpha_i \ln(1 + \hat{d}_{i+1}) + s(\alpha_{i+1}), \quad (IC1) \tag{6}$$

$$\ln(\omega - \hat{d}_i) + \alpha_i \ln(1 + \hat{d}_i) + s(\alpha_i) \geq \ln(\omega - \hat{d}_{i-1}) + \alpha_i \ln(1 + \hat{d}_{i-1}) + s(\alpha_{i-1}), \quad (IC2) \tag{7}$$

IR is satisfied for the selfish type given normalization $s(0) = 0$. For all non-selfish types, if IR is satisfied for the selfish type and payoffs are increasing in types, then IR is satisfied for all types α_i.

The incentive compatibility constraint for the selfish type (IC1 only) is satisfied assuming $**$ holds. Define $\hat{d}_i$ as the sequence of non-selfish type donations in the fully separating equilibrium.[1] Now suppose IC2 is binding for type i. For the fully separating equilibrium to hold, it must be that agents with type $i-1$ do not want to mimic agents with type i. This condition is IC1 for agent type $i-1$ but with strict difference.

$$\ln(\omega - \hat{d}_i) + \alpha_{i-1} \ln(1 + \hat{d}_i) + s(\alpha_i) < \ln(\omega - \hat{d}_{i-1}) + \alpha_{i-1} \ln(1 + \hat{d}_{i-1}) + s(\alpha_{i-1}) \tag{8}$$

[1]The sequence $\hat{d}_i$ can be constructed by iteration when setting IC1 to equality.

Combining equations 7 and 8, IC1 and IC2 are satisfied when:

$$\alpha_i > \alpha_{i-1}. \tag{9}$$

This condition is always true. The same condition in equation 9 applies when IC1 is binding and IC2 must be satisfied with strict difference. This holds for arbitrary non-selfish type i. □

A.4 Additional Results

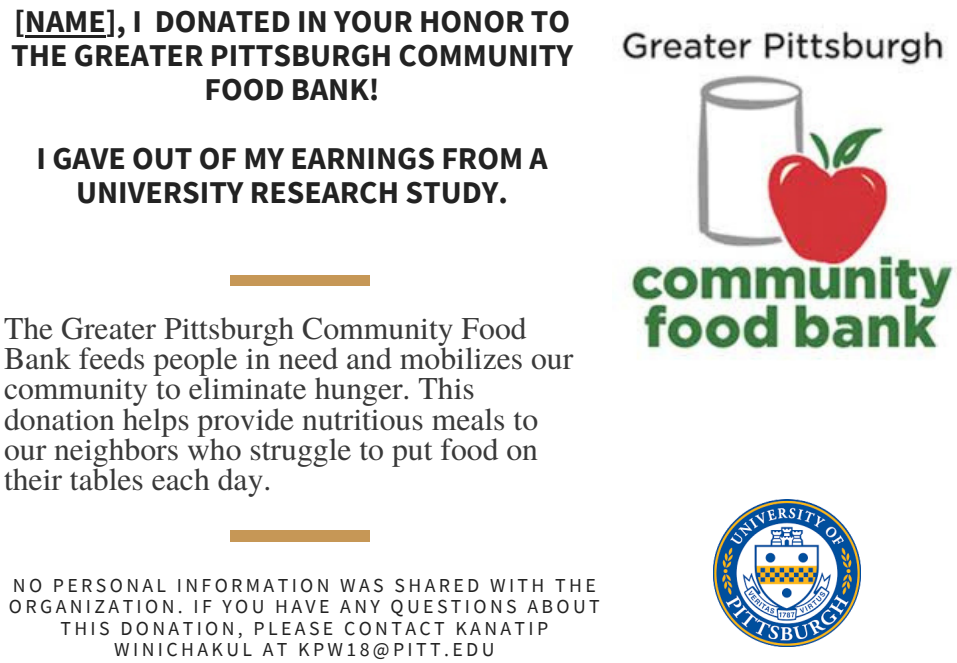

Figure A.2.1: Example ID Treatment Card

[NAME], I DONATED IN YOUR HONOR TO THE GREATER PITTSBURGH COMMUNITY FOOD BANK!

I GAVE $[AMOUNT] OUT OF $18 I EARNED FROM A UNIVERSITY RESEARCH STUDY. MY DONATION WAS MATCHED FOR A TOTAL DONATION OF $[AMOUNT].

The Greater Pittsburgh Community Food Bank feeds people in need and mobilizes our community to eliminate hunger. This donation helps provide nutritious meals to our neighbors who struggle to put food on their tables each day.

NO PERSONAL INFORMATION WAS SHARED WITH THE ORGANIZATION. IF YOU HAVE ANY QUESTIONS ABOUT THIS DONATION, PLEASE CONTACT KANATIP WINICHAKUL AT KPW18@PITT.EDU

Figure A.2.2: Example ID&Amount Treatment Card

Table A.4.1: Participant Characteristics

	Full Sample	ID	ID&Amount	Choose Info	p-value (B vs. $R)	p-value (All)
	(1)	(2)	(3)	(4)	(5)	(6)
Age	18.32	18.26	18.26	18.45	1.00	0.18
Grade	0.19	0.12	0.12	0.33	1.00	0.01
Pct. Female	56	60	57	51	0.76	0.53
Current Charitable Giving	0.79	0.66	0.79	0.93	0.42	0.26
Volunteering	1.70	1.60	1.89	1.62	0.04	0.07
Religiosity	1.08	1.05	0.94	1.26	0.53	0.22
Political Ideology	1.30	1.32	1.25	1.34	0.58	0.80
Observations	240	80	80	80	—	—

Notes: Reported numbers are means for the specified sample in each column. Summary statistics are for all participants. For the Grade measure, values are coded from 0-4 and correspond to the following categories: "Freshman," "Sophomore," "Junior," and "Senior." For the Current Charitable Giving measure, participants are asked, "On average, how much do you donate to charitable organizations per year?" Participants choose from one of the following categories, which are coded with values ranging from 0-4: "$0-$20," "$20-$50," "$50-$100," "$100-$500," and "Over $500." For the Volunteer variable, participants are asked, "On average, how often do you volunteer for a good cause?" Participants choose from one of the following categories, which are coded with values ranging from 0-4: "Never," "Once a year," "Once a month," "Every week," "Several times a week." For the political ideology variable, participants choose from one of five categories, also coded from 0-4, and range from "Very Liberal" to "Very Conservative." Finally for the religiosity variable, participants respond to the question, "On average, how often do you attend religious service?" They choose from one of the following five categories (coded 0-4): "Never," "Once a year," "Once a month," "Every week," and "Several times a week." The p-values in column 5 are reported from t-tests comparing mean differences across *ID*, *ID&Amount*. The p-values in column 6 are reported from F-tests comparing against the null of no differences across *ID*, *ID&Amount*, and *Choose Info*.

Table A.4.2: Effect of Revealing Amount on Donation Size (*Other*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	OLS	Tobit	OLS	Tobit	OLS	OLS	Tobit	OLS	Tobit
Dep. Var.:	*Donation 2 Amount*									
	Unconditional					*Conditional*				
Donation 1	0.745***	0.735***	0.966***	0.702***	0.918***	0.753***	0.766***	1.076***	0.771***	1.082***
	(0.064)	(0.065)	(0.091)	(0.066)	(0.089)	(0.046)	(0.045)	(0.076)	(0.047)	(0.077)
ID&Amount	1.126*	1.243*	1.643*	-0.362	-0.215	1.179*	1.289*	1.641*	-0.094	-0.239
	(0.668)	(0.723)	(0.860)	(1.050)	(1.150)	(0.624)	(0.700)	(0.873)	(1.192)	(1.422)
Tribute				1.938**	2.249**				0.279	0.455
				(0.994)	(1.118)				(1.021)	(1.193)
ID&Amount X Tribute				1.981	2.306				1.674	2.247
				(1.224)	(1.434)				(1.345)	(1.659)
Observations	240	240	240	240	240	204	204	204	204	204
R-squared	0.449	0.495		0.542		0.513	0.533		0.542	
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes

Notes: Columns 1-2, 4, 6-7, and 9 report ordinary least squares (OLS) estimates with the donation amount (D2) as the dependent variable. Columns 3, 5, 8, and 10 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Columns 1-5 include all observations, while columns 6-10 condition on observations where participants donate a positive amount. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

Table A.4.3: Effect of Revealing Amount on Future Donation Size (*Other*)

	(1) OLS	(2) OLS	(3) Tobit	(4) OLS	(5) Tobit
Dep. Var.:	*Future Donation Amount*				
Donation 1	0.606***	0.590***	0.714***	0.582***	0.704***
	(0.118)	(0.127)	(0.146)	(0.142)	(0.155)
ID&Amount	1.336	1.430	1.841	-0.539	-0.654
	(1.134)	(1.381)	(1.376)	(1.545)	(1.499)
Tribute				-0.556	-0.745
				(1.825)	(1.969)
ID&Amount X Tribute				2.931	3.764
				(2.362)	(2.355)
Observations	80	80	80	80	80
R-squared	0.342	0.395		0.412	
Controls	No	Yes	Yes	Yes	Yes

Notes: Columns 1-3 report OLS estimates using future donation amounts as the dependent variable. Column 4 reports results using a tobit specification accounting for right-censored observations. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses.
***p<0.01, ** p<0.05, * p<0.1

Table A.4.4: Effect of Revealing Amount on Any Future Donation (*Other*)

	(1) OLS	(2) OLS	(3) Probit	(4) OLS	(5) Probit
Dep. Var.:	*Making Any Future Donation*				
𝟙(Donation 1)	0.195	0.118	0.024	0.113	0.022
	(0.198)	(0.213)	(0.109)	(0.219)	(0.118)
ID&Amount	0.035	0.060	0.082	0.026	0.053
	(0.066)	(0.066)	(0.062)	(0.135)	(0.082)
Tribute				0.046	0.036
				(0.127)	(0.084)
ID&Amount X Tribute				0.025	0.025
				(0.176)	(0.132)
Observations	80	80	80	80	80
R-squared	0.040	0.129		0.136	
Controls	No	Yes	Yes	Yes	Yes

Notes: Columns 1-2 and 4 report OLS estimates using an indicator for whether participants make a future donation as the dependent variable. Columns 3 and 5 reports the marginal effects from a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.5: Effect of Revealing Amount on Receiving Charity News (*Other*)

