Complex Disclosure

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Abstract

We implement experimentally a simple game of mandatory disclosure in which senders are required to disclose their private information truthfully, but can choose how complex to make their reports. If senders choose complex reports, receivers must exert costly cognitive effort to correctly determine the sender's private information. We find that senders use complex disclosure when their private information would lead receivers to act against their interests. This obfuscation is sustained by two types of mistakes that receivers make when they face complexity. First, receivers who make quick decisions act in accordance with their prior beliefs, but these priors are often incorrect, which reflects incorrect beliefs about sender strategies. Second, receivers who make considered decisions do not act in accordance with their prior beliefs. Instead they appear to ignore their prior beliefs entirely, consistent with base rate neglect.

[1] The views expressed are those of the authors and do not necessarily represent those of the U.S. Federal Trade Commission or any individual Commissioner.

1. Introduction

Across a large number of industries, firms are mandated by law to disclose information about their financial health or about the nature and quality of their products and services. Yet in many cases, firms are allowed to determine the format of this information. For instance, the properties of many products are stipulated by contractual agreements or "terms of service", which can take a variety of formats. In credit card, rental and insurance contracts, one can present payment schedules, penalties, and fees clearly or bury them in the fine print. In data usage agreements, the term of service can range from one single line to multiple pages. In disclosures made by public companies, the disclosed financial information can be summarized into one headline or require a team of accountants to decipher.

The prevalence of complex disclosure appears at odds with the classical disclosure theory. If consumers are skeptical of firms using complex disclosures, then firms that offer better terms or have higher quality products will want to present their information clearly and simply. If they use complex reports instead, consumers may not bother to read through or comprehend the information that is provided. As a result, consumers may not know that they are offering better terms or higher quality products. Further, if the best firms use simple disclosures, then firms that only offer decent terms or medium quality products will also want to use simple disclosures in order to separate themselves from very worst firms. As a result, we would expect all but the worst firms to offer simple disclosure. This is similar to the "unraveling" logic in the case of voluntary disclosure.

So why do we see so much complex disclosure in practice? Two possibilities are that firms are required for legal reasons to use complex reports or that some products are so complicated that it is impossible to present them in a simple way. However, it is also possible that consumers are not sufficiently skeptical of complex disclosures, or they make systematic mistakes when they try to extract truth from complex reports. We look for evidence of such mistakes by implementing a laboratory experiment in which legal concerns and inherent complexity are eliminated.

In our experiment, there are two roles: an information sender (e.g., the firm) and an information receiver (e.g., the consumer). The sender observes the true state, which is a number, and chooses how complex to make their report of this number. When the report is simple, the number is presented as a single number, and when the report is complex, the number is presented a numeric string that adds up to that number. There is a clear conflict of interest: senders would like receivers to guess that the true state is as high as possible and the receiver would like to guess as accurately as possible.

Our main findings are that for lower than average true states, senders use complex reports with high frequency, and that when senders use complex reports and the true state is low, receivers guess higher than they should. This positive bias in receiver mistakes provides an incentive for senders to engage in obfuscation when the true state is low. By measuring response times and eliciting beliefs, we find evidence that the positive bias in mistakes is driven by two forces. First, receivers who make quick decisions act in accordance with their prior beliefs, but these priors are often incorrect, which reflects incorrect beliefs about sender strategies. Second, receivers who make considered decisions do not act in accordance with their prior beliefs. Instead they appear to ignore their prior beliefs entirely, consistent with base rate neglect (see Kahneman and Tversky, 1982).

We contribute to a broad set of literatures in economics and finance. In finance, Carlin (2009) theoretically investigates why firms might use complex pricing when some consumers are myopic, Carlin and Manso (2011) use a model to study why obfuscation might occur with retail financial products, and Carlin, Kogan, and Lowery (2013) use experiments to see how subjects trade assets after viewing information of different complexity levels.

In economics, theories of voluntary disclosure[2] suggest that market forces can drive firms to voluntarily and completely reveal information about their quality when such information is verifiable and the costs of verification and disclosure are low. In Section 2, we show that the same "unraveling" logic can apply to our setting if receivers are fully rational. More specifically, rational receivers should recognize that the choice of complex reports in mandatory disclosure regimes, like the choice of non-disclosure in voluntary disclosure, is bad news about the underlying state, and therefore adjust for the adverse selection inherent in complex disclosure. As a result, the unraveling logic should drive every sender but the worst to choose simple disclosure. Given the failure of unraveling in our lab results, we find conditions under which complex reporting could occur in equilibrium when some receivers attempt to parse the complex report, but only get a noisy signal of the true state.

Empirically, the economics literature has documented examples of incomplete disclosure when disclosure is voluntary[3] and examples of obfuscation when sellers do disclose.[4] While external and strategic factors may explain incomplete or complex disclosure[5], behavioral economics has suggested a third explanation. For example, Chetty et al. (2009) ran an experiment in which they compare two price regimes. In the first, customers are shown prices including tax. In the second, customers are shown prices excluding tax, but know the tax rate. These two conditions contain equivalent information; the customer can easily compute the total price in the second condition as well. However, people are much less responsive to tax in the second condition, because taxes are more complicated to compute. Pope (2009) and Luca and Smith (2013) show that the salience of quality disclosure also determines the extent to which customers respond. In a variety of settings, people are found inattentive to relevant details even after

[2] The theories of voluntary disclosure date back to Viscusi (1978), Grossman and Hart (1980), Grossman (1981), and Milgrom (1981).
[3] See Mathios (2000), Jin (2005), Bollinger et al (2011), Bederson et al. (2015), Anderson et al. (2015), Fung et al. (2007), and Luca and Smith (2015) for these examples.
[4] For example, Brown, Hossain and Morgan (2010) show that shipping and handling cost is often shrouded on e-commerce platforms. Ben-Shahar and Schneider (2015) show that complex disclosure appear in many industries subject to mandatory disclosure.
[5] See Matthews and Postlewaite (1985), Board (2009), Feltovich, Harbaugh, and To (2002), Grubb (2011), Marinovic and Varas (2015) for specific theories and Dranove and Jin (2010) for a literature review.

disclosure occurs (Armstrong and Chen, 2010; DellaVigna and Pollet, 2005; DellaVigna and Pollet, 2009; Lacetera et al., 2011). Finally, Hanna, Mullainathan, and Schwartzstein (2014) show that consumers often only attend to certain once-overlooked information when information is presented in a summary form. All of this suggests a strong behavioral component to the economics of disclosure.

A growing theoretical literature has begun to disentangle the way in which people respond to information, and the strategic implications this has for firm behavior. Gabaix and Laibson (2006) develop a model in which firms can *shroud* – or make less salient – dimensions of product information, which can lead to a breakdown in the unraveling result.[6] Eyster and Rabin (2005) develop a model of cursed equilibrium, in which people underestimate the relationship the information others have and the action they take. Schwartzstein (2014) presents a model in which an individual only updates based on prior beliefs about whether the information given is predictive; if previously discarded information turns out to actually be predictive, the individual will not return to his initial decision to condition his beliefs on this information and thus will make sub-optimal choices.

This paper aims to complement the above literatures by using lab experiments to study complex disclosure. Our experiments eliminate external factors that may occur in the real commercial or financial contexts, thus allowing us to focus on the fundamental economic incentives and behavior biases underlying the use of complex disclosure. Methodologically, our work is similar to an existing experimental literature that focuses on voluntary disclosure (for instance, Jin, Luca, and Martin, 2015) and cheap talk disclosure (for instance, Cai and Wang, 2006), and not on complex disclosure.

The rest of the paper is organized as follows. Section 2 presents a theoretical analysis of a simple game of complex disclosure. Section 3 presents our experimental design. Section 4 presents our experimental results for the baseline treatment. Section 5 compares these results to those from the robustness treatments. Section 6 concludes with a discussion.

2. One simple game of complex disclosure

This section analyzes a simple game of complex disclosure that mimics our lab experiment. The goal is to understand the incentives of the players in each role and how these incentives affect the equilibrium outcome.

2.1 Basic setup

Consider a one-shot game between two parties. The sender has perfect knowledge of his own product attribute x, but the receiver only knows the statistical distribution of x.

[6] Gabaix and Laibson (2006) model shrouded attributes that are both truly hidden and those that are hidden in plain sight. However, consumers in their model are unable to "unhide" the shrouded attribute, and this is the main point of departure with our paper.

The game has two stages. In the first stage, the sender moves first by deciding whether to report his private information in a simple or complex way, which we denote as

$$y^s = \begin{Bmatrix} 1_{complex} \\ x \end{Bmatrix}.$$

A simple disclosure is just revealing the actual value of x, while a complex disclosure renders a complicated report that will cost the receiver some time and energy to read. We refer to the time and energy spent in understanding a complex report as reading cost c, whose value is known to the receiver and may differ across receivers. The sender does not know the particular c facing the receiver he plays with, but he does know the distribution of c in the receiver population.

The receiver acts in the second stage. Given a complex report, the receiver may decide not to read the report. In that case, her guess of x is only conditional on the fact that the sender has sent a complex report. Alternatively, the receiver may pay the reading cost $c \geq 0$, obtain a potentially noisy signal $\tilde{x}$, and then make a point guess of x. We assume the signal is unbiased and independent of the truth:

$\tilde{x} = x + \epsilon,$

where $E(\epsilon) = 0, \epsilon \perp x,$ and the pdf of ϵ – $f(\epsilon)$ – is positive and continuous everywhere for any real number of ϵ. Under this assumption, $E(x|\tilde{x}) = \tilde{x}$ if the sender's equilibrium decision y^s does not depend on x. However, if the choice of complexity is a non-increasing function of x, then $E(x|\tilde{x}) < \tilde{x}$ because observing $\tilde{x}$ is conditional on the sender choosing a complex report and that choice alone is a negative signal about the true x. In contrast, when the report is simple, we assume reading is automatic and the receiver knows x for sure. Either way, we label the receiver's guess as x^g.

We assume the receiver faces a convex loss function L_r that will be minimized if her guess is equal to the truth ($x^g = x$), while the sender's payoff π_s increases with x^g regardless of the true x. In particular:

(1) Receiver:
$min_{\{1_{readcomplex}, x^g\}}\ L_r(x^g, y^s) = E_x[(x^g - x)^2 | y^s] + c \cdot 1_{complex} \cdot 1_{readcomplex},$

(2) Sender: $max_{\{y^s\}}\ \pi_s(y^s) = E_{x^g}[G_s(x^g)|y^s],$

where $G_s(.)$ is a monotonic and differentiable function, E_x denotes expectation over variable x, and $1_{readcomplex}$ is a dummy equal to one if the receiver decides to read a complex report and zero otherwise. We define the receiver's loss function as quadratic, to be consistent with our experiment. Both the sender and the receiver are assumed to be risk neutral.

