Essays on Strategic Naivety and Disclosure of Verifiable Information

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ABSTRACT

The unravelling prediction of the disclosure theory relies on the idea that market forces lead firms (information senders) to voluntarily disclose information about the quality of their products provided the information disclosed is verifiable and the costs of disclosure are negligible. This theoretical prediction requires that consumers (information receivers) hold correct beliefs and, in equilibrium, treat all non-disclosed information with extreme scepticism. Previous research finds that receivers are insufficiently sceptical, i.e. are 'strategically naïve', about non-disclosed information, which leads to the failure of complete unravelling.

This doctoral thesis systematically manipulates features of the decision environment in an experimental sender-receiver disclosure game to examine, a) how naivety responds to the inclusion of naturalistic elements in the experiment such as allowing communication opportunities between receivers and providing a natural context to the experiment, i.e. about the disclosure of a restaurant hygiene rating, and b) whether changing the market structure by introducing competition between senders of information encourages disclosure of information. We further explore whether comparative evaluation of information, which is a feature of introducing competition between senders, attenuates strategic naivety about non-disclosed information.

We find that complete unravelling of information, as predicted by theory, fails to occur in all our settings. Further, providing communication opportunities to receivers and providing a natural context to the experiment does not change the overall amount of information that is revealed by senders. However, we find that manipulating the sender's side, i.e. by introducing competition between senders, increases disclosure of information compared to a setting without competition between senders.

On the receivers' side, we find that strategic naivety is robust to, a) providing communication opportunities to receivers (i.e. consultation), b) adding a natural context to the experiment, and c) providing a setting with comparative evaluation of information, i.e. inferring about missing information in the presence of available information. Interestingly, we find that receivers' welfare improves when we introduce competition between senders despite the presence of strategic naivety. Finally, we find that strategic naivety stems from receivers' miscalibrated beliefs about the revealing behaviour of senders with intermediate and high quality (draws), as opposed to beliefs about the revealing behaviour of senders with low quality (draws).

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CHAPTER 1:

INTRODUCTION TO THE THESIS

1.1. Motivation

Consumers often rely on information about quality to decide which products or services to consume. This is because products are often complex and their quality is hard to judge without experiencing or using them. Therefore, disclosure of product quality by firms can address asymmetric information problems between firms and consumers about quality and also across various other domains. For example, a consumer may decide which air conditioner to buy based on its energy efficiency rating provided by the manufacturer, or the consumer may invest in certain assets based on their credit rating. Unless legislation obliges firms to make quality information public, they have the option to voluntarily disclose (or withhold) this information about their products or services to consumers.

In such cases, the theory of voluntary disclosure of information posits that firms completely disclose the quality of their products provided the information disclosed is verifiable, and the costs of disclosure are negligible (Viscusi 1978, Grossman and Hart 1980, Grossman 1981, Milgrom 1981, for a review see Milgrom 2008). The intuition behind this idea is that, in any equilibrium, all firms that do not disclose information are treated in the same manner (according to the average quality of a non-disclosing firm) by consumers. This incentivises firms that are better relative to others to differentiate themselves by disclosing their information. When this logic is applied iteratively, it leads all firms to disclose their information, except the lowest quality firm – which is indifferent between disclosing and withholding. This mechanism is known as *unravelling*. In equilibrium, consumers treat all non-disclosed information with extreme scepticism and conclude that the products of non-disclosing firms are of the lowest quality.[1, 2]

However, the logic behind unravelling rests on consumer's ability to make adverse inferences about non-disclosed information. One of the reasons unravelling can fail to occur is when consumers are too optimistic about non-disclosed information, given the firm's actual quality level.[3] Recent papers have documented evidence in support of incomplete unravelling of information under voluntary disclosure settings (Jin and Leslie 2003, Jin 2005, Mathios 2000, Benndorf et al. 2015, Luca and Smith 2015) and insufficient scepticism about non-disclosed information, or as Jin et al. (2017) term it, '*strategic naivety*' (Brown et al. 2012, Sah and Read 2017, Deversi et al. 2018).[4]

Brown et al. (2012) show that cold-opened movies, i.e. movies that are withheld from movie critics before their release, increase box-office revenues and, at the same time, show a consistent pattern of fan disappointment. This suggests that movie-goers display limited strategic thinking, which is perhaps because movie-goers do not draw correct inferences about the quality of the

[1] The standard theories in the cheap-talk literature such as Crawford and Sobel (1982) permit firms to lie about their private information. In contrast, the theories on verifiable disclosure of information require firms' messages about their private information to be truthful.

[2] Information can be disclosed either directly by firms (provided that it is verifiable), or via third-parties such as certification agencies. Viscusi (1978) and Grossman (1981) propose certification of verifiable quality information (for example, certifying the percentage of gold in jewellery or mobile phone warranty), to help consumers overcome uncertainty about the quality of the product and ensure that prices reflect true quality (see Dranove and Jin 2010, for a review on quality disclosure and certification).

[3] Unravelling can fail to occur for other reasons such as firms' strategic and dynamic incentives like not disclosing favourable information today to avoid future disclosure when the information may turn out to be unfavourable (Grubb 2011, Marinovic and Varas 2016), or for countersignalling purposes in the presence of multiple quality dimensions (Feltovich et al. 2002).

[4] Theories by Eyster and Rabin (2005) (cursed-equilibrium concept), Mullainathan et al. (2008) (coarse thinking), Gabaix and Laibson (2006) and Heidhues et al. (2016) (shrouded attributes) posit that firms may not reveal their private information if consumers are 'naive' about the quality of non-disclosed information. A related notion of naivety is considered in Cartwright and Patel (2010).

movie from the actions of the production house, i.e. of not screening the movie to critics before its release. Jin and Leslie (2003) examine how a change from a voluntary disclosure policy to a mandatory disclosure policy of restaurant hygiene ratings affect disclosure of health inspection scores (quality) of restaurants and consumer sensitivity to the quality of restaurants. They find that the average quality of restaurants increased after the policy change and consumers became sensitive to information about the quality of restaurants. One plausible explanation for this change in consumer sensitivity is that consumers failed to draw correct inferences from missing information about restaurants' hygiene rating when disclosure was voluntary.[5]

Many of the existing studies mentioned above use field data to study changes in consumer responses and draw conclusions about the unravelling prediction of the theory. However, non-disclosure in the field can be due to various reasons, thereby making it difficult to causally identify the impact of strategic naivety on the disclosure of information by firms. Laboratory experiments offer a way to systematically test theoretical predictions under the conditions posited in economic theory (i.e. information revealed must be true and can be revealed at no cost) and help establish causal links in support of (or against) the theory.

One of the leading experimental papers in this literature, Jin et al. (2017), offers a clear test of the unravelling prediction of the voluntary disclosure theory using a laboratory experiment. They find evidence of strategic naivety in

[5] Another plausible explanation is that consumers were unaware that restaurants were awarded health inspection scores but chose not to display it. Alternatively, consumers can also be inattentive to disclosed information which is modelled by Hirshleifer and Teoh (2003). Under these conditions, i.e. uncertainty about the information that restaurants possess, the unravelling prediction of the voluntary disclosure theory is inapplicable.

verifiable disclosure settings and conclude that complete unravelling fails to occur as receivers are strategically naïve (too optimistic) about information that is not disclosed.[6] Hagenbach and Perez-Richet (2017) also use a laboratory experiment to study the effect of senders' non-monotonic incentive structure on the disclosure of information. They find that senders use vagueness, which takes the form of silence and partial disclosure in their setting, to partially disclose information and effectively exploit receivers. This is especially true when the senders and receivers have misaligned incentives.

Li and Schipper (2018) study strategic thinking in games and examine whether receivers learn the process of iterative reasoning required for unravelling quicker in games with two quality levels compared to games with four quality levels. They find higher unravelling, but not at statistically significant rates, in games with two compared to four quality levels. King and Wallin (1991) and Dickhaut et al. (2003) investigate the impact of vague messages in a double-auction setting on the prices offered by buyers. They find suggestive evidence for strategic naivety, which takes the form of overpricing of products that are accompanied by vague messages from senders. In their experiment, while complete unravelling occurs after many rounds, there are other potential explanations for receivers' behaviour in an auction setting (see Dodonova and Khoroshilov 2009, for a review on behavioural biases in auctions). Overall, this experimental literature finds that complete unravelling often fails to occur as senders use silence or vague messages and effectively exploit receivers' naivety about private information.

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[6] We use the terms "senders/sellers" interchangeably to refer to "firms" and "receivers/buyers" to refer to "consumers".

1.2. Contribution

In this doctoral thesis, we explore the extent to which naivety responds systematically to features of the decision environment, namely the availability of opportunities to communicate with others, providing a natural context to the experimental setting, and changing the market structure by introducing competition between senders of information. In doing so, we aim to contribute to the aforementioned literature in three broad ways. First, this investigation allows us to study the robustness of strategic naivety in face of naturalistic features that exist in field settings which could help markets to unravel. Second, this study indirectly allows us to investigate the nature and roots of strategic naivety by examining questions such as whether alleviating cognitive constraints by providing communication opportunities attenuates strategic naivety. Third, this study allows us to examine conditions or market structures under which we might be more likely to see increased disclosure of information, i.e. with or without sender competition.

Building primarily on the design proposed by Jin et al. (2017), we develop three distinct experimental setups, the details of which are relegated to the following Chapters. All three experiments share the following features. There are two types of players: a sender and a receiver.[7] The sender obtains a private number that is randomly drawn from a set of numbers which are uniformly distributed. The sender must decide whether to reveal this number or withhold it from the receiver. If the sender reveals the number, the receiver

[7] The number of senders and receivers varies across treatments. Chapters 2 and 3, consist of one sender and two receivers. In Chapter 4, the Baseline consists of one sender and one receiver, and the treatment consists of two senders and one receiver.

knows the number with certainty, else, the receiver knows that the sender chose not to reveal the number.

After observing the sender's decision to reveal or not, the receiver must report (or guess in the event of non-disclosure) the value of the number based on the sender's decision and on the probability distribution of the numbers. Since the sender cannot lie or misreport the true number (thereby, mimicking a verifiable disclosure setting), in every sequential equilibrium of the game, all but the senders with the lowest number reveal the number, and the receivers are sceptical about the value of the true number upon observing non-disclosure and, thereby guess the lowest possible number. In our experiment, we observe actions of both players as well as elicit their beliefs, in an incentivised manner, about the actions of the other player. This provides insights into the mechanism driving players' behaviour and specifically, whether beliefs influence, a) receivers' actions and drive strategic naivety, and b) the revealing behaviour of senders and lead to the failure of complete unravelling of information.

In Chapter 2, we manipulate the receiver's side to explore whether allowing receivers to consult with another receiver, before they make decisions, affects their inferences and guesses about non-disclosed information. By doing so, we aim to examine whether consultation, which we conjecture to alleviate or ease cognitive constraints in receivers, attenuates strategic naivety about non-disclosed information. Allowing receivers to consult with another receiver mimics real-life situations where consumption and other economically significant decisions are often made after discussing with others.

In Chapter 3, we examine whether strategic naivety is an artefact of laboratory experiments that are abstract and neutrally framed and whether it is attenuated when a natural context is provided to the experimental setup. In doing so, we also contribute to the methodological debate in experimental economics around the use of context in experiments and analyse whether 'in-context' and 'context-free' experiments generate different results with regard to the behaviour and beliefs of players in disclosure games. The context we use in our experiment is regarding the disclosure of food hygiene ratings of restaurants which are commonly observed in restaurants and eateries in the United Kingdom. We conjecture that this context would exemplify the incentives of high quality restaurants to disclose information and help attenuate strategic naivety about non-disclosed information among receivers.

In Chapters 2 and 3, we find that complete unravelling fails to occur in all our treatments. Receivers are strategically naïve about non-disclosed information even after we allow them to consult with others before they make decisions and inferences about the non-disclosed information. This indirectly suggests that alleviating cognitive constraints by providing communication opportunities to receivers does not seem to attenuate naivety. Receivers in 'context-free' and 'in-context' experiments do not make significantly different decisions or form significantly different inferences about non-disclosed information. These results suggest that strategic naivety about non-disclosed information may be a strong contributor to the failure of complete unravelling of information in the field. The robustness of strategic naivety among information receivers brings the policy of voluntary disclosure of information under scrutiny. This is because the strength of the policy is based on the intuition that it can solve asymmetric information problems between senders and receivers of information and restore efficiency in markets.

In Chapter 4, we investigate whether manipulating the sender's side by introducing competition between them and changing the market structure increases disclosure of information, and affects welfare of receivers in our setting. We also examine whether comparative evaluation of missing information in the presence of disclosed information, which can be a feature of introducing competition between senders, attenuates strategic naivety about non-disclosed information in receivers. In Chapter 4, we show that changing the market structure by introducing competition between senders increases disclosure of information and leads to higher welfare for receivers. This result is especially intriguing because we do not find evidence for a significant improvement in receivers' ability to make correct inferences about non-disclosed information. Interestingly, as also in earlier Chapters, we find that receivers have miscalibrated beliefs about the revealing behaviour of intermediate and high quality senders as opposed to the revealing behaviour of low quality senders. This implies that receivers do not sufficiently infer that the number withheld by a sender must be low.

In Chapter 5, we summarise the key contributions and the main findings of the doctoral thesis, highlight the limitations of our research, and offer concluding remarks with some directions for future research.

CHAPTER 2:

STRATEGIC NAIVETY IN EXPERIMENTS: THE ROLE OF COMMUNICATION

2.1. Introduction

In day-to-day life, individuals often turn to professionals, friends, and family for advice before they make important decisions. For example, a couple consults with each other, or with a financial consultant before deciding on important financial matters. Consulting with others can have consequences on the quality of decisions made - it can improve or worsen the decisions made by individuals (Iyengar and Schotter 2008, Charness et al. 2010, Isopi et al. 2014). Individuals are often required to make decisions and inferences not only about information that is readily available to them but also about the informational content that is contained in the act of non-disclosure by a potential sender of information (Grossman and Hart 1980, Grossman 1981, Milgrom 1981).

In this Chapter, we examine whether the opportunity to consult with others before making decisions and inferences about non-disclosed information has an impact on the extent of strategic naivety. By strategic naivety we mean that individuals do not sufficiently infer that non-disclosure implies bad news. This investigation would provide insights into the robustness of strategic naivety about non-disclosed information and its effects on unravelling of information as documented by Jin et al. (2017) and Sah and Read (2017).

The social psychology and experimental economics literature has explored differences between decisions made by individuals in isolation versus the decisions made by individuals after receiving advice from peers. There is evidence showing that individuals change their behaviour in response to advice from peers in coordination games (Schotter and Sopher 2003), public good games (Chaudhuri et al. 2006), ultimatum games (Schotter and Sopher 2007), social-learning situations (Çelen et al. 2010), investment games (Bougheas et al. 2013, Ambuehl et al. 2018), and ambiguity tasks (Charness et al. 2013). Schotter and Sopher (2003) find that advice is more crucial than observing history of others' actions for the creation and evolution of social conventions regarding coordination. In a laboratory social-learning situation and strategic games such as the *p*-beauty contest game, Çelen et al. (2010) and Kocher et al. (2014) find that advice from peers improves performance more than learning from observation, and advice also increases subjects' welfare in a laboratory social-learning situation.[8]

The idea behind improvement in performance by individuals after receiving advice from peers is put succinctly by Schotter (2003). He claims that advising others or receiving advice from others makes decision-makers think about the problem at hand differently than in the absence of the advice. Iyengar and Schotter (2008), and Charness et al. (2010) also find that advice (or consultation) helps individuals learn better about the decision problem and helps overcome some anomalies that individual decision-makers are otherwise prone to. On the contrary, Isopi et al. (2014) find that consultation worsens individual decision-making in situations where the solution to a problem has low demonstrability.[9] We contribute to this literature by investigating the effect of

[8] These studies are a subset of the broader literature on peer effects in labour productivity experiments (Falk and Ichino 2006, Eriksson et al. 2009), gift-exchange games (Gächter et al. 2012, Gächter et al. 2013), sequential dictator games (Cason and Mui 1997, Cason and Mui 1998), experiments on choice under uncertainty (Cooper and Rege 2011), and coordination games (Cartwright and Singh 2018).

[9] The solution to a problem has 'low demonstrability' if the solution cannot be identified by individuals using basic reasoning or conceptual system (ex: logic or mathematics).

consultation on individual decision-making in a setting where individuals are required to infer from non-disclosed information.[10]

The potentially beneficial effects of consultation on decision-making can be attributed to differences in *beliefs* held by individuals who do and do not consult. We explore the role of beliefs because Chaudhuri et al. (2006) and Schotter and Sopher (2007) provide evidence suggesting individuals who consult construct different beliefs about the actions of the other player relative to the individuals who make decisions in isolation. Based on the literature, we conjecture that individuals who consult form better, i.e. more accurate, beliefs about the actions of the other player. This is because consulting and receiving advice from peers may help individuals reason better about the game, help understand the logic of unravelling, and think about the problem at hand in the right way, i.e. as per the correct reasoning of the game.

In addition to examining whether consulting in a strategic task enables players to form accurate beliefs and inferences about the actions of the other player, we also contribute to the literature by examining whether players best respond to their beliefs. Costa-Gomes and Weizsacker (2008) find that subjects do not always best-respond to their beliefs about the strategies of the other player in almost half of the 14 two-person normal form games that subjects play in their

[10] A subjects' earnings depend only on her or his own choices in the consultation literature mentioned above. On the contrary, the literature on group decision-making differs from this because the earnings of group members depend on the decision of the group as a whole. The literature on group-decision making finds that groups are more rational and strategic than individual decision-makers and are better at constructing realistic beliefs about the strategy of the other player (Robert and Carnevale 1997, Bornstein et al. 2004, Cooper and Kagel 2005, Kocher and Sutter 2005, Charness et al. 2007, Kugler et al. 2007, Song 2009, Sutter et al. 2013, for a review see Charness and Sutter 2012).

experiment. On the other hand, Rey-Biel (2009) find that subjects in fact do best-respond to their beliefs in 10 simpler, two-person normal form games.

To investigate whether consultation can attenuate strategic naivety about non-disclosed information, we design an experiment consisting of two treatments: a 'No Consultation' treatment and a 'Consultation' treatment. The basic design of the experiment is built on the experiment in Jin et al. (2017) with some variations, details of which are mentioned in the experimental design section of this Chapter. The setup of our experiment is as follows: There are three players - one information sender and two information receivers. The sender obtains a private number (for example, this could be the firm's quality level in field settings) randomly drawn from a uniformly distributed set of numbers. He then decides whether to reveal this number to the receivers or stay silent. After observing the sender's decision to reveal or not, each receiver must make a report about the private number. If the sender has revealed the number, he cannot misreport it (mimics legal regulations relating to, for example, truth-in-advertising laws), and both receivers know the number with certainty. Else, the receivers must make a guess about the true number based on the sender's decision not to reveal the number, and on the probability distribution of the numbers, which is common knowledge.

In our No Consultation treatment, the two receivers do not have the opportunity to consult with each other before entering their reports individually. In our Consultation treatment, the two receivers are given the opportunity to consult with each other before entering their reports individually. The sender and the receivers have a conflict of interest as the sender gets a higher payoff if each of the receivers reports a higher number, and each of the receivers gets a higher payoff when her individual report is closer to the true number. The payoff structure of the sender and the receivers is such that in every sequential equilibrium of the game, senders always reveal their information, with the possible exception of the sender with the lowest number (who is indifferent between revealing or not), and the receivers make adverse inferences upon observing non-disclosure, and thereby, guess the lowest possible number.

We find that complete unravelling fails to occur in both our treatments. That is, a large proportion of senders with a number higher than the lowest possible number choose not to reveal information to the receivers. We also find that there is no statistically significant difference between the reports of the receivers in the Consultation treatment and the reports of those in the No Consultation treatment. Regarding beliefs that we elicit in the experiment, we observe that receivers' beliefs about the non-disclosed number are significantly higher relative to the average actual non-disclosed number by senders in both treatments. Overall, we find that consultation neither leads to lower reports nor reduces naivety (or in other words, increases scepticism) about non-disclosed information.

The rest of this Chapter is organised as follows. Section 2.2 outlines the methodology including the disclosure game and the experimental design. Section 2.3 reports the results and examines the role of beliefs in the decisions made by the players. Section 2.4 offers a brief discussion of the results and concludes the Chapter.

2.2. Methodology

In this section, we present an overview of the disclosure game and a detailed discussion of the experimental design.

2.2.1. The Disclosure Game

We study a three-player disclosure game which consists of one information sender and two information receivers. The basic structure of the game is as follows.

At $t = 0$: Computer determines the true private number which is taken from the set {1, 2, 3, 4, 5} with a uniform probability. The probability distribution is common knowledge to the sender and the receivers.

At $t = 1$: The sender observes the true private number with certainty and decides to reveal or withhold the private number to/from the receivers. The sender's decision is common to both receivers. If the sender chooses to reveal the number, he cannot be vague or untruthful.

At $t = 2$: The receivers *ex ante* know only the probability distribution of the private numbers. They observe whether the sender reveals or not. If the sender reveals, the receivers observe the true private number with certainty. If the sender does not reveal, receivers do not observe anything, except that the sender chose not to reveal the true private number.

(In the Consultation treatment, receivers can communicate with each other after observing sender's decision and before proceeding to $t=3$.)

At t = 3: The receivers must individually take an action from the set {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. We call this action as *reporting* the private number or *guessing* the private number in case the sender chooses not to reveal the number.

The sender's payoff is given by the following function:

$$U_s = \frac{(110 - 20\,|5 - \text{receiver 1's report}|^{1.4}) + (110 - 20\,|5 - \text{receiver 2's report}|^{1.4})}{2}$$

The function is concave, independent of the actual private number, and monotonically increasing in the report of receiver 1 and receiver 2. The payoff of receiver 1 and receiver 2 is given by the following function, where *i* = receiver 1 or 2:

$$U_{Ri} = 110 - 20\,|\text{actual private number} - \text{receiver } i\text{'s report}|^{1.4}$$

This function is concave in the receiver's own action and reaches its peak when the receiver's report is equal to the true private number. The payoffs are such that there is a conflict of interest between the sender and the receivers when the state is low, i.e. the sender obtains a higher payoff when each of the receivers reports a higher number, whereas, each of the receivers obtains a higher payoff when her report is closer to the true number. These specifications are akin to the ones used by Jin et al. (2017).

Milgrom (1981) shows that when the receivers' action space is sufficiently rich[11], the sequential equilibrium of the disclosure game is such that

[11] For the equilibrium to hold, the action space needs to be sufficiently rich so that each receiver can report a number that is as close as possible to her preferred number, given the true number, upon observing non-disclosure. In our experiment, if the action set coincides with the set of private numbers, {1, 2, 3, 4, 5}, draws 3, 4, 5 will unravel, but draws 1 and 2 may be withheld from receivers. Allowing action 1.5 ensures that a sender with draw 2 also reveals the draw in equilibrium. The intuition of the equilibrium is explained in the main text.

the sender always reveals the true private number, unless it is the lowest number, and each receiver takes an action corresponding to the lowest possible private number, i.e. 1, upon observing non-disclosure. If the sender reveals the number, then the receivers obtain the highest payoff when they choose the action corresponding exactly to that private number. This is the only sequentially rational equilibrium of this game.

The intuition behind the sequential rational equilibrium is the following: Suppose that no sender reveals their private number, then receivers should guess a number that is as close as possible to the mean of the non-disclosed numbers, i.e. number 3 in our game. However, senders with private numbers 4 and 5 can profitably deviate by disclosing their number and differentiating from the lower numbers, i.e. 1, 2, and 3. The receivers, anticipating this, will then revise their guess about the non-disclosed number to the average between 1 and 3, i.e. 2. This therefore, incentivises the sender with the private number 3 to reveal the number and differentiate from the lower numbers, i.e. 1 and 2.

In the final step, receivers revise their guess downwards anticipating the non-disclosed numbers to be either 1 or 2. Therefore, the option to guess a number between 1 and 2, for instance, 1.5, incentivises the sender with the private number 2 to disclose it, thereby leading to complete unravelling. The sender with the lowest number, i.e. 1, is indifferent between revealing and not in equilibrium as the payoffs for this sender are going to be the same, given receiver's guess in equilibrium. The last step of reasoning is also why the action space of receivers needs to be sufficiently rich in order to allow for complete unravelling.

2.2.2. Experimental Design

Our experiment consists of two treatments that differ in whether receivers are given the opportunity to consult with each other. We call the treatment where receivers are not given the opportunity to consult as the 'No Consultation' treatment and the one where they are given the opportunity to consult as the 'Consultation' treatment.[12] In both treatments, subjects complete 30 rounds (divided into 6 blocks with 5 rounds in each block) of the disclosure game described in section 2.2.1.

At the beginning of the experiment, the experimental instructions are read aloud publicly to ensure common knowledge. After the brief instruction period (see Appendix 2.B), the subjects answer control questions that gauge their understanding of the main features of the game. Subjects are then assigned to be either information senders or information receivers, and they remain in these roles for the entire duration of the experiment. Two receivers are paired together, and these pairs remain fixed for the entire duration of the experiment. Each receiver pair is randomly matched with a different sender in each round to maintain the one-shot nature of the game, and to minimise any reputation effects that might affect disclosure decisions. We pair two receivers together in both No Consultation and Consultation treatments to ensure that all other elements of the experiment are kept constant across the two treatments.

In each round, and for every matching of a sender with a pair of receivers, the computer program generates a 'private number' from the set $\{1, 2, 3, 4, 5\}$.[13]

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[12] The 'No Consultation' treatment is a conceptual replication of the 'No Feedback' treatment in Jin et al. (2017).

[13] We randomly generated a series of 30 private numbers corresponding to the 30 rounds of the game for each sender in a session and kept the series constant across all sessions to ensure comparability.

Each of these numbers have an equal probability of being drawn. Both senders and receivers know the probability distribution over the set of private numbers. The computer program then sends the private number to the sender. After receiving this number, the sender decides between 'Reveal' and 'Do not reveal' the private number to the receivers. If the sender chooses to 'Reveal' the private number, the receivers see this message: "The number I received is:" and then the actual private number. If the sender decides 'Do not reveal', the receivers see the message: "The number I received is:" followed by a blank and the receivers are aware that the sender chose not to reveal the private number.[14]

After observing the message from the sender, the receivers report (or guess, in the event of non-disclosure by the sender) the value of the private number. In the Consultation treatment, before the receivers report the value of the private number, they see a computerised chat program to consult with the other receiver. We inform receivers that they can use the chat program to get help from or offer help to the other receiver about the report they want to submit. Receivers are given 60 seconds to chat with each other, but can leave the chat program by clicking on the 'Leave chat' button if they wish to proceed to the decision-making stage before the 60 seconds lapse.

The receivers then enter their reports about the value of the private number individually, i.e. receivers do not have to agree on a report to submit jointly. Receivers can report any number from the following {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. Note that receivers can report numbers that are not in the actual

[14] These design features ("The number I received is:", type space, payoffs, private number) are borrowed from Jin et al. (2017) and from the cheap-talk literature Cai and Wang (2006) and Wang et al. (2010).

set of private numbers (i.e., 1.5, 2.5, 3.5 and 4.5). These numbers have been added to the action space to make it sufficiently rich so that the sequential equilibrium involves complete unravelling, i.e. unravelling of all numbers with the possible exception of the lowest number, i.e. 1. We inform receivers of the richness of the action space by telling them that they can use the numbers {1.5, 2.5, 3.5, 4.5}, for example, if they are unsure about the value of the private number.

The incentives are such that the sender earns more when each of the receivers makes a higher report, and each of the receivers earn more when their own reports are closer to the true private number implying that the money earnings of the receivers are not interdependent. The payoffs in the experiment are derived using the payoff functions mentioned above. Subjects did not have to know how to interpret the functional forms because the payoffs were shown in two tables (see Table 2.2A in Appendix 2.A).

We do not provide any feedback to senders or receivers after each round, i.e. senders learn nothing about the reports of the receivers and receivers learn only the disclosed number or that the sender did not disclose the number. This is equivalent to the 'No feedback' treatment in Jin et al. (2017). We depart from Jin et al. (2017) by eliminating role reversal and eliciting beliefs every 5 rounds instead of eliciting them only once at the end of the 30 rounds. We also elicit beliefs in an incentivised manner. We eliminate role reversal as it makes subjects play more rationally and strategically, and it changes the nature of the same game played without role-reversal (Burks et al. 2003, Weimann and Richmann 2003). Jin et al. (2017) find failure of complete unravelling despite role reversal and we seek to examine whether consultation, as opposed to role reversal, makes subjects play more rationally. Further, since we match one sender and a pair of receivers together in every round of the game and keep receiver pairs fixed for the whole experiment, it complicates the implementation of role reversal as there would be more senders than receivers in alternate rounds. We elicit beliefs more frequently than in Jin et al. (2017) as we want to examine the mechanism that drives better (or worse) decisions through consultation.

