Competition, Preference Uncertainty, and Jamming: A Strategic Communication Experiment*

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Abstract

Modern democracies are built on an edifice of competition and information asymmetry, yet citizens must remain uncertain about the preferences of those they rely upon. But competition and preference uncertainty enable the informed to jam, thereby impeding communication. We describe a game-theoretic laboratory experiment in which subjects play an information transmission game with two senders who have private information about their preferences. Although we find support for many equilibrium predictions, we also observe that senders "overjam," exaggerating even when they are predicted to tell the truth. This finding stands in contrast to the well-documented overcommunication that emerges in noncompetitive information transmission experiments. Overjamming can be explained by a framework in which senders choose messages based on experience within the experiment, exaggerating more when they have observed their opponents exaggerate more previously. Interestingly, we also find that senders overjam in an understated way, exaggerating less than would maximize their payoffs.

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Competition permeates democracies. Candidates for high office offer competing visions for national policy, lobbyists and legislators construct arguments for and against legislation, and adversarial courts rely on opposing advocates to inform their decisions. Although these highly specialized institutions differ significantly in their details, each involves informed and interested parties pressing rival ideas on key decisionmakers. But no matter how well known these informed parties are, each retains a penumbra of privacy about their fundamental motives, leaving everyone else uncertain about what they really want or believe.

Our understanding of competitive political communication is limited, despite its seeming ubiquity. In the abstract, *competition* between information providers creates opportunities to obfuscate rather than resolve controversy (Minozzi 2011). When faced with competing claims, relatively uninformed decisionmakers can be left without the means to make informed judgments. Although an information provider may be counted on to tell the truth when it suits her purposes, another for whom the truth has unpalatable consequences might *jam* her opponent's message and leave decisionmakers uncertain about who told the truth. Because the informed parties retain private information about their own preferences, key decisionmakers may not be able to infer the truth from these claims.

If competition—a linchpin of democracy—impedes the communication of information to those who need it, the implications for democratic governance will be significant. On the institutional side, legislatures are organized around delegating information-intensive tasks to committees(Kiewiet and McCubbins 1991), but these committees may be composed of opposing interests (Gilligan and Krehbiel 1989). Legislators design bureaucracies in part to generate useful information (Bawn 1995; Bendor, Taylor, and Van Gaalen 1985), but they delegate tasks to many overlapping agents (Landau 1969). Courts depend on advocates to articulate legal rationales and fact patterns (Bailey, Kamoie, and Maltzman 2005), but opposing sides of an issue are represented. An informed public plays a key role in the legitimacy of judicial institutions (Carrubba 2009; Staton 2006; Vanberg 2001), but legitimacy can be contested and threatened by competing interests. Information cues can help voters learn from and be persuaded by elites (Lupia and McCubbins 1998), public opinion develops amidst a cacophony of conflicting messages (Zaller 1992). And representative government itself depends on a division of intellectual labor between office-holders and citizens, but competitive elections are the chief mechanism of accountability (Canes-Wrone, Herron, and Shotts 2001; Fox and Shotts 2009). Each of these environments is characterized by communicative competition.

Some argue that competition facilitates the communication of information. Indeed, more heterogeneous committees are thought to yield more credible information (Gilligan and Krehbiel 1989; Krehbiel 1991). Competition among lobbyists may lead to better decisions by legislators (Austen-Smith and Wright 1992), and competition among elites may engender a more fully informed public (Page and Shapiro 1992). But such competition may also simply offer more chances to jam rather than to facilitate learning. Thus, it is crucial to learn how people behave when faced with this strategic situation.

Despite the importance of communicative competition, our understanding of how people behave in such situations is crucially limited. A large body of experimental work in the social psychological vein explores how and to what extent subjects' decisions can be manipulated via agenda setting, priming, and framing (e.g., Chong and Druckman 2007; Iyengar and Kinder 1987; Nelson, Clawson, and Oxley 1997). Although we have learned a great deal from this work, it cannot tell us how would-be manipulators might behave. This omission is surprising, not least because the terms themselves—agenda setting, priming, and framing—imply that someone is engaged in strategic action. One reason for this omission might be that social psychological experiments are not typically designed to explore the extent of strategic political behavior.

Experimental economists have thoroughly studied Crawford and Sobel's (1982) seminal cheap talk, sender-receiver game to understand how and why subjects stray from game-theoretic, equilibrium behavior (e.g., Blume et al. 1998, 2001; Cai and Wang 2006; Crawford 1998; Dickhaut, McCabe, and Mukherji 1995; Gneezy 2005; Hurkens and Kartik 2009).

And in political science, Lupia and McCubbins (1998) conduct experiments based on similar games in which the decisionmaker is uncertain about the (single) informed player's preferences. The universal finding from these experiments is the "overcommunication" phenomenon: in experiments, subjects tell the truth far more often and lie far less egregiously than equilibrium analysis predicts they will. But none of these studies focuses on communicative competition with uncertainty about the preferences of the information providers.[1]

We report on experiments that test the predictions of a game-theoretic model of communicative competition. Our findings lend support to many of the equilibrium predictions, including a set of seemingly counterintuitive comparative static hypotheses. But we also identify key points of departure from equilibrium predictions. Surprisingly, senders in the competitive communication experiment "overjam" rather than overcommunicate. That is, senders who are predicted to tell the truth instead send messages that are false. To explore the etiology of this phenomenon, we explore a range of hypotheses based on bounded rationality and limited strategic sophistication. We find that overjamming is best explained by a framework in which subjects play best responses based on their experience within the experiment. Intriguingly, senders seem to counteract the exaggerations they would expect their opponents to make, exaggerating more when they have observed their opponents exaggerate more previously.

Theory and Hypotheses

The strategic situation faced by competitive political elites and experts is far different from that facing an information provider in a non-competitive environment. Competition among information providers allows receivers to compare and contrast the messages they receive,

[1] Boudreau and McCubbins (2008) have their subjects solve math problems with the help of "experts"—other subjects who have access the correct answers. These experts are privately informed of whether they will benefit if the problem-solver gets the answer right, or if she gets it wrong, and then they offer her suggested answers. While our setting shares some limited features with theirs, ours affords a much richer information structure and set of messages and, therefore, much more varied communication strategies. And our setting is spatial, meaning that our findings have straightforward applications to many fundamental formal models of politics.

but it also breeds novel opportunities for senders to obfuscate. These opportunities can change dramatically from setting to setting; arguments before the Supreme Court can look far different from presidential debates. However, the strategic aspects of communicative competition can be boiled down to their most elemental form using tools from game theory.

Consider the simplest political environment in which there can be communicative competition: one with two senders and one receiver.[2] At the outset, both senders observe a *target* T, which functions as the "truth" in the game. Each sender i also privately knows his[3] own preferences, represented by a *shift* S_i. The senders then select messages m_i to send to the receiver, who then chooses an action c. The receiver prefers c to be as close as possible to T; her payoff is $-|c - T|$. In contrast, each sender i prefers that c be as close as possible to $T + S_i$, and his payoff is $-|c - (T + S_i)|$. In terms familiar from the spatial model of politics, T is the receiver's ideal point and $T + S_i$ is sender i's ideal point.

More specifically, the target T is uniformly distributed on the interval $[-100, 100]$, senders choose messages $m_i \in [-150, 150]$, and the receiver chooses $c \in [-150, 150]$.[4] In our discussion, we designate one sender as the *left sender* with shift $S_L \in [-50, 0]$, so that his ideal point is always to the left of the receiver's. The other sender is the *right sender* with shift $S_R \in [0, 50]$, so that his ideal point is always to the right of receiver's. Thus, the senders are *opposed* but the receiver is uncertain who is *closer* to her. In our *Symmetric Baseline* experimental condition, both shifts are uniformly distributed.

The distributions of T, S_L, and S_R are common knowledge. Equilibria of this game involve messages that are either *truthful* or that *jam* truthful messages.[5] In equilibrium, the receiver learns exactly what the target is if and only if both senders send truthful messages, and so messages agree. In this case, she responds by choosing $c = T$. If the messages do disagree, the receiver understands that at least one sender must have jammed, and she

[2] For a detailed formal analysis of this game, see Minozzi (2011).

[3] We use male pronouns to refer to senders and female pronouns to refer to receivers.

[4] In equilibrium, no player should ever choose m or c less than -100 or greater than 100, and, in the experimental sessions, most do not.

[5] "Equilibrium" refers to Perfect Bayesian equilibrium with beliefs as specified in Minozzi (2011).

therefore cannot determine the true target. In that case, the receiver's equilibrium response is to choose a default action $c_0 = 0$, which is the receiver's expectation of the Target based on her prior beliefs and hence in the absence of messages.[6]

If the receiver uses this strategy, the sender recognizes that the only possible equilibrium path actions are $c = T$ and $c = c_0$. Truthtelling might lead the receiver to choose $c = T$ (depending on the other sender's message), but jamming will always lead to $c = c_0$. Thus, sender i prefers to tell the truth when her ideal point $T + S_i$ is closer to T than to c_0. In contrast, i prefers to jam when $T + S_i$ is closer to c_0. Given these options, senders reveal T when it is on "their" side of c_0. When $T < c_0$, T is to the left of c_0, truthtelling is optimal for the left sender because his ideal point is to the left of both equilibrium actions. When $T > c_0$, the right sender similarly prefers to tell the truth. But senders also prefer to tell the truth for very extreme targets on their opponent's side. For example, when $T > c_0 + 2|S_L|$, the target is to the right of c_0. However, truthtelling is optimal for the left sender because T is so extreme that c_0 is further from the left sender's ideal point than is T. Similarly, the right sender will reveal the truth when $T < c_0 - 2|S_R|$. Otherwise, senders will jam. We refer to $[c_0, c_0 + 2|S_L|]$ and $[c_0 - 2|S_R|, c_0]$ as the *jamming regions* for the left and right sender, respectively.

The receiver's strategy outlined above is optimal given these message strategies. If the receiver observes messages that disagree, she does not know who has jammed and who has revealed the target. However, she can use her prior information to judge the likelihood that each sender has lied, based on the messages she observed. She chooses the optimal c based on this information.

To ensure that in equilibrium the receiver always wants to choose $c = c_0$ when she observes messages that disagree, a sender's jamming message m_J depends on how likely his opponent would have been to jam if m_J were the true target rather than T. In our

[6] Other default actions can support other equilibria, but $c_0 = 0$ is a natural focal point. For example, in the Symmetric Baseline condition, any $c_0 \in (-100, 100)$ can support an equilibrium with similar strategies to those we describe. The differences between the strategies entail the emergence of intervals of the Target space in which senders pool. Details are available upon request.

Symmetric Baseline Condition, both senders have shifts that are uniformly distributed. In this case, $m_J = 2c_0 - T$, which is the opposite of T reflected through c_0. Intuitively, to choose the default action, the receiver must be completely unable to determine which sender lied. Because the senders' shifts are symmetrically distributed, the receiver infers that T and $2c_0 - T$ are equally likely, and settles on their average, which is exactly c_0.

Our first set of hypotheses summarizes the equilibrium point predictions from the analysis above, predicated on the focal default action $c_0 = 0$.

Equilibrium Messages Hypothesis. *In the Symmetric Baseline Condition, when the target is in a sender's jamming region, she will send the jamming message, $m_i = -T$. Otherwise, she will send the truthful message, $m_i = T$.*

Equilibrium Actions Hypothesis. *In the Symmetric Baseline Condition, when messages agree, $m_L = m_R = m$, the receiver will choose an action equal to the target, $c = T$. Otherwise, the receiver's will choose the default action, $c_0 = 0$.*

The Equilibrium Messages Hypothesis recapitulates the message strategy described above and indicates that jamming messages will be "countervailing"; they move in opposite direction of the target. The Equilibrium Actions Hypothesis predicts that the receiver will choose the target exactly only if she is absolutely sure what it is, which occurs only when the messages match. Otherwise, the receiver will choose the focal default action $c_0 = 0$.

