Pathways to Prosocial Leadership: An Online Experiment on the Effects of External Subsidies and the Relative Price of Giving[1]

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Abstract

Leaders are a part of virtually every group and organization, and while they help solve the various collective action problems that groups face, they can also be unprincipled and incompetent, pursuing their own interests over those of the group. What types of circumstances foster prosocial leadership and motivate leaders to pursue group interests? In a modified dictator game (N = 798), we examine the effects of piece-rate subsidies (or pay per unit of work performed) and the relative price of giving (or the size of the benefit to others for giving) on prosocial behavior and norms about giving. We find that subsidies increase giving by leaders, that the relative price of giving is unrelated to prosocial behavior, and that neither affects norms about giving. Furthermore, the introduction and removal of a subsidy does not undermine giving over time. Our results imply that subsidies increase group welfare by motivating leaders to allocate a larger share of resources to group members.

Introduction

Social order hinges on the alignment of individual and collective interests (Hardin 1982; Hechter 1987; Olson 1965). When personal desires and group goals are in conflict, work team members are less productive, bystanders free-ride on the efforts of protestors, and residents fail to police their own neighborhoods (Lichbach 1995). As Mancur Olson observed, "Unless the number of individuals in a group is quite small, or unless there is coercion or some other special device to make individuals act in their common interest…self-interested individuals will not act to achieve their common or group interest (1965: 2)." This tension at the intersection of individual and group interests is what underpins collective action problems (Kollock 1998; Olson 1965; Hardin 1982).

There are two common solutions to collective action problems. The first is the emergence of spontaneous cooperation without top-down interventions (Axelrod 1984; Taylor 1976). When actors do not heavily discount future rewards and repeated interactions are indefinite, social order can emerge spontaneously due to the long-term, mutual benefits of cooperation (Taylor 1976). Although common, spontaneous cooperation—and the *decentralized* solution to collective action problems more generally—becomes difficult to sustain as groups grow in size and as the spatial distance between group members increases (Ostrom 1990). Under these conditions, monitoring capacity declines, anonymity and the benefits of malfeasance increase, and the costs of maintaining institutions of informal social control, such as peer-punishment systems and reputation mechanisms, rise (Kollock 1998; Lichbach 1995).

The limits of spontaneous cooperation often motivate the second, *centralized*, solution to collective action problems (Ahlquist and Levi 2011). When faced with a social dilemma, groups will sometimes designate a single person to absorb monitoring costs and mete out punishments (or rewards) to individuals who underproduce or overuse common goods (Kollock 1998; Ostrom 1990; van Vugt 2006). Within the centralist tradition, several studies have explored how groups designate single leaders to solve collective action problems (Harrell 2018, 2019; Harrell and Simpson 2016) and, when they do, the impact these leaders have on social order (Arbak and Villeval 2013; Grossman and Baldassarri 2012; Baldassarri and Grossman 2011; Kosfeld and Rustagi 2015; Rivas and Sutter 2011; Van Vugt and De Cremer 1999). What these studies show is that prosocial individuals tend to increase their contributions to groups after ascending to leadership positions (Harrell and Simpson 2016), and encourage contributions to collective action by non-leaders (Harrell 2019). When leaders are selected endogenously (e.g., through democratic elections), group members are also more likely to contribute to collective efforts than members of groups whose leaders were selected exogenously (Andersson et al. 2020; Arbak and Villeval 2013; Bendahan et al. 2015; Berger et al. 2020; Grossman and Baldassarri 2012; Baldassarri and Grossman 2011; Rivas and Sutter 2011).

While centralizing power in a single individual can be an effective solution to collective action problems (Bass and Bass 2008; Yuki and van Fleet 1992), previous research has found that leaders can be unprincipled (Ahlquist and Levi 2011), self-regarding (Arbak and Villeval 2013), overconfident (Hayward et al. 2006), and incompetent (Hogan et al. 1994). When leaders are granted the authority not only to monitor and sanction group members, but also to redistribute wealth through goods and services, it is common for leaders to use their positions of power to extract resources for their own personal gain (Hogan and Kaiser 2005; Hogan et al. 1994). Political actors who draw from public coffers filled with tax revenues, corporate CEOs who pocket economic stimuli rather than redistribute them, and community leaders who retain foreign development aid for themselves and their allies are examples of this phenomenon. As the literature shows, the prospects of an unprincipled and incompetent leader can be devastating for any community, group, or organization (Ahlquist and Levi 2011).

In light of the compounding rewards of benevolent leadership undermined by potentially malevolent leadership, what extrinsic devices can be used to induce prosocial behavior in leaders who have been exogenously selected to redistribute wealth? To answer this question, we investigate whether and under what conditions private rewards foster prosocial leadership (i.e., giving), and whether the introduction and subsequent removal of private rewards have unintended consequences for the welfare of group members.

We focus on exogenous leader selection, private external subsidies, and the removal of subsidies for two reasons.[1] First, exogenous leader selection, or when leaders are imposed on group members without their consent, is common in the real-world: monarchs, foreign rulers, CEOs, and even small business owners are examples of leaders who have been exogenously imposed on groups (Hechter 2013).[2] And these kinds of leaders generally harm groups more in the long-run than endogenously chosen leaders (Grossman and Baldassarri 2012; Baldassarri and Grossman 2011; Harrell 2018, 2019; Harrell and Simpson 2016), regardless of an endogenously selected leader's potential motives, be it personal gain, benevolence, or social image (Arbak and Villeval 2013). It is thus crucial to identify circumstances that are conducive to prosocial leadership among exogenously selected leaders.

Second, a substantial literature suggests that extrinsic motivators (Deci 1971; Deci et al. 1999) and economic incentives (Bowles and Polania-Reyes 2012; Frey and Jegen 2001) designed to promote prosocial behavior can be counterproductive: economic incentives sometimes

[1] We treat "external subsidy," "private transfer," and "piece-rate transfer" synonymously, and use all three terms interchangeably to mean that leaders are paid per unit of work performed.
[2] Endogenous leader selection occurs when members of a group volunteer for a leadership position and are selected by other group members to lead.

replace—or *crowd out*—social preferences, and reduce an actor's intrinsic desire to perform a given act (Titmuss 1970). This is an important question to address if one's solution to prosocial leadership hinges on extrinsic motivators and economic incentives. Private external subsidies, such as performance-based pay and stock options, are finite and volatile. In this paper, we examine whether the introduction and subsequent removal of a private external subsidy undermines—or *crowds out*—giving by leaders.

To achieve these goals, we designed an online experiment in which subjects played two rounds of a modified dictator game. Dictators, or "leaders" in our experiment, completed a computerized real-effort task and then divided the earned tokens between themselves and recipients (Gill and Prowse 2012). Randomly selected leaders and recipients were exposed to three conditions: a private subsidy condition, which varied the introduction and subsequent removal of a private (piece-rate) transfer that paid leaders per unit of work performed; a relative price of giving condition, which varied the size of the benefit to others for giving (Andreoni and Miller 2002; Fisman et al. 2015; Jakiela 2013); and a norm elicitation condition, which varied the introduction of a task to measure subjects' normative judgments of what constitutes appropriate leader behavior (Krupka and Weber 2013).

Our experiment yields two principal discoveries. First, we find that external subsidies increase giving by leaders, that the relative price of giving is unrelated to prosocial leadership, and that norms about giving are not a function of external subsidies or the relative price of giving. Second, external subsidies do not undermine group welfare over time: the introduction and subsequent removal of a private transfer did not change the behavior of group leaders. In other words, paying leaders privately for their efforts creates conditions conducive to group welfare.

Experimental Design and Theoretical Expectations

Our experiment captures two key components for studying the effects of external subsidies on prosocial leadership: (1) leaders have complete discretion over how to distribute wealth to group members, and (2) external subsidies, or piece-rate transfers, are private and temporary. The first assumption allows for the possibility that external subsidies fail to increase group welfare because leaders retain private transfers for themselves and do not distribute a greater share of public resources to group members. The second assumption allows us to examine the effects of volatile private transfers, which is important given the classic proposition that extrinsic rewards undermine intrinsic motivation in the long run (Deci 1971).

Basic Experimental Framework and Justification

The game consisted of two stages, stage A and stage B. In both stages, subjects were matched into pairs and played a modified version of the dictator game (Hoffman et al. 1996). In our experiment, one subject, called the *Leader*, decides how much of the available surplus he or she wants to give to another subject, called the *Recipient*. Recipients make no decisions and are thus passive participants in the game. The dictator game was originally designed to test whether rational actors would seize an entire surplus if given the chance. A robust result in experimental economics is that many subjects playing the dictator game do not take the entire surplus and are willing to share it with others (see Engel 2011 for a review).

Note that our experimental setting differs from the standard dictator game in a number of important respects. First, as mentioned above, surplus allocation is determined by the leaders in two stages. This allows us to study how the removal of private transfers affects behavior. Second, leaders face a pure redistribution problem under some treatment conditions (i.e., the recipient receives one token for each token the leader decides to give), while under other treatment conditions each token the leader gives to a recipient is multiplied by two (Andreoni and Miller 2002). This allows us to study how external subsidies affect behavior under different conditions. Third, we introduce an additional asymmetry between the leader and the recipient: the surplus available for distribution is generated by the leader in a real-effort task (Gill and Prowse 2012). The task consists of 50 math problems in which leaders sum three 3-digit integers. Leaders have 120 seconds to answer as many math problems as possible. Each correctly answered math problem generates a certain number of tokens (each worth \$0.01 USD), depending on the manipulation. We implement the real-effort task to underscore leaders and their role: leaders decide how to distribute surplus within the group and, given their social position, are expected to provide distributive services to group members. From an experimental design perspective, the real-effort task does not exhibit strong learning effects.[3] A subject's ability to generate tokens is thus comparable across stages.

In the literature, leaders are generally conceptualized as heads of groups and organizations who use coercion, incentives, and persuasion to motivate group members to produce desired outcomes (Ahlquist and Levi 2011). Because leaders are in positions of power, they often have discretion to redistribute resources to group members in certain institutional contexts (Bass and Bass 2008). The strength of our experimental design is that it allows us to rigorously test and evaluate one aspect of leadership, specifically the redistribution of resources earned by leaders. While our research cannot speak to real-world situations in which resources are earned by group members and then redistributed by leaders (e.g., church fundraising), we can

[3] Gill and Prowse (2012) find that subjects exhibit learning effects. They also test whether higher monetary incentives lead to a larger number of completed tasks and find a small positive effect. In our experiment, the average number of correctly answered math problems was 9.23 in stage A and 11.11 in stage B. A linear regression model regressing the number of completed tasks on a dummy variable for stage and the experimental conditions shows that the number of correctly answered math problems were statistically comparable across experimental conditions but not stages, $\Delta M = 1.87$, $SE = 0.11$, $p < .001$.

realistically speak to prosocial leadership in groups and organizations in which the skills and abilities of leaders generate a surplus for organizations. Examples include CEOs who generate profits for corporations, managers of common-pool resources who produce a surplus of resources, and leaders of nonprofit organizations who successfully raise funds.

Experimental Manipulations

Figure 1 depicts the timeline and design of the experiment. After reading the instructions and evaluating a series of questions and answers about the experiment, subjects were randomly assigned to either a leader role or a passive recipient role. The roles remained fixed for the duration of the experiment. In both stages, leaders generated tokens during the real-effort task and then distributed tokens between themselves and recipients. Finally, leaders and recipients were randomly paired in stage A and then randomly paired again in stage B. At the beginning, subjects were told that the experiment consisted of different parts, but were not informed about the specific details of each part at that point in the study.