At the end of each block, senders (receivers) are asked to state their beliefs about the actions of the receivers (senders). We ask senders what percentage of receivers they think guessed 1 or 1.5, 2 or 2.5, 3 or 3.5, 4 or 4.5, and 5 when a number was not disclosed. We ask receivers what percentage of senders they think revealed when the true number was 1, 2, 3, 4, and 5.[15] To incentivise the belief elicitation questions, we adopt the linear scoring rule used by Kugler et al. (2007) with slight modifications.[16] Using responses of the receivers to these belief questions, and applying Bayes' rule, we infer what the receivers should have guessed based on the beliefs they hold about the sender's strategy. Similarly, using the sender's responses to these questions, we infer what they believe the receivers would guess upon observing non-disclosure. We examine whether subjects hold correct beliefs about the action of the other player, and whether they best respond to their beliefs.

At the end of the final block, two out of six blocks are chosen at random for payment. Subjects are paid for the decision-making stage of one of the blocks, and for the accuracy of their beliefs about the actions of the other player

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[15] The belief elicitation questions are relegated to the experimental instructions in Appendix 2.B.

[16] Each belief question at the end of the block was incentivised using the following rule: $70 - |\text{Correct Answer} - \text{Subject's Answer}|$. We use this rule because it is simple and less time consuming to explain to the subjects while still ensuring we elicit the mean belief of the subject.

in the other block. The reason we do not pay the same blocks for both the decision-making stage as well as the accuracy of the belief is to avoid hedging actions with responses to the belief questions. Subjects answer a socio-demographic questionnaire at the end of the final block and questions about reciprocity, risk and cognitive abilities. We use questions from Falk et al. (2016) to elicit risk preferences, and positive and negative reciprocity. We also use the Cognitive Reflection Task questions (Frederick 2005) in the post-experimental questionnaire.

We used a between-subjects design and ran the experiment at the Centre for Decision Research and Experimental Economics laboratory at the University of Nottingham. Subjects were recruited using ORSEE (Greiner 2015) and the sessions were run on z-Tree (Fischbacher 2007).[17] Table 2.1 provides an overview of the treatments. The average payment was £13 (converted at the rate of 75 points = £1) which comprised of a fixed participation fee of £4, and an additional amount based on the decisions in the blocks chosen for payment.

Table 2.1: Overview of treatments

	No Consultation	**Consultation**
Number of sessions*	6	6
Number of senders	34	34
Number of receiver pairs	34	34
Number of rounds	30	30
Average minutes per session	70	90
Total number of subjects	102	102

*Note: Each session comprised of 15-18 subjects and was one matching group.

[17] The sample summary statistics are relegated to Table 2.1A in Appendix 2.A.

2.3. Results

Overall, we find that the extent of unravelling in both the No Consultation and Consultation treatments is not significantly different and far from complete. We also find no effect of Consultation on the guesses of non-disclosed numbers by receivers. When we look at the beliefs of receivers, we find that receivers in both treatments are insufficiently sceptical about non-disclosed information. We conclude that consultation does not lead to lower reports or attenuate naivety about non-disclosed information.

2.3.1. Analysis of senders' behaviour

Panel A in Table 2.2 summarises the revealing rates by senders in both treatments. In general, we see that senders are more likely to reveal higher draws. When the draw is 4 or 5, the revealing rate is over 80% in both treatments. The revealing rate drops below 70% when the draws are 2 and 3 and the percentage of disclosure is nowhere close to the equilibrium prediction of full unravelling. We perform a non-parametric test on the average revealing rate of each sender (excluding private number 1 from the test because the senders with draw 1 are indifferent between revealing and not revealing in equilibrium). We find that there is a stark deviation from the theoretical prediction of full unravelling in both treatments even when we allow for a 10% error rate from the equilibrium prediction (Wilcoxon rank-sum test p=0.000 for both treatments).[18]

[18] We did not provide any feedback to senders between rounds in the experiment. Therefore, we consider each sender as an independent unit of observation and run all tests at the subject level for the senders. Receivers observed the sender's decision and they were randomly re-matched with a different sender after each round. Therefore, we conduct all tests for receivers at the session level, which is also a matching group and is an independent unit of observation. The

Table 2.2: Summary of senders' disclosure decisions

Panel A				
Variables	**No Consultation**		**Consultation**	
	N	**% revealed**	**N**	**% revealed**
Draw = 1	214	8.41	214	6.54
Draw = 2	266	27.44	263*	33.08
Draw = 3	186	63.44	186	69.35
Draw = 4	180	83.89	183*	84.15
Draw = 5	174	90.23	174	87.36

Panel B		
	No Consultation	**Consultation**
Total non-revealed draws	503	484
Average non-revealed draw	1.962	1.960

Note: In Panel A, the column "% revealed" reports the average revealing rate across all senders when they observed that particular draw. In Panel B, we report the total number of non-revealed draws and the average draw when senders did not disclose the draw in the respective treatments. *The number of draw 2 and draw 4 vary across treatments as we made changes to the sessions after running the first (pilot) session.

Figure 2.1 shows the average revealing rate of senders across blocks by draw in both treatments. We see that none of the revealing rates are significantly different across treatments (Wilcoxon rank-sum test p=0.687 for Draw 1, p=0.791 for Draw 2, p=0.660 for Draw 3, p=0.481 for Draw 4, p=0.378 for Draw 5). This reflects in the average non-disclosed number which is strikingly similar across treatments, i.e. 1.962 in the No Consultation treatment and 1.960 in the Consultation treatment, as also shown in Panel B in Table 2.2. This difference between treatments is not statistically significant (Wilcoxon rank-sum test p=0.564). The average revealing rate of senders across treatments is also not significantly different (Wilcoxon rank-sum test p=0.475).

paired tests conducted to compare the treatment averages of receivers with senders also consider each session as the independent unit of observation (for example: comparing the average non-disclosed number with the average guess of the non-disclosed number across treatments).

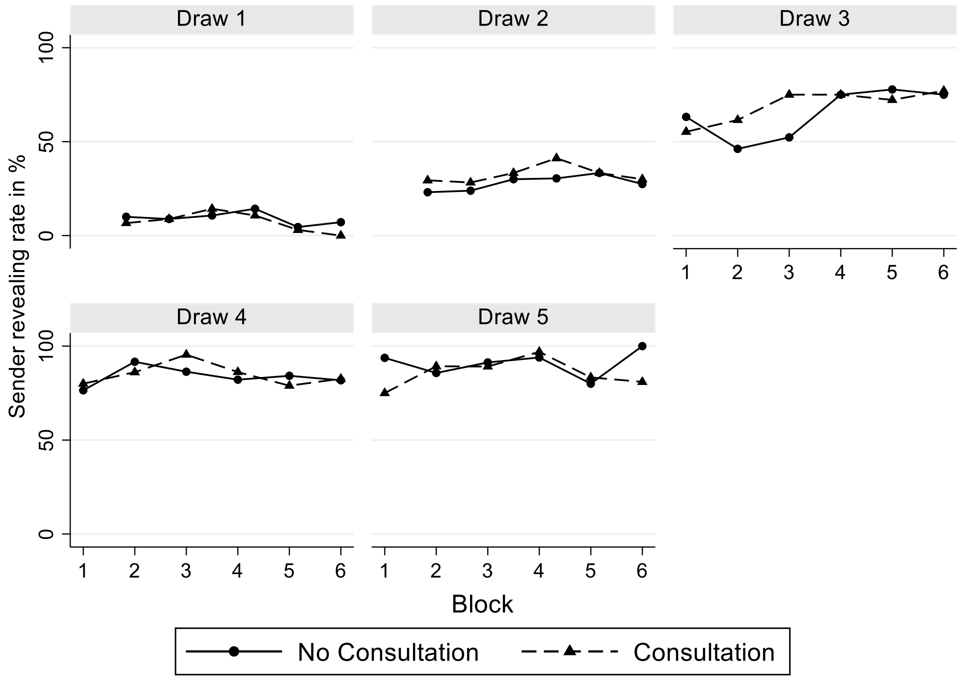

Figure 2.1: Sender revealing rate by draw across blocks

We also analyse the differences in revealing behaviour of senders across the No Consultation and Consultation treatments using regression analysis. Table 2.3 presents the marginal effects from Probit estimations with standard errors clustered at the subject level, which is an independent unit of observation. The dependent variable takes values 1 if the sender's decision is to reveal, and 0 otherwise. The independent variable of interest is the treatment dummy which takes the value of 1 for senders in the Consultation treatment, and 0 for the senders in the No Consultation treatment.

Model I (column I) is estimated without any socio-demographic controls. The sender's decision to reveal the draw is regressed on the treatment dummy, the individual draw dummies, and on the number of the round. The main result is that the revealing rate of senders in the Consultation treatment is not significantly different from the revealing rate of senders in the No Consultation treatment (p=0.730). The estimates for the draw coefficients are statistically significant which implies that senders reveal higher draws significantly more than draw 1 (p=0.000 for all 'Draw' coefficients). We see that senders reveal more over rounds, although this is only marginally significant (p=0.077) and is not robust across specifications.

Table 2.3: Regressions on senders' behaviour

	Sender reveals? (0/1)		
Variables	**I**	**II**	**III**
Consultation treatment	0.014	0.005	0.008
	(0.041)	(0.039)	(0.036)
Draw=2	0.243***	0.229***	0.174***
	(0.041)	(0.037)	(0.033)
Draw=3	0.490***	0.483***	0.381***
	(0.033)	(0.031)	(0.039)
Draw=4	0.640***	0.629***	0.490***
	(0.028)	(0.027)	(0.050)
Draw=5	0.696***	0.690***	0.548***
	(0.029)	(0.029)	(0.047)
Round	0.001*	0.001	0.001
	(0.000)	(0.000)	(0.000)
Dummy=1 if believed guess is below the actual number			0.126***
			(0.037)
Controls	No	Yes	Yes
Observations	2,040	2,040	2,040
(Pseudo) R-squared	0.323	0.348	0.357

Note: Probit for senders. Robust standard errors in parentheses (clustered at the subject level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability. ***p<0.01, **p<0.05, *p<0.1.

Model II (column II) is estimated with socio-demographic controls such as sex, field of study (Economics), being a native English speaker, having a friend in the session, risk aversion, and cognitive ability.[19] We control for the subject's field of study being Economics because these subjects might be familiar with equilibrium concepts in games such as ours. Controlling for the

[19] The post-experimental questionnaire used to measure the controls is provided in Appendix 2.C. Being a native English speaker, and having a friend in the session are included in the regression to replicate the analysis in Jin et al. (2017) who include these terms in their regressions.

cognitive ability of a subject helps us examine whether subjects with a higher cognitive ability (as measured by three questions of the Cognitive Reflection Task (Frederick 2005) who might understand the game better than others, tend to reveal more compared to other subjects in the experiment.[20] We control for subjects' risk preferences which might influence revealing behaviour, i.e. risk-averse subjects might reveal more to avoid a low payoff from receivers' low guess of a non-disclosed draw.[21]

We find that qualitatively the main result remains unchanged after including controls. That is, the revealing behaviour of senders across treatments is not significantly different (p=0.894). The term identifying a native English speaker is statistically significant which implies that the probability of a native English speaker revealing the draw is 11% higher compared to a non-native English speaker (p=0.004). All other controls are insignificant.

Model III (column III) includes socio-demographic controls and a variable capturing senders' belief about the receivers' guess conditional on non-disclosure. We use senders' responses to the belief questions to infer what they believe the receivers would guess, on average, upon observing non-disclosure. We call this the *'believed guess'* of the sender. We calculate the believed guess for each sender and for every block as follows: For example, if the sender believes that 30% receivers who observe non-disclosure guess 1 or 1.5 as the private number, 25% guess 2 or 2.5, 20% guess 3 or 3.5, 15% guess 4 or 4,5,

[20] The control variable for cognitive ability is a dummy which takes the value of 1 if the subject correctly answers at least one of the three Cognitive Reflection Task questions (Frederick 2005), and the dummy takes the value of 0 otherwise.

[21] The risk aversion dummy takes the value of 1 if the subjects indicate a score that is below or equal to 5 on the risk preference question, and a value of 0 otherwise (Falk et al. 2016, also see Appendix 2.C). We also estimate the regressions using continuous scales of cognitive ability and risk aversion and find that our main results, qualitatively, remain the same.

10% guess 5, then the 'believed guess' of the sender is calculated using the lower numbers in the formula as follows:[22]

$$\frac{[(30) * 1 + (25) * 2 + (20) * 3 + (15) * 4 + (10) * 5]}{100} = 2.50$$

We include a dummy variable in the regression which takes values 1 if the draw in a given round is higher than senders' believed guess elicited at the end of that block, and the dummy takes the value of 0 otherwise. This dummy gives us insights into whether senders' revealing behaviour is influenced by their beliefs about the actions of the receivers. The believed guess dummy is highly significant implying that the probability of a sender revealing a draw is 12% higher if their believed guess is below the actual draw they observe (p=0.001). The probability of a native English speaker revealing the draw is 9% higher compared to a non-native English speaker (p=0.007). The treatment dummy and other control variables remain statistically insignificant.

Result 2.1: Being in a treatment where receivers can consult with other receivers before making their decisions does not change the revealing behaviour of senders compared to the senders in the No Consultation treatment. In both treatments, senders' revealing behaviour is far from the unravelling prediction.

[22] We also calculate the believed guess of the sender by taking 1.5 (instead of 1), 2.5 (instead of 2), 3.5 (instead of 3), 4.5 (instead of 4) and 5 as the guess of the receiver and present the results in Appendix 2.A (see Table 2.3A).

2.3.2. Analysis of receivers' behaviour

Table 2.4 presents the average receiver report in the No Consultation and the Consultation treatments when the sender reveals private number 1, 2, 3, 4, 5, and when he does not reveal the number, i.e. blank. The data in the 'Consultation' column come from a pooled sample of those in the Consultation treatment who consult as well as those who did not consult when they were given the opportunity to do so.[23] On average, receivers' reports when the draws are revealed are accurate. Figure 2.2 shows the average guess of non-disclosed numbers across blocks by receivers in both treatments. The average guess of a non-disclosed number is 2.263 and 2.232 in the No Consultation and Consultation treatments respectively and this difference is not statistically significant (Wilcoxon rank-sum test p=0.990).

We calculate the difference between the average non-disclosed number by senders and the average guess of the non-disclosed number by receivers in both treatments to calculate how accurate receivers' guesses are about the non-disclosed number. We find that, in the No Consultation treatment, this difference is 0.301 and it is 0.272 in the Consultation treatment implying that the average guess of the non-disclosed draw is significantly above the average non-disclosed number in both treatments (Wilcoxon signed-rank test p=0.027 for No Consultation treatment and p=0.046 for the Consultation treatment).

[23] Consulting or not was a choice given to receivers in the Consultation treatment, and some receivers did not consult during the experiment. Overall, out of 1,020 interactions (rounds) in the Consultation treatment between senders and receiver pairs, in 444 interactions receivers actually send messages, i.e. in 43.52% of the interactions. In particular, out of 34 pairs of receivers in the Consultation treatment, 31 pairs of receivers send a message in at least one round of the experiment.

Table 2.4: Summary of receivers' reports

Variables	No Consultation		Consultation	
	N	Mean	N	Mean
Report (reveal=1)	36	1.04	28	1.25
Report (reveal=2)	146	2.00	174	2.10
Report (reveal=3)	236	3.00	258	3.02
Report (reveal=4)	302	4.00	308	4.00
Report (reveal=5)	314	4.98	304	4.95
Guess (reveal = blank)	1006	2.263	968	2.232
Guess - draw (not revealed)	1006	0.301	968	0.272

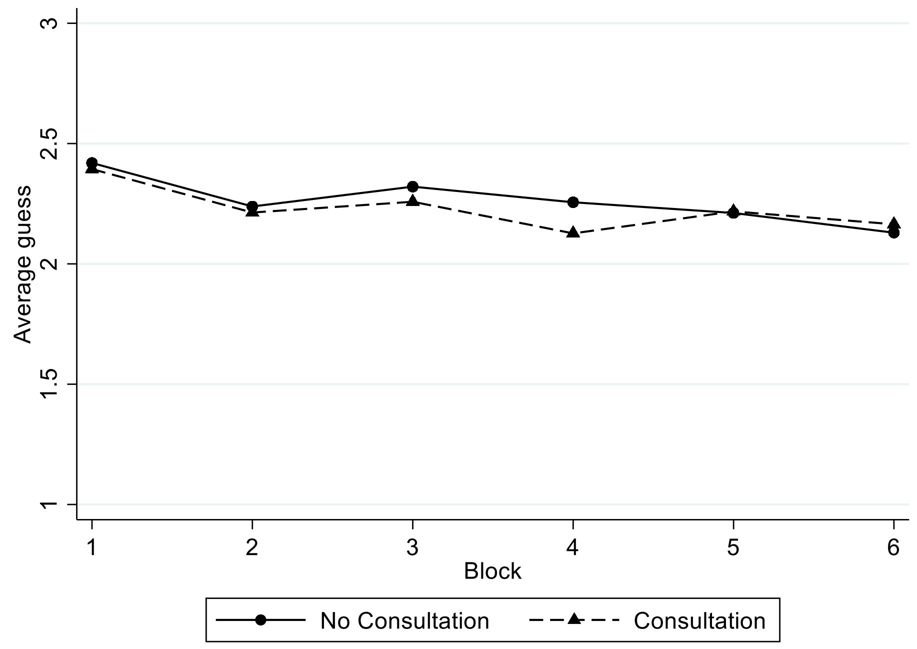

Figure 2.2: Average guess of non-disclosed numbers by receivers

Results from a regression analysis are presented in Table 2.5 below. We estimate three models to examine receivers' guess of a non-disclosed number. We estimate our regressions using the Ordinary Least Squares Method (OLS) with standard errors clustered at the session level as each session, which is a matching group, is an independent unit of observation. This is because receiver pairs received indirect feedback and were re-matched with a different sender in each round. Models II and III include socio-demographic controls like the ones used in the sender regressions - sex, field of study (Economics), being a native English speaker, having a friend in the session, risk aversion, and cognitive ability. We include these terms for similar reasons mentioned earlier. We also include additional controls for the receiver regressions such as negative reciprocity and general reciprocity as a measure of subjects' social preferences because we conjecture that reciprocal receivers might guess lower upon observing non-disclosure to punish the sender for not disclosing the draw.[24]

Table 2.5: Regressions on receivers' behaviour

Variables	Receiver guess of non-disclosed numbers		
	I	II	III
Consultation treatment	-0.029	-0.040	-0.003
	(0.092)	(0.090)	(0.067)
Round	-0.007***	-0.007***	-0.001
	(0.002)	(0.002)	(0.002)
Implied guess calculated using receiver's stated beliefs			0.710***
			(0.060)
Constant	2.382***	2.468***	0.480
	(0.059)	(0.313)	(0.296)
Controls	No	Yes	Yes
Observations	1,974	1,974	1,974
R-squared	0.009	0.049	0.312

Note: OLS for receivers. Robust standard errors in parentheses (clustered at the session level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability, negative reciprocity, and reciprocity. ***p<0.01, **p<0.05, *p<0.1.

Model I (column I) compares the guesses of receivers across treatments without including any socio-demographic controls. Our independent variable of interest is the Consultation treatment dummy which is coded 1 if the receiver is

[24] The reciprocity dummies included in the regression take values 1 (indicating reciprocal subjects) for subjects with a score above 5 on the reciprocity questions, and 0 otherwise (Falk et al. 2016, see Appendix 2.C). We also estimate the regressions using the continuous scales for cognitive ability, risk aversion, negative reciprocity and reciprocity and find that the regression results qualitatively remain the same.

in the Consultation treatment, and 0 for the receivers in the No Consultation treatment. The coefficient of the Consultation treatment dummy is not statistically significant (p=0.758). This implies that guesses of receivers do not differ significantly across treatments. The round variable is negative and significant implying that receivers guess lower over rounds (p=0.004).

Model II (column II) builds on Model I by including all the socio-demographic controls mentioned above. The coefficient on the Consultation treatment dummy remains insignificant (p=0.660). We also find that receivers' guess of the non-disclosed number increases by 0.191 if they have a friend in the same session of the experiment and this is marginally significant (p=0.066). All other controls are statistically insignificant.

Model III (column III) includes a variable that measures receivers' beliefs about the non-disclosed number. We calculate what the receivers should have guessed based on the beliefs they held using responses to belief questions and applying Bayes' rule. We refer to this calculated number as the *'implied guess'* of the receiver following the terminology of Jin et al. (2017). The implied guess is calculated for each receiver and for every block as follows: For example, if the receiver believes that the sender revealing rate is 10% for a private number of 1, 40% for 2, 70% for 3, 90% for 4, and 95% for 5, then the implied guess of this receiver conditional on non-disclosure is:

$$\frac{[(100-10)*1+(100-40)*2+(100-70)*3+(100-90)*4+(100-95)*5]}{[(100-10)+(100-40)+(100-70)+(100-90)+(100-95)]}=1.871$$

We compare receivers' guess of the non-disclosed number in a given round with their implied guess elicited at the end of that block. We estimate Model III to examine whether guesses are influenced by the beliefs receivers hold about the non-disclosed number. The coefficient on receivers' belief is positive and significant (p=0.000). This suggests that receivers' guess of the non-disclosed number increases by 0.710 with a one-point increase in their implied guess. The Consultation treatment dummy and all controls are statistically insignificant.

Result 2.2: Receivers in the Consultation treatment do not guess significantly lower upon observing non-disclosure compared to the receivers' in the No Consultation treatment. In both treatments, receivers' guesses are significantly above the non-disclosed draw.

In the next sub-section, we analyse whether the results we find in this sub-section are robust.

2.3.3. Robustness check for receivers

Receivers in the Consultation treatment were given the option to consult and therefore, they could choose not to consult with the other receiver during the experiment. We estimate a regression on receivers' guess of non-disclosed numbers using only the observations (rounds) in the Consultation treatment where chat actually did occur and the rounds thereafter, and with all observations (rounds) in the No Consultation treatment. We present the results in Table 2.6 below and include the same socio-demographic controls as in earlier regressions on receivers' behaviour for comparability.

Table 2.6: Robustness regression on receivers' behaviour

Variables	Receiver guess of non-disclosed numbers
	OLS
Consultation and Chat	-0.038
	(0.103)
Round	-0.007***
	(0.002)
Constant	2.479***
	(0.317)
Controls	Yes
Observations	1,840
R-squared	0.047

Note: OLS for receivers. Robust standard errors in parentheses (clustered at the session level). 'Consultation and Chat' dummy = 1 for all rounds after which a receiver first chats in the Consultation treatment, and 0 for all rounds in the No Consultation treatment. Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability, negative reciprocity, and reciprocity. ***p<0.01, **p<0.05, *p<0.1.

We estimate this regression because we intended to treat all receiver observations (i.e. all receivers and in all 30 rounds) in the Consultation treatment but couldn't, i.e. in a few initial rounds, some receivers did not chat and there were also some receivers who never chatted in the experiment. Therefore, our independent variable of interest is the 'Consultation and Chat' dummy which we code as 1 if the receiver in the Consultation treatment chats in a given round, and for all subsequent rounds after the chat occurs, and 0 in all rounds for the receivers in the No Consultation treatment. We code the 'Consultation and Chat' dummy = 1 for all rounds after the round in which receiver chats for the first time to account for spill-over effects of consultation. We reason that once the receiver consults to clarify a feature of the game or suggests a reason for his/her guess or anticipation of the sender's strategy, they may not need to chat again but the effect of that chat lasts for all subsequent rounds of the experiment.

The coefficient of the 'Consultation and Chat' dummy is not statistically significant (p=0.715). This implies that guesses in rounds where chat occurs and the guesses thereafter in the Consultation treatment do not differ from the guesses of the receivers in the No Consultation treatment. The round variable is statistically significant implying that receivers guess lower over rounds (p=0.008). None of the controls included are significant.[25, 26]

Result 2.3: Receivers' guesses after they chat do not differ significantly from receivers' guesses in the No Consultation treatment.

2.3.4. Analysis of the chat content

In this sub-section, we analyse the chat content of receivers in the Consultation treatment. To do this, we first assign the messages to one of the following four categories:

Receiver's action: Messages where receivers chat about own action or the action of the other receiver. For example, "What will you report?", "I will choose 2."

Sender's action: Messages where receivers chat about the sender's action, or what they think a non-disclosed number is. For example, "...well but he [sender] revealed 2 previously".

---

[25] We also compare receivers' guesses after chat occurs with receivers' guesses before chat occurs combined with guesses of receivers who never chat in the Consultation treatment. We find that receivers' guesses after chat occurs do not differ significantly from the guesses before chat occurs and guess of receivers who never chat.

[26] We investigate whether there is any selection into consulting based on the observed characteristics of receivers such as sex, field of study (Economics), having a friend in the session, being a native English speaker, risk aversion, and cognitive ability. For each receiver, we take the average of the Chat dummy across rounds in the Consultation treatment (the Chat dummy is coded as 1 if a receiver chats in a given round of the experiment and 1 for all rounds thereafter, and 0 before any chat occurs, or if no chat occurs). This average gives us a measure of how early (or how much quicker) a receiver decides to chat in the experiment and we regress this average on the characteristics of the receivers. We find that none of the characteristics are statistically significant and therefore, we conclude that we do not find any evidence for selection into chatting based on the characteristics of the receivers we observe in our data.

Equilibrium (like) reasoning: Messages where receivers provide a plausible reasoning for senders not disclosing the number. This reasoning may be close to or be the actual equilibrium reasoning. For example, "He [sender] didn't reveal it, probably means its low", "He'd [sender] definitely reveal if its 3, 4, 5."

Payoffs: Messages where receivers chat about the payoffs they would earn or the payoffs the senders would earn, for instance, from reporting a given number. For example, "He (sender) will lose 14.5 points if we report 1."

In total, based on the inspection of all messages, we manually code 837 messages out of 2,302 messages into one of the four categories mentioned above.[27] The remaining messages comprised of content such as exchanging greetings and game-irrelevant chats. Table 2.7 below presents the categorisation of messages.

Table 2.7: Categorisation of messages

Variable	***Receiver's action***	***Sender's action***	***Equilibrium (like) reasoning***	***Payoffs***
Number of messages	611	85	70	71
% out of all messages sent	26.54%	3.69%	3.04%	3.08%
% of receiver pairs who send at least one message	79.41%	50%	55.88%	47.05%

[27] When subjects hit "Enter" on the keyboard to send a message during the chat stage, we record it as one message in our dataset. Subjects could send as many messages as they wished in each round of the experiment within the 60 seconds of the chat stage. Examples of chat transcripts are included in Appendix 2.E.

After categorising the relevant chats into the aforementioned categories, we estimate a regression with the receivers' guess of the non-disclosed number as the dependent variable and the frequency of a given chat category for a receiver pair as the independent variable. The regression includes observations from the Consultation treatment, and from the No Consultation treatment where the dummies identifying the chat categories are all 0. We conjecture that receivers who message about the equilibrium reasoning will guess lower compared to the receivers who do not message about this or do not chat at all. We present the results from the regression analysis in Table 2.8 below.[28]

Table 2.8: Regression of receivers' guess on the number of messages in a chat category

Variables	Receiver guess of non-disclosed numbers
	OLS
Receiver's action	0.005*
	(0.002)
Sender's action	-0.045*
	(0.021)
Equilibrium (like) reasoning	-0.036**
	(0.011)
Payoffs	0.002
	(0.005)
Constant	2.294***
	(0.050)
Observations	1,974
R-squared	0.037

Note: OLS for receivers. Robust standard errors in parentheses (clustered at the session level). ***p<0.01, **p<0.05, *p<0.1.

We find that receivers who send messages about their own action or the action of the other receiver in the pair tend to guess higher than those who either do not

[28] Each message falls under one category only, i.e. categories are mutually exclusive. We have included only a simplified analysis of the chat content in this Chapter for two reasons: a) quantifying the messages by providing a categorical breakdown for different types of messages is difficult primarily due to introducing an experimenter bias and would therefore, need to be coded by independent research assistants before publishing in a peer-reviewed journal and, b) a more nuanced categorisation of the chat content results in a very small number of observations in some categories, thereby, substantially reducing the power of statistical tests conducted on this categorical data.

send messages at all in either of the treatments or send messages about different things in the Consultation treatment (p=0.071). We find that receivers who message about the sender's action guess lower upon observing a non-disclosed number and this is marginally significant (p=0.060). In line with our conjecture, we find that receivers who message about the equilibrium (like) reasoning guess lower upon observing a non-disclosed number and this is statistically significant (p=0.010). However, these types of messages are rather infrequently observed in our data. Finally, we find that messaging about payoffs (either their own or of the senders) does not affect receivers' guesses of the non-disclosed number.

