The Puzzle of Political Ambition and Prosociality

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Abstract

This study explores the effect of prosocial interventions on political engagement, particularly whether a prosocial, redistributive option can narrow the gender gap in willingness to enter elections (Pate and Fox, 2018). Previous evidence suggests that prosocial incentives boost women's desire to compete (Cassar and Rigdon, 2021a,b). Building on this, our research investigates whether mechanisms that enable elected leaders to share their gains can increase women's political ambition. We conducted a laboratory experiment at two distinct laboratories with 320 participants using a within-subjects design to compare a standard election with one where the elected leader could redistribute earnings to the lowest earner. Our results show that prosocial opportunities increase participation rates for both women and men, yet the gender gap in ambition persists. Notably, we also find a puzzling result: although the prosocial intervention boosts individual willingness to run for office, greater generosity is negatively associated with the decision to run for election. These findings reveal a paradox: those most inclined to help others are often the least likely to seek positions that would enable them to do so.

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1 Introduction

In the 119th Congress, convened in January 2025, women make up just over a quarter ($\approx$ 28%) of all members of the U.S. Senate and House of Representatives. Although this marks a record high, it remains far below women's share of the overall U.S. population. One proposal to address this persistent gender gap in representation is to find environments and institutions that encourage more women to run for elected office. This line of inquiry seeks to explain the paradox of underrepresentation: even when women win at similar rates as men (Schwarz and Coppock, 2022), they remain less likely to run in the first place (Cook, 1998; Dolan, 1998; Fox, 2022; Hayes and Lawless, 2016; Lawless and Pearson, 2008; Seltzer and Newman, 1997).

The persistent gender gap in political ambition, evident even among highly educated and well-qualified professionals, diminishes the likelihood that women will enter electoral contests. Recent experimental research has tested several strategies to reduce this gap, including improving candidates' self-assessment of qualifications, increasing familiarity with electoral procedures, and tailoring recruitment messages to resonate more with women (Eckel et al., 2020; Pate and Fox, 2018, 2025). These approaches align with findings that individual behavior in competitive settings is shaped by group identity and social context (Charness et al., 2007). Despite these efforts, the gender gap in political ambition has proved to be stubborn. Regardless of the intervention, women's preferences toward entering elections appears largely fixed.

Our research builds on the experimental design in Pate and Fox (2018) to test whether the presence of a prosocial opportunity can reduce the gender gap in political ambition. Prosocial incentives have proven to be a powerful mechanism for narrowing gender differences in competitiveness. For example, including rewards that matter more to women, like book vouchers for children (Cassar et al., 2016), beauty products and clothing items (Cassar and Zhang, 2022), payment schemes that allow winners to share a portion of their earnings with a low-performing participant (Cassar and Rigdon, 2021a,b) or benefit a

charity of their choice (Drouvelis and Rigdon, 2023) have been shown to significantly increase women's willingness to compete in tournament settings.

These findings suggest that women become more likely to compete when the competitive environment includes an opportunity to benefit others. We extend this logic to the political domain by examining whether the opportunity to redistribute earnings would similarly increase women's willingness to run for election. This approach builds on the foundational work of Charness and colleagues, who have shown that social preferences and opportunities to benefit others can significantly influence behavior in competitive environments (see e.g., Charness and Rabin, 2002; Charness et al., 2007; Fehr and Charness, 2025).

Previously, men were generally unaffected by the introduction of prosocial incentives (Cassar and Rigdon, 2021a,b). Thus, when prosocial incentives increased women's willingness to compete but had no effect on men, the gender gap in competitiveness closed. Whether a similar pattern holds in elections depends not only on whether more women choose to run but also on how men respond. A decrease in men's willingness to run could narrow the gap, while a simultaneous increase could offset any gains. Prior research has shown that community-service or female-stereotyped framing can reduce men's willingness to enter elections (Pate and Fox, 2018, 2025), raising the possibility that prosocial framing might work similarly by making the role less appealing to men.

Our experimental design builds on this body of work by testing whether a prosocial opportunity, in which elected representatives can redistribute earnings to the lowest earner, can similarly increase women's willingness to run for election. If the treatment produces a comparable effect in this setting, it would encourage greater participation among women while having little impact on men, narrowing the gender gap in political ambition.

Our results show that the prosocial option increases women's willingness to run for election, consistent with prior findings on competitiveness. However, men's willingness to run also increases under the same condition. Although participation rises overall, the

gender gap in political ambition remains, contrary to our initial hypothesis.

We also uncover a puzzling pattern: women who are more likely to redistribute to the lowest earner are less likely to run for election. Instead, the increase in women's participation appears to be driven by those who are less prosocial. This counterintuitive result raises questions about the motivations behind entry into elections and other competitive environments and resulting gender disparities. Our findings contribute to a growing body of literature using experimental methods to examine strategies for reducing gender gaps across domains such as education, labor markets, risk preferences, and political behavior (e.g., Kahn (2000); Gneezy and List (2006); Eckel and Grossman (2008b); see also Charness and Kuhn (2011); Charness and Gneezy (2012); Charness et al. (2013) for key insights on gender and competitiveness).

The following section introduces the election context and outlines how the prosocial treatment was implemented, along with our pre-registered hypotheses. Section 3 details the experimental design and procedures. Section 4 compares behavior in the experimental treatments. Section 5 considers the broader implications of our findings and proposes avenues for future research.

2 Prosocial Treatment and Hypotheses

This study bridges two lines of research aimed at understanding and reducing women's underrepresentation in leadership roles from corporate boardrooms to elected office. One line focuses on women's willingness to compete and has identified promising mechanisms for increasing participation, including the use of prosocial incentives. Cassar and Rigdon (2021b), for example, find that giving competitors the option to share earnings with a low-performing peer significantly increases women's entry into tournaments while leaving men's behavior unchanged.

A second body of work investigates women's willingness to run for election and has

tested a wide range of interventions designed to encourage candidacy. These include exposure to political role models, targeted recruitment of women, and changes to the structure of political framing. This literature identifies promising strategies for increasing women's political ambition without altering formal electoral rules or eligibility criteria (see e.g., Fox and Lawless, 2005; Pate and Fox, 2018; Fox and Pate, 2023). Both lines of research point to strategic interventions that may shift participation without changing required qualifications or electoral rules.

We bring these strands together by taking a close approximation of the Cassar and Rigdon (2021a,b) prosocial mechanism and embedding it within a well-established experimental election design (see e.g., Pate and Fox, 2018). This allows us to test whether the same opportunity that increased women's competitiveness in tournaments can also increase their willingness to run for office. From this, we pre-registered the following hypotheses:

H1: The presence of a prosocial option (in the form of the possibility for the leader to share part of the winnings with the lowest earner) increases women's willingness to run in the election.

H2: The presence of a prosocial option does not affect men's willingness to run in the election.

H3: The prosocial context reduces the gender gap in political ambition.

H4: The level of giving by elected representatives does not differ by gender.

By applying a previously validated intervention to an electoral context, we assess whether a mechanism that increases women's entry into competitive environments can similarly affect willingness to run for election. These pre-registered expectations guide our analysis.

3 Experimental Design and Procedures

Building on the methodologies developed by Kanthak and Woon (2015) and Pate and Fox (2018), we utilize a laboratory experiment designed to examine the effect of the prosocial treatment on willingness to enter elections. The subjects in our experiment were primarily undergraduates at Loyola Marymount University and the University of Alabama. A total of 160 students were recruited from each university using their respective email-based recruitment systems. All experimental sessions were conducted using z-Tree (Fischbacher, 2007). In total, 330 subjects participated in the experiment (173 women and 147 men), with each subject taking part in only one session. The sessions were conducted first in April and then August-October 2024.

The instructions (in the Appendix) were distributed and read aloud before each part of the experiment. At the conclusion of each session, subjects completed a demographic and political interest questionnaire (summary statistics appear in Tables A1 and A2 in the Appendix). Subjects were paid in cash at the end of the experiment based on their decisions. Each session lasted just over an hour, and the average earnings were $25.13, in addition to a $5.00 participation payment.

Upon arrival, subjects were randomly assigned to workstations. They were informed that the experiment consisted of five main parts, each with payments linked to their decisions. Subjects were then randomly placed into groups of five.[1] Subjects could observe the gender composition of the session as a whole but not the gender composition of their group. The full experimental design is summarized in Table 1, and we describe each component in detail below.

[1] This design feature reduces the likelihood of ties in the election.

Table 1: Experimental Design

Part 1: Risk Measurement	Choices among 9 lotteries
Part 2: Altruism Measurement*	Dictator game for $5.00
Part 3a: Real-effort Task	Piece-rate $0.75/correct
Rank self-assessed	Learn own score & estimate each group members' score
Part 3b: Elections	Step 1: Elicit willingness to stand for election
	Step 2: Candidates send messages
	Step 3: Vote in election
	Step 4: Real-effort task - **Standard Payment**
Part 3c: Elections	Step 1: Elicit willingness to stand for election
	Step 2: Candidates send messages
	Step 3: Vote in election
	Step 4: Real-effort task - **Prosocial Payment**

*Results from Part 1 and Part 2 are withheld and paid at the end of the experiment.

3.1 Measuring Risk Tolerance and Altruism: Parts 1 and 2

The experiment begins with two parts designed to measure individual differences in risk tolerance and altruism. In Part 1, subjects make choices across nine paired lotteries, each presenting a trade-off between a riskier and a safer option. This measure, previously used by Pate and Fox (2018) and consistent with methods developed in the literature on risk preferences (e.g., Charness and Gneezy, 2012; Charness et al., 2013), serves as a covariate to assess how risk preferences relate to willingness to enter elections. Because outcomes are determined probabilistically, the task introduces uncertainty in a controlled way, helping to reveal underlying attitudes toward risk.

In Part 2, subjects participate in a one-shot dictator game with a five-dollar endowment. Each subject decides how much of their five dollars to allocate to a randomly assigned, anonymous partner. This decision serves as a proxy for baseline prosocial behavior, including the willingness to consider others' welfare and to act in the interest of the

group. These are traits that are relevant for individuals in representative roles. Feedback from both tasks is withheld until the end of the experiment to avoid influencing decisions in subsequent parts.

3.2 Real-effort Task and Payment Schemes: Part 3

The core of the study lies in Part 3, where subjects complete a real-effort addition task introduced by Niederle and Vesterlund (2007). This task is widely used in experimental economics because it avoids political and social content, and men and women tend to perform equally well.[2] Participants are asked to add as many sets of five two-digit numbers as possible within a fixed time limit. They are provided with scratch paper and a pencil but cannot use calculators.

Each subject completes the addition task under three different incentive schemes. In Part 3a, they are paid a fixed amount for each correct answer (a piece-rate payment). In Part 3b, earnings depend on both individual performance and the performance of a group representative selected through an election. In Part 3c, the election is repeated, but the elected representative can share a portion of their earnings with the lowest earner in the group. Participants are given four minutes to complete as many addition problems as possible in all three parts. Compensation is determined by the number of correctly solved problems, referred to as their score. The specific payment structure for each condition is explained in the following sections.