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Probit	OLS	Probit
Dep. Var.:	*Subscribe to Charity Emails*				
𝟙(Donation 1)	0.216***	0.079	0.149	0.067	0.137
	(0.059)	(0.068)	(0.112)	(0.069)	(0.111)
ID&Amount	0.001	0.019	0.017	-0.039	-0.049
	(0.056)	(0.057)	(0.054)	(0.084)	(0.092)
Tribute				0.035	0.034
				(0.084)	(0.076)
ID&Amount X Tribute				0.079	0.087
				(0.119)	(0.116)
Observations	240	240	240	240	240
R-squared	0.025	0.108		0.100	
Controls	No	Yes	Yes	Yes	Yes

Notes: Columns 1-2 and 4 report OLS estimates using an indicator for whether participants add their email to the charity's mailing list as the dependent variable. Columns 3 and 5 report the marginal effects from a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.6: Effect of Revealing Amount on Any Future Donation (*Self*)

	(1)	(2)	(3)
	OLS	OLS	Probit
Dep. Var.:	*Making Any Future Donation*		
𝟙(Donation 1)	0.553***	0.556***	0.261***
	(0.177)	(0.195)	(0.059)
ID&Amount	0.020	0.001	-0.016
	(0.062)	(0.070)	(0.049)
Observations	80	80	80
R-squared	0.341	0.407	
Controls	No	Yes	Yes

Notes: Columns 1-2 report OLS estimates using an indicator for whether participants make a future donation as the dependent variable. Column 3 reports the marginal effects using a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses.
***p<0.01, ** p<0.05, * p<0.1

Table A.4.7: Effect of Revealing Amount on Receiving Charity News (*Self*)

	(1)	(2)	(3)
	OLS	OLS	Probit
Dep. Var.:	*Subscribe to Charity Emails*		
𝟙(Donation 1)	0.164	0.173	0.212
	(0.154)	(0.139)	(0.149
ID&Amount	0.034	0.010	0.012
	(0.109)	(0.121)	(0.108)
Observations	80	80	80
R-squared	0.015	0.091	
Controls	No	Yes	Yes

Notes: Columns 1-2 report OLS estimates using an indicator for whether participants add their email to the charity's mailing list as the dependent variable. Column 3 reports the marginal effects from a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.8: Donor Preferences toward Revealing Contribution Amounts

	Donations	
	On Behalf of Others	On Behalf of Self
Prefer to Reveal	35	50
Prefer not to Reveal	39	48
Total	74	98
p-val: Reveal=Not Reveal	0.64	0.84

Notes: This table reports the proportion of individuals who prefer to reveal their contribution amounts, split by whether participants donate on behalf of others or oneself. Sample is restricted to those in *Choose Info* who choose either to make a tribute donation (for *Other* sessions) or make any positive donation (for *Self* session). The reported p-values are from a χ^2-test of whether aggregate participants preferences differ from an equivalent number of participants preferring to reveal and not reveal amounts, within *Other* and *Self* sessions, respectively.

Table A.4.9: Correlates with Choice-to-Reveal

	(1) OLS	(2) OLS	(3) OLS
Dep. Var.:	$\mathbb{1}$(*Choose to Reveal Contribution Amount*)		
Donation 1	-0.025***	-0.021***	-0.023***
	(0.007)	(0.008)	(0.007)
Donation 2	0.021***	0.023***	0.022***
	(0.007)	(0.008)	(0.008)
Age		-0.018	
		(0.129)	
Grade		-0.000	
		(0.152)	
Female		-0.281***	
		(0.083)	
Current Charitable Giving		-0.068	
		(0.046)	
Volunteering		0.183***	
		(0.065)	
Religiosity		0.005	
		(0.037)	
Political Ideology		0.046	
		(0.040)	
Principle of Care			-0.008
			(0.043)
Empathic Concern			-0.090**
			(0.042)
Observations	172	172	172
R-squared	0.038	0.184	0.067

Notes: Columns 1-3 report associations between the choice-to-reveal contribution amounts and other participant characteristics. Observations are pooled across *Self* and *Other*. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.10: Effect of Option to Reveal on Donation Rates – *Other*

	(1) OLS	(2) OLS	(3) Probit	(4) OLS	(5) OLS	(6) Probit
Dep. Var.:	$\mathbb{1}(Donation\ 2 > 0)$			*Make Tribute Donation*		
$\mathbb{1}$(Donation 1)	0.514***	0.495***	0.300***	0.366***	0.340***	0.335***
	(0.076)	(0.083)	(0.040)	(0.071)	(0.079)	(0.077)
ID&Amount	-0.034	-0.025	-0.034	0.154**	0.174***	0.172***
	(0.043)	(0.045)	(0.045)	(0.062)	(0.065)	(0.060)
Choose Info	-0.033	-0.040	-0.218	0.139**	0.143**	0.144**
	(0.040)	(0.042)	(0.043)	(0.062)	(0.064)	(0.060)
Observations	360	360	360	360	360	360
R-squared	0.219	0.247		0.083	0.114	
Controls	No	Yes	Yes	No	Yes	Yes
p-val.: *ID&Amount=Choose Info*	0.99	0.84	0.87	0.81	0.63	0.67

Notes: Columns 1-3 report estimates using an indicator variable for whether participants make any donation (D2) as the dependent variable. Columns 4-6 report estimates using an indicator variable for whether participants make a tribute donation as the dependent variable. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. All specifications control for whether participants donate in private (D1). MacKinnon-White HC3 robust standard errors reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4.11: Effect of Option to Reveal on Size of Donation – *Other*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	OLS	Tobit	OLS	Tobit	OLS	OLS	Tobit	OLS	Tobit
Dep. Var.:	*Donation 2 Amount*									
	Unconditional					*Conditional*				
Donation 1	0.800***	0.810***	1.055***	0.738***	0.960***	0.768***	0.786***	1.085***	0.774***	1.070***
	(0.046)	(0.047)	(0.066)	(0.051)	(0.067)	(0.039)	(0.038)	(0.057)	(0.040)	(0.058)
ID&Amount	1.037	1.079	1.441*	-0.659	-0.587	1.148*	1.194*	1.539**	-0.180	-0.411
	(0.660)	(0.712)	(0.852)	(1.056)	(1.060)	(0.612)	(0.681)	(0.851)	(1.161)	(1.381)
Choose Info	0.413	0.359	0.639	-1.070	-0.877	0.251	0.009	0.098	-1.744*	-2.015*
	(0.612)	(0.625)	(0.739)	(0.840)	(0.893)	(0.616)	(0.551)	(0.774)	(0.940)	(1.039)
Tribute				2.127**	2.401**				0.429	0.608
				(0.985)	(1.111)				(1.017)	(1.177)
ID&Amount X Tribute				2.150*	2.502*				1.646	2.296
				(1.227)	(1.433)				(1.327)	(1.629)
Choose Info X Tribute				1.820*	1.967				2.208*	2.719*
				(1.097)	(1.256)				(1.205)	(1.404)
Observations	360	360	360	360	360	302	302	302	302	302
R-squared	0.509	0.531		0.595		0.529	0.554		0.573	
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
p-val.: *ID&Amount=Choose Info*	0.26	0.26	0.30			0.12	0.06	0.07		
I&AXTrib.=CI X *Trib.*				0.75	0.67				0.62	0.77

Notes: Columns 1-2, 4, 6-7, and 9 report ordinary least squares (OLS) estimates with the donation amount as the dependent variable. Columns 3, 5, 8, and 10 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Columns 1-5 include all observations, while columns 6-10 condition on observations where participants donate a positive amount. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

Table A.4.12: Effect of Option to Reveal on Future Giving – *Other*

	(1) OLS	(2) OLS	(3) Tobit	(4) OLS	(5) Tobit
Dep. Var.:	*Future Donation Amount*				
Donation 1	0.652***	0.669***	0.835***	0.634***	0.794***
	(0.088)	(0.086)	(0.109)	(0.096)	(0.114)
ID&Amount	1.309	1.277	1.715	-1.095	-1.233
	(1.120)	(1.328)	(1.380)	(1.534)	(1.535)
Choose Info	0.657	0.565	1.014	-1.632	-1.766
	(1.062)	(1.150)	(1.239)	(1.373)	(1.358)
Tribute				-0.187	-0.427
				(1.923044)	(1.961)
ID&Amount X Tribute				3.304	4.202*
				(2.302)	(2.391)
Choose Info X Tribute				3.402*	4.453**
				(2.899)	(2.131)
Observations	120	120	120	120	120
R-squared	0.396	0.419		0.459	
Controls	No	Yes	Yes	Yes	Yes
p-val.: *ID&Amount=Choose Info*	0.52	0.60	0.60		
I&A X *Trib.=CI* X *Trib.*				0.96	0.90

Notes: Columns 1-2 and 4 report OLS estimates using future giving amounts as the dependent variable. Columns 3 and 5 reports results using a tobit specification accounting for right-censored observations of the dependent variable. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.13: Effect of Revealing Amount on Donation Rates - *Other* vs. *Self*

	(1) OLS	(2) OLS	(3) Probit	(4) OLS	(5) Probit
Dep. Var.:			$\mathbb{1}$(*Donation 2)*		
$\mathbb{1}$(Donation 1)	0.594***	0.584***	0.317***	0.584***	0.318***
	(0.061)	(0.062)	(0.029)	(0.062)	(0.027)
ID&Amount	0.025	0.029	0.027		
	(0.028)	(0.029)	(0.029)		
Self X Reveal				0.058**	0.073**
				(0.029)	(0.034)
Other X Reveal				-0.002	-0.008
				(0.042)	(0.036)
Observations	480	480	480	480	480
R-squared	0.311	0.337		0.340	
Controls	No	Yes	Yes	Yes	Yes
p-val.: *Other=Self*				0.16	0.05

Notes: Columns 1-2 and 4 report OLS estimates using an indicator for whether participants make any donation as the dependent variable. Columns 3 and 5 reports the marginal effects from a probit specification. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1

