Women Don't Run?
Election Aversion and Candidate Entry

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Abstract

In an effort to control for confounding factors that might affect the relative propensities for women and men to enter politics, we take the question of candidate emergence into the laboratory. We find evidence that women are *election averse* while men are not. This difference does not arise from disparities in abilities, risk aversion, or beliefs, but rather from the specific competitive and strategic context of campaigns and elections. The key features of our experimental design involve (1) an objective task that represents policymaking ability, (2) monetary rewards that ensure that all subjects, regardless of gender, face the same incentives to select a representative with the highest task ability, and (3) a comparison of alternative selection mechanisms. In Experiment 1, we find that men and women are equally likely to volunteer when the representative is selected randomly, but that women are less likely to be candidates when the representative is chosen through an election. In Experiment 2, we find that women's election aversion persists with variations in the electoral environment; it disappears only when campaigns are both more truthful and less costly.

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Many democratically elected legislatures fail to resemble the people they purport to represent. Only two African Americans currently serve in the US Senate (neither of whom was elected), despite the fact that African Americans comprise more than 13 percent of the US population. More than 50 percent of the US population are women, yet women comprise only 18 percent of the U.S. House of Representatives and 20 percent of the U.S. Senate. These numbers are troubling to those who are concerned with descriptive representation because they make clear that the demographic characteristics of the U.S. Congress do not reflect the characteristics of the constituencies that elect its members. Yet Pitkin (1967) argues that descriptive representation is far less normatively important than substantive. Hence, empirical scholars of representation—following the lead of Miller and Stokes (1963)—tend to eschew the notion of descriptive representation and focus on questions of policy congruence and constituency influence.

While the literature has made important advances since Miller and Stokes (e.g., Ansolabehere and Jones 2010, Bartels 1991, Bafumi and Herron 2010), a narrow focus on the relationship between constituency opinion and congressional voting behavior misses much of what may be important about the process of translating *constituents' interests* into *public policy*. For example, if legislatures included more diverse voices and perspectives, the issues upon which legislators voted would themselves be different (Bratton and Haynie 1999, Swers 2005). Similarly, the translation of interests to policy may not be straightforward. To the extent that it is not, potential representatives may differ in their abilities to make those translations.

More important, a narrow conception of representation that prioritizes what is readily measurable may miss other facets of representation that are central to our rich conception of liberal democracy (Achen 1978). Indeed, representation is much more than correctly deciding how constituents would vote on a dichotomous roll call. It is about knowing which issues ought to be brought to the fore. It is about generating solutions to tough social and economic problems. It is about finding common ground and forging consensus on policies that benefit the public good. Thus, properly representing constituents' interests might require a variety of strengths, cognitive styles, and interpersonal skills. Indeed, diverse groups typically make better decisions than do homogeneous ones (Page 2007). The descriptive under-representation of certain groups of people therefore matters not because descriptive representation is itself inherently important—a point we do not contest—but rather because it has powerful implications for substantive representation, broadly construed.

In this sense, then, the question of why American legislatures are not more diverse is an important one for the quality of representation. What factors contribute to the under-representation of women, minorities, and other types of representatives? While it is easy to suspect that biased voters, partisan politics, or an otherwise skewed electoral process might be to blame, we know, instead, that when women, for example, run for office, they win with at least as much frequency as do men (Darcy, Welch and Clark 1994). Thus, we look for another potential culprit: the possibility that certain groups of people systematically choose not to become candidates in the first place. The central issue is therefore one of candidate emergence.

In this study, we focus on the emergence of a particular group—women—and conduct a laboratory experiment to investigate whether the under-representation of women relative to men stems from differences in how these groups decide to run for office.[1] We pay particular attention to the prevailing explanation that women with qualifications similar to those of men are significantly less likely to perceive themselves as qualified to run for office (Lawless and Fox 2005). By taking the question to the laboratory, we can directly measure task ability—normally unobservable outside the lab—and exercise careful control over an environment that is free from external, confounding factors that may create other differences in the willingness to run for office.

[1]The dearth of women is itself a significant problem for democracy, a problem we do not wish to minimize with our focus here on ability to represent, braodly defined. Legislatures with more women enjoy greater legitimacy among the governed (Schwindt-Bayer and Mishler 2005), diverse groups in general make better decisions (Page 2007), and groups with more women tend to work together more effectively (Woolley, Chabris, Pentland, Hashmi and Malone 2010). Women also tend to mitigate the deleterious effects of ideological preference divergence in legislatures (Kanthak and Krause 2010, Kanthak and Krause 2012). Furthermore, observing women candidates creates an increased sense of efficacy among girls (Campbell and Wolbrecht 2006).

Our findings point to a distinct phenomenon, *election aversion*, whereby elections themselves—rather than differences in ability or relative confidence—dissuade women from entering the fray. Specifically, we find that both men and women volunteer to be the representative of a group at equal rates, and they are equally responsive to task ability, provided that the selection of the representative does not involve an election. However, when selection involves an election, women's willingness to represent decreases substantially, and we show that the decline in candidate entry cannot be attributed to differences in ability, confidence about relative ability, or risk aversion. Instead, our findings indicate twin concerns: Campaigns are at once too costly and too noisy affairs. The din of the typical campaign environment and the arduousness of properly communicating their qualifications to voters renders the whole process an insufficiently worthwhile undertaking. It is bearing the costs of running for election, coupled with a campaign setting in which low-information voters cannot properly discern women's qualifications, that impedes women from entering the electoral arena.

Differences in Candidate Emergence

Research across several social science disciplines points to a variety of behavioral differences between men and women. Women communicate differently (Gilligan 1993), are less likely to engage in negotiation (Babcock and Laschever 2003), show less confidence in their abilities (Furnham and Rawles 1995), tend to be more risk averse (Croson and Gneezy 2009, Eckel and Grossman 2002), more prosocial (Eckel and Grossman 1998), and less competitive (Niederle and Vesterlund 2007) than their male counterparts. Taken together, these gender differences may imply that women and men take different approaches to the decision of whether or not to run for office. More specifically, women may be more election averse than are men, where election aversion is a phenomenon that is perhaps related to the other behavioral differences outlined above, but is also wholly distinct from them.

In our theoretical framework, we decompose the component parts of that decision and propose three factors that contribute to an individual's decision to run for elective office. This decomposition allows us to better understand how each of these factors affects an individual's level of election aversion. By considering and accounting for each of these factors, we can not only determine whether men and women differ in their choices to run for office, but also investigate which portion of the decision-making process plays the strongest role. Furthermore, we can rely on the extant literature on the differences between men and women to guide our theoretical expectations at each stage. We now discuss each of the factors that comprise election aversion in more detail.

The first factor any prospective candidate must consider is whether or not they want to act on behalf of—or represent—others. That is, at a basic level, individuals may differ in their willingness to serve. The extant literature would lead us to believe that the reluctance of women to run for public office is not located here. If anything, women are more likely to serve than men. Women are more likely than men to engage in a number of political activities, including attending rallies and meetings (Conway, Steurnagel and Ahern 1997), and women perform volunteer service at a much higher rate than men, across age group, level of education, and other demographic characteristics, although slightly more men than women volunteer for "civic, political, professional, or international" groups (Bureau of Labor Statistics 2011). Furthermore, we know that although women do more volunteer work than men do, they are less visible (Margolis 1979) and tend to downplay their efforts and abilities in volunteerism (Abrahams 1996, Blackstone 2004, Daniels 1985). This would lead us to expect that women would be at least as likely to *volunteer* to represent a group as would men.

Second, a candidate must be willing to run in an election. This factor is conceptually distinct from beliefs about ability or willingness to serve and involves beliefs about one's likely success in the electoral process, due to need to campaign for votes as well as expectations about how others may judge one's ability to serve. Our expectations of a gender difference are heightened here. This is largely because running for election requires asking voters to support one's candidacy. If, as previous research suggests, women are less likely to ask for raises or other tangible rewards for their performance (Babcock and Laschever 2003), then they are likely similarly unwilling to ask voters to "hire" them for the role of their representative. Indeed, women may be wise to be reluctant to ask for such support. Women are both perceived to be less capable (Goldin and Rouse 2000) and are less likely to boast about their abilities than men, but their claims are believed equally, leading to a gender gap in perceived ability (Reuben, Sapienza and Zingales 2009). In this manner, if women feel they want to represent and also feel capable of representing, but they do not enter the election, we can infer that it is something inherent in the election itself (likely, having to ask for support from potential constituents or beliefs about the likelihood of receiving enough support) that affects the decision not to enter. Notably, this line of thinking implies that women are simply less likely to run for political office in general. And if it is correct, women simply don't run, regardless of their perception, or the reality, of their capability of serving. In other words, women don't run *despite* feeling that they are qualified.

Third, potential candidates may differ both in their abilities and in how they assess those abilities in relation to others. In other words, candidates and officeholders will vary in their underlying ability to perform the duties of office (whatever those duties may be) and the higher one's actual ability or level of competence, the more worthwhile and attractive it will be to hold office (both for the representative and for his or constituents). However, individuals also vary in the extent to which they correctly judge their own abilities, with men being generally more overconfident than women (Lichtenstein, Fischoff and Phillips 1982, Lundeberg, Fox and Punccohar 1994). Here, we may also expect to see gender differences, with men being more likely to enter because they have greater confidence in their own ability to perform the duties of office. For example, women underestimate their own intelligence, whereas men overestimate theirs (Furnham and Rawles 1995) and men tend to be overconfident about their own skill set (Kling, Hyde, Showers and Buswelll 1999). Similarly, despite there being no gender difference, men, in fact, rate their math skills higher than do women (Wigfield, Eccles and Pintrich 1996). If these differences affect women and men differentially as they consider whether or not to serve, we would expect them to display differing levels of sensitivity to changes in their ability to perform a task.

In our framework, women and men may differ in terms of each of these three factors. We can test each of these implications in the laboratory, where our ability to control the experimental environment allows us to identify the distinct effects of each of these components. Our approach, moreover, allows us to consider the effect of ability more finely than previous research, which relies on answers to survey questions and finds that women self-report that they think they are not qualified. But our research can allow us to consider what, precisely, are the qualifications women think they lack. When they report not feeling qualified, do women mean they are not up to the job of the representative? If so, that will manifest equally in an unwillingness to volunteer and an unwillingness to become a candidate in an election to represent the group. On the other hand, do women mean they do not feel they are able to *convince others* that they are qualified to represent the group? If that is the case, we should see the effect manifest only in the decision to become a candidate, but not in the decision to volunteer—that is, we would observe evidence of election aversion.

Experiment 1: Volunteers and Candidates

Our first experiment tests whether differences in the willingness to become a candidate depend on confidence in one's underlying abilities or whether such differences arise instead from the nature of electoral competition.[2] Three features of the design are central to distinguishing between these non-environmental factors that may comprise election aversion as a phenomenon distinct from other gender-based behavioral differences. First, the experiment

[2]The lab also allows us to ignore external factors, such as familial responsibilities, access to campaign fundraising money, or political networks that may also affect women's and mens' choices to run differentially. But we maintain that this is a feature, not a bug, of our laboratory approach. Is it possible to find behavioral evidence of gender-based election aversion even without accounting for these other—certainly powerful—factors?