Under rational expectation, a sub-game perfect Bayesian equilibrium shall satisfy three conditions: (1) given the private knowledge of x, the sender chooses an optimal reporting decision $y^s(x)$ that maximizes his own payoff π_s in expectation; (2) given the sender's reporting choice y^s, the receiver chooses an optimal reading and guessing decisions $\{1_{readcomplex}, x^g|y^s\}$ that minimizes her loss function L_r; and (3) both parties' belief satisfy rational expectation, which implies that the receiver's belief about the distribution of x under complex reporting matches the sender's strategy of reporting and the sender's belief of the receiver's strategy $\{1_{readcomplex}, x^g|y^s\}$ matches the receiver's actual strategy.

An equilibrium with bounded rationality may occur if the receiver's belief about complex reports is inconsistent with the sender's reporting strategy, or if the sender's belief of receiver strategy is inconsistent with the receiver's actual strategy. As shown below, our experimental results suggest that senders tend to respond optimally to receiver behavior, so towards the end of this section, we focus the discussion of bounded rationality on receivers.

There could be multiple equilibria. To be consistent with what we observe in the lab, we focus on a particular type of equilibrium in which the receiver will read a complex report if her reading cost is below certain threshold $\bar{c}$ and the sender will send a complex report if his true x is below certain threshold $\bar{x}$. Note that when $\bar{c}$>0 and $\bar{x} > \min(x)$, it is a separate equilibrium. When $\bar{c}$=0, it is a pooling equilibrium on the receiver's side as every receiver will read the complex report. If $\bar{x} = \min(x)$, it is a pooling equilibrium on the sender's side as every sender but the worst one will choose simple report. In that case, complex report reveals the worst type of the sender, and is equivalent to simple report. For simplicity, we assume the sender will always choose simple report if he is indifferent between simple and complex and the receiver will always choose not to read the complex report if she is indifferent between reading and non-reading. These tie-breaking assumptions should not matter when the reading cost and true x are continuous.

2.2 Equilibrium under rational expectation

To understand the role of rationality in player behavior, we start with equilibrium under rational expectation.

Let us consider the receiver's strategy first. Obviously, if the reading cost is prohibitively high for all receivers, this game reduces to the typical game of voluntary disclosure. From the literature, we know from "unraveling" logic that full disclosure is the unique sequential equilibrium in that case. In particular, the sender always chooses simple disclosure and the receiver believes that any complex disclosure implies sufficiently bad news about x. This equilibrium remains an equilibrium even if the reading cost is not high, because receivers always have the choice of not reading a complex report and if all receivers believe that complex report implies sufficiently bad news about x, there is no need to read the complex report due to "unraveling".

If the reading cost is zero for all receivers and the signal is always precise (i.e. the variance of noise ϵ is zero), receivers will always learn the true x no matter whether the report is complex or simple. Hence, the sender does not have any incentive to engage in complex disclosure.

The interesting case is when the reading cost is positive and heterogeneous. Because we assume the realization of $\tilde{x}$ is independent of the reading cost, the benefits of reading the report is the same for two receivers with different reading costs. In other words, if a receiver with reading cost c_1 finds it worthwhile to read the report, any receiver with $c < c_1$ should find it worthwhile as well. It follows that, if receiver's optimal guess x^g (after reading the complex report) depends on $\tilde{x}$ (otherwise we go back to the unraveling equilibrium), then there must exist a cutoff $\bar{c}$ such that receivers will read the complex report if and only if $c < \bar{c}$.

In particular, when $c \geq \bar{c}$, according to rational expectation, the receiver's strategy is to guess

(3) $x^g = E\left(x \middle| 1_{complex}\right)$.

When $c < \bar{c}$, the optimal guess is:

$$x^g = argmin_{\{x^g\}}\, E_{x|\tilde{x}}\left\{(x^g - x)^2 \middle| \tilde{x}, 1_{complex}\right\}$$
$$= argmin_{\{x^g\}} \int_{x|\tilde{x}} (x^g - x)^2 f\left(x \middle| \tilde{x}, 1_{complex}\right) dx.$$

The first order condition yields:

(4) $$x^g = \int_{x|\tilde{x}} x \cdot f\left(x \middle| \tilde{x}, 1_{complex}\right) dx = E\left(x \middle| \tilde{x}, 1_{complex}\right) = \tilde{x} - \kappa(\tilde{x}),$$

where $\kappa(\tilde{x})$ presents the discount that a receiver will include in her optimal guess after reading a complex report and obtaining the signal $\tilde{x}$. It arises out of the facts that only senders with inferior enough x will choose a complex report and there is noise in the signal. In the extreme case of perfect signal (i.e. the variance of the noise ϵ is zero), reading the report reveals the true x, hence $x^g = \tilde{x} = x$ and $\kappa(\tilde{x}) = 0$.

When $c = \bar{c}$, the receiver is indifferent between reading and non-reading the complex report:

(5) $$E_x\left(E\left(x \middle| 1_{complex}\right) - x\right)^2 = E_x\left(E_{\tilde{x}|x}(\tilde{x} - \kappa(\tilde{x}) - x)^2\right) + \bar{c}.$$

When the signal noise is sufficiently small, the cutoff reading cost $\bar{c}$ can be strictly positive because the receiver's loss function is convex thus there is a positive value to (partially) resolve the uncertainty of a complex report by reading it. In the extreme case of perfect signal, equation (5) reduces to $E_x\left(E\left(x \middle| 1_{complex}\right) - x\right)^2 = \bar{c}$. In other words, if some senders choose complex report, there will be some receivers choosing not to read the report even if reading the report reveals the truth with certainty.

Now turn to the sender's side. Given that the receiver will read the complex report if $c < \bar{c}$, the sender's expected payoff will be:

$$\pi_s(y^s = x) = G_s(x)$$
$$\pi_s\left(y^s = 1_{complex}|x\right)$$
$$= \Pr(c \geq \bar{c}) \cdot G_s\left(x^g \middle| 1_{complex}\right) + \Pr(c < \bar{c}) \cdot \int_{\tilde{x}|x} G_s\left(x^g \middle| 1_{complex}, \tilde{x}\right) f(\tilde{x}|x) d\tilde{x}.$$

Plugging in the optimal x^g derived from the receiver's side, we have:

$$\pi_s\left(y^s = 1_{complex}|x\right)$$
$$= \Pr(c \geq \bar{c}) \cdot G_s\left[E\left(x \middle| 1_{complex}\right)\right] + \Pr(c < \bar{c}) \cdot E_{\tilde{x}|x}(G_s(\tilde{x} - \kappa(\tilde{x})).$$

The sender will choose complex report if and only if $\pi_s(y^s = x) < \pi_s\left(y^s = 1_{complex}|x\right)$, which can be rewritten as:

(6) $$\Pr(c \geq \bar{c}) \cdot \{G_s\left[E\left(x \middle| 1_{complex}\right)\right] - G_s(x)\}$$
$$> \Pr(c < \bar{c}) \cdot \{G_s(x) - E_{\tilde{x}|x}(G_s(\tilde{x} - \kappa(\tilde{x}))\}.$$

For a sender with $x < E\left(x \middle| 1_{complex}\right)$, the left hand side represents the benefits he expects from fooling a high-cost receiver that will not read the complex report. The right hand side reflects the expected risk of a low-cost receiver reading the complex report and forming a belief of x that could be below the actual x. When the left side dominates the right side, it is worthwhile to send a complex report. For a sender with $x > E\left(x \middle| 1_{complex}\right)$, the left hand side is the loss he expects from choosing a complex report thus hiding his relatively high x from a high-cost receiver not reading the report. The right hand side includes the potential gain he could get from a low-cost receiver reading the report and getting a signal significantly higher than the actual x.

Depending on the curvature of G_s, $E(G_s(.))$ can be greater or smaller than $G_s(E(.))$. If we define their difference as $\lambda(.)$, we can rewrite equation (6) as:

(7) $$\Pr(c \geq \bar{c}) \cdot \{G_s\left[E\left(x \middle| 1_{complex}\right)\right] - G_s(x)\}$$
$$> \Pr(c < \bar{c}) \cdot \{G_s(x) - G_s[x - E_{\tilde{x}|x}(\kappa(\tilde{x})) + \lambda(x)]\}$$

where $E_{\tilde{x}|x}(\kappa(\tilde{x}))$ is the expected discount that the receiver will put in her optimal guess due to the sender's adverse selection incentive, and $\lambda(x)$ accounts for the value of uncertainty for the sender: when $G_s(.)$ is convex, the sender gains more from a high draw of $\tilde{x}$ than from a low draw of $\tilde{x}$. Hence there is a lottery value to roll the dice in $\tilde{x}$. Put it another way, $\lambda(x)$ captures the compensation the sender will demand in the actual x before he is willing to give up the right to roll the dice. This lottery value $\lambda(x)$ is positive if $G_s(.)$ is convex, and negative if $G_s(.)$ is concave.

For the sender's equilibrium strategy to be $y^s = 1_{complex}$ if $x < \bar{x}$, we need a single crossing property such that if equation (7) is satisfied for x_1, then it must be satisfied for any $x < x_1$. Although the left hand side of (7) is a strict decreasing function of x by definition, this property does not come by automatically, as how the adverse selection term ($E_{\tilde{x}|x}(\kappa(\tilde{x}))$) and the lottery value term ($\lambda(x)$) vary by x depends on the curvature of $G_s(.)$ and the noisiness of the signal.

Because we have lab experiments to empirically demonstrate the single-crossing property, we do not attempt to derive the theoretical conditions that will give rise to the single-crossing property. Rather, we assume it exists and focus on the property of $\bar{x}$.

At the cutoff $\bar{x}$, the sender is indifferent between complex and simple report, which implies:

$$(8) \qquad \Pr(c \geq \bar{c}) \cdot \{G_s[E(x|1_{complex})] - G_s(\bar{x})\}$$
$$= \Pr(c < \bar{c}) \cdot \{G_s(x) - G_s[x - E_{\tilde{x}|x}(\kappa(\tilde{x})) + \lambda(x)]\}.$$

In the extreme case of perfect signal (i.e. $\tilde{x} = x$ with certainty), equation (8) boils down to:

$$E(x|1_{complex}) = \bar{x},$$

which can be only satisfied in rational expectation when $\bar{x} = \min(x)$. This brings us back to the unraveling equilibrium. In other words, even if there are some high-cost receivers who will not read a complex report, rational expectation will ensure that any sender with a true x above the average x of complex report will have an incentive to choose simple report. This incentive leads to unraveling.

That implies that a separate equilibrium can only occur when the signal is noisy. In that case, a sender at the cutoff $\bar{x}$ must expect that some low-cost receivers will read the complex report and get higher-than-truth signals. Moreover, the lottery value created by these high draws must be big enough to trade off the loss from the high-cost receivers who do not read the complex report but will believe the type of the sender to be lower than his true type.

