# Numeric vs. Natural Language Messages in Experimental Cheap Talk Games *

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**Abstract**

We compare different forms of communication in the context of cheap talk sender-receiver games. While previous experiments find evidence supporting the comparative statics prediction that more preference divergence leads to less information transmission, there is also a consistent pattern of overcommunication and exaggeration, not predicted by theory, in which subjects convey more information than predicted in equilibrium. The latter of these findings may be due to the restricted nature of the message space in most experimental cheap talk games, encouraging subjects to engage in exaggeration artificially, rather than allowing it to emerge naturally. We tested this hypothesis with an incentivized lab experiment, and found evidence both phenomena persist with natural language (text-based) communication. Moreover, we probe the consequences of this expanded message space for outcomes, showing that senders benefit more than receivers, but that the most notable effect is that text messages improve efficiency. (Word Count: 4,263)

**Keywords:** Strategic information transmission; Sender-receiver games; Natural language; Text messages; Laboratory experiment

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# 1 Introduction

Ordinary language is rich with meaning and possibility. In fact, language is so rich as to be ungainly. To study strategic communication profitably, game-theoretic models typically abstract away from this richness, studying incentives over much more limited message spaces. As in many other areas, laboratory experiments based on cheap talk sender-receiver games have generally followed the strategic model. Rather than offer opportunities for subjects to communicate with the full richness of any language they might have in common, most lab experiments instead offer message spaces limited to literal translations of whatever information is hidden, typically the numeric value of the state variable. For example, the experimental setup from Cai and Wang (2006) includes five possible values for the hidden state, and five, similarly labeled values for messages. Similarly, Minozzi and Woon (2016) present senders with a number line that simultaneously illustrates the hidden state, the preference divergence between players, and all possible messages. The implicit assumption is that there are no material or inferential consequences of flattening ordinary language down to its game-relevant numeric content.

Yet, it remains unclear whether such limited message spaces suffice in experimental settings. Consider overcommunication. Over the last 25 years, evidence from experimental cheap talk games has yielded two stylized facts (Blume, Lai and Lim, 2020). First, there is support for the key comparative statics prediction from theoretical sender-receiver games (Crawford and Sobel, 1982). As the preferences of senders and receivers diverge, less information is transmitted by experimental subjects (Dickhaut, McCabe and Mukherji, 1995). Second, there is little to no support for the main theoretical prediction, that messages and actions correspond to a partially informative equilibrium in which there is a partition of the state space. Instead, senders appear to overcommunicate and naïvely exaggerate, simply adding an amount to the hidden state in a strategically unstable way. This instability leaves receivers with the capacity to infer more about the hidden state than predicted by equilibrium (Blume et al., 2001; Cai and Wang, 2006; Kawagoe and Takizawa, 2009; Wang, Spezio and Camerer, 2010; Minozzi and Woon, 2016). If generalizable, the second of these regularities casts doubt on the inferences warranted by the first. On the one hand, while the evidence for the comparative statics prediction is robust, it is also consistent with behavioral rules beyond optimization and equilibrium. On the other hand, the evidence for overcommunication and naïve exaggeration may itself be an artifact of the lab, including choices about how subjects communicate.

Does overcommunication generalize, or does it depend on the common elements used in most laboratory experiments? Most sender-receiver experiments bind the message space to the state space. This technology may artificially encourage subjects to exaggerate and thus overcommunicate. Specifically, numeric messages may make it more difficult to send the partially informative messages predicted by Crawford and Sobel (CS). CS's equilibria require messages that partition the state space, offering only the information that the state lies in one of the partition's subsets. The problem is that asking subjects to provide particular numbers means asking for precision. For subjects to use partition equilibria, they would either have to randomize over points, which is cumbersome, or coordinate on any one of many different equilibria in message meanings, which is difficult. Thus, many subjects might instead employ simpler, albeit unstable strategies in the lab—even though they could have easily offered partially informative messages if allowed access to a richer language.[1] If this account is correct, the evidence for the second regularity from cheap-talk experiments would not be robust. Similarly, if the comparative statics prediction remained, the result would be stronger evidence for the predictions of equilibrium analysis. And both would be artifactual, depending on the limited language used in cheap talk experiments.

In this paper, we experimentally manipulate the available communication technology to probe the generalizability of overcommunication and naïve exaggeration, exploring the consequences of allowing senders to communicate via natural language, without any encouragement to use text-based messages artificially lashed to the state space. Specifically, we conduct an experiment to explore whether play in sender-receiver games changes with the set of possible messages. In one of our conditions, senders select messages from the set of possible values of the hidden state

[1] Put differently, different elicitation methods may entail different lying costs. For example, it may be more cognitively costly to misrepresent information using a less familiar message space. Since participants will have relatively less experience using numeric messages as opposed to ordinary language, we expect numeric messages to yield more informative behavior by senders.

information. In the other condition, we enrich this set of messages by providing senders with an open text box.[2] The latter is an enrichment because senders could choose to type messages such as, "Your hidden number is 12." For each condition, we also have subjects play multiple rounds of the game under two regimes: one in which there is an informative partition equilibrium, and another in which the only equilibrium is babbling.

Our main goal was to test the hypothesis that overcommunication, operationalized as the difference between equilibrium predictions about receivers' actions and observed behavior, is caused by the use of numeric messages. Our expectation is that such overcommunication would either be limited to the numeric condition or, more weakly, would be greater in the numeric condition than in the text condition.

We ultimately produce several findings. First, in contrast to our main expectation, we continue to observe evidence of overcommunication even in the text condition, mitigating concerns about the artificiality of previous experiments. Second, we continue to observe evidence of the comparative statics prediction even in the case of text messaging. The combination of these two findings at once eases concerns about the generalizability of the overcommunication finding, but also further problematizes the inference from the comparative statics prediction to claims based on equilibrium analysis. Third, we show that natural language increases payoffs and efficiency, although most of these benefits accrue to senders, not receivers. Fourth, we go on to identify how and when senders use particular messaging tactics with their capacity to use text, particularly focusing on their use of informativeness, normative concerns like honesty and fairness, and which quantities are mentioned. Finally, we connect these two measures, showing that senders and receivers benefited differently from different messaging tactics, and that many of the increases in efficiency were split between the two.

[2]Previous experiments with treatments comparing numeric and text messages are Wilson and Sell (1997) and Bochet, Page and Putterman (2006) in public goods games, and Ben-Ner and Putterman (2009) and Ben-Ner, Putterman and Ren (2011) in trust games. Anbarci, Feltovich and Gürdal (2019) compares numeric, one-way text, and two-way chat in games with multiple senders, whereas our experiment compares numeric and one-way natural-language messages in single-sender environments. Elsewhere, Lai and Lim (2018) investigates the role of cardinality of the message space and symbolic messages on experimental cheap talk games, treating the addition of new messages as neologisms. Blume, Lai and Lim (2023) considered mediated talk in a cheap talk environment, classifying different types of message spaces, including Directives, Declaratives, and non-confirming language.

