Essays in Experimental Economics

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ABSTRACT

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This dissertation comprises three essays in experimental economics. The first investigates the extent of strategic behaviour in jury voting models. Existing experimental evidence in jury voting models shows subjects largely act in accordance with theoretical predictions, implying that they have the insight to condition their votes upon their own pivotality. The experiment presented here tests the extent of these abilities, finding that a large portion of subjects behave consistently with such insight in the face of several variations on the basic jury voting game, but largely fail to do so in another, perhaps due to the difficulty of extracting informational implications from counterintuitive strategies.

The second investigates the extent to which hypothetical thinking - the ability to condition upon and extract information from hypothetical events - persists across different strategic environments. Two games of considerable interest in the experimental literature - jury voting games and common value auctions - each contain the feature that a sophisticated player can simplify the problem by conditioning upon a hypothetical event - pivotality and winning the auction, respectively - and extract from it information about the state of the world that might affect their own behaviour. This common element suggests that the capability that leads to sophisticated play in one should lead to the same in the other. This paper tests this connection through a within-subject experiment in which subjects each play both games. Little evidence is found that play in one relates to play in the other in any meaningful way.

Finally, the third, co-authored with Evan Friedman, investigates the nature of errors relative to Nash equilibrium play in a family of two-by-two games. Using data on one-shot games, we study the mapping from the distribution of player j's actions to the distribution of player i's beliefs (over player j's actions) and the mapping from player i's payoffs (given beliefs) to the distribution over player i's actions. In our laboratory experiment, subjects play a set of fully mixed 2×2 games without feedback and state their beliefs about which actions they expect their opponents to play. We find that (*i*) belief distributions tend to shift in the same direction as changes in opponents' actions, (*ii*) beliefs are systematically biased–"conservative" for one player role and "extreme" for the other, (*iii*) rates of best response vary systematically across games, and (*iv*) systematic failures to maximize expected payoffs (given beliefs) are well explained by risk aversion. To better understand the belief formation process, we collect subject-level measures of strategic sophistication based on dominance solvable games. We find that (*v*) the player role itself has a strong effect on sophistication, (*vi*) sophistication measured in dominance solvable games strongly predicts behavior in fully mixed games, and (*vii*) belief elicitation significantly effects actions in a direction consistent with increasing sophistication.

Contents

List of Figures

List of Tables

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Chapter 1

Pivotality and Sophistication

1.1 Introduction

Rarely is the agreement of experimental evidence and theory as startling as that of the jury voting literature. The underlying model, as described by Austen-Smith and Banks (1996) and Feddersen and Pesendorfer (1998), features equilibria in which a rational juror maximizes the likelihood of a correct verdict by voting in contrast with her own private information. The juror is led to this behaviour by the recognition that she should not vote for the state that is most likely given her private information, but rather for the state that is most likely given her private information *and the event that her vote is pivotal*. Optimal behaviour thus relies on subjects having this insight, forming reasonable beliefs about others' strategies, and from these beliefs making correct inferences about the likelihood of each state under the assumption of pivotality - a process that Palfrey (2016) refers to as *pivotal calculus*. It is reasonable to expect that few will have such insights and capabilities.

And yet, an experimental literature led by Guarnaschelli, McKelvey, and Palfrey (2000) and Goeree and Yariv (2011) finds behaviour that is entirely consistent with these predictions: subjects vote against their own private information under exactly the conditions theory predicts, and in numbers that, in the aggregate, are not far from equilibrium predictions - a startling result. These papers, along with subsequent others discussed further in the following section, focus primarily on the impact of the jury's mechanisms - including communication and group formation - upon aggregate outcomes and juries' overall efficiency, with the individual only treated briefly. And yet the truly surprising aspect of these results is the individual's apparent ability to solve the game despite the complexity of the thought process that theory postulates. Given this, and the potential implications of this ability in other contexts, further investigation of the individual's understanding of pivotality and its implications is warranted.

More recently, Esponda and Vespa (2014) take a step in this direction, focusing on the individual and asking, in particular, whether it is the hypothetical nature of pivotality that makes the problem difficult. Their experiment, utilizing a related game, finds much higher rates of nonstrategic behaviour when pivotality is hypothetical than when it is directly observed, suggesting that this is the case. Interestingly, however, they also find much higher rates of nonstrategic behaviour when pivotality is hypothetical than in previous jury voting experiments where this is also the case, including both Guarnaschelli, McKelvey, and Palfrey (2000) and Goeree and Yariv (2011). Because the game they employ differs in several important ways from those used in other studies, including changes to the information structure, the use of computer players, and complicated computer-player strategies that may confuse subjects, it is not clear what exactly drives this divergence.

The experiment presented in this paper thus follows a similar path of inquiry, with a focus on individual behaviour, while retaining the structure of the Guarnaschelli et al. (2000) game (henceforth GMP), in order to present a clearer image of subjects' capabilities in these games. In a within-subject design, subjects play five variants of the GMP game. The first two follow the standard GMP procedure with majority and unanimity voting, respectively. The remaining three are new variations in which individual subjects are grouped with computer players with known strategies, thus removing issues of belief formation and social preferences.

These three new variations allow for three contributions. In the first, in which computer players vote fully informatively, strategic and non-strategic behaviour are clearly separated, allowing for better identification of subjects with strategic capabilities than the standard GMP game, in which certain behaviours, such as fully informative voting, can result from strategic or naive thought.

The second is similar but requires subjects to choose between receiving an informative private signal and the ability to select their own vote. Optimal voting behaviour is unchanged and can be achieved by the second option, but this requires subjects to overcome the intuitive notion that giving up private information is costly. A preference for this option suggests that subjects' strategic understanding of the game extends beyond voting decisions.

Finally, the third follows the first but inverts the strategies played by computer players - that is, they vote against their private information, behaviour that is itself fully informative - such that pivotality now holds the opposite informational content and demands the opposite response. While this computer behaviour is unintuitive, it is mathematically identical to the first computer game, providing a minimal test of how subjects' ability to extract information from others' behaviour is dependent on that behaviour. There is strong evidence that it makes a substantial difference, suggesting subjects may be prone to failures to extract even mathematically simple information from pivotality, and not just failure to condition upon pivotality in the first place, which may go some way to explaining the higher rates of nonstrategic behaviour in Esponda and Vespa (2014), where computer player behaviour is similarly unusual.

The structure of the paper is as follows. Section 1.2 outlines the standard jury voting game, its predictions, and existing experimental results in this and related games. Section 1.3 describes the experimental design, with results discussed in Section 1.4. Section 1.5 concludes.

1.2 A Model of Jury Voting

The canonical model of jury voting is due to Feddersen and Pesendorfer (1998), which builds upon that of Austen-Smith and Banks (1996). A jury of n voters must determine whether to convict or acquit a passive defendant. The defendant is guilty with probability $\rho \in (0, 1)$ and innocent otherwise. While this state of the world is unobserved, each juror receives a private, noisy, conditionally independent signal, again either innocent or guilty, which matches the true state with probability $p \in (\frac{1}{2}, 1)$. These signals represent each juror's independent, but potentially flawed, interpretation of the evidence presented to them. Each juror then votes either to convict or acquit. The jury's verdict is to convict if at least r jurors vote to convict, and to acquit otherwise. The conviction threshold, r, ranges from r^M to n where $r^M = \lceil \frac{n}{2} \rceil$ represents majority voting and $r = n$ represents *conviction unanimity*. Jurors' utility is normalized to 0 in the event of correct verdicts, $-q$ in the event of a wrongful conviction, and $-(1-q)$ in the event of a wrongful acquittal, where $q \in (0, 1)$ sets the relative weights of these errors.

In testing the implications of this model, discussed below, GMP abstract from jury-specific concepts about which subjects might have preconceived notions, replacing them instead with colours. As their model forms the basis for those presented in the paper, I will use this terminology throughout, including the remainder of the theoretical discussion. In this version, isomorphic to the game above, a group of n voters are assigned either a red jar or a blue jar, taking the roles of guilt and innocence respectively, with the red jar assigned with probability $\rho \in (0, 1)$. Each jar contains 10 balls, with $10p$ matching the colour of the jar and $10(1-p)$ matching the colour of the other jar. Each subject then selects a ball from the assigned jar, with replacement, filling the role of the juror's signal with precision p.[1] Each player then votes for either the red or blue jar, with the group selecting the red jar (i.e. convicting the defendant) if at least r vote for it, and the blue jar otherwise. I will refer to $r = n$, in which the group only selects the red jar if all members vote for it, as *red unanimity*.

Let $\omega \in \{r, b\}$ denote the colour of the group's assigned jar and $s \in \{r, b\}$ a subject's selected ball. Define $\sigma(r)$ and $\sigma(b)$ to be the probability with which a subject votes for the red jar when selecting a red and blue ball respectively. A subject votes *responsively* when $\sigma(r) \neq \sigma(b)$, and *informatively* when always voting for the jar that matches their

[1] This probability is equal across states in both the Feddersen and Pesendorfer (1998) and GMP formulations, but there is no reason that this must be the case more generally.

signal, $\sigma(r) = 1, \sigma(b) = 0$.[2] Throughout, I follow Feddersen and Pesendorfer (1998) and GMP in fixing $\rho = \frac{1}{2}$, so that each state is equally likely, and in assuming that p is equal across states. I follow the latter in fixing $q = \frac{1}{2}$, so that utility depends only on whether the group's decision matches the state, but is otherwise equal across states. That is, correctly selecting the red jar and correctly selecting the blue jar result in the same payoff, as do incorrectly selecting the red and blue jar.

The primary insight of Austen-Smith and Banks (1996) is that while previous jury-voting models implicitly assumed that jurors would vote informatively, such behaviour may not be rational and, more specifically, may not constitute an equilibrium. The literature focuses in particular upon symmetric responsive equilibria.[3] Under majority voting the unique such equilibrium is informative voting. Under a more demanding decision threshold, $r > r^M$, the unique such equilibrium is $\sigma(r) = 1$ and

$$\sigma(b) = \frac{pK_{n,r} - (1-p)}{p - (1-p)K_{n,r}} \in (0, 1) \tag{1.1}$$

where

$$K_{n,r} = \left(\frac{1-p}{p}\right)^{\frac{n-(r-1)}{r-1}}.$$

That is, when more than a majority of the group must vote red in order to select the red jar, in equilibrium players should vote red with positive probability even when their own signal is blue. The comparative static of note is that, for a fixed n and p, this probability is strictly increasing in the decision threshold, r. Moreover, Feddersen and Pesendorfer (1998) find that under certain conditions higher decision thresholds result in higher wrongful conviction rates in equilibrium, which contradicts their apparent purpose.

[2] Here 'informative' is used as a shorthand for '*perfectly* informative'. Of course, $\sigma(r) = 0, \sigma(b) = 1$ would be equally informative, but is never observed in the standard game and is thus ignored.

[3] Responsiveness simply rules out those trivial equilibria in which players always vote for the same jar regardless of signal; behaviour that is optimal only because each player has zero probability of pivotality.

The intuition for non-informative equilibria is simple: a player's vote is pivotal when exactly $r-1$ other players have voted for the red jar, and $n-r$ for the blue jar. If other players vote informatively, $r > r^M$ then implies that the number of red signals exceeds the number of blue signals even if the player's own signal is blue. Thus the player should vote for the red jar. The equilibrium attains where the common $\sigma(b)$ is large enough that the conditional probability of a red signal given a red vote is low enough for the states to be conditionally equally likely when the player's own signal is blue.

On an individual level, then, different levels of understanding of pivotal calculus may lead to different behaviours and, because rational behaviour is counter-intuitive if $r > r^M$, we expect to see deviations especially in such a case. A subject that understands the informational content of their own signal, $P(\omega = s|s) > P(\omega \neq s|s)$, but not the importance of pivotality, will vote informatively. Such a subject is *naive*.

For those who understand the informative content of pivotality, behaviour may differ not only from the naive, but also from other strategic subjects, as any $\sigma(b) \in [0,1]$ can be optimal given plausible beliefs about others. If we assume, for simplicity, that subjects form beliefs as if others are homogeneous, or at least drawn randomly from a large population, then $\sigma(b) = 1$ is optimal when the ratio of these beliefs, $\frac{\sigma(b)}{\sigma(r)}$, is below the symmetric responsive equilibrium level, and $\sigma(b) = 0$ is optimal when this ratio is above that level.

Experimentally, this results in difficulties determining what drives many behaviours, all of which have been observed in previous experiments discussed below. Does a subject vote informatively for strategic reasons or because she is naive to pivotal calculus? Does she always vote red because her beliefs about others' make it optimal to do so, or is she incapable of understanding how the implications of pivotality change as other subjects play $\sigma(b) > 0$ rather than voting informatively? Does she mix because she does have such an understanding, and grasps the nature of the equilibrium, or due to changing beliefs about others, or doubt about her own pivotal reasoning? The experiment presented in this paper seeks to shed light on these questions through treatments utilizing computer players with known behaviour, thus fixing these beliefs, and a within-subject design that allows comparison of behaviour in these games to behaviour in the standard game.

Although not widely discussed in the literature, it is also plausible that apparently strategic behaviour has drivers other than pivotal calculus. For example, a subject might vote red despite a blue signal as a form of quasi-abstention, since a single blue vote ensures that the group selects the blue jar and puts the subject at risk of the responsibility of a unilateral decision. Alternately, subjects may simply wish to herd in the face of uncertainty, and the vote-rule serves as an anchor for their beliefs about others' behaviour. The second computer-player game presented in this paper sheds light on these questions by changing computer-player behaviour while retaining the vote-rule, such that sophisticated play is different while the play induced by these alternate drivers is unchanged.

1.2.1 Existing Evidence

Both Guarnaschelli, McKelvey, and Palfrey (2000), and Goeree and Yariv (2011), henceforth GY, employ the jar game described in the previous section, with subjects randomly re-matched between rounds, jars equally likely ($\rho = \frac{1}{2}$), subjects paid an equal amount for each correct group decision ($q = \frac{1}{2}$), and a signal strength of $p = 0.7$ for both jars. n and r varies between treatments but is fixed between rounds within a treatment and is known to subjects. The focus of both is on aggregate behaviour, which is summarized in Table 1.1, with $\hat{\sigma}(s)$ representing the frequency of red votes conditional upon signal $s \in \{r, b\}$. In each case, behaviour is largely informative under majority voting,[4] but $\sigma(b)$ strictly increases as r increases to a supermajority (in GY), and then to red unanimity $r = n$ (in both). As well as matching the theorized comparative static qualitatively, the results are also relatively close to the empirical point predictions.[5]

Table 1.1: Observed (and equilibrium) behaviour in previous experiments

	n	r	$\hat{\sigma}(b)$	$\hat{\sigma}(r)$
GMP	3	2	0.057 (0.000)	0.972 (1.000)
GMP	3	3	0.360 (0.314)	0.954 (1.000)
GMP	6	4	0.209 (0.000)	0.979 (1.000)
GMP	6	6	0.478 (0.651)	0.897 (1.000)
GY	9	5	0.07 (0.00)	0.91 (1.000)
GY	9	7	0.24 (0.31)	0.89 (1.000)
GY	9	9	0.39 (0.77)	0.90 (1.000)

It is worth noting, however, that both studies observe substantial heterogeneity under red unanimity, including subjects responding to blue signals in all three ways: by always voting blue, by always voting red, and by mixing. Thus these aggregate measures provide an imperfect indication of the proportion of subjects that are capable of pivotal calculus, and the earlier questions remain regarding the extent and nature of these capabilities. While GMP do categorize subjects by individual behaviour - whether they play $\hat{\sigma}(b) = 0$, $\hat{\sigma}(b) \in (0, 1)$, or $\hat{\sigma}(b) = 1$ - these classifications still leave questions about subjects' capabilities, as discussed above. For example, it is not clear whether a subject playing $\hat{\sigma}(b) = 0$ does so because of naivety or simply a belief that the aggregate $\hat{\sigma}(b)$ is too high, in which case playing $\hat{\sigma}(b) = 0$ is optimal. Likewise, it is unclear whether a subject playing $\hat{\sigma}(b) \in (0, 1)$ does so strategically, or simply has

[4] The largest deviation occurs in $\sigma(b) = 0.209$ in GMP's $n = 6$, $r = 4$ treatment, but is made less surprising by the fact that in equilibrium a player receiving a blue signal should be indifferent between the two jars. This contrasts to other treatments, in which one jar is strictly preferred.

[5] Regardless, theoretical predictions for group decision accuracy - the type I errors should decrease and type II errors increase as the group size increases - are violated for both majority and unanimity voting.

a predilection for tremors. The experiment presented here seeks to ameliorate these concerns, both by fixing beliefs and including $\hat{\sigma}(r)$ when classifying subjects' behaviour.

Like this paper, Esponda and Vespa (2014), henceforth EV, move away from questions of institutional efficiency and a focus on aggregate outcomes to an investigation of individual strategic capability, finding that subjects have difficulty conditioning on hypothetical events in particular. They do so through a related design in which groups consist of one subject and two computer players. Each computer player observes the state directly then votes red when the state is red and mixes otherwise. Both this mixing probability and the probability with which the state is red vary between rounds but are known to the subject. Although the pivotal calculus is similar, the implication here is even sharper: the subject can only be pivotal when the state is blue, so should always vote blue. Subjects' actions differ, however, depending on whether voting is simultaneous or whether they first observe the computer players' votes. In the simultaneous case, in which pivotality is hypothetical, 78% are non-strategic, failing to converge on always voting for the blue jar, compared to 24% when pivotality can be directly observed. This gives the primary result, that the hypothetical nature of pivotality drives much of subjects' failure to act strategically in these games. Learning effects also are also substantial, with subjects becoming more strategic over time; a result that is found again in the experiment presented in this paper.

There are also differences between subjects' behaviour in EV and the equivalent treatments of GMP and GY, and the substantial differences in the games' structures leave it unclear as to what drives this. In particular, the EV results also show much lower rates of strategic behaviour than GMP and GY when pivotality is not observer and subjects are similarly experienced. When restricting to the first 15 rounds of the treatment in which subjects vote without seeing computer player votes and receive feedback after each round - the conditions most closely resembling each of GMP, GY, and the experiment presented below - subjects vote blue just 26.8% of the time when the state is unconditionally more likely to be red - perhaps the closest proxy we can draw for $\hat{\sigma}(b)$ in the jury voting game. Of concern is that this is not much greater than the 21.5% probability with which subjects vote red when the state is unconditionally more likely to be blue, behaviour which cannot be justified regardless of whether or not a subject understands pivotality, akin to voting blue with a red signal in the standard game.

It is unclear what drives such behaviour, but given its irrationality we must consider that subjects may simply be confused. One possible cause of this is the computer players' behaviour, which is both complex, in that it is semi-mixed and asymmetric in the state, and unlike that which we would expect of a human player, in that they vote red with some probability after observing a blue state. While the implication of pivotality is mathematically simple - any blue computer vote implies a blue state - it is easy to imagine that such a strategy may confuse subjects. The experiment presented here tests this by adjusting computer-player behaviour, with results suggesting confusion may indeed play a role.

We must also consider that perhaps the simple fact of playing with computer players is enough to cause subjects difficulty. Moreover, it is worth noting that the observed errors (voting for the red jar when the prior favours blue) mirror those of the computer players (voting with some probability for the red jar when the state is blue) suggesting that subjects' may simply use computer player behaviour as a guide. The experiment presented below finds little support for either of these hypotheses, however, with subjects displaying little irrational behaviour when computer players play simple, seemingly rational strategies, and differing from computer players in their irrational deviations when they don't.

While the above papers are the most relevant to the study prevented here, the jury voting literature - and strategic voting literature more generally - is much broader, featuring many issues not yet discussed. One common theme, including in the papers discussed above, is communication between voters. Coughlan (2000) seeks more realistic models of jury voting, first adding the possibility for mistrial when neither outcome receives enough votes, finding informative voting to be a symmetric equilibrium under relatively lenient conditions, and then separately adding pre-vote communication via a straw poll, finding equilibria in which voters truthfully reveal their signals then voting for the most common. The latter is the secondary focus of GMP, who find that subjects do generally reveal their signals truthfully, but may overweight their private information when voting. Likewise, GY allow for free-form communication via a chat box, again finding that subjects reveal information truthfully, then take into account the collective information when voting, particularly when preferences are aligned.

Ali et al. (2008) recreate the unanimity no-communication treatments of GMP with standing committees in which subjects are not randomly rematched between rounds, finding negligible differences from GMP's results. The same paper then considers the case in which jurors vote sequentially, focusing on the rate at which subjects who may still be pivotal - that is, all previous voters have voted to convict - vote also to convict. While overall such subjects with innocent signals tend to vote guilty more often than under simultaneous voting, the magnitude of the differences is perhaps smaller than we would expect, particularly given the equivalent EV results, perhaps suggesting some unrecognized differences between the games. Battaglini, Morton, and Palfrey (2008a) similarly consider sequential voting, with the addition of voting costs and the possibility of abstention under majority voting. As in previous majority voting results, subjects generally vote informatively when not abstaining, although voting order and cost have large effects on abstention decisions. Abstention decisions are further investigated in Battaglini, Morton, and Palfrey (2008b), Bhattacharya, Duffy, and Kim (2014) and others.

Another strand of the literature investigates the impact of changes to the game's information structure. Kawamura and Vlaseros (2017) allow for a public signal in addition to subjects' private signals, finding that when the two disagree, subjects follow the former more often than equilibrium would predict. Invernizzi (2018) further tests a similar setup, finding that recency of information is of particular importance. Costly information acquisition is also considered, with Großer and Seebauer (2016) are more likely to acquire information under majority voting than unanimity voting. A different take on information acquisition is considered in the Option Game presented below.

1.3 Experimental Design

1.3.1 Games

The experiment consists of five variants of the GMP game, each played with parameters of $n = 5$, $p = 0.7$, and either majority ($r = 3$) or red unanimity ($r = 5$) voting. Each game is played 15 times consecutively, with each subject playing all games, for a total of 75 rounds, with the order outlined in Section 1.3.2. After each round subjects are told their group's decision, the number of votes for each jar, and the true colour of the jar. A running tally of the subject's correct group decisions is also available at all times. All parameters remain identical between rounds.[6]

The five jury voting games are as follows, with equilibrium behaviour for each summarized in Table 1.3a, along with aggregate results.

[6]Subjects also each played 15 rounds of the Charness and Levin (2009) single-player bidding game, which is the focus of Chapter 2 but omitted from the remainder of this Chapter. 110 subjects played this game before the jury voting games and 110 played it after. Regressions throughout the results section find no difference in behaviour when subjects have previously played the bidding game, thus justifying its omission.

1.3.1.1 Human Game with Majority Voting (HGM) and Red Unanimity (HGU)

The first two variants are simply the GMP game itself with majority ($r = 3$) and red unanimity ($r = 5$) voting respectively, providing a baseline for comparison to the previous experiments, and for examination of subjects' apparent strategic abilities. Groups consist of 5 human subjects and are randomly rematched between rounds. The symmetric, responsive equilibria are sincere voting under majority, and $\sigma(r) = 1$, $\sigma(b) = 0.583$ under red unanimity. The threshold belief ratio at which a subject receiving a blue signal is indifferent between jars is thus $\frac{\sigma(b)}{\sigma(r)} = 0.583$. As discussed above, any $\sigma(b)$ along with $\sigma(r) = 1$ can be a best response to plausible beliefs in HGU, and some behaviours - most notably informative voting - are consistent with pivotal understanding but may still be observed in those subjects not capable of it.

1.3.1.2 Informative Computer Game with Red Unanimity (ICG)

In ICG, each group consists of one subject and four computer players, each of which randomly selects a ball from the jar, with replacement, then votes for the jar that matches the selected ball. That is, each computer player acts exactly like a fully informative subject. This behaviour is known to subjects. Group decisions are made by red unanimity, $r = 5$.

Optimal behaviour in this game is to always vote for the red jar: $\sigma(r) = \sigma(b) = 1$. To see this, note that pivotality implies all four computer players received red signals, resulting in a conditional probability of a red state of 0.986 when the subject's own ball is red, and 0.927 when it is blue, and thus voting for the red jar is always optimal.

This treatment thus provides perhaps the simplest possible application of pivotal reasoning in a jury voting environment: subjects are not required to form beliefs over others' strategies, the strategies are trivial, natural, and easily understood, and there is no need for statistical estimation, as it is clear that four red signals outweigh a single blue signal without any calculation. Moreover, since the best response is a pure strategy, and beliefs are anchored, we can more easily distinguish those who are capable of pivotal understanding, as informative or mixing behaviour - which can be optimal in HGU - are no longer so.

1.3.1.3 Reverse Computer Game with Red Unanimity (RCG)

RCG differs from ICG only in the computer players' behaviour. Here each computer player votes for the jar that *does not match* its selected ball. Pivotality still thus implies four red votes, but that now implies four *blue* signals, and as such the subject should always vote for the blue jar regardless of signal: $\sigma(r) = \sigma(b) = 0$.

This reverse informative strategy is in theory no more complicated than the standard informative strategy played by computer players in ICG: it is also pure and informative, and thus pivotal thinking demands no more strategic insight or mathematical capability, nor requires subjects to vote against their private information any more than in ICG. It should thus be no more difficult. The computer strategy is less natural than regular informative voting, however, which may prove problematic to human subjects. The comparison to ICG tests this.

Moreover, RCG separates those who are pivotally capable from those who appear so in HGU and ICG but are actually motivated by social drivers. While the pivotally capable switch to always voting blue, those who voted red in ICG simply to avoid the responsibility of unilaterally assuring a blue group decision will continue to do so. Likewise, since the vote rule remains unchanged at $r = 5$, those who use it as an anchor for herding and thus vote red in ICG should also do the same here.

1.3.1.4 Option Computer Game with Red Unanimity (OCG)

OCG differs from ICG in that at the beginning of each round, each subject is afforded two options. Under the *Information* option the subject selects a ball from the jar as previously, but their vote is then automatically cast for the jar matching their selected ball. Under the *Choice* option the subject does not select a ball from the jar but chooses which jar she wishes to vote for.[7] Under both options, each computer player selects a ball from the jar and then votes informatively, as in ICG.

I define σ_R as the probability with which a subject selects *Choice* and votes for the red jar, and σ_B the probability with which she selects *Choice* and votes for the blue jar. Given the computer behaviour is unchanged from ICG, optimal behaviour is again to vote for the red jar regardless of signal, which can be achieved here by selecting *Choice* and forgoing the signal altogether, that is $\sigma_R = 1$. This reasoning should be clear to those who are pivotally capable. For those who are not so, it may be that the instinctive value of private information may be too great to give up. The game thus provides a test of the strength of subjects' confidence in discarding their own private information, and more generally of their ability to extend pivotal thinking from voting decisions to more institutional decisions about the informational structure of the game itself.

1.3.2 Order, Subjects, and Payments

The order of the five games, summarized in Table 1.2, varies between sessions along two dimensions: whether HGM is played before or after HGU, and whether HGU is played before or after ICG. While the former has no impact on results and is ultimately ignored, the latter is important and discussed below. In each case, these three games are followed by RCG and then OCG. This results in four basic orders, each played over

[7] In the laboratory the options are referred to only as *Option A* and *Option B* respectively.

four sessions of either 10 or 15 subjects.[8]

Table 1.2: Treatment Orders

	Treatment Order	Subjects
Order 1	HGM - HGU - ICG - RCG - OCG	60
Order 2	HGU - HGM - ICG - RCG - OCG	50
Order 3	HGM - ICG - HGU - RCG - OCG	50
Order 4	ICG - HGM - HGU - RCG - OCG	60

Overall, 220 subjects took part in the experiment over 16 sessions at the Columbia Experimental Laboratory for the Social Sciences (CELSS) from November 2016 to October 2017. The subject pool consists of Columbia students, primarily undergraduates. Subjects were paid a $5 show-up fee and 25c for each of the 75 jury voting rounds in which their group made a correct decision.[9] Subjects also played two practice rounds before the first jury voting game, and two more before OCG, in which the interface updates to incorporate the option screen. Sessions lasted from 50 to 75 minutes, with an average payment of $20.13.

The interface was programmed using the oTree software package (Chen et al. (2016)) and subjects recruited via ORSEE (Greiner (2004)). The experimental instructions are in Appendix A.1 with the accompanying overheads in Appendix A.2 and the interface presented in Appendix A.3.

[8]Within each order, exactly half of the subjects play the CL auction game before the jury games and half play it after - with two sessions of each - but again this has no impact on the results and is ultimately ignored, with results pooled.

[9]Subject also earn 25c for every 120 points earned in CL.

1.4 Results

1.4.1 Aggregate Behaviour

Aggregate observed behaviour for each of the five games is displayed in Table 1.3a, with (symmetric, responsive) equilibrium predictions in parentheses. $\hat{\sigma}(b)$ represents the aggregate frequency with which subjects vote for the red jar conditional upon a blue signal - the empirical equivalent of $\sigma(b)$, while $\hat{\sigma}(r)$ is the same for red signals. In OCG, $\hat{\sigma}_R$ shows the frequency with which subjects select *Choice* and vote for the red jar, with $\hat{\sigma}_R$ the frequency with which they select *Choice* and vote for the blue jar. Table 1.3a shows p-values for pairwise two-tailed permutation tests of the differences between these frequencies, with differences between $\hat{\sigma}(b)$ below the diagonal and differences between $\hat{\sigma}(r)$ above. These permutation tests are described in Appendix A.4.

Table 1.3: Aggregate Behaviour by Treatment.

Treatments	$\hat{\sigma}(b)$	$\hat{\sigma}(r)$
HGM	0.069 (0.000)	0.948 (1.000)
HGU	0.476 (0.583)	0.912 (1.000)
ICG	0.560 (1.000)	0.919 (1.000)
RCG	0.315 (0.000)	0.613 (0.000)
	$\hat{\sigma}_R$	$\hat{\sigma}_B$
OCG	0.653 (1.000)	0.054 (0.000)

(a) Observed (and equilibrium) behaviour.

Treatments	HGM	HGU	ICG	RCG
HGM		0.015	0.055	0.000
HGU	0.000		0.694	0.000
ICG	0.000	0.001		0.000
RCG	0.000	0.000	0.000	

(b) p-values for between-treatment differences.

The aggregate results for the human group games fit well with those of GMP and GY, with the aggregate strategy close to fully informative under majority voting, while under red unanimity $\hat{\sigma}(b)$ increases substantially and significantly. This increase is in accordance with equilibrium predictions, although the magnitude of the change is not as great. $\hat{\sigma}(b)$ increases further in ICG, which again is qualitatively consistent with strategic behaviour, but falls further short of the equilibrium strategy. It is also notable that $\hat{\sigma}(r)$ is significantly lower in each of HGU and ICG than HGM, despite a theoretical prediction of $\hat{\sigma}(r) = 1$ in each. It is not clear why this should be, although it is perhaps relevant that it is lower - that is, subjects are more likely to vote blue in contrast to a red signal - in the games where they are also more likely to vote red in contrast to a blue signal.

OCG also appears to be well understood in the aggregate, with optimal behaviour - selecting *Choice* and voting for the red jar - in almost two thirds of decisions. Subjects rarely select *Choice* to vote for the blue jar, and select *Information*, and thus vote informatively, with frequency $1 - 0.653 - 0.054 = 0.293$.

RCG, however, is something of an outlier, with aggregate behaviour much further from the equilibrium than in any of the other games. Relative to ICG, which is mathematically equivalent, subjects are less likely to cast the optimal vote - blue in RCG, red in ICG - both when their signal matches that vote ($1 - \hat{\sigma}(b) = 0.685$ in RCG and $\hat{\sigma}(r) = 0.919$ in ICG) and when it does not ($1 - \hat{\sigma}(r) = 0.387$ in RCG and $\hat{\sigma}(b) = 0.560$), with each of these differences significant with $p < 0.001$ using the permutation tests described in Appendix A.4. Subjects' apparent difficulty in RCG is discussed further in Section 1.4.2.6.

In each game, however, a focus on aggregate results belies the substantial heterogeneity we see amongst subjects in all but HGM. The remainder of the analysis thus focuses on individual behaviour.

1.4.2 Individual Behaviour

Figure 1.1 shows individual voting behaviour for each of the jury voting games, with each point representing the observed behaviour of one subject across 15 rounds of the given treatment. Measures of behaviour are equivalent to those of the aggregate analysis in the previous section. A small amount of random noise is added to each point such that those which overlap can be seen. Histograms of the distributions over each axis, which do not include this noise, are shown at the top and right of each chart. In all charts, a red circle shows aggregate behaviour while a red diamond shows the symmetric responsive equilibrium, each of which can be found in Table 1.3a. Table 1.4 classifies subjects as relevant types for each game based on the same measures, showing counts of those falling into each category.

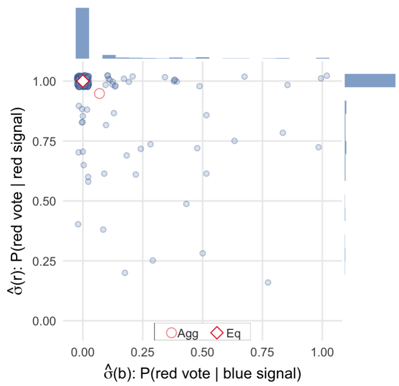
Table 1.4: Individuals by Behaviour Type

	$\hat{\sigma}(r)=1$			$\hat{\sigma}(r)<1$	
	$\hat{\sigma}(b)=0$	$\hat{\sigma}(b)\in(0,1)$	$\hat{\sigma}(b)=1$		
HGM	167 (75.9%)	20 (9.1%)	2 (0.9%)	31 (14.1%)	
HGU	49 (22.3%)	68 (30.9%)	54 (24.5%)	49 (22.3%)	
ICG	47 (21.4%)	42 (19.1%)	86 (39.1%)	45 (20.5%)	
	$\hat{\sigma}_B=0$			$\hat{\sigma}_B>0$	
	$\hat{\sigma}_R=0$	$\hat{\sigma}_R\in(0,1)$	$\hat{\sigma}_R=1$		
OCG	37 (16.8%)	27 (12.3%)	117 (53.2%)	39 (17.7%)	
	$\hat{\sigma}(r)=1$		$\hat{\sigma}(r)=0$		Other
	$\hat{\sigma}(b)=1$	$\hat{\sigma}(b)=0$	$\hat{\sigma}(b)=1$	$\hat{\sigma}(b)=0$	
RCG	24 (10.9%)	26 (11.8%)	3 (1.4%)	30 (13.6%)	137 (62.3%)

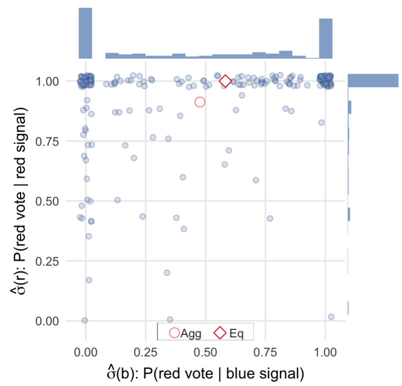
1.4.2.1 Majority Voting and Basic Rationality

Individual behaviour in HGM is generally very close to the equilibrium, with 167 of 220 subjects (75.9%) never deviating from informative voting. This suggests that subjects

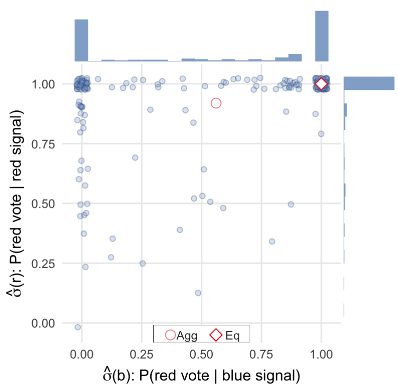
Figure 1.1: Individual Behaviour by Treatment.