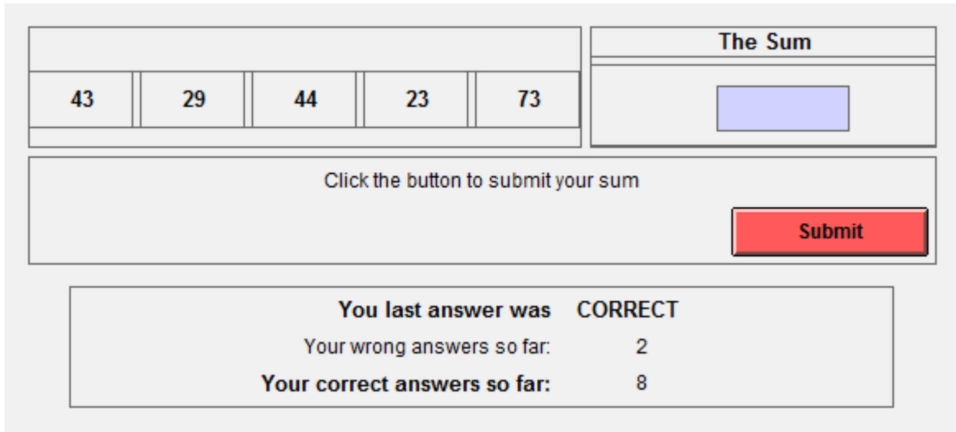
revolves around an objective problem-solving task. Performance on the task serves as an observable and objective measure of underlying ability, which we can think of as the laboratory analogue of policy-making ability or the ability to translate constituents' interests into good policy—in other words, the potential quality of the representative. Such ability is not normally observable using non-experimental data. Second, we designed the monetary payment scheme so that all members of a group have common incentives to select the highest ability member as their representative. This allows us to control for heterogeneity in the value of holding office that is likely to exist between individuals. Third, we compare the decision to enter the pool of potential representatives under two alternative group selection mechanisms: one with an election and one without. Thus, we can carefully assess whether gender differences depend specifically on the prospect of electoral competition or whether they arise from differences in relative confidence in ability.

Design and Procedures

We selected the Five-Minute Addition Task used by Niederle and Vesterlund (2007) to study preferences for competition as the task for our experiment. It involves computing the sum of five randomly selected two-digit numbers and doing as many of these sums as possible correctly within five minutes. For our purposes, the Addition Task has several desirable properties. It is specifically void of ideological or political content, which controls for subjects' knowledge and interest in particular political questions of the day. In this sense, then, it is akin to the intangible qualities that allow some representatives to maximize the congruence between constituency interests and policy outcomes better than others. It is also a task for which there is heterogeneity between subjects, and that previous research suggests is gender neutral. Figure 1 illustrates the computer interface for the task, which we implemented in z-tree (Fischbacher 2007).[3]

[3] After a subject entered a sum in the computer, the computer immediately presents the next series of random numbers and simultaneously provides feedback about whether or not the previous sum was correct as well as a running tally of the number of correct sums. Subjects were not allowed to use calculators, but

Figure 1: Addition task screen



Even though previous studies using the Addition Task demonstrate there are no gender differences in performance, the fact that the task involves doing math problems raises the possibility that stereotype threat (Spencer, Steele and Quinn 1999, Steele and Aronson 1995) may play a role in the decisions we are interested in. That is, negative stereotypes about women's math abilities might negatively affect the confidence that women have in their ability or in the beliefs they hold about how their ability compares to others in their group. We took precautions to guard against this (while also being careful not to specifically cue gender) by informing subjects that the "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." Nevertheless, if negative stereotypes are deeply held, they might still adversely influence women's decisions about representing their group. In our view, this is actually a desirable feature of our experimental design for the simple reason that like math, *politics is traditionally viewed as a task that belongs in the masculine domain* (Conway, Steurnagel and Ahern 1997). The choice of a math task therefore enhances the external and ecological validity of our study because the presence of negative stereotypes in our experimental context is similar to the real-world political context of choosing to run for office. Furthermore, we can directly test for this stereotype threat because we would expect

could use scratch paper to complete the task.

it to reveal itself as part of confidence in ability to do the job for which a representative is being selected. Because of this, we should see the effects of stereotype threat in *both* electoral and non-electoral settings.

We conducted our experiments at the Pittsburgh Experimental Economics Laboratory at the University of Pittsburgh. A total of 130 subjects (65 men and 65 women) participated in Experiment 1. There were ten participants (five men and five women) in each session, which lasted under an hour. All interaction between subjects took place anonymously via a computerized interface. Subjects were assigned ID numbers and were identified in their interactions with other group members only by their ID number. While subjects could observe the gender of the other participants in the session, we randomly assigned them to two groups of five members so that they would know neither which of the other participants were in their group nor its exact gender composition. Experiment 1 involves a within-subject design, so the rules and payoff structure for every session was identical. Each session was divided into three parts, and at the beginning of each part we distributed and read the instructions aloud for that part of the experiment. At the end of the experimental session, subjects completed a questionnaire that included demographic questions, one of the three parts was randomly selected for payment (to guard against subjects using one part to hedge against decisions in other parts), and subjects were paid. In addition to their earnings from one of the parts of the experiment, subjects received a $7 show-up fee.

In Part 1, which we designated *Piece Rate*, we introduced the task and paid subjects for their performance. Each subject earns 75 cents for each correct sum if this part is selected for payment and receives feedback only about his or her own individual performance. No subject learns anything about the performance of the other members of the group. The purpose of Part 1 is for subjects to learn their ability in absolute, but not relative, terms.

In Part 2, we introduce *Group Representation*. Subjects first decide whether or not they are willing to be selected as the group representative, and then a representative is randomly selected from the set of willing members of each group.[4] We deem such willing members *volunteers* (although we do not use the term in our instructions so as not to induce or activate social desirability, norms, or other-regarding preferences). Subjects then repeat the task and, if Part 2 is selected for payment, are paid 50 cents for each of the representative's correct answers plus 25 cents for each of their own correct answers. (Although we provide feedback about subjects' own performance on the task in Part 2, we do not provide feedback about payoffs or the representative's performance until the end of the experiment so that subjects do not gain any information about relative abilities.) Subjects maximize their payoffs if the highest performer in the group is selected as the representative, and thus group members' preferences are aligned. As we explain in the next section, the decision to volunteer should primarily depend on subjects' *beliefs about their relative ability* and therefore serves as our non-electoral baseline for comparison.

In Part 3, we introduce an *Election* for selecting the group representative, which replaces the random selection mechanism from Part 2. After the election, subjects repeat the Addition Task and are paid for performance on the task the same way as in Part 2: They earn 50 cents for each of the representative's correct answers and 25 cents for each of their own. The incentives for selecting the highest performer are therefore the same as in Part 2. We also include two features of the electoral environment that we believe may affect subjects' willingness to run for office: a campaign and costs and benefits of running for office.

The campaign works as follows. If there are two or more *candidates* in a group, each candidate writes a brief text message. This message is the only information that other group members have when they vote. Candidates can write whatever they like, so the message is "cheap talk." Moreover, if subjects choose to do so, they can lie about their past performance on the task. Subjects then each cast one vote for a candidate (voting for oneself is permitted) and the representative is selected by plurality rule with ties broken randomly.

[4]If no group member is willing, we randomly select one member from all members of the group.

The costs and benefits associated with becoming a candidate are as follows. Subjects receive \$1 for each vote they receive in the election while incurring a fixed cost of running for office of \$2. Introducing these costs and benefits ensures that subjects' expectations about their electoral performance are relevant to their incentives to run for office. Note that the cost-benefit structure is designed so that there is no net benefit to running if a subject expects to receive two votes (e.g., votes for himself or herself and garners one other vote), a net benefit for receiving three or more votes, and a net cost if a candidate cannot garner at least one other vote. If there is only one candidate, then that candidate automatically becomes the representative and earns a net benefit of \$3 from running, but if there are no candidates, a representative is selected at random from all members of the group and does not receive any additional benefit from being selected.[5]

Theoretical Expectations

Our analysis of the incentives for choosing to be a volunteer or a candidate in the experiment helps to identify potential sources of behavioral differences between men and women within a common theoretical framework. We begin by considering the decision problem from the perspective of an expected payoff-maximizing individual, which provides a gender-neutral benchmark for behavior.[6] We then discuss how men and women might differ within this framework and discuss observable implications of these differences.

Volunteers. An individual's decision to volunteer or not boils down to a comparison between her own score and the beliefs she holds about other volunteers' scores.[7] If she is risk neutral, then she will prefer to volunteer if and only if her score, s_i, is above the average score of the other volunteers, $\overline{v}_j$. The intuition is straightforward. Recall that in Part 2, each volunteer is equally likely to be selected as the group's representative. If her score is

[5] In game theoretic terms, our incentive structure ensures that there cannot be a Nash equilibrium in which there are no candidates or in which all group members become candidates.

[6] Implicit in the assumption of expected payoff maximization is risk neutrality. We will consider the implications of risk aversion later in the analysis.

[7] See the Theoretical Appendix for formal analysis.

above $\overline{v}_j$, then volunteering raises the expected score of the group's representative, but if her score is below $\overline{v}_j$, then volunteering lowers it.

If men and women have the same beliefs about others and if there are no gender differences in performance, then we would also expect there to be no differences in volunteering decisions. If there are no gender differences in beliefs, but men have higher task ability than women, then we would expect men to be more likely to volunteer than women only in the aggregate but for there to be no gender differences in the probability of volunteering when controlling for task ability. We should therefore expect differences in the probability of entering (when controlling for task ability) only when men and women have different beliefs about their ability relative to others. Thus, all else equal, to the extent that men are overconfident in their abilities, and women underconfident, we would expect to observe men volunteering at greater rates than women. However, if men and women are equally confident in their abilities, we would expect the rates of volunteering to be the same.

The above argument appears to depend on the assumption that men and women are both risk neutral. If we allow for risk preferences to vary, the decision to volunteer still takes the form of a cutoff rule: i volunteers if and only if s_i is above some cutoff k, where k can be thought of as the minimum willingness to run and depends on a subject's risk preferences. Different rates of volunteering might therefore be explained by differences in risk preferences, even when subjects have the same beliefs about others.

Intuition suggests that subjects who are more risk averse should be less likely to volunteer, but this intuition is wrong. Instead, *greater risk aversion should increase the willingness to volunteer.* That is, risk aversion lowers the value of the threshold k. To see why, consider a subject whose score is equal to the expected value of other volunteers' scores. A risk neutral subject would be indifferent between volunteering and not volunteering. A risk averse subject, however, has a strict preference for volunteering because doing so *reduces the variance of the representative's score* while leaving the expected value unchanged. Thus, if women are generally more risk averse than men, then they should be more likely to volunteer than men with the same ability and beliefs.

Candidates. In Part 3, moving from a random selection mechanism to an election introduces new considerations into the decision calculus. The addition of a campaign stage with costs and benefits of running for office implies that expectations of electoral success should play an important role alongside beliefs and risk preferences. In this sense, differences in willingness to enter bewteen Parts 2 and 3 may capture how election seeking or election averse subjects are. We postulate that such expectations will depend, in large part, on how informative individuals expect the electoral process to be. The informativeness of the election depends, in turn, on the degree of honesty one expects of other group members as well as one's own aptitude for conveying to others his or her task ability.

To analyze how the informativeness of the election (combined with direct costs and benefits of office-seeking) affects the decision to run, we consider the polar cases in which elections are either completely uninformative or completely informative. Although extreme, the case of uninformative elections is not entirely unreasonable to the extent that campaigning typically involves strategic incentives for candidates to misrepresent their true abilities. In our experiment, such incentives are present but small since group members have common incentives to elect the best representative possible.[8] At the other extreme, the election might be completely informative if all group members recognize their shared goals and candidates report their scores honestly because they view the purpose of the election as a way to find the best group member.

In the case in which the election is uninformative, we can expect the votes of non-candidates—and thus the results of the election—to be determined randomly. Hence, choosing to run affects the expected quality of the representative in the same way that it does in Part 2 with random selection. Holding ability and beliefs constant, the expected net benefit of running depends on the number of other candidates expected (because the cost is fixed

[8]The best possible outcome for an election-seeking individual is to win all five votes and earn a net benefit of \$3. So, if an individual believes he will win all five votes with certainty (which is an unreasonable, highly optimistic belief) and his score is within 6 of the highest score, then he has an incentive to lie to seek votes. However, the fewer votes he expects to receive (e.g., the more intense he expects electoral competition to be), then his score must be closer to what he believes to be the highest score for vote-seeking to be preferable to allowing the best representative to be elected.

while the expected benefit is decreasing in the number of candidates). Formally, a expected payoff-maximizing subject prefers to become a candidate if her score exceeds $\bar{c}_j + 4n - 6$ where $\bar{c}_j$ is the average score of the n other candidates. The threshold is increasing in n, which implies that all else equal, the willingness to run is decreasing in the expectation of greater electoral competition. For example, if $n = 1$, then any subject whose score is *below* the average by 2 is indifferent between running and not running, and any subject whose score is above this value strictly prefers to run. In contrast, if $n = 3$, a subject must have a score at least 6 *above* the average to be willing to run.