Table A.4.14: Effect of Revealing Amount on Donation Size - *Other* vs. *Self*

	(1) OLS	(2) OLS	(3) Tobit	(4) OLS	(5) Tobit	(6) OLS	(7) OLS	(8) Tobit	(9) OLS	(10) Tobit
Dep. Var.:	*Donation 2 Amount*									
	Unconditional					*Conditional*				
Donation 1	0.797***	0.799***	1.013***	0.797***	1.009***	0.775***	0.784***	1.060***	0.776***	1.048***
	(0.042)	(0.044)	(0.063)	(0.044)	(0.063)	(0.033)	(0.033)	(0.052)	(0.033)	(0.052)
Reveal	1.067**	1.083**	1.077**			0.812*	0.825*	0.722		
	(0.436)	(0.435)	(0.508)			(0.426)	(0.426)	(0.516)		
Self X Reveal				0.910**	0.757				0.338	0.044
				(0.466)	(0.533)				(0.458)	(0.542)
Other X Reveal				1.277**	1.448**				1.415**	1.597**
				(0.576)	(0.690)				(0.575)	(0.726)
Observations	480	480	480	480	480		403 403	403	403	403
R-squared	0.493	0.537		0.537		0.498	0.545		0.549	
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
p-val.: *Other=Self*				0.52	0.31				0.07	0.04

Notes: Columns 1-2, 4, 6-7, and 9 report ordinary least squares (OLS) estimates with the donation amount (D2) as the dependent variable. Columns 4 and 8 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Columns 1-5 include all observations, while columns 6-10 condition on observations where participants donate a positive amount. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

Table A.4.15: Effect of Revealing Amount on Future Donation Size - *Other* vs. *Self*

	(1) OLS	(2) OLS	(3) Tobit	(4) OLS	(5) Tobit
Dep. Var.:	*Future Donation Amount*				
Donation 1	0.636***	0.640***	0.787***	0.641***	0.788***
	(0.082)	(0.084)	(0.108)	(0.085)	(0.109)
Reveal	1.008	0.998	1.235		
	(0.788)	(0.822)	(0.908)		
Self X Reveal				1.082	1.338
				(1.015)	(1.121)
Other X Reveal				0.906	1.123
				(1.030)	(1.147)
Observations	160	160	160	160	160
R-squared	0.346	0.384		0.384	
Controls	No	Yes	Yes	Yes	Yes
p-val.: *Other=Self*				0.88	0.88

Notes: Columns 1-2 and 4 report ordinary least squares (OLS) estimates with the future donation amount as the dependent variable. Columns 3 and 5 reports estimates from a tobit regression accounting for right-censored observations of the dependent variable. Donations are out-of-pocket amounts. Control variables are: gender, age, year in school, self-reported volunteering frequency, self-reported average charitable donations per year, political ideology, and religiosity. MacKinnon-White HC3 robust standard errors reported in parentheses. ***p<0.01, ** p<0.05, * p<0.1.

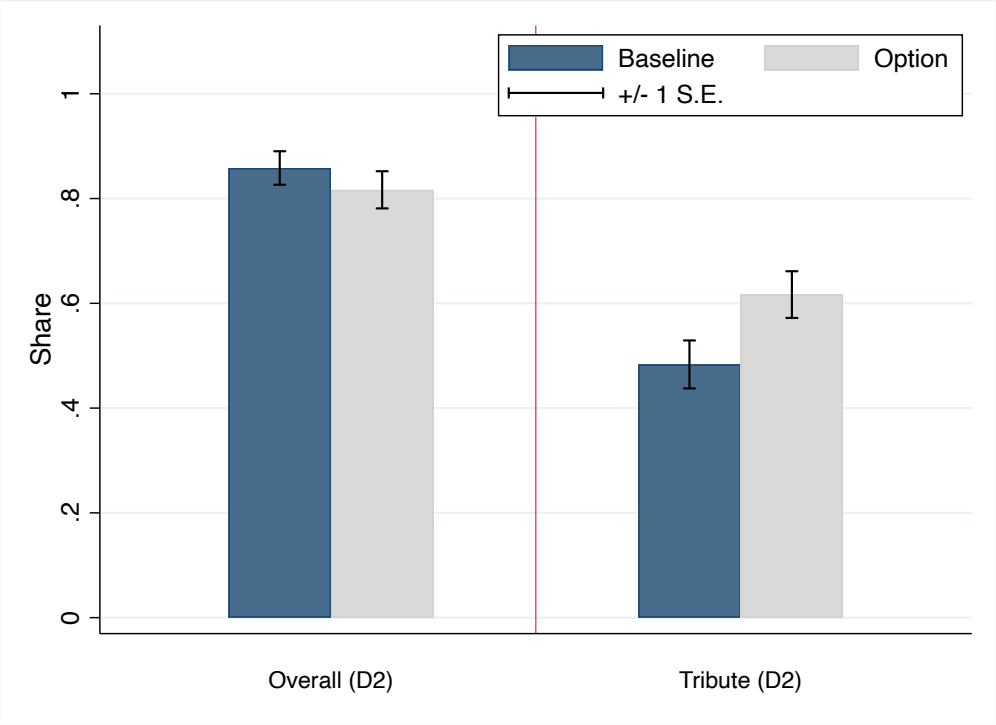

Figure A.4.1: Donation Rates *ID* vs. *Choose Info* (*Other*)

Appendix B Do Actions Speak Louder than Motives? Evaluating The Effectiveness of Image-Fundraising

B.1 Instructions

Note: Additional language in the experiment instructions for the Image-Fundraising treatment is *italicized and underlined*. The rest of the experiment instructions were identical across both Baseline and Image-Fundraising treatments.

Introduction

Thank you for participating in our study. This is a study about decision making. The other people in this room are also participating in the study. You must not talk to them or communicate with them in any way. If you have a question, please raise your hand and one of us will come to where you are sitting to answer your question in private. Please follow along as I read the instructions.

Roles and Earnings

In this study, you will be assigned to one of two roles: **Blue Player** or **Green Player**. You will receive your role assignment on your computer screen once I finish reading the instructions. You will maintain this role throughout the study. This study will consist of two rounds. Instructions will be provided before each round. All participants will receive a $6 show up fee. Your additional earnings will depend on the decisions you make in each of the two rounds. In addition to your show up fee you will receive an initial $10 which you may use to make decisions in this study.

At the top of your station you will see a card holder with a number. This number is your **Participant Number**. Your **Participant Number** will be used to pay you at the end of this study.

Some decisions will be revealed to others in today's study. Prior to making a decision, you will be informed whether and to whom your decision will be revealed. You will also be informed whether your **Participant Number** will be used to identify you and your decision.

Instructions: Round 1, Decision 1

Round 1 is a charitable giving round. If you are a Blue Player, you will make two donation decisions in this round. One of your donation decisions will be randomly chosen to be implemented and counted toward final payment. Note that each decision is equally likely to be selected for payment. Therefore you should treat each decision as if it is the one that will be implemented. If you are a Blue Player, you will for both donation decisions have the opportunity to make a matched donation to children at the Child Life Department at the Children's Hospital of Pittsburgh of UPMC.

The Child Life Department supports children hospitalized for long-term care. One way the Child Life Department supports children is through their Creative and Expressive Arts Therapy Program. The program uses music and art to help children process the feelings that accompany being in the hospital. According to the Child Life Department, "making art and music offers an outlet to communicate when there are no words. Our board-certified therapists utilize music and/or art interventions to meet the physical, mental, emotional, social, and medical needs of hospitalized patients."

To support this program, we will use donations made today to purchase art materials such as paint brushes, marker sets, and drawing pads for children at the Children's Hospital. Donations must be made in increments of $2. Every donation will be matched "one-for-one" by a research foundation, i.e., a $2 donation will be matched with an additional $2 to make a $4 donation to children at the Children's Hospital. We will display a receipt of the total donation made to the children at the Children's Hospital in the Economics Department outside of Posvar Room 4521. If you would like an individual receipt for your donation, we will provide instructions on how to do so at the end of the study.

If you are a Blue Player, for your first decision you will be asked how much of your initial $10 you wish to keep and how much you wish to donate to children at the Children's Hospital. Your

donation amount will be kept private. It will not be shown to any other participant in the room.

If you are a Green Player, you will not have the opportunity to make a matched donation to the children at the Children's Hospital.

Instructions: Round 1, Decision 2

If you are a **Blue Player**, for your second donation decision you will again be able to make a matched donation to children at the Children's Hospital. Donations must again be made in increments of $2, and will be matched one-for-one by a research foundation. For your second donation decision you must as a **Blue Player** decide how much of the initial $10 you wish to keep and how much you wish to donate to children at the Children's Hospital.

For this second donation decision, your donation amount will be shown to a randomly chosen **Green Player** with whom you will interact with in later rounds of the study. However, your **Participant Number** will not be used to identify you and your donation decision to the **Green Player**, and the **Green Player** will not know who you are. Note that your **Green Player** only sees your donation and does not see the donation of any other **Blue Player**. [*Your second donation decision will also be ranked relative to donations made by the other* ***Blue Players****. The* ***Blue Player*** *that donates the most will be ranked first, and the* ***Blue Player*** *that donates the least will be ranked last. Once all donation decisions have been submitted, rankings will appear on each* ***Blue Player's*** *computer screen. Your* ***Participant Number*** *will be used to identify you, your donation amount, and your ranking, but this information is only shown to the other* ***Blue Players****. That is the* ***Blue Player*** *will know who donated what.*]

As mentioned before, at the end of the study, only one of your donation decisions from this round will be randomly selected to be implemented and counted toward final payment. Again, if you are a **Green Player**, you will not have the opportunity to make a matched donation to the children at the Children's Hospital.

Instructions: Round 2, Decision 1

In Round 2, every **Blue Player** will be randomly matched with a **Green Player**. If you are

assigned the role of a **Green Player**, on your screen you will see your matched **Blue Player's** second donation decision from Round 1[*, when it was ranked against other **Blue Players***].

In Round 2, if you are a **Green Player**, you will make two decisions. One of these decisions will be randomly chosen to be implemented and counted toward final payment. Again, please note that each decision is equally likely to be selected for payment. Therefore you should treat each decision as if it is the one that will be implemented. For your first decision you will have the opportunity to make a monetary transfer to your matched **Blue Player**. You will decide how much of the initial $10 you wish to keep and how much you wish to transfer. Any amount of money you transfer will be tripled by the researchers, such that any $2 you transfer will result in your matched **Blue Player** receiving $6. Transfers must be made in increments of $2. **Participant Numbers** will not be used to identify any participants nor their decisions during this round.