Result 2.4: In the Consultation treatment, receivers who send messages about sender's action and about equilibrium (like) reasoning guess lower upon observing non-disclosure, although such messages are infrequently observed in our dataset.

Looking at each round in isolation, we examine whether receiver pairs consult more often in rounds where the sender reveals the number or where the sender does not reveal the number. Out of 444 interactions (rounds) in which receiver pairs consult, in 258 rounds senders did not reveal the number, and in 186 rounds the number is revealed. This implies that receivers consult more often when the number is not revealed than when the number is revealed, however, this difference is not statistically significant (Wilcoxon rank-sum test p=0.336).[29]

[29] In each session, we count the number of rounds in which receiver pairs consult when the senders do not reveal the number and when they reveal the number. We then compare whether

2.3.5. Beliefs

In this section, we explore the role of beliefs in driving receivers' guesses, and senders' revealing decision. We first compare senders' believed guess across treatments. The average believed guess is 2.291 in the No Consultation treatment and 2.328 in the Consultation treatment. We find that the difference between senders' believed guess across treatments is not statistically significant (Wilcoxon rank-sum test p=0.917). We also compare the believed guess with the actual guess by receivers in both treatments. We find that the difference between the believed guess and the actual guess is not statistically significant in either of the treatments (Wilcoxon signed-rank test p=0.463 for the No Consultation treatment and p=0.345 for the Consultation treatment). This implies that senders in both treatments form accurate beliefs about the receivers' guess conditional on non-disclosure.[30]

We further analyse the consistency of senders' actions in a block with their elicited beliefs in that block in both treatments and present these numbers in Table 2.9 below.[31] When the believed guess is lower than the actual draw in a given round of the experiment, we find that senders reveal optimally 75.17%

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the number of rounds in which receiver pairs consult when the number is revealed is significantly different from when the number is not revealed.

[30] We also calculate the believed guess of the sender as explained in sub-section 2.3.1 by taking 1.5 (instead of 1), 2.5 (instead of 2), 3.5 (instead of 3), 4.5 (instead of 4) and 5 as the guess of the receiver. We test whether the believed guess, calculated using the higher numbers of the interval, is significantly different from the actual guess in both treatments. We find that the believed guess and actual guess are significantly different in both No Consultation and Consultation treatments (Wilcoxon signed-rank test p=0.027 for both treatments). This implies that if the senders indicate their beliefs with the higher numbers of the interval as receivers' guesses about a non-disclosed draw, then their beliefs about the guesses are not accurate given receivers' actual behaviour. However, since receivers use whole numbers more often (55%) compared to the numbers in increments of 0.5 (45%) while making a guess of a non-disclosed draw, we conduct all analyses relating to the believed guess by taking the lower number of the interval, unless otherwise stated.

[31] In Appendix 2.A, we reproduce Table 2.9 using higher numbers of the interval for the believed guess calculation (see Table 2.3A).

in the No Consultation treatment and 76.92% in the Consultation treatment and this difference between treatments is not statistically significant (Wilcoxon rank-sum test p=0.597). When the believed guess is higher or equal to the actual draw in a given round of the experiment, we find that senders did not reveal the draw 83.18% in the No Consultation treatment and 80.23% in the Consultation treatment and this difference between treatments is also not statistically significant (Wilcoxon rank-sum test p=0.764). The numbers in Table 2.9 are similar to those in Jin et al. (2017).

These results imply that, given the receivers' behaviour, the senders in both treatments form correct beliefs about the actions of the receivers but they do not always best-respond to their beliefs.[32]

Table 2.9: Summary of elicited beliefs of senders

	No Consultation	**Consultation**
% of observations revealed when believed guess < draw	**75.17**	**76.92**
Rounds 1-10	70.30	70.59
Rounds 11-20	73.93	83.82
Rounds 21-30	82.12	75.77
% of observations not revealed when believed guess >= draw	**83.18**	**80.23**
Rounds 1-10	84.78	77.78
Rounds 11-20	78.29	74.26
Rounds 21-30	85.71	88.36

Result 2.5: There is no significant difference between the believed guesses of senders across treatments. Though senders form accurate believed guesses given

[32] We find that senders do not best-respond to their believed guess when it is lower than the actual draw, even after allowing for a 10% error from the 100% best-response case (Wilcoxon rank-sum test p=0.001 in the No Consultation treatment and p=0.003 in the Consultation treatment). However, senders best-respond when their believed guess is greater or equal to the actual draw after allowing for a 10% error (Wilcoxon rank-sum test p=0.653 in the No Consultation treatment and p=0.370 in the Consultation treatment).

receivers' behaviour, they do not always best-respond to their believed guess in either of the treatments.

We next analyse the average implied guess of receivers across treatments. The implied guess is calculated using receivers' stated beliefs and applying Bayes' rule as explained before, and on average, it is 2.186 in the No Consultation treatment and 2.139 in the Consultation treatment. This difference is not statistically significant (Wilcoxon rank-sum test p=0.748). When we compare the implied guess with the actual non-disclosed number in the No Consultation treatment, we find that the difference is statistically significant (Wilcoxon signed-rank test p=0.027). Similarly, the difference between the implied guess and the actual non-disclosed number in the Consultation treatment is marginally significant (Wilcoxon signed-rank test p=0.074). We use the term 'strategic naivety' to refer to the difference between the implied guess and the actual non-disclosed number following the terminology of Jin et al. (2017). This implies that receivers in neither of the treatments sufficiently infer that non-disclosure of a draw implies that it is a low draw and receivers in neither of the treatments form correct beliefs about the revealing behaviour of senders.

Receivers in the No Consultation treatment usually best-respond to their incorrect beliefs, whereas the receivers in the Consultation treatment do not always best-respond to their incorrect beliefs about the senders' revealing behaviour and in fact, guess higher than their implied guess. That is, the difference between the implied guess and the actual guess is not statistically significant in the No consultation treatment (Wilcoxon signed rank-test p=0.115) but is marginally significant in the Consultation treatment (Wilcoxon signed rank-test p=0.074).

Figure 2.3 shows the actual revealing rate of each draw by senders in both treatments and the receivers' belief about the senders' revealing rate, which we elicit during the experiment. We interestingly find that receivers hold accurate beliefs about the revealing behaviour of senders with low draws. However, receivers have miscalibrated beliefs about the senders' revealing rates when the draws are intermediate and high. In other words, receivers incorrectly believe that senders with intermediate and high draws are not disclosing, while they are sufficiently pessimistic about the senders' revealing behaviour of low draws. This implies that receivers are too optimistic about the non-disclosed draw given senders' revealing behaviour and this is similarly seen in Jin et al. (2017).

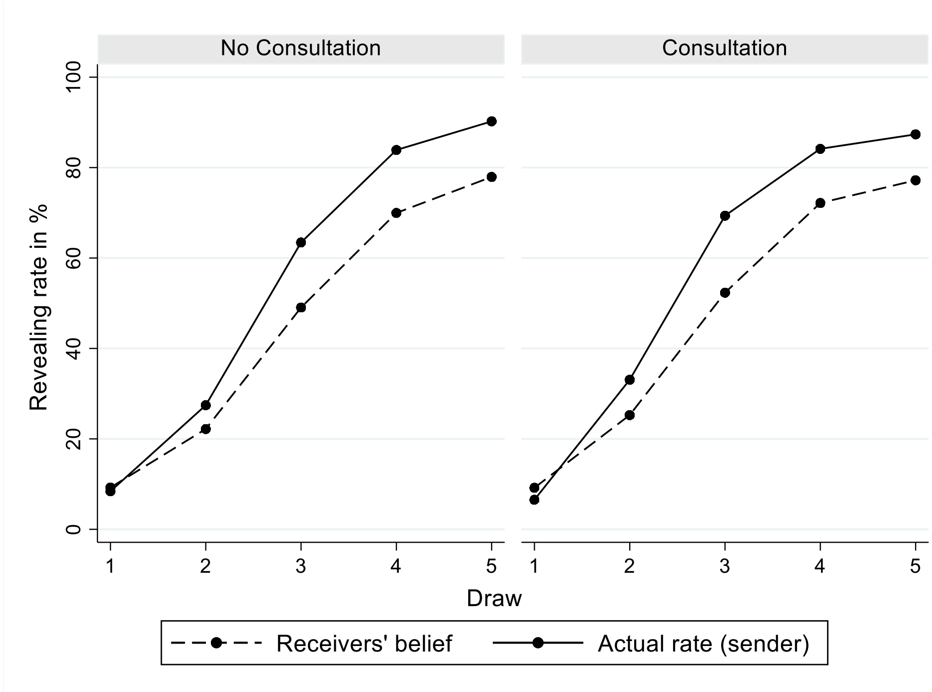

Figure 2.3: Actual revealing rate and receivers' belief about revealing rate

Result 2.6: The difference between the implied guess across treatments is not statistically significant. The difference between the implied guess and the actual non-disclosed number is statistically significant in both treatments implying that strategic naivety is robust under consultation.

We analyse whether the implied guess of receivers varies systematically with their characteristics that are measured in the post-experimental questionnaire. We present these results in Table 2.10 below. We conjecture that subjects studying economics and subjects with a higher cognitive ability, as measured by three Cognitive Reflection Task questions (Frederick 2005), hold more accurate implied guesses compared to other subjects in the experiment. This is because they may be familiar with equilibrium reasoning of such games and would be better at inferring the actions of senders. Therefore, we regress the implied guess on the treatment dummy, and on the characteristics such as sex, field of study being Economics, risk aversion, and cognitive ability. In line with our conjecture, we find that receivers with a higher cognitive ability have a lower implied guess, and therefore, their beliefs are closer to accuracy given senders' revealing behaviour. This is marginally significant (p=0.070). These results are akin to the results in Li and Schipper (2018) where subjects with higher cognitive abilities display significantly higher levels of reasoning in such games.

Result 2.7: Higher cognitive ability in receivers is correlated with lower, and therefore, more accurate beliefs about the non-disclosed number.

Table 2.10: Regression on the implied guess of receivers

Variables	OLS
Consultation Treatment	-0.082
	(0.077)
Male	-0.085
	(0.085)
Field of study being Economics	0.006
	(0.150)
Risk aversion	0.075
	(0.093)
Cognitive ability	-0.227*
	(0.113)
Constant	2.370***
	(0.096)
Observations	816
R-squared	0.057

Note: Regression estimated using OLS. Robust standard errors in parentheses (clustered at the session level). ***p<0.01, **p<0.05, *p<0.1.

2.4. Discussion and conclusion

This Chapter examines how consultation affects subsequent individual decisions and beliefs about non-disclosed information. Overall, we find that complete unravelling fails to occur in both No Consultation and the Consultation treatments. We do not find a significant treatment effect of consultation on the reports of receivers, and we also find that consultation does not attenuate naivety about non-disclosed information. We provide a brief discussion of our results in this section.

We find that our results regarding the average revealing rate by senders and the average guess of non-disclosed numbers by receivers are qualitatively similar to those in Jin et al. (2017) suggesting a successful replication of their experiment. Our finding regarding the revealing rate of each of the numbers by senders is similar to the results in Mathios (2000) who studies disclosure of fat content in salad dressings using field data. He finds that fat content in salad dressings is disclosed almost always when it is low (i.e. when it is of high quality, similar to the 4s and 5s in our experiment), medium fat content in salad dressings is disclosed about 60% of the time (similar to the 3s), and high fat content is disclosed about 9% of the times (similar to the revealing rate of 1s in our experiment). This suggests the generalisability of our results to natural settings and in addition, provides further insights about the nature of strategic naivety about non-disclosed information.

Charness et al. (2010) remark that merely deliberating alternative courses of action improves understanding of the problem and lead to accurate responses by subjects. The main result of our study departs from theirs, and there are two reasons that might explain this. First, Charness et al. (2010) study an individual decision-making problem which requires subjects to undertake basic reasoning to overcome a decision anomaly (i.e. conjunction fallacy in their setup). Consultation helps in this case as the solution to the problem is easily demonstrable once one of the subjects identifies the solution to the problem and communicates it to the other subject.

On the contrary, we examine behaviour in a strategic setting where receivers need to anticipate senders' behaviour to choose a response. In such cases, it is not clear how consulting might help receivers better anticipate senders' behaviour for reasons such as strategic uncertainty regarding the senders' decision-making ability. Second, even if a receiver conveys the equilibrium reasoning to the other receiver during the chat stage, there is no direct way to verify whether the reasoning led to a more accurate guess of the non-disclosed number relative to previous rounds, except by observing what numbers were revealed earlier. Since many natural settings are characterised similarly as our game, i.e. without direct and immediate feedback, we do not expect consultation in the real-world to improve receivers' inference about non-disclosed information.

Using the methodological tools available in experimental economics and combining them with elements we see in natural settings, we are interested in investigating whether strategic naivety exists in natural settings. In Chapter 3, we address one of the longstanding methodological debates in the experimental economics - about providing 'in-context' versus 'context-free' experiments to subjects in the lab. In doing so, we systematically examine whether naivety exists due to the abstractness of neutrally framed disclosure experiments, and whether it can be overcome when a natural context is provided to the game similar to what subjects might encounter in the field.

CHAPTER 3:

STRATEGIC NAIVETY IN EXPERIMENTS: THE ROLE OF NATURAL CONTEXT

3.1. Introduction

In Chapter 2, we examined whether allowing subjects to consult with others before they make decisions improves inferences and actions regarding non-disclosed information. We found that consulting with others does not change behaviour or attenuate strategic naivety about non-disclosed information. By strategic naivety we mean that individuals do not sufficiently infer that non-disclosure of information implies that the information contains bad news (for example, about the quality of a product, or the type of the sender). In this Chapter, we examine whether adding a natural context to the experimental setup improves inferences about non-disclosed information compared to an experiment with an abstract setting.

3.1.1. Motivation

Smith (1976) is one of the earliest advocates of using abstract terminology and induced value methodology to elicit preferences of subjects in experiments. He opines that adding context distorts and weakens the control over subjects' valuation and, hence, the explicit reward structure should act as the only source of valuation for subjects. Smith's approach became the norm in experimental economics with some early exceptions like Alm et al. (1992) who systematically compared subjects' responses in 'context-free/abstract' relative to 'in-context' experiments.

The motivation for employing context in an analysis of strategic naivety stems from the arguments posed by advocates of contextual experimental presentation. In natural environments, individuals do not face strategic situations in an abstract and context-free setting, as is widely the norm in lab experiments, but within a certain relevant contextual setting. Providing a context to the game enables individuals to have a nuanced sense of the setting by influencing beliefs, motivations, and therefore, behaviour (Barr and Serra 2009, Dufwenberg et al. 2011, for a review see Alekseev et al. 2017). Context can also highlight the essential features of an abstract game and thereby enhance subjects' understanding of the decision problem, especially in tasks that require sophisticated reasoning.

More importantly, in the setting we investigate, we conjecture that strategic naivety about non-disclosed information, which stems from receivers' miscalibrated beliefs about senders revealing rate of intermediate and high numbers, may be attenuated by adding a natural context to our experiment. This is because making decisions in a natural context, in our experiment, regarding the disclosure of the hygiene rating of a restaurant, may elucidate the incentives and the motives for high quality restaurants (senders with high numbers) to disclose their hygiene rating (private number), and thereby, correct receivers' beliefs about the non-disclosed number. This, therefore, may generate responses which are similar to those in the field where customers may be aware of a restaurant's intentions for disclosing and withholding information about their hygiene rating. These reasons make it appealing to investigate whether strategic naivety is, therefore, partly an artefact of abstract, context-free lab experiments which lack a familiar context.

To investigate our question, we design an experiment with a 'No Context' treatment and a 'Context treatment' based on the experimental disclosure game outlined in Chapter 2.[33] The treatments vary in the way the instructions are framed, in particular, in the labelling that is used to convey the key features of the game. We provide a context of the restaurant owner's (sender's) decision to display the restaurant hygiene rating. We frame the role of the sender as the "Restaurant owner" and the receivers as "Restaurant Customer 1" or "Restaurant Customer 2". We call the draw as the "Food hygiene rating" of the restaurant. We present a more detailed discussion of the experimental design later in this Chapter. The senders and receivers play the disclosure game for 30 rounds and we elicit beliefs about the actions of the other player after every 5 rounds of the game. This allows us to examine whether the potential effect of context on the decisions made by subjects is mediated through the beliefs that they hold about the actions of the other player.

We find that adding a context to the experiment does not increase disclosure of information. Moreover, context neither significantly affects receivers' actions nor their beliefs about the non-disclosed information. However, we find that context leads senders to hold lower beliefs about the receivers' guess of the non-disclosed number; yet this does not appear to affect senders' overall disclosure behaviour. In general, we find that 'context-free' experiments and 'in-context' experiments yield similar results in our setting and that strategic naivety is a robust phenomenon.

[33] The No Context treatment is indeed the No Consultation treatment mentioned in the previous Chapter.

3.1.2. Related literature

The literature on "context-free" versus "in-context" presentation of experiments is largely divided with respect to the effect of context on behaviour. Alm et al. (1992) (in a tax evasion experiment), Abbink and Hennig-Schmidt (2006) and Banerjee (2016) (in bribery experiments) examine behaviour in an 'abstract/neutral' treatment and a 'loaded/framed' treatment. They find that context does not have a significant effect on the behaviour of subjects in these experiments with respect to the amount of tax evaded in the tax evasion experiment, and the amount or frequency of bribes offered and rejected in the bribery experiments.

On the contrary, Krajcova and Ortmann (2008) varied endowment levels of subjects in a bribery experiment and examined whether adding a context to the experiment produced significantly different behavioural responses compared to an abstract setting. They find heterogeneous effects based on the gender of subjects in response to adding a context to the experiment - males increased bribing in the Context treatment in low endowment cases compared to females. Samuelson and Allison (1994) (in a public good game), Cooper and Kagel (2003) (in a signalling game), and Chou et al. (2009) (in a guessing game) find an effect of context on subjects' behaviour, although this effect is weak in the study by Cooper and Kagel (2003).[34] In particular, Chou et al. (2009) attribute the reason for subjects' failure to act strategically in a guessing game to the abstractness of the game. They find that presenting the experiment in a familiar

[34] Cooper and Kagel (2003) find a short-term but weak effect of context on strategic play for certain types of players. In particular, they find that strategic play was more frequent in a Meaningful Context treatment among incumbents in a market-entry signalling game. However, the effect became weaker over time. The play of entrants was not significantly different between the Context and the Neutral treatments.

context confirms the ability of subjects to make strategic decisions. This suggests that subjects in our experiment may form better, i.e. more accurate, beliefs about non-disclosed numbers and best-respond to them in a natural context, thereby, attenuating strategic naivety.[35]

Dufwenberg et al. (2011) (in a public good game) also find that adding context to the experiment changes subjects' behaviour. In addition to systematically examining differences between 'in-context' and 'context-free' experiments, they examine the mechanism that mediates the effect of context on behaviour. They find that the use of context in a public good game (i.e. framed as a "community game") and its effect on behaviour is mediated by the beliefs that subjects hold about the contributions of other subjects. Our study relates to their paper by also examining whether adding a natural context to the experiment affects players' beliefs about the actions of the other player.

More related to our disclosure game is the study by Benndorf et al. (2015) who examine the extent to which subjects reveal information and whether choices are in line with the equilibrium predictions. They examine revelation behaviour in a neutral and a contextually framed setting and demonstrate that providing a context to the game decreases disclosure of personal, sensitive information. They observe subjects' choices in a labour-market frame where "workers" decide about revealing their "health status" (indicator of their productivity) to the employer. Unlike in our experiment, they computerised the

[35] The difference between in-context and context-free presentation of experiments has also been studied in the social psychology literature. Wason and Shapiro (1971) and Griggs and Cox (1982) (in Wason's Selection Task), and Kay and Ross (2003) (in a Prisoner's Dilemma) find a significant effect of context on subjects' behaviour. In particular, Kay and Ross (2003) find that subjects who are primed with a "cooperative" prime as opposed to a "competitive" prime are more likely to associate the Prisoner's Dilemma game with cooperative labels and state cooperative actions.

employers (i.e. receivers) and analysed only the worker's (sender's) revealing behaviour with respect to the equilibrium prediction. As they computerised the strategy of the receiver, strategic naivety about non-disclosed information cannot be examined in their setting, which is the primary aim of our study.

The rest of this Chapter is organised as follows. Section 3.2 presents the experimental design and highlights differences between treatments. In Section 3.3, we outline the main results and the effect of context on the beliefs that subjects hold. In section 3.4, we discuss our results and conclude the Chapter.

3.2. Experimental Design

To examine the effect of context on behaviour and beliefs of players, we design an experiment that is primarily built on the design by Jin et al. (2017). Our experiment consists of two treatments in a between-subjects design: the 'No Context' treatment and 'Context' treatment. The No Context treatment is in fact the No Consultation treatment outlined in Chapter 2, and the data reported here for this treatment are the same as those presented in Chapter 2 for the No Consultation treatment. In other words, we compare our Context treatment results to the same control group as in Chapter 2 as we were successful in replicating the baseline results of the presence of strategic naivety as in Jin et al. (2017).

The setup of our experiment is similar to the design mentioned in Chapter 2. Recall that our experiment consists of one sender and two receivers. The two receivers are paired together and they remain fixed in these pairs for the entire duration of our 30-round experiment (5 rounds=1 block). The receiver pairs are matched with a different sender in each round to implement a one-shot nature of the game and minimise any reputational effects that might drive senders' disclosure decisions.

In each round of the experiment and for every matching of a sender with a pair of receivers, the sender obtains a randomly generated private number drawn from the uniform distribution over the set {1, 2, 3, 4, 5}.[36] The probability distribution of the private numbers is common knowledge. The sender observes his private number and decides whether to reveal or withhold the number from the receivers. If the sender decides to reveal the number, both receivers are shown this message from the sender: "The number I received is:" followed by the actual private number. In case the sender decides to withhold the number, the field for the actual private number is left blank and receivers know that the sender decided to withhold the number.

After observing the message from the sender, if any, the receivers independently (and without consulting with each other) report the value of the private number. If the private number is not revealed, then the receivers guess the value of the private number based on the probability distribution over the set of private numbers and the sender's decision to withhold the number. As before, the receivers can choose any number from the set {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5} to make their reports.

As in the experiment of Chapter 2, receivers are told that they can guess numbers in increments of 0.5 (1.5, 2.5, 3.5, 4.5) for example, in case they are

[36] We generated a random series of 30 private numbers corresponding to the 30 rounds of the game for each sender in a session and kept the series constant across all sessions to ensure comparability between treatments.

unsure about the private number of the sender. As explained in the previous Chapter, the action space of receivers is designed to ensure that the sequential equilibrium of the game involves full unravelling, i.e. all numbers (except the lowest number, i.e. 1) are always disclosed by the sender and receivers guess the lowest number upon observing non-disclosure.

The incentives for the senders and receivers are such that the sender obtains a higher payoff when the receiver reports a higher number, and each of the receivers earns a higher payoff when their own reports are closer to the true private number. The payoffs for senders (U_S) and receivers (U_R) are calculated using the following payoff functions:

$$U_s = \frac{(110 - 20\,|5 - \text{receiver 1's report}|^{1.4}) + (110 - 20\,|5 - \text{receiver 2's report}|^{1.4})}{2}$$

$$U_R = 110 - 20\,|\text{actual private number} - \text{receiver's own report}|^{1.4}$$

These payoff functions are akin to those used by Jin et al. (2017) and those used in Chapter 2. Subjects did not see these payoff functions because the payoffs are presented in a table for subjects' ease of comprehension.[37]

Neither senders nor receivers obtain direct feedback about the report/guess of the number or the true private number after each round. The only kind of feedback receivers obtain is the number that is revealed by the senders (or the fact that the sender did not reveal) over rounds. Senders obtain no feedback at all. At the end of every block, subjects answer 5 questions that are

[37] See payoff tables in Appendix 3.B - Experimental instructions.

used to calculate their beliefs about the action of the player in the opposite role.[38] These belief questions are incentivised using the following rule:

$$70 - |\text{Correct Answer} - \text{Subject's Answer}|$$

This rule is similar to the rule in Kugler et al. (2007). As discussed in the previous Chapter, we use this scoring rule as it is simple for subjects to understand and less time consuming to convey the rule to the subjects in the experimental instructions while still ensuring that we elicit the mean belief of the subjects. We use subjects' responses to these belief questions (and apply Bayes' rule for receivers) to calculate the action they should have taken based on the beliefs they hold.

The subjects are paid for two randomly chosen blocks at the end of the experiment. For one of the blocks, receivers are paid for the accuracy of their reports of the private number in each of those 5 rounds. The senders are paid based on the reports of the receivers in each of those 5 rounds. For the other block chosen for payment, subjects are paid for the accuracy of their answers to the belief questions at the end of that block. As explained earlier, we employ this incentive scheme to prevent subjects from hedging the actions they take with their responses to the belief questions, i.e. low payoffs due to inaccurate reports can't be compensated by obtaining high payoffs in the belief elicitation task (or vice versa) in the same block. Subjects answer a socio-demographic

[38] We ask senders what percentage of receivers they think guessed 1 or 1.5, 2 or 2.5, 3 or 3.5, 4 or 4.5, and 5 when they did not reveal the private number in that block. We ask receivers what percentage of senders they think revealed when the private number was 1, 2, 3, 4, and 5 in that block. The specific questions are relegated to the experimental instructions in Appendix 3.B.

questionnaire at the end of the final block and questions about reciprocity, risk and cognitive abilities.[39]

While the basic game of our experiment is common to both the No Context and the Context treatments, the key difference between the treatments is how we frame the instructions. In the Context treatment, we frame the instructions to provide a natural setting to the game, i.e. of a Food Hygiene Rating of a restaurant. We do this by providing a context of a restaurant owner's (sender's) decision to display or withhold the food hygiene rating of the restaurant (private number), and restaurant customers' (receivers') decision to note down or guess the hygiene rating based on the restaurant owner's decision to display or withhold it. We use the context of a restaurant hygiene rating as we run the experiment with student subjects in the United Kingdom and food hygiene ratings are commonly seen at restaurants and eateries that students often visit.

The Food Hygiene Ratings are issued by the Food Standards Agency, which is an independent government department operating in England, Wales and Northern Ireland in partnership with local authorities. The FSA runs a scheme called the Food Hygiene Rating Scheme as part of their agenda to protect public health by ensuring food safety. This scheme gives restaurants and businesses that deal with food a Food Hygiene Rating between {0, 1, 2, 3, 4, 5}, where 0 implies that the business is required to make urgent improvements to their hygiene standards and 5 implies very good hygiene standards. Displaying

[39] We used the questions from Falk et al. (2016) to elicit risk preferences, and positive and negative reciprocity. We also used questions from the Cognitive Reflection Task (Frederick 2005) in the post-experimental questionnaire to measure subjects' cognitive ability as in the previous Chapter.

these ratings is mandated by law in Wales and Northern Ireland but is on a voluntary basis in England, i.e. restaurants and other food businesses can choose whether to display their rating ("Food Standards Agency", 2018).

In our experiment, we use a modified version of the food hygiene rating card, one which closely resembles the food hygiene rating cards issued by the FSA. This ensures that the experiment closely resembles a natural setting. In Figure 3.1, Panel A shows the actual hygiene rating card issued by the FSA and Panel B shows the modified hygiene rating card used in the experiment.

Table 3.1 provides a summary of the key differences in the terminology used in the instructions of the Context treatment and the No Context treatment. The instructions for the treatments are presented in Appendix 3.B.

Table 3.1: Framing used in the Context treatment

No Context treatment	Context treatment
S Player	Restaurant Owner
R Player 1	Restaurant Customer 1
R Player 2	Restaurant Customer 2
Private number	Food Hygiene Rating
Reveal	Display
Do not reveal	Do not display
Report the private number	Note down the hygiene rating

The experiment was conducted at the Centre for Decision Research and Experimental Economics laboratory at the University of Nottingham. Subjects were recruited using ORSEE (Greiner 2015). We ran a total of 12 sessions, 6 sessions of the No Context treatment and 6 sessions of the Context treatment on z-Tree (Fischbacher 2007). Each session comprised of one matching group.

There were 102 subjects in the No Context treatment and 99 subjects in the Context treatment with 15-18 subjects per session/matching group.[40] The sessions lasted around 70 minutes. The average payment was £13 including a fixed participation fee of £4.

