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Costly and discrete communication: an experimental investigation

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Abstract Language is an imperfect and coarse means of communicating information about a complex and nuanced world. We report on an experiment designed to capture this feature of communication. The messages available to the sender imperfectly describe the state of the world; however, the sender can improve communication, at a cost, by increasing the complexity or elaborateness of the message. Here the sender learns the state of the world, then sends a message to the receiver. The receiver observes the message and provides a best guess about the state. The incentives of the players are aligned in the sense that both sender and receiver are paid an amount which is increasing in the accuracy of the receiver's guess. We find that the size of the language endogenously emerges as a function of the costs of communication. Specifically, we find that higher communication costs are associated with a smaller language. Although the equilibrium predictions do not perform well, this divergence occurs in a manner which is consistent with the experimental communication literature: overcommunication. We find that the sender's payoffs, relative to equilibrium payoffs, are decreasing in the cost of communication. We also find that the receiver's payoffs, relative to equilibrium payoffs, are increasing in the cost of communication. Finally, we find imperfections in coordination on the basis of the experimental labels.

Keywords Information transmission · Cheap talk · Overcommunication · Bounded rationality · Experimental game theory

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1 Introduction

The properties of words are very different from the properties of real numbers. For instance, it is not the case that there exists a word with a meaning *between* any two words. However, words are used to construct statements which convey information about a complex and nuanced reality. One can use words to express more detailed and nuanced information, but only at a cost to the sender. It is our view that language is an imperfect and coarse means of communicating information about a complex and nuanced world. We report on an experiment designed to capture this feature of communication. In our experiment, the language available to the sender imperfectly describes the state of the world. By this, we mean that the sender cannot fully and costlessly communicate. However, the sender can improve communication, at a cost, by increasing the complexity or elaborateness of the message.

By way of example, suppose that your advisee has been invited to present at a conference. Your preferences and the preferences of your advisee are identical with regards to her performance at the conference: to sound competent, to receive helpful comments, etc. In order to facilitate this success, you wish to provide her with information about how to best have a successful conference. However, there is not a single word to convey the full extent of your knowledge regarding how best to present, how best to prepare the slides, how best to respond to potential questions, etc. You can increase the amount of information conveyed only by constructing additional statements. As a result, you are unlikely to communicate *all* of the relevant information. Further, the amount of information which you provide will be related to the costs which you bear in the construction of the statements.

Hertel and Smith (2012) provide a theoretical account of such communication by adapting the uniform-quadratic version of Crawford and Sobel (1982) so that messages available to the sender are constrained to be costly and discrete. Although there are many equilibria, the authors employ an out-of-equilibrium condition which identifies the equilibria with the largest possible number of transmitted messages, which we refer to as *most informative*. The paper makes the prediction that more costly signals will be conserved (sent on smaller regions of the state space) and that the size of the language used will emerge in equilibrium as a function of the costs of communication.

We design an experiment in order to investigate communication in the Hertel and Smith setting. The questions are then, how does the most informative equilibrium identified by Hertel and Smith (2012) perform in the laboratory, and, as predicted, does the size of the language emerge as a function of the communication costs.

In this experiment, the subjects are anonymously divided into pairs, one as a *sender* and one as a *receiver*. The sender learns the state of the world then sends a message to the receiver. The receiver observes the message and selects an action which affects the payoffs of both players. The incentives of the players are aligned in the sense that both sender and receiver are paid an amount which is increasing in the accuracy of the receiver's action. In our experiment, messages imperfectly describe the underlying state space. Specifically, due to the constraints of the message space, the sender is not able to fully and costlessly communicate. However, the sender is able to transmit more information by constructing an elaborate, but costly, message.

Here the state space is an integer between -3 and 3. The sender can send a costless message, which we refer to as the "Empty" message.[1] Additionally, the sender can compose a costly message consisting of two possible elements "High" and "Low." These message elements would seem to provide a natural ordering given our state space. The cost of a message is then a function of the number of elements in the message. Therefore, the empty message can be transmitted at a cost of 0; the messages "High" and "Low" can be transmitted at a cost of c; and the messages "High High," "High Low," "Low High," and "Low Low" can be transmitted at a cost of $2c$, where we vary c.

We find that the size of the language emerges endogenously as a function of the costs of communication. On the other hand, we find that the equilibrium predictions do not perform well. However, our experimental observations differ from the theoretical predictions in a manner consistent with other experimental communication papers: the senders overcommunicate.

Previous experimental communication papers have found that senders often communicate more information than that which is in their best interest. In the context of cheap talk experiments, where senders and receivers do not have aligned preferences over the action of the receiver, overcommunication implies that the sender's message is suboptimally precise in that it allows the receiver to select an action which is closer to the receiver's preferred action than the action which the sender prefers. In other words, since the sender and receiver have different preferences over the action, overcommunication implies that the receiver selects an action which is suboptimally far from the action preferred by the sender. In our setting, where preferences over the action are aligned but sending messages is potentially costly, overcommunication implies that senders convey information which is suboptimally precise and, as a result, they incur excessive communication costs. Overcommunication in our setting implies that senders would be better off by communicating with a lower precision and, therefore, incurring smaller communication costs.

In our setting, one way in which overcommunication could be detected would be the observation that senders experience worse outcomes as communication costs increase and the receivers experience better outcomes as communication costs increase. This would suggest that senders are not sufficiently sensitive to the costs of communication, and the receivers are benefiting from this behavior. We find that the sender's payoffs, relative to the equilibrium payoffs, are decreasing in the cost of communication. We also find that the receiver's payoffs, relative to the equilibrium payoffs, are increasing in the cost of communication.

Finally, we find imperfections in coordination on the basis of the experimental labels. We find that subjects are better able to coordinate on some states rather than others, despite that there does not exist an a priori reason to expect such differences.

[1] Throughout the paper we describe the costless message as *empty* rather than the condition of having not sent a message. This is because, it might not be easy to distinguish between the case where the sender decided not to send a message and the case where the sender has not yet sent a message. To rule out this confusion we describe the costless message as empty.