Instructions: Round 2, Decision 2

If you are a **Green Player** for your second decision, you will have another opportunity to make a monetary transfer to your matched **Blue Player**. You will decide how much of the initial $10 you wish to keep and how much you wish to transfer. Transfers will again be tripled. However for this decision, the **Blue Player** will be able to send money back to their matched **Green Player**.

Blue Players will be asked to decide how much to send back to their matched **Green Player** for each possible transfer they may receive from the matched **Green Player**. For example, **Blue Players** will decide how much they would send back if they receive $6 (3x a $2 transfer), $12 (3x a $4 transfer), and so on. Any amount sent back to a **Green Player** will not be tripled. **Blue Players** can send any part of the tripled monetary transfer back to their matched **Green Player**. The amount sent back to the matched **Green Player** must be in increments of $2. **Participant Numbers** will not be used to identify any participants nor their decisions during this round.

As mentioned before, at the end of the study one of the two decisions from this round will be randomly selected for payment.

B.2 Additional Experimental Results

As mentioned in Section 2.5, Figure B.2.2 describes results from a non-parametric test where Observer observations are matched by their partner Donor's donation (D2) across treatments. This simulation is intended to proxy for a within-subject scenario where a single Observer encounters two Donors — one whose donation is solicited using image-fundraising, and another whose donation is not. The paired observations compare the different Observer reactions to the two Donors. Given the possible combinations of observation pairings, the random matching procedure is iterated 1,000 times to construct a proxy large-sample distribution of dictator transfer differences.[1] The mean of the distribution shown in Figure B.2.2 is a negative effect of \$0.51, which can be interpreted as Observers transferring 51 cents less to Donors in the IMG treatment relative to Donors in the Baseline treatment. This is equivalent to over a 15% reduction in dictator transfers relative to the average Baseline dictator transfer. The 95th percentile of this distribution is a *negative* effect of \$0.21, equivalent to a 6% decrease in dictator transfers. The results from this non-parametric estimation provides additional support for RESULT 2 from Section 2.5.2, and suggests that image-fundraising significantly decreases Observer dictator transfers.

Beyond the dictator transfer, I leverage the same matching iteration to construct a distribution trustor transfer differences across treatments. The distribution is shown in Figure B.2.3 below. The mean of this distribution is a negative effect of \$0.64, which is equivalent to a 19% decrease relative to the average Baseline trustor transfer. The 95th percentile of this distribution is a *negative* effect of \$0.30, equivalent to a 6% decrease in trustor transfers. This matching exercise suggests image-fundraising significantly decreases Observer trustor transfers, complements the results regarding dictator transfers above, and lends further evidence that is consistent with RESULT 2 from Section 2.5.2 that Observers discount donations made in response to IMG.

[1] I match pairs based on participants in the IMG treatment. This gives us a common support across the two treatments that yields 47 pairs for each match iteration.

B.3 PBE Satisfying Intuitive Criterion

PROPOSITION 1: In the Perfect Bayesian Equilibrium that satisfies the intuitive criterion, donations are defined by:

$$d = \begin{cases} \dfrac{\alpha_D[(1+m)w + \gamma s(d)]}{1 - \gamma s'(d) + \alpha_D}, & \text{if } \theta = H \\ 0, & \text{if } \theta = L, \end{cases}$$

given Observer beliefs, and Observer posteriors are defined by:

$$p = \begin{cases} 1, & \text{if } d = d^{H*} \\ 0, & \text{if } d = d^{L*}, \end{cases}$$

where d^{H*} and d^{L*} are defined as the high- and low-type Donor equilibrium transfers, respectively. Observers transfer:

$$t_R = \begin{cases} (\frac{\alpha_R m - 1}{(1+\alpha_R)m})w + (\frac{1}{(1+\alpha_R)m})[d - \gamma s(d)], & \text{if } d = d^{H*} \\ 0, & \text{if } d = d^{L*}. \end{cases}$$

In the equilibrium described in PROPOSITION 1, high-type Donors donate as a signal to Observers to differentiate themselves from low-type Donors. For any separating equilibrium to exist, neither Donor type should have an incentive to deviate and mimic the other type's donation. First, high-type Donors have no incentive to mimic low-types by choosing $d^{L*} = 0$. High-types will never deviate to $d^{L*} = 0$ as $\lim_{d \to 0} \psi_D(\alpha_D^H) = -\infty$. Second, low-type Donors do not mimic high-types as long as:

$$\ln(w) \geq \ln\Big(\Big[\frac{\alpha_R(1+m)}{1+\alpha_R}\Big]w - \Big[\frac{\alpha_R}{1+\alpha_R}\Big][d - \gamma s(d)]\Big). \tag{10}$$

That is, equation (10) highlights the necessary condition where utility from not donating for low-types is greater than mimicking high-types by giving d^{H*}, and receiving positive Observer transfer $t_R$in equilibrium. Further, it must be true that:

$$\frac{\alpha_D[(1+m)w+\gamma s(d^{H*})]}{1-\gamma s'(d^{H*})+\alpha_D} \geq (\tfrac{\alpha_R m-1}{\alpha_R})w+\gamma s(\underline{\mathrm{d}}).$$

The left-hand side represents the high-type Donor equilibrium donation, d^{H*}. The right-hand side of the above expression represents the donation, $\underline{\mathrm{d}}$, derived from equation (10) when the inequality that ensures that low-type donors prefer not to donate is satisfied with equality. High-type Donor giving maximizes their utility, and is simultaneously too costly for low types to mimic. Observers recognize this separation and update their beliefs accordingly and accurately to reflect the two types of Donors they observe.

PROOF: First, I show that an arbitrary set of pooling equilibria do not satisfy the intuitive criterion. Consider a deviation d', away from a pooling donation $\tilde{d}$, where $d' > \tilde{d}$. Define d' such that for low-type Donors:

$$\ln\Big(\Big[\tfrac{(1+\alpha_R)p_0+\alpha_R p_0 m-1}{(1+\alpha_R)p_0}\Big]w+\Big[\tfrac{1-(1+\alpha_R)p_0}{(1+\alpha_R)p_0}\Big][\tilde{d}-\gamma s(\tilde{d})]\Big)=\ln\Big(\Big[\tfrac{\alpha_R(1+m)}{1+\alpha_R}\Big]w-\Big[\tfrac{\alpha_R}{1+\alpha_R}\Big][d'-\gamma s(d')]\Big), \quad (11)$$

whereby deviating I assume Donors receives the best possible benefit from Observers ($p = 1$). I know an upward deviation that yields the same utility for low-type Donors as the pooled donation exists. This is because increasing the likelihood that a Donor is a high type improves the Observer transfer, $\dfrac{\partial t_R^*}{\partial p} = \frac{w-d+\gamma s(d)}{(1+\alpha_R)mp^2} > 0$. Therefore, a donation d', must exist where the utility loss from the larger donation is offset by the increase in utility from a larger Observer transfer, and $\psi_D(\alpha_D^L)$ remains the same.

For high-type Donors, deviating to donation d' results in greater utility than what they obtain in the pooling equilibrium if I also assume they receive the best possible benefit from Observers

$(p = 1)$. This can be represented by:

$$\ln\left(\left[\tfrac{(1+\alpha_R)p_0+\alpha_R p_0 m-1}{(1+\alpha_R)p_0}\right]w+\left[\tfrac{1-(1+\alpha_R)p_0}{(1+\alpha_R)p_0}\right][\tilde{d}-\gamma s(\tilde{d})]\right)+\alpha_D^H\ln(\tilde{d})<\ln\left(\left[\tfrac{\alpha_R(1+m)}{1+\alpha_R}\right]w-\left[\tfrac{\alpha_R}{1+\alpha_R}\right][d'-\gamma s(d')]\right)+\alpha_D^H\ln(d').$$

I know that high types obtain greater utility by donating d' because $\alpha_D^H\ln(d')>\alpha_D^H\ln(\tilde{d})$ for $d'>\tilde{d}$, while the other terms are equal in value, as defined in equation (11). Now consider a small deviation from d', denoted as $d''<d'$. The donation d'' is equilibrium-dominated for low-type Donors, but not for high-type Donors. In particular:

$$\ln\left(\left[\tfrac{(1+\alpha_R)p_0+\alpha_R p_0 m-1}{(1+\alpha_R)p_0}\right]w+\left[\tfrac{1-(1+\alpha_R)p_0}{(1+\alpha_R)p_0}\right][\tilde{d}-\gamma s(\tilde{d})]\right)>\ln\left(\left[\tfrac{\alpha_R(1+m)}{1+\alpha_R}\right]w-\left[\tfrac{\alpha_R}{1+\alpha_R}\right][d''-\gamma s(d'')]\right),$$

but,

$$\ln\left(\left[\tfrac{(1+\alpha_R)p_0+\alpha_R p_0 m-1}{(1+\alpha_R)p_0}\right]w+\left[\tfrac{1-(1+\alpha_R)p_0}{(1+\alpha_R)p_0}\right][\tilde{d}-\gamma s(\tilde{d})]\right)+\alpha_D^H\ln(\tilde{d})<\ln\left(\left[\tfrac{\alpha_R(1+m)}{1+\alpha_R}\right]w-\left[\tfrac{\alpha_R}{1+\alpha_R}\right][d''-\gamma s(d'')]\right)+\alpha_D^H\ln(d'').$$

Because d'' is equilibrium-dominated for low-types, Observers should only believe high-types would donate d'', and adjust their transfer t_R, accordingly. High-type Donors should deviate to d'', as they benefit relative to the pooling equilibrium donation $\tilde{d}$, given Observers accurately updating beliefs. Because the pooling equilibrium was defined for arbitrary $\tilde{d}$, it follows that all pooling equilibrium fail to satisfy the intuitive criterion.

For separating equilibria other than that defined in PROPOSITION 1, consider a deviation $\acute{d}\in[\underline{d},w]$, but $\acute{d}\neq d^{H*}$. Recall that the threshold value $\underline{d}$ defined above is generally applicable for *all* separating equilibrium, such that low-type Donors have no incentive to deviate from giving nothing. Therefore, low-type Donors have no incentive to deviate to donating $\acute{d}$ from their equilibrium donation $d^{L*}=0$, as $\acute{d}\in[\underline{d},w]$. Meanwhile, $\acute{d}$ by definition does not satisfy the high-type Donor's first-order condition. Any deviation $\acute{d}\neq d^{H*}$ makes high-type Donors worse off. This leaves the equilibrium defined in PROPOSITION 1 as the only PBE that satisfies the intuitive criterion.