At the other extreme, if elections are perfectly informative, then the highest ability candidate will always be elected. Thus, the relevant belief about the distribution of scores is not whether one's own score s_i is above or below the mean, but the probability that s_i is the *highest* score among the pool of candidates. By choosing to become a candidate, individual i effectively sets the *minimum* score of the representative (i.e., the representative's score cannot not be below s_i because no candidate with a score below s_i will be elected as the representative). For example, if s_i is the average score, then being a candidate guarantees that the expected representative's score will be *above* average (i.e., average if i wins and above average if i loses). Thus, the more informative individuals believe the campaign to be, the more likely they are to run (because it lowers the minimum willingness to run).

In addition, the probability that s_i is the highest score is decreasing in the number of candidates. This is because with more candidates, repeated draws from same distribution provide more frequent opportunites to draw a higher score. The expected net benefit from choosing to run in terms of the effect on the expected quality of the representative is therefore decreasing in the number of candidates, as it is with uninformative elections. In this case, the more intense electoral competition is expected to be, the less likely that s_i is the highest score, and the less likely that individual i is to run.

Summary. In establishing a benchmark for what expected payoff-maximizing individuals would do in our experiment, our theoretical analysis generates several key insights about factors that contribute to the decision to volunteer and to run for office. Central to our experimental design is an objective measure of candidate ability, and our analysis verifies that any differences we observe between men and women under the random selection mechanism in Part 2 should be due primarily to differences in their *beliefs about the distribution of ability*. This allows us to test directly a central component of much of the literature on gender differences in candidate emergence: the notion that women are more likely to think they are unqualified for office. If men underestimate the overall distribution (leading to overconfidence in their abilities) or if women overestimate it (leading to underconfidence), our analysis implies that we would indeed observe gender differences in Part 2.

If candidate entry decisions instead hinge on factors related specifically to electoral competition—such as inhibitions about asking for votes or a lack of confidence in the potential for electoral success—rather than on confidence in one's relative ability as a representative, then we would not expect to see gender differences in Part 2 decisions but instead observe them only in Part 3 decisions. Our theoretical analysis identifies two potential reasons for this.

First, gender differences in candidate entry might be due to different *beliefs about electoral competition*. Individuals are more likely to run if they expect less electoral competition. If men underestimate or women overestimate the degree of electoral competition, then men are more likely to run than women even holding ability constant. This is an explanation that, to our knowledge, is not recognized in the existing literature.

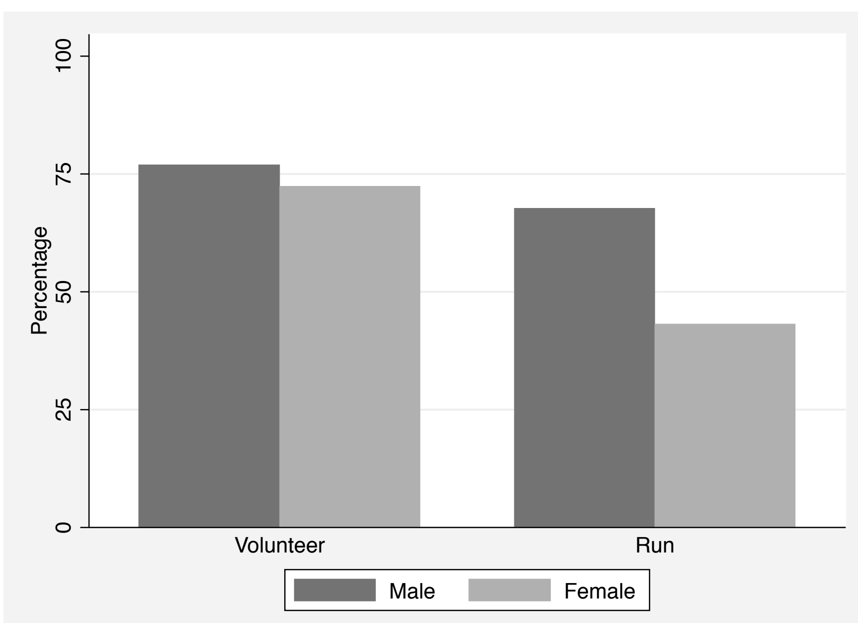
Second, gender differences in elections might arise from differing *beliefs about the informativeness of elections*. Running for office is less beneficial (both personally and for the group) the more randomness there is in electoral outcomes. Thus, we would expect women to be less likely than men to run if they believed elections were less informative—in other words, if they tend to believe (more than men do) that elections are less about merit and more about strategic posturing, misrepresentation, or vote-seeking.

Results

The results of Experiment 1 point to dramatic differences in how men and women approach the question of whether or not to volunteer or run in an election to represent their groups. Figure 2 presents the main results for subjects' willingness to be the group representative for each of our selection mechanisms. The bars on the left show that men and women are equally likely to volunteer to be the representative when the representative is selected randomly in Part 2. Men and women are both likely to volunteer (77% of men volunteer and 72% of women), and the difference is not statistically significant ($\chi^2_{(1)} = 0.37, p = 0.55$). Given the fact that we do not see gender-based differences in the volunteer decision, our theoretical analysis suggests that we should similarly not expect to see gender-based differences in the candidate decision if confidence in ability to do the job is the major factor in determining whether or not subjects are willing to represent their groups.

Yet this is not the case. Rather, there is a substantial gender difference in the willingness to run for representative when the selection mechanism involves an election. The proportion of men who run in Part 3 (67%) is only slightly less than the proportion who

Figure 2: Choices to represent



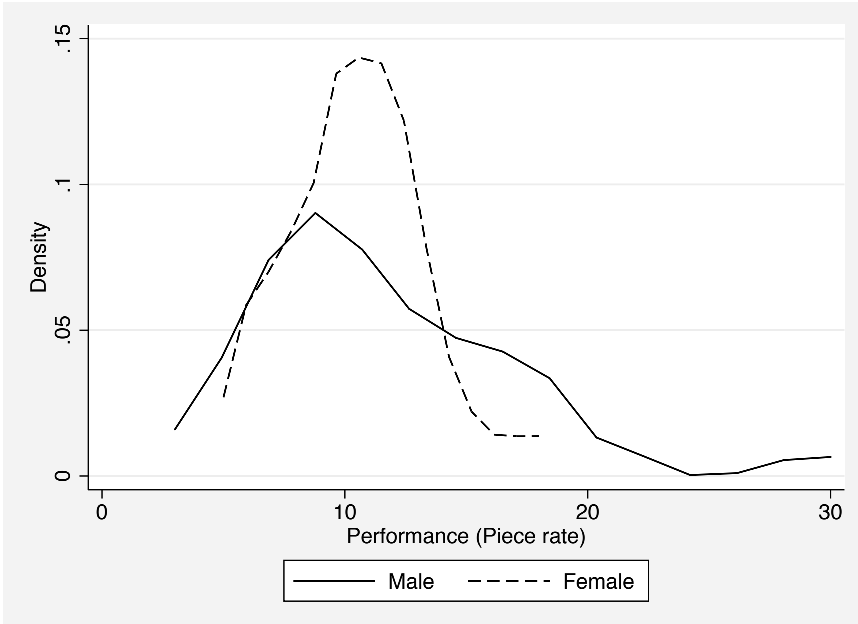
volunteer in Part 2, while the proportion of women who run drops substantially, to fewer than a majority (43%), a difference that is statistically significant ($\chi^2_{(1)} = 7.97, p < 0.01$). Men and women differ dramatically in their willingness to run for office, even in the absence of external forces that the extant literature suggests are important, such as family obligations. Most significantly, *lack of confidence in doing the job cannot explain these gender differences.* Previous research finds that women *report* that they think they are not qualified to run for election, but our results here indicatee that women are quite capable of responding appropriately to task ability, provided that they are in a non-electoral environment. Women know when they are capable of serving as quality representatives, and they can effectively bring that knowledge to bear on the question of whether or not to volunteer. Yet when it comes to elections, women are far less willing than men to enter the fray.

Of course, we also need to take advantage of our experimental data and examine whether there are underlying differences in task ability. We find that there are no differences in the average performance of each gender: The mean number of correct sums is 11.6 for men and 10.5 for women, and a t-test shows that the difference is not statistically significant ($p = 0.12$). There is, however, an important difference in the distribution of scores. As the kernel density plots in Figure 3 show, the variance in performance is quite a bit higher for men. There are a few men with very high scores, and women's scores are clustered more tightly around the mean; a Kolmogorov-Smirnov test rejects the equality of distributions ($p = 0.04$). Interestingly, our findings differ from those of Niederle and Vesterlund (2007); In their experiment, distributions of men's and women's scores were nearly identical.

To investigate more precisely the relationship between task ability and choice, we estimate probit models and report the results in Table 1. The dependent variable is the choice of whether or not to enter the potential pool of representatives, and we structure the data so that there are two observations for each subject (one observation for their Part 2 choice and one observation for their Part 3 choice).[9] Structuring the data in this way

[9] In other words, the data for the probit analysis resembles a panel structure where the panel variable is the subject and the time variable is the part of the experiment. Because there are two non-independent

Figure 3: Task performance (kernel densities)



allows us to estimate the effects of task ability (performance in Part 1) on choices, as well as to estimate the "treatment effect" of electoral competition and the interaction between the electoral environment and performance. We also estimate the model separately for each gender, which is equivalent to estimating a single encompassing model in which all of the coefficients are gender-specific.

Men's choices in both Part 2 and Part 3 are sensitive to their task ability, but their decisions depend somewhat less on task ability when the selection mechanism is an election than when it involves random selection. The main coefficient for performance is positive and statistically significant, which implies that men are more likely to volunteer the higher their task ability in Part 2. However, the interaction between electoral competition and performance is negative and statistically significant, which means that men are less likely to run for election in Part 3 as a function of task ability than they are to volunteer in Part 2. The sum of this interaction coefficient and the main coefficient for performance remains

observations per subject, we cluster the standard errors by subject.

Table 1: Probit analysis of entry choices in Experiment 1

	Men		Women	
Score	0.18**	0.38**	0.07*	0.18**
	(0.04)	(0.11)	(0.04)	(0.07)
Election	-0.40*	1.97*	-0.79**	1.15
	(0.22)	(1.03)	(0.20)	(0.86)
Election x Score		-0.26**		-0.19**
		(0.12)		(0.08)
Constant	-1.02**	-2.72**	-0.11	-1.23*
	(0.42)	(0.93)	(0.47)	(0.69)
Log likelihood	-61.03	-57.51	-81.38	-79.03
N	130	130	130	130

Notes: * p < 0.10, ** p < 0.05. Standard errors clustered by subject in parentheses.

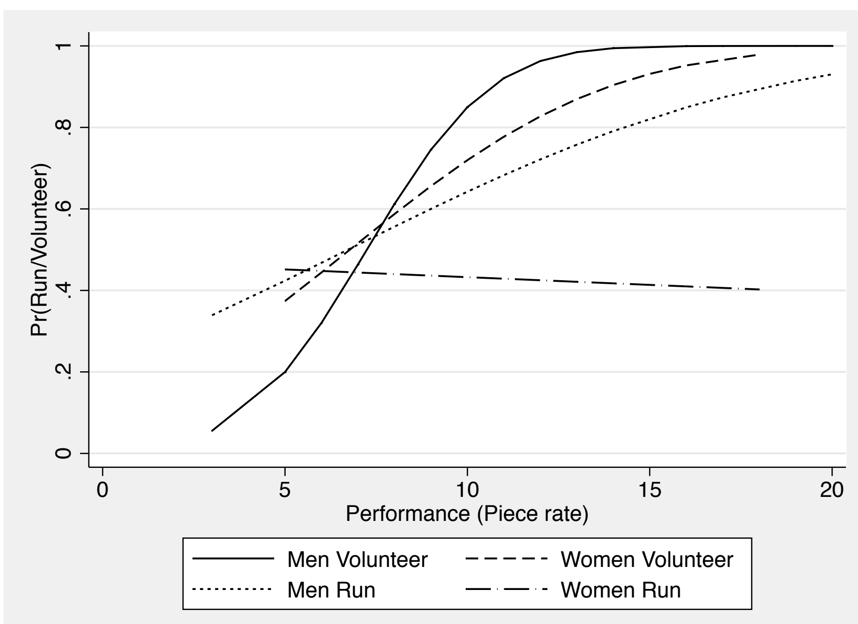
positive and significant (the combined coefficient is 0.12, the standard error is 0.03, and $p < 0.01$), so it still is the case that higher ability men are more likely to run for election.