With this experimental framework, we manipulated three variables, each consisting of two-to-three levels, resulting in a $2 \times 3 \times 3$ within-subjects factorial design. The first variable, the *Relative price of giving*, randomly varied the magnitude of a multiplier. We manipulated two values of the multiplier: 1 or 2. When the multiplier was 1, each token the leader allocated to recipients was worth 1 token to the recipient. If the multiplier was 2, each token the leader allocated to recipients was doubled and worth 2 tokens to the recipient. In economics, multipliers are sometimes referred to as the "multiplier of redistribution" (Grech and Nax 2020) or the "relative price of redistribution" (Fisman et al. 2015). Following the work of Andreoni and Miller (2002), multipliers have been used in dictator games to reveal other-regarding preferences and to measure distributional preferences: tradeoffs between self-interest and altruism, and between equality and efficiency (Jakiela 2013).

The second variable, *Subsidy*, randomly varied the presence or absence of an external subsidy (or private transfer). In the control condition, *No subsidy*, each correctly answered math problem yielded 2 tokens. This occurred in stage A and stage B, as well as in both relative price of giving conditions (i.e., multipliers of 1 and 2). In other words, leaders earn a piece-rate of 2 in the *No subsidy* condition. In the treatment conditions, leaders earned an additional token for each correctly answered math problem (a piece-rate of 3). This additional token was either introduced in stage A and then removed in stage B, the *Stage A removal* condition, or absent in stage A and then introduced in stage B, the *Stage B introduction* condition. In the presence of an external subsidy, each correctly answered math problem generated 3 tokens: 3 tokens in stage A and 2 tokens in stage B for the *Stage A removal* condition, and 2 tokens in stage A and 3 tokens in stage B for the *Stage B introduction* condition. The extra token earned in the treatment conditions can be interpreted as a private transfer or an external subsidy, such as performance-based pay.[4] Finally, the two treatment conditions allow us to examine whether the effects of external subsidies are the same regardless of the stage in which they are introduced.

Because our goal was to create a situation in which leaders perceived themselves as such, we did not use neutral language. The instructions described the situation as we do in the present manuscript, referring to the different roles as *leaders* and *recipients*. The two tokens earned in the real-effort task were referred to as *community tokens*. We told subjects that leaders could

[4] In classical dictator games, leaders are "endowed" with *x* number of monetary units to keep or to give to recipients; a design feature that conflates the endowment itself with external subsidies. To address this issue, we endogenize the "endowment" with a real-effort task. This task is the leader's labor effort. Some leaders are more principled than others and must decide—given their ability—how to allocate earned endowments, if at all, within the group.

freely distribute these tokens. In the presence of private transfers when leaders earned 3 tokens per math problem, the extra token was called a *private token*. Leaders were told that they would receive the private token as compensation for performing the real-effort task. In line with this, private tokens could not be given to the recipient. Our design, in other words, was conducive to giving in that the experimental situation framed roles as *leaders* and *recipients* and tokens as *community tokens* (Smith and Wilson 2018). In this regard, the dictator game is ideal for studying giving and prosocial behavior (Hoffman et al. 1996).

The third variable, *Norm elicitation*, randomly varied the presence or absence of a norm elicitation task. Norms are generally defined as rules with some degree of consensus that prescribe or proscribe certain actions and behaviors (Coleman 1990; Horne and Mollborn 2020). Our focus here is on injunctive social norms, or what one "should" or "should not" do (Hechter and Opp 2001), as opposed to common customs or typical behaviors, which norm scholars refer to as "descriptive" norms (Bicchieri 2006). The present study is directly concerned with norms about giving, or the socially appropriate amount of tokens that leaders should or ought to send to recipients.

To measure norms, we rely on the approach developed by Krupka and Weber (2013). Subjects were presented with a description of a choice environment that included actions available in the dictator game. The actions involved the allocation of 50 community tokens under 11 different hypothetical outcomes. The outcomes decreased for leaders (increased for recipients) in increments of 5 for each subsequent outcome, starting with "Leader gets 50, Recipient gets 0" and ending with "Leader gets 0, Recipient gets 50", with "Leader gets 25, Recipient gets 25" as the midpoint (see Krupka and Weber 2013). Subjects were then asked to judge the social appropriateness of each action with a 4-point item ranging from "very socially inappropriate", "somewhat socially inappropriate", "somewhat socially appropriate", to "very socially appropriate". Subjects were incentivized not to reveal their own personal preferences but to match the responses of others, resulting in a pure matching coordination game. Subjects earned money if their evaluations of the social appropriateness of different allocations matched the responses of most others.

We thus manipulated three levels of the *Norm elicitation* variable. In the control condition, *No elicitation*, norms about giving were not elicited from subjects. In the two treatment conditions, norms about giving were elicited either at the end of stage A, *Stage A elicitation*, or at the end of stage B, *Stage B elicitation*. These manipulations allowed us to control for and examine possible priming effects of norm elicitation on giving, and to investigate whether norms about giving varied by the stage at which they were elicited. Finally, we elicited norms at the end of a stage to (1) logically follow the behaviors observed in the modified dictator game, and (2) ensure that norm elicitation did not bias the allocation of tokens in either stage A or stage B.[5]

The last part of the experiment elicited demographic information from the subjects and included questions about age, gender, and level of education. Further details on how these variables were measured can be found in the Supplemental Materials online.

We conducted a pilot study to evaluate our design, which was a simplified version of the experiment shown in Figure 1. After collecting data, we realized that certain changes were necessary, such as manipulating the norm elicitation task and collecting a larger sample size. Although certain design elements of the pilot study and the main study differ, the pilot study produced substantively similar results to the main study, even though the estimates for external

[5] In theory, stage A norm elicitation could influence decision-making in stage B. We explore this empirically in the results section.

subsidies and the relative price of giving were statistically non-significant. This also increases our confidence in the robustness of the main findings presented below. The pilot study is described in more detail in the Supplemental Materials online.

Hypotheses

In this section, we derive a set of hypotheses to guide our data analysis. In the Supplemental Materials online, we formally specify the utility functions discussed below. It is apparent that selfish leaders, whose only goal is to maximize their own payoffs, should keep all tokens for themselves. The literature offers various reasons why we might still observe some giving, such as altruism, fairness, or preferences for efficiency (Andreoni 1990; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). Indeed, Andreoni and Miller (2002) have shown that subjects' altruistic behavior is often consistent with rationality, or the existence of a well-behaved utility function that rationalizes subjects' choices in different situations.

To allow for the possibility that preferences go beyond pure payoff maximization, we assume that a leader's utility depends on both their own payoff and the payoff of the recipient (Andreoni and Miller 2002). We also allow for reference-dependent preferences (Köszegi and Rabin 2006). That is, the utility derived from the current earnings potentially depends on some reference payoff. The reason we are interested in reference points is that they allow us to capture potential crowding out effects of giving. Reference points affect a leader's giving in three ways: (1) when reference points are low, they have no effect on giving; (2) at intermediate reference points, the leader chooses to keep their earnings the same as in the previous stage; and (3) when reference points are high, the leader accepts some reduction in earnings between stages to accommodate their altruism. These observations have direct implications for the various experimental manipulations. First, giving occurs as long as the leader's altruism is not zero, and increases with their level of altruism. Since the piece-rate subsidy is a function of the number of tokens generated in the real-effort task, the following hypothesis is proposed:

> **Hypothesis 1:** Private transfers increase community welfare. That is, external subsidies increase the share of tokens given by leaders to recipients.

Similarly, if the relative price of giving decreases (i.e., the multiplier increases), the amount of tokens sent by leaders to recipients increases. Thus:

> **Hypothesis 2:** Community welfare increases as the relative price of giving decreases. That is, larger multipliers increase the share of tokens given by leaders to recipients.

Second, if reference point effects exist, they will occur in the transition from stage A to stage B when a subsidy is introduced and then removed, but there will be no reference point effects in the *No subsidy* condition. While the proposed mechanism differs from those found in the larger crowding-out literature—reference points vs. decrease in intrinsic motivation—our expectations are similar to those found in the literature (Deci 1971; Deci et al. 1999; Frey and Jegen 2001; Irwin et al. 2014; Mulder et al. 2006). Thus:

> **Hypothesis 3:** Private transfers reduce giving after removal. That is, leaders in the *Stage A removal* condition give a smaller share of tokens to recipients in stage B than leaders in the *No subsidy* condition.

Finally, we expect that external transfers and the relative price of giving will change norms about the socially appropriate amount of giving. As Horne and Mollborn (2020) write, "when the consequences of a behavior or the social understandings of those consequences change, norms shift in response (p. 473)." Specifically, the polarity of the norm—or the fraction of tokens that leaders should or ought to give to recipients—will increase in the presence of an external subsidy and as the relative price of giving decreases, since both should serve as normative cues about giving as a socially appropriate behavior (Cialdini et al. 1991). This leads to our final two hypotheses:

Hypothesis 4: Private transfers reinforce norms about giving. That is, among leaders and recipients, external subsidies increase the socially appropriate share of tokens that leaders ought to give to recipients.

Hypothesis 5: The relative price of giving undermines norms about giving. That is, among leaders and recipients, larger multipliers increase the socially appropriate share of tokens that leaders ought to give to recipients.

Procedures

From November 2021 to December 2022, 798 members of the Prolific.co subject pool participated in 33 online sessions conducted through SoPHIELabs (www.sophielabs.com).[6] Each session had between 10 and 50 subjects, with an average size of 28 participants. Prolific is an online platform similar to Amazon.com's Mechanical Turk, where people can complete tasks, including experiments and surveys, in exchange for payment (Palan and Schitter 2018). Payments on Prolific consist of a fixed hourly wage plus potential incentives earned through participation. Cash earnings ranged from $3.00 to $7.23, with an average of $3.62, based on tokens earned in stage A and stage B, payments from the norm elicitation task, and a show-up fee of $3.00.[7] A typical session lasted 25 minutes.

[6] 148 Prolific.co users partially completed or dropped out of the experiment. To assess the extent to which attrition (or dropouts) biased giving by leaders, we estimated full information maximum likelihood linear regression models for missing data. This resulted in 74 additional leader observations. These additional analyses, which can be found in the Supplemental Materials online, yielded substantively similar results to those presented in Table 2.

[7] Meta-analyses show that differences in stakes across studies do not affect the behavior of dictators (Engel 2011).

We used Prolific.co's online recruitment system to draw a non-probability sample of U.S. adults. In terms of relative majorities, 50.07 percent of the subjects were male, 56.20 percent had a bachelor's degree or greater, and 44.19 percent were between the ages of 18 and 34 (M = 39.07, SD = 12.78, min = 18, max = 84). For these demographic characteristics, we observe covariate balance across experimental treatments.[8] A sample selection of instructions for the experiment can be found in the Supplemental Materials online.

Results

Giving by Leaders

We begin by presenting descriptive statistics from stage A and stage B. Using this as a benchmark, we then estimate models predicting giving by leaders, and then finish with an analysis of norms about giving. To operationalize giving, we use the share or proportion of community tokens that leaders send to recipients.