## 2 Design

Our study builds on the experimental cheap talk setup used by Minozzi and Woon (2013, 2016, 2019, 2020). Similar to Crawford and Sobel (1982), there is an unobserved state of the world, which we call the *Target*, $t$, an integer randomly selected from $-100$ to $100$. The first player $S$ (Sender) observes the *Target*, and then sends a *Message* to the second player $R$ (Receiver). We experimentally manipulate the set of messages available to $S$. $R$ does not learn the *Target* directly but observes the *Message* and then chooses an *Action*, $a$. Like the *Target*, the *Action* is an integer from $-100$ to $100$.

As in Crawford and Sobel (1982), the sender and receiver have partially overlapping incentives. The degree of overlap is inversely related to the *Shift*, $s$, which measures the preference divergence between the players. All players in our experiment participated in 30 rounds of the game, separated into two sets of 15 rounds, one in which $s$ was $80$ (*High Shift*) and another in which $s$ was $40$ (*Low Shift*). Payoffs were denominated in points: given values of $t$, $s$, and $a$, $R$ earns $320 - |t - a|$ points and $S$ earns $320 - |t + s - a|$ points.

In theory, both *Shift* values include the possibility for uninformative babbling equilibria. But this is the only equilibrium outcome for the *High Shift* case. While many messages would be consistent with such equilibria, all of them require $R$ to choose $a = 0$, the ex ante expected value of the *Target*. For the *Low Shift* case, there is also an informative, partition equilibrium. Here, $S$ would use the cutpoint $-80$ to send two sorts of messages: one indicating target values below the cutpoint, and another indicating targets above the cutpoint. In turn, $R$ would choose a *Low Action* equal to $-90$ when the *Target* is below the cutpoint, and a *High Action* equal to $10$ otherwise.

Our experiment includes two conditions: *Numeric* and *Text*. Each session was assigned to one condition, and each subject participated in one session. In each session, subjects were randomly assigned to a fixed role, either $R$ (receiver) or $S$ (sender). They then played $30$ rounds of a sender-receiver game, first playing 15 rounds with the *High Shift* and then 15 more rounds with the *Low Shift*. In each round, subjects were randomly matched into pairs. The sender is shown the *Target*, a random integer from $-100$ to $100$, and selects a message. In the *Numeric* condition, senders

Table 1: Session Details

| Condition | Session | # Subjects | # Pairs | # Obs. |
|---|---|---|---|---|
| Numeric | 1 | 14 | 7 | 210 |
| | 2 | 12 | 6 | 180 |
| | 4 | 10 | 5 | 150 |
| | | 36 | 18 | 540 |
| Text | 3 | 10 | 5 | 150 |
| | 5 | 12 | 6 | 180 |
| | 6 | 16 | 8 | 240 |
| | | 38 | 19 | 570 |
| Total | | 74 | 37 | 1110 |

Pairs were randomly assigned every round An observation is a single interaction. There were 30 observations per pair.

could select a number from the *Target* space using a slider. In the *Text* condition, senders entered messages in an open text field. After each round, subjects were shown all results from that round for their pair.

In October 2017, we recruited 74 subjects through the Pittsburgh Experimental Economics Laboratory's database. Most subjects were undergraduates at the University of Pittsburgh. Our experiment was conducted using z-Tree (Fischbacher, 2007). Each session lasted under 2 hours. At the end of a session, one round was randomly selected to calculate payoffs. Points from that round were converted to cash at the rate of $1 per 20 points. Sessions lasted under 2 hours. Payments ranged between $15.25 and $23.00, averaging $20.90, including a $7 show-up fee. Table 1 presents details by session.

# 3 Results

We first focus on evidence of overcommunication in each condition. Overcommunication is typically measured with the correlation between senders' messages and the hidden state information. However, many senders chose not to send messages including numbers, and so message behavior is difficult to compare across conditions with this measure. Therefore, we focus instead on the relationship between receivers' actions.

We begin with a simple comparison of the *Target* and *Action* in each condition. Recall that we expected the *Text* condition to result in less overcommunication, which would manifest here in a lower correlation coefficient relative to the *Numeric* condition. Instead, we see the opposite. Figure 1 presents scatterplots and linear regressions of *Target* and *Action* in each condition, and it is clear that the relationship between the two is stronger with *Text* rather than *Numeric* messages. Similarly, the Pearson correlation coefficient between *Target* and *Action* is $0.64$ ($95\%$ interval = $[0.59, 0.69]$) in the *Text* condition but only $0.47$ ($95\%$ interval = $[0.40, 0.54]$) in the *Numeric* condition, and the mean absolute difference between *Target* and *Action* is $39.0$ ($95\%$ interval = $[38.2, 39.8]$) in *Text* vs. $43.6$ ($95\%$ interval = $[42.8, 44.5]$) in *Numeric*. Because of non-independence across observations, we eschew straightforward tests of the differences between these statistics by condition, but the initial evidence directly contradicts our expectation and suggests that overcommunication is not due artificially to numeric messages. If anything, numeric messages may lead us to underestimate the potential scope of overcommunication.

We more carefully probe for differences in overcommunication by using regression to examine the extent to which receivers exhibited equilibrium-like behavior across conditions. Specifically, we compare the negative distance between the *Actions* expected in equilibrium and those selected by receivers. In the *High Shift* case, the only equilibrium is babbling, and so the unique equilibrium action is $a^* = 0$. In the *Low Shift* case, the most informative equilibrium is a two-partition of the target space, with $a^* = -90$ for target $t < -80$ and $a^* = 10$ for $t > -80$. In both cases, we regress the negative distance between $a^*$ and the *Actions* actually chosen. Observations are not independent because they are derived from a limited number of participants, and so we address this non-exchangeability using multilevel models, in keeping with related previous work (e.g., Minozzi and Woon 2013, 2016, 2019, 2020). The multilevel models we estimate include random intercepts at the session, period, sender, and receiver levels, thus adjusting for average levels of *Target-Action* proximity within each group at each level.