Of greater interest to us is whether the regression results show women to be less sensitive to their task ability than men. The coefficient for performance for women is positive and statistically significant, as it is for men, and the magnitude is clearly lower. Women, like men, are more likely to volunteer in Part 2 the higher their task ability. However, an F test for the equality of coefficients in an encompassing model cannot reject the hypothesis that the coefficient for performance for men and women are equal. But the story is different for Part 3—when there is an election. The interaction between election and performance is negative and statistically significant, which implies that women are even less sensitive to their task ability when there is an election (just as it implied the same for men). To estimate women's sensitivity to task ability in Part 3, we must add the main effect of performance with the interaction. The combined coefficient is *not* significantly different from 0 (the combined coefficient is -0.01, the standard error is 0.06, and $p = 0.86$), which implies that a woman's decision to run for election in Part 3 does not depend on her task ability at all.

To assess the overall probability of volunteering or running for different levels of task ability, we compute the predicted probabilities from the probit model and plot them separately for each gender and selection mechanism in Figure 4. While the order of the predicted probabilities on the right side of the figure reinforces our discussion of sensitivities to task ability, what is striking about the figure is the fact that the overall probability that a woman runs for election is so much lower for many levels of performance. The predicted probability curves intersect near the point where the score is 7, which is roughly the 25th percentile and far below the mean of score of 11. Figure 4 illustrates that although an average ability woman is not much less likely to volunteer as an average ability man, she is substantially less like to run for election, and the disparity is even greater for high ability women.[10]

But before we make a policy recommendation that we replace democratic elections with random selection mechanisms, we may want to consider carefully what it is about

Figure 4: Predicted probabilites of volunteering and running



[10]The predicted probabilities of volunteering for a woman whose score is the average, 11, is 0.78 versus 0.90 for a man, a difference of 0.12. The corresponding probabilities of running are 0.43 and 0.68, a difference that more than doubles to 0.25. For a woman with a score of 14, which is above the 75th percentile, the predicted probability that she runs for election is 0.40 vs 0.79 for a man, a substantial difference of 0.39.

elections that cause men, and especially women, to be insensitive to their own abilities when deciding to run. Again, we can rely on the use of laboratory experiments to provide us with an opportunity to consider carefully and independently the myriad factors that affect the entry decisions of potential candidates. We take advantage of those opportunities in Experiment 2, which allows us to decompose election aversion into its various parts and to determine which are most significant.

Experiment 2: Costless and Truthful Campaigns

To more systematically investigate the potential causes of the substantial gender differences we uncovered in Experiment 1, we devised a second experiment that leverages the many advantages of experimental control, relying on both within and between subject comparisons as well as incentivized measurement procedures. To test whether the prospect of strategic campaigning deters women from becoming candidates, we vary the campaign environment and compare *truthful* campaigns with the free-form *chat* campaigns used in Experiment 1. To test whether the financial costs of campaigning may be the culprit, we compare volunteer and candidate decisions *with* and *without costs and benefits*, holding these incentives constant within sessions. Varying these two factors yields four treatments in a 2×2 factorial design: chat with costs and benefits (*CCB*), chat without costs and benefits (*CNO*), truth with costs and benefits (*TCB*), and truth without costs and benefits (*TNO*). Note that the design also allows us to assess the interaction between these factors.

In all four treatments, we also add two incentived measurement tasks. The first is a belief elicitation procedure. Because beliefs about others' abilities play a crucial role in maximizing expected payoffs, differences in behavior in Experiment 1 might have arisen from unobserved heterogeneity in subjects' beliefs about others. Thus, we elicit beliefs about the distribution of other group members' scores directly so that tests for differences, control for them in the statistical analysis, and assess whether such beliefs are sensitive to changes in the campaign environment. The second incentivized measurement task elicits risk preferences. Although the results of Experiment 1 contradict the theoretical prediction that greater risk aversion among women should lead to higher levels of candidate entry, it is nevertheless possible that decisions may be related to risk preferences in unexpected ways. Thus, it is important that we measure risk preferences directly.

Design and Procedures

Many of the basic procedures for Experiment 2 are similar to those for Experiment 1. Part 1 consists of the Addition Task with the individual *Piece Rate* compensation, Part 2 is *Group Representation* involving the random selection of the representative from the set of volunteers, and Part 3 involves selecting the representative by *Election*. Rewards from performance are also identical: 75 cents for each correct answer in Part 1, 50 cents for each of the representative's correct answers in Parts 2 and 3, and 25 cents from one's own correct answers in Parts 2 and 3. As before, each session involved an equal number of men and women, randomly divided into groups of 5, who interacted anonymously through networked computers. We did, however, increase the number of subjects from 10 to 20.

In the campaign chat treatments (*CCB* and *CNO*), the sequence of actions in the *Election* stage was identical to that of Experiment 1. Subjects first decided whether to become candidates, candidates then wrote free-form text campaign messages, and group members voted to elect the representative. In contrast, candidates cannot send messages in the truthful campaign treatments (*TCB* and *TNO*). Instead, the only information that group members have when voting is an accurate report of a candidate's Part 1 score.

In treatments with costs and benefits (*CCB* and *TCB*), the payoff structure differs somewhat from that of Experiment 1. In Experiment 2, subjects pay an entry cost of $1 if they are willing to be the representative and earn a reward of $2 if they are both willing and selected as the representative. These costs and benefits apply both to the decision to volunteer and to the decision to become a candidate. Note that the incentives are weaker or more subtle than in Experiment 1, but also that the costs and benefits of being willing to enter the pool of potential representatives are identical in Parts 2 and 3 within the *CCB* and *TCB* treatments, which allows for a cleaner within-subject comparison. In the treatments without costs and benefits (*CNO* and *TNO*), there are simply no costs or benefits associated with the Part 2 and Part 3 decisions other than the payoffs from task performance. Removing the costs and benefits provides for a cleaner between-subjects comparison to assess the effects of the truthfulenss of the campaign environment as well as to assess the effects of costs and benefits within campaign environments.

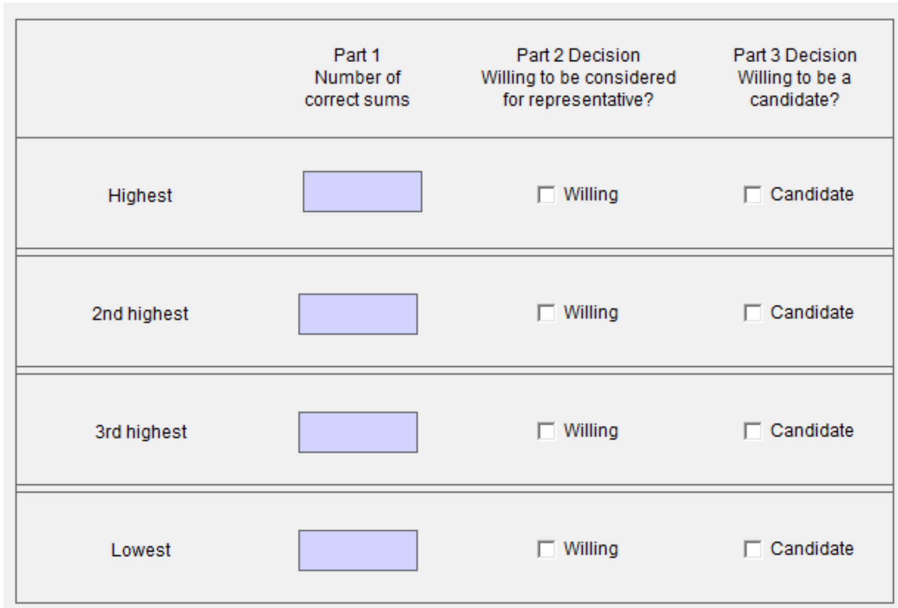
In Part 4, which we called *Estimation*, we elicited subjects' beliefs about other group members' performance and entry decisions.[11] We did so by asking subjects to guess the Part 1 scores of the other four members of their group in order (highest, second highest, etc.) and rewarding them for their accuracy. Guessing a score exactly earns \$10 while other guesses earn \$5 divided by the absolute difference between the guess and true score. Thus, payments for guesses are increasing in the accuracy of the guess.[12] The interface for the estimation task is shown in Figure 5.

We also asked subjects to guess what the other members' decisions were—to volunteer or to run—in Part 2 and Part 3 and paid them \$5 for each decision guessed correctly. To guard against hedging, if Part 4 was selected for payment, we also randomly selected the guesses corresponding to only one of the other members for payment. For example, a subject who correctly guesses the highest score and both Parts 2 and 3 decisions of the high scorer would earn a maximum of \$20 if we selected guesses about the highest scorer for payment.

[11]The timing of Part 4 in the experiment was a bit tricky. First, we needed to elicit beliefs before subjects learned anything about other members' scores (as they would in the truthful campaign treatments). Second, we needed to ensure that the process of estimating others' scores would not influence or otherwise bias their decisions to become a candidate. That is, to the extent that subjects do not normally estimate the scores of others explicitly, asking them to do so alters their decision-making process. Thus, we interrupted Part 3 of the experiment by announcing and completing the Part 4 estimation task *after* subjects made their candidate entry decisions and *before* the election.

[12]This incentive structure is used by Gächter and Renner (2010) and Croson (2000). While there is a growing experimental literature on belief elicitation, most experiments elicit probabilities of binary outcomes using complicated scoring rules. In contrast, we needed to elicit beliefs about a multinomial distribution in a way that could be explained relatively simply and without the need to instruct subjects in the meaning of probabilities.

Figure 5: Estimation task screen



Alternatively, if a subject's guess about the lowest scorer differs by 10 from the true score and neither guess about the lowest scorer's Part 2 or Part 3 decisions are correct, then the subject would earn only $0.50 if we selected guesses about the lowest scorer for payment. Our method of belief elicitation provides us with enough information to compute subjects' beliefs about abilities of the pool of volunteers and about the pool of candidates by combining information about their guesses of Part 1 scores with their guesses about the Part 2 and Part 3 decisions. Similarly, it can be trivially easy to use a subject's own task performance to determine how they think they measure up in comparison with the other subjects in their group.

In Part 5, we measured risk preferences with an incentivized *Lottery Choice* task. The choice task we designed is similar to Holt and Laury (2002) except that we tailored the lotteries to correspond exactly to potential payoffs from Part 2 of the experiment (random selection without costs and benefits). We presented subjects with 9 binary choices between a riskier lottery (Option A, corresponding to not volunteering) and a less risky lottery (Option B corresponding to volunteering), as shown in Figure 6.[13] Subjects had to choose A or B for

[13]To construct the payoffs in Figure 6, each row represents a situation in which a subject with a score

Figure 6: Lottery choice task screen

Chance	Option A 1/2	Option A 1/2	Option B 1/3	Option B 1/3	Option B 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

each of the 9 choices. Note the net expected benefit of choosing B over A is decreasing in x, with the expected values of A and B equal when $x = 5$. Perfectly risk neutral expected payoff-maximizing subjects will switch from choosing B when $x < 5$ to choosing A when $x > 5$. Because choosing B is less risky, subjects who exhibit greater risk aversion will switch from B to A at higher values of x. The number of times a subject chooses B therefore provides a measure of risk aversion.

A total of 200 subjects participated in Experiment 2 (thus far) in 10 sessions, 2 of *CCB*, 2 of *CNO*, 3 of *TCB*, and 3 of *TNO*.[14]

of 10 knows that two other group members, with scores of x and $x + 10$ respectively, have already decided to volunteer. The rows correspond to integer values of x from 1 to 9. Option A then corresponds to the lottery from not volunteering, while Option B corresponds to the lottery from volunteering. For example, if $x = 1$, then Option A yields payoffs of $1 \times \$0.50 + 10 \times \$0.25 = \$3.00$ and $11 \times \$0.50 + 10 \times \$0.25 = \$8.00$ with equal probability. Option B yields payoffs of $\$3.00, \$7.50$, and $8.00 with equal probability, where the middle option is the subject's own payoff from being selected as the representative and having a score of 10.

