Elections, Ability, and Candidate Honesty

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Abstract

An important function of elections is to select the best representative, a task facilitated when candidates are honest about their qualifications. But are they? To what extent do candidates' claims depend on the alignment of incentives between themselves and voters? We conduct an incentivized laboratory experiment in which candidates can choose to be honest or to exaggerate, varying the benefits of winning office. We find that strong office motives clearly induce exaggeration and, surprisingly, that only about half of laboratory candidates tell the truth even when incentives are completely aligned. We show that the prevalence of lying in elections results not from impure (e.g., Machiavellian) motives, but rather as a rational response to the expectation that other candidates will lie. Although honesty and integrity are desirable virtues in elected officials, our experiment suggests that the nature of electoral processes can make dishonesty endemic to the democratic selection of leaders. (JEL Codes: D72, D82, C92)

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1 Introduction

Governing is a skill much like any other: Politicians and public officials vary in how much of that skill they possess. Some are effective at solving problems and building consensus, while others are committed to the public good and inspire trust and confidence. Great leaders have all of these characteristics, and bad ones lack them. An important function of democratic elections is to select the best, most virtuous, leaders (Besley 2005). And indeed, democratic elections seem to do just that (Alt, Bueno de Mesquita and Rose 2011, Besley, Montalvo and Reynal-Querol 2011, Besley and Reynal-Querol 2011, Buttice and Stone 2012, De Paola and Scoppa 2010, Galasso and Nannicini 2011, Hirano and Snyder 2014, Jones and Olken 2005, Mondak 1995, McCurley and Mondak 1995, Stone, Maisel and Maestas 2004).

The extent to which elections facilitate the selection of the best leaders depends, in large part, on the ability of voters to discern good candidates from bad.[1] The voter's problem is made complicated, however, by the fact that candidates do not always communicate their qualifications or accomplishments honestly. Candidates in the United States, for example, have a tendency to embellish their military records. In one notable case, Representative Bruce Caputo withdrew from challenging Daniel Patrick Moynihan's re-election to the Senate when the media exposed inconsistencies about his claim to have served in Vietnam.[2] However, similar exaggerations by Richard Blumenthal did not impede his election to the Senate in 2010.[3] Governors, too, inflate their accomplishments by varying degrees: Jeb Bush "cut taxes by $19 billion" in Florida, Mike Huckabee "raised average family income by 50%"

[1]Career decisions and self-selection into politics are also important factors that affect quality selection. For example, see Caselli and Morelli (2004), Gagliarducci, Nannicini and Naticchioni (2010), and Messner and Polborn (2004) for theoretical models. Also see Kanthak and Woon (2015) for an experimental investigation of self-selection in the context of gender and diversity.

[2]Caputo claimed to have been drafted and to have served as lieutenant in the Army, when he had instead avoided military service by working as a civilian at the Pentagon. The inconsistency was pointed out to the media by Tim Russert, then Moynihan's campaign manager. See Maurice Carroll, "Caputo Quitting Race for Senate Over Inaccuracy" (*New York Times*, March 9, 1982) and Richard Stengel, "Tim Russert: A Man This Good is Hard to Find" (*Rolling Stone*, February 14, 1985).

[3]http://www.nytimes.com/2010/05/18/nyregion/18blumenthal.html

in Arkansas, and John Kasich "took the state of Ohio from an $8 billion hole...to a $2 billion surplus."[4]

Formal models of campaigns and elections demonstrate that office-seekers frequently have incentives to obfuscate: about their policy positions (Callander and Wilkie 2007), their opponents (Davis and Ferrantino 1996), and their plans for office (Corazzini et al. 2014).[5] Empirically, however, candidate honesty has received relatively little attention in the political science literature, largely because of the difficulty of collecting large-scale data on moments of obfuscation.[6] In overlooking candidates' strategic incentives to lie, scholars have ignored an important reason why voters would not bother to become informed. If there are no real means by which voters can discern truth from candidates' statements, then there is little reason to know what those statements are.

The central issue we address is one of strategic communication in which the electoral environment itself, not the moral failings of candidates, may be a significant source of lying behavior. Whereas the literature on political selection tends to discuss both honesty and competence as intrinsic characteristics of leaders (i.e., valence dimensions), we are interested in understanding honesty as a behavior that responds to incentives and beliefs, rather than as a fixed trait.[7] Of course, real world politicians may be intrinsically dishonest, as many people perceive them to be, but by turning to laboratory experiments we can minimize

[4]The *Washington Post* gave Bush's claim 3 Pinocchios (http://www.washingtonpost.com/news/fact-checker/wp/2015/10/07/jeb-bushs-misleading-claim-that-i-cut-taxes-by-19-billion-over-eight-years-fact-checker-biography/), while the website Politifact rated Huckabee's claim as "mostly false" (http://www.politifact.com/truth-o-meter/statements/2015/may/04/mike-huckabee/mike-huckabee-says-he-raised-average-family-income/) and Kasich's claim as "mostly true." (http://www.politifact.com/truth-o-meter/statements/2015/aug/06/john-kasich/kasich-i-took-state-ohio-8-billion-hole-2-billion-/).

[5]In Corazzini et al. (2014), campaign promises serve as commitments that may actually alter actions once in office despite being cheap talk. In Callander and Wilkie (2007), the extent to which some candidates' willingness to lie undermines elections is limited by the fact that voters recognize this problem and make appropriate inferences. Other models of lying and lying aversion in candidate positioning include Banks (1990), Kartik and McAfee (2007), and Huang (2010).

[6]The few observational studies of lying in politics we are aware of include Pfiffner (1999), Bucciol and Zarri (2013), Ringquist and Dasse (2004), and Armstrong-Taylor (2012).

[7]In other words, we distinguish between competence as a *trait* and honesty as a *behavior* in our analysis. When honesty is viewed as a trait, it may be correlated with and thus signal other qualities. For example, perceived honesty may signal competency, or alternatively, it may signal trustworthiness. As our design does not elicit perceptions of candidate honesty, we can address such issues directly.

intrinsic dishonesty and focus specifically on investigating how electoral context affects the strategic choices of candidates to hew to the truth or exaggerate. Under what conditions do candidates in elections campaign honestly? To what extent do elections and electoral incentives encourage candidates to exaggerate their qualifications?

We answer these questions using an incentivized laboratory experiment in which candidates can choose to be honest, to obfuscate, or to exaggerate. In our experiment, subjects complete a problem-solving (anagram) task, and their performance on this task serves as a measure of their quality, which is privately known. Subjects then choose campaign messages that either honestly convey information about their performance or distort their performance through ambiguity or exaggeration. Because these messages are the only information that voters have about candidates, the information transmitted in the election depends crucially on candidates' message strategies. We find that a substantial proportion of candidates lie, even when there is no private incentive to do so. Our analysis suggests, however, that such lies are not attempts to mislead voters. Instead, they are rational responses to their beliefs that other candidates will be lying. These findings allow us to reconceptualize lying on the campaign trail not as a moral failing of an unsavory political class, but as a strategic response to electoral politics.

The details of our experiment are as follows: In the baseline condition, candidates and voters are rewarded only for the quality of the elected leader. The strategic problem they face is purely informational and revolves around coordination. Given the complete alignment of incentives, honest campaigning transmits information accurately and facilitates the common goal of identifying the best representative. However, honesty is not necessarily the best policy—because our election game admits multiple equilibria, honesty is rational only if all other candidates are also honest. Exaggeration can be individually and socially rational for highly qualified candidates (given our communication technology) if they believe that other candidates, especially low quality candidates, are likely to exaggerate. Although theory provides some basis for forming expectations in our setting, experimental analysis is ideal for understanding how real people actually behave when there is tension between the virtue of honesty and the social good of selecting the most qualified leader.

We manipulate the benefit of winning the election to include a private bonus from winning in our treatment conditions. In these settings, the incentives for lying are much clearer. If messages are taken at face value, then lower quality candidates personally gain by exaggerating their qualifications at the expense of voter welfare. In addition, we vary whether the benefits of winning accrue solely to the individual candidate or to the candidate's group. This manipulation varies other-regarding or group-based interests while holding individual incentives constant, and it allows us to test whether candidates who may be reluctant to lie for selfish reasons might instead lie for social (albeit parochial) ones.

Our experimental study yields two key findings. First, we find that the degree of honesty in laboratory elections indeed varies with office incentives. When candidates stand to earn individual benefits of office, they are more likely to exaggerate than they are in the baseline condition. We find no difference between the individual and group bonuses in terms of overall exaggeration, but we do find a stronger relationship between task ability and exaggeration in the group bonus condition than in the individual bonus condition.

Second, and more surprisingly, we find that in the absence of office benefits—when candidates and voters share the common goal of selecting the best representative—only half of candidates are honest, while the other half exaggerates. In other words, candidates lie when deception provides no obvious, direct benefit. This finding is striking because in many laboratory experiments on communication, people exhibit an aversion to lying even if they are rewarded for doing so (Gneezy 2005, Gneezy, Rockenbach and Serra-Garcia 2013, Hurkens and Kartik 2009, Lundquist et al. 2009, Sanchez-Pages and Vorsatz 2007, Wang, Spezio and Camerer 2010).[8] Our results also stand in contrast to laboratory studies of elections that find that candidates and political decision makers refrain from pandering to voters

[8]Other studies of experimental sender-receiver games find that while senders are more than willing to exaggerate, they end up overcommunicating because they fail to conceal their information optimally (Cai and Wang 2006, Minozzi and Woon 2013).

(Landa and Duell 2014, Woon 2012, 2014). In these studies, laboratory politicians can gain sizable electoral rewards from exploiting voters' concern about selection under conditions of incomplete information, but instead they forgo opportunistic gains and act in voters' best interests. Whereas previous studies find that experimental subjects are reluctant to lie when incentivized, we find the opposite. In our baseline elections, there is no obvious incentive for candidates to lie, yet they do so anyway.

Given that there are multiple equilibria in our game, a plausible interpretation of our finding is that the level of candidate exaggeration we observe is consistent with instrumental rationality. That is, when candidates agree that elections should be informative, exaggeration is a rational best response when others are exaggerating as well. Hence, lying behavior need not betray the baser instincts of candidates engaged in politics as usual, but possibly reveals honest intentions tinged with a degree of strategic distrust of others. To investigate more carefully the source of candidate exaggeration, we take advantage of the measurement tools that the laboratory provides us. Specifically, we use an incentivized belief elicitation procedure and contrast beliefs against a personality-based explanation for lying. We find that exaggeration in the baseline can be explained primarily by beliefs: The probability of exaggeration increases significantly with the strength of a candidate's belief that the opposing candidate exaggerated. In contrast, "Machiavellian" personality traits (such as the desire for control or status, e.g., Dahling, Whitaker and Levy 2008) are unrelated to lying behavior. That we find a greater degree of lying than in the existing literature may be due to the specific strategic and competitive environment of electoral politics that features widespread expectations of dishonesty.

2 Experimental Procedures

In the experiment, subjects first completed a skill-based problem-solving task (anagrams), received individual feedback about their performance on the task, and then made a series of political decisions.[9] Task performance can be thought of as a laboratory analogue of policy-making skill or competence. This skill is valuable to all group members, but it is privately known only to each subject. The political decision in this experiment is the subject's choice of campaign message, and the type of message that a subject chooses affects the information conveyed to other subjects. Importantly, we vary the incentives for winning the election, and subjects choose their campaign messages under different conditions.