Table 1 provides information on the decisions made by leaders across experimental conditions. The results show that, on average, the proportion of tokens sent by leaders to recipients decreases from 30 percent in stage A to 25 percent in stage B, which is a common finding in the literature on repeated dictator games (Engel 2011). Table 1 also shows that there is considerable variation within *and* between stages, depending on the experimental conditions. In particular, the proportion of tokens sent tends to be higher in the presence of subsidies than in the absence of subsidies. The decisions made by leaders in the two stages also suggest that there is

[8] To test whether covariates are balanced across experimental conditions, we estimated multinomial logit and logit models regressing nominal variables for each experimental manipulation on a vector of demographic variables. Overall model fit is statistically non-significant for *Subsidy*, $\chi^2(14, N = 757) = 22.48, p > .05$, *Relative price of giving*, $\chi^2(5, N = 753) = 1.08, p > .10$, and *Norm elicitation*, $\chi^2(14, N = 757) = 11.95, p > .10$.

no consistent effect of the *Relative price of giving* or *Norm elicitation* on the proportion of tokens sent.

To examine whether these patterns are statistically significant, we estimate two-level mixed-effects models in which stages are nested within leaders (see Table 2). In model 1, Table 2, we find that the main effects of *Subsidy*, $\chi^2(2, N = 798) = 0.76$, $p > .10$, and *Relative price of giving*, $b = -0.011$, $SE = 0.020$, $p > .10$, are statistically non-significant, which does not support Hypothesis 1 or Hypothesis 2. Consistent with findings from previous research (Engel 2011), we observe a statistically significant decrease in giving across stages, $b = -0.039$, $SE = 0.009$, $p < .001$, and a decrease in giving as the number of real-effort tasks completed increases, $b = -0.005$, $SE = 0.002$, $p < .01$. Finally, we find that the norm elicitation task is not statistically significantly related to giving, $\chi^2(2, N = 798) = 0.07$, $p > .10$.

While model 1 provides weak support for Hypothesis 1, the coefficients reflect the effects of subsidies pooled across stages. Because the presence or absence of *Subsidy* treatments varies between stage A and stage B, model 2 includes interaction terms for *Subsidy* × *Stage*, yielding statistically significant effects, $\chi^2(2, N = 798) = 21.18$, $p < .001$.

We plot the interaction effects in Figure 2, which show that the *Stage A removal* treatment leads to greater amounts of giving in stage A than either the *No subsidy* control or the *Stage B introduction* treatment (both of which yield comparable levels of giving). Recall that in stage A, leaders do not receive private transfers in the *No subsidy* or *Stage B introduction* conditions. In stage B, we see that giving increases in the *Stage B introduction* condition, while giving decreases to comparable levels in the *No subsidy* and *Stage A removal* conditions. These dynamics provide support for Hypothesis 1 (i.e., private transfers increase community welfare), but not for Hypothesis 3 (i.e., private transfers reduce giving over time), since leaders in the

Stage A removal and *No subsidy* conditions send a comparable fraction of tokens to recipients in stage B.

Since we find no support for Hypothesis 3, we pool the two treatment conditions together, *Stage A removal* and *Stage B introduction*, to examine the overall effect of *Subsidy* on giving. We replace this new *Subsidy* variable with the one from model 1, Table 2, and re-estimate the model. The coefficient is positive and statistically significant, $b = 0.045$, $SE = 0.013$, $p < .01$, indicating that private transfers increase giving by leaders, providing further support for Hypothesis 1.[9]

Finally, we also explored a number of robustness and sensitivity checks (see the Supplemental Materials online). These checks support the conclusions presented here.

Norms about Giving

We now review the results of our norm elicitation task, which measures leaders' and recipients' beliefs about what everyone else thinks is an appropriate amount of giving by leaders ($N = 5{,}577$, $M = 2.39$, $SD = 1.20$, Min = 1, Max = 4). Figure 3 reports the proportion of socially appropriate ratings selected for the leader's share of tokens, pooled across the two roles (leaders and passive recipients) and experimental conditions. Figure 3 shows that there is some consensus about the leader's share of tokens. When the share of tokens for leaders is 0 or 50, the majority of subjects favor a socially inappropriate opinion, with the bulk of ratings "very socially inappropriate". When the share of leader tokens is 20, 25, or 30, we see a similar level of

[9] Note that in the private subsidy condition, a recipient may be worse off relative to the leader while receiving more community tokens in absolute terms. Although it is difficult to compare the total amount of tokens in the *No subsidy* (community tokens only) and private subsidy (community tokens plus private tokens) conditions because private tokens cannot be sent to recipients, we find that recipients benefit from an external subsidy only in the absolute amount of community tokens sent, not in their token position relative to leaders. See the Supplemental Materials online for the results. We thank one of the anonymous reviewers for bringing this issue to our attention.

consensus, but in the opposite direction (i.e., socially appropriate). Where we see the most disagreement is for values where leaders get some (10 and 15) or most (35) of the tokens. In these situations, there seem to be polarized opinions about the socially appropriate share of tokens.

In Table 3, we evaluate Hypotheses 4 and 5 by regressing ratings of socially appropriate shares of tokens on the leader's share of tokens (e.g., 25-25), role (leader vs. recipient), the key experimental conditions, and design controls. The model presented in Table 3 is a multilevel ordered logit model in which norm ratings are nested within subjects. The model shows that only the leader's share of tokens is statistically significantly related to norm ratings, $\chi^2(10, N = 5577) = 497.21$, $p < .001$. Thus, neither Hypothesis 4 nor Hypothesis 5 is supported by the data. We also find that relevant interaction effects added to model 1 yield statistically non-significant familywise tests, including *Leader's share of tokens* × *Subsidy*, $\chi^2(20, N = 5577) = 25.60$, $p > .10$, *Leader's share of tokens* × *Relative price of giving*, $\chi^2(10, N = 5577) = 15.47$, $p > .10$, and *Leader's share of tokens* × *Norm elicitation*, $\chi^2(10, N = 5577) = 13.92$, $p > .10$. Although not hypothesized, the only interaction effect driving norm ratings is *Leader's share of tokens* × *Leader*, $\chi^2(10, N = 5577) = 28.44$, $p < .01$, suggesting that leaders differ from recipients in the norms about giving to which they subscribe. This indicates that leaders believe that it is more socially appropriate for leaders to have a greater share of tokens than recipients (see Figure 4).[10]

Interestingly, giving by leaders across two stages of the dictator game does not fully parallel the norms measured in our norm elicitation task. The average share of tokens sent by leaders to recipients is 0.30 in stage A and 0.25 in stage B. Depending on the stage, this share of

[10] Note that including interactions between *Leader's share of tokens* × *Leader* × *Subsidy*, $\chi^2(20, N = 5577) = 20.07$, $p > .10$, or *Leader's share of tokens* × *Leader* × *Relative price of giving*, $\chi^2(10, N = 5577) = 10.02$, $p > .10$, does not improve model fit.

tokens roughly corresponds to a 35-15 split in the norm elicitation task (i.e., leader gets 35, recipient gets 15), which is the share of tokens that exhibits the greatest amount of disagreement between subjects (see Figure 3). Norms, in other words, do not appear to determine actual giving, since the most socially appropriate share of tokens with the greatest amount of consensus is a 25-25 split (or a 0.50 share of tokens). Most leaders give a smaller share of their tokens than what norms about giving dictate. This finding represents a situation in which leaders' preferences do not align with what they think other people believe leaders should or should not do. In short, it seems that leaders find a compromise between their own self-interest and norms about giving.

Discussion and Conclusion

Prior research shows that designating a single group leader to monitor and sanction group members promotes social order in a variety of ways (Grossman and Baldassarri 2012; Baldassarri and Grossman 2011; Harrell 2018, 2019; Harrell and Simpson 2016). But, there are downsides to leadership (Ahlquist and Levi 2011; Arbak and Villeval 2013; Hayward et al. 2006; Hogan et al. 1994). When leaders are granted the authority to redistribute wealth through goods and services, it is common for leaders to use their positions of power to extract resources for their own personal gain (Bendahan et al. 2015; Berger et al. 2020). Here, we consider whether and how external subsidies and the relative price of giving promote principled, other-regarding leadership. In doing so, we offer a more thorough understanding of the pathways to prosocial leadership.

We designed an online experiment in which private transfers and the relative price of giving varied over two rounds of play in a modified dictator game, and measured norms about giving with an incentivized norm elicitation task. A sample of Prolific.co users served as our subjects. With this data and design, we found that external subsidies increase giving: the benefits of a private transfer are effectively redistributed to group members through greater sharing of public resources. Unlike external subsidies, we found no difference in the behavior of leaders across different values of the relative price of giving, a result that suggests how important private transfers are for uncovering prosocial leadership. We also found that external subsidies did not undermine giving over time or affect norms about giving: the introduction and subsequent removal of a private transfer did not change the behavior of leaders or norms about socially appropriate amounts of giving.

Our results have two broad implications. First, a key finding of our experiment is that external subsidies, operationalized as private piece-rate transfers, increase group welfare. Private transfers motivate leaders to distribute a greater share of public resources to group members, thereby extending the benefits of external subsidies to the less powerful. This result is particularly important in light of recent research showing that wealth transfers are a boon to local economies and the overall psychological well-being of recipients (Haushofer and Shapiro 2016).

Second, another key finding of our experiment is that the introduction and subsequent removal of an external subsidy does not crowd out or undermine giving. Leaders do not take public resources from group members in order to compensate for the volatility of private transfers. Our results also imply that neither a shift in reference points (Köszegi and Rabin 2006) nor a decrease in intrinsic motivations (Frey and Jegen 2001) follows from the removal of an external subsidy (assuming that the observed behavior captures intrinsic motivations). Moreover, norms about giving are strikingly similar across the various experimental manipulations. Taken together, we find that external subsidies do not weaken preferences for—or norms about—giving in the short or long run, with no evidence of downsides to external subsidies; an important finding that supports recent evidence from field experiments (e.g., Casey et al. 2012).

Yet, a common finding in economics, political science, and sociology is that incentives designed to promote prosocial behavior, such as giving, are sometimes counterproductive in that these incentives counterintuitively crowd out social preferences, social norms, and intrinsic motivations (Frey and Jegen 2001). In some ways, our findings are consistent with this research. A recent review of the literature suggests that incentives can promote (i.e., crowd in) social preferences and social norms depending on the conditions (Bowles and Polania-Reyes 2012): (1) when an incentive provides information about the principal's intentions or type (administering rewards instead of fines), it may trigger the target's social preferences to align with the incentive (in this case, a reward), or (2) when an incentive frames decision situations and provides cues about appropriate behavior, social preferences and giving behavior may align with the frame. In relation to the present study, our incentive is a private piece-rate transfer for completing a task. And our design is conducive to giving in that the experimental situation frames roles as *leader* and *recipient*, and tokens as *community tokens*. Both of these parallel conditions conducive to "crowding in". However, crowding out or shifting reference points may require longer time frames to undermine giving, not just minutes in an experimental setting, but years in the real world. As a result, future research should examine the long-term effects of reference point shifts over different time frames.

Our results also have implications for research on leadership more broadly. Ostrom (1990), for instance, identified self-governance as critical to the maintenance of common goods. Ostrom (1990) showed that "…neither the state nor the market is uniformly successful in enabling individuals to sustain long-term, productive use of natural resource systems (p. 1)." Instead, Ostrom finds that collective-choice arrangements, monitoring, graduated sanctions, and other community-based social mechanisms are sufficient to avoid a "tragedy of the commons."

In the context of the present study, Ostrom precisely states the necessity and power of leadership. Our study suggests that communities are capable of developing prosocial leadership without undermining community welfare or destroying local institutions. Organizations can accomplish this task by, first, designating group leaders to oversee common goods (Grossman and Baldassarri 2012; Baldassarri and Grossman 2011; Harrell 2018, 2019; Harrell and Simpson 2016) and, second, providing group leaders with private transfers for their work. Under these conditions, the benefits of private transfers are effectively passed on to group members through the sharing of public resources by group leaders. Then again, our proposed solution may be context dependent (Berger 2023; Herrmann et al. 2008). For instance, in situations where corruption is prevalent, local leaders may abuse their power and misappropriate public resources despite being paid for their efforts. Exploring the relationship between context, private transfers, and prosocial leadership is an important goal for future research.