Table B.2.1: Participant Demographics

	(1)	(2)	(3)	(4)
	Full Sample	Baseline	IMG	p-value
Panel B: Donors				
Age	19.41	19.46	19.37	0.69
	(1.22)	(1.13)	(1.30)	
Pct. Male	0.55	0.61	0.48	0.20
	(0.53)	(0.56)	(0.50)	
Year in School	1.32	1.37	1.27	0.58
	(1.04)	(1.02)	(1.07)	
Volunteering	1.84	1.80	1.88	0.58
	(0.85)	(0.83)	(0.88)	
Giving History	0.96	0.86	1.05	0.32
	(1.00)	(0.94)	(1.06)	
Political Ideology	1.39	1.36	1.42	0.74
	(0.98)	(1.01)	(0.96)	
Observations	119	59	60	
Panel B: Observers				
Age	19.28	19.34	19.22	0.59
	(1.23)	(1.12)	(1.34)	
Pct. Male	0.58	0.56	0.60	0.67
	(0.51)	(0.53)	(0.49)	
Year in School	1.13	1.22	1.03	0.30
	(0.99)	(1.00)	(0.97)	
Volunteering	1.75	1.80	1.70	0.55
	(0.87)	(0.92)	(0.81)	
Giving History	0.99	0.92	1.07	0.43
	(1.04)	(1.13)	(0.94)	
Political Ideology	1.47	1.37	1.57	0.27
	(0.95)	(1.00)	(0.89)	
Observations	119	59	60	

Notes: Reported numbers are means for the specified sample in each column. Standard deviations are reported in parentheses. The p-values are reported from two-sided t-tests comparing means of participants in the Baseline and IMG treatments, separately by role. The "Year in School" is measured on a 0-3 scale corresponding to categorical answers of "Freshman," "Sophomore," "Junior," and "Senior or Higher." Participants' political leanings are defined by the "Political Ideology" variable. The Political Ideology variable is scaled 0-4 from "Very Liberal" to "Very Conservative." For the "Volunteering" variable, participants are asked, "On average, how often do you volunteer for a good cause?" Answers are scaled 0-4 corresponding to categorical answers of "Never," "Once a year," "Once a month," "Every week," and "Several times a week." For the "Giving History" variable, participants are asked, "On average, how much do you donate to charitable organizations per year?" Answers are scaled 0-4 corresponding to categorical answers of "$0-$20," "$20-$50," "$50-$100," "$100-$500," and "Over $500."

Table B.2.2: Effect of Image-Fundraising on Increasing Donations

	(1)	(2)
	Probit	Probit
	Dependent Variable: Indicator for D2 > D1	
IMG	0.79***	0.79***
	(0.25)	(0.25)
Controls	No	Yes
N	119	119

Notes: Standard errors in parentheses. Probit regressions with and without controls are reported in columns 1 and 2. The dependent variable is binary measure for whether Donor donations (D2) are larger than the value of their altruistic type (D1). Observations correspond to Donors only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2.3: Correlations between Trustworthiness and Altruistic Type

		Trustworthiness					Altruistic Type
		\$6	\$12	\$18	\$24	\$30	(D1)
Trustworthiness	\$6	1.00					
	\$12	0.89	1.00				
	\$18	0.81	0.94	1.00			
	\$24	0.77	0.91	0.97	1.00		
	\$30	0.76	0.89	0.95	0.97	1.00	
Altruistic Type	(D1)	0.55	0.62	0.67	0.65	0.62	1.00

Notes: Cells are labeled by the corresponding trustor transfer level for each Donor trustworthiness decision. Observations correspond to Donors only.

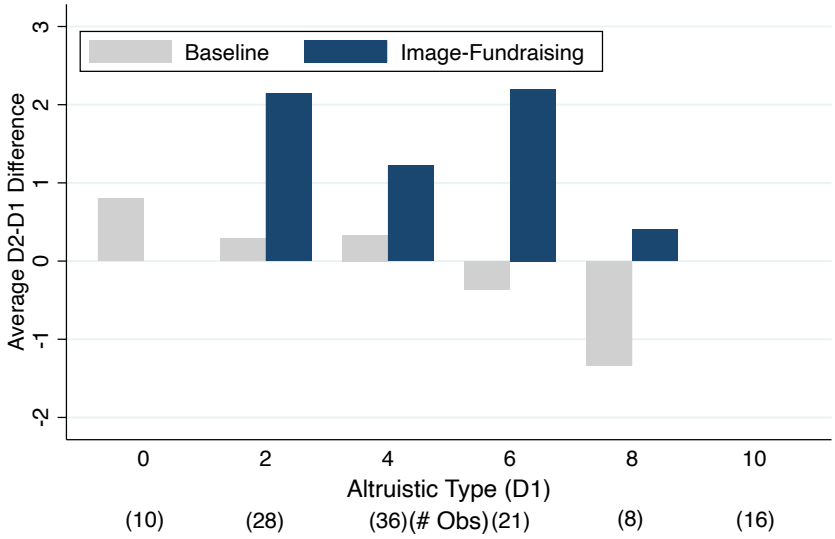

Figure B.2.1: D2-D1 Difference, by Treatment & Altruistic Type

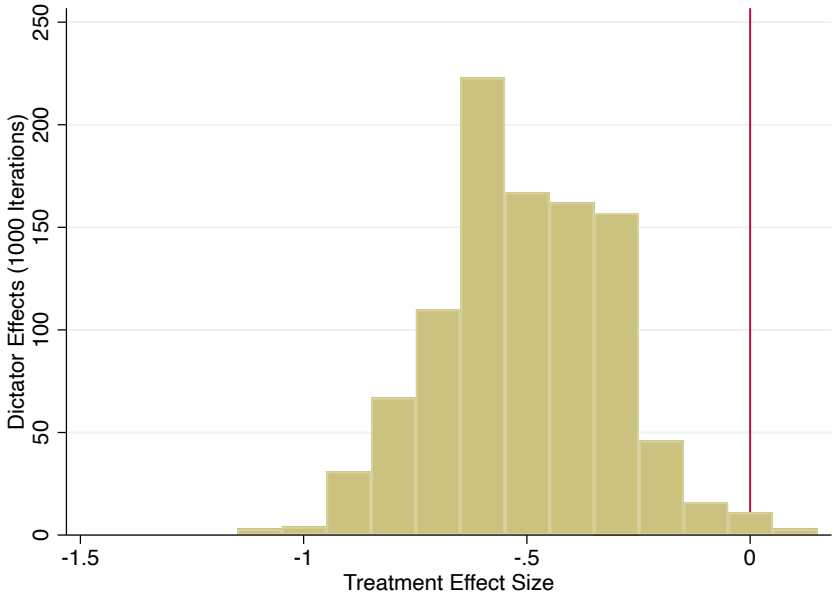

Figure B.2.2: Dictator Transfer Differences, Based on Matching Procedure on Partner's Donation across Treatments

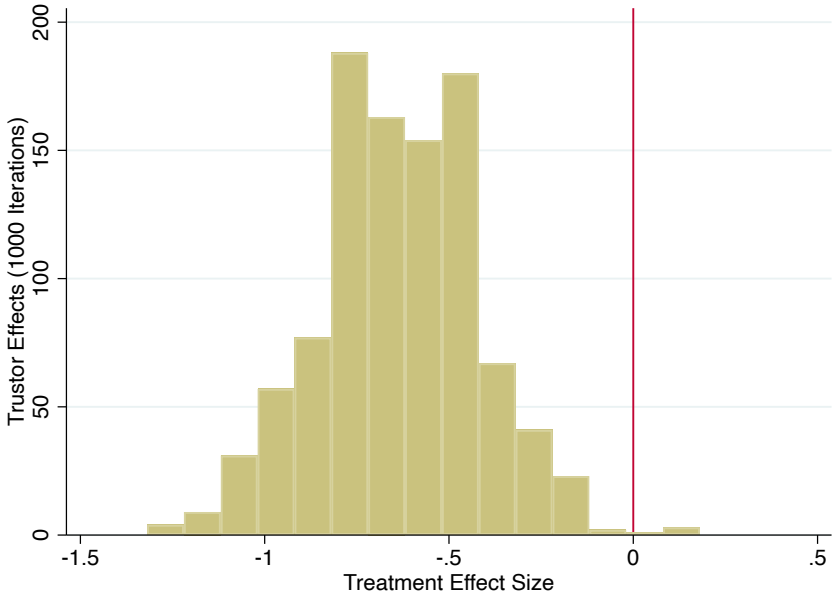

Figure B.2.3: Trustor Transfer Differences, based on Matching Procedure on Partner's Donation across Treatments

Appendix C Paying for What Kind of Performance? Performance Pay, Multitasking, and Mission-Oriented Jobs

C.1 Additional Tables & Figures

Table A4 considers three alternative measures of quality: "Counted points per word" divides total points accumulated (counting words worth less than 5 points as 0 points, as would be the experience of the participant) by total words spelled. This, then, is closer to the measure the participants would experience. Two further measures are aimed at assessing the margin along which participants changed their behavior to increase points and therefore money for charity, in the motivated treatments. Participants might have focused on spelling higher quality words (an intensive margin response), or they might have focused on making sure more of their words were at least five points (an extensive margin response). To get at this, we construct "Counted points per counted word", which divides total points accumulated from words worth at least five points by the number of words spelled that were worth at least five points, and "Share of words that are counted", which is the number of words worth at least five points divide by the total words spelled. The first of these would reveal the intensive margin response, the second would reveal the extensive margin response. In practice, we find that both follow similar patterns, so participants respond both by increasing the average quality of words (even conditional on the word already achieving five points) and by increasing the fraction of words that count.

C.2 Additional Details from the Theory Model

Prediction 1a.

Totally differentiating the F.O.C.s with respect to p yields: $\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_1}\frac{de_1}{dp}+\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2}\frac{de_2}{dp}-\gamma=0$ and $\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_1}\frac{de_1}{dp}+\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}\frac{de_2}{dp}=0$. Solving the second equation for $\frac{de_2}{dp}$ and substituting into the first gives after rearrangement $\frac{de_1}{dp}=\frac{\gamma\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}}{\frac{\partial c(e_1,e_2)}{\partial e_1 e_1}\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}-(\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2})^2}>0$ because of the convexity of the cost function. Also, $\frac{de_2}{dp}=-\frac{\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_1}\frac{de_1}{dp}}{\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}}<0$. Thus, relative to the case of a flat wage where the agent chooses $e^*=(e_1^*,e_2^*)$ given by $\frac{\partial c(e_1,e_2)}{\partial e_1}=\frac{\partial c(e_1,e_2)}{\partial e_2}=0$, introducing a piece rate p increases e_1 and decreases e_2.