[14]We plan to run a total of 5 sessions of each treatment so that we have 50 men and 50 women per cell of our design. Despite our best efforts to ensure an equal number of men and women participated in each session, there was one session of *CNO* in which an insufficient number of male subjects showed up, so we ran this session with 9 male subjects and 11 female subjects. We do not believe this affects the results, as there are sufficiently many subjects that participate in each session that they are unlikely to notice the exact gender composition of each session. Furthermore, our post-experiment questionnaire reveals that subjects

Results

Consistent with our findings in Experiment 1, we find that there are no gender differences in the rate of volunteering in Part 2 of Experiment 2 when the representative is selected by the random mechanism. Table 2 presents the aggregate rates of volunteering and running for office for each selection mechanism by gender; it shows that 76.0% of men and 68.0% of women volunteer when there are direct costs and benefits associated with volunteering while 85.7% of men and 78.4% of women volunteer when those costs and benefits are removed. Although men volunteer at slightly higher rates than women, the differences are not statistically significant. However, both men and women are more likely to volunteer in the no cost treatments (*CNO* and *TNO*), which is consistent with the effect of introducing costs and benefits predicted by expected payoff maximization.

Turning to the Part 3 election decisions, Table 2 shows persistent gender differences in candidate entry in all but one of our election treatments. Men continue to run for office

Table 2: Decisions in Experiment 2

	Men		Women		
	Pct	N	Pct	N	p value
Volunteer, Cost	76.0%	50	68.0%	50	0.37
Volunteer, No Cost	85.7%	49	78.4%	51	0.34
Election, Chat + Cost	75.0%	20	50.0%	20	0.10
Election, Chat + No Cost	78.9%	19	47.6%	21	0.04
Election, Truth + Cost	73.3%	30	50.0%	30	0.06
Election, Truth + No Cost	80.0%	30	76.7%	30	0.75

Note: p values are for two-tailed difference in proportions tests

almost never guess that the experiment has anything to do with gender. Only two subjects in Experiment 1 thought the experiment had anything to do with gender: one guessed that it was about "gender and math" while another thought it had to do with reducing stereotype threat. No subjects in Experiment 2 mentioned gender at all.

when there is an election at about the same rate that they volunteer when the representative is chosen randomly (between 73.3% and 80.0%), but women's willingness to run drops to around 50%—to almost the same level as in Experiment 1—in the *CCB, CNO,* and *TCB* treatments. In other words, we continue to find that, in contrast to men, women exhibit a high degree of election aversion.

The only exception to this pattern occurs when we simultaneously guarantee that elections truthfully reveal candidates' abilities and remove the direct costs of entry. In the *TNO* treatment, 76.6% of women run for office, a rate that is statistically indistinguishable from that of men (80.0%) and comparable to the rate of volunteering. Our findings therefore suggest that removing the strategic elements of campaigning and removing (or reducing) the financial downsides to running for office may *both* be necessary to eliminate election aversion in women.

To assess the extent to which our aggregate findings may be due to differences in normally unobservable beliefs or risk preferences, we next analyze the data from our incentivized measurement tasks. We then use these measures as controls in our regression analysis.

Beliefs. Table 3 summarizes the results from our belief elicitation task (Part 4 of the experiment). The first four rows show the mean guesses about other group members' scores (by rank, as described in the procedures) broken down by the gender of the subject making the guesses. The fifth row shows the mean belief about the average score, which is computed by taking the average across a subject's four guesses. The remainder of the table summarizes the beliefs held about other group members' decisions and the mean average scores of others willing to be the representative for each selection mechanism and treatment.[15] For comparison, the right-most column of the table presents the actual values of each quantity.

[15] Although subjects do not guess the averages directly, they are implied by the beliefs we elicited. Individual i's belief about the average score of the (other) volunteers is $\frac{\sum_{j=1}^{4} V_j s_j}{\sum_{j=1}^{4} V_j}$ where V_j is a dummy variable indicating whether i believed that the member with rank j volunteered and s_j is the score that i guessed about j. Similarly, the average implied score of (other) candidates is $\frac{\sum_{j=1}^{4} C_j s_j}{\sum_{j=1}^{4} C_j}$, where C_j indicates whether i believed j was a candidate.

Table 3: Beliefs

	Estimates			Actual
	Men	Women	p value	
All treatments				
Highest score	13.0	13.1	0.82	13.9
2nd highest score	10.8	10.9	0.78	11.0
3rd highest score	9.1	8.9	0.62	8.8
Lowest score	7.4	7.1	0.28	6.8
Average score	10.1	10.0	0.83	10.1
Volunteer, Cost				
Number of volunteers	2.2	2.2	0.86	2.9
Average volunteer score	9.8	9.7	0.88	10.6
Volunteer, No Cost				
Number of volunteers	2.6	2.6	0.80	3.3
Average volunteer score	10.8	11.0	0.86	10.9
Election, Chat + Cost				
Number of candidates	1.9	1.6	0.35	2.5
Average candidate score	9.6	8.8	0.62	10.8
Election, Chat + No Cost				
Number of candidates	2.1	2.0	0.88	2.5
Average candidate score	12.3	9.5	0.04	11.1
Election, Truth + Cost				
Number of candidates	1.9	2.0	0.79	2.5
Average candidate score	9.6	10.4	0.51	10.4
Election, Truth + No Cost				
Number of candidates	2.2	2.6	0.24	3.1
Average candidate score	10.0	11.4	0.22	10.7

Note: p values are for two-tailed difference in means tests

By and large, our results show that men and women hold remarkably similar beliefs about the abilities of others. The mean guesses of the highest other group members are 13.0 for men and 13.1 for women; for the second highest are 10.8 and 10.9; for the third highest are 9.1 and 8.9; for the lowest are 7.4 and 7.1; and for the average are 10.1 and 10.0. Not only are their mean beliefs nearly identical, but they are also quite accurate, especially for the middle scores: compare these guesses to the actual scores of 13.9, 11.0, 8.8, and 6.8, respectively. It is interesting to note that both men and women tend to underestimate the highest score and overestimate the lowest score, suggesting some degree of conservatism in beliefs.

Men and women also form indistinguishable beliefs about the decisions of others to join the pool of candidates. Although subjects of both genders tend to underestimate the number of volunteers and candidates by a slight amount, there are no statistically significant differences between their estimates of the number of other group members who are willing to be considered for the representative. There are no differences in the implied average scores either, except in the *CNO* treatment. In general, we also find that beliefs appear to be responsive to our manipulation of the selection mechanism. For example, the mean number of expected volunteers or candidates and their average scores are higher in the treatments without costs and benefits than in the treatments with costs and benefits, as payoff maximization implies. This suggests that subjects not only recognize the role that costs and benefits play in the willingness to represent one's group but that they recognize that other subjects' actions will depend on them as well.

The results of our incentivized belief elictation task provide direct evidence that women's election aversion cannot be attributed to underconfidence in their Addition Task ability, a finding that contrasts with the extant literature's conventional wisdom that women do not run because they do not feel qualified. Our results show that it is *not* underconfidence in ability to do the task for which the election is held that explains women's greater election aversion. This corroborates our claim that the equal rates of volunteering in Part 2 decisions indeed stem primarily from parity in relative confidence in abilities. Of course, there remains some possibility that beliefs might still play a role. Even if men and women have similar beliefs, they might be differentially responsive to those beliefs. We will address this in the regression analysis after we examine whether there are differences in risk preferences in the context of volunteering choices.

Risk Preferences. Recall that we designed our second incentivized measurement task so that subjects faced a series of risky choices with financial incentives identical to the decision to volunteer. Subjects made nine choices in which Option A, the riskier choice, is equivalent to a subject with a score of 10 randomly selecting group members with scores of x and $x+10$ as the representative, with $x \in \{1, \ldots, 9\}$. Option B, the safer choice, is equivalent to the decision to volunteer, in which there is an equal probability of being selected oneself, selecting the member with a score of x, and selecting the member with a score of $x + 10$. This option is less risky because increasing the probability of selecting an intermediate value reduces the variance in the outcome.

Table 4 shows the percentage of men and women who chose the safer Option B for each of the nine choices. For both men and women, we find that as x increases, the proportion choosing Option B decreases. This pattern is consistent with expected payoff maximization, as the expected net benefits of choosing Option B decrease in x, with those benefits being positive for $x < 5$ and negative for $x > 5$. Not surprisingly, the fact that 75% of men and 82% of women choose the safer option when $x = 5$ (when A and B are equal in expected value) suggests that both genders exhibit some degree of risk aversion.

However, we also see that women are more likely to choose the safer option than men for values of x above 6, and all of these gender differences are statistically significant. By the time $x = 9$, when the expected benefit of choosing Option A is greatest, the proportion of men who chose Option B drops to 19% while the proportion of women who made the same choice is more than twice as high, at 42%. Thus, our results strongly suggest that, with respect to the risk involved with entry decisions, women indeed exhibit greater risk aversion

Table 4: Lottery choices

	Men	Women	p value
Choice 1	91%	93%	0.57
Choice 2	88%	88%	0.96
Choice 3	86%	87%	0.79
Choice 4	88%	87%	0.87
Choice 5	75%	82%	0.20
Choice 6	52%	61%	0.16
Choice 7	27%	42%	0.03
Choice 8	21%	41%	< 0.01
Choice 9	19%	42%	< 0.01
Total number of safe choices	5.5	6.2	< 0.01
N	198	202	

Note: p values are for two-tailed difference in means tests

than men. Whether or not these differences in risk preferences are responsible for differences in election aversion remains to be seen.

Regression Analysis. The purpose of our regression analysis is to investigate more carefully how the decision to enter the pool of potential representatives depends not only on changes in the selection mechanism, but also on task ability, beliefs, and risk preferences. Our strategy is to estimate a series of separate models for men and women and then to conduct specification tests (i.e., "Chow tests") for the the equality of coefficients across estimates from the gender-specific subsets of data, as appropriate. In other words, what we are interested in is understanding how each factor (electoral environment, task ability, etc) affects the decision to enter and whether different factors are more important for one gender than another.

As we did for the analysis of the Experiment 1 data, we stack the data so that the dependent variable is the *Choice* to represent so that there are two observations for each subject (one for the choice to volunteer and one for the choice to be a representative). Thus, we can estimate both the between- and within-subjects effects within the same model, clustering the standard errors by individual.

The first model specification provides us with baseline estimates of the effects of varying the selection mechanism on the probability of entering while controlling for task ability. The specification includes a subject's *Score* on the Piece Rate task and set of dummy variables for the electoral environment. The baseline (omitted) case is the decision to volunteer in the *CCB* treatment. *No Cost* is 1 for the *CNO* and *TNO* treatments for both the Part 2 and Part 3 decisions. *Election* is 1 for Part 3 (candidate) decisions. *No Cost Election* is 1 for the Part 3 decision if the treatment is *CNO* or *TNO*. *Truth Election* is 1 for the Part 3 decision if the treatment is *TCB* or *TNO*. The results are shown in Table 5.

The results for the first model are consistent with our interpretation of the aggregate results. Men and women appear to be equally sensitive to task ability and, when controlling for this ability, women are less likely to run than men. Thus, even when we account for task ability, women appear to be election averse. Although the effect of making elections truthful is positive, it is not statistically significant, which suggests that making elections more truthful is not sufficient to mitigate election aversion. However, the combined effect of truthful elections and the removal of costs and is statistically significant when controlling for task ability.[16]

In the second specification, we add belief and risk aversion measures to the model. We include the two summary measure of beliefs suggested by our theoretical analysis. *Number Willing* is a measure of the expected competition for representative. It is the number of other group members that a subject expects to be in the pool of potential representatives (the number of volunteers if the observation corresponds to the Part 2 decision and the

[16]The combined effect is the sum of the coefficients for *No Cost*, *No Cost Election*, and *Truthful Election*. The sum is 0.76 with a standard error of 0.39, which is statistically significant at the 0.05 level.

