Online Appendix
(For Online Publication Only)

Is No News (Perceived As) Bad News?
An Experimental Investigation of Information Disclosure

Ginger Zhe Jin
University of Maryland and NBER

Michael Luca
Harvard Business School

Daniel Martin
Northwestern Kellogg School of Management

1 Experimental Instructions

Welcome

You are about to participate in an experiment on decision-making, and you will be paid for your participation in cash, privately at the end of the experiment. What you earn depends partly on your decisions, partly on the decisions of others, and partly on chance.

Please silence and put away your cellular phones now.

The entire session will take place through your computer terminal. Please do not talk or in any way communicate with other participants during the session.

We will start with a brief instruction period. During the instruction period you will be given a description of the main features of the experiment and will be shown how to use the computers. If you have any questions during this period, raise your hand and your question will be answered so everyone can hear.

Instructions

The experiment you are participating in consists of 45 rounds. At the end of the final round, you will complete an additional task, be asked to fill out a questionnaire, and then will be paid the total amount you have accumulated during the course of the session (in addition to the $5 show up fee). Everybody will be paid in private. You are under no obligation to tell others how much you earned.

The currency used during these 45 rounds is what we call "Experimental Currency Units" (ECU). For your final payment, your earnings during these 45 rounds will be converted into US dollars at the ratio of 200:1 (200 ECU=$1). They will then be rounded up to the nearest (non-negative) dollar amount.

In the first round, you will be matched with one other person, and you are equally likely to be matched with any other person in the room. You will not know whom you are matched with, nor will the person who is matched with you. One of you will be assigned to be S Player and the other to be the R Player for that round. You are equally likely to be assigned to either role. In the second round, you will once again be randomly matched with one other person (most likely with a different person than in the first round) and randomly assigned a role, and this will be repeated until 45 rounds are complete.

In each round and for every pair, the computer program will generate a secret number that is randomly drawn from the set {1,2,3,4,5}. The computer will then send the secret number to the S Player. After receiving this number, the S Player will choose whether or not to report the secret number to the R Player. If the S Player chooses to report the number, the R Player will receive this message from the S Player: "The number I received is" followed by the actual secret number. Otherwise, the R Player will receive no message.

After seeing the message or not, the R Player will guess the value of the secret number. The earnings of both players depend on the value of the secret number and the R Player's guess.

The specific earnings are shown in the table below, which is displayed again before the S Player and R Player make their choices. In each cell of the table, the payoff for the S Player is on the left, and the payoff for the R Player is on the right. As you can see from the table, the S Player earns more when the R Player makes a higher guess, and the R Player earns more when their guess is closer to the secret number.

PAYOFFS: S , R	R's guess: 1	R's guess: 1.5	R's guess: 2	R's guess: 2.5	R's guess: 3	R's guess: 3.5	R's guess: 4	R's guess: 4.5	R's guess: 5
Secret number: 1	-29 , 110	-6 , 102	17 , 90	38 , 75	57 , 57	75 , 38	90 , 17	102 , -6	110 , -29
Secret number: 2	-29 , 90	-6 , 102	17 , 110	38 , 102	57 , 90	75 , 75	90 , 57	102 , 38	110 , 17
Secret number: 3	-29 , 57	-6 , 75	17 , 90	38 , 102	57 , 110	75 , 102	90 , 90	102 , 75	110 , 57
Secret number: 4	-29 , 17	-6 , 38	17 , 57	38 , 75	57 , 90	75 , 102	90 , 110	102 , 102	110 , 90
Secret number: 5	-29 , -29	-6 , -6	17 , 17	38 , 38	57 , 57	75 , 75	90 , 90	102 , 102	110 , 110

2 Screenshots

In this section of the appendix, we present representative screen captures from the experiment.

Sender

You are the S player.

The secret number is 5.