2 Related literature

There is a literature which tests communication models, in general, and the Crawford and Sobel (1982) model, in particular. Perhaps the first paper testing Crawford and Sobel was Dickhaut et al. (1995) whereas more recent examples include Cai and Wang (2006) and Kawagoe and Takizawa (2009). Due to the limited ability of subjects to find complex equilibria in novel situations, testing communication equilibria typically uses simplified versions of the model. A natural way to accomplish this simplification is to specify the state space as a set of integers rather than the unit interval. For instance, Dickhaut et al. specifies the state space as the integers between 1 and 4 and Cai and Wang specifies the state space as the odd integers between 1 and 9. We select a state space as the set of integers between -3 and 3 in order to render the signal elements of "High" and "Low" relatively meaningful. This would seem to aid in the coordination problem[2] between the sender and receiver. Also note that in Dickhaut et al. (1995), Cai and Wang (2006), and Kawagoe and Takizawa (2009) the set of costless messages is not smaller than the state space. By contrast, in our setting, the number of costless messages is less than the number of states.

Experimental studies of cheap talk communication find that the senders often overcommunicate.[3] Specifically, these studies find that in a cheap talk setting where senders and receivers do not have aligned preferences, the senders often communicate more information than that which is in their best interest. Also, there is a literature which finds that subjects can have an aversion to lying.[4] Again, these findings can be interpreted as overcommunication by the senders. Note that our subjects never have an incentive to mislead the sender because the sender and receiver have identical preferences over the action of the receiver.

Despite the obvious differences between these cheap talk settings and our setting, we find that senders reveal too much information. While the senders are sensitive to the costs of communication, they are not sufficiently sensitive. We observe that senders communicate the state with a greater precision than that which is in their best interest. As a result, the senders incur excessive communication costs. The senders could improve their outcomes if they were to communicate with a lower precision and thus reduce their communication costs. Although our setting is quite different from that found in the experimental communication literature, we also find overcommunication. Given that we observe similar behavior in such different settings, we argue that overcommunication is a robust experimental phenomenon.

[2] Prior work finds that subjects can resolve similar coordination problems (Blume et al. 1998, 2001; Blume and Gneezy 2000; Kreps 1990). However this is not the focus of our paper.

[3] For example, see Cai and Wang (2006) and Kawagoe and Takizawa (2009).

[4] For instance, Gneezy (2005), Hurkens and Kartik (2009), and Sanchez-Pages and Vorsatz (2007), Sanchez-Pages and Vorsatz (2009).

3 Equilibrium predictions

Here we provide an account of the equilibrium predictions. We conclude by providing a summary of the properties of a possible equilibrium. Recall that our state space is $s \in \{-3, -2, -1, 0, 1, 2, 3\}$. Our message space is $m \in \varnothing \cup (\cup_{i=1}^{2}\{High, Low\}^{i})$. The communication costs $c(m)$ are a function of the number of elements transmitted. The message without an element (Empty message) costs $c(m) = 0$, a message with a single element costs $c(m) = c$, a message with two elements costs $c(m) = 2c$. The receiver has an action space of $a \in \{-3, -2.5, -2, -1.5, -1, -0.5, 0, 0.5, 1, 1.5, 2, 2.5, 3\}$. Both the sender and receiver prefer the receiver to select the action as close to the state as possible. The payoff to the receiver is the nonnegative integer closest to:

$$\mathrm{U^R} = 100 - 25(a - s)^2. \tag{1}$$

The payoff to the sender is the integer closest to:

$$\mathrm{U^S} = \mathrm{U^R} - c(m). \tag{2}$$

Hertel and Smith (2012) characterize the equilibria.[5] The authors show that in any equilibrium, messages are sent on adjacent states and the empty message must be sent on at least one state. As mentioned previously, there are many equilibria. We now discuss the most informative equilibria.

For $c \in [0, 12.5]$, any fully revealing equilibria will exist. In other words, each of the 7 messages are transmitted on one of the 7 states. Further, each permutation of messages and states can form an equilibrium. For $c \in (12.5, 25]$, fully revealing equilibria will still exist; however, it cannot be that messages on adjacent states have a difference in communication costs of $2c$.

For $c \in [25, 94]$, the messages "High" and "Low" are each sent on 2 adjacent states and the empty message is sent on 3 adjacent states. We note that the messages with two elements are not used.

For $c \in [94, 100]$, the empty message is sent on 5 adjacent states and the message "High" and the message "Low" are both sent on a remaining state. Again, the messages with two elements are not used. Given the empty message, the receiver is indifferent[6] between selecting −0.5 and 0.5.

For $c > 100$, the only equilibria is where the sender exclusively sends the empty message for all states. The receiver has no additional information about the state and is, therefore, indifferent among selecting −1.5, −0.5, 0.5, and 1.5.

We summarize this discussion with the following Table 1. As the above discussion indicates, there is a multiplicity of equilibria. However, given the labels of the states

[5] See Hertel and Smith (2012) for further discussion of the modeling choices. For reasons which are specified in their paper, Hertel and Smith assume that each message has a unique cost of transmission. This would seem to be less natural in an experimental setting.

[6] Note that the receiver is indifferent between selecting −0.5 and 0.5 but not 0. If the sender is pooling on more than 3 states, the expected payoff of selecting −0.5 or 0.5 is $2 \cdot \frac{94}{7} + 2 \cdot \frac{44}{7} = \frac{286}{7}$ and the expected payoff of selecting 0 is $\frac{100}{7} + 2 \cdot \frac{75}{7} = \frac{250}{7}$. Therefore, selecting the integer action yields a slightly lower payoff.

Table 1 Properties of a focal equilibrium

c	Sender's strategy given state							EU^R	EU^S
	−3	−2	−1	0	1	2	3		
10	LL	L	LH	E	HL	H	HH	100.00	85.71
30	L	L	E	E	E	H	H	89.43	72.29
50	L	L	E	E	E	H	H	89.43	60.86
96	L	E	E	E	E	E	H	68.00	40.57

Given each communication cost which we use in the subsequent experiment, the table indicates a focal equilibrium strategy of the sender for each state, and the implied expected payoffs for receiver and sender. See the discussion above for more on possible multiplicity. We note that, for a given communication cost, the multiplicity would affect the strategies of the players but not the expected payoffs

and the messages, we offer a characterization of a particular equilibrium, which we consider to be focal. For each communication cost which we use in the subsequent experiment, we characterize one possible equilibrium strategy for the sender and the payoffs implied by the equilibrium. The sender's strategy is characterized by providing a message for each state which satisfies the conditions for equilibrium. In equilibrium, the receiver would best respond to the strategy of the sender.

4 Experimental design

A total of 48 subjects participated in the experiment. The subjects were undergraduate and graduate students at Rutgers University-Camden. The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). Sessions lasted from 90 to 120 min.