2.3 Interpreting experimental results in light of the rational-expectation equilibria

To summarize, we have identified two possible equilibria with rational expectation: one is the unraveling result in which the receiver always associates complex report with the worst sender type, which in turn motivates all types of senders to disclose the true x in a simple report. The second possibility is a separate equilibrium in which the sender chooses a complex report if $x < \bar{x}$ while a receiver with $c \leq \bar{c}$ will read the complex report and guess $x^g = \tilde{x} - \kappa(\tilde{x})$ and a receiver with $c > \bar{c}$ will not read the complex report and guess $x^g = E(x|x < \bar{x})$. For the second equilibrium to exist, we need three elements: (1) at least some receivers have low enough reading cost to read the complex report; (2) the signal that these receivers get from reading the complex report must be noisy; and (3) the sender must face a sufficiently convex payoff so that it is worthwhile to introduce uncertainty via a complex report.

Obviously, the "unraveling" equilibrium does not occur in our lab experiments. Surprisingly, player behavior does not even move towards unraveling as subjects repeat the game for 30 rounds and receive full feedback of the true x at the end of each round. This is robust to the number of complex levels we allow and whether we provide full or no feedback of the true x after each round. Moreover, in the lab experiment, we give the sender a concave payoff function, which should discourage him from playing up uncertainty via a complex report. This is at odds with condition #3 as stated above for the separating equilibrium.

So how could the separate equilibrium arise in the lab? We can think of two possibilities: first, our subjects are so risk loving that they see a substantial lottery value in the complex report, even though we set their payoff function to be concave. This is unlikely to be the explanation, as experiments regularly find that most subjects are risk adverse based on the Holt-Laury measure of risk aversion (see for instance, Jin, Luca, and Martin, 2015, which ran the measure on a similar subject pool).

The second possibility is that receivers have bounded rationality. In our model, receiver rationality may affect the equilibrium in three ways. First, receivers may not correctly anticipate the sender's reporting strategy. This undermines the fundamental logic of best response in Nash Equilibrium. Second, even if receivers understand the sender's reporting strategy perfectly, they may have problem forming beliefs about the underlying true state conditional on the observed action of the sender. Both of the above two problems in receiver beliefs may lead the receiver to not fully understand the adverse selection embedded in complex reporting and therefore over-expect the average type of senders that choose a complex report. This is similar to the failure of unraveling results documented in our earlier paper (Jin, Luca and Martin 2015): if receivers over-guess the average quality of non-disclosing senders, it will discourage low-type senders from disclosing.

While the above two belief problems may occur to all receivers, receivers with lower costs may face a third problem: if they read the complex report because their reading cost is sufficiently low, they may encounter multiple problems in Bayesian updating. For instance, they may ignore their prior beliefs and just guess based on their noisy signal. Base rate neglect, in a number of forms, has been well documented in other settings (see Kahneman and Tversky, 1982). Also, they may draw a biased signal from the report and they may not incorporate this bias into their posterior beliefs if they are naïve to its existence.

With these forces of bounded rationality, the lottery embedded in complex reports could have a positive value to the sender, not because the sender is risk loving, but because the receiver on average over-guess the true x when the reports are complex.

Using standard choice data, we do not know if a receiver has skipped reading the complex report or has misread the complex report, so it is hard to disentangle these explanations. However, by asking subjects about their beliefs, we are able to understand the first two problems directly, and infer whether the third problem is occurring by comparing the reported beliefs with actual guesses. Also, response times can give us an insight into whether attention was paid to the content of complex reports (as in Caplin and Martin, 2015). In the rest of the paper, we will first present lab results and then document evidence for some of these behavioral problems on the receiver's side.

3. Experimental design

Our experiment has three treatments, which vary in terms of the actions available to subjects and the feedback provided to subjects. These are divided into one "baseline" treatment and two robustness treatments. Subjects in a session complete just one of the three treatments. Full instructions for the baseline treatment are provided in the appendix.

In all three treatments, subjects complete 30 rounds, and depending on the session, are then asked to complete an optional questionnaire that includes questions about how they thought others had played and questions about demographic details. Specifically, subjects are asked for their gender, if they are a native English speaker, their year in school, and if they have a friend participating in that session.

At the end of each session, subjects are privately paid in cash a show up fee of $5 plus all additional earnings they accumulate over the course of the session. These earnings are denominated in "Experimental Currency Units" (ECU), but are converted to U.S. dollars at a rate of 150 to 1 (rounded up to the nearest dollar). While it is possible for subjects to end up with a negative balance of ECU, because subjects are paid for every round, this outcome is extremely unlikely and never came close to occurring in the sessions we ran. However, because subjects are paid for every round, there is the potential for intentional variation in play (a "portfolio" strategy), but we find little evidence of such behavior.

3.1 In each round

In all three treatments of our experiment, study participants are matched together in pairs for a round, and then in each pair, a subject is assigned either the role of information sender, which we can think of as the firm, or the role of information receiver, which we can think of as the consumer.

Each round has two stages. First, the sender is presented with a randomly determined whole number between 1 and 10 called the "secret number". Each number is equally likely and both senders and receiver are told this in the instructions. After being presented the secret number, the sender chooses how complex to make his/her report of that number to the receiver. There is no time limit on the sender's decision.

In our experiment, report complexity takes a specific form. The sender chooses a “report length”, which is a whole number X between 1 and 20. After the sender chooses a report length, the computer program randomly selects X numbers between -10 and 10 until those numbers add up to the secret number. The sender does not know what these numbers will be, just that there will be X of them and the process used to generate them. The receiver is also told this process.

In the second stage, the receiver is then presented with these X numbers and is told that they add up to the value of the secret number. Regardless of which report complexity the sender chooses, the receiver has 60 seconds to view the sender’s report and guess the secret number. This guess can be any whole number from 1 to 10. Subjects are only given whole number guesses in order to keep the payoff table simple and easy to read and interpret. If nothing is guessed after 60 seconds, a random guess (each equally likely) is entered for the receiver. Both senders and receivers are informed of this in the instructions.

Based on the guess and the actual secret number, the receiver earns ECU equal to $ECU_R = 110 - 20|(S - A)/2|^{1.4}$, where S is the secret number and A is the receiver’s guess. With this payoff function, a risk neutral receiver would guess closest to their expected value of the secret number. Based on the receiver’s guess, a sender earns ECU equal to $ECU_s = 110 - 20|(5 - A)/2|^{1.4}$. These payoffs do not depend on the actual secret number, but are strictly increasing with the receiver’s guess. The payoffs for senders and receivers are shown in a table, so that a subject does not need to know or interpret these functional forms.

With these payoff functions, there was a clear misalignment of interests between senders and receivers. Receiver payoffs were higher when their guesses were closer to the secret number, and sender payoffs were higher when the receiver made higher guesses. Subjects were told these two broad features of sender and receiver payoffs.

3.2 Between and across rounds

At the beginning of a session, the instructions are read aloud. A paper copy is also given to subjects so that they can review them at any point during the experiment

Subjects complete 30 rounds, and in each round, they are anonymously matched with a new partner, and they are equally likely to be matched with any other subject in the session. If there are 14 subjects in a session, the probability of being matched with the same subject in the subsequent round is just 0.6%.

Once matched with a new partner, subjects are randomly assigned, with equal probability, to be the sender or receiver. The purpose of switching roles is to insure that both sides have a good sense for the incentives and actions available to the other side. To reduce framing effects, the sender was call the “A Player”, and the receiver was called the “B Player”.

After all senders have made their choices, the receivers are given 60 seconds to make their choices. Once all receivers have made their choices, the subjects are shown feedback (if any) before starting the next round.

Once all rounds are complete, subjects are asked questions about their beliefs of how other subjects played in their session. First, subjects are asked what the average report length was that senders chose for each secret number. Second, subjects are asked what the average secret number was when sender's chose complexity levels between 1 and 5, between 6 and 10, between 11 and 15, and between 16 and 20.

3.3 Treatment variation

Our three treatments vary on two dimensions: the number of complexity levels that senders can choose among and the feedback provided to subjects after each round.

In our baseline treatment, senders can choose any report length between 1 and 20 after being presented with the secret number. In one of our robustness treatments, senders could only choose a report length of 1 and a report length of 20. The reason for this robustness treatment is to determine whether play is substantially different if the "strategic complexity" of the game is reduced for both senders and receivers.

Also, in our baseline treatment, subjects are provided the following feedback after each round: the actual secret number for their pairing, the report length chosen for their pairing, the guess made for their pairing, and the subject's payoff for that round. In both of the robustness treatments, we shut down in this channel to see if it is an important source of learning.

3.4 Related experiments

The framing and payoffs in this experiment are similar to those used in the cheap talk experiments of Cai and Wang (2006) and the voluntary disclosure experiments of Jin, Luca, and Martin (2015). The key difference between our experiment and these experiments is that only the senders in our experiment had access to complex disclosure. Moreover, in the experiment presented in Cai and Wang (2006), senders had the option to misrepresent the secret number, and in the experiment presented in Jin, Luca, and Martin (2015), senders had the option not to disclose anything.

Kalayci and Potters (2011) implement a laboratory experiment where sellers have control over the complexity of product quality, but in their experiment buyers are given no information about the objectives and incentives of sellers, so it is difficult to know what buyers believe about why sellers present products in a complex way. In Martin (2015), buyers are given information about the seller's incentives, but the complexity of product quality is determined exogenously.

Several other recent experiments have presented numbers as the sum of a string of numbers in order to generate cognitive costs for subjects. For instance, Caplin, Dean, and

Martin (2011) find evidence of sacrificing behavior by having subjects choose among strings of numbers, where the value of an option is determined by the sum of the string. Caplin and Martin (2015) also ask subjects to choose among sums of strings and find evidence consistent with a dual-process model of choice.

4. Experimental results (baseline treatment)

Our experiments were run at the Computer Lab for Experimental Research (CLER) facility at the Harvard Business School. Subjects did not have to be Harvard University students, but they were restricted to be no older than 25 years old. The software used to run the experiments was the z-Tree software package (Fischbacher 2007).

In the baseline treatment, 160 subjects completed a total of 4,774 rounds. For the baseline treatment, the average earnings were $19.93, with a minimum payment of $12 and a maximum of $25.

Table 1 provides summary information for the baseline treatment. Of the students who volunteered their demographic information, approximately 60% were undergraduate students, and over 60% were female. Most were native English speakers, and less than 10% had a friend in the room.

Around 3.3% of receivers did not make a choice before the 60-second time limit was hit, so a random guess was made on their behalf. We exclude these decisions from all of the analyses that follow, and the resulting summary statistics are provided in the final three columns of table 1.

4.1 Sender behavior

In these sessions, senders made use of the option to engage in complex disclosure. From table 1, we can see that on average, senders chose a report length of 9.633 out of a possible 20.

In addition, there was a strong relationship between the secret number drawn and the complexity used. Table 2 indicates that for the lowest possible secret number, the average is above 15, and for the highest possible secret number, the average is less than 4. This trend is present across the distribution of complexity levels, as is indicated by the last two columns. The percentage of rounds that high complexity (a report length of 16 to 20) is used falls from 69.0% to 9.8% from the lowest to highest secret number, and the percentage of rounds that low complexity (a report length of 1 to 5) is used rises from 15.1% to 79.6%. More interestingly, senders are more likely to choose the complexity levels that are either at the two extremes or equal to a multiple of 5 (i.e. 1, 5, 10, 15, 20), suggesting that the actual choice set in the sender's mind is more discrete than continuous.