Report number

Skip

PAYOFFS: S , R	R's guess: 1	R's guess: 1.5	R's guess: 2	R's guess: 2.5	R's guess: 3	R's guess: 3.5	R's guess: 4	R's guess: 4.5	R's guess: 5
Secret number: 1	-29 , 110	-6 , 102	17 , 90	38 , 75	57 , 57	75 , 38	90 , 17	102 , -6	110 , -29
Secret number: 2	-29 , 90	-6 , 102	17 , 110	38 , 102	57 , 90	75 , 75	90 , 57	102 , 38	110 , 17
Secret number: 3	-29 , 57	-6 , 75	17 , 90	38 , 102	57 , 110	75 , 102	90 , 90	102 , 75	110 , 57
Secret number: 4	-29 , 17	-6 , 38	17 , 57	38 , 75	57 , 90	75 , 102	90 , 110	102 , 102	110 , 90
Secret number: 5	-29 , -29	-6 , -6	17 , 17	38 , 38	57 , 57	75 , 75	90 , 90	102 , 102	110 , 110

Receiver (Secret Number Reported)

You are the R player .

Message from S player (if any):

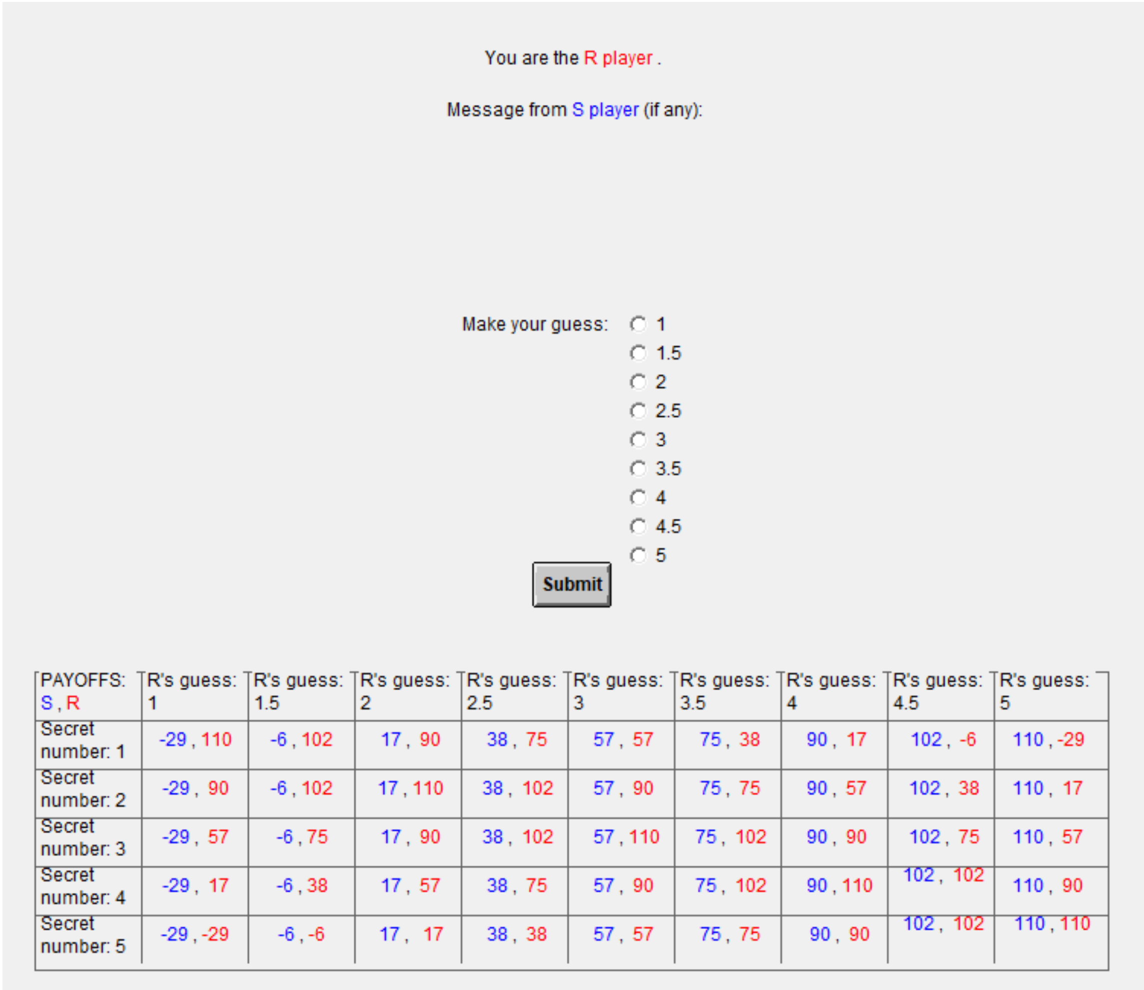
"The number I received is 5."