Table 2 presents the results of these regressions. If the *Text* condition had moved behavior

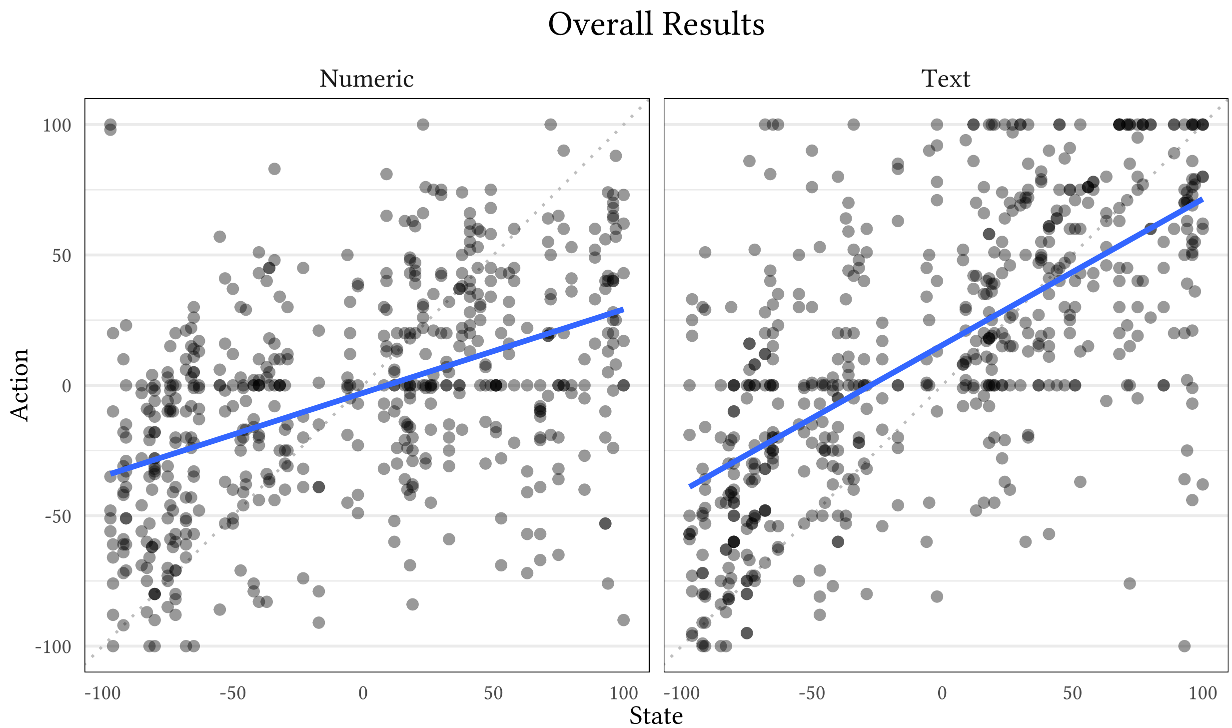


Figure 1: The figure displays scatterplots of *Target* and *Action* in each experimental condition, along with regression lines. Against expectations, the relationship between the two is more informative with *Text* rather than *Numeric* messages.

Table 2: Text Increases Distance from Equilibrium Predictions

| | Low Shift | High Shift |
|---|---|---|
| Intercept | $-33.41^*$ | $-29.47^*$ |
| | $(4.93)$ | $(4.64)$ |
| Text | $-7.80$ | $-12.88^*$ |
| | $(6.88)$ | $(6.18)$ |
| $n$ Observations | 530 | 555 |
| $n$ Session | 6 | 6 |
| $n$ Sender | 37 | 37 |
| $n$ Receiver | 37 | 37 |
| $n$ Period | 15 | 15 |
| *Error terms* | Group SD | |
| Session | 7.18 | 5.42 |
| Sender | 7.06 | 3.68 |
| Receiver | 4.13 | 10.78 |
| Period | 4.30 | 3.63 |
| Residual | 25.56 | 26.96 |

The table presents mixed effects linear models of the negative distance between equilibrium predictions for actions and those actually selected by subjects. The models includes random intercepts for session, period, sender, and receiver. $^*$ Zero is not included in the 95% interval.

closer to equilibrium, we would have seen positive and significant coefficients. Instead, both estimates are negative; in the *High Shift* case, significantly so. Thus, rather than moving play closer to equilibrium predictions, the *Text* condition seems to have widened the gap between theory and evidence.

Next, we verify that the comparative statics prediction—decreasing the divergence between preferences of senders and receivers should increase informativeness—persists with *Text* communication. Again, message behavior is difficult to compare across conditions because many senders chose not to send messages including numbers. Therefore, we focus on *Actions.* To test the comparative statics prediction, we regress the negative distance between *Target* and *Action* on a dummy for *Low Shift*, separately by treatment condition. We present results from mixed effects linear models, which include random intercepts at the session-, sender-, receiver-, and period-levels, to account for the panel structure of the data.

Table 3 presents the results. In both cases, the comparative statics prediction is consistent

Table 3: Evidence for the Comparative Statics Prediction Persists

| | Numeric | Text | Numeric | Text |
|---|---|---|---|---|
| Intercept | $-49.92^*$ | $-47.30^*$ | $-50.09^*$ | $-47.23^*$ |
| | (6.55) | (8.46) | (6.49) | (8.74) |
| Low Shift | $12.74^*$ | $17.49^*$ | $14.51^*$ | $18.45^*$ |
| | (4.03) | (3.01) | (4.27) | (3.15) |
| Target below Cutoff | | | $-10.65$ | $-5.82$ |
| | | | (5.72) | (5.02) |
| $n$ Observations | 540 | 570 | 540 | 570 |
| $n$ Session | 3 | 3 | 3 | 3 |
| $n$ Receiver | 18 | 19 | 18 | 19 |
| $n$ Sender | 18 | 19 | 18 | 19 |
| $n$ Period | 30 | 30 | 30 | 30 |
| *Error terms* | Group SD | | | |
| Session | 9.00 | 13.78 | 9.55 | 13.77 |
| Receiver | 3.32 | 13.30 | 3.21 | 13.34 |
| Sender | 6.19 | 8.21 | 6.31 | 8.19 |
| Period | 8.24 | 4.74 | 8.77 | 4.91 |
| Residual | 30.88 | 28.85 | 30.72 | 28.81 |

The table presents mixed effects linear models of the negative distance between targets and actions. The models includes random intercepts for session, period, sender, and receiver. * Zero is not included in the 95% interval.

with positive coefficients on *Low Shift*. Indeed, that is what we find. Regardless of condition, the effect of *Low Shift* appears to be to increase the proximity of *Targets* and *Actions*, as expected.[3] If anything, it appears that this effect may have been increased by the *Text* condition.[4]

Based on these results, we conclude that the number line technology has not artificially caused overcommunication or naïve exaggeration to occur in the lab. If anything, it appears instead that the numeric communication technology may have inhibited overcommunication, even more than has been previously documented.

[3] As a robustness check, the third and fourth columns of Table 3 include a dummy indicating whether the *Target* was below the cutoff of $-80$ in the Low Shift case, in which case the cutoff equilibrium implies a different Action than when *Target* $> -80$. Our findings are unaltered in that case.

[4] We cannot perform a similar analysis for senders because not all senders' messages in the *Text* condition included numbers.