In addition to these point predictions, we test several comparative static hypotheses. For example, the receiver may believe that senders' messages include small errors, in which case messages are unlikely to ever agree. In this case, the receiver will treat messages that are closer to each other as more indicative of the underlying target than messages that are far from each other. Thus, the receiver should be better able to guess the target when the difference between messages is small.

Message Difference Hypothesis. *In comparative terms, the receiver should be less able to guess the target when the difference between the senders' messages is larger.*

We also test a set of seemingly counterintuitive comparative static predictions about how sender behavior changes when we relax symmetry. To that end, we posit an *Asymmetric Condition* in which the left sender has a shift that is more likely to be further from 0, while the right sender's shift distribution is unchanged. In this Asymmetric Condition, we call the left sender the *extremist*, and the right sender the *moderate*.[7]

Perhaps surprisingly, the extremist is predicted to send more moderate jamming messages in equilibrium. The reason is that even though shifts are asymmetrically distributed, the jamming messages must still ensure that the receiver chooses $c = c_0$ when messages disagree. Suppose that messages were still symmetric reflections of each other. If the receiver believes the extremist is more likely to have lied than the moderate, she choose an action that is on the Moderate's side of c_0. Thus, the extremist will have to moderate his messages to offset this change. In turn, the moderate's jamming messages must become more extreme, via the usual equilibrium requirements. Importantly, this change should occur even though the right sender has the same shift distribution in both conditions.

Extremist Moderation Hypothesis. *In the Asymmetric Condition, the Extremist will send more moderate jamming messages than will the Moderate.*

Moderate Extremism Hypothesis. *Suppose a sender's shift distribution does not change across conditions. If that sender is the Moderate in the Asymmetric Condition, he will send more extreme jamming messages in that condition than he does in the Symmetric Baseline Condition.*

In experimental settings, it is possible that substantial differences between predictions and evidence will emerge. Such explanatory lacunae may reflect many underlying reasons. For example, overcommunication regularly emerges in economic experiments based on (non-competitive) communication games (Gneezy 2005; Cai and Wang 2006), and are thought to be reflect truthtelling norms (Sánchez-Pagés and Vorsatz 2007). Alternatively, subjects may

[7] We formally define, a sender to be more *moderate* if the distribution of $|S_{Moderate}|$ is first-order stochastically dominated by the distribution of $|S_{Extremist}|$ for the other sender.

merely be boundedly rational or cognitively constrained. An even more likely possibility is that subjects will not play best responses perfectly. In that case, higher stakes should encourage subjects to think more about the game and thus to be more likely to play equilibrium strategies. Subjects should also learn to play equilibrium strategies as they gain more experience.

Payment Sensitivity Hypothesis. *Higher payments will lead subjects to play strategies that are more similar to those predicted in equilibrium.*

Learning Hypothesis. *After many rounds of the game, subjects will play more in line with equilibrium predictions.*

These hypotheses offer a well-defined set of expectations for the behavior of senders and receivers in communicative competition. However, to accurately and credibly test them, we need to know not only what senders say, but what they believe to be true. This is extremely difficult using observational data, and, thus, we turn to the lab.

Experimental Procedures

We conducted our experiments at the Pittsburgh Experimental Economics Laboratory using subjects recruited through the lab's centralized database. Most subjects were undergraduates from the University of Pittsburgh or Carnegie Mellon University, and no subjects were recruited from the authors' classes. Each subject participated in only one session.

Upon arriving at the lab, subjects gave informed consent and were seated at separate computer terminals. All interactions between subjects took place anonymously through the networked computers using software programmed and conducted using z-tree (Fischbacher 2007). Subjects received strict instructions not to communicate with one another in any way throughout the session. The instructions were presented on their computer screens and read aloud in an effort to induce common knowledge among the participants. Subjects received printed copies of the instructions, to which they were encouraged to refer as often as they needed, and were given a quiz about the instructions in order to ensure comprehension. The quizzes were administered through the computers so that subjects privately received immediate feedback about whether or not they answered questions correctly and explanations of the correct answers. Consistent with the lab's governance policy, no deception or false feedback was used in the experiment.

After the instructions and quiz, the software randomly assigned subjects to one of the roles in the game: A (left sender), B (right sender), or C (receiver). The instructions only referred to the roles as "A," "B," or "C" and made no reference to "senders," "receivers," "left," "right," "moderate," or "extremist." In our presentation and discussion, however, we continue to use these terms. Subjects proceeded to play between 12 and 32 rounds of the game (depending on the session and condition), with fixed roles throughout the session.[8]

At the beginning of every round, subjects were randomly matched into groups of three, with one subject in each role in each group. Groups were selected with replacement so that it was possible to be matched with the same group in different rounds. To preclude reputation effects, subjects never knew the ID numbers of the other subjects in their group.

The targets T and shifts S_L and S_R were then drawn independently for each group. In all conditions of the experiment, T was drawn uniformly from the integers between -100 and 100, and the right sender's shift S_R was drawn uniformly from integers between 0 and 50. The distribution of the left sender's shift S_L varied across treatment conditions. In the *Symmetric Baseline Condition*, S_L was drawn uniformly from integers between -50 and 0; in the *Asymmetric Condition*, integers from -50 to -25 were three times as likely as integers from -24 to 0 (with each element of each region equally likely). In the instructions and throughout the experiment, we referred to each player's ideal action as a "target." That is, T is referred to as "C's target," $T + S_L$ is "A's target," and $T + S_R$ is "B's target."[9]

We applied an innovative stratified sampling procedure to ensure an even distribution of

[8] Table A-1 in the Appendix provides summary statistics on the sessions.

[9] In our presentation, we continue to refer to "targets" and "shifts." When we do so, the "target" is understood to be C's target.

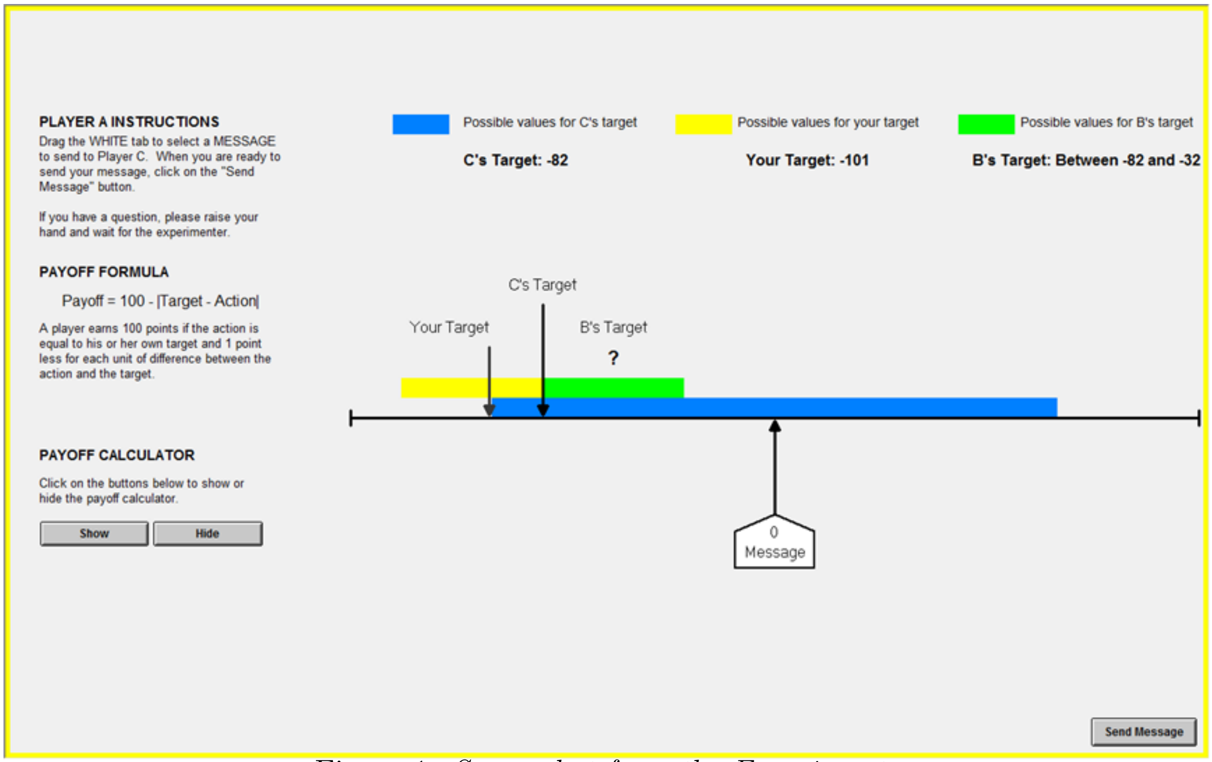

Figure 1: *Screenshot from the Experiment*

targets and shifts. Specifically, our procedure divides the set of targets into 8 subsets and each sender's set of shifts into 2 subset, resulting in $8 \times 2 \times 2 = 32$ regions. Each round's target and shifts correspond to one of the regions. The order in which the combinations are considered is randomized for every session. Thus, from the subjects' point of view, targets and shifts are distributed as described above. Although the supports of the target and shifts are technically discrete, they are fine-grained enough to be considered continuous. Our use nearly continuous distributions is in contrast with previous experiments on cheap talk games that typically involve a small state and action space (4 states in Dickhaut, McCabe, and Mukherji (1995) and 5 states in Cai and Wang (2006)). Without this innovation, we could not have accurately conveyed the notion of the spatial model to the subjects.

The experimental interface we used presents information to subjects textually as well as graphically (see Figure 1). The graphical display intuitively conveys the notion of spatial distance inherent in the utility functions.[10] We reasoned that this would allow subjects to

[10]We thank Stephen Haptonstahl for providing the initial z-Tree code to produce the slider in Haptonstahl

focus their cognitive resources on thinking strategically rather than on computing payoffs. Although the instructions describe the set of targets and shifts as integers, our visual display reinforces the notion that the distributions are to be treated as continuous and spatial.

In every round, each sender simultaneously observed the receiver's target and his own target (but not the other sender's target), and then chose a message. As shown in Figure 1, possible messages and actions are displayed on a horizontal axis in our interface. To send a message, senders use the mouse to drag a slider along this axis to a position that corresponds to the desired message (any position between -150 and 150). The interface also displays the range of possible targets for the receiver, the realized target, the range of possible targets for the sender and for the opposing sender, and the sender's own target, all of which is also presented textually at the top of the screen. The sender's interface also features a payoff calculator (manipulated via a separate slider) that shows the sender's and receiver's payoffs for each possible action the receiver might choose. The receiver observed messages simultaneously after both senders had finished, and the interface displayed this information both graphically and textually. The receiver then dragged a slider to select an action (any position between -150 and 150).

At the end of every round, subjects were informed of all of the results from the round for their group: both messages, the action, every player's target, and every player's payoff. Subjects also observed the results from all previous rounds they played, but they never observed the results for groups to which they did not belong. Payoffs for each round were denominated in "points," with 100 points being the maximum possible points a player could earn in a round (if the receiver's action matched their own target exactly). In terms of points, the receiver's payoff function was $100 - |c - T|$ and a sender's payoff function was $100 - |c - (T + S_i)|$.

At the end of the experimental session, total points were converted to cash at the rate of $1 for every 150 points. Subjects were paid the sum of their earnings plus a $7 show-up

(2009), which we modified substantially.

fee. To investigate the Payment Sensitivity Hypothesis, we ran additional sessions under a *High Payment Condition*, in which the information structure of the Symmetric Baseline Condition was replicated, with two changes: the cash conversion rate was increased to $1 for every 40 points (so that every round in the High Payment Condition was worth $2.50), and subjects played only 12 rounds.