Make your guess:
- 1
- 1.5
- 2
- 2.5
- 3
- 3.5
- 4
- 4.5
- 5

Submit

PAYOFFS: S , R	R's guess: 1	R's guess: 1.5	R's guess: 2	R's guess: 2.5	R's guess: 3	R's guess: 3.5	R's guess: 4	R's guess: 4.5	R's guess: 5
Secret number: 1	-29 , 110	-6 , 102	17 , 90	38 , 75	57 , 57	75 , 38	90 , 17	102 , -6	110 , -29
Secret number: 2	-29 , 90	-6 , 102	17 , 110	38 , 102	57 , 90	75 , 75	90 , 57	102 , 38	110 , 17
Secret number: 3	-29 , 57	-6 , 75	17 , 90	38 , 102	57 , 110	75 , 102	90 , 90	102 , 75	110 , 57
Secret number: 4	-29 , 17	-6 , 38	17 , 57	38 , 75	57 , 90	75 , 102	90 , 110	102 , 102	110 , 90
Secret number: 5	-29 , -29	-6 , -6	17 , 17	38 , 38	57 , 57	75 , 75	90 , 90	102 , 102	110 , 110

Receiver (Secret Number Not Reported)



3 NYU Robustness Sessions

We ran additional sessions of the "no feedback & random role" treatment with five secret numbers (as in the main sessions) in the Center for Experimental Social Science (CESS) laboratory at New York University (NYU). These sessions were excluded from our main sessions because beliefs were not elicited from subjects in these sessions. Instead, we use these sessions to perform additional robustness checks for our results that do not require data on beliefs. We also use them to examine behavior in other additional tasks.

3.1 Experimental Design: Other Additional Tasks

In addition to the "aggregate feedback" additional task, we ran five other additional tasks at NYU that do not appear in our main sessions. In the first, which we call the "risk" task, subjects completed the well-known measure of risk aversion introduced by

Holt and Laury (2002). For this measure, subjects make 10 choices between a safer lottery (payments of \$2.00 or \$1.60) and riskier lottery (payments of \$3.85 or \$0.10) in which the probability of the high payment was the same within each choice, but varied across choices. A risk-neutral decision maker would choose the lottery with a 40% chance of \$2 over the lottery with a 40% chance of \$3.85, but the lottery with a 50% chance of \$3.85 over the lottery with a 50% chance of \$2. The switching point in this "multiple price list" can be viewed as a reflection of the risk preferences of each subject. This task was incentivized by randomly selecting one of their 10 choices, realizing the chosen lottery, and adding any earnings to the show-up fee and earnings from the first 45 rounds.

The aim of this task was to see whether subject choices were related to the risk preferences of subjects. Risk preferences can impact receiver guesses when there is uncertainty about the underlying state, and risk aversion can push guesses of non-reported secret numbers higher when the distribution of non-reported secret numbers is skewed towards lower numbers because higher guesses produce lower variation in payments. For instance, given the overall reporting rates for the "fixed role & no feedback" treatment reported in Table 2, the unconstrained optimal guess for a risk neutral agent would be 1.6156, and for a risk averse agent with the preferences U(x)=x^.75, the optimal guess would be 1.6725. Note that the predicted difference is small, even for substantial changes in risk preferences, so we might not expect to see a strong relationship between receiver guesses and risk preferences.