Table 4: Payoffs Depend on Communication Technology

| | Sender | Receiver | Pareto |
|---|---|---|---|
| Intercept | 11.83* | 13.50* | 0.45* |
| | (0.28) | (0.24) | (0.07) |
| Text | 1.16* | 0.14 | 0.17* |
| | (0.22) | (0.33) | (0.08) |
| Low Shift | 1.74* | 0.65* | −0.08 |
| | (0.37) | (0.16) | (0.06) |
| Text × Low Shift | −0.78* | 0.23 | −0.06 |
| | (0.24) | (0.18) | (0.06) |
| $n$ Observations | 1110 | 1110 | 1110 |
| $n$ Session | 6 | 6 | 6 |
| $n$ Sender | 37 | 37 | 37 |
| $n$ Receiver | 37 | 37 | 37 |
| $n$ Period | 30 | 30 | 30 |
| *Error terms* | | Group SD | |
| Session | 0.17 | 0.29 | 0.07 |
| Sender | 0.15 | 0.34 | 0.09 |
| Receiver | 0.16 | 0.46 | 0.06 |
| Period | 0.91 | 0.26 | 0.11 |
| Residual | 2.00 | 1.51 | 0.47 |

The first two columns present mixed effects linear models of payoffs. The last presents a similar model of an indicator for whether the action selected was on the Pareto frontier, i.e., between the target and the target + shift. The models includes random intercepts for session, period, sender, and receiver. * Zero is not included in the 95% interval.

# 4 Text Messages Increased Payoffs & Efficiency

Given that *Text* caused improvements in informativeness, a reasonable first question is who benefited: senders or receivers? To investigate this, we estimated mixed effects regressions of senders' payoffs and receivers' payoffs on Text, Low Shift, and their interaction. All observations are used in both cases.

The first two columns of Table 4 present the resulting models of payoffs. It appears that senders benefited more than receivers from the ability to send text-based messages. On average, players earned about \$0.50 (95% interval = [\$0.11, \$0.93]) more in *Text* than *Numeric*. But most of that difference appears for senders in the *High Shift* case, when the only equilibrium is babbling. There is a smaller, weakly positive effect for both senders and receivers in the *Text* condition with the *Low Shift*, but these do not reach statistical significance.[5]

There are a few possibilities for how senders' eked this pay rise from the *Text* condition. They could either have used the technology to successfully persuade receivers to move actions closer to the senders' shifted targets and away from the receiver's ideal points. Or, they could have moved actions from outside the Pareto optimal region—actions between the target and the shifted target—into that range. Finally, they might have used other aspects of text to establish trust with receivers. Given that receivers seem to have, on balance, benefited slightly from the difference in communication mode, the latter two possibilities appear more likely.

To further probe whether senders might have benefited by moving suboptimal actions into the Pareto region, we coded a dummy variable called *Pareto* that is 1 when the action is between the *Target* and *Target + Shift*, and 0 otherwise. We regressed *Pareto* on *Text*, *Low Shift*, and their interaction, using the familiar mixed effects model from above. The results appear in the third column of Table 4, and support the main claim. The coefficient on *Text* is positive and significant, indicating that this mode of communication resulted in about a $17\%$ increase in Pareto-optimal actions in the *High Shift* condition. The increases were smaller and not statistically significant in the *Low Shift*.[6] One possibility for why the effect was more pronounced with the High Shift is that the Pareto region is simply larger in this case. We reconsider the relationship between changes in efficiency and payoffs below.

## 5 Did Text Change Content and Outcomes?

*Text* communication increased the gap between equilibrium predictions and experimental evidence. This technology also seems to have benefited senders, affording them the chance to move suboptimal actions into the Pareto region. But how did they achieve this?

To answer this question, we first code the *Text* messages, identifying a variety of tactics that senders used (e.g., sending precise messages, appealing to fairness, and more described below).

[5]For senders' payoffs, the effect of *Text* in the *Low Shift* condition was $\$0.38$ $[-\$0.05, \$0.81]$, and for receivers it was $\$0.39$ $[-\$0.24, \$1.07]$.

[6]In the *Low Shift* condition, the effect of *Text* on *Pareto* was an increase of $11\%$ $[-5\%, 26\%]$.

Since we lack *a priori* expectations about the conditions under which subjects will use these tactics, we conducted exploratory analyses. Finally, we compare the text messages that included numeric content to the numeric messages themselves.

Based on these analyses, we conclude that senders achieved the Pareto improvement by selecting when and for what target values they sent precise messages, as opposed to other less informative options. Senders in the *Text* condition used numeric messages disproportionately when the *Target* was higher, that is, in the higher pooling region predicted by equilibrium analysis.

Senders used Text messages in many ways. We isolated a set of eleven different tactics that we observed subjects use, and coded each message for each tactic.[7] These tactics are clustered into a few groups. First, we coded messages based on their putative informational content. Some senders chose to send precise messages with numeric content, e.g., "Your target is 89." Others refrained from sending any information at all, e.g., "Rainy days are not fun." We therefore coded messages as being *Empty* if they included no relevant information to the decision at hand, and *Precise* if they identified specific, relevant information. Similarly, some senders split the difference, indicating that the *Target* lay in some *Interval* ("target is between -40 and 20"), or used *Noisy* language ("a medium positive number"), and so we coded each of these as well.[8]

These four informational content categories are mutually exclusive.[9] Most messages—about 72%—were *Precise* (i.e., numeric messages, excluding those mentioning intervals), meaning that the sender either identified a unique *Target* value or suggested a unique *Action.* About 13% of messages were *Noisy* and slightly smaller fraction (11%) were completely *Empty.* Only the remaining 4% used explicit *Interval* messages.

[7]We identified the categories we describe below and utilized three research assistants to code messages based on our coding rules. Research assistants read the coding sheet and took a short quiz to ensure comprehension. Interrater reliability was excellent, with Cronbach's $\alpha$ ranging from 0.83 to 0.98 across tactics. See appendix for the coding sheet and quiz.

[8]Our original coding scheme differentiated qualitative description ("relatively high") from noisy quantitative descriptions ("near zero"), but we combined these categories because of relatively low numbers in each and a substantial proportion of border cases ("small positive number close to zero").

[9]After reconciling the smaller fraction of coder disagreements, only 11 of the 570 messages were coded in more than one of these categories. We resolved these few cases so that all messages belong to only one informational content category.

Next, we coded whether senders invoked normative considerations. The most frequent such considerations were importuning receivers with pleas of *Honesty* ("Your target is 75, but can you put 90 cause I'm honest, pleaseeee") and *Fairness* ("'Aha!' he shouted. 'Pick 60 and we can share the spoils evenly!"'). Both tactics were relatively rare, with invocations of *Honesty* appearing in 11% of messages, and *Fairness* in 16%. These tactics were not mutually exclusive; in fact, they were weakly correlated. Conditional on appealing to Fairness, the frequency of mentioning *Honesty* increased from 11% to 17%. In the complementary case, conditional of invoking Honesty, the frequency of mentioning Fairness rose from 16% to 24%.