In each period, the sender was shown the state, which we referred to as the "secret number." The state s consisted of an integer between -3 and 3 . In order to inform the receiver of its content, the sender was able to transmit a possibly costly message. The message "Empty" costs $c(m) = 0$, the messages "High" and "Low" both cost $c(m) = c$, the messages "High High," "High Low," "Low High," and "Low Low," each cost $c(m) = 2c$, where $c \in \{10, 30, 50, 96\}$. Upon observing the message, the receiver selected a best guess about the state. The receiver's action a was selected from the action space of half-integers between -3 and 3.

The per period payoff to the receiver was the nonnegative integer closest to $100 - 25(a - s)^2$. The per period payoff to the sender was the receiver's payoffs minus $c(m)$. The sender and receiver were each given 30 s in which to make a decision. In order to facilitate their understanding of the setting, the subjects were given a table indicating the payoffs associated with each action-state pair.[7] The subjects were given a \$5 show up fee and \$1 for every 300 points accumulated.[8]

[7] This table is provided in Appendix 1.

[8] The total amount earned in the experiment ranged from \$6.29 to \$20.54, with an average of \$15.62.

Sender and receiver were matched and played the game for 15 periods where c was held fixed. A complete history within each match was available to both sender and receiver. See Figs. 1 and 2 in Appendix 1 for the screen shot of both the sender and the receiver. After the 15 periods, each subject was rematched with a different opponent, each switched their role (as sender or receiver), and played with a new value of c. Each session consisted of 4 rounds of 15 periods. The subjects were made aware of these matching procedures. Each subject experienced only one of two sequences of communication costs. One sequence was ordered 10-50-30-96 and the other was ordered 96-30-50-10. We ran two sessions which consisted of 8 subjects and two sessions of 16 subjects. Therefore, we have a total of 1,440 data points for both senders and receivers.

A few comments on our experimental design are in order. Since we expected over-communication, even though only the senders incurred the communication costs, we designed the experiment to reduce the social preferences of the sender toward the receiver. First, the payoffs of both sender and receiver were displayed to both players in each period. We hoped that if the sender was confronted with the information that the receiver was obtaining much higher payoffs than the sender, then this information would prompt the sender to act less generously toward the receiver. Second, we emphasized the anonymous matching whereby, after each round of 15 periods, the players would be rematched with a new partner. This was done in order to discourage implicit reciprocal play.

Additionally, many experimental communication papers rematch the subjects after each period. However, we decided not to rematch, as there is a reasonably difficult coordination problem which would be aggravated by rematching after every period. For these reasons, despite our fixed matching protocol, we also do not expect repeated game effects to be present. However, we expect the fixed matching to yield better coordination outcomes.

Finally, note that we constrain the receiver's payoffs to be nonnegative. In order to make the payoffs as transparent as possible, we specified that the receiver's payoffs would range from 0 to 100. Additionally, given our setup, we did not expect that the absolute value of the difference between the action and the state (the *error*) would be very large. As a result, we designed the payoffs so that the marginal penalty for small errors is larger than the marginal penalty for large errors. For instance, the difference in the receiver's payoffs for an error of 1 and an error of 1.5, is 31. However, the difference in the receiver's payoffs for an error of 2 and an error larger than 2, is 0. Normalizing the uncensored quadratic loss function so that the receiver's payoffs range from 0 to 100 would produce the opposite marginal incentives: the marginal penalty for small errors would be smaller than the marginal penalty for large errors.

In the end, it seems to be most reasonable to censor the receiver's payoffs at 0. Although this decision to censor the payoffs of the receivers would affect the babbling equilibrium, we did not expect that we would observe this behavior in the laboratory. We also note that, had we not censored their payoffs at 0, only a small fraction of outcomes in our experiment would have resulted in a negative payoff to the receiver. Only 14.75 % (59 of 384) of actions in periods 1–4 and 8.36 % (92 of 1,056) in periods 5–15 would have resulted in a negative receiver payoff, if we did not censor the receiver's payoffs at 0.

There are interesting questions related to the differences in the meaning of the messages within a match and whether these change across matches.[9] However, our experimental setup is not designed to investigate these issues. First, within each match, a particular state only occurs a few times. Further, each subject only plays the game with a particular communication cost during a single round. As a result, our experimental setup does not facilitate the investigation of these interesting questions.

5 Results

5.1 Preliminary analysis

Before we begin our main analysis, we investigate two potentially problematic issues in our data: whether the order of the communication costs affects behavior and the extent to which the subjects exhibit learning across periods.

First, we investigate whether the order of the sequences of communication costs affected the earnings of the sender. In order to accomplish this, we perform a regression with sender payoffs as the dependent variable. To account for the fact that not every match observes the same sequence of states, the independent variables include the cost of communication, a dummy variable indicating one of the two sequence orders, the communication cost-order interaction, and dummy variables accounting for the state.[10] We find that neither the order dummy ($p = 0.81$) nor the communication cost-order interaction ($p = 0.54$) are significant. Thus, we do not find evidence that the order of the communication costs is related to the sender's payoffs.

We also investigate whether there is learning across the 15 periods within each round. We perform a Chow test of a structural break with the sender's payoffs as the dependent variable and the period as the independent variable. We examine 4 potential candidates for a break point: between periods 3 and 4, between periods 4 and 5, between periods 5 and 6, and between periods 6 and 7. We present the summary of this analysis in Table 2.

According to the analysis summarized in Table 2, the only break point which is significant at 0.05 is between periods 4 and 5. Also providing evidence of the break point, we run a regression of the sender's payoffs across periods. For periods 1–15, the relationship between the sender's payoffs and the period is significant ($p = 0.01$).[11] However, for only periods 5–15, the relationship is not significant ($p = 0.7$). Therefore, for the bulk of the analysis, within each round we exclude from consideration the data obtained in periods 1–4.

Finally, we provide a summary of the raw data in Appendix 2. There we provide the distribution of the sender's messages for each state and each of the four possible communication costs. We also provide the distribution of receiver's actions for each of the messages and each of the four communication costs.

[9] For instance, Weber and Camerer (2003).

[10] These results are available from the corresponding author upon request.

[11] Here, as we do in the following sentence, we report the p value of the period coefficient in an OLS regression with the sender's payoffs as the dependent variable and the period as the independent variable.