We conducted the experiment at the Pittsburgh Experimental Economics Laboratory with subjects recruited using standard procedures from the lab's general subject pool. There were 12 sessions with a total of 120 participants (61 women and 59 men, 10 unique subjects per session). Upon entering the lab, subjects were seated at computer terminals separated by partitions between them for privacy. All interactions between subjects took place through a computer interface programmed in z-tree (Fischbacher 2007). Although subjects could see the other participants in the experiment prior to being seated, they were randomly assigned an ID number through the computer interface and were identified in all their interactions with other subjects only by the ID number. After reading the instructions aloud, subjects were randomly placed in one of two groups. They did not know which other subjects were in their group, nor did they have any information about their fellow group members at the start of the experiment. Subjects were also informed that the experiment consisted of several parts, that we would randomly select one part for payment, and that they would receive written instructions before beginning each part. We did this so that the anticipation of later parts of the experiment would not influence choices in earlier parts.

The task we used in the experiment is a simple anagram task, which involves unscrambling letters to form words. Subjects received instructions about how to perform the anagram task and were allowed to practice with the graphical interface (shown in Figure 1). Each anagram consisted of five jumbled letters that could be rearranged to form a common word, with each set of letters having a unique solution (e.g., the solution to the anagram in

[9]The basic structure is similar to Kanthak and Woon (2015).

Figure 1: Sample screenshot of the anagram task

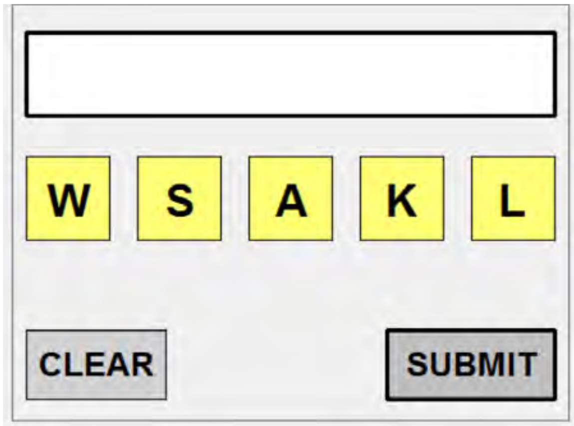


Figure 1 is "walks").[10] In Part 1 of the experiment, subjects had four minutes to solve as many of the anagrams as they could and were paid 75 cents for each anagram puzzle they could solve in the four minutes. (The payment incentivizes effort.) There were 40 puzzles in total and all subjects saw the puzzles in the same order.[11] Subjects could move to a new puzzle by submitting an incorrect guess and incorrect guesses did not count against their winnings. At the end of Part 1, subjects received individual feedback about the number of anagrams they solved correctly but did not learn how well any of the other subjects performed on the task.

In Part 2 of the experiment, subjects participated in a series of elections to select a representative and knew that they would complete the anagram task again after completing their election choices. Each set of elections involved a different set of incentives (explained below), and in each set, every subject would be a candidate in at most one election and a voter in every election. We elicited choices in a way analogous to the strategy method (Morton and Williams 2010, pp. 87-90). Specifically, we explained the rules for the first set of elections (Elections 1-5), subjects made their choices as the candidate in these elections,

[10]Detailed instructions can be found in the Appendix. Similar word-unscrambling or anagram tasks are used in Charness and Villeval (2009) and Shurchkov (2012).

[11]We chose 40 puzzles because the maximum score in a pilot study was 20 correct puzzles. While there might be some potential for censoring because subjects know the maximum possible score, such concerns are negligible. Only one subject had a score higher than 34 (it was 35) such that telling the big lie would reveal the message to be a lie.

then we explained the rules for a second set (Elections 6-10), and then subjects made their choices for the second set; only after all candidate made their choices did we hold the elections, and then subjects completed the four-minute anagram task again.[12] This order of events is crucial for preventing subjects from learning anything about the distribution of anagram scores as candidates or from learning anything more about their own task ability from completing the task again between elections. If we selected Part 2 for payment, then we also randomly selected one of the elections to count. Thus, each subject made choices as a candidate under different conditions, but in the end we would only select (at most) one representative and subjects completed the anagram task only once after the elections.

Each participant was a candidate in exactly one election in the first five elections (Elections 1-5), and was randomly matched with one candidate from the other group. As a candidate, each subject chose one of four possible campaign messages and knew that the message was all that other subjects would know about them during their election.[13] The message could be:

Ambiguous – "I solved a lot of puzzles"

Truthful – "I solved X puzzles"

Small Lie – "I solved $X + 3$ puzzles"

Big Lie – "I solved $X + 6$ puzzles"

where X is the true number of puzzles the subject solved in Part 1 of the experiment.[14] We did not use any of the terms "ambiguous," "truthful," "small lie," or "big lie" to describe the messages, but instead referred to them in abstract terms as "Message 1," "Message 2," and so on. Note that *all* subjects made a choice about which campaign message to use because

[12] There was an additional election, Election 11, but we do not analyze it in this paper.

[13] Candidates were randomly assigned letters, which were uncorrelated with group membership or ID numbers. In this way, subjects had no information about the identity or group membership of each candidate

[14] For example, if the subject solved 10 puzzles and chose the Big Lie, voters would see the message "I solved 16 puzzles."

every subject was a candidate in one of the elections.[15]

If we selected one of Elections 1-5 to count, we paid subjects 50 cents for each correct answer the representative provided and 25 cents for each of their own correct answers. Subjects received no bonus for winning the election, and we refer to this first set of elections as the *Performance Pay* (baseline) condition. In this condition, subjects have common incentives, regardless of their group membership, to identify and select the most competent representative.

In Elections 6-10, each subject was also a candidate in exactly one election and chose from the same set of campaign messages, but with one important difference in the incentive structure. In addition to the same performance incentives as in Elections 1-5 (50 cents for each of the representative's correct answers and 25 cents for each of the subject's own correct answer), the winning candidate received a bonus of $2.50 if we selected their election for payment. Thus, in Elections 6-10, there is a bonus for winning the election, which we can think about as an office-holding benefit.

We varied the exact nature of the bonus across sessions. In the *Individual Bonus* condition only the candidate who won received the $2.50 while in the *Group Bonus* condition all members of the winning candidate's group received the $2.50 bonus. We designed the different bonus conditions so that a candidate's individual benefit is constant across all sessions while the amount of the group's benefit varies by condition. From the perspective of a purely selfish, individual payoff-maximizing candidate, the two conditions are identical, but from the perspective of a candidate with pro-social motivations, the Group Bonus condition provides a stronger incentive for winning the election. By varying the bonuses in this way, we could see how tapping into other-regarding preferences might affect candidates' message

[15]It is important to note that our experiment does not include any means by which voters can confirm the veracity of candidates' claims. This is primarily for analytical reasons, as we wish to focus on the relationship between incentives and performance while minimizing the influence of other factors that might affect message choices (e.g., credibility and monitoring). Nevertheless, our design reflects the fact that in many real elections, the claims candidates make are, by their very nature, largely unverifiable. Such claims include, for example, that the candidate is "working hard to bring government money to the district" or promises of "new leadership" on complex issues (Tomz and Van Houweling 2009, Grimmer 2013, Grimmer, Westwood and Messing 2015).

choices.

3 Theoretical Model and Hypotheses

We begin our analysis by considering the strategic incentives candidates face in the Performance Pay condition (our baseline). In this condition, subjects earn twice as much for the elected representative's performance as for their own, so if we reasonably assume that subjects care only about their own monetary payoffs, there is a common incentive for members of both groups in the experiment to elect the best possible representative (i.e., the highest performing member regardless of group membership). That is, individual and group incentives are aligned. For purposes of exposition, let us also assume that a subject's score in the individual anagram task (Part 1) is a perfect indicator of their underlying task ability. Because these abilities are privately known, subjects can use campaign messages to share and transmit information, thereby making abilities public knowledge and allowing the electorate to correctly identify and elect the best representative.

We formalize these ideas in a simple theoretical model. The model captures the key features of our experimental design but, for analytical purposes, generalizes from the specific details of our parameterization. Following our experimental setup, there are two candidates, X and Y. Let the candidates' skill levels be denoted by s_X and s_Y, which are independent draws from a uniform distribution over the interval $[0, 1]$. Each candidate's skill is private information (they observe their own skill but not the other candidate's). To conserve on notation, we will also use $x = s_x$ and $y = s_y$ to denote the skill levels. Simultaneously, each candidate $i \in \{X, Y\}$ chooses a message $m_i \in [s_i, s_i + E] \cup \{\phi\}$, where $E > 0$. A message from the interval $[s_i, s_i + E]$ corresponds to a numerical claim about i's skill level, while the message ϕ corresponds to an ambiguous, non-numeric message. If candidate i chooses a numeric message, we can more conveniently characterize the message in terms of a level of exaggeration $e_i \in [0, E]$, where $e_i = 0$ denotes an "honest" message and $e_i = E$

denotes maximum exaggeration (i.e., the "big lie" in our experiment). In the baseline model, candidates and voters care only about the skill level of the winning candidates. Thus, we assume the common-value payoff is x if X wins the election and y if Y wins.

In order to focus on the behavior of candidates, we assume that voters are credulous and follow a simple behavioral rule of taking the candidates' messages at face value. If both candidates use numeric messages, then the candidate with the higher-valued message wins: X wins the election if $m_X > m_Y$ and Y wins if $m_Y > m_X$. Since x and y are continuous random variables, we ignore ties as they are zero-probability events. If candidate i uses the ambiguous message, then voters consider the message to be uninformative and, since they are risk neutral, treat the candidate as a random draw from $U[0, 1]$. If m_i is ambiguous while m_j is numeric, then the voter compares the numeric value m_j to the expected value $E[s_i] = \frac{1}{2}$, so j wins if $m_j > \frac{1}{2}$ and m_i wins if $m_j < \frac{1}{2}$.

Analyzing message strategies in this baseline case, with candidates who have common interests in finding the best representative, yields two main results.[16] First, except for a few knife-edge cases, ambiguity is never a best response. The intuition can be illustrated in the situation where i's opponent j is ambiguous. By being honest, i will either reveal herself to be above average (thus winning the election and receiving a higher than average payoff $s_i > \frac{1}{2}$) or below average (thus losing the election but receiving the average payoff $\frac{1}{2} > s_i$). By revealing information, honesty makes candidates and voters better off than ambiguity.

Second, candidate i's best response to any amount of exaggeration e_j by j is to match that exaggeration exactly. To see this, consider the perspective of candidate X, who wins the election if $m_X > m_Y$. When both candidates exaggerate, this condition corresponds to $x + e_X > y + e_Y$. Since y is unknown to X, we can rewrite this inequality as $x + \delta < y$ (where $\delta = e_X - e_Y$ is the difference in exaggeration) and then compute X's expected payoff as the piecewise integral

$$\int_0^{x+\delta} x dy + \int_{x+\delta}^1 y dy = \frac{1}{2} + \frac{x^2}{2} - \frac{\delta^2}{2} \,, \tag{1}$$

[16]For complete details of our analysis, see the Appendix.

where the integral on the left corresponds to x winning the election and the integral on the right to y winning. This is maximized when $\delta = 0$, which means that the optimal amount of exaggeration is $e_X = e_Y$. Thus, in our experiment, honesty is a best response to honesty, the small lie is a best response to the small lie, and the big lie is a best response to the big lie. It follows that there are multiple equilibria and that in any equilibrium, candidates choose the same type of message (other than ambiguity).

