The Puzzle of Political Ambition and Prosociality

Alessandra Cassar*, Jennifer Pate†, and Mary L. Rigdon††

June 21, 2026

Abstract

We study whether embedding a redistribution opportunity in political competition alters political ambition and narrows the gender gap in willingness to run for office. Existing evidence indicates that women are more inequality-averse and less likely to enter competitive settings, but perhaps more responsive to redistributive incentives. We test whether redesigning competitive political environments to include a redistributive opportunity, more specifically the option to transfer money to the lowest earner, can mitigate gender disparities in candidate entry. We conducted a laboratory experiment with 320 participants from the University of Alabama and Loyola Marymount University using a within-subject design. We find that the Redistribute-to-Lowest treatment increases willingness to run for both women and men by nearly 5%, but does not reduce the gender gap in political ambition: women report 17.6% lower willingness to run than men. We also find a negative correlation between willingness to run and giving, revealing a paradox: those most inclined to help others are often the least likely to seek positions that would allow them to do so.

Keywords: Elections, Voting, Gender, Willingness-to-compete, Politics, Experiment

*Professor of Economics, University of San Francisco. Email: acassar@usfca.edu. †Professor of Economics, Loyola Marymount University. Email: jennifer.pate@lmu.edu. ††Associate Professor of Political Economy and Moral Science and Director of the Freedom Center, University of Arizona. Email: mrigdon@arizona.edu.

The study received funding from the National Science Foundation #1919535 to Cassar and Rigdon. Rigdon thanks the Institute for Humane Studies for a Course Support grant in Fall 2024. Ethical clearance for human subjects review was approved by Loyola Marymount University. The experiment was pre-registered and the pre-analysis plan is available at: Open Science Framework "Social Incentives to Close the Political Ambition Gap".

1 Introduction

In the 119th Congress, convened in January 2025 and serving through 2027, 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 under-representation: 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).

Our study investigates whether introducing a redistribution-based incentive into electoral competition can serve as a structural mechanism to increase women's participation and reduce the ambition gap. It is motivated by evidence of gender differences in distributional and competitive preferences: although findings regarding overall gender differences in prosociality are mixed (Croson and Gneezy, 2009; Spadaro et al., 2022; Balliet et al., 2011; Falk and Hermle, 2018; Kamas and Preston, 2015; Cassar and Rigdon, 2022), women appear to favor egalitarian allocations more than men (Engel, 2011; Sharma, 2015; Arau'jo et al., 2017; Falk and Hermle, 2018), place less emphasis on maximizing aggregate resources–a different form of prosociality (Schwartz et al., 2002; Cassar et al., 2026), and exhibit lower levels of competitiveness in winner-take-all tournaments (Niederle and Vesterlund, 2011; Buser et al., 2014; Flory et al., 2015; Cassar and Zhang, 2022). Examining gender differences in these core economic preferences is particularly important, as such differences have been shown to translate into disparities in economic outcomes, occupational sorting, and educational choices (Fehr and Charness, 2025).

Building on the experimental paradigms developed by Kanthak and Woon (2015) and extended by Pate and Fox (2018) to study elections in the laboratory, we introduce a

redistribution-based treatment to investigate whether incorporating an inequality-reducing incentive — specifically, the opportunity for elected leaders to transfer some earnings to the lowest earners — can mitigate the gender gap in political ambition. Prosocial incentives, other-regarding preferences, and opportunities to benefit others have been shown to significantly influence behavior (see e.g., Charness and Rabin, 2002; Charness et al., 2007; Fehr and Charness, 2025), and can be harnessed as a powerful mechanism for narrowing gender differences in competitive environments. For example, including payment schemes that allow winners to share a portion of their earnings with a low-performing participant (Cassar and Rigdon, 2021a,b), or rewards that benefit their children (Cassar et al., 2016; Cassar and Zhang, 2022), or 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 directly.

We extend this logic to the political domain and test whether the opportunity to redistribute earnings to the group's lowest-earning member would similarly increase women's willingness to run for election. In previous work, men were generally not affected by the introduction of a redistributive form of prosocial incentive (Cassar and Rigdon, 2021a,b). Thus, when these incentives increased women's willingness to compete but had no effect on men, the gender gap in competitiveness closed. Whether a similar pattern will hold in elections depends not only on whether more women choose to run, but also on how men respond to the same incentive structure. Unlike competitiveness studies, prior research has shown that community service or female-stereotyped framing reduces men's willingness to enter elections (Pate and Fox, 2018), raising the possibility that a redistribution-based incentive aimed at reducing inequity might also make the role less attractive to men.

Here, we compare men's and women's willingness to run under two treatments: a standard incentive structure in which the players' earnings are automatically linked to the

elected leader's performance (Standard treatment) and a redistribution-based condition in which elected leaders are given the opportunity to transfer a portion of their earnings to the lowest earner in their group (Redistribute-to-Lowest, or RTL treatment).

The Standard and RTL treatments reflect two distinct logics of democratic leadership. In the Standard condition, in addition to anticipating higher individual earnings, participants may run because they believe their competence will improve aggregate group outcomes—a form of efficiency-oriented prosociality. In contrast, the RTL condition introduces an explicit redistribution mechanism targeting the lowest earner, reflecting inequality-reducing motivations. Political theory has long identified this efficiency–equality tension as a central axis of democratic competition (Okun, 1975; Meltzer and Richard, 1981; Alesina and Angeletos, 2005). Our design allows us to examine how these competing conceptions of leadership shape the willingness to seek elected office.[1]

We conducted our experiment in two laboratories at Loyola Marymount University and University of Alabama with 320 undergraduate subjects. Our results show that the option for leaders to redistribute some of their earnings increases women's willingness to run for election, consistent with prior findings in competitive, tournament environments. However, the same redistributive option also increases men's willingness to run. Although the new condition increases overall participation, the gender gap in political ambition remains virtually unchanged: 1.20 points in the Standard treatment vs. 1.25 points in the RTL treatment, which translates in men reporting 21% higher willingness to run than women or women reporting 17.6% lower willingness than men.

We also uncover a puzzling pattern: women who are more likely to redistribute to the lowest earner in their group — measured by the percentage they report they would be willing to share if elected — are less likely to run for office. Instead, the increase in women's

[1]Experimental evidence also finds gender differences in willingness to assume leadership responsibility. For example, Ertac and Gurdal (2012) show that men are substantially more likely than women to volunteer to make a risky decision on behalf of a group.

participation appears to be driven by the less generous individuals. This counterintuitive result raises questions about the motivations for entry into elections and other competitive environments, as well as the resulting gender disparities.

Our findings contribute to a growing body of literature using experimental methods to examine strategies to reduce gender gaps in 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 provides a conceptual framework, drawing on the inequality-aversion model of Fehr and Schmidt (1999), to elucidate two possible leadership motives: improving aggregate group earnings and reducing the payoff gap for the lowest earner. 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.

2 Conceptual Framework

This study bridges two lines of research aimed at understanding and reducing women's under-representation in leadership roles from corporate boardrooms to elected office. One line investigates women's willingness to run for office and has tested a wide range of interventions designed to encourage candidacy. These include exposure to female political role models (Beaman et al., 2009; Campbell and Wolbrecht, 2006), targeted recruitment efforts (Crowder-Meyer, 2013; Sanbonmatsu, 2010), and experimental modifications to political framing and messaging (Kanthak and Woon, 2015; Fox and Lawless, 2005; Pate and Fox, 2018; Fox and Pate, 2023). Despite these efforts, the gender gap in political ambition remains substantial.

A second body of work examines gender differences in willingness to compete in winner-take-all competitions via tournament-entry experiments (Niederle and

Vesterlund, 2007) and identifies mechanisms that increase women's entry rates into the tournament, including modifications to incentive structures, altering the gender composition of the group, the nature of the real-effort task and incorporating prosocial components (Booth and Nolen, 2012; Balafoutas and Sutter, 2012; Buser, 2012; Wozniak et al., 2014; Cassar et al., 2016; Healy and Pate, 2011; Cassar and Rigdon, 2021a,b; Halladay and Landsman, 2022; Cassar and Zhang, 2022; Baier et al., 2024). The study most closely related to our design is Cassar and Rigdon (2021b), which finds that allowing competitors to share earnings with a low-performing peer reduces the gender gap by significantly increasing women's tournament entry without affecting men's participation. These findings suggest that altering the incentive structure of competitive environments can affect self-selection, raising the possibility that embedding redistribution-based incentives in electoral competition may similarly influence political entry decisions.

In this study, we bridge these literatures by embedding a close approximation of the prosocial mechanism introduced by Cassar and Rigdon (2021a) within an established experimental election framework (see, e.g., Pate and Fox, 2018), and examine whether the same opportunity that increased women's entry into competitive tournaments can also increase their willingness to run for office.

Our experiment models leadership entry as a costly choice: after completing a real-effort task, participants decide whether to run for a group leadership position in an election. In the baseline condition (Standard treatment), leaders are paid solely on the basis of their performance, whereas group members' earnings depend on both their own performance and the group representative selected during the election. In the second condition (RTL treatment), elected leaders are additionally given the option to transfer part of their earnings to the lowest-earning group member.

We conceptualize entry into electoral competition as a utility-based decision in which individuals compare the expected payoff (material and non-material) of seeking leader-

ship to that of remaining a group member. In our setting, willingness to run for office provides a behavioral measure of political ambition, defined as the choice to incur costs and enter a competitive selection process to obtain a leadership position (Kanthak and Woon, 2015; Pate and Fox, 2018). An individual's utility from entering can be expressed as the sum of material benefits associated with leadership (x_i), non-material returns such as status or competitiveness intended as a trait (S_i), prosocial or other-regarding motivations (P_i), minus the costs of entry, including campaign expenditures, effort, and risk exposure (C_i). Individuals enter when the expected utility of running exceeds that of not running.

Under this framework, leadership entry is shaped not only by monetary incentives but also by social preferences and institutional structure. A central distinction of our study is between two forms of prosocial motivation embedded in leadership roles: efficiency-based and redistribution-based prosociality. In addition to earning material benefits and gaining status, leadership in democratic institutions entails an implicit prosocial dimension: individuals who believe they can improve group performance may be further motivated to run for office out of concern for aggregate welfare. In this sense, willingness to run may reflect efficiency-oriented prosociality, consistent with models of social welfare maximization (Charness and Rabin 2002). In our experimental environment, this efficiency-based prosocial motive can be formalized as $\gamma_i \sum_j x_j$, where γ_i denotes the weight individual i assigns to aggregate welfare. Because both treatments preserve this aggregate-performance component, and our within-subject design varies only the availability of redistribution, this dimension of prosocial motivation is held constant across conditions. Thus, the Standard condition does not entirely rule out prosocial motivations. It is purely self-interested only when γ_i=0; when $\gamma_i > 0$, individuals derive utility from improving aggregate group outcomes.