Table 2 Chow test for a structural break of sender's payoffs across periods

Structural break between:	*F* statistic	*p* value
Periods 3 and 4	1.20	0.301
Periods 4 and 5	3.42	0.033
Periods 5 and 6	2.86	0.057
Periods 6 and 7	2.56	0.077

Results of a Chow test for a structural break in the regression with the sender's payoffs as the dependent variable and the period as the independent variable

5.2 Size of the language used

We ask whether the size of the language arises endogenously as a function of the cost of communication. In other words, are expensive signals conserved when communication is costly? The raw data provided in Appendix 2 suggest that expensive signals are conserved. To address whether this impression is correct, we run logistic regressions with two different measures of the size of the language. In the first specification, the dependent variable is a dummy indicating whether the message had one or two elements. We assign a value of 1 in the event that the message had either one or two elements, and 0 otherwise. We refer to this regression as "One or Two." In the second specification, the dependent variable is a dummy indicating whether the message had two elements. We assign a value of 1 to the variable if the message had 2 elements, and 0 otherwise. We refer to this regression as "Two." Since the communication costs could affect the size of the language in a nonlinear manner, we include dummy variables indicating the communication costs. In the regressions below, we include controls for the state, because it is not obvious, given a particular communication cost, that a message would be appropriate for all states. We accomplish this by including dummy variables indicating the state.

Since we have repeated observations of binary outcomes, we perform repeated measures logistic regressions. Due to the binary nature of the dependent variable, our repeated measures analysis assumes an exchangeable log odds ratio, clustered by subject. In other words, we assume a constant log odds ratio relating any two observations involving a particular subject. However, observations involving two different subjects are considered to be independent. The regressions are estimated using generalized estimating equations (GEE). Since GEE is not a likelihood-based method, Akaike's information criterion is not available; therefore, we provide the quasilikelihood information criterion (QIC).[12]

Due to the monotonic nature of the communication costs, we provide the estimates of the marginal effect of a larger communication cost. For instance, the term "Communication costs 30 versus 10" estimates the difference between the estimate at a communication cost of 30 minus the estimate at a communication cost of 10. Finally, we provide the Wald χ^2 statistic of the Wald test that the communication cost dummies are equal to zero. We summarize the analysis below in Table 3.

[12] For more on this, see Pan (2001).

Table 3 Repeated measures logistic regressions of the size of the language

	One or two		Two	
Communication costs 30 vs. 10	−0.385	−1.261**	−0.427	−0.531
	(0.2964)	(0.482)	(0.232)	(0.273)
Communication costs 50 vs. 30	−0.524	−0.798	−0.984**	−1.185**
	(0.2963)	(0.511)	(0.359)	(0.439)
Communication costs 96 vs. 50	−0.569**	−0.960***	−0.0453	−0.206
	(0.2085)	(0.227)	(0.299)	(0.322)
Wald χ^2 statistic: cost dummies equal 0	26.70***	41.32***	24.41***	26.38***
State dummies	No	Yes	No	Yes
QIC	1165.63	699.37	1294.88	1132.09

Results of repeated measures logistic regressions where *** indicates significance at $p < 0.001$, and ** indicates significance at $p < 0.01$. Each regression accounts for the repeated observations by assuming an exchangeable log odds ratio, clustered by subject. Each regression has 1,056 observations from 24 senders in 4 rounds of 11 periods. The Wald χ^2 statistic is from the Wald test that the three communication cost dummy variables are equal to zero. QIC refers to the quasilikelihood information criterion. We do not list the estimates of the intercepts or the three communication cost dummies

In each of our four specifications, we find evidence that the size of the language is affected by the communication costs. We find that the estimated marginal effect is negative and the Wald test that the three estimated communication cost dummies are equal to zero is significant at 0.001. We note that we observe this whether we include the state dummy variables or not. In the "One or Two" specification, we see that higher communication costs are associated with a lower likelihood of sending a message with one or two elements. We see the analogous result in the "Two" specification: there is a negative relationship between communication costs and the transmission of a message with two elements. In short, the results summarized in Table 3 suggest that the size of the language arises endogenously as a function of the cost of communication.

However, we note that not every marginal effect is significant. Due to our specification of employing communication cost dummy variables, we are able to account for the nonlinear changes in the size of the language as a function of the communication costs. We find that significantly fewer messages of either one or two elements are transmitted when the cost of communication is 96 rather than when it is 50. We also note that for specification involving the state dummy variables, there are significantly fewer messages of either one or two elements when the cost of communication is 30 rather than when it is 10. Finally, we note that significantly fewer messages with two elements are transmitted when the communication cost is 50 rather than when it is 30. This is notable because the equilibrium predictions for $c = 30$ and 50 are identical. Therefore, Table 3 provides evidence that the size of the language is decreasing in the communication costs, but these changes do not occur in a linear fashion.

5.3 Payoffs relative to equilibrium payoffs: overcommunication

We now turn our attention to the overall performance of the subjects, as measured by their payoffs. This allows us to investigate how the subjects are performing relative

Table 4 Equilibrium payoffs and actual payoffs

	Sender		Receiver	
	Equilibrium	Actual	Equilibrium	Actual
$c = 10$	85.71	67.13***	100.00	81.03***
$c = 30$	72.29	47.16***	89.43	84.09**
$c = 50$	60.86	29.60***	89.43	76.00***
$c = 96$	40.57	−6.14***	68.00	69.86

Results of paired t tests each with 263 degrees of freedom, where *** indicates significance of a two-sided test at $p < 0.001$ and ** indicates significance of a two-sided test at $p < 0.01$. Each cell is associated with 264 observations from 24 subjects across 11 periods

to the equilibrium predictions. We first note that the equilibrium predictions do not perform particularly well. We perform a test of the differences between the equilibrium payoffs and the actual payoffs within each communication cost treatment. Within each treatment, we find a significant difference between the sender's payoffs and the equilibrium prediction. In all but the highest cost treatment, there is a significant difference between the receiver's payoffs and the equilibrium prediction. This analysis is summarized in Table 4.

First, we note that when $c = 96$, the average payoffs of the senders are less than 0. In other words, the senders would improve their payoffs by sending the empty message given any state. Sending the empty message for every state would imply that the senders would obtain a payoff of at least 0. This seems to suggest that senders are overcommunicating in that they are incurring communication costs larger than that which is in their best interest.

Second, a glance at Table 4 seems to suggest that the difference between the sender's actual and equilibrium payoffs is increasing in the cost of communication. Further, the difference between the receiver's actual and equilibrium payoffs seem to be decreasing in the cost of communication. This is consistent with the contention that the senders are overcommunicating and the receivers are benefiting from this overcommunication. We perform the following analysis in order to more carefully test this speculation.