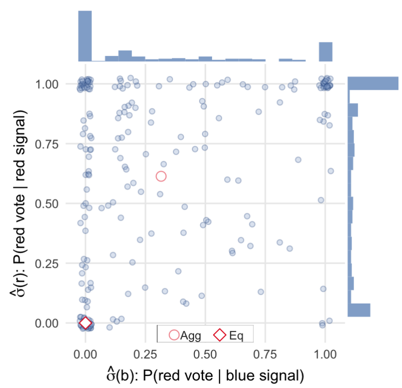
(a) HGM



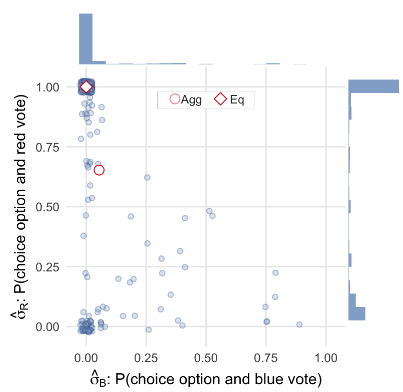
(b) HGU



(c) ICG



(d) RCG



(e) OCG

largely understand the game to at least the level of a naive Bayesian, recognizing that a private signal of a given colour implies that the same coloured jar is conditionally more likely than the other.

Indeed, in each of HGU, ICG, and OCG, irrational deviations from informative voting are rare - such as voting blue with a red signal in HGU or ICG, resulting in $\hat{\sigma}(r) < 1$, or selecting *Choice* and voting blue in OCG, resulting in $\hat{\sigma}_B > 0$. This further supports that subjects generally understand the game to at least a naive level. RCG sees many more irrational deviations, however, and is discussed further below.

1.4.2.2 Red Unanimity and Strategic Behaviour

In HGU, heterogeneity in individual behaviour tells a more nuanced story than the aggregate increase in $\hat{\sigma}(b)$. Now 49 of 220 subjects (22.3%) always vote informatively, 54 (24.5%) always vote red regardless of signal, and 68 (30.9%) always vote red given a red signal and mix otherwise. The ratio of observed aggregate strategies is $\frac{\hat{\sigma}(b)}{\hat{\sigma}(r)} = \frac{0.476}{0.912} = 0.522$, which is below the indifference threshold given homogeneous beliefs, 0.583, and thus always voting red is the best response to the aggregate distribution. However the similarity of the observed and equilibrium ratios make beliefs on either side of the equilibrium threshold appear reasonable, particularly given the limited information available to subjects with which to form these beliefs. Thus any strategy in which $\hat{\sigma}(r) = 1$ can be consistent with pivotal understanding, and therefore up to 171 subjects (77.7%) may be acting strategically without error; the HGU data alone cannot tell us more about whether they understand pivotal calculus or are driven by something else. More about this potential for rational heterogeneity is discussed below.

By controlling beliefs, ICG removes much of this ambiguity. There, the 47 subjects (21.4%) who vote informatively cannot be doing so optimally, but rather must fail to grasp the implications of pivotality. Likewise, the 42 (19.1%) who mix when receiving a blue signal and always vote red given a red signal, can no longer be doing so as part of an optimal strategy. More about mixing is said below. 86 (39.1%) play optimally, always voting red. Note in particular that this proportion is lower than the 77.7% of subjects whose behaviour is consistent with an understanding of pivotal calculus in HGU. This suggests that the figure for HGU may indeed be inflated by the ambiguity of informative voting, which can be driven by naivety or sophistication.

As in ICG, optimal behaviour is unambiguous in OCG. There, 117 subjects (53.2%) always play optimally, $\hat{\sigma}_R = 1$. That is, these subjects are willing to eschew their private information altogether in order to select *Choice* and vote for the red jar. 37 (16.8%) never select *Choice*, $\hat{\sigma}_R = \hat{\sigma}_B = 0$, instead receiving private information and thus voting informatively. By valuing private information in a setting in which pivotal calculus leads to red votes regardless of this information, these subjects show themselves to be unaware of this reasoning and incapable of pivotal calculus. Notably, however, the number doing this is lower than the number who always vote informatively in HGU and ICG, although this may simply be the result of greater experience (since OCG is always played last) leading to more sophisticated behaviour, as discussed below. 39 subjects (17.7%) select *Choice* and vote for the blue jar at least once - behaviour which is never optimal.

RCG sees much more heterogeneity and less consistency in individual behaviour. The number of subjects who always vote optimally - in this case, for the blue jar regardless of signal - drops to 30 of 220 (13.6%). 24 (10.9%) continue to always vote red, despite no longer being optimal, and 26 (11.8%) vote informatively. Just as noticeable is the 65 (29.5%) that now have non-pure empirical frequencies given both signals, appearing in the interior of Figure 1.1d. This is more than three times the 17 (7.7%) who do the same in ICG, despite the computer player strategies being mathematically identical. These unusual results are discussed further in Section 1.4.2.6.

1.4.2.3 Experience, Learning, and Knowledge Transfer

HGU and ICG I turn now to changes in subjects' behaviour over time, with a particular focus on HGU and ICG, for which there are interesting order and experience effects. Table 1.5 shows logistic regressions of the probability of a red vote in HGU and ICG, with each column restricting to different signals, treatments, and/or orders. Each of these two treatments is denoted *first* in sessions in which it is played before the other, and *second* in sessions in which it is played after the other. Independent variables are the round number within each treatment (1 to 15) and a series of dummies - for ICG, for whether the treatment is first of the two in that session, and for whether subjects have already played HGM.[10]

Table 1.5: Logit Regressions of Probabilty of Red Vote by Treatment, Order, and Signal.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	Both	Both	HGU	ICG	HGU	HGU	ICG	ICG
Order	Both	Both	Both	Both	First	Second	First	Second
Ind. Variable	$\hat{\sigma}(r)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$	$\hat{\sigma}(b)$
Constant	2.016	0.417	0.458	0.700	-0.638	0.289	-0.135	0.470
	0.000	0.165	0.255	0.096	0.020	0.278	0.664	0.089
ICG	0.123	0.348						
	0.468	0.001						
Round	0.001	0.001	-0.011	0.015	0.068	-0.012	0.053	0.016
	0.958	0.947	0.437	0.265	0.000	0.433	0.001	0.267
First	0.006	-1.019	-1.189	-0.832				
	0.987	0.000	0.000	0.013				
First*Round	-0.017	0.061	0.079	0.038				
	0.555	0.000	0.000	0.076				
HGM Prev	0.530	-0.158	-0.078	-0.236	-0.080		-0.236	
	0.112	0.507	0.810	0.496	0.806		0.497	
Obs	3,355	3,245	1,657	1,588	820	837	797	791

Notes: Standard errors are clustered by subject, with p-values presented below coefficient estimates.

Before discussing learning results, I first note the treatment effects between HGU and ICG, seen through Columns (1) and (2), which consider both games and segment the data by signal. These results agree with those of the earlier analyses: there is no

[10]In all regressions, dummies for having already played the Charness and Levin (2009) bidding game are insignificant and thus omitted.

difference in behaviour between the two treatments when subjects receive red signals, but given a blue signal, the increase in the likelihood of a red vote from HGU to ICG is both large and highly significant.

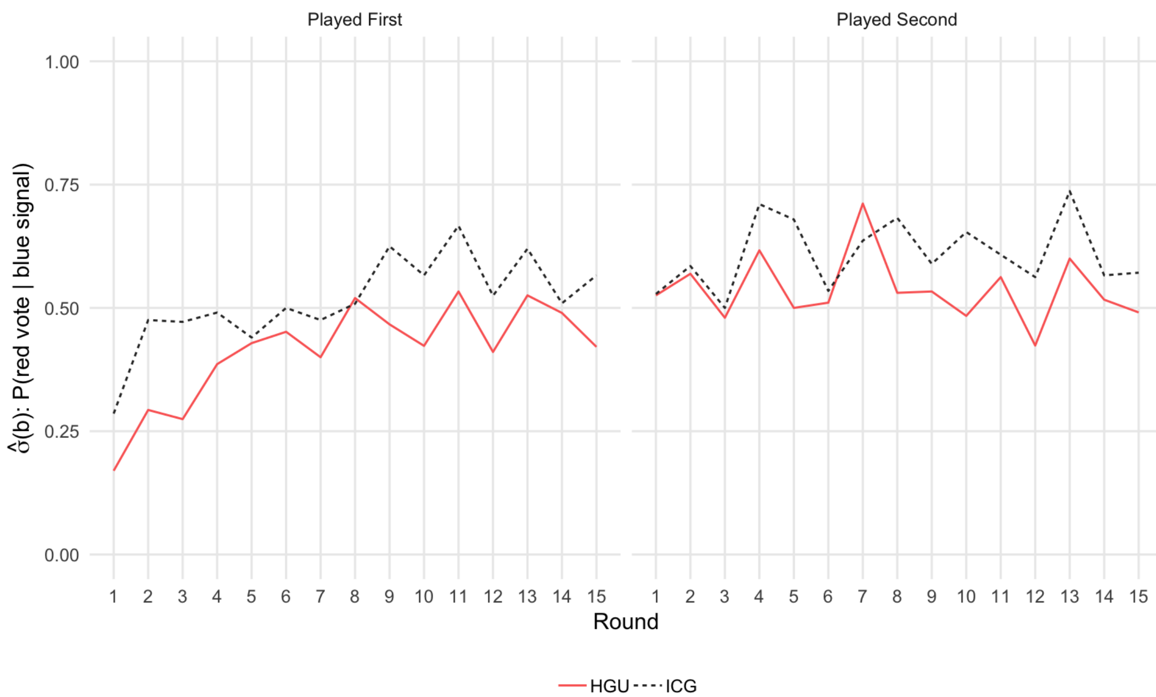
The regressions show substantial evidence that learning plays an important role in these two games when subjects receive blue signals. Moreover, this learning is not uniform throughout the session, but occurs only through the first of the two treatments played in each session. This can be seen in the fact that the coefficient on the round number is significant only in those regressions that restrict to the first treatment of each session (Columns 5 and 7), or when interacted with the *First* dummy in the regressions that pool over orders (Columns 2 through 4), and is insignificant otherwise. The coefficient is positive, and the magnitudes similar, in each case in which it is significant, implying that $\hat{\sigma}(b)$ increases through time. It is worth noting, however, that the point estimates are smaller and less significant in ICG than HGU, perhaps reflecting that there is less to learn about, given beliefs about others' behaviour are fixed.

In each of the regressions that pool data over both orders, Columns 2 through 4, we also see that the coefficient on the *First* dummy itself is large, negative, and significant at a high level.

Together, these results tell us that in the first red unanimity treatment that subjects play - either HGU or ICG - $\hat{\sigma}(b)$ begins relatively low but increases, while in the second it begins higher but then does not continue to increase. This is supported by Figure 1.2, which shows the aggregate $\hat{\sigma}(b)$ for each round of the two games, divided by order.

An interesting implication of these results is that that which subjects are learning in the first of these game appears to then be applied to the second - that is, the increase in $\hat{\sigma}(b)$ through the first carries over into the second. This suggests that subjects must be learning about something common to the two games, such as red unanimity and the resulting pivotal calculus, rather than something that differs, such as beliefs about others' strategies, which are fixed in ICG. Moreover, the fact that experience in each of HGU and ICG has such an effect on the other, but that prior experience in HGM has no effect on either - as shown in the insignificant coefficients on the *HGM Prev* dummy - suggests that it must be red unanimity that subjects are learning about.

Figure 1.2: Aggregate Behaviour Given Blue Signal in HGU and ICG by Order



There is also some evidence that this learning manifests in one-time insights rather than random exploration or short-sighted reactions to gains and losses. In particular, I distinguish here between persistent behavioural changes, which are consistent with insights, such as that of pivotal understanding, and non-persistent changes, which are not. To this end, I classify a subject as *switching* if after the first time they vote red with a blue signal they do so for the remainder of the treatment, and as *mixing* otherwise, that is if after voting red with a blue signal they later return to voting blue.[11] Since such a classification is sensitive to random errors in actions, I restrict here to those 175 subjects who play $\hat{\sigma}(r) = 1$ across both treatments, which I take as a sign that they are less prone to this. Table 1.6 shows that switching is substantially more common in the first treatment each subject plays than in the second (significant in a difference of proportions test with $p = 0.017$), but that switching rates do not differ between the two treatments. Mixing rates, however, are the same in subjects' first and last treatments, but is significantly more common ($p = 0.026$) in HGU, where it may be rational, than ICG, where it is not. These results are consistent with the notion that switching is due to subjects learning about something common to the two games, and that this largely occurs in the first of these two treatments then persists to the second.

Table 1.6: Switching and Mixing subject counts by treatment and experience

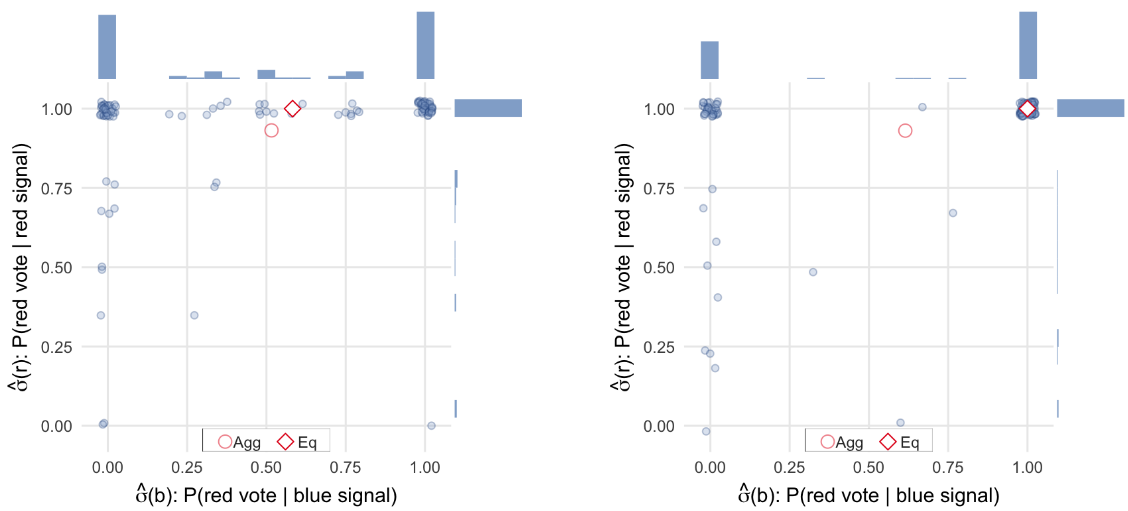
	Mix	Switch	Total
Total	51	45	96
HGU	38	22	60
ICG	13	23	36
First	25	33	58
Second	26	12	38

Given the clear importance of learning in these games, it is instructive in classifying subjects' overall capabilities to consider their behaviour when most experienced. Figure 1.3 shows each subject's behaviour in the last half (rounds 9-15) of the second HGU or ICG treatment that each subject plays. Since each subject only plays one of these games second, the charts each feature 110 subjects, and are thus less dense than those of Figure

[11] Note that this may yet exclude some who develop pivotal capability, including, for example, those who mix as a form of exploration before switching to always voting red, but avoids arbitrary switching thresholds as would be required for a more inclusive definition.

1.1, but the contrasts are clear. Aggregate behaviour and individual classifications are presented in Table 1.7.

Figure 1.3: Individual Behaviour when Experienced (HGU and ICG)



(a) HGU (last 7 rounds)

(b) ICG (last 7 rounds)

Table 1.7: Aggregate Behaviour and Individuals by Behaviour Type when Experienced (HGU and ICG)

	Aggregates		$\hat{\sigma}(r) = 1$			$\hat{\sigma}(r) < 1$
	$\hat{\sigma}(b)$	$\hat{\sigma}(r)$	$\hat{\sigma}(b) = 0$	$\hat{\sigma}(b) \in (0, 1)$	$\hat{\sigma}(b) = 1$	
HGU	0.516	0.932	32 (29.1%)	21 (19.1%)	43 (39.1%)	14 (12.7%)
ICG	0.615	0.931	29 (26.4%)	1 (0.9%)	68 (61.8%)	12 (10.9%)

The most stark result is that mixing has all but disappeared as a strategy in ICG, despite figuring substantially in the overall data presented in Figure 1.1 and Table 1.4, with just one subject mixing when experienced. This compares to 21 mixing subjects in HGU (significant in a difference of proportions test with $p \approx 0.000$). The difference is accounted for by more experienced subjects always voting red in ICG than in HGU ($p = 0.001$). Notably, these differences are consistent with theory, with always voting red the only rational strategy in ICG, but mixing also rationalizable in HGU. The treatments do not differ significantly in the number of subjects that always vote informatively, or that play $\hat{\sigma}(r) < 1$. The aggregate difference in $\hat{\sigma}(b)$, significant at $p = 0.007$ by the permutation test described in Appendix A.4, is thus driven by the differences in mixing and red-voting behaviour.

Overall, some 68 of 110 (61.8%) play optimally in ICG when experienced, always voting red. If we take informative voting in HGU to be naive rather than sophisticated, as discussed above, then some 64 (58.2%) play optimally in HGU when experienced, playing $\hat{\sigma}(r) = 1$ and $\hat{\sigma}(b) > 0$. These proportions are not significantly different.

Finally, it is interesting to note that in both groups, those who maintain $\hat{\sigma}(r) < 1$ once experienced largely play $\hat{\sigma}(b) = 0$, suggesting that blue votes given red signals are driven by something more fundamental than a tendency towards randomization. That it persists in both treatments implies that it is not simply a (misguided) reaction to others subjects' unusual behaviour, but since such votes are never optimal for any beliefs in either treatment it is not clear what might drive such behaviour.

HGM, RCG, and OCG Table 1.8 presents learning regressions for each of the remaining jury voting games. Columns 1 through 4 present logistic regressions of the probability of a vote for the red jar conditional upon each signal type for each of HGM and RCG. Columns 5 and 6 present logistic regressions of the probability of selecting *Choice* followed by each vote colour - that is $\hat{\sigma}_R$ and $\hat{\sigma}_B$ respectively - in OCG. The HGM regression includes dummies for prior experience in HGU and ICG. These are not relevant for RCG and OCG, which always follow the other games.[12]

In HGM, there is no evidence of learning for either signal type. This likely reflects the fact that the optimal behaviour coincides with what we would expect of a naive subject - i.e. informative voting - and that subjects arrive relatively quickly at this behaviour. That is, since there is no distinction between naive behaviour and sophis-

[12]As above, in all regressions dummies for having already played the Charness and Levin (2009) bidding game are insignificant and thus omitted.

Table 1.8: Logit Regressions of Probabilty of Red Vote by Treatment and Signal.

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	HGM	HGM	RCG	RCG	OCG	OCG
Ind. Variable	$\hat{\sigma}(r)$	$\hat{\sigma}(b)$	$\hat{\sigma}(r)$	$\hat{\sigma}(b)$	$\hat{\sigma}_R$	$\hat{\sigma}_B$
Constant	2.947	-2.465	0.670	-0.500	0.480	-3.314
	0.000	0.000	0.000	0.008	0.011	0.000
Round	-0.011	-0.025	-0.034	-0.044	0.021	0.012
	0.717	0.328	0.003	0.001	0.000	0.360
HGU Prev	0.231	-0.109				
	0.672	0.814				
ICG Prev	0.064	-0.214				
	0.906	0.677				
Obs	1,638	1,662	1,715	1,585	3,300	3,300

Notes: Standard errors are clustered by subject, with p-values presented below coefficient estimates.

ticated behaviour, there is not as much for subjects to learn given a naive starting point.

In RCG there is evidence of learning, with the coefficient on the round variable negative and highly significant for both signals. Since optimal behaviour is to always vote for the blue jar, the negative coefficient reflects improvement over time. Recall that subjects playing RCG have already played HGU and ICG and, as per the above discussion, do not learn through the second of these. That they return to learning in RCG underlines the implication from the aggregate results that RCG is in some way different to, and perhaps more difficult than, the other games, despite the pivotal calculus being mathematically identical to ICG. More is said about RCG in Section 1.4.2.6.

OCG shows similar learning results. Here the pivotal calculus is identical to ICG - pivotality implies four red computer signals, which implies a red vote is optimal regardless of the signal - which in this case should lead subjects to select *Choice* and vote for the red jar. Again, however, the rate at which subjects do this increases over time, despite subjects already having played HGU and ICG. Like RCG, that subjects continue to learn here suggests that OCG imposes some additional difficulty upon subjects beyond simply red unanimity and the resulting pivotal calculus. That is, the decision between *Choice* and *Information* is non-trivial, even with subjects experienced enough to have exhausted the learning opportunities of the earlier red unanimity games.

1.4.2.4 HGU and Heterogeneous Strategic Voting

The comparison between HGU and ICG also sheds light upon a difficult and previously unanswered question, namely which strategies are played in HGU by those who are pivotally capable. In particular, while both informative voting and mixing can be rational in HGU, it has been unclear whether those playing such strategies do so due to pivotal thinking and appropriate beliefs, or due to other drivers.

Informative voting provides an especially difficult case in that, while it can be rational, it is also the behaviour we expect to see from those naive subjects who understand the basic information structure of the game but do not understand the importance of pivotality. The results here, however, suggest that sophisticated subjects do not vote informatively. To see this, we use the fact that informative voting in ICG can only be explained by naivety, and thus use its prevalence there - 47 of 220 subjects (21.4%) overall and 29 of 110 (26.4%) when experienced - as a baseline estimate of the naivety amongst the subject pool. That its prevalence in HGU is almost identical - 49 of 220 subjects (22.3%) overall and 32 of 110 (29.1%) when experienced - suggests that subjects are not voting informatively in HGU for reasons beyond the naivety observed in ICG.

Within-subject evidence is presented in Table 1.9, which classifies subjects by observed strategies across both of HGU and ICG.[13] Of those 33 subjects who always vote red when playing ICG first, the only behaviour consistent with pivotal capability, just

[13]As above, I restrict the analysis to those who play $\hat{\sigma}(r) = 1$ through both treatments in order to minimize the effects of those with a tendency for random exploration or errors.

Table 1.9: Individual behaviour in HGU and ICG by treatment and order

HGU (First)	ICG (Second) Inform.	Red	Switch	Mix	Total
Inform. ($\hat{\sigma}(b) = 0$)	16	3	5	0	24
Red ($\hat{\sigma}(b) = 1$)	0	17	1	0	18
Switch ($\hat{\sigma}(b) \in (0, 1)$)	1	16	2	0	19
Mix ($\hat{\sigma}(b) \in (0, 1)$)	0	11	1	4	16
Total	17	47	9	4	

(a) HGU (First) and ICG (Second)

ICG (First)	HGU (Second) Inform.	Red	Switch	Mix	Total
Inform. ($\hat{\sigma}(b) = 0$)	13	1	2	6	22
Red ($\hat{\sigma}(b) = 1$)	1	23	1	8	33
Switch ($\hat{\sigma}(b) \in (0, 1)$)	2	8	0	4	14
Mix ($\hat{\sigma}(b) \in (0, 1)$)	2	3	0	4	9
Total	18	35	3	22	

(b) ICG (First) and HGU (Second)

one (3.0%) votes informatively when later playing HGU. This is clearly lower than the 17 of 45 (38.6%) of those who play other strategies when playing ICG first (significant in a difference of proportions test with $p \approx 0.000$). Likewise, of the 24 who vote informatively when playing HGU first, just 3 switch to always voting red in ICG (12.5%), which is lower than those 44 of 53 (83.0%) who play other strategies in HGU ($p \approx 0.000$).

Likewise mixed strategies, that is $\hat{\sigma}(r) = 1, \hat{\sigma}(b) \in (0, 1)$, have multiple possible drivers in HGU. While the symmetric responsive equilibrium predicts this behaviour in response to the belief that the aggregate strategy mixes similarly and in the correct ratio, it may also be driven by random errors from those otherwise playing pure strategies, or by those switching from one pure strategy to the other, as discussed above. Here, the contrast in prevalence between HGU and ICG, the latter of which shuts down rationality as a driver by fixing beliefs but should have no impact on subject's tendency towards errors or uncertainty, is telling. As discussed above, this is most starkly seen amongst those who are experienced, where 22 of 110 (19.1%) mix in HGU but just one (0.9%) does so in ICG ($p \approx 0.000$), strongly suggesting that this mixing in HGU is driven by rationality.

Again, within-subject evidence supports this, with 9 of the 33 (24.2%) who always vote red when playing ICG first proceeding to play $\hat{\sigma}(b) \in (0,1)$ in HGU, roughly in line with the 16 of 45 (35.6%) of others who do so. Likewise, 27 of the 35 (77%) who play $\hat{\sigma}(b) \in (0,1)$ when playing HGU first then always vote red in ICG, significantly more than the 20 of 42 (47.6%) of others who do so ($p = 0.016$).

Of course, it may not be that all such behaviour is genuine mixing. As discussed above, more than a third of those playing $\hat{\sigma}(b) \in (0,1)$ in HGU are in fact *switching*, which may be indicative of those developing insights about the game and switching from one pure strategy to another. Regardless, there is substantial evidence for genuine mixing among the pivotally capable - and thus presumably beliefs that others are also pivotally capable - in HGU.

1.4.2.5 OCG and Strategic Behaviour

As discussed above, the fact that subjects learn through OCG even after they have stopped learning in HGU and ICG suggests that the new task it presents is not trivially different to the tasks in those games. How, then, does subjects' behaviour relate between these games? In general, behaviour in OCG is as if those who appear to be capable of pivotal thinking in HGU and ICG truly are, and those who don't appear so are not. As OCG is played last of the five jury voting games, I again classify subjects' behaviour in those earlier games according to their play when most experienced - that is, in the last 7 rounds of the second red unanimity treatment played, as in the previous section. Of those 111 who always vote red there, 91 (82.0%) always select *Choice* and vote red in OCG, while just six such subjects (5.4%) always choose *Information*.

Subjects who have previously displayed pivotal capability thus largely appear capable of eschewing private information in order to vote red.

Likewise, of those 61 who vote informatively when experienced in the earlier red unanimity games, 28 (45.9%) always choose *Information* in OCG. However 10 (16.4%) always choose *Choice* and vote red, thus playing optimally.[14] This behaviour is not easily explained, but perhaps the most plausible cause is simply that since OCG is played last of all the jury voting games they have had additional time to learn.

Overall, behaviour in OCG aligns well with an understanding of pivotal calculus, particularly amongst those whose behaviour is consistent with it in previous treatments. That performance is worse than ICG, as discussed above, suggests that the additional element is non-trivial, but the division of subjects' capabilities seems similar between the two games.

1.4.2.6 The Reverse Computer Game

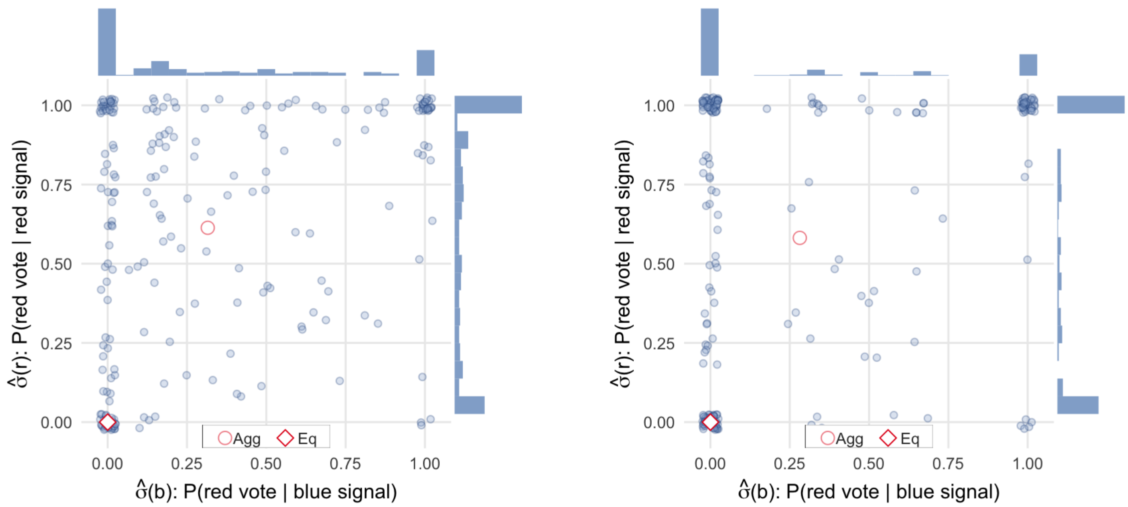
Throughout, behaviour in RCG has been something of an outlier. As well as aggregate behaviour being further from theoretical predictions than other treatments, individual behaviour is notably more heterogeneous and less consistent, with each semi-mixed strategy observed, along with complete mixing. Many of these strategies cannot be easily explained. This comes despite the fact that, in theory, the pivotal calculus is no more complicated than in ICG.

One possible explanation is that subjects are accustomed to the previous computer behaviour (that is, that of ICG) and take time to come to understand that the implications of pivotality are now reversed, thus appearing to be mixing when really switching. Indeed some 29 (21.2%) of those 137 who cast votes in both directions appear to be switching, which I define equivalently to the above - that is, as always voting blue with

[14] Four of these played ICG after HGU and are thus classified based on that behaviour, so it is not simply the case that this informative voting may have been rational.

a red signal once doing so for the first time. We can see the results of this in Figure 1.4b, which restricts to only data from the last 7 rounds of RCG, and shows much less apparent mixing than Figure 1.4a, which uses all rounds (and thus simply recreates Figure 1.1d).

Figure 1.4: Individual Behaviour, Reverse Computer Game



(a) All Rounds

(b) Last 7 Rounds

Even with experience, however, many fewer play optimally than in previous treatments. Just 55 (25.0%) always vote blue when experienced (i.e. in the last 7 rounds of RCG), while 33 (15.0%) continue to always vote red, which is now the strategy with the lowest expected payoff. This includes 30 (27.0%) of those 111 who always voted red in the earlier red unanimity games when experienced, suggesting that they were capable of pivotal reasoning.

As discussed in Section 1.2, one possible explanation for the failure to play rationally here is that while the computer players' behaviour is no more complex mathematically than in ICG, it is far enough from expected or 'reasonable' behaviour - in fact, it clearly works against subjects' best interests in that computer players sometimes for red given a blue state - that subjects find it more difficult to reason about, and become confused. While there is no clear reason why this should cause subjects' reasoning to fail, this confusion hypothesis is supported by the fact that subjects don't simply revert to naive informative voting, but in many cases do worse than this. In particular, 84 of 220 (38.2%) of subjects are more likely to vote against their information when it is irrational to do so than when it is rational ($\hat{\sigma}(b) > 1 - \hat{\sigma}(r)$). This is much more common than in ICG, where 33 of 220 (15.0%) do the equivalent ($1 - \hat{\sigma}(r) > \hat{\sigma}(b)$).

As discussed above, equivalent behaviour is seen in the equivalent treatment (simultaneous, with feedback) of Esponda and Vespa (2014), in which the semi-mixed computer-player strategies may also be seen as unusual or confusing. There, subjects' information takes the form of a prior rather than a signal, but the results are similar. Over the first 15 rounds, which gives the same amount of experience as those playing RCG here, 30 of 58 subjects (52.7%) vote red with a blue prior more often than they vote blue with a red prior - that is, they vote against their information more often when it is irrational to do so than when it is rational. That subjects are apparently confused by the mathematically simple computer play of RCG makes it easier to believe that they could be similarly confused in Esponda and Vespa (2014), and goes some way to explaining the lower levels of strategic behaviour seen in that paper relative to other existing studies using the more standard game, discussed in Section 1.2.1.

It seems clear, then, that while subjects may appear strategic elsewhere, it is not necessarily the case that these capabilities extend well to even very closely related games. Where exactly the breakdown occurs warrants further investigation.

1.5 Conclusions

Overall, the variations on the Guarnaschelli et al. (2000) game presented here provide encouraging evidence for subjects' understanding of some amount of pivotal calculus. Perhaps the cleanest test of this is the Informative Computer Game, which simplifies the pivotal calculus relative to games played in human groups. There, 39.1% of subjects act optimally throughout, increasing to 61.8% when subjects are experienced. This behaviour correlates strongly with apparently sophisticated behaviour in the basic jury voting game with human groups, in that those who appear sophisticated in one are more likely to appear so in the other, supporting the idea that those who have appeared as if they were sophisticated in previous experiments may truly be so. Learning also plays an important role, as previously suggested by Esponda and Vespa (2014). Much apparent mixing behaviour appears to be a result of subjects switching from pure informative voting to pure strategic voting (that is, always voting red), rather than actual randomization. Notably, however, mixing persists once subjects are experienced in HGU, where it is potentially rational, but not in ICG, where it is not.

Further support for pivotal thinking is found in many subjects' willingness to forgo private information in order to behave strategically in the Option Game. Moreover, a substantial proportion of subjects that vote informatively in the human games continue to do so in the ICG and Option treatments, implying that their behaviour in the human-group games was a result of lacking pivotal understanding, rather than due to forming beliefs about others that make such behaviour optimal. There is evidence, however, that those who mix in human-group games do so due to pivotal reasoning.

Finally, however, the results of the RCG treatment suggest that if subjects truly do understand pivotality, their ability to respond optimally to this understanding must be limited, as a large majority of previously sophisticated subjects fail to behave optimally in the face of this new group-mate behaviour. Alternately, it may be that some subjects have alternate drivers of seemingly-strategic behaviour, such as a desire to herd or abstain, that does not track with pivotal understanding in all games. In either case, the external validity of subjects' apparent pivotal capabilities is not guaranteed, even in closely related games. Further, this suggests that a failure of pivotal capability can be driven not just by a failure to condition on hypothetical events, as per Esponda and Vespa (2014), but also in the information extraction stage, even for those who appear capable of hypothetical thinking elsewhere.

Chapter 2

Hypothetical Thinking Across Games

2.1 Introduction

Many games ask players to make inferences about the state of the world from other players' actions or other state-dependent events. Distinct among them are those in which these events are *hypothetical* in that they have not yet and may not occur. This paper focusses on two such games - jury voting games and a single-player equivalent of a common value auction - and investigates whether this common element leads to common capability within subjects playing both, as is often assumed.

In the canonical model of jury voting due to Austen-Smith and Banks (1996) and Feddersen and Pesendorfer (1998) a small group determine through voting whether to convict or acquit a passive defendant. The jury does not observe whether the defendant is guilty or innocent, each of which is equally likely, but rather each juror receives an independent noisy signal of guilt then votes for either outcome. The defendant is convicted if the number of conviction votes exceeds some threshold and is acquitted otherwise, with jurors rewarded when a guilty defendant is convicted or an innocent defendant acquitted. The crucial insight of the model is that an optimizing player should condition on her own hypothetical pivotality, and thus her own vote's payoff-relevance. That is, if conviction requires r votes then she should vote as if $r - 1$ others will vote for it, despite a generally low probability that this occurs. If r is high enough relative to the size of the group, the informational content of these hypothetical votes may induce her to vote for conviction even when this contradicts her own private signal. Amazingly, Guarnaschelli, McKelvey, and Palfrey (2000) and Goeree and Yariv (2011) show that many subjects display behaviour consistent with this. Yet crucially, Esponda and Vespa (2014) show, through a related setup, that the hypothetical nature of pivotality causes subjects considerable difficulty, with many more capable of the same informational inferences when pivotality is already observed.

In a common value auction, meanwhile, players bid for a prize after each receiving independent noisy signals about its randomly drawn but unobserved value, with the highest bidder receiving that value in exchange for their bid. Here the relevant hypothetical event is that a player makes the winning bid - or, said another way, that each other player's private information led them to make lower bids. A sophisticated player should recognize that the prize's expected value conditional upon winning the auction - and thus making a transaction - is lower than its value conditional only upon her private signal, and should decrease her bid accordingly. Failure to do so results in overbids and expected losses - the so-called Winner's Curse, first described by Capen et al. (1971). Charness and Levin (2009) discuss substantial previous evidence of the Winner's Curse in various settings before presenting experimental evidence that it occurs extensively even in a simplified single-player environment.