Concern for the total amount earned by the group reflects only one specific form of prosociality, one that experimental evidence suggests is relatively frequent across cul-

tures but more robustly associated with male preferences than female preferences (Falk and Hermle, 2018; Cassar et al., 2026). Here, we examine a form of prosociality linked to inequality aversion. To formalize redistribution-based preferences, we draw on the model of inequality aversion in Fehr and Schmidt (1999). In their framework, individuals experience disutility from both disadvantageous inequality when they earn less than others (captured by their parameter α_i) and from advantageous inequality when they earn more than others (captured by their parameter β_i). In our framework, low-earning players cannot take from higher-earning players, so this form of aversion, even if present behind the scenes, is constant across the two treatments. In contrast, players could exhibit aversion to advantageous inequality in the RTL treatment. This type of preference can be formalized as: $\beta_i \frac{1}{n-1} \sum_{j \neq i} \max(x_i - x_j, 0)$, and simplified to $\beta_i(x_L - x_G)$, where x_L indicates earnings to the Leader and x_G earnings to the group member, to reflect the specifics of our game. Appendix 5 presents a more detailed sketch of the model.

In the Standard condition, a leader who earns more than other group members therefore incurs disutility proportional to $(x_L - x_G)$. In the RTL condition, the ability to transfer earnings reduces this payoff gap and, consequently, the inequality-induced disutility. Redistribution thus increases the utility of leadership for individuals with sufficiently high β_i. If women, on average, exhibit greater aversion to advantageous inequality, the RTL treatment should increase their willingness to run more strongly.

To summarize, within this framework, we may interpret the gender gap in political ambition as reflecting either higher average S_i (status or competition utility) and/or higher γ_i (efficiency-oriented prosociality). Adding a second form of prosociality introduces an additional channel of utility, which should increase the attractiveness of leadership for individuals with higher β_i. This framework predicts that, if women have higher inequality aversion (higher average β_i), redistribution opportunities increase women's entry more than men's. In the game we use, this type of redistribution leaves overall efficiency and status incentives intact. Thus, if men also derive utility from this added dimension of

leadership, the treatment may increase entry similarly across genders.[2] From this, we pre-registered the following hypotheses, which guide our analysis:

H1: The introduction of a redistribution-based incentive increases women's willingness to run in the election.

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

H3: The redistribution option reduces the gender gap in political ambition.

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

3 Experimental Design and Procedures

The subjects in our experiment were primarily undergraduates at Loyola Marymount University ("Experimental Economics Lab") and the University of Alabama ("The Interactive Decision Experiment Lab"). The students were recruited to the laboratory using the laboratory's respective email recruitment systems.[3] All experimental sessions were conducted using z-Tree (Fischbacher, 2007). In total, 320 subjects participated in the experiment, equally divided between the two universities, with each subject taking part in only one session (173 women and 147 men). The sessions were conducted in April 2024 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 are presented in Tables A1 and

[2]Experimental evidence shows that individuals sort into or out of environments based on their social preferences (Lazear et al., 2012). This interpretation is consistent with Kanthak and Woon's (2015) evidence on election aversion: some individuals may be willing to serve as representatives but reluctant to enter when representation requires standing for election.

[3]For recruitment, LMU uses ORSEE and the University of Alabama uses Sona Systems, which are common subject recruitment and study management tools.

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 - **RTL Payment**

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

A2 in the Appendix). Subjects were paid in cash at the end of the experiment based on their decisions. Each session lasted about an hour, and the average earnings were $25.13, in addition to a participation fee of $5.00.

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. The subjects were then randomly placed into groups of five.[4] Subjects could observe the gender composition of the session as a whole, but not the gender composition of their group.[5] The complete experimental design is summarized in Table 1, and we describe each component in detail below.

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

[5]Recent research suggests that the gender composition of the team directly impacts willingness to lead, with women preferring to be leaders when the group is majority women (Born et al., 2022).

3.1 Measuring Risk Tolerance and Altruism: Parts 1 and 2

The experiment begins with two parts designed to measure an individual's risk tolerance and degree of altruism. In Part 1, subjects make choices in 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.[6] The computer randomly determines with equal chance if the subject is a proposer or a receiver. Feedback from both tasks is withheld until the end of the experiment to avoid influencing subsequent decisions.

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.[7] Subjects 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 may not 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

[6] This decision is a standard experimental proxy for altruistic preferences, reflecting willingness to sacrifice personal earnings to benefit others—an attribute relevant to representative leadership.

[7] 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.

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 may share a portion of their earnings with the lowest-earning member of the group.[8] Subjects are given four minutes to complete as many addition problems as possible in all three parts. Compensation is determined by the number of problems correctly solved, which is 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, the subjects are shown only their score. They are then asked to estimate the performance of the other four members in their group by assigning ranks. Specifically, they report how many problems they believe were solved by the top performer, second-highest performer, and so on. 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 Redistribute-to-Lowest Incentive (Part 3c)

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

First, subjects receive detailed instructions on the election procedure, including how

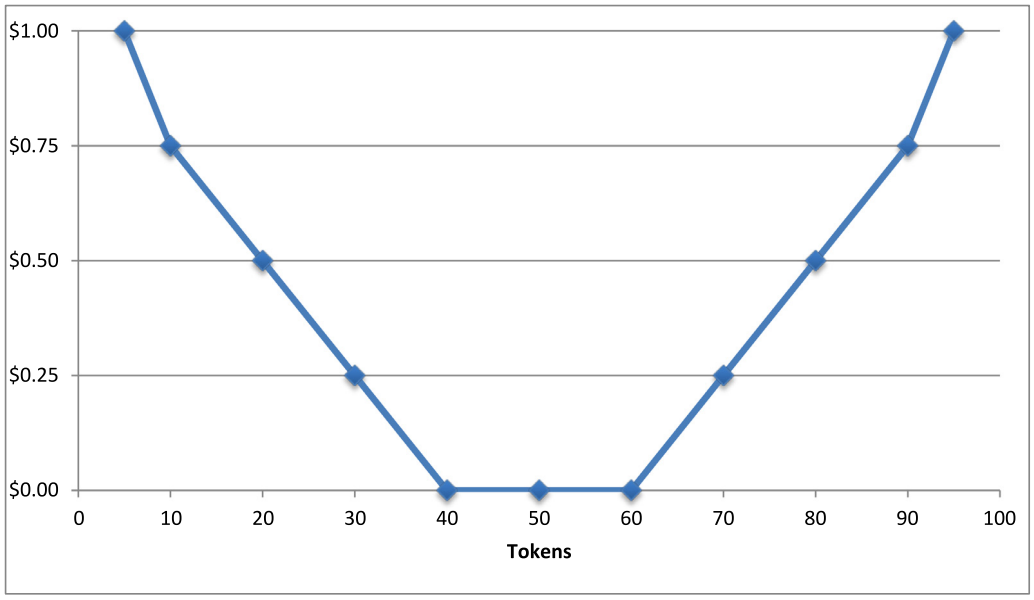
[8]The identity of the lowest earner is determined based on pre-transfer earnings, so the redistribution does not change which participant receives the transfer even if the payment alters the final rank ordering.

candidates will be selected. They are then asked to indicate their willingness to run for election as the group's representative using a bi-directional elicitation mechanism (see Figure 1 for the ambition mechanism and what subjects saw to record their willingness to run). Each subject begins with 50 tokens, which represent entries in 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 enter fewer tokens into the lottery. Reducing to 40 tokens is free, whereas lowering the total to 30, 20, or 5 tokens costs \$0.25, \$0.50, or \$1.00, respectively. The subjects are told in the instructions that "The computer will randomly draw tokens to select two candidates for the election. At the start, each person in the group 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 select your token as a candidate. Having fewer tokens in the drawing decreases the likelihood that the computer will draw your token to be a candidate."

This design for selecting the two candidates who run for election prevents subjects from perfectly controlling the outcome while enabling them to express nuanced preferences regarding the candidates. The fine-grained nature of the measure allows us to capture small changes in political ambition that would be lost in a binary entry decision.[9] If this part is selected for payment, any costs incurred for increasing or decreasing entry are deducted from the subject's final earnings.

Second, following each subject's decision about their willingness to enter the election, two candidates are selected from each group of five via a weighted lottery. The selection

[9]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, but we can approximate a binary decision by grouping those who increase their tokens (60 to 95 tokens) versus those who decrease their tokens (5 to 40 tokens).



	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

Figure 1: (a) Election Ambition Measure (parameters) (b) Election Ambition Measure (subject view)

is based on tokens that 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, each participant retains a nonzero probability of being selected for the election, regardless of how many tokens they submit. 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.[10] This message can be used to express their motivation for running, describe their qualifications, or make another appeal to voters. Third, all group members vote to elect one of the two candidates as the group's representative. Candidates

[10]Subjects are told in the instructions that candidates can write anything they choose, provided that it is under 150 characters and does not contain any obscene or offensive language.

may 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 and an additional $0.50 for each correct answer solved by the elected representative. The representative receives $0.75 per correct answer, plus a $2.00 bonus. After completing the task, subjects are shown only their score for 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 another option. Before the election, all participants are asked to determine how they would redistribute earnings to the lowest earner if they were selected as the representative, allowing us to observe this decision for all subjects. We intentionally do not reverse the order of Parts 3b and 3c, as the design is structured to introduce the redistribution opportunity only after participants have experienced the baseline election environment.[11]

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 is shown 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

[11] Although two-thirds of earnings depend on the leader's performance, meaningful dispersion remains because one-third depends on individual performance. In our data, task scores ranged from 0 to 27 correct answers, creating sufficient dispersion such that redistribution could meaningfully affect the lowest earner's payoff.

chosen, you should act as if each part will be paid."

The election procedures used in Parts 3b and 3c yield a key variable of interest: an individual's willingness to run for office, which we interpret as a proxy for political ambition. The redistribution decision made in Part 3c constitutes an additional outcome of interest, capturing how much individuals are willing to share with the lowest-earning individual when that individual is placed in a leadership role. This provides a behavioral measure of intended redistribution to the lowest earner among those willing to run for election.

3.5 Key Variables of Interest

To identify the causal effect of the RTL treatment on candidate entry, we compare behavior across two experimental conditions that differ only in the opportunity for redistribution, enabling a within-subject comparison of entry across incentive 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 (Fox and Pate, 2023). 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 the introduction of the redistribution option rather than to repetition, learning, or feedback effects. Moreover, because our objective is to test the impact of introducing redistribution authority after participants experience the baseline election environment, the fixed order is a deliberate design feature rather than a confound.

Several additional variables are central to our analysis. One is self-perceived rank (Rank), explicitly influenced by the study's design. Participants estimated the performance of their group's four anonymous members and ranked them by perceived score. Because they know their score, this is a structured exercise to elicit self-assessed relative standing, a proxy for confidence. By comparing beliefs about others to known perfor-

mance, we examine how perceived competitiveness affects willingness to run and whether it interacts with the incentive structure.