In the regressions below, we investigate overcommunication by performing linear regressions with the dependent variable of the actual payoffs minus the equilibrium payoffs. Recall that the receiver's payoffs correspond to the accuracy of the receiver's action and the sender's payoffs correspond to this accuracy minus the cost of the message sent. Therefore, the receiver's payoffs can be seen as a measure of the accuracy of the action, without consideration of the communication costs, and the sender's payoffs can be seen as a measure of the accuracy of the action, when considering the communication costs. If we find that the difference between the receiver's actual and equilibrium payoffs are decreasing with communication costs and if we find that the difference between the sender's actual and equilibrium payoffs are increasing with communication costs, then this would provide evidence of overcommunication. Note that in our analysis of overcommunication, we do not utilize the actions of the players because, unlike other experimental communication settings, our message space is not ordered in the same manner as our state and action spaces.

Table 5 Sender's payoffs minus equilibrium payoffs

	(S1)	(S2)	(S3)	(S4)
Communication costs 30 vs. 10	−6.554	−6.554	−7.139	−7.306
	(4.484)	(4.109)	(4.076)	(3.627)
Communication costs 50 vs. 30	−6.123	−6.123	−6.442	−6.427
	(4.484)	(7.239)	(4.080)	(7.169)
Communication costs 96 vs. 50	−15.456***	−15.456***	−14.141***	−14.337***
	(4.484)	(4.109)	(4.079)	(3.629)
Repeated measures	No	Yes	No	Yes
State dummies	No	No	Yes	Yes
Wald χ^2 statistic: cost dummies equal 0	43.19***	23.37***	49.84***	26.25***
-2 Log likelihood	11,322.2	11,216.6	11,117.9	10,963.2

Results of regressions where the dependent variable is the sender's actual payoffs minus sender's equilibrium payoffs, where *** indicates significance at $p < 0.001$. The repeated measures specifications account for the repeated observations by assuming an exchangeable covariance matrix, clustered by subject. Each regression has 1,056 observations from 24 senders in 4 rounds of 11 periods. The Wald χ^2 statistic is from the Wald test that the three communication cost dummy variables are equal to zero. We do not list the estimates of the intercepts or the three communication cost dummies

In regressions (S1–S4) of Table 5, the dependent variable is the sender's actual payoffs minus the sender's equilibrium payoffs. In regressions (R1–R4) of Table 6, the dependent variable is the receiver's actual payoffs minus the receiver's equilibrium payoffs. In regressions (S1) and (R1) we do not employ additional controls. In regressions (S2) and (R2) we account for the repeated nature of the observations. In regressions (S3) and (R3), we account for the information known by the subject at the time of the decision. In the case of the receiver (R3), this is the observed message, and in the case of the sender (S3), this is the observed state. Finally, in regressions (S4) and (R4) we account for the repeated nature of the observations and the information known by the subject at the time of the decision.

We account for the repeated nature of the observations by conducting a repeated measures analysis. Due to the continuous nature of the dependent variable, we assume an exchangeable covariance matrix, clustered by subject. In other words, we assume a single correlation between any two observations involving a particular subject. However, we assume that observations involving two different subjects are statistically independent. The regressions are estimated using maximum likelihood. We also report the result of a Wald test that the communication cost dummy variables are equal to 0. We summarize the analysis in Tables 5 and 6.

In Table 5, every specification has a negative coefficient estimate for the marginal effects of the communication costs and the Wald test that the three estimated communication cost dummy variables are equal to zero is significant at 0.001. This suggests that, as communication costs increase, the senders do worse relative to the equilibrium predictions. Whereas the increase in communication costs affect receiver payoffs in a monotonic fashion, the effect is not significant across every pair of adjacent communication costs. For all four specifications, the only significant difference between adjacent communication costs appears between the costs of 96 and 50. This

Table 6 Receiver's payoffs minus equilibrium payoffs

	(R1)	(R2)	(R3)	(R4)
Communication costs 30 vs. 10	13.627***	13.627***	12.688***	12.605***
	(2.977)	(2.765)	(2.943)	(2.709)
Communication costs 50 vs. 30	−8.083**	−8.083	−5.498	−5.560
	(2.977)	(4.589)	(2.951)	(4.571)
Communication costs 96 vs. 50	15.282***	15.282***	13.828***	13.772***
	(2.977)	(2.765)	(2.931)	(2.713)
Repeated measures	No	Yes	No	Yes
Message dummies	No	No	Yes	Yes
Wald χ^2 statistic: cost dummies equal 0	56.46***	57.20***	52.32***	49.96***
-2 Log likelihood	10,457.2	10,373.2	10,403.9	10,313.3

Results of regressions where the dependent variable is the receiver's actual payoffs minus receiver's equilibrium payoffs, where *** indicates significance at $p < 0.001$, and ** indicates significance at $p < 0.01$. The repeated measures specifications account for the repeated observations by assuming an exchangeable covariance matrix, clustered by subject. Each regression has 1,056 observations from 24 receivers in 4 rounds of 11 periods. The Wald χ^2 statistic is from the Wald test that the three communication cost dummy variables are equal to zero. We do not list the estimates of the intercepts or the three communication cost dummies

implies that the senders are performing worse relative to the equilibrium predictions for $c = 96$.

Whereas the difference between sender's actual payoffs and equilibrium payoffs are decreasing in communication costs, we note the opposite effect for the receivers, although the effect does not appear to be monotonic. Table 6 shows that the estimated marginal effects, which are significant across each specification, are also positive. These positive and significant marginal effects appear between communication costs of 30 and 10, and also between 96 and 50. However, we note the negative estimates for marginal effects between communication costs of 50 and 30, although these negative estimates are largely not significant. We also find that the Wald test that the three communication cost dummy variables are equal to zero, is significant at 0.001. Therefore, as the communication costs increase, the receivers do better relative to the equilibrium predictions.

In our view, the results of Table 5 are not consistent with an equilibrium selection explanation. Most notably, if $c > 10$ then there does not exist an equilibrium where a message with two elements is sent. However, we often see senders transmitting such messages despite their high cost.[13] It is this overcommunication which appears to be driving the results above.

The results summarized in Tables 5 and 6 provide evidence that the senders are overcommunicating. In other words, the senders are not sufficiently conserving expensive messages and as a result, there is a negative relationship between communication costs and sender payoffs relative to equilibrium payoffs. However, the

[13] See Appendix 2.

receivers appear to be benefiting from the excessive communication of the senders, as their actual payoffs minus equilibrium payoffs are increasing in the communication costs.

5.4 Coordination imperfections and labels

Finally, we investigate the relationship between the coordination outcomes and the experimental labels. Specifically, we investigate whether there is evidence that the labels of the states are related to the coordination outcomes.