Testing Equilibrium Predictions

We conducted four sessions of the Symmetric Baseline Condition, two of the Asymmetric Condition, and two of the High Payment Condition. Each session involved between 12 and 18 subjects (4 to 6 groups), and ran 12 to 32 rounds. To test the hypotheses using the data from these sessions, we estimate a series of multilevel models to control for subject-, round-, and condition-level heterogeneity. Before beginning, we rescale Targets, Messages, and Actions, dividing each by 100, so that coefficients are all on the same scale with our indicator variables.[11] We report two-tailed tests when testing point predictions, and one-tailed tests for comparative statics.

Sender Messages. In the sessions, senders exhibit some of the behaviors predicted by the Equilibrium Messages Hypothesis. However, significant differences emerge between the theory and the evidence, and there is inconsistent evidence that bounded rationality can explain these differences, at least insofar as predicted by the Payment Sensitivity and Learning Hypotheses. We test the Equilibrium Messages Hypothesis by fitting models with each sender's ***Message*** as the dependent variable. We regress Message on the receiver's ***Target***; an indicator ***Jam***, which equals 1 if the target lies in the sender's jamming region; the interaction ***Target*** × ***Jam***; and a separate intercept for ***Left Sender*** and ***Right Sender***.

Before discussing the results, Figure 2 allows a visual inspection of what we observed in the Symmetric Baseline Condition. Each point represents a Target-Message pair, and these points are presented as (upper- or lowercase) letters: "L" and "l" refer to messages

[11] The Appendix includes tables for all results we discuss.

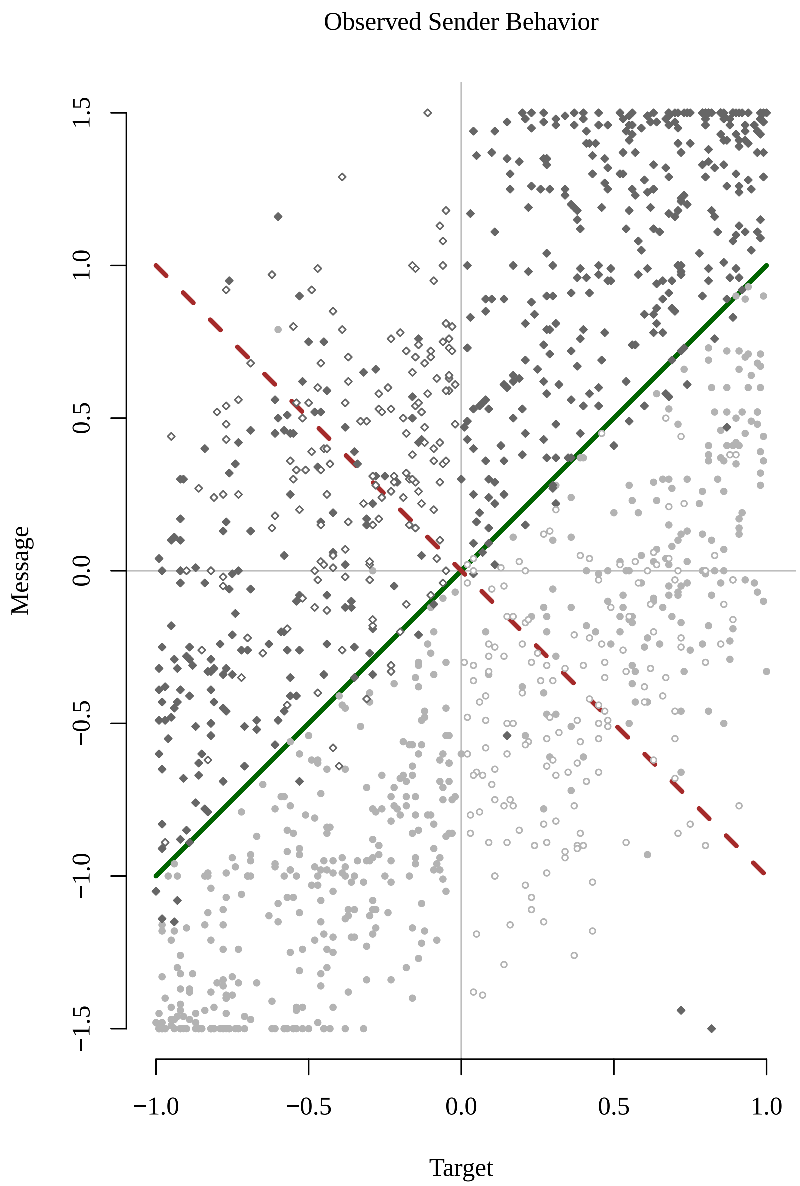

Figure 2: *Observed Sender Behavior.* The figure juxtaposes the theoretical predictions with experimental evidence from the Symmetric Baseline Condition, and clear discrepancies form these predictions are visible. The up-sloping unbroken line depicts the hypothesized relationship between Target and Message when the Target is outside the jamming region, and the down-sloping dashed line represents the hypothesized relationship when the Target is in the jamming region. Turning to the experimental evidence, messages from Right Senders are notated with (darker) diamonds; messages from Left Senders by (lighter) circles. Filled markers represent Targets outside the jamming region, and empty markers represent Targets within the jamming region.

from senders on the left, and "R" and 'r" refer to messages from those on the right. A point is lowercase if Jam = 0 and uppercase if Jam = 1. Equilibrium point predictions are displayed by the unbroken lines (up-sloping for Jam = 0 and down-sloping for Jam = 1), and nonparametric regression lines are fit and displayed by sender type and Jam. According to the equilibrium predictions, lowercase points should hew to the unbroken line, and uppercase points should stick closely to the dashed line with positive slope. There are clearly substantial differences between the equilibrium predictions and the observed evidence.

The Equilibrium Messages Hypothesis predicts that when Jam is 0, senders will tell the truth; thus, the coefficient on Target is predicted to be 1. When Jam = 1, senders are predicted to Jam, implying a coefficient of -2 on Target $\times$ Jam (so that the slope of Target becomes $1 - 2 = -1$). Finally, senders should not add arbitrary constants to their messages. Therefore, the coefficients of Left Sender and Right Sender should both be 0.

The first column (Baseline) of the "Sender Messages" panel in Figure 3 illustrates the coefficients and standard error bars from this model.[12] Starting from the top, the coefficient on Target is 0.92 (0.03), and so we reject the hypothesis that it equals 1 ($p \approx 0.003$, two-tailed). Moving down, the coefficient on Target $\times$ Jam is -0.43 (0.05), and we reject the hypothesis it equals -2 ($p < 0.001$, two-tailed). And the last two rows in the column show that the intercepts for Left and Right Senders are both significantly different from 0, again, contra the point prediction. Interestingly, the Left Sender intercept is significantly less than 0 (i.e., on the left, $p < 0.001$, two-tailed), while the Right Sender intercept is significantly greater than 0 (on the right, $p < 0.001$, two-tailed). Thus, senders seem to "overjam" by exaggerating even when they are predicted to tell the truth. This finding stands in stark contrast to the "overcommunication" commonly observed in non-competitive communication experiments.

One possible reason for the discrepancy between hypotheses and evidence is that subjects are boundedly rational. For example, the stakes for play may be too low to stimulate careful

[12]See Model [3] from Appendix Table A-2 for details.

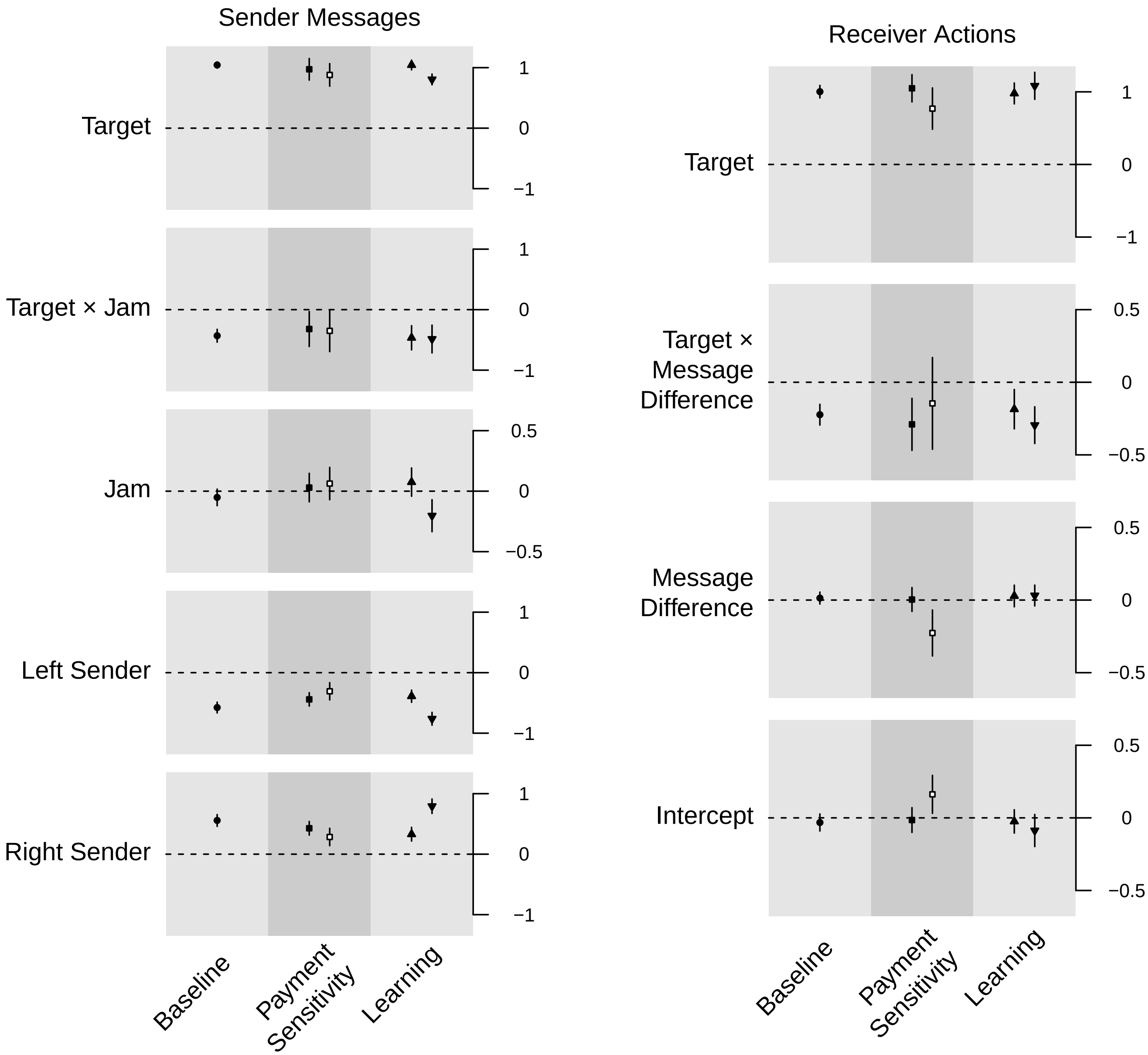

Figure 3: *Equilibrium Messages and Actions.* Each column displays coefficients and standard errors from a separate regression. Comparative statics predictions receive broad support. For senders, the coefficient on Target is not far from its predicted value of 1 (first column). The coefficient on Target × Jam is far from its predicted value −2, but it is negative. But there is also consistent evidence that senders "overjam," or systematically exaggerate their messages. The intercepts on Left Sender and Right Sender are different from 0 in opposite directions, although this effect is moderated in the High Payment Condition (compare white boxes to black boxes in the second column). But senders appear to overjam more in the last round than in the first round (compare down-triangles to up-triangles in the third column). On the receiver side, the coefficient on Target × Message Difference is negative, as predicted. Receivers also seem to be sensitive to payment and to learn, but these effects are also inconsistent.

thought about best responses. To test this possibility, we amend the original regression models to include interactions with an indicator for the ***High Payment***, and use data from both the Symmetric Baseline and the High Payment Conditions.[13] The second column (Payoff Sensitivity) of the Sender Messages panel in Figure 3 presents the results. In the figure, the black squares indicate results for the Symmetric Baseline Condition, while the white squares indicate results for the High Payment Condition. The Payment Sensitivity Hypothesis predicts that the empty squares will be closer to their predicted values than the filled squares. For example, the empty square by Target should be closer to 1 than the filled square.