Panel A: Actual Hygiene Rating Card (FSA)

Panel B: Modified Hygiene Rating Card (Experiment)

Figure 3.1: Food hygiene rating cards

[40] The sample summary statistics are relegated to Table 3.1A in Appendix 3.A.

3.3. Results

The main results of this Chapter are outlined in this section. On average, there is no significant difference in the revealing rate of private numbers by senders across the No Context and the Context treatments even though senders in the Context treatment hold lower beliefs about receivers' guess conditional on non-disclosure. Receivers' guess about non-disclosed numbers do not differ across treatments and neither do their beliefs about the non-disclosed number. This implies that strategic naivety about non-disclosed information is not an artefact of abstract experiments and is robust to adding a natural context to the experiment.

3.3.1. Analysis of senders' behaviour

Table 3.2 shows senders' revealing behaviour. In general, higher draws are revealed more often than low draws in both treatments. A non-parametric test conducted on the average revealing rates of each sender, excluding draw 1 from the test (because senders with draw 1 are indifferent between revealing and withholding), confirms that there is a deviation from the theoretical prediction of full unravelling in both the No Context and the Context treatments even after allowing for a 10% error rate from the equilibrium prediction (Wilcoxon rank-sum test p=0.000 for both the treatments).[41]

[41] Since senders did not receive any feedback between rounds, we consider each sender as an independent unit of observation and thus, run all tests at the subject level on senders' data. Since receivers observed sender's decision and they were randomly re-matched with a different sender after each round, we conduct all tests for receivers at the session level, which is one matching group, as it is an independent unit of observation. We conduct paired tests to compare the treatment averages of receivers with senders and, therefore, also consider each session as the

Table 3.2: Summary of senders' disclosure decisions

Variables	No Context		Context	
Panel A				
	N	**% revealed**	**N**	**% revealed**
Draw = 1	214	8.41	207	2.90
Draw = 2	266	27.44	261	32.18
Draw = 3	186	63.44	180	63.33
Draw = 4	180	83.89	174	92.53
Draw = 5	174	90.23	168	95.24

Panel B

	No Context	Context
Total non-revealed draws	503	465
Average non-revealed draw	1.962	1.817

Note: In Panel A, the column "% revealed" reports the average revealing rate across all senders when they observed that particular draw. In Panel B, we report the total number of non-revealed draws and the average draw when senders did not disclose the draw in the respective treatments.

Figure 3.2 displays the revealing rate of draws across blocks in both treatments. The revealing rate of draw 1 is lower in the Context treatment compared to the No Context treatment and this is marginally significant (Wilcoxon rank-sum test p=0.086). The revealing rates of none of the other draws are significantly different. The average non-disclosed number by senders is 1.962 in the No Context treatment and 1.817 in the Context treatment and this difference is also not statistically significant (Wilcoxon rank-sum test p=0.651). We also find that the average revealing rate of senders across treatments is not significantly different (Wilcoxon rank-sum test p=0.546).

independent unit of observation (for example: comparing the average non-disclosed number with the average guess of the non-disclosed number across the treatments).

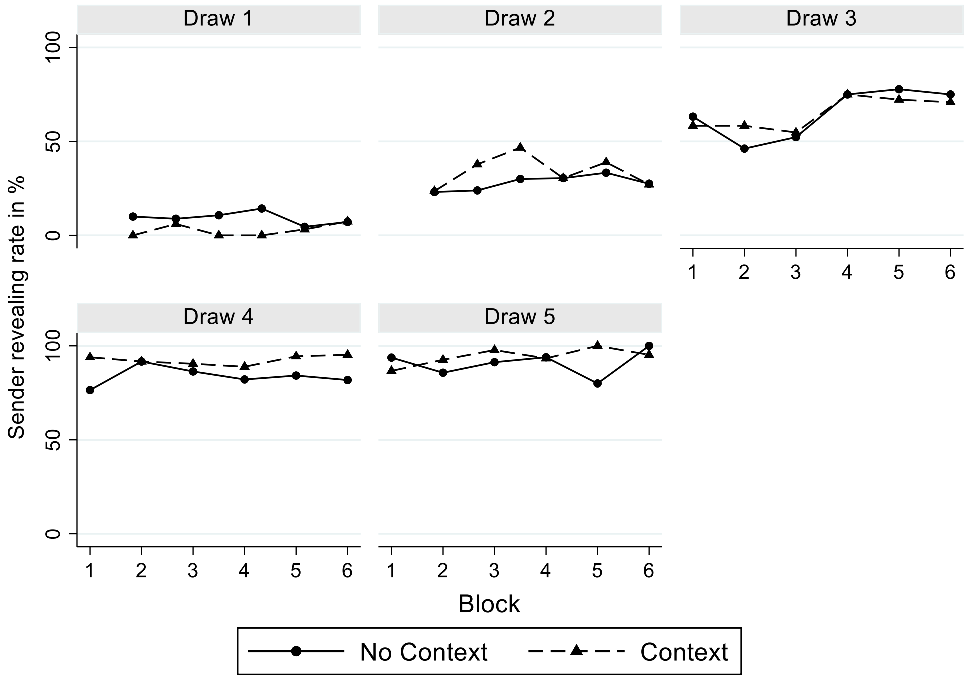

Figure 3.2: Sender revealing rate by draw across blocks

We conduct a regression analysis to further analyse the differences in revealing behaviour across treatments. The senders' decision to reveal (1 if sender reveals and 0 otherwise) is the dependent variable and the main independent variable of interest is a dummy variable that takes the value of 1 for senders in the Context treatment and 0 for the senders in the No Context treatment. Table 3.3 provides marginal effect estimates from a Probit estimation for two regression specifications. The standard errors are clustered at the subject level treating each sender is an independent unit of observation.

Model I (column I) includes socio-demographic controls. The controls included for the sender regressions are the same controls used in the previous Chapter and are included for similar reasons.[42] We find that the coefficient on

[42] Appendix 3.C provides the post-experimental questionnaire used to measure these controls.

the Context treatment is negative but not statistically significant (p=0.886) confirming that the revealing rate of senders does not differ significantly across treatments. The Draw variable estimates are positive and statistically significant (p=0.000 of all the 'Draw coefficients'). The Round variable is significant which suggests that senders tend to increasingly reveal draws over rounds (p=0.036). This suggests some form of learning, despite the absence of feedback across rounds.

This type of learning corresponds to what Rick and Weber (2010) refer to as 'meaningful learning'. According to this type of learning, the senders in our experiment reveal more over rounds as they understand the nature of the experiment better once they have had some experience playing and deliberating about the game even though they do not have the opportunity to learn from feedback. Being a native English speaker increases the probability of revealing a draw by 7% and this is marginally significant (p=0.053). The other controls are insignificant.

In Model II (column II) we include a dummy variable based on the senders' believed guess as mentioned in the previous Chapter.[43] The coefficient on the sender beliefs dummy is significant implying that senders have a 11% higher probability of revealing a given draw if their believed guess is below the

[43] Recall that the believed guess (i.e. what the senders think the receivers will guess conditional on non-disclosure) is calculated as follows: For example, if the senders believe that 30% receivers who observe non-disclosure guess 1 or 1.5 as the private number, 25% guess 2 or 2.5, 20% guess 3 or 3.5, 15% guess 4 or 4,5, 10% guess 5, then the 'believed guess' of senders is calculated as follows: $\frac{[(30)*1+(25)*2+(20)*3+(15)*4+(10)*5]}{100} = 2.50$. The dummy variable included in the regression takes the value 1 if the draw in a given round is higher than the sender's believed guess elicited at the end of that block, and the dummy variable takes the value of 0 otherwise.

actual draw they observe (p=0.001). The coefficient on the Context treatment remains statistically insignificant.

Table 3.3: Regressions on senders' behaviour

Variables	Sender reveals? (0/1) I	Sender reveals? (0/1) II
Context treatment	-0.005	-0.016
	(0.037)	(0.036)
Draw=2	0.247***	0.186***
	(0.034)	(0.027)
Draw=3	0.460***	0.364***
	(0.033)	(0.037)
Draw=4	0.663***	0.545***
	(0.028)	(0.036)
Draw=5	0.728***	0.607***
	(0.024)	(0.036)
Round	0.002**	0.002**
	(0.001)	(0.001)
Dummy=1 if believed guess is below the actual number		0.107***
		(0.031)
Controls	Yes	Yes
Observations	2,010	2,010
(Pseudo) R-squared	0.394	0.402

Note: Probit for senders (marginal effects reported in the table). Robust standard errors in parentheses (clustered at the subject level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, and cognitive ability. ***p<0.01, **p<0.05, *p<0.1.

Result 3.1: Context does not change the revealing behaviour of senders compared to the behaviour of the senders in the No Context treatment. The revealing behaviour of senders is far from the equilibrium prediction of complete unravelling in both treatments.

3.3.2. Analysis of receivers' behaviour

We show receivers' reports across treatments when the senders reveal each of the draws and when the senders do not reveal the draw in Table 3.4. The receivers in the No Context treatment guess 2.263 conditional on non-disclosure and the receivers in the Context treatment guess 2.189 conditional on non-disclosure and this difference is not statistically significant (Wilcoxon rank-sum test p=0.423). This is also shown in Figure 3.3 below.

Table 3.4: Summary of receivers' reports

Variables	No Context		Context	
	N	**Mean**	**N**	**Mean**
Report (reveal=1)	36	1.04	12	1.12
Report (reveal=2)	146	2.00	168	1.99
Report (reveal=3)	236	3.00	228	3.00
Report (reveal=4)	302	4.00	322	4.01
Report (reveal=5)	314	4.98	320	4.99
Guess (reveal = blank)	1006	2.263	930	2.189
Guess - draw (not revealed)	1006	0.301	930	0.372

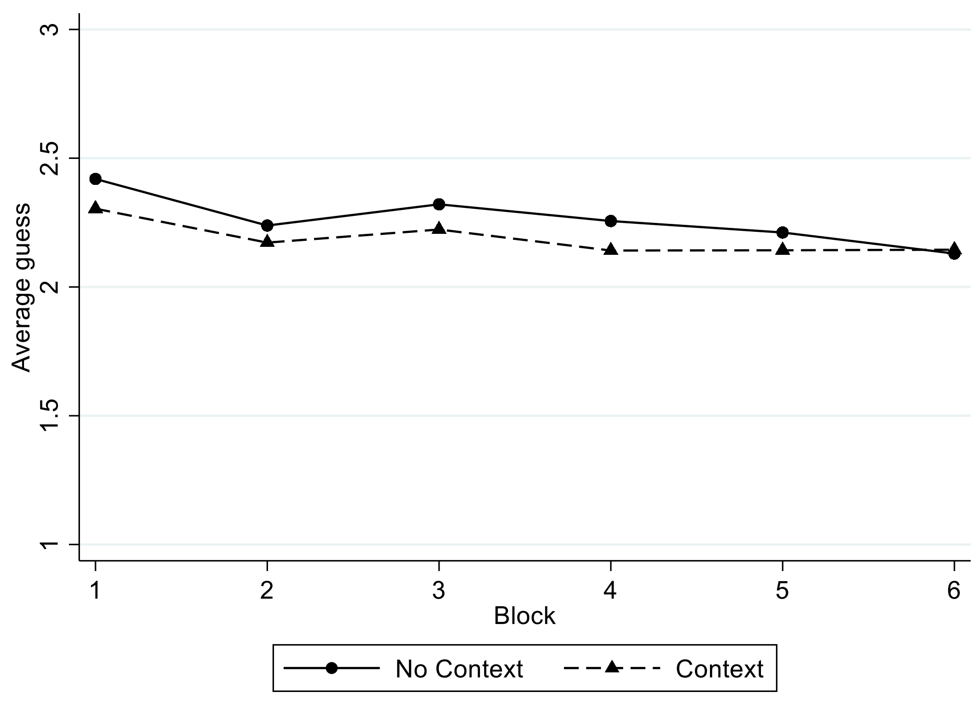

Figure 3.3: Average guess of non-disclosed numbers by receivers

We find that receivers' guess of the non-disclosed number is too optimistic given the average actual non-disclosed number in both treatments. This is reflected in the statistically significant difference between the non-disclosed number and the guess of the non-disclosed number in both treatments (Wilcoxon signed-rank test p=0.027 in both treatments).

We conduct a regression analysis to further confirm our results and present the regression estimates in Table 3.5. We examine receivers' guess when the number is not revealed by senders. Our independent variable of interest is the treatment dummy which takes the value of 1 if the receiver is in the Context treatment and 0 for the receivers in the No Context treatment. We estimate our regressions using the Ordinary Least Squares Method (OLS) with standard errors clustered at the session level, which is also the matching group.

Model I (column I) is a regression specification with the treatment dummy, the number of the round, and control variables which are the same as in the previous Chapter and are included for similar reasons. The treatment dummy is negative but not statistically significant implying that Context does not help receivers make lower guesses about non-disclosed numbers (p=0.564). The round variable is statistically significant implying that receivers guess lower over rounds (p=0.001). We also find that being a student of Economics decreases receivers' guess of the non-disclosed number by 0.306 and this is marginally significant (p=0.085). All other controls are not statistically significant.

Model II (column II) builds on Model I by including a variable capturing receivers' beliefs about non-disclosed draws. Using receivers' stated beliefs, we calculate their implied guess.[44] The coefficient on the implied guess is positive and significant (p=0.000) implying that a one-point increase in receivers' implied guess increases their guess of the non-disclosed number by 0.703. All other variables are not statistically significant.

Table 3.5: Regressions on receivers' behaviour

	Receiver guess of non-disclosed numbers	
Variables	**I**	**II**
Context treatment	-0.065	-0.043
	(0.109)	(0.096)
Round	-0.006***	-0.001
	(0.001)	(0.002)
Implied guess calculated using receiver's stated beliefs		0.703***
		(0.052)
Constant	2.823***	0.812**
	(0.347)	(0.302)
Controls	Yes	Yes
Observations	1,936	1,936
R-squared	0.052	0.269

Note: OLS for receivers. Robust standard errors in parentheses (clustered at the session level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability, negative reciprocity, and reciprocity. ***p<0.01, **p<0.05, *p<0.1.

Result 3.2: Context does not have a statistically significant effect on the receivers' guess of the non-disclosed number compared to the treatment without context. Receivers' guesses of a non-disclosed draw are significantly above the actual non-disclosed draw in both treatments.

[44] Recall that the implied guess (i.e. what the receivers believe the non-disclosed number is) is calculated for each receiver and for every block as follows: For example, if the receiver believes that senders with private number 1 revealed 20% of the time, with private number 2 revealed 30% of the time, 70% for 3, 90% for 4, and 95% for 5, then the implied guess of this receiver conditional on observing non-disclosure is:

$$\frac{[(100-20)*1+(100-30)*2+(100-70)*3+(100-90)*4+(100-95)*5]}{[(100-20)+(100-30)+(100-70)+(100-90)+(100-95)]} = 1.923$$

We compare receiver's guess of the non-disclosed number in a given round with their implied guess elicited at the end of that block.

3.3.3. Beliefs

We explore senders' and receivers' beliefs about the action of the player in the opposite role. In Table 3.6, we summarise senders' believed guess and analyse the consistency of their actions in a block with the optimal action given their believed guess. The believed guess is 2.291 in the No Context treatment and 1.886 in the Context treatment. The difference between the believed guess of senders across treatments is statistically significant (Wilcoxon rank-sum test p=0.008).[45] This implies that senders in the Context treatment believe that receivers will guess lower upon observing non-disclosure compared to the senders in the No Context treatment.

Table 3.6: Summary of elicited beliefs of senders

	No Context	Context
% of observations revealed when believed guess < draw	**75.17**	**73.39**
Rounds 1-10	70.30	69.09
Rounds 11-20	73.93	74.15
Rounds 21-30	82.12	77.00
% of observations not revealed when believed guess >= draw	**83.18**	**89.41**
Rounds 1-10	84.78	85.45
Rounds 11-20	78.29	92.55
Rounds 21-30	85.71	90.60

Note that the effect on believed guesses in the Context treatment does not alter senders' overall revealing behaviour as discussed earlier. We believe that the differences in senders' believed guess across treatments is reflected in the differences in their revealing behaviour of draw 1. That is, senders in the

[45] We also calculate the believed guess of the sender by taking 1.5 (instead of 1), 2.5 (instead of 2), 3.5 (instead of 3), 4.5 (instead of 4) and 5 as the guess of the receiver. The difference between senders' believed guess across treatments with this calculation remains significant (Wilcoxon rank-sum test p=0.008). In Appendix 3.A, we reproduce Table 3.6 with numbers from this calculation (see Table 3.2A).

Context treatment reveal draw 1 significantly fewer times (2.9%) compared to senders in the No Context treatment (8.4%) (Wilcoxon rank-sum test p=0.086).

We also compare the accuracy of senders' believed guess with the actual guess of the receivers in both treatments. We find that the difference between the believed guess and the actual guess in the No Context treatment is not significantly different (Wilcoxon signed-rank test p=0.463). This implies that senders in the No Context treatment form accurate beliefs about the receivers' guess conditional on non-disclosure. On the contrary, the difference between the believed guess and the actual guess in the Context treatment is statistically significant (Wilcoxon signed-rank test p=0.027) implying that believed guess in the Context treatment is not accurate, i.e. lower, given the receivers' actual guess of the non-disclosed number.

We analyse the percentage of observations that senders reveal when their believed guess indicates it is optimal to do so, i.e. when the believed guess elicited at the end of a block is lower than the actual draw in a given round of that block. We find that senders reveal optimally 75.17% in the No Context treatment and 73.39% in the Context treatment and this difference between treatments is not statistically significant (Wilcoxon rank-sum test p=0.522). When the optimal action is to withhold the draw, i.e. when the believed guess is greater or equal to the draw, senders in the No Context treatment withhold the draw 83.18% of the times compared to 89.41% of the times in the Context treatment and this difference between treatments is statistically significant (Wilcoxon rank-sum test p=0.024). We conclude that senders in the Context treatment behaved significantly closer to their optimal action, given their beliefs, in withholding the draw from receivers (reflected in revealing rates of draw 1).

These results imply that, senders in the No Context treatment form accurate beliefs about the actions of receivers whereas, senders in the Context treatment do not form accurate beliefs about the actions of the receivers. However, senders in the No Context treatment do not always best-respond to their beliefs. On the other hand, senders in the Context treatment tend to best-respond to their beliefs relatively more often when withholding the draw.

Result 3.3: Senders in the Context treatment hold a lower believed guess than the senders in the No Context treatment. This implies that context affects senders' beliefs about the receivers' guess of non-disclosed numbers.

The implied guess, which we calculate using receivers' stated beliefs, is 2.186 in the No Context treatment and 2.108 in the Context treatment and this not significantly different between treatments (Wilcoxon rank-sum test p=0.423).[46] In Figure 3.4, we show receivers' beliefs about the senders' revealing rate, which we elicit in our experiment, and compare it with the actual revealing rate of senders in the No Context and the Context treatments. The figure shows that receivers have miscalibrated beliefs about senders' revealing rate for intermediate and high draws in both treatments as also found in the previous Chapter. This miscalibration is driven by receivers' incorrect belief that senders with intermediate and high draws do not reveal as often as the senders

[46] The difference between the implied guess and the actual guess is neither statistically significant in the No Context treatment (Wilcoxon signed rank-test p=0.115) nor in the Context treatment (Wilcoxon signed rank-test p=0.463) implying beliefs play a significant role in receivers' naïve guesses about the non-disclosed number.

actually did in the experiment. The receivers almost accurately calibrate their beliefs about the revealing behaviour of the senders with low draws, i.e. they are sufficiently pessimistic about the revealing behaviour of the senders with the low draws.

These miscalibrated beliefs imply that, given the actual revealing behaviour of senders, the receivers are too optimistic about the value of the non-disclosed draw. The miscalibrated beliefs of receivers indicate that the average implied guess is significantly higher than the actual average non-disclosed draw (Wilcoxon signed-rank test p=0.028 for both treatments). These results imply that adding context to the experimental setting does not reduce naivety in beliefs about non-disclosed numbers.

Overall, receivers in neither of the treatments form correct beliefs about the actions of the senders and they usually best-respond to their incorrect beliefs.

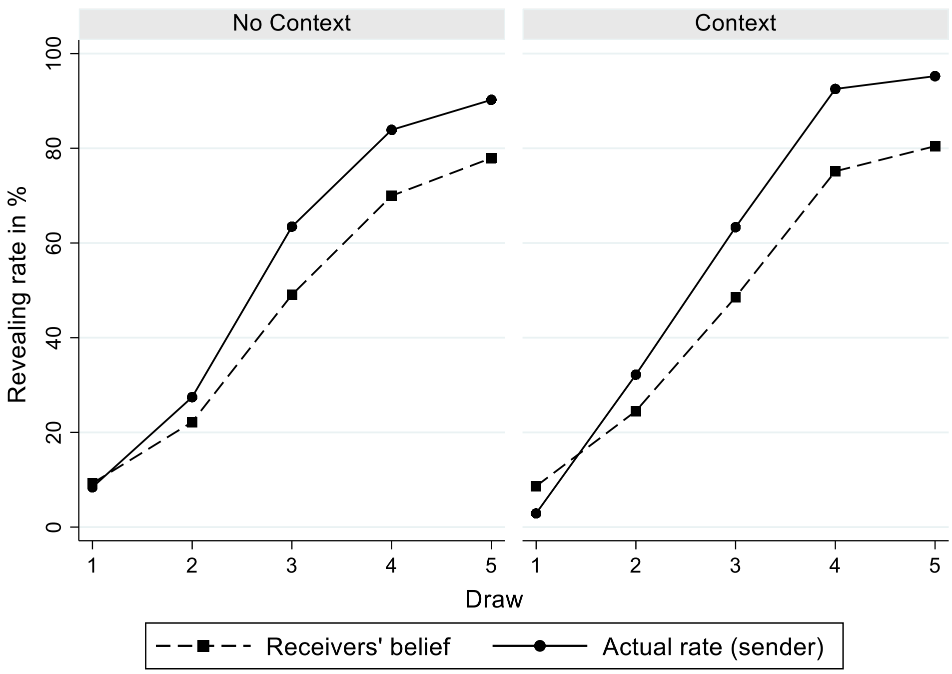

Figure 3.4: Actual revealing rate and receivers' belief about revealing rate

Result 3.4: The difference between the implied guess across treatments is not statistically significant. Since the difference between the implied guess and the actual non-disclosed number is statistically significant in both treatments, this implies that strategic naivety is not an artefact of abstract experiments.

3.4. Discussion and conclusion

The experiment reported in this Chapter examines whether ‘in-context’ experiments improve inferences about non-disclosed information compared to ‘context-free’ experiments. To explore this question, we construct a disclosure experiment built on the design by Jin et al. (2017) in which subjects play a one sender-two receivers game. In one treatment, we provide abstract and neutrally framed instructions to subjects. In the other, we provide the context of the disclosure of a restaurant hygiene rating with subjects in the roles of a restaurant owner (sender) and restaurant customers (receivers). We conjecture that context will lead receivers to form better inferences about non-disclosed information.

Contrary to our conjecture, we find that adding context does not change subjects’ behaviour and strategic naivety is not an artefact of context-free experiments but is in fact robust to providing a natural context to the experimental setting. In particular, the revealing rate by senders does not differ significantly across treatments even though the beliefs of senders about receivers’ guess of non-disclosed numbers in the Context treatment is lower compared to that of the senders in the No Context treatment. We also find that receivers’ guess of non-disclosed numbers does not differ significantly between treatments and neither does context improve receivers’ inferences about the non-disclosed number. These results are robust to a number of different regression specifications using various controls.

We believe that the 'context-free' experiment communicates the structure of the game clearly such that providing a context to the game does not change subjects' behaviour. That is, essential features of the game, like the conflict-of-interest between the sender and the receivers, are clearly highlighted in the instructions of the 'context-free' treatment. Hence, subjects may have "transcended the frame" in the 'in-context' treatment and therefore, we observed actions and beliefs (of receivers) similar to those in the 'context-free' treatment (Chatterjee et al. 2000). This suggests that future research should examine not only whether context affects behaviour but also the conditions under which we are likely to see an effect of context on the behaviour of subjects.

Our results are also in contrast to the results in Benndorf et al. (2015) who find less revelation in a contextualised setting. We conjecture that the difference between our results may be due to the nature of information that subjects (workers) had to reveal, i.e. in their experiment, information is personal and sensitive (about health status) which is not the case in our experiment.

In the next Chapter, we examine how to increase disclosure of information despite the presence of naïve receivers. In this spirit, we examine whether manipulating the sender's side by changing the market structure, i.e. by introducing competition between senders, increases unravelling of information and attenuates strategic naivety in receivers.

CHAPTER 4:

CAN COMPETITION INCREASE DISCLOSURE OF INFORMATION?

4.1. Introduction

In Chapters 2 and 3, we analysed whether strategic naivety about non-disclosed information is robust to manipulations on the receivers' side such as: a) allowing a receiver to consult with another receiver before making their reports about the non-disclosed number and, b) providing a natural context to the experiment about the disclosure of restaurant food hygiene ratings. These manipulations were conjectured to help receivers make better inferences about non-disclosed information and therefore, attenuate strategic naivety documented in the literature by Jin et al. (2017) and Sah and Read (2017). We find that strategic naivety is robust to these manipulations on the receivers' side and complete unravelling of information fails to occur.

In this Chapter, we experimentally examine whether manipulating the senders' side, i.e. introducing competition between senders, increases disclosure of information. We examine the effect of competition on information disclosure because we conjecture that if consumers prefer firms that disclose information over those that do not, then competition between firms may generate unravelling of information as firms may start competing in disclosing information to the consumers. Therefore, in this Chapter, we experimentally examine whether introducing competition between firms ('senders'), i.e. competing to be chosen by a consumer ('receiver'), increases disclosure of information. We believe it is intuitive and natural to observe competition between firms as conceptualised in our experiment – where firms compete with each other to be able to trade with a consumer.[47]

We believe that a second mechanism, i.e. comparative evaluation of information, will help attenuate strategic naivety in a setting with competing firms. Our conjecture is that comparing information that is revealed with information that is not revealed will affect consumer's inferences about non-disclosed information. This conjecture finds support in the social psychology and marketing literature which provides extensive evidence suggesting that human judgements are comparative in nature.

The comparative modes of evaluation may be deeply rooted in an individual's psyche such that individuals evaluate a given piece of information in relation to other pieces of information available to them which act as a standard for comparison (Dehaene et al. 1998, Mussweiler 2003). For example, while deciding which firms' product to buy, consumers judge whether the product of firm A is better than the product of firm B on a given dimension and evaluate the products of the two firms in a comparative manner. The underlying mechanism for comparison to affect decision-making is that the presence of a standard influences judgements by making a particular knowledge unit accessible to the consumer (Higgins 1996). The more accessible a given piece

[47] Milgrom and Roberts (1986) propose a form of competition between firms which generates unravelling of information in the presence of strategically naïve consumers. Their theoretical result postulates that if each piece of information in the marketplace favours at least one firm, then all relevant information unravels in the presence of competition between firms. That is, even a naïve consumer can elicit all relevant information from firms by promoting information competition, thus, motivating each firm to explain why its product is better than the competitor's product. See Milgrom (2008) for an overview of the theoretical arguments about how firms disclose information to influence buyers.

of information is, the more likely it will be used in the decision-making process and the more likely it is to influence the decision (Bollinger et al. 2011).

Since comparative evaluation forms a core mechanism in decision-making, we conjecture that comparing information that is disclosed with information that is not disclosed might help consumers make sceptical inferences about information that is not disclosed to them. In other words, the absence of a particular piece of information may be compared with a salient piece of information which might generate scepticism about the absent information and attenuate strategic naivety that is documented in the literature.

This Chapter closely relates to the papers by Forsythe et al. (1989) and Penczynski and Zhang (2017) who study unravelling of information in an auction setting. Forsythe et al. (1989) find that the sequential equilibrium, where buyers "assume the worst", is reached through an unravelling process over the course of their experiment. Penczynski and Zhang (2017) also examine in an auction setting how the market structure and the information structure affect disclosure of information. They find that introducing competition between sellers increases disclosure of information, however, buyers do not compensate for sellers' selective disclosure of evidence in their bids and their welfare does not improve under competition. In this Chapter, we examine the role of competition in the amount of information that is disclosed by senders, and in addition, explore the effect of comparative evaluation of information on receivers' sophistication, which may also help increase disclosure of information in natural settings.