Prediction 1b.

Implicit differentiation of $\frac{\partial c(e_1,e_2)}{\partial e_1}=\gamma\theta$ yields $\frac{de_2}{d\gamma}=\frac{\theta}{\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}}>0$.

Prediction 2a.

Totally differentiating the F.O.C.s with respect to p yields: $\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_1}\frac{\partial e_1}{\partial \theta}+\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2}\frac{\partial e_2}{\partial \theta}=0$ and $\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_1}\frac{\partial e_1}{\partial \theta}+\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}\frac{\partial e_2}{\partial \theta}-\gamma=0$ Solving the second equation for $\frac{\partial e_2}{\partial \theta}$ and substituting into the first gives after rearrangement $\frac{\partial e_2}{\partial \theta}=\frac{\gamma\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2}}{\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_1}\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}-(\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2})^2}>0$ because of the convexity of the cost function. Similarly, it can be shown that $\frac{\partial e_2}{\partial \theta}=-\frac{\gamma\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2}}{\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_1}\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}-(\frac{\partial c(e_1,e_2)}{\partial e_1 \partial e_2})^2}<0$ Thus, relative to the case of a flat wage where the agent chooses $e^*=(e_1^*,e_2^*))$ given by $\frac{\partial c(e_1,e_2)}{\partial e_1}=\frac{\partial c(e_1,e_2)}{\partial e_2}=0$, when the agent derives an intrinsic benefit θ from producing quality e_1 is lower and

e_2 is higher.

Prediction 2b.

Implicit differentiation of $\frac{\partial c(e_1, e_2)}{\partial e_2} = \gamma\theta$ yields $\frac{de_2}{d\gamma} = \frac{\theta}{\frac{\partial c(e_1,e_2)}{\partial e_2 \partial e_2}} > 0.$

Prediction 3a, 3b.

Similar to 1a, and 1b respectively, so omitted.

Prediction 4:

Follows from the fact that $\frac{\partial e_2}{\partial \theta} > 0$ and $\frac{\partial e_1}{\partial \theta} < 0.$

Phase 3

Flat-rate Motivated (F-M)

The agent chooses F-M when the utility it furnishes is higher than or equal to the utility from the outside option:

$w + \theta e_2^{FM} - \frac{1}{\gamma} c(e_1^{FM}, e_2^{FM}) > w^O - \frac{1}{\gamma} c(e_1^O, e_2^O)$, where $w < w^0$,

$$\theta > \frac{w^O - w + \frac{1}{\gamma}[c(e_1^{FM}, e_2^{FM}) - c(e_1^O, e_2^O)]}{e_2^{FM}}.$$

Piece-rate Non-Motivated (P-NM)

The agent chooses the piece rate scheme if:

$pe_1^{PNM} - \frac{1}{\gamma}c(e_1^{PNM}, e_2^{PNM}) > w^O - \frac{1}{\gamma}c(e_1^O, e_2^O)$, with $c(e_1^{PNM}, e_2^{PNM}) > c(e_1^O, e_2^O)$,

$$\gamma > \frac{c(e_1^{PNM}, e_2^{PNM}) - c(e_1^O, e_2^O)}{pe_1^{PNM} - w^O}.$$

Piece-rate Motivated (P-M)

In the mission case, the agent chooses the piece rate scheme if:

$pe_1^{PM} + \theta e_2^{PM} - \frac{1}{\gamma}c(e_1^{PM}, e_2^{PM}) > w^O - \frac{1}{\gamma}c(e_1^O, e_2^O)$, with $c(e_1^{PM}, e_2^{PM}) > c(e_1^O, e_2^O)$,

$$\theta > \frac{w^O - pe_1^{PM} + \frac{1}{\gamma}[c(e_1^{PM}, e_2^{PM}) - c(e_1^O, e_2^O)]}{e_2^{PM}} = \hat{\theta};$$

or

$$\gamma > \frac{c(e_1^{PNM}, e_2^{PNM}) - c(e_1^O, e_2^O)}{pe_1^{PNM} +_2^{PM} - w^O} = \hat{\gamma}.$$

The above inequalities suggest that the proposed scheme is more likely to be chosen by high θ and/or γ agents. Note also that $\frac{d\hat{\theta}}{d\gamma} < 0$ and $\frac{d\hat{\gamma}}{d\theta} < 0$, suggesting that ability and motivation are substitutes for sorting, that is, a higher ability individual requires less motivation to sort and similarly a higher motivated individual requires less ability to find sorting in optimal.

C.3 Experimental Instructions & Questionnaires

This appendix reproduces the instructions that were provided to participants during our experimental sessions. Sections of the instructions that are specific to the flat-rate treatment are preceded by "F-NM/{M}". Sections of the instructions that are specific to the piece-rate treatment are preceded by "P-NM/{M}". Within those sections, text within curly brackets ("{...}") are specific to mission-oriented treatments. All other sections of the instructions are common across treatments. Instructions prior to "Phase 1" were presented on participants' screens, but also read aloud by the experimenter. All instructions from "Phase 1" onward were presented on participants' screens as they preceded through the experiment at their own pace and were not read aloud.

Introduction

This experiment is a study of decision-making. Your earnings will depend on the actions that you take during the experiment. At the end of the experiment, you will be paid $5 for showing up plus whatever you earn during the course of the experiment. Throughout the experiment, your earnings will be reported in "Experimental Currency Units" or "ECUs". At the end of the experiment, we will convert however many ECUs you have earned into dollars at a rate of X ECU's = $1. Payments will be made privately and in cash. All decisions are made anonymously. Please do not talk to other participants during the experiment. If at any point you have a question, raise your hand and an experimenter will come to you to provide an answer.

The experiment will consist of three task phases and a questionnaire. In the three task phases, you will perform a *word formation task*. At the end of the experiment, we will pay you based on your performance in the first phase and your performance in *either* the second or third phase (plus the $5 show-up fee). We will randomly select whether you (and other participants in the room) are

paid for the second or third phase, both of which are equally likely to be selected. We will discuss the specific procedures of each of these phases and how they may impact your earnings as they occur. First, we will describe the word formation task and software interface in detail.

screen break

The word formation task

During the experiment, you will be asked to complete a number of "word formation tasks." In the word formation task, you will be presented with a set of seven letters as in the figure below.

Your task is to form a word using at least two of the letters available to you. The word must be spelled as it is in the dictionary. For example, if you were provided the set of letters "D I L N O R T", you might spell the word "ON" or the word "LORD". To spell a word, click each letter in order and each letter's tile will move to the lower row. To clear the letters you have entered, click "Clear." Click "Submit" when you are done. If the word you have submitted is not an acceptable word or is incorrectly spelled, your chosen letters will be cleared and you will be asked to try again.

Each letter has a certain number of points associated with it, which is noted in the lower right hand corner of each letter square. When you submit a word, these points will be added up to determine your score for that word. If the total number of points associated with a word is less than five, your score for that word is zero. If the total number of points associated with a word is five or greater, your score for that word is given by the number of points. For example, the word "ON" is made up of two one-letter tiles and as such is worth zero points. The word "LORD" is a total of five points $(1 + 1 + 1 + 2)$, and as such is worth five points.

So that you can practice working with the software, form a word with the seven letters on the

next screen, then click “Submit”.

screen break

Phase 1

We will now begin the first phase. This phase consists of five “sections”, each of which has slightly different instructions. Follow the instructions on each screen.

screen break

Phase 1, Section 1

In this first section, you will be asked to complete 5 word formation tasks. Spell a word for each set of letters. You will have 2 minutes to do so. Remember, after spelling a word click “Submit” to move on to the next set of letters.

((task proceeds))

screen break

Phase 1, Section 2

In this section, the task is again to complete 5 word formation tasks. This time (and in all of the remaining sections of this phase), you have the opportunity to earn ECUs.

In this section, you will start with 20 ECUs. Once you click "Start" the first task will appear. You then have 60 seconds to complete all 5 tasks. You lose $\frac{1}{3}$ ECU for each second that it takes you to complete and submit all five tasks. For example, if it takes you 6 seconds to complete each of the five tasks, you will have used a total of 30 seconds. As a result, your earnings would be 20 ECUs - $(\frac{1}{3})30$, or 10 ECUs.

((task proceeds))

screen break

Phase 1, Section 3

This section is slightly different. You will again have the opportunity to earn money by completing word formation tasks. This time, you will have 3 minutes to complete as many word formation tasks as you can. You will earn 1 ECU for each word formation task you complete.

((task proceeds))

screen break

Phase 1, Section 4

In this section, you will complete five word formation tasks. You have 25 seconds to complete each one. In this section, the amount of money you earn will depend on the score you accumulate rather than the number of tasks you complete. For each point, you will earn 0.2 ECUs. Remember,

words with point totals less than five receive a score of zero points. There is no penalty for taking the full 25 seconds for each task, and no advantage from not doing so, so take the time to spell the best word you can.

((task proceeds))

screen break

Phase 1, Section 5

In this section, you will have 3 minutes to complete word formation tasks. There is no limit to the number of words you can form during the 3 minute period. This time, your payment again depends on the number of points you accumulate. Every time you spell a word worth five or more points, you will accumulate points from that word. You will be paid 0.2 ECUs for the total number of points you have accumulated at the end of the three minutes. For example, if you spell 9 words that are worth 4 points and 10 words that are worth 8 points, you will be paid for the points accumulated on the 10 words worth at least 5 points. That is, you would receive 8 pts * 10 words * 0.20 ECUs, or 16 ECUs.

((task proceeds))

Phase 2

We will now begin the second phase of the experiment. Unlike the previous phase, there is only one section. We are hoping to learn how many points people can accumulate in a fixed period of

time.

The rules are as follows:

In this phase, you will have 8 minutes to complete word formation tasks. There is no limit to the number of words you can form during the 8 minute period. Again, you can earn money. {In this phase, your efforts can also benefit a charity of your choosing. You will be allowed to choose a charity in a moment.} You {and a charity} will be paid for the outcome of this phase only if this phase is randomly chosen instead of the third phase. You will not know which phase is being randomly selected until the end of the experiment, so you should proceed as though you may receive payment. There is a 50% chance this Phase will be selected, and a 50% chance Phase 3 will be selected.