We also elicit two measures of other-regarding behavior. The first is a baseline measure of altruism from the five-dollar dictator game in Part 2 (DG Give). The second measure is collected in the treatment condition, where participants indicate, on a 0%-50% scale, how much of their earnings they would share with the lowest-earning member of the group if elected (Leader Share). These two measures allow us to test whether more altruistic individuals are more likely to enter the election and whether the opportunity to reallocate a portion of earnings draws in candidates with stronger redistributive preferences.

4 Results

Our subject pool consists of $N = 320$ subjects, split equally between the University of Alabama (UA) and Loyola Marymount University (LMU). Women represent 54.1% of the sample, and participants are on average 20.6 years old, with no gender differences in age, racial composition or major (see Appendix Table A1). Consistent with national trends, women are more likely to identify as Democrats (41.6% vs. 27.2%, $p = 0.007$), marginally less conservative, while other ideological differences are small. Across laboratories (see Appendix Table A2), the participants differed substantially in political orientation with UA students more conservative (4.4 vs. 3.2, $p = 0.000$), more likely to identify as Republican (45.6% vs. 11.9%, $p = 0.000$), and less likely to identify as Democrats (18.8% vs. 51.2%, $p = 0.000$). LMU was more ethnically diverse, with more social science majors and fewer business and economics students. Despite these differences, political ambition does not vary significantly by gender or institution, and therefore, we include the institution as a control in all analyses. For further information about the characteristics of the subject pool, see Appendix 5.1.

4.1 The Redistribute-to-Lowest Option Increases Entry; the Gender Gap Persists

Table 2 presents the summary statistics and the uncontrolled t-test between genders for the key variables used in the analysis.

Table 2: Experimental Outcomes by Gender

	(1) All	(2) Men	(3) Women	(4) t-test p-value	(5) K-S p-value
Willingness to run - Standard	6.175	6.823	5.624	0.000	0.000
	(2.255)	(2.151)	(2.200)		
Willingness to run - RTL	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)		
Performance - Standard	7.984	8.313	7.705	0.128	0.129
	(3.560)	(4.103)	(3.008)		
Performance - RTL	8.371	8.891	7.931	0.019	0.125
	(3.665)	(4.070)	(3.229)		
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.

In the first election, under the Standard treatment, 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 $= 0.000$). The distributions also differ significantly

(Kolmogorov-Smirnov p-value $= 0.000$). This gap – men reporting approximately 21% higher willingness to run than women – is consistent with longstanding findings on gender differences in political ambition and provides a clear baseline for evaluating the treatment effect.

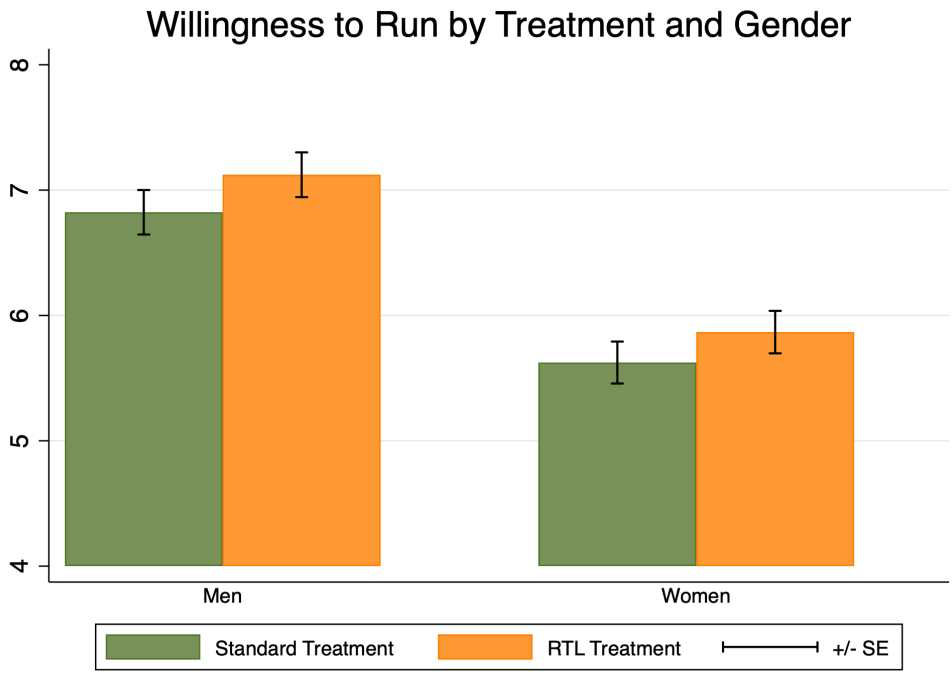
Following the first election, we introduced the RTL treatment, allowing elected leaders to redistribute up to 50% of their earnings to the lowest-earning member. This treatment tested Hypothesis 1, which predicted that a redistribution-based incentive would increase women's willingness to run. The results support this prediction. Among women, average willingness increases from 5.62 to 5.87, a statistically significant increase of approximately 4.5% (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 RTL treatment, rising from 6.82 to 7.12 – a significant increase of approximately 4.4% (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 RTL treatment, men's willingness to run (7.12) is approximately 21% higher than women's (5.87), with the gender difference again highly significant (t-test p-value $= 0.000$). Notably, this is almost identical to the 21% gap in the Standard treatment. These findings do not support Hypothesis 3, which predicted that the redistribution-based incentive would reduce the gender gap in political ambition. Figure 2 illustrates the main results: parallel increases in willingness to run without convergence across genders.

Table 2 also reports performance, confidence and risk preferences, possible confounders of willingness to run. On average, men score somewhat higher than women in the baseline (7.14 vs. 6.50, $p = 0.068$), and in both the Standard (8.31 vs. 7.71,

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 RTL treatment (orange). Error bars represent mean +/- SE. While the RTL treatment significantly increases the willingness to run of men (Standard: 6.82, RTL: 7.12, paired t-test p-value=0.022) and women (Standard: 5.62, RTL: 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 RTL treatment (diff: 1.25, t-test p=0.000).

$p = 0.128$) and RTL (8.89 vs. 7.93, $p = 0.019$) treatments, reaching statistical significance only in the latter. Performance increased significantly for both men and women moving from the baseline to the first election ($p = 0.000$ for both), and from the first to the second election, although marginally for women ($p = 0.000$ for men, $p = 0.106$ for women). Self-assessed rank also differs, with men perceiving themselves as performing better relative to others in their group ($p = 0.000$). In contrast, average risk preferences were similar.

To assess whether the main results persist after accounting for differences in individual characteristics, we estimate regression models. Table 3 reports fixed effect and linear regressions to assess the robustness of the treatment effects and explore how willingness

to run is shaped by gender, performance, self-assessed rank, and risk tolerance.[12] The fixed effects specification in column (1) confirms that the RTL treatment significantly increases overall willingness to run by approximately 4.4%. 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 (17.6% with respect to men, col. (3)) and the RTL treatment (17.6% with respect to men, 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, albeit smaller.

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.[13] Risk tolerance has mixed effects: it is not significant in the Standard treatment. However, it is marginally significant in the RTL treatment ($p = 0.08$), suggesting that the willingness to run for election may be modestly more responsive to redistribution opportunities among individuals who are more comfortable with risk.

In terms of risk aversion levels, there is no statistical difference between men and women on average, and also no difference in choice among the safer five gambles (gambles

[12] In the Appendix, we present a series of regression models that incorporate additional demographic and attitudinal covariates, including age, race, ideological orientation, self-identified party affiliation, and political ambition. The inclusion of these variables does not substantively alter the main findings. Importantly, the effects of gender and the redistribution treatment on willingness to run for election remain stable across specifications.

[13] The Appendix presents Figures A1–A6, which depict the distribution of subject responses by gender for risk tolerance, willingness to run in each treatment, self-assessed rank, dictator game giving, and leader sharing.

Table 3: Willingness to Run

	(1) Panel	(2) Panel	(3) Standard	(4) Standard	(5) RTL	(6) RTL
RTL treatment	0.269***	0.299**				
	(0.085)	(0.125)				
RTL*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 RTL treatment. (1)-(2) Fixed effects. (3)-(6) OLS, standard errors in parentheses clustered at the session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1-5). However, a gender difference emerges for the riskier four gambles (gambles 6-9).[14] In many studies, women are found to be more risk-averse than men; see the systematic evidence reported in Eckel and Grossman (2008a); Croson and Gneezy (2009); Charness and Gneezy (2012).[15]

[14]Summing the number of safer choices in gambles 6 through 9, the average risk score for women (1.925) is significantly higher than the average risk score for men (1.510) – indicating significantly greater risk aversion over the four most risky gambles (t-test p-value = 0.0099). This result is shown in Figure A1 in the Appendix.

[15]There are important methodological considerations beyond the scope of this paper surrounding measurement and how contextual differences matter to this gender difference in risk attitude, e.g. see Dave et al. (2010); Charness et al. (2013); Filippin and Crosetto (2016).

4.2 Willingness to Run is Inversely Related to Giving and Intended Redistribution for Women

We now turn to the relationship between redistributive preferences and willingness to run for election, examining two dimensions of other-regarding preferences: generosity in the dictator game (DG Give) and redistribution decisions leaders would make if elected group representative (L. Share).

Table 4 shows that women report higher intended redistribution if placed in the leadership role (marginally significant in columns 1–4), while individuals with greater political ambition report lower intentions to redistribute earnings when serving as leader (cols. 2 and 4). In contrast, dictator-game giving shows no gender differences and is unrelated to political ambition (cols. 5-6). However, we still find a strong correlation between dictator-game giving and leader sharing, indicating that individuals who are more generous in the baseline altruism task are also more likely to indicate they would share earnings if elected leader.

We next examine whether willingness to run for election is associated with higher giving or sharing. Contrary to expectations, the data suggest the opposite pattern, but only for women. Figure 3 illustrates these relationships, with the dictator giving shown in the left panel and the leader sharing (level of redistribution %) in the right panel. In the left panel, the fitted lines indicate a negative relationship between giving and willingness to run among women, whereas no meaningful pattern is evident among men. In the right panel, a similar negative relationship between willingness to run and intended redistribution emerges, again only for women.