Figure 1 gives more detail on the use of complexity for each secret number. At the lowest secret number, the largest bubble is around the highest complexity level (of 20), and for the highest secret number, the largest bubble is around the lowest complexity level. The linear fit is downward sloping, and there is an S-shape in the median complexity levels. However, the bubbles show that even for a secret number of 5, the most frequently used complexity levels are 1 and 20.

Figures 2a and 2b give a sense for how complexity use varies over the course of the experiment. Looking across rounds, there is variation in the average complexity level for low, medium, and high secret numbers. However, the average complexity levels are ordered throughout the experiment. Moreover, the linear fits reveal a larger decrease for the lowest secret numbers and a smaller decrease for middle secret numbers. For the largest secret numbers, we see no evidence of a decrease in the average complexity level.

In fact, figure 2b demonstrates that there is little change in the use of high complexity reports for all secret numbers, though senders with low secret numbers appear to use them less as the experiment goes along. Instead, figure 2a shows that the changes in average complexity levels are driven by a substantial increase in the use of low complexity reports by senders with low secret numbers.

Table 5 quantifies these patterns using a regression analysis. Looking at regressions 1 and 2, we see that complexity decreases over rounds and is smaller for higher secret numbers. Looking at the probability of using the five highest complexity levels (regressions 3 and 4), there is not a significant impact of round, but it is significantly less likely to be used by senders with higher secret numbers. Looking instead at the probability of using the five lowest complexity levels (regressions 5 and 6), there is a small increase in the probability over rounds and senders with higher secret numbers are more likely to use it.

4.2 Receiver behavior

As table 3 shows, on average receivers made lower guesses when the complexity level was higher. They also made larger mistakes as the complexity level increased, which is indicated by the average absolute difference between their guess and the actual secret number. To see if there was a bias in these mistakes, we can examine the average of this difference without taking the absolute value. For all complexity levels above 13, the average of this difference is positive. Figure 3a represents these patterns visually.

If the average receiver mistake with complex reports is positive for every secret number, every sender would have an incentive to use complex report. However, we only observe complex reports concentrating in lower secret numbers. As shown in table 4, this makes sense because the average receiver mistake with complex reports varies by secret number. For lower secret numbers, the bias is positive (guesses are above the actual number), and for higher secret numbers, the bias is negative (guesses are below the actual number). For lower secret number, this positive bias is larger for higher complexity levels, which can be seen visually in figure 3b. This gives senders with low secret numbers an incentive to use higher complexity levels, as higher guesses result in higher earnings. In the meantime, the negative bias on complex reports of high secret numbers gives senders an incentive to use lower complexity levels, which explains why over time there is an increase in simple reports for high secret numbers.

Figure 4b shows that for the highest complexity levels there is a general decrease in the size of mistakes over rounds, but figure 4a shows that for the highest complexity levels, there is insignificant decrease in the bias of these mistakes.

Table 6 quantifies these patterns using a regression analysis. Higher complexity corresponds to lower guesses and this relationship is statistically significant, but higher complexity also corresponds to larger mistakes and a significantly higher positive bias in mistakes over the group of the highest complexity levels (16 and above). These results are robust to using subject level fixed effects.

Above all, these results suggest a separating equilibrium: senders with few low secret numbers tend to choose high complexity and stay there overtime, while senders with middle to high secret numbers gravitate to low complexity. These behaviors can be explained by receiver mistakes, because receivers tend to make bigger mistakes for complex reports and these mistakes are only positive if the secret number is below the population average (5). Now we turn to detect factors contributing to these receiver mistakes.

4.3 Explanations for receiver mistakes

According to the model presented in Section 2, we can think of several behavioral explanations for the systematic receiver mistakes when senders use complex reports.

First, receivers may not anticipate the sender's reporting strategy correctly. We directly asked most subjects in our baseline treatment at the end of the 30th round to state their beliefs of sender strategy for each specific secret number. Table 7 presents the average of these reported beliefs, along with the actual reporting strategy used by the subject as a sender, and the reporting strategy used by all senders that play in the same session as the subject. Clearly, our subjects believe in a monotonic relationship between the true state and sender choice of complexity, and the average belief presented in column 3 is highly correlated with the actual strategy played by self (column 5) or by other senders (column 7).[7] The absolute magnitudes are slightly different: our subjects believe that senders with the lowest secret numbers report slightly higher complexity than they actually do and senders with the highest secret numbers report slightly lower complexity than they actually do.

However, these averages mask substantial heterogeneity across individual subjects. For any specific secret number, we can compute the across-individual correlation between reported beliefs and actual strategies. This correlation ranges from 0.33 to 0.56 between reported beliefs and the subject's own reporting strategy as a sender, and from 0.19 to 0.37 between reported beliefs and all senders' strategy. Above all, evidence suggests that

[7] These correlations are slightly higher than 0.99.

our subjects possess heterogeneous beliefs, but on average have reasonable beliefs about the sender's reporting strategy.

From these beliefs about sender's strategies, we can compute the guess that a receiver would have made for the average secret number for each complexity level if they applied Bayes' rule. A comparison between this "implied guess" and the same subject's actual guess", should help us to understand how these beliefs contribute to their actual behavior as a receiver.

More specifically, given the sender's tendency to choose complexity at round or extreme levels (1, 5, 10, 15, 20), we group complexity levels in four categories: 1-5, 6-10, 11-15, and 16-20. For each category and each subject, we compute the implied guess by Bayesian updating, using the fact that each secret number is equally likely. For example, if a subject believes that senders will choose level 20 if the secret number is 1 or 2, level 15 if 3-5, level 10 if 6-7, level 5 if 8-9, and level 1 if 10, then his/her implied guess should be 9 for level group 1-5, 6.5 for level group 6-10, 4 for level group 11-15, and 1.5 for level 20.

Treating each subject as one observation, panel A of table 8 reports the average implied guess by the four complexity groups separately (column 3). Comparing implied guesses with actual guesses in column 7, we find that receivers should have guessed higher for low complexity levels and lower for high complexity levels, according to their belief of sender's strategies and Bayesian updating. Moreover, panel B of table 8 shows a low and insignificant correlation between an individual's implied guess and his/her actual guess for every complexity group (-0.02 to 0.10). This suggests that, although our subjects on average have a reasonable prediction about the sender's reporting strategy, it has little influence on their actual guesses. In other words, there must be other important factors when receivers translate their beliefs into guesses.

One such factor could be that receivers may have problem forming beliefs about the underlying true state conditional on the observed action of the sender. In panel A of table 8, we summarize subjects' reported belief about the average secret number for each level of complexity, which we refer to as "complex guess." By definition, the reported complex guess includes both the receiver's belief of the sender's reporting strategy and the method that the receiver uses in translating that belief into a guess. If the method is Bayesian, implied guess should be equal to or at least highly correlated with complex guess. It is worth noting that complex guess is not necessarily equal to actual guess, because when we ask about complex guess, the subject faces a specific complexity level without a string of numbers that adds up to the truth. In other words, complex guess does not allow the receiver to exert any effort in reading a specific report. This is equivalent to receivers having a prohibitively high cost of reading in our model.

Table 8 shows that the average complex guess is different from both the implied guess and the actual guess. While complex guess is correlated with both, the correlation is only 0.04-0.32 with the actual guess, and 0.31-0.43 with the implied guess. Moreover, the correlation between complex and actual guesses goes up with complexity, while the correlation between complex and implies guesses is relatively stable across the four complexity groups.

The significant and positive correlation between complex and actual guesses for higher complexity levels is reinforced by a regression analysis presented in table 9. In columns 1 and 2, we regress actual guess on implied guess, complex guess, complexity group fixed effects, and, some control of subject attributes (either subject demographic variables in column 1 or subject fixed effects in column 2). Regression results suggest that complex guess has a positive and significant relationship with the actual guess, but the coefficient on implied guess is insignificant. In the rest of table 9, the dependent variable changes to receiver mistake (i.e. guess minus secret number) in columns 3 and 4, and to the absolute receive mistake in columns 5 and 6. We find that complex guess is significantly and positively correlated with receiver mistake, which suggests that part of what is driving the positive bias in mistakes is that some receivers have formed overly positive beliefs about the secret number when disclosure is complex. In contrast, implied guess is not significant in all columns except for column 5, where the coefficient is significant at a 10% confidence level but becomes insignificant after we control for subject fixed effects.

More importantly, table 9 continues to show a significant positive bias in high complexity levels, even after we control for implied guess and complex guess. This indicates that receiver beliefs alone cannot explain all the receiver mistakes. This brings up the third behavioral explanation: could there be a systematic error in assessing the information contained in complex messages?

Before answering that question, one may ask whether receivers read complex reports at all. For this, we can look for evidence from response times. For complexity levels from 16 to 20, receivers spent an average of 41.51 seconds. However, receivers sometimes spent substantially less time on their decisions. The 10^{th} percentile was 24.98 seconds, and the 25^{th} percentile was 31.87 seconds. We refer to the former as "very quick" decisions and the latter as "quick decisions".

Decisions of all response time durations have a positive bias on average. For complexity levels from 16 to 20, the guess is 0.778 above the actual secret number on average for very quick decisions, 0.433 above for quick decisions, and 0.420 above for non-quick decisions ("considered" decisions). However, there are large differences in beliefs and behavior between quicker and considered decisions, suggesting different mechanisms are at work in producing the positive biases.

Looking first at quick and very quick decisions, the relationship between complex guess and actual guess is strong for complexity levels from 16 to 20. If we regress the actual guess onto complex guess for very quick decisions (with cluster robust standard errors), the coefficient is 0.655, and it is significant at a 1% level. For quick decisions, the coefficient is 0.503, and it is also significant at a 1% level. These results imply that for quicker decisions, subjects are deciding based largely on their prior beliefs.

For these decisions, incorrect beliefs about sender strategies seem to be the driving force behind incorrect beliefs. If we regress the complex guess onto the implied guess, the coefficient is 0.930 for quick decisions and 1.280 for very quick decisions.

On the other hand, for considered decisions where receivers incorrectly guessed the secret number, the relationship between complex guess and actual guess not significantly different from 0 for complexity levels from 16 to 20. If we regress the actual guess onto complex guess for very quick decisions (with cluster robust standard errors), the coefficient is 0.105, and the p-value is 0.494. This suggests that for these decisions, prior beliefs (whatever their source) are not factoring into posterior beliefs.

5. Experimental results (robustness treatments)

For the robustness treatments, 134 subjects completed 4,020 rounds. Of these, 68 subjects were in the treatment with only two complexity levels (report lengths of 1 or 20) and no feedback, and 66 subjects were in the treatment with many complexity levels (report lengths of anywhere from 1 to 20) and no feedback.