3.3 Piece-rate Payment and Measuring Self Rank: Part 3a

In Part 3a, subjects are informed that they will receive \$0.75 for each correctly solved problem. This constitutes the piece-rate payment scheme. After the four-minute task is complete, subjects are shown only their score. They are then asked to estimate the performance

[2]The instructions explicitly state that the addition task was selected to address potential stereotype-based biases because performance does not vary by education level, socioeconomic status, gender, or race.

of the other four members in their group by assigning a rank order. Specifically, they report how many problems they believe were solved by the top performer, second-highest performer, and so on, excluding themselves. These estimates are used to generate a self-assessed rank variable and to evaluate whether subjects' perceptions are influenced by overconfidence or underconfidence. This measure reflects how potential candidates may assess their relative standing in a competitive environment and is included as a covariate in the analysis.

3.4 Elections under Standard Incentive (Part 3b) and Prosocial Incentive (Part 3c)

In Part 3b, subjects are informed that they will complete the addition task again. This time, their earnings will depend on both their performance and the performance of a group representative selected through an election. This portion of the experiment consists of four key steps.

First, subjects receive detailed instructions on the election procedure, including how candidates will be selected. They are then asked to indicate their willingness to run for election as the group's representative using a bidirectional elicitation mechanism (see Figure 1). Each subject begins with 50 tokens, which represent entries into a lottery that determines candidate selection. To increase their likelihood of being chosen, subjects can purchase additional tokens. Buying 10 tokens costs nothing, while increasing to 70, 80, or 95 tokens costs \$0.25, \$0.50, and \$1.00, respectively.

To reduce the likelihood of being selected as a candidate in the election, subjects can reduce the number of tokens they enter into the lottery. Reducing to 40 tokens is free, while lowering their total to 30, 20, or 5 costs \$0.25, \$0.50, or \$1.00, respectively. This design prevents participants from perfectly controlling the outcome but enables them to express nuanced preferences regarding candidacy. If this part is selected for payment, all costs are deducted from the final earnings. The fine-grained nature of the measure

allows us to capture small shifts in political ambition that would be lost in a binary entry decision.[3]

Figure 1: Cost Structure (as seen by subjects)

	Decrease your likelihood of being selected ←					Current tokens	Increase your likelihood of being selected →				
	○	○	○	○	○	◉	○	○	○	○	○
Number of Tokens	5	10	20	30	40	50	60	70	80	90	95
Cost	$1.00	$0.75	$0.50	$0.25	$0	$0	$0	$0.25	$0.50	$0.75	$1.00

Second, following each subject's decision about how willing or unwilling they are to enter the election, two candidates are selected from each group of five through a weighted lottery. The selection is based on tokens, which are drawn without replacement. Those who allocated more tokens to increase their chances are more likely to be selected, while those who allocated fewer tokens are less likely. However, because the draw remains probabilistic, every participant retains a nonzero chance of being selected for the election, regardless of how many tokens they submitted. This approach maintains uncertainty in candidate selection and avoids deterministic outcomes.

After selection, each of the two candidates is given the opportunity to send a brief message to the rest of the group. This message can be used to express their motivation for running, describe their qualifications, or make another appeal to voters. Third, all members of the group vote to elect one of the two candidates as the group's representative. Candidates are allowed to vote for themselves if they choose.

Fourth, following the election, all participants complete the addition task once again. During this round, each subject earns $0.25 for each correct answer they solve on their own

[3]This token-based elicitation mechanism was introduced by Pate and Fox (2018) to capture subtle variation in political ambition that was unobservable using a binary elicitation. However, we can approximate a binary decision by grouping those who opt into the election (60 to 95 tokens) versus those who opt out (5 to 40 tokens).

and an additional $0.50 for each correct answer solved by the elected representative. The representative receives $0.75 per correct answer along with a $2.00 bonus. After completing the task, subjects are only shown their score from that round, mirroring the feedback provided in Part 3a.

In Part 3c, subjects are informed that they will complete the addition task once more. As in Part 3b, earnings depend on both individual performance and the performance of a group representative selected through an election. However, this round introduces a prosocial incentive. Before the election takes place, all participants are asked to decide how they would redistribute earnings if they were selected as the representative.

The instructions state: "If you are selected group representative, you can choose how much you want to keep from your total earnings and how much the lowest earner in the group will receive in Part 3C." Since the selection of representatives is probabilistic, every subject has a chance of being chosen, which makes it necessary for all participants to submit their decision of whether and how much to share with the lowest earner. They do so using a slider that ranges from "Keep All Earnings" to "Half of My Earnings Go to the Lowest Earner," increasing in ten percent increments. A screenshot of the decision interface appears in the instructions in the Appendix. At the end of the experiment, one of the three real-effort parts (3a, 3b, or 3c) is randomly selected for payment and added to any earnings from Parts 1 and 2. Subjects are told in advance: "Because you do not know which part will be chosen, you should act as if each part will be paid."

The election procedures used in Parts 3b and 3c generate a key variable of interest: an individual's willingness to participate as a candidate, which we interpret as a proxy for political ambition. The redistribution decision made in Part 3c serves as an additional outcome of interest, capturing how much individuals are willing to share with the lowest earner if placed in a leadership role. This provides a behavioral measure of prosocial tendencies among those willing to compete for power.

3.5 Key Variables of Interest

To identify the causal effect of the prosocial treatment on candidate entry, we compare behavior across two experimental conditions that differ only in the opportunity for redistribution. In the control condition (Part 3b - standard treatment), no redistributive option is available, and the decision to run for office is based solely on private monetary incentives. In the treatment condition (Part 3c - prosocial treatment), participants are informed that the elected representative may choose to transfer up to fifty percent of their earnings to the lowest earner in the group. This introduces a prosocial component into the electoral environment, enabling a within-subject comparison of entry behavior across structures.

Importantly, prior experimental work using the same election design without a redistribution option found that entry decisions were highly stable across repeated rounds, with no significant changes by gender or over time (Pate and Fox, 2025). In that setting, participants had little reason to revise their decisions, and most maintained their baseline preferences. As a result, we interpret any within-subject change in willingness to run between Part 3b and Part 3c as a response to introducing the prosocial option rather than to repetition, learning, or feedback effects.

Several additional variables are central to our analysis. One is self-perceived rank, explicitly influenced by the study's design. Participants estimated the performance of their group's four anonymous members and ranked them in order of perceived scores. Because they know their score, this is a structured exercise to elicit self-assessed relative standing. By comparing beliefs about others to known performance, we examine how perceived competitiveness affects willingness to run and whether it interacts with the incentive structure.

We also incorporate two measures of prosocial preferences. The first is a baseline measure of altruism from the five-dollar dictator game in Part 2. The second measure is collected in the treatment condition, where participants indicate how much of their earnings, from 0%-50%, they would share with the lowest earner in the group if elected. These

two measures allow us to test whether more altruistic individuals are more likely to enter the election and whether the prosocial opportunity draws in candidates with stronger redistributive preferences.

Finally, gender serves as a non-randomized grouping variable that is central to our hypotheses. Although not directly manipulated, gender is expected to moderate responses to the incentive structure. We analyze male and female participants separately to assess whether the prosocial treatment reduces the gender gap in political ambition and whether the effects vary with competitiveness or prosocial preferences.

4 Results

4.1 Subject Characteristics

Our subject pool is comprised of $N = 320$ participants, equally split between the University of Alabama and Loyola Marymount University. Table A1 provides the summary statistics for various demographic and political orientation characteristics by gender, alongside the balance check for statistical equivalence across male and female participants. Women make up 54.1% of the sample, indicating a fairly balanced gender representation in the study. Age has an overall mean of 20.6 years with no significant difference between men and women. Racial composition is diverse but similar across genders. Caucasians comprise approximately 50.9% of the sample, followed by Asians at 16.9%, Hispanics at 15.9%, and Black participants at 9.1%. Political affiliation shows that 35% of participants identify as Democrats, 28.7% as Republicans, and 24.4% as Independents (the remaining participants selected "Other"), indicating a broadly representative political distribution.

However, we do find a significant difference in political affiliation by gender. Among women, 41.6% identify as Democrats, compared to 27.2% of men (p-value $= 0.007$). Ideological orientation, measured on a scale from 1 (extremely liberal) to 7 (extremely con-

servative), shows a slight difference across gender, with men reporting an average score of 3.918 and women reporting 3.630. This difference is marginally significant (p-value $= 0.106$). These patterns are consistent with recent findings documenting increasing divergence in political and social values between men and women, particularly among young adults aged 18 to 29, like in our sample (Saad, 2024).

In contrast, political ambition, measured on a scale from 1 (never thought of having a career in politics) to 5 (thinking about it a lot), does not differ significantly between men and women. This suggests that men and women in our sample report similar levels of interest in pursuing political leadership, regardless of their ideological or partisan leanings.

Given the potential for ideology and political preferences to influence an individual's willingness to run for election and potentially affect sensitivity to prosocial incentives, we utilized two ideologically distinct institutions for the sessions. One lab is "The Interactive Decision Experiment Lab" (TIDE) at the University of Alabama (UA), and the other is the "LMU Experimental Economics Lab (LEEL)" at Loyola Marymount University (LMU). Table A2 presents subject characteristics by university, allowing comparisons across the two universities, such as the gender composition of the samples and the demographic and political characteristics of the subjects.

Although the two samples are similar in gender composition (no statistically significant difference, p-value $= 0.576$) and age (UA students are slightly older with a mean age of 21 years compared to 20 years at LMU, p-value $= 0.064$), they differ significantly in racial makeup and importantly in political orientation. Caucasian students make up a larger share of the UA sample (60.6%) compared to LMU (41.2%) (p-value $= 0.000$), while LMU has a significantly higher proportion of Hispanic students (21.9% at LMU versus 10.0% at UA, p-value $= 0.004$).

Political ideology also varies sharply between the two sites. UA students report a more conservative ideological orientation on average (mean $= 4.350$) than LMU students

(mean $= 3.175$), a statistically significant difference (p-value $= 0.000$). This is reflected in partisan identification: a majority of LMU students identify as Democrats (51.2%), while nearly half of UA students identify as Republicans (45.6%), with both differences being statistically significant (p-value $= 0.000$). Despite these ideological and demographic differences, we find no significant difference in political ambition between students from UA and LMU (p-value $= 0.695$). Given these differences across the two universities, we include institution as a control variable in all analyses.

4.2 Impact of Prosocial Opportunity on Willingness to Run

We begin by examining baseline willingness to run for office, elicited before any treatment intervention. In the first election, conducted under the standard condition without redistribution, men exhibited significantly higher willingness to run than women. On a 0–10 scale, men reported an average willingness of 6.82 (SD = 2.15), compared to 5.62 (SD = 2.20) for women. This difference of 1.20 points is statistically significant (t-test p-value $= 0.000$), and the distributions also differ significantly (Kolmogorov-Smirnov p-value $= 0.000$). This gap is consistent with longstanding findings on gender differences in political ambition and provides a clear baseline for evaluating treatment effects.