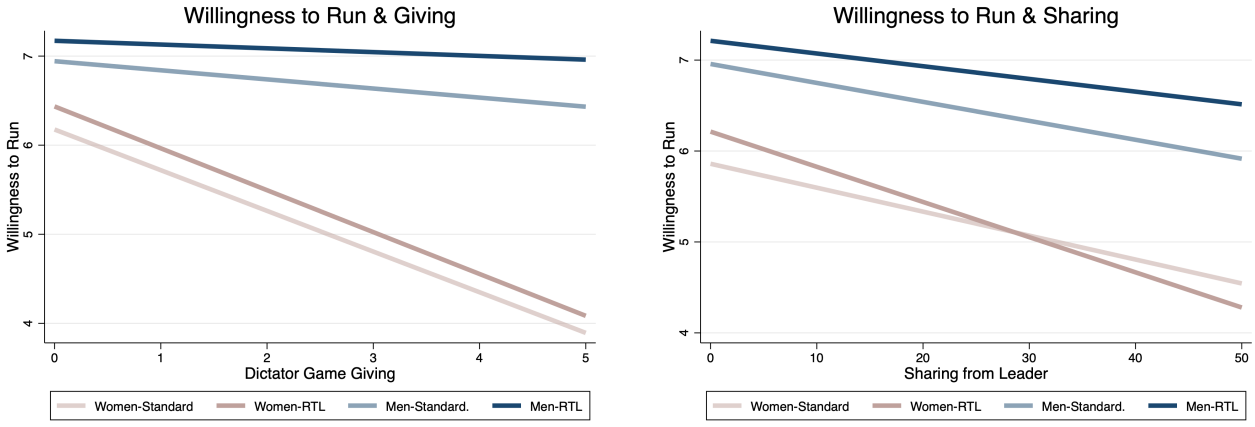
These visual findings suggest that more altruistic women are less likely to be willing to run for election — a pattern that replicates the findings observed in Cassar and Rigdon (2021b) where those most inclined to act prosocially were less likely to enter the tournament competition. This raises an important distinction: the intervention in our ex-

Table 4: Leader Sharing & Dictator Giving

	(1) L. Share	(2) L. Share	(3) L. Share	(4) L. Share	(5) Give	(6) Give
Woman	2.456*	2.402*	2.333*	2.236*	0.035	0.047
	(1.376)	(1.440)	(1.286)	(1.346)	(0.139)	(0.147)
DG give			3.541***	3.541***		
			(0.518)	(0.521)		
Political ambition		-1.439**		-1.479**		0.011
		(0.690)		(0.644)		(0.070)
Constant	7.134***	14.145**	3.174**	6.221	1.118***	2.238***
	(1.235)	(6.864)	(1.292)	(6.516)	(0.125)	(0.700)
Obs	320	320	320	320	320	320
R2	0.013	0.044	0.140	0.169	0.002	0.020
Mean of DV	7.812	7.812	7.812	7.812	1.194	1.194
SD of DV	12.301	12.301	12.301	12.301	1.239	1.239
Control I	Y	Y	Y	Y	Y	Y
Control II	N	Y	N	Y	N	Y
Model	OLS	OLS	OLS	OLS	OLS	OLS

Notes: OLS models. Dep. var. is Leader Share in col. (1)-(4) and DG Give in col. (5)-(6).
Control I: LMU (always insignificant).
Control II: Performance, Rank, Risk A., Age, Caucasian, Ideology, Democrat (always insign.).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3: Willingness to Run and Redistribution Preferences



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 the percentage of earnings shared if elected as the leader.

periment invites individuals to share with the lowest earner if they are elected, but those most inclined to share are also the least likely to run. Yet, across all levels of giving and sharing, the redistribution option is effective in increasing individual willingness to run for election.[16]

Table 5 analyzes the interaction between willingness to run and the RTL condition by pooling observations within subjects and using OLS with errors clustered at the individual level. Several key findings emerge. First, the observed increase in willingness to run under the RTL treatment remains significant and statistically similar after the inclusion of giving (cols. 1-3) and sharing (cols. 4-6).

Second, women, overall, exhibit a lower willingness to run than men with coefficients that remain consistently negative and significant even after accounting for individual measures of giving and sharing, suggesting that gender differences in political ambition are robust and persistent.[17] Multiple interventions have attempted to reduce this gap. Even in the most recent study by Pate and Fox (2025), which utilizes a facial emotion recognition task in which women reported high confidence in their ability, their willingness to run for election did not increase.

Third, it appears that altruism, measured by dictator game giving, is negatively related to willingness to run (col. 1). This relationship is concentrated among women: the significantly negative interaction between woman and giving in columns 2 and 3 suggests that more generous women are less inclined to run. The positive interactions between the RTL treatment and giving in columns 2 and 3 indicate that the treatment may be somewhat more effective among individuals who are initially more altruistic, although these

[16] We also examine realized redistribution in the RTL treatment. Although most participants chose a zero sharing rate, 39.7% selected a positive sharing rate. Among those who chose to share, the modal sharing rate was 10%, followed by 20% and 50%. Because transfers were implemented only when the participant was elected representative, aggregate realized redistribution was mechanically limited: $26.20 was redistributed across the 320 participants. Conditional on a transfer being made, however, the average transfer was $1.74. This amount is substantively meaningful relative to the average round earnings of $7.13, representing nearly one-quarter of that amount.

[17] A minor exception is col. 3, where gender loses significance after controlling for performance, confidence, and risk aversion, in addition to the interactions between giving and gender and giving and the treatment.

Table 5: Willingness to Run & RTL Preferences

	(1) All	(2) All	(3) All	(4) All	(5) All	(6) All
RTL 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)			
RTL*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)
RTL*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 RTL treatment.

OLS on all data, errors clustered at individual level.

FE models could not be used since giving and sharing do not differ within subjects.

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

effects are largely insignificant. This likely reflects limited statistical power, as there are relatively few observations at the higher end of the altruism distribution. As a result, we cannot rule out the possibility that a larger sample could detect both a stronger treatment effect and meaningful heterogeneity in responsiveness to the RTL incentive across levels of altruism.

In line with previous research (Pate and Fox, 2018; Fox and Pate, 2023), stronger task performance is positively associated with willingness to run, suggesting that individuals who perform well may feel more confident or motivated to pursue leadership roles. Similarly, individuals who report lower self-assessed rankings exhibit reduced willingness to run, indicating that participants internalize performance signals when evaluating their suitability for leadership. Risk aversion shows only small, statistically insignificant negative effects. This creates a puzzle: the treatment invites candidates to serve others, yet those most motivated to serve are the least willing to run.

Although the redistributive opportunity increases individuals' willingness to run for election, it does not reduce the gender gap. Moreover, those most inclined to favor redistribution – particularly women – are also the least likely to seek an elected role. One possible interpretation is that the opportunity to reduce inequality in this manner does not fully offset the costs associated with entering a competitive election. Kanthak and Woon (2015) show that women may be less willing to become candidates when representation is determined by an election, even when they are willing to serve as representatives under other selection mechanisms (e.g., being appointed to serve). In our setting, the ability to transfer resources to the lowest earner is available only after entering a competitive and positional process that requires standing for election, sending a message, and seeking votes. Thus, the institutional path to redistribution may matter: the ability to transfer resources may not be sufficient to induce entry among participants who are reluctant to seek office through election.

This paradox highlights the challenge of designing interventions to close gender gaps

in political ambition. It suggests that while the redistribution opportunity increases overall participation, it does not appear to shift the deeper motivations that influence who chooses to run.

4.3 Content Analysis of Candidates' Messages and Impact on Voting Behavior

Next, to understand the motivations behind the increased willingness to run under the RTL treatment, we drawon 232 campaign messages to examine whether it altered how candidates framed their motivation for leadership. Importantly, sending messages is not universal and may be endogenous for two reasons. First, only individuals who stood as candidates were eligible to send a message; the sample, then, is generally restricted to those expressing a relatively greater willingness to run. Second, even among candidates, sending a message was voluntary (after selection, each of the two candidates was given the opportunity to send a brief message to the group). As a result, the set of observed messages reflects both selection into candidacy and selection into communication.

Messages were coded using a large language model (OpenAI API) along eight non-mutually exclusive dimensions. Running the LLM on the same sample yielded 95% agreement rate. We retain for analysis three categories with a prevalence above 10% and derive a fourth one to isolate efficiency and/or redistribution signals. Specifically, we retained the following categories: *Competence*, which captures a candidate's reference to ability or qualifications; *Prosociality*, which reflects generalized other-regarding motivations (e.g., fairness or concern for group welfare); and *Redistribute*, which denotes explicit references to sharing or transferring earnings. Finally, *Efficiency* is defined as the contemporaneous presence of *Prosociality* and *Competence*, identifying a prosocial framing centered on improving aggregate outcomes.[18]

[18]Excluded categories are *Dominance*, which captures a candidate's assertion of superiority or competitive advantage; *Duty*, which captures framing of leadership as responsibility or obligation; *Minimal*, which captures messages lacking substantive justification; *Quantified*, which captures numerical references; and

Figure 4 displays bar charts comparing the proportions of men and women in each treatment condition whose campaign messages contain specific motivational signals.[19] The statistical significance of these differences is formally tested in Table 6, which examines gender differences within each condition and assesses how the RTL treatment affects the social content of campaign messages.

In pooled models, women are more likely to emphasize competence and less likely to rely on prosocial content with no significant baseline gender differences in efficiency or redistribution language. The RTL treatment does not affect competence framing but significantly increases signaling of prosociality, efficiency, and redistribution. When disaggregated by gender, redistribution content rises for both men and women, while increases in prosocial and efficiency framing are somewhat stronger for women. These shifts provide evidence that institutional manipulation altered candidates' stated motivations for leadership by activating redistribution-based considerations in their messaging.

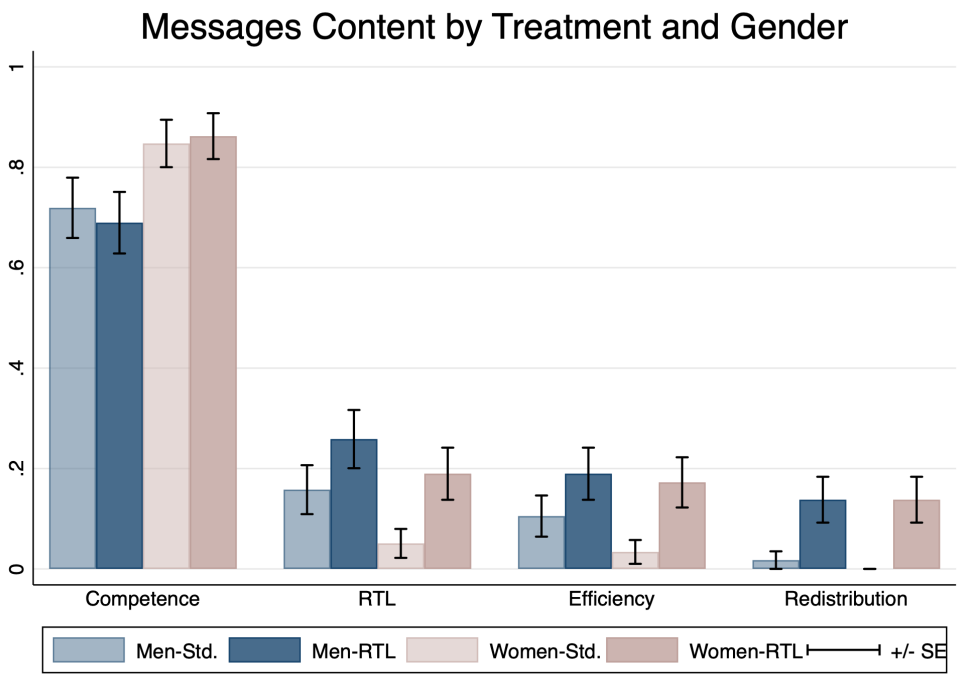
When we analyze voting behavior, we find that candidate messages mattered for electoral support. Voters responded especially strongly to concrete information about prior task performance: messages that included explicit performance or score information received significantly more votes than messages without such information (2.84 vs. 1.95 votes; Welch t-test, $p < 0.001$). Redistribution, help, and fairness language also received above-average support, although these appeals were relatively uncommon. Importantly, explicit performance messages did not receive significantly more votes than redistribution-oriented messages when messages were classified by primary content (2.84 vs. 2.40 votes; Welch t-test, $p = 0.346$). Thus, the results suggest that voters rewarded concrete performance signals over messages lacking such information, while redistribution-oriented appeals were also favorably received but were used less often by the candidates running for election.