The results conform to those we hypothesized in only a few cases. The coefficient on Target actually moves down a bit under the High Payment Condition, against the predicted direction. Similarly, the coefficient on Target $\times$ Jam moves up, again opposite to the predicted direction. However, under the High Payment condition, the Jam coefficient does not change significantly, and the intercepts on Left and Right Sender move closer to their predicted equilibrium values of 0. Left Sender becomes 0.13 (0.09) larger ($p \approx 0.07$, one-tailed), while Right Sender becomes 0.14 (0.09) smaller ($p \approx 0.06$, one-tailed). Stakes alone do not seem sufficient to encourage senders to play best responses.

Subjects may also learn how to play best responses as they gain experience. To test this possibility, we again amend the original regression, now using data from the Symmetric Baseline Condition and including interactions with a variable for the ***Round*** of play.[14] The rightmost column (Learning) of the Sender Messages panel in Figure 3 presents these results. Up-pointing triangles indicate results for the first round of play, and down-pointing triangles indicate results for the last. The Learning Hypothesis predicts that the down-pointing triangles should be closer to their predicted values than the up-pointing triangles.

We find no evidence that senders learn to play best responses over time. In the first

[13]To ensure that the difference in number of rounds played in the conditions does not confound the results, these models use only the first 12 rounds of data. See Model [5] from Appendix Table A-3 for details.

[14]Round has been rescaled to lie between 0 and 1. See Model [4] from Appendix Table A-2 for details.

round, the coefficient on Target is indistinguishable from its equilibrium point prediction of 1, but then then it decreases significantly by the last round. There is negligible movement in the coefficient on Target × Jam, which remains far from its predicted value of -2. Finally, the Left and Right Sender intercepts start closer to their predicted values of 0, and then move significantly away. Thus, over time, senders seem to overjam even more.

Some intriguing patterns emerge from this analysis. There is a fairly tight relationship between messages and targets, even when the latter lie outside the jamming region. While jamming messages are decidedly *not* countervailing in practice, senders do temper the correlation between their messages and targets when the latter lie within the jamming region. But senders also seem to overjam by adding large plenary shifts to all their messages, irrespective of the jamming region.

Receiver Actions. Turning to receiver behavior, we find very limited support for the lone point prediction. Receivers are predicted to choose exactly $c_0 = 0$ whenever senders' messages diverge, however minimally. Using the receivers' ***Action*** as the dependent variable, we can quickly reject this point prediction—in the 1024 rounds played in all our sessions, senders *never* sent identical messages, yet receivers chose $c = 0$ in only 34 cases!

We test the comparative static prediction in the Message Difference Hypothesis by regressing Action on ***Message Difference***, the absolute difference between the senders' messages, the interaction ***Target*** × ***Message Difference***, and an intercept. Here, we expect the coefficient on Target × Message Difference to be negative. That is, as the distance between senders' messages grows, the receiver should believe it to be less likely that mere errors caused the discrepancy and more likely to believe that one or both sender has jammed.

The first column of the "Receiver Actions" panel in Figure 3 displays the results.[15] As predicted, we cannot reject the hypothesis that the coefficient on Target × Message Difference is negative, ($p < 0.001$, one-tailed). In the High Payment Condition, receivers' behavior again tends away from equilibrium prediction (second column, "Receiver Actions" panel);

[15] See Model [6] from Appendix Table A-4.

the coefficient on Target $\times$ Message Difference is larger in the High Payment Condition. Receivers also do not display much evidence of learning (this column, "Receiver Actions" panel). The coefficient on Target $\times$ Message Difference decreases by -0.11 (0.11) from the first round to the last ($p < 0.16$, one-tailed).

Although the point predictions are rejected, we find evidence in support of the comparative statics predictions. In the next section, we test the set of seemingly counterintuitive comparative statics that relax the symmetry of the senders.

Extremist Moderation and Moderate Extremism

We next test our main comparative statics predictions, Extremist Moderation and Moderate Extremism Hypotheses, using data from the Symmetric Baseline and Asymmetric Conditions. In the latter, the left sender's shift is likely to be larger than the right sender's; thus, we refer to the left sender as the "Extremist" and the right sender as the "Moderate." We implemented this asymmetry by altering the distribution of the left sender's shift, leaving that of the right sender unchanged across conditions. There is consistent and broad support for these hypotheses, and this support increases the longer they play, as per the Learning Hypothesis.

The Extremist Moderation Hypothesis predicts that the Extremist will send more moderate jamming messages than the Moderate. The Moderate Extremism Hypothesis predicts that the right sender, whose distribution does not change across conditions, sends more extreme jamming messages in the Asymmetric Condition than in the Symmetric Baseline Condition. We test these hypotheses by regressing the ***Distance*** between a sender's message and the receiver's target on a series of indicator variables—Jam, Left Sender, Right Sender, and an indicator for the ***Asymmetric*** Condition—and all their interactions. According to the Extremist Moderation Hypothesis, the Average Distance of jamming messages for Extremists in the Asymmetric Condition should be smaller than that for Moderates. According to the Moderate Extremism Hypothesis, the Average Distance for the right sender should be larger in the Asymmetric Condition than in the Symmetric Baseline Condition. We also estimated a regression including interactions with Round to test the Learning Hypothesis in this context. Here, we expect the findings associated with the comparative statics hypotheses to be more evident in the last round than in the first round.

Figure 4 presents the results from these two regressions. Filled markers indicate the Average Distance when the target is not in the jamming region, and empty markers indicate that Average Distance when the target does lie in the jamming region. The circles present point estimates of Average Distance across rounds.

In almost every comparison, Average Distance is higher in the jamming region.[16] Moreover, across the two conditions, these differences increase for the right sender—who becomes more moderate (relatively)—but decrease for the left sender—who becomes more extremist.

We now focus on the empty circles, which average Distance for jamming messages in a session. In the Asymmetric Condition, (empty circles in the gray boxes), Average Distance is 0.18 (0.09) lower for the Extremist than for the Moderate, in support of the Extremist Moderation Hypothesis ($p \approx 0.03$, one-tailed). To test the Moderate Extremism Hypothesis, compare Average Distances for the right sender across the two conditions (the empty circles in the top half of the figure). Here, we see that the right sender's Average Distance is 0.03 (0.07) higher in the Asymmetric Condition, as predicted by the Moderate Extremism Hypothesis, although this difference is not statistically significant ($p \approx 0.35$, one-tailed).

Just as subjects might have learned to play as specified by the equilibrium point predictions, they may learn to play as in these comparative statics predictions. The triangles indicate average Distance in different rounds. Up-pointing triangles refer to results for the first round, and down-pointing triangles refer to the last.

In every case, Average Distance is larger in the last round than in the first. During the first round, there is little difference across senders, conditions, and types. In the last round,

[16]Comparing when the target is in the jamming region and not: Average Distance is 0.16 (0.02, $p < 0.01$) higher for the right sender in the Symmetric Condition; 0.21 (0.04, $p < 0.01$) for the (moderate) right sender in the Asymmetric Condition; 0.23 (0.02, $p < 0.01$) higher for the left sender in the Symmetric Condition; and 0.056 (0.035, $p \approx 0.05$) higher for the (extremist) left sender in the Asymmetric Condition.

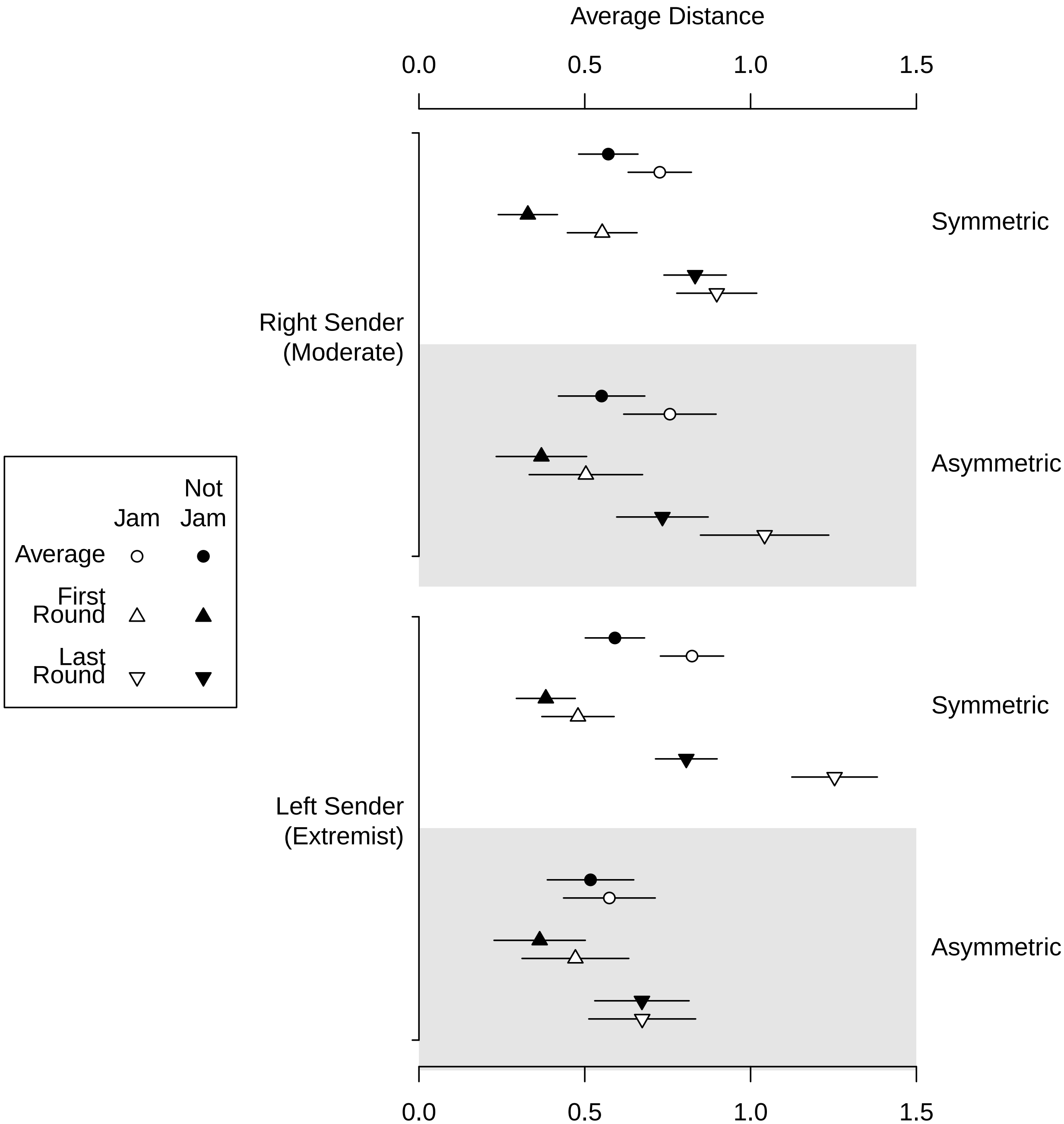

Figure 4: *Extremist Moderation and Moderate Extremism.* There is consistent evidence for both comparative statics predictions. Extremists send more moderate jamming messages than Moderates on average (empty circles in gray boxes). And they learn to do so. In the first round there are no differences between senders (empty up-triangles), yet by the last round, large differences emerge (empty down-triangles). Moreover, right senders' jamming changes across conditions even though their shift distributions do not. By the last round (top half of the figure, empty down-triangles), right senders send more extreme jamming messages in the Asymmetric than the Symmetric Condition. Compared with the first round (empty up-triangles), right senders become more extreme in the Asymmetric Condition.

we see evidence in support of both comparative statics hypotheses. When the target is in the jamming region in the Asymmetric Condition (empty down-pointing triangles in the gray boxes), Average Distance is 0.37 (0.13) less for the Extremist than for the Moderate ($p \approx 0.001$, one-tailed). Comparing the results for the right sender across conditions (empty down-pointing triangles in the top half of the figure), the Average Distance is 0.14 (0.11) higher in the Asymmetric Condition ($p \approx 0.10$, one-tailed). While this finding grazes conventional significance thresholds, the learning gap between Average Distance in the first and last rounds for Right Sender in the Symmetric Condition is also 0.194 (0.145) smaller than that for the Asymmetric Condition, and this finding is more significant ($p \approx 0.091$, one-tailed).