To examine how competition affects information unravelling and explore its effects on receiver sophistication, we construct an experiment with two treatments, the 'Baseline' and the 'Competition' treatment, which builds on the design proposed by Jin et al. (2017). The basic setup of the experiment is based on the sender-receiver disclosure game as we outlined in the previous Chapters. In the Baseline, there is one sender and one receiver who play the disclosure game as described earlier. In the Competition treatment, there are two senders and one receiver. The two senders separately obtain a private number from the set {1, 2, 3, 4, 5}. Senders simultaneously and individually decide whether to reveal their private number to the receiver. The receiver observes both senders' decision alongside each other and reports or guesses the numbers of each sender.

The differences in the Competition treatment compared to the Baseline are that: a) there are two senders in the Competition treatment and, b) the receiver chooses her preferred sender after reporting the numbers of both senders. This choice of the receiver determines the probability with which a sender is selected by the computer to earn a payoff (specifically, the preferred sender is more likely to be selected). The sender selected by the computer earns a payoff based on the receiver's report, and the other sender earns zero. The receiver earns a payoff based on the accuracy of her report of the selected sender. The receiver's choice of the preferred sender introduces competition between senders.

We find that competition between senders significantly increases the amount of information that is disclosed to receivers. In particular, the intermediate numbers are more likely to be revealed in the presence of competition between senders. Our conjecture about the effect of comparative evaluation on receivers' sophistication fails to hold as competition does not attenuate receiver's naivety about non-disclosed information. We do not find significant differences in guesses of non-disclosed numbers by receivers across the Baseline and the Competition treatment.

Interestingly, we find that the average payoff of receivers in the Competition treatment is higher than the average payoff of receivers in the Baseline despite no marked improvement in inferences about non-disclosed information. This effect is largely driven by a higher rate of information disclosure by senders in the Competition treatment which led receivers to report accurately when the senders revealed the number. As in previous Chapters, we find that naivety in receivers is driven by miscalibrated beliefs about the actions of senders with intermediate and high numbers rather than miscalibrated beliefs about the actions of senders with low numbers. That is, receivers underestimate how often intermediate and high numbers are revealed by senders but correctly estimate how often low numbers are revealed by senders.

Our findings suggest that even in the presence of strategically naïve consumers, competition in the marketplace can increase disclosure of information by firms. Therefore, tools that aim to increase competition between firms, which are at the disposal of regulators like the Competition and Markets Authority in the United Kingdom, can be used to generate increased disclosure of information.

The rest of this Chapter is organised as follows. Section 4.2 outlines the details of the design of the experiment. Section 4.3 presents the results. This section also outlines the welfare effects of competition on receivers and the role of beliefs in driving the decisions of senders and receivers. Section 4.4 discusses the results and offers some concluding remarks.

4.2. Experimental design

We design a sender-receiver game to examine whether competition between senders increases information disclosure and affects receivers' scepticism about non-disclosed information. The basic setup of our game is like the experiment outlined in the previous Chapters. There are two player roles – senders and receivers. The sender observes a private number which is drawn from the set {1, 2, 3, 4, 5}. Each of these numbers is equally likely to be generated and this probability distribution over the set of numbers is common knowledge. The sender then decides whether to reveal this number to the receiver or stay silent. If he decides to reveal the number, he cannot lie or misreport the true number and the receiver is sent this message from the sender: "The number I received is:" followed by the actual private number.

The receiver knows the number with certainty following this message from the sender. Otherwise, the receiver observes: "The number I received is:" followed by a blank. The receiver observes the sender's decision to reveal or not and must report or guess the true number of the sender from the set {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. The incentives of the game are such that receivers earn a higher payoff if they report the true number accurately, and the senders earn a higher payoff when the receiver makes a higher report. The specific payoffs in the experiment are derived using the same payoff functions mentioned in Jin et al. (2017) and in the previous Chapters. Subjects did not see these functional forms as the payoffs were shown in a table.[48] The payoff functions for the sender (U_S) and the receiver (U_R) respectively are:

$$U_s = 110 - 20\,|5 - \text{receiver's report}|^{1.4}$$

$$U_R = 110 - 20\,|\text{actual private number} - \text{receiver's report}|^{1.4}$$

When the receiver's action space is sufficiently rich, as in our experiment, the sequential equilibrium of the disclosure game, as shown by Milgrom (1981), is such that the sender always reveals the true private number, unless it is the lowest number, i.e. private number 1 - in which case he is indifferent between revealing and not, and the receiver guesses the lowest number upon observing non-disclosure.[49]

Based on the game described above, our experiment consists of two treatments - the Baseline and the Competition treatment. The treatments primarily vary based on the number of senders that are matched with the receiver: one in Baseline, and two in Competition. In addition, the receiver in the Competition treatment must choose her preferred sender. In the Competition treatment, the two senders are paired together, and the senders remain in their fixed pairs for the entire duration of the experiment which mimics competition between sellers in natural environments. We randomly rematch a receiver with a different sender [sender pair] in each round of the Baseline [Competition treatment] to minimise reputational effects that might drive disclosure decisions.

[48] The specific payoffs are shown in Table 4.1A in Appendix 4.A.
[49] Recall that we ensure richness in receiver's action space by allowing them to guess in increments of 0.5, i.e. 1.5, 2.5, 3.5, and 4.5.

At the beginning of the experiment, subjects are assigned to one of the two roles, i.e. sender or receiver. They remain in these roles for the whole experiment. The subjects play 30 rounds of this game in both treatments and we do not provide feedback to subjects between rounds. The Baseline is akin to the "No feedback" treatment in Jin et al. (2017) where senders receive no feedback and receivers observe the draw, if the sender decides to reveal, or no message, if the sender does not reveal the draw.[50]

In each round of the Competition treatment, the two senders receive numbers that are independently and uniformly drawn from the set of private numbers. The senders then individually and simultaneously decide whether to reveal their own number to the receiver or stay silent. After observing both senders' decision to reveal or not, the receiver must make reports about the true numbers of both senders. If no number is revealed, the receiver must make a guess about the true numbers based on the sender's decision not to reveal. The receiver also chooses her preferred sender.

This choice of the preferred sender determines the probability with which the sender is selected by the computer to be matched with the receiver to earn a payoff for that round. The preferred sender is selected by the computer with a 75% probability and the non-preferred sender is selected by the computer with a 25% probability to be matched with the receiver to earn a payoff. This incentive mechanism ensures that the receiver is incentivised to choose her preferred sender as well as to report the number of the sender that she does not

[50] The Baseline is a conceptual replication of the 'No feedback' treatment in Jin et al. (2017) who borrow elements from Cai and Wang (2006) and Wang et al. (2010). We depart from Jin et al. (2017) in the same way as mentioned in the previous Chapters, i.e. by eliminating role reversal and eliciting beliefs every 5 rounds in an incentivised manner instead of eliciting them only once at the end of the 30 rounds.

prefer. This helps us gain meaningful insights about the receiver's report even when the number is not disclosed, and when the receiver does not choose that sender. The receiver's choice of the preferred sender ensures that there is a conflict of interest between senders which is what we refer to as competition.

In both treatments, we also elicit subjects' beliefs about the actions of the other player after every 5 rounds (5 rounds = 1 block).[51] We incentivised each question in the belief elicitation stage using the same scoring rule as in the previous Chapters and similar to Kugler et al. (2007): 70 - |Correct answer – Subject's answer|. Using responses to these questions and applying Bayes' rule, we infer what the receivers should have guessed based on the beliefs they held about the sender's strategy. Similarly, using the sender's responses to these questions, we infer what senders believed the receivers would guess upon observing non-disclosure. This gave us insights into the relationship between the senders' and receivers' actions and their beliefs.

After the final round of the experiment, subjects are paid for two different blocks at random - once for the decision-making stage, and once for the accuracy of the answers to the belief questions. We paid two different blocks to avoid subjects hedging their actions with their responses to the belief questions in the same block. This ensured incentive compatibility of both the beliefs and the actions of subjects. Subjects also answered a questionnaire where we obtained information on their gender, field of study, risk preference, social preferences using the questions in Falk et al. (2016), cognitive ability using the

[51] Recall that we ask senders what percentage of receivers they thought guessed 1 or 1.5, 2 or 2.5, 3 or 3.5, 4 or 4.5, and 5 when they observed non-disclosure in that block. We ask receivers what percentage of senders they thought revealed in that block when the true private number was 1, 2, 3, 4, and 5. The specific belief questions are relegated to the experimental instructions in Appendix 4.B.

three Cognitive Reflection Task questions (Frederick 2005), and scepticism level using the Hurtt's scepticism scale (Hurtt 2010).

The experiment was programmed on z-Tree (Fischbacher 2007) and was run at the Centre for Decision Research and Experimental Economics Laboratory at the University of Nottingham. We used ORSEE (Greiner 2015) to recruit 173 subjects from a diverse, mostly undergraduate, subject pool.[52] In total, we conducted 9 sessions, i.e. 3 sessions with two matching groups in each session for the Baseline, and 6 sessions for the Competition treatment with each session as one matching group. Each matching group in the Baseline comprised of 10 to 12 subjects (i.e. two matching groups comprised of 10 subjects), and each matching group in the Competition treatment comprised of 15 to 18 subjects (i.e. one matching group comprised of 15 subjects). Subjects were paid in cash privately at the end of the experiment. Sessions lasted, on average, 70 minutes, and the average payment per subject was £12 which included a show-up fee of £4.

[52] Table 4.2A in Appendix 4.A provides summary statistics of the subjects in the experiment.

4.3. Results

In this section, we present the main results of the experiment with regard to the behaviour and beliefs of senders and receivers.

4.3.1. Analysis of senders' behaviour

Panel A in Table 4.1 provides a summary of the senders' actions in the Baseline and the Competition treatment. In both treatments, on average, senders reveal higher draws more often than the lower draws. When the draw is 4 or 5, the revealing rate is above 90% in both the Baseline and the Competition treatment but not 100% as per the equilibrium prediction. The revealing rate drops below 75% in the Baseline and below 85% in the Competition treatment for draws of 2 and 3 even when full unravelling is the equilibrium prediction.

Table 4.1: Summary of senders' disclosure decisions

Panel A

Variables	Baseline		Competition	
	N	% revealed	N	% revealed
Draw=1	180	6.67	416	8.65
Draw=2	240	24.58	475	41.26
Draw=3	192	71.35	379	84.96
Draw=4	168	91.07	397	97.73
Draw=5	240	97.08	433	98.15

Panel B

	Baseline	Competition
Total non-revealed draws	426	733
Average non-revealed draw	1.854	1.616

Note: In Panel A, the column "% revealed" reports the average revealing rate across all senders when they observed that particular draw. In Panel B, we report the total number of non-revealed draws and the average draw when senders did not disclose the draw in the respective treatments.

According to the theoretical prediction, we expect to see non-disclosure at most 20% of the times since senders with draw 1 in both treatments are indifferent between revealing and not revealing the number in equilibrium. A non-parametric test (excluding draw 1 from the analysis) performed on the average revealing rate of each sender suggests that there is a deviation from the full unravelling equilibrium in both the Baseline and the Competition treatment even after allowing for a 10% deviation from the unravelling prediction due to error (Wilcoxon rank-sum test p=0.000 for both Baseline and Competition treatment).[53]

We next analyse the revealing rates of specific draws by senders. Figure 4.1 displays the revealing rates by block for each draw across the Baseline and the Competition treatment. We find that senders in the Competition treatment reveal draws 2, 3, and 4 significantly more than the senders in the Baseline (Wilcoxon rank-sum test p=0.037 for draw 2, p=0.009 for draw 3, p=0.002 for draw 4). We do not find a significant difference in the revealing rates of draw 1 and 5 because a sender with draw 5 has no incentive to withhold the draw from the receiver in either treatments. In contrast, a sender with draw 1 has little reason to reveal in either treatments. Especially in the Competition treatment, a sender with draw 1 would never want to be chosen and hence, would never want to reveal a draw 1.

[53] All tests conducted on senders' data consider each sender as an independent unit of observation because senders did not receive any feedback between rounds. All tests conducted on receivers' data are conducted at the matching group level which is an independent unit of observation because receivers obtained information if the draw was revealed and the receivers were randomly re-matched with a different sender pair after each round. The tests conducted to compare treatment averages of receivers with senders consider each matching group as the independent unit of observation (for example: comparing the average non-disclosed draw with the average guess of the non-disclosed draw across treatments). This is because we use paired difference tests such as the Wilcoxon signed-rank test.

The significant difference in revealing rates across the Baseline and the Competition treatment explains a lower average non-disclosed draw by senders in the Competition treatment than in the Baseline (1.854 in the Baseline and 1.616 in the Competition treatment), as shown in Panel B of Table 4.1. This difference is statistically significant (Wilcoxon rank-sum test p=0.000).

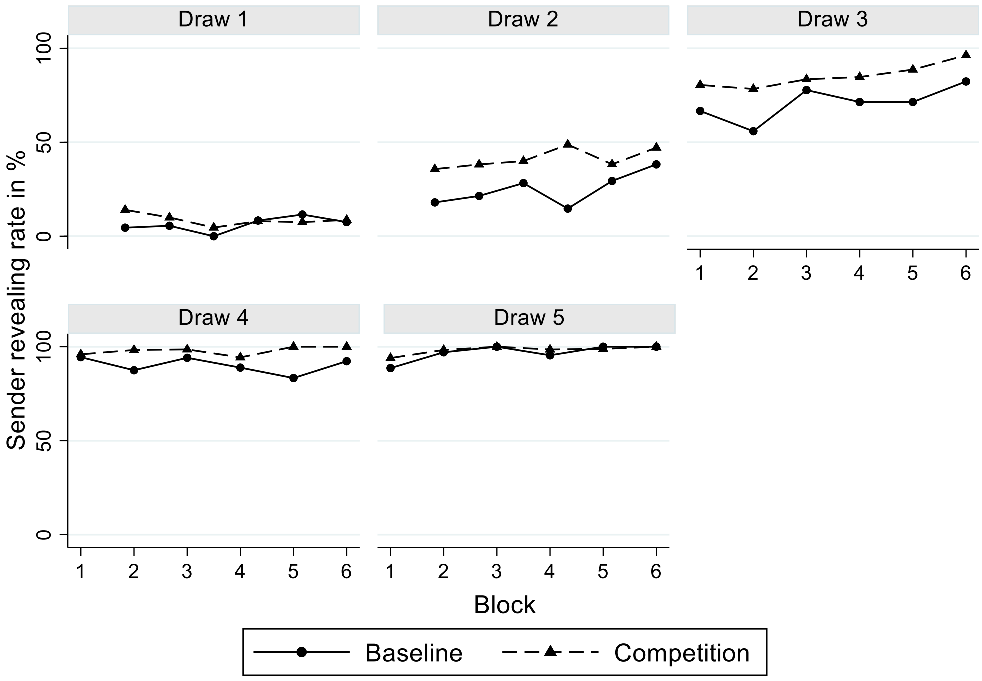

Figure 4.1: Sender revealing rate by draw across blocks

Result 4.1: Intermediate draws, i.e. draw 2, draw 3, and draw 4, are revealed more often in the Competition treatment. The average non-disclosed draw is significantly lower in the Competition treatment compared to the Baseline.

To analyse differences in the revealing behaviour of senders in the Baseline and the Competition treatment further, we conduct a regression analysis of senders' decisions to reveal (1 if the draw is revealed and 0 if it is not revealed). Our regressor of interest is a treatment dummy indicator which takes the value of 1 for senders in the Competition treatment, and 0 for senders in the Baseline. Table 4.2 provides marginal effect estimates from a Probit estimation for three model specifications. The standard errors are clustered at the subject level as each sender is an independent unit of observation. This is because we do not provide feedback to senders between rounds in the experiment.

Table 4.2: Regressions on senders' behaviour

	Sender reveals? (0/1)		
Variables	**I**	**II**	**III**
Competition treatment	0.079***	0.073***	0.072***
	(0.026)	(0.023)	(0.022)
Draw=2	0.202***	0.200***	0.148***
	(0.024)	(0.024)	(0.027)
Draw=3	0.435***	0.432***	0.342***
	(0.017)	(0.017)	(0.030)
Draw=4	0.606***	0.604***	0.489***
	(0.020)	(0.020)	(0.037)
Draw=5	0.661***	0.655***	0.538***
	(0.026)	(0.024)	(0.042)
Round	0.002***	0.002***	0.002***
	(0.0007)	(0.0007)	(0.0007)
Dummy=1 if believed guess is below the actual number			0.097***
			(0.025)
Controls	No	Yes	Yes
Observations	3,120	3,120	3,120
(Pseudo) R-squared	0.486	0.500	0.511

Note: Probit for senders (marginal effects reported in the table). Robust standard errors in parentheses (clustered at the subject level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability questions. ***p<0.01, **p<0.05, *p<0.1.

Model I (column I) is a simple specification without any socio-demographic controls. We regress whether the sender reveals the draw in a given round on the Competition treatment dummy, individual draw dummies, and the number of the round. The statistically significant estimate of the treatment dummy coefficient suggests that the probability of the senders revealing the draw in the Competition treatment, on average, is 8% higher than in the Baseline (p=0.001). The omitted draw, i.e. Draw 1, is the base against which the coefficients of Draw 2, Draw 3, Draw 4, and Draw 5 can be evaluated. The significance of these Draw coefficients suggests that higher draws are revealed significantly more than draw 1 (p=0.000 of all the 'Draw' coefficients). The significant effect of the Round variable suggests that senders learn over rounds even though we do not provide feedback between rounds. Rick and Weber (2010) call this 'Meaningful learning' in the absence of feedback.[54]

Model II (column II) specification builds on Model I by including controls such as sex, field of study being economics, being a native English speaker, having a friend in the session, risk aversion, cognitive ability questions that were measured by a questionnaire.[55, 56] We include these controls for the same reasons mentioned in the previous Chapters. We find that our main result of competition increasing the likelihood of revealing numbers remains significant (p=0.002). Cognitive ability has a positive and significant effect on the revealing rate of senders implying that senders who correctly answer at least one of the three cognitive ability questions are more likely to reveal a draw (p=0.001). All other controls are insignificant.

[54] According to Rick and Weber (2010), 'meaningful learning' in the absence of feedback is like the learning that is produced when players are required to explain their behaviour (which is intended to increase deliberation). In other words, meaningful learning in the absence of feedback encourages players to think deeper about the game.

[55] The post-experimental questionnaire used to measure these control variables is provided in Appendix 4.C. Some of the controls (like being a native English speaker, having a friend in the session) are included in the regression to replicate the analysis of the Baseline ("No Feedback treatment") results in Jin et al. (2017).

[56] We asked subjects to answer three questions from the Cognitive Reflection Task (Frederick 2005) and coded the cognitive ability dummy included in the regression as 1 if the subject answered at least one of the three questions correctly, and 0 if no question was answered correctly by the subject. The three questions are presented in Appendix 4.C.

Model III (column III) specification includes controls and a dummy variable which takes the value of 1 if the sender's believed guess is below the actual number drawn, and 0 otherwise.[57] We include this dummy in the regression to analyse whether senders' actions are related to their beliefs about the actions of the receivers. The coefficient on sender beliefs is highly significant implying that senders reveal 10% more often if their belief of the receiver's guess upon observing non-disclosure is below the actual draw they observe (p=0.000). The coefficient on the Competition treatment remains positive and statistically significant suggesting that the effect of Competition on senders' revealing behaviour is robust, and competition increases information disclosure.

Result 4.2: Senders in the Competition treatment reveal 7-8% more than the senders in the Baseline. This is reflected in a significantly lower average non-disclosed number in the Competition treatment compared to the Baseline. However, the amount of information disclosed is still far from the complete unravelling prediction of the theory in both treatments.

In the next sub-section, we analyse receivers' actions across treatments.

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[57] Recall that we elicit senders' beliefs after every 5 rounds. Using the elicited beliefs, we calculate what the senders believe the receivers would guess upon observing non-disclosure and call it the *believed guess* of the sender. The believed guess is calculated for each sender and for every block just as we mentioned in the previous Chapters. For example, if the sender believes that upon observing non-disclosure, 30% of receivers guess 1 or 1.5, 25% guess 2 or 2.5, 20% guess 3 or 3.5, 15% guess 4 or 4.5, and 10% guess 5, then the believed guess of this sender is: $\frac{[(30)*1+(25)*2+(20)*3+(15)*4+(10)*5]}{100} = 2.50$. We assigned a value of 1 to the sender belief dummy (used in the regression) if the draw in a given round is higher than the believed guess elicited at the end of that block, and the dummy took the value of 0 otherwise.

4.3.2. Analysis of receivers' behaviour

Table 4.3 reports the average report by receivers in the Baseline and the Competition treatment. On average, receivers' report is very close to the actual draw when the draw is revealed by senders. The average guess of a non-disclosed number is 2.291 in the Baseline and 2.233 in the Competition treatment. This difference is not statistically significant (Wilcoxon rank-sum test p=0.150). Figure 4.2 illustrates the average guess of non-disclosed draws by receivers across the six blocks in the Baseline and the Competition treatment.

The difference between the average non-disclosed draw and the guess of the non-disclosed draw by receivers is 0.437 in the Baseline and 0.617 in the Competition treatment. This difference between the non-disclosed draw and the guess of the non-disclosed draw is statistically significant in both treatments implying that receivers are too optimistic in their guesses of the non-disclosed draw (Wilcoxon signed-rank test p=0.027 for both Baseline and Competition treatment). The higher difference between the guess and the non-disclosed draw in the Competition treatment is driven by a significant reduction in the average non-disclosed draw by senders.

Table 4.3: Summary of receivers' reports

Variables	Baseline		Competition	
	N	Mean	N	Mean
Guess (reveal=1)	12	1.08	36	1.02
Guess (reveal=2)	59	2.04	196	2.01
Guess (reveal=3)	137	2.95	322	3.01
Guess (reveal=4)	153	4.02	388	4.01
Guess (reveal=5)	233	4.98	425	4.99
Guess (reveal=blank)	426	2.291	733	2.233
Guess - draw (not revealed)	426	0.437	733	0.617

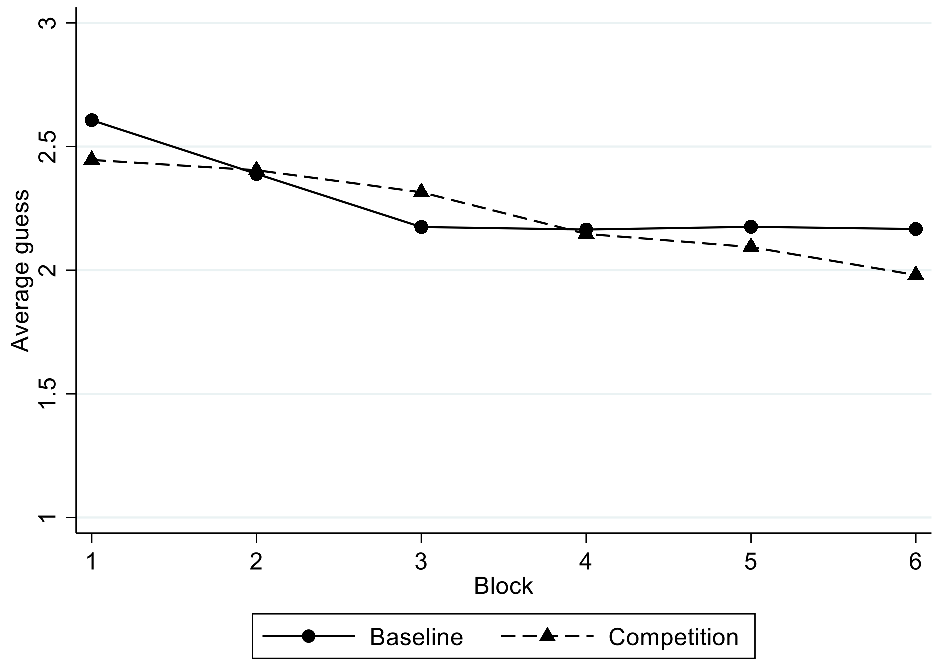

Figure 4.2: Average guess of non-disclosed numbers by receivers

The regressions in Table 4.4 confirm these results for receivers' behaviour. Models I, II and III compare receivers' behaviour across the Baseline and the Competition treatment when they observe non-disclosure. The regressions for receivers are estimated using the Ordinary Least Squares (OLS) method. The standard errors are clustered at the matching group level.

Model I (column I) is a specification without controls that examines receivers' guess of the non-disclosed number across the Baseline and the Competition treatment. The coefficient on the Competition treatment dummy is negative but not statistically significant (p=0.504). The Round variable is negative and significant implying that receivers learn to guess lower over rounds with experience of playing the game and this is robust across specifications.

Table 4.4: Regressions on receivers' behaviour

Variables	Receiver guess of non-disclosed numbers		
	I	II	III
Competition treatment	-0.046	-0.022	-0.079
	(0.067)	(0.107)	(0.154)
Round	-0.017***	-0.017***	-0.012*
	(0.004)	(0.004)	(0.006)
Implied guess calculated using receivers' beliefs			0.546***
			(0.065)
Round*Competition			0.0005
			(0.007)
Constant	2.553***	3.371***	1.877***
	(0.063)	(0.232)	(0.249)
Controls	No	Yes	Yes
Observations	1,159	1,159	1,159
R-squared	0.040	0.141	0.243

Note: OLS for receivers. Robust standard errors in parentheses (clustered at the matching group level). Controls include: sex, field of study (Economics), native English speaker, having a friend in the session, risk aversion, cognitive ability questions, negative reciprocity, and reciprocity. ***p<0.01, **p<0.05, *p<0.1.

Model II (column II) builds on Model I by including socio-demographic controls such as sex, field of study being economics, being a native English speaker, having a friend in the session, risk aversion, cognitive ability, negative reciprocity and general level of reciprocity that were measured by a questionnaire. We include these controls for the same reasons mentioned in the previous Chapters. We conjecture that subjects motivated by reciprocal concerns guess lower upon observing non-disclosure to punish senders for not revealing a draw.[58]

We find that the Competition treatment dummy remains insignificant. In addition, we find that most controls are insignificant, except having a friend in the session and the reciprocity term which are negative and significant (p=0.024 and p=0.004 respectively). This implies that receivers who have a friend in the

[58] The reciprocity dummies included in the regression take the value of 1 for the receivers with a score of 5 and above on the reciprocity questions, and 0 otherwise. Subjects with a score of 5 and above on the reciprocity questions are regarded as reciprocal. The specific questions are provided in Appendix 4.D and are taken from Falk et al. (2016).

same session guess 0.236 lower upon observing non-disclosure than receivers without a friend in the session. Receivers who are reciprocal guess 0.585 lower than receivers who are not reciprocal.

Model III further includes an interaction term between Round and Competition treatment to examine whether receivers in the Competition treatment guess lower over rounds upon observing non-disclosure. This is because receivers in the Competition treatment interact with two senders in a given round as opposed to one sender in the Baseline and therefore, observe more disclosure of draws over rounds. This might affect receivers' guess of non-disclosed draw, i.e. make them guess lower in the Competition treatment in the later rounds of the experiment. Model III also includes the implied guess of the receiver to examine whether their guesses are influenced by the beliefs they hold about the non-disclosed draw.[59]

The coefficient on receiver's beliefs about the non-disclosed number is positive and significant (p=0.000). This suggests that receivers' guess of the non-disclosed number increases by 0.546 with a one point increase in their belief of the non-disclosed number. This also gives us a measure of the receivers' ability to best respond to their beliefs about the revealing behaviour of the senders. The interaction term between Round and Competition treatment is not significant however, implying that receivers' guess of the non-disclosed draw

[59] Recall that we calculate what the receivers believe the average non-disclosed draw is and call it the *implied guess* of the receiver. The implied guess is calculated for each receiver and for every block as follows: For example, if the receiver believes that the sender revealing rate is 10% for a private number of 1, 40% for 2, 70% for 3, 90% for 4, and 95% for 5, then the implied guess of this receiver conditional on non-disclosure is:

$$\frac{[(100-10)*1+(100-40)*2+(100-70)*3+(100-90)*4+(100-95)*5]}{[(100-10)+(100-40)+(100-70)+(100-90)+(100-95)]} = 1.871$$

over rounds in the Competition treatment does not differ compared to the guess of the receivers in the Baseline (p=0.942).

Result 4.3: Despite higher revealing rates in the Competition treatment, there is no significant difference in the average guess of the non-disclosed number by receivers across the Baseline and the Competition treatment.

We conjecture that the variance of receivers' guesses of non-disclosed numbers will be larger in the Competition treatment compared to the Baseline. This is because receivers in the Competition treatment have a more complex decision-problem to solve as they have to base their guesses of non-disclosed numbers on not only what they think the senders' beliefs are about their guess, but also what they think the senders' beliefs are about the other sender's revealing behaviour. We confirm this using a variance ratio test which compares the variances of the two distributions that show the receivers' guess of non-disclosed numbers in Figure 4.3 below. In line with our conjecture, the result suggests that the variance of the guesses in the Competition treatment is significantly higher than the variance of the guesses in the Baseline (p=0.004).