Common to both games is that a hypothetical event, necessary to the payoff-relevance of one's action, makes clear implications about the state of the world and thus the player's optimal action. Although there are differences between them - most notably that a player's bid in a common value auction impacts the probability that the hypothetical event occurs, while their jury vote does not - it seems reasonable to conclude that the insights required for one carry over to the other. Surely the capability to condition upon the hypothetical - which Esponda and Vespa (2014) refer to as *hypothetical thinking* - applies to both, and for this reason the two games are commonly linked, both in common usage and the literature. Esponda and Vespa (2014), for example, in presenting a jury-voting experiment, utilize common-value auctions as the primary motivating example, and freely connect their results to those of the auction game of Charness and Levin (2009). A second paper, Esponda and Vespa (2019), tests the extent to which one aspect of hypothetical thinking, the *sure-thing principle*, connects these and other games. Koch and Penczynski (2018) suggest that a transformation of an auction game to remove conditional reasoning could be similarly used in jury voting and persuasion games. Perhaps the clearest connect between the two is due, like the canonical jury game itself, to Feddersen and Pesendorfer (1996):

> "Both in auctions and in elections an agent's action only matters in particular circumstances: when an agent is the high bidder in an auction or when an agent is a swing voter in an election. In either case, when other agents have private information that may be useful to an agent, the agent must condition his action not only on his information but also on what-must be true about the world if his action matters."

A more general common element - that players must make make inferences about others' information from their actions, whether hypothetical or not - is the focus of Eyster and Rabin (2005) *cursed* equilibrium. The authors seek to reconcile equilibrium theory with the observation that experimental subjects often fail to respond fully to the informational implications of others' actions. To this end they present a model in which each player, when forming beliefs about others' strategies, will with some positive probability fail to recognize that different types will act in different ways, instead assigning them the average strategy across all types. Relative to standard equilibria, this results in higher bids in common value auctions and less strategic voting in jury voting games. Given the extent to which the cursed equilibrium model is associated with the games presented here, I discuss it in the context of the results for both, but find it adds little to our understanding of the data.

The primary purpose of this paper is to test this link between these games through a within-subject design in which subjects play both games, allowing direct investigation of whether capability in one correlates to capability in the other. To my knowledge, only one other experiment, Esponda and Vespa (2019), sees subjects play versions of both games. There, however, the relationship between play in one game and play in the other is not reported, with the focus instead on changes in play across multiple versions of each game. Moreover, the focus on testing the implications of the *sure-thing principle* necessitates that each game is significantly transformed, and each is played only once each, in contrast to the experiment presented here in which the canonical versions of each are played for multiple rounds each.

2.2 Experimental Design and Game Results

The experiment presented here features a within-subject design in which subjects play 15 rounds each of six games: an auction game designed by Charness and Levin (2009), designed to investigate the winner's curse, and five variations on the jury voting game designed by Guarnaschelli, McKelvey, and Palfrey (2000). The auction game and its results are presented in Section 2.2.1 and the jury games and their results in Section 2.2.2. This primary focus of this paper is the relationship between the auction and jury voting games, presented in Section 2.3, while the relationships between the five jury voting games are the focus of Chapter 1.

The experiment was conducted over 16 sessions with 220 subjects at the Columbia Experimental Laboratory for the Social Sciences (CELSS) from November 2016 to October 2017. Of these, 110 played the auction game before the five jury voting games, and 110 played it after. The order of the jury voting treatments also varies, as outlined in Section 2.2. The subject pool consists of Columbia students, primarily undertaking undergraduate studies. Subjects were paid a $5 show-up fee, 25c for each of the 75 jury voting rounds in which their group made a correct decision, and 25c for each 120 points earned in the auction game. Sessions lasted from 50 to 75 minutes, with an average payment of $20.13.

The interface was programmed using the oTree software package (Chen et al. (2016)) and subjects recruited via ORSEE (Greiner (2004)). Experimental instructions are presented in Appendix A.1 of Chapter 1, the jury voting interface in Appendix A.3 of

Chapter 1, and the auction game interface in Appendix B.1.

2.2.1 Auction Game

2.2.1.1 Design

The Charness and Levin (2009) auction game was designed as a single-person variation of Samuelson and Bazerman (1985) 'Acquire a Company' game (itself an application of the Akerlof (1970) 'lemons' model), in which a player bids on a company of unknown value, acquiring it if her bid is high enough.

In each round, the subject is endowed with 120 points, and is shown an array of 100 virtual cards numbered 20 through 119 inclusive, arranged in random order with these values hidden.[1] The subject is asked to make a whole-numbered bid, b, from 0 to 120 inclusive, and then selects a card, effectively drawing a value from $U[20, 119]$. The number on this card, its value v, is then revealed. If the subject's bid is at least the value of the revealed card, the subject pays their bid and receives 1.5 times the value of the card. No transaction occurs otherwise. That is, payoffs, net of endowment, are given by

$$\pi(b, v) = \begin{cases} 1.5v - b & \text{if } b \geq v \\ 0 & \text{if } b < v \end{cases}$$

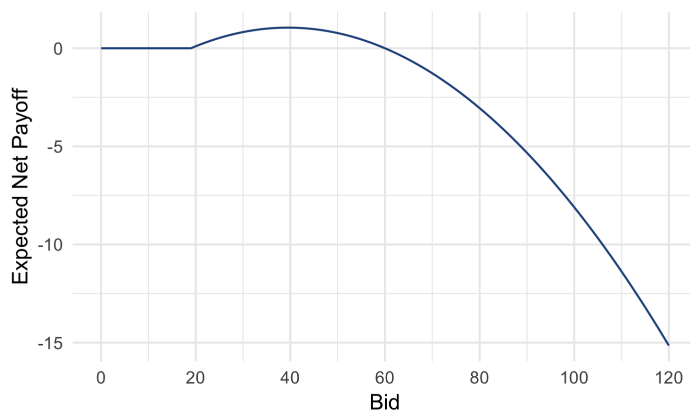
For a given bid, the expected payoff, net of endowment, is given by

$$E[\pi|b] = P[v \leq b] \cdot E[1.5v - b|v \leq b] = \begin{cases} -15 & \text{if } b = 120 \\ \frac{b-19}{100} \cdot [\frac{3}{2}\frac{20+b}{2} - b] & \text{if } b \in [20, 119] \\ 0 & \text{if } b < 20 \end{cases}$$

[1]The game is thus equivalent to Charness and Levin's 'Shifted-100VT' treatment.

Figure 2.1 shows this expected payoff for each possible bid. Subjects are told their payoff at the end of each round, and at the end of the session are paid 25 cents for every 120 points earned, rounded up to the nearest 25c.

Figure 2.1: Expected Net Payoff by Bid in the Auction Game

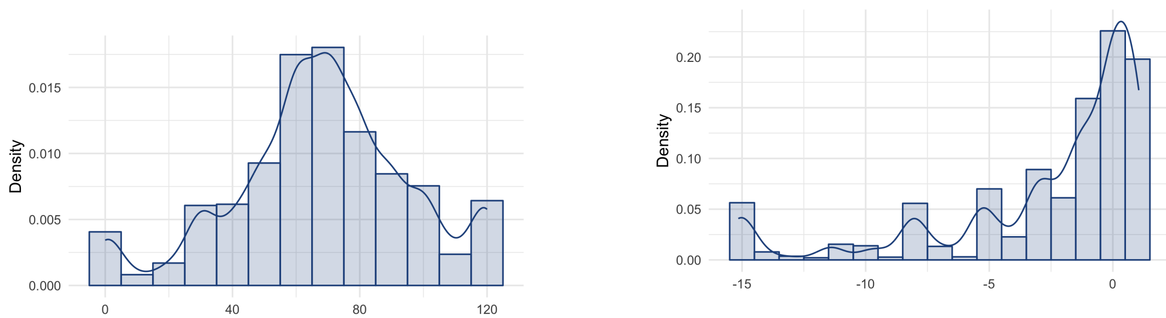


Solving for the optimal bid, $b = 40$, is not trivial given that b impacts both terms of the expectation. A less demanding standard is that subjects avoid *overbids* - bids which have negative net expectation, $b > 60$, which depends only on the sign of $E[1.5v - b|v \leq b] = 1.5 \cdot E[v|v \leq b] - b$. While generally this requires us to take expectations over the distribution of b, which we cannot expect subjects in the lab to do, the uniform distribution used here means that reasonable rules of thumb lead to the correct answer. In particular, since $E[v|v \leq b] = \bar{v}(b)$ where $\bar{v}(b)$ is the median of the range $[20, b]$ - that is, the median of all values that lead to a transaction - a bid b gives an expected loss if and only if this median value would result in a loss, $1.5\bar{v}(b) - b < 0$. Thus a subject that uses $\bar{v}(b)$ as a reference for evaluating a bid b - which seems intuitively reasonable - may avoid overbidding even without taking expectations more formally. Thus avoiding overbids provides a more reasonable benchmark for behaviour than attaining the optimal bid of 40.

2.2.1.2 Results

Figure 2.2a and Table 2.1 replicate the presentation of Charness and Levin (2009), the former showing the distribution of bids and the latter presenting summary statistics and categorizing bids as being greater than, equal to, or less than the optimal bid of 40. Figure 2.2b displays the bid distribution in terms of expected payoff. Throughout, all payoffs are net of the endowment.

Figure 2.2: Distributions of Aggregate Bids



(a) Distribution of Bids

(b) Distribution of Bids by Expected Payoff

Table 2.1: Aggregate Bidding Behaviour

Source	Obs	Avg Bid	Bid > 40	Bid 40	Bid < 40
This paper	3,300	67.9	2,772 (84.0%)	101 (3.1%)	427 (12.9%)
Charness and Levin (2009)	1,770	68.4[2]	1,561 (88.2%)	46 (2.6%)	163 (9.2%)

The results are largely similar to those of Charness and Levin (2009), and in particular show strong evidence for the Winner's Curse, with subjects frequently bidding too much. The average bid of 67.9 points not only exceeds the optimal bid of 40, but also the break-even bid of 60, and thus results in a negative expected payoff. The rates at which subjects bid more than the optimal amount is much larger than the rate at which they bid below it, and in each case these rates are similar to those of Charness and Levin (2009), with a χ^2 test finding no difference in the distributions presented on the right of Table 2.1 ($p = 0.1991$).

Table 2.2: Aggregate Underbidding and Overbidding

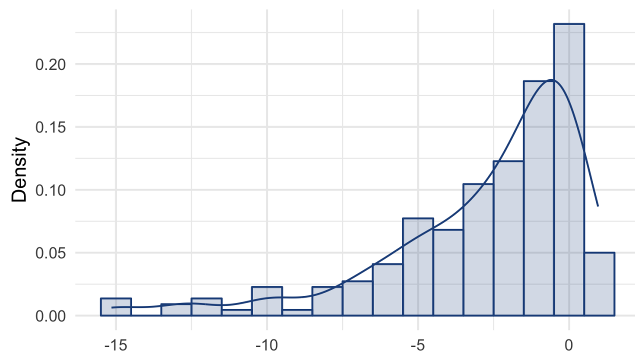
Rounds	Avg Bid	Avg Exp	Underbids ($b < 20$)	Good Bids ($b \in [20, 60]$)	Overbids ($b > 60$)	Small Over. ($b \in (60, 90)$)	Large Over. ($b \geq 90$)
All	67.9	-2.78	166 (5.0%)	1165 (35.3%)	1969 (59.7%)	1208 (36.6%)	761 (23.1%)
First 7	68.3	-2.58	62 (3.5%)	526 (34.9%)	952 (61.6%)	718 (40.1%)	233 (21.0%)
Last 7	67.6	-2.94	93 (6.2%)	560 (37.4%)	887 (56.5%)	589 (32.1%)	298 (24.4%)
Change	-1.6	-0.32	31 (2.7%)	34 (2.5%)	-65 (5.2%)	-129 (8.5%)	65 (3.4%)
p-value	0.442	0.015	0.004	0.160	0.013	0.000	0.005

As discussed in the previous section, I wish to focus in particular on the less demanding standard of whether subjects are able to avoid overbids with negative expectation, $b > 60$. Table 2.2 recategorizes bids along these lines, further splitting these overbids into *large* overbids - those of at least 90 points, which result in expected losses of at least 5.5 points - and and *small* overbids - those of between 60 and 90. Overbids make up 59.7% of all bids submitted, with 23.1% large overbids. Underbids - those bids below 20, which ensure that no transaction takes place regardless of the value of the drawn card - account for 5.0% of all bids.

This susceptibility to the Winner's Curse is not driven by just a few badly performing individuals, but rather is widespread in the individual-level data. Figure 2.3 shows the distribution of subjects by their mean expected payoff across all 15 rounds. 186 of 220 (84.5%) have negative expected payoffs overall, while 71 (32.3%) have a mean expected payoff below -3.4, which is the figure attained by simply randomizing uniformly over $[20, 120]$, the range of bids that can result in a transaction. That is, the vast majority of subjects would expect to be better off by always bidding below 20 and ensuring they keep the endowment, while almost a third would expect to be better off by randomizing than by casting their observed bids. A majority, 137 of 220 (62.3%), overbid more often than not, with 34 (15.5%) overbidding in all 15 rounds. Complete bidding behaviour is presented in Figure B.2 in Appendix B.2.

Behaviour is also highly variable on a within-subject basis, with a mean within-subject range of 53.5 points and standard deviation of 16.5 points. The mean absolute

Figure 2.3: Distribution of Subjects by Mean Expected Payoff

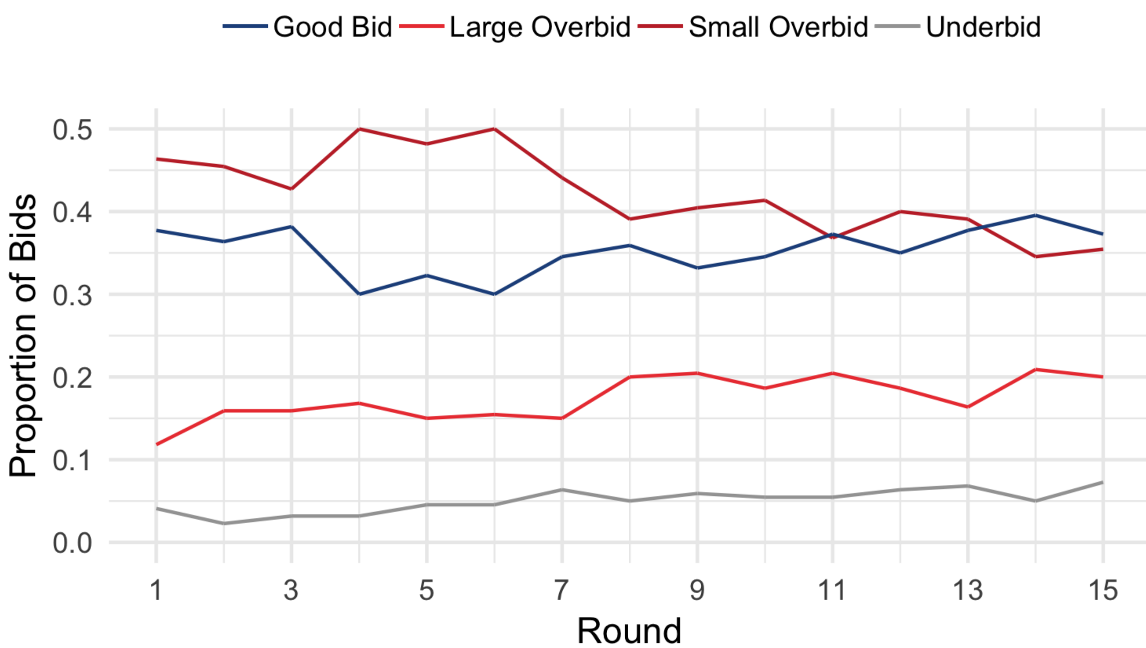


difference between a bid and the same subject's previous bid is 11.6 points. Moreover, the changes in a subject's bids are generally non-monotonic, with the movements changing direction - that is, the subject increases (decreases) their bid when the last change was to decrease (increase) it - on average 4.8 times over 15 rounds of play. A reproduction of the Charness and Levin (2009) analysis, presented in Appendix B.2, shows that bid adjustments are highly dependent on the subject's gains and losses from the previous round, with those making losses more likely to decrease their bid, and those making gains more likely to maintain or increase it.

There is also some evidence that behaviour changes across time in a more persistent way - that which we would call learning if it were an improvement. The second and third rows of Table 2.2 compare aggregate behaviour from the first seven of the Auction Game rounds (that is, the first half, rounded down to avoid and overlap) to the last seven, with the differences in the fourth row. The final row shows p-values for two-tailed permutation tests for each difference. These tests are described in Appendix B.3. In particular, the rate of underbids increases from the first half to the last half of the treatment while the rate of overbids decreases between the same periods. The change in good bids, those between 20 and 60, is not significant. But while the overall rate of overbids decreases, the rate of large overbids - bids of at least 90 points - increases, while the rate of small overbids decreases. That is, while bids with negative expectation become less common, those that remain become larger and thus worse. As a result, while the average bid does not change significantly, the average expected payoff decreases (that is, the expected loss increases) over time.

Figure 2.4 shows the rate of each bid type by round, again showing the rate of large overbids increasing while the rate of small overbids decreases. Further evidence of behavioural changes are seen in the regressions presented in Tables 2.6 and 2.7 of Section 2.3.

Figure 2.4: Proportion of Bids in Each Range of Net Expectation by Round



Finally, I follow Charness and Levin (2009) in adapting Eyster and Rabin (2005) cursed equilibrium model to this game. To this end we must interpret the decision over whether a transaction occurs as being made by a 'seller', who accepts the player's bid, b, if and only if it exceeds the card's value, v. Given the uniform distribution of v over

$[20, 119]$, the seller thus accepts bid b with probability

$$P(accept|b) = \begin{cases} 1 & \text{if } b = 120 \\ \frac{b-19}{100} & \text{if } b \in [20, 119] \\ 0 & \text{if } b < 20 \end{cases}$$

A subject who is $\chi-$cursed thus believes with probability χ that the seller accepts the bid with this probability *regardless of* v, and thus chooses b to maximize

$$E_\chi[\pi|b] = \begin{cases} \sum_{v=20}^{119} \left[\frac{3}{2}v - b\right] & \text{if } b = 120 \\ \chi \sum_{v=20}^{119} \left[\frac{3}{2}v - b\right] \cdot \frac{b-19}{100} + (1-\chi) \sum_{v=20}^{b} \left[\frac{3}{2}v - b\right] & \text{if } b \in [20, 119] \\ 0 & \text{if } b < 20 \end{cases}$$

For a fully cursed subject, that is, one for which $\chi = 1$, signifying that she always fails to understand the relationship between v and the seller's acceptance of the bid, the optimal is $b = 62$. This is notably much closer to the mean observed bid of 67.9 than the non-cursed optimal bid, $b = 40$, is. Clearly, however, subjects' failures go beyond that which can be predicted by cursedness, with 1,935 of 3,300 bids (58.6%) exceeding this level. Even if we follow Eyster and Rabin (2005) by allowing χ values outside the unit interval, a numerical analysis suggests the optimal resulting bid is bounded above by $b = 70$, which is lower than both 1,398 of 3,300 bids (42.4%) bids and the average bid for 100 of 220 subjects (45.5%). For almost half of all subjects, then, attempting to estimate a cursedness level is futile. Thus while the theoretical basis of cursed equilibrium - that subjects fail to fully account for the informational content of others' information - is intuitively appealing, and its predictions differ from the standard optimum in the right direction, it cannot account for the extent of subjects' failures.

2.2.2 Jury Voting Game

2.2.2.1 Design

The five jury voting games are variations on those of Guarnaschelli, McKelvey, and Palfrey (2000), which abstract the jury voting game of Feddersen and Pesendorfer (1998). In each round, groups of 5 players are each assigned either a red jar or a blue jar with equal probability. The red jar contains seven red balls and three blue balls, and the blue jar contains seven blue balls and three red balls. The group are not told which jar has been assigned to them. Each group member is then shown an image of the jar with the balls in random order and with all colours obscured, and clicks one of the ten balls to reveal its colour, providing a noisy signal about the colour of the jar which is correct with probability $p = 0.7$. Each subject then votes for either the red or blue jar. If at least r group members vote for the red jar then the group selects the red jar, otherwise the group selects the blue jar. At the end of each round, the player is told the colour of the jar, shown the colours of the ten balls, and is told the number of group members that voted for each jar. At the end of the session, each subject earns 25 cents for every round in which her group selected the correct jar. A player's strategy is denoted by $\sigma(b)$ and $\sigma(r)$ which give the probability that the player votes for the red jar with blue and red signals respectively. The literature focuses on symmetric, responsive equilibria, where the latter is defined as $\sigma(b) \neq \sigma(r)$.[3]

The five variants of the game are as follows:

2.2.2.2 Human Groups with Majority Voting (HGM)

The game proceeds as above with groups of size $n = 5$ and majority voting, $r = 3$. Groups are randomly rematched each round. The unique symmetric, responsive

[3] This simply rules out equilibria in which each player is trivially best responding by virtue of never being pivotal such as $\sigma(b) = \sigma(r) = 1$ under majority voting or $\sigma(b) = \sigma(r) = 0$ under any vote-rule.

equilibrium entails (fully) informative voting, defined as $\sigma(b) = 0$, $\sigma(r) = 1$.

2.2.2.3 Human Groups with Red Unanimity Voting (HGU)

The game proceeds as above with groups of size $n = 5$ and a vote rule of $r = 5$. That is, the group selects the red jar only if all group members vote for it, and selects the blue jar otherwise. I refer to this as *red unanimity* voting. Here the unique symmetric, responsive equilibrium is given by $\sigma(r) = 1$ and

$$\sigma(b) = \frac{pK_{n,r} - (1-p)}{p - (1-p)K_{n,r}} = 0.583 \tag{2.1}$$

where

$$K_{n,r} = \left(\frac{1-p}{p}\right)^{\frac{n-(r-1)}{r-1}}.$$

In particular, symmetric fully informative voting is no longer an equilibrium.[4] This is most easily seen by noting that when a player's vote is pivotal, all four other group members must have voted for the red jar. A player capable of pivotal calculus thus infers that all four must have received red signals, which together outweigh her own signal, inducing her to vote red regardless of its colour.

2.2.2.4 Informative Computer Groups with Red Unanimity Voting (ICG)

This game proceeds as HGU, with $n = r = 5$, except each group consists of one subject and four computer players, each of which votes fully informatively, i.e. $\sigma(b) = 0$, $\sigma(r) = 1$. Since these strategies are known to the subject, by the above pivotal reasoning she should always vote for the red jar regardless of her signal, i.e. $\sigma(b) = \sigma(r) = 1$.

[4]If we relax the symmetry requirement, any value of $\sigma(b) \in [0, 1]$, along with $\sigma(r) = 1$, can be a best response to reasonable beliefs about others.

2.2.2.5 Reverse Computer Groups with Red Unanimity Voting (RCG)

This game proceeds as ICG, with $n = r = 5$, except each computer player votes for the jar that *does not* match its selected signal, i.e. $\sigma(b) = 1$, $\sigma(r) = 0$. Thus while pivotality still implies four red votes, this now implies four blue signals, inducing a subject capable of pivotal reasoning to vote for the blue jar regardless of her signal, $\sigma(b) = \sigma(r) = 0$.

2.2.2.6 Option Computer Groups with Red Unanimity Voting (OCG)

This game proceeds as ICG, with $n = r = 5$ and computer players that vote informatively, except prior to each round subjects must choose one of two options. Under Option A, referred to here as the *Information* option, subjects receive a signal as above, by selecting a ball and observing its colour, but their vote is automatically cast to match the colour of their signal. That is, they vote informatively. Under Option B, referred to here as the *Choice* option, subjects do not receive a signal, but may choose which jar to vote for.

As computer players vote informatively, as in ICG, optimal behaviour remains to always vote for the red jar, which can be achieved under the *Choice* option. That is, this option allows subjects to vote optimally, but requires subjects to overcome the reasonable intuition that eschewing private information is costly.

I denote the probability with which a player chooses *Choice* and then votes red by σ_R and the probability with which she selects *Choice* and votes blue by σ_B. Thus the probability with which she selects *Information* is $1 - \sigma_R - \sigma_B$, and optimal behaviour is $\sigma_B = 0$, $\sigma_R = 1$.

2.2.2.7 Order

HGM, HGU, and ICG are played in four different orders, which vary along two dimensions, as summarized in Table 2.3. First, half of all subjects (Orders 1 and 2) play HGU before ICG while the remainder play the reverse. This order impacts behaviour in each of these two games, as discussed below and at more length in Chapter 1. Secondly, half of all subjects (Orders 1 and 3) play HGM first, followed by HGU and ICG in some order, while the remainder play HGM between these two games. This has no impact on behaviour in any of these games. In each order, these are then followed by RCG and OCG. Finally, within each of these orders, half play CL before the jury voting games and half play it after. Again, this appears to have no impact on behaviour in any game.

Table 2.3: Treatment Orders of Jury Voting Games.

	Jury Game Order	n
Order 1	HGM - HGU - ICG - RCG - OCG	60
Order 2	HGU - HGM - ICG - RCG - OCG	50
Order 3	HGM - ICG - HGU - RCG - OCG	50
Order 4	ICG - HGM - HGU - RCG - OCG	60

Note: within each, half play CL before the jury voting games, and half play it after.

2.2.2.8 Results

Table 2.4: Observed (and Equilibrium) Aggregate Behaviour, Jury Voting Games

Treatments	$\hat{\sigma}(b)$	$\hat{\sigma}(r)$
HGM	0.069 (0.000)	0.948 (1.000)
HGU	0.476 (0.583)	0.912 (1.000)
ICG	0.560 (1.000)	0.919 (1.000)
RCG	0.315 (0.000)	0.613 (0.000)
	$\hat{\sigma}_R$	$\hat{\sigma}_B$
OCG	0.653 (1.000)	0.054 (0.000)

Table 2.4 summarizes the aggregate behaviour for each game. In the first four

games, $\hat{\sigma}(b)$ and $\hat{\sigma}(r)$ show proportion of votes cast for the red jar given a blue or red signal respectively. In OCG, $\hat{\sigma}_R$ is the proportion of rounds in which a subject selects *Choice* and votes for the red jar, and $\hat{\sigma}_B$ the proportion of rounds in which a subject selects *Choice* and votes for the blue jar. Individual behaviour is shown in Figure 2.5, where each point shows one subject's observed behaviour over the 15 rounds played. Equilibrium and aggregate behaviour are shown by red diamonds and circles respectively. Table 2.5 categorizes subjects by this behaviour. Each of these tables and charts is recreated from Chapter 1.

Table 2.5: Individual Behaviour, Jury Voting Games

	$\hat{\sigma}(r)=1$			$\hat{\sigma}(r)<1$
	$\hat{\sigma}(b)=0$	$\hat{\sigma}(b)\in(0,1)$	$\hat{\sigma}(b)=1$	
HGM	167 (75.9%)	20 (9.1%)	2 (0.9%)	31 (14.1%)
HGU	49 (22.3%)	68 (30.9%)	54 (24.5%)	49 (22.3%)
ICG	47 (21.4%)	42 (19.1%)	86 (39.1%)	45 (20.5%)

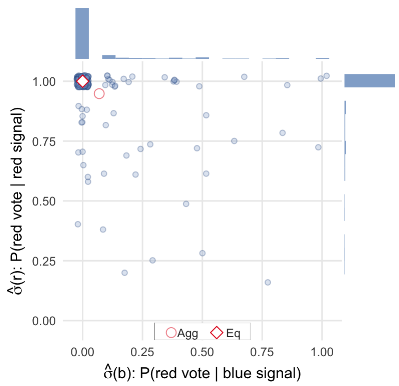
	$\hat{\sigma}_B=0$			$\hat{\sigma}_B>0$
	$\hat{\sigma}_R=0$	$\hat{\sigma}_R\in(0,1)$	$\hat{\sigma}_R=1$	
OCG	37 (16.8%)	27 (12.3%)	117 (53.2%)	39 (17.7%)

	$\hat{\sigma}(r)=1$		$\hat{\sigma}(r)=0$		Other
	$\hat{\sigma}(b)=1$	$\hat{\sigma}(b)=0$	$\hat{\sigma}(b)=1$	$\hat{\sigma}(b)=0$	
RCG	24 (10.9%)	26 (11.8%)	3 (1.4%)	30 (13.6%)	137 (62.3%)

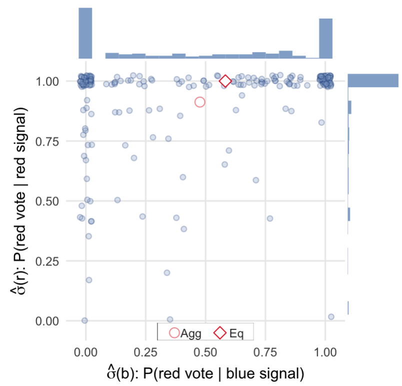
The first notable aspect of the results is that subjects overwhelmingly vote informatively in HGM. 75.9% never deviate from this, with 83.6% deviating at most once. This suggests that subjects generally understand the information structure to at least a basic level, whether or not they display evidence of pivotal thinking elsewhere. This is further supported by the large majorities playing $\hat{\sigma}(r)=1$ in each of the first three treatments - rational behaviour that is common to the sophisticated and the naive alike.

HGU and ICG provide both an opportunity for and evidence of strategic behaviour,

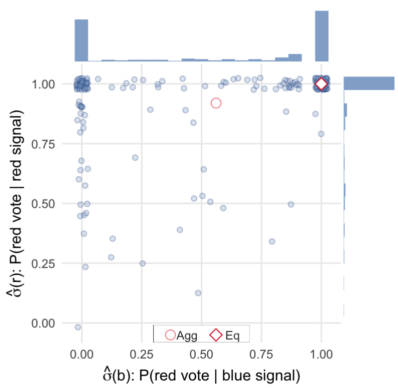
Figure 2.5: Individual Behaviour by Treatment.



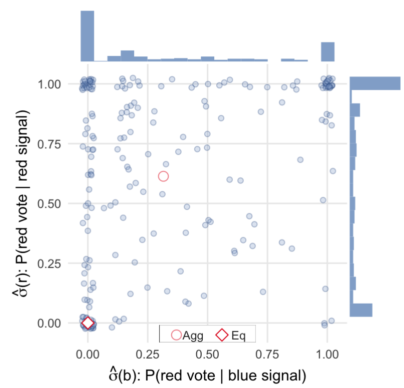
(a) HGM



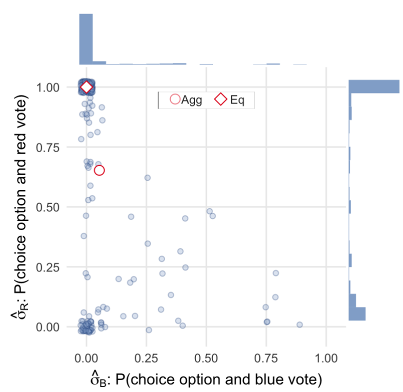
(b) HGU



(c) ICG



(d) RCG



(e) OCG

with the increase in $\hat{\sigma}(b)$ relative to HGM consistent with pivotal thinking. In particular, relative to HGM, many more individuals mix when receiving a blue signal,

$\hat{\sigma}(b) \in (0, 1)$, or to always vote red, $\hat{\sigma}(b) = 1$. The latter is particularly prevalent in ICG, in which it is the only rational strategy, whereas any $\hat{\sigma}(b)$ can be rationalized given reasonable beliefs in HGU. In the aggregate, however, the increase in $\hat{\sigma}(b)$ falls short of theoretical predictions in HGU, and even more so in ICG.

While not the focus of this paper, Chapter 1 considers further the relationship between these games, and finds evidence of substantial learning in each of HGU and ICG, with $\hat{\sigma}(b)$ increasing over time. Interestingly, learning in each of these games appears to apply to the other, with the increase only occurring throughout the first of the two that a subject plays, then maintaining its level (but not increasing further) through the second. Experience in HGM does not affect either, and vice-versa. This suggests that subjects are learning about something that the two red-unanimity games have in common, such as pivotal thinking, as opposed to something that only applies to one of the two games, such as changing beliefs about others' behaviour.

OCG behaviour is largely in line with that of ICG, in which the computer players and the resulting optimal behaviour - always voting red - are the same. Here, however, always voting red requires subjects to first select *Choice* and thus to forgo private information. 117 of 220 subjects (53.2%) always vote optimally, selecting *Choice* and voting for the red jar. 37 (16.8%) always select *Information* and thus vote informatively. Although 39 subjects select *Choice* and vote blue at least once - the behaviour with the lowest expected payoff - they do so relatively rarely, with subjects that select *Choice* then voting red 92.4% of the time. This suggests that subjects see little reason to select *Choice* other than that it facilitates red votes. Overall, there is little evidence that pivotally capable subjects struggle with the additional option presented here.

Behaviour in RCG, however, deviates substantially from previous jury voting games. 30 of 220 (13.6%) always vote blue, the optimal behaviour. This represents a substantial decrease from the 86 (39.0%) that optimally always vote red in ICG. 24 (10.9%)

always vote red while 26 (11.8%) always vote informatively, despite neither being optimal any longer. RCG behaviour is thus very poor. The overwhelming difference in subjects' capabilities between ICG and RCG is interesting given information extraction is mathematically identical in both. One possible explanation, discussed further in Chapter 1, is that the counterintuitive computer player behaviour in RCG confuses subjects, or induces them to abandon sophisticated reasoning.

Cursedness can also be applied to jury voting games with unanimity voting $r = 5$.[5] Specifically, since a cursed individual interprets others' behaviour as being less informative than it is, voting in equilibrium must be more informative - that is, $\sigma(b)$ decreases - in order for subjects to be willing to mix. Even symmetric fully informative voting may be a (heavily) cursed equilibrium. Eyster and Rabin (2005) note that aggregate support from Guarnaschelli, McKelvey, and Palfrey (2000) - the only jury voting data at the time of publication - is weak, in that observed $\hat{\sigma}(b)$ is below the non-cursed equilibrium level in only one of two treatments. More recent data is more promising, however, in that the result holds in both non-majority treatments of Goeree and Yariv (2011) and the HGU data presented here.

Estimating subjects' cursedness appears relatively fruitless, however. The nature of the game, in which subjects make binary decisions, means that these estimates are necessarily coarse. In ICG, for example, it is relatively simple to show that a player receiving a blue signal is indifferent between red and blue votes only if $\chi = 0.681$.[6] For a subject that votes informatively, all that we can say is that $\chi \geq 0.681$, while for a subject that always votes red, $\chi \leq 0.681$. The same applies to OCG and, with colours inverted, to RCG. The threshold for HGU, assuming players are drawn from a large population with the observed aggregate behaviour $\hat{\sigma}(r) = 0.913$, $\hat{\sigma}(b) = 0.476$, is $\chi = 0.171$.

[5] In HGM, fully informative voting is optimal regardless of beliefs about others, and thus cursedness has no bite.

[6] See Eyster and Rabin (2002) for the mathematical details of incorporating cursedness into the jury voting model.

That is, in each case, estimating cursedness simply attaches numerical labels to three observed behaviours - always voting informatively, always voting red, and mixing - but absent evidence that cursedness levels persists across strategic environments, it is not clear what value these new labels provide.[7]

More generally, then, while the core idea of cursedness - that subjects do not fully account for the informational content of others' behaviour - seems a reasonable description of the cognitive failing that leads to naivety in jury voting games, the contribution of the model - that it allows *partial* failings through the parameter χ - does not appear especially pertinent in this environment.

2.3 Comparative Results

2.3.1 General Behaviour

The primary focus of this paper is the relationship between individuals' bidding behaviour in the auction game, henceforth AG, and voting behaviour in the jury voting games. Tables 2.6 and 2.7 show linear regressions of the expected payoff (Columns (a) through (c)) and logistic regressions of the probability of an overbid (Columns (d) through (f)) in AG upon various measures of behaviour in HGM, HGU, and ICG. RCG and OCG are treated in Appendix B.4. In each case, informative voting is omitted and thus acts as the base case.

Table 2.6 measures behaviour in terms of subjects' observed $\hat{\sigma}(r)$ and $\hat{\sigma}(b)$. Equilibrium in each of the three games requires $\sigma(r) = 1$, and thus we expect the same of the observed $\hat{\sigma}(r)$. Deviations from this are necessarily irrational in HGM and ICG, and can be explained in HGU only by implausible beliefs, such as other subjects always

[7] Of course, testing the persistence of χ across the two environments presented here - CL and the jury voting games - is made impossible by the fact that most subjects' CL behaviour is not consistent with any χ.