Table 5: Probit analysis of entry choices in Experiment 2

	Men			Women		
Score	0.17** (0.05)	0.32** (0.06)	0.34** (0.07)	0.19** (0.04)	0.29** (0.07)	0.24** (0.11)
No Cost	0.35 (0.32)	0.29 (0.34)	0.8 (1.10)	0.32 (0.29)	0.24 (0.32)	-1.56 (1.44)
Election	-0.08 (0.32)	0.02 (0.37)	0.11 (0.83)	-0.76** (0.31)	-0.55 (0.35)	-3.06** (1.36)
No Cost Election	-0.2 (0.36)	-0.09 (0.40)	0.03 (1.06)	0.02 (0.32)	-0.05 (0.38)	4.03 (1.66)
Truth Election	0.03 (0.31)	-0.06 (0.34)	-0.67 (1.08)	0.42 (0.27)	0.28 (0.30)	1.38 (1.21)
Safe Choices		-0.09 (0.08)	-0.09 (0.08)		-0.09 (0.07)	-0.09 (0.07)
Number Willing		0.17* (0.10)	0.17* (0.10)		0.60** (0.11)	0.61** (0.12)
Average Willing		-0.23** (0.07)	-0.22** (0.07)		-0.18** (0.05)	-0.19** (0.05)
No Cost x Score			-0.06 (0.11)			0.2 (0.16)
Election x Score			-0.01 (0.08)			0.25* (0.14)
No Cost Election x Score			-0.01 (0.10)			-0.43** (0.18)
Truth Election x Score			0.06 (0.11)			-0.1 (0.12)
Constant	-0.91 (0.51)	0.33 (0.88)	0.11 (0.99)	-1.28** (0.42)	-1.04 (0.72)	-0.43 (0.96)
Log likelihood	-86.92	-78.34	-77.82	-109.49	-86.24	-83.23
N	198	198	198	202	202	202

Notes: * $p < 0.10$, ** $p < 0.05$. Standard errors clustered by subject in parentheses.

number of candidates for the Part 3 decision). *Average Willing* is the implied belief about average ability of the pool. It is the average expected volunteer's score if the number of volunteers is non-zero in Part 2, the average expected candidate's score if the number of candidates is non-zero in Part 3, and the average group score if the belief about the number of volunteers or candidates is 0. *Safe Choices* is the number of times that the subject chose Option B in the Lottery Choice task; higher numbers of safe choices indicate greater risk aversion.

When we add beliefs and risk measures, the magnitudes of the coefficients for the selection mechanism variables diminish slightly but remain qualitatively similar. For both men and women, removing costs increases both the probability that men and women volunteer and run; it decreases the probability that women run in any electoral environment; and it increases the probability that women run in elections with truthful campaigns. However, none of these coefficients are statistically significant (perhaps due to the small sample size we currently have for each treatment and gender).

The estimates for the second specification also show that beliefs affect the decision to enter although risk aversion does not. Although the coefficients for *Safe Choices* are negative for both men and women, they are close to zero and neither one is statistically significant. Thus, contrary to what intuition might suggest, the evidence instead demonstrates that risk aversion plays little part in subjects' decision calculus. In contrast, the coefficients for *Average Willingness* are negative and statistically significant. This is consistent with the predictions of the theoretical analysis: When subjects believe that the pool of volunteers or candidates is of higher quality, they are less likely to enter. It is also worth noting that the magnitudes appear to be similar across gender. (The F test, or Chow test, cannot reject the hypothesis that the two coefficients are equal, $\chi^2_{(1)} = 0.4$ and $p = 0.52$.)

The coefficients for *Number Willing* are also statistically significant, but positive. The directions of these latter coefficients, however, are inconsistent with our theoretical expectations: Entry decisions should be negatively related to the number of other group members in the pool because increasing competition reduces the expected benefit from running. Thus, it is somewhat puzzling that we find subjects to be more likely to enter when they think others are more likely to enter as well. We speculate that this might be because *Number Willing* captures beliefs about descriptive social norms (e.g., Cialdini, Reno and Kallgren 1990, Gerber and Rogers 2009) or the social desirability of representing others rather than expectations about the intensity of competition. With respect to this factor, we also find sizable gender differences. (The difference is statistically significant, as the Chow test can reject the equality of the coefficients; $\chi^2_{(1)} = 7.57$ and $p < 0.01$.)

In the third specification, we allow subjects' level of responsiveness to task ability to vary across selection mechanisms and treatments by interacting each of the dummy variables with *Score*. As we found in Experiment 1, there appear to be gender differences in the responsiveness to task ability. For men, all of the coefficients for the interactions are close to zero and none of them are statistically significant. Thus, the relationship between ability and the probability of entering appears to be the same for men regardless of how the representative is selected. For women, however, responsiveness to task ability does appear to be sensitive to the details of the selection mechanism. In contrast to the results from Experiment 1, moving from the random selection to the election mechanism appears to increase women's responsiveness to their task ability, as the coefficient for the interaction *Election* $\times$ *Score* is positive and statistically significant. But removing costs and benefits from the election treatments appears to restore the relationship between task ability and candidate entry to the same level as the responsiveness of the volunteering decision. (The sum of the interactions *Election* $\times$ *Score*, *No Cost* $\times$ *Score*, and *No Cost Election* $\times$ *Score* is 0.02 with a standard error of 0.14.)

Finally, we note that the full Chow test for structural differences between men and women does suggest that the parameter estimates differ between genders ($\chi^2_{(12)} = 21.03, p = 0.05$). Testing the equality of coefficients individually, however, points to the main differences being in the *Election* and *Number Willing* coefficients. Thus, save for the effects of these two factors, the decision-making of men and women appears to be substantially similar.

Campaigns and Candidate Quality

How well do elections select the *best* representative? What factors might hinder voters in their effort to select them? One of the benefits of the laboratory setting is that assessing this question is straightforward. We know who the best representatives are: They are the ones who perform the addition task with the greatest success. We can also compare differences in campaign behavior and informativeness of elections in our different electoral environments.[17]

One potential factor that might hinder voters in their quest for the best candidate is the campaign itself. In our experiment, the simple 140-character message comprises the entirety of the campaign. Table 6 provides an outline of the content of those messages. Notably, women were much more likely to be vague in two of the three treatments. Vague messages involved messages about task ability without mentioning specific numbers. They included statements like “I’m great at math!!!!!!!!!!!” or “I think I would be a great representative because Math is my strongest subject & I strive to succeed.” Men tended to be more specific in their campaign messages, making numeric claims such as “In Part 2, I answered 20 correctly with 0 incorrect” or “Hey guys. I got 15+ right for the last 2 rounds and I’m a math major... haha I got this I promise.”

Table 6: Campaign message characteristics

	Experiment 1		Experiment 2 - CCB		Experiment 2 - CNO	
	Men	Women	Men	Women	Men	Women
Vague Messages	7%	50%	20%	20%	7%	50%
Numeric Messages	83%	42%	67%	60%	73%	50%
Exaggerated Messages	43%	43%	33%	10%	7%	10%
Average Exaggeration	5.6	4.3	5.8	5.0	1.0	1.0
N	42	26	15	10	15	10

[17]Keeping in mind, of course, that the sample of candidates and messages are obviously biased due to the self-selection that is the main focus of our study.

Yet, as is also clear from Table 6, specific messages were not always informative. Men and women—provided they actually made a specific claim—both tended to exaggerate how well they did, particularly when there was a benefit to winning votes. This is rather surprising given that there is little or nothing to be gained from exaggeration. It is certainly true in the *CNO* treatment (without benefits of winning). Even when there are some potential gains from winning the election (at most \$3 in Experiment 1 and \$1 in Experiment 2), exaggerating creates welfare losses for the entire group (including the lying candidates themselves) if doing so causes the group to select the wrong candidate.[18] But even then, all subjects know the message is cheap talk, and rational voters should discount such messages accordingly. There is no reason to exaggerate because you are unlikely to be believed in any event.

But Table 7 shows that at least some of the time, exaggerating works: Subjects voted for the highest *claim* 81 percent of the time in Experiment 2's *CCB* treatment, when the motivation to mislead is higher. These are low-information environments, however, so voters may be using any possible information they have when making a choice, despite

Table 7: Voting behavior and election results

	Experiment 1	Experiment 2			
		CCB	CNO	TCB	TNO
Vote self	83%	68%	60%	59%	38%
Vote highest claim	47%	81%	93%		
Vote highest candidate	36%	53%	53%	85%	82%
Best member elected	54%	50%	50%	75%	83%
Best candidate elected	50%	63%	63%	92%	93%

Notes: Votes are percentages of total votes; election results are percentages of groups

[18] The incentive to select the best representative is stronger than the incentive to win the election. Although a gain of \$3 is equivalent to a large difference of 6 sums in Experiment 1, a subject can gain this amount from winning only if *every* group member votes for him or her. In contrast, if there are other candidates and every candidate votes for him or herself, the number of expected votes is likely to be in the range of 1-3, which implies an expected benefit of winnning of at most \$1. This amounts to a difference in scores of 2. Thus, if the best candidate's score is known or expected to be h, then a candidate with a score of x prefers to *lose* the election to the best cndidate if $h > x + 2$. In other words, exaggeration is only rational for very high scoring candidates; it is almost never rational for low scoring candidates.

knowing the information is useless. This tendency also has important implications for our normative questions about selecting the best representative. If some people are willing to lie, and most people are willing to believe their lies, selecting the highest-quality candidate becomes most difficult. We strongly suspect that the noisiness of the campaign environment is, to a large extent, an important source of election aversion. Perhaps women are averse to elections because they don't want to lie, or on the other side of the same coin, they fear their honest statements of their qualities will not believed through the campaign babel spewed by others. Indeed, these fears are not unfounded, as groups are much more likely to select the best candidates—and thereby more successful in selecting the best representatives—in the truthful campaign treatments than in the chat campaign treatments.

Conclusion

According to traditional democratic theory, elections are supposed to be the catalyst for proper representation. They are what encourages representatives to translate their constituents' interests into policy outcomes. But we also expect people—potential candidates—to differ in their ability to perform the tasks of the representative well and for elections to select those candidates who truly have the best ability to do so. Our analysis indicates that this may not be the case. Ideally, we would hope that election aversion would be inversely related to task ability: Those worst at the task of representing would be the same people who would be most averse to running for election to perform the task for the group. Alas, this is not the case.

Instead, election aversion is distributed across people in a way that is much more highly correlated with gender than with task ability. Notably, both men and women become less sensitive, we find, to task ability in an electoral, as opposed to a volunteer, environment. Yet the decrease in sensitivity is dramatically higher in women than in men. Of course, this pattern has important normative implications for women's representation which we do not intend to minimize. But even beyond issues of descriptive representation, we contend that fewer women candidates translates directly into lower of quality representation and of representative government, as less qualified representatives attempt to make policy decisions that highly qualified people—who did not even choose to run—would be better able to make. The relationship between voters and their representatives requires high quality electoral candidates, but election aversion seems to preclude their availability.

But getting back to the issue of women specifically, gender-based election aversion may play a significant role in the ability, or lack thereof, of Congress to perform its essential functions. Research in behavioral economics and psychology indicates that having more women in a group of decision-makers fundamentally alters how that group performs. Men tend to suffer from a "mythical fixed pie bias" that causes them to see negotiations as a zero-sum game (Babcock and Laschever 2003) and to be more confrontational in negotiations (Kimmel, Pruitt, Magenau, Konar-Goldband and Carnevale 1980), whereas women tend to be more cooperative (Walters, Stuhlmacher and Meyer 1998) and are concerned more with sharing information with those with whom they have negotiated (Helgesen 1990). These differences in how women and men interact may cause groups with more women to be "smarter" (that is, more effective at collective decision-making) than groups with fewer women (Woolley et al. 2010). How might the recent fiscal cliff negotiations have worked differently if Congress were peopled with more representatives who saw negotiation as a means to find tractable solutions in the common interest, rather than opportunities to dominate their foes? Would we remain a nation that operates, as President Obama expressed in his 2013 State of the Union address, by "conducting its business by drifting from one manufactured crisis to the next"?