We can interpret this result as follows. Despite the fact that campaign messages have literal meanings, they remain cheap talk, so a message's correct interpretation depends critically on subjects' expectations of the behavior of others (e.g., Crawford and Sobel 1982, Crawford 2003, Kartik and Van Weelden 2014). In our setup, sharing information entails solving a coordination problem: All candidates must choose exactly the same type of message for elections to be perfectly informative. Thus, since incentives are aligned, there is no incentive to lie. If every subject chooses the truthful message, then it will be obvious to everyone else which of the two candidates is the highest performer in every election, as the ranking of candidates according to their messages reflects their true ranking.

But although there is no incentive to lie, there is no real incentive to tell the truth either. That is, the incentive to tell the truth is not a strict one. There are also equilibria with lying because the literal messages do not matter, so long as campaign messages correctly reveal the *relative* ordering of candidates' ability. This ordering is preserved if everyone lies by the same amount. Thus, when everyone shares the common goal of selecting and identifying the best possible representative, any campaign message (other than the ambiguous message) can be supported as rational behavior. At the same time, we believe it is reasonable to argue that the most natural or focal belief arises when subjects reason that since everyone has a common incentive to select the best representative, there is no reason for lying and therefore everyone is likely to tell the truth. Combining this focality argument with the equilibrium predictions yields our first hypothesis.

Hypothesis 1. *Preference alignment and common norms of honesty imply that an equilib-*

rium with truthful messages will be focal. Truthful messages will be the most likely type of message in the Performance Pay condition and ambiguity will be the least likely.

Truthful campaign messages depend on the expectation that others will be truthful, but what if subjects harbor doubts about the honesty or motives of other candidates? For example, there might be subjects who care about and enjoy winning the election, even if doing so prevents the highest ability candidate from becoming the representative, leading to lower monetary payoffs for everyone. Since exaggerating is a best response to exaggerating, the expectation that other candidates might exaggerate will lead rational—and otherwise honest—candidates to exaggerate as well.

We can take this a step further and formalize this argument by considering the expected gain in X's payoffs from exaggeration instead of honesty against Y, modeled as a behavioral type (as a probability distribution over discrete strategies). To simplify the analysis, suppose that Y uses an ambiguous message with probability β, exaggerates by a fixed amount e with probability γ, and is honest with the remaining probability. We can also interpret the probability distribution over types as X's (possibly out-of-equilibrium) beliefs about Y's strategy. Similarly, restrict X's strategies to ambiguity, exaggeration (by e), and honesty. Consider first the case where $x > \frac{1}{2}$, so there would be no expected gain from exaggeration against ambiguity by Y (because X would win for sure by either exaggerating or being honest). If Y exaggerates, from equation (1), the gain from also exaggerating is $\frac{e^2}{2}$ (ensuring when X is the best candidate that X wins), while if Y is honest, the expected gain is $-\frac{e^2}{2}$ (a loss because X will sometimes win when X is the worse candidate). The expected gain from exaggerating given beliefs $(\beta, \gamma, 1 - \beta - \gamma)$ is

$$(\beta + 2\gamma - 1)\frac{e^2}{2} \ . \tag{2}$$

Since (2) is increasing in β and γ, candidate X gains more from exaggerating when they expect that Y is more likely to be ambiguous or more likely to exaggerate, or equivalently, the less likely Y is to be honest.

If X's skill is below average, specifically $x \in [\frac{1}{2} - e, \frac{1}{2}]$, then exaggeration incurs an additional cost when Y's realized message is ambiguous. While exaggerating when one already expects to win (as is the case when $x > \frac{1}{2}$) does not affect the outcome and is inconsequential, a below-average candidate who exaggerates against ambiguity will win and be worse off by guaranteeing a below-average payoff than by losing and receiving the expected average payoff. Thus, the expected benefit from exaggerating is lower than the quantity given in (2) for below-average candidates. In our experiment, however, information about the distribution of scores is not provided. Nevertheless, if we think of the difference between above-average and below-average abilities in terms of subjects' confidence or their subjective beliefs, we obtain our next hypothesis.

Hypothesis 2. *If candidates believe that other candidates are more likely to exaggerate, or if they are more confident in their abilities than others, then they are themselves more likely exaggerate their qualifications.*

As one might imagine, introducing an individual-level bonus for winning the election (as in the Individual Bonus and Group Bonus conditions) naturally provides a greater incentive for lying, even for low or moderate ability candidates. The availability of the bonus disrupts the alignment of preferences since some subjects would prefer to obtain the bonus even if doing so prevents the selection of the best ability representative. Specifically, since the \$2.50 bonus for winning is equivalent to a difference in 5 anagrams solved correctly, anyone who believes her own score is within 4 correct answers of the highest score has an incentive to tell the big lie in order to win the election. For example, if a subject's score is 10, she would earn $10 \times \$0.75 + \$2.50 = \$10$ for winning the election herself, whereas if the best score is 14, the payoff from electing the best representative would be $14 \times \$0.50 + 10 \times \$0.25 = \$9.50$. In other words, the individual bonus from winning the election provides an incentive to exaggerate simply to deceive others in the hopes of winning the election. The next hypothesis is therefore a straightforward implication of varying candidates' incentives.

Hypothesis 3. *Candidates are more likely to exaggerate their qualifications in the Bonus Conditions than in the Performance Pay Condition.*

Our last prediction is that candidates are more willing to exaggerate when winning the election benefits others rather than just themselves. Let $B > 0$ be the bonus from winning the election, and let $\alpha \geq 0$ be an altruism parameter in X's utility function (i.e., the weight that a candidate places on the payoffs of in-group members). Note that since in-group members receive identical payoffs, we do not need to consider the number of other group members. Modifying (1) to include the bonus and altruism, the expected payoff in the individual bonus condition is

$$\int_0^{x+\delta} ((1+\alpha)x + B)\, dy + \int_{x+\delta}^1 y dy \; , \tag{3}$$

whereas in the group bonus condition it is

$$\int_0^{x+\delta} (1+\alpha)(x+B) dy + \int_{x+\delta}^1 y dy \; . \tag{4}$$

Comparing these conditions, subtracting (3) from (4) yields the additional utility an altruist receives in the Group Bonus condition over the Individual Bonus condition,

$$\alpha B(x+\delta) \; . \tag{5}$$

Notice that there is no difference for purely self-interested candidates (when $\alpha = 0$), but that if X exaggerates *more* than Y (i.e., $\delta > 0$), then altruists indeed gain more by lying in the Group Bonus condition than in the Individual Bonus condition. This implies our final hypothesis.

Hypothesis 4. *If candidates are other-regarding in the sense that they are altruistic (seek to maximize the payoffs of their fellow group members), then they are more likely to exaggerate in the Group Bonus condition than in the Individual Bonus condition.*

We should note that we are least confident about the prediction stated in Hypothesis 4 for several reasons. Other-regarding preferences in terms of group welfare is only one form of altruism or social preference, and groups in our setting are extremely minimal. There is no opportunity to communicate or form a group identity or cohesion, so it may be that subjects don't particularly care about the payoffs of other group members in the experiment. Another reason for our hesitation is the possibility that in activating or priming other-regardingness, the group bonus may activate other forms of social preference in ways distinct from altruism. For example, it might strengthen norms of honesty or enhance lying aversion in some candidates. Overall, then, we do not have strong expectations regarding Hypothesis 4.

4 Results

Performance on the anagram task varied sufficiently across subjects to make the selection of a quality representative a reasonably difficult coordination problem. Figure 2 shows kernel densities of performance on the anagram task in both parts of the experiment (before and after the elections). On average, subjects correctly solved 15.6 anagrams in the first task, with a minimum of 2, a maximum of 35, and a standard deviation of 6.4. In the second task, the average was 14.4, the minimum was 2, the maximum was 34, and the standard deviation was 5.7.

Our central research question involves subjects' choices of campaign message under different electoral incentives, and specifically, the effect of office-motives on the extent of deception and exaggeration. Will subjects tell the truth? Or will they exaggerate their qualifications to try to win office? Figure 3 shows the distribution of messages in the Performance Pay condition. Recall that in this baseline condition, subjects' incentives are aligned to elect the highest-performing candidate. Consistent with Hypothesis 1, truth telling is the modal message type. However, coordinating on the truth is by no means universal. While

46% of subjects told the truth about their scores, an equal proportion exaggerated (almost equally divided between small lies and big lies). Ambiguity was the least popular message type, at 8%. What is striking about this result is that even when candidates have common incentives to share information (and hence to refrain from using deception), a substantial proportion of them lie.

Figure 2: Task performance (kernel densities)

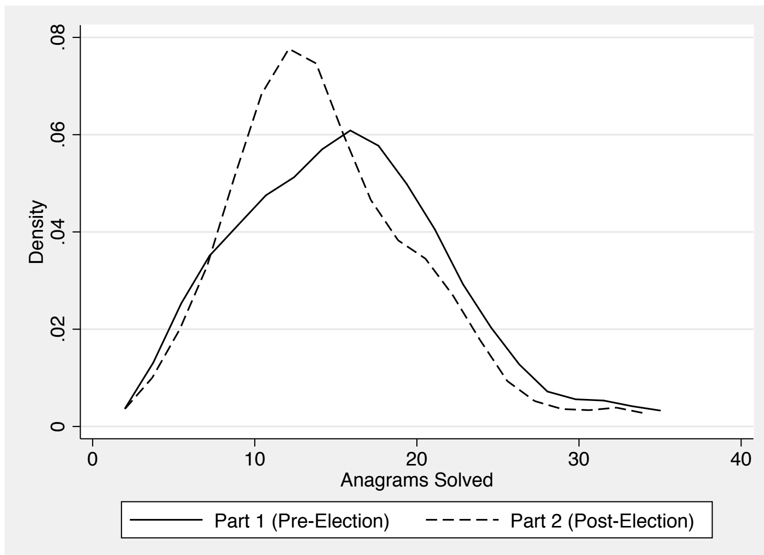
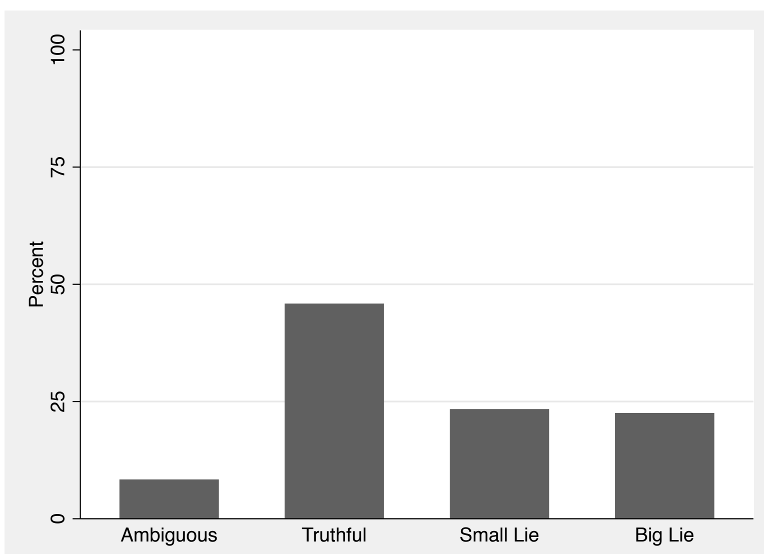
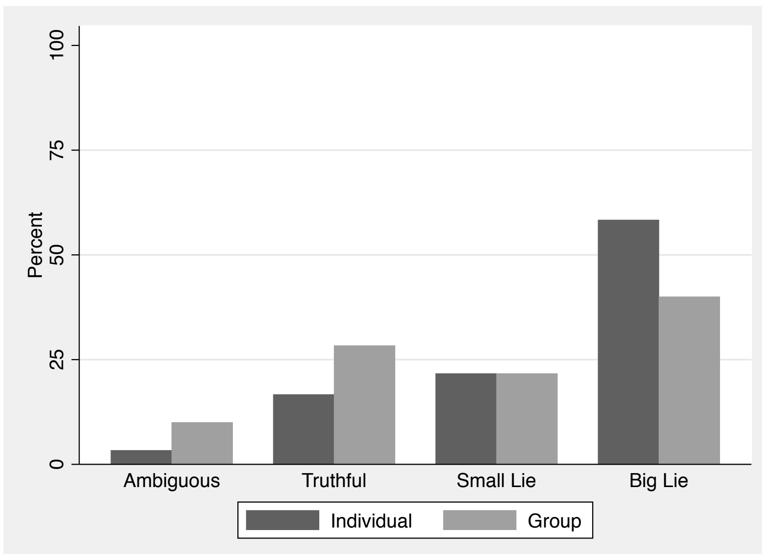


Figure 3: Messages in Performance Only Condition



When we motivate subjects by giving them explicit bonuses for winning, we find, not surprisingly, a substantial shift in campaign message behavior from truth telling to exaggeration. This result supports Hypothesis 3. Figure 4 shows that in both bonus conditions, the big lie is the modal message type (58% in the Individual Bonus condition and 40% in the Group Bonus condition). Indeed, exaggeration—in the form of either the small lie or big lie—accounts for a substantial majority of messages (80% and 61%, respectively) when there is a bonus for winning. At the same time, truth telling declines (to 17% and 28%).