Table 2.6: Regressions of AG Behaviour on Jury Voting Frequencies

Variable	Exp Payoff			Overbid		
	(a)	(b)	(c)	(d)	(e)	(f)
Jury	HGM	HGU	ICG	HGM	HGU	ICG
Constant	-3.696	-4.523	-5.716	1.000	1.097	1.088
	0.021	0.001	0.000	0.063	0.006	0.008
Round	-0.040	-0.040	-0.040	-0.019	-0.020	-0.019
	0.064	0.064	0.064	0.081	0.080	0.080
$\hat{\sigma}(b)$	-3.763	-0.384	0.242	0.064	0.145	-0.007
	0.081	0.503	0.638	0.915	0.554	0.976
$\hat{\sigma}(r)$	1.538	2.507	3.333	-0.620	-0.835	-0.715
	0.344	0.105	0.017	0.257	0.061	0.118
AG First	0.077	-0.084	0.102	0.272	0.304	0.257
	0.859	0.849	0.818	0.148	0.110	0.174
Obs	3,300	3,300	3,300	3,300	3,300	3,300

Notes: In all regressions standard errors are clustered by subject and p-values are presented below the estimates.

voting against their own private information. The regressions presented here find some evidence that such deviations correspond to worse performance in AG, with the signs of the coefficients as expected, although this is significant at a reasonable level only for expected payoffs in ICG (Column (c)).

$\hat{\sigma}(b)$, meanwhile, allows us to differentiate between the naive and the sophisticated, in that positive values are consistent with sophistication but not naivety in both HGU and ICG. This is particularly true in ICG, where $\sigma(b) = 1$ is the only rationalizable strategy. Indeed, the estimated coefficients have the expected signs but are not significant. In HGU, where any $\sigma(b)$ can be a best response to reasonable beliefs in HGU, the effect is also insignificant in both regressions. In HGM, on the other hand, positive values of $\hat{\sigma}(b)$ show *irrational* behaviour, as only informative voting is rational. Again, the signs on the estimated coefficients are as expected, with weak significance in the

Table 2.7: Regressions of AG Behaviour on Jury Voting Classifications

Variable	Exp Payoff			Overbid		
	(a)	(b)	(c)	(d)	(e)	(f)
Jury	HGM	HGU	ICG	HGM	HGU	ICG
Constant	-1.804	-1.989	-2.085	0.298	0.164	0.110
	0.000	0.000	0.000	0.070	0.516	0.667
Round	-0.040	-0.040	-0.040	-0.020	-0.020	-0.019
	0.006	0.064	0.064	0.081	0.080	0.081
Pure Red	-4.852	-0.805	-0.206	-0.142	0.266	0.224
$\hat{\sigma}(b) = \hat{\sigma}(r) = 1$	0.287	0.232	0.740	0.909	0.369	0.400
Mixed	2.994	0.262	-0.016	0.762	0.178	0.482
$\hat{\sigma}(b) \in (0,1), \hat{\sigma}(r) = 1$	-2.447	0.607	0.981	0.033	0.493	0.121
Red Errors	0.003	-1.439	-1.611	0.406	0.537	0.548
$\hat{\sigma}(r) < 1$	0.859	0.043	0.026	0.084	0.069	0.050
AG First	0.026	-0.052	0.093	0.270	0.3040	0.305
	0.951	0.906	0.831	0.152	0.112	0.100
Obs	3,300	3,300	3,300	3,300	3,300	3,300

Notes: In all regressions standard errors are clustered by subject and p-values are presented below the estimates.

first column. That is, more red votes given blue signals in HGM, where this behaviour is irrational, relates to lower expected payoffs in AG.

Table 2.7 repeats the above but measures behaviour via dummies for the categorizations of individuals presented in Table 2.5. In each, the baseline is informative voting. The results are qualitatively similar to the above, in that irrational deviations from informative voting appear to have some relationship with worse performance in AG, while rational deviations do not. In particular, the dummies for Pure Red and Mixed voting, which help distinguish between the naive and the sophisticated in HGU and ICG, are mostly insignificant. The one exception is in Column (d), where the coefficient on the Mixed dummy is positive and significant, suggesting higher overbid rates in HGM. Given the only rational strategy in this game is informative voting, the Mixed dummy shows irrational behaviour, and thus worse performance is consistent with the above. Likewise, the Red Errors variable, which dummies for those who vote blue with a red signal at least once in a given treatment, is significant, or close to significant, in five of six regressions, with the coefficients reflecting worse AG performance in each case.

Overall, then, the results suggest that irrational deviations from informative voting correspond to worse AG performance, while rational deviations - such as voting red with a blue signal in HGU or ICG, which we expect of those who are capable of hypothetical thinking - has no relationship with AG. This suggests that a basic level of comprehension (or perhaps effort or consistency) in the jury voting games relates to better outcomes in AG - an unsurprising result - but there is no evidence that pivotal thinking in the former has any effect.

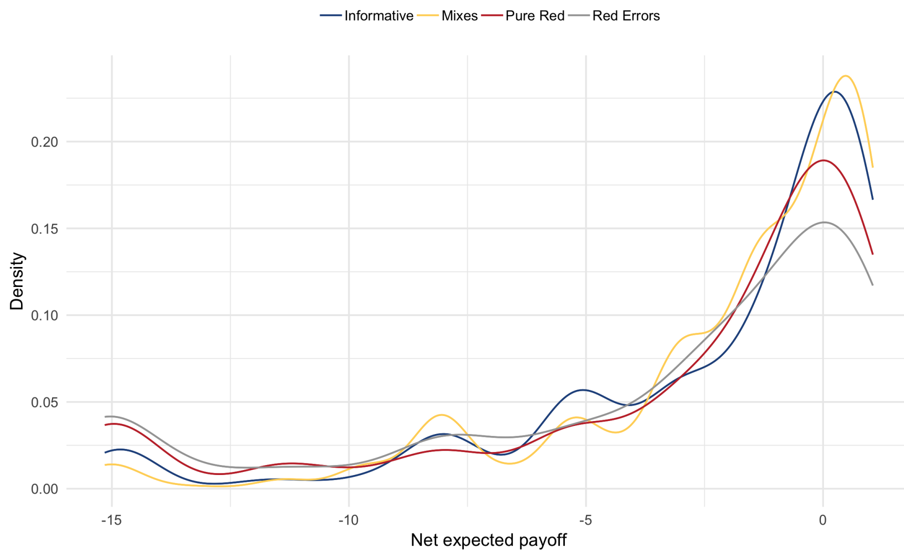
Finally, note that while informative voting is the baseline group in each regression, changing this does not result in any additional significant differences between groups. Figure 2.6 shows the distribution of bids, measured by expected (net) payoff, for each of the HGU and ICG categories from Table 2.5, where we see again that those who vote blue with a red signal, $\hat{\sigma}(b) < 1$, and thus display evidence of basic misunderstandings of the information structure of the game, are less likely to bid with positive expectation in AG, while the differences between other groups - through which we see evidence of pivotal thinking in the jury voting games - are less prevalent.

2.3.2 Learning and Insight

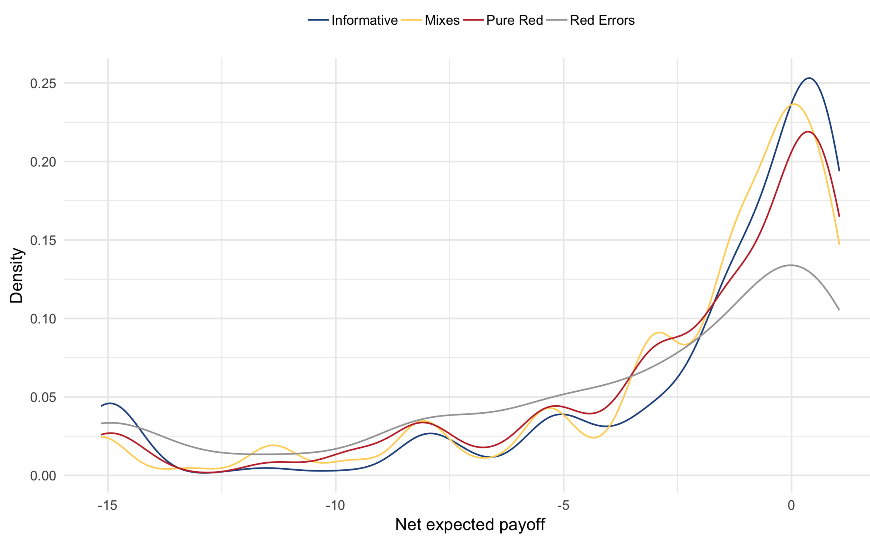
I consider next whether there is any connection between subjects' apparent propensity to learn in these various games. As discussed in Section 2.2, there is some evidence that behaviour changes throughout both AG and the jury voting games. The regressions in Tables 2.6 and 2.7 provide further support for this, with the coefficient on the round number (weakly) significant in each. In keeping with the previous discussion, the signs

Figure 2.6: Distributions of Net Expected Payoffs by Jury Voting Strategies

(a) By HGU Classification



(b) By ICG Classification



Notes: *Informative* subjects are those who play $\hat{\sigma}(r) = 1, \hat{\sigma}(b) = 0$, *mixers* play $\hat{\sigma}(r) = 1, \hat{\sigma}(b) \in (0, 1)$, *pure red* subjects play $\hat{\sigma}(r) = 1, \hat{\sigma}(b) = 1$, and *red errors* subjects play $\hat{\sigma}(r) < 1$.

on the coefficients suggest that while overbids become less common over time, the overall expected payoff also decreases. Again, this is because while subjects become less likely to make overbids, those that they do make become worse on average, with the rate of large overbids (bids of at least 90 points) increasing at the expense of smaller overbids.

I thus measure each subject's long-term behavioural changes in AG by measuring the increase in each of their mean expected payoffs and overbid rates from the first seven rounds (that is, the first half of the rounds) to the last seven rounds. By averaging behaviour over several rounds, I hope to account somewhat for the high short-term variability of bids discussed above.

To measure each subject's learning in the jury voting games I take a similar approach, taking the difference of the observed $\hat{\sigma}(b)$ between the first and last seven rounds of each of HGU and ICG. To reflect the fact that learning in each of these games appears to carry over to the other, as discussed in Chapter 1, I also compare the first seven rounds of the first of these two treatments that a subject plays to the last seven rounds of the second. Although such measures could be misleading in HGU, in which any $\hat{\sigma}(b)$ can be a best response to reasonable beliefs about others' behaviour, red votes with blue signals remain the best available indicator of sophisticated behaviour.[8]

Table 2.8 shows linear regressions of these changes in AG behaviour - mean expected payoff in Column (a) and overbid rate in Column (b) - on the above changes in jury voting behaviour. Within-treatment changes in $\hat{\sigma}(b)$ do not appear to impact learning in AG, but the change in $\hat{\sigma}(b)$ across the two treatments together does. In particular, more learning across HGU and ICG combined - as measured by a higher increase in $\hat{\sigma}(b)$ from the first seven rounds of the first of these treatments to the last seven rounds of the second - results in a greater decrease (or lower increase) in the rate of overbids from the first seven rounds of AG to the last seven.

It is also worth noting that the overbid rate increases more for subjects that play AG before the jury voting games. Said another way, the overbid rate increases less (or decreases more) amongst those who have already played the jury voting games. It is not

[8]Moreover, Chapter 1 presents evidence that in HGU informative voting, in which only $\sigma(b) = 0$ is rational, is generally a result of naivety rather than sophistication and appropriate beliefs.

clear, however, whether this effect is due to experience playing the jury voting games in particular, which would suggest a link between the games, or if it is simply due to playing at the end of an experimental session rather than the beginning. Equivalent regressions presented in Chapter 1 show that behaviour in the jury voting games is unaffected by whether or not subjects have first played AG.

Table 2.8: Regressions of AG learning on changes in jury voting behaviour.

Variable	ΔE[Payoff] (a)	Δ Overbid Rate (b)
Constant	-0.354	-0.082
	0.230	0.024
$\Delta\hat{\sigma}(b)$, HGU	-0.697	-0.022
	0.275	0.760
$\Delta\hat{\sigma}(b)$, ICG	-1.139	0.100
	0.214	0.326
$\Delta\hat{\sigma}(b)$, Overall	0.389	0.108
	0.397	0.053
AG First	0.066	0.095
	0.870	0.043
Obs	220	220

Notes: In all regressions standard errors are clustered by subject and p-values are presented below the estimates.

The question arises, then, as to whether any changes in jury voting behaviour carry over to AG, as they presumably would if they were driven by insights relevant to both games. In particular, if that which is learned in the jury voting games is applicable to AG then we would expect those who learn more in the former to then perform better in the latter when playing it afterwards. Table 2.9 thus restricts to those who play the jury voting games before AG and regresses the two measures of overall AG performance - that is, the levels rather than the changes over time - upon the previous measures of learning in the jury voting games. There is no evidence that learning in the jury voting games carries over, however, as none of the coefficients are significant.

Table 2.9: Regressions of AG behaviour on jury voting behaviour.

	(a) Exp Payoff	(b) Overbid
Constant	-2.810	0.262
	0.000	0.066
$\Delta\hat{\sigma}(b)$, HGU	-0.262	0.084
	0.850	0.880
$\Delta\hat{\sigma}(b)$, ICG	-0.427	0.471
	0.783	0.388
$\Delta\hat{\sigma}(b)$, Overall	0.897	-0.359
	0.222	0.303
Obs	1650	1650

Notes: In all regressions standard errors are clustered by subject and p-values are presented below the estimates.

2.4 Conclusions

It is often assumed that sophistication in jury voting games and resilience to the Winner's Curse are related, due to both relying on the ability to condition on, and extract information from, hypothetical events. The experiment presented here shows little evidence for this, however. While those who deviate from informative voting in jury voting games in irrational ways tend to do worse in AG - overbidding more and achieving lower expected payoffs - there is no difference for those who deviate in ways that suggest hypothetical thinking, whether measured directly by raw strategy values or by categorization of subjects.

There is some evidence that those whose behaviour changes more in the jury voting games also improve more in AG, but it is not clear whether this is due to developing insights into hypothetical thinking, or to simply due to a more ready response to gains and losses. Regardless, those who learn more in the jury voting games do not appear to then play better in AG, suggesting that this learning does not translate between games.

Thus while the games are clearly bound in theory by the potential for inference from hypothetical events, there remains no evidence of a relationship in practice.

Chapter 3

Stochastic Choice in Games: an Experiment

This chapter is co-authored with Evan Friedman.

3.1 Introduction

Game theory rests on Nash equilibrium (NE) as its central concept, but despite its appeal and influence, it fails to capture the richness of experimental data. Systematic deviations from NE predictions have been documented, even in some of the simplest games.

NE rests on two assumptions. First, players form accurate beliefs over the distribution of opponents' actions. Second, players best respond to these beliefs. Efforts to reconcile theory with data typically amount to weakenings of these strict assumptions.

One leading example is *quantal response equilibrium* (QRE) (McKelvey and Palfrey (1995)), which is very much like NE, but relaxes the assumption of best response. That is, each player forms correct beliefs over the distribution of opponents' actions, and though he tends to take better actions (by expected utility), he fails to do so with probability one. Simply put, QRE is an equilibrium model with "noise in actions".

However, recent work by Friedman (2018) introduces *noisy belief equilibrium* (NBE), an equilibrium model with "noise in beliefs" that maintains best response, and shows that it can explain several of the same phenomena as QRE. Specifically, when only considering actions data, the two models make similar predictions in many games commonly played in the lab.

The primitive of QRE is the quantal response function–the mapping from payoffs (given beliefs) to actions. The primitive of NBE is the noisy belief mapping—the mapping from opponents' actions to beliefs. In order for these theories to have empirical content, these primitives are restricted to satisfy several behavioral axioms which capture what is meant by noisy actions and noisy beliefs.

The axioms capture forms of bounded rationality. For QRE, better actions are played with higher probability, and an all-else-equal increase in the payoff to some action increases the probability it is played. For NBE, belief distributions are unbiased and shift around in the same direction as changes in the opponents' actions.

Since QRE and NBE incorporate noisy actions and noisy beliefs, respectively–the two fundamental sources of stochasticity in games–it is natural to distinguish these models in data. One approach would be to design games in which the models' predictions diverge and then collect standard actions data. However, since it is obvious that both sources of noise will be present in almost any experimental dataset, we take a different approach.

We run a laboratory experiment in which we augment standard actions data with elicited (and incentivized) beliefs; and by playing a series of games with systematically varied payoffs, we "trace out" the mappings from the opponent's actions to beliefs and from payoffs (given beliefs) to actions. These correspond to the primitives of NBE and QRE, respectively. With these empirical primitives, we test the axioms. We emphasize that the games we play do not allow for a strong separation of the theories in terms of *predictions*, but by collecting beliefs data, we test the *assumptions* underlying them. Understanding the extent to which these axioms fail will help to discipline modelling assumptions and, we argue, be of reduced-form general interest independent of the models.

Our experiment has two parts. In the first part, subjects play a set of fully mixed 2×2 games without feedback. In the second part, subjects state their beliefs and take actions for these games. The games are the same up to a single payoff parameter for player 1, and by varying this parameter, we generate the desired variation in beliefs and expected payoffs for *both* players. Player 2's payoff parameters remain fixed across games, and so there is an asymmetry in player roles which we explore.

We find that (i) belief distributions tend to shift in the same direction as changes in opponents' actions, (*ii*) beliefs are systematically biased–"conservative" for one player 1 and "extreme" for player 2, (*iii*) rates of best response vary systematically across games, and (*iv*) systematic failures to maximize expected payoffs (given beliefs) are well explained by risk aversion despite our efforts to mitigate it in the experiment. Digging deeper into the process of belief formation, we collect subject-level measures of strategic sophistication based on dominance solvable games. We find that (*v*) experience as player 1 *causes* higher levels of sophistication than player 2 despite playing exactly the same games, (*vi*) sophistication measured in dominance solvable games strongly predicts behavior in fully mixed games, and (*vii*) belief elicitation significantly effects actions in a direction consistent with increasing sophistication.

3.2 Theoretical Background

Though defined for any normal form game, we provide the definitions of QRE and NBE for 2×2 games with unique, mixed strategy Nash equilibria.[1] These games, sometimes referred to as "generalized matching pennies" or simply "matching pennies", are frequently played in experiments and form the basis of this experiment.

Matching pennies is defined by the payoff matrix in Figure 3.1.[2] The parameters a_L, a_R, b_U, and b_D give the base payoffs. The parameters c_L, c_R, d_U, and d_D are the payoff differences, which we assume are strictly positive to maintain the relevant features.[3] The row player's actions are U and D ("up" and "down"); the column player's actions are L and R ("left" and "right").

[1]Friedman (2018) defines NBE for normal form games. The fully mixed 2×2 case obscures some of its general properties. For example, NBE is a refinement of rationalizability (Bernheim (1984) and Pearce (1984)) in the sense that only rationalizable actions are played with positive probability in equilibrium.

[2]This notation is borrowed from Selten and Chmura (2008) with slight modification.

[3]Games in which the payoff differences are all strictly negative are equivalent up to the labelling of actions.

	L (q)	**R**
U (p)	$a_L + c_L$, b_U	a_R, $b_U + d_U$
D	a_L, $b_D + d_D$	$a_R + c_R$, b_D

U: up **D**: down
L: left **R**: right

Player 1's payoff in lower-left corner
Player 2's payoff in upper-right corner

$a_L, a_R, b_U, b_D \in \mathbb{R}$
$c_L, c_R, d_U, d_D > 0$

Figure 3.1: Matching Pennies

We use i and j as player indices. In particular, we always refer to player i as forming beliefs about the behavior of opponent j. Reserving k and l for action indices, we write, for example, a_{ik} as action k of player i. To minimize the use of subscripts, we use p and q for the probabilities of playing U and L, respectively, which we refer to simply as "actions". We use $r \in [0, 1]$ to refer to the action of player j, which should be understood as p or q depending on context.

The Nash equilibrium of any matching pennies games is given as $\{p_{NE}, q_{NE}\} = \{\frac{d_D}{d_U+d_D}, \frac{c_R}{c_L+c_R}\}$, which depends only on the payoff differences. The predictions of other concepts may depend on more features of the game (as well as exogenously specified primitives from outside the game).

3.2.1 Quantal Response Equilibrium

In a QRE, a player's behavior depends on the expected payoffs to each action. To this end, let $\bar{u}_i(r') = (\bar{u}_{i1}(r'), \bar{u}_{i2}(r')) \in \mathbb{R}^2$ be the vector of i's (subjective) expected utilities given belief $r' \in [0, 1]$ over the behavior of player j. We use $v_i = (v_{i1}, v_{i2}) \in \mathbb{R}^2$ as shorthand for an arbitrary vector of expected utilities. That is, v_i is understood to satisfy $v_i = \bar{u}_i(r^{'})$ for some $r^{'}$.

In a QRE, player i's *quantal response function* $Q_i = (Q_{i1}, Q_{i2}) : \mathbb{R}^2 \to [0,1]^2$ maps his vector of expected utilities to a distribution over actions. The quantal response function is the primitive and assumed to satisfy the following *regularity axioms* (Goeree et al. (2005)):

(A1) **Interiority**: $Q_{ik}(v_i) \in (0,1)$ for all $k \in 1,2$ and for all $v_i \in \mathbb{R}^2$.

(A2) **Continuity**: $Q_{ik}(v_i)$ is a continuous and differentiable function for all $v_i \in \mathbb{R}^2$.

(A3) **Responsiveness**: $\frac{\partial Q_{ik}(v_i)}{\partial v_{ik}} > 0$ for all $k \in 1,2$ and $v_i \in \mathbb{R}^{J(i)}$.

(A4) **Monotonicity**: $v_{ik} > v_{il} \implies Q_{ik}(v_i) > Q_{il}(v_i)$.

Axioms (A1)-(A2) are technical. Behavioral axioms (A3)-(A4) require that an all-else-equal increase in the payoff to some action increases the probability it is played and that higher payoff actions are played with higher probability.

To close the model, it is assumed that each player has correct beliefs over the distribution of the opponent's actions and that actions are consistent with the quantal response functions. Specializing notation in the obvious way by letting Q_U and Q_L be quantal responses and $\bar{u}_U$, $\bar{u}_D$, $\bar{u}_L$, and $\bar{u}_R$ be expected utilities to actions:

Definition 1. A QRE is any $(p,q) \in [0,1]^2$ such that $p = Q_U(\bar{u}_U(q), \bar{u}_D(q))$ and $q = Q_L(\bar{u}_L(p), \bar{u}_R(p))$.

3.2.2 Noisy Belief Equilibrium

In an NBE (Friedman (2018)), players' beliefs are drawn from distributions that depend on the opponents' equilibrium behavior. If QRE adds "noise to actions", NBE adds "noise to beliefs".[4]

[4] For similar approaches of injecting noise into equilibrium beliefs, see Friedman and Mezzetti (2005) from which NBE adopts the basic idea of belief mappings and Rubinstein and Osborne (2003) which assumes that players are frequentists whose beliefs are formed from observing random samples

Given player j's action $r \in [0,1]$, we assume that player i's belief over j's action is drawn from a distribution that depends on r. In other words, player i's belief over j's action is a random variable that we denote $r^*(r)$, which depends on r and is supported on $[0,1]$. We call this family of random variables *noisy beliefs*, and they are defined by a family of CDFs: for any potential belief $\bar{r} \in [0,1]$, $F^i(\bar{r}|r)$ is the probability of realizing a belief less than or equal to $\bar{r}$ given that player j is playing r. Noisy beliefs are assumed to satisfy the following axioms:

(B1) **Interior full support**: For any $r \in (0,1)$, $F^i(\bar{r}|r)$ is strictly increasing and continuous in $\bar{r} \in [0,1]$; $r^*(0) = 0$ and $r^*(1) = 1$ with probability 1.

(B2) **Continuity**: For any $\bar{r} \in (0,1)$, $F^i(\bar{r}|r)$ is continuous in $r \in [0,1]$.

(B3) **Responsiveness**: For all $r < r' \in [0,1]$, $F^i(\bar{r}|r') \leq F^i(\bar{r}|r)$ for $\bar{r} \in [0,1]$ and $F^i(\bar{r}|r') < F^i(\bar{r}|r)$ for $\bar{r} \in (0,1)$.

(B4) **Unbiasedness**: $F^i(r|r) = \frac{1}{2}$ for $r \in (0,1)$.

Axioms (B1)-(B2) are technical. Behavioral axioms (B3)-(B4) require that beliefs shift up in the sense of first-order stochastic dominance when the opponent's action frequency increases and that beliefs are unbiased *on median.*[5]

To close the model, it is assumed that players best respond to realized beliefs and that belief distributions depend on the opponent's expected action. To this end, we define reaction functions which give the probabilities with which U (for player 1) and L (for player 2) are best responses to realized beliefs:

$$\Psi_U(q) \equiv 1 - F^1(q_{NE}|q)$$
$$\Psi_L(p) \equiv F^2(p_{NE}|p).$$

of their opponents' equilibrium behavior.

[5] Friedman (2018) discusses microfoundations and explores alternate notions such as unbiasedness on *mean*, which is compatible with (B1)-(B4) and so could be imposed in addition.

These reactions depend on game's payoffs only through the Nash equilibrium, as this defines the cutoff beliefs that make players indifferent.

Definition 2. An NBE is any $(p, q) \in [0,1]^2$ such that $\Psi_U(q) = p$ and $\Psi_L(p) = q$.

3.3 Experimental Design

3.3.1 Overall Structure

The experiment consists of two treatments, summarized in table 3.1. The final design was determined after running two types of pilot sessions, which are not discussed here.[6] Our sessions were run in the Columbia Experimental Laboratory in the Social Sciences (CELSS). Subjects were mainly undergraduate students at Columbia and Barnard Colleges.

Treatment	Player 1-subjects	Player 2-subjects	Total
A-BA	54	56	110
A-A	11	11	22

Table 3.1: Overview of experiment

The main treatment is A-BA, which we describe here. The treatment A-A is similar, but does not involve belief elicitation; it is included to test whether belief elicitation itself has an effect on behavior, and we defer its discussion to Section 3.8. For aggregate tests using actions data, we pool together the first sections of A-A and A-BA since that data was collected under identical conditions.

The experiment involves 2×2 matrix games, and at the beginning of the experiment, subjects are divided into two equal-sized subpopulations of row and column players, which we refer to as players 1 and 2, respectively. The A-BA treatment consists of two

[6] We feared the first type was too long, and the second type did not collect a type of data that we wished to analyze. To the extent these sessions overlap with our final design, the results are very similar.

sections, "action" (A) and "belief-action" (BA). In each of the 20 rounds of Section A, players are anonymously and randomly paired and take actions simultaneously. In each of the 40 rounds of Section BA, subjects are presented with a payoff matrix that appeared in A. Then, before taking an action, they state a belief about their opponent's behavior. The BA-belief is over actions taken by subjects in A and the BA-action is paired against an action taken in A. In this way, BA-subjects form beliefs about and play against A-subjects. Subjects in BA are not paired since they are playing against subjects in A, and so are allowed to play at their own pace, though in both sections subjects must wait for 10 seconds before submitting their answers. Screenshots of the experimental interface are given in Appendix C.4.

Before the start of Section A, instructions (see Appendix C.1) were read aloud accompanied by slides (see Appendix C.2). These instructions describe the strategic interaction and teach subjects how to understand 2×2 payoff matrices. Subjects then answered 4 questions to demonstrate their understanding of how to map players' actions in a game to payoff outcomes. All subjects were required to answer these correctly. Subjects then played 4 unpaid practice rounds before proceeding to Section A. After Section A, additional instructions for Section BA were given. Only at that point were subjects introduced to the notion of a belief and the elicitation mechanism described. Subjects then played 3 unpaid practice rounds before proceeding to Section BA.

At no point during the experiment (including the unpaid practice rounds) were subjects provided any feedback. In particular, no feedback was provided about other subjects' actions, the outcomes of games, or the accuracy of belief statements. Only at the end of the experiment did subjects learn about the outcomes of the games and belief elicitations that were selected for payment. This simplifies the analysis because subjects cannot condition on the history of play. It is also conceptually important as we are interested in observing stochasticity in beliefs, not changes in beliefs that are due to new information.

Each game is played mutliple times. This is necessary because we wish to analyze patterns in individual subjects' belief data. However, we take several measures to approximate a situation in which each game is seen as if for the first time. First, there is no feedback as described. Second, there is a large "cross section", i.e. more distinct games than the number of times each game is played. Third, the games appear in a random order subject to the same game not appearing more than once within 3 consecutive rounds.

Subjects were paid according to one randomly selected round (based on actions) from A, and four randomly selected rounds from BA–two rounds based on actions and two rounds based on beliefs.[7] Since there are twice as many rounds in BA as in A, this equates the incentives for taking actions across sections. Each unit of payoff corresponded to a probability point of earning \$10 (e.g. 20 is a lottery that pays \$10 with probability 20% and \$0 otherwise). This is to mitigate the effects of risk aversion as expected utility is linear in probability points.[8]

3.3.2 The Games

Central to our design are the games whose payoffs are in Table 3.2, indexed by different values of $X > 0$. As shorthand, we refer to the game $X = 80$ as "$X80$" and similarly for different values of X.

These games have several important features. First, they are fully mixed, so we would not expect there to be much no-feedback learning (e.g. Weber (2003)). Second, they are of low dimension in the sense that each player's beliefs are one-dimensional.

[7] To allay any hedging concerns, all five payments were based on different matrices and this was emphasized to subjects.

[8] Evidence suggests that this only partially linearizes payoffs in the sense that people still behave as if they have a utility function over probability points with some curvature. See for example, Harrison et al. (2013).

X	**L** (q)	**R**
U (p)	X, 0	0, 20
D	0, 20	20, 0

Table 3.2: Game X

This is of obvious practical importance for eliciting beliefs, and means that it is feasible to "tile" the space of possible beliefs (the unit interval) with relatively few games (i.e. we avoid the curse of dimensionality). Third, they are sparse in the sense that the base payoffs are set to 0. Such a restriction has no impact on equilibria but makes the game's structure more transparent and makes it easier to calculate best responses and perceive differences across games. Fourth, by varying X, the theories under scrutiny predict systematic variation in actions and beliefs, as we now show.

By varying X, we vary the ratio of payoff differences, and hence the equilibria. Using p and q to denote predicted probabilities of U (for player 1) and L (for player 2), respectively, the NE predictions are $q_{NE} = \frac{20}{X+20}$ and $p_{NE} = 0.5$ (constant for all X). As is well-known, NE predicts each player must mix to make the other player indifferent, and this is why $p_{NE} = 0.5$ for all X.

For any fixed $X > 0$, both QRE and NBE give the same set predictions (i.e. that can be achieved for some primitive satisfying the axioms); and these feature systematic deviations from the NE, with the NE prediction at an extreme point of the set. Furthermore, whereas NE predicts that p is unaffected by changes in X, both QRE and NBE make the prediction that p increases in X. We are interested in these predictions for different values of X, but since q_{NE} is a strictly decreasing function of X, we can parameterize predictions by q_{NE} directly as in Figure 3.2 and summarized Lemma 1.

Lemma 1. *In game X:*

(i) $p_{NBE}, p_{QRE} \in (\frac{1}{2}, 1)$ for $q_{NE} < \frac{1}{2}$; $p_{NBE}, p_{QRE} \in (0, \frac{1}{2})$ for $q_{NE} > \frac{1}{2}$.

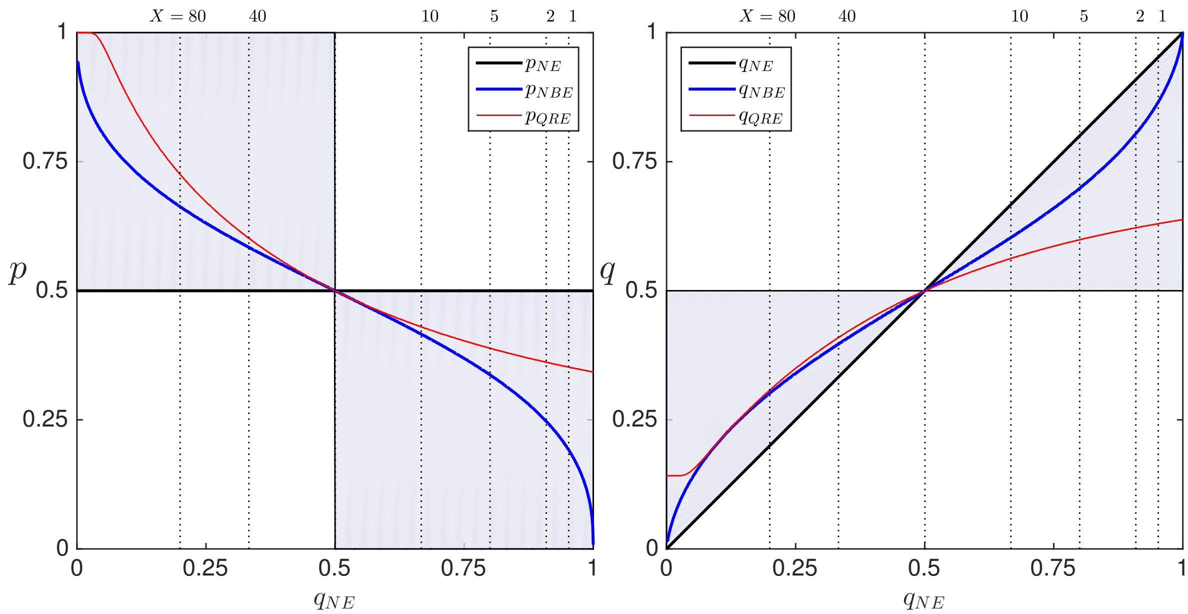


Figure 3.2: Equilibrium Predictions for Game X as a function of q_{NE}
The left panel plots values of p predicted by NE and QRE/NBE. The right panel plots values of q predicted by NE and QRE/NBE. All QRE/NBE must fall within the gray regions, and QRE/NBE predict that p decreases in q_{NE} and q increases in q_{NE}. The red lines correspond to logit QRE for some fixed λ, and the blue lines correspond to a particular parametrization of NBE.

(ii) $q_{NBE}, q_{QRE} \in (q_{NE}, \frac{1}{2})$ *for* $q_{NE} < \frac{1}{2}$; $q_{NBE}, q_{QRE} \in (\frac{1}{2}, q_{NE})$ *for* $q_{NE} > \frac{1}{2}$.

(iii) p_{NBE}, p_{QRE} *are strictly decreasing in* $q_{NE} \in (0, 1)$.

(iv) q_{NBE}, q_{QRE} *are strictly increasing in* $q_{NE} \in (0, 1)$.

Proof. See Appendix C.3. □

Parts (i)-(ii) of the lemma say that for any one game, the QRE and NBE predictions systematically deviate from the NE prediction. Visually, the QRE/NBE predictions must fall in the gray regions of Figure 3.2. Parts (iii)-(iv) of the lemma give comparative static predictions as model primitives (quantal response function or noisy belief mapping) are held fixed as the game varies. This suggests that, by varying X, we may observe the desired variation in beliefs and payoffs.[9]

[9] Such a comparative static represents an example of the "own payoff effect" (see, for example, Ochs (1995) and Goeree et al. (2003)).

The example NBE in Figure 3.2 (blue line) is shown as symmetric about $q_{NE} = \frac{1}{2}$. This holds whenever the noisy beliefs satisfy a condition called *label invariance*.[10] On the other hand, the QRE will not be symmetric if the quantal response function is *translation invariant*[11] and *label invariant*[12], properties that holds for the common logit QRE (red line) and more generally for any structural QRE.[13]

Lemma 2. *In game X:*

(i) If NBE is label invariant, q_{NBE} are p_{NBE} are symmetric about $q_{NE} = \frac{1}{2}$.

(ii) If QRE is translation invariant and label invariant, q_{QRE} and p_{QRE} are not *symmetric about $q_{NE} = \frac{1}{2}$.*

Proof. See Appendix C.3. □

For the experiment, we choose the six values of X given in Table 3.3. We choose values so that, by Lemma 2, label invariant NBE predicts symmetry about $X = 20$ ($q_{NE} = 0.5$). This allows for a basic benchmark prediction to which we can compare deviations.[14] We represent the selection of X-games with vertical lines in Figure 3.2 (labelled along the top axis).