Despite this broad support for an intricate set of seemingly counterintuitive comparative statics, evidence of the overjamming phenomenon again emerges. In Figure 4, each black circle and triangle is predicted to be equal to 0. All exceed that mark. Moreover, overjamming seems to increase as play proceeds. It is therefore implausible that mere lack of best response play is responsible for overjamming. If that were the case, the effects should decrease over time. Instead, we must consider other possible explanations for this phenomenon.

Limited Strategic Sophistication

In equilibrium, each player is assumed to choose the best response given her beliefs about what others will do, but those beliefs are also assumed to be consistent with what others actually do. Indeed, the fundamental idea underlying equilibrium analysis is the *mutual consistency* of beliefs and actions. The Payment Sensitivity and Learning Hypotheses we tested above relaxed the best response part of equilibrium analysis, but with yielded inconsistent results. In this section, we instead maintain the best response assumption, but relax mutual consistency of beliefs and actions.

To relax mutual consistency is to allow subjects to vary in their strategic sophistication. Although a substantial literature in economics investigates limited strategic sophistication—experimentally (e.g., Camerer, Ho, and Chong 2004; Costa-Gomes, Crawford, and Broseta

2001; Nagel 1995; Stahl and Wilson 1995) and theoretically (e.g., Crawford 2003)—such models are more rare in political science.[17] Here, we apply three different frameworks for studying limited strategic sophistication. To be clear, our goal is not to offer a complete model of behavior. Rather, we use these frameworks to understand aspects of competitive communication, especially overjamming.

In each framework, we assume that senders form beliefs about what other players do and then choose the message that maximizes their payoffs given those beliefs. The frameworks differ in how they treat those beliefs. Throughout, we maintain the assumption that all senders believe receivers will choose the action equal to the average of the two messages she observes. Given that receivers observe neither the target nor the senders' shifts, this assumption is plausible. Each framework then focuses on a sender's beliefs about the messages likely to be sent by his opponent (i.e., the other sender).

First, we apply a "level-K" framework, in which subject use strategies based on iterated reasoning, starting from naivete (level-0), proceeding to the best response to a naive opponent (level-1), and so on.[18] Second, we develop a model of "experiential best responses," in which each sender expects that his opponent's message will be the average of the messages he has observed his opponents send previously. Third, we present a model of "inferred expectations," in which each sender is assumed to choose the best response to an (idiosyncratic) expectation about the message his opponent would send. Here, rather than modeling a sender's beliefs directly (as in the first two frameworks), we *infer* each sender's beliefs based on the (known) target and shift, and the message that we observe the sender send.

Level-K. First, we study a "level-K" framework, in which K denotes the degree of sophistication a subject evinces. Level-0 senders are non-strategic and use naive decision

[17]Dickson, Hafer, and Landa (2008) classify subject behavior in their experiment into several boundedly rational types. For a theoretical application in political economy, see Binswanger and Prufer (2010).

[18]Our model is closer to Nagel (1995) and Stahl and Wilson (1995) than to Camerer, Ho, and Chong (2004). The latter develop a cognitive hierarchy model that generalizes level-K analysis by assuming that senders form beliefs about the distribution of other players' strategies. Instead, we assume that senders form the simple belief that the opposing sender uses a particular strategy with probability one.

rules. Level-1 senders believe their opponents are level-0 and choose the appropriate best response. In general, level-K senders best respond given the belief that their opponents are level-$(K-1)$. Thus, K refers to the number of steps of iterated reasoning.

Although the reasoning is a bit subtle, a pattern emerges between K and a sender's best response.[19] According to the model, each sender should send a message equal to his own target $(T+S)$ plus an additional ***Exaggeration***. Left senders exaggerate messages to the left (i.e., by subtracting), and right senders exaggerate to the right (by adding). The size of the Exaggeration increases with sophistication level. Specifically, the Exaggeration is $|S|$, the magnitude of the sender's shift, plus a multiple of $|E(S_{opp})|$, the magnitude of the expected value of the opposing sender's shift.[20] Thus, the level-K framework has the potential to explain the overjamming phenomenon.

We classify a subject as level-K if M, the message they send, is generally consistent with m_K, the best response corresponding to level $K \in \{0, 1, 2, 3\}$. Specifically, if $|M - m_K|$ is less than 0.35 (about 12% of the message space), we consider the message as being consistent with the type. We then classify a subject as being type K if at least 70% of the subjects' messages are consistent with type K messages. We do not attempt to make strict classifications; consequently, some subjects' behavior falls into more than one category. We chose these classification thresholds because they maximize the percentage of subjects uniquely classified (54.3%). These thresholds are also consistent with those used by Cai and Wang (2006) and Costa-Gomes, Crawford, and Broseta (2001).

Our analysis reveals a distribution of subjects' levels of strategic sophistication. We classify 72% of subjects, most of whom possess a some degree of strategic sophistication beyond naivete (i.e., $K > 0$). Specifically, 24% are classified as level 0, 61% as level 1, and 4% as level 2. No senders are classified as having sophistication greater than level 2.

To vet this classification, we augmented two of our experimental sessions with a Beauty

[19] For simplicity of presentation, we do not derive this pattern in the text, instead relegating its derivation to the appendix.

[20] In the Symmetric Baseline Condition, $|E(S_{opp})| = 25$.

Contest, the game for which the level-K model was originally developed (Nagel 1995). Subjects first played the strategic communication game and then played a "Bonus Game". In the "Bonus Game," subjects guess a number between 0 and 100. The subject who chooses the number closest to 2/3 of the average guess is paid an extra $6. In equilibrium, everyone should guess 0, but this strategy is based on many iterations of strategic reasoning. Thus, the more iterations of reasoning one applies, the lower one's guess will be. Using the data from the Beauty Contest, we identify best responses for each level K and classify a subject as level-K if her guess is within a threshold of the level-K best response.[21]

Surprisingly, the level-K framework classifies subjects in our strategic communication game and the Beauty Contest very differently. Only 15 of the 24 subjects were classified in both games, and, of those, only 4 were classified at the same level. To ensure that this mismatch does not owe to overly stringent thresholds, we reclassified senders as being type K if at least 50% of the subjects' messages are consistent with type K messages; the result does not change. The mismatch may owe to the different sorts of sophistication induced by the two games, e.g., social norms activated by communication. Whereas the Beauty Contest implicates few obvious rules of good behavior, communicating immediately raises the prospect of truthtelling and lying.

Despite the poor performance of classification in the level-K framework, we can generalize the idea behind it to shed light on overjamming. Best responses in the level-K framework differed subtly yet fundamentally from those in the equilibrium analysis. Specifically, Messages in the level-K framework are functions of Targets and Shifts, rather than Targets and Jam (the indicator for the jamming region). Therefore, we generalize the level-K framework by regressing Message on Target, Shift, and intercepts for Left and Right Sender. Under the level-K framework, the coefficient on Target should be 1, the coefficient on Shift should be 2, and the intercepts should depend on a subject's sophistication level.

[21] Following Nagel (1995), the best response for level-K is $50(\frac{2}{3})K$, and the tolerance thresholds are given by $[50(\frac{2}{3})^{K-.5}, 50(\frac{2}{3})^{K+.5}]$. All subjects who guess a number more than 50 are unclassified; 17 of 24 subjects were successfully classified. We studied several alternative thresholds, none of which substantively altered the results.

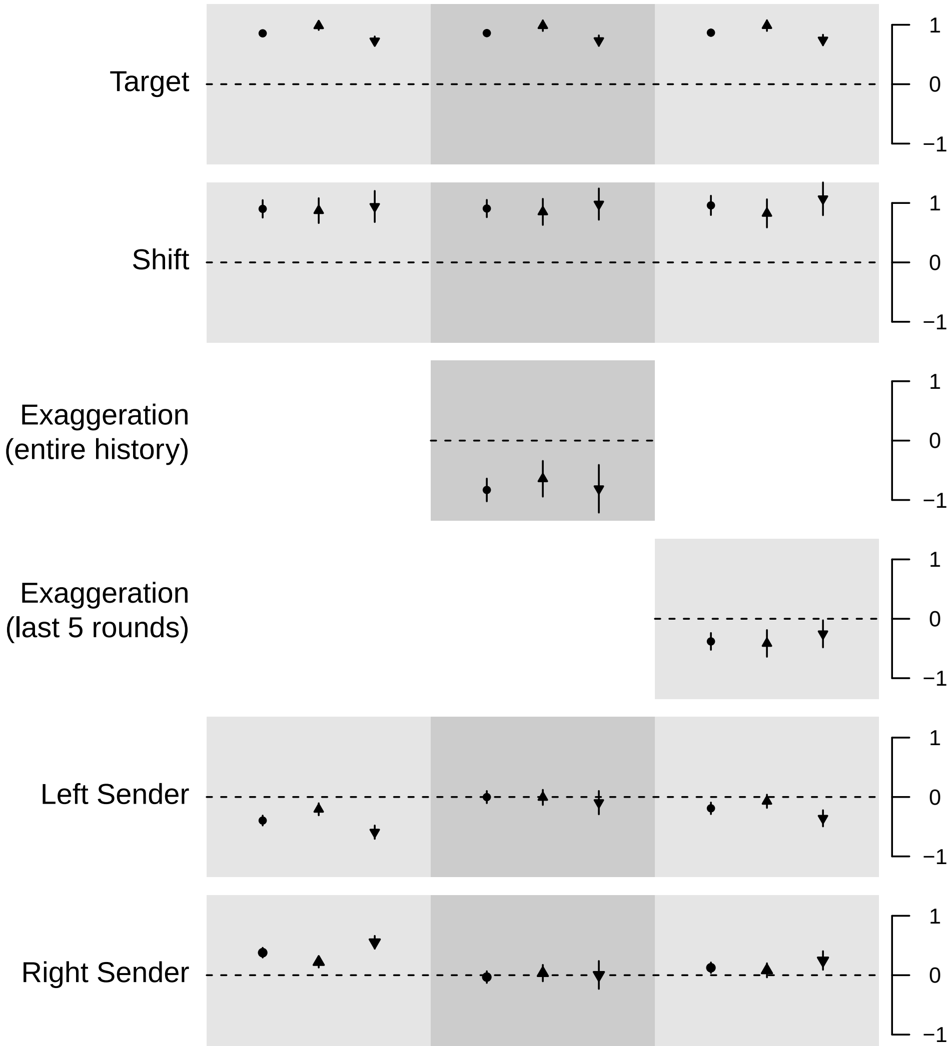

Figure 5: *Limited Strategic Sophistication.* Each column displays coefficients and standard errors from a separate regression of Message. The first column shows the level-K-inspired regression, which indicates that overjamming in the intercepts may be related to differences in iterative reasoning. The next two columns show regressions associated with the experiential best response framework, which predicts that senders exaggerate messages more if they have experienced their opponents exaggerate more in the past. However, the two measure of Exaggeration offer conflicting results. Exaggeration based on the entire history of play seems to effectively explain overjamming, while Exaggeration in the last 5 rounds does not. Moreover, the coefficient on Shift is systematically lower than its predicted value of 2 across the board, which identifies a new phenomenon: underexaggeration.