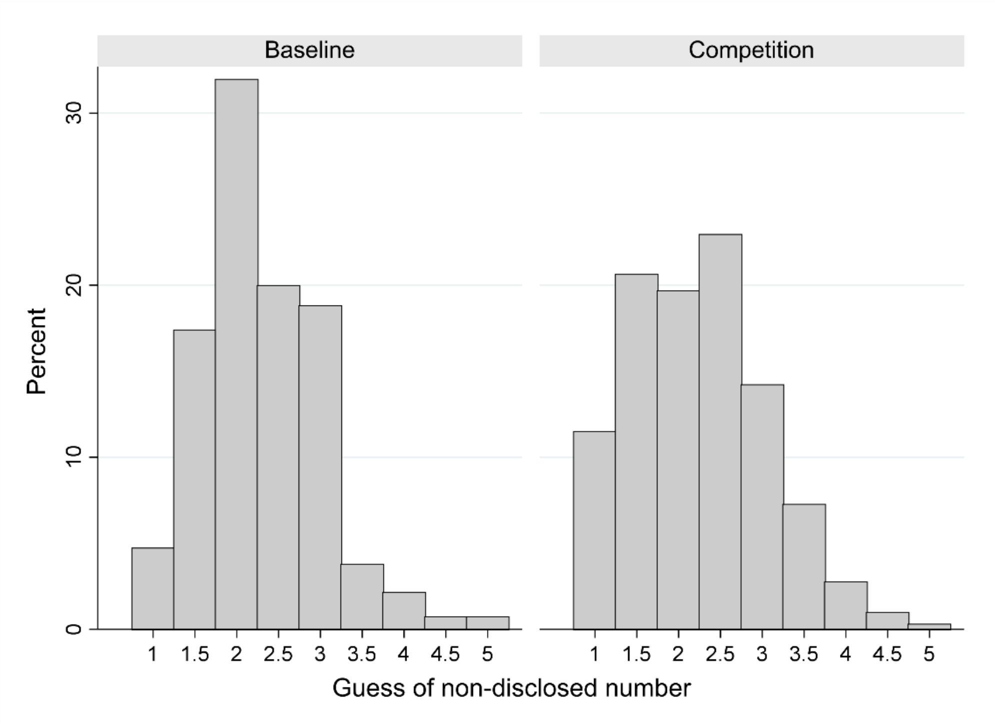

Figure 4.3: Distribution of guesses of non-disclosed numbers

In the next section, we examine receivers' choice of the preferred sender in the Competition treatment.

4.3.3. Analysis of receivers' choice of the sender

Table 4.5 provides descriptive statistics of receivers' choice of the preferred sender (in percentages) in the Competition treatment. Recall that receivers are incentivised for accurate reports of the senders' draw, regardless of what the draw actually is. Consistent with these incentives, when one of the two senders reveals the draw, but the other doesn't, receivers tend to prefer the sender who reveals the draw. When both senders reveal, receivers seem to prefer senders who reveal high draws, while they choose less frequently senders who reveal low draws. The preference for senders who reveal higher draws may be explained by receivers' concern to maximise total payoffs – senders with high revealed draws earn more than senders with low revealed draws. When both draws are not revealed, we expect receivers to choose the preferred sender randomly. This expectation is confirmed as seen on the bottom right-hand side in the table where receivers choose sender 1 half of the time (50.42%) when both senders send a blank message.

Table 4.5: Descriptive statistics when receivers' choice is Sender 1 (in percentage) and the number of observations per cell (in brackets)

Sender 1/Sender 2	Reveal (draw=1)	Reveal (draw=2)	Reveal (draw=3)	Reveal (draw=4)	Reveal (draw=5)	Reveal (blank)
Reveal (draw=1)	-	0	20	0	0	100
	-	(n=1)	(n=5)	(n=1)	(n=4)	(n=2)
Reveal (draw=2)	100	55.56	10	20	13.33	97.44
	(n=1)	(n=9)	(n=20)	(n=10)	(n=15)	(n=39)
Reveal (draw=3)	75	88.89	66.67	20	9.52	98.51
	(n=4)	(n=18)	(n=12)	(n=70)	(n=21)	(n=67)
Reveal (draw=4)	75	50	92.31	48.48	26.47	97.18
	(n=4)	(n=14)	(n=13)	(n=33)	(n=34)	(n=71)
Reveal (draw=5)	66.67	80.95	78.13	89.47	42.55	97
	(n=9)	(n=21)	(n=32)	(n=38)	(n=47)	(n=100)
Reveal (blank)	20	7.69	2.08	0	1.75	50.42
	(n=5)	(n=39)	(n=48)	(n=67)	(n=57)	(n=119)

Result 4.4: Receivers choose senders that reveal the draw. When both senders disclose, most receivers choose the sender with the higher revealed draw. When neither sender reveals the draw, receivers choose randomly between the senders.

We next examine whether comparative evaluation of information leads to better guesses about non-disclosed numbers by receivers. To investigate this, we pool our data into two groups. One group consists of receivers' guesses of non-disclosed numbers when one sender does not reveal but the other sender reveals the number. The other group consists of receivers' guesses of non-disclosed numbers when both senders do not reveal the number. We average the guesses for each session in both groups (as each session is an independent unit of observation for receivers in the Competition treatment) and run a non-parametric test on this dataset. We find that receivers' guesses do not differ significantly across the two groups (Wilcoxon rank-sum test p=0.631). This suggests that comparative evaluation of information does not lead to significantly different guesses of non-disclosed numbers relative to the situations where receivers guess in the absence of any information (i.e. number) alongside the non-disclosed number.

Result 4.5: Comparative evaluation of information does not affect receivers' guesses of non-disclosed numbers.

4.3.4. Analysis of receivers' payoffs

In this sub-section, we examine the effect of competition between senders on the welfare of receivers. To do this, we analyse receivers' welfare across treatments using their realised payoffs. The realised payoff is the payoff that the receiver earns after the computer selects the payoff-relevant sender.[60] We find that the realised payoff of receivers in the Competition treatment is significantly higher than the realised payoffs of the receivers in the Baseline (Wilcoxon rank-sum

[60] The realised payoffs in the Competition treatment are determined by the computer's selection of the payoff-relevant sender, i.e. with a 75% probability, receiver's preferred sender is selected by the computer, and with a 25% probability the sender who is not preferred by the receiver is selected by the computer as the payoff-relevant sender.

test p=0.010). This implies that receivers are better-off in the Competition treatment compared to the Baseline in terms of their realised payoffs.

We identify three potential channels that affect receivers' payoffs in the Competition treatment compared to the Baseline. First, senders reveal more in the Competition treatment than in the Baseline which helps receivers earn a higher payoff. Second, receivers do worse under Competition than in the Baseline when they are matched with a sender that did not disclose the draw. This is because receivers do not guess lower in the Competition treatment compared to the Baseline, while the actual non-disclosed numbers are lower in the Competition treatment.[61] Third, since receivers could choose a sender in the Competition treatment, they could do better by reducing the probability of them being matched with a non-disclosing sender.

Therefore, a more conservative comparison of payoffs switches off the third channel by considering the average payoffs of receivers in the Competition treatment and comparing it to the payoffs of receivers in the Baseline. The average payoffs of receivers in the Competition treatment are calculated by placing an equal weight on the receiver's reports of both senders' draws, i.e. by taking the average of the payoffs that result from the receiver's report of Sender 1's draw and of Sender 2's draw in a given round of the experiment. This method of comparison assumes away the possibility of choosing a sender and provides a conservative estimate of receivers' welfare. We find that, even after abstracting away from the possibility of choosing a sender, receivers earn significantly more in the Competition treatment than in the Baseline and this is marginally

[61] Recall that receivers' payoff function implies that the larger the difference between the report of the private number and the true private number, the lower is the payoff for the receiver.

significant (Wilcoxon rank-sum test p=0.054). This implies that increased disclosure by senders in the Competition treatment outweighs the fact that receivers do worse when the number is not disclosed to them in the Competition treatment. In conclusion, despite a conservative estimate of receivers' welfare obtained using average payoffs, we find a positive and (marginally) significant effect of competition on the payoffs of receivers.

Result 4.6: Receivers in the Competition treatment are better-off (in monetary terms) compared to the receivers in the Baseline despite no significant differences in their guesses about the non-disclosed number. This is largely driven by higher revealing rates in the Competition treatment, accompanied with accurate reports of the revealed draws by the receivers.

4.3.5. Beliefs

We now analyse the role of beliefs in driving the behaviour of senders and receivers in the Baseline and the Competition treatment. Senders' beliefs are summarised in Table 4.6. On average, the believed guess of senders is 2.213 in the Baseline and 2.184 in the Competition treatment. The difference between the senders' believed guess across treatments is not statistically significant (Wilcoxon rank-sum test p=0.827).[62] We analyse what percentage of the observations were revealed when it was optimal to do so, i.e. when the believed guess was less than the draw. We find that the senders reveal optimally 82.18%

[62] We also calculate the believed guess of the sender by taking 1.5 (instead of 1), 2.5 (instead of 2), 3.5 (instead of 3), 4.5 (instead of 4) and 5 as the guess of the receiver. In Appendix 4.A, we reproduce Table 4.6 with numbers from this calculation (see Table 4.3A).

in the Baseline and 88.78% in the Competition treatment and this difference is statistically significant (Wilcoxon rank-sum test p=0.006).

In the Baseline, the optimal action was not to reveal the draw when the believed guess was greater or equal to the draw, provided senders are risk-neutral. 86.03% of the observations in the Baseline correspond to the optimal action of not revealing. In the Competition treatment, when the believed guess is greater or equal to the draw, we cannot draw inferences about the optimal action because the revealing behaviour of the sender would also depend on their beliefs about the revealing behaviour of the other sender in the pair. 77.19% of the observations in the Competition treatment were not revealed by senders and this is not significantly different from the withholding rates in the Baseline, i.e. 86.03% (Wilcoxon rank-sum test p=0.197).[63]

Table 4.6: Summary of elicited beliefs of senders

	Baseline	**Competition**
% of observations revealed when believed guess < draw	**82.18**	**88.78**
Rounds 1-10	79.13	85.26
Rounds 11-20	79.31	88.86
Rounds 21-30	87.95	92.11
% of observations not revealed when believed guess >= draw	**86.03**	**77.19**
Rounds 1-10	88.06	70.27
Rounds 11-20	87.04	77.69
Rounds 21-30	82.76	84.02

Result 4.7: Senders in the Baseline and the Competition treatment have similar believed guesses. When the optimal action is to reveal the draw, senders in the

[63] The difference between the believed guess and the actual guess is not statistically significant (Wilcoxon signed-rank test p=0.600 in both treatments). This implies that senders in both treatments form correct beliefs about the receivers' guess conditional on non-disclosure.

Competition treatment reveal significantly more often than the senders in the Baseline.

Receivers' beliefs about the sender revealing rate and the actual sender revealing rate across the Baseline and the Competition treatment are shown in Figure 4.4. The implied guess is 1.985 in the Baseline and 2.088 in the Competition treatment and the difference between the implied guesses across treatments is not statistically significant (Wilcoxon rank-sum test p=0.521). Receivers' beliefs about senders' revealing rate are miscalibrated for intermediate draws in the Baseline and for intermediate and high draws in the Competition treatment. The insufficient scepticism about non-disclosed information, or in other words, strategic naivety, among receivers stems from miscalibrated beliefs about intermediate and high draws as opposed to their beliefs about the revealing rate of low draws, as we also find in the previous Chapters.

These miscalibrated beliefs of receivers indicate that the average belief of the non-disclosed draw (1.985 in Baseline and 2.088 in the Competition treatment) is higher than the actual average non-disclosed draw (1.846 in Baseline and 1.582 in the Competition treatment), and these differences are statistically significant (Wilcoxon signed-rank test p=0.074 for Baseline and p=0.027 for the Competition treatment). We refer to this difference as strategic naivety following Jin et al. (2017). Put differently, receivers do not sufficiently infer that non-disclosure of a draw implies that the draw is low.[64]

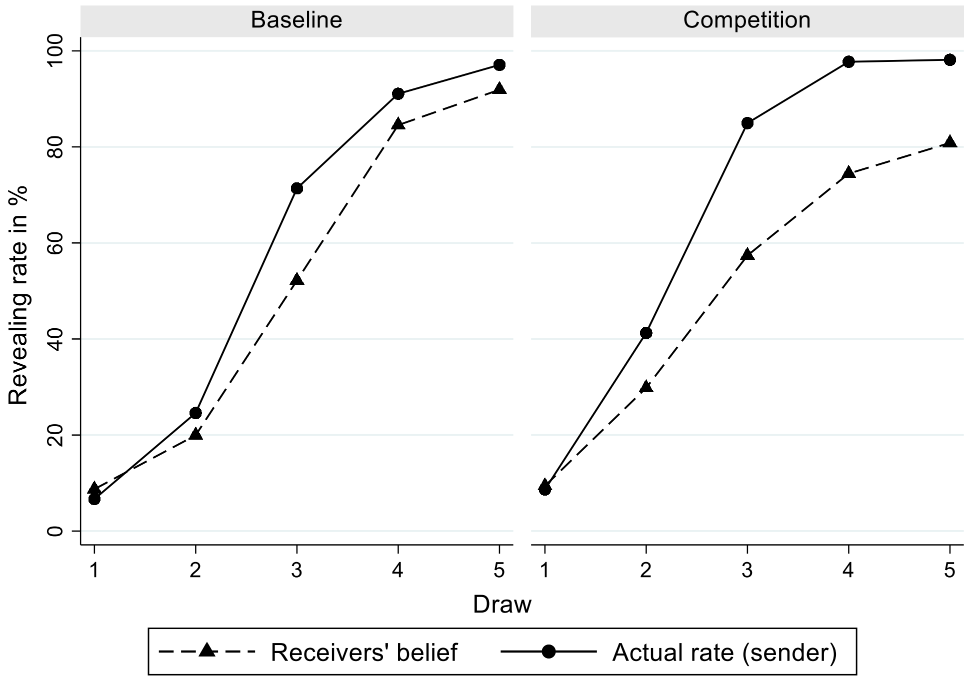

Figure 4.4: Actual revealing rate and receivers' belief about revealing rate

Result 4.8: The implied guess of receivers does not differ significantly across treatments. Strategic naivety in receivers stems from miscalibrated beliefs about the senders' revealing rate of intermediate and high draws as opposed to beliefs about the revealing rate of low draws.

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[64] Receivers in the Baseline do not best-respond to their incorrect beliefs, and in fact their guesses are much higher than their beliefs indicate (Wilcoxon signed-rank test p=0.027 between implied guess and actual guess). Receivers in the Competition treatment best-respond to their incorrect beliefs about the non-disclosed number (Wilcoxon signed-rank test p=0.173 between implied guess and actual guess). See Figure 4.1A and Figure 4.2A in Appendix 4.A for a graphical representation of these statistics.

As in Chapter 2, we now explore whether receivers' beliefs about the non-disclosed number are systematically explained by their characteristics. To do this, we regress receiver's implied guess on the Competition treatment dummy and the characteristics of receivers such as sex, field of study being economics, risk aversion, cognitive ability questions, negative reciprocity (Falk et al. 2016), and the level of scepticism (Hurtt 2010) measured by a questionnaire. We do a median split of the receivers' score on the Hurtt's scepticism scale (Hurtt 2010) and calculate a dummy variable which takes the value of 1 if the receiver's scepticism score is above the median scepticism score, and 0 otherwise. We think that the receivers who score high on the scepticism scale tend to form more accurate, i.e. sceptical beliefs about the non-disclosed number. We present the regression results in Table 4.7 below.

We find that the implied guess of the receivers with a higher cognitive ability is lower and this implies that these receivers are closer to accuracy given the average revealing behaviour of senders (p=0.002). Our results are similar to those in Gill and Prowse (2016) and Li and Schipper (2018) who find a positive relationship between cognitive ability and participants' level of reasoning in a *p*-beauty contest game. As mentioned earlier, the implied guess of receivers is not significantly different across treatments and this is confirmed with an insignificant Competition treatment dummy (p=0.376).

Result 4.9: Receivers with a higher cognitive ability hold lower, and therefore more accurate beliefs about the non-disclosed number.

Table 4.7: Regression on the implied guess of receivers

Variables	OLS
Competition treatment	0.110
	(0.119)
Male	0.069
	(0.123)
Field of study being Economics	-0.196
	(0.134)
Cognitive ability	-0.450***
	(0.108)
Scepticism	-0.160
	(0.169)
Constant	2.355***
	(0.155)
Observations	624
R-squared	0.193

Note: Regression estimated using OLS. Robust standard errors in parentheses (clustered at the matching group level). ***p<0.01, **p<0.05, *p<0.1.

4.4. Discussion and conclusion

This Chapter examines whether introducing competition between senders to influence the receiver's decision increases information disclosure. In addition, this Chapter also explores whether comparative evaluation of information affects the guesses of non-disclosed numbers by receivers. In general, we find that competition increases disclosure of information but has no significant effect on the inferences or guesses receivers make about non-disclosed information.

We provide experimental evidence to examine whether competition in the market among information senders can lead to the "truths' becoming known…" by substituting the need for receiver scepticism in generating unravelling (Milgrom and Roberts 1986). In our experiment, we examine whether senders reveal more information under competition than without competition. We find that senders who must compete to be chosen by the receivers disclose significantly more than the senders who do not have to compete. This is largely driven by differences in the revealing rates of intermediate draws. The higher rate of information disclosure under competition is consistent with the results in Penczynski and Zhang (2017) who also find that senders in a duopoly reveal more pieces of information in comparison with the senders in a monopoly market structure.

Our results are in contrast to those in Jin (2005) who empirically studies whether Health Maintenance Organisations (HMOs) voluntarily disclose quality information through National Committee of Quality Assurance (NCQA). She finds that the primary reason for disclosing via NCQA is to differentiate from competitors, and in relatively competitive areas, HMOs are less likely to disclose via NCQA over time. We conjecture that the difference between our results is because HMOs could build a reputation over time based on quality in their local areas over time, and therefore, compete on a different dimension (such as price as opposed to quality) to attract consumers. In contrast, we prevented the possibility of senders forming a reputation over rounds by design (i.e. implementing random re-matching) and gave them no other dimension to compete on. Therefore, we find that competition increases disclosure of information in situations characterised by our setting.

On the receivers' side, we explore whether comparative evaluation of information, which is a feature of introducing competition between senders, leads to increased scepticism in receivers' and lower guesses about non-disclosed numbers. We find that receivers who are presented with information in a comparative manner do not guess significantly lower upon observing non-disclosure than when they do not directly have the option of evaluating information in a comparative manner. Receivers also do not form significantly different beliefs about non-disclosed numbers in the Competition treatment compared to the Baseline.

We believe that comparative evaluation may not have helped in our set-up due to two reasons. First, it is possible that receivers may not have engaged with the "additional" and readily available comparative information provided to them with the presence of another sender alongside. This is because receivers may already have had enough information to consider while making their decisions - such as the payoff table and the draws revealed by senders in the experiment that the receiver was matched with. Therefore, receivers may be imperfectly attentive or cognitively limited to consider any additional information provided to them while making their decisions. Second, receivers' may have thought about what information *is* revealed rather than what *is not* revealed during the belief elicitation stage. In other words, framing the belief questions differently may have an impact on the way receivers interpret the non-disclosed information, and this is open to future research.[65]

Interestingly, we find that receivers earn a higher payoff in the Competition treatment than in the Baseline despite no significant differences in their guesses of non-disclosed numbers. The results regarding higher welfare in the Competition treatment are in contrast with the results in Penczynski and Zhang (2017). We reason that the difference may be due to the nature of the games that subjects play in both experiments. In Penczynski and Zhang (2017), buyers bid a price for the seller's product and in the process, were required to

[65] Currently, we elicit beliefs about senders' revealing behaviour by asking receivers to think about how many senders *revealed* a given draw. The alternative would be to elicit these beliefs by asking receivers to think about how many senders *did not* reveal a given draw.

discount for the publication bias of the disclosed pieces of evidence from the set of all available pieces of evidence that the sellers had available to them. In contrast, the decision problem for the receivers in our experiment is simpler compared to theirs, i.e. receivers choose the number if the seller disclosed it to earn the highest possible payoff, and guess in the absence of disclosure from the sender.

In conclusion, we provide experimental evidence that shows competition between senders increases information disclosure even when receivers are naïve about non-disclosed information.

CHAPTER 5:

CONCLUSION

5.1. Overview

This doctoral thesis provides fundamental insights into the robustness of strategic naivety about non-disclosed information, and the conditions under which there can be increased disclosure of information. This final Chapter summarises the key advances made, highlights the findings obtained in the individual Chapters of the thesis and their potential limitations, and outlines the possible directions for future research.

5.2. Summary of key contributions

The existing literature documents various reasons for the failure of complete unravelling of information, such as strategic and dynamic incentives of sellers (Grubb 2011), and countersignalling with multiple dimensions of quality (Feltovich et al. 2002). However, to date, relatively little research has been conducted to establish the link between strategic naivety and the failure of complete unravelling of information. In a seminal experimental study, Jin et al. (2017) document this relationship with a simplified yet rich setup which captures the main essence of the theory.

In a first step, we build on their research and provide new evidence confirming the robustness of strategic naivety and examining the extent to which it responds to systematic manipulations of the decision environment on both sides of the market ("consumers/receivers" and "firms/senders"). We establish the robustness of strategic naivety to the inclusion of natural elements in the experimental setup which are commonly observed in natural settings. We also indirectly provide evidence suggesting that providing communication opportunities to reduce cognitive constraints does not attenuate naivety. To this end, we use well-proven findings from the social psychology and experimental economics literature on the differences between decisions made by individuals in isolation and after consulting with others to investigate whether consultation helps attenuate strategic naivety about non-disclosed information.

A second contribution is made to the methodological framework employed in experimental economics. We do this by examining whether strategic naivety is an artefact of abstract experiments and if 'in-context' and 'context-free' experiments generate significantly different behavioural responses from subjects in the experiment. While the existing literature is divided on the effect of context on the responses of subjects, there are few papers that examine the mechanism that mediates the effect of context on behaviour. By contrast, we examine not only whether context affects subjects' behaviour, but also whether the effect is mediated by the beliefs that subjects hold about the actions of the other player in a strategic setting. We find no effect of context on subjects' behaviour, but show that, for certain types of players (senders in our experiment), context affects beliefs and enables subjects to best-respond to their beliefs about the actions of the other player.

A third contribution we make with this doctoral thesis is to identify a market setting to increase disclosure of information in spite of the presence of naïve receivers. To investigate this, we compare a setting where a sender competes with another sender to be chosen by the receiver (which can be thought of as firms competing to trade with a receiver) to a setting without any competition between senders. We find that when receivers prefer senders that disclose information over those that do not, senders start competing in disclosing information to receivers, thereby increasing the overall amount of information that is disclosed. This finding can inform policy-makers about conditions under which unravelling of information is more likely to occur. Regulatory authorities can use the tools at their disposal to facilitate competition between firms to increase information disclosure and consumer welfare, particularly in environments that are closely captured by our setup.

5.3. Summary of findings

Our findings suggest that complete unravelling fails to occur in all treatments, i.e. many senders with a number higher than the lowest possible number choose not to reveal to the receivers. However, competition between senders increases information disclosure relative to a treatment without competition. Furthermore, receivers' reports about the non-disclosed number do not differ significantly between treatments, i.e. between the No Consultation and the Consultation treatments, between the No Context and the Context treatments, and between the Baseline and the Competition treatment. Despite no significant difference between receivers' guesses of non-disclosed numbers between the Baseline and the Competition treatment, we find that receivers are better-off under Competition than without it.

Regarding senders' beliefs that we elicit in the experiment, we find that senders in the No Consultation (No Context) treatment, Consultation treatment, the Baseline and the Competition treatment form correct beliefs about receivers' guess conditional on non-disclosure, however, they do not always best-respond to their beliefs. On the contrary, senders in the Context treatment do not form accurate beliefs about receivers' guess conditional on non-disclosure (i.e. they think receivers will guess lower than they actually do) but they best-respond to their beliefs by withholding the lowest draw, i.e. draw 1, more often.

Regarding receivers' beliefs that we elicit in the experiment, there is a statistically significant difference between receivers' beliefs about the non-disclosed number and the actual non-disclosed number in all treatments implying that strategic naivety is a robust phenomenon. In particular, in all treatments we find that strategic naivety stems from receivers' miscalibrated beliefs about senders' revealing behaviour of intermediate and high numbers rather than beliefs about senders' revealing behaviour of low numbers. Put differently, receivers do not sufficiently infer that non-disclosure contains bad news and this stems from their incorrect belief that senders with high numbers do not disclose enough, while being sufficiently pessimistic about the revealing behaviour of senders with low numbers.

5.4. Potential limitations and directions for future research

The findings of Chapters 2 and 3 suggest that strategic naivety is difficult to attenuate in experimental settings. Therefore, the robustness of naivety casts doubts on the effectiveness of voluntary disclosure policies to meet their intended effects of full disclosure of information through receivers' sceptical inferences about non-disclosed information. It would be beneficial for future research to further examine *why* naivety exists in the first place. Additionally, though the restaurant hygiene grade context best captures the incentives and essence of the theoretical setting given our subject pool, it would be interesting to explore whether different manipulations of context generate responses from subjects that are consistent with the unravelling prediction.

We recognise that the results of Chapter 4 apply to a subset of cases, and there could be instances where the effect of competition on information disclosure might differ from what we find. This could include cases where the incentives of receivers are based on selecting the best sender and not on the accuracy of their reports of the sender's draw (as in our experiment). In such cases, it is unclear ex-ante whether competition would increase disclosure of information. It would be a fruitful direction for future research to explore whether the beliefs that senders hold about the revealing behaviour of the other sender is the driver behind the effect of competition on information disclosure.

We also acknowledge that our treatment with competition between senders changes two things at the same time compared to the Baseline (i.e. introducing an additional sender as well as the option for receivers to choose a preferred sender). A clearer evaluation of treatments might have been to compare a treatment with one sender and one receiver ('Baseline' treatment) with a treatment comprising of two senders and one receiver without including the option to choose a preferred sender ('No choice' treatment). One could then compare the 'No choice' treatment with a treatment comprising of two senders and one receiver with the option to choose a preferred sender (as our 'Competition' treatment). However, we believe that eliminating such a 'No choice' treatment from our analysis would have only been a cause for concern if we had found a significant difference in receivers' behaviour between the Baseline and the Competition treatment. This is because we would have had to identify whether the significant difference in guesses about the non-disclosed number is due to the choice available to receivers, or due to observing more numbers being disclosed by two senders over rounds in the Competition treatment as opposed to one sender in the Baseline.

It would also be beneficial for future research is to investigate which theoretical model best explains the data from our experiments, such as a model based on level-*k* reasoning (Stahl and Wilson 1995), a model based on the concept of cursed equilibrium (Eyster and Rabin 2005), or a model based on the concept of quantal response equilibrium (Mckelvey and Palfrey 1998). Understanding the drivers behind strategic naivety and incomplete disclosure of information therefore remains an open and promising research agenda whose findings have the potential to yield important policy implications. This doctoral thesis forms a substantial basis for this agenda.