In addition to the X-games, we also play the games given in Table 3.4. $X80s$ ("s" for "scale") is the same as $X80$, except with all payoffs divided by 10. This is included

[10] r^* is label invariant if the distribution of $r^*(r)$ is the same as the distribution of $1 - r^*(1 - r)$. Equivalently, r^* is label invariant if $F^i(\bar{r}|r) = 1 - F^i(1 - \bar{r}|1 - r)$ for all $\bar{r}, r \in [0, 1]$.

[11] Q_i is translation invariant if $Q_i(v_i) = Q_i(v_i + \gamma e_{J(i)})$ for all $\gamma \in \mathbb{R}$ where $e_{J(i)} = (1, ..., 1)$ is the vector of ones.

[12] Q_i is label invariant if, for all j and k, $Q_{ij}(v_i) = Q_{ik}(v_i^{'})$ if $v_{ij} = v_{ik}^{'}$ and if, for all l, $v_{il} = v_{il'}^{'}$ for some $l^{'}$.

[13] A structural quantal response function is derived by applying additive errors to the expected utilities. Goeree et al. (2005) show that structural quantal response functions are translation invariant.

[14] Based on our pilot in which we played $X20$, we take as given that behavior would be essentially uniform and that nearly all belief statements would indicate uniform play, so we omit $X20$ in our final design.

X	80	40	10	5	2	1
q_{NE}	0.2	0.333	0.667	0.8	0.909	0.952
p_{NE}	0.5	0.5	0.5	0.5	0.5	0.5

Table 3.3: Selection of X-games

as both QRE and NBE give very precise predictions on the effects of scaling payoffs (Friedman (2018)). $T1$ and $T2$ are "target" games, both of which are similar to $X5$, except the symmetry of player 2's payoffs have been broken. These are included to test the robustness of the estimated noisy beliefs: can we predict behavior in these games using beliefs elicited in the X-games? $D1$ is dominance solvable: player 1 has a dominant action to which player 2 has a unique best response; player 2's other action is a best response to uniform play. $D2$ is the same up to changing player roles and relabelling actions, i.e. it is player 2 with the dominant action. These games are included as measures of attention (i.e. do you believe your opponent will take *his* strictly dominant action?) and strategic sophistication (i.e. do you believe your opponent will best respond to *your* strictly dominant action?)

Table 3.5 summarizes the games played in both sections and the number of rounds for each.

3.3.3 Eliciting Beliefs Using Random Binary Choice

We used the random binary choice (RBC) mechanism (see, for example, Karni (2009)) to incentivize subjects to state their beliefs accurately. In an RBC, subjects are asked which they prefer from a list of 101 binary choices, as in Table 3.6 with option A on the left and option B on the right. If a subject holds belief $b\%$ over the probability an event E occurs and his preferences respect stochastic dominance (in particular, they do not have to be risk-neutral), it is optimal to choose option A for questions numbered *less*

X80s	**L**	**R**
U	0 8	2 0
D	2 0	0 2

T1	**L**	**R**
U	0 5	20 0
D	10 0	0 20

T2	**L**	**R**
U	0 5	20 0
D	40 0	0 20

D1	$\mathbf{L}_2$	**R**
U	0 6	20 0
$\mathbf{D}_1$	20 8	4 20

D2	**L**	$\mathbf{R}_1$
U	0 20	20 4
$\mathbf{D}_2$	6 0	8 20

Table 3.4: Additional Games

Section	Games	Rounds of each	Rounds
	$X1$, $X2$, $X5$, $X10$, $X40$, $X80$; $X80s$	2	
A	$T1$, $T2$	2	20
	$D1$, $D2$	1	
	$X1$, $X2$, $X5$, $X10$, $X40$, $X80$; $X80s$	5	
BA	Di	3	40
	Dj	2	

Table 3.5: Games by Section

than b and option B for questions numbered *greater than* b.[15] Otherwise, the subject is failing to choose the option that he believes gives the highest probability of receiving the prize.

In Section BA of the experiment, the event E is that a randomly selected player chose a particular action. Specifically, subjects were shown a matrix that appeared in

[15] Another popular method for incentivizing beliefs is the quadratic scoring rule (see, for example, Nyarko and Schotter (2002)), which has advantages but requires risk neutrality for incentive compatibility. Schotter and Trevino (2014) reviews these and other elicitation mechanisms.

	Would you rather have:		
	Option A:		Option B:
Q.0	\$5 if the event E occurs	or	\$5 with probability 0%
Q.1	\$5 if the event E occurs	or	\$5 with probability 1%
Q.2	\$5 if the event E occurs	or	\$5 with probability 2%
	⋮		⋮
Q.99	\$5 if the event E occurs	or	\$5 with probability 99%
Q.100	\$5 if the event E occurs	or	\$5 with probability 100%

Table 3.6: Random Binary Choice

Section A and told that "The computer has randomly selected a round of Section 1 in which the matrix below was played." Player 1 (blue) subjects were then asked "What do you believe is the probability that a randomly selected red player chose L in that round?", and similarly for player 2 (red) subjects. By entering their belief, a whole number between 0 and 100 inclusive, the rows of the table were filled out optimally given the stated belief (indifference broken in favor of option B).

If a round is selected for a belief payment, one of the 101 rows is randomly chosen and subjects receive their chosen option. If the subject chose option A in the selected row, a subject of the relevant type is selected and he receives \$5 if he chose the relevant option. If he chose option B in the selected row, he receives \$5 with the probability given. Since each row is chosen for payment with positive probability, subjects are incentivized to state their beliefs accurately. In addition, subjects are told explicitly that it is in their best interest to state their beliefs accurately.

3.4 Overview of the Data

We begin with a holistic view of the data. In Figure 3.3, we reproduce Figure 3.2 superimposed with individual belief data and various aggregate measures. The first panel plots player 1's action data as well as player 2's beliefs about player 1. Specifically, we plot (1) p_A: player 1's action frequency from Section A; (2) p_{BA}: player 1's action frequency from Section BA; (3) p_{br}: the action frequency that player 1 *would have* if he were to have *b*est *r*esponded to all belief statements in BA; and (4) $med(p^*)$: the median of player 2's beliefs about player 1. The second panel is the analogue for player 2.

For player 1, we find that the empirical action frequencies p_A and p_{BA} are inconsistent with theory in several games and do not match the comparative static predictions in X, which are non-monotonic. For player 2, we find that the empirical action frequencies q_A and q_{BA} are consistent with theory in most but not all games, and the comparative static in X holds. Interestingly, the median beliefs are consistent with NBE: if the action frequencies matched the median beliefs, they would be consistent with NBE.

The aggregate data hides considerable subject-level heterogeneity. In Figures C.8 and C.9 of Appendix C.5.1, we give some representative individual subject plots. Overwhelmingly, an individual subject's beliefs for a given game are distributed around a central tendency that varies sytematically across games, though the dependence of beliefs on game varies considerably across subjects. That the individual subject belief distributions vary so systematically across games suggests that the beliefs data are meaningful.

Finally, by comparing p_A to p_{BA} and q_A to q_{BA}, there are systematic differences in action frequencies across the two sections of the experiment, with the "direction" of the difference being uniform across all games for both players. In Section 3.8, we argue that these differences are *caused by* the belief elicitation itself.

3.5 Testing the Axioms

We test the axioms of model primitives–the quantal response function in the case of QRE and noisy beliefs in the case of NBE. In testing QRE's axioms, we take belief

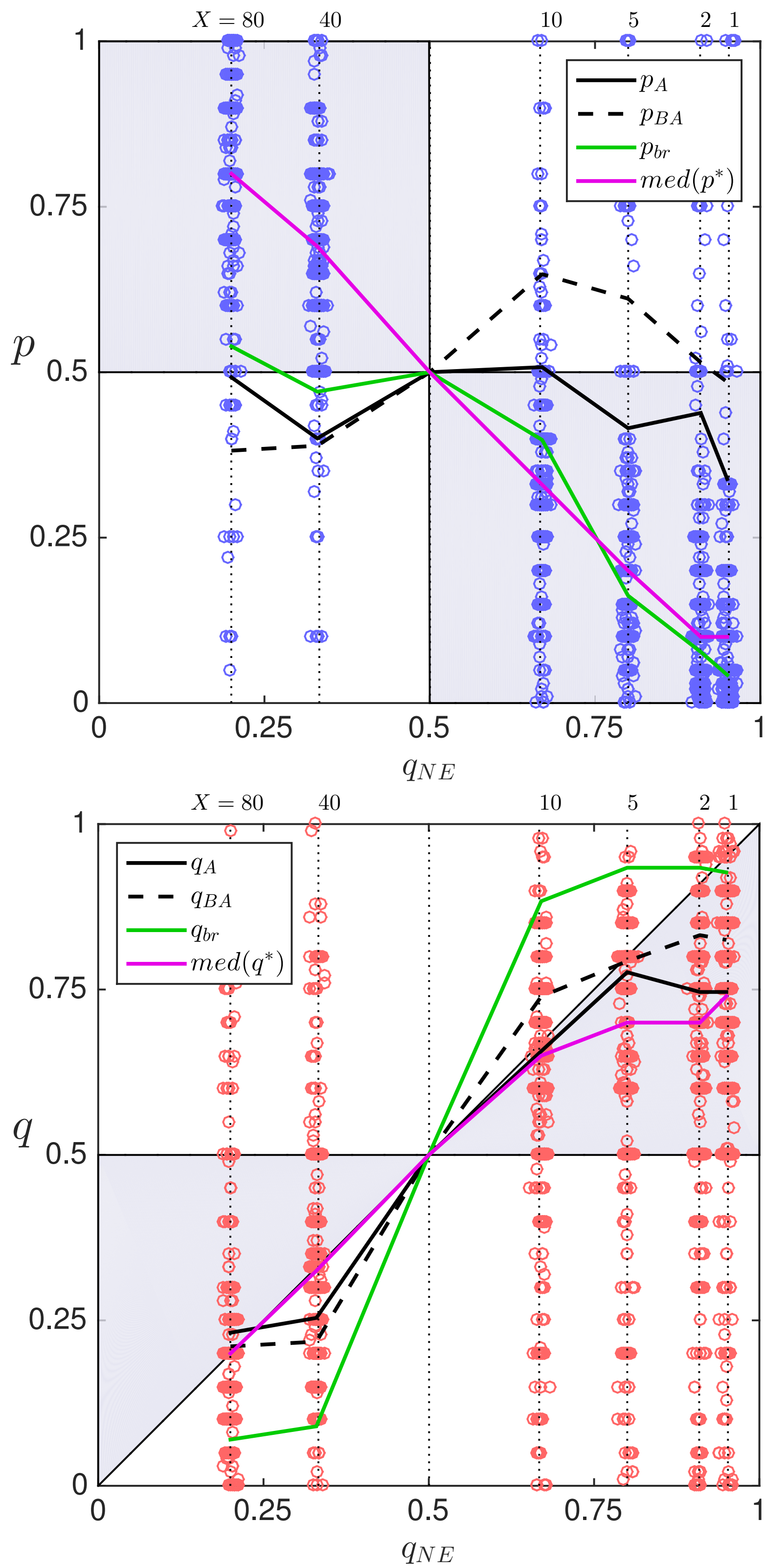


Figure 3.3: Equilibrium predictions for game X as a function of q_{NE}

statements as given and must associate individual beliefs with actions, and hence we use data from Section BA. In testing NBE's axioms, we must compare the belief data from Section BA with the actions from Section A (recall that beliefs were elicited about subjects' behavior in A).

3.5.1 QRE

A weak implication of *monotonicity* in binary action games is that best responses will be taken with probability greater than one-half. Tables 3.7 and 3.8 show that best responses are taken with probability greater than one-half in all games, though in not all cases is this significant.

Unsurprisingly, player 2's rates of best response are uniformly higher than those of player 1. By construction, for any given belief other than $p^* = \frac{1}{2}$, one of player 2's actions stochastically dominates the other, and hence should be taken by all subjects who do not tremble, independent of risk attitude. Perhaps the lower rates of best response for player 1 are due to the relative complexity of "calculating" the utility to each action. Another possibility is misspecification in assuming risk neutrality, and perhaps by allowing curvature the two players would have similar rates.

Two interesting facts emerge from the best response rates. First, player 1's best response rates are uniformly higher for games $X > 20$ than for $X < 20$. Second, player 2's best response rates are essentially constant across games. This is despite the fact that the belief distributions vary systematically across games.

To test *monotonicity* fully requires that we estimate the probability of best response conditional on all realized beliefs. To this end, we estimate for each game the action frequencies predicted by beliefs, which we plot in Figure 3.4. It is important that the relationship is kept flexible, so we fit linear splines (see figure caption for details).

Monotonicity is satisfied if and only if the action with the highest payoff is played

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1	(7) all
best response rate	0.741***	0.737***	0.667***	0.600**	0.544	0.544	0.639***
	(0.000)	(0.000)	(0.000)	(0.026)	(0.356)	(0.414)	(0.000)
Observations	270	270	270	270	270	270	1620

p-values in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.7: Player 1's Rates of Best Response

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1	(7) all
best response rate	0.836***	0.857***	0.854***	0.836***	0.854***	0.857***	0.849***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	280	280	280	280	280	280	1680

p-values in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.8: Player 2's rates of best response

with probability greater than one-half. Accordingly, in each panel, we plot the "indifferent belief" as a vertical line. *Monotonicity* is satisfied for player 1 (player 2) if and only if the action frequency is weakly less than (greater than) one-half for beliefs lower than the indifferent belief and weakly greater than (less than) one-half for beliefs higher than the indifferent belief.

We find that *monotonicity* cannot be rejected in all games for player 2, but *is* rejected for all games of player 1. In particular, monotonicity is violated over a range of roughly one-fifth of possible beliefs. The nature of violations is systematic. For $X > 20$, the violations are for beliefs just "right of" indifference, and for $X < 20$, the violations are just "left of" indifference. Such a pattern is consistent qualitatively with risk aversion or some desire to minimize the probability of receiving the low payoff of zero or equivalently maximize the probability of "winning".

Responsiveness is satisfied if and only if the predicted action frequences are in-

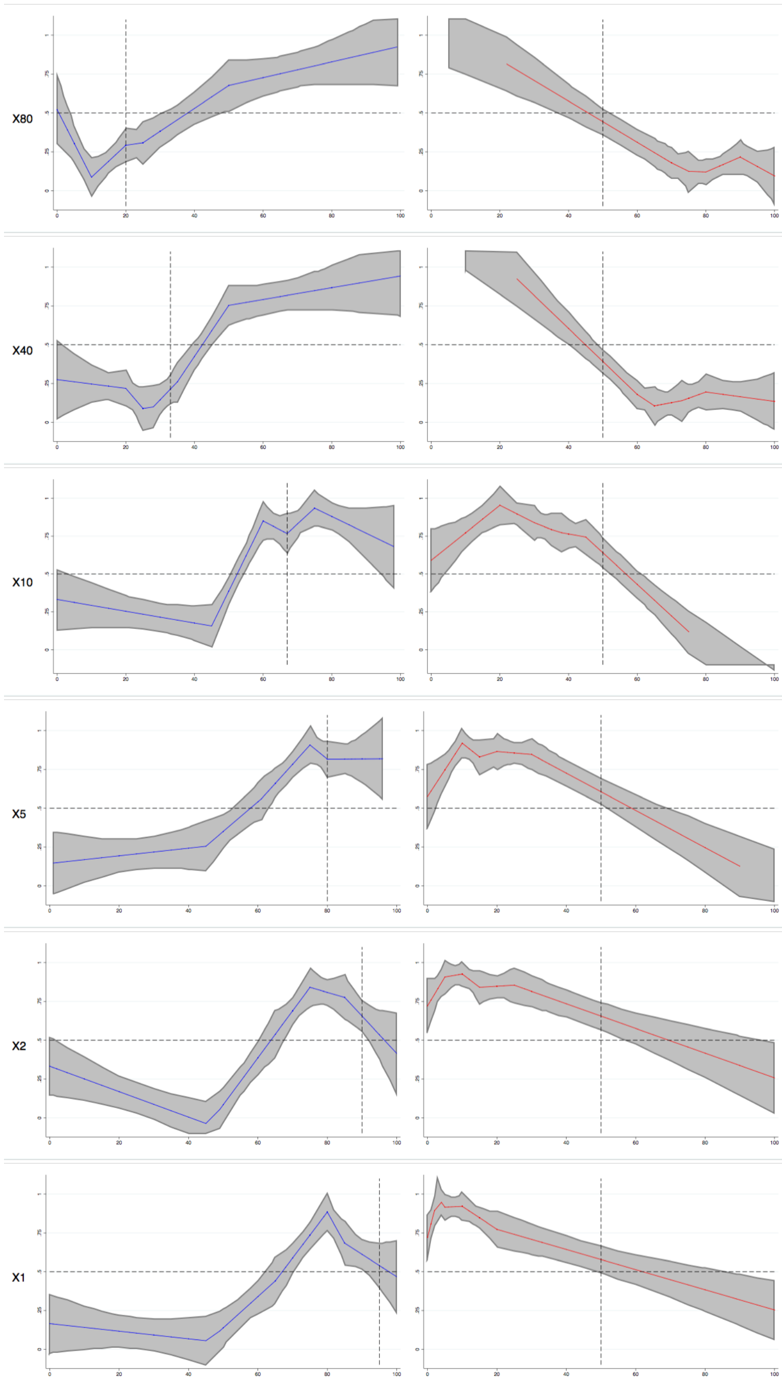


Figure 3.4: Action frequencies predicted by beliefs
Using a linear spline with 4 knots (determined by belief quintiles), we plot the action frequncies predicted by beliefs. Standard errors are clustered by subject. The left panels are for player 1 and the right panels are for player 2. The vertical dashed line gives the "indifferent belief" and the horizontal line is set to one-half.

creasing in beliefs for player 1 and decreasing in beliefs for player 2. We say that *repsonsiveness* is rejected if the slope of one of the line segments in the estimated spline is significantly negative for player 1 and significantly positive for player 2. We cannot reject *responsiveness* in $X40$ and $X5$ for player 1 and $X40$ for player 2, but we do reject it in all other cases. That *responsiveness* is rejected in the majority of games is both surprising and difficult to explain.

3.5.2 NBE

The NBE axiom *unbiasedness* requires that beliefs are correct on median. Tables 3.9 and 3.10 report the bias of beliefs for each game that are formed about player 1 and player 2, respectively. To determine significance, we bootstrap confidence intervals for the difference between median belief and (mean) action frequency and estimate the two-sided $(1-x)$%-confidence interval[16]. We report the smallest x such that this confidence interval excludes 0, which is conceptually similar to a p-value of the hypothesis of unbiasedness. In Appendix C.5, we report similar tables based on the mean of beliefs.

We find that player 1's beliefs about player 2 are remarkably accurate in that we fail to reject unbiasedness in four of six games individually. What is more, significant or not, the direction of bias is not systematic. When using the mean belief instead of median (Appendix C.5), we again find that we cannot reject unbiasedness in most games individually. However, based on the mean, the direction of bias is "conservative" in the sense that mean beliefs are more uniform that the actual distribution of actions. Such bias has been documented by Huck and Weizsacker (2002) in other settings, and is relatively common in experiments in which beliefs are elicited.

[16] To preserve the within-subject correlation structure, each bootstrap sample is generated as follows. For beliefs, we re-sample (with replacement) A-BA subjects, and conditional on drawing a subject, we re-sample from *his* belief data (with replacement). Independently, for actions, we re-sample (with replacement) the pooled A-BA and A-A subjects, and conditional on drawing a subject, we re-sample *his* action data from the first section.

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1
beliefs - actions	30.000***	28.259***	-18.852***	-21.667***	-34.444***	-26.111***
	(0.000)	(0.000)	(0.002)	(0.001)	(0.000)	(0.000)
Observations	410	410	410	410	410	410

Bootstrapped "p-values" in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.9: Player 2's beliefs versus player 1's actions
We report the difference between the median belief and the action frequency for each game. We bootstrap this difference and estimate the two-sided $(1-x)$%-confidence interval. We report the smallest x such that this confidence interval excludes 0, which is similar to a p-value.

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1
beliefs - actions	-3.134	7.127*	-0.672	-7.612	-4.627	-0.627
	(0.232)	(0.065)	(0.433)	(0.156)	(0.164)	(0.457)
Observations	404	404	404	404	404	404

Boostrapped "p-values" in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.10: Player 1's beliefs versus player 2's actions
We report the difference between the median belief and the action frequency for each game. We bootstrap this difference and estimate the two-sided $(1-x)$%-confidence interval. We report the smallest x such that this confidence interval excludes 0, which is similar to a p-value.

More interestingly, we find that player 2's beliefs about player 1 are very "extreme" and we reject *unbiasedness* for all games (and similarly for mean-unbiasedness). Whereas player 1's actions are relatively close to uniform for all values of X, we see that player 2's beliefs are too high when $X > 20$ and too low when $X < 20$.

We plot the empirical distributions of beliefs by player and game in Figure 3.5. We see that, as X varies, the distributions appear to shift monotonically in X in the sense of FOSD, and this is confirmed by Kolmogorov-Smirnov tests in Table 3.11 (only for player 2's beliefs across $X2$ and $X1$ is the difference not significant at conventional levels). Since the empirical distribution of actions does *not* change monotonically in X in the same direction as the beliefs, this implies that the beliefs distributions fail

Pair of Games	p^*	q^*
$X80 - X40$	0.332 (0.000)	0.253 (0.000)
$X40 - X10$	0.764 (0.000)	0.526 (0.000)
$X10 - X5$	0.404 (0.000)	0.179 (0.001)
$X5 - X2$	0.225 (0.000)	0.104 (0.110)
$X2 - X1$	0.229 (0.000)	0.078 (0.388)

Table 3.11: Relationship between belief distributions.
We report Kolmogorov-Smirnov statistics of the hypothesis that distributions of beliefs across pairs of games are *not* ordered by FOSD. p-values are reported in parenthesis.

responsiveness. Interestingly, however, the belief distributions do move with X as predicted by the theory even though the actions do not.

3.6 Scale Invariance

How does equilibium behavior vary with changes in payoff magnitude or scale? Friedman (2018) shows that NBE is invariant to changes in scale, whereas translation invariant regular QRE (a large axiomatic class that includes structural QRE with i.i.d. errors such as logit) is sensitive to scale.

To answer this question, we compare behavior in $X80$ to that in $X80s$. The latter is the same as the former, except with all payoffs divided by 10. A t-test shows that we cannot reject that both games have the same average frequency of actions for both players, as reported in Table 3.12. This is consistent with the study by McKelvey et al. (2000) on payoff magnitude, though unlike their design, ours does not involve feedback and features a larger scaling factor (10 as opposed to 4).

What about beliefs? Plotting the belief distributions in Figure 3.6 shows that the belief distributions are slightly more uniform in $X80s$ than in $X80$, with a central

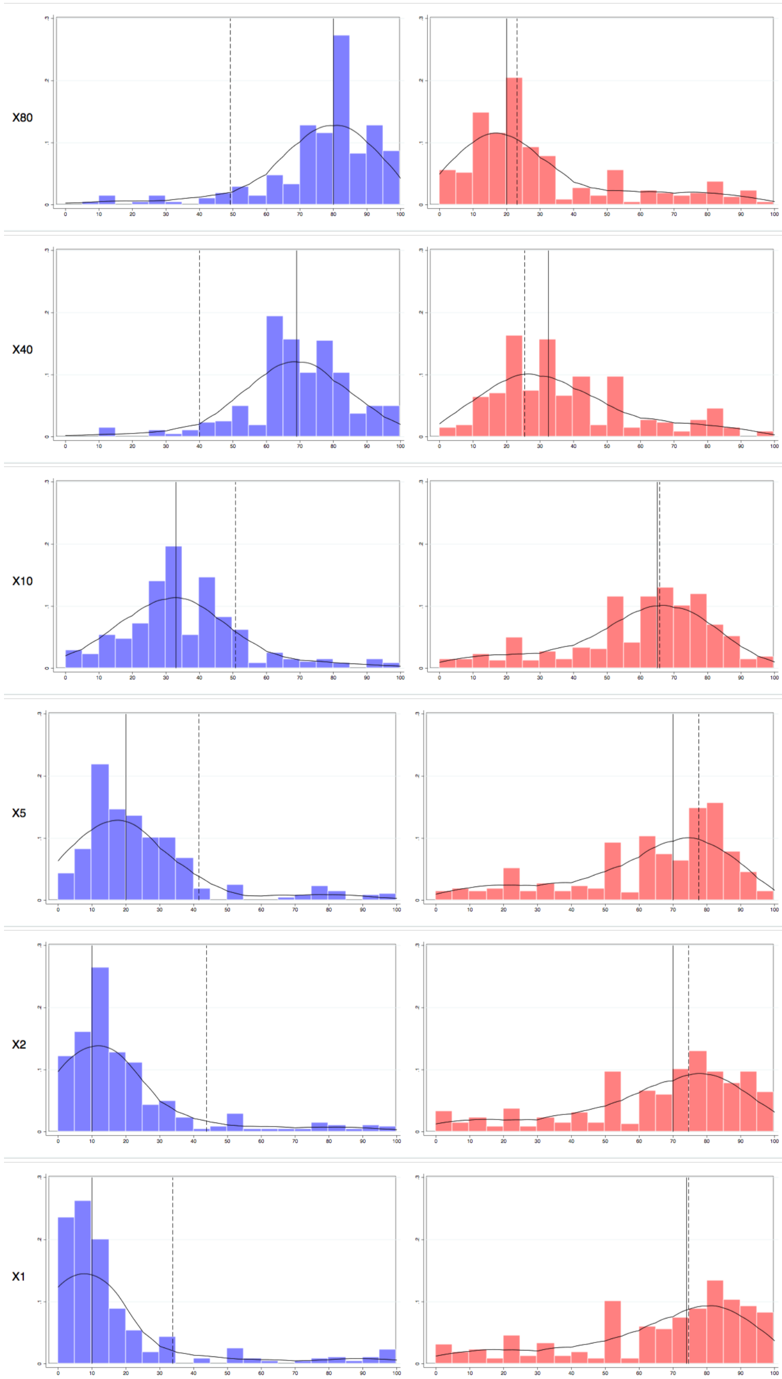


Figure 3.5: Belief Distributions

The left panel is for player 2's beliefs about player 1, and the right panel is for player 1's beliefs about player 2. The solid lines mark the mean of i's beliefs and the dashed line marks the empirical frequency of j's actions.

	(1) p	(2) q
scale	-0.023	-0.015
	(0.654)	(0.755)
Constant	0.515***	0.246***
	(0.000)	(0.000)
Observations	260	268

p-values in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.12: Scale invariance in actions
We report t-tests of the differences between action frequencies of $X80$ and $X80s$. Standard errors are clustered by subject.

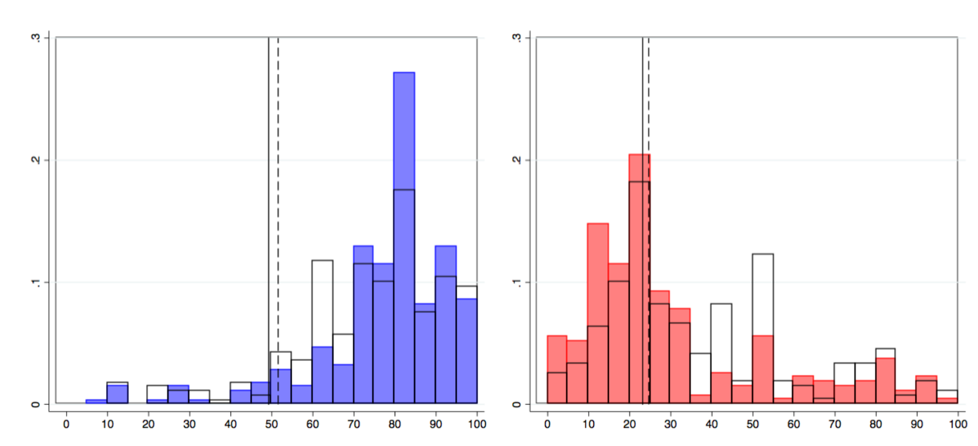


Figure 3.6: Belief distributions across scaled games
The solid colors give beliefs in $X80$, and the black outline gives beliefs in $X80s$. The left panel is player 2's beliefs about player 1, and the right panel gives player 1's beliefs about player 2.

tendency slightly closer to one-half. Though the effect seems small, it is statistically significant, as shown in Table 3.13. This suggests that subjects believe that others are more random when the stakes are low. That beliefs change (albeit slightly) with scale but actions do not poses a minor puzzle.

Pair of Games	p^*	q^*
$X80 - X80s$	0.160 (0.003)	0.188 (0.000)

Table 3.13: Scale invariance in beliefs
We report Kolmogorov-Smirnov statistics of the hypothesis that distributions of beliefs are *not* ordered by FOSD. p-values are reported in parenthesis.

3.7 Sophistication

In experimental studies of strategic sophistication, it is typical to play games that are dominance solvable. Commonly played games are the beauty contest game (e.g. Nagel (1995)), 11-20 game (e.g. Alaoui and Penta (2015)), or 3×3 dominance solvable games (e.g. Costa-Gomes and Weizsacker (2008)). This is because the framework of level k typically assigns a unique level to each action except those that are part of the Nash equilibrium profile, which are consistent with all levels above a certain threshold. Therefore, it is easy to infer strategic sophistication from behavior. In generic fully mixed games however, the levels "cycle"[17], meaning that no action is "more sophisticated" than another. For this reason, there have been no attempts (to our knowledge) to behaviorally identify sophistication in fully mixed games.

Since we have enriched standard actions data with stated beliefs in an array of games with systematically varied payoffs, our data is ideal for understanding sophistication in such games. In addition, we have included the dominance solvable games $D1$ and $D2$, which we use to measure sophistication at the subject level. In game Di, player i has a strictly dominant action. Of player j's two actions, one is the unique best response to i's dominant action and the other is the unique best response to a uniform (or sufficiently uniform) distribution. In other words, this first action of player j is that

[17] For example, for games of the form of Figure 3.1, the best response to L is U to which the best response is R to which the best response is D to which the best response is L. If level 0 is taken to uniformly mix, and assuming L is the unique best response to $q = \frac{1}{2}$ and R is the unique best response to $p = \frac{1}{2}$ (as is the case for almost all games in this family), then the actions taken by levels $k = 0, 1, 2, 3, ...$ for players 1 and 2 are $p = \frac{1}{2}, U, D, D, U, U, D, D, ...$ and $q = \frac{1}{2}, R, R, L, L, R, R, ...$, respectively.

taken by a player with Level 2 or higher whereas the second action would be taken by Level 1. We thus use the belief that a player i-subject places on the first of these j-actions as a measure of strategic sophistication.

3.7.1 Sophistication and Player Role

How does the player role itself affect strategic sophistication? To answer this question, we compare our simple sophistication measure across player 1- and player 2-subjects. Importantly, since $D1$ and $D2$ are exactly the same (up to permutation of rows and columns), this measure is *exactly the same* for both players, and hence and any difference across players in sophistication must be due to experience in different roles of the X-games.

Figure 3.7 plots histograms of subjects' average sophistication (averaged across three instances of Di) by player-type, with player 1 on the left and player 2 on the right. Comparing average levels of sophistication (solid lines) across players reveals that player 1's average sophistication of 56% is much greater than player 2's average of 33%. Interestingly, both players best respond to their opponents' dominant action at nearly the same frequency, 80% for player 1 and 76% for player 2 (dashed lines).

Player 1 is much more sophisticated than player 2. One hypothesis is that something about player 1's role in the X-games is more difficult, and hence player 1 subjects end up thinking harder on the X-games and this somehow "spills over" to the Di games. However, despite having slightly slower response times on the X-games, player 1-subjects do not spend longer on Di than player 2-subjects. Furthermore, we define player i's "attention" as his average belief that j takes his dominant action in Dj.[18] As shown in Figure 3.8, player 1 and player 2-subjects have similar distributions of attention and take dominated actions in Di at very nearly the same rate (dashed lines).

[18] We call this attention because it is a belief over an opponent whose optimal action is independent of strategic considerations.

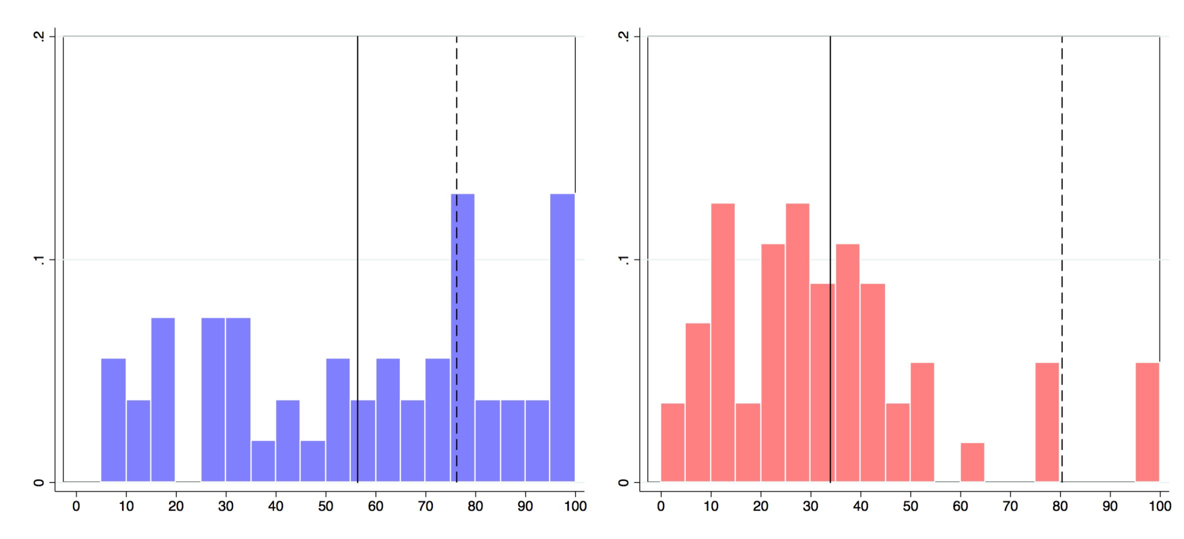


Figure 3.7: Sophistication

We measure player i's sophistication as his belief that player j plays the best response to i's dominant action instead of a best response to a uniform distribution in game Di. This figure plots a histogram of subjects' average sophistication. The left panel is for player 1, and the right panel is for player 2. The solid line marks i's average sophistication, and the dashed line is the empirical frequency with which j best responds to i's dominant action.

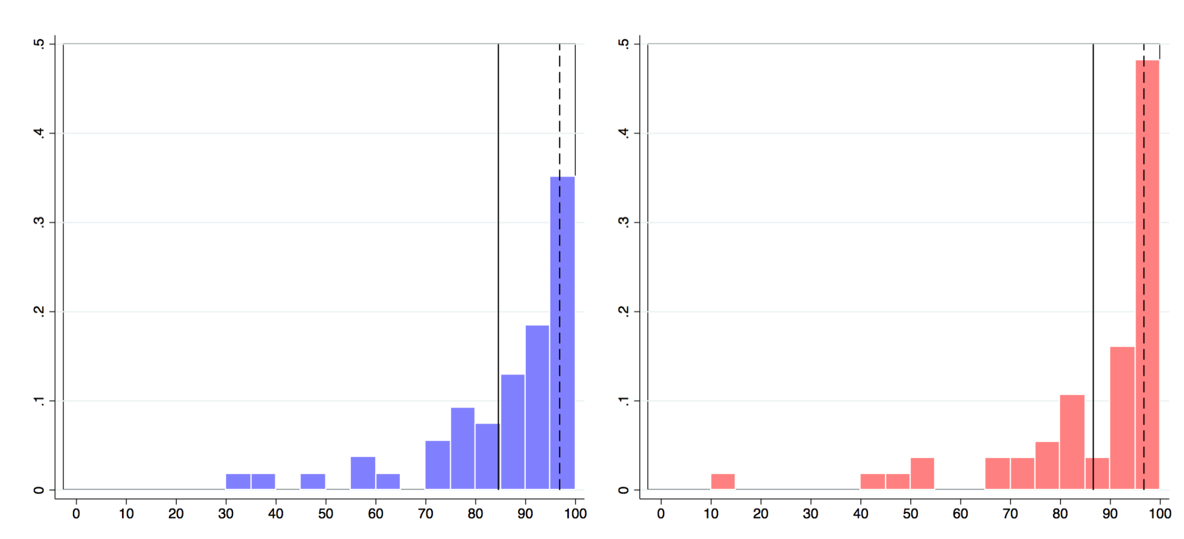


Figure 3.8: Attention

We plot subject i's belief that j takes j's dominant action in Dj, a measure we call "attention". The left panel is for player 1, and the right panel is for player 2. The solid line marks i's average attention, and the dashed line is the empirical frequency with which j takes his dominant action.