If this phase is selected for payment:

F-NM/{M}: [Regardless of how many word formation tasks you complete in the next eight minutes, you will receive 70 ECUs. {Additionally, when you score 5 or more points while spelling a word, you will generate some money for charity. For every point that you score on that word, you will generate 0.2 ECUs for charity. For example, a 5 point word will generate the monetary equivalent of 1 ECU for charity. A 6 point word will generate 1.2 ECUs for charity. A 4 point word will generate 0 ECUs for charity (because a word must be worth at least 5 points to generate money for charity). }

For example, if you complete 40 word formation tasks {and accumulate 150 points on words worth at least 5 points}, you will receive 70 ECUs {and your charity will receive the equivalent of 30 ECUs}. If you complete 100 word formation tasks {and accumulate 250 points on words worth at least 5 points}, you will receive 70 ECUs {and your charity will receive 50 ECUs}.]

P-NM/{M}: [For each word formation task that you complete, you will receive 1 ECU. {Additionally, when you score 5 or more points while spelling a word, you will generate some money for charity. For every point that you score on that word, you will generate 0.2 ECUs for charity. For example, a 5 point word will generate the monetary equivalent of 1 ECU for charity. A 6 point word will generate 1.2 ECUs for charity. A 4 point word will generate 0 ECUs for charity (because a word must be worth at least 5 points to generate money for charity). }

For example, if you complete 40 word formation tasks {and accumulate 150 points on words worth at least 5 points}, you will receive 40 ECUs {and your charity will receive the equivalent of 30 ECUs }. If you complete 100 word formation tasks {and accumulate 250 points}, you will receive 100 ECUs {and your charity will receive 50 ECUs}.]

{We will donate money generated for charities after the experiment has ended. So that you can verify that this has happened, we will post receipts from the charities on the experimenter's website. Before we begin this Phase, please indicate which of the following charities you would like to benefit.}

((Participants presented with list of charities with radio buttons.))

We will now ask you a few questions to make sure the instructions are clear. After you have answered the questions, raise your hand and the experimenter will come check your answers.

((Comprehension Check (below) passed out on paper))

1. This phase will end after:
   a. 50 words are spelled
   b. 8 minutes
   c. 20 minutes

d. The end is randomly determined

2. Suppose: At the end of this phase you have spelled a total of 25 words. For the sake of this example, suppose that 10 of the words were worth 2 points apiece and the remaining 15 words were all worth 10 points apiece. If this phase is randomly selected for payment, your earnings from this phase are **{BLANK}** and charity receives **{BLANK}**. (Fill in the blanks.)
3. Suppose: At the end of this phase you have spelled a total of 50 words. For the sake of this example, suppose that 49 of the words were worth 4 points apiece and the remaining word was worth 15 points. If this phase is randomly selected for payment, your earnings from this phase are **{BLANK}** and charity receives **{BLANK}**. (Fill in the blanks.)
4. Which of the following is accurate:
   a. You will be paid for Phase 1 OR Phase 2 OR Phase 3.
   b. You will be paid for Phase 1 AND, either Phase 2 OR Phase 3.
   c. You will be paid for Phase 1 AND Phase 2 AND Phase 3.
   d. You will be paid for Phase 1 OR Phase 2, AND Phase 3.

Phase 3

We will now begin the third phase of the experiment. This is the final phase that will require you to complete word formation tasks. After this phase, you will complete a questionnaire and then the experiment will be over.

This phase is similar to Phase 2. Once again you will have 8 minutes to complete as many word formation tasks as you want. The main difference is that this time you can choose how you are paid. You have two options:

- (Previous Option) [Repeat treatment specific payment information.] {The money generated for charity will go to the charity that you chose in the previous phase.}

- (New Option) Regardless of how many word formation tasks you complete in the next eight minutes, you will receive 90 ECUs.

Recall that you will receive payment {and benefit charity} as according to your choice only if this phase is the one that is randomly selected for payment at the end of the experiment.

Please respond to two quiz questions to ensure that these instructions are clear. After you have answered the questions, raise your hand and the experimenter will come check your answers.

((Comprehension Check (below) passed out on paper))

1. Suppose you have selected the "Previous Option". At the end of this phase you have spelled a total of 25 words. For the sake of this example, suppose that 10 of the words were worth 2 points apiece and the remaining 15 words were all worth 10 points apiece. If this phase is randomly selected for payment, your earnings from this phase are **{BLANK}** {and charity receives **{BLANK}**}. (Fill in the blanks.)
2. Suppose you have selected "New Option". Again suppose that at the end of this phase you have spelled a total of 25 words. For the sake of this example, suppose that 10 of the words were worth 2 points apiece and the remaining 15 words were all worth 10 points apiece. If this phase is randomly selected for payment, your earnings from this phase are **{BLANK}** {and charity receives **{BLANK}**}. (Fill in the blanks.)

Now, please select the option you prefer. After you have made your selection, this phase will begin.

Additional decisions and questionnaire

You have now completed the part of the experiment that involves word formation tasks. We will now ask you to make a few more decisions and answer a brief questionnaire. After the questionnaire

is completed, your payment for the experiment will be determined.

Decision task 1: Lottery vs. sure payment decisions

This part concerns the choice between a **lottery** and a **sure payment**. On the following screen, 14 situations will be displayed. The lottery is the same in each situation, but the sure payment varies. In the lottery you get 30 ECUs with 50 percent probability and 0 points with 50 percent probability (determined by a random draw of the computer). The following screen will present the 14 situations. Please decide in each situation whether you opt for the lottery or the sure payment. Once you have made your choice in each situation, the computer will randomly select one situation. If you selected the sure payment in the randomly selected situation, you will receive the sure payment associated with that situation. If you selected the lottery in the selected situation, you will face a 50 percent probability of receiving 30 ECUs and a 50 percent probability of receiving 0 points.

((Show multiple price list – sure payments range from 2 to 28))

Decision task 2: Opportunity to make a donation

In this part you can choose how to allocate **$10** between yourself and a charity of your choosing. The 10 dollars you are allocating in this section are new and do not come out of your earnings from the previous phases. You will decide how much of the **$10** to keep for yourself and how much to pass to your selected charity. You may elect to keep it all for yourself and pass nothing to the charity, keep nothing for yourself and pass it all to the charity, or keep some for yourself and pass the remainder to the charity.

The choice you make in this part will be implemented at the end of the experiment with a 10%

probability. That is, on average, 1 out of every 10 participants will receive some money and/or generate money for a charity based on his/her decision in this part.

To make your choice you will need to select ONE charity from the list provided and the amount, if any, you wish to pass to your charity of choice.

Please click "Continue" to make your decisions.

Procedures Questionnaire

Please respond to the following items by clicking the number on the rating scale that best represents your opinion about that item.

1. The money you passed to the Charity will be sent to the charity.
2. The instructions about the task were clear and easy to follow.
3. The instructions about my compensation were clear and easy to follow.
4. The recipients of donations to the Charity are deserving of support.

((Responses recorded on five-point scale ranging from "Strongly Disagree" to "Strongly Agree"))

General Questionnaire

Please, answer the following questions. There are no correct or wrong answers. Your responses are completely confidential.

1. Gender (Male/Female)
2. Ethnicity (White/Asian/Black/Hispanic or Latino/None of These/Would rather not say)
3. Age
4. Native English Speaker (Yes/No)

5. Area of Study (Arts and Sciences/Business/Education/Engineering and Computing/Law/Medic ine/Other)
6. Year of Study and University (Freshman/Sophomore/Junior/Senior/Graduated)
7. Highest Level of Education Expected to Complete (Bachelor's/Master's/Doctorate/Other Professional Degree/None of these)
8. Type of Job Sought After Graduation (For-profit sector/Non-profit sector/Government/Any of these sectors, will seek best fit/Any of these, will seek highest payment)
9. Current Work Type (Part-time/Full-time/Neither)
10. If yes to above, what is your hourly wage?
11. Regular Religious Service Attendance (Yes/No)
12. In last 12 months, did you make a monetary donation to a charitable organization? (Yes/No)
13. In last 12 months, did you donate goods or clothes to a charity? (Yes/No)
14. In last 12 months, did you do volunteer work for a charity? (Yes/No)
15. In last 12 months, have you donated blood? (Yes/No)

On a scale of 1 to 5, please indicate the degree to which you agree or disagree with the following statements. "1" indicates strongly disagree, "5" indicates that you strongly agree, and "3" indicates that you neither agree nor disagree.

- Politics is a dirty word.
- I respect public officials who can turn a good idea into law.
- Ethical behavior of public officials is as important as competence.
- The give and take of public policy making doesn't appeal to me.
- I don't care much for politicians.
- People may talk about the public interest, but they are really concerned only about their self-interest.

- It is hard for me to get intensely interested in what is going on in my community.
- I unselfishly contribute to my community.
- Meaningful public service is very important to me.
- I would prefer seeing public officials do what is best for the whole community even if it harmed my interests.
- An official's obligation to the public should always come before loyalty to superiors.
- I consider public service my civic duty.
- I believe that there are many public causes worth championing.
- I do not believe that government can do much to make society fairer.
- If any group does not share in the prosperity of our society, then we are all worse off.
- I am willing to use every ounce of my energy to make the world a more just place.
- I am not afraid to go to bat for the rights of others even if it means I will be ridiculed.
- When public officials take an oath of office, I believe they accept obligations not expected of other citizens.
- I am willing to go great lengths to fulfill my obligations to my country.
- Public service is one of the highest forms of citizenship.
- I believe everyone has a moral commitment to civic affairs no matter how busy they are.
- I have an obligation to look after those less well off.
- To me, the phrase "duty, honor, and country" stirs deeply felt emotions.
- It is my responsibility to help solve problems arising from interdependencies among people.
- I am rarely moved by the plight of the underprivileged.
- Most social programs are too vital to do without.
- It is difficult for me to contain my feelings when I see people in distress.
- To me, patriotism includes seeing to the welfare of others.
- I seldom think about the welfare of people whom I don't know personally.