A remaining question is how to interpret the external validity of our results. A common criticism of laboratory experiments is the extent to which behavior observed in the lab generalizes to other populations, settings, or situations. We address this classic criticism in two ways. First, laboratory and online experiments are best used when the abstract nature of an experiment allows researchers to (1) explore important theoretical features of an empirical phenomenon, or (2) test general theory (Jackson and Cox 2013). Both were driving forces behind our study. Second, we believe that our online experiment encapsulates common situations in which group leaders are tasked with distributing wealth among group members. Given the abstract nature of our experiment, we would argue that this includes—but is not limited to—heads of rentier states, local development projects, nonprofit organizations, and corporations.

To conclude, if groups wish to promote the benevolence of their leaders, group members must decide whether to subsidize the work of their leaders. Our results imply that group members would do well to subsidize their leader’s efforts.

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Table 1. Leaders' Decisions in Stage A and Stage B of the Modified Dictator Game

				Stage A	Stage B
Subsidy	Relative Price of Giving	Norm Elicitation	*N*	*M* (*SD*)	*M* (*SD*)
No subsidy	Multiplier × 1	No elicitation	22	0.26 (0.23)	0.19 (0.22)
	Multiplier × 2	No elicitation	22	0.31 (0.22)	0.23 (0.23)
No subsidy	Multiplier × 1	Stage A elicitation	21	0.29 (0.22)	0.24 (0.19)
	Multiplier × 2	Stage A elicitation	21	0.26 (0.19)	0.20 (0.19)
No subsidy	Multiplier × 1	Stage B elicitation	21	0.36 (0.23)	0.30 (0.21)
	Multiplier × 2	Stage B elicitation	22	0.28 (0.19)	0.28 (0.24)
Stage A removal	Multiplier × 1	No elicitation	22	0.33 (0.27)	0.22 (0.22)
	Multiplier × 2	No elicitation	22	0.42 (0.25)	0.25 (0.19)
Stage A removal	Multiplier × 1	Stage A elicitation	31	0.35 (0.21)	0.32 (0.21)
	Multiplier × 2	Stage A elicitation	20	0.25 (0.22)	0.17 (0.20)
Stage A removal	Multiplier × 1	Stage B elicitation	24	0.27 (0.24)	0.17 (0.20)
	Multiplier × 2	Stage B elicitation	19	0.35 (0.29)	0.21 (0.18)
Stage B introduction	Multiplier × 1	No elicitation	21	0.33 (0.20)	0.34 (0.24)
	Multiplier × 2	No elicitation	21	0.27 (0.23)	0.25 (0.23)
Stage B introduction	Multiplier × 1	Stage A elicitation	21	0.25 (0.24)	0.25 (0.24)
	Multiplier × 2	Stage A elicitation	25	0.28 (0.17)	0.31 (0.21)
Stage B introduction	Multiplier × 1	Stage B elicitation	22	0.26 (0.23)	0.25 (0.22)
	Multiplier × 2	Stage B elicitation	22	0.26 (0.28)	0.23 (0.25)
Total			399	0.30 (0.23)	0.25 (0.22)

Notes : *N* = 399 leaders. Proportion of community tokens sent by leaders to recipients shown in Stage A and Stage B columns.

Table 2. Multilevel Mixed-Effects Regression Models Predicting the Proportion of Community Tokens Sent in a Modified Dictator Game

	Model 1	Model 2
Subsidy (ref. = No subsidy)		
Stage A removal	0.015 (0.025)	0.038 (0.028)
Stage B introduction	0.024 (0.030)	-0.0004 (0.031)
Relative price of giving (ref. = Multiplier × 1)	-0.011 (0.020)	-0.011 (0.020)
Norm elicitation (ref. = No elicitation)		
Stage A elicitation	-0.004 (0.026)	-0.004 (0.026)
Stage B elicitation	-0.008 (0.030)	-0.008 (0.030)
Stage (ref. = Stage A)	-0.039*** (0.009)	-0.039** (0.015)
Subsidy × Stage		
Stage A removal × Stage		-0.046* (0.022)
Stage B introduction × Stage		0.049* (0.020)
Completed tasks	-0.005** (0.002)	-0.005** (0.002)
Intercept	0.385*** (0.051)	0.383*** (0.052)
Stage observations	798	798
Leader observations	399	399
SD (intercept)	0.184	0.185
SD (residuals)	0.126	0.123
Design controls	Yes	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note : unstandardized slopes (robust standard errors in parentheses). Design controls include two session-level variables: size of session and days since first session.

Table 3. Multilevel Ordered Logistic Regression Model of Ratings of Socially Appropriate Shares of Tokens

	Model 1
Leader's share of tokens (ref. = 0-50)	
5-45	0.17** (0.05)
10-40	0.76*** (0.07)
15-35	1.46*** (0.10)
20-30	2.37*** (0.14)
25-25	3.81*** (0.23)
30-20	1.72*** (0.17)
35-15	0.61*** (0.15)
40-10	-0.47** (0.16)
45-5	-1.46*** (0.18)
50-0	-2.02*** (0.19)
Leader (ref. = Recipient)	0.07 (0.13)
Subsidy (ref. = No subsidy)	
Stage A removal	-0.19 (0.16)
Stage B introduction	-0.22 (0.25)
Relative price of giving (ref. = Multiplier × 1)	-0.07 (0.14)
Norm elicitation (ref. = Stage A elicitation)	-0.06 (0.14)
Constant (Somewhat inappropriate)	-0.17 (0.44)
Constant (Somewhat appropriate)	1.27** (0.45)
Constant (Very appropriate)	2.72*** (0.46)
Norm observations	5577
Leader and recipient observations	507
SD (intercept)	1.38
Design controls	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note: log-odds ratios (robust standard errors in parentheses). Dependent variable is the rating of socially appropriate shares of tokens: "very socially inappropriate" (baseline), "somewhat socially inappropriate", "somewhat socially appropriate", and "very socially appropriate." Design controls include two session-level variables: size of session and days since first session.

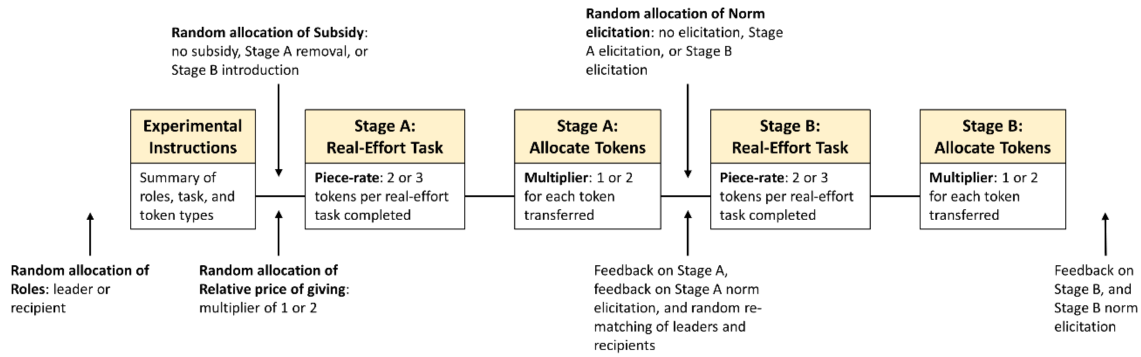

Figure 1. Timeline and Design of Experiment
Notes: The manipulations are *Subsidy* (i.e., no subsidy, stage A removal, or stage B introduction), *Relative price of giving* (i.e., multiplier × 1 or multiplier × 2), and Norm elicitation (i.e., no elicitation, stage A elicitation, or stage B elicitation). We used a 2 × 3 × 3 factorial design varying *Subsidy*, *Relative price of giving*, and *Norm elicitation*.

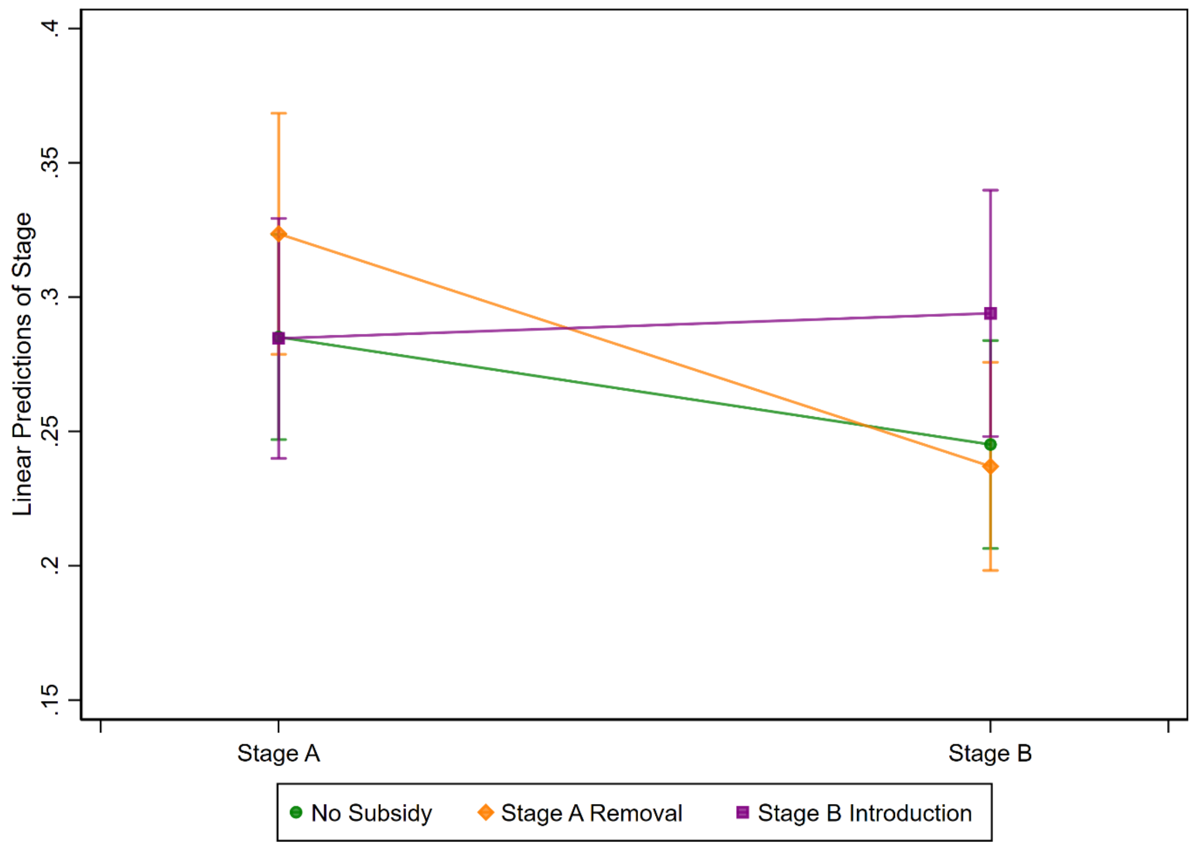

Figure 2. Interaction Effects between Subsidy and Stage on the Proportion of Community Tokens Sent by Leaders to Recipients (Table 2, Model 2)
Notes: All covariates constrained to their mean values.