	(1) Soph.	(2) Soph.	(3) Soph.	(4) Soph.	(5) Soph.	(6) Soph.
Player 1	22.465***	20.846***	21.418***	25.509***	23.492***	22.730***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Response time		0.582***	0.588***		0.655***	0.693***
		(0.004)	(0.003)		(0.004)	(0.002)
Attention			0.290**			0.359**
			(0.047)			(0.032)
Constant	33.887***	17.137**	-8.144	33.150***	14.880**	-17.036
	(0.000)	(0.011)	(0.567)	(0.000)	(0.042)	(0.298)
Observations	110	110	110	92	92	92

p-values in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3.14: Effect of player role on sophistication. Column 1 regresses sophistication on an indicator for player 1. Column 2 controls for subject-average response time on the three rounds of Di. Column 3 additionally controls for the attention measure. Columns 4-6 are the same, except after dropping subjects who ever took a dominated action throughout the experiment.

Hence, the difference in sophistication does not seem to reflect simple differences in effort. Indeed, Table 3.14 shows that the sophistication gap between players is robust to controlling for response times and attention as well as dropping subjects who took at least one dominated action in the experiment.

3.7.2 The Relationship Between Sophistication and Behavior

Does sophistication measured in dominance solvable games predict behavior in fully mixed games? To answer this, we divide player 1-subjects into equal-sized "sophisticated" and "un-sophisticated" groups based on the player 1-median of the sophistication measure, and similarly for player 2-subjects. Figure 3.9 replicates Figure 3.3, but is broken down into sophisticated and un-sophisticated groups. We find that sophistication, measured in the dominance solvable Di-games, is strongly predictive of behavior in the fully mixed X-games. This is particularly interesting as other studies find no "persistence of strategic sophistication" across different types of games (see, for

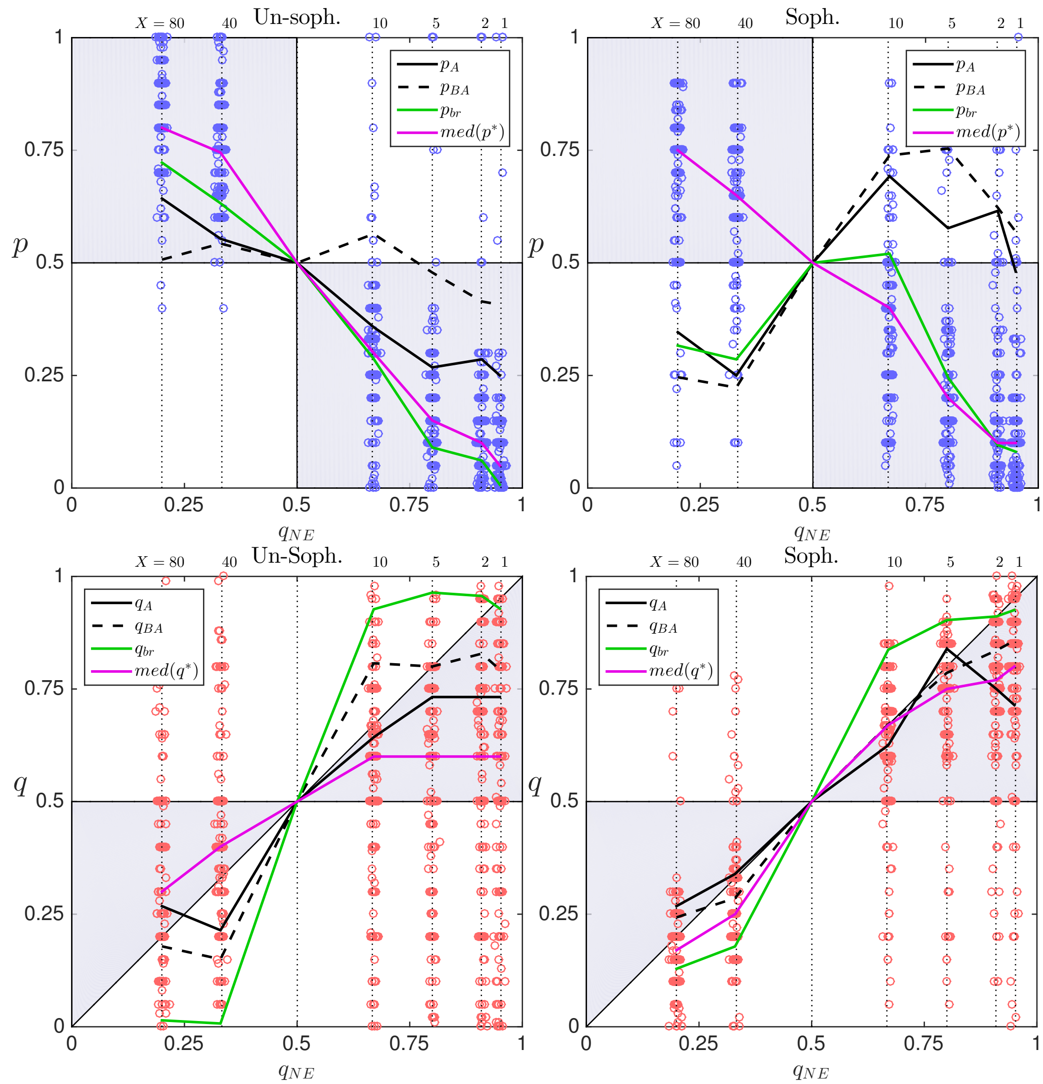


Figure 3.9: Sophistication and behavior

example, Georganas et al. (2015)).

For $X > 20$ (the case of $X < 20$ being symmetric), player 1-subjects believe player 2 will play R more often than L. Less sophisticated player 1's tend to believe player 2's behavior is relatively more uniform. Consistent with these beliefs, un-sophisticated player 1's tend to take U, whereas sophisticated player 1's tend to take D. Player 2-subjects believe player 1 will play U more often than L. Less sophisticated player 2's tend to believe player 1 takes U relatively more often (naively responding to large X perhaps), and accordingly play R relatively more often.

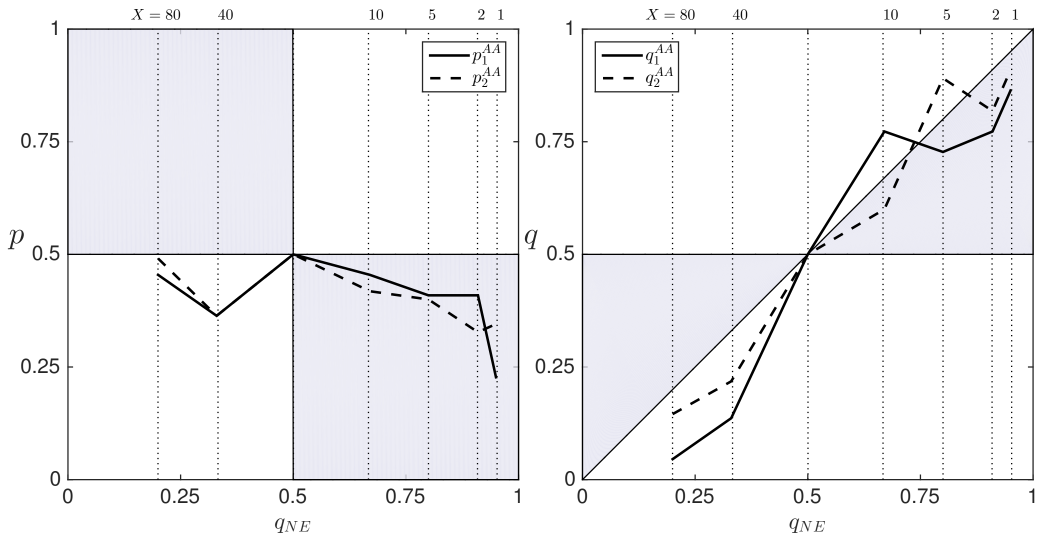


Figure 3.10: Actions without belief elicitation

Interestingly, it is the *un*-sophisticated player 1-subjects and the sophisticated player 2-subjects who are most consistent with the joint QRE-NBE predictions.

3.8 The Effects of Belief Elicitation

We show that the actions data from Section A differs significantly from that of Section BA. To this end, we run F-tests (clustering by subject) and reject the joint hypothesis that the action frequencies from Section A equal the action frequencies from Section B game-by-game for all six X-games (p-values of 0.00 and 0.02 for players 1 and 2, respectively).

Our hypothesis is that this difference is *caused by* belief elicitation. However, the two sections differ in their order, the fact that the games in BA are played against previously recorded actions, and very slightly in their composition of games. To nail down the cause, we run an additional treatment called A-A. This is identical to the A-BA treatment except beliefs are not elicited (and instructions never mention belief elicitation).

In Figure 3.10, we plot the action frequencies separately for the two stages of A-A.

We find that the first-section action frequencies cannot be distinguished statistically from the second-section action frequencies, with very similar averages quantitatively (p-values of 0.94 and 0.22 for players 1 and 2, respectively). We conclude that belief elicitation *does* effect actions. This finding adds to a literature with mixed results on the issue, with some studies claiming no such effect. For discussions, see Schotter and Trevino (2014), Aguirregabiria and Xie (2017), and Schlag et al. (2015).

Importantly, the "direction" of the change in actions data due to belief elicitation is systematic. Hence, there is hope that, even though we cannot say that the stated beliefs are necessarily a good approximation of the beliefs subjects held when playing the games without elicitation, we may be able to de-bias the effects of elicitation to infer those beliefs. What is more, based on the analysis of the Section 3.7, we can say that the direction of the change in action frequencies is consistent with increasing sophistication.

3.9 Conclusion

We run a laboratory experiment in which subjects play games and state their beliefs over their opponents' actions. By using a family of games that vary systematically in payoffs, we observe the mapping from opponents' actions to beliefs and the mapping from expected payoffs (given beliefs) to actions. Our results have direct implications for the validity of assumptions underlying broad families of stochastic equilibrium models. In particular, we find systematic bias in beliefs and systematic failures of best response, which we relate to features of the underlying game. By relating subject-level behaviors to measures of sophistication, we find that the player role itself can have an important effect on sophistication, and sophistication has a surprising effect on behavior in fully mixed games. We provide evidence that belief elicitation effects actions in a direction consistent with increasing sophistication.

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Appendix A

Appendix to Chapter 1

A.1 Experimental Instructions

This is an experiment in decision making. All interaction will be completed on the screen in front of you, and from this point on you may not talk to other participants until the conclusion of the experiment. If you have any problems or questions during the experiment, raise your hand and wait for the assistance of an experimenter. We ask at this point that you make sure you have reviewed and signed both consent forms on your desk.

Before we begin, the rules of the lab are that you not eat or drink while you are in the lab, and you also must have your cellphones turned off. This lab has a 'no-deception' policy, which says that neither I nor the experiment can lie to or deceive you in any way - the game is exactly as I present it, with no hidden elements, and should have no partial or unclear information.

The experiment will consist of six sections. Within each section, the procedure for each of the 15 rounds will be identical, but there will be differences between each of the sections. Before each section we will thus read instructions regarding the rules for that section.

Your earnings will consist of a $5 fixed fee for taking part in the experiment, and an additional payment determined by your actions in the game, by chance, and by the actions of others. The details of this will be made explicit as we describe each game.

Section 1 [GMP w/ Majority Voting]

In Section 1, you will play 15 rounds of a group voting game. At the start of each round, you will be randomly sorted into a group of five players. You will not know which other participants are in your group.

Jar Overhead

Next, one of two virtual jars will be randomly assigned to each group. The two possible jars are shown on the overhead now. The 'red jar', shown on the left, contains 7 red balls and 3 blue balls. The 'blue jar', shown on the right, contains 7 blue balls and 3 red balls. For each group, each jar is equally likely to be assigned to it. Each group's task is to determine the colour of its assigned jar.

To help you with this decision, you will be shown an image of your group's jar with the order of the balls randomized and all colours obscured. You will then select a ball from the jar and observe its colour. The overhead shows an example of this process - the blue jar is randomly assigned to the group, the order of its balls are randomized, the colours of the balls are obscured, and the player has then selected a blue ball.

After you have selected a ball, you will then vote for either the red jar or the blue jar. Your group's decision will be made by simple majority - if three or more of the five group members vote for the red jar, the group will select the red jar, otherwise the group will select the blue jar. In each round, if your group makes the correct decision, it will be added to your running total of correct group decisions, and you will receive an additional 25 cents at the end of the experiment.

I will now walk you through the first practice round to teach you the experimental interface. Throughout, certain actions may take you away from the relevant screen, so please do not take any actions until I ask.

Main Screen

You should now see the first screen of the first round, both on the screen in front you and the overhead. Again, please do not click on anything until asked. You will note first that the round and section numbers are shown at the top of the screen, as well as a record of the number of correct decisions your groups have made. At the bottom of the screen you will see the section number, and the rules of the game - that is, the group decision procedure on the left, which says that group decisions are made by majority, and on the right a reminder that your group has five members, and that for each of the two possible jars 7 of the 10 balls match the colour of the jar. All of this information will be presented on every screen of every round of the first section. Note that the game is identical in every round of this section, and thus this information does not change.

In the middle of the screen, you will see the assigned jar with its colours obscured. To select a ball and reveal its colour, you simply click on it. You may do this now.

Once you have selected a ball, you will be asked to vote. To do this, click on the button for the color you wish to vote for. Once you have selected a colour, a new button of the same colour will appear. Clicking this button will finalize and cast your vote. Prior to clicking this button, you can change your selection by clicking the 'red' and 'blue' buttons as many times as you wish, but once you have clicked the vote button, your vote will be final. Please select a colour and cast your vote now.

Results screen

Once everyone has voted, you will be shown the colour of the jar, and told which colour your group voted for. You will also see the number of votes your group cast for each colour, as well as the colour of both your own vote and your selected ball. Finally, you will see an updated count of your correct group decisions, both in the center of the screen, and in the usual position at the top. When you are ready, click 'continue' to indicate that you are ready to move on to the next round.

You have now completed the first practice round for the first section. Every round in the first section will follow the exact procedure as the game you have just played. *Are there any questions at this time about the game, the rules, the computer players, or the interface?* You will now play a second practice round, unguided. You may play the second practice round now.

Paid Section 1

You have now completed the practice rounds for this section, and we will proceed to the paid rounds. This section will consist of 15 rounds, each of which follows exactly the same procedure as the practice rounds you have just played. In each round you will be randomly sorted into a group of five, and one of the two jars will be randomly assigned to your group, always with each having equal likelihood of being assigned, so that each round is completely independent from the others.

Are there any questions about the rules before we begin? You may begin the paid rounds.

Section 2 [GMP w/ Red Unanimity]

You have now completed the first section of the experiment, and we proceed to the second. In the second section of the experiment the game follows exactly the same procedure as the first section, except the group decision process is changed. In this section, the group only selects the red jar if all five group members vote for it, otherwise the group selects the blue jar. This decision procedure will be known as red unanimity. The game is otherwise unchanged. The screens you face will be identical, with the exception that the information regarding the voting procedure on the bottom left of each screen is updated to reflect the red unanimity rules.

This section will again consist of 15 rounds, each following these exact rules. In each round you will be randomly sorted into a group of five, and one of the two jars will be randomly assigned to your group, always with each having equal likelihood of being assigned, so that each round is completely independent from the others.

Are there any questions about the rules before we begin? You may begin the second section.

Section 3 [Informative Computer Game]

You have completed the second section of the experiment, and we proceed to the third. In this section the game follows the same procedure as the previous section except that the group composition is now changed. Now, instead of playing in groups with other participants, your group will consist of you and four computer players, for a total of five players.

Computer players follow the same steps as human players, and act completely independently of you and of each other. That is, each computer player will privately select a ball from the chosen jar exactly as a human player would, and will then privately vote for a colour. Each computer player will always vote for the colour matching the ball that it selected - if a computer player selects a red ball from the jar, it will vote for the red jar, and if it selects a blue ball from the jar, it will vote for the blue jar. Again, computer players act completely independently - they each randomly select their own ball, and do not share that information with each other, then each independently cast their own vote. For example, if three computer players in your group select blue balls, and one computer player selects a red ball, then three computer players will vote for the blue jar, and one computer player will vote for the red jar, as shown on the overhead.

The game is otherwise the same as in the previous section, including that the group's decisions are made by red unanimity. The interface is identical, with the exception of the information regarding computer players and their behaviour, which is added to the bottom-right of the screen.

This section will again consist of 15 rounds, each following these exact rules. In each round, one of the two jars will be randomly assigned to your group, always with each having equal likelihood of being assigned, so that each round is completely independent from the others.

Are there any questions about the rules before we begin? You may begin the third section.

Section 4 [Reverse Computer Game]

You have completed the third section of the experiment, and we proceed to the fourth. In this section the game follows the same procedure as the previous section except that now each computer player will vote for the jar that does not match its selected ball. That is, a computer player that selects a red ball will vote for the blue jar, while a computer player that selects a blue ball will vote for the red jar. For example, if three computer players select red balls and one computer player selects a blue ball, now three computer players will vote for the blue jar and one computer player will vote for the red jar, as shown on the overhead.

The game is otherwise the same as in the previous section, including that the group's decisions are made by red unanimity. The interface is identical, with the exception of the information regarding computer players and their behaviour, which is updated at the bottom-right of the screen.

This section will again consist of 15 rounds, each following these exact rules. In each round one of the two jars will be randomly assigned to your group, always with each having equal likelihood of being assigned, so that each round is completely independent from the others.

Are there any questions about the rules before we begin? You may begin the fourth section.

Section 5 [Option Game]

You have completed the fourth section of the experiment, and we proceed to the fifth. In this section the game will change in two ways. The first is that computer players will revert to voting for the colour of their selected ball. That is, a computer player that selects a blue ball will vote for the blue jar, while a computer player that selects a red ball will vote for the red jar. This information is again shown on the overhead.

The second change is that at the beginning of each round you will be given two options, labelled Option A and Option B. If you select Option A, you will then select a ball from the jar as previously, but your vote will then be cast to automatically match the colour of your selected ball. That is, if you select a blue ball then you will automatically vote for the blue jar, while if you select a red ball then you will automatically vote for the red jar.

If you select Option B, you will still click on a ball, but doing so will have not reveal its colour - that is, all 10 balls will remain grey. You will then choose which colour you wish to vote for, as in previous sections.

Again, under Option A, clicking a ball reveals its colour, and your vote is automatically cast to match that colour. Under Option B, clicking a ball does not reveal its colour, and you then select which colour you wish to vote for.

It is important to note that the option you choose has no impact on the computer players. Regardless of which option you choose, each computer player selects a ball from the jar, observes its colour, and then votes for that colour.

I will now walk you through the first of two practice rounds to teach you the experimental interface. Throughout, certain actions may take you away from the relevant screen, so please do not take any actions until I ask.

Option Screen

You should now see the first screen of the first practice round, in which you will select between these two new options. In the center of the screen you will see the two options, along with the rules for each option. For Option A, you can see that selecting a ball reveals its colour, and your vote is automatically cast to match the colour of your selected ball. For Option B, you can see that selecting a ball has no impact, and you select which colour you wish to vote for. Underneath, you can see information that is the same for both options. Specifically, under both options, when each computer player selects a ball, its colour is revealed, and the player then votes for the colour that matches the selected ball. This information is also included on the bottom-right of the screen, as in previous sections.

Are there any questions about these options, or the information shown on this screen?

You may now select an option. To do so, just click on the button containing the words 'Option A' or 'Option B'.

Voting Screen

Once each player has selected an option, the interface is similar to previous sections. Regardless of which option you selected, you will be asked to click on a ball. If you selected Option A, this will reveal the colour of the ball and automatically cast your vote to match that colour. If you selected Option B clicking the ball will have no impact, and you will then be asked to select your vote. You may now select a ball and, if necessary, vote.

Once all players have voted, you will be taken to the results screen, which is the same as previous sections.

Are there any questions about the game, the new rules, or the interface? You may click continue when you are ready and proceed to play the second practice round.

Paid Section 5

You have now completed the practice rounds for this section, and we will proceed to the paid rounds.

This section will again consist of 15 rounds, each following these exact rules. In each round one of the two jars will be randomly assigned to your group, always with each having equal likelihood of being assigned, so that each round is completely independent from the others.

Are there any questions about the rules before we begin? You may begin the fifth section.

Section 6 Instructions [Bidding Game]

You have now completed the fifth section, and we proceed to the sixth. In this section, we move from the group voting game to an individual decision game. As in previous sections, you will play 15 rounds of this game. You will also play two practice rounds, so you can familiarize yourself with the rules and the interface.

In this game, you will earn points. At the end of the experiment, you will be paid 25c for every 120 points you earn, in addition to your earnings from the previous sections.

In each round, you will see 100 cards on your screen, in a 5 x 20 array. Each of these cards has a value between 20 points and 119 points inclusive, with each value in this range assigned to exactly one card. The order of the cards is randomized, with the values obscured.

In each round, you will be given an endowment of 120 points. You will then choose a bid (a whole number between 0 and 120 inclusive) and will then select one of the cards. The value of this card will then be revealed to you.

If your bid is greater than or equal to the selected card's value, you receive 150 percent of this card's value, minus your bid, in addition to your 120 point endowment. If your bid is less than the card's value, nothing happens, and you simply retain your 120 point endowment.

Suppose, for example, you bid 62:

- Suppose the value of your selected card is 56. Then its worth to you is 56 x 1.5 = 84 points. Since your bid was 62, you add 84 - 62 = 22 to your endowment, and thus earn 142 points overall.

- Suppose the value of your selected card is 30. Then its worth to you is 30 x 1.5 = 45 points. Since your bid was 62, you add 45 - 62 = -17 to your endowment, and thus earn 103 points overall.
- Suppose the value of your selected card is 67. Since your bid was 62, no transaction takes place and you add nothing to your endowment, and thus earn 120 points overall.

You will play this game for 15 rounds, each time being asked to submit a bid and select a card, and with the cards randomly rearranged in each round. A running total of your accumulated points will be shown at the top right of the screen.

Are there any questions about this game before we begin the practice rounds?

I will now walk you through the first practice round to teach you the experimental interface. Throughout, certain actions may take you away from the relevant screen, so please do not take any actions until I ask.

Bid/Card Screen

You should now see the first screen of the first round, both on the screen in front you and the overhead. Again, please do not click on anything until asked. Note that the rules of the game, which are identical for each round, are available at the bottom of the screen. At the top of the screen you will again see the section and round number, and the total number of points you have accumulated in this section.

Above the cards in the centre of the screen you will see a box into which you will type your bid. Your bid must be a whole number between 0 and 120 inclusive. Once you have typed a bid, you may click on one of the cards to select it. Do this now.

Results Screen Overhead

Once you have selected a card, its value will be revealed to you in black, with the values of the other cards revealed in grey. Below the cards you will see your results - your bid, the value of your selected card, and your overall earnings for the round. You will also see an updated count of your total points earned in this section, which is also shown at the top right of the screen.

Once you are ready to continue, please click continue. Once all have done so, you can play the second practice round.

You have now completed the practice rounds of the sixth section. *Are there any questions about the rules or the interface before we begin the paid rounds?* There are 15 paid rounds, each of which follows exactly the same procedure as the practice rounds you have just played. In each round, the cards are randomly rearranged, so that each round is independent of the others. You may play the 15 paid rounds now.

A.2 Experimental Overheads

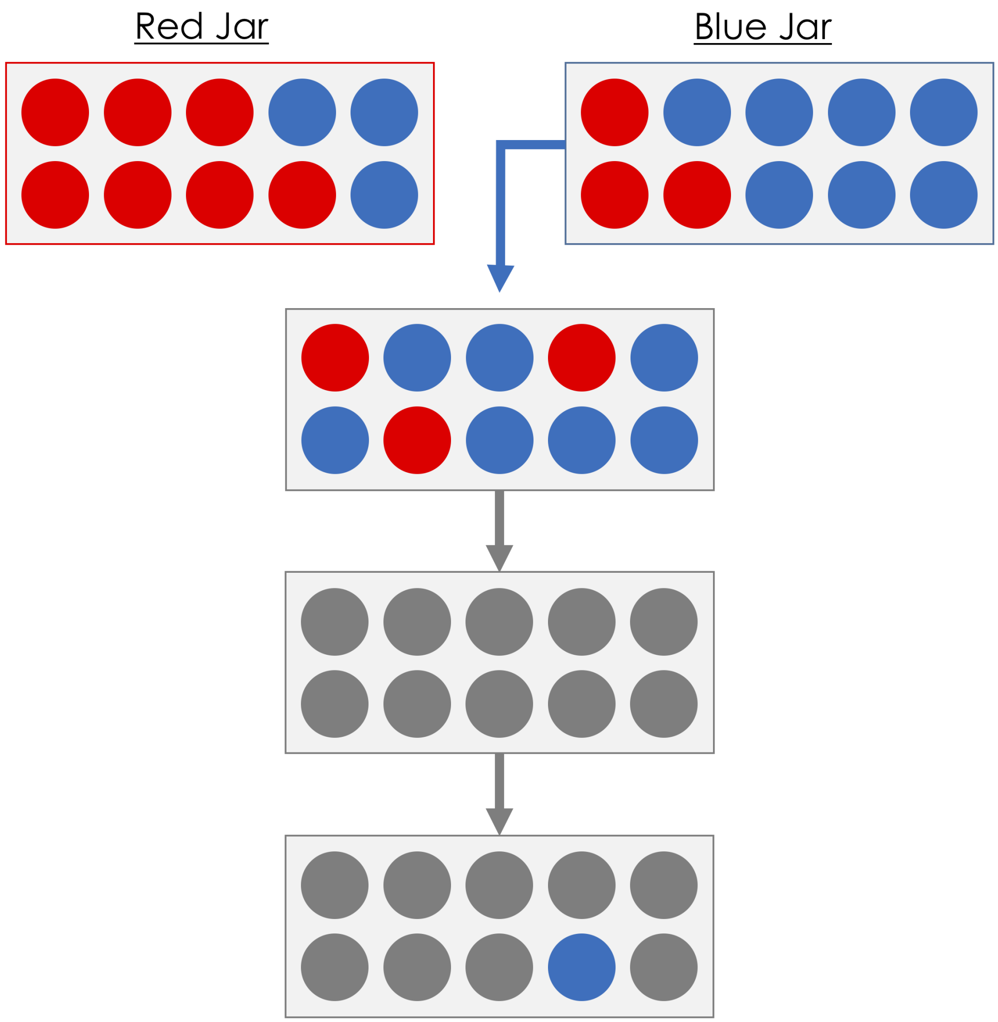
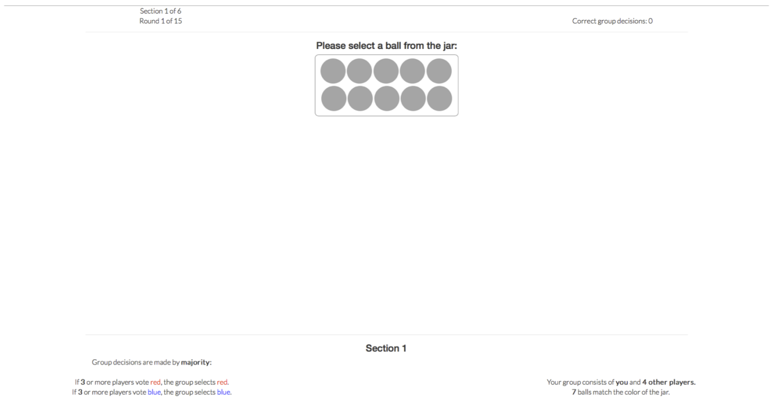


Figure A.1: Experimental Overheads - Slide 1



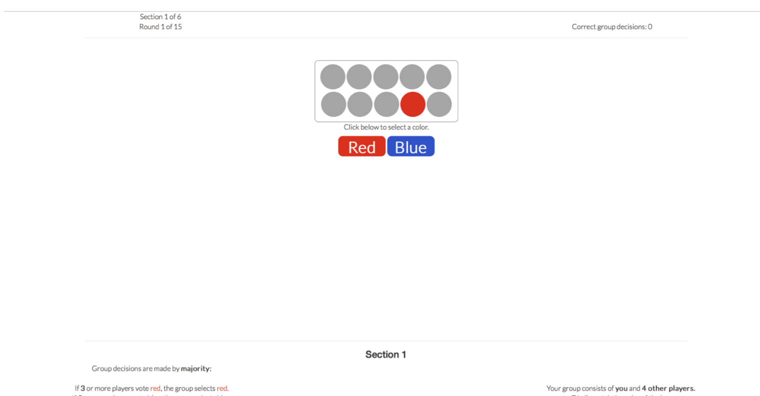
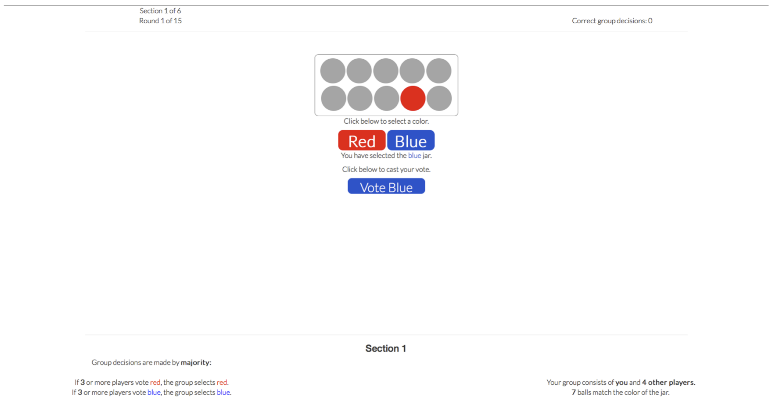


Figure A.2: Experimental Overheads - Slides 2 and 3



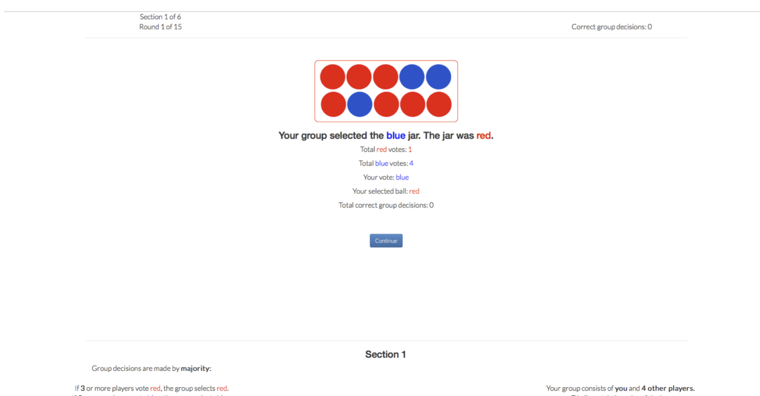


Figure A.3: Experimental Overheads - Slides 4 and 5

Section 1

Groups consist of **5 players.**

7 of 10 balls match the color of the jar.

Decisions are made by **majority.**

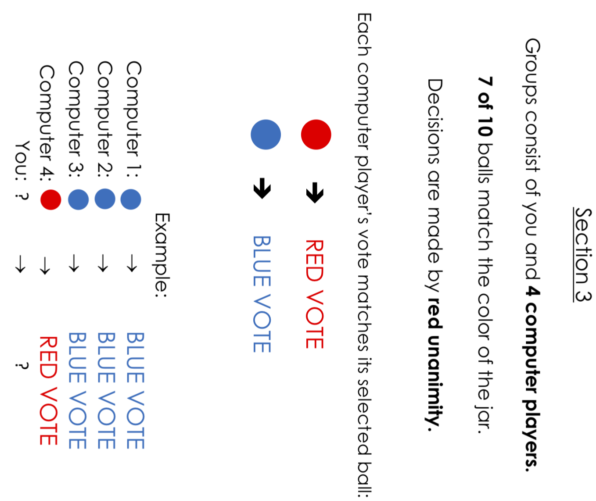
Section 2

Groups consist of **5 players.**

7 of 10 balls match the color of the jar.