Figure 4: Messages in Bonus Conditions



Recall that an individual subject's reward for winning the election is the same across both bonus conditions and that the group bonus condition was designed to activate other-regarding preferences. Figure 4 shows that there are more truthful messages and fewer big lies in the Group Bonus condition than in the Individual Bonus condition. Although Pearson's chi-squared test cannot reject the equality of the overall distributions at conventional levels of significance ($\chi^2 = 5.866, p = 0.12$), we do find that there is a significant difference between the proportion of big lies ($\chi^2 = 4.035, p = 0.05$). The data therefore contradict our expectation in Hypothesis 4 that the Group Bonus would activate social preferences and increase candidates' lying on behalf of their group. The data suggest instead that the

Group Bonus condition prompts increased lying aversion, perhaps because subjects think more directly about the costs and benefits to others.

Turning now to the relationship between candidate quality and the honesty of their campaign messages, Figures 5 and 6 present box and whiskers plots summarizing the distributions of scores by message type in the Performance Pay and Bonus conditions. Figure 5 suggests that message behavior may be weakly related to underlying task performance. Candidates who choose the big lie as their campaign message have a higher median task score (17) than candidates who send truthful messages (15), and candidates who send ambiguous messages have a lower median score (11). The top part of Figure 6 shows that median scores do not vary much by message type in the Individual Bonus condition, suggesting a weaker relationship than in the baseline, which is not particularly surprising given that most subjects choose the big lie. In contrast, the bottom part of Figure 6 suggests an intriguing finding with respect to the relationship between scores and messages in the Group Bonus condition. The relationship between task performance and message choice appears to be much stronger when the bonus is given not just to the winning candidate, but also to every member of the candidate's group: The median score of candidates choosing the big lie is 20, which far exceeds the median score conditional on any other type of message (13 given a small lie, 12 given a truthful message, and 8 given an ambiguous message). The Group Bonus appears to suppress lying behavior by weaker candidates, perhaps lending credence to our interpretation that it increases the salience of lying aversion or norms of honesty.

To assess these relationships more systematically, we estimate a series of linear probability regression models. For each model, we dichotomize the dependent variable in a different way while including the same set of covariates: dummy variables for the bonus treatment (i.e., Part 2, regardless of whether the bonus is for the winning candidate or the entire winning group), the group bonus (to distinguish between the individual and group bonus effects), Part 1 score, and their relative beliefs about their scores. We measured relative beliefs (that is, confidence) with a questionnaire that followed the incentivized part of

Figure 5: Scores in Performance Only Condition

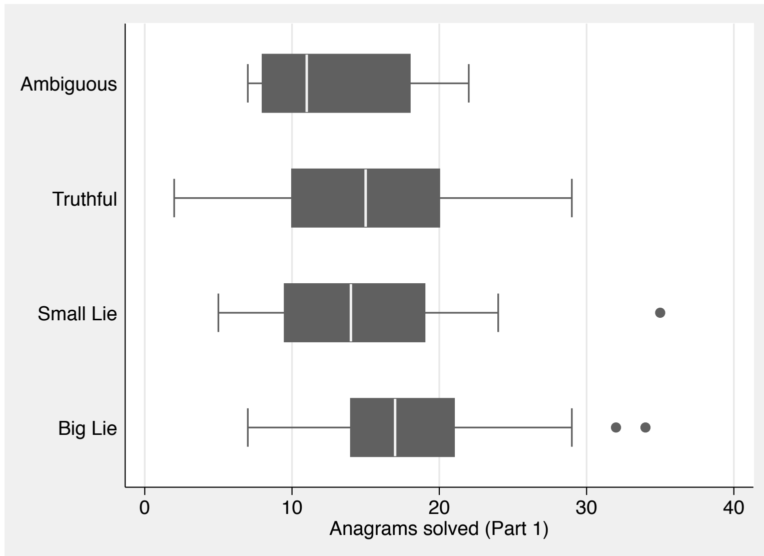
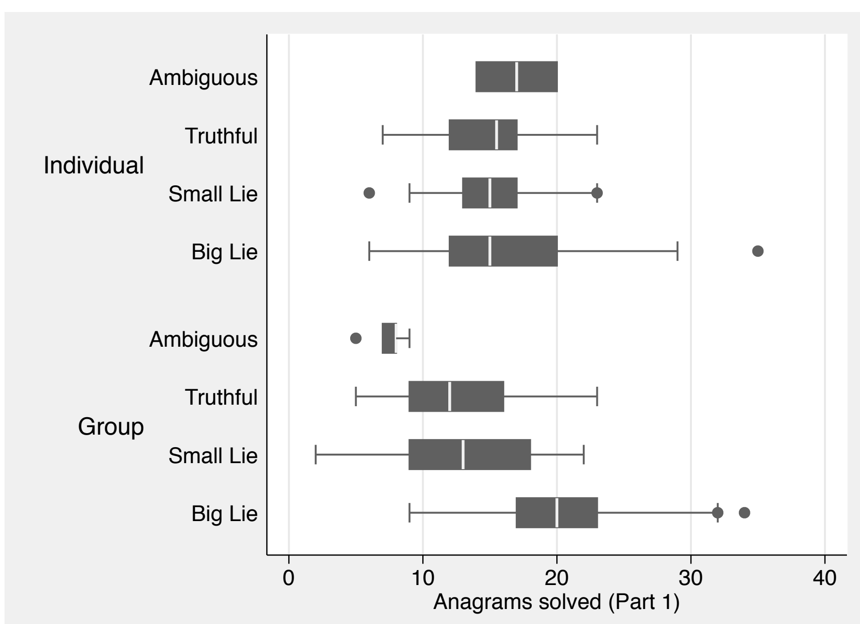


Figure 6: Scores in Bonus Conditions



the experiment, asking each subject the degree to which they believed their score was higher than other subjects' scores. This measure takes on integer values from 0 (higher than no one else's score) to 4 (higher than everyone else's score).[17] Given that this measure of beliefs is not incentivized, we caution that we are not particularly confident that it has a high degree of measurement reliability. Nevertheless, we include it as control variable.[18]

Table 1: Linear probability model analysis of message behavior

	(1) Ambiguous b/se	(2) Truth b/se	(3) Small Lie b/se	(4) Big Lie b/se	(5) Non-Truth b/se	(6) Any Lie b/se
Bonus Treatments	-0.017	-0.233**	-0.017	0.267**	0.017	0.250**
	(0.03)	(0.06)	(0.05)	(0.06)	(0.03)	(0.06)
Group Bonus	-0.029	-0.014	0.076	-0.034	0.029	0.042
	(0.03)	(0.06)	(0.05)	(0.06)	(0.03)	(0.06)
Part 1 Score	-0.004	-0.003	-0.018**	0.026**	0.004	0.008
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Relative Beliefs	0.029	0.031	-0.100**	0.040	-0.029	-0.060
	(0.02)	(0.04)	(0.03)	(0.04)	(0.02)	(0.04)
Constant	0.142	0.473**	0.587**	-0.202	0.858**	0.384*
	(0.11)	(0.17)	(0.16)	(0.14)	(0.11)	(0.18)
R^2	0.042	0.071	0.048	0.163	0.043	0.107
Observations	240	240	240	240	240	240

* $p < .05$, ** $p < .01$; robust standard errors clustered by subject

The first four columns of Table 1 present the results when we dichotomize the dependent variable for each type of message (whether the message was ambiguous, truthful, etc).[19] These results show that electoral incentives clearly decrease the probability of choosing truthful messages (column 2) while increasing the probability of choosing the big lie (column 4), providing further support for Hypothesis 3. In contrast, electoral incentives do not affect the prevalence of ambiguous messages (column 1) or small lies (column 3). In column 5, the

[17] The intermediate values are "higher than about 1/4 of them" (1), "higher than 1/2 of them" (2), and "higher than 3/4 of them" (3).

[18] In a later section, we report additional results with incentivized beliefs.

[19] We obtain similar results when we estimate a multinomial probit model.

dependent variable is any non-truthful message (anything but the truth), while in column 6 it is any amount of exaggeration (pooling the small lies and big lies, excluding ambiguity); the difference between the last two columns suggests that incentives work by increasing the prevalence of exaggeration rather than ambiguity. No matter how we code the dependent variable, the coefficient for group bonus is not significant in any of the models. We can infer from this that there is no difference between the group and individual bonus in terms of message behavior. That is, the individual bonus for winning the election seems to be sufficient to encourage candidates to lie, while adding the group bonus neither significantly increases nor decreases such behavior.

We also find that lying behavior depends partly on ability and beliefs. Part 1 scores have a positive and statistically significant effect on the probability of big lies and a negative and statistically significant effect on the probability of small lies. These results mean that higher ability candidates are more likely to tell the big lie and less likely to tell the small lie, consistent with Hypothesis 2. The coefficient estimate for relative beliefs is negative and statistically significant only for small lies, which means that candidates who are more confident in their scores are less likely to use the small lie (which would be less effective). The coefficient is positive but not significant for big lies. This exaggeration by high ability candidates can be consistent with instrumental rationality if they believe low ability candidates might lie, thereby countering the distortions from low ability candidates and increasing the informativeness of the overall campaign environment. We cannot say definitively whether such lying behavior is consistent with instrumental rationality, however, because we do not have direct measures of candidates' beliefs about the behavior of their opponents.

To investigate whether the relationship between ability and confidence differs across electoral incentives, we report a series of separate ordered probits in Table 2. In this analysis, we can think of the type of message as being ordered in terms of the degree of precision and exaggeration. Since ambiguous messages are the least precise, we treat them as the lowest category, and then we order the other types of messages in terms of the degree of

Table 2: Ordered probit analysis of message behavior

	Performance		Individual		Group	
	b/se	b/se	b/se	b/se	b/se	b/se
Part 1 Score	0.03*	0.03	0.02	0.01	0.14**	0.11**
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)
Relative Beliefs		-0.02		-0.07		-0.25
		(0.14)		(0.20)		(0.21)
μ_1	-0.88**	-0.93	-1.59**	-1.84*	0.36	-0.47
	(0.28)	(0.55)	(0.53)	(0.94)	(0.40)	(0.81)
μ_2	0.64*	0.59	-0.60	-0.84	1.64**	0.86
	(0.27)	(0.54)	(0.46)	(0.88)	(0.42)	(0.77)
μ_3	1.31**	1.25*	0.03	-0.21	2.38**	1.61*
	(0.28)	(0.54)	(0.46)	(0.88)	(0.46)	(0.79)
Observations	120	120	60	60	60	60
Log likelihood	-146.4	-146.4	-63.31	-63.25	-62.46	-61.75

* $p < .05$, ** $p < .01$

exaggeration.[20] Consistent with Figures 5 and 6, the results suggest that precision and exaggeration are weakly related to task scores in the Performance Pay condition, unrelated in the Individual Bonus condition, and strongly related in the Group Bonus condition. Thus, although the group bonus does not have an overall direct effect on campaign messages, we find that it does have an interactive effect with task ability.