The two panels of Table 10 show the summary statistics for the robustness treatments. The average sender choices of complexity are similar to before, though slightly lower. In the baseline treatment, the average was 9.633, in the robustness treatment with many complexity levels the average was 8.490 and in the two complexity level robustness treatment the average was 8.544.

As in the baseline treatment, there was a strong relationship between the secret number drawn and the complexity used in both of the robustness treatments. In table 11, there is a general decrease in the mean complexity level and in the percentage of rounds that high complexity reports are used as the secret number increases.

Figure 5 allows for an easy comparison of this relationship across treatments. Naturally, there is more use of the lowest and highest complexity reports with the two complexity level treatment, but these differences disappear when they are used less often.

We also see robustness in receiver decisions when looking at table 12 and figure 6. At the lowest level of complexity and highest level of complexity, the average secret number and average mistake are very similar across all three treatments.

6. Discussion and Future Work (Incomplete)

To better understand subject choices in our experiment, we plan to run a number of additional treatments. For instance, we will use an additional task to identify the risk preferences of subjects, which may explain why there is heterogeneity in the choice of complexity among senders. In addition, to see if receiver inattention to complex reports is sensible, we plan to vary the incentives in the experiment and to run additional rounds where subjects have the option to pay to reduce complexity.

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Figure 1. Sender choice of complexity by secret number (baseline treatment)

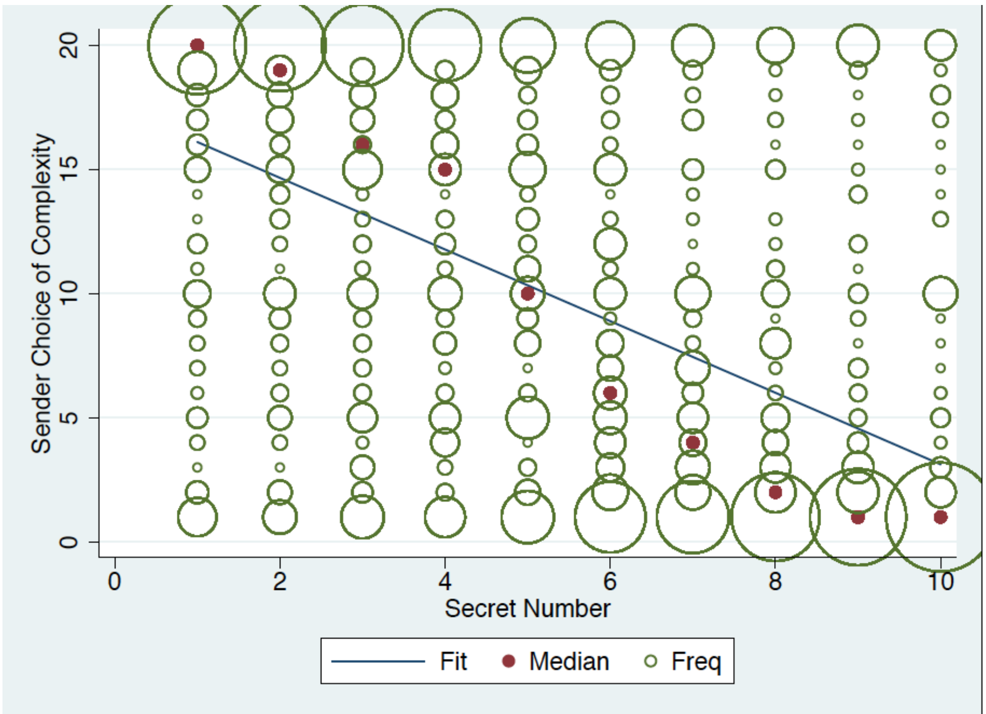


Figure 2a. Sender choice of complexity by round and secret number (baseline treatment)

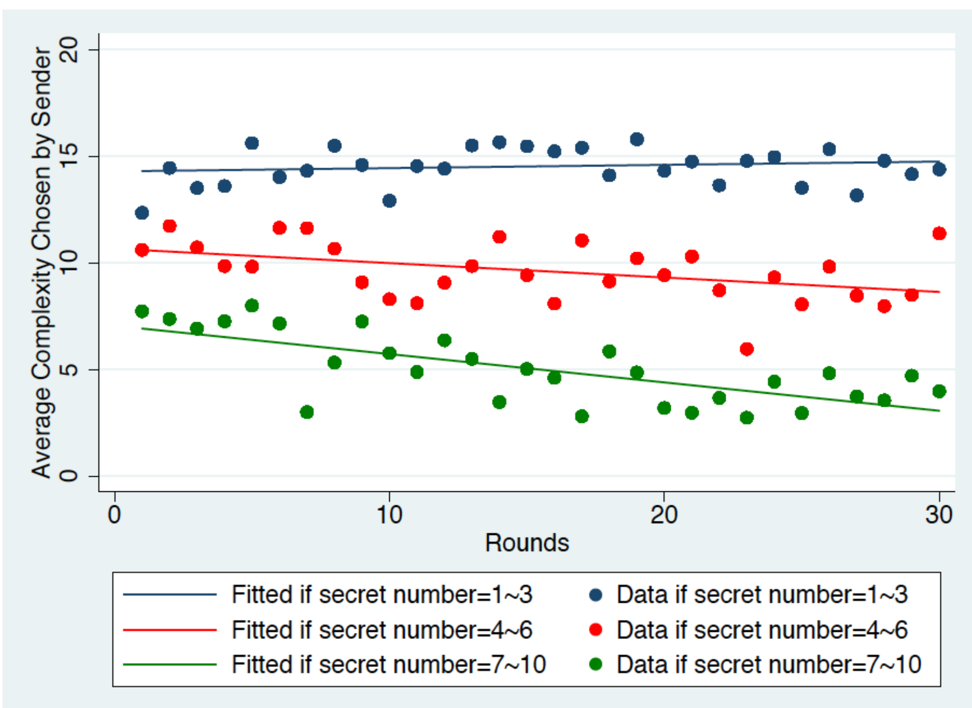


Figure 2b. Sender choice of low complexity (1-5) by round and secret number (baseline treatment)

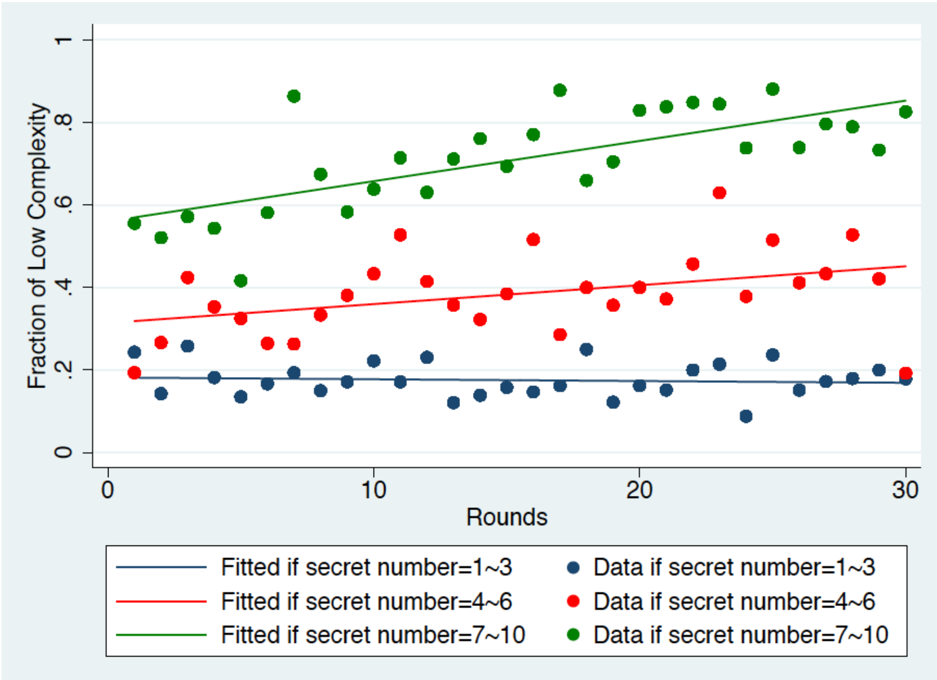


Figure 2b. Sender choice of high complexity (16-20) by round and secret number (baseline treatment)

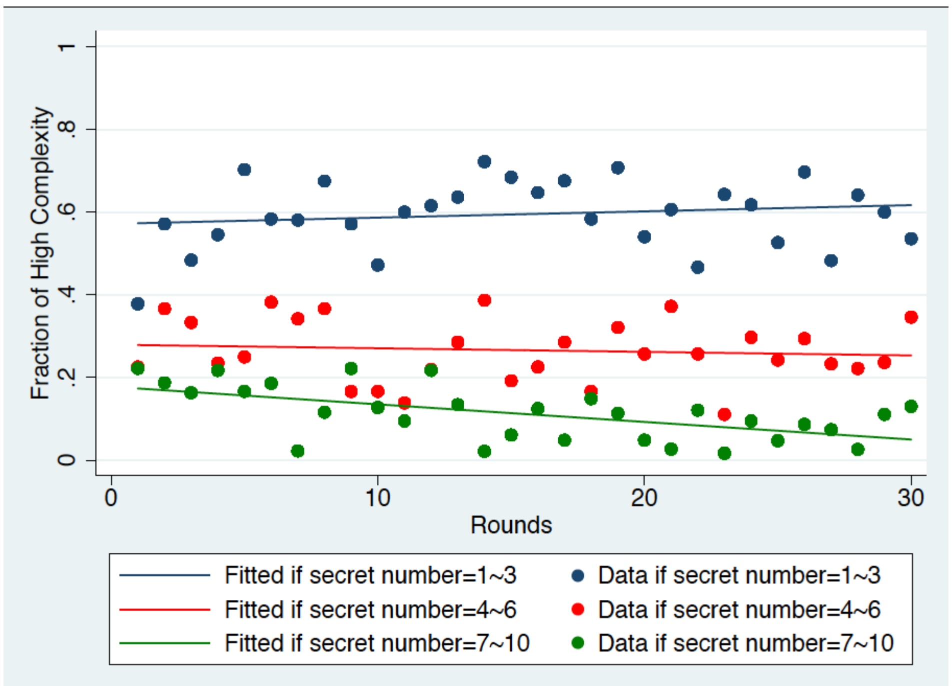


Figure 3a. Average secret number, receiver guess, receiver mistake (guess – secret number), and absolute receiver mistake (absolute value of guess – secret number) by complexity level (baseline treatment)