Overjamming is evident in these results (see Figure 5, circles in the first column). Starting at the top, the coefficient on Target is close to (albeit significantly different from) 0. However, the coefficient on Shift, which is predicted to be 2 by the level-K framework, is significantly less than 1. Thus, it seems that, relative to the baseline established by the level-K framework there is systematic "underexaggeration." However, we also continue to see that the Left and Right Sender intercepts are significantly different from zero, meaning that the overjamming phenomenon may indeed owe to variation in strategic sophistication.

To further probe this explanation for overjamming, we revisit the Learning Hypothesis, according to which senders become more sophisticated with experience. We repeat the regression, now with interactions with Round (triangles in the first column). Up-pointing triangles show for the first round; down-pointing triangles, the last. The intercepts on Left Sender and Right Sender move further from 0 over time, which is consistent with the idea that senders become more sophisticated with experience.

On its own, the level-K framework has not adequately explained overjamming. Although classifications based on the framework have proven unreliable, similar regression models identify a plausible origin for overjamming. Interestingly, these models also identify a second, as yet unexplained phenomenon. If, in fact, senders are engaged in best response play based on mutually inconsistent beliefs, then senders appear to "underexaggerate." That is, senders do not fully incorporate their own shifts into their messages. That said, we do find evidence that experience matters, as messages become more exaggerated over time. Our next framework focuses explicitly on the role of such experience.

Experiential Best Response In the level-K framework, we relaxed the assumption that beliefs and actions were mutually consistent, modeling sophistication as reasoning. We now dispense with the idea that senders engage in iterative reasoning, and instead assume senders play the best response to the empirical distribution of messages they have *experienced*. In our "experiential best response" framework, each sender expects his opponent to send a message m_{opp} equal to the Target plus the average Exaggeration he has observed.

The experiential best response framework is plausible for at least two reasons. First, subjects are reminded of their history at the end of each round; thus, they may simply be acting on the information we offer them. Second, we found above that senders exaggerate more over time. Rather than becoming more sophisticated, it is possible that senders are instead simply responding to a (self-perpetuating) trend in the messages they observe. By focusing on the alternative framework, we attempt to disentangle these two explanations.

According to this framework, a sender believes that the expected value of his opponent's message is $E(m_{opp}) = T + \bar{\xi}$, where $\bar{\xi}$ is the sample average Exaggeration of his opponent's messages.[22] If the sender also believes the receiver's action will be the average of the messages she observes, then his best response will be the message M such that $T+S = \frac{1}{2}E(M+m_{opp})$, or $M = T + 2S - \bar{\xi}$. To apply this framework to our data, we regress Message on Target, Shift, Left and Right Sender intercepts, and average Exaggeration. Because senders may have short or long memories, we use two different measures of average Exaggeration. First, we measure Exaggeration over the entire history a sender experienced, from round 1 up to the most previous round. Second, we measure Exaggeration as a moving average over the last five rounds. In each case, we expect the coefficient on Exaggeration to be -1.

In both models, the coefficient on Shift remains far less than 2; thus, underexaggeration remains prevalent. Using Exaggeration over the entire history of play, we see that its coefficient is $-0.83(0.10)$, so that the predicted value of -1 is within its confidence interval (see Figure 5, circles in the second column). Moreover, the "overjamming" that had been evident in the intercepts has been eliminated. These results are strengthened by examining the results from a regression that includes interactions with Round (triangles, the second column). The coefficient on Exaggeration is closer to -1 in the last round than in the first, while the intercepts do not move away from 0. That said, similar but weakened results hold if we use the moving average measure of Exaggeration (third column). The predicted value of -1 is no longer in the confidence interval of Exaggeration, and overjamming in the inter-

[22] Thus, in round t, $\bar{\xi} = \sum_{\tau=1}^{t-1}(m_{opp}^{\tau} - T^{\tau})$, where superscript τ indicates values from a previous round.

cepts seems to reemerge by the last round. And standard model selection criteria indicate the latter model provides the better fit (e.g., deviance is much lower in the latter model).

The experiential best response framework indicates that senders condition their messages on their own personal histories. In particular, it seems that senders try to counteract the messages they expect their opponents to send, in a manner reminiscent of but fundamentally different from jamming. However, analysis with this framework again indicates that senders underexaggerate. Combining insights from the level-K and the experiential best response frameworks, it is possible that senders misperceive the extent to which their opponents will exaggerate. That is, senders might think they are one strategic step ahead of their opponents, even though they are not. To explore this possibility, we utilize one final framework for limited strategic sophistication.

Inferred Expectations In our last framework, we shift perspectives. Rather than positing possible beliefs and deriving best responses, we assume that senders have beliefs, and, however idiosyncratic those beliefs may be, that they choose the best response conditional on those beliefs. According to this framework, a sender who expects that his opponent will send the message $E(m_{opp})$ will maximize his payoffs by choosing $M = 2(T + S) - E(m_{opp})$. Thus, as we observe T, S, and M, we can infer that the sender must have had expectation $E(m_{opp}) = 2(T + S) - M$.

Given these inferred expectations, we can investigate two separate questions. First, how well do these inferred expectations match the messages sent by senders' opponents? And second, do inferred expectations match opponents' messages more closely given more experience? To answer these questions, Figure 6 presents a scatterplot of Opponent's Message against Inferred Expectation. Up-pointing triangles are points from the first 16 rounds of play, and down-pointing triangles are from the remaining rounds. If inferred expectations matched opponents' messages perfectly, they would hew to the dashed 45° line. Of course, senders' opponents have private information about their own Shifts, and therefore, we should not expect perfect matches. However, if senders expectations are correct on average, points should be gathered about the dashed line.

Left senders (filled triangles) appear to systematically underestimate their Opponents' Messages, while right senders (empty triangles) overestimate those messages. These directions are consistent with the idea that senders believe they are one step more sophisticated than their opponents, even though they are not. To further test this possibility, examine how the differences inferred expectations and opponents' messages change throughout the game. To that end, Figure 6 displays nonparametric regression lines by sender type and round (thin lines for the first 16 rounds, thick lines for the remaining rounds). For left senders (the lower lines), it appears that the thick line is closer to the dashed line, meaning that left senders may be developing more accurate beliefs as the game progresses. However, for right senders (the upper lines), the two lines cross, meaning that there is no clear improvement in beliefs over time.

We have offered several expectations for overjamming, including variation in strategic sophistication, basing beliefs exclusively on experience, and a combination of the two. In the process, we uncovered evidence of a second phenomenon: underexaggeration. At present, we do not have a consistent theoretical explanation for these findings, and much more study will be necessary to resolve the inconsistencies we observed.

Conclusion

We report on an experimental investigation of communicative competition in a strategic, political setting. Unlike previous analyses, our experiments allow nearly continuous differentiation between choices of messages in a setting that mirrors the spatial model of politics. This abstract environment reflects the underlying structure of many political arenas in established democracies, and our findings help us to better understand how and why competitive information providers in politics behave as they do.

Many of the comparative statics predicted in that analysis are extant. In particular, evidence of jamming emerged. When senders are predicted to jam, they send messages that

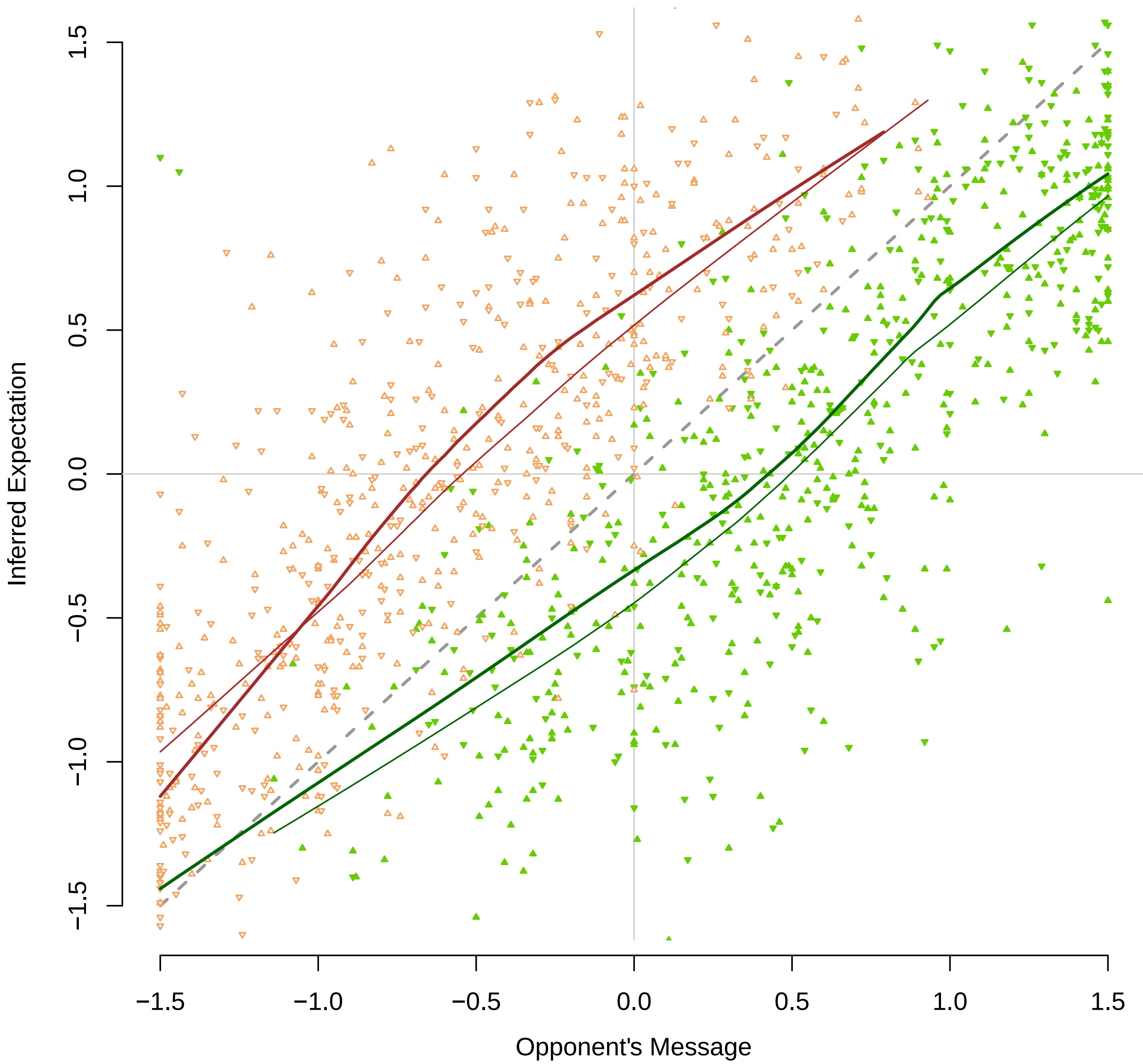

Figure 6: *Inferred Expectations.* Given observed behavior, we infer what senders expected their opponents to do, assuming best response play. Here, senders appear to underestimate their opponents' exaggerations. The dashed line represents a perfect match between inferred expectations and opponents' messages; the points mark observed data. Separate nonparametric regression lines are fitted and displayed for left senders (below the dashed line) and right senders (above the dashed line) for the first 16 rounds (up-pointing triangles, thin lines) and the remaining rounds (down-pointing triangles, thick lines). Although there is a positive relationship between the two, both are clearly distinct from the dashed line. Moreover, left senders seem to improve their inferences over time, while right senders do not.

are further from the true targets, on average. And moderate senders send more extreme messages than their extremist opponents, and senders whose shift distributions do not change send more extreme messages if their opponents become more extreme. Moreover, both comparative statics become more apparent the longer subjects play. We interpret these findings as evidence in support of the jamming theory.