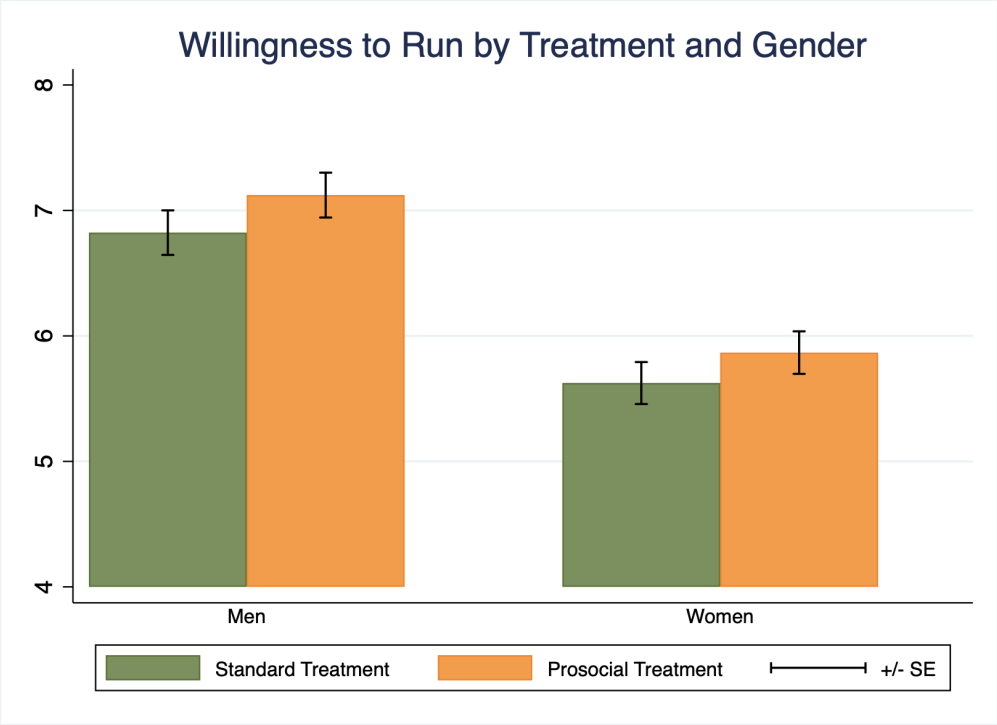
Following this first election, we introduced the prosocial treatment, where all group members were given the option to redistribute up to 50% of their earnings to the lowest earner in the group if elected as the leader. This treatment was designed to test Hypothesis 1, which predicted that the presence of a prosocial incentive would increase women's willingness to run for election. The results support this hypothesis. Among women, average willingness increases from 5.62 to 5.87, a statistically significant increase (paired t-test p-value $= 0.031$). The distributional change is also significant (Kolmogorov-Smirnov p-value $= 0.000$).

Men's willingness to run also increases under the prosocial treatment, rising from 6.82 to 7.12 (paired t-test p-value $= 0.022$), with a similarly significant change in distribution

(Kolmogorov-Smirnov p-value $= 0.000$). This finding contradicts Hypothesis 2, which anticipated little or no effect on men's willingness to run.

Despite these increases in absolute willingness to run, the gender gap persists. Under the standard treatment, the gap is 1.20 points. Under the prosocial treatment, it remains essentially unchanged at 1.25 points (men: 7.12; women: 5.87), with the difference again highly significant (t-test p-value $= 0.000$). This result does not support Hypothesis 3, which predicted that the prosocial incentive would reduce the gender gap. Figure 2 visually illustrates these patterns, showing parallel increases in willingness to run without convergence across gender.

Figure 2: Willingness to Run



Notes. Bars display the average willingness to run of men (left) and women (right) under the Standard Treatment (green) and Prosocial Treatment (orange). Error bars represent mean +/- SE. While the Prosocial Treatment significantly increases the willingness to run of men (Standard: 6.82, Prosocial: 7.12, paired t-test p-value=0.022) and women (Standard: 5.62, Prosocial: 5.87, paired t-test p-value=0.031) the significant difference under the Standard treatment (diff.: 1.20, t-test p=0.000) remains unchanged under the Prosocial treatment (diff: 1.25, t-test p=0.000).

We present a series of regression models in Table 3 to assess the robustness of the

Table 2: Experimental Outcomes by Gender

	(1) All	(2) Men	(3) Women	(4) t-test p-value	(5) K-S p-value
Will. to run - Standard tr.	6.175	6.823	5.624	0.000	0.000
	(2.255)	(2.151)	(2.200)		
Will. to run - Prosocial tr.	6.444	7.122	5.867	0.000	0.000
	(2.285)	(2.167)	(2.228)		
Dictator game giving	1.194	1.177	1.208	0.823	0.237
	(1.239)	(1.403)	(1.085)		
Leader sharing	7.812	6.463	8.960	0.070	0.222
	(12.301)	(11.634)	(12.762)		
Performance	6.794	7.143	6.497	0.068	0.588
	(3.150)	(3.267)	(3.024)		
Rank	3.197	2.864	3.480	0.000	0.000
	(1.311)	(1.368)	(1.194)		
Risk aversion	5.459	5.333	5.566	0.293	0.767
	(1.974)	(2.085)	(1.875)		
Observations	320	147	173		

Notes: Entries report means with standard deviations in parentheses, overall and by gender.
Col. (4) reports p-values of t-tests for equality of means across gender.
Col. (5) reports the results of Kolmogorov-Smirnov tests (p-value) for equality of distribution functions between men and women.

treatment effects and to explore how willingness to run is shaped by gender, performance, self-assessed rank, and risk tolerance. The fixed effects specification in column (1) confirms that the prosocial treatment significantly increases willingness to run. Column (2) includes an interaction between treatment and gender, revealing that the effect of treatment is statistically indistinguishable between men and women.

Despite the overall increase in willingness to run, women remain significantly less likely to enter elections than men in both the standard treatment (col. 3) and the prosocial treatment (col. 5). The estimated gender gap is similar for the two conditions. Including covariates – performance, self-assessed rank, and risk tolerance in columns (4) and (6) – reduces but does not eliminate the gender effect; the coefficient on the female indicator remains negative and significant in both models.

Performance on the real-effort task is positively associated with willingness to run for election in both treatments, indicating that individuals with stronger performance are more inclined to pursue the representative role. In contrast, reporting a lower self-assessed rank within the group is negatively associated with entry in both treatments, consistent with the idea that perceived competitiveness shapes political ambition.[4]

Risk tolerance has mixed effects: it is not significant in the standard treatment. However, it is marginally significant in the prosocial treatment (p-value $= 0.08$), suggesting that the willingness to run for election may be modestly more responsive to redistribution opportunities among individuals who are more comfortable with risk. We also find differences between men and women on the risk measure, where women are significantly less likely to take higher-level risks (diff: -0.41, t-test p-value $= 0.000$). This result was also found in earlier studies (for a complete review of the literature, see Eckel and Grossman (2008a); Croson and Gneezy (2009); Charness and Gneezy (2012) and e.g. Charness et al. (2013); Dave et al. (2010) for important methodological considerations surrounding measurement).[5]

In Table 4, we present a series of regression models that incorporate additional demographic and attitudinal covariates, including age, race, ideological orientation, self-identified party affiliation (e.g., Democrat), and political ambition. The inclusion of these variables does not substantively alter the main findings. In particular, the effects of gender and the prosocial treatment on willingness to run for election remain stable across specifications.

We also test for site-specific differences by including a binary indicator for university location (UA vs. LMU). Across all models, we find no significant differences in willingness to run based on university affiliation, suggesting that the observed patterns are consistent

[4]The Appendix presents Figures A1–A6, which depict the distribution of participant responses by gender across various stages of the experiment, including risk tolerance, willingness to run in each treatment, self-assessed rank, dictator game giving, and leader sharing.

[5]As seen in Figure A1 in the Appendix, men and women are relatively similar in their response to the risk until the survey reaches the higher-level risks.

Table 3: Willingness to Run

	(1) Panel	(2) Panel	(3) Standard	(4) Standard	(5) Prosocial	(6) Prosocial
Prosocial treatment	0.269***	0.299**				
	(0.085)	(0.125)				
Prosocial*Woman		-0.057				
		(0.170)				
Woman			-1.199***	-0.834***	-1.255***	-0.862***
			(0.198)	(0.208)	(0.199)	(0.157)
Performance				0.107**		0.137***
				(0.043)		(0.036)
Rank self-assessed				-0.464***		-0.457***
				(0.117)		(0.109)
Risk tolerance				-0.045		-0.101*
				(0.037)		(0.054)
Constant	6.175***	6.175***	6.823***	7.624***	7.122***	7.994***
	(0.060)	(0.060)	(0.193)	(0.764)	(0.208)	(0.702)
Observations	640	640	320	320	320	320
R^2	0.031	0.031	0.070	0.217	0.075	0.248
Model	FE	FE	OLS	OLS	OLS	OLS

Notes: Dep. variable is willingness to run (between 0 and 10) under the Standard and Prosocial treatment. (1)-(2) Fixed effects. (3)-(6) OLS, standard errors in parentheses clustered at the session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

across distinct institutional and political environments.

Among the covariates, only one variable consistently predicts willingness to run for election: political ambition. This measure, collected in the post-experiment survey, asks respondents to indicate how often they have considered a political career on a five-point scale ranging from "never" to "a lot." The strong positive correlation between this attitudinal item and actual entry behavior in the experiment lends support to its construct validity.

Table 4: Willingness to Run with Socio-Demographic Controls

	(1) All	(2) Standard	(3) Standard	(4) Prosocial	(5) Prosocial
Prosocial treatment	0.299**				
	(0.130)				
Woman	-0.775***	-1.144***	-0.765***	-1.242***	-0.842***
	(0.243)	(0.182)	(0.189)	(0.215)	(0.182)
Prosocial*Woman	-0.057				
	(0.172)				
Performance	0.128***		0.114**		0.141***
	(0.044)		(0.044)		(0.035)
Rank self-assessed	-0.458***		-0.461***		-0.455***
	(0.106)		(0.127)		(0.113)
Risk aversion	-0.061		-0.028		-0.094
	(0.060)		(0.033)		(0.057)
Age	-0.019	-0.031	-0.025	-0.022	-0.014
	(0.036)	(0.035)	(0.047)	(0.042)	(0.048)
Caucasian	-0.075	-0.193	-0.116	-0.117	-0.033
	(0.217)	(0.132)	(0.191)	(0.241)	(0.184)
Business	0.259	0.165	0.333	0.016	0.186
	(0.209)	(0.258)	(0.266)	(0.328)	(0.317)
Conservative ideology	0.004	0.005	-0.020	0.064	0.028
	(0.084)	(0.086)	(0.089)	(0.093)	(0.088)
Democrat	-0.134	-0.236	-0.298	0.064	0.031
	(0.269)	(0.259)	(0.263)	(0.333)	(0.333)
Political ambition	0.194	0.213*	0.235**	0.129	0.153*
	(0.120)	(0.114)	(0.108)	(0.085)	(0.083)
LMU	0.002	-0.115	0.010	-0.102	-0.006
	(0.234)	(0.321)	(0.350)	(0.228)	(0.275)
Constant	7.337***	6.990***	7.380***	7.051***	7.594***
	(1.103)	(0.624)	(0.716)	(1.010)	(1.175)
Observations	640	320	320	320	320
R^2	0.246	0.088	0.237	0.082	0.255

Notes: Dep. variable is willingness to run (0-10) in the Standard and Prosocial treatment.
Col. (1) OLS on entire dataset with errors clustered at the individual level.
Col. (2)-(5) OLS separately by treatment, errors clustered at the session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