Our experimental analysis shows that the degree of honesty in campaigns varies considerably across electoral contexts. Office motivations in the form of private benefits of

[20]The results do not change appreciably if we omit ambiguous messages from the analysis and treat the dependent variable as being ordered in terms of the level of exaggeration.

winning office clearly induce candidates to exaggerate their qualifications. Strikingly, we also find that in an environment in which winning is not inherently beneficial (because candidates and voters have common incentives to elect the best representative), many candidates campaign honestly, but many nevertheless exaggerate. The relationship between messages and candidate quality (task ability) suggests that such exaggeration could be instrumental, perhaps reflecting a strategic response to distrust of others in political settings rather than an outright intention to deceive.

5 Beliefs and Motives

Cleanly differentiating between instrumental versus corrupt motives requires additional data on beliefs and as well as motives. By beliefs, we mean probability beliefs in the standard sense used in decision theory and game theory. Specifically, to determine whether exaggeration is instrumental (a best response) or not, we need to know how likely a candidate believed that others exaggerated or lied. We therefore designed a set of incentivized belief elicitation procedures that we added to the end of the Individual Bonus treatment, and we were careful to elicit beliefs after subjects chose their campaign messages (to avoid influencing their choices) but before the elections or any of the results were revealed.[21] We refer to the modified treatment as the Belief Treatment, and we ran 10 sessions of this treatment (100 subjects). Incentivizing beliefs is important because it forces subjects to think about their beliefs in a way that is consequential and ensures that subjects understand beliefs in the same way, thereby enhancing the reliability of our belief measures.

Our belief elicitation procedure is based on probability matching (Healy 2011, Trautmann and van de Kuilen 2014). For each belief that we elicit, subjects face a series of binary

[21]For full details of our procedure, see the text of the instructions in the Appendix. We ensured comprehension of the procedure with two comprehension checks; in addition, after the first comprehension check, we provided an additional explanation of the procedure. Because the belief elicitation requires a lengthy instruction period, we only elicited beliefs about the first election. Note that timing of the elicitation procedure means that the campaign message decisions are *identical* in the Belief Treatment and the Individual Bonus Treatment.

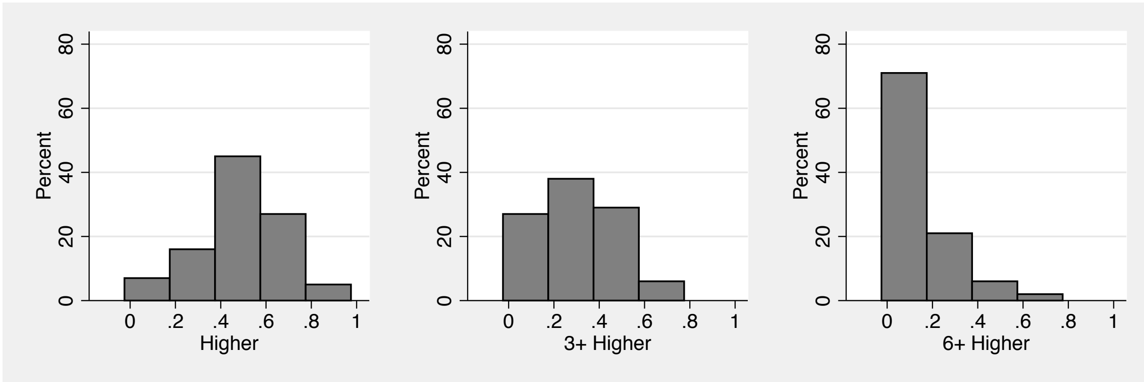
choices between two gambles: one based on their subjective belief and one based on an objective probability. To illustrate, consider the belief held by candidate i that candidate j chose the big lie (i.e., that j exaggerated their score by 6). The subjective belief is represented by Option A, which pays a subject \$15 if candidate j actually chose the big lie and pays \$0 otherwise. The objective probability is represented by Option B, which is a binary lottery with a known probability of winning. We describe this probability to subjects in terms of a "virtual bingo cage" with red and white balls, where a subject wins \$15 if we draw a virtual red ball and wins \$0 if we draw a virtual white ball. Subjects then face a series of choices between Option A and Option B where we vary the probability that Option B wins, from 0% to 100% in increments of 5 percentage points. The probability belief that j chose the big lie is the objective probability for which subject i switches from preferring to be paid for Option A to preferring to be paid for Option B—that is, the proability that makes them *indifferent* between their subjective probability and the objective probability.

We used this procedure to elicit beliefs about relative ability and about campaign messages. Specifically, we elicited the following beliefs each subject held about his or her election opponent in the Performance Pay condition (the first set of elections):

$$
\begin{aligned}
\text{Higher} &= \Pr(i\text{'s score higher than } j\text{'s score}) \\
\text{3+ Higher} &= \Pr(i\text{'s score more than 3 higher than } j\text{'s score}) \\
\text{6+ Higher} &= \Pr(i\text{'s score more than 6 higher than } j\text{'s score}) \\
\text{Truthful} &= \Pr(j \text{ chose truthful message}) \\
\text{Small lie} &= \Pr(j \text{ chose small lie}) \\
\text{Big lie} &= \Pr(j \text{ chose big lie})
\end{aligned}
$$

Figure 7 presents histograms of elicited beliefs about relative ability. Although we are not particularly interested in the accuracy of beliefs, it appears that in the aggregate, subjects hold reasonable beliefs about their relative abilities: 40% believe themselves to be

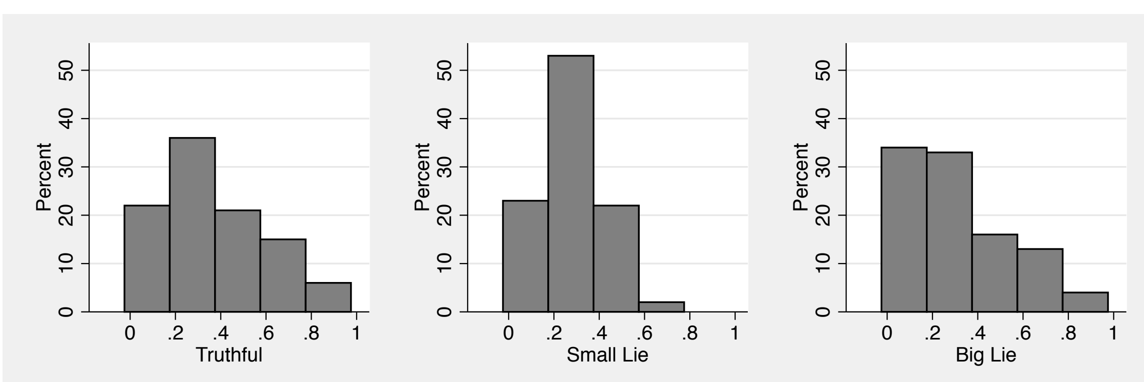
Figure 7: Beliefs about others' scores



below the median, 18% believe themselves to be at at the median, and 42% believe themselves to be above the median (see the left part of Figure 7). Few subjects are confident that their scores are extremely high (more than 6 higher than others' scores): 71% of subjects indicate such a probability belief less than 20%, with the highest such belief being 65%. For subjects who are extremely confident that their ability is in the upper tail of the distribution, there would be little cost to exaggerating their campaign message (given their level of confidence, it would be unlikely that exaggerating would prevent a higher ability subject from being elected).

More relevant to understanding whether exaggeration is rational are the histograms presented in Figure 8, which shows subjects' beliefs about the types of messages chosen by opposing candidates. The histogram on the left of Figure 8 suggests that subjects are

Figure 8: Beliefs about others' messages



not especially confident about the honesty of others: Over half of subjects (62%) hold a probability belief of less than 50% that the opposing candidate told the truth. Similarly, the histogram on the right shows that the belief that other candidates told the big lie is not uncommon: 19% of subjects have a probability belief greater than 50% while over half of subjects (56%) hold a probability belief of greater than 20%. Indeed, the distribution of beliefs about the big lie is not dissimilar from the distribution of beliefs about truthful messages.

To what extent do these beliefs affect candidates' messages? Table 3 reports linear probability model regressions similar to the ones reported previously, in which we dichotomize the dependent variable in several ways. The results suggest that beliefs indeed account for candidate exaggeration. More specifically, beliefs about the lying behavior of others matter most, rather than beliefs about relative ability. Column 2 shows that as the beliefs that other candidates chose the small lie or big lie increase, the probability of choosing a truthful message decreases. Column 3 shows that the probability of a small lie is positively and significantly related to the belief that others chose the small lie. Similarly, column 4 shows that the probability of a big lie is positively and significantly related to the belief that others told the big lie. Overall, these results provide a strong indication that the level of exaggeration is consistent with an instrumentally rational best response: Candidates are more likely to choose the type of message that they believe others selected.

We also investigate whether we could account for differences in lying behavior by considering heterogeneity in personality traits or attitudes that might predispose some subjects to engage in either honest or dishonest behavior. We therefore included several attitudinal measures and personality scales in the questionnaire we administered after the incentivized portion of the Belief Treatment. The most relevant measure is the Machiavellian Personality Scale (MPS, Dahling, Whitaker and Levy 2008), which attempts to measure several aspects of manipulative personalities: one's propensity to distrust others (Distrust), engage

Table 3: Linear probability model analysis of beliefs and messages

	(1) Ambiguous b/se	(2) Truth b/se	(3) Small Lie b/se	(4) Big Lie b/se	(5) Non-Truth b/se	(6) Any Lie b/se
Part 1 Score	-0.005	-0.002	0.004	0.003	0.005	0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Belief, Score 3+ higher	0.263	-0.063	0.074	-0.273	-0.263	-0.199
	(0.41)	(0.41)	(0.38)	(0.33)	(0.41)	(0.36)
Belief, Score 6+ higher	-0.439	-0.196	0.320	0.315	0.439	0.635
	(0.38)	(0.46)	(0.47)	(0.36)	(0.38)	(0.44)
Belief, Other Small Lie	-0.021	-0.823**	0.860**	-0.016	0.021	0.844**
	(0.16)	(0.28)	(0.28)	(0.21)	(0.16)	(0.28)
Belief, Other Big Lie	-0.057	-1.220**	0.073	1.204**	0.057	1.277**
	(0.14)	(0.15)	(0.13)	(0.14)	(0.14)	(0.15)
Constant	0.162+	1.133**	-0.171	-0.124	0.838**	-0.295*
	(0.09)	(0.14)	(0.14)	(0.09)	(0.09)	(0.13)
R^2	0.0371	0.335	0.139	0.418	0.0371	0.405
Observations	100	100	100	100	100	100

+ $p < .10$, * $p < .05$, ** $p < .01$; robust stanard errors in amoral manipulation (Amoral), seek control over others (Control), and seek status for oneself (Status). If these personality traits drive lying behavior, we would expect that subjects scoring higher in each dimension of Machiavellianism to be more likely to exaggerate their qualifications for office.