Decisions are made by **red unanimity.**

Figure A.4: Experimental Overheads - Slides 6 and 7



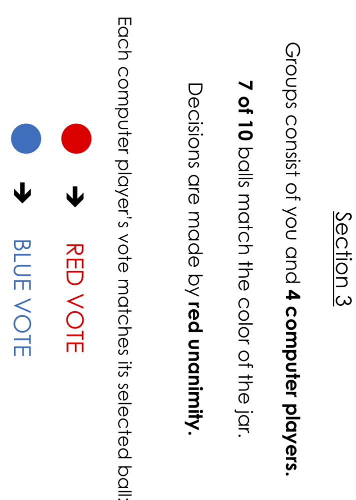
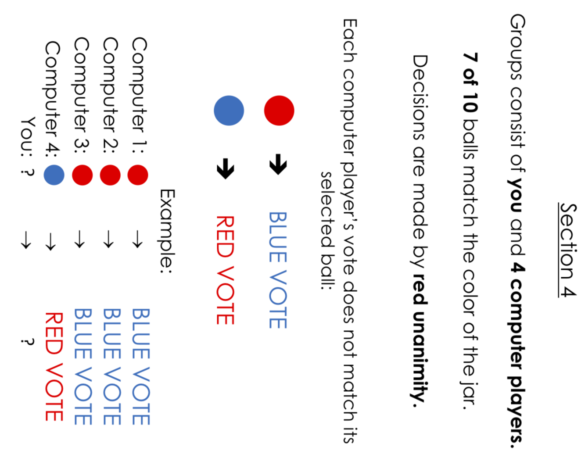


Figure A.5: Experimental Overheads - Slides 8 and 9



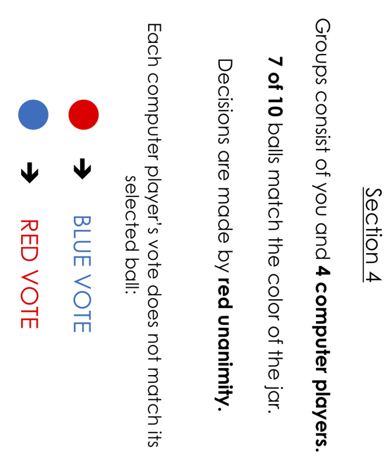


Figure A.6: Experimental Overheads - Slides 10 and 11

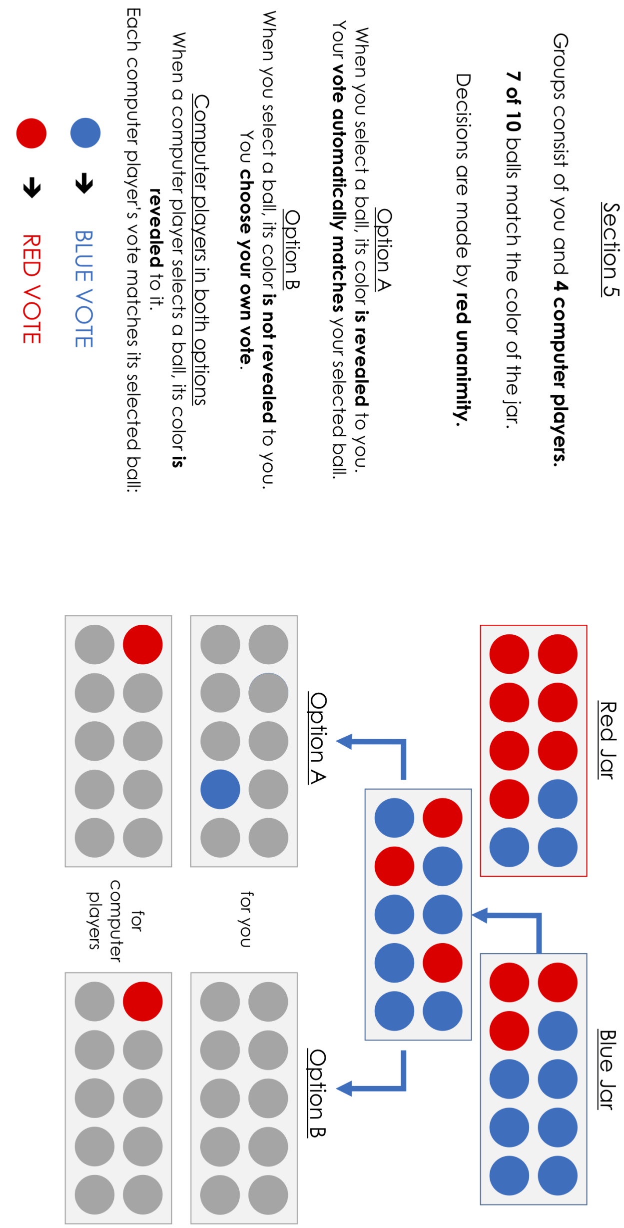
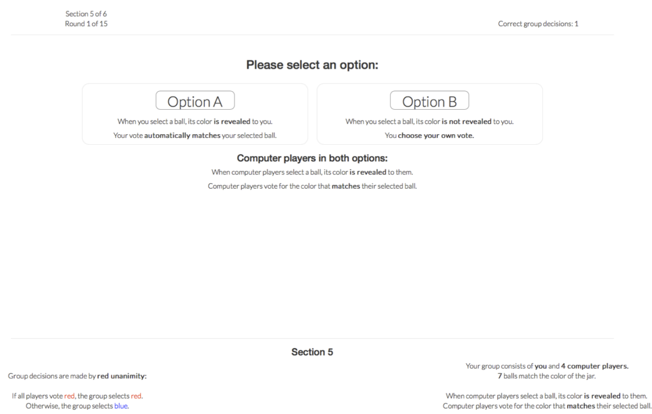


Figure A.7: Experimental Overheads - Slide 12 and 13



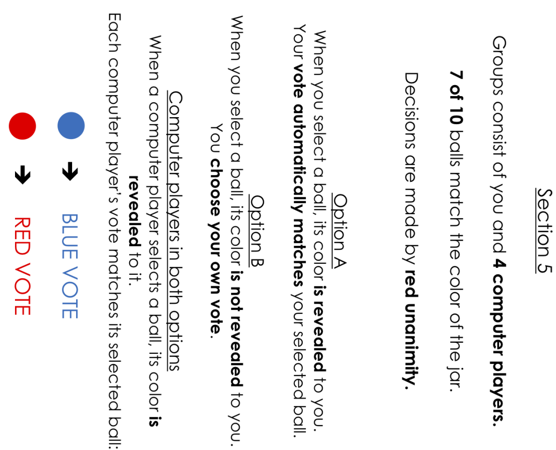
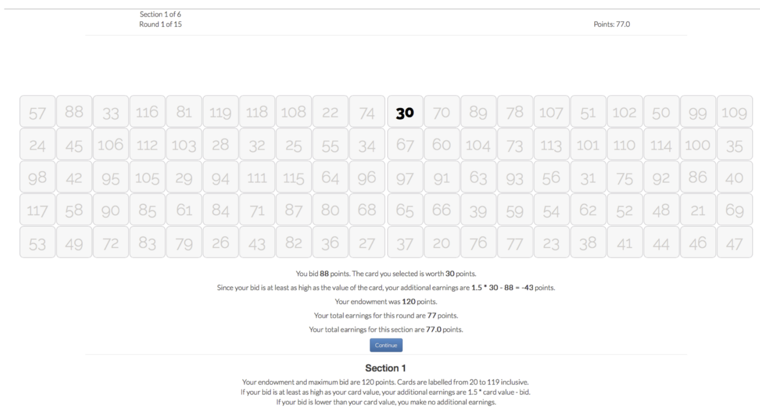


Figure A.8: Experimental Overheads - Slides 14 and 15



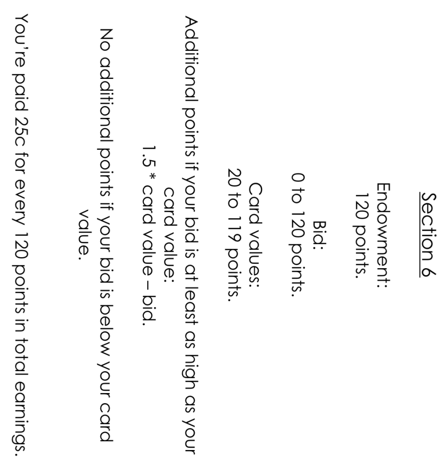
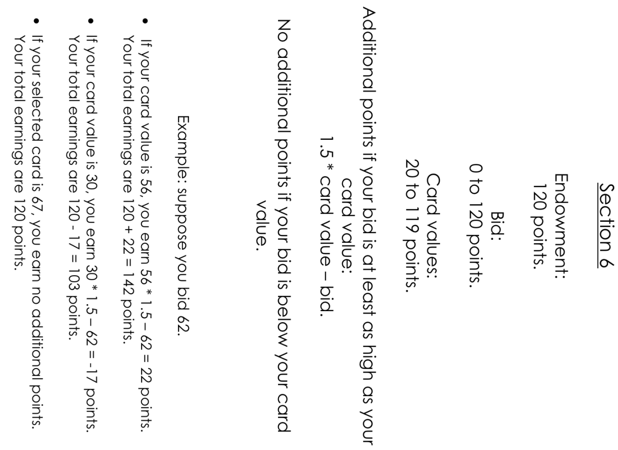


Figure A.9: Experimental Overheads - Slides 16 and 17



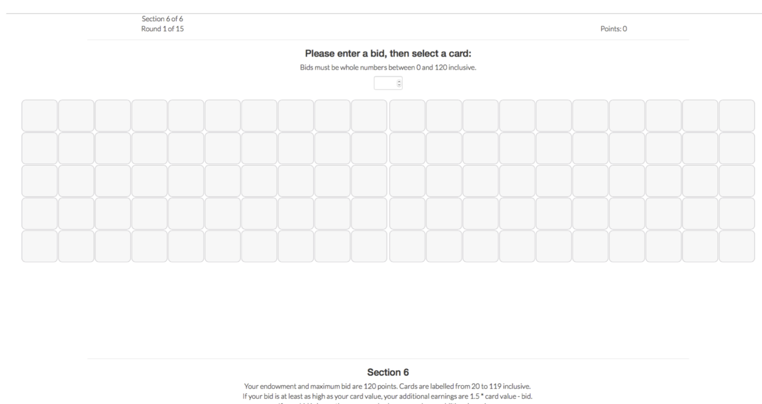
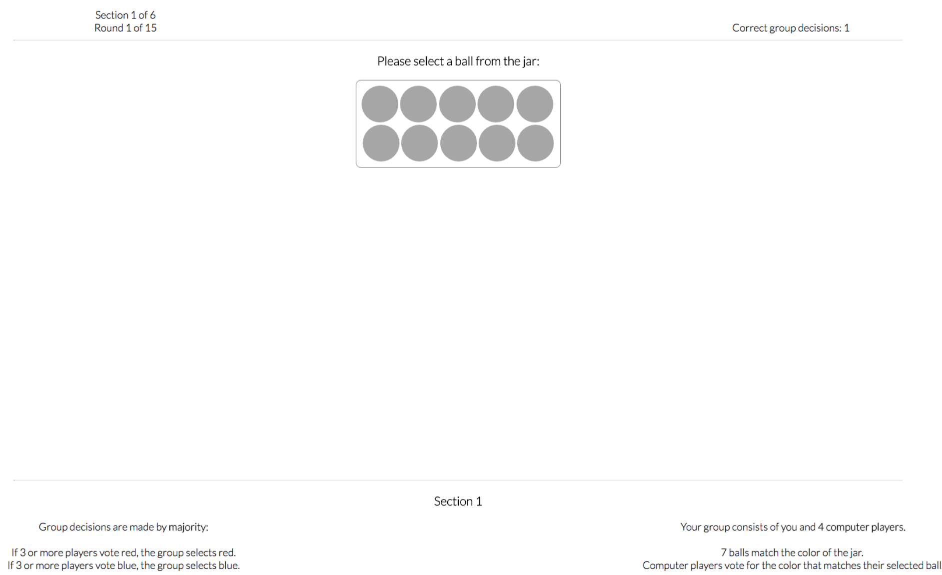


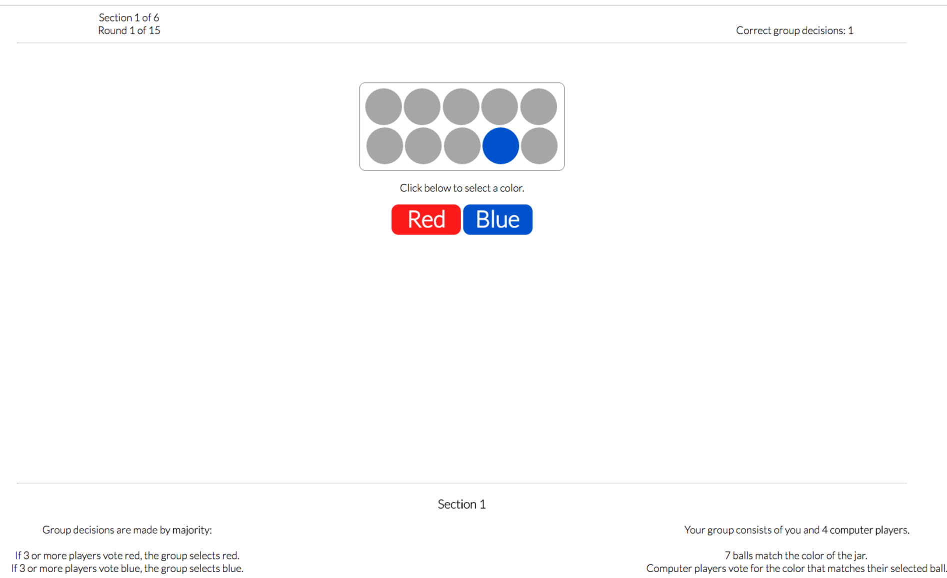
Figure A.10: Experimental Overheads - Slides 18 and 19

A.3 Experimental Interface

Figure A.11: Jury Voting Interface

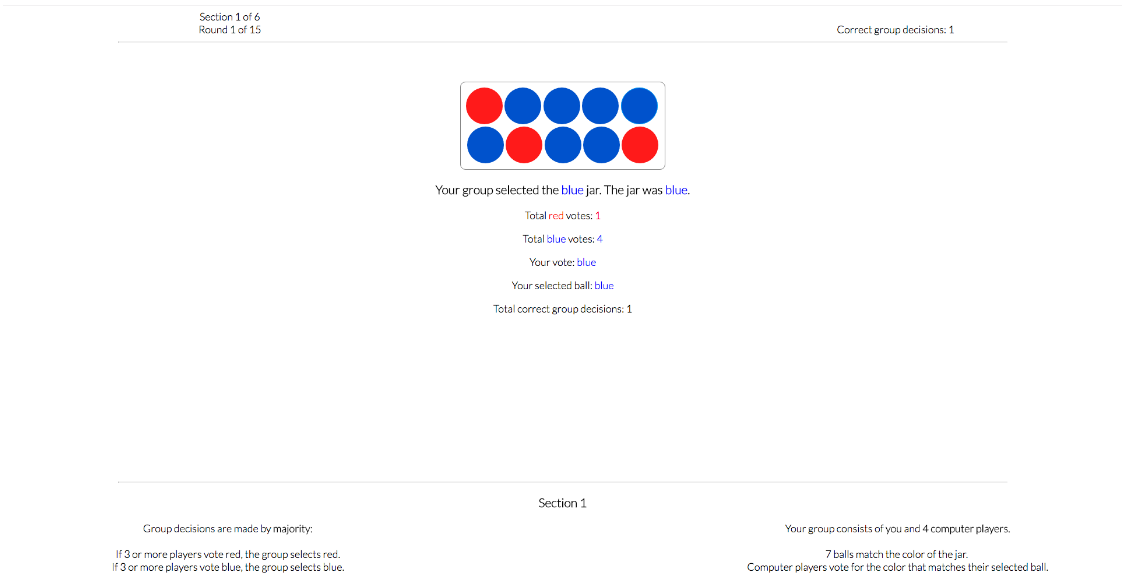


(a) Ball Selection

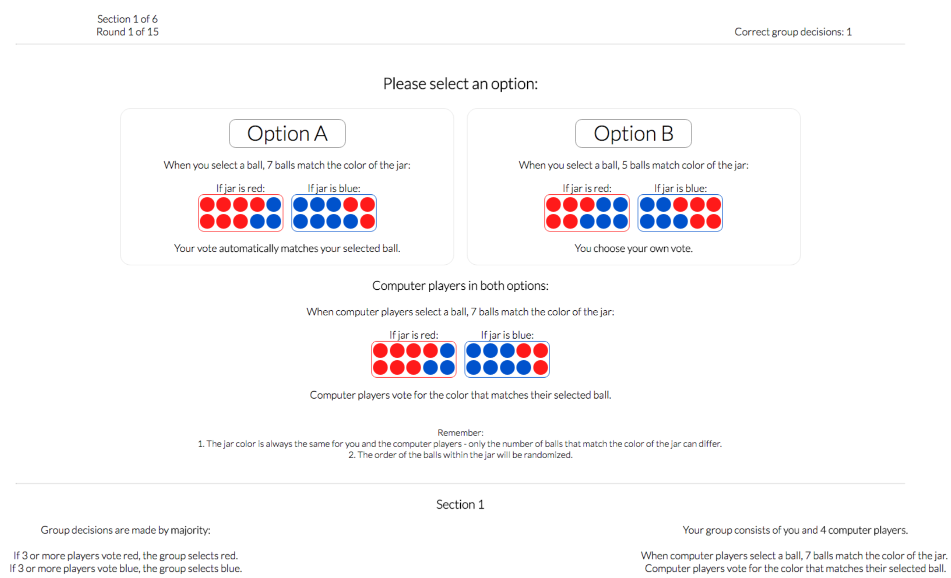


(b) Voting

Figure A.12: Jury Voting Interface, Continued



(a) Results



(b) Option Screen (OCG Only)

A.4 Permutation Tests

Consider the comparison of $\hat{\sigma}(s)$ across treatments A and B for some signal $s \in \{r, b\}$. A standard difference in proportions test is invalidated by the fact that the samples are non-independent in two ways: (a) the within-subject design means each subject appears in both treatments, and (b) each subject plays multiple rounds and so accounts for multiple observations within each treatment. To account for this, I create a distribution for the statistic $|\hat{\sigma}_A(s) - \hat{\sigma}_B(s)|$ under the null hypothesis that behaviour in the two treatments is the same, $\hat{\sigma}_A(s) = \hat{\sigma}_B(s)$.

To do this two new datasets, A' and B', are created by randomly determining for each subject whether their data will be assigned to the matching dataset or the mismatching dataset. That is, for each subject $i \in \{1, 2, ..., 220\}$, with probability $\frac{1}{2}$ subject i's observations from treatment A will be assigned to A' and her observations from treatment B will be assigned to B', while with probability $\frac{1}{2}$ her observations from treatment A will be assigned to B' and her observations from treatment B will be assigned to A'. Whether a subject's observations are assigned to the matching or mismatching datasets is independent between subjects.

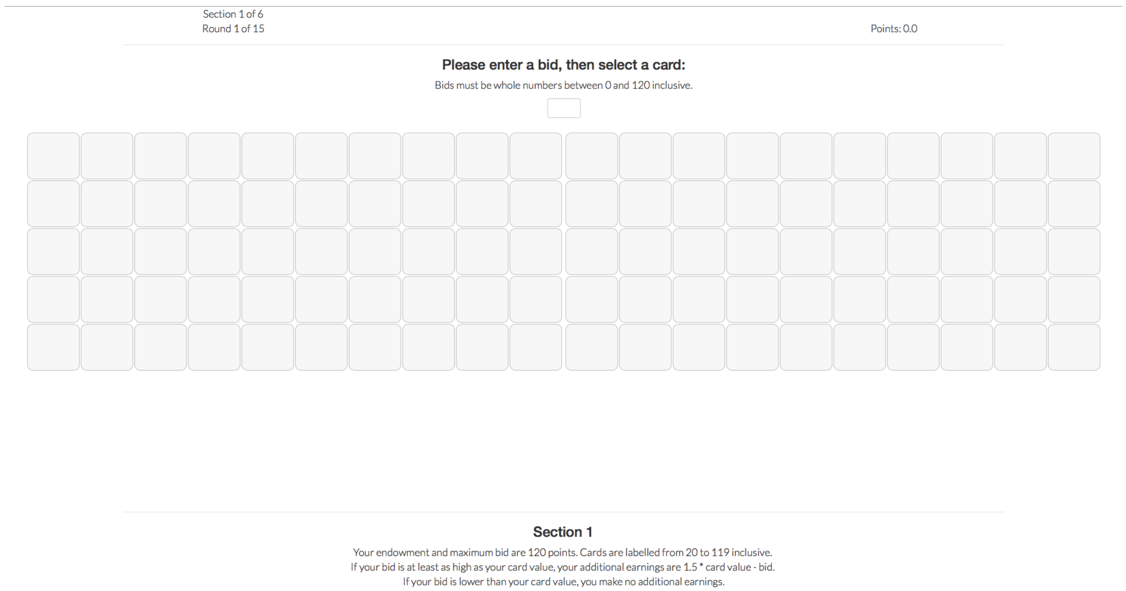
This resampling procedure is repeated 10,000 times, with the resampled test statistic $|\hat{\sigma}'_A(s) - \hat{\sigma}'_B(s)|$ recorded for each. This results in a distribution of the statistic that is valid under the null hypothesis. The proportion of values in this distribution that exceed the observed value $|\hat{\sigma}_A(s) - \hat{\sigma}_B(s)|$ provides the p-value for the two-tailed test of the alternate hypothesis that $\hat{\sigma}_A(s) \neq \hat{\sigma}_B(s)$.

Appendix B

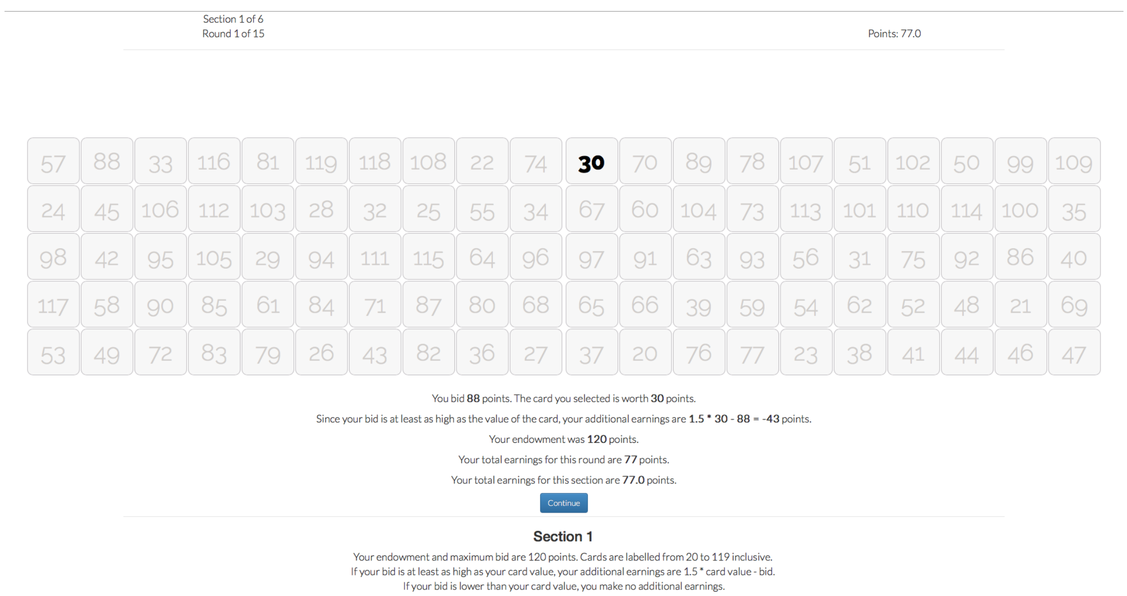
Appendix to Chapter 2

B.1 Experimental Interface

Figure B.1: Auction Game Interface



(a) Card Selection



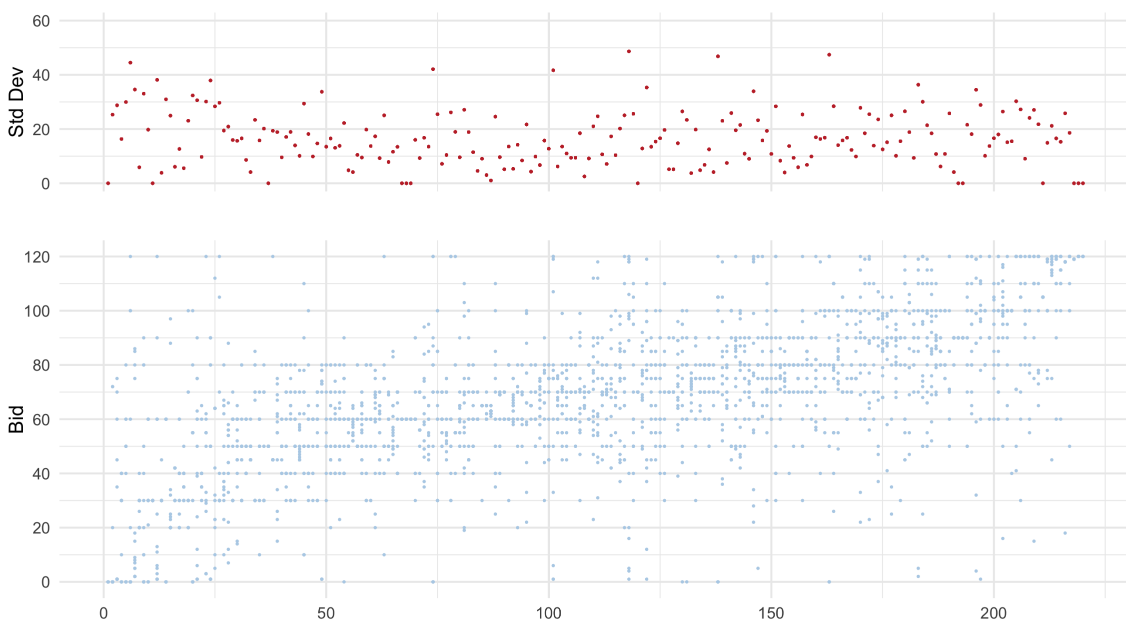
(b) Results

B.2 Additional Auction Game Results

Bidding Behaviour

Figure B.2 shows all bids, and the standard deviation of those bids, for all subjects. Subjects are ordered by median bid, increasing from left to right.

Figure B.2: All Bids by Subject



Short-term Bid Changes

The following recreates Charness and Levin's (2009) analysis of round-to-round changes in subjects' bids. Table B.1 shows how often subjects increase, decrease, or maintain their bids relative to the previous bid as a function of realized gains and losses of that bid. The results are largely similar to Charness and Levin's, including that subjects who make losses tend to decrease or maintain their bids in the following round, those who make gains are most likely to maintain their bids, and those who make neither gains nor losses are more likely to increase or maintain their bids.

Table B.1: Bid Changes by Previous Outcome

	Increase Bid	No Change	Decrease Bid
Gain	180 (26.5%)	310 (45.7%)	188 (27.7%)
No Gain/Loss	637 (40.9%)	635 (40.8%)	286 (18.4%)
Loss	137 (16.2%)	292 (34.6%)	415 (49.2%)
Total	954 (31.0%)	1237 (40.2%)	889 (28.9%)

Table B.2 presents a linear regression of the change in a subject's bid relative to the previous bid upon lagged payoffs. It suggests that outcomes with no gains or losses - which generally implies no transaction - lead to substantially increased bids in the next round, while gains have a smaller positive effect, as does experience, which is discussed further in the main text.

Table B.2: Linear Regression of Bid Changes on Previous Payoffs.

	Δ(bid)
Constant	-3.968
	0.000
Round	-0.159
	0.009
Lag Net Payoff	0.131
	0.000
Lag Net Payoff = 0	11.287
	0.000
AG First	0.245
	0.658
Obs	3,080
R^2	0.1027

Notes: Standard errors are clustered by subject and p-values are presented below the estimates.

B.3 Permutation Tests

Here I describe the permutation tests used to test the significance of the changes in AG behaviour over time presented in Table 2.2. The null hypothesis of each test is that round numbers are not predictive of behaviour, an implication of behaviour not changing over time. Let R_p be a random permutation of the set of round numbers $R = [1, ..., 15]$. I perform 10,000 such permutations, in each case relabelling the data using R_p. That is, if the first element of R_p is 5, all data from the first round is relabelled as coming from round 5. I then calculate the absolute value of the applicable statistic for each column of Table 2.2 - that is, the absolute change in behaviour between the first and last seven relabelled rounds. This creates a distribution for each statistic under the null hypothesis. I then compare the absolute value of the observed statistic for each column to the distribution, and report the percentile as the $p-$value. This constitutes a two-tailed test of the null hypothesis against the alternative that experience matters.

B.4 Comparative Results - OCG and RCG

Table B.3 extends Table 2.6 to OCG and RCG, presenting linear regressions of a bid's expected payoff (Columns (a) and (b)) and logistic regressions of the probability that a bid is an overbid (Columns (c) and (d)) upon the subject's behaviour through each of RCG and OCG.

The results generally follow those presented in the main text. For RCG, coefficients on jury voting strategies are insignificant, showing no evidence of a relationship between behaviour here and in AG. For OCG, on the other hand, a higher $\hat{\sigma}_B$ results in a lower expected payoff in AG. Recall that $\hat{\sigma}_B$ measures the frequency with which a subject selects *Choice* and then votes for the blue jar - behaviour which results in the lowest expected payoff, and which we would not expect of either those capable of pivotal reasoning (who we expect to select *Choice* and vote red) nor those who are not (who we expect to select *Information* and thus vote informatively). Thus this is also in keeping with the results of the main text, in that those who deviate from expected behaviour in irrational ways in the jury voting games do worse in AG. The behaviour that differentiates the sophisticated from the naive - selecting *Choice* and voting red, rather than selecting *Information* - appears to have no relationship with performance in AG, with the coefficients on $\hat{\sigma}_R$ insignificant.

Table B.3: Regressions of AG Behaviour on Jury Voting Behaviour.

Variable	Exp Payoff		Overbid	
	(a)	(b)	(c)	(d)
Jury	RCG	OCG	RCG	OCG
Constant	-2.495	-2.502	0.515	0.330
	0.000	0.000	0.016	0.138
Round	-0.040	-0.040	-0.019	-0.019
	0.064	0.064	0.080	0.081
$\hat{\sigma}(b)$	-0.885		0.191	
	0.165		0.476	
$\hat{\sigma}(r)$	0.546		-0.274	
	0.274		0.298	
$\hat{\sigma}_B$		-5.354		0.738
		0.012		0.276
$\hat{\sigma}_R$		0.397		0.082
		0.470		0.731
AG First	-0.008	0.156	0.281	0.256
	0.986	0.713	0.136	0.172
Obs	3,300	3,300	3,300	3,300

Notes: In all regressions standard errors are clustered by subject and p-values are presented below the estimates.

Appendix C

Appendix to Chapter 3

C.1 Experimental Instructions

Welcome!

This is an experiment in decision making, and you will be *paid for your participation in cash*. Different subjects may earn different amounts of money. What you earn depends partly on your decisions, partly on the decisions of others, and partly on luck. In addition to these earnings, each of you will receive $10 just for participating in and completing the experiment.

It is the policy of this lab that we are strictly forbidden from deceiving you, so you can trust the experiment will proceed exactly as we describe, including the procedures for payment.

The entire experiment will take place through your computers. It is important that you do not talk or in any way try to communicate with other subjects during the experiment.

Please turn off your cellphones now.

On the screen in front of you, you should see text asking you to wait for instructions, followed by a text box with a button that says "ID". Your computer ID is the number at the top of your desk, which is between 1 and 24. In order to begin the experiment, you must enter your computer ID into the box and press 'ID'. Please do that now.

You should all now see a screen that says "please wait for instructions before continuing". Is there anyone that does not see this screen? This screen will appear at various points throughout the experiment. It is important that whenever you see this screen, you do not click 'continue' until told to do so.

The experiment has *two sections*. We will start with a brief instruction period for Section 1, in which you will be familiarized with the types of rounds you will encounter. Additional instructions will be given for Section 2 after Section 1 is complete.

If you have any questions during the instruction period, raise your hand and your question will be answered so everyone can hear. If any difficulties arise after the experiment has begun, raise your hand, and an experimenter will come and assist you.

At the beginning of the experiment, each subject will be assigned the color RED or the color BLUE. There will be an equal number of RED and BLUE subjects. If you are assigned RED, you will be RED for the entire experiment. If you are assigned BLUE, you will be BLUE for the entire experiment.

Section 1 consists of *several rounds*. I will now describe what occurs in each round. First, you will be randomly paired with a subject of the opposite color. Thus, if you are a BLUE subject, you will be paired with a RED subject. If you are a RED subject, you will be paired with a BLUE subject. You will not not know who you are paired with, nor will the other subject know who you are. Each pairing lasts only one round. At the start of the next round, you will be randomly re-paired.

[**SLIDE 1**]

In each round, you will see a *matrix* similar to the one currently shown on the overhead, though the numbers will change every round. In every round, you and the subject you are paired with will both see the *same matrix*, but remember that one of you is BLUE and one of you is RED.

Both subjects in the pair will *simultaneously* be asked to make a choice. BLUE will choose one of the two rows in the matrix, either 'Up' or 'Down', which we write as 'U' or 'D'. RED will choose one of the two columns, either 'Left' or 'Right', which we write as 'L' or 'R'. We refer to these choices as "actions". Notice that each pair of actions corresponds to one of the 4 cells of the matrix. For instance, if BLUE chooses 'U' and RED chooses 'L', this corresponds to the top-left cell, and similarly for the others.

Thus, depending on *both players' actions*, there are 4 possible outcomes:

- If BLUE chooses 'U' and RED chooses 'L', BLUE receives a payoff of 10, since that is the blue number in the UP–LEFT cell, and RED receives 20, since that is the RED number.

- If BLUE chooses 'D' and RED chooses 'R', BLUE receives a payoff of 11 and RED receives 75.
- And the other two cells UP–RIGHT and DOWN–LEFT are similar.

We reiterate: each number in the matrix is a payoff that might be received by one of the players, depending on *both* players' actions. Are there any questions?

In this section, you will play for 20 rounds and *1 of your rounds* will be chosen for your payment. This 1 round will be selected randomly for each subject, and the payment will depend on the actions taken in that round by you and the subject you were paired with. In the selected round, your payoff in the chosen cell denotes the probability with which you will receive \$10. For example, if you receive a payoff of 60, then for that round you would receive \$10 with 60% probability and \$0 otherwise.

Since every round has an equal chance of being selected for payment, and you do not know which will be selected, *it is in your best interest that you think carefully about all of your choices*.

During the experiment, no feedback will be provided about the other player's chosen action. Only *at the end of the experiment* will you get to see the round that was chosen for your payment and the actions taken by you and the player you were paired with in that round.

Before we begin the first section, you will answer 4 training questions to ensure you understand this payoff structure. In each of these 4 questions, you will be shown a matrix and told the actions chosen by both players. You will then be asked with what probability a particular player earns \$10 if this round were to be selected for payment. That is, you are being asked for their payoff in the appropriate cell. To answer, simply type the probability as a whole number into the box provided and click 'continue'. The page will only allow you to 'continue' when your answer is correct, at which point you may proceed to the next question. Please click ‘continue’ and answer the 4 training questions now.

[**SLIDE 2**]

Now that you’ve completed the training questions and understand the payoff matrices, we will proceed to Section 1. In each round of this section you will be randomly paired with another subject. If you are BLUE, you will be paired with a RED subject, and if you are RED, you will be paired with a BLUE subject. Recall that, at the start of each round, you will be randomly re-paired.

In each round, for each pair, the RED player’s task will be to select a column of the matrix, and the BLUE player’s task will be to select a row of the matrix, and these actions determine both players’ payoffs for the round.

[**SLIDE 3**]

You should now see an example round on the overhead. This shows the screen for a BLUE player, who is asked to choose between ‘U’ and ‘D’. Notice however that the text instructing you to make a choice is faded. This is because you must wait for 10 seconds before you are allowed to make a decision. Once 10 seconds has passed, the text will darken, indicating that you can now make a selection. The number of seconds remaining until you are able to choose is shown in the bottom right corner. Now the overhead shows what the screen will look like after the 10 seconds have passed.

[**SLIDE 4**]

The 10 seconds is a minimum time limit. There is no maximum time limit on your choices, and you should feel free to take as much time as you need, even after the 10 seconds has passed. In order to make your selection, simply click on the row or column of your choice. Once you have done so, your choice will be highlighted, and a ‘submit’ button will appear, as we now show on the overhead.

[SLIDE 5]

You may change your answer as many times as you like before submitting. If you would like to undo your choice, simply click again on the highlighted row or column. Once you are satisfied with your choice, click 'submit' to move on to the next round.

Before beginning the paid rounds of Section 1, we will play 4 practice rounds to familiarize you with the interface. These rounds will *not* be selected for payment. Are there any questions about the game, the rules, or the interface before we begin the practice rounds?

Please click 'continue' and begin the practice rounds now. You will notice that you have been assigned either RED or BLUE. This will be your color throughout the experiment. Please continue until you have completed the 4 practice rounds.

You have now completed the practice rounds, and we will proceed to the paid rounds of Section 1. Section 1 consists of 20 rounds, exactly like those you have just played. Recall that, in each round, you will be randomly paired with another subject and that one round will be randomly selected for payment. Are there any questions about the game, the rules, or the interface before we begin?

[SLIDE 6]

Please click 'continue' and play Section 1 now. The rules we discussed for Section 1 will be shown on the overhead as a reminder throughout.

[SLIDE 7]

We will now have a brief instruction period for Section 2, in which you will be familiarized with the types of rounds you will encounter.

If you have any questions during the instruction period, raise your hand and your question will be answered so everyone can hear. If any difficulties arise once play has begun, raise your hand, and an experimenter will come and assist you.

In this section, each round will be similar to those from Section 1. You will see some of the same matrices and your assignment of RED or BLUE will be the same as before.

Now, however, after being shown a matrix, your task will be to give your *belief* or *best guess* about the probability that a randomly selected subject chose a particular action when playing the same matrix in Section 1. That is, you will be shown a matrix, and the computer will randomly select a round from Section 1 in which the same matrix was played. Then,

- If you are RED, you will be asked for the probability that a randomly selected BLUE player chose ‘U’ in that round in Section 1.
- If you are BLUE, you will be asked for the probability that a randomly selected RED player chose ‘L’ in that round in Section 1.

As before, you will be paid for your responses. We will now describe this payment mechanism.

[SLIDE 8]

Consider first the matrix that is shown on the overhead. Please imagine that the computer has randomly selected a round from Section 1 in which this matrix was played. We wish to know your belief about the probability that a randomly selected RED player chose 'L' in that round. Please, take some time now to think carefully about what you believe this probability to be.

[SLIDE 9]

Consider the question that is now shown on the overhead, which asks which of the following you would prefer:

- Under Option A, you receive $5 if a randomly selected RED player chose 'L' in that round, and $0 otherwise.

- Under Option B, you receive \$5 with probability 75%, and \$0 otherwise.

Please think carefully about which of these two options you would prefer.

Presumably, if you believe the probability that a randomly selected RED player chose 'L' is greater than 75%, then you would prefer Option A, which you believe gives you the highest probability of a \$5 prize. For example, if you believe this probability is 89%, you would choose Option A since 89 is greater than 75.

If, on the other hand, you believe the probability that a randomly selected RED player chose 'L' is less than 75%, then you would prefer Option B, which you believe gives you the highest probability of a \$5 prize. For example, if you believe this probability is 22%, you would choose Option B since 22 is less than 75.

In this way, your answer to this question will tell us whether you believe this probability is greater than or less than 75%.