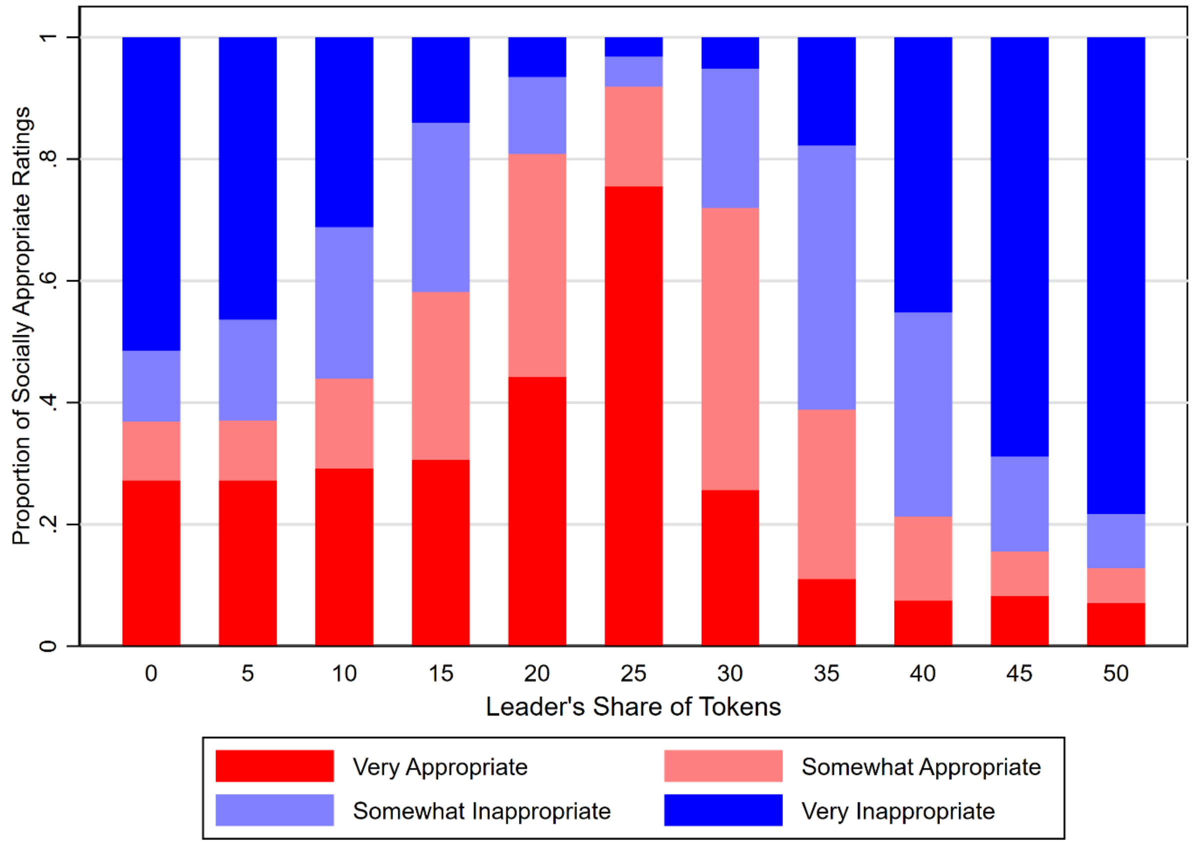

Figure 3. Proportion of ratings of socially appropriate shares of tokens pooled over all experimental conditions

Notes: The *x*-axis refers to the leader's share of tokens. For instance, 0 indicates that "Leader gets 0, Recipient gets 50", 25 indicates that "Leader gets 25, Recipients get 25", and 50 indicates that "Leader gets 50, Recipient gets 0." 507 subjects in the *Stage A elicitation* and *Stage B elicitation* conditions, yielding 5,577 ratings in total.

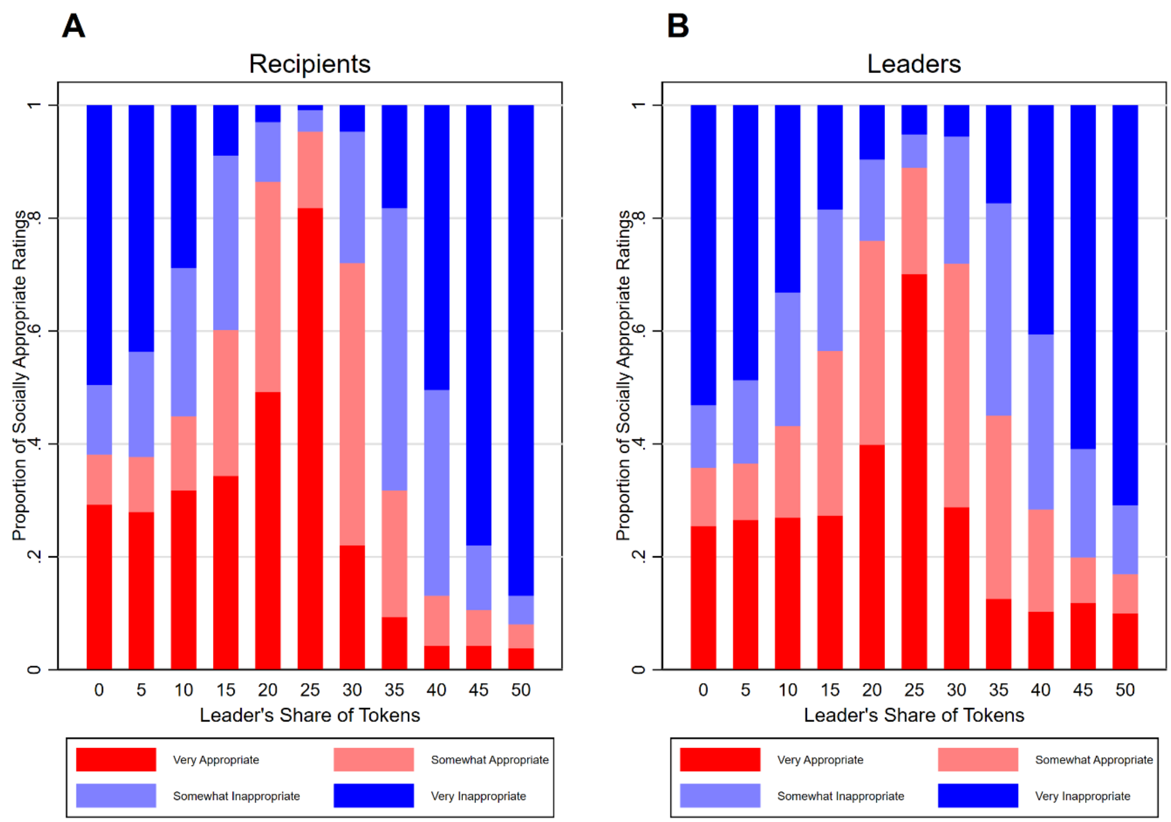

Figure 4. Proportion of ratings of socially appropriate shares of tokens by role (recipients in Panel A, leaders in Panel B)

Notes: For panels A and B, the *x*-axis in each experimental condition refers to the leader's share of tokens. For instance, 0 indicates that "Leader gets 0, Recipient gets 50", 25 indicates that "Leader gets 25, Recipients get 25", and 50 indicates that "Leader gets 50, Recipient gets 0." 507 subjects in the *Stage A elicitation* and *Stage B elicitation* conditions, yielding 5,577 ratings in total.

Supplemental Materials

Pathways to Prosocial Leadership:
An Online Experiment on the Effects of
External Subsidies and the Relative Price of Giving

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Utility Functions

To incorporate the possibility of preferences that go beyond pure payoff maximization, we assume that a leader's utility depends on their own payoff π_L as well as on the payoff of the recipient π_R. In addition, we allow for reference-dependent preferences (Köszegi and Rabin 2006). That is, the utility derived from the current earnings π_L potentially depends on some reference payoff, denoted by $\tilde{\pi}_L$. To sum up, the leaders' utility function takes the following form:

$$U_L = u_L(\pi_L, \pi_R; \tilde{\pi}_L) \quad (1)$$

The reason we are interested in reference points is that they allow us to capture potential crowding out effects with respect to leaders' willingness to give. In the *Stage A removal* condition, a piece-rate of 3 in stage A should lead to high earnings, compared to stage B where the piece-rate is 2. If subjects in the *Stage A removal* condition use their earnings in stage A as a reference point, this will reduce giving relative to the leaders' behavior observed in the stage B *No subsidy* condition.

We next describe these effects formally, using a Cobb-Douglas utility function. The only modification to the standard case is that there is also a term that depends on the reference point $\tilde{\pi}_L$:

$$U_L = (\pi_L - b\, max[0, \tilde{\pi}_L - \pi_L])^{1-a}\, {\pi_R}^a \quad (2)$$

Note that $a \in [0,1]$ is the weight the leader places on the recipient's payoff. Thus, it can be interpreted as the leader's level of altruism. Also, the utility derived from π_L is reduced when earnings fall short of the reference point $\tilde{\pi}_L$. If e is the number of tokens generated in the real-effort task, the budget constraint is $e = \pi_L + (1/m)\, \pi_R$, where m is the multiplier. The leader's optimal amount of giving follows from maximizing eq. 2, subject to the budget constraint. We summarize the result in Observation 1.

> **Observation 1:** Let $q = (1 - a)e$ and $r = \frac{ab}{1+b}$. The leader's optimal allocation of tokens is given by $\pi_L^* = q$ if $q > \tilde{\pi}_L$, $\pi_L^* = \tilde{\pi}_L$ if $q \leq \tilde{\pi}_L < \frac{q}{1-r}$, and $\pi_L^* = q + r\tilde{\pi}_L$ otherwise. It follows that the recipient's welfare $\pi_R^* = m(e - \pi_L^*)$ is increasing in the total amount of tokens e, the multiplier m, and the leader's level of altruism a, and decreasing in the level of the reference point $\tilde{\pi}_L$.

Observation 1 shows that the reference point can affect the leader's giving in three ways: (1) low levels of $\tilde{\pi}_L$ have no effect on giving; (2) at intermediate levels of $\tilde{\pi}_L$, the leader chooses to keep their earnings the same as in the previous period; and (3) at high levels of $\tilde{\pi}_L$, the leader accepts some reduction in earnings between stages to accommodate their altruism.

Pilot Study

Figure S1 depicts the timeline and design of the pilot study. After subjects read the instructions and correctly answered a series of control questions, the experiment began with a practice round of the real-effort slider task. The purpose of the practice round was to further mitigate potential learning effects that might occur from stage A to stage B. At the end of the practice round, subjects were randomly assigned to either a leader or a recipient role. The roles remained fixed throughout the experiment. In both stages, leaders generated tokens during the slider task and then distributed tokens between themselves and recipients. Finally, leaders and recipients completed a norm-elicitation task to measure normative judgments about what constitutes appropriate and inappropriate leader behavior. That is, we asked subjects about the "socially appropriate" amount of tokens a leader should transfer to a recipient. At the outset, subjects were told that the experiment consisted of several parts, but were not informed of the specific details of each part at that point in the study.