That said, we found significant and intriguing differences between equilibrium predictions and observed behavior. First, senders reveal more information than is predicted by equilibrium analysis. Thus, they seem to "overjam," sending exaggerated messages when they are predicted to tell the truth. Increasing payoffs and learning have conflicting effects on this overjamming, and we therefore rejected the hypothesis that subjects are simply poor best responders. Instead, we developed three alternative frameworks of limited strategic sophistication that relax the equilibrium requirement that beliefs and actions be mutual consistent. While we cannot conclusively identify the origin of overjamming, we offer two possible causes: level-K reasoning and beliefs based on experience. Both these frameworks, as well as a third that uses subjects' choices to identify their expectations, isolate a second intriguing phenomenon: underexaggeration. That is, subjects seem to both jam when they should not, and to underexaggerate when they do.

Based on these findings, we can revise our expectations about how strategic competition among information providers affects those who depend on that information to make political decisions. Communicative competition does not seem to encourage truthtelling; rather, competitive senders systematically distort their messages. Moreover, the most seasoned political elites are bound to more closely resemble the subjects near the end of the sessions, when they have learned to jam. A competitive marketplace of ideas does not seem sufficient for the truth to emerge.

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Experimental Instructions

Instructions for both the main experimental setting and the Beauty Contest appear below. The section marked “Bonus Game” was available only to subjects in the experimental conditions that included the Beauty Contest.

Instructions

General Information

This is an experiment in communication. The University of Pittsburgh has provided funds for this research. If you follow the instructions closely and make appropriate decisions, you may make a considerable amount of money. In addition to the $7 participation payment, these earnings will be paid to you, in cash, at the end of the experiment.

During the experiment, all earnings will be denominated in points, which will be converted to cash at the rate of $1 per 150 points. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. You will be paid your earnings privately, meaning that no other participant will find out how much you earn. Also, each participant has a printed copy of these 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 for the duration of the experiment. Participants intentionally violating the rules will be asked to leave the experiment and may not be paid.

Roles, Rounds, and Matching

Each participant will be assigned to one of three roles: A, B, or C. Your role will be assigned before the first round and will remain fixed throughout the experiment.

In this experiment you will make decisions in a series of rounds, and there are a total of 32 rounds. **Each round is a separate decision task. Before every round, you will be randomly matched with two other participants.** In every group of three participants there will be one player in each role (one A player, one B player, and one C player).

You will not know the identity of the other participants you are matched with in any round, and your earnings for each round depend only on your action in that round and the actions of the participants you are matched with in that round.

Targets

At the beginning of every round, the computer will randomly select a **target** for each player.

Player C’s target will be a number between -100 and 100. Each number is equally likely to be C’s target.

Player A's target will be less than Player C's target. The difference between A's target and C's target will be some amount between 0 and 50 units. Each amount is equally likely, and the exact amount will be selected at random in every round.

Player B's target will be greater than Player C's target by some amount between 0 and 50 units and each amount of difference is equally likely.

For example, suppose that the computer selects 25 as Player C's target. For Player A's target, the computer will randomly select a number from -25 to 25. Likewise, Player B's target will be a randomly selected number from 25 to 75.

It is important to note that Player A's target and Player B's target are randomly selected by the computer independently. That is, the value of Player A's target does not affect the value of Player B's target and vice versa.

Similarly, the computer will randomly determine each player's target at the beginning of the round so that the targets in one round are selected independently of the targets in another round.

Sequence of Decisions

The sequence of decisions in every round is as follows:

1. Players A and B each find out the value of Player C's target and the value of their own target. (Note that Player A does not see Player B's target, nor does Player B see Player A's target.) Independently and simultaneously, Players A and B each select a **message** to send to Player C.
2. Player C sees the messages sent by Player A and Player B. Player C then chooses an **action** (any number between -150 and 150). (Note that Player C sees both messages but none of the targets.)

Payoffs

Each player's payoff depends only on how close Player C's action is to his or her own target. More specifically, a player earns 100 points if the action is equal to his or her own target and 1 point less for each unit of difference between the action and the target. This is described by the following formula (where the straight lines indicate absolute value):

$$\text{Player's Payoff} = 100 - |\text{Player's Target} - \text{C's Action}|$$

Note that the messages sent by Player A and Player B are not part of the payoff formula.

To illustrate, consider a few examples. Suppose you are Player A, your target is 10 and Player C chooses the action 40. The difference between your target and the action is 30, so your payoff would be 70. If Player C's target is 25, then the difference between C's target and the action is 15, so C's payoff would be 85.

Now suppose instead that Player C chooses the action -40. If Player A's target is 20, then the difference between A's target and the action is 60 and A's payoff would be 40. If Player B's target is 80, then the difference between B's target and the action is 120, so B's payoff would be -20. If Player C's target is 45, then the difference between C's target and the action is 85, so C's payoff would be 15. (Note that it is possible for payoffs to be negative.)

Sample Screens

We will now see what the screens look like for each type of player during the experiment.

This is the screen that will be seen only by Player A. There is a brief set of instructions in the upper left-hand corner. A description of the payoff formula is also shown on the left side of the screen. The top of the screen shows several values: C's actual target, A's target (which is labeled "your target"), and the range of possible targets for B.

The targets are indicated graphically in the figure in the middle of the screen, which also indicates the possible range of values for each player's target. Player A chooses a message by dragging the white tab to any position along the horizontal black line. After moving the tab, it will indicate the value of the selected message.

Note that there is also a section on the left marked "payoff calculator." Click on the "Show" button to reveal an orange tab that can be used to calculate hypothetical payoffs for each possible action that Player C can take. If you move the orange tab to different positions, the bold text at the bottom of the screen changes to indicate what Player A's payoff and player C's payoff would be. Note that the payoff calculator does not show B's hypothetical payoff because you do not know the value of B's target. Note also that you can hide the payoff calculator by clicking on the "hide" button.

When Player A is ready to send the message, he or she will click on the "Send Message" button in the lower right-hand corner of the screen. Feel free to move the message tab and try out the payoff calculator. When you are ready to continue, click on the "Send Message" button.

This is the screen that only Player B will see. B players see this screen at the same time that the A players see their screens. It is pretty much the same as Player A's screen except that B's target is known while A's is not. When you are done looking at this screen, click on the "Send Message" button to continue.

After Player A and Player B send their messages, Player C will see this screen. In the upper-left corner there is again a brief set of instructions. The top of the screen shows the numerical values of the messages. The messages are also indicated graphically in the middle of the screen. To select an action, Player C moves the red tab to the desired location. As with the other tabs, it shows the numerical value of its location after it is moved. Note that Player C does not have a payoff calculator because the actual values of the targets are not known. Try moving the "Action" tab and the click on "Choose Action" button when you are ready to continue.

At the end of every round, you will see this screen, which shows you the results from the round—including the actual targets of every player, both messages, and the action chosen by Player C, and the payoffs earned by every player in your group. At the bottom of the screen, it will show the results of every previous round that you played.

QUIZ INSTRUCTIONS.

To check your understanding of the decision tasks, please answer the questions below as best you can. **Note that your quiz answers do not affect your earnings,** and you may refer to your printed instructions as often as you like. When you are finished, feedback about the correct answers will be shown on the screen. You must attempt to answer all of the questions. If you have any further questions at this time, please raise your hand and the

experimenter will come to you.
1. C's target can be any number from: [0 to 10, 0 to 100, -100 to 100, -150 to 150]
2. If C's target is -40, then A's target can be any number from: [-100 to 0, -90 to -40, -40 to 10, 40 to 90]
3. If C's target is 30, then B's target can be any number from: [-20 to 30, 0 to 50, 30 to 80, 50 to 100]
4. If you are Player C, your target is 85, and you choose the action 45, how many points will you receive? [15, 40, 60, 85]
5. If you are Player A, your target is -70, and Player C chooses the action 50, how many points will you receive? [-70, -20, 30, 50]
6. Suppose that you are Player B, your target is 10 and Player C's target is -15. If you send the message 10 and Player C chooses the action 0, how many points will you receive? [10, 15, 85, 90]
7. Suppose that you are Player C. Player A sent you the message -50 while Player B sent you the message 50. If you choose the action 30 and your actual target was 50, how many points will you receive? [20, 30, 70, 80]
8. In every round, will you be matched with same participants? [Yes, No]

Bonus Game

Before we conclude the experiment, there will be a bonus game.
The total prize for winning the bonus game is $6, and the rules are simple.
· In this game, you may choose any whole number from 0 to 100.
· The computer will calculate the average of all the numbers submitted.
· The winner is the person who chooses the number closest to 2/3 of the average.
· There can be more than one winner in the case of ties. If there is more than one winner, then the winners will split the prize equally.
If you have a question about the bonus game, please raise your hand.

Supplemental Web Appendix

Derivation of Best Responses in Level-K Model

In our strategic communication game, there are two plausible level-0 strategies senders might employ. Senders might be *naive truthful* types ($t0$) who report the truthful message $m_{t0} = T$. Alternatively, senders might be *naive selfish* types ($s0$) who instead report their own targets, $m_{s0} = T + S_j$. The latter type of sender attempts to maximize his utility but does not consider how his opponent's strategy affects the receiver's action.

If a subject has sophistication $K > 0$, his reasoning process must ultimately be based on one of these two level-0 types. Suppose first that the naive truthful type anchors the iterated reasoning process. Type $t1$ denotes the level-1 subject who believes he is playing a truthful opponent. A subject of this type believes that the receiver will choose $c = \frac{1}{2}(T + m_{t1})$, and so his best response is to choose $m_{t1} = T + 2S$.[23] At the next level of sophistication, type $t2$ believes he faces a type $t1$ opponent. Type $t2$ believes that $c = \frac{1}{2}(T + 2S_{opp} + m_{t2})$, where$S_{opp}$ is the opponent's shift. Although he does not know his opponent's shift, each sender does know its sign and distribution. The best response is to choose the message that will ensure $E(c) = T + S$, which is $m_{t2} = T + 2S - 2 * E(S_{opp})$. For example, the left sender knows that S_{opp} is distributed uniformly between 0 and 50; therefore, his best response is $m_{t2} = T + 2S_L - 50$. Similar reasoning implies that a right sender with type $t2$ has best response $m_{t2} = T + 2S_R + 50$. Furthermore, continuing this pattern of reasoning indicates that at the next level, $m_{t3} = T + 2S_L - 100$ for left senders and $m_{t3} = T + 2S_R + 100$ for right senders.