Affect, which captures emotional or relational language.

[19] Important caveats: content categories are not mutually exclusive, and less than half of the candidates got to send a message in both treatments.

Finally, when we examine whether these rhetorical shifts explain willingness to run (Appendix Table A6), message content is not strongly associated with entry. The gender gap in willingness to run remains large and significant, and framing variables are generally weak or insignificant predictors once treatment and gender are included. Thus, while the RTL treatment changes how candidates describe their motivations, those rhetorical differences do not appear to mediate entry decisions directly. Institutional incentives alter both entry and messaging, but changes in framing are not the primary mechanism driving willingness to run.

Figure 4: Messages Content



Note: Bars display the average proportion of named content of men (blue) and women (tan) under the Standard Treatment (lighter bars) and RTL treatment (darker bars). Error bars represent mean +/- SE.

Table 6: Content Analysis of Candidates' Messages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Competence			Prosociality			Efficiency			Redistribute		
	All	Men	Wom.	All	Men	Wom.	All	Men	Wom.	All	Men	Wom.
RTL treatment	-0.007	-0.036	0.019	0.120***	0.102	0.134**	0.112***	0.084	0.136**	0.129***	0.122**	0.136***
	(0.046)	(0.073)	(0.059)	(0.044)	(0.065)	(0.057)	(0.040)	(0.059)	(0.054)	(0.033)	(0.049)	(0.045)
Woman	0.148**			-0.087*			-0.044			-0.008		
	(0.060)			(0.052)			(0.046)			(0.034)		
LMU	0.087	0.098	0.080	-0.055	-0.024	-0.084	-0.025	-0.001	-0.046	-0.036	-0.032	-0.039
	(0.061)	(0.098)	(0.076)	(0.052)	(0.085)	(0.061)	(0.046)	(0.073)	(0.056)	(0.034)	(0.050)	(0.046)
Constant	0.662***	0.671***	0.801***	0.177***	0.170**	0.099**	0.104**	0.106*	0.060*	0.032	0.033	0.023
	(0.068)	(0.085)	(0.075)	(0.054)	(0.071)	(0.045)	(0.048)	(0.063)	(0.033)	(0.028)	(0.035)	(0.027)
Obs	232	115	117	232	115	117	232	115	117	232	115	117
R2	0.044	0.012	0.013	0.046	0.016	0.062	0.035	0.014	0.058	0.067	0.054	0.081
Mean of DV	0.780	0.704	0.855	0.164	0.209	0.120	0.125	0.148	0.103	0.073	0.078	0.068
SD of DV	0.415	0.458	0.354	0.371	0.408	0.326	0.331	0.356	0.305	0.261	0.270	0.253
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: Dependent variable is message characteristic (0-1) in pooled data and separate by gender.

All models OLS, errors clustered at the individual level.

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

5 Discussion

This study provides new insights into the relationship between redistribution opportunities and political ambition, particularly regarding persistent gender differences in candidate emergence. Despite a growing body of research focused on encouraging more women to seek elected office, including recent interventions targeting performance beliefs and stereotype framing (see, for example, 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 intervention by Cassar and Rigdon (2021b), which demonstrated that when tournament winners could share a portion of their earnings with a low-performing peer, women were more likely to enter the competition. However, that design involved simple entry decisions in a winner-take-all tournament setting. In contrast, we apply a similar redistribution opportunity within a more socially embedded and politically relevant framework using a repeated-election design from Fox and Pate (2023), in which participants must decide whether to stand for election, send messages, and compete for a leadership role under public scrutiny. Our design studies one specific redistribution institution: an elected representative's voluntary option to transfer some percentage of her earnings, from 0% to 50%, to the lowest-earning group member. We therefore interpret the results as evidence about this redistribution mechanism rather than about prosociality in general.

Our findings support the hypothesis that women's willingness to run increases when the elected representative is given the opportunity to redistribute earnings to the lowest earner in the group (H1). However, men's willingness to run also increases by a similar amount (H2 not supported). As a result, the gender gap in willingness to run remains unchanged (H3 not confirmed). This result points to a central puzzle in the study: While the redistribution incentive is effective at increasing participation among both men and women, it does not alter the underlying gender 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 report that they would share more of their earnings if elected are significantly less likely to express political ambition to serve in the electoral process. Although the redistributive incentive encourages individuals to serve others, those most inclined to do so remain reluctant to seek elected office. This suggests a tension between prosocial motivation and the willingness to assume leadership roles that involve public competition. Our finding contributes to the recent literature on the relationship between distributional preferences and competitive choices (Bartling et al., 2009; Eckel and Fúllbrunn, 2015; Dasgupta et al., 2019).

Interestingly, we find no evidence that men perceive the environment differently in the redistribution setting. This contrasts with earlier findings by Pate and Fox (2018, 2025), in which framing the task as female-typed or as service-focused 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 winners of a tournament competition.

While previous research has found that more 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) with those of the current study, which embeds redistribution within a public election involving self-selection, campaigning, and more concentrated competition. 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 redistributive incentive closes the competitive gender gap, is of the latter variety. This

contrast demonstrates the importance of context and social framing on the effectiveness of redistribution interventions.

In our design discussed here, leaders could share only with the lowest earner, as in Cassar and Rigdon (2021a,b), where inequality was known and unidirectional. Our next step is to explore additional forms of redistribution in order to examine mechanisms that more closely mirror real-world political discretion. This approach will allow elected leaders to reallocate earnings among all group members, including themselves, after observing the actual performance and earnings of all group members. With additional choices available for redistribution, this variation may allow inequity concerns 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). By incorporating experimental treatments along these lines we aim to be better positioned to design interventions that align ethical motivations with public leadership, particularly for individuals hesitant to seek elected office.

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Appendix

Appendix: Leadership Entry and Inequality Aversion

In this appendix, we adapt the inequality-aversion framework of Fehr and Schmidt (1999) to derive how the introduction of a redistribution option could affect the utility of leadership.

Environment. Consider a group of n individuals. If individual i becomes leader, she earns x_L while each of the remaining $n-1$ group members earns x_G, where $x_L > x_G$.

Preferences follow Fehr and Schmidt (1999), to which we add a last term representing total group payoff:

$$U_i(x) = x_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} \max(x_j - x_i, 0) - \beta_i \frac{1}{n-1} \sum_{j \neq i} \max(x_i - x_j, 0) + \gamma_i \sum_j x_j, \quad (1)$$

where:

- $\alpha_i \geq 0$ captures disutility from disadvantageous inequality,
- $\beta_i \geq 0$ captures disutility from advantageous inequality,
- $\gamma_i \geq 0$ captures utility from increasing the group payoff.

Since the leader earns more than other group members, only advantageous inequality is relevant. Thus, leader utility simplifies to:

$$U_i^L = x_L - \beta_i(x_L - x_G) + \gamma_i \sum_j x_j. \quad (2)$$

We abstract from disadvantageous inequality since $x_L > x_{FG}$ by construction.

Redistribution-based Prosociality. In the RTL condition, the leader may transfer an amount $t \geq 0$ to the lowest-earning group member. After transfer:

$$x'_L = x_L - t, \tag{3}$$

$$x'_G = x_G + t. \tag{4}$$

The payoff gap becomes:

$$x'_L - x'_G = (x_L - x_G) - 2t. \tag{5}$$

Leader utility under redistribution is therefore:

$$U_i^L(t) = (x_L - t) - \beta_i \left[(x_L - x_G) - 2t\right] + \gamma_i \sum_j x_j. \tag{6}$$

Expanding:

$$U_i^L(t) = x_L - \beta_i(x_L - x_G) + t(2\beta_i - 1) + \gamma_i \sum_j x_j. \tag{7}$$

Comparative Statics. Since redistribution does not change the group total payoff, the marginal effect of redistribution on leader utility is:

$$\frac{\partial U_i^L}{\partial t} = 2\beta_i - 1. \tag{8}$$

Thus:

- If $\beta_i > \frac{1}{2}$, redistribution increases the utility of leadership.
- If $\beta_i < \frac{1}{2}$, redistribution decreases the utility of leadership.

Therefore, introducing a redistribution option increases the attractiveness of leadership for individuals with sufficiently high advantageous inequality aversion.

Implications for Entry. Let the net utility of running be:

$$V_i = U_i^L(t) + S_i - C_i, \tag{9}$$

where S_i captures status or competitive utility and C_i captures entry costs. Individuals enter when $V_i \geq 0$.

Since redistribution increases U_i^L when $\beta_i > 1/2$, the RTL condition increases entry for individuals with sufficiently high advantageous inequality aversion.

If women have higher average β_i, the redistribution treatment should increase women's entry more strongly. However, if status motives S_i are larger for men, the treatment may increase entry for both genders without eliminating the gap.

5.1 Appendix: Subject Characteristics

Women make up 54.1% of the sample, indicating a fairly balanced gender representation in our 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 conservative), 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 redistribution incentives, we replicated the experiment in two ideologically distinct institutions. 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.