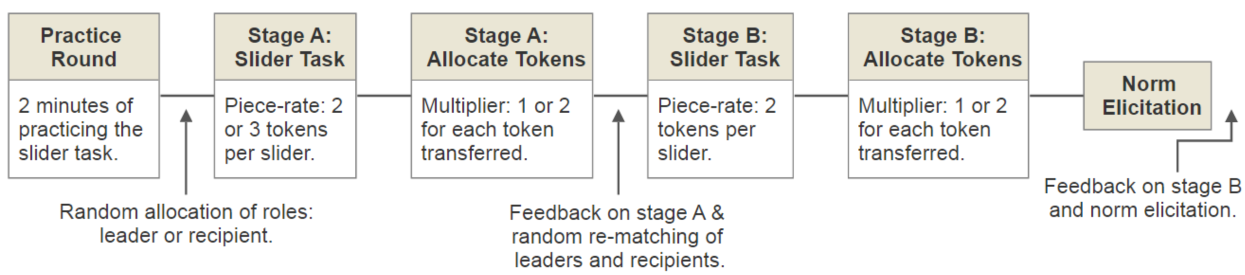

Figure S1. Timeline and Design of the Pilot Study
Notes: The manipulations are subsidy (i.e., piece-rate of 2 or 3) and relative price of giving (i.e., multiplier × 1 or × 2). We used a 2 × 2 factorial design varying the piece-rate in stage A and the multiplier (in both stages).

With this basic experimental framework, we manipulated two variables consisting of two levels each, yielding a 2 × 2 within-subject factorial design. The first variable, *relative price of giving*, varied the magnitude of a multiplier. We manipulated two values of the multiplier: 1 or 2. If the multiplier was 1, each token the leader allocated to recipients was worth 1 token to the recipient. If the multiplier was 2, each token the leader allocated to recipients was doubled and worth 2 tokens to the recipient.

The second variable, *subsidy*, varied the presence or absence of an external subsidy. In the control condition, each correctly positioned slider yielded 2 tokens. This occurred in stage A and stage B as well as under both relative price of giving conditions (i.e., multiplier of 1 and 2). We refer to this baseline control condition as a *piece-rate* of 2. In the treatment condition, leaders earned an additional token for each correctly positioned slider (a piece-rate of 3). This additional token was introduced in stage A and then removed in stage B. In other words, each correctly positioned slider generated 3 tokens in stage A and 2 tokens in stage B. The extra token earned in stage A can be interpreted as an external transfer or subsidy. In sum, our design yields four experimental conditions: NET1 (no external transfer and a multiplier of 1), ET1 (external transfer and a multiplier of 1), NET2 (no external transfer and a multiplier of 2), and ET2 (external transfer and a multiplier of 2).

Because our goal was to create a situation in which leaders perceived them as such, we did not use neutral language. The instructions described the situation as we do in the main text, referring to the different roles as *leaders* and *recipients*. The two tokens earned per slider in each game were referred to as *community tokens*. We told subjects that leaders could freely distribute these tokens. In the presence of external transfers when leaders earned 3 tokens per slider (i.e., stage A of ET1 and ET2), the extra token was called a *private token*. Leaders were told that they would receive the private token as compensation for performing the slider task. In line with this, private tokens could not be given to the recipient.

The next part of the experiment elicited social norms. To measure norms, we used the approach developed by Krupka and Weber (2013), which is how we measure norms about giving in the main study (see the main study for more details). Unlike the main study, we did not manipulate the presence or absence of the norm elicitation task in the pilot study. Instead, all subjects participated in the norm elicitation task, regardless of the treatment conditions. In the final part of the experiment, we measure subjects' demographic information, including questions about sex, age, nationality, and major.

Procedures

We conducted our pilot study at the University of California, Berkeley's Experimental Social Science Laboratory (XLab) in February 2016. A total of 158 subjects participated in the experiment. We ran three sessions of NET1 and ET1, and four sessions of NET2 and ET2. Each session had between 8 and 12 participants. The number of subjects in the role of a leader (the number of independent observations) was 17 for NET1, 18 for ET1, 21 for NET2 and 23 for ET2. Participants were students from various disciplines at the University of California, Berkeley. The mean age was 20.5 years, 73 percent of the participants were female.

The experiment was implemented with z-Tree. Written instructions were handed out for stage A of the experiment and were also read aloud by the experimenter, who was the first author. The experiment started after all participants had correctly answered a set of control questions handed out along with the instructions. The instructions for stage B (which were similar to the instructions for stage A) and the norm elicitation task were displayed on the computer screen in z-Tree. In the instructions for stage B, we highlighted the differences between stage A and stage B, which was either no difference or the removal of the private tokens (external transfer). A typical session lasted 45 minutes. Earnings were given in experimental currency units (ECU) and converted into US Dollars at the end of the experiment (1 ECU = $0.20 USD). Subjects earned an average of $18.30 USD, including a show-up fee of $5 USD.

Results

We begin by presenting the results from stage A and stage B. Against this benchmark, we finish with a discussion of the norm elicitation task.

Regarding hypothesis 1, the difference in means between the external transfer (M = 12.05, SD = 9.98) and no external transfer (M = 8.47, SD = 7.58) conditions shows that subsidies increase giving (ΔM = 3.58). Yet, model 1 in Table S1 reveals this difference to be statistically non-significant at the $p < .05$ level (β = 3.58, SE = 2.01, p = .07). Subsidies, therefore, do not seem to foster giving behavior (we will qualify this observation below). Regarding hypothesis 2, the difference in means between the multiplier × 1 (M = 10.18, SD = 8.26) and the multiplier × 2 (M = 10.52, SD = 9.71) conditions shows that the relative price of giving does not affect the mean number of tokens leaders transfer to recipients (ΔM = 0.34). Model 1 in Table S1 shows that this effect is statistically non-significant at the $p < .05$ level.

Table S1. OLS regression of tokens transferred in a dictator game with a real-effort task

	Stage A	Stage B
	Model 1	Model 2
Subsidy	3.58 (2.01)	1.80 (1.84)
	.20	**.11**
Relative price of giving	0.42 (2.06)	0.49 (1.91)
	.02	**.03**
Constant	8.23*** (1.64)	7.41*** (1.80)
N	77	77
R^2	.04	.01

*** *p* < 0.001, ** *p* < 0.01, * *p* < 0.05 (two-tailed)

Note : unstandardized slopes (bootstrap standard errors in parentheses, 5000 replications) **standardized slopes in bold**. The reference category for *subsidy* is piece-rate of 2. The reference category for *relative price of giving* is multiplier × 1.

Regarding hypothesis 3, we find no support for the notion that external transfers reduce giving in the long-run. That is, leaders who were not subsidized for their efforts in stage A *and* stage B did not transfer greater amounts of tokens to recipients in stage B than leaders who experienced the introduction (stage A) and then removal (stage B) of external transfers. In fact, leaders under the subsidy condition (M = 9.49, SD = 8.65) during stage B transferred 1.79 more tokens to recipients than leaders who never received a subsidy (M = 7.69, SD = 7.26). According to model 2 in Table S1, however, this effect is statistically non-significant at the $p < .05$ level (β = 1.80, SE = 1.84, p = .33). To emphasize this point further, we also investigated within-individual changes in giving. In line with the previous findings, only 11 percent (26 percent) of leaders in ET1 (ET2) reduced their share of tokens transferred to recipients between stage A and B, while a larger proportion of leaders increased their share of tokens transferred to recipients in ET1 (56 percent) *and* ET2 (43 percent).

Regarding hypotheses 4 and 5, ordered logit regression shows that private transfers nor the relative price of giving affects norms about giving (see Table S2).

Table S2. Ordered logistic regression of ratings of socially appropriate shares of tokens

	Model 1
Leader's share of tokens (ref. = 0-50)	
5-45	0.30** (0.09)
10-40	0.71*** (0.12)
15-35	1.34*** (0.17)
20-30	1.82*** (0.20)
25-25	3.00*** (0.28)
30-20	2.93*** (0.31)
35-15	2.63*** (0.34)
40-10	1.53*** (0.30)
45-5	0.26 (0.28)
50-0	-1.05** (0.31)
Leader (ref. = Recipient)	-0.36* (0.16)
Subsidy (ref. = No subsidy)	-0.15 (0.16)
Relative price of giving (ref. = Multiplier × 1)	-0.20 (0.16)
Constant (somewhat inappropriate)	0.02 (0.23)
Constant (somewhat appropriate)	1.39*** (0.27)
Constant (very appropriate)	2.57*** (0.32)
Norm bservations	1606
Leader and recipient observations	146

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note : log-odds ratios (cluster-robust standard errors in parentheses). Dependent variable is the rating of socially appropriate shares of tokens: "very socially inappropriate" (baseline), "somewhat socially inappropriate", "somewhat socially appropriate", and "very socially appropriate". The reference category for *leader's share of tokens* is 0-50 . The reference category for *role* is passive recipient. The reference category for *subsidy* is piece-rate of 2. The reference category for *relative price of giving* is multiplier × 1.

Accounting for Attrition

Table S3. Full Information Maximum Likelihood Linear Regression Models for Missing Data Predicting the Proportion of Tokens Sent in a Modified Dictator Game

	Model 1	Model 2
Subsidy (ref. = No subsidy)		
Stage A removal	0.019 (0.024)	0.047 (0.027)
Stage B introduction	0.020 (0.029)	-0.002 (0.031)
Relative price of giving (ref. = Multiplier × 1)	-0.016 (0.020)	-0.016 (0.020)
Norm elicitation (ref. = No elicitation)		
Stage A elicitation	-0.012 (0.026)	-0.012 (0.026)
Stage B elicitation	-0.015 (0.030)	-0.015 (0.030)
Stage (ref. = Stage A)	-0.038*** (0.010)	-0.034* (0.016)
Subsidy × Stage		
Stage A removal × Stage		-0.056* (0.022)
Stage B introduction × Stage		0.048* (0.020)
Completed tasks	-0.008** (0.002)	-0.008** (0.002)
Intercept	0.470*** (0.065)	0.468*** (0.066)
Stage observations	946	946
Leader observations	473	473
Design controls	Yes	Yes
Auxiliary variables for missing data	Yes	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note : unstandardized slopes (cluster-robust standard errors in parentheses). Design controls include two session-level variables: size of session and days since first session. Auxiliary variables include age, education, and gender.

Robustness and Sensitivity Checks

We also explored a number of robustness and sensitivity checks. First, although we find that the norm elicitation task does not affect giving by leaders, we explore whether this varies across stages. A *Norm elicitation* × *Stage* interaction included in model 1, Table 2 is statistically non-significant, $\chi^2(2, N = 798) = 4.84, p > .05$. Second, a series of nested interactions between key manipulations and stage all yield statistically non-significant results, including *Subsidy* × *Relative price of giving*, $\chi^2(2, N = 798) = 0.02, p > .10$, *Subsidy* × *Relative price of giving* × *Stage*, $\chi^2(2, N = 798) = 2.20, p > .10$, and *Subsidy* × *Relative price of giving* × *Norm elicitation* × *Stage*, $\chi^2(4, N = 798) = 1.92, p > .10$. These results indicate that the effects observed in model 2, Table 2 are robust across different experimental conditions. Third, we explore whether the statistically significant *Subsidy* × *Stage* interaction observed in model 2 is robust to the number of completed tasks. Model 2 estimated with a three-way interaction between *Subsidy* × *Stage* × *Completed tasks*, $\chi^2(2, N = 798) = 0.34, p > .10$, shows that the *Subsidy* × *Stage* interaction does not vary by leaders who complete a different number of real-effort tasks. Fourth, we find that the results are robust to alternative modeling procedures, namely fractional logit regression (see Table S4). Fifth, we find that recipients benefit from an external subsidy only in the absolute amount of community tokens sent, not in their income position relative to the leader (see Table S5 and Figure S2).