We call the second additional task the "other" task. In this task, subjects played once more in the role of sender and once more in the role of receiver, but in both cases, they played against a computer instead of a human (and were told this was the case). This computer played a strategy designed to mimic the past decision of another player. This type of task is designed to keep the strategic decisions the same as in previous choices, but to remove the payoff implications for others. By comparing these choices with previous choices, we can determine whether sender and receiver choices were impacted by social concerns related to the payoffs of the subject they were paired with.[1] Niederle and Vesterlund (2007) use a similar approach to separate preferences for competition from social preferences. Note that this task is identical to the "high incentives" task, but with a normal payoff rate. As in the "high incentives" task, guesses in this task are potentially impacted both by changes in the payoff implications for senders and by the fact that behavior may be changing over rounds because of learning.[2]

[1] The various ways in which social considerations could potentially impact receiver guesses in our game is discussed in this appendix.
[2] The latter produces several possible confounds. For example, subjects may have learned to play differently, or subjects may have changed their beliefs about what other subjects have learned. We thank an anonymous referee for pointing this out.

In the third task, which we call the "self" task, subjects played once more in the role of sender and once more in the role of receiver, and in both cases, they also played against a computer instead of a human. However, this time the computer played a strategy designed to mimic the past decisions of that same subject. This type of task is designed to assess whether subjects can best respond to accurate beliefs, under the assumption that they form accurate beliefs about their own strategies. A similar approach was used by Ivanov, Levin, and Niederle (2010) in examining the role of beliefs in the Winner's Curse. However, guesses in this task are also potentially impacted by the fact that behavior may be changing over rounds.

In the fourth task, which we call the "computer" task, subjects played 5 additional rounds in the role of receiver against a computer sender. In this task, subjects were told that the S player (computer) would report the secret number if that would "maximize their earnings given the guesses of all other participants (besides yourself) in the proceeding round." In practice, this meant that the computer reported the secret number if it was above the average guess for all other subjects in the previous round who did not receive a report. The payoffs from this task were added to the ECU earned in the first 45 rounds. The aim of this task was to assess whether any failures of unraveling in the first 45 rounds were due solely to the fact that receivers believe senders were potentially non-optimizing or poorly informed humans, which may be a good assumption for small firms, but not necessarily large firms.

In fifth task, which we call the "average reports" task, subjects were shown the average *reported* secret number from all subjects in that session from the first 45 rounds and then completed the same steps as in the "aggregate feedback" task. Because the number of rounds in which the secret number was reported was not provided, there was not enough information for subjects to fully pin down the average non-reported secret number. For instance, the average reported secret number would be 4 if the secret number was reported in just one round where the secret number was 4 (leaving the average non-reported secret number near 3), and the average reported secret number would also be 4 if all senders with secret numbers of 3, 4, and 5 reported (leaving the average non-reported secret number near 1.5). However, by placing additional assumptions on the actions of senders, more information can be gleaned from the average reported secret number. For instance, by assuming monotonic reporting rules and the same cut-off for all senders, the average non-reported secret number can fully pinned down from the average reported secret number.

3.2 Results: Robustness

In our sessions at NYU, we also used a show-up fee of $5, and on average subjects earned $25.25. Table A1 shows the summary statistics for the NYU sessions. All 212 subjects at NYU were assigned to the "no feedback & random role" treatment. NYU subjects were more likely to self-report as undergraduates, female, and non-native English speakers than our HBS subjects.

Table A1. Summary statics. Observation is per subject. Value is missing if demographic information not provided by the subject.

Panel A: Main sessions.			
VARIABLES	N	mean	sd
Number of subjects in the session	324	17.16	7.789
Feedback provided (dummy)	324	0.278	0.449
Random role (dummy)	324	0.648	0.478
Undergraduate (dummy)	324	0.713	0.453
Male (dummy)	324	0.494	0.501
Native English speaker (dummy)	321	0.850	0.357
Friend in the session (dummy)	324	0.145	0.353

Panel B: NYU sessions.		
VARIABLES	N	mean
Number of subjects in the session	212	13.40
Feedback provided (dummy)	212	0
Random role (dummy)	212	1
Undergraduate (dummy)	212	0.858
Male (dummy)	212	0.368
Native English speaker (dummy)	212	0.698
Friend in the session (dummy)	212	0.0943

Table A2 compares the actions of senders and receivers across schools. At NYU, there is less disclosure of less favorable draws and more disclosure of more favorable draws, which produces a lower average secret number when senders do not report. Also, at NYU receivers are more pessimistic about non-reported secret numbers. However, the extent to which receivers overestimate non-reported secret numbers is very similar between schools (0.481 and 0.448) and is not statistically significant at the 10% level using a 2-sided t-test.