Finally, we coded whether senders mentioned particular quantities with their messages. Recall that the Sender's *Target* is simply the value of the hidden state, $t$, and the Receiver's *Target* is the shifted value, $t + s$. Senders might have indicated the *Sender's Target*, the *Receiver's Target*, called out a particular Action, or suggested a value for a *Midpoint* that lay between the two targets.[10] The most commonly mentioned quantity was the *Receiver's Target*, which was called out in 46% of messages. Both the *Sender's Target* and *Action* were also not uncommon, each appearing in about 30% of messages. The *Midpoint* was comparatively rare, with mentions in only 6% of messages, likely reflecting the relative rarity of *Fairness*. As with normative considerations, these mentioned quantities are not mutually exclusive. In fact, senders mentioned both *Sender's* and *Receiver's Targets* in almost $25\%$ of cases.

The first five columns of Table 5 present mixed effects regression models of each of the categories.[11] In each case, we regress the indicated dummy variable on the *Target* value (rescaled to run from -1 to 1), *Time* during the session (which has been rescaled to run from 0 to 1), and *Low Shift*. Recall that all *Low Shift* rounds occurred during the second half of sessions, and so *Time* and *Low Shift* are correlated. We also continue to rely on our random intercept strategy from above.

[10] Analytically, it should not matter whether senders mention their own target or the receiver's because the difference between the two is common knowledge, which is emphasized in the instructions. Nevertheless, we employ two distinct categories here because is is conceivable that some receivers would not clearly track this linear dependence, and it is therefore an empirical question as to whether there is variation in the consequences of this difference in messages.

[11] Multinomial logistic regression yields similar results.

Table 5: Varieties of Text Messages

| | Empty | Precise | Interval | Noisy | Honesty | Fairness |
|---|---|---|---|---|---|---|
| Intercept | 0.15 | 0.61* | 0.06 | 0.18 | 0.02 | 0.10 |
| | (0.09) | (0.13) | (0.06) | (0.09) | (0.10) | (0.10) |
| Target | −0.06* | 0.05* | 0.00 | 0.00 | 0.02 | 0.00 |
| | (0.02) | (0.02) | (0.01) | (0.02) | (0.02) | (0.02) |
| Time | −0.06 | 0.22* | −0.04 | −0.13* | 0.07 | 0.12 |
| | (0.07) | (0.08) | (0.06) | (0.07) | (0.06) | (0.08) |
| Low Shift | −0.04 | 0.01 | 0.01 | 0.03 | 0.10* | −0.02 |
| | (0.04) | (0.05) | (0.03) | (0.04) | (0.04) | (0.05) |
| $n$ Observations | 570 | 570 | 570 | 570 | 570 | 570 |
| $n$ Session | 3 | 3 | 3 | 3 | 3 | 3 |
| $n$ Sender | 19 | 19 | 19 | 19 | 19 | 19 |
| $n$ Period | 30 | 30 | 30 | 30 | 30 | 30 |
| *Error terms* | | | Group SD | | | |
| Session | 0.13 | 0.18 | 0.08 | 0.13 | 0.16 | 0.14 |
| Sender | 0.22 | 0.38 | 0.13 | 0.29 | 0.22 | 0.27 |
| Period | 0.02 | 0.02 | 0.03 | 0.02 | 0.01 | 0.02 |
| Residual | 0.23 | 0.26 | 0.16 | 0.21 | 0.21 | 0.27 |

The table presents the results of mixed effects linear probability regression models. The models includes random intercepts for period and sender to control for the panel structure of the data.

Regressions of each normative indicator appear in the final two columns of Table 5. Again, both models indicate a great deal of idiosyncrasy, although claims of *Honesty* were about 5% more frequent in the *Low Shift* case.

These regression models yield three systematic findings. First, senders relied on more precise messages for higher *Target* values. There is a negative, significant coefficient on *Target* in the first column, which models the choice of an *Empty* message, and there is a concomitant, positive significant coefficient on *Target* in the second column, which models *Precise* choices. Both indicate that higher *Targets* led to more precise messages. Second, senders relied on more precise messages later in the session. The positive coefficient on *Time* in the model of *Precise* messages indicates that these messages were used more late in sessions, while the negative coefficient on *Time* in the model of *Qualitative* messages in the fourth column suggests that the letter were used less over time. Third, not much else is well predicted by these models. In fact, the large group standard deviations reported in the bottom rows of the table suggest that senders behaved somewhat idiosyncratically. Perusing the messages themselves confirms this suspicion, as many

Table 6: What Gets Mentioned?

| | Sender's Target | Receiver's Target | Action | Midpoint |
|---|---|---|---|---|
| Intercept | 0.26 | 0.45* | 0.32* | 0.09 |
| | (0.14) | (0.19) | (0.13) | (0.06) |
| Target | 0.00 | 0.05* | 0.06* | 0.02 |
| | (0.02) | (0.02) | (0.02) | (0.02) |
| Time | 0.19 | 0.09 | −0.03 | 0.00 |
| | (0.10) | (0.10) | (0.10) | (0.06) |
| Low Shift | −0.08 | −0.05 | 0.00 | −0.05 |
| | (0.06) | (0.06) | (0.06) | (0.04) |
| $n$ Observations | 570 | 570 | 570 | 570 |
| $n$ Session | 3 | 3 | 3 | 3 |
| $n$ Sender | 19 | 19 | 19 | 19 |
| $n$ Period | 30 | 30 | 30 | 30 |
| *Error terms* | | Group SD | | |
| Session | 0.20 | 0.31 | 0.18 | 0.09 |
| Sender | 0.35 | 0.39 | 0.34 | 0.14 |
| Period | 0.03 | 0.03 | 0.03 | 0.01 |
| Residual | 0.33 | 0.31 | 0.34 | 0.21 |

The table presents the results of mixed effects linear probability regression models. The models includes random intercepts for session, period, and sender to control for the panel structure of the data.

senders would rely on a particular tactic for several periods in a row, then abruptly switch to another tack.

Table 6 shows the results of mixed effects regressions of the variables measuring which quantities senders mentioned. The results here suggest the most systematic variation in mentions of the *Sender's Target* and *Action*. In both cases, higher *Target* values were associated with more mentions. The effect of *Time* played differently for these two variables, however, as the *Sender's Target* was more likely to be mentioned later on, just as the *Action* was being mentioned less often. Nothing systematic emerged for either *Receiver's Target* or *Midpoint*.[12]

Finally, to bring our analysis of payoffs and efficiency together with that of senders' use of text messages, we estimate the marginal effects of each tactic on outcomes. Specifically, we used the evidence from the *Text* condition to estimate the effects of each tactic on each outcome: sender's

[12] When the *Target* is higher, precise messages are more frequent (Table 5, column 2) and the Receiver's Target gets mentioned more often (Table 6, column 2). Since potentially truthful messages are capped at 100, this pattern may result from fundamentally different behavior at high *Target* values. To test for this, we checked robustness on the subsets of observations with *Target* less than 60, yet observe no substantive differences for these lower *Target* values (see Appendix Tables A1 and A2).

payoff, receiver's payoff, and *Pareto*. Because senders sometimes used more than one of these tactics within the same text message, we use support vector machine regression to isolate the effect of each tactic. In each case, we included all 11 tactics we modeled above, as well as *Target*, *Time*, and Low Shift. Support vector machine regressions flexibly adjust for all interactions between covariates, and so we focus on calculating marginal effects for each tactic and each outcome. To do so, we used our fitted models to predict outcomes, switching each tactic "on" and "off" and calculating the difference in predicted values. For inference, we rely on the interquartile range of pointwise estimates from the dataset.