There could exist differences in the success of coordinating if there is an obvious message which is associated with a particular state. For instance, perhaps the state 0 would closely be associated with the empty message. If this is the case then we would expect that, when the state is 0, subjects are likely to coordinate. However, suppose that state 1 could possibly be associated with the messages H, LH, or HL. If this is the case then it could be difficult to coordinate when the state is 1. This discussion illustrates how it is possible that the label of the state might affect the coordination outcomes.

In the analysis which follows, we use a coordination dummy variable as the dependent variable.[14] This coordination dummy attains a value of 1 if the sender and receiver coordinated according to a specified minimum precision. For instance, we first investigate the coordination outcomes when $c = 10$. In this case, we would expect that subjects would fully coordinate. Therefore, we consider the coordination thresholds of $\mathrm{U}^{\mathrm{R}} = 100$ (perfect coordination) and $\mathrm{U}^{\mathrm{R}} \geq 94$ (where the error, or absolute value of the difference between state and action, is less than or equal to 0.5).

We note that there does not exist a natural ordering of the states in terms of the difficulty of coordination. As a result, we conduct the analysis by modeling the states with dummy variables. Further, since we did not have an a priori reason to expect that a state would be more difficult on which to coordinate than another, we do not provide the estimates of the state dummy variables. However, we report the result of a Wald test that each of the six state dummy variables are equal to 0.

In the analysis below, we include the state repeated dummy variable, which takes a value of 1 if the state had been repeated within the match, and a 0 otherwise. We also include the interaction between the state repeated variable and the state dummy variables. Additionally, we include the number of elements variable, which takes a value of 0 for the empty message, a value of 1 for the messages "High" or "Low" and, a 2 otherwise. Note that for the remaining analysis, we include the data from all periods because we are interested in all data in which subjects attained a minimum coordination. As in the analysis summarized in Table 3, we employ a repeated measures logistic regression with an exchangeable log odds ratio, clustered by subject. Additionally, the regressions are estimated using GEE, and, therefore, the QIC is reported. The analysis for $c = 10$ is summarized in Tables 7 and 8.

The results of Tables 7 and 8 provide evidence that, although full communication is predicted on all states, the ability of subjects to coordinate is affected by the label of

[14] We provide a summary of these coordination outcomes in Appendix 3.

Table 7 Coordination of $U^R = 100$ where $c = 10$

	(1)	(2)	(3)	(4)
Wald χ^2 statistic: state dummies equal 0	31.23***	35.48***	36.62***	38.36***
State repeated	No	Yes	Yes	Yes
State dummies-state repeated interaction	No	No	Yes	Yes
Number of elements	No	No	No	Yes
QIC	465.78	457.56	465.81	454.07

Results of repeated measures logistic regressions where *** indicates significance at $p < 0.001$. Each regression accounts for the repeated observations by assuming an exchangeable log odds ratio, clustered by subject. The dependent variable is a dummy indicating that the subjects coordinated on an outcome of $U^R = 100$ for $c = 10$. Each regression has 360 observations, from 24 subjects in 15 periods. The Wald χ^2 statistic is from the Wald test that the six state dummy variables are equal to zero. QIC refers to the quasilikelihood information criterion. We do not list the estimates of the intercepts or the six state dummy variables

Table 8 Coordination of $U^R \geq 94$ where $c = 10$

	(1)	(2)	(3)	(4)
Wald χ^2 statistic: state dummies equal 0	30.62***	34.94***	36.49***	36.62***
State repeated	No	Yes	Yes	Yes
State dummies-state repeated interaction	No	No	Yes	Yes
Number of elements	No	No	No	Yes
QIC	431.66	424.40	429.13	421.13

Results of repeated measures logistic regressions where *** indicates significance at $p < 0.001$. Each regression accounts for the repeated observations by assuming an exchangeable log odds ratio, clustered by subject. The dependent variable is a dummy indicating whether the subjects coordinated on an outcome of $U^R \geq 94$ for $c = 10$. Each regression has 360 observations, from 24 subjects in 15 periods. The Wald χ^2 statistic is from the Wald test that the six state dummy variables are equal to zero. QIC refers to the quasilikelihood information criterion. We do not list the estimates of the intercepts or the six state dummy variables

state. We note that in all four specifications, the Wald test that all of the state dummy variables are equal to zero is significant at 0.001. That is, despite that we expect to observe coordination on each state, there are significant differences in coordination outcomes based on the label of the state.

The analysis summarized in Tables 7 and 8 provide evidence that subjects are affected the labels of the states for the case of $c = 10$. We now perform a similar analysis for the case of $c = 30$ and 50. In each state, the equilibrium behavior would imply that the receiver would obtain a payoff of at least $U^R = 75$. Therefore, we consider a coordination threshold of $U^R \geq 75$ (where the error, or the absolute value of the difference between state and action, is less than or equal to 1).

As in the analysis summarized in Tables 7 and 8, we again observe that the state is related to the ability to coordinate. In specifications (1), (3), and (4), we find that the Wald test that the state dummy variables are equal to zero is significant at 0.05. Therefore, although the evidence is less compelling than that summarized in Tables 7

Table 9 Coordination of $U^R \geq 75$ where $c = 30$ and 50

	(1)	(2)	(3)	(4)
Wald χ^2 statistic: state dummies equal 0	12.88*	12.37	15.07*	16.60*
State repeated	No	Yes	Yes	Yes
State dummies-state repeated interaction	No	No	Yes	Yes
Number of elements	No	No	No	Yes
QIC	637.18	625.02	615.46	606.58

Results of repeated measures logistic regressions where * indicates significance at $p < 0.05$. Each regression accounts for the repeated observations by assuming an exchangeable log odds ratio, clustered by subject. The dependent variable is a dummy indicating that the subjects coordinated on an outcome of $U^R \geq 75$ for $c = 30$ and 50. Each regression has 720 observations, from 24 subjects across 2 rounds of 15 periods. The Wald χ^2 statistic is from the Wald test that the six state dummy variables are equal to zero. QIC refers to the quasilikelihood information criterion. We do not list the estimates of the intercepts or the six state dummy variables

and 8, we find that, when $c = 30$ and 50, the label of the state affects the coordination outcomes.

The analysis summarized in Tables 7, 8, and 9 provides evidence that the relationship between the experimental labels and the success at coordination is robust across different experimental treatments. The differences in the coordination outcomes based on the label of the state appears in two different types of equilibria and also appears to be robust to different modeling specifications.