In addition to MPS, we used a short version of the Big Five personality factors (TIPI, Gosling, Rentfrow and Swann 2003), which measures Extraversion, Agreeableness, Conscientiousness, (Emotional) Stability, and Openness. We have no prior expectations regarding which of these factors would be related to greater deceptive behavior. The questionnaire also included a generalized measure of trust, measures of risk aversion (Kam 2012), and basic demographics (e.g., gender).

In Table 4, we report various specifications of ordered probit analyses using the additional data from the Belief Treatment, with the incentivized beliefs and questionnaire measures as covariates. The factor that clearly stands out is a subject's belief that others are likely to exaggerate greatly (the probability belief that the other candidate chose the big lie). This coefficient is large, positive, and statistically significant across specifications. Beliefs about relative ability are positive and significant at the .05 level only when personality factors are excluded from the analysis (column 1). None of the Machiavellianism factors are significantly correlated with lying behavior, which is striking because these are the factors most relevant to the propensity to lie and cheat. Gender, generalized trust, and risk aversion play no role in accounting for candidate exaggeration as well. Interestingly, we find that subjects who perceive themselves as more emotionally stable as measured by the TIPI scale are less likely to exaggerate. Although this factor is negatively and significantly related to exaggeration, it does not affect our central conclusions about the importance of beliefs. If anything, it strengthens the importance of beliefs in the analysis, as the coefficient for beliefs about big lie are higher when we include TIPI covariates.

Table 4: Ordered probit analysis of beliefs, traits, and message behavior

	(1)	(2)	(3)	(4)	(5)	(6)
	b/se	b/se	b/se	b/se	b/se	b/se
Part 1 Score	0.04*	0.02	0.02	0.02	0.03	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Belief, Score Higher		-0.44	-0.41	-0.31	-0.25	-0.26
		(0.79)	(0.80)	(0.82)	(0.83)	(0.88)
Belief, Score 6+ higher		1.84	1.80	1.73	1.05	0.48
		(1.11)	(1.12)	(1.13)	(1.14)	(1.20)
Belief, Other Small Lie		0.82	0.87	0.62	0.93	1.06
		(0.80)	(0.81)	(0.83)	(0.83)	(0.88)
Belief, Other Big Lie		3.61**	3.69**	3.68**	4.28**	4.61**
		(0.63)	(0.65)	(0.65)	(0.70)	(0.74)
Female			0.04			-0.07
			(0.24)			(0.28)
Trust			0.36			0.53
			(0.56)			(0.66)
Risk Aversion			0.12			-0.26
			(0.79)			(1.13)
Amoral				0.36		1.15
				(0.84)		(0.96)
Control				-0.08		-1.24
				(0.67)		(0.92)
Status				0.73		0.81
				(0.57)		(0.64)
Distrust				-0.83		-0.40
				(0.85)		(0.95)
Extraversion					0.37	0.30
					(0.50)	(0.62)
Agreeableness					0.63	0.34
					(0.57)	(0.69)
Conscientiousness					0.87	1.37*
					(0.53)	(0.66)
Stability					-1.15*	-1.45*
					(0.52)	(0.58)
Openness					0.95	1.35
					(0.77)	(0.87)
μ_1	-0.82**	-0.06	0.19	0.10	1.50	1.96
	(0.28)	(0.44)	(0.56)	(0.67)	(0.87)	(1.35)
μ_2	0.64*	1.66**	1.91**	1.84**	3.38**	3.91**
	(0.27)	(0.46)	(0.59)	(0.69)	(0.92)	(1.40)
μ_3	1.23**	2.51**	2.76**	2.71**	4.30**	4.88**
	(0.29)	(0.50)	(0.61)	(0.71)	(0.95)	(1.42)
Observations	100	100	100	100	100	100
Log likelihood	-122.8	-101.9	-101.6	-100.6	-96.53	-93.71

* $p < .05$, ** $p < .01$

6 Conclusion

Elections are the cornerstone of representative democracy. But elections also include a critical information problem: Candidates know their own quality but voters do not. We use laboratory elections to investigate the circumstances under which candidates either honestly convey (or conversely, obfuscate) their credentials. Several results are worth emphasizing.

First, we find that the private benefit of winning the election induces candidates to inflate their qualifications. Incentives clearly matter in ways that we would expect. When candidates have a stronger motivation to win, they are more likely to lie. We caution that because our experimental setup is specifically tailored to address behavioral questions about candidate honesty (by severely constraining the message space), we cannot say whether exaggeration necessarily undermines the informational quality of elections under more general circumstances.[22] For example, our framework does not address how lying varies with the risk of discovery (Boudreau 2009, Lupia and McCubbins 1998). It may also be more applicable to understanding electoral environments in which candidates are not widely known and voters prospectively evaluate them—as in in open seat races without incumbents or in young democracies (e.g., Keefer 2007)—than in retrospective evaluations of incumbent performance. Our setup could be extended in future work to address such differences in the information environment.

Second, we find a sizeable proportion of candidates who exaggerate when the only goal of the election is to select the highest ability representative. Interestingly, this finding contrasts with the experimental literature on strategic communication and lying aversion (e.g., Gneezy 2005, Gneezy, Rockenbach and Serra-Garcia 2013) as well as the literature on pandering and elections (e.g., Woon 2012, 2014) which finds that informed players are often reluctant to exploit their informational advantage by lying. At first glance, this result might seem troubling because it suggests that lying may be prevalent in even the most benign

[22] Indeed, because greater coordination leads to greater information transmission in our game, the high degree of exaggeration in the Individual Bonus condition implies that it has the greatest informational content. We make no claims whatsoever that this logic applies to elections in general.

political settings. Upon further inspection, however, we show that exaggeration is driven primarily by candidates who hold strong beliefs that other candidates exaggerate. These beliefs are specific to the electoral environment and are unrelated to generalized trust or distrust of others or to power-seeking proclivities. Candidates in our experiment seem to have a sufficient degree of strategic sophistication to understand that conveying information to voters depends not just on what they say, but what they say in relation to the claims that other candidates make.

Notably, our results indicate that the particular strategic and competitive context of elections may make untruthfulness endemic to democratic elections. One need not be a dishonest sort of person to grasp that lying in elections need not benefit only oneself, but provided that one is highly qualified, exaggerating may be in the public's best interest. The alternative to lying may be worse for the public at large if only the most competent candidates are honest while the least competent are deceitful. Noted American humorist Will Rogers famously said "If you ever injected truth into politics, you'd have no politics." Our results support Rogers' conclusion, but we would add that the fault may not lie solely with politicians, but with politics itself.

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Appendix

A Theoretical analysis

A.1 Baseline model

Opponent is ambiguous

We first determine the best response of candidate X if the opponent Y chooses the ambiguous message, $m_Y = \phi$. If X also chooses an ambiguous message, then each candidate is equally likely to win. If X wins, then the payoff is given by X's skill x, while if Y wins, then from X's perspective the expected payoff is $E[y] = \frac{1}{2}$. Candidate X's expected payoff from using an ambiguous message $m_X = \phi$ in response to $m_Y = \phi$ is therefore

$$\frac{1}{2}x + \frac{1}{4}\ . \tag{A1}$$

If X sends the truthful message, $m_X = x$, then X wins if $x > \frac{1}{2}$, ties if $x = \frac{1}{2}$, and loses if $x < \frac{1}{2}$. We can therefore write X's expected utility from sending a truthful message as

$$\begin{cases} \frac{1}{2} & \text{if } x \leq \frac{1}{2} \\ x & \text{if } x > \frac{1}{2} \end{cases} \tag{A2}$$

If $x > \frac{1}{2}$, then X will prefer to send the truthful message, win the election, and guarantee a payoff of $x > \frac{1}{2}$. If $x < \frac{1}{2}$, then X will also prefer to send the truthful message, but then lose the election, which gives an expected payoff of $E[y] = \frac{1}{2} > x$. Only in the knife-edge case $x = \frac{1}{2}$ will X be indifferent between ambiguity and truthfulness.

If X exaggerates by any amount e, then X wins the election if $x + e > \frac{1}{2}$, ties if $x + e = \frac{1}{2}$, and loses otherwise. Given some value of e, then we can then partition the values of x into three regions. If x is high enough ($x > \frac{1}{2}$), then X wins the election by exaggerating, but is indifferent between exaggeration and honesty. If x is low enough ($x < \frac{1}{2} - e$), then X loses the election regardless of whether the message is exaggerated or honest. However, if $\frac{1}{2} - e < x < \frac{1}{2}$, then X will prefer to be honest and lose the election (yielding expected payoff $\frac{1}{2}$) to exaggerating and winning (yielding expected payoff $x < \frac{1}{2}$).

We can conclude that for every value of x, honesty is a best response (at least as good) as ambiguity. For some values (between $\frac{1}{2} - e$ and $\frac{1}{2}$), it is strictly preferred. We can therefore state the following result.

Lemma 1. *For any skill x, honesty ($m_X = x$) is a best response to ambiguity ($m_Y = \phi$). Except for $x = \frac{1}{2}$, honesty is strictly better than ambiguity, and if $\frac{1}{2} - e < x < \frac{1}{2}$, honesty is strictly better than exaggeration.*

It follows that except for the knife-edge case where $x = y = \frac{1}{2}$, it cannot be an equilibrium for both candidates to choose ambiguous messages.

Opponent is honest

Next, we analyze how X should respond if Y is honest and sends message $m_Y = s$ (i.e., $e_Y = 0$). First, if X selects the ambiguous message $m_x = \phi$, then X will win if $y < \frac{1}{2}$ and lose if $y > \frac{1}{2}$. The expected value of y conditional on X losing is $E\left[y|y > \frac{1}{2}\right] = \frac{3}{4}$. Hence, X's expected payoff from selecting the ambiguous message is

$$\frac{1}{2}x + \frac{3}{8}\ . \tag{A3}$$

If X is honest, then X wins if $x > y$ and loses if $x < y$. (Ties are knife-edged and zero probability, so we ignore them.) To compute X's expected payoff, we compute the piecewise integral, where the integral on the left corresponds to X winning ($x > y$) and the integral on the right corresponds to Y winning ($y > x$)

$$\int_0^x xdy + \int_x^1 ydy = \frac{1}{2} + \frac{x^2}{2} \tag{A4}$$

Comparing the expressions in (A3) and (A4), honesty is strictly preferred to ambiguity for all x except when $x = \frac{1}{2}$ since

$$\begin{aligned} \frac{1}{2} + \frac{x^2}{2} &\geq \frac{x}{2} + \frac{3}{8} \\ (2x-1)^2 &\geq 0\ . \end{aligned}$$

If X exaggerates by e against Y's honesty, then X wins if $x + e > y$ and loses if $x + e < y$. We can compute the expected payoff for X using the piecewise integral similar to (A4) but with the limits of integration adjusted accordingly,

$$\int_0^{x+e} xdy + \int_{x+e}^1 ydy = \frac{1}{2} + \frac{x^2}{2} - \frac{e^2}{2}\ . \tag{A5}$$

Since (A5) is clearly less than (A4), exaggeration is a worse strategy than honesty. The intuition here is that by exaggerating, X will win the election but will be worse off for some realizations of y (when $y - e < x < y$).

Lemma 2. *For any skill x, honesty ($m_X = x$) is a best response to honesty ($m_Y = y$), and it is strictly better than exaggeration. Except for $x = \frac{1}{2}$, it is also strictly better than ambiguity.*

Opponent exaggerates

Suppose now that Y exaggerates by the amount e_Y so that $m_Y = y + e_Y$. The analysis is simpler if we assume that X exaggerates by some amount e_X and then define $\delta = e_X - e_Y$ (how much more X exaggerates than Y) so that X wins if $x + \delta > y$ and loses if $x + \delta < y$.