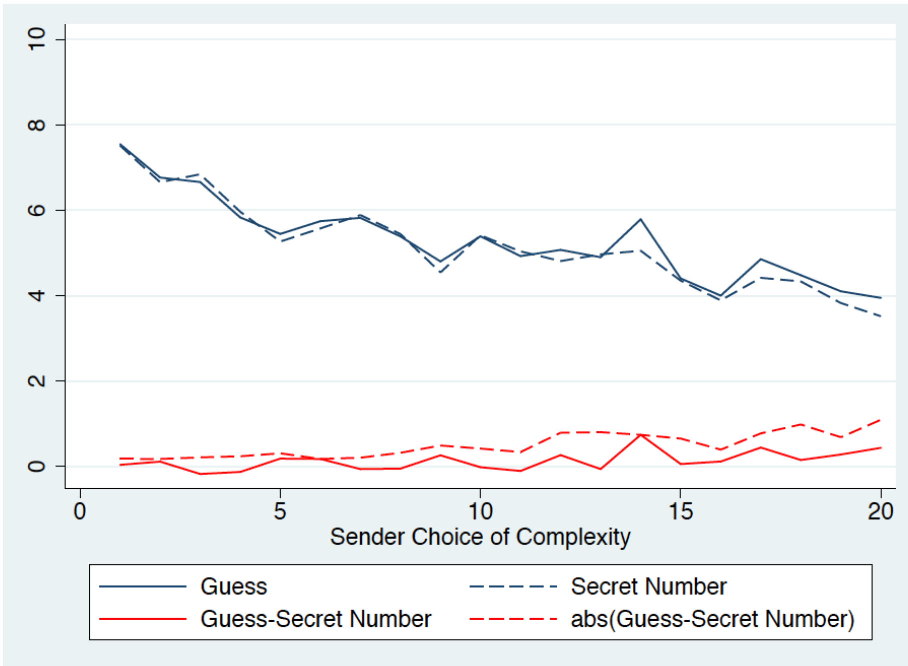


Figure 3b. Average receiver mistake (guess – secret number) by secret number (baseline treatment)

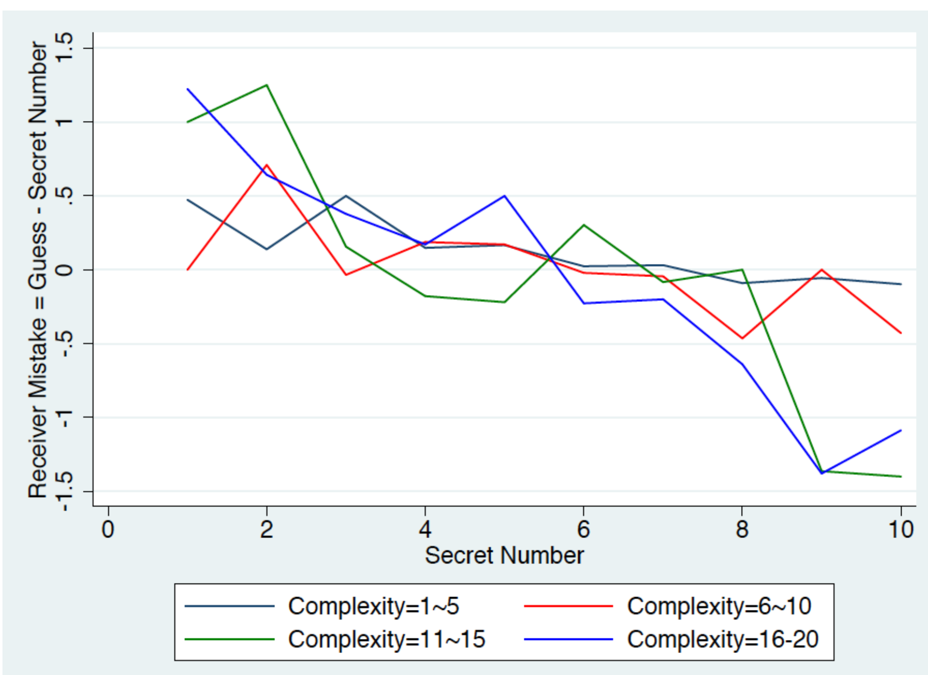


Figure 4a. Average receiver mistake (guess – secret number) by round (baseline treatment)

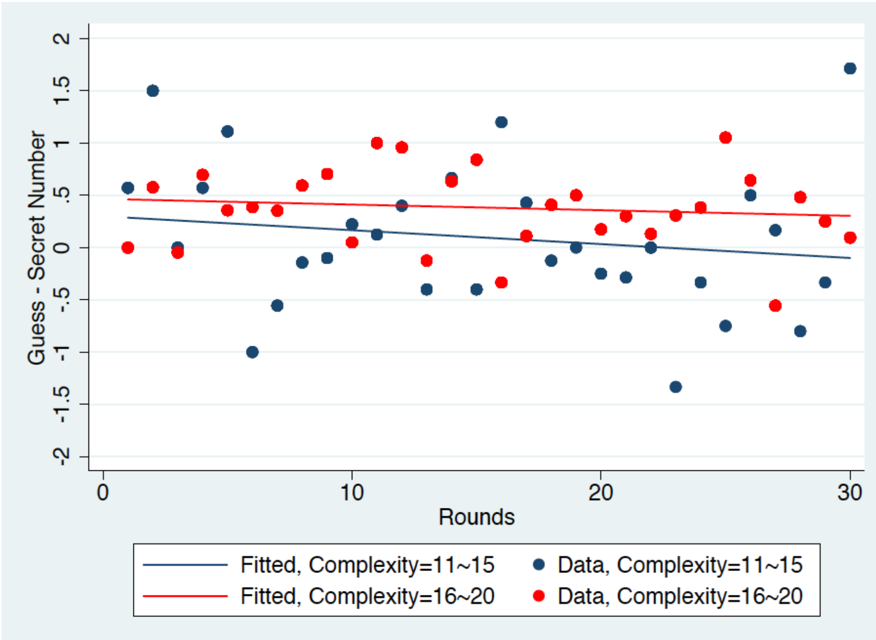


Figure 4b. Average absolute receiver mistake (absolute value of guess – secret number) by round (baseline treatment)

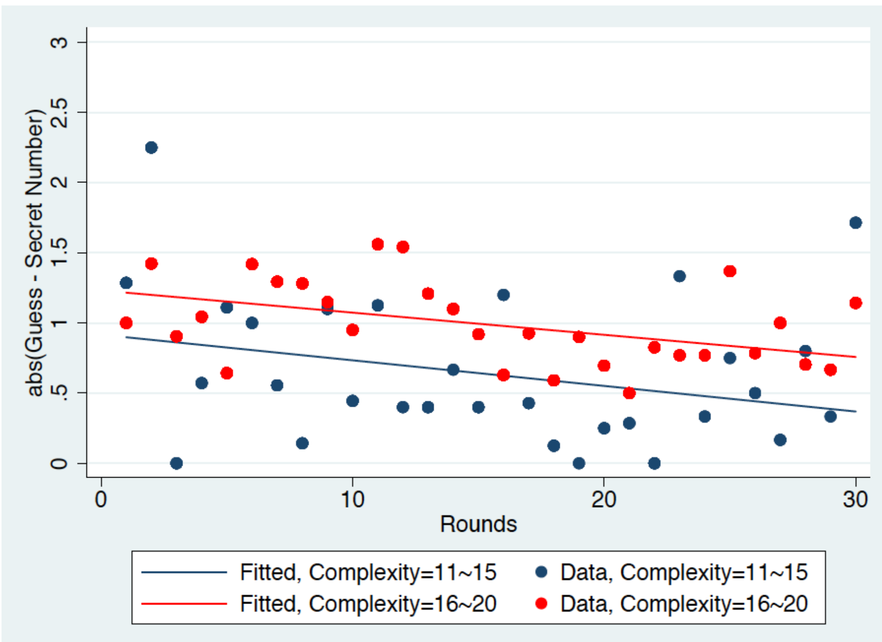
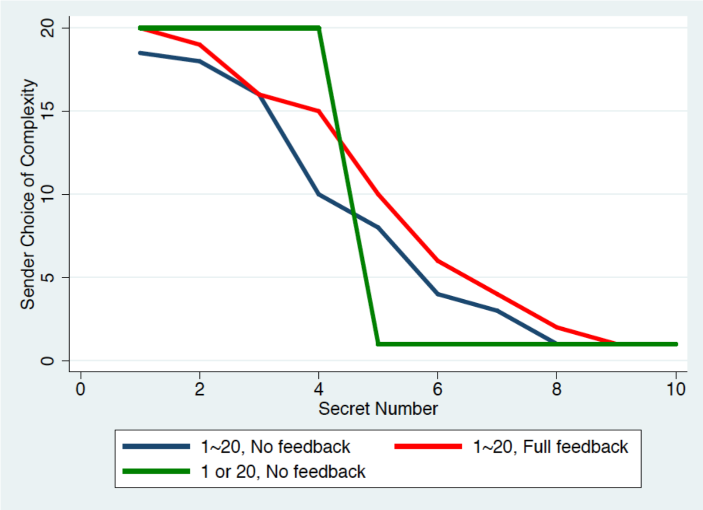
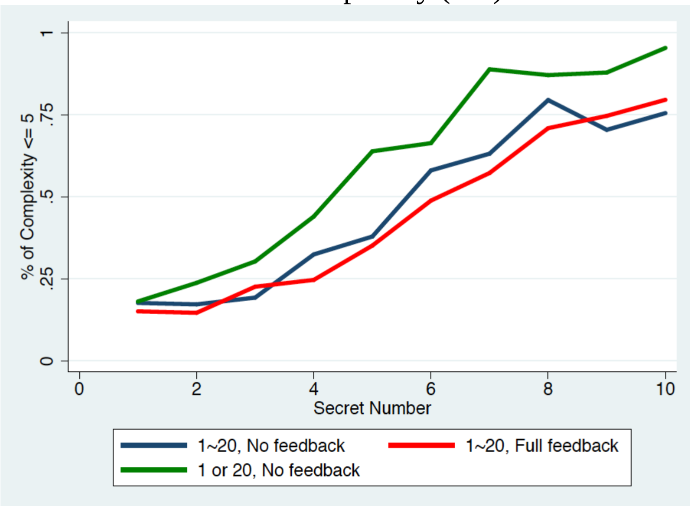


Figure 5. Sender choice of complexity by secret number (all treatments)

Panel A. Median complexity level



Panel B: Fraction low complexity (1-5)



Panel C. Fraction high complexity (16-20)

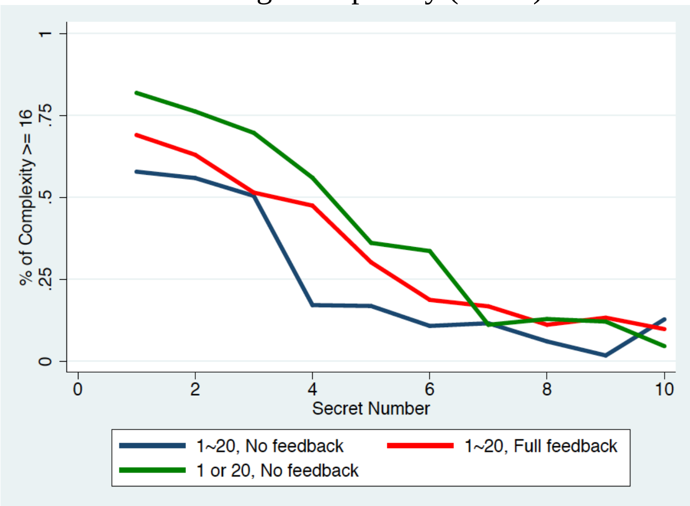


Figure 6: Average secret number and absolute receiver mistake (absolute value of guess – secret number) by complexity level (all treatments)