Table A2. Summary of player actions for "no feedback & random role" sessions at HBS and NYU. Note: *** p<0.01; ** p<0.05; * p<0.1.

	HBS		NYU		
VARIABLES	N	mean	N	mean	p-value (=)

Report (secret number=1)	568	0.107	929	0.0614	0.0013***
Report (secret number=2)	552	0.426	986	0.398	0.2813
Report (secret number=3)	507	0.779	973	0.860	0.0001***
Report (secret number=4)	540	0.926	960	0.977	0.0000***
Report (secret number=5)	533	0.929	922	0.972	0.0001***
Secret number (no report)	1,014	1.802	1,650	1.628	0.0000***
Guess (report=1)	61	1.533	57	1.219	0.0551*
Guess (report=2)	235	2.243	390	2.097	0.0005***
Guess (report=3)	395	3.061	836	3.039	0.2622
Guess (report=4)	500	4.009	937	4.012	0.8409
Guess (report=5)	495	4.825	895	4.960	0.0000***
Guess (no report)	1,014	2.283	1,648	2.076	0.0000***
Guess - secret number (no report)	1,014	0.481	1,648	0.448	0.5060

3.3 Results: Other Additional Tasks

In the NYU sessions, 38 subjects completed the risk additional task, 26 the other task, 38 the self task, 42 the computer task, 30 the average reports task, and 38 the aggregate feedback task.[3] Here we examine results for the risk task and the other task, as they were designed specifically to examine other forces besides belief biases that could explain receiver over-guessing. Results for the other tasks are available in Jin, Luca, and Martin (2015).

We first look at the 38 subjects who completed the risk additional task. When a subject has more than one switch point in the Holt-Laury multiple price list, then risk preferences are hard to ascertain, but just 3 subjects had multiple switch points. For the 35 subjects that had consistent switch points, 5 had a switch point that is consistent with risk neutrality. Another 3 subjects had switch points consistent with being risk loving, and the rest of subjects were consistent with being risk averse. There was a fair bit of variation in switch points: 5 subjects switched from the safe lottery to the risky lottery when there was a 50% chance of the high payment, 8 switched when there was a 60% chance, 7 when there was a 70% chance, and 5 when there was an 80% chance.

We used an OLS regression of receiver guess onto switch point. Controlling for the number of rounds that a receiver had spent as a sender or receiver up to that point and for subject fixed effects, the coefficient on switch point is positive, but is small (0.031) and not significant (p=0.261).[4]

[3] Unlike the additional tasks completed in our main sessions, the addition tasks completed in our NYU sessions are potentially under-powered because even though there were 212 subjects in total, each subject only completed one additional task and there were six possible additional tasks.

[4] However, there is just a single data point per subject, so it should be note that this analysis may be underpowered.

Second, we explore the role of social preferences in guessing of non-reported secret numbers. For evidence of this, we examine 26 NYU subjects who completed the other additional task. As mentioned previously, these subjects guessed the secret number from an earlier round, but now without payoff implications for the sender. If social preferences were a leading explanation for higher guesses, we would expect a decrease in guesses in this task. Instead, the average guess increased by 0.206, which is not statistically significant at a 10% level (two-sided t-test, p=0.1866).[5]

The increase in guesses after social considerations are minimized provides suggestive evidence of a punishment motive towards those who do not disclose. Instead of providing a force pushing away from equilibrium (higher guesses for non-reported secret numbers), social considerations appear to be pushing behavior towards equilibrium (lower guesses for non-reported secret numbers).

[5] Once again, there is just a single data point per subject, so this analysis may be underpowered.

4 Social Preferences

Social preferences can impact receiver behavior differently depending on whether or not the sender discloses the secret number, and when the sender discloses, the realization of the secret number. The realization of the state matters because it impacts the distribution of payoffs and individuals have been shown to hold preferences over the distribution of payoffs.