6 Conclusions

We report on an experiment where the messages available to the sender imperfectly describe the state of the world. The sender can improve communication, at a cost, by increasing the complexity or elaborateness of the message. The incentives of the players are aligned in that both sender and receiver are paid an amount which is increasing in the accuracy of the receiver's action. Although the equilibrium predictions of Hertel and Smith (2012) do not perform particularly well, our experimental results corroborate some of the qualitative predictions. In particular, we find that the size of the language emerges endogenously as a function of the cost of communication.

Further, the differences between our observations and the equilibrium predictions are consistent with other experimental communication papers: the senders overcommunicate. As a result of this overcommunication, the sender's payoffs, relative to equilibrium payoffs, are decreasing in the cost of communication. The receivers benefit from this overcommunication, as we observe that their payoffs, relative to equilibrium payoffs, are increasing in the cost of communication.

We also observe coordination imperfections on the basis of the labels of the states. Subjects in the treatment where full communication is predicted are affected by the state on which coordination is sought. We also find some evidence of a similar result for the case of intermediate communication costs. To summarize, we find evidence that the experimental labels of states affect the ability of subjects to coordinate.

Although to our knowledge, we are the first paper to examine the experimental implications of costly and discrete communication, our results could also be relevant for understanding cheap talk experiments. Sobel (2012) notes that models of costly communication with aligned preferences can have parallel results to models of costless communication where preferences are not aligned (or cheap talk models). As a basic prediction of the models, Sobel notes that increases in communication costs will decrease the quality of communication in a fashion similar to that in response to increases in the difference of the preferences of sender and receiver in cheap talk models. This is precisely what is found in the laboratory. Previous studies of experimental cheap talk have found that diverging preferences will lead to lower quality communication. We find the analogous effect for increases in communication costs.

Although there are significant differences between the equilibrium predictions and our observations, we are encouraged by our results. As mentioned, these differences are largely due to the overcommunication of the senders. Since observing overcommunication in experimental settings is common, we do not find this divergence to be problematic. Further, the main insights from Hertel and Smith (2012) are observed in our experimental setting: the size of the language is determined by the cost of communication. As a result, it would seem to be profitable to think more about the theoretical and experimental issues related to costly and discrete communication.

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Appendix 1

Although the payoffs were specified by Eqs. (1) and (2), the subjects were also presented with the following table.

		Action												
State		−3	−2.5	−2	−1.5	−1	−0.5	0	0.5	1	1.5	2	2.5	3
	−3	100	94	74	44	0	0	0	0	0	0	0	0	0
	−2	75	94	100	94	74	44	0	0	0	0	0	0	0
	−1	0	44	75	94	100	94	74	44	0	0	0	0	0
	0	0	0	0	44	75	94	100	94	74	44	0	0	0
	1	0	0	0	0	0	44	75	94	100	94	74	44	0
	2	0	0	0	0	0	0	0	44	75	94	100	94	75
	3	0	0	0	0	0	0	0	0	0	44	75	94	100

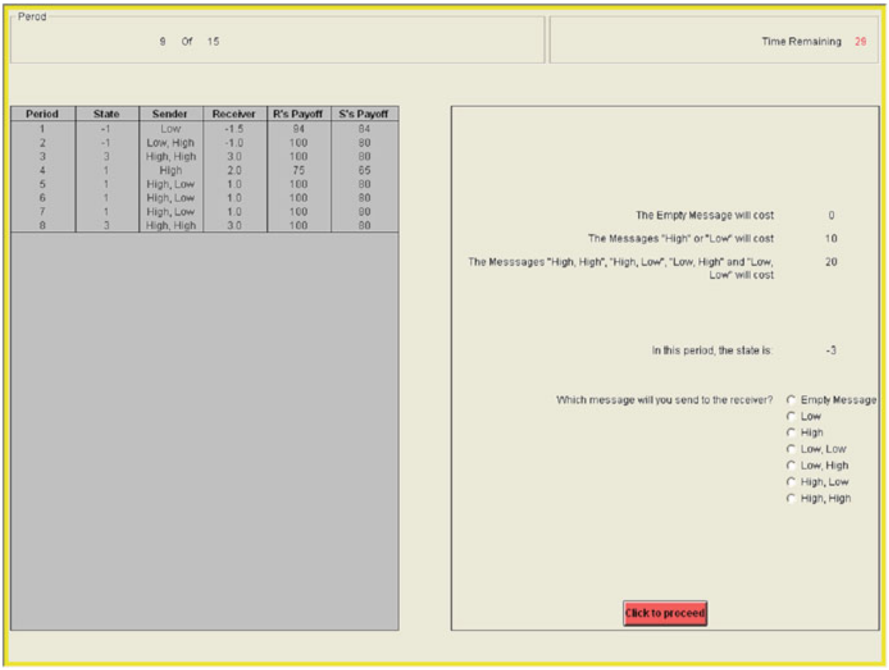

Fig. 1 Sender's screen

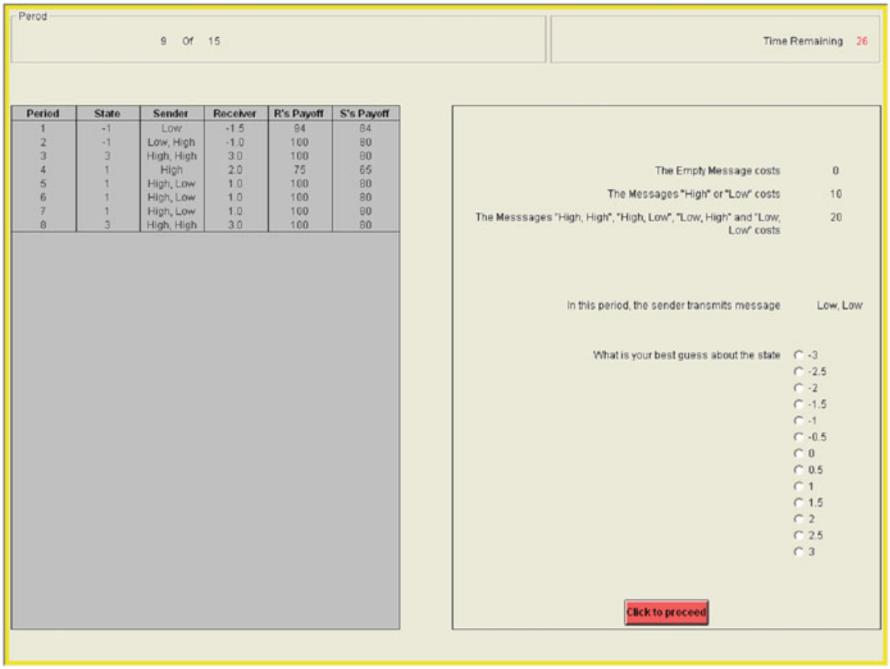

Fig. 2 Receiver's screen

Appendix 2

Messages sent by the senders given the state observed and the communication costs