Rather than taking the naive truthful type as the anchor of the iterative reasoning process, suppose instead that a the naive selfish type is the base. Very similar reasoning yields the following conclusions. A left sender with type $s1$ will have the best response $m_{s1} = T + 2S_L - 25$; a right sender with type $s1$ has $m_{s1} = T + 2S_L + 25$. At the next level, $m_{s2} = T + 2S_L - 75$ for left senders and $m_{s2} = T + 2S_R + 75$ for right senders.

In general, our simple model of limited strategic sophistication implies that message strategies will be a linear combination of T, S, and a constant (summarized in Table **??**). Messages that reflect a Level-1 or higher degree of strategic sophistication take the general form $m = \alpha + T + 2S$ where α is generally some multiple of 25 (and $\alpha \leq 0$ for left senders and $\alpha \geq 0$ for right senders). In contrast, naive strategies are either less responsive or unresponsive to the shift parameter and do not involve a constant term.

[23] To see this, recall that the sender wants to induce the receiver to choose an action equal to his own target, $T + S$. Thus, $E(c) = \frac{1}{2}E(T + m_{t1}) = T + S$ if and only if $m_{t1} = T + 2S$. This argument is equally valid for left and right senders, regardless of the sign of S.

Table A-1: Summary Statistics for Experimental Sessions

Session	Condition	Subjects	Rounds	T	S_L	S_R	m_L	m_R	c
1	Symmetric	18	32	0.01	-0.26	0.25	-0.59	0.41	-0.08
	Baseline			(0.58)	(0.15)	(0.15)	(0.59)	(0.67)	(0.58)
2	Symmetric	15	32	0.00	-0.25	0.26	-0.73	0.78	0.03
	Baseline			(0.58)	(0.15)	(0.15)	(0.60)	(0.66)	(0.58)
3	High Payment	15	12	-0.01	-0.24	0.27	-0.28	0.27	0.03
	(Symmetric)			(0.57)	(0.16)	(0.14)	(0.55)	(0.58)	(0.57)
4	High Payment	15	12	-0.02	-0.24	0.26	-0.42	0.39	-0.05
	(Symmetric)			(0.60)	(0.14)	(0.15)	(0.60)	(0.57)	(0.60)
5	Asymmetric	12	28	-0.04	-0.31	0.26	-0.63	0.56	0.00
				(0.57)	(0.14)	(0.15)	(0.61)	(0.62)	(0.57)
6	Asymmetric	15	32	0.01	-0.31	0.25	-0.32	0.57	0.08
				(0.59)	(0.13)	(0.14)	(0.66)	(0.58)	(0.59)
7	Symmetric with	18	24	0.04	-0.25	0.24	-0.57	0.63	0.02
	Beauty Contest			(0.59)	(0.15)	(0.14)	(0.62)	(0.63)	(0.59)
8	Symmetric with	18	24	0.05	-0.25	0.25	-0.60	0.57	0.05
	Beauty Contest			(0.63)	(0.14)	(0.15)	(0.64)	(0.63)	(0.63)

Sample means and standard deviations are reported by session for target T, left sender's shift S_L, right sender's shift S_R, left sender's message m_L, right sender's message m_R, and receiver's action c. All parameters are have been rescaled by 1/100.

Table A-2: Senders in the Symmetric Baseline Condition

DV = Message	[1]	[2]	[3]	[4]
Target	0.93	0.94	0.92	1.04
	(0.02)	(0.02)	(0.03)	(0.04)
Target × Jam	-0.44	-0.45	-0.43	-0.24
	(0.05)	(0.07)	(0.05)	(0.07)
Jam	-0.04	-0.06	-0.05	0.08
	(0.02)	(0.04)	(0.04)	(0.06)
Left Sender	–	–	-0.58	-0.39
			(0.05)	(0.05)
Right Sender	–	–	0.56	0.33
			(0.05)	(0.06)
Round × Target	–	–	–	-0.24
				(0.07)
Round × Target × Jam	–	–	–	-0.02
				(0.19)
Round × Jam	–	–	–	-0.28
				(0.11)
Round × Left	–	–	–	-0.37
				(0.07)
Round × Right	–	–	–	0.46
				(0.08)
Intercept	-0.02	-0.01	–	–
	(0.09)	(0.09)		
Subject σ				
Target	–	0.06	0.08	0.07
Target × Jam	–	0.08	0.11	0.10
Jam	–	0.04	0.07	0.07
Intercept	0.59	0.58	0.18	0.18
Round σ				
Target	–	0.09	0.10	0.08
Target × Jam	–	0.26	0.10	0.14
Jam	–	0.19	0.15	0.15
Left Sender	–	–	0.14	0.08
Right Sender	–	–	0.18	0.11
Intercept	0.05	0.07	–	–
Residual σ	0.30	0.28	0.25	0.25

The reported models includes random intercepts and slopes by subject and round, as indicated. There are 1280 observations, 46 subjects, and 32 rounds. The Baseline column of the Senders panel in Figure 3 is based on Model [3], and the Learning column is based on Model [4].

Table A-3: Sender Payment Sensitivity

	[5]	
DV = Message	Coef.	SE
Target	0.97	0.09
Target × Jam	-0.32	0.15
Jam	0.03	0.06
Left Sender	-0.44	0.06
Right Sender	0.43	0.06
High Pay × Target	-0.09	0.13
High Pay × Target × Jam	-0.03	0.23
High Pay × Jam	0.03	0.09
High Pay × Left	0.13	0.09
High Pay × Right	-0.14	0.09
Subject σ		
Intercept	0.17	
Target	0.10	
Round σ		
Intercept	0.04	
Target	0.03	
Block σ		
Intercept	0.005	
Target	0.12	
Residual σ	0.24	
n Observations	792	
n Subjects	66	
n Rounds	12	
n Blocks	8	

In the High Payment Condition, each round was worth four times as much as a round in the Symmetric Baseline Condition. The reported model includes random intercepts and slope on Target by subject, round, and block (i.e., condition, sender type, jam or not jam). This model is the basis for the middle column in the "Senders' Messages" panel of Figure 3.

Table A-4: Actions in the Symmetric Baseline Condition

DV = Action	[6]	[7]	[8]
Target	1.01	1.00	1.03
	(0.04)	(0.04)	(0.08)
Message Difference	0.01	0.01	0.03
	(0.02)	(0.02)	(0.04)
Target ×	-0.23	-0.22	-0.21
Message Difference	(0.03)	(0.04)	(0.08)
Round	–	–	-0.04
			(0.10)
Round × Target	–	–	-0.10
			(0.15)
Round ×	–	–	-0.01
Message Difference			(0.07)
Round × Target	–	–	0.01
× Message Difference			(0.12)
Intercept	–	–	-0.02
			(0.05)
Subject σ			
Intercept	0.04	0.04	0.03
Round σ			
Target	–	0.06	0.08
Message Difference	–	0.05	0.05
Intercept	0.02	0.08	0.08
Residual σ	0.24	0.24	0.24

The reported models includes random intercepts and slopes by subject and round, as indicated. There are 640 observations, 23 subjects, and 32 rounds. The Baseline column of the Receivers panel in Figure 3 is based on Model [5], and the Learning column is based on Model [7].

Table A-5: Receiver Payment Sensitivity

	[9]	
DV = Action	Coef.	SE
Target	1.05	0.10
Message Difference	0.004	0.042
Target × Message Difference	-0.29	0.09
Intercept	-0.01	0.04
High Pay × Target	-0.28	0.17
High Pay × Message Difference	-0.23	0.09
High Pay × Target × Message Difference	0.14	0.19
High Pay	0.18	0.08
Subject σ		
Intercept	0.09	
Target	0.29	
Message Difference	0.09	
Target × Message Difference	0.26	
Round σ		
Intercept	0.00	
Residual σ	0.26	
n Observations	396	
n Subjects	33	
n Rounds	12	

In the High Payment Condition, each round was worth four times as much as a round in the Symmetric Baseline Condition. The reported model includes random intercepts and slope on Target by subject and round. This model is the basis for the middle column in the "Receivers' Actions" panel of Figure 3.

Table A-6: Distance and Learning in Symmetric and Asymmetric Environments

DV = Distance	[10]	[11]
Right Sender		
SRN	0.57	0.33
	(0.05)	(0.05)
SRJ	0.73	0.55
	(0.05)	(0.05)
Round × SRN	–	0.50
		(0.05)
Round × SRJ	–	0.35
		(0.08)
Moderate		
ARN	0.55	0.37
	(0.07)	(0.07)
ARJ	0.76	0.50
	(0.07)	(0.09)
Round × ARN	–	0.36
		(0.06)
Round × ARJ	–	0.54
		(0.13)
Left Sender		
SLN	0.59	0.38
	(0.05)	(0.05)
SLJ	0.82	0.48
	(0.05)	(0.06)
Round × SLN	–	0.42
		(0.05)
Round × SLJ	–	0.77
		(0.09)
Extremist		
ALN	0.52	0.36
	(0.07)	(0.07)
ALJ	0.57	0.47
	(0.07)	(0.08)
Round × ALN	–	0.31
		(0.07)
Round × ALJ	–	0.20
		(0.10)
Subject Intercept σ	0.18	0.18
Round Intercept σ	0.13	0.04
Residual σ	0.25	0.24

Regressors are indicators for each block, which are notated by **S**ymmetric or **A**symmetric Condition, **L**eft or **R**ight Sender, and **J**amming Region or **N**ot.

Table A-7: Limited Strategic Sophistication

DV = Message	[12]	[13]	[14]	[15]	[16]	[17]
Target	0.86	0.98	0.86	0.99	0.87	0.99
	(0.02)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)
Shift	0.90	0.87	0.91	0.85	0.96	0.83
	(0.08)	(0.11)	(0.07)	(0.11)	(0.08)	(0.12)
Exaggeration	–	–	-0.83	-0.64	–	–
(Entire History)			(0.10)	(0.15)		
Exaggeration	–	–	–	–	-0.38	-0.41
(Last 5 Rounds)					(0.07)	(0.11)
Left Sender	-0.40	-0.21	0.00	-0.01	-0.19	-0.07
	(0.04)	(0.05)	(0.05)	(0.07)	(0.05)	(0.06)
Right Sender	0.38	0.22	-0.03	0.04	0.13	0.08
	(0.04)	(0.05)	(0.05)	(0.07)	(0.05)	(0.06)
Round × Target	–	-0.26	–	-0.25	–	-0.24
		(0.06)		(0.08)		(0.08)
Round × Shift	–	0.07	–	0.13	–	0.25
		(0.19)		(0.20)		(0.21)
Round × Exag.	–	–	–	-0.17	–	–
(Entire History)				(0.27)		
Round × Exag.	–	–	–	–	–	0.16
(Last 5 Rounds)						(0.18)
Round × Left	–	-0.39	–	-0.09	–	-0.29
		(0.08)		(0.11)		(0.08)
Round × Right	–	0.33	–	-0.03	–	0.17
		(0.07)		(0.15)		(0.10)
Subject σ						
Target	0.07	0.07	0.09	0.09	0.09	0.09
Shift	0.38	0.39	0.37	0.38	0.38	0.39
Exaggeration	–	–	0.35	0.37	0.28	0.28
Intercept	0.15	0.15	0.16	0.18	0.17	0.17
Round σ						
Target	0.10	0.08	0.12	0.10	0.12	0.10
Shift	0.04	0.02	0.09	0.08	0.21	0.15
Exaggeration	–	–	0.31	0.30	0.24	0.22
Left Sender	0.14	0.09	0.06	0.06	0.08	0.04
Right Sender	0.11	0.05	0.12	0.12	0.09	0.08
Residual σ	0.24	0.24	0.22	0.22	0.22	0.21
Deviance	268	204	98	59	74	29
n Observations	1280	1280	1234	1234	1234	1234
n Subjects	46	46	46	46	46	46
n Rounds	32	32	31	31	31	31

These models are the basis for Figure 5.