First, when senders *disclose* the secret number, social preferences could lead receivers to over-guess very low secret numbers. Because of the concavity of the payoff function, when receivers make very low guesses, sender payoffs are very low. In many standard social preference models, agents lose utility when they experience guilt over making much higher payoffs than their opponent. Such models would predict that receivers would make higher guesses, even when the secret number is not reported. For instance, standard models of fairness would say that some individuals could feel "bad" about accurately guessing the revealed state when the state is 1 (payoffs: -29, 110) due to feelings of guilt.

However, as shown in structural model of receiver guesses, the estimated prevalence of social preference is under 5% for disclosed secret numbers in all three treatments. At this rate, social preferences are not prevalent enough to impact the strategic incentives for unraveling.

Second, when senders *do not disclose* the secret number, the same forces of guilt may be at work. However, because receivers are now uncertain of the state, we have to account for the interaction between risk and social preferences. We know of no model of fairness under risk that would suggest that the impact of social motivations would get stronger with risk. As a consequence, we would expect the impact of guilt to be no greater with non-disclosure than with disclosure.

Thus, given the small size of the possible impact of social preferences in the case of disclosure, it seems unlikely that social preferences are driving a substantial part of the over-guessing when senders do not disclose. In fact, our structural estimation shows that without naivete, a combination of social preferences and confusion is insufficient to explain the extent of receiver over-guessing.

On top of this, if there is a social norm of disclosing, then receivers might wish to punish non-disclosure, even if the chances of re-matching are low. We find evidence of just such behavior in the "other" additional task conducted in our NYU sessions. These punishments should further dampen receiver guessing with non-disclosure.

5 Regression Results with Subject Clustering

Table A3. Regressions on sender disclosures (main sessions).

Sample	All draws		Only draws of 2		Rounds 6-45	
Dependent variable	Report or not		Report or not		Distance from highest expected payoff (fraction)	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy=1 if in the first 5 rounds	-0.0442**	-0.0464***	-0.0652	-0.0636		
	(0.0173)	(0.0178)	(0.0422)	(0.0419)		
Round # (1 to 45)	0.00113*	0.00118*	0.00277	0.00207	-0.000413*	-0.000417*
	(0.000655)	(0.000681)	(0.00203)	(0.00208)	(0.000232)	(0.000232)
Round # * random role * no feedback	0.000686	0.000329	0.00378	0.00225	1.95e-05	7.20e-05
	(0.000876)	(0.000899)	(0.00258)	(0.00271)	(0.000448)	(0.000463)
Round # * random role * feedback	0.00180*	0.00167*	0.00706**	0.00651**	-0.000878*	-0.000737
	(0.000939)	(0.000955)	(0.00278)	(0.00300)	(0.000503)	(0.000549)
Dummy=1 if sender belief of receiver guess upon non-report is below the actual draw	0.241***		0.321***		-0.0402***	
	(0.0360)		(0.0561)		(0.0119)	
Dummy=1 if draw=2	0.219***	0.335***			0.00586	-0.0138
	(0.0284)	(0.0269)			(0.0183)	(0.0181)
Dummy=1 if draw=3	0.523***	0.700***			-0.0194	-0.0468**
	(0.0351)	(0.0246)			(0.0204)	(0.0181)
Dummy=1 if draw=4	0.607***	0.837***			-0.0442**	-0.0808***
	(0.0365)	(0.0219)			(0.0198)	(0.0162)
Dummy=1 if draw=5	0.613***	0.839***			-0.0493**	-0.0847***
	(0.0383)	(0.0224)			(0.0208)	(0.0170)
Individual demographics	x	absorbed	x	absorbed	x	absorbed
Session fixed effects	x	absorbed	x	absorbed	x	absorbed
Subject fixed effects		x		x		x
Observations	7,224	7,224	1,477	1,477	5,742	5,742
R-squared	0.512	0.580	0.180	0.629	0.075	0.224

Notes: In parentheses are robust standard errors clustered by subject. *** p<0.01, ** p<0.05, * p<0.1 We define every 5 rounds as one block. Highest expected payoff is based on the distribution of receiver behavior he/she has observed in the last block of the same session. Columns (5) and (6) exclude the first 5 rounds because we need to construct the initial condition from the first 5 rounds. In all regressions, the default is the no feedback & fixed role treatment, and draw=1.

Table A4. Regressions on receiver guesses of non-reported secret numbers (main sessions).