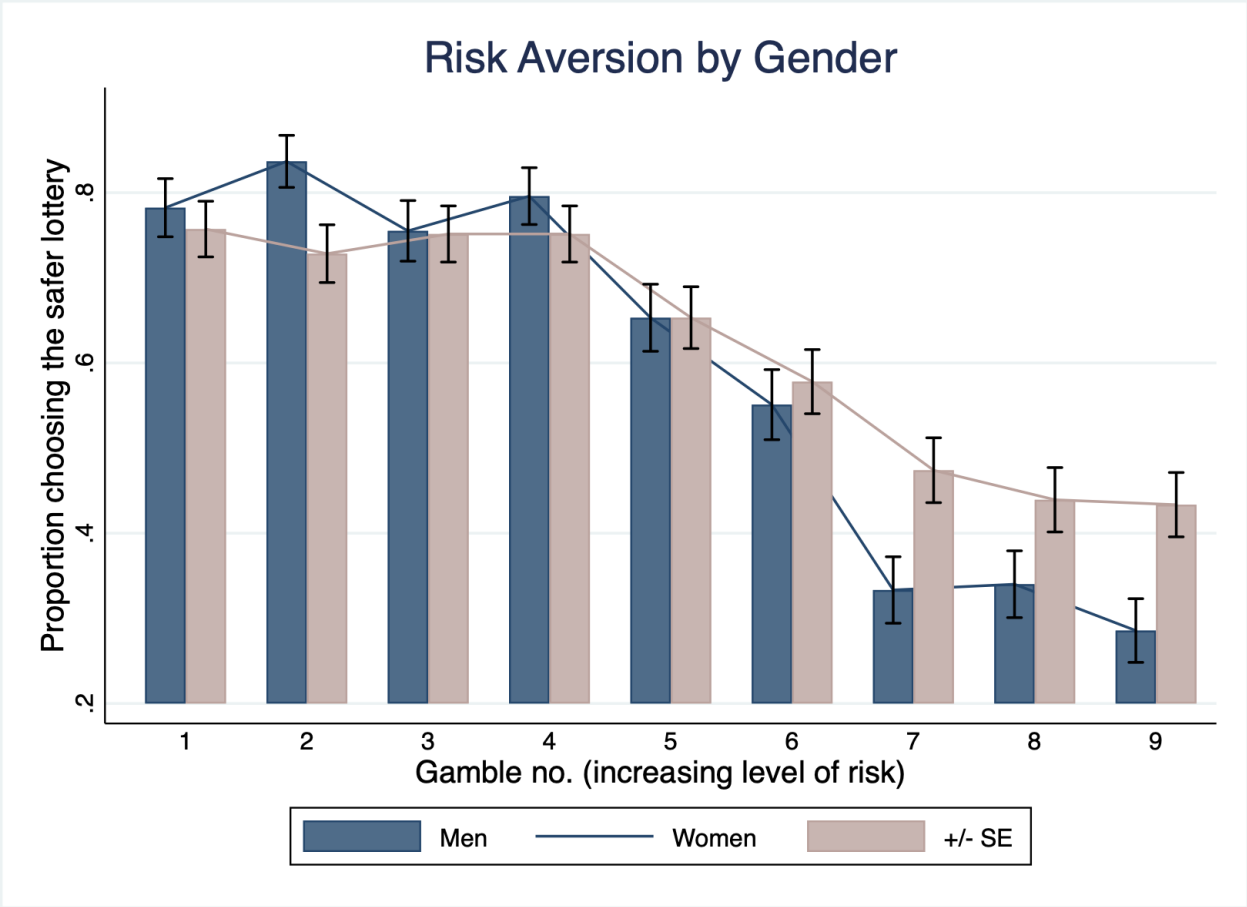
Although the two samples are similar in gender composition (p-value $= 0.576$) and age (UA students are slightly older with a mean age of 21 years compared to 20 years at LMU, p-value $= 0.064$), they differ significantly in racial makeup and importantly in political orientation. Caucasian students make 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 in-

clude institution as a control variable in all analyses.

Appendix Figures

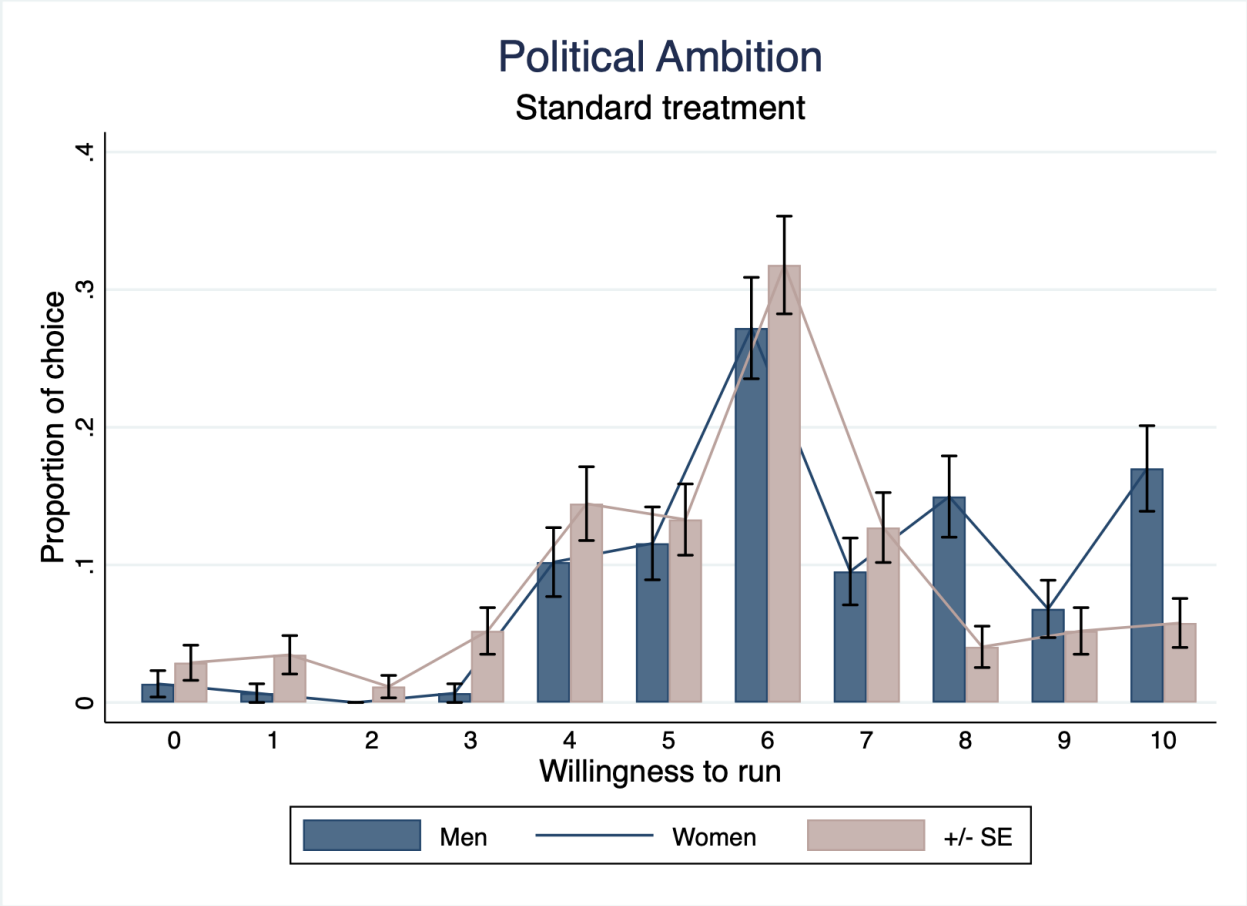
Figure A1: Risk Aversion by Gender



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 $= 0.0099$)

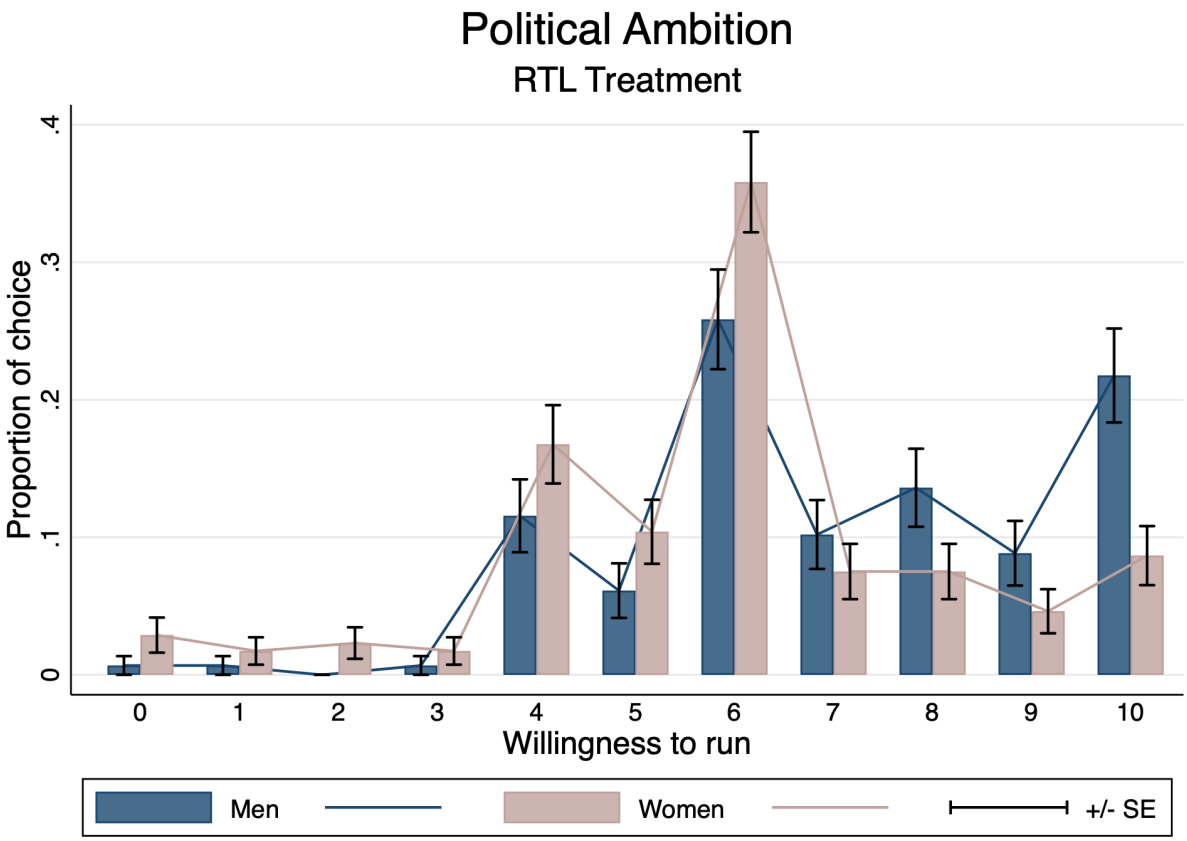
.