APPENDIX TO CHAPTER 2

APPENDIX 2.A

Table 2.1A: Summary statistics

Variables	N	Mean	Standard deviation
Consultation treatment (dummy)	6120	0.500	0.500
Economics (dummy)	6120	0.127	0.333
Male (dummy)	6120	0.392	0.488
Native English (dummy)	6120	0.441	0.496
Friend in the session (dummy)	6120	0.161	0.368
Cognitive ability (dummy=1 if at least 1 correct answer)	6120	0.686	0.464
Age	6120	21.35	3.728

Table 2.2A: Payoff tables of the game

Points S, **R1**	**R 1's report: 1**	**R 1's report: 1.5**	**R 1's report: 2**	**R 1's report: 2.5**	**R 1's report: 3**	**R 1's report: 3.5**	**R 1's report: 4**	**R 1's report: 4.5**	**R 1's report: 5**
Private number 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Private number 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Private number 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Private number 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Private number 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

Points S, **R2**	**R 2's report: 1**	**R 2's report: 1.5**	**R 2's report: 2**	**R 2's report: 2.5**	**R 2's report: 3**	**R 2's report: 3.5**	**R 2's report: 4**	**R 2's report: 4.5**	**R 2's report: 5**
Private number 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Private number 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Private number 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Private number 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Private number 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

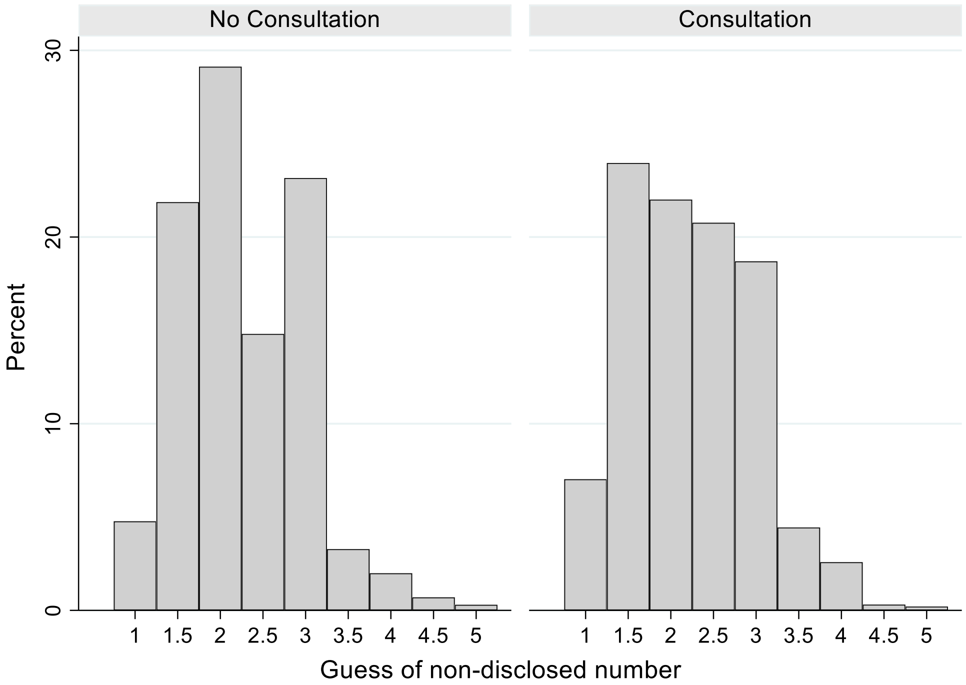


Figure 2.1A: Distribution of guesses of non-disclosed numbers

Table 2.3A: Summary of elicited beliefs of senders

	No Consultation	Consultation
% of observations revealed when believed guess < draw	**84.90**	**82.39**
Rounds 1-10	82.35	76.10
Rounds 11-20	73.93	88.70
Rounds 21-30	87.65	81.65
% of observations not revealed when believed guess >= draw	**83.18**	**75.48**
Rounds 1-10	80.21	75.14
Rounds 11-20	78.18	69.94
Rounds 21-30	84.27	80.77

Note: The believed guess is calculated taking the higher point of the interval (Table 2.9 is calculated taking the lowest point of the interval).

APPENDIX 2.B: Experimental Instructions

Welcome

Thank you for taking part in this experiment on decision-making. For participating in this experiment, you will be paid a £4 show up fee. Moreover, you will be paid an additional amount of money that will depend on yours and other participants' decision, and on chance. You will be paid in cash, privately at the end of the experiment.

Please silence and put away your mobile phones now.

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

During this experiment we will calculate your earnings using points. For your final payment, your earnings will be converted into Pounds at the ratio of 75:1 (75 points=£1). Any negative earnings you may make during the experiment will be subtracted from your show-up fee of £4.

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. If you have any questions during this period, please raise your hand and the experimenter will come to you to answer your question.

Instructions

The experiment consists of 6 blocks with 5 rounds in each block, making it a total of 30 rounds. At the end of each block, you will be asked to answer 5 questions, and at the end of the final block, you will be asked to fill out a questionnaire.

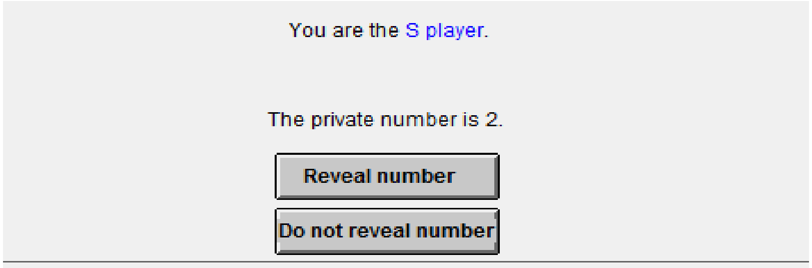
At the beginning of block 1, some of you will be randomly assigned to be the S Player and the others to be the R Player. If you are assigned to be the R Player, you will further be assigned to be either R Player 1 or R Player 2. You will remain in these roles for the entire duration of this experiment. The computer will randomly pair one R Player 1 with one R Player 2 and you will remain in these pairs for the entire duration of this experiment.

Decision making stage

In each round, one S Player will be randomly matched with a pair of R Players and in each new round, the S Player will be matched with a new pair of R players. You will not learn the identity of the other participants you are matched with, nor will the other participants learn your identity.

For every matching of an S Player with a pair of R Players, the computer program will generate a private number that is randomly drawn from the set {1, 2, 3, 4, 5}. Each of these numbers is equally likely to be generated. The computer will then send the private number to the S Player. After receiving this number, the S Player will choose whether or not to reveal the private number to both the R Players. If the S Player chooses to reveal the number, both the R Players will receive this message from the S Player: "The number I received is:" followed by the actual private number. Otherwise, both the R Players will receive no message.

This is a screenshot of the S Player's screen. As you can see, the S Player **cannot lie or misreport** the true private number.



After seeing the message from the S Player, each of the two R Players will independently report the value of the private number revealed by the S Player. If the private number is not revealed by the S Player, the R Players will guess the value of the private number. R Players can report the following numbers {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. The numbers {1.5, 2.5, 3.5, 4.5} can be used, for example, when the R Players are unsure about the true value of the private number.

Earnings for the decision making stage

The S Player will earn the sum of the points that result from the reports of both the R Players. The R Players will earn points based on their own report and the true value of the private number. The specific earnings are shown in the two tables below, which are displayed again before the S Player and the R Players make their decisions. In each cell of the two tables, the points for the S Player are on the left, and the points for the R Players are on the right. As you can see from the tables, the S Player earns more when the R Players make a higher report, and each of the R Players earns more when their report is closer to the true private number. The two tables are identical for both the R Players and the S Player.

The tables show the earnings in points for the **S Player** and **R Player 1**.

Points **S, R1**	**R 1's report: 1**	**R 1's report: 1.5**	**R 1's report: 2**	**R 1's report: 2.5**	**R 1's report: 3**	**R 1's report: 3.5**	**R 1's report: 4**	**R 1's report: 4.5**	**R 1's report: 5**
Private number 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Private number 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Private number 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Private number 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Private number 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

The table shows the earnings in points for the **S Player** and **R Player 2**.

Points **S, R2**	**R 2's report: 1**	**R 2's report: 1.5**	**R 2's report: 2**	**R 2's report: 2.5**	**R 2's report: 3**	**R 2's report: 3.5**	**R 2's report: 4**	**R 2's report: 4.5**	**R 2's report: 5**
Private number 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Private number 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Private number 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Private number 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Private number 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

Example – Earnings for a decision making stage

Suppose in Round 18 of the experiment:

The computer program generates a private number: **3** and sends it to the S Player.

The S Player chooses not to reveal the private number to the R Players.

Hence, both the R Players see no message on their screen

R Player 1 guesses the value of the private number as: **4**

R Player 2 guesses the value of the private number as: **2.5**

Therefore,

S Player earns = Points resulting from R Player 1's guess + Points resulting from R Player 2's guess

= 45 + 19

= **64 points** (for Round 18)

R Player 1 earns = **90 points**

R Player 2 earns = **102 points**

Now please answer the following questions. After you have finished answering all the questions, please raise your hand and the experimenter will come to you to check your answers.

1. Suppose you are R Player 1 and the S Player sends the following message to you: *"The number I received is: 3"*. Which number that you report will earn you the highest points? ______

2. Suppose you are R Player 2 and the S Player sends the following message to you: *"The number I received is: 3"*. Which number that you report will earn you the highest points? ______

3. Suppose you are the S Player and you send the following message to the R Players: *"The number I received is: 2"*. Which number that R Player 1 or R Player 2 reports will earn you the highest points? ______

4. Suppose the S Player did not reveal the private number 4. R Player 1 guesses 2.5 and R Player 2 guesses 3. What would be:

   a) S Player's earnings: _____

   b) R Player 1's earnings: _____

   c) R Player 2's earnings: _____

[Discussion Stage - *Only Consultation treatment*

Before the R Players submit their own report about the value of the private number, they will have **60 seconds** to use a computerised chat program to get help from or offer help to the other R Player about the report they want to submit. Messages will be shared only among the two R Players that are paired together. That is, a pair of R Players will not be able to see the messages exchanged between another pair of R Players. The S Players cannot see any messages that are shared between any pair of R Players. If both the R Players have finished discussing before the 60 seconds lapse, they can click on the **"Leave chat"** button on the chat window to enter their reports for that round.

Except for the following restrictions, R Players can type whatever they want in the chat window.

Restrictions on messages:

1. You must not identify yourself or send any information that could be used to identify you.

2. You must not make any threats, insult or use any obscene or offensive language.

If you violate these rules, your payment will be forfeited.]

Questions at the end of each block

At the end of each block, S Players will answer the following 5 questions:

Think about the last 5 rounds. When the S Players did not reveal the private number, what percentage of R Players in this room do you think reported the number:

a) 1 or 1.5:

b) 2 or 2.5:

c) 3 or 3.5:

d) 4 or 4.5:

e) 5:

In a block, if all S Players have revealed the private number the earnings for that block will be determined at random for the S Players.

At the end of each block, R Players will answer the following 5 questions:

What percentage of S Players do you think revealed the private number:

a) When the true private number was 1:

b) When the true private number was 2:

c) When the true private number was 3:

d) When the true private number was 4:

e) When the true private number was 5:

The following rule will be used to calculate your earnings for the answers to each of the 5 questions:

70 - (Your answer - Correct answer), if your answer is higher than the correct answer of that question

Or

70 - (Correct answer - Your answer), if your answer is lower than the correct answer of that question

Example – Earnings for the questions at the end of each block

Suppose you are the **S Player**. You answer the following question:

Think about the last 5 rounds. When the S Players did not reveal the private number, what percentage of R Players in this room do you think reported the number:

a) 1 or 1.5: ____

If your answer is: **30**

But the correct answer is: **50**

Your answer is lower than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Correct answer - Your answer)

= 70 - (50 - 30)

= 70 - 20

= **50 points**

Suppose you are one of the two **R Players**. You answer the following question:

What percentage of S Players do you think revealed the private number:

a) When the true private number was 1:

If your answer is: **45**

But the correct answer is: **30**

Your answer is higher than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Your answer - Correct answer)

= 70 - (45 - 30)

= 70 - 15

= **55 points**

Now please answer the following questions. After you have finished answering all the questions, please raise your hand and the experimenter will come to you to check your answers.

1. Suppose you are the S Player. If your answer to any one of the 5 questions above is **55** but the correct answer is **50**, what would be your earnings in points for that question? ______

2. Suppose you are R Player 1. If your answer to any one of the 5 questions above is **25** but the correct answer is **40**, what would be your earnings in points for that question? ______

Payment

At the end of the experiment, the experimenter will call two participants to roll two six-sided dice one after another. The number rolled on the two dice will determine the two blocks that all participants will be paid for. You will be paid the total earnings accumulated for the 5 rounds in the decision making stage for the block determined by the first die roll. For example, if the number rolled on the first die is 1, you will be paid the total amount you have accumulated in the 5 rounds in the decision making stage in block 1. You will be paid for the accuracy of your answers to the 5 questions at the end of the block determined by the second die roll. For example, if the number rolled on the second die is 2, you will be paid for the accuracy of your answers to the 5 questions in block 2. If the number rolled on the second die is the same as the number rolled on the first die, the second die will be rolled again. In other words, you will be paid once for the total amount you have accumulated in the 5 rounds in the decision making stage of one of the blocks, and once for the accuracy of your answers to the 5 questions in a different block.

Please raise your hand if you have any questions.

APPENDIX 2.C: Post-experimental questionnaire

General questions:

1. What is your gender?
2. Is English your first or native language?
3. How old are you?
4. Do you have a friend participating in the session today?
5. What advice would you give to a friend if they were going to take your spot in this experiment?
6. Answer if you were an S Player. How did you decide whether or not to reveal the private number?
7. Answer if you were an R Player. How did you decide which number to report or guess?
8. What is your academic major or your planned academic major?

Cognitive ability questions:

9. A bat and a ball cost £1.10 in total. The bat costs £1 more than the ball. How much does the ball cost? ______ pence
10. If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? _____ minutes
11. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half the lake? ______ days

Risk preference:

12. Please tell me, in general, how willing or unwilling you are to take risks. Please use a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use any numbers between 0 and 10 to indicate where you fall on the scale.

Negative Reciprocity:

13. On a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so", how willing are you to punish someone who treats YOU unfairly, even if there may be costs for you?

General reciprocity:

14. How well does the following statement describe you as a person? Please indicate your answer on a scale from 0 to 10, where 0 means "does not describe me at all" and a 10 means "describes me perfectly".

    When someone does me a favour I am willing to return it.

Positive reciprocity:

15. Please think about what you would do in the following situation. You are in an area you are not familiar with, and you realise that you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 20 Pounds in total. However, the stranger says he or she does not want any money from you. You have 6 presents with you. The cheapest present costs 5 Pounds, the most expensive one costs 30 Pounds. Do you give one of the presents to the stranger as a "thank-you"-gift? If so, which present do you give to the stranger?

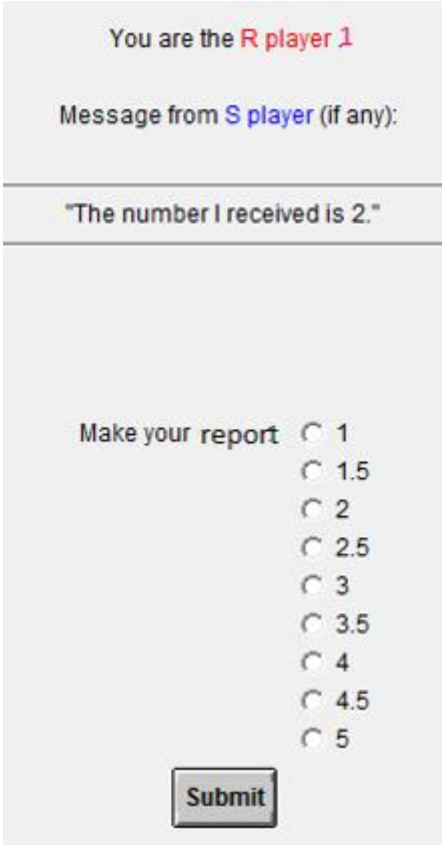
    No present
    The present worth 5 Pounds
    The present worth 10 Pounds
    The present worth 15 Pounds
    The present worth 20 Pounds
    The present worth 25 Pounds
    The present worth 30 Pounds

APPENDIX 2.D: Screenshots of players' screen

Senders' screen – No Consultation and Consultation treatments

Receivers' chat screen – Consultation treatment

Receivers' screen - No Consultation and Consultation treatments



APPENDIX 2.E: Examples of chat message categorisation

1. Receiver's action

Round	Chat	SubjectID	GroupID
1	What number are you going to choose?	713	5
1	3?	714	5
1	yeah me too I think	713	5
4	so go for 4?	711	4
4	4 for this one	710	4
4	yesss	711	4
4	i will try 3.5 this time	714	5
4	i might try 2.5	713	5
6	im gonna do 2.5	716	6
6	ill go 3,5	717	6
6	great	716	6
7	im thinking 3	701	1
7	im thinking below 2	702	1
7	cos its is an average	701	1
7	i think i will try 2,5	717	6
7	same	716	6

2. Sender's action

Round	Chat	SubjectID	GroupID
3	so what do you think the number is??	716	6
3	he hadnt reveal	717	6
3	yeah well but he revealed 2 previously	716	6
5	i guess its 1?	716	6
5	me too	717	6
6	its a different player now, so it may not be 1	716	6
9	the s player is quite conservative dont you think??	716	6
2	why isnt he revealing	805	2
2	It's v annoying that he isn't revealing	804	2
6	id say he has either 1, 2, 3	816	6
6	most likerly between 1, 2	816	6
7	they got a 1 :')	811	4
8	you knot it could have been a 5 just now..	810	4
8	either this guy keeps getting low numbers or he is a moron	816	6
8	lets hop he keeps getting low numbers	816	6

3. Equilibrium (like) reasoning

Round	Chat	SubjectID	GroupID
1	if they didnt say it that probably means low	807	3
1	if he doesnt report a number we can assume he has a low number	816	6
1	if he had a high number he would report it	816	6
2	Must be a low number again?	817	6
3	haha, maybe they are just getting all low numbers	817	6
3	surely they would reveal it if it were 4 or 5	817	6
5	must be 1 or 2 because they would reveal it again if it was 3	817	6
5	yeah id say so	816	6
2	i think its likely to be a lower number seen as they didnt reveal it this time	1008	3
2	yeah i agree	1007	3
6	I'm assuming that if they don't reveal the value then it's probably <3 and I'm not sure that's the right way to go about it	1014	5
10	i think private number must be 1 or 2	1004	2
10	if it was 3 they would have reported it	1004	2
17	Anything 3 or above will be revealed I feel	1005	2
22	Yeah I do think most people report 2	1005	2
22	yeah i think people do	1004	2
23	this must be 1 then	1004	2
23	they would report 2	1004	2

4. Payoffs

Round	Chat	SubjectID	GroupID
20	they are going to lose -14.5 points	613	5
20	so -29	613	5
9	we both report 1, get 110 each, and they get negative points	710	4
11	110 points guarunteed	710	4
13	we are on full points this round	710	4
13	hopefully this one is chosen by the dice	710	4
13	we get 90 each	1117	6
13	he gets 17	1117	6
19	why dont we choose 4, we can get 110	1205	2

APPENDIX TO CHAPTER 3

APPENDIX 3.A

Table 3.1A: Summary statistics

Variables	**N**	**Mean**	**Standard deviation**
Context treatment (dummy)	6030	0.492	0.499
Economics (dummy)	6030	0.144	0.351
Male (dummy)	6030	0.393	0.488
Native English (dummy)	6030	0.497	0.500
Friend in the session (dummy)	6030	0.208	0.406
Cognitive ability (dummy=1 if at least 1 correct)	6030	0.711	0.453
Age	6030	21.72	3.839

Table 3.2A: Summary of elicited beliefs of senders

	No Context	**Context**
% of observations revealed when believed guess < draw	**84.90**	**82.16**
Rounds 1-10	82.35	77.65
Rounds 11-20	73.93	82.29
Rounds 21-30	87.65	86.41
% of observations not revealed when believed guess >= draw	**83.18**	**84.14**
Rounds 1-10	80.21	80.79
Rounds 11-20	78.18	82.61
Rounds 21-30	84.27	89.04

Note: The believed guess is calculated taking the higher point of the interval (Table 3.6 is calculated taking the lowest point of the interval).

APPENDIX 3.B: Experimental Instructions

[Context treatment]

Welcome

Thank you for taking part in this experiment on decision-making. For participating in this experiment, you will be paid a £4 show up fee. Moreover, you will be paid an additional amount of money that will depend on yours and other participants' decision, and on chance. You will be paid in cash, privately at the end of the experiment.

Please silence and put away your mobile phones now.

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

During this experiment we will calculate your earnings using points. For your final payment, your earnings will be converted into Pounds at the ratio of 75:1 (75 points=£1). Any negative earnings you may make during the experiment will be subtracted from your show-up fee of £4.

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. If you have any questions during this period, please raise your hand and the experimenter will come to you to answer your question.

Instructions

The experiment consists of 6 blocks with 5 rounds in each block, making it a total of 30 rounds. At the end of each block, you will be asked to answer 5 questions, and at the end of the final block, you will be asked to fill out a questionnaire.

At the beginning of block 1, some of you will be randomly assigned to be the Restaurant Owner and the others to be the Restaurant Customers. If you are assigned to be the Restaurant Customers, you will further be assigned to be either Restaurant Customer 1 or Restaurant Customer 2. You will remain in these roles for the entire duration of this experiment. The computer will randomly pair one Restaurant Customer 1 with one Restaurant Customer 2 and you will remain in these pairs for the entire duration of this experiment.

Decision making stage

In each round, one Restaurant Owner will be randomly matched with a pair of Restaurant Customers and in each new round, the Restaurant Owner will be matched with a new pair of Restaurant Customers. You will not learn the identity of the other participants you are matched with, nor will the other participants learn your identity.

For every matching of a Restaurant Owner with a pair of Restaurant Customers, the computer program, which will act like the Food Standards Agency, will generate a food hygiene rating for the **Restaurant Owner**. This hygiene rating will be drawn from the set {1, 2, 3, 4, 5}. Each of these hygiene ratings is equally likely to be generated. The Food Standard Agency will then send the hygiene rating to the Restaurant Owner. After receiving this hygiene rating, the Restaurant Owner will choose whether or not to display the hygiene rating to both the Restaurant Customers. If the Restaurant Owner chooses to display the hygiene rating, both the Restaurant Customers will receive this message from the Restaurant Owner: “The food hygiene rating I received is:” followed by the actual hygiene rating. Otherwise, both the Restaurant Customers will receive no message.

This is a screenshot of the Restaurant Owner’s screen. As you can see, the Restaurant Owner **cannot lie or misreport** the true hygiene rating.

After seeing the message from the Restaurant Owner, each of the two Restaurant Customers will independently report the hygiene rating displayed by the Restaurant Owner. This can be thought of as noting down the hygiene rating of a Restaurant Owner for future restaurant visit decisions. If the hygiene rating is not displayed by the Restaurant Owner, the Restaurant Customers will guess the value of the hygiene rating. Restaurant Customers can report the following hygiene ratings {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. The hygiene ratings {1.5, 2.5, 3.5, 4.5} can be used, for example, when the Restaurant Customers are unsure about the true value of the hygiene rating.

Earnings for the decision making stage

The Restaurant Owner will earn the sum of the points that result from the report of both the Restaurant Customers. The Restaurant Customers will earn points based on their own report and the true value of the hygiene rating. The specific earnings are shown in the two tables below, which are displayed again before the Restaurant Owner and the Restaurant Customers make their decisions. In each cell of the two tables, the points for the Restaurant Owner are on the left, and the points for the Restaurant Customers are on the right. As you can see from the tables, the Restaurant Owner earns more when the Restaurant Customers make a higher report, and each of the Restaurant Customers earns more when their report is closer to the true hygiene rating. The two tables are identical for both the Restaurant Customers and the Restaurant Owner.

The tables show the earnings in points for the **Restaurant Owner** and **Restaurant Customer 1**.

Points O, C1	**C 1's report: 1**	**C 1's report: 1.5**	**C 1's report: 2**	**C 1's report: 2.5**	**C 1's report: 3**	**C 1's report: 3.5**	**C 1's report: 4**	**C 1's report: 4.5**	**C 1's report : 5**
Food hygiene rating 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Food hygiene rating 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Food hygiene rating 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Food hygiene rating 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Food hygiene rating 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

The table shows the earnings in points for the **Restaurant Owner** and **Restaurant Customer 2**.

Points O, C2	**C 2's report: 1**	**C 2's report: 1.5**	**C 2's report: 2**	**C 2's report: 2.5**	**C 2's report: 3**	**C 2's report: 3.5**	**C 2's report: 4**	**C 2's report: 4.5**	**C 1's report: 5**
Food hygiene rating 1	-14.5, 110	-3, 102	8.5, 90	19, 75	28.5, 57	37.5, 38	45, 17	51, -6	55, -29
Food hygiene rating 2	-14.5, 90	-3, 102	8.5, 110	19, 102	28.5, 90	37.5, 75	45, 57	51, 38	55, 17
Food hygiene rating 3	-14.5, 57	-3, 75	8.5, 90	19, 102	28.5, 110	37.5, 102	45, 90	51, 75	55, 57
Food hygiene rating 4	-14.5, 17	-3, 38	8.5, 57	19, 75	28.5, 90	37.5, 102	45, 110	51, 102	55, 90
Food hygiene rating 5	-14.5, -29	-3, -6	8.5, 17	19, 38	28.5, 57	37.5, 75	45, 90	51, 102	55, 110

Example – Earnings for a decision making stage

Suppose in Round 18 of the experiment:

The Food Standards Agency generates a hygiene rating: **3** for the Restaurant Owner.

The Restaurant Owner chooses not to display the hygiene rating to the Restaurant Customers.

Hence, both the Restaurant Customers see no message on their screen

Restaurant Customer 1 guesses the value of the hygiene rating as: **4**

Restaurant Customer 2 guesses the value of the hygiene rating as: **2.5**

Therefore,

Restaurant Owner earns = Points resulting from Restaurant Customer 1's guess + Points resulting from Restaurant Customer 2's guess

= 45 + 19

= **64 points** (for Round 18)

Restaurant Customer 1 earns = **90 points**

Restaurant Customer 2 earns = **102 points**

Now please answer the following questions. After you have finished answering all the questions, please raise your hand and the experimenter will come to you to check your answers.

1) Suppose you are Restaurant Customer 1 and the Restaurant Owner sends the following message to you: *"The food hygiene rating I received is: 3"*. Which hygiene rating that you report will earn you the highest points? ______

2) Suppose you are Restaurant Customer 2 and the Restaurant Owner sends the following message to you: *"The food hygiene rating I received is: 3"*. Which hygiene rating that you report will earn you the highest points? ______

3) Suppose you are the Restaurant Owner and you send the following message to the Restaurant Customers: *"The food hygiene rating I received is: 2"*. Which number that Restaurant Customer 1 or Restaurant Customer 2 reports will earn you the highest points? ______

4) Suppose the Restaurant Owner did not display the hygiene rating 4. Restaurant Customer 1 guesses 2.5 and Restaurant Customer 2 guesses 3. What would be:

a) Restaurant Owner's earnings: _____

b) Restaurant Customer 1's earnings: _____

c) Restaurant Customer 2's earnings: _____

Questions at the end of each block

At the end of each block, Restaurant Owners will answer the following 5 questions:

Think about the last 5 rounds. When the Restaurant Owners did not display the food hygiene rating, what percentage of Restaurant Customers in this room do you think reported the food hygiene rating:

a) 1 or 1.5:

b) 2 or 2.5:

c) 3 or 3.5:

d) 4 or 4.5:

e) 5:

In a block, if all Restaurant Owners have displayed the hygiene rating, the earnings for the questions in that block will be determined at random for the Restaurant Owners.

At the end of each block, Restaurant Customers will answer the following 5 questions:

Think about the last 5 rounds. What percentage of Restaurant Owners do you think displayed the food hygiene rating:

a) When the true food hygiene rating was 1:

b) When the true food hygiene rating was 2:

c) When the true food hygiene rating was 3:

d) When the true food hygiene rating was 4:

e) When the true food hygiene rating was 5:

The following rule will be used to calculate your earnings for the answers to each of the 5 questions:

70 - (Your answer - Correct answer), if your answer is higher than the correct answer of that question

Or

70 - (Correct answer - Your answer), if your answer is lower than the correct answer of that question

Example – Earnings for the questions at the end of each block

Suppose you are the **Restaurant Owner**. You answer the following question:

Think about the last 5 rounds. When the Restaurant Owners did not display the food hygiene rating, what percentage of Restaurant Customers in this room do you think reported the food hygiene rating:

a) 1 or 1.5: ____

If your answer is: **30**

But the correct answer is: **50**

Your answer is lower than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Correct answer - Your answer)

= 70 - (50 - 30)

= 70 - 20

= **50 points**

Suppose you are one of the two **Restaurant Customers**. You answer the following question:

Think about the last 5 rounds. What percentage of Restaurant Owners do you think displayed the food hygiene rating:

a) When the true food hygiene rating was 1:

If your answer is: **45**

But the correct answer is: **30**

Your answer is higher than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Your answer - Correct answer)

= 70 - (45 - 30)

= 70 - 15

= **55 points**

Now please answer the following questions. After you have finished answering all the questions, please raise your hand and the experimenter will come to you to check your answers.