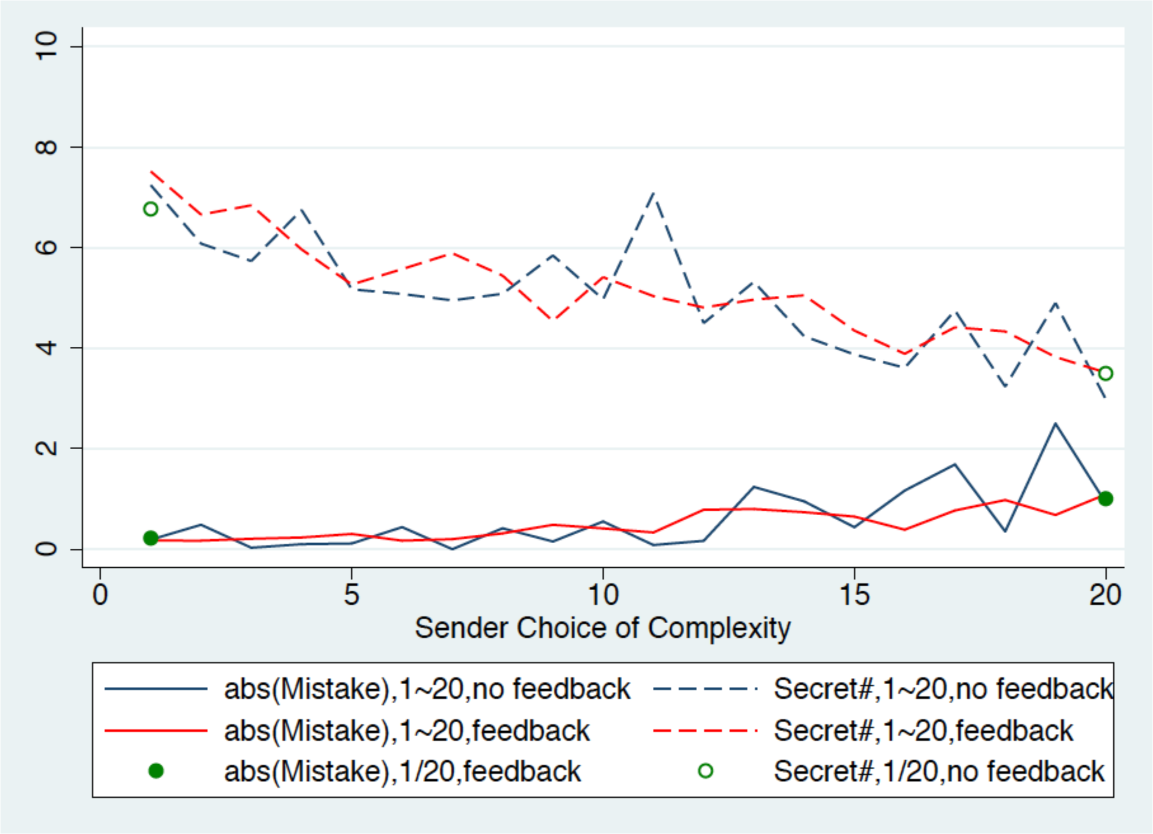


Table 1. Summary statistics (baseline treatment)

Variable	All			Conditional on the receiver submitting a guess before time limit		
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
Session ID (total 15)	4,774	7.472	4.636	4,695	7.472	4.642
Round (1 to 30)	4,774	15.421	8.613	4,695	15.454	8.621
Role (1=sender, 0=receiver)	4,774	0.500	0.500	4,695	0.508	0.500
<u>Sender:</u>						
Secret number (1 to 10)	2,387	5.488	2.903	2,387	5.488	2.903
Sender choice of complexity (1 to 20)	2,387	9.633	7.859	2,387	9.633	7.859
Sender time used (in seconds)	2,387	10.110	7.580	2,387	10.110	7.580
<u>Receiver:</u>						
Receiver's guess (1 to 10)	2,387	5.684	2.914	2,308	5.688	2.913
Receiver time used (in seconds)	2,387	25.858	18.879	2,308	24.690	18.092
Receiver made no decision within time limit (60 s)	2,387	0.033	0.179	2,308	0.000	0.000
<u>All subjects:</u>						
Male	4,020	0.381	0.486	3,952	0.380	0.486
Native English speaker	3,930	0.863	0.344	3,873	0.863	0.344
Undergrad	4,020	0.597	0.491	3,952	0.598	0.490
Had friend(s) in the room	4,020	0.075	0.263	3,952	0.074	0.262

Note: All sessions in this sample give full feedback after each round. The time limit for the receiver is 60 seconds. If a receiver does not make a decision within 60 seconds, time used is coded as 60 and the computer generates a random number between 1 and 10 as the receiver's guess. There is no time limit for the sender.

Table 2. Summary of sender's choice of complexity (baseline treatment)

	Sender choice of complexity (1 to 20)					
Secret number	Mean	Median	Std. dev.	N	Percent high complexity (16-20)	Percent low complexity (1-5)
1	15.560	20	6.648	252	69.0%	15.1%
2	15.081	19	6.525	246	63.0%	14.6%
3	13.544	16	7.141	239	51.5%	22.6%
4	12.877	15	7.028	219	47.5%	24.7%
5	10.577	10	7.038	222	30.2%	35.1%
6	7.878	6	6.827	262	18.7%	48.9%
7	6.819	4	6.654	227	16.7%	57.3%
8	5.132	2	6.083	234	11.1%	70.9%
9	4.892	1	6.491	241	13.3%	74.7%
10	3.939	1	5.758	245	9.8%	79.6%
Total	9.633	8	7.859	2387	33.2%	44.4%

Table 3. Summary of receiver guesses by complexity (baseline treatment)

Sender choice of complexity	Frequency	Average secret number	Average guess	Average (guess - secret number)	Average abs(guess - secret number)
1	677	7.517	7.549	0.032	0.171
2	129	6.659	6.767	0.109	0.171
3	77	6.844	6.662	-0.182	0.208
4	60	5.967	5.833	-0.133	0.233
5	112	5.268	5.446	0.179	0.304
6	47	5.574	5.745	0.170	0.170
7	45	5.889	5.822	-0.067	0.200
8	54	5.444	5.389	-0.056	0.315
9	35	4.543	4.800	0.257	0.486
10	128	5.414	5.391	-0.023	0.414
11	27	5.037	4.926	-0.111	0.333
12	42	4.810	5.071	0.262	0.786
13	30	4.967	4.900	-0.067	0.800
14	19	5.053	5.789	0.737	0.737
15	97	4.351	4.402	0.052	0.649
16	36	3.889	4.000	0.111	0.389
17	48	4.417	4.854	0.438	0.771
18	48	4.333	4.479	0.146	0.979
19	69	3.826	4.101	0.275	0.681
20	528	3.515	3.947	0.432	1.091
Total	2308	5.538	5.688	0.150	0.507

Table 4. Summary of receiver mistakes by secret number and complexity (baseline treatment)

Secret number	Average of (guess - secret number)				
	All complexity levels	Complexity =1 to 5	Complexity =6 to 10	Complexity =11 to 15	Complexity =16 to 20
1	0.975	0.474	0.000	1.000	1.224
2	0.636	0.139	0.710	1.250	0.643
3	0.323	0.500	-0.034	0.156	0.378
4	0.122	0.148	0.188	-0.179	0.172
5	0.190	0.167	0.171	-0.220	0.500
6	0.008	0.024	-0.021	0.303	-0.227
7	-0.027	0.031	-0.044	-0.083	-0.200
8	-0.190	-0.090	-0.464	0.000	-0.640
9	-0.275	-0.056	0.000	-1.364	-1.379
10	-0.246	-0.097	-0.429	-1.400	-1.087
Total	0.150	0.032	0.026	0.116	0.383
Obs.	2308	1055	309	215	729

Table 5. Dynamics of sender choice (baseline treatment)

Dependent Variable	Complexity level (1)	Complexity level (2)	Complexity>=16 (3)	Complexity>=16 (4)	Complexity<=5 (5)	Complexity<=5 (6)
Round	-0.0708***	-0.0715***	-0.000909	-0.000998	0.00612***	0.00595***
	(0.0155)	(0.0138)	(0.001000)	(0.000914)	(0.00103)	(0.000951)
Constant	19.22***	16.70***	0.831***	0.706***	-0.0875**	0.0521*
	(0.688)	(0.477)	(0.0457)	(0.0328)	(0.0412)	(0.0292)
Secret number = 2	-0.636	-0.669	-0.0662	-0.0688*	0.00664	0.0150
	(0.591)	(0.570)	(0.0425)	(0.0401)	(0.0319)	(0.0330)
Secret number = 3	-2.188***	-1.520***	-0.183***	-0.141***	0.0872**	0.0594*
	(0.627)	(0.568)	(0.0438)	(0.0399)	(0.0356)	(0.0349)
Secret number = 4	-2.894***	-3.207***	-0.225***	-0.250***	0.109***	0.119***
	(0.630)	(0.576)	(0.0447)	(0.0415)	(0.0368)	(0.0352)
Secret number = 5	-5.278***	-5.384***	-0.400***	-0.401***	0.220***	0.231***
	(0.620)	(0.558)	(0.0420)	(0.0381)	(0.0386)	(0.0363)
Secret number = 6	-7.779***	-7.491***	-0.508***	-0.498***	0.344***	0.334***
	(0.585)	(0.539)	(0.0375)	(0.0356)	(0.0377)	(0.0356)
Secret number = 7	-8.784***	-8.992***	-0.524***	-0.539***	0.425***	0.438***
	(0.598)	(0.551)	(0.0379)	(0.0357)	(0.0395)	(0.0366)
Secret number = 8	-10.58***	-10.50***	-0.587***	-0.576***	0.569***	0.570***
	(0.573)	(0.547)	(0.0356)	(0.0354)	(0.0371)	(0.0350)
Secret number = 9	-10.81***	-10.75***	-0.567***	-0.570***	0.603***	0.597***
	(0.588)	(0.569)	(0.0364)	(0.0353)	(0.0355)	(0.0354)
Secret number = 10	-11.79***	-11.28***	-0.600***	-0.562***	0.656***	0.637***
	(0.553)	(0.543)	(0.0347)	(0.0346)	(0.0340)	(0.0342)
Subject demographics	Included	Subject fix effects	Included	Subject fix effects	Included	Subject fix effects
Observations	2,387	2,387	2,387	2,387	2,387	2,387
R-squared	0.311	0.523	0.222	0.428	0.261	0.469

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Dynamics of receiver guesses (baseline treatment)

