Title
Cognition and Behavior in Normal-Form Games: An Experimental Study

Authors
Costa-Gomes, Miguel
Crawford, Vincent P.
Broseta, Bruno

Publication Date
1998-09-01

98-22

UNIVERSITY OF CALIFORNIA, SAN DIEGO

DEPARTMENT OF ECONOMICS

COGNITION AND BEHAVIOR IN NORMAL-FORM GAMES: AN EXPERIMENTAL STUDY

BY

MIGUEL COSTA-GOMES