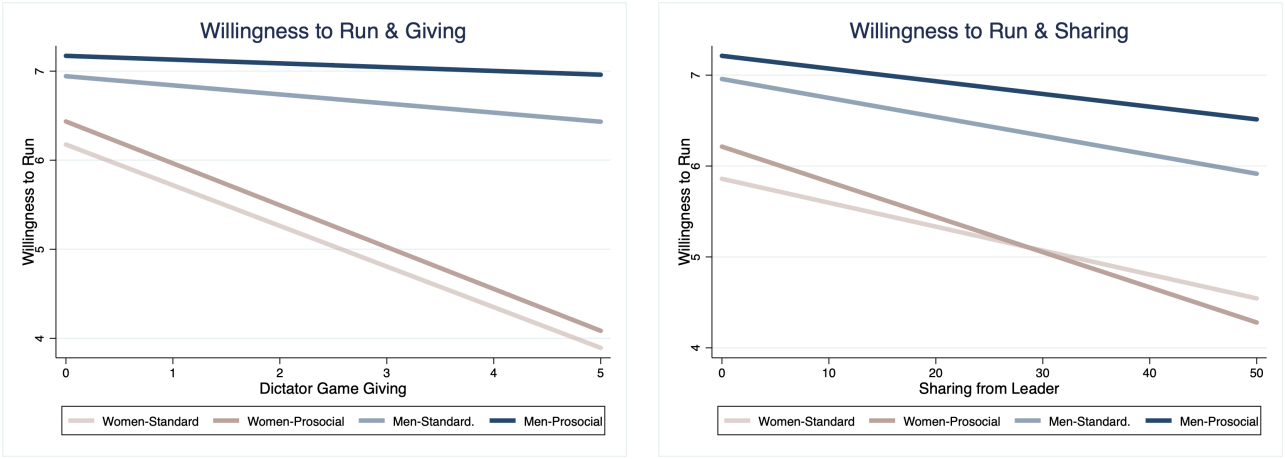
4.3 Willingness to Run is Inversely Related to Prosocial Behavior for Women

We now turn to the relationship between prosociality and the main variable of interest: willingness to run for election. We investigate whether individuals who are more willing to become leaders are also more prosocial, examining two dimensions. First, we test whether willingness to run is associated with generosity in the dictator game in the standard treatment. Second, we examine whether willingness to run correlates with prosocial redistribution decisions leaders would make if elected in the prosocial treatment. Contrary to expectations, the data suggest the opposite pattern for the relationship, but only for women. Figure 3 displays these relationships, with the dictator giving on the left and the leader sharing on the right. In the left panel, fitted lines show a negative relationship between giving and willingness to run among women, with no significant pattern for men. These results are confirmed in Table 3, where the negative relationship between giving and willingness to run remains significant for women, even after controlling for performance, rank, risk aversion, and lab site.

In the right panel of Figure 3, a similarly negative relationship appears between redistribution behavior and willingness to run, but again, only for women. As shown in Table A4, this relationship is significantly negative only for women in the prosocial treatment. These findings suggest that more altruistic women are less likely to express political ambition – a pattern that replicates the findings observed in Cassar and Rigdon (2021b), where those most inclined to act prosocially were not the ones most likely to enter tournament competition. In both studies, the individuals who most value fairness and redistribution are systematically less willing to place themselves in leadership or winner-take-all roles. This raises an important distinction: the intervention invites individuals to act prosocially if they are elected, but those most inclined to share are also the least likely to run. The puzzle is not just about who shares but about how seemingly altruistic and prosocial preferences influence the decision to enter in the first place.

At the same time, we find support for Hypothesis 4, which predicted that the level of giving by elected representatives would not differ significantly by gender based on the findings of Cassar and Rigdon (2021a). As reported in Table 2, the average redistribution amounts are only marginally different between men and women overall (p-value = 0.07), and the distributions are statistically similar. As seen in Figure 3, individuals most willing to enter the election were the least likely to share their earnings, regardless of gender. The decision to share, once elected, appears broadly consistent across men and women, consistent with Hypothesis 4.

Figure 3: Willingness to Run and Prosociality



Note: The left panel displays the average willingness to run in each election and dictator game giving. The right panel shows the average willingness to run in each election and percent of earnings shared if elected as the leader.

Yet, across all levels of giving and sharing, the prosocial option is effective in increasing individual willingness to run for election. Table 5, which pools both observations per subject and uses OLS with errors clustered at the individual level, systematically explores the interactions between political ambition and prosociality, focusing especially on gender differences. Several key findings emerge across the results.

First, introducing the prosocial opportunity significantly increases individual willingness to run across all models. This effect holds after controlling for gender and both prosociality measures, as shown in Table 5 by the significant treatment coefficients in columns (1) through (6).

Second, women, overall, exhibit a lower willingness to run compared to men. This is evident from the consistently negative and significant coefficients across specifications in Table 5. The effect remains even after accounting for the prosocial context and individual measures of altruism and sharing, suggesting that gender differences in political ambition are robust and persistent. Multiple interventions have attempted to reduce this gap. Even in the most recent study by Pate and Fox (2025), where women demonstrated a high level of confidence in their ability to perform the task well, their willingness to run for election did not increase.

Third, it appears that altruism, as measured by dictator game giving, is negatively related to everyone's willingness to run (col. (1)). However, this effect is concentrated among women, for whom the significantly negative coefficients on the interaction between woman and giving in columns (3) and (5) suggest that more generous women are less inclined to run. Yet, the positive coefficients on the interactions between the prosocial treatment and giving in columns (2) and (3) indicate that the treatment may be more effective among individuals who are more altruistic to begin with, although these effects are largely insignificant. This insignificance may be due to limited statistical power, as the number of observations at the higher end of the altruism distribution is small. As a result, we cannot rule out the possibility that a larger sample could detect not only an overall treatment effect but also meaningful heterogeneity in responsiveness to the prosocial incentive across levels of altruism.

Fourth, the relationship between political drive and a leader's willingness to share part of their gains appears to be negative for all participants (col. (4)). When interactions with gender and treatment are included, this relationship loses significance for both men and women. The interaction between sharing and the prosocial treatment is also insignificant and close to zero (cols. (5) and (6)). As a result, we cannot conclude that the prosocial option was particularly appealing to those most inclined to share their earnings. These findings suggest that the prosocial context does not fundamentally change the

Table 5: Willingness to Run & Prosociality

	(1) All	(2) All	(3) All	(4) All	(5) All	(6) All
Prosocial treatment	0.269***	0.234**	0.234**	0.269***	0.305***	0.305***
	(0.085)	(0.117)	(0.118)	(0.085)	(0.100)	(0.100)
Woman	-1.223***	-0.754**	-0.433	-1.166***	-1.046***	-0.722***
	(0.228)	(0.317)	(0.294)	(0.227)	(0.275)	(0.263)
Dictator game give	-0.232**	-0.084	-0.069			
	(0.092)	(0.119)	(0.122)			
Woman*Give		-0.393**	-0.352*			
		(0.190)	(0.189)			
Prosocial*Give		0.029	0.029			
		(0.070)	(0.070)			
Leader share				-0.027***	-0.015	-0.012
				(0.009)	(0.012)	(0.012)
Woman*Share					-0.016	-0.011
					(0.018)	(0.017)
Prosocial*Share					-0.005	-0.005
					(0.008)	(0.008)
Performance			0.124***			0.115**
			(0.045)			(0.045)
Rank self-assessed			-0.442***			-0.459***
			(0.109)			(0.110)
Risk aversion			-0.071			-0.067
			(0.058)			(0.059)
LMU	-0.106	-0.117	-0.035	-0.167	-0.179	-0.081
	(0.228)	(0.228)	(0.208)	(0.226)	(0.225)	(0.206)
Constant	7.166***	6.998***	7.695***	7.097***	7.025***	7.809***
	(0.242)	(0.262)	(0.688)	(0.219)	(0.232)	(0.685)
Observations	640	640	640	640	640	640
R^2	0.093	0.104	0.255	0.097	0.099	0.248
Model	OLS	OLS	OLS	OLS	OLS	OLS

Notes: Dependent variable: willingness to run (0-10) in Standard and Prosocial treatment.
OLS on all data, errors clustered at individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

underlying relationship between redistribution preferences and willingness to run.

Fifth, consistent with previous research (Pate and Fox 2018; 2025), higher performance on the task is positively associated with willingness to run, suggesting that individuals who perform well may feel more confident or motivated to seek leadership roles. Similarly, individuals who report lower self-assessed rankings exhibit reduced willingness to run, indicating that participants do internalize objective performance signals when evaluating their suitability for leadership. Sometimes, opting out may also reflect an altruistic motivation, particularly if someone believes their leadership would not best serve the group. Risk aversion shows only minor, non-significant negative effects. Despite the demographic and ideological differences between participants from UA and LMU, their behavioral patterns in the experiment were remarkably similar. As shown in Table A3 and Table A4 in the Appendix, there are no significant site-level differences in the main outcomes.

Summarizing the results, although the prosocial opportunity increases individuals' willingness to run for election, it does not reduce the gender gap. Moreover, those most inclined to act prosocially—particularly women—are also the least likely to seek an elected role. This creates a puzzle: the treatment invites candidates to serve others, yet those most motivated to serve are the least willing to run. This paradox highlights the challenge of designing interventions to close gender gaps in political ambition. It suggests that while prosocial framing increases overall participation, it may not shift the deeper motivations that influence who chooses to run.

5 Discussion

This study provides new insights into the relationship between prosociality and political ambition, particularly in the context of persistent gender differences in candidate emergence. Despite a growing body of research focused on encouraging more women to

pursue elected office, including recent interventions that target performance beliefs and stereotype framing (for example, see Eckel et al. (2020); Pate and Fox (2025)), few have succeeded in narrowing the gender gap in political ambition. This makes it valuable to explore interventions that directly align with motivations to serve others.

We adapt a previously successful treatment intervention tested by Cassar and Rigdon (2021b), who showed that when winners of a tournament could share a portion of their earnings with a low-performing peer, women were more likely to enter the competition. That design, however, involved simple entry decisions in a relatively low-stakes setting. In contrast, we apply a similar prosocial opportunity in a more socially embedded and politically relevant framework using a repeated election design from Fox and Pate (2023), where participants must decide whether to stand for election, send messages, and compete for a leadership role under public scrutiny.

Our findings support Hypothesis 1: Women's willingness to run increases when the elected representative is given the opportunity to redistribute earnings to the lowest earner in the group. However, Hypothesis 2 is not supported. Men's willingness to run also increases by a similar amount. As a result, Hypothesis 3 is not confirmed. The gender gap in willingness to run remains unchanged. This result points to a central puzzle in the study. While the prosocial incentive is effective in increasing participation for both men and women, it does not shift the underlying distribution of political ambition.

We also find evidence that more altruistic individuals are less likely to run. This pattern is especially pronounced among women. Women who give more in the dictator game and who say they would share more if elected are significantly less likely to express political ambition to serve in the election process. Although the prosocial incentive invites individuals to serve others, those most inclined to serve remain reluctant to seek elected office. This suggests a tension between prosocial motivation and the willingness to take on leadership roles that require public competition. Our finding contributes to the recent literature aiming to better understand the relationship between distributional preferences

and competitive choices (Bartling et al., 2009; Eckel and Fúllbrunn, 2015; Dasgupta et al., 2019).

One possible explanation for this pattern draws on evolutionary and social psychological research. The "staying alive" hypothesis proposes that women's psychology has evolved to avoid physical and social conflict to preserve safety for themselves and their offspring (Campbell, 1999; Benenson and Markovitz, 2014). Cassar (2022) and Cassar and Zhang (2022) extend this idea, arguing that women may also adopt strategies that signal egalitarian intent, downplaying competitiveness, and fostering cooperation within groups. These preferences may guide behavior in electoral settings as well. If women perceive leadership as a context where prosocial intentions are difficult to maintain, they may opt out even when given the opportunity to redistribute earnings. At the same time, women may prefer environments that enable cooperation and equal outcomes without the burdens of visibility, competition, and status-seeking. This perspective may help explain why prosocial framing encourages participation but not necessarily from those with the strongest prosocial tendencies.