Dependent variable	Receiver guess		Distance from highest expected payoff (fraction)	
	(1)	(2)	(3)	(4)
Dummy=1 if in the first 5 rounds	0.159***	0.111**		
	(0.0584)	(0.0551)		
Round # (1-45)	-0.00358	-0.00382	0.000247	0.000133
	(0.00247)	(0.00247)	(0.000482)	(0.000509)
Round # * random role * no feedback	-0.00466	-0.00564*	-0.000162	-0.000369
	(0.00324)	(0.00324)	(0.000765)	(0.000792)
Round # * random role * feedback	-0.0173***	-0.0182***	-0.00197***	-0.00208***
	(0.00374)	(0.00391)	(0.000744)	(0.000774)
Implied average non-reported number given receiver stated beliefs	0.695***		0.120***	
	(0.102)		(0.0226)	
Individual demographics	x	absorbed	x	absorbed
Session fixed effects	x	absorbed	x	absorbed
Subject fixed effects		x		x
Observations	2,551	2,551	2,204	2,204
R-squared	0.315	0.680	0.204	0.636

Notes: In parentheses are robust standard errors clustered by subject. *** p<0.01, ** p<0.05, * p<0.1. We define every 5 rounds as one block. Highest expected payoff is based on the distribution of sender behavior in the last block of the same session. Columns (3) and (4) exclude the first 5 rounds because we need to construct the initial condition from the first 5 rounds.

Table A5: Regressions on receiver guesses of non-reported secret numbers (main sessions).

Sample	All last guesses		Last guess = 2, 2.5, or 3	
Dependent variable	Guess with no report – last guess with no report			
	(1)	(2)	(3)	(4)
Dummy=1 if in the first 5 rounds	-0.027	-0.014	0.065	0.048
	(0.052)	(0.059)	(0.063)	(0.066)
Round # (1-45)	-0.000	-0.000	0.000	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)
Round # * random role * no feedback	0.002	0.003	0.003	0.002
	(0.002)	(0.002)	(0.003)	(0.004)
Round # * random role * feedback	0.001	-0.000	-0.003	-0.010
	(0.003)	(0.003)	(0.007)	(0.008)
Over-guessed last time	-0.191***	-0.249***	-0.072	-0.094*
	(0.054)	(0.070)	(0.048)	(0.054)
Over-guessed last time * random role * no feedback	-0.129	-0.228*	0.028	-0.020
	(0.094)	(0.119)	(0.092)	(0.098)
Over-guessed last time * random role * feedback	-0.386***	-0.519***	-0.384***	-0.360*
	(0.118)	(0.169)	(0.145)	(0.182)
Under-guessed last time	-0.005	0.005	-0.073	-0.095
	(0.050)	(0.058)	(0.059)	(0.068)
Under-guessed last time * random role * no feedback	0.103	0.131	0.209	0.200
	(0.101)	(0.118)	(0.133)	(0.152)
Under-guessed last time * random role * feedback	0.037	0.015	0.124	0.120
	(0.075)	(0.094)	(0.157)	(0.198)
Individual demographics	x	absorbed	x	absorbed
Session fixed effects	x	absorbed	x	absorbed
Subject fixed effects		x		x
Observations	2,287	2,306	1,151	1,154
R-squared	0.078	0.124	0.087	0.386

Note: Robust standard errors clustered by subject. *** p<0.01, ** p<0.05, * p<0.1.

REFERENCES (ONLINE APPENDIX ONLY)

Holt, C. A., and S. K. Laury. “Risk aversion and incentive effects.” *American Economic Review 92* no. 5 (2002): 1644–1655.

Ivanov, A., D. Levin, and M. Niederle. “Can relaxation of beliefs rationalize the winner's curse?: An experimental study.” *Econometrica* 78 no. 4 (2010): 1435–1452.

Jin, G. Z., M. Luca, and D. Martin. “Is no news (perceived as) bad news? An experimental investigation of information disclosure.” NBER Working Paper 21099 (2015).

Niederle, M., and L. Vesterlund. “Do women shy away from competition? Do men compete too much?” *Quarterly Journal of Economics* 122 no. 3 (2007): 1067–1101.