$c = 10$		Messages						
		E	H	L	L L	L H	H L	H H
States	−3	1	1	1	31	3	2	0
	−2	1	1	21	4	19	1	0
	−1	0	0	16	0	14	3	0
	0	34	0	1	0	1	0	0
	1	0	4	14	0	7	18	0
	2	1	19	0	1	2	9	3
	3	0	9	0	0	0	0	22

$c = 30$		Messages						
		E	H	L	L L	L H	H L	H H
States	−3	0	1	5	26	0	1	0
	−2	0	0	27	5	5	3	0
	−1	6	1	10	0	11	2	0
	0	27	4	1	0	2	2	0
	1	16	7	4	0	6	10	1
	2	2	29	0	0	1	11	0
	3	0	12	0	0	0	0	26

$c = 50$		Messages						
		E	H	L	L L	L H	H L	H H
States	−3	2	0	23	14	0	0	2
	−2	1	1	32	0	3	0	0
	−1	16	0	16	1	7	0	0
	0	36	0	2	1	0	0	0
	1	18	4	8	1	1	4	0
	2	3	29	0	0	2	9	0
	3	0	15	1	0	0	0	12

$c = 96$		Messages						
		E	H	L	L L	L H	H L	H H
States	−3	4	2	20	18	1	1	0
	−2	8	0	12	1	2	2	0
	−1	20	0	13	2	5	0	0
	0	35	0	0	0	0	0	0
	1	25	4	3	0	0	5	0
	2	13	27	0	1	0	1	0
	3	5	18	0	0	0	0	16

Action selected by the receivers given the message and the communication costs

$c = 10$		Action												
		−3	−2.5	−2	−1.5	−1	−0.5	0	0.5	1	1.5	2	2.5	3
Messages	E	0	0	0	0	0	0	36	1	0	0	0	0	0
	H	0	0	0	0	0	0	0	0	1	2	22	7	2
	L	3	2	19	11	6	3	1	1	6	1	0	0	0
	L L	30	2	0	0	2	0	0	1	1	0	0	0	0
	L H	3	3	14	4	10	1	1	0	6	3	0	1	0
	H L	0	0	3	0	2	0	0	0	15	7	6	0	0
	H H	0	0	0	0	0	0	0	0	0	0	0	4	21

$c = 30$		Action												
		−3	−2.5	−2	−1.5	−1	−0.5	0	0.5	1	1.5	2	2.5	3
Messages	E	0	0	0	0	0	2	49	0	0	0	0	0	0
	H	0	0	0	0	0	0	1	0	3	8	22	17	3
	L	2	6	20	9	6	0	1	0	2	0	1	0	0
	L L	28	0	1	0	0	0	1	0	1	0	0	0	0
	L H	0	0	4	1	11	1	1	0	5	0	2	0	0
	H L	0	0	4	1	2	0	2	0	9	4	7	0	0
	H H	0	0	0	0	0	0	0	0	0	1	0	0	26

$c = 50$		Action												
		−3	−2.5	−2	−1.5	−1	−0.5	0	0.5	1	1.5	2	2.5	3
Messages	E	0	1	2	0	1	2	59	5	4	1	1	0	0
	H	1	0	1	0	0	0	0	0	6	3	15	15	8
	L	6	12	16	18	20	2	2	0	3	3	0	0	0
	L L	14	1	0	0	2	0	0	0	0	0	0	0	0
	L H	0	0	2	0	6	0	0	2	2	1	0	0	0
	H L	0	0	0	0	0	1	0	0	2	2	7	1	0
	H H	1	1	0	0	0	0	0	0	0	0	0	0	12

$c = 96$		Action												
		−3	−2.5	−2	−1.5	−1	−0.5	0	0.5	1	1.5	2	2.5	3
Messages	E	3	4	1	7	4	4	58	8	8	6	4	1	2
	H	0	1	1	0	0	0	1	1	3	2	17	9	16
	L	11	6	12	3	8	1	0	0	5	0	0	2	0
	L L	18	1	0	0	1	0	0	1	0	0	0	0	1
	L H	0	0	1	0	6	0	0	1	0	0	0	0	0
	H L	0	0	0	0	2	0	0	0	5	1	1	0	0
	H H	0	0	0	0	0	0	0	0	0	0	0	1	15

Appendix 3

A summary of the coordination outcomes. Note that periods 1–15 are included in the tables below.

$c = 10$		Messages							$U^R = 100$	Total
		E	H	L	L L	L H	H L	H H		
States	−3	0	0	0	31	2	0	0	33	50
	−2	0	0	13	0	12	1	0	26	59
	−1	0	0	3	0	10	3	0	16	44
	0	44	0	0	0	0	0	0	44	51
	1	0	0	4	0	1	18	0	23	66
	2	0	16	0	0	0	3	0	19	45
	3	0	3	0	0	0	0	30	33	45

$c = 10$		Messages							$U^R \geq 94$	Total
		E	H	L	L L	L H	H L	H H		
States	−3	0	0	0	33	2	0	0	35	50
	−2	0	0	22	1	15	1	0	39	59
	−1	0	0	6	0	13	3	0	22	44
	0	45	0	0	0	0	0	0	45	51
	1	0	2	4	0	3	20	0	29	66
	2	0	22	0	0	1	8	2	33	45
	3	0	7	0	0	0	0	32	39	45

$c = 30$		Messages							$U^R \geq 75$	Total
		E	H	L	L L	L H	H L	H H		
States	−3	0	0	7	33	0	1	0	41	49
	−2	0	0	33	5	6	3	0	47	56
	−1	8	1	10	0	13	2	0	34	40
	0	39	0	0	0	1	1	0	41	48
	1	16	8	2	0	5	11	0	42	53
	2	0	37	0	0	3	13	0	53	57
	3	0	15	0	0	0	0	37	52	57

$c = 50$		Messages							$U^R \geq 75$	Total
		E	H	L	L L	L H	H L	H H		
States	−3	0	0	20	16	0	0	2	38	50
	−2	0	1	40	0	1	0	0	45	56
	−1	17	0	19	0	7	0	0	43	56
	0	51	0	3	0	0	0	0	54	60
	1	22	2	3	0	0	4	0	31	55
	2	1	39	0	0	3	10	0	53	58
	3	0	16	0	0	0	0	15	31	36

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