Interestingly, we find no evidence that men perceive the environment differently in the prosocial setting. This contrasts with earlier findings by Pate and Fox (2018, 2025), where framing the task as female-typed or focused on service reduced men's willingness to run. Those effects are not observed here. We also do not observe a gender difference in actual redistribution: women are no more likely than men to share earnings if elected. This confirms Hypothesis 4 and replicates the findings of Cassar and Rigdon (2021a,b), who found no gender difference in the amount shared by the participants.

While previous research has found that prosocial environments can increase women's competitiveness, the political context introduces barriers that reduce the appeal of candidacy even when redistribution is possible. These differences are especially striking when comparing the results of the experiment in Cassar and Rigdon (2021) to the current study, which embeds redistribution within a public election involving self-selection, campaign-

ing, and more concentrated competition. Implementing the election as a lottery also provides a concrete measure of the intensive choices around willingness to run. Results have shown that women are far less competitive under competitive pressure than when the measure is a compete-or-not decision (Saccardo et al., 2018). The design by Cassar and Rigdon, where the prosocial incentive closes the competitive gender gap, is of the latter variety. This contrast highlights the importance of context and social framing in the effectiveness of prosocial interventions.

Looking forward, future studies could explore alternative redistribution mechanisms that more closely mirror real-world political discretion. In our design, leaders could share only with the lowest earner, as in Cassar and Rigdon (2021a,b), where inequality was known and unidirectional. A more flexible approach would allow elected leaders to allocate earnings among all group members, including themselves, after observing actual performance. This variation would enable status concerns and strategic tradeoffs to emerge more clearly and continue to build on Charness's foundational research on trust, fairness, and distributional preferences in group settings (e.g., Charness and Levine, 2000; Charness and Rabin, 2002; Charness and Levine, 2002; Charness, 2004; Charness and Shmidov, 2019; Fehr and Charness, 2025). Incorporating insights along these lines can help design interventions that better align ethical motivations with public leadership, especially for individuals who are hesitant to seek elected office.

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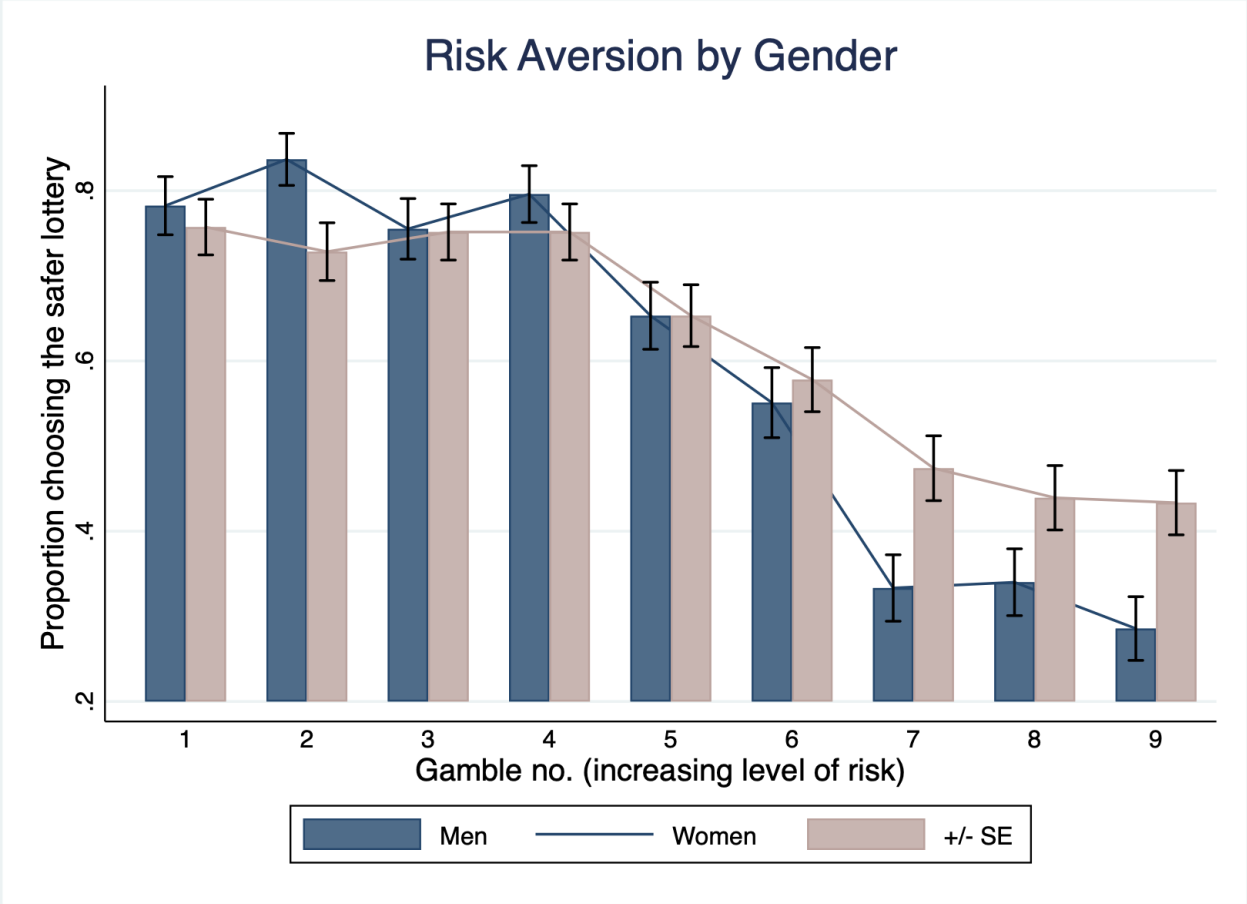
Appendix

Appendix Figures

Figure A1: Risk Aversion by Gender



Notes. Bars display the proportion of individuals choosing the safer lottery separately for men (navy) and women (rose), with lines that connect the proportions across the nine gambles. Error bars represent mean +/- SE. While there is no statistical difference between men and women in the safer five gambles (gambles 1-5), a gender difference emerges for the riskier four gambles (gambles 6-9). Summing the number of safer choices in gambles 6 through 9, the average women's score (1.925) is significantly higher than the average men's score (1.510) – indicating greater risk aversion – a highly statistical difference (t-test p-value $= 0.0099$)

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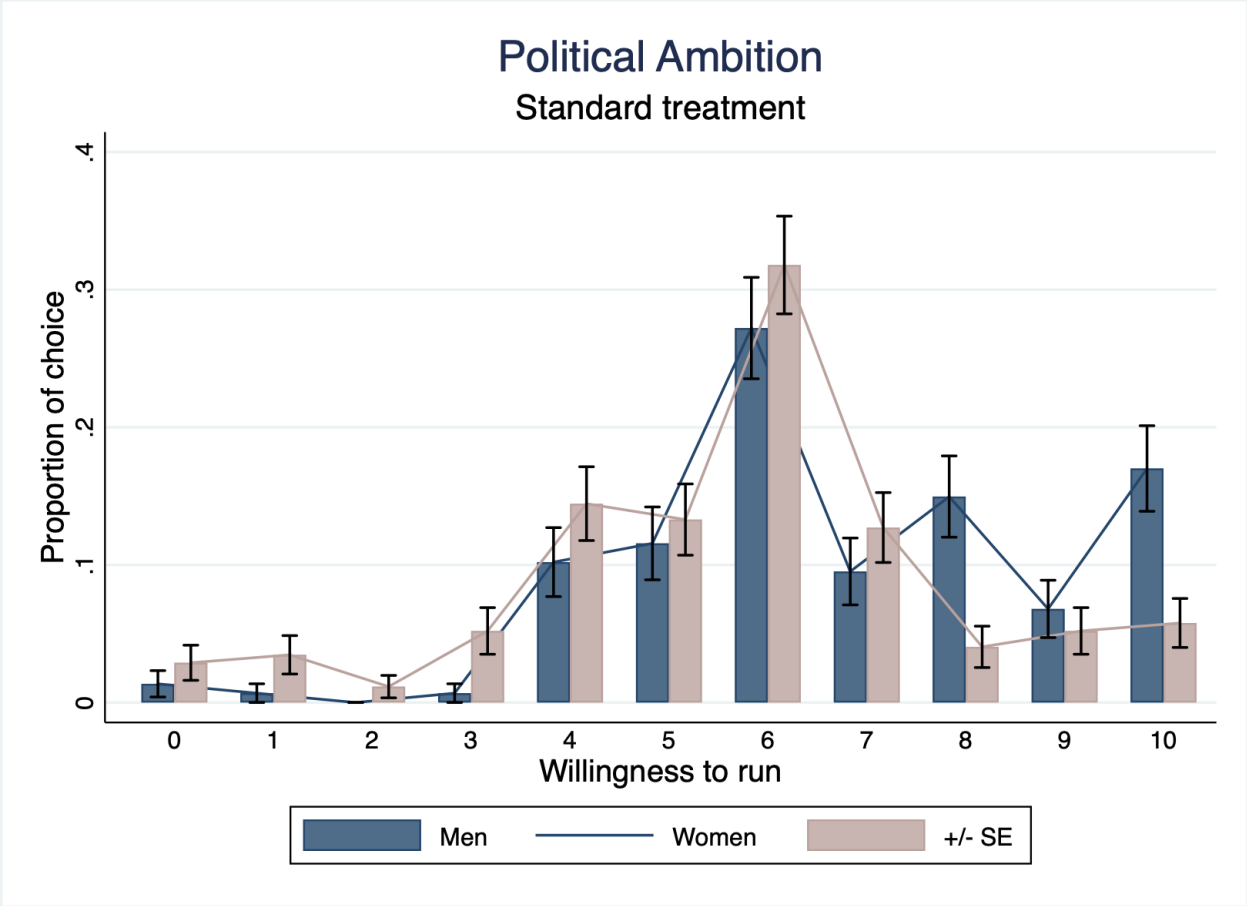
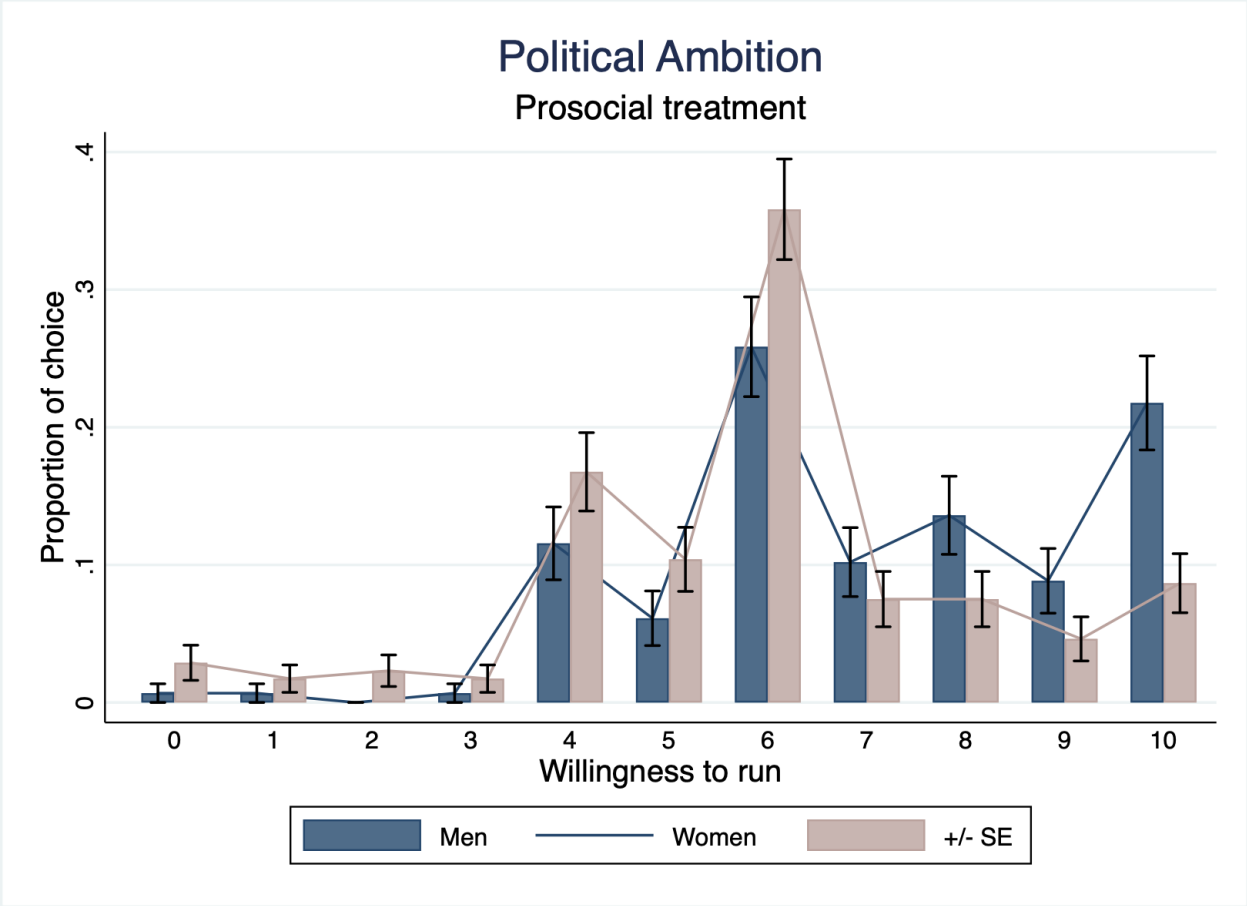
Figure A2: Willingness to Run - Standard Treatment