Dependent variable	Receiver guess		Guess - Secret number		abs (Guess - Secret number)	
	(1)	(2)	(3)	(4)	(5)	(6)
Round	-0.0111*	-0.0135**	0.000171	-0.00148	-0.00896***	-0.00943***
	(0.00633)	(0.00634)	(0.00365)	(0.00348)	(0.00333)	(0.00303)
Constant	7.241***	7.355***	-0.0135	0.0689	0.406***	0.369***
	(0.236)	(0.133)	(0.142)	(0.0697)	(0.130)	(0.0605)
Complexity = 6~10	-1.663***	-1.748***	-8.56e-05	0.0105	0.122*	0.0342
	(0.170)	(0.172)	(0.0752)	(0.0803)	(0.0710)	(0.0707)
Complexity = 11~15	-2.301***	-2.389***	0.110	0.0912	0.477***	0.482***
	(0.181)	(0.181)	(0.116)	(0.117)	(0.107)	(0.103)
Complexity = 16~20	-3.026***	-3.170***	0.355***	0.298***	0.789***	0.743***
	(0.127)	(0.127)	(0.0803)	(0.0780)	(0.0728)	(0.0682)
Subject demographics	Included	Subject fixed effects	Included	Subject fixed effects	Included	Subject fixed effects
Observations	2,308	2,308	2,308	2,308	2,308	2,308
R-squared	0.217	0.306	0.023	0.157	0.083	0.269

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Summary of reported belief of sender strategy

Secret Number	Reported belief of sender strategy		Own strategy as a sender		Strategy of all senders in the same session		Correlation (belief, own strategy as a sender)	Correlation (belief, strategy of all senders)
	N	Mean	N	Mean	N	Mean		
1	134	16.119	132	15.289	132	15.616	0.3268***	0.2189**
2	134	15.224	131	15.048	131	14.943	0.3810***	0.1937**
3	134	13.448	118	13.901	118	13.697	0.4653***	0.2808***
4	134	11.358	107	12.193	107	12.446	0.3893***	0.3107***
5	134	9.612	128	10.487	128	10.554	0.4982***	0.3243***
6	134	7.955	123	8.064	123	7.890	0.4215***	0.3285***
7	134	6.097	124	6.940	124	6.846	0.5576***	0.3722***
8	134	3.993	119	4.767	119	4.961	0.4814***	0.3083***
9	134	3.090	124	4.883	124	4.811	0.5184***	0.2870***
10	134	2.597	122	4.080	122	3.988	0.5335***	0.2731***

***p<0.01; **p<0.05.

Table 8. Summary of receiver beliefs (baseline treatment)

Unit of observation = per subject that answered our belief questions

Implied guess = mean of secret number inferred from receiver-reported complexity per secret number
Complex guess = receiver belief of average secret number for the group of complexity
Actual guess = average guess that a receiver reports in actual plays, conditional on the group of complexity

Panel A. Summary statistics

	Secret Number		Implied guess		Complex guess		Actual guess		Time used	
Complexity	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
1~5	134		127	7.981	134	7.664	134	7.125	134	10.172
6~10	134		110	5.238	134	5.675	115	5.563	115	23.685
11~15	134		91	3.591	134	3.896	102	4.839	102	35.847
16~20	134		113	2.369	134	2.545	130	3.954	130	41.520

Panel B. Correlations

Complexity	Correlation (implied guess, complex guess)	Correlation (implied guess, actual guess)	Correlation (complex guess, actual guess)	Correlation (implied guess, time used)	Correlation (complex guess, time used)	Correlation (actual guess, time used)
1~5	0.3837***	0.0259	0.1168***	0.035	-0.0838	-0.2623***
6~10	0.3143***	-0.0215	0.0381	0.0593	-0.0122	-0.1833**
11~15	0.4285***	0.1002	0.2489**	0.0176	0.0346	-0.0334
16~20	0.3544***	0.044	0.3196***	-0.0323	-0.1216	-0.036

*** p<0.01, ** p<0.05, * p<0.1.

Table 9. Regression of receiver beliefs (baseline treatment)

Dependent Variable	Guess		Guess-Secret Number		abs(Guess-Secret Number)	
	(1)	(2)	(3)	(4)	(5)	(6)
Round	-0.00618	-0.00838	0.00146	0.00112	-0.00882**	-0.00824**
	(0.00750)	(0.00760)	(0.00384)	(0.00368)	(0.00356)	(0.00332)
Impliedguess_thisround	0.0328	-0.112	0.0337	0.0303	0.0471*	0.0292
	(0.0494)	(0.0700)	(0.0297)	(0.0476)	(0.0276)	(0.0393)
Complexguess_thisround	0.0783*	0.118**	0.0488*	0.0887**	-0.00223	0.0675**
	(0.0438)	(0.0526)	(0.028)	(0.038)	(0.026)	(0.033)
Constant	6.242***	7.233***	-0.824***	-0.927**	0.00195	-0.417
	(0.531)	(0.579)	(0.319)	(0.381)	(0.299)	(0.336)
Complexity=6~10	-1.301***	-1.659***	0.161	0.266*	0.279**	0.272**
	(0.244)	(0.283)	(0.116)	(0.151)	(0.110)	(0.133)
Complexity=11~15	-1.643***	-2.285***	0.483**	0.623**	0.690***	0.865***
	(0.312)	(0.384)	(0.218)	(0.275)	(0.200)	(0.240)
Complexity=16~20	-2.395***	-3.176***	0.725***	0.919***	0.925***	1.174***
	(0.336)	(0.449)	(0.228)	(0.305)	(0.212)	(0.267)
Subject demographics	included	subject fixed effects	included	subject fixed effects	included	subject fixed effects
Observations	1,634	1,634	1634	1634	1634	1634
R-squared	0.210	0.310	0.021	0.156	0.064	0.243

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Summary statistics (robustness treatments)

Panel A: Complexity level 1 to 20 and no feedback

	All			Conditional on the receiver submitting a guess before time limit		
Variable	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
Session ID (total 5)	1,980	2.939	1.325	1,958	2.936	1.325
Round (1 to 30)	1,980	15.500	8.658	1,958	15.551	8.652
Role (1=sender, 0=receiver)	1,980	0.500	0.500	1,958	0.506	0.500
Sender:						
Secret number (1 to 10)	990	5.451	2.884	990	5.451	2.884
Sender choice of complexity (1 to 20)	990	8.490	7.359	990	8.490	7.359
Sender time used (in seconds)	990	8.547	6.667	990	8.547	6.667
Receiver:						
Receiver's guess (1 to 10)	990	5.595	2.886	968	5.588	2.889
Receiver time used (in seconds)	990	24.399	18.484	968	23.590	17.886
Receiver made no decision within time limit (60 s)	990	0.022	0.147	968	0.000	0.000

Panel B: Complexity level 1 or 20 and no feedback

Variable	All			Conditional on the receiver submitting a guess before time limit		
	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
Session ID (total 5)	2,040	3.029	1.404	2,010	3.026	1.406
Round (1 to 30)	2,040	15.500	8.658	2,010	15.534	8.650
Role (1=sender, 0=receiver)	2,040	0.500	0.500	2,010	0.507	0.500
Sender:						
Secret number (1 to 10)	1,020	5.477	2.874	1,020	5.477	2.874
Sender choice of complexity (1 or 20)	1,020	8.544	9.301	1,020	8.544	9.301
Sender time used (in seconds)	1,020	6.040	4.895	1,020	6.040	4.895
Receiver:						
Receiver's guess (1 to 10)	1,020	5.625	2.871	990	5.625	2.870
Receiver time used (in seconds)	1,020	23.856	21.094	990	22.761	20.436
Receiver made no decision within time limit (60 s)	1,020	0.029	0.169	990	0.000	0.000

Table 11: Summary of sender's choice of complexity (robustness treatments)

Panel A: Complexity level 1 to 20 and no feedback

	Sender choice of complexity (1 to 20)					
Secret number	Mean	Median	Std. dev.	N	Percent high complexity (16-20)	Percent low complexity (1-5)
1	14.382	18.5	7.013	102	57.8%	17.6%
2	14.161	18	6.930	93	55.9%	17.2%
3	13.349	16	7.043	109	50.5%	19.3%
4	9.018	10	6.153	111	17.1%	32.4%
5	8.653	8	6.058	95	16.8%	37.9%
6	6.151	4	5.945	93	10.8%	58.1%
7	5.989	3	6.234	95	11.6%	63.2%
8	3.699	1	5.045	83	6.0%	79.5%
9	4.017	1	4.666	115	1.7%	70.4%
10	4.606	1	6.450	94	12.8%	75.5%
Total	8.490	7	7.359	990	24.3%	46.4%

Panel B: Complexity level 1 or 20 and no feedback

	Sender choice of complexity (1 or 20)					
Secret number	Mean	Median	Std. dev.	N	Percent high complexity (16-20)	Percent low complexity (1-5)
1	16.562	20	7.350	105	81.9%	18.1%
2	15.485	20	8.127	101	76.2%	23.8%
3	14.242	20	8.776	99	69.7%	30.3%
4	11.640	20	9.479	100	56.0%	44.0%
5	7.861	1	9.169	108	36.1%	63.9%
6	7.388	1	9.015	116	33.6%	66.4%
7	3.111	1	6.005	90	11.1%	88.9%
8	3.446	1	6.394	101	12.9%	87.1%
9	3.297	1	6.228	91	12.1%	87.9%
10	1.872	1	3.993	109	4.6%	95.4%
Total	8.544	1	9.301	1020	39.7%	60.3%

Table 12. Summary of receiver guesses by complexity (robustness treatments)

Panel A. Complexity level 1 to 20 and no feedback

Sender choice of complexity	Frequency	Average secret number	Average guess	Average (guess - secret number)	Average |guess - secret number|
1	332	7.244	7.169	-0.075	0.190
2	37	6.081	6.081	0.000	0.486
3	34	5.735	5.765	0.029	0.029
4	20	6.750	6.850	0.100	0.100
5	35	5.171	5.229	0.057	0.114
6	25	5.080	5.520	0.440	0.440
7	21	4.952	4.952	0.000	0.000
8	36	5.083	5.333	0.250	0.417
9	26	5.846	5.692	-0.154	0.154
10	78	4.974	4.756	-0.218	0.551
11	12	7.083	7.000	-0.083	0.083
12	12	4.500	4.500	0.000	0.167
13	25	5.320	5.200	-0.120	1.240
14	21	4.238	4.714	0.476	0.952
15	32	3.875	4.250	0.375	0.438
16	18	3.611	4.000	0.389	1.167
17	16	4.750	4.938	0.188	1.688
18	17	3.235	3.588	0.353	0.353
19	10	4.900	7.400	2.500	2.500
20	161	2.988	3.391	0.404	0.925
Total	968	5.481	5.588	0.106	0.472

Panel B. Complexity level 1 or 20 and no feedback

Sender choice of complexity	Frequency	Average secret number	Average guess	Average (guess - secret number)	Average |guess - secret number|
1	615	6.771	6.787	0.016	0.221
20	375	3.499	3.720	0.221	1.005
Total	990	5.531	5.625	0.094	0.518