The current project does not attempt to offer an answer to that question, but it does present a potential roadmap for finding out. Specifically, our results provide direction for those practitioners who seek to decrease the deleterious effects of election aversion on the emergence of women candidates. In contrast to what other research might suggest, potential candidates do not need convincing that they can do the job—they know when they can, and respond correctly to their own task ability. But they do so only when that response does not require the traditional difficulties that come with running for office. Our results in Experiment 2 clearly point to two of those difficulties: paying the costs of running as a candidate paired with substantial uncertainty about the efficacy of messages that convey to voters that women are qualified.

Not only have we identified election aversion as an important behavioral phenomenon, but we have also discovered a remedy for curtailing its undesirable consequences. Election aversion is at its greatest for women either when elections are high-stakes *or* when there inadequate mechanisms by which to report qualifications to voters. Men show dramatically lower levels of election aversion: High stakes and low information environments deter them much less frequently.

In other words, women know they are qualified. But what they may be unsure about is how to convince voters of that and how to do that convincing in a more cost-effective manner. Organizations like Emily's List, that provide funds for women candidates in the early stages of election, may help mitigate the effects of campaign costs on election aversion. Similarly, organizations might pledge assistance to women candidates in helping to convince others of their qualifications. If women know they will be judged on their true qualifications and they are not worried about the burdens and the hefty tolls that campaigning extracts, election aversion disappears. The truth, we find, convinces women to run.

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Theoretical Appendix

Let s_i denote individual i's ability or score.[19] For all i, we assume that scores are continuous random variables drawn independently from the same distribution with PDF $f(s)$. Let the probability that i volunteers be p_i, and let v_i be an indicator denoting the realization of whether i volunteers for office ($v_i = 1$) or not ($v_i = 0$). We use s, p, and v (without subscripts) to denote the appropriate vectors, and follow the usual convention that the subscript $-i$ denotes a vector of all other individuals excluding i. Note that in our model there are two forms of uncertainty: uncertainty about abilities represented by the distribution f and uncertainty about whether or not others run for office represented by p.

Assume that every individual has a Bernoulli utility function $u(x)$ that is increasing and weakly concave, $u'(x) > 0$ and $u''(x) \leq 0$, where x denotes i's monetary payoff. In our experimental setup, $x = s_i + 2r$, where r denotes the score of the selected representative.[20] For convenience, we will let μ_i denote i's expected utility from the event that another member j is elected with certainty.

$$\mu_i = \int u(s_i + 2s_j) f(s_j) ds_j \tag{1}$$

We begin by assuming that i is uncertain only about others' scores but not about who runs. This simplification helps to understand the basic elements of the decision to run for office in our framework as well as the role that risk aversion might play in the decision. Formally, this case corresponds to the assumption that $p_i \in \{0, 1\}$ for all i and implies that we can omit notation for p_i since $v_i = p_i$. We also assume that i knows her own score with certainty, which nevertheless still allows i to be uncertain about her *relative* ability. Our first insight is that i's decision rule boils down to a comparison of her own ability with the distribution of others' ability.

Proposition 1. *Given the decisions of other members v_{-i}, member i prefers to volunteer if and only if she would also prefer being selected as the group's sole representative with certainty to the selection of any other group member whose ability is unknown.*

Proof. If i does not volunteer, her expected utility is

$$EU_i(v_i = 0) = \frac{1}{n} \sum_{j \neq i} v_j \mu_i = \mu_i \tag{2}$$

where $n = \sum_{j \neq i} v_j$ is simply the number of other members who volunteer. If i instead volunteers, her expected utility is

$$EU_i(v_i = 1) = \frac{1}{n+1} \left(u(3s_i) + \sum_{j \neq i} v_j \mu_i \right) = \frac{u(3s_i) + n\mu_i}{n+1} \tag{3}$$

[19] The notation in this Appendix might differ from the main text of the paper, as it is from a previous version of the paper and we have not updated it.

[20] Technically, $x = \frac{s_i + r}{4}$, but we can omit the denominator since doing so simply amounts to a rescaling of the scores s_i.

Individual i will prefer to volunteer if $EU_i(v_i = 1) \geq EU_i(v_i = 0)$. Substituting the right-hand sides of (2) and (3) into the inequality and algebraic manipulation yields

$$u(3s_i) \geq \mu_i \tag{4}$$

$\square$

Note that as (2) indicates, it does not actually matter how many other members choose to volunteer. This is because i knows only that all s_j ($j \neq i$) are independently and identically distributed; the number of volunteers does not affect the distribution of outcomes.

If i is risk neutral, then it is appropriate to substitute expected scores for expected utility in the decision rule ($\mu_i = 3s_i$ and $EU_i(v_i = 0) = s_i + 2E[s]$). It then follows that i's decision rule amounts to a comparison of her own score with the expected *score* of the other volunteers.

Corollary 1. *Given the decisions of other members v_{-i}, if member i is risk neutral, she prefers to volunteer if her own ability exceeds the expected ability of the other volunteers; that is, $s_i \geq E[s]$.*

In other words, when there is no uncertainty about who volunteers, the decision rule that risk neutral members involves a simple cutoff: members prefer to volunteer if they are above the average ability of the other volunteers.[21]

Although risk aversion changes the exact value of the cutoff, it does not dramatically affect the basic nature of the decision rule. To see why, note that the decision rule in (4) can be rewritten using a multiple of the certainty equivalent, $c = \frac{1}{3}EU_i(v_i = 0)$: volunteer if $s_i \geq c$. The cutoff rule for risk neutral preferences described in Corollary 1 is simply a special case of the certainty equivalent form of the cutoff rule with $c^N = E[s]$. For an increasing utility function representing preferences that exhibit risk aversion, the certainty equivalent is well-known to be lower than under risk neutrality, $c < c^N$ Thus, i's decision rule involves a lower cutoff than that of an otherwise identical agent with risk neutral preferences.

Corollary 2. *Given ability s_i and others' decisions v_{-i}, member i will volunteer if $s_i \geq c$ where $c \leq c^N$. If i's preferences exhibit risk aversion, then $c < c^N$.*

To provide a clearer picture of the logic underlying this somewhat counterintuitive result, consider an example in which only the best and worst members volunteer (with abilities b_i and w_i, respectively) and where i's ability is exactly the midpoint between them. If i is risk neutral, she will be indifferent between volunteering and not volunteering because the expected score of a randomly selected representative remains the same (i.e., her score s_i equals the cutoff $\frac{b_i+w_i}{2}$. If she is risk averse, however, she prefers to volunteer because, even though volunteering leaves the expected score unchanged, it lowers the probability of extreme scores and raises the probability of a moderate score, thereby *reducing the variance* of the outcomes.

[21] Although Nash equilibrium is an unrealistic benchmark, with complete information about others' scores, in any Nash equilibrium, only the highest ability members volunteer.

Importantly, our analysis of the effects of risk aversion on the decision to volunteer has two implications for our experiment. First, every group member who believes their ability is above average should choose to volunteer *regardless of their degree of risk aversion.* Second, below average members will be willing to volunteer if they are sufficiently risk averse; correspondingly, greater risk aversion implies a lower average quality of volunteers.

When we generalize the model to allow each group member to choose non-degenerate probabilities of volunteering, $p_i \in [0,1]$, the decision rule described in Proposition 1 continues to hold. The only difference is that the proof is slightly more complicated.

Proposition 2. *Given the probabilistic volunteering choices of other members, p_{-i}, member i prefers to volunteer if and only if she would also prefer being selected as the group's sole representative with certainty to the selection of any other group member whose ability is unknown.*

Proof. For any realization of volunteering choices, let k be a random variable that denotes the number of volunteers (other than i). (Although $\Pr(k)$ is a function of p_{-i}, its exact form is immaterial to the proof.) Recall from the proof of Proposition 1 that for any number of volunteers, i's expected utility is μ_i. We can then write i's expected utility from not volunteering as

$$EU(v_i = 0) = \sum_{k=0}^{4} \Pr(k)\mu_i \tag{5}$$

Since μ_i is a constant, the expected utility reduces to μ_i as before.

If i volunteers, her expected utility is

$$EU(v_i = 1) = \sum_{k=0}^{4} \Pr(k) \left(\frac{u(3s_i) + k\mu_i}{k+1} \right) \tag{6}$$

Comparing these two expressions and rearranging the resulting inequality, i prefers to volunteer if

$$\sum_{k=0}^{4} \Pr(k) \left(\frac{u(3s_i) - \mu_i}{k+1} \right) \geq 0 \tag{7}$$

Because $\Pr(k)$ and $k+1$ are always positive, the sign of the sum on the left-hand side of the inequality depends only on the sign of $u(3s_i) - \mu_i$. Thus, i prefers to volunteer if and only if $u(3s_i) - \mu_i$, which is the condition identical to that in Proposition 1. □

Running for Office

The decision to run for office (in the election treatment) is potentially more complicated. The choice may depend on the overall shape of the distribution $f(s)$ (not just its mean), beliefs about the number of other candidates who decide to run, and additional considerations stemming from strategic campaigning. To make our initial analysis tractable, we make several simplifying assumptions. We restrict our attention to the case of linear utility, $u(x) = x$. We also consider two alternative assumptions about the nature of campaigns with two or more candidates. On one extreme, campaigns may be completely uninformative with respect to candidates' abilities. This would be the case if campaigning involves "babbling" messages. On the other extreme, campaigns may be informative in that group members may be able to learn which candidate has the highest ability. This might be because candidates truthfully reveal their abilities or, even if they exaggerate their abilities, they exaggerate by equal amounts.

In this section, we define the monetary payoff as $x = \frac{1}{4}s_i + \frac{1}{2}s_j + \nu - 2$, where ν is the number of votes received by i in the election. We also let r_i be an indicator denoting i's decision to run ($r_i = 1$) or not ($r_i = 0$). As we did in our initial analysis of volunteering, we restrict our attention to the analysis of i's best response to pure strategies of the other players. That is, we analyze i's decision as if she knew exactly how many other members volunteered, which we denote by $k = \sum_{j \neq i} r_j$.

Running Alone

If there are fewer than two candidates, then it is irrelevant whether or not campaigns are informative because there will be no campaign or election. If no candidates run, then the representative will be selected at random from the entire group (including i) but will receive no electoral benefits. Thus, i's expected utility if there are no candidates is

$$EU_i(r_i = 0|k = 0) = \frac{1}{4}s_i + \frac{1}{2}\left(\frac{1}{5}s_i + \frac{4}{5}E[s]\right) \tag{8}$$

If i decides to run unopposed, she automatically receives 5 votes and wins for sure. In this case, her expected utility is

$$EU_i(r_i = 1|k = 0) = \frac{3}{4}s_i + 3 \tag{9}$$

Solving for the cutoff rule, she runs for office if

$$s_i \geq E[s] - \frac{15}{2} \tag{10}$$

The decision to run continues to take the form of a cutoff rule, but the cutoff is well below the average ability. Not surprisingly, if there are no other candidates, the lure of receiving a lump sum of benefits from winning office looms large, drawing in a much poorer set of candidates than does a random selection rule. Only the worst candidates will stay out if they expect no one else to run.[22]

Uninformative Campaigns

If campaigns are uninformative, then we can treat the electoral process as one of purely random selection. We would expect candidates who run to vote for themselves (to ensure themselves a minimum benefit of $\nu = 1$) and for other group members to randomize their votes uniformly over the set of candidates.

[22]Except in very unlikely groups where *every* member's realized ability turns out to be below the cutoff, there clearly will not be a Nash equilibrium in which no one runs for office.

If $k = 1$ and i does not run, her expected utility is

$$EU_i(r_i = 0|k = 1) = \frac{1}{4}s_i + \frac{1}{2}E[s] \tag{11}$$

If she runs, she will receive $2\frac{1}{2}$ votes on average (one from herself and in expectation receives $\frac{1}{2}$ of the remaining 3 votes). Thus, i expects to win half the time. Her expected utility is therefore

$$EU_i(r_i = 1|k = 1) = \frac{1}{4}s_i + \frac{1}{2}\left(\frac{s_i + E[s]}{2}\right) + \frac{5}{2} - 2 \tag{12}$$

It then follows that i prefers to run if

$$s_i \geq E[s] - 2 \tag{13}$$

As with the decision to run alone, the cutoff for running is below the mean $E[s]$, but it is not as far below because competition with another candidate decreases the expected benefits from winning the spoils of office.