The results appear in Figure 2, and they support several conclusions. First, we see that mentions of *Honesty* led to both increases in efficiency (i.e., *Pareto*, bottom panel) and receiver payoffs (middle panel), but not sender payoffs (top panel). Second, mentions of the Sender's target were more likely to coincide with increases in sender payoffs and increases in efficiency, but not receiver payoffs. Third, the tactics that increased efficiency did not always redound mainly to either senders or receivers. Thus, it seems that the increases in sender payoffs and efficiency that we identified in the previous sections are partially, but not wholly related to each other. In particular, many aspects of text-based communication increased efficiency in ways that did not directly benefit the sender.

Finally, we focus on the text messages that included numeric content. Specifically, we analyze the subset of cases in the *Text* condition that offered precise values for the *Receiver's Target*. Therefore, we drop all cases in which senders in the Text condition sent imprecise, interval, or exclusively non-numeric content, which total 164 of the 1110 observations, leaving us with about $85\%$ of cases (i.e., all numeric messages, including both precise and those mentioning intervals). Of course, these cases were not randomly selected, and we do not claim that they are. Nevertheless, exploring the differences between these two cases helps illuminate how the communication technology affected message accuracy. We estimate two dependent variables on this subsample. First, we modeled the proximity (negative absolute difference) of *Target* and *Message* for all cases sent in the *Numeric* condition, and second we focused on the *Message* itself.

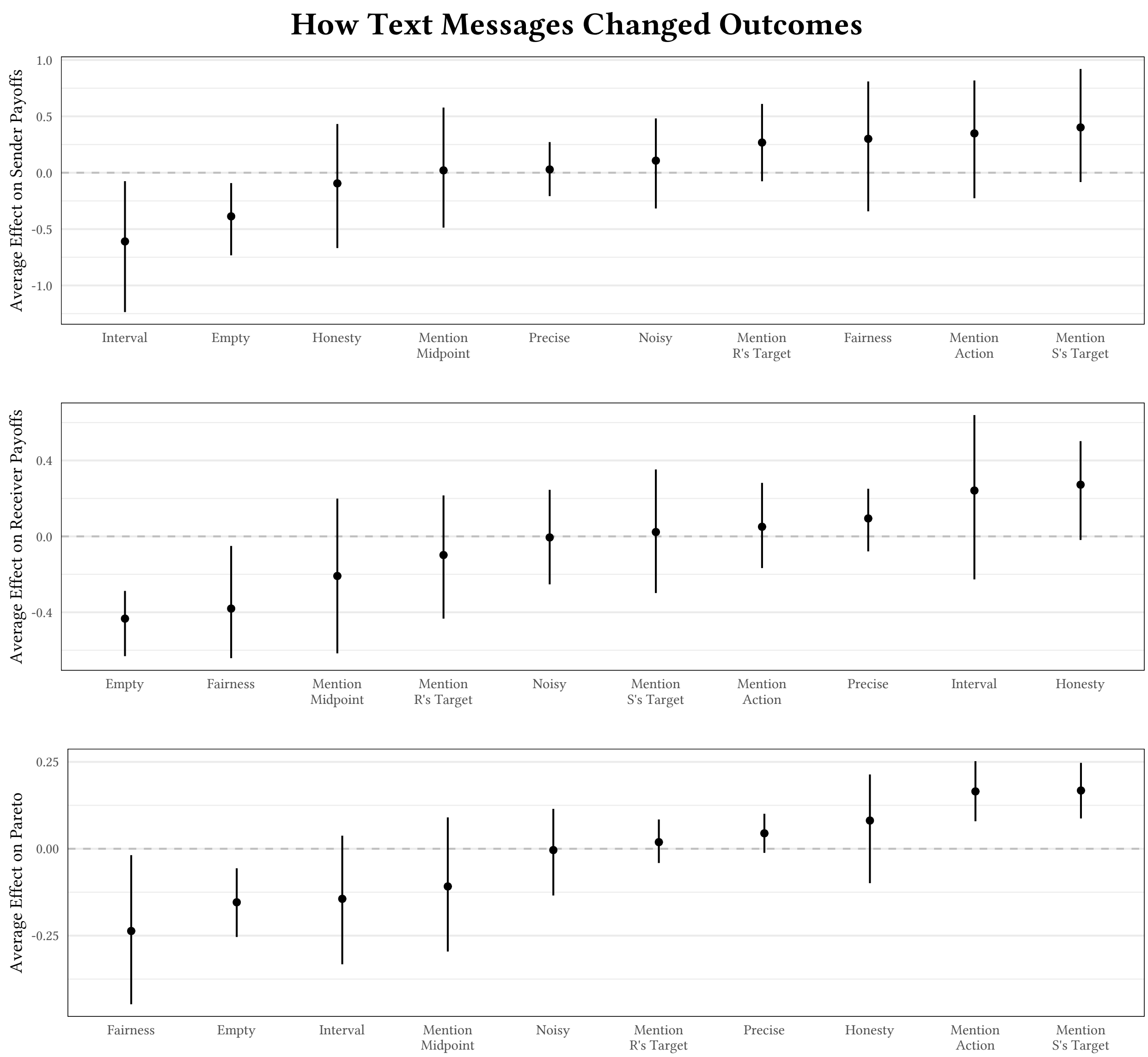


Figure 2: The figure displays summaries of estimated pointwise marginal effects with means depicted by points and interquartile ranges by segments. All estimates are based on support vector machine regressions.

Table 7: Evidence for the Comparative Statics Prediction Persists

| | $-\|Target-Message\|$ | $Message$ |
|---|---|---|
| Intercept | $-80.17^{*}$ | $62.90^{*}$ |
| | (7.18) | (12.20) |
| Low Shift | $17.20^{*}$ | $-5.32$ |
| | (5.50) | (4.48) |
| Text | $25.71^{*}$ | $-17.07$ |
| | (8.55) | (16.95) |
| Low Shift × Text | 7.57 | $-11.53^{*}$ |
| | (4.64) | (4.62) |
| Target | | $0.16^{*}$ |
| | | (0.04) |
| Target × Low Shift | | $0.47^{*}$ |
| | | (0.06) |
| Target × Text | | $0.47^{*}$ |
| | | (0.06) |
| Target × Low Shift × Text | | $-0.23^{*}$ |
| | | (0.08) |
| $n$ Observations | 946 | 946 |
| $n$ Sender | 37 | 37 |
| $n$ Period | 30 | 30 |
| $n$ Session | 6 | 6 |
| *Error terms* | Group SD | |
| Sender | 14.52 | 15.13 |
| Period | 13.23 | 8.92 |
| Session | 8.17 | 17.35 |
| Residual | 35.05 | 34.95 |

The table presents mixed effects linear models of the negative distance between targets and actions. The models includes random intercepts for session, period, and sender. The number of observations is 946 rather than 1110 because we can only include messages with numeric content in this analysis. * Zero is not included in the 95% interval.