Analogous to (A4) and (A5), X's expected payoff if both candidates exaggerate is

$$\int_0^{x+\delta} x dy + \int_{x+\delta}^1 y dy = \frac{1}{2} + \frac{x^2}{2} - \frac{\delta^2}{2} \, , \tag{A6}$$

which is clearly maximized if $\delta = 0$. In other words, the best level of exaggeration for X is to match Y: $e_X = e_Y$.

The remaining possibility is for X to choose an ambiguous message. If so X will win only if Y's message, $m_y = y + e$ exceeds $E[x] = \frac{1}{2}$. In this case, the expected payoff is

$$\int_0^{\frac{1}{2}-e} x dy + \int_{\frac{1}{2}-e}^1 y dy = \frac{x}{2} + \frac{3}{8} - \frac{e^2 - e + 2ex}{2} \, , \tag{A7}$$

Except for the knife-edge case where $x = \frac{1}{2} - e$ (in which X is indifferent), (A6) is greater than (A7) when $\delta = 0$. Hence, matching Y's level of exaggeration is always weakly better than ambiguity and is therefore a best response.

Lemma 3. *For any skill x, it is a best response for a candidate to exaggerate by the same amount as the opponent ($e_Y = e_X$ for $m_X = x + e_x$ and $m_Y = y + e_Y$), and except for $x = \frac{1}{2} - e$, it is strictly better.*

A.2 Behavioral types

Suppose now that Y plays a behavioral strategy and sends $m_Y = \phi$ with probability β, $m_Y = y + e$ with probability γ, and $m_Y = y$ with the remaining probability. We can also think of X as having arbitrary (out-of-equilibrium) beliefs $(\beta, \gamma, 1 - \beta - \gamma)$ about Y's behavior.

Having already shown in the preceding analysis that ambiguity is always weakly dominated, we focus here on identifying the conditions under which honesty or exaggeration is a best response. To simplify the analysis, we assume a single level of exaggeration e is possible.

From (A6), X's expected payoff from exaggerating against exaggeration ($\delta = 0$) is

$$\frac{1}{2} + \frac{x^2}{2}$$

while the expected payoff from honesty against exaggeration ($\delta = -e$) is

$$\frac{1}{2} + \frac{x^2}{2} - \frac{e^2}{2} \, .$$

Hence, the net gain in expected payoffs when Y exaggerates is

$$\frac{e^2}{2} \, .$$

Similarly, if X's expected payoff from exaggerating against Y's honesty is given in (A5) while the expected payoff from being honest is given in (A4), then the net gain from exaggerating against honesty is

$$-\frac{e^2}{2}\ .$$

Against the possibility of Y's ambiguity, there are three cases to consider: (i) if $x > \frac{1}{2}$, then X wins from either exaggerating or being honest, (ii) if $x < \frac{1}{2} - e$, X loses regardless of the message, and (iii) X wins from exaggerating and loses from being honest if $\frac{1}{2} - e < x < \frac{1}{2}$. In both of cases (i) and (ii), X's expected payoff will be the same from exaggeration or honesty. Hence, it these cases, we can write X's expected gain from exaggeration given beliefs $(\beta, \gamma, 1-\beta-\gamma)$ as

$$\Delta^E = \gamma\left(\frac{e^2}{2}\right) + (1-\beta-\gamma)\left(-\frac{e^2}{2}\right) = (\beta + 2\gamma - 1)\left(\frac{e^2}{2}\right) \tag{A8}$$

Since $\frac{\partial \Delta^E}{\partial \gamma} > 0$, the gain from exaggerating is increasing in the probability that Y exaggerates. Similarly, $\frac{\partial \Delta^E}{\partial \beta} > 0$ means that exaggeration is also increasing in the probability that Y is ambiguous. Equivalently, we can say that the gain in exaggeration is increasing in Y's *dishonesty*.

Lemma 4. *The expected gain from exaggeration is increasing in the probability that the opponent exaggerates (γ) and increasing in the probability that the opponent is ambiguous (β).*

In the remaining case (iii), we know from Lemma 1 that exaggerating against ambiguity is costly. Hence, we can state the following difference between candidates who are just above and just below average.

Lemma 5. *The expected gain from exaggeration is greater for candidates who are above average, $x > \frac{1}{2}$, than candidates who are just below average, $\frac{1}{2} - e < x < \frac{1}{2}$.*

A.3 Effect of individual versus group bonus

Let $\alpha \geq 0$ parameterize candidate X's degree of altruism toward in-group members, and let B denote the bonus payoff from winning the election. In this section, we focus our attention on comparing the gain from exaggeration against honesty in the group and individual bonus conditions. In the individual bonus condition, X's expected utility from exaggerating against honesty is

$$E^I = \int_0^{x+e} \left((1+\alpha)x + B\right) dy + \int_{x+e}^1 (1+\alpha) y dy \tag{A9}$$

while being honest against honesty yields

$$H^I = \int_0^{x} \left((1+\alpha)x + B\right) dy + \int_{x}^1 (1+\alpha) y dy \tag{A10}$$

so the gain from exaggerating in the individual bonus condition is

$$E^I - H^I = Be - \frac{(1+\alpha)e^2}{2}\ . \tag{A11}$$

This gain in utility is positive as long as X is not too altruistic (α is not too high relative to the bonus B and degree of exaggeration e. The intuition here is that while X gains from the individual bonus B and is individually better off, group members sometimes collectively lose because the best representative is not elected ($x < y$).

In contrast, this tension is absent in the group bonus condition, where the expected payoff from exaggerating is

$$E^G = \int_0^{x+e} (1+\alpha)(x+B)dy + \int_{x+e}^1 (1+\alpha)ydy \tag{A12}$$

while being honest against honesty yields

$$H^G = \int_0^{x} (1+\alpha)(x+B)dy + \int_{x}^1 (1+\alpha)ydy \tag{A13}$$

so the gain from exaggerating in the group bonus condition is

$$E^G - H^G = (1+\alpha)\left(Be - \frac{e^2}{2}\right) . \tag{A14}$$

Subtracting (A11) from (A14), the additional benefit from exaggerating in the group bonus condition compared to the benefit from exaggerating in the individual bonus condition is

$$\alpha Be . \tag{A15}$$

Clearly, the conditions are equivalent if a candidate is not altruistic ($\alpha = 0$), while for any degree of altruism, the benefit is greater in the group bonus condition. Thus, altruistic candidates are more likely to exaggerate in the group bonus than in the individual bonus condition.

B Subject demographics and balance

The table below provides average demographic and other characteristics for our sample, by treatment (Individual Bonus, Group Bonus). All variables except for political ideology are dichotomous, so the mean gives the percentage of subjects within each treatment. Political ideology is measured on a 7 point scale (from 1 to 7). The fourth column provides the p-value for a t-test of the difference by treatment. The last column gives the averages within the additional Beliefs treatment. Overall, our sample is well-balanced, with the one exception that there are somewhat more science majors in the group treatment than in the individual treatment. Otherwise, the characteristics of the Belief treatment resemble the characteristics of the original Individual Bonus and Group Bonus treatments.

Table 5: Demographics by treatment

Characteristic	Indiv.	Group	Total	p-value	Belief
Female	0.50	0.52	0.51	0.86	0.51
Asian	0.13	0.23	0.18	0.16	0.16
Black	0.08	0.05	0.07	0.47	0.03
Hispanic	0.02	0.03	0.03	0.56	0.03
White	0.72	0.58	0.65	0.13	0.73
Humanities	0.13	0.15	0.14	0.80	0.11
Sciences	0.12	0.30	0.21	0.01	0.23
Business & Soc. Sci	0.47	0.37	0.42	0.27	0.43
Native English speaker	0.90	0.88	0.89	0.77	0.93
Democrat	0.35	0.33	0.34	0.85	0.43
Republican	0.28	0.27	0.28	0.84	0.27
Political ideology	3.78	3.63	3.71	0.54	3.51
N	60	60	120		100

Instructions

General Information

This is an experiment on decision making. ***** and the National Science Foundation have provided funds for this research.

There are several parts to this experiment. At the end of the experiment, one of the parts will be randomly chosen to determine your payment for the experiment. In the experiment, you will be asked to perform a problem-solving task and will be paid based at least partly on your ability to perform this task well. Your earnings may also depend partly on your decisions, partly on the decisions of others, and partly on chance.

Pay attention and follow the instructions closely, as we will explain how you will earn money and how your earnings will depend on the choices that you make. In addition to the $7 participation payment, these earnings will be paid to you, in cash, at the end of the experiment.

You will be paid your earnings privately, meaning that no other participant will find out how much you earn. We will also hand out and read the instructions for each part before beginning that part. Each participant will have a printed copy of the instructions. You may refer to your printed instructions at any time during the experiment.

If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Also, please ensure that your cell phones are turned off and put away. Participants intentionally violating the rules will be asked to leave and may not be paid.

Groups

The computer will randomly divide you into two groups of participants. Each group will have an equal number of participants and will have the same members in all parts of the experiment. You will not know who among the other participants are in your group and they will not know that you are in theirs. Please look at your computer screens now to see your group assignment. (If you do not see your group assignment, raise your hand.) When you are done, click on the OK button.

[over]

Online Appendix: Sample Instructions for Individual Bonus and Belief Treatment

Part One: Anagram Task

In the first part of the experiment, you will be asked to solve a series of anagram puzzles. Each puzzle involves a set of five scrambled letters, and your task is to unscramble the letters to form a word. Each puzzle has a unique solution. You will have 4 minutes to solve as many of these puzzles as you can.

Please look at your screens now to see what the graphical interface for the task looks like. You should see five scrambled letters in yellow boxes. (Please raise your hand if you do not see the task interface on your screen.)

- To use the interface, click on the letters in the yellow boxes in the order you think the letters should appear. As you use the letters, they will appear in the white box.

- Once you have used all five letters, click on the red SUBMIT button. You must use all 5 letters before you can submit a guess.

- When you submit your guess, you will see feedback on your performance in the lower box and a new puzzle will appear. The feedback includes a running tally of how many puzzles you have solved correctly. This is private information for you only. None of the other participants in the experiment will see how many correct and incorrect answers you have.

- For any puzzle, you can clear your entry and start a word over by clicking on the gray CLEAR button.

- If you get stuck, you can obtain a new set of letters by submitting an incorrect guess. Wrong answers do not count against you.

- There are a total of 40 possible puzzles and you will see each puzzle exactly once. We will also present the puzzles in exactly the same order to every participant.

If Part One is randomly selected for payment, you will be paid 75 cents for each anagram puzzle you solve correctly in the 4 minutes. Note that your payment will not decrease for incorrect answers.

You can try the interface and unscrambling a few practice words now. There are a total of 3 practice puzzles, and we will begin once everyone has attempted all of the practice puzzles.

Please do not talk with one another.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Part Two: Elections

As in Part One, you will be given 4 minutes to solve as many anagram puzzles as you can (and there will be a completely different set of 40 puzzles). However, your payoffs will be based on both your own performance and the performance of an elected Representative. We will hold a series of elections to choose the Representative, and you will be a candidate in some of these elections.

There are three steps in this part of the experiment.

Step #1: Make decisions as a candidate.

Step #2: Vote in a series of elections.

Step #3: Complete the anagram task again.