Figure A2: Willingness to Run - Standard Treatment



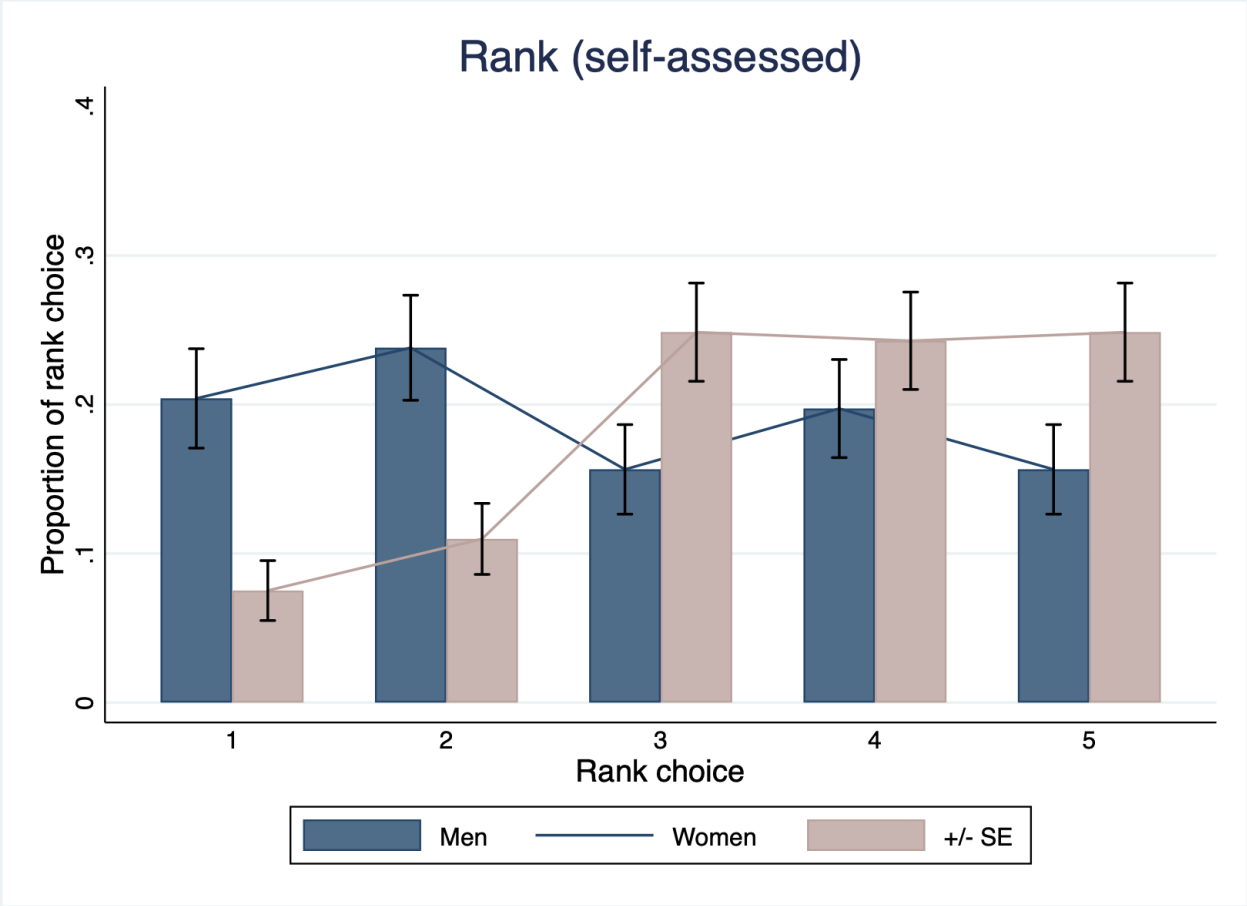
Notes. Bars display the proportion of individuals choosing the level of willingness to run separately for men (navy) and women (rose) with lines that connect the proportions across the eleven choices. Error bars represent mean +/- SE.

Figure A3: Willingness to Run - RTL treatment



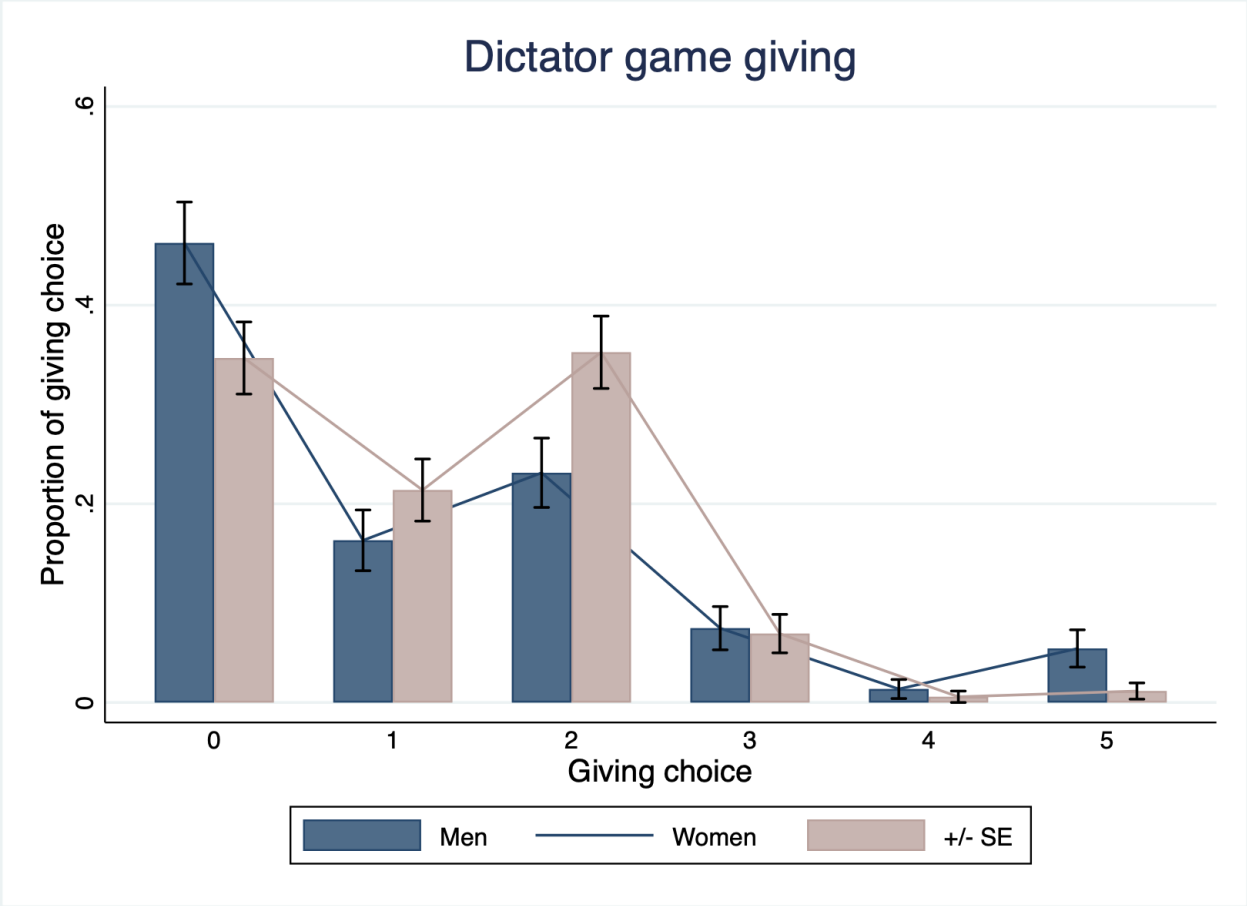
Notes. Bars display the proportion of individuals choosing the level of willingness to run separately for men (navy) and women (rose), with lines that connect the proportions across the eleven choices. Error bars represent mean +/- SE.

Figure A4: Rank (self-assessed)



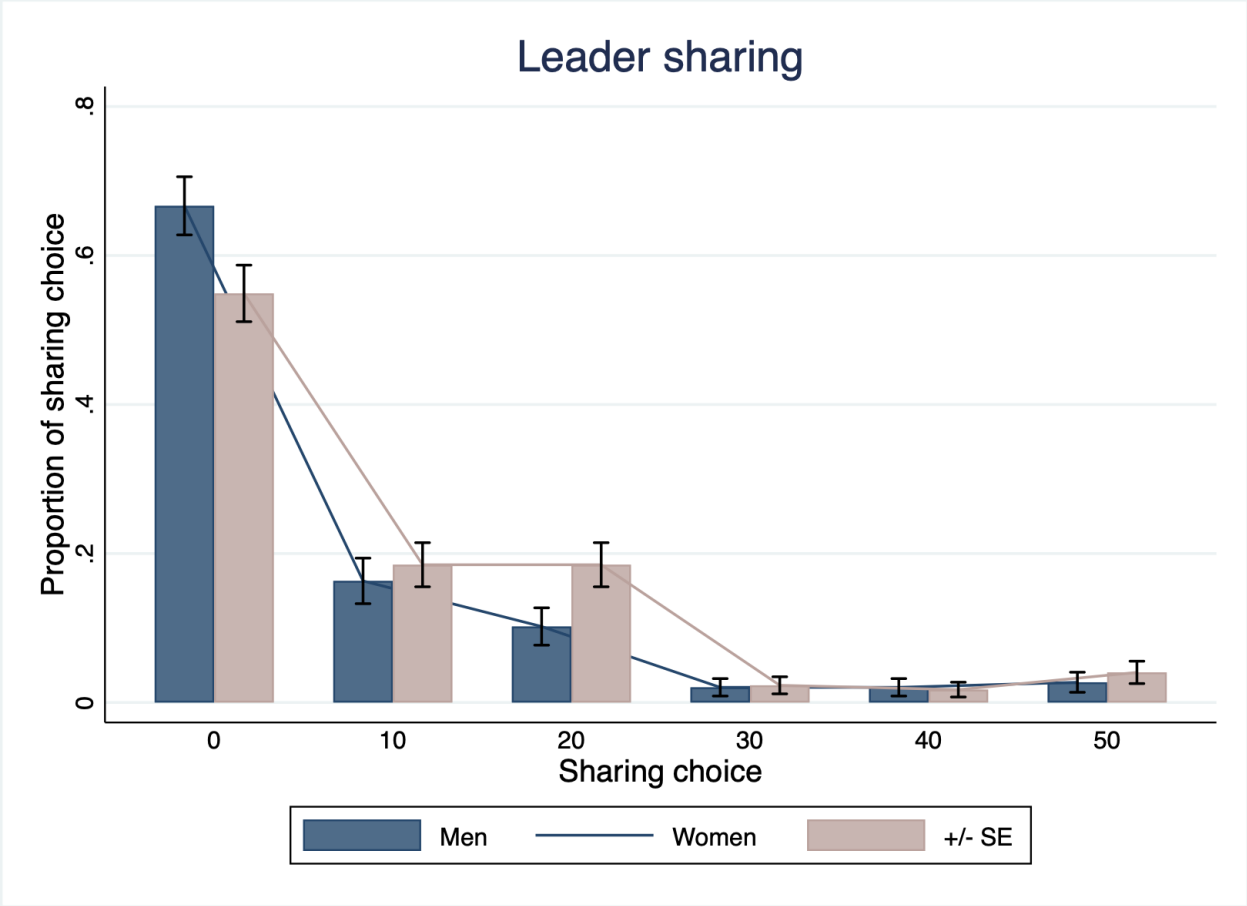
Notes. Bars display the proportion of individuals choosing the respective rank (self-assessed) separately for men (navy) and women (rose) with lines that connect the proportions across the five possible ranks. Error bars represent mean +/- SE.

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 and Econ major	0.459	0.503	0.422	0.146
	(0.499)	(0.502)	(0.495)	
Social Sciences major	0.156	0.129	0.179	0.221
	(0.364)	(0.337)	(0.385)	
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 and Balance Check

	(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 and Econ major	0.459	0.519	0.400	0.033
	(0.499)	(0.501)	(0.491)	
Social Sciences major	0.156	0.106	0.206	0.014
	(0.364)	(0.309)	(0.406)	
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 with Socio-Demographic Controls

	(1) All	(2) Standard	(3) Standard	(4) Prosocial	(5) Prosocial
Redistribute-to-Lowest	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

Note: Dependent variable: willingness to run (0-10) in Standard/RTL.