- I am often reminded by daily events about how dependent we are on one another.
- I have little compassion for people in need who are unwilling to take the first step to help themselves.
- There are few public programs that I wholeheartedly support.
- Making a difference in society means more to me than personal achievements.
- I believe in putting duty before self.
- Doing well financially is definitely more important to me than doing good deeds.
- Much of what I do is for a cause bigger than myself.
- Serving citizens would give me a good feeling even if no one paid me for it.
- I feel people should give back to society more than they get from it.
- I am one of those rare people who would risk personal loss to help someone else.
- I am prepared to make enormous sacrifices for the good of society.

Table C.1.1: Summary Statistics by Treatment

Treatment:	F-M	F-NM	P-M	P-NM
Female	0.59	0.63	0.57	0.50
	(0.50)	(0.49)	(0.50)	(0.51)
Age	19.61	19.96	19.66	19.46
	(1.02)	(2.60)	(1.70)	(1.45)
Year in College	2.37	2.33	2.28	2.24
	(1.05)	(0.94)	(1.06)	(1.13)
Business Major	0.53	0.48	0.38	0.46
	(0.50)	(0.51)	(0.49)	(0.50)
Native English?	0.94	0.98	0.87	0.90
	(0.24)	(0.15)	(0.34)	(0.30)
Currently Works	0.63	0.59	0.68	0.68
	(0.49)	(0.50)	(0.47)	(0.47)
Current Wage	7.79	7.02	7.89	11.18
	(7.38)	(7.27)	(6.22)	(21.28)
Any monetary donations?	0.69	0.70	0.64	0.72
(past year)	(0.47)	(0.47)	(0.48)	(0.45)
Any goods donations?	0.88	0.72	0.70	0.78
(past year)	(0.33)	(0.46)	(0.46)	(0.42)
Any volunteering?	0.69	0.72	0.66	0.66
(past year)	(0.47)	(0.46)	(0.48)	(0.48)
Any blood donations?	0.33	0.33	0.21	0.24
(past year)	(0.47)	(0.47)	(0.41)	(0.43)
Holt-Laury: Share of	0.52	0.49	0.50	0.51
risky choices	(0.20)	(0.20)	(0.19)	(0.17)
Dictator: Share contributed	0.48	0.48	0.45	0.41
	(0.31)	(0.33)	(0.29)	(0.32)
PSM Survey score	0.68	0.68	0.70	0.70
	(0.11)	(0.08)	(0.09)	(0.09)
Observations	61	61	61	61

Notes: We have conducted pairwise comparisons across all treatments for all outcomes. Six (of 84 pairwise comparisons) are statistically significantly different: in F-NM vs. P-M, "currently works" (p¡10%) and "native English" (p¡5%); in F-NM vs. P-NM, "native English" (p¡5%); in F-M vs. P-M, "any goods donation" (p¡5%) and "native English" (p¡5%); in F-M vs. P-NM, "any goods donation" (p¡10%). We note that 6 of 84 pairwise comparisons is roughly 7%, a share we might expect to randomly appear as statistically significant at the 5-10% level. Standard deviations in parentheses.

Table C.1.2: Averages of Phase 3 Outcome Variables by Treatment

Treatment:	F-M	F-NM	P-M	P-NM
Phase 3: Words Spelled	37.21	39.33	48.85	64.62
	(1.54)	(2.80)	(3.39)	(4.22)
Phase 3: Points per Word	6.62	6.17	5.98	4.94
	(0.09)	(0.16)	(0.13)	(0.16)
Phase 3: Share of words	0.82	0.76	0.71	0.55
counted (>4pts.)	(0.01)	(0.02)	(0.02)	(0.03)
Phase 3: Earnings	7.92	8.97	7.44	8.60
	(0.13)	(0.03)	(0.33)	(0.31)
Phase 3: Charitable contribution	3.61	0.00	3.22	0.00
	(0.28)	(0.00)	(0.35)	(0.00)
Observations	61	61	61	61

Notes: Standard deviations in parentheses.

Table C.1.3: Testing Differences in Phase 2 Outcomes Across Treatments, Add'l Controls

Variables:	(1) Total Words	(2) Points per Word
Treatment: F-NM	(omitted)	(omitted)
Treatment: F-M	-1.70	0.27*
	(2.45)	(0.16)
Treatment: P-NM	23.22***	-0.76***
	(4.13)	(0.20)
Treatment: P-M	5.45	0.01
	(3.31)	(0.18)
Observations	244	244
R-squared	0.26	0.21
P-M vs. F-M p-val.	0.02	0.09
P-M vs. P-NM p-val.	0.00	0.00
DinD est. (PM-FM)-(PNM-FNM)	-16.07	0.50
DinD p-val.	0.00	0.04

All specifications include the following additional controls: main Phase 1 ability measure, gender, years in college, business major indicator, native English speaker indicator, current work status. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.1.4: Testing Differences in Phase 2 Outcomes With Alternative Quality Measures

Variables:	(1) Counted Points per Word	(2) Counted Points per Counted Word	(3) Share of Words that are Counted
Treatment: F-NM	(omitted)	(omitted)	
Treatment: F-M	0.43**	0.27*	0.04*
	(0.20)	(0.16)	(0.02)
Treatment: P-NM	-1.12***	-0.29*	-0.13***
	(0.26)	(0.16)	(0.03)
Treatment: P-M	-0.05	0.04	-0.00
	(0.21)	(0.15)	(0.02)
Observations	244	244	244
R-squared	0.19	0.07	0.21
P-M vs. F-M p-val.	0.01	0.03	0.04
P-M vs. P-NM p-val.	0.00	0.00	0.00
DinD est. (PM-FM)-(PNM-FNM)	0.63	0.07	0.09
DinD p-val.	0.05	0.73	0.02

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.1.5: Nonparametric Tests of Differences in Primary Phase 2 Outcomes

	(1)	(2)
Variables:	Total Words	Points per Word
Treatment	*Diff. in means (X vs. Y, reported as X-Y)*	
Comparison	*[p-value from Wilcoxon test]*	
F-M vs. F-NM	-2.34	0.34
	[0.44]	[0.11]
P-NM vs. F-NM	22.21	-0.71
	[0.001]***	[0.0001]***
P-M vs. F-NM	-4.60	-0.01
	[0.31]	[0.81]
P-NM vs. F-M	-24.56	-1.06
	[0.0001]***	[0.000]***
P-M vs. F-M	6.95	-0.33
	[0.05]*	[0.065]**
P-NM vs. P-M	17.60	0.72
	[0.0001]***	[0.0003]***

Each cell reports the difference in means between an outcome (noted in the column header) between two treatments. We conduct a Wilcoxon Rank-Sum Test for each treatment comparison and report the p-value in brackets beneath the differences in means. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.1.6: Heterogeneity in Phase 2 & Sorting Outcomes by Ability and Dictator Share

Variables:	(1) Total Words	(2) Points per Word	(3) Sorting: Choose Treatment-Assigned Payment
Treatment: F-NM	(omitted)	(omitted)	(omitted)
Treatment: F-M	-2.90	0.33*	0.51***
	(1.80)	(0.17)	(0.06)
Treatment: P-NM	21.70***	-0.71***	0.36***
	(3.45)	(0.20)	(0.06)
Treatment: P-M	5.42**	-0.00	0.49***
	(2.24)	(0.17)	(0.06)
Dictator Donation Share (DDS)	-0.93	0.03	0.02
(mean 0, std. dev. 1)	(1.46)	(0.09)	(0.01)
Treatment: F-M X DDS	1.77	0.15	0.19***
	(1.76)	(0.12)	(0.06)
Treatment: P-NM X DDS	0.35	0.04	0.01
	(3.34)	(0.19)	(0.06)
Treatment: P-M X DDS	0.40	-0.03	0.05
	(2.88)	(0.15)	(0.07)
Phase 1 Ability Measure	9.23***	0.14	0.01
(mean 0, std. dev. 1)	(1.40)	(0.09)	(0.01)
Treatment: F-M X Ability	-0.88	-0.16	0.04
	(1.94)	(0.14)	(0.07)
Treatment: P-NM X Ability	4.68	-0.36	0.20***
	(3.47)	(0.22)	(0.06)
Treatment: P-M X Ability	3.96*	-0.21*	0.10*
	(2.32)	(0.13)	(0.05)
Observations	244	244	244
R-squared	0.50	0.18	0.28

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

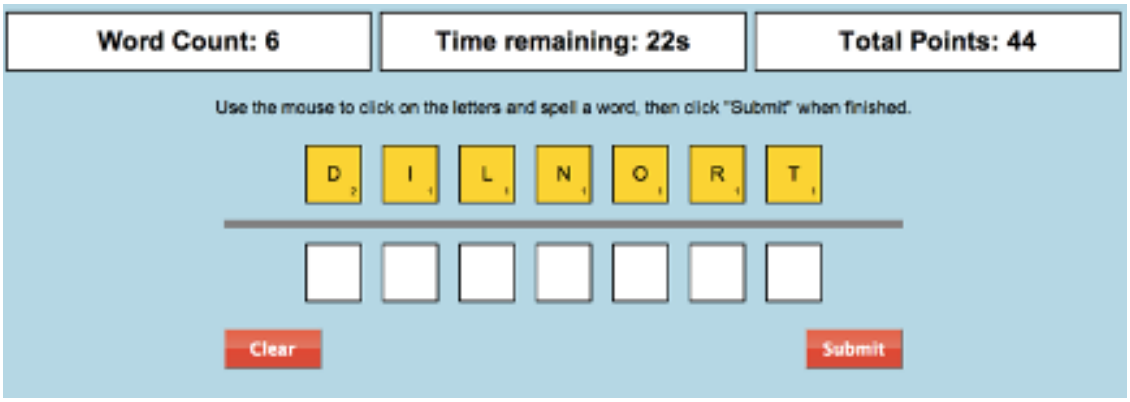

Figure C.3.1: Screenshot of Word Formation Task

Table C.3.1: List of Charities

South Carolina (in-person sessions)	**Pennsylvania (online sessions)**
Harvest Hope Food Bank	Greater Pittsburgh Community Food Bank
The South Carolina Historical Society	Heinz History Center
Make-a-wish Foundation of South Carolina	Make-a-wish Foundation of Greater PA and WV
Coastal Conservation League	Western Pennsylvania Conservancy
ETV Endowment of South Carolina	WQED Multimedia
United Way of the Midlands, South Carolina	United Way of Allegheny County
Humane Society Columbia	Western Pennsylvania Humane Society
South Carolina Cancer Alliance	Mario Lemiuex Foundation

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