1. Suppose you are the Restaurant Owner. If your answer to any one of the 5 questions above is **55** but the correct answer is **50**, what would be your earnings in points for that question? ______

2. Suppose you are Restaurant Customer 1. If your answer to any one of the 5 questions above is **25** but the correct answer is **40**, what would be your earnings in points for that question? ______

Payment

At the end of the experiment, the experimenter will call two participants to roll two six-sided dice one after another. The number rolled on the two dice will determine the two blocks that all participants will be paid for. You will be paid the total earnings accumulated for the 5 rounds in the decision making stage for the block determined by the first die roll. For example, if the number rolled on the first die is 1, you will be paid the total amount you have accumulated in the 5 rounds in the decision making stage in block 1. You will be paid for the accuracy of your answers to the 5 questions at the end of the block determined by the second die roll. For example, if the number rolled on the second die is 2, you will be paid for the accuracy of your answers to the 5 questions in block 2. If the number rolled on the second die is the same as the number rolled on the first die, the second die will be rolled again. In other words, you will be paid once for the total amount you have accumulated in the 5 rounds in the decision making stage of one of the blocks, and once for the accuracy of your answers to the 5 questions in a different block.

Please raise your hand if you have any questions.

APPENDIX 3.C: Post-experimental questionnaire

General questions:

1. What is your gender?
2. Is English your first or native language?
3. How old are you?
4. Do you have a friend participating in the session today?
5. What advice would you give to a friend if they were going to take your spot in this experiment?
6. Answer if you were an S Player. How did you decide whether or not to reveal the private number?
7. Answer if you were an R Player. How did you decide which number to report or guess?
8. What is your academic major or your planned academic major?

Cognitive ability questions:

9. A bat and a ball cost £1.10 in total. The bat costs £1 more than the ball. How much does the ball cost? ______ pence
10. If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? _____ minutes
11. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half the lake? ______ days

Risk preference:

12. Please tell me, in general, how willing or unwilling you are to take risks. Please use a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use any numbers between 0 and 10 to indicate where you fall on the scale.

Negative Reciprocity:

13. On a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so", how willing are you to punish someone who treats YOU unfairly, even if there may be costs for you?

General reciprocity:

14. How well does the following statement describe you as a person? Please indicate your answer on a scale from 0 to 10, where 0 means "does not describe me at all" and a 10 means "describes me perfectly".

    When someone does me a favour I am willing to return it.

Positive reciprocity:

15. Please think about what you would do in the following situation. You are in an area you are not familiar with, and you realise that you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 20 Pounds in total. However, the stranger says he or she does not want any money from you. You have 6 presents with you. The cheapest present costs 5 Pounds, the most expensive one costs 30 Pounds. Do you give one of the presents to the stranger as a "thank-you"-gift? If so, which present do you give to the stranger?

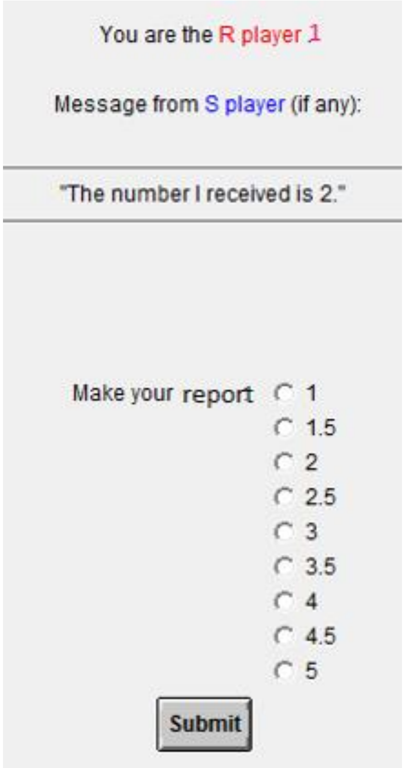
    No present
    The present worth 5 Pounds
    The present worth 10 Pounds
    The present worth 15 Pounds
    The present worth 20 Pounds
    The present worth 25 Pounds
    The present worth 30 Pounds

APPENDIX 3.D: Screenshots of players' screen

Senders' screen – No Context treatment

Senders' screen – Context treatment

Receivers' screen – No Context treatment



Receivers' screen – Context treatment

APPENDIX TO CHAPTER 4

APPENDIX 4.A

Table 4.1A: The table shows the earnings in points for the [selected] S Player and R Player

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

Table 4.2A: Summary statistics

Variables	**N**	**Mean**	**Standard deviation**
Economics (dummy)	5190	0.248	0.432
Competition treatment	5190	0.606	0.488
Male (dummy)	5190	0.416	0.492
Native English (dummy)	5190	0.624	0.484
Friend in the session (dummy)	5190	0.179	0.383
Cognitive ability (dummy=1 if at least 1 correct answer)	5190	0.682	0.465
Age	5190	21.21	3.76

Table 4.3A: Summary of elicited beliefs of senders

	Baseline	**Competition**
% of observations revealed when believed guess < draw	**88.95**	**94.41**
Rounds 1-10	86.03	91.44
Rounds 11-20	87.18	93.20
Rounds 21-30	93.27	98.47
% of observations not revealed when believed guess >= draw	**80.67**	**71.22**
Rounds 1-10	84.47	65.95
Rounds 11-20	80.69	71.95
Rounds 21-30	76.39	76.05

Note: The believed guess is calculated taking the higher point of the interval (Table 4.6 is calculated taking the lowest point of the interval).

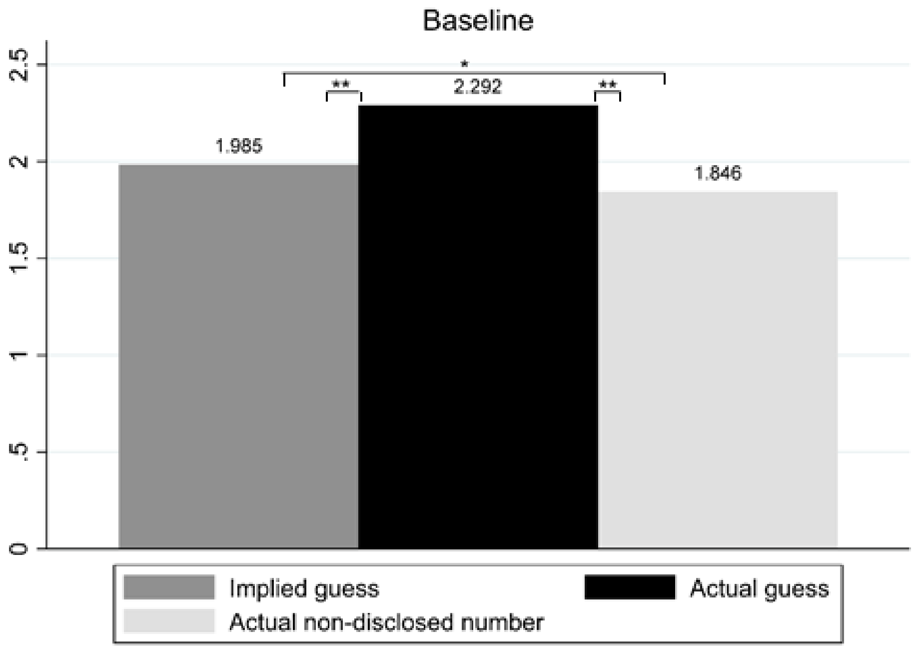


Figure 4.1A: Actual guess, implied guess and actual non-disclosed number in the Baseline

Note: ***p<0.01, **p<0.05, *p<0.10

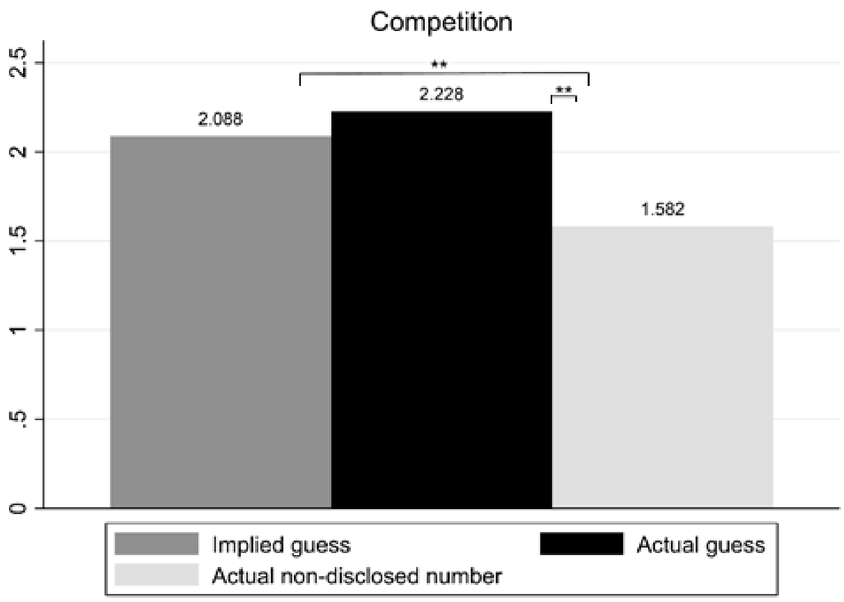


Figure 4.2A: Actual guess, implied guess and actual non-disclosed number in the Competition treatment

Note: ***p<0.01, **p<0.05, *p<0.10

APPENDIX 4.B: Experimental Instructions

*{*Baseline only*}* [Competition treatment only]

Welcome

Thank you for taking part in this experiment on decision-making. For participating in this experiment, you will be paid a £4 show up fee. Moreover, you will be paid an additional amount of money that will depend on yours and other participants' decision, and on chance. You will be paid in cash, privately at the end of the experiment.

Please silence and put away your mobile phones now.

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

During this experiment we will calculate your earnings using points. For your final payment, your earnings will be converted into Pounds at the ratio of 75:1 (75 points=£1). Any negative earnings you may make during the experiment will be subtracted from your show-up fee of £4.

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. If you have any questions during this period, please raise your hand and the experimenter will come to you to answer your question.

Instructions

The experiment consists of 6 blocks with 5 rounds in each block, making it a total of 30 rounds. At the end of each block, you will be asked to answer 5 questions, and at the end of the final block, you will be asked to fill out a questionnaire.

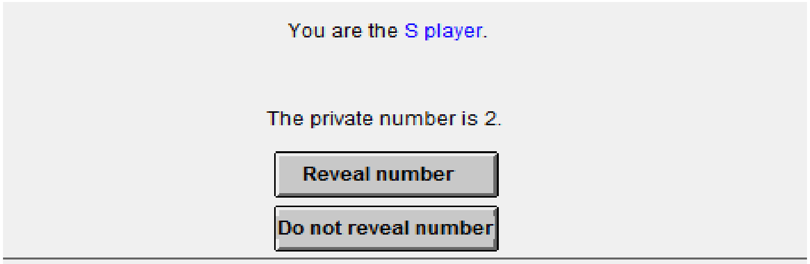
At the beginning of block 1, some of you will be randomly assigned to be the S Player and the others to be the R Player. [If you are assigned to be the S Player, you will further be assigned to be either S Player 1 or S Player 2.] You will remain in these roles for the entire duration of this experiment. [The computer will randomly pair one S Player 1 with one S Player 2 and you will remain in these pairs for the entire duration of this experiment.].

Decision-making stage

In each round, an R Player will be randomly matched with *{*an S Player*}* [a pair S Players] and in each new round, the R Player will be matched with a *{*different S Player*}* [new pair of S Players]. You will not learn the identity of the other participants you are matched with, nor will the other participants learn your identity.

For every matching of an R Player with *{*an S Player*}* [a pair of S Players], the computer program will [separately] generate *{*a*}* [two] private number[s] that *{*is*}* [are] randomly drawn from the set {1, 2, 3, 4, 5} [,one for S Player 1 and the other for S Player 2.]. Each of these numbers is equally likely to be generated. The computer will then send *{*the private number to the S Player} [one private number to S Player 1 and the other private number to S Player 2. Each S Player will see only their own private number.]. After receiving the private number, {the} [each] S Player will choose whether to reveal his private number to the R Player. If {the} [an] S Player chooses to reveal [his] number, the R Player will receive this message: “The number S Player [1] received is:” [or “The number S Player 2 received is:”] followed by the actual private number. Otherwise, the R Player will receive no message from {the S Player} [an S Player who chooses not to reveal his private number].

This is a screenshot of {the} [an] S Player’s screen. As you can see, the S Player **cannot lie or misreport** the true private number.



After seeing the message from [each of] the S Player[s], the R Player will report the value[s] of the private number[s] revealed by the S Player[s]. If the private number is not revealed by {the} [an] S Player, the R Player will guess the value of the private number. R Player can report the following numbers {1, 1.5, 2, 2.5, 3, 3.5, 4, 4.5, 5}. The numbers {1.5, 2.5, 3.5, 4.5} can be used, for example, when the R Player is unsure about the true value of the private number.

[R Player will also choose her preferred S Player by clicking on the button “Choose S Player 1” or “Choose S Player 2”. This choice will determine the earnings for the decision-making stage as described below.]

Earnings for the decision-making stage

[The preferred S Player will be selected by the computer with a 75% probability, and the other S Player with a 25% probability, to be matched with the R Player to earn points. The S Player not selected by the computer will earn 0 points. In other words, the preferred S Player has a higher probability of being selected by the computer to earn points, and the R Player has a higher probability of earning points for her preferred S Player.]

The S Player [selected by the computer] will earn the points that result from the report of the R Player. The R Player will earn points based on her own report and the true value of the private number [of the selected S Player]. The specific earnings [for the selected S Player and the R Player] are shown in the table below, which is displayed again before the S Player[s] and the R Player make their decisions. In each cell of the table, the points for the [selected] S Player are on the left, and the points for the R Player are on the right. As you can see from the table, the [selected] S Player earns more when the R Player makes a higher report, and the R Player earns more when her report is closer to the true private number [of the selected S Player].

The table shows the earnings in points for the [selected] **S Player** and the **R Player**.

Points **S, R**	**R 1's report: 1**	**R 1's report: 1.5**	**R 1's report: 2**	**R 1's report: 2.5**	**R 1's report: 3**	**R 1's report: 3.5**	**R 1's report: 4**	**R 1's report: 4.5**	**R 1's report: 5**
Private number 1	-29, 110	-6, 102	17, 90	38, 75	57, 57	75, 38	90, 17	102, -6	110, -29
Private number 2	-29, 90	-6, 102	17, 110	38, 102	57, 90	75, 75	90, 57	102, 38	110, 17
Private number 3	-29, 57	-6, 75	17, 90	38, 102	57, 110	75, 102	90, 90	102, 75	110, 57
Private number 4	-29, 17	-6, 38	17, 57	38, 75	57, 90	75, 102	90, 110	102, 102	110, 90
Private number 5	-29, -29	-6, -6	17, 17	38, 38	57, 57	75, 75	90, 90	102, 102	110, 110

{Example 1 – Earnings for a decision-making stage

Suppose in Round 18 of the experiment:

The computer program generates the private number for S Player as: **2**

S Player chooses not to reveal the private number 2 to the R Player.

Hence, the R Player sees this message on her screen:

"The number S Player received is: "

R Player guesses the value of the private number of S Player as: **4.5**

Therefore,

S Player earns = **102 points** for that round

R Player earns = **38 points** for that round

Example 2 – Earnings for a decision-making stage

Suppose in Round 26 of the experiment:

The computer program generates the private number for S Player as: **3**

S Player chooses to reveal the private number 3 to the R Player.

Hence, the R Player sees this message on her screen:

"The number S Player received is: 3"

R Player reports the value of the private number of S Player as: **3**

Therefore,

S Player earns = **57 points** for that round

R Player earns = **110 points** for that round

Now please answer questions 1 and 2. After you have finished answering both the questions, please raise your hand and the experimenter will come to you to check your answers.

1. Calculate the points earned by the players in this round. Suppose that:

The computer program generates the private number for S Player as: **5**

S Player chooses <u>not to reveal</u> the private number 5 to the R Player.

Hence, the R Player sees this message on her screen:

"The number S Player received is: "

R Player guesses the value of the private number of S Player as: **2.5**

S Player earns = _____ **points**

R Player earns = _____ **points**

2. Calculate the points earned by the players in this round. Suppose that:

The computer program generates the private number for S Player as: **2**

S Player chooses <u>to reveal</u> the private number 2 to the R Player.

Hence, the R Player sees this message on her screen:

"The number S Player received is: **2**"

R Player reports the value of the private number of S Player as: **2**

S Player earns = _____ **points**

R Player earns = _____ **points}**

[Example 1 – Earnings for a decision-making stage

Suppose in Round 18 of the experiment:

The computer program generates the private number for S Player 1 as: **1**

The computer program generates the private number for S Player 2 as: **4**

S Player 1 chooses <u>not to reveal</u> the private number 1 to the R Player.

S Player 2 chooses <u>to reveal</u> the private number 4 to the R Player.

Hence, the R Player sees these messages on her screen:

"The number S Player 1 received is: "

"The number S Player 2 received is: **4**"

R Player guesses the value of the private number of S Player 1 as: **1.5**

R Player reports the value of the private number of S Player 2 as: **4**

R Player chooses S Player 2. The computer selects S Player 2. Therefore,

S Player 1 earns = **0 points** for that round

S Player 2 earns = **90 points** for that round

R Player earns = Points based on her report of the private number of the selected S Player (S Player 2)

= **110 points** for that round

Example 2 – Earnings for a decision-making stage

Suppose in Round 26 of the experiment:

The computer program generates the private number for S Player 1 as: **3**

The computer program generates the private number for S Player 2 as: **4**

S Player 1 chooses to reveal the private number 3 to the R Player.

S Player 2 chooses not to reveal the private number 4 to the R Player.

Hence, the R Player sees these messages on her screen:

"The number S Player 1 received is: **3**"

"The number S Player 2 received is: "

R Player reports the value of the private number of S Player 1 as: **3**

R Player guesses the value of the private number of S Player 2 as: **2**

R Player chooses S Player 2. The computer selects S Player 2. Therefore,

S Player 1 earns = **0 points** for that round

S Player 2 earns = **17 points** for that round

R Player earns = Points based on her report of the private number of the selected S Player (S Player 2)

= **57 points** for that round

Now please answer questions 1 and 2. After you have finished answering both the questions, please raise your hand and the experimenter will come to you to check your answers.

Calculate the points earned by the players in this round. Suppose that:

The computer program generates the private number for S Player 1 as: **5**

The computer program generates the private number for S Player 2 as: **3**

S Player 1 chooses not to reveal the private number 5 to the R Player.

S Player 2 chooses to reveal the private number 3 to the R Player.

Hence, the R Player sees these messages on her screen:

"The number S Player 1 received is: "

"The number S Player 2 received is: **3**"

R Player guesses the value of the private number of S Player 1 as: **2.5**

R Player reports the value of the private number of S Player 2 as: **3**

R Player chooses S Player 2.

1. If the computer selects S Player 1

S Player 1 earns = _____.**points**

S Player 2 earns = _____ **points**

R Player earns = _____ **points**

2. If the computer selects S Player 2

S Player 1 earns = _____ **points**

S Player 2 earns = _____ **points**

R Player earns = _____ **points]**

Questions at the end of each block

At the end of each block, S Players will answer the following question:

Think about the last 5 rounds. When S Players did not reveal the private number, what percentage of R Players in this room do you think reported the number:

a) 1 or 1.5:

b) 2 or 2.5:

c) 3 or 3.5:

d) 4 or 4.5:

e) 5:

In a block, if all S Players have revealed the private number the earnings for that block will be determined at random for the S Players.

At the end of each block, R Players will answer the following 5 questions:

What percentage of S Players do you think revealed the private number:

a) When the true private number was 1:

b) When the true private number was 2:

c) When the true private number was 3:

d) When the true private number was 4:

e) When the true private number was 5:

The following rule will be used to calculate your earnings for the answers to each of the 5 questions:

70 - (Your answer - Correct answer), if your answer is higher than the correct answer of that question

Or

70 - (Correct answer - Your answer), if your answer is lower than the correct answer of that question

Example – Earnings for the questions at the end of each block

Suppose you are an **S Player** and your answer to one of the above questions is: **30**

But the correct answer is: **50**

Your answer is lower than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Correct answer - Your answer)

= 70 - (50 - 30)

= **50 points**

Suppose you are the **R Player** and your answer to one of the above questions is: **45**

But the correct answer is: **30**

Your answer is higher than the correct answer. Therefore, we will use the following rule to determine your earnings for that question:

Your earnings = 70 - (Your answer - Correct answer)

= 70 - (45 - 30)

= **55 points**

Now please answer questions 1 and 2. After you have finished answering both the questions, please raise your hand and the experimenter will come to you to check your answers.

1. Suppose the S Player's answer to any one of the 5 questions above is **55** but the correct answer is **50**, what would be the earnings in points for that question? ______

2. Suppose the R Player's answer to any one of the 5 questions above is **25** but the correct answer is **40**, what would be the earnings in points for that question? ______

Payment

At the end of the experiment, the experimenter will call two participants to roll two six-sided dice one after another. The number rolled on the two dice will determine the two blocks that all participants will be paid for. You will be paid the total earnings accumulated for the 5 rounds in the decision-making stage for the block determined by the first die roll. For example, if the number rolled on the first die is 1, you will be paid the total amount you have accumulated in the 5 rounds in the decision-making stage in block 1. You will be paid for the accuracy of your answers to the 5 questions at the end of the block determined by the second die roll. For example, if the number rolled on the second die is 2, you will be paid for the accuracy of your answers to the 5 questions in block 2. If the number rolled on the second die is the same as the number rolled on the first die, the second die will be rolled again. In other words, you will be paid once for the total amount you have accumulated in the 5 rounds in the decision-making stage of one of the blocks, and once for the accuracy of your answers to the 5 questions in a different block.

Please raise your hand if you have any questions.

APPENDIX 4.C: Post-experimental questionnaire

General questions:

1. What is your gender?
2. Is English your first or native language?
3. How old are you?
4. Do you have a friend participating in the session today?
5. What advice would you give to a friend if they were going to take your spot in this experiment?
6. Answer if you were an S Player. How did you decide whether or not to reveal the private number?
7. Answer if you were an R Player. How did you decide which number to report or guess?
8. What is your academic major or your planned academic major?

Cognitive ability questions:

9. A bat and a ball cost £1.10 in total. The bat costs £1 more than the ball. How much does the ball cost? ______ pence
10. If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? _____ minutes
11. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half the lake? ______ days

Risk preference:

12. Please tell me, in general, how willing or unwilling you are to take risks. Please use a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use any numbers between 0 and 10 to indicate where you fall on the scale.

Negative Reciprocity:

13. On a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so", how willing are you to punish someone who treats YOU unfairly, even if there may be costs for you?

General reciprocity:

14. How well does the following statement describe you as a person? Please indicate your answer on a scale from 0 to 10, where 0 means "does not describe me at all" and a 10 means "describes me perfectly".

    When someone does me a favour I am willing to return it.

Positive reciprocity:

15. Please think about what you would do in the following situation. You are in an area you are not familiar with, and you realise that you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 20 Pounds in total. However, the stranger says he or she does not want any money from you. You have 6 presents with you. The cheapest present costs 5 Pounds, the most expensive one costs 30 Pounds. Do you give one of the presents to the stranger as a "thank-you"-gift? If so, which present do you give to the stranger?

    No present
    The present worth 5 Pounds
    The present worth 10 Pounds
    The present worth 15 Pounds
    The present worth 20 Pounds
    The present worth 25 Pounds
    The present worth 30 Pounds

Hurtt’s Scepticism Scale:

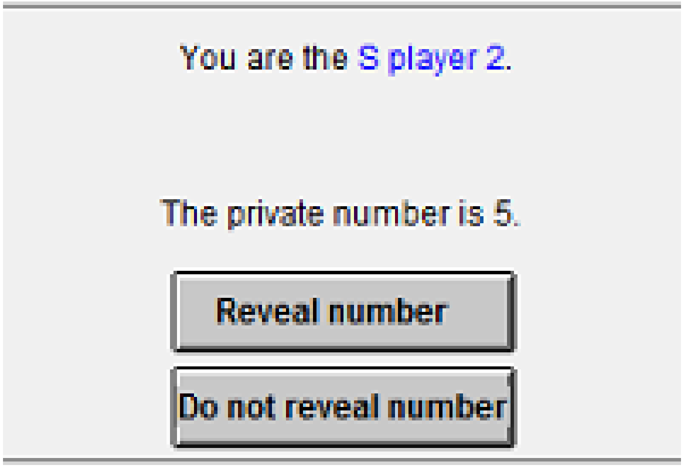
Please indicate your answer on a scale from 1 to 6, where 1 means "Strongly disagree" and a 6 means "Strongly agree".

1. I often accept other people's explanations without further thought.
2. I feel good about myself.
3. I wait to decide on issues until I can get more information.
4. The prospect of learning excites me.
5. I am interested in what causes people to behave the way that they do.
6. I am confident of my abilities.
7. I often reject statements unless I have proof that they are true.
8. Discovering new information is fun.
9. I take my time when making decisions.
10. I tend to immediately accept what other people tell me.
11. The actions people take and the reasons for those actions are fascinating.
12. I am self-assured.
13. My friends tell me that I usually question things that I see or hear.
14. I like to understand the reason for other people's behavior.
15. I think that learning is exciting.
16. I usually accept things I see, read or hear at face value.
17. I don’t feel sure of myself.
18. I usually notice inconsistencies in explanations.
19. Most often I agree with what the others in my group think.
20. I relish learning.
21. I have confidence in myself.
22. I don’t like to decide until I’ve looked at all of the readily available information.
23. I like searching for knowledge.
24. I frequently question things that I see or hear.
25. It is easy for other people to convince me.

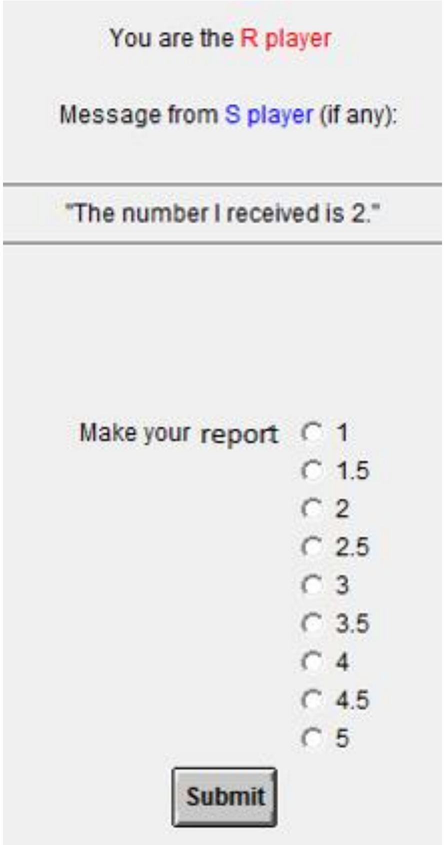
APPENDIX 4.D: Screenshots of players' screen

Senders' screen - Baseline

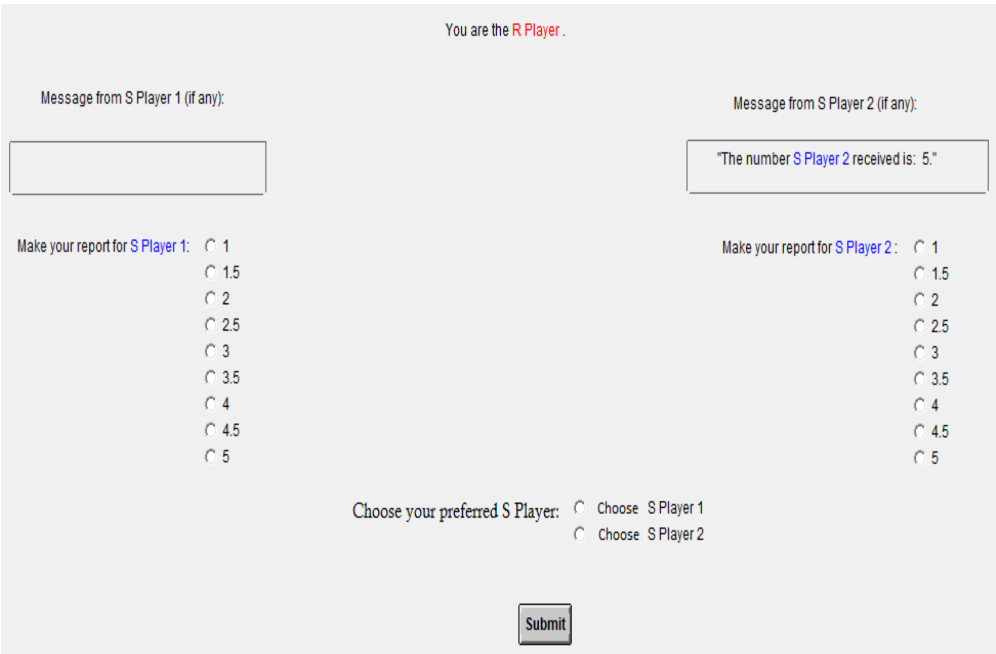
Senders' screen - Competition



Receivers' screen - Baseline



Receivers' screen – Competition treatment



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