Table S4. Fractional Logit Models Predicting the Proportion of Tokens Sent in a Modified Dictator Game

	Model 1	Model 2
Subsidy (ref. = No subsidy)		
Stage A removal	0.081 (0.128)	0.186 (0.135)
Stage B introduction	0.124 (0.149)	0.002 (0.152)
Relative price of giving (ref. = Multiplier × 1)	-0.061 (0.104)	-0.061 (0.104)
Norm elicitation (ref. = No elicitation)		
Stage A elicitation	-0.031 (0.131)	-0.031 (0.131)
Stage B elicitation	-0.049 (0.154)	-0.049 (0.155)
Stage (ref. = Stage A)	-0.166** (0.050)	-0.169* (0.082)
Subsidy × Stage		
Stage A removal × Stage		-0.228* (0.115)
Stage B introduction × Stage		0.248* (0.103)
Completed tasks	-0.047*** (0.012)	-0.047*** (0.012)
Intercept	-0.269 (0.258)	-0.268 (0.258)
Stage observations	798	798
Leader observations	399	399
Design controls	Yes	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note : log-odds ratios (cluster robust standard errors in parentheses). Design controls include two session-level variables: size of session and days since first session.

Table S5. Multilevel Mixed-Effects Regression Models Predicting the Proportion of Tokens Sent in a Modified Dictator Game Relative To Leaders

	Model 1	Model 2
Subsidy (ref. = No subsidy)		
Stage A removal	-0.040 (0.023)	-0.072** (0.023)
Stage B introduction	-0.022 (0.027)	-0.001 (0.030)
Relative price of giving (ref. = Multiplier × 1)	-0.011 (0.018)	-0.011 (0.018)
Norm elicitation (ref. = No elicitation)		
Stage A elicitation	-0.0007 (0.023)	-0.0005 (0.023)
Stage B elicitation	-0.002 (0.027)	-0.002 (0.027)
Stage (ref. = Stage A)	-0.032*** (0.008)	-0.040** (0.015)
Subsidy × Stage		
Stage A removal × Stage		0.064** (0.020)
Stage B introduction × Stage		-0.043* (0.019)
Completed tasks	-0.005** (0.001)	-0.005** (0.001)
Intercept	0.372*** (0.044)	0.374*** (0.045)
Stage observations	798	798
Leader observations	399	399
SD (intercept)	0.160	0.161
SD (residuals)	0.117	0.112
Design controls	Yes	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note : unstandardized slopes (robust standard errors in parentheses). Design controls include two session-level variables: size of session and days since first session.

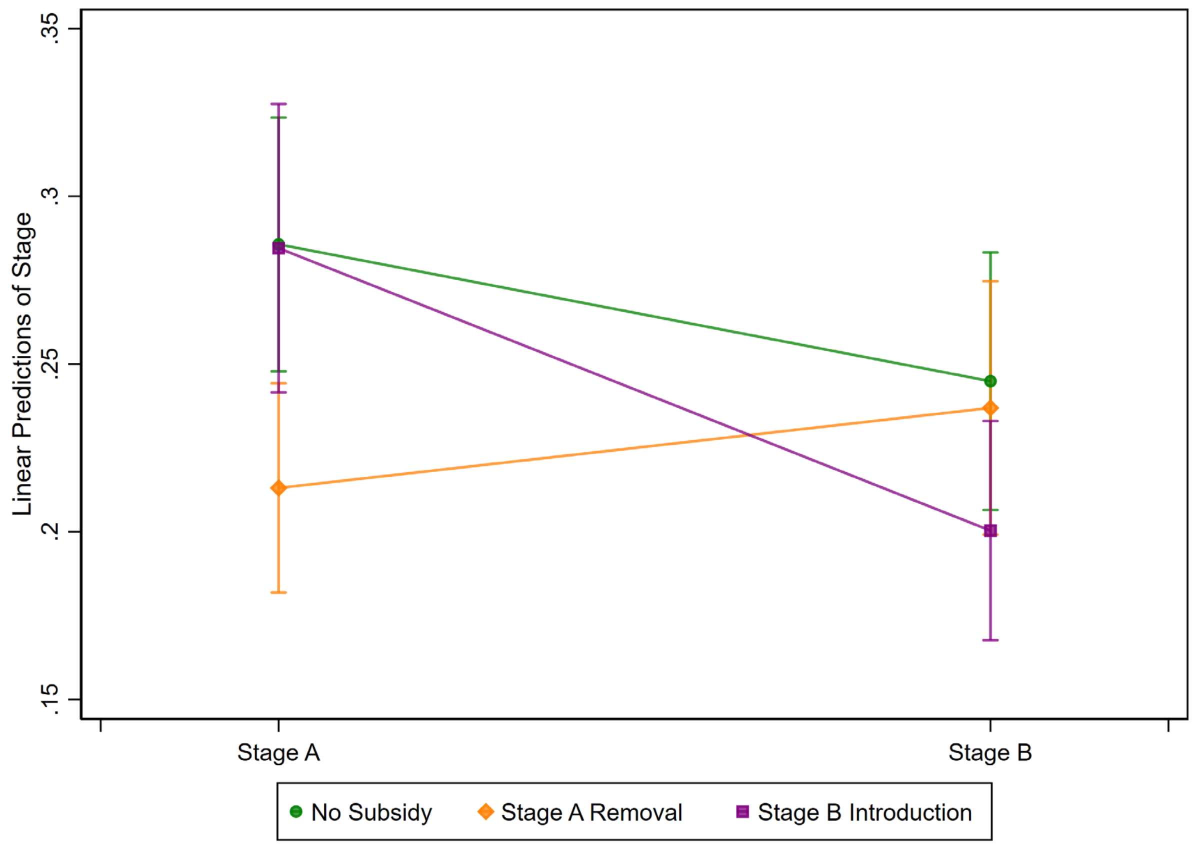

Figure S2. Interaction Effects between Subsidy and Stage on the Proportion of Tokens Sent by Leaders to Recipients Relative to Leaders (Table S5, Model 2)
Notes: All covariates constrained to their mean values.

Sample Instructions

Welcome to our study.

Your earnings from today's experiment will largely depend on your decisions and the decisions of others. Therefore, it is important that you read the instructions carefully.

Throughout the experiment, we will not speak of US Dollars, but rather of experimental tokens. At the end of the experiment the total amount of tokens you earned will be converted to USD at the **exchange rate 1 Token = 1 cent**. You will also receive a **show up fee** of **$3**. You will be paid your earnings at the end of the session.

The experiment consists of four parts. You will receive instructions at the beginning of each part.

Instructions for Part 1

Matching, Leaders and Recipients

At the start of Part 1, you will be **randomly matched with another participant in the experiment.** You will never know the identity of the person with whom you are matched and the other person will never know your identity. You will never be matched with the same participant again in following parts.

In each pair, one person will be chosen to be the **leader**. The other person will be the **recipient**. Whether you will be the leader or the recipient is randomly determined by the computer.

The leader of a pair has two tasks:

- Perform an effort task to earn tokens.
- Decide on how to divide the tokens earned in the effort task between herself / himself and the recipient. Tokens which are allocated to the recipient will be multiplied by a factor of 2.

A detailed description of both tasks will be given below.

Recipients do not actively influence their earnings. Their earnings will only depend on the amount of tokens the leader allocates to them.

The Effort Task
The leader of each pair will undertake a task lasting 120 seconds. The task will consist of summing three 3-digit numbers. **Your "points score" in the task will be the number of correct math problems** at the end of the 120 seconds.

The score in the effort task determines the total number of tokens earned, where the leader *and* the recipient will both observe the realized points score in the effort task.

Community Tokens
We refer to tokens as community tokens, because the leader can share them with the recipient. **For each correct math problem, 2 community tokens are earned.** For example, if after 120 seconds, the number of correct math problems is 4, the number of earned community tokens is 8. If the number of correct math problems is 39, the number of earned community tokens is 78.

Upon completion of the effort task, **the leader will be asked to divide the earned community tokens between herself/himself and the recipient**. Any division of community tokens is possible.

Community Tokens Allocated to the Recipient
Each community token the leader chooses to allocate to the recipient will be multiplied by 2. Hence, each community token the recipient receives increases his/her earnings by 2 tokens and decreases the earnings of the leader by 1 token.

For example, if the leader correctly answered 4 math problems, s/he will be asked to divide 8 community tokens. If the leader chooses to keep all community tokens, s/he earns 8 community tokens and the recipient earns 0 community tokens. If the leader chooses to allocate all community tokens to the recipient, s/he earns 0 tokens and the recipient earns **2***8 = 16 community tokens. If the leader chooses to keep 4 community tokens and allocate 4 community tokens to the recipient, the leader earns 4 community tokens and the recipient earns **2***4 = 8 community tokens.

Earnings
Each community token is exchanged for 1 cent at the end of the experiment.

This completes the description of Part 1. Please proceed to answer the following questions (on the next screen). The purpose of the questions is to make sure that you understand the different elements of the experiment.

Questions

a. You will be randomly matched in pairs and will never know the identity of the participant matched to you. In addition, you will not be matched with the same person again in the following parts.
Is this statement true or false? ______

b. Suppose you are the leader. In the effort task, you have correctly answered 13 math problems. What is the number of community tokens earned? ______ Tokens

c. Suppose Person A is the leader and Person B the recipient. The leader has correctly answered 20 math problems in the effort task. Decide which of the 4 distributions of the total number of tokens below are possible.

	Distribution 1	Distribution 2	Distribution 3	Distribution 4
Person A	40	30	60	0
Person B	30 (earning 60 tokens after multiplying by 2)	20 (earning 40 tokens after multiplying by 2)	0	40 (earning 80 tokens after multiplying by 2)
Possible?	YES / NO	YES / NO	YES / NO	YES / NO

d. Suppose the leader has correctly answered 20 math problems. Suppose the leader keeps all community tokens, i.e. allocates none of the community tokens to the recipient.
What is the number of community tokens earned by the leader? ______ Community Tokens
What is the number of community tokens earned by the recipient? ______ Community Tokens

e. Suppose the leader has correctly answered 20 math problems. Suppose the leader allocates all community tokens to the recipient.
What is the number of community tokens earned by the leader? ______ Community Tokens
What is the number of community tokens earned by the recipient? ______ Community Tokens

f. Suppose the leader has correctly answered 20 math problems. Suppose the leader allocates half of the community tokens to the recipient.
What is the number of community tokens earned by the leader? ______ Community Tokens
What is the number of community tokens earned by the recipient? ______ Community Tokens

Once everyone has answered all questions correctly, we will proceed to the experiment.

Part 1

<u>**Introductory Screen**</u>

[**If Leader**]
You and another person have been matched into a pair. **You are the leader of this pair**. The other person is the recipient.

As the leader, you will answer math problems, which determines the amount of community tokens earned. For each math problem answered correctly, **2 community tokens are earned**. The recipient will not answer math problems.

After you have completed the task, you will be asked to **divide the acquired community tokens** between you and the recipient. **Each token the leader chooses to allocate to the recipient will be multiplied by 2**.

[**If Recipient**]
You and another person have been matched into a pair. **You are the recipient of this pair**. The other person is the leader. The leader will answer math problems, which determines the amount of community tokens earned.

<u>**Real-Effort Task Screens**</u>

[**If Leader**]
Real-effort task. 50 screens where each screen consists of a math problem, which will be sums of three 3-digit numbers.