In each election, we will randomly select one member from each group to be a candidate (so that there are exactly two candidates in each election). Each candidate will be assigned a letter, and each letter is unrelated to your ID number or your group number. The order and pairing of candidates will be completely random.

You will then vote for one of the two candidates and the winner of each election will be the candidate with the most votes. When you vote, the only information you will have about each candidate is the candidate's assigned letter and a campaign message (explained later). You will not know which group the candidate is from. If there is a tie, the winner will be determined randomly. We will then randomly pick one of the elections to count and the winner of that election will be the Representative.

If Part Two is selected for payment, we will randomly select one election to count and compute your payoffs based on the results of that election as well as the results of the anagram task. Each election is equally likely to be selected.

The rules for each election will be slightly different, so pay close attention to the instructions that follow.

[over]

Rules for Elections 1-5

In the first set of elections (1-5), you will be a candidate exactly once. As a candidate, you will choose a campaign message and this will be the only information that voters will have about you.

There are four possible messages you can send. The content of those messages will depend on how many anagrams you correctly solved in Part One of the experiment in the following way. If the actual number of anagrams you solved is some number X, the four possible messages are:

Message 1: "I solved a lot of puzzles."

Message 2: "I solved X puzzles."

Message 3: "I solved X+3 puzzles."

Message 4: "I solved X+6 puzzles."

For example, if you solved 10 puzzles correctly, then voters will see "I solved 10 puzzles" if you choose Message 2; they will see "I solved 13 puzzles" if you choose Message 3; and they will see "I solved 16 puzzles" if you choose Message 4. If you solved 13 puzzles, then voters will see "I solved 13 puzzles" if you choose Message 2; they will see "I solved 16 puzzles" if you choose Message 3; and they will see "I solved 19 puzzles" if you choose Message 4. No matter what your score X, voters will always see "I solved a lot of puzzles" if you choose Message 1.

Note that voters will only see the content of your message, not the number of the message you choose. In other words, if you choose Message 1, voters will only see "I solved a lot of puzzles," and if you choose Message 2, they will only see "I solved X puzzles." They will NOT see "Message 1" or "Message 2."

If one of these elections (1-5) is randomly selected for payment, you will be paid 25 cents for each anagram you correctly solve in Part Two and 50 cents for each anagram the Representative correctly solves during Part Two. (If you are elected as the Representative, you will be paid 75 cents for each correct answer you provide; other group members will earn 50 cents for each correct answer you provide.)

Before we explain the next set of elections, we would like you to choose your campaign message for Elections 1-5 now.

Remember not to talk with anyone during the experiment.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Rules for Elections 6-10

In the next set of elections (6-10), you will also be a candidate exactly once. The procedures for this series of elections are exactly the same as the previous series, with one difference. [If you are elected as the Representative and one of these elections is selected for payment, you will be paid a bonus of $2.50 in addition to your earnings from the anagram task. Only the Representative earns this bonus.] This bonus is in addition to your earnings from the anagram task (50 cents for each of the Representative's correct answers and 25 cents for each of your own). Other than this, everything else about this series of elections is the same as in the previous series.

Before we move on to the next series of elections, we would like you to choose your campaign message for Elections 6-10 now.

Remember not to talk with anyone during the experiment.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

[In the Group Bonus Treatment, the text above in brackets is replaced with the following: "If you are elected as the Representative and this part of the experiment is selected for payment, every member of your group will be paid a bonus of $2.50. In other words, every member of the elected Representative's group will earn the bonus, not just the individual Representative."]

Part Three: Probability Beliefs

We will return to **Part Two** and finish the election in a moment. Before we do, we'd like to ask you what you believe the probability of different events to be. For example, we'd like to know how likely you think it is in the election that your Part 1 score is higher than your opponent's Part 1 score, or how likely you think your opponent chose the message "I solved X puzzles." These quantities will be expressed in terms of a "percentage chance." If we select this part for payment, we will pay you $5 in addition to whatever you earn based on the choices you make in this part. We will ask you for your beliefs using the following procedure.

Suppose, for example, that we have a coin that is slightly bent so that we don't know exactly what the probability is that the coin comes up heads. To elicit your probability belief that a random toss of this coin comes up heads, we can ask you a series of questions about whether you would prefer to be paid based on the coin toss or paid based on a random draw of a (virtual) bingo ball with a known probability.

More specifically, suppose there is a virtual Bingo Cage with 100 balls. Some of these balls are RED and some of these balls are WHITE. If we draw a RED ball from the cage, you win a $15 prize. You will know how many RED balls are in the cage and the number of RED balls corresponds to your probability of winning. For example, if there are 75 RED balls out of 100, then you have a 75% chance of winning. Similarly, if there are 30 RED balls out of 100, then you have a 30% chance of winning.

We could then ask you a series of questions about whether you would prefer to be paid based on the outcome of the coin toss or the outcome of the Bingo Cage for different probabilities (one question for each probability value):

- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage with 0 red balls (0% chance of getting $15)?
- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage with 5 red balls (5% chance of getting $15)?
- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage with 10 red balls (10% chance of getting $15)?

  …
- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage with 90 red balls (90% chance of getting $15)?
- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage with 95 red balls (95% chance of getting $15)?
- Would you rather have $15 if the coin comes up heads, or use a Bingo Cage to get a 100% chance of getting $15?

Presumably, for the first question you would choose the coin toss instead of the Bingo Cage with the 0 red balls because your probability belief that the coin comes up heads is higher than the 0% chance of winning the Bingo Cage. Similarly, in the last question you would choose the Bingo Cage with 100 red balls instead of the coin toss since the Bingo Cage chance of winning is 100% and is higher than your probability belief. At some point, you would switch to where the probability of winning the Bingo Cage is exactly equal to your belief that the coin will come up heads. We would say that you are indifferent between the two choices because you are just as likely to get heads as you are to win the Bingo Cage, and the point at which you are indifferent is called your "probability value."

Instead of having to answer all such questions, you will click on a table to indicate your probability value and then we will automatically fill in your answers to the rest of the questions based on your probability value. You will see a table similar to the one below. When you click on your probability value, we will select the Bingo Cage for all questions where the Bingo Cage has a higher percentage chance than your probability value, and we will select your probability belief for all questions where the Bingo Cage has a lower percentage chance than your probability value. (You will be paid for the Bingo Cage in the case where the Bingo Cage probability is the same as your probability belief.) The probability belief will be displayed at the top of the screen, and your choice between the probability belief and the Bingo Cage for each row will be highlighted in red.

A

What is your belief that your Part 1 score was HIGHER than your opponent's score?

___ %

Click on a box below on the right to indicate your probability belief

Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (0 red balls out of 100 = 0% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (5 red balls out of 100 = 5% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (10 red balls out of 100 = 10% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (15 red balls out of 100 = 15% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (20 red balls out of 100 = 20% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (25 red balls out of 100 = 25% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (30 red balls out of 100 = 30% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (35 red balls out of 100 = 35% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (40 red balls out of 100 = 40% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (45 red balls out of 100 = 45% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (50 red balls out of 100 = 50% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (55 red balls out of 100 = 55% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (60 red balls out of 100 = 60% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (65 red balls out of 100 = 65% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (70 red balls out of 100 = 70% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (75 red balls out of 100 = 75% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (80 red balls out of 100 = 80% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (85 red balls out of 100 = 85% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (90 red balls out of 100 = 90% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (95 red balls out of 100 = 95% chance)
Win $15 if your score is higher than opponent's	Win $15 if red ball drawn (100 red balls out of 100 = 100% chance)

Notice that if the coin were truly fair and was equally likely to come up heads as tails, it would make sense to choose 50% as your probability value. For a coin that is bent and is not exactly fair, you would state whatever the probability value that you believed to be the true value. There is no right or wrong answer except that your probability value should reflect your true belief. This is because the value of the prize is the same regardless of whether you choose to be paid for your probability value or the Bingo Cage, and the only difference between the two is the probability of winning. Thus, stating your true belief ensures that you will maximize your chances of winning the prize.

If we select Part Three for payment, then we will randomly select one of the beliefs that we ask you about to pay you (A, B, C, D, E, or F as described below), then we will randomly select one of the rows of the table, and then finally, we will pay you if your belief is correct or for the outcome of the Bingo Cage.

For example, suppose the event we pay you for is your belief that your Part 1 score is higher than your opponent's score, and suppose also that your probability value for this event is 40%. Let's say that we then randomly select the row in the table where the Bingo Cage has a 65% chance of winning. Since your probability value is 40% you will be paid for the Bingo Cage (because the Bingo Cage probability is higher and maximizes the probability you win the prize). We would then draw a ball from the (virtual) Bingo Cage and determine the amount to pay you based on the color of the virtual ball.

Before we proceed with the probability belief task, we will do a practice exercise to make sure everyone understands how this procedure works. Suppose we roll a 10-sided die and that you win a prize if the die shows the numbers 1, 2, or 3. What is your probability belief that you will win a prize from the roll of the 10-sided die? In a moment, please look at your screens and select your probability belief that the die will land 1, 2, or 3. Click OK when you have selected your belief.

IF YOU HAVE ANY QUESTIONS, PLEASE RAISE YOUR HAND.

Answer to comprehension exercise

For the practice question, the correct probability belief is 30% because we know that there are 3 out of 10 possible winning numbers.

Notice that if you entered a probability belief higher than 30%, then this means that there are some rows for which we would pay you for your belief (the outcome of the die roll) even though the Bingo Cage has a higher probability of winning. For example, if you answered a probability belief of 50%, then if we pay you for the choice between the die roll or the Bingo Cage with 50 balls, we would pay you for the die roll (30%) even though your chance of winning the Bingo Cage is higher (50%).

Similarly, if you entered a probability belief lower than 30%, then there are some rows for which we would pay you for the Bingo Cage even though the Bingo Cage has a lower probability of winning. For example, if you put 10% as your probability belief and we pay you for the choice between the die roll or the Bingo Cage with 10 balls, we would pay you for the Bingo Cage (10%) even though your chance of winning the die roll is higher (30%).

For the probability beliefs that follow, there is no right or wrong answer except that the best thing to do is to enter your true beliefs. Entering your true beliefs maximizes your chance of winning the prize. Remember that we will pay you $5 for sure if we select this part for payment and an additional $15 depending on the outcome of your belief or the Bingo Cage.

We are only interested in the beliefs you have about your first election opponent (elections 1-5). Recall that if we select the first election for payment, you receive $0.50 for each of the representative's correct answers and $0.25 for each of your own, and that there is no bonus for winning or cost of being a candidate.

Specifically, we are interested in your probability beliefs about the following:

A) The probability you solved more puzzles than your first election opponent.

B) The probability you solved more than 3 puzzles than your first election opponent.

C) The probability you solved more than 6 puzzles than your first election opponent.

D) The probability your first election opponent chose the message "I solved X puzzles."

E) The probability your first election opponent chose the message "I solved X + 3 puzzles."

F) The probability your first election opponent chose the message "I solved X + 6 puzzles."

In addition, notice that according to the laws of probability, your beliefs must obey the following conditions: $A \geq B \geq C$ and $D + E + F \leq 100$. The interface will ensure that these conditions are met. After you enter your probability beliefs, we will also give you a chance to confirm or adjust your beliefs. **Other than these rules, remember that there are no right or wrong answers, and the best thing you can do is to indicate your true beliefs.**

There will be one more practice question before we begin the probability belief task. Suppose instead of a 10-sided die that we have a FOUR-SIDED DIE and that you win the prize if a roll of this die lands 1, 2, or 3. What is your probability belief that you would win a prize from a roll of this four-sided die?

After you answer this question, you will receive feedback on your computer screen and then you will proceed to the probability belief task.