The first column of Table 7 reports the results of a mixed effects regression of the proximity of *Target* and *Message* on the *Low Shift* and *Text* dummies and their interaction. As expected, *Target* and *Message* are more proximate in the Low Shift case, and the *Text* condition led to more accurate messages, conditional on the sender choosing to send precise messages. This accuracy effect of *Text* was larger in the *Low Shift* case, given the positive, albeit insignificant, coefficient on the interaction term.

The second column of Table 7 displays the details of a saturated mixed effects regression of *Message* on the *Target*, *Low Shift*, and *Text*. The results resonate with those from the first column.

While there is an intercept shift for *Low Shift*, it seems mostly limited to the *Text* condition, as seen by comparing the coefficients on *Low Shift* and *Low Shift* $\times$ *Text*. There are also large differences in the slopes on *Target*. With *Text*, numeric messages were most closely tied to the target, with the slope in the *Low Shift* case being 0.87, closer to 1 than in any of the other three cases.[13] Thus, the difference in proximity demonstrated in the first column is corroborated by the message strategies documented in the second.

That said, these two conditions are not exactly comparable, as we can only compare messages in the *Numeric* condition to the subset of *Text* messages with precise, numeric content. However, this evidence does entail one clear inference: receivers in the *Text* condition who observed a precise, numeric message could feel more confident about its accuracy than their counterparts in the *Numeric* condition. Even when given a rich text box format to communicate, many senders feel bounded by the conceptual limits of the numeric state space. Moreover, it does not appear that the numeric message space caused the overcommunication findings from the literature. Instead, it seems like the limited communication technology may have inhibited some subjects' tendency to convey accurate information about the hidden state.

# 6 Discussion

We reported on how senders in a cheap talk game used different communication technologies to communicate, contrasting the numeric messages familiar from other experiments with unrestricted natural language messages. Our research produced several results. The comparative statics prediction—that less preference divergence leads to more informativeness—persists, even in the case of text messaging. But the overcommunication finding, in which senders reveal more information than predicted by equilibrium not only persists, it is somewhat exacerbated by this alternative technology. As a consequence, we cannot conclude that the lack of equilibrium play observed in existing sender-receiver experiments is an artifact of the lab. Instead it seems to be a

[13] Specifically, the slope coefficients were $0.16+0.47+0.47-0.23 = 0.87$ in the *Low Shift*/*Text* case, $0.16+0.47 = 0.63$ in the *Low Shift*/*Numeric* case, $0.16+0.47 = 0.63$ in the *High Shift*/*Text* case, and $0.16$ in the *High Shift*/*Numeric* case.

stylized fact of interpersonal communication.

A key implication of our findings is that overcommunication is not an artifact of requiring experimental subjects to use numeric messages. Yet the difference in behavior caused by using natural language to communicate also has implications for how we explain overcommunication. Common explanations of overcommunication include level-$k$ thinking, lying costs, and other-regarding preferences (Lafky, Lai and Lim, 2022). Both lying costs and other-regarding preferences suggest that there is a dispositional character to overcommunication, a feature that is invariant to the linguistic medium used to convey information. Our findings, therefore, are more supportive of the position that overcommunication is tied to strategic thinking as constrained by cognitive limits.

We go on to explore how senders used text, and who benefited from it. Specifically, we showed that text increased both payoffs and efficiency, but mostly for senders and not receivers. These increases emerged from the use of a variety of tactics, including proclamations of honesty and invocations of fairness, but also mentions of specific quantities, including the sender's own ideal action. On balance, many of these tactics improved efficiency, moving the receivers' suboptimal actions into the Pareto region. Most of these benefits were split between players, but some accrued more to one than the other. Senders seemed to benefit more across the board, while receivers only benefited from isolated tactics. There are a few possible explanations for this phenomenon, notably, that precise mentions of specific information mean more when they might have been replaced by more nebulous, ambiguous signals. Future work should focus on the mechanisms that explain how these tactics improved efficiency and payoffs.

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# Online Appendix

## Instructions and Interface

The experimental instructions provided to subjects appear below. The instructions for the two treatments differ in only a few places, and so the two are printed together to emphasize exactly how they differed. In each case that they differ, the differences appear within square brackets. Inside the brackets, the instructions for the Numeric treatment appear before the slash, and the instructions for the Text treatment appear after the slash. When a condition includes no text to substitute, it reads "$\langle$ nothing $\rangle$".

# Instructions

## General Information

This is an experiment on the economics of communication. The University of Pittsburgh and Ohio State University have provided funds for this research. You will be paid in cash for your participation, and 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. These earnings will be paid to you at the end of the experiment along with the $7 participation payment.

Each participant has a printed copy of these instructions, and you may refer to them at any time.

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 in any way. Also, please ensure that your phones are off and your personal belongings are 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.

## Parts and Rounds

This experiment consists of two parts, and we will explain the instructions for each part before beginning that part. Each part consists of 15 rounds, and each round is a separate decision task.

We will **randomly select one round** to count for payment from the entire session. Each round is equally likely to be selected. The points you receive from that round will be used to calculate your payment for the experiment, and points will be converted to cash at the rate of $1 for every 20 points. More specifically, we will take the total number of points you earned in the round that counts, divide by 20, and then round this amount to the nearest quarter. We will pay you this amount in addition to the $7 participation payment.

## Roles and Matching

Each participant will be assigned to one of two roles: S or R. Your role will be assigned before the first round and will remain fixed throughout the experiment; it will be the same in both parts.

Before every round, you will be randomly matched with one other participant. In every pair of participants there will be one player in each role (one S player and one R player).

Note that you will not know the identity of the other participant you are matched with in any round, and your earnings for each round depend only on your decision in that round and the decision of the participant you are matched with in that round.

**Part 1**

## Targets

In every round there will be a set of targets:

**Player R's Target** will be a randomly selected number between $-100$ and $100$. Each number is equally likely to be R's target. R's Target for one round does not affect the value that is randomly selected for any other round.

**Player S's Target** will always be $80$ more than Player R's Target. For example, if R's Target is $100$, then S's Target is $180$; if R's Target is $0$, then S's Target is $80$, etc.