[**If Recipient**]
Please wait for the experiment to continue

<u>**Allocation Screens**</u>

[**If Leader**]
Community tokens
Your points score: _____
Community tokens earned per correct math problem: 2
Community tokens earned: _____

Decide on how to divide the total amount of community tokens
Community tokens for the leader (you): _________
Community tokens for the recipient: _________

[**If Recipient**]
Please wait for the experiment to continue

Instructions for Part 2: Norm Elicitation Task

Part 2 will take place before the outcome of Part 1 is shown. The leader has already distributed the community tokens in Part 1, and thus your choices in Part 2 will not affect the outcome in Part 1.

On the following screen, you will see **10 hypothetical outcomes** of Part 2. Those outcomes were predetermined and will not generally correspond to the real outcomes in Part 1. Each hypothetical outcome consists of a total amount of community tokens and a corresponding distribution of these tokens between the leader and the recipient. **The total amount of community tokens will be 50 for each hypothetical outcome**. Only the distribution of the tokens between the leaders and the recipient will change.

You will be asked to evaluate different distributions of the 50 community tokens that the leader can choose. You can do this by stating, for each possible distribution, whether the leader's action would be "**socially appropriate**" and "consistent with moral and proper social behavior" or "**socially inappropriate**" and "inconsistent with moral or proper social behavior." By socially appropriate, we mean behavior that most people agree is the "correct" or "ethical" thing to do. Another way to think about what we mean is that if the leader were to select a socially inappropriate choice, then the recipient might be angry at the leader for doing so.

In each of your responses, we would like you to answer as truthfully as possible, based on your opinions of what constitutes socially appropriate or socially inappropriate behavior. For each of the choices, you can select one of four possible ratings: very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, and very socially appropriate. You will be able to indicate in your response by placing a checkmark in the corresponding box.

Your payment in this part will depend on the choices of other participants. At the end of Part 2, we will randomly select one of the 10 hypothetical outcomes. For the choice selected, we will determine which response (social appropriateness rating) was selected by most other people who were in the same role as you in the experiment. **Since you were a [insert role here], we will determine which response was selected by most other [insert role here]s. If you give the same response as that most frequently given by other [insert role here]s, then you will receive an additional $1**. This amount will be paid to you at the conclusion of the experiment.

For example, suppose the selected hypothetical outcome is that the leader gets 0 tokens and the recipient gets 50 tokens. Suppose you have evaluated this distribution to be "somewhat socially appropriate". If the majority of the [insert role here]s in today's session have also chosen "somewhat socially appropriate", you earn $1. If the majority of the [insert role here]s in today's session have chosen "very socially inappropriate", "somewhat socially inappropriate", or "very socially appropriate", you earn $0.

Norm Elicitation Task

Reminder of the Situation

Recall the situation in Part 1 of the experiment. Each participant was randomly paired with another participant. One participant was a leader, the other participant was a recipient. The leader was asked to solve math problems. For each correct math problem, 2 community tokens were earned. Suppose the number of correctly answered math problems was 25 and, thus, the total number of community tokens to be distributed was 50. The leader has been asked to divide the total amount of community tokens between herself/himself and the recipient.

Your task is to evaluate whether the ten hypothetical distributions given below are socially appropriate or socially inappropriate.

The table below gives a list of the possible choices to the leader. For each of the choices, please indicate whether you believe choosing that option is very socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, or very socially appropriate. To indicate your response, please place a checkmark in the corresponding box.

You were a [insert role here] in Part 1. Remember that you will earn money ($1) if your response to a randomly-selected question is the same as the most common response provided by the other [insert role here] in today's session.

Leader's Decision on how to divide the 50 community tokens	Appropriateness Ratings			
Leader gets 50, Recipient gets 0	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 45, Recipient gets 5	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 40, Recipient gets 10	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 35, Recipient gets 15	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 30, Recipient gets 20	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 25, Recipient gets 25	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 20, Recipient gets 30	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 15, Recipient gets 35	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 10, Recipient gets 40	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 5, Recipient gets 45	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate
Leader gets 0, Recipient gets 50	Very Socially Inappropriate	Somewhat Socially Inappropriate	Somewhat Socially Appropriate	Very Socially Appropriate

Allocation Screens for Part 1

[**If Leader**]
Summary of Part 1
Your role: Leader
Number of math problems answered correctly: __________

Community tokens earned per correct math problem: __________
Community tokens earned by the leader: __________

Division of community tokens as chosen by the leader
Leader's community tokens: __________
Recipient's community tokens: __________

Total amount of tokens (community tokens)
Leader's total amount of tokens: __________
Recipient's total amount of tokens: __________

[**If Recipient**]
Summary of Part 1
Your role: Recipient
Number of math problems answered correctly by the leader: __________

Community tokens earned per correct math problem: __________
Community tokens earned by the leader: __________

Division of community tokens as chosen by the leader
Leader's community tokens: __________
Recipient's community tokens: __________

Total amount of tokens (community tokens)
Leader's total amount of tokens: __________
Recipient's total amount of tokens: __________

Allocation Screens for Part 2

[**If Leader**]
Summary of Part 2
The appropriateness rating randomly selected for payment was for distribution [insert distribution here].

Your rating was [insert rating here].
The most common rating by other leaders was [insert rating here].

Your earnings in Part 2 (in USD): _____________

[**If Recipient**]
Summary of Part 2
The appropriateness rating randomly selected for payment was for distribution [insert distribution here].

Your rating was [insert rating here].
The most common rating by other recipients was [insert rating here].

Your earnings in Part 2 (in USD): ____________

Instructions for Part 3

Please carefully read the instructions below. Part 3 is similar to Part 1. However, there are some important differences: the leader can now earn private tokens, and you will be matched with a different participant.

At the start of Part 3, you will be randomly matched with another participant. You will never know the identity of the person with whom you are matched and the other person will never know your identity. **The person matched to you in Part 3 is not the same person that was matched to you in Part 1**.

If you were chosen to be the leader in Part 1, you are again the leader in this part. If you were chosen to be the recipient in Part 1, you are again the recipient in this part.

The leader of a pair has two tasks: (1) answer math problems to earn tokens, and (2) decide how much to divide the tokens earned between herself/himself and the recipient. Recipients do not actively influence their earnings. They will only receive the amount of tokens the leader decides to allocate to them.

As before, the number of math problems correctly answered determines the number of community tokens earned. Community tokens can be divided between the leader and the recipient. Unlike before, the number of math problems correctly answered also determines the number of private tokens earned. Private tokens cannot be divided between the leader and the recipient.

The number of community tokens earned for each correct math problem is the same as the number of tokens earned per correct math problem in Part 1. **In particular, the number of community tokens earned for each math problem correctly answered is 2**. For example, if after 120 seconds the number of correct math problems is 4, then the number of earned community tokens is 8. If the number of correct math problems is 39, then the number of earned community tokens is 78.

The second type of token is private tokens. **For each correct math problem, the leader earns 1 private token**. These tokens are paid to the leader as a compensation for answering the math problems. The leader cannot share these tokens with the recipient.

It follows that each correct math problem generates 3 total tokens for the leader: 2 community tokens and 1 private token. Only community tokens can be shared with the recipient. Private tokens cannot be shared and are paid to the leader because s/he is the one answering the math problems.

Upon completing the effort task, the leader is asked to divide the earned community tokens between herself/himself and the recipient. Any division of community tokens is possible as long as the sum of community tokens given to the leader and recipient equals the total number of community tokens. **Each community token the leader chooses to allocate to the recipient will be multiplied by 2**. Hence, each community token the recipient receives increases his/her earnings by 2 tokens and decreases the earnings of the leader by 1 token. For example, if after 120 seconds the number of correct math problems is 4, s/he will be asked to divide 8 community tokens. If the leader chooses to keep all community tokens, s/he earns 8 community tokens and the recipient earns 0 community tokens. If the leader chooses to allocate all community tokens to the recipient, s/he earns 0 tokens and the recipient earns **2***8 = 16 community tokens. If the leader chooses to keep 4 community tokens and allocate 4 community tokens to the recipient, the leader earns 4 community tokens and the recipient earns **2***4 = 8 community tokens.

Each community token and each private token is exchanged for 1 cent at the end of the experiment.

This completes the description of Part 3. If you are ready, please click on Continue.

Part 3

Introductory Screen

[**If Leader**]
You and another person have been matched into a pair. **You are the leader of this pair**. The other person is the recipient.

As the leader, you will answer math problems, which determines the amount of community tokens earned. For each math problem answered correctly, **2 community tokens are earned**. The recipient will not answer math problems.

After you have completed the task, you will be asked to **divide the acquired community tokens** between you and the recipient. **Each token the leader chooses to allocate to the recipient will be multiplied by 2**.

You will receive an additional compensation for completing the task. In particular, for each math problem answered correctly, you will receive **1 additional private token**. Private tokens are for the leader (you) and cannot be allocated to the recipient.

[**If Recipient**]
You and another person have been matched into a pair. **You are the recipient of this pair**. The other person is the leader. The leader will answer math problems, which determines the amount of community tokens earned.

The leader will receive an additional compensation for answering math problems. In particular, for each math problem answered correctly, the leader will receive 1 additional private token. These tokens are for the leader and cannot be allocated to the recipient (you).

Real-Effort Task Screens

[**If Leader**]
Real-effort task. 50 screens where each screen consists of a math problem, which will be sums of three 3-digit numbers.

[**If Recipient**]
Please wait for the experiment to continue

Allocation Screens

[**If Leader**]
Private tokens
Your points score: _____
Private tokens earned per correct math problem: 1
Private tokens earned: _____

Community tokens
Your points score: _____
Community tokens earned per correct math problem: 2
Community tokens earned: _____

Decide on how to divide the total amount of community tokens
Community tokens for the leader (you): _________
Community tokens for the recipient: _________

[**If Recipient**]
Please wait for the experiment to continue

Allocation Screens for Part 3

[**If Leader**]
Summary of Part 3
Your role: Leader
Number of math problems answered correctly: _________

Private tokens earned per correct math problem: _________
Private tokens earned by the leader: _________

Community tokens earned per correct math problem: _________
Community tokens earned by the leader: _________

Division of community tokens as chosen by the leader
Leader's community tokens: _________
Recipient's community tokens: _________

Total amount of tokens (community and private tokens)
Leader's total amount of tokens: _________
Recipient's total amount of tokens: _________

[**If Recipient**]
Summary of Part 3
Your role: Recipient
Number of math problems answered correctly by the leader: _________

Private tokens earned per correct math problem: _________
Private tokens earned by the leader: _________

Community tokens earned per correct math problem: _________
Community tokens earned by the leader: _________

Division of community tokens as chosen by the leader
Leader's community tokens: _________
Recipient's community tokens: _________

Total amount of tokens (community and private tokens)
Leader's total amount of tokens: _________
Recipient's total amount of tokens: _________

Part 4: Survey Questionnaire

Q1. What is your gender?

- Male
- Female
- Other
- Prefer not to say

Q2. What is your age? (in years)

____

Q3. What is your nationality?

[**drop-down menu**]

Q4. What is the highest degree that you have received?

- None
- GED or alternative credential
- High school diploma (regular 12-year program)
- Associate/Junior college degree (AA, AS)
- Bachelor’s degree (BA, BS)
- Master’s degree (MA, MS, Meng, MBA, MPH, Med, MSW, MPA, etc.)
- Professional degree (DDS, LLB, JD, MD, OD, DVM, or other Advanced Professional Degree)
- Doctorate (PhD, EdD)
- Prefer not to say

Part 5: Total Earnings

Total Earnings Screen

Your earnings in the experiment (in USD): ______________
Show-up fee (in USD): $3.00

Your total earnings (in USD): ____________

This is the end of the experiment. You will now be paid out your earnings shortly.

End Screen

Thank you for participating in this experiment.