Note that introducing direct costs and benefits of running for office (ν and the fixed cost of 2) means that an individual's decision to run can be thought of as comprising two components. One is the effect of running on the quality of the representative that is selected. This is entirely what drives the decision to volunteer in the previous section. When campaigns are uninformative this component of the decision to run is indeed identical to that in the decision to volunteer. The other is the expected benefits of winning office, which decreases as the number of candidates increases.

Proposition 3. *If campaigns are uninformative and i is risk neutral, then the minimum ability of members willing to run is increasing in the number of other candidates.*

Proof. For any $k > 0$, i's expected utility of not running is $\frac{1}{4}s_i + \frac{1}{2}E[s]$. If i runs for office, her expected utility is a function of k:

$$EU_i(r_i|k) = \frac{1}{4}s_i + \frac{1}{2}\left(\frac{s_i + kE[s]}{k+1}\right) + \frac{4-k}{k+1} - 1 \tag{14}$$

The fraction $\frac{4-k}{k+1}$ is the expected number of votes i will receive from non-candidates.

Setting up and rearranging the inequality implies that the cutoff rule is to prefer running for office if

$$s_i \geq E[s] + 4k - 6 \tag{15}$$

and it is apparent that the cutoff is increasing in k. □

Informative Campaigns

When campaigns are informative, the highest ability candidate will receive all of the votes of non-candidates and win the election.[23] Member i's decision to run for office will depend, to some extent, on the probability of being the highest ability candidate as well as on the

[23] For simplicity, we ignore the possibility of ties for highest ability.

distribution of the highest ability candidates. The latter distribution will be conditional on the number of candidates since more candidates implies more chances to select a candidate with very high ability.

Consider the case with one other candidate, $k = 1$. If i does not run, her expected utility is the same as in (11). If she does run for office, her expected utility can be decomposed into the expected utility from winning and the expected utility from losing, each weighted by their respective probabilities, $\Pr(\text{win})EU(\text{win}) + \Pr(\text{lose})EU(\text{lose})$. With one other candidate j, the probability of winning is $\Pr(s_i \geq s_j)$, the probability that s_i is higher than a randomly drawn score s_j from $f(s)$. This probability is $F(s_i)$, where $F(\cdot)$ is the CDF of the distribution. Finally, if candidate j wins, the expected score must be conditional on s_j being higher than s_i. Taking these facts into consideration, i's expected utility from running against a single candidate j can be written

$$EU_i(v_i = 1|k = 1) = F(s_i)\left(\frac{3}{4}s_i + 2\right) + (1 - F(s_i))\left(\frac{1}{4}s_i + \frac{1}{2}E[s|s \geq s_i] - 1\right) \tag{16}$$

Setting up and rearranging the appropriate inequality, we obtain

$$(6F(s_i) - 2) + F(s_i)s_i + (1 - F(s_i))E[s|s \geq s_i] - E[s] \geq 0 \tag{17}$$

This expression cannot be simplified any further without specifying the functional form of $F(s)$. Nevertheless, we can gain some insight by analyzing the expression and establishing sufficient conditions for i to run.

Note that the first term in parentheses, $6F(s_i) - 2$, represents the expected net benefits accrued directly from the election. This term is positive if $F(s_i) > \frac{1}{3}$. In other words, the potential for winning office provides a strong incentive for running, even for low ability individuals. A member's ability need only be higher than the lowest third of the distribution (thus needing only a 33% chance of winning) to expect positive direct benefits from running. This alone does not imply, however, that all such members will want to run if it is offset by the effect of lowering the quality of the elected representative.

The remaining terms represent the net effect of i's decision to run on the expected ability of the elected representative. Together, the middle two terms (those weighted by $F(s_i)$ and $1 - F(s_i)$) represent the expected ability of the elected representative; it is a weighted average of s_i and the expected value of all abilities greater than s_i. The terms in the weighted average suggests that running for office in an informative campaign environment can be beneficial in two ways. First, by running, i sets the minimum ability for an elected representative. Second, even if i loses the election, she benefits from the fact that a higher ability group member is elected in her place. Whether or not this weighted average exceeds the last term, $E[s]$, then establishes whether running for office raises or lowers the ability of the elected representative. If $f(s)$ is a symmetric distribution, $s_i \geq E[s]$ guarantees that i benefits both from the potential prospective spoils from office as well as from ensuring that the elected representative will be above average. Furthermore, the level of ability for which i is indifferent between the elected representative's expected ability and the unconditional expected ability must be less than $E[s]$, and from this it follows that the cutoff must also be less than $E[s]$.

Proposition 4. *If i expects only one other member to run, campaigns are informative, $f(s)$ is symmetric, and i is risk neutral, then (i) above average ability $s_i \geq E[s]$ is a sufficient condition for her to prefer running for office and (ii) the minimum ability for i to prefer running is strictly less than $E[s]$.*

Constructing i's expected utility for $k > 1$ proceeds in a similar fashion. Intuitively, because the highest ability candidate is always selected, the more candidates there are (the more draws of the random variable), the higher the expected ability of the representative. For example, if $k = 2$, then either i wins and the representative's ability is s_i or i loses and the representative's ability is $\max\{s_1, s_2\}$. This should have the effect of raising both i's expected utility from running and from not running, so it is unclear in general whether this should raise or lower the i's threshold for running. To gain additional insight, it is useful to impose an explicit functional form on $f(s)$, which we do next.

Numerical Example

We can compute explicit cutoff values if we make explicit assumptions about the distribution of ability $f(s)$ and agents' utility functions. Thus, for simplicity, we assume that s is uniformly distributed between 0 and 20. To analyze the possible effects of risk aversion, we compare linear utility $u^n(x) = x$ with negative quadratic utility $u^a(x) = -(x - 15)^2$. Table 8 presents the computed cutoff values for both utility functions across the three scenarios: volunteering (random selection), running with uninformative campaigns, and running with informative campaigns.

Uninformative Campaigns

If i does not run, then her expected utility is $E[u(x_j)]$, where $x_j = \frac{1}{4}s_i + \frac{1}{2}s_j$, just as in (2). If $k = 1$, provided that non-candidates cast their votes randomly, i is equally likely to receive any number of votes between 1 and 4. Her expected utility is

$$EU^N(k=1) = \frac{1}{4}u\left(x_i + 2\right) + \frac{1}{4}u\left(x_i + 1\right) + \frac{1}{4}E\left[u\left(x_j\right)\right] + \frac{1}{4}E\left[u\left(x_j - 1\right)\right] \tag{18}$$

where $x_i = \frac{3}{4}s_i$ and $x_j = \frac{1}{4}s_i + \frac{1}{2}s_j$.

In general, if there are k other candidates, we must account for the possible ways in which the remaining group members vote for i. If $k = 2$ and assuming a random tie-breaking rule, there is a $\frac{1}{9}$ probability that i wins 3 votes, a $\frac{2}{9}$ chance i receives 2 votes and wins, a $\frac{2}{9}$ that i receives 2 votes and loses, and a $\frac{4}{9}$ chance that i receives only 1 vote. If $k = 3$ and i runs, then there is only 1 remaining vote and i has a $\frac{1}{4}$ probability of receiving 2 votes and a $\frac{3}{4}$ probability of receiving only 1 vote (her own). Finally, if $k = 4$ and i runs, she will receive one vote (again, her own) with certainty while her probability of winning is $\frac{1}{5}$.

$$EU^N(k=2) = \frac{1}{9}u\left(x_i + 1\right) + \frac{2}{9}u\left(x_i\right) + \frac{2}{9}E\left[u\left(x_j\right)\right] + \frac{4}{9}E\left[u\left(x_j - 1\right)\right] \tag{19}$$

$$EU^N(k=3) = \frac{1}{4}u(x_i) + \frac{3}{4}E\left[u(x_j - 1)\right] \tag{20}$$

$$EU^N(k=4) = \frac{1}{5}u(x_i - 1) + \frac{4}{5}E\left[u(x_j - 1)\right] \tag{21}$$

Informative Campaigns

If campaigns are informative, computing i's expected utility involves dividing the support of the joint random variable and integrating over the region where i wins and where the other k candidates win. If $k = 1$, then the appropriate expressions for not running and running are

$$EU^A(r=0|k=1) = \int_a^b u(x_j - 1)f(s_j)ds_j \tag{22}$$

$$EU^A(r=1|k=1) = \int_a^{s_i} u(x_i + 2)f(s_j)ds_j + \int_{s_i}^b u(x_j - 1)f(s_j)ds_j \tag{23}$$

where a and b are the endpoints of the support of $f(s)$.

If $k = 2$, the expected utility of running is

$$EU^A(r=0|k=2) = 2\int_a^b \int_a^{s_j} u(x_j)f(s_k)f(s_j)ds_k ds_j \tag{24}$$

where the double integral accounts for values of s_j for which $s_j \geq s_k$; that is, we integrate over s_k from a to s_j and then over s_j from a to b. Since $f(s)$ is symmetric, the part of the expected utility due to j winning is identical to the expected from k winning, hence we multiply by 2.

When i runs, this is modified to take into account the possibility that i might win as well:

$$\begin{aligned} EU^A(r=1|k=2) = &\int_a^{s_i} \int_a^{s_i} u(x_i + 1)f(s_k)f(s_j)ds_k ds_j \\ &+ 2\int_{s_i}^b \int_a^{s_j} u(x_j - 1)f(s_k)f(s_j)ds_k ds_j \end{aligned} \tag{25}$$

For $k > 2$, the relevant expressions are:

$$EU^A(r=0|k=3) = 3\int_a^b \int_0^{s_j} \int_0^{s_j} u(x_j)f(s_\ell)f(s_k)f(s_j)ds_\ell ds_k ds_j \tag{26}$$

$$\begin{aligned} EU^A(r=1|k=3) = &\int_a^{s_i} \int_a^{s_i} \int_a^{s_i} u(x_i)f(s_\ell)f(s_k)f(s_j)ds_\ell ds_k ds_j \\ &+ 3\int_{s_i}^b \int_0^{s_j} \int_0^{s_j} u(x_j - 1)f(s_\ell)f(s_k)f(s_j)ds_\ell ds_k ds_j \end{aligned} \tag{27}$$

$$EU^A(r=0|k=4) = 4\int_a^b \int_0^{s_j} \int_0^{s_j} \int_0^{s_j} u(x_j - 1)f(s_m)f(s_\ell)f(s_k)f(s_j)ds_m ds_\ell ds_k ds_j \tag{28}$$

$$EU^A(r=1|k=4) = \int_a^{s_i}\int_a^{s_i}\int_a^{s_i}\int_a^{s_i} u(x_i-1)f(s_m)f(s_\ell)f(s_k)f(s_j)ds_m ds_\ell ds_k ds_j \tag{29}$$

$$+\,4\int_{s_i}^{b}\int_0^{s_j}\int_0^{s_j}\int_0^{s_j} u(x_j-1)f(s_m)f(s_\ell)f(s_k)f(s_j)ds_m ds_\ell ds_k ds_j \tag{30}$$

Table 8: Comparison of Cutoff Values

	Volunteering		Running for Office			
			Uninformative		Informative	
k	$u^n(x)$	$u^a(x)$	$u^n(x)$	$u^a(x)$	$u^n(x)$	$u^a(x)$
1	10	9.0	8	7.1	4.8	4.0
2	10	9.0	12	11.2	10.4	9.1
3	10	9.0	16	17.7	14.2	12.8
4	10	9.0	20	∞	19.7	16.0

Several patterns in the numerical example are noteworthy. The most interesting to us is that the cutoff values for informative campaigns are lower than for uninformative campaigns (holding k and risk aversion constant). This suggests that for any given ability, an individual would be more willing to run the more informative they believe the election to be. Second, for both uninformative and informative campaigns, the cutoff value is increasing in the number of other candidates k. We showed that this holds generally for the case of a risk neutral agent in an uninformative campaign, and the example suggests that the relationship may hold more generally. Intuitively, increasing the number of candidates should decrease the expected number of votes and the probability of winning. Third, the numerical example suggests that risk aversion implies only small decreases in the numerical cutoff values. In general, as we discussed above, risk aversion implies a greater willingness to run (in order to reduce the variance in outcomes).