(1) All data OLS, errors clustered at individual level.

(2)-(5) OLS separate by treatment, errors clustered at the session level.

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

Table A4: Willingness to Run & Dictator Giving

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men		Women		Standard tr.		Prosocial tr.	
RTL treatment	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)
RTL*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 RTL 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 A5: Willingness to Run & Leader Sharing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8
	Men		Women		Standard tr.		Prosocial tr.	
Redistribute-to-Lowest	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***	-0.0
	(0.012)	(0.012)	(0.014)	(0.013)	(0.010)	(0.013)	(0.007)	(0.0
RTL*Share		0.007		-0.012				
		(0.013)		(0.009)				
Woman					-1.144***	-1.095***	-1.188***	-0.99
					(0.184)	(0.309)	(0.212)	(0.3
Woman*Share						-0.006		-0.0
						(0.021)		(0.0
LMU	-0.098	-0.098	-0.249	-0.249	-0.182	-0.187	-0.152	-0.1
	(0.332)	(0.333)	(0.306)	(0.306)	(0.275)	(0.267)	(0.221)	(0.2
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)	(0.2
Observations	294	294	346	346	320	320	320	32
R^2	0.014	0.014	0.041	0.042	0.089	0.089	0.100	0.1
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OL

Note: Dependent variable: willingness to run (0-10) in Standard and RTL 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 A6: Willingness to Run and Message Contents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Woman	-1.456***	-0.399	-1.380***	-1.253***	-1.393***	-1.287***	-1.385***	-1.267***
	(0.286)	(0.664)	(0.298)	(0.371)	(0.297)	(0.363)	(0.296)	(0.349)
RTL treatment	0.103	0.238	0.088	0.192	0.110	0.217	0.032	0.107
	(0.198)	(0.287)	(0.199)	(0.282)	(0.198)	(0.281)	(0.201)	(0.286)
Competence	0.455	0.902						
	(0.391)	(0.553)						
Wom*Comp		-1.173*						
		(0.690)						
Prosocial			0.097	0.172				
			(0.347)	(0.504)				
Fem*Pro				-0.187				
				(0.664)				
Efficiency					-0.092	-0.092		
					(0.366)	(0.536)		
Wom*Eff.						0.014		
						(0.719)		
Redistribute							0.528	0.844
							(0.414)	(0.565)
Wom*Red.								-0.670
								(0.829)
LMU	0.065	0.066	0.110	0.100	0.103	0.097	0.124	0.115
	(0.293)	(0.290)	(0.286)	(0.287)	(0.287)	(0.287)	(0.285)	(0.286)
Constant	7.228***	6.845***	7.512***	7.450***	7.539***	7.489***	7.513***	7.455***
	(0.393)	(0.516)	(0.270)	(0.300)	(0.264)	(0.292)	(0.261)	(0.280)
Obs	232	232	232	232	232	232	232	232
R2	0.129	0.143	0.121	0.122	0.121	0.121	0.125	0.127
Mean of DV	6.935	6.935	6.935	6.935	6.935	6.935	6.935	6.935
SD of DV	2.013	2.013	2.013	2.013	2.013	2.013	2.013	2.013
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Note: Dependent variable is willingness to run.

All models OLS, errors clustered at the individual 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.

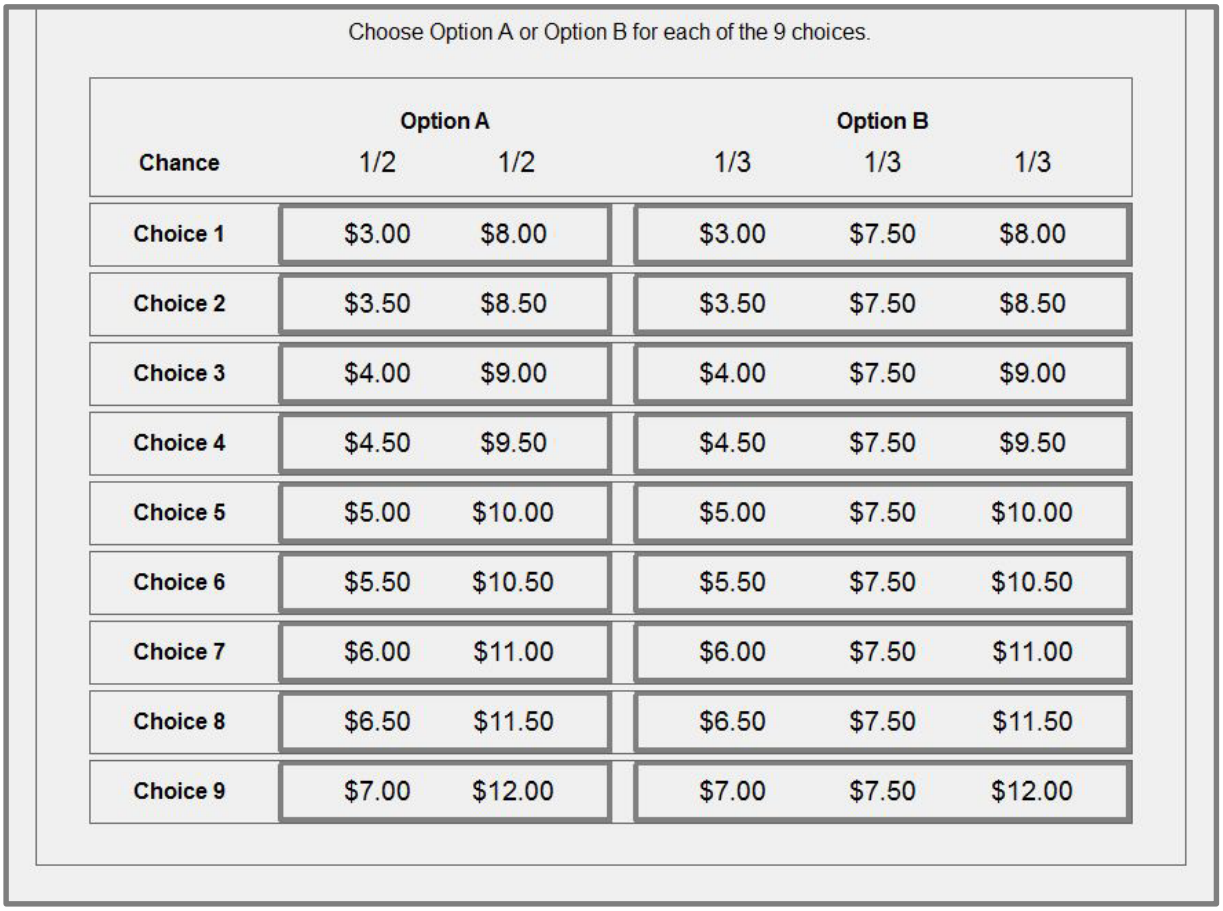
We will now begin the first part of the experiment.

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

Figure A7



Choose Option A or Option B for each of the 9 choices.

	Option A		Option B		
Chance	1/2	1/2	1/3	1/3	1/3
Choice 1	$3.00	$8.00	$3.00	$7.50	$8.00
Choice 2	$3.50	$8.50	$3.50	$7.50	$8.50
Choice 3	$4.00	$9.00	$4.00	$7.50	$9.00
Choice 4	$4.50	$9.50	$4.50	$7.50	$9.50
Choice 5	$5.00	$10.00	$5.00	$7.50	$10.00
Choice 6	$5.50	$10.50	$5.50	$7.50	$10.50
Choice 7	$6.00	$11.00	$6.00	$7.50	$11.00
Choice 8	$6.50	$11.50	$6.50	$7.50	$11.50
Choice 9	$7.00	$12.00	$7.00	$7.50	$12.00

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.

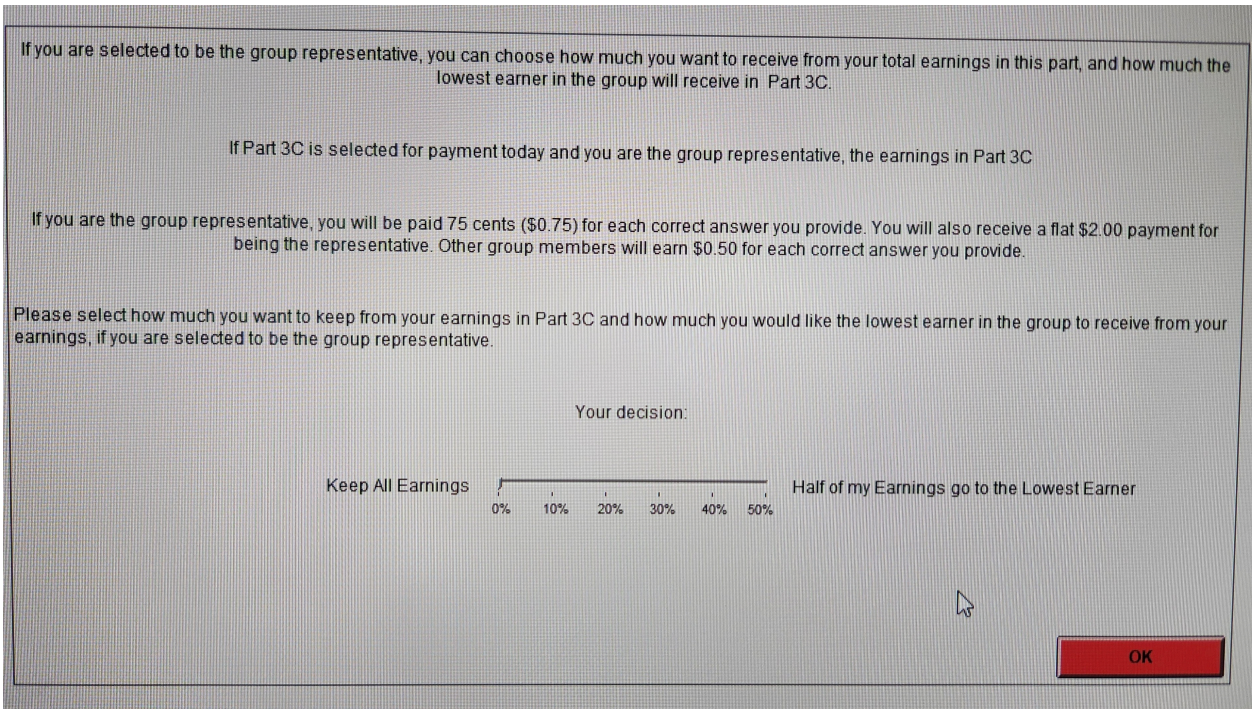
If you are the group representative and Part 3C is randomly selected for payment, you will be paid 75 cents ($0.75) for each correct answer you provide. You will also receive a flat $2.00 payment for being the representative. Other group members will earn $0.50 for each correct answer you provide. You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