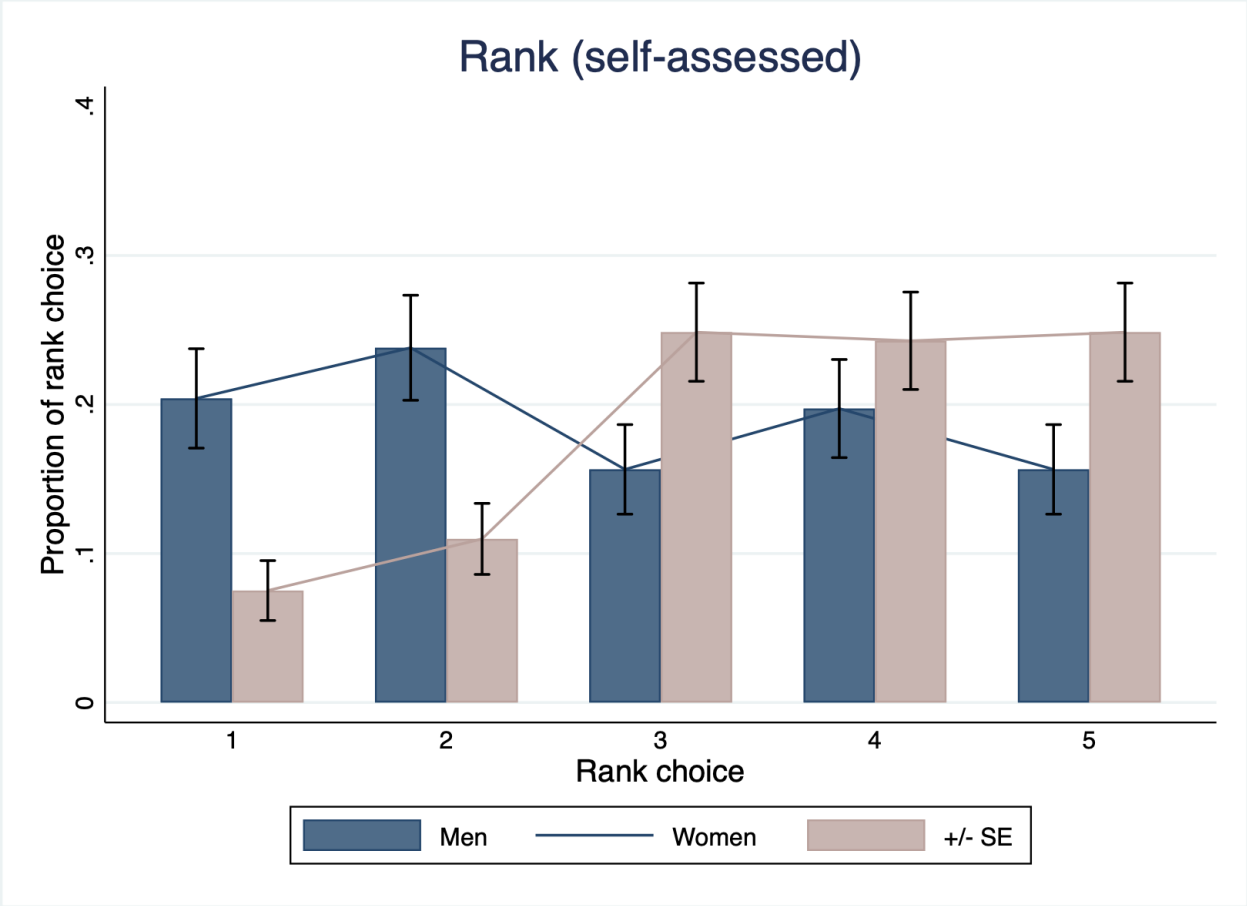
Notes. Bars display the proportion of individuals choosing the level of willingness to run separately for men (navy) and women (rose) with lines that connect the proportions across the eleven choices. Error bars represent mean +/- SE.

Figure A3: Willingness to Run - Prosocial Treatment



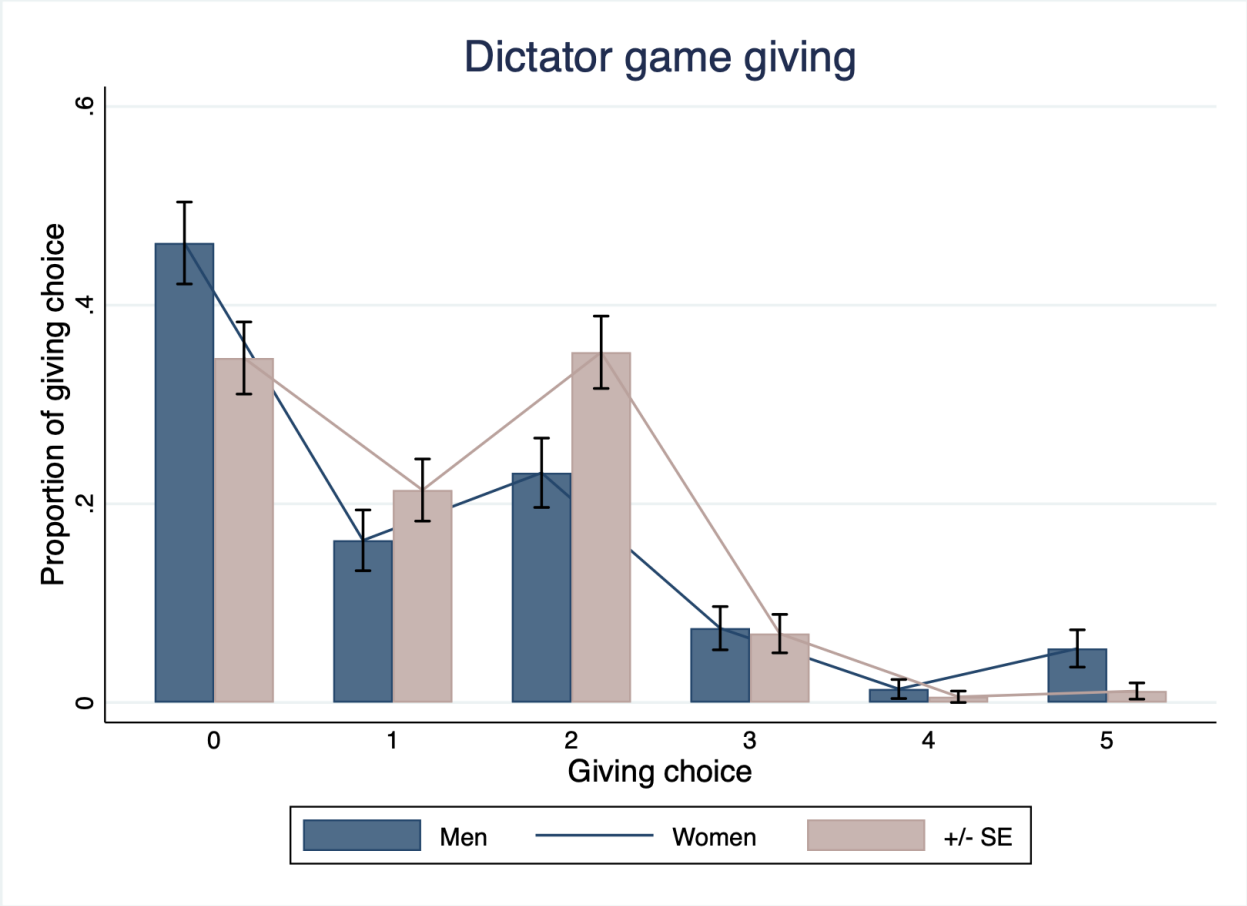
Notes. Bars display the proportion of individuals choosing the level of willingness to run separately for men (navy) and women (rose), with lines that connect the proportions across the eleven choices. Error bars represent mean +/- SE.