[**SLIDE 10**]

Now imagine we asked you 101 of these questions, with the probability in Option B ranging from 0% to 100%. Presumably you would answer each of these questions as described previously. That is, for questions for which the probability in Option B is *below* your belief, you would choose Option A, and for questions for which the probability in Option B is *above* your belief, you would choose Option B. Imagine, for example, you believe that there is a 64% probability that a randomly selected RED player chose 'L' in the selected round. Then, you would select Option A for all questions before #64, and Option B for all questions after #64. For Question #64, you could make either selection.

[**SLIDE 11**]

In this case, your selections would be as shown on the overhead, with the chosen options in black and the unchosen options in gray. From these answers, we could determine that you believe the probability that a randomly selected RED player chose 'L' is 64%.

In each round of this section, you will be faced with a table of 101 questions as shown on the overhead. To save time, instead of having you answer each question individually, we will simply ask you to type in your belief, and the answers to these 101 questions will be automatically filled out as above. That is, for rows of the table in which the probability in Option B is below your stated belief you will automatically select Option A, and for rows of the table in which the probability in Option B is at or above your stated belief you will automatically select Option B.

If this round is chosen for payment, one of the 101 rows of the table will be randomly selected and you will be paid according to your chosen option in that row. If you chose Option A in that row, a subject of the relevant color will be randomly chosen, and you will receive $5 if they played the relevant action in the selected round of Section 1. If you chose Option B in that row, you will receive $5 with the probability given in that option.

It is thus in your best interest, given your belief, to state your belief accurately. Otherwise, if you type something other than your belief, there will be rows of the table for which you will not be selecting the option that you believe gives you the highest probability of receiving a $5 prize.

In this section you will play 40 rounds, giving 40 such beliefs. At the end of the section, 2 rounds will be randomly chosen for payment. For each of these rounds, one of the 101 rows of the table will be randomly selected and you will be paid according to your chosen option in that row.

Are there any questions about this?

In addition to stating a belief, in each round you will also be asked to choose an action, as you did in Section 1. Now, however, the other action will not be determined by another subject acting simultaneously. Instead, recall that the computer has randomly selected a round from Section 1 featuring the matrix shown on your screen. The computer will also randomly select a player of the other color and record the action they took in that round. This is the action that you will be paired with. That is:

- If you are RED, the BLUE action will be that which a randomly selected BLUE player chose in the selected round of Section 1.
- If you are BLUE, the RED action will be that which a randomly selected RED player chose in the selected round of Section 1.

Again, the randomly selected round from Section 1 will feature the same matrix shown on your screen, so your payoff is determined as if you were paired with a randomly selected player from Section 1, rather than being paired with a player who chooses an action simultaneously.

As in Section 1, your payoff from taking an action gives the probability of earning $10 if the round is chosen for payment.

At the end of the section, 2 rounds will be randomly chosen for payments based on your actions. This is in addition to the 2 rounds randomly chosen for payments based on your beliefs. Moreover, the randomization algorithm that selects these rounds will ensure that all 4 rounds feature different matrices and that these matrices will be different from that selected for payment in Section 1. In particular, this means that if a round is selected for an action-payment, it cannot also be selected for a belief-payment and vice versa.

As before, since you do not know which round will be selected for payment, nor which type of payment it will be selected for, these payment procedures ensure that, in each round, *it is in your best interest to both state your belief accurately and choose the action that you think is best.*

[SLIDE 12]

You should now see an example round on the overhead. This shows the screen for a BLUE player. As in Section 1, you will see the matrix in the middle of the screen. At the top of the screen, you are told that the computer has randomly selected a round of Section 1 in which this matrix was played.

Below this, the instructions are shown, and are again faded for 10 seconds. Once 10 seconds has passed, the text asking you for your belief will darken as now shown on the overhead.

[**SLIDE 13**]

You will not be able to select an action until after you have entered your belief.

Once you have entered your belief, the resulting probabilities will appear below or beside the matrix and the text asking you to select your action will darken, as now shown on the overhead.

[**SLIDE 14**]

Your belief must be a whole number between 0 and 100 inclusive. Once you enter your belief, we will automatically 'fill out' the questions in the 101 rows based on your belief as previously described. If you wish, at any time you may scroll down to observe the 101 rows.

As in Section 1, once you have selected an action, it will be highlighted on the matrix, as now shown on the overhead.

[**SLIDE 15**]

At this point, you may freely modify both your belief and action as many times as you wish before pressing 'submit'. Remember that there is no upper time limit on your choices, and you should feel free to take as much time as you need, even after the minimum 10 seconds has passed.

Before beginning the paid rounds of Section 2, we will play 3 practice rounds to familiarize you with the interface. These rounds feature the same matrices as the practice rounds from Section 1, and will not be selected for payment. Are there any questions about the game, the rules, or the interface before we begin the practice rounds?

Please click 'continue' to be taken to the first practice round now. Recall that your belief must be a whole number between 0 and 100 inclusive, and at any time you may scroll down to see the table of 101 questions. Please continue until you have completed the 3 practice rounds.

You've now completed the practice rounds, and we will proceed to the paid rounds of Section 2.

[**SLIDE 16**]

Recall that Section 2 consists of 40 rounds, exactly like those you have just played. 4 rounds will be randomly selected for payment–2 rounds for beliefs and 2 rounds for your actions. Again, these 4 rounds will feature different matrices to each other and to the matrix selected for payment in Section 1. The payment procedures ensure that *it is always in your best interest to both state your belief accurately and choose the action that you think is best*. Unlike Section 1, Section 2 will be played at your own pace without waiting for other subjects between rounds. Once you have completed Section 2, please remain seated quietly until all subjects have finished.

Are there any questions about the game, the rules, or the interface? If you have any questions during the remainder of the experiment, raise your hand, and an experimenter will come and assist you. You may click 'continue' and play Section 2 now. The rules we discussed for Section 2 will be shown on the overhead as a reminder throughout.

You have now completed the experiment. All that remains is to organize payments. To do this, you will be shown a page with all of your randomly selected rounds and your earnings in each. This page will also show you how to fill out the payment receipt at your desks. Before reaching this page, you will see an explanation page describing how the results are determined and how to read them. You may click ‘continue’ now and read through the explanation page. Then continue to the payments page, where you will see your results and fill out your receipt.

C.2 Experimental Overheads

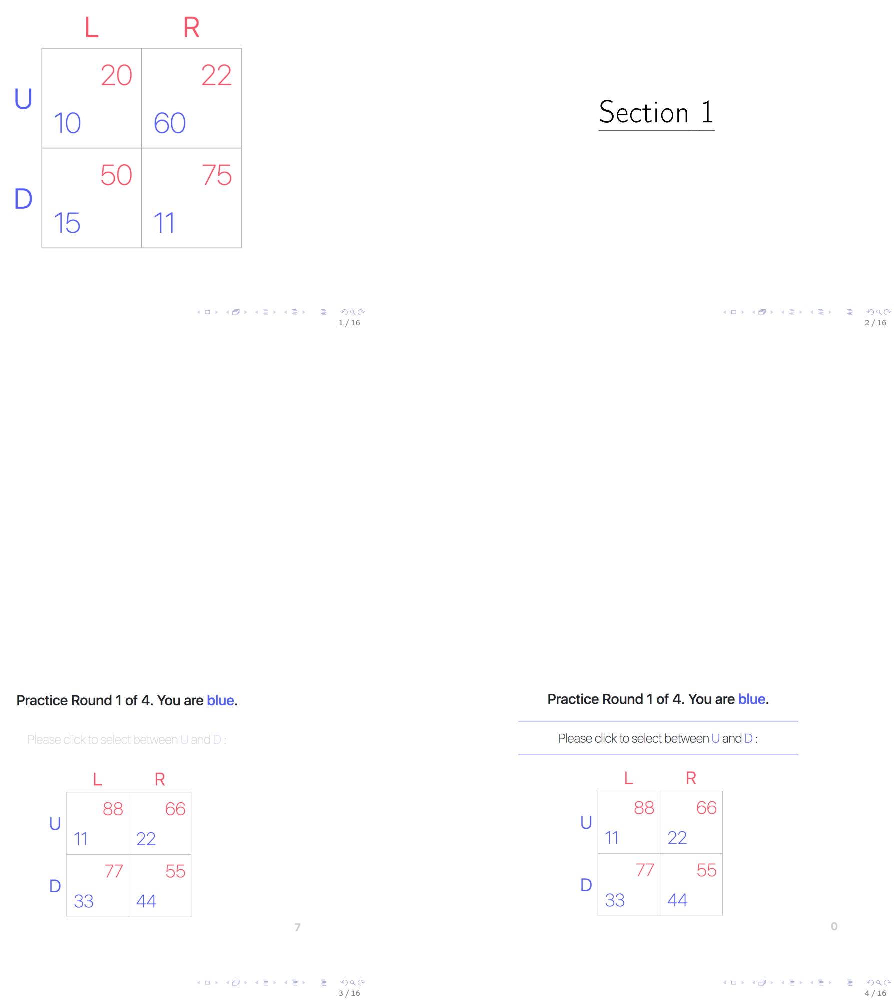
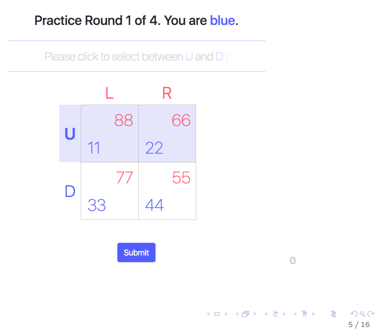
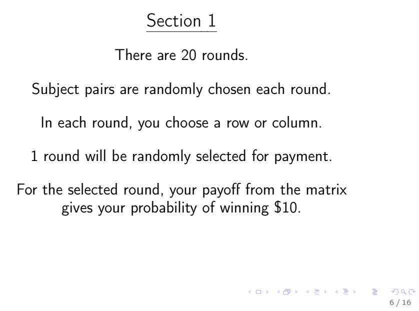
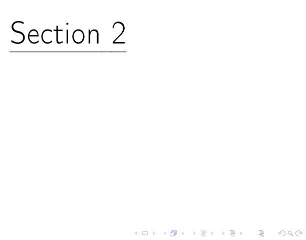


Figure C.1: Experimental Overheads - Slides 1 to 4







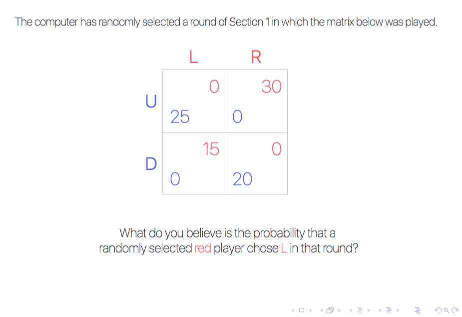


Figure C.2: Experimental Overheads - Slides 5 to 8

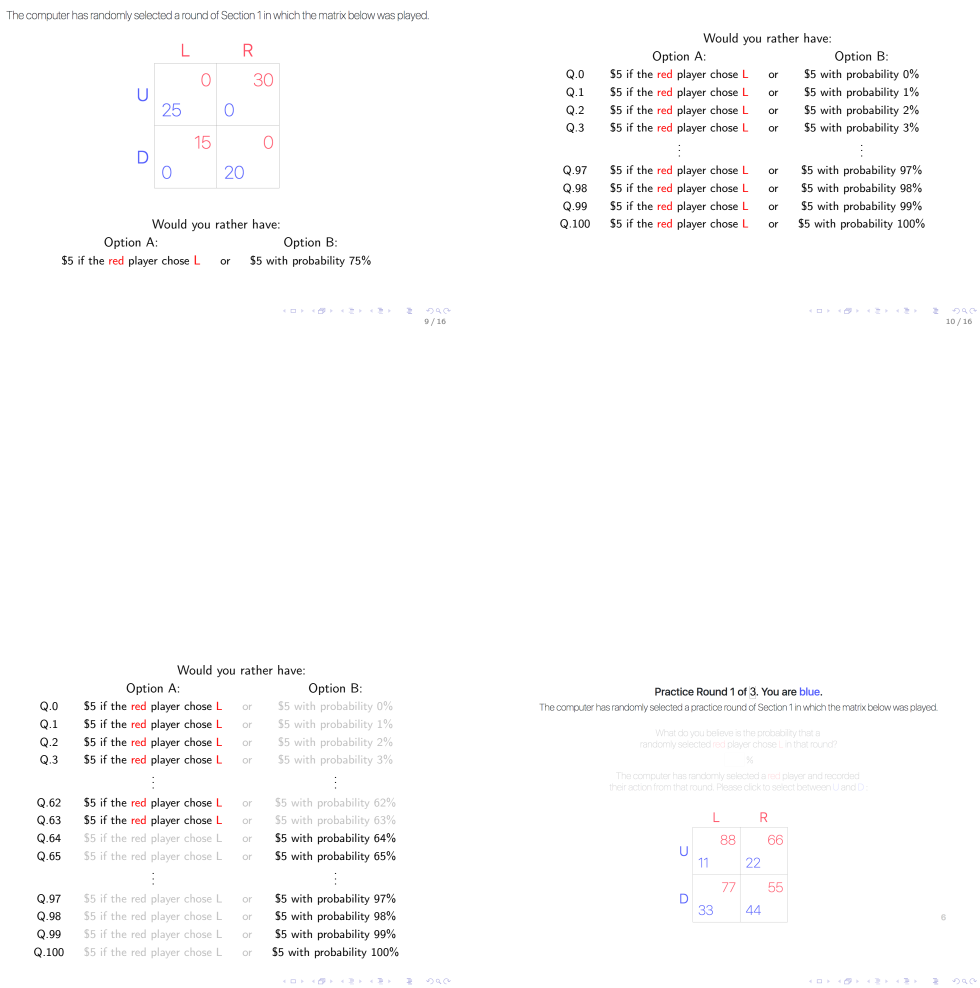
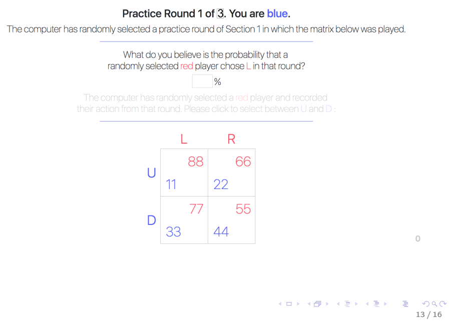
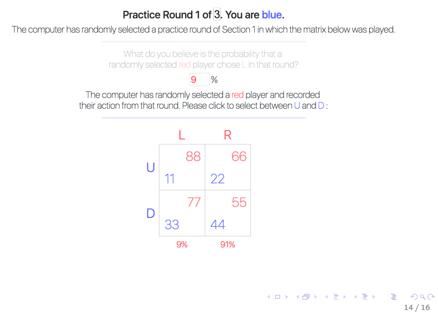
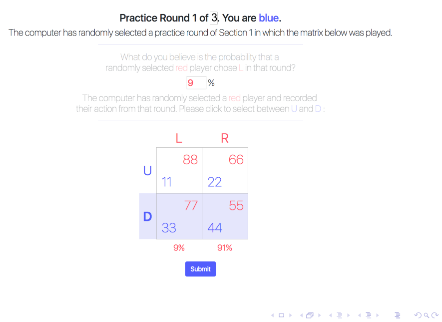


Figure C.3: Experimental Overheads - Slides 9 to 12







Section 2

There are 40 rounds.

In each round, the computer will randomly select a round of Section 1 with the same matrix as shown on your screen:

- *State your belief* about the probability of a randomly selected player's action in that round.
- *Take an action*–this will be paired against the action of a randomly selected player in that round.

4 rounds will be randomly selected for payment:

- 2 rounds for beliefs
- 2 rounds for actions

16 / 16

Figure C.4: Experimental Overheads - Slides 13 to 16

C.3 Proofs

Lemma 1. *In game X:*

(i) $p_{NBE}, p_{QRE} \in (\frac{1}{2}, 1)$ *for* $q_{NE} < \frac{1}{2}$; $p_{NBE}, p_{QRE} \in (0, \frac{1}{2})$ *for* $q_{NE} > \frac{1}{2}$.

(ii) $q_{NBE}, q_{QRE} \in (q_{NE}, \frac{1}{2})$ *for* $q_{NE} < \frac{1}{2}$; $q_{NBE}, q_{QRE} \in (\frac{1}{2}, q_{NE})$ *for* $q_{NE} > \frac{1}{2}$.

(iii) p_{NBE}, p_{QRE} *are strictly decreasing in* $q_{NE} \in (0, 1)$.

(iv) q_{NBE}, q_{QRE} *are strictly increasing in* $q_{NE} \in (0, 1)$.

Proof. In an NBE, p and q solve

$$\begin{aligned} p = \Psi_U(\underset{+}{q}; \underset{-}{q_{NE}}) \equiv & 1 - F^1(q_{NE}|q) \\ q = \Psi_L(\underset{-}{p}) \equiv & F^2(\frac{1}{2}|p). \end{aligned} \tag{C.1}$$

In a QRE, p and q solve

$$\begin{aligned} p =& Q_U(\underset{+}{q}X, (1-\underset{-}{q})20) \\ q =& Q_L((1-\underset{+}{p})20, \underset{-}{p}20). \end{aligned} \tag{C.2}$$

From (B3)/(B4) and (A3)/(A4), respectively, it is easy to show that all NBE/QRE satisfy:

$$\begin{cases} p < (=)\frac{1}{2} & \text{if } q < (=) q_{NE} \\ p > (=)\frac{1}{2} & \text{if } q > (=) q_{NE} \end{cases} \text{ and } \begin{cases} q > (=)\frac{1}{2} & \text{if } p < (=)\frac{1}{2} \\ q < (=)\frac{1}{2} & \text{if } p > (=)\frac{1}{2}. \end{cases}$$

This shows *(i)* and *(ii)*. Suppose q_{NE} increases. From (Ψ_U, Ψ_L), as q_{NE} increases, it must be that either p decreases and q increases, p increases and q decreases, or that both p and q remain constant. The latter two cases are impossible since Ψ_U implies that as q_{NE} increases, p decreases if q is constant or decreases. Thus, as q_{NE} increases, p must strictly increase and q must strictly decrease. From (Q_U, Q_L), as q_{NE} increases, or equivalently, as X decreases, it must be that either p decreases and q increases, p

increases and q decreases, or that both p and q remain constant. The latter two cases are impossible since Q_U implies that as X decreases, p decreases if q is constant or decreases. Thus, as q_{NE} increases, p must strictly increase and q must strictly decrease. This shows *(iii)* and *(iv)*. $\square$

Lemma 2. *In game X:*

(i) If NBE is label invariant, q_{NBE} *are* p_{NBE} *are symmetric about* $q_{NE} = \frac{1}{2}$.

(ii) If QRE is translation invariant and label invariant, q_{QRE} *and* p_{QRE} *are* not *symmetric about* $q_{NE} = \frac{1}{2}$.

Proof. Fix game X with $q_{NE} = \frac{20}{X+20}$. If $\{p, q\}$ is an NBE, it solves (C.1). If F^1 and F^2 are label invariant (see footnote 10), then $F^1(q_{NE}|q) = 1 - F^1(1 - q_{NE}|1 - q)$ and $F^2(\frac{1}{2}|p) = 1 - F^1(\frac{1}{2}|1 - p)$, and therefore $\{1 - p, 1 - q\}$ is an NBE for game X with $q'_{NE} = 1 - q_{NE}$. This shows *(i)*. Fix game X with $q_{NE} = \frac{20}{X+20}$. If $Q = (Q_U, Q_L)$ is translation invariant, then quantal response depends only on the expected payoff *differences* between actions and the QRE $\{p, q\}$ can be rewritten as the solution to

$$p = Q_U\underbrace{(20\frac{q}{q_{NE}} - 20)}_{+}$$

$$q = Q_L(\underbrace{20 - 40p}_{+}).$$

If Q_U is label invariant, the QRE will be symmetric about q_{NE} only if $20\frac{q}{q_{NE}} - 20 = -(20\frac{1-q}{1-q_{NE}} - 20)$, but it is easy to show that this has no solution in q for any $q_{NE} \neq \frac{1}{2}$. This shows *(ii)*. $\square$

C.4 Experimental Interface

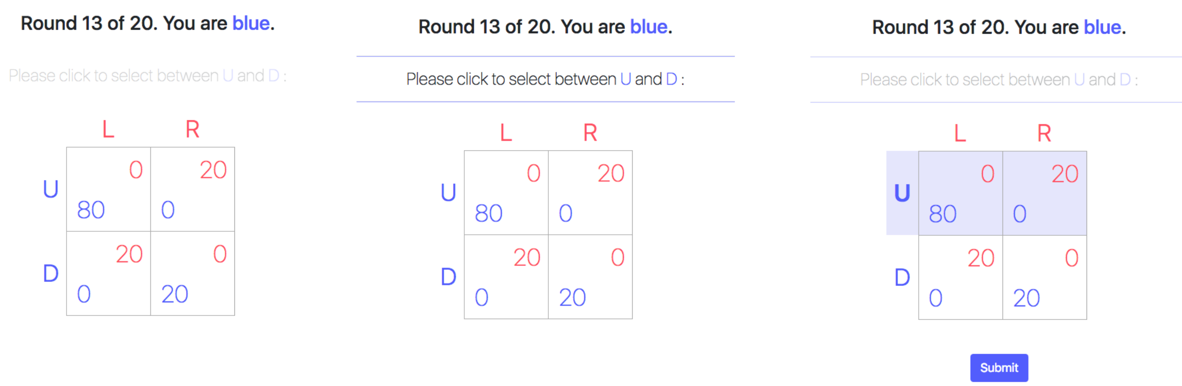


Figure C.5: Screenshots from Section A

This figure shows an example round from the perspective of a player 1-subject (blue). At the start of the round, the subject sees the payoff matrix (left screen), and a 10 second timer counting down to 0 (not shown here) is seen at the bottom right corner of the screen. After 10 seconds pass, the text "Please click to select between U and D:" darkens (middle screen) indicating that the subject may take an action. To select an action, the subject clicks on a row of the matrix. The row becomes highlighted and a 'Submit' button appears (right screen). At this point, the subject may freely modify his answer before submitting. The subject may undo his action choice by clicking again on the highlighted row.

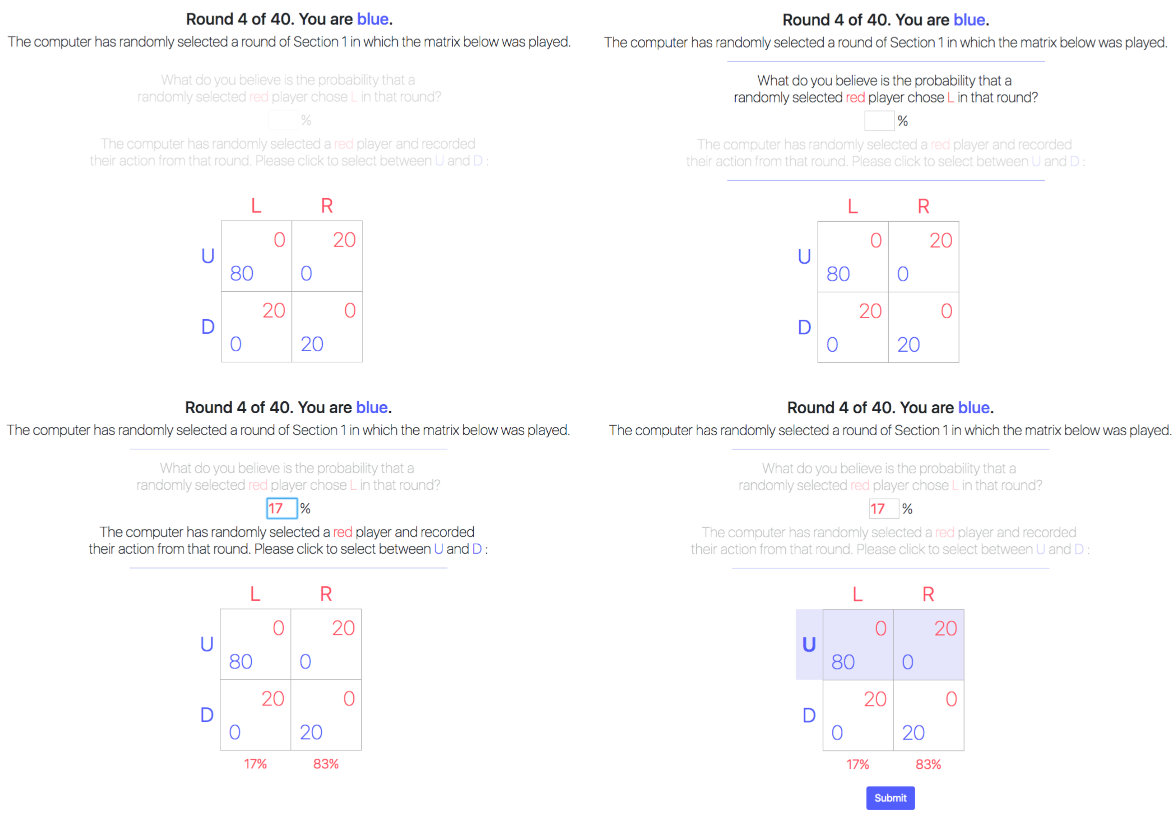


Figure C.6: Screenshots from Section BA

This figure shows an example round from the perspective of a player 1-subject (blue). At the start of the round, the subject sees the payoff matrix (top-left screen) and is told “The computer has randomly selected a round of Section 1 in which the below matrix was played.” After 10 seconds pass, the text “What do you believe is the probability that a randomly selected red player chose L in that round?” darkens (top-right screen) indicating that the subject may state a belief. The subject enters a belief as a whole number between 0 and 100. Once the belief is entered, the corresponding probabilities appear below the matrix and the text “The computer has randomly selected a red player and recorded their action from that round. Please click to select between U and D:” darkens (bottom-left screen) indicating that the subject may take an action. Only after stating a belief may the subject select an action, but after the belief is stated, the subject may freely modify both his belief and action before submitting. After a belief is entered and an action is selected, the ’Submit’ button appears (bottom-right screen).

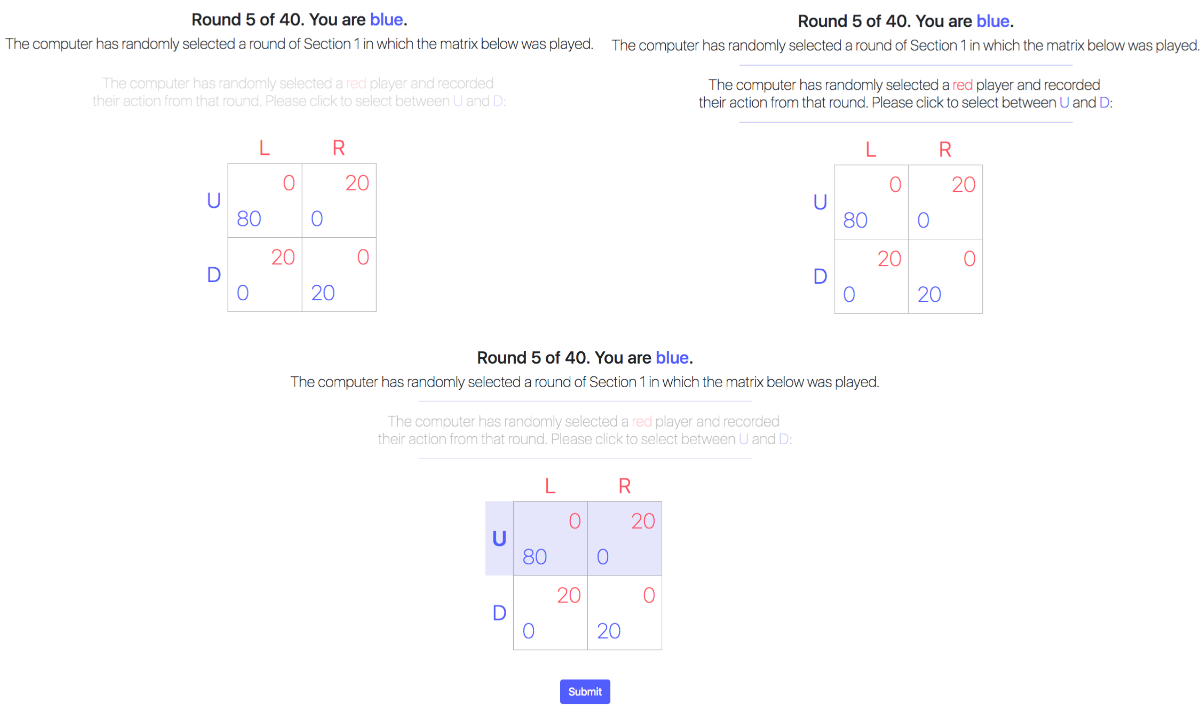


Figure C.7: Screenshots from A-A treatment
The first section of the A-A session is identical to that of the A-BA session. The second section of the A-A session is the same as that of the A-BA session, except beliefs are not elicited.

C.5 Mean Bias

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1
beliefs - actions	26.280***	27.996***	-16.473***	-18.563***	-25.750***	-18.618***
	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.001)
Observations	410	410	410	410	410	410

p-values in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table C.1: Player 2's beliefs versus player 1's actions
We report t-tests of the differences between the mean belief and the action frequency for each game. Standard errors are clustered by subject.

	(1) X80	(2) X40	(3) X10	(4) X5	(5) X2	(6) X1
beliefs - actions	5.518	10.249**	-7.312	-16.130***	-10.971**	-10.201*
	(0.306)	(0.040)	(0.193)	(0.001)	(0.045)	(0.064)
Observations	404	404	404	404	404	404

p-values in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table C.2: Player 1's beliefs versus player 2's actions
We report t-tests of the differences between the mean belief and the action frequency for each game. Standard errors are clustered by subject.

C.5.1 Individual Subjects' Data

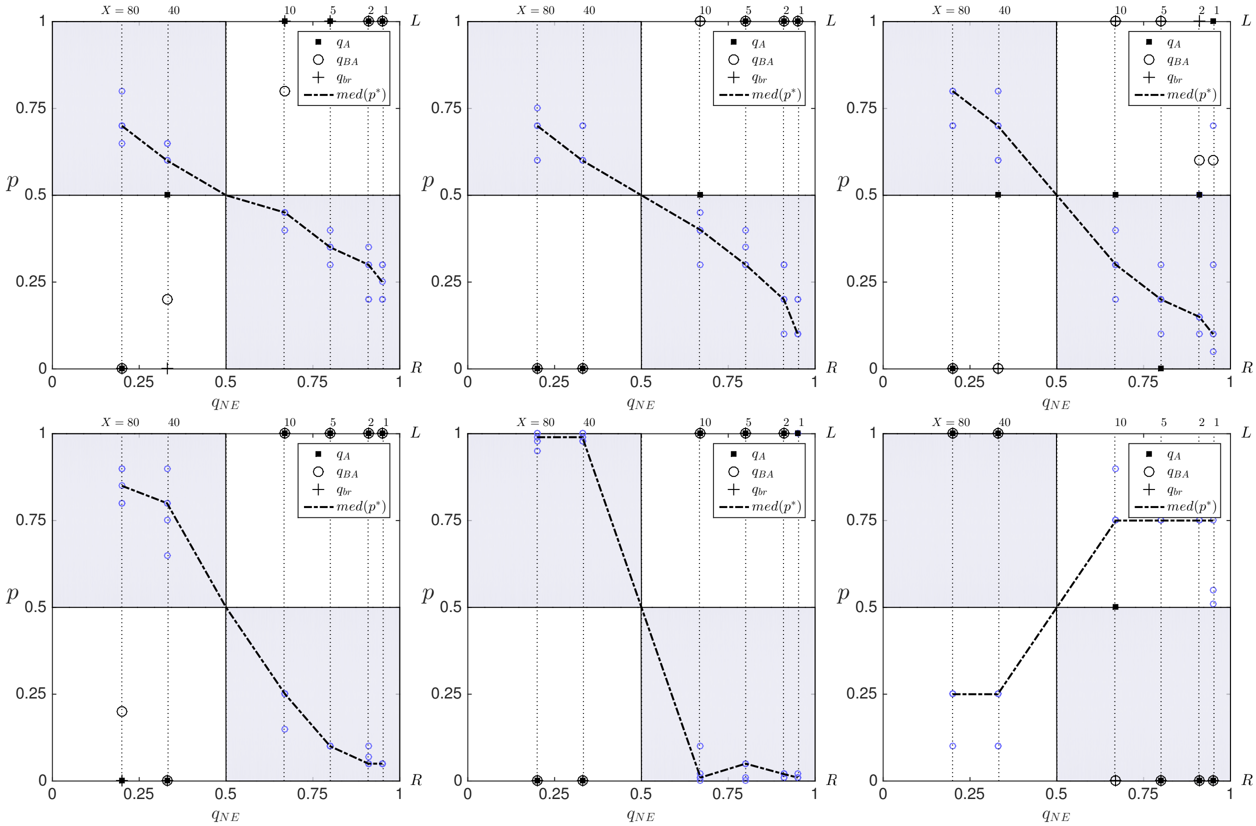


Figure C.8: Individual Subjects: Player 2's Beliefs and Actions.

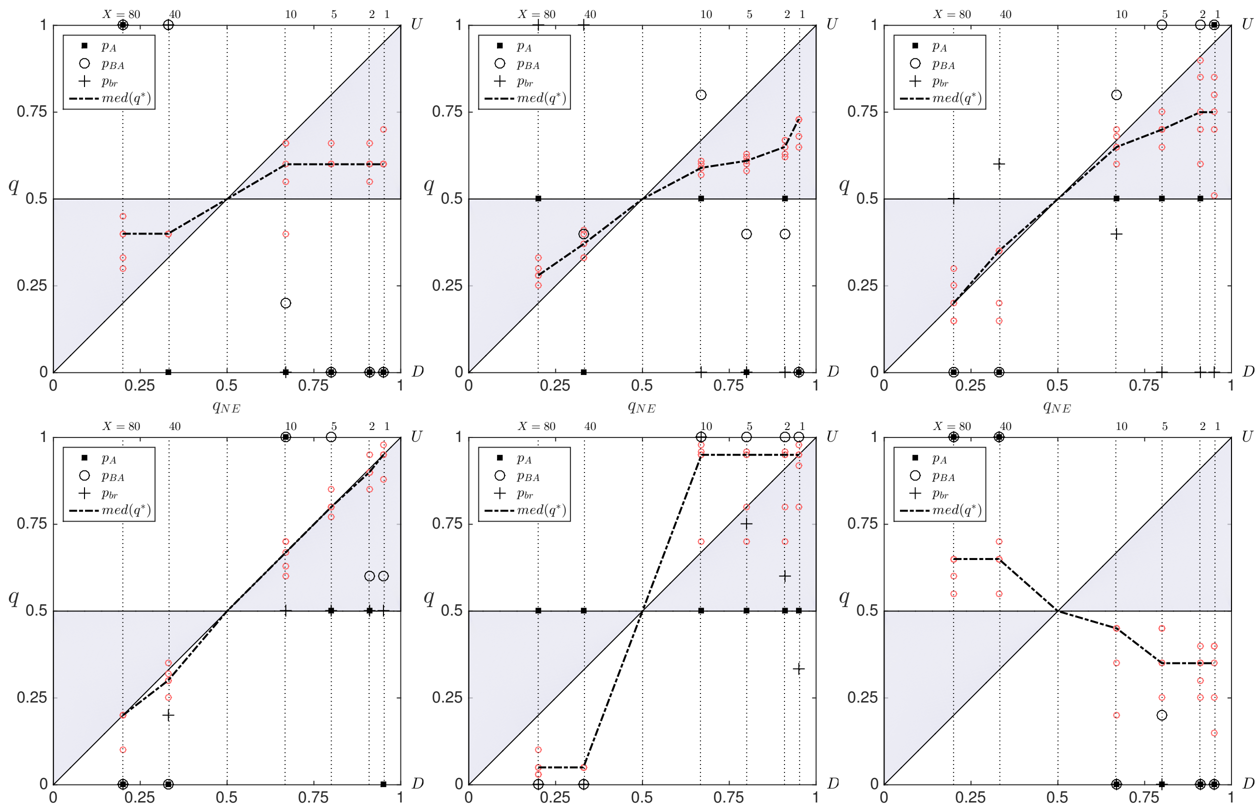


Figure C.9: Individual Subjects: Player 1's Beliefs and Actions.