## Sequence

The sequence of actions in every round is as follows:

1. **Player S** observes the set of targets and [chooses a **Message**, which can be any whole number from $-100$ to $100$./ types a text **Message**, which can be up to $120$ characters in length (including spaces). You can type any message you want, but we ask that you refrain from using obscene language or providing any information that might identify who you are.]
2. **Player R** observes the Message, but not either of the Targets, and chooses an **Action**, which can also be any whole number from $-100$ to $100$.
3. The computer determines each player's **Payoff** as a function of his or her Target and the Action chosen by R.

## Payoffs

In each round, each player's payoff depends on how close R's Action is to his or her own Target. Specifically, each player earns 320 points if R's Action equals his or her own Target and 1 point less for each unit of difference between R's Action and his or her Target. Mathematically, this is described by the following formula, where the straight lines indicate absolute value:

$$\text{Player's Payoff} = 320 - |\text{Player's Target} - \text{R's Action}|$$

[Note that the Message is not part of the Payoff formula. / $\langle$ nothing $\rangle$]

To illustrate, consider a few examples.

Example 1: R's Target is $50$, so S's Target is $130$. If R chooses the Action $50$, R's payoff is $320$ since the Action equals R's Target. The difference between R's Action and S's Target is $80$, so S's payoff is $240$. If R instead chooses the Action $0$, then R's payoff would be $270$ and S's payoff would be $190$.

Example 2: R's Target is $-60$, so S's Target is $20$. If R chooses the Action $-90$, then R's payoff is $290$ and S's payoff is $210$. If R instead chooses the Action $40$, then R's payoff would be $220$ and S's payoff would be $300$.

Of course these are only a few examples. During the experiment, the software will provide you with a "Payoff Calculator" that will compute each player's payoff for any combination of R's Target and Action.

## SUMMARY

**Targets**

R's Target $=$ Number between $-100$ and $100$

S's Target $=$ R's Target $+\ 80$ Note that it is possible for S's Target to be outside the set of possible Actions.

**Sequence**

1. S sees both targets, then [chooses a Message from $-100$ to $100$ / types a text Message (up to $120$ characters long)]
2. R sees only the Message and chooses an Action from $-100$ to $100$

**Payoffs**

$$\text{Player's Payoff} = 320 - |\text{Player's Target} - \text{R's Action}|$$

**Payment**

One round randomly selected for payment.

**INSTRUCTION QUIZ.** To check your understanding of the decision tasks, please answer the questions below. When you are finished, the computer will check your answers and feedback will be shown on the screen. Note that your quiz answers do not affect your earnings, but you must attempt to answer all of the questions before the computer will check your answers. During the quiz, you are free to refer to your printed instructions.

Once everyone has completed the instruction questions, we will begin the experiment. If you have any further questions at this time, please raise your hand and the experimenter will come to you.

1. In every round, will you be matched with same participant? $\{$Yes, No$\}$
2. Player R's target can be any number from: $\{0$ to $10$, $0$ to $100$, $-100$ to $100$, $-150$ to $150\}$
3. If Player R's target is $-40$, then what is **Player S's** target? $\{-160, -120, -80, 40\}$
4. If Player R's target is 55, then what is **Player S's** target? $\{$20, 55, 90, 135]
5. [Player S's message can be any number from $\{0$ to $10$, $0$ to $100$, $-100$ to $100$, $-150$ to $150\}$. / $\langle$ nothing $\rangle$]
6. Suppose [the Message M is $0$ and / $\langle$ nothing $\rangle$] Player R chooses the Action $30$. If the Target turns out to be $20$, how many points will **Player R** receive? $\{10, 20, 270, 310\}$
7. Suppose [the Message M is $100$ and / $\langle$ nothing $\rangle$] Player R chooses the Action $80$. If the Target turns out to be $-60$, how many points will **Player R** receive? $\{80, 140, 180, 260\}$
8. If Player R's Target is $50$ and Player R chooses the Action $100$, how many points will **Player S** receive? $\{50, 270, 290, 320\}$
9. If Player R's Target is $-80$ and Player R chooses the Action $-80$, how many points will **Player S** receive? $\{80, 160, 240, 320\}$
10. How many points will **Player S** receive if **Player S's Target** is $120$ and Player R chooses the Action $0$? $\{120, 200, 240, 320\}$

## Part 2

The game in Part 2 is almost exactly the same as in Part 1, except for one difference: In Part 2, Player S's Target will always be $40$ more than Player R's Target. For example, if R's Target is $100$, then S's Target is $140$; if R's Target is $0$, then S's Target is $40$, etc.

## PART 2 SUMMARY

### Targets

R's Target $=$ Number between $-100$ and $100$

**S's Target** $=$ **R's Target** $+$ **40**

Note that it is possible for S's Target to be outside the set of possible Actions.

### Sequence

1. S sees both targets, then [chooses a Message from $-100$ to $100$ / types a text Message (up to $120$ characters long)]
2. R sees only the Message and chooses an Action from $-100$ to $100$

### Payoffs

$$\text{Player's Payoff} = 320 - |\text{Player's Target} - \text{R's Action}|$$

### Payment

One round randomly selected for payment.

Table A1: Varieties of Text Messages (Robustness)

| | All Observations | $r < 60$ |
|---|---|---|
| Intercept | $0.61^*$ | $0.62^*$ |
| | (0.13) | (0.14) |
| Target | $0.05^*$ | $0.07^*$ |
| | (0.02) | (0.03) |
| Time | $0.22^*$ | $0.19^*$ |
| | (0.08) | (0.09) |
| Low Shift | 0.01 | 0.03 |
| | (0.05) | (0.05) |
| $n$ Observations | 570 | 468 |
| $n$ Session | 3 | 3 |
| $n$ Sender | 19 | 19 |
| $n$ Period | 30 | 30 |
| *Error terms* | Group SD | |
| Session | 0.18 | 0.18 |
| Sender | 0.38 | 0.39 |
| Period | 0.02 | 0.02 |
| Residual | 0.26 | 0.26 |

The table presents the results of mixed effects linear probability regression models. The models includes random intercepts for period and sender to control for the panel structure of the data.

Table A2: What Gets Mentioned? (Robustness)

| | All Observations | $r < 60$ |
|---|---|---|
| Intercept | $0.45^{*}$ | $0.42^{*}$ |
| | (0.19) | (0.20) |
| Target | $0.05^{*}$ | 0.04 |
| | (0.02) | (0.03) |
| Time | 0.09 | 0.15 |
| | (0.10) | (0.10) |
| Low Shift | $-0.05$ | $-0.08$ |
| | (0.06) | (0.06) |
| $n$ Observations | 570 | 468 |
| $n$ Session | 3 | 3 |
| $n$ Sender | 19 | 19 |
| $n$ Period | 30 | 30 |
| *Error terms* | Group SD | |
| Session | 0.31 | 0.30 |
| Sender | 0.39 | 0.41 |
| Period | 0.03 | 0.03 |
| Residual | 0.31 | 0.29 |

The table presents the results of mixed effects linear probability regression models. The models includes random intercepts for session, period, and sender to control for the panel structure of the data.