Figure A4: Rank (self-assessed)



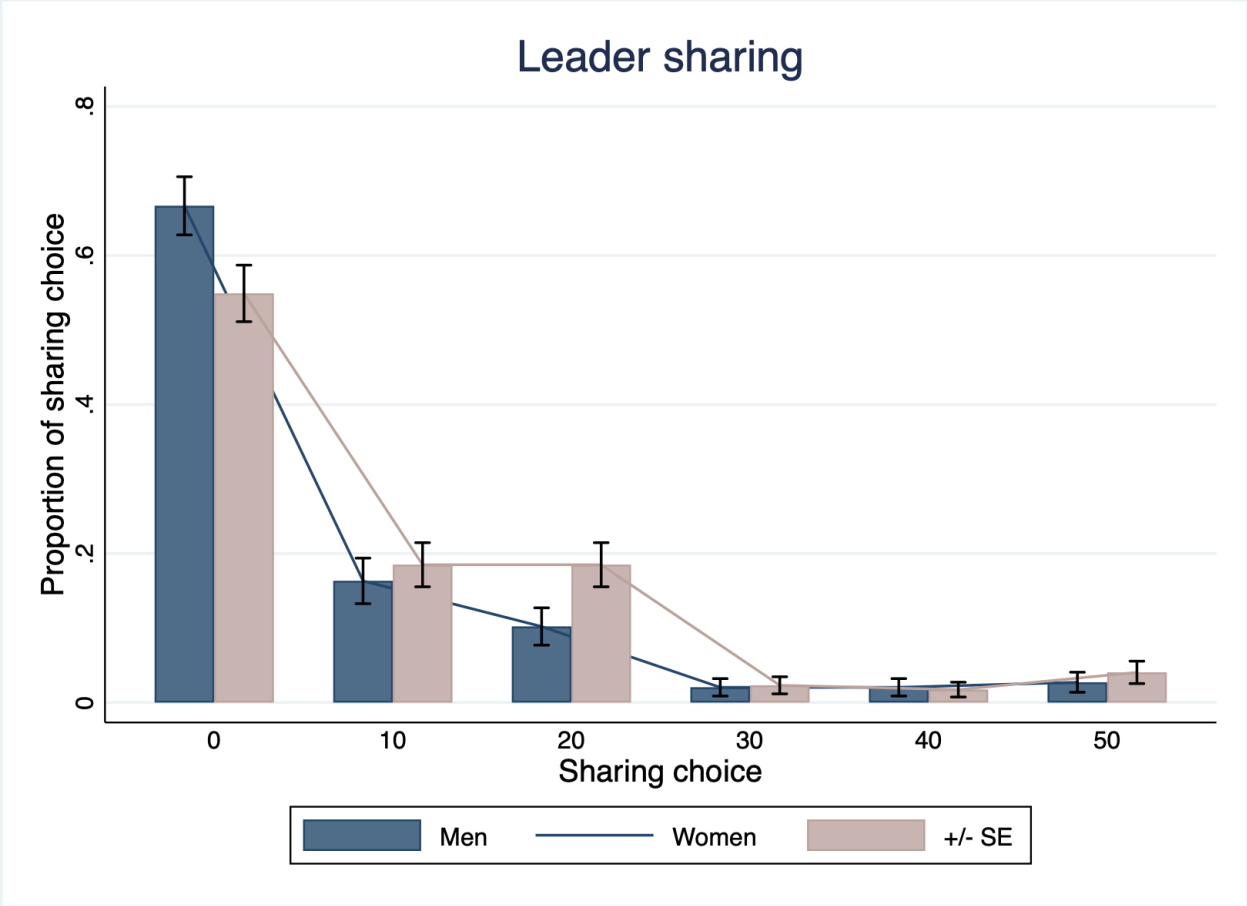
Notes. Bars display the proportion of individuals choosing the respective rank (self-assessed) separately for men (navy) and women (rose) with lines that connect the proportions across the five possible ranks. Error bars represent mean +/- SE.

Figure A5: Dictator game giving by gender



Notes. Bars display the proportion of individuals choosing the respective amount of dictator game giving separately for men (navy) and women (rose) with lines that connect the proportions across the six possible dictator giving choices. Error bars represent mean +/- SE.

Figure A6: Leader sharing by Gender



Notes. Bars display the proportion of individuals choosing the respective amount of leader sharing separately for men (navy) and women (rose), with lines that connect the proportions across the six possible sharing choices if elected as the leader. Error bars represent mean +/- SE.

Appendix Tables

Table A1: Summary Statistics by Gender and Balance Check

	(1) All	(2) Men	(3) Women	(4) p-value
Woman	0.541			
	(0.499)			
Age	20.647	20.483	20.786	0.438
	(3.475)	(2.849)	(3.933)	
Asian	0.169	0.190	0.150	0.340
	(0.375)	(0.394)	(0.358)	
Black	0.091	0.095	0.087	0.792
	(0.288)	(0.295)	(0.282)	
Caucasian	0.509	0.497	0.520	0.675
	(0.501)	(0.502)	(0.501)	
Hispanic	0.159	0.136	0.179	0.295
	(0.367)	(0.344)	(0.385)	
Business major	0.459	0.503	0.422	0.146
	(0.499)	(0.502)	(0.495)	
Conservative ideology	3.763	3.918	3.630	0.106
	(1.588)	(1.542)	(1.618)	
Democrat	0.350	0.272	0.416	0.007
	(0.478)	(0.447)	(0.494)	
Republican	0.287	0.293	0.283	0.856
	(0.453)	(0.456)	(0.452)	
Independent	0.244	0.279	0.214	0.178
	(0.430)	(0.450)	(0.411)	
Political ambition	2.753	2.741	2.763	0.847
	(0.994)	(0.966)	(1.021)	
Observations	320	147	173	

Note: Summary statistics for the total sample and by gender.
Entries indicate mean with standard deviations in parentheses.
p-values test (t-test) for differences between men and women.

Table A2: Subject Characteristics by University

	(1) All	(2) UA	(3) LMU	(4) p-value
Woman	0.541	0.556	0.525	0.576
	(0.499)	(0.498)	(0.500)	
Age	20.647	21.006	20.288	0.064
	(3.473)	(3.472)	(3.441)	
Asian	0.169	0.156	0.181	0.552
	(0.375)	(0.364)	(0.386)	
Black	0.091	0.075	0.106	0.332
	(0.287)	(0.264)	(0.309)	
Caucasian	0.509	0.606	0.412	0.000
	(0.500)	(0.489)	(0.493)	
Hispanic	0.159	0.100	0.219	0.004
	(0.366)	(0.300)	(0.414)	
Business major	0.459	0.519	0.400	0.033
	(0.499)	(0.501)	(0.491)	
Conservative ideology	3.763	4.350	3.175	0.000
	(1.586)	(1.552)	(1.392)	
Democrat	0.350	0.188	0.512	0.000
	(0.477)	(0.391)	(0.501)	
Republican	0.287	0.456	0.119	0.000
	(0.453)	(0.499)	(0.324)	
Independent	0.244	0.231	0.256	0.604
	(0.430)	(0.422)	(0.437)	
Political ambition	2.753	2.731	2.775	0.695
	(0.994)	(0.969)	(1.021)	
Observations	320	160	160	

Note: Summary statistics for the total sample and by university.
Entries indicate mean with standard deviations in parentheses.
p-values test (t-test) for differences between UA and LMU.

Table A3: Willingness to Run & Dictator Giving

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men		Women		Standard tr.		Prosocial tr.	
Prosocial tr.	0.299**	0.229	0.243**	0.259				
	(0.130)	(0.167)	(0.112)	(0.163)				
Dictator give	-0.070	-0.100	-0.462***	-0.456***	-0.247***	-0.099	-0.218**	-0.040
	(0.114)	(0.125)	(0.152)	(0.156)	(0.075)	(0.067)	(0.083)	(0.104)
Prosocial*Give		0.060		-0.014				
		(0.102)		(0.089)				
Woman					-1.195***	-0.769***	-1.251***	-0.738**
					(0.195)	(0.249)	(0.203)	(0.321)
Woman*Give						-0.357**		-0.430*
						(0.159)		(0.202)
LMU	-0.093	-0.093	-0.137	-0.137	-0.122	-0.132	-0.090	-0.102
	(0.335)	(0.335)	(0.311)	(0.312)	(0.283)	(0.280)	(0.223)	(0.216)
Constant	6.953***	6.989***	6.250***	6.241***	7.176***	7.008***	7.425***	7.222***
	(0.289)	(0.295)	(0.257)	(0.261)	(0.258)	(0.277)	(0.246)	(0.276)
Observations	294	294	346	346	320	320	320	320
R^2	0.007	0.008	0.055	0.055	0.090	0.099	0.090	0.103
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Note: Dependent variable: willingness to run (0-10) in Standard and Prosocial treatments.

Col. (1)-(4) Men and Women OLS, errors clustered at individual level.

Col. (5)-(8) OLS by treatment, errors clustered at the session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Willingness to Run & Leader Sharing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men		Women		Standard tr.		Prosocial tr.	
Prosocial	0.299**	0.255*	0.243**	0.354***				
	(0.130)	(0.152)	(0.112)	(0.129)				
Leader share	-0.017	-0.021*	-0.034**	-0.027**	-0.024**	-0.021	-0.029***	-0.014
	(0.012)	(0.012)	(0.014)	(0.013)	(0.010)	(0.013)	(0.007)	(0.015)
Prosocial*Share		0.007		-0.012				
		(0.013)		(0.009)				
Woman					-1.144***	-1.095***	-1.188***	-0.997***
					(0.184)	(0.309)	(0.212)	(0.307)
Woman*Share						-0.006		-0.026
						(0.021)		(0.021)
LMU	-0.098	-0.098	-0.249	-0.249	-0.182	-0.187	-0.152	-0.172
	(0.332)	(0.333)	(0.306)	(0.306)	(0.275)	(0.267)	(0.221)	(0.213)
Constant	6.986***	7.008***	6.046***	5.991***	7.075***	7.053***	7.387***	7.300***
	(0.270)	(0.272)	(0.217)	(0.219)	(0.261)	(0.317)	(0.260)	(0.298)
Observations	294	294	346	346	320	320	320	320
R^2	0.014	0.014	0.041	0.042	0.089	0.089	0.100	0.104
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Note: Dependent variable: willingness to run (0-10) in Standard and Prosocial treatments.

Col. (1)-(4) Men and Women OLS, errors clustered at individual level.

Col. (5)-(8) OLS by treatment, errors clustered at the session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Experiment Instructions

General Information

This is an experiment on decision-making. Several research agencies have provided funds for this study.

There are multiple parts to this experiment. Follow the instructions closely, as we will explain how you will earn money and how your earnings will depend on the choices that you make. All of your earnings will be paid to you (in cash) at the end of the experiment. You will be paid your earnings privately, meaning that no other participant will find out how much you earn. Also, for simplicity, we will hand out and read the instructions for each part before beginning that part. Each participant will have a printed copy of the instructions. You may refer to your printed instructions at any time during the experiment. If you have any questions, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Also, please ensure that your cell phones are turned off and put away. Participants intentionally violating the rules will be asked to leave and will not be paid.

We will now begin the first part of the experiment.

Part One

In this part, you will make a series of nine choices. For each choice, you will decide between two lotteries. You must pick option A or option B. The option you have selected will turn red to indicate your choice. You may change your mind for any choice up until the point that you click the "OK" button. You must make a selection for each of the nine choices.

At the end of the experiment, we will roll a ten-sided die to determine the choice for which you will be paid. If the result of the die roll is a number from one to nine, that number corresponds with the line for which you will be paid. If the result of the die roll is a ten, we will re-roll the die until it lands on a number from one to nine, and that number will

correspond with the choice for which you will be paid.

Once we have randomly selected a lottery, we will then roll a six-sided die to determine the amount you will be paid. If you have chosen Option A, a roll of one through three corresponds with the smaller payment, and a roll of four through six corresponds with a larger payment. If you have chosen Option B, a roll of one or two corresponds with the smaller payment, a roll of three or four corresponds with the middle payment, and a roll of five or six corresponds with the higher payment.

Remember that you must pick an option for each of the nine choices. The actual earnings for this part will be determined at the end of the experiment and will be independent of your earnings from other parts.

When you are satisfied with all of your choices, click the "OK" button to submit them. Once everyone clicks "OK" we can move to the next part of the experiment.

Please make your decisions now. If you have any questions, raise your hand.

Part Two

In this part of the experiment, you will be randomly paired with one other person in the room. You will not know the identity of the other person and they will not know who you are.

All participants in the room will be asked to allocate $5 between you and the other randomly matched participant. There are 6 ways that $5 can be allocated between two people in even dollar amounts (see the table below). You must select one (and only one) of these listed options.

Check one of the following boxes:

- $0 for you, $5 for the other person
- $1 for you, $4 for the other person
- $2 for you, $3 for the other person
- $3 for you, $2 for the other person

- $4 for you, $1 for the other person
- $5 for you, $0 for the other person

After all participants have made their choices, the computer will randomly determine if you are the proposer or the receiver. If you are the proposer then your proposed allocation is implemented. For example, suppose you proposed $X for you and $5-X for the other person, then your earnings for this part of the experiment is $X. However, there is an equal chance that you are a receiver, and in that case your earnings will be determined according to the allocation proposed by the other person in your group.

The selection of your role (proposer/receiver) and the actual earnings for this part will be determined at the end of the experiment, and will be independent of your earnings from other parts.

Please make your decision now. If you have any questions, raise your hand.

Part Three

This section of the experiment consists of **three parts, 3A, 3B, and 3C**, only one of which will be chosen for payment. At the end of the experiment, we will roll a 3-sided die to determine which part is chosen, thus each part is equally likely to be selected.

For this section of the experiment, the computer has randomly placed you into a group with four other participants. You will not know who among the other participants in this experiment are in your group and they will not know that you are in theirs. Your only communication with other group members will be through the computer.

In each part, you will be asked to perform a mathematical task and you will be paid based at least partly on your ability to perform this task well. This task has been chosen because there are no differences based on education level, socio-economic status, gender, or race in the ability of people to perform the task well.

In this first part, Part 3A, you will be asked to calculate the sum of five randomly chosen two-digit numbers. You will have 4 minutes to solve as many of these sums as possible.

You can use the provided scratch paper to help you, but you may not use a calculator. When you have an answer, enter it into the provided space and click the "Submit" button. The computer will automatically tell you if your answer is correct. The computer will also keep a running tally of how many correct and incorrect answers you have entered. This is private information for you only. None of the other participants in the experiment will see how many correct and incorrect answers you have.

If this part is randomly selected for payment, you will be paid 75 cents ($0.75) for each correct answer you provide in addition to your earnings from other parts of the experiment. Note that your payment will not decrease if you provide an incorrect answer.

At the end of the experiment, one of the parts from this section will be randomly selected to determine your payment for this portion of the experiment. Because you do not know which part will be chosen, you should act as if each part will be paid.

Please do not talk with one another.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Part Four

We will return to the section with the math task in just a moment. For now, we'd like to ask you a few questions about how you think the other members of your group performed on the task in Part 3. You will be paid based on how accurate your predictions are. Remember that you will be making predictions only about the other four members of your group, not yourself. In other words, you will be asked to make predictions about the performance of the highest performer, the second highest performer, the third highest performer, and the lowest performer. These rankings do not include you; we are asking only about the other four members of the group.

Specifically, we want to know how well you think each person in your group did on the task in Part 3. Once we have completed reading the instructions, you will be able to enter and submit your estimates in the table provided on the screen.

Your earnings for Part 4 will be determined as follows. First, we will randomly select one of the other members of your group to use to determine your earnings (i.e., the highest performer, second highest, third highest, or lowest) by rolling a die. Thus, each of the other members is equally likely to be selected. We will then compute your payment based on the accuracy of your predictions for that member. If your estimate of their Part 3 score is exactly correct, you will receive $10, but if your estimate is not exactly correct, you will be paid $5 divided by the (absolute) difference between your estimate and that member's actual score (with the amount rounded to the nearest quarter).

For example, if the highest scorer's true score in Part 3 was X and your estimate was X exactly, you will earn $10. And, for example, if the correct score for, say, the second-highest performer was Y and your estimate was Y+1 or Y-1, you would be paid $5.00/1 = $5.00. If your estimate was Y+5 or Y-5, then your estimate was off by 5 and you will be paid $5.00/5 = $1.00. In other words, your payment will go down as your estimate decreases in accuracy, and it will go up as your estimate increases in accuracy.

Your actual earnings for this part will be determined at the end of the experiment, and will be independent of earnings from other parts.

When you are satisfied with all of your choices, click the "OK" button to submit them.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Part 3B

Returning to the previous section: As in Part 3A, you will be given 4 minutes to calculate the sums of a series of five randomly chosen two-digit numbers. However, your payoffs will now be based upon both your own performance and the performance of an elected representative from your group. This part of the experiment has four steps:

Step #1: Indicate your willingness to run in an election to be the group representative.

Step #2: If you are a candidate, you will send a message to your group.

Step #3: Everyone will vote in the election.

Step #4: Complete the mathematical task again.

How the Candidates are Determined

The computer will randomly draw tokens to select two candidates for the election. At the start, each person in the group currently has 50 tokens or "entries" in the drawing to be a candidate. However, you have the ability to *increase* or *decrease* your chances of being selected as a candidate by *increasing* or *decreasing* the number of tokens you have in the drawing. Having *more* tokens in the drawing *increases* the likelihood that the computer will draw your token to be a candidate. Having *fewer* tokens in the drawing *decreases* the likelihood that the computer will draw your token to be a candidate.

You may increase or decrease your total number of tokens by 10 at no additional cost (change to either 40 tokens or 60 tokens), but there is a cost beyond 10 tokens, which will be deducted from your payment for this part of the experiment (if this part is selected for payment). The minimum number of tokens per person is 5, and the maximum number of tokens per person is 95. Thus, there is no option that allows you to guarantee that you either will or will not be a candidate; you can only increase or decrease your likelihood of being selected.

The cost to increase or decrease your total number of tokens appears in the table below:

	Decrease your likelihood of being selected					**Current tokens**	**Increase your likelihood of being selected**				
	○	○	○	○	○	◉	○	○	○	○	○
Number of Tokens	**5**	**10**	**20**	**30**	**40**	**50**	**60**	**70**	**80**	**90**	**95**
Cost	$1.00	$0.75	$0.50	$0.25	$0	**$0**	$0	$0.25	$0.50	$0.75	$1.00

After everyone has decided, the computer will draw tokens until **two people from the group** are selected to be candidates in the election.

The Election Process

If you are selected to be a candidate in the election, you will be able to send a message to the other members of your group of 150 characters or less. Members of your group will see your message before they make their voting decisions. This message is the only information members of the group will have about each candidate before they decide for whom to vote. Candidates may write anything they choose, provided that it is under 150 characters and does not contain any obscene or offensive language. Be sure to hit enter when you are finished with your message. You will have an opportunity to confirm that your message is correct before it is sent to the other members of your group. You must vote in the election; you can vote only once, and you may vote for yourself if you choose.

After the election, everyone will perform the math task again, exactly as before.

If you are the group representative and Part 3B is selected for payment, you will be paid 75 cents ($0.75) for each correct answer you provide. (Other group members will earn $0.50 for each correct answer you provide.) You will also receive a flat $2.00 payment for being the representative.

If you are not the group representative, you will be paid 25 cents ($0.25) for each correct answer you provide and 50 cents ($0.50) for each correct answer your group representative provides during this part.

– –

We will hand out the additional instructions for Part 3C in just a moment. Remember that you will be randomly reassigned by the computer to a new group with 4 other participants each time. At the end of the experiment, one of the three parts from this section (Part 3A, Part 3B, or Part 3C) will be randomly selected for payment and added to your other earnings from the experiment. Because you do not know which part will be chosen, you should act as if each part will be paid.

Remember not to talk with anyone during the experiment.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Part 3C

As in Part 3A and Part 3B, you will be given 4 minutes to calculate the sums of a series of five randomly chosen two-digit numbers and your payoffs will be based upon both your own performance and the performance of an elected representative from your group.

The group representative will be selected exactly as in the elections process outlined in Part 3B. However, this time, **the elected representative will have a decision to make**: the representative can choose how much they want to receive from their total earnings in Part 3C and how much the lowest earner in the group will receive in Part 3C.

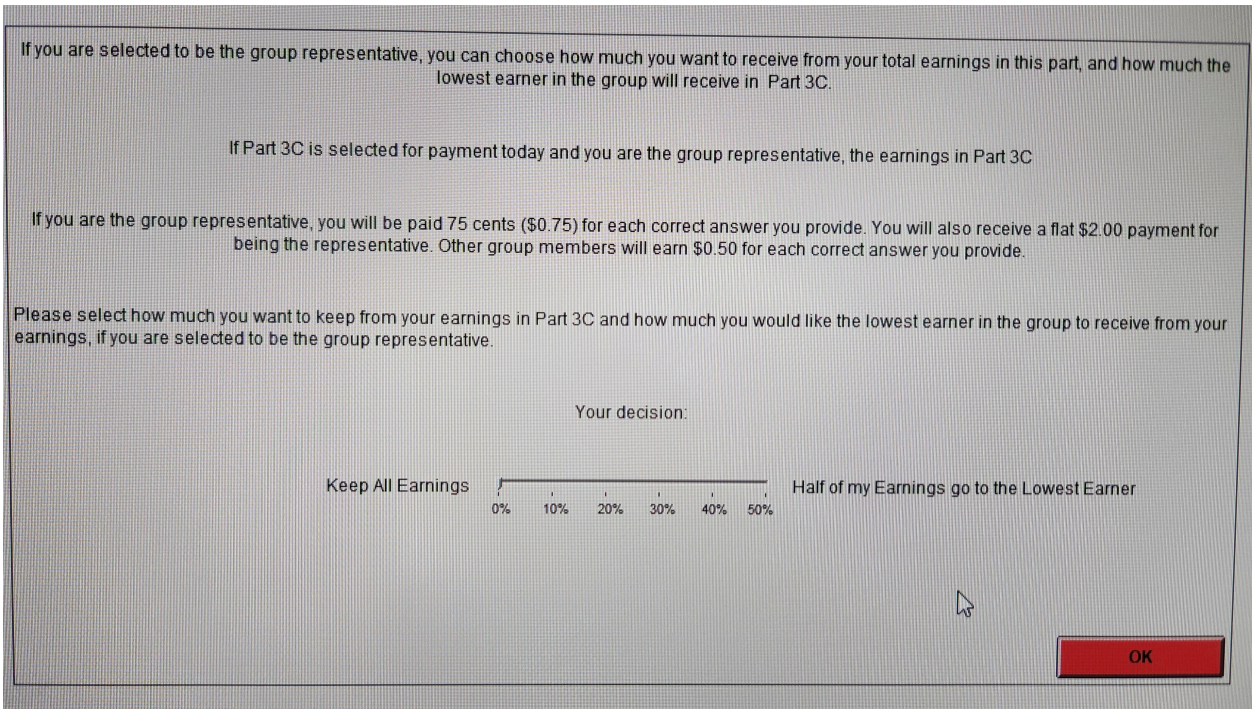
If you are the group representative and Part 3C is randomly selected for payment, you will be paid 75 cents ($0.75) for each correct answer you provide. You will also receive a flat $2.00 payment for being the representative. Other group members will earn $0.50 for each correct answer you provide. You can also choose how much of the amount you earned in Part 3C that you will receive and how much of the amount you earned the lowest earner in the group will receive. In the case of a tie, the lowest earner will be determined randomly.

If you are not the group representative, you will be paid 25 cents ($0.25) for each correct answer you provide and 50 cents ($0.50) for each correct answer your group representative provides during this part. If you are the lowest earner in the group, you may also receive an additional amount from the group representative.

At the end of the experiment, one of the three parts from this section (Part 3A, Part 3B, or Part 3C will be randomly selected for payment and added to your other earnings from the experiment. Because you do not know which part will be chosen, you should act as if each part will be paid.

Remember not to talk with anyone during the experiment.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.



Decision screen as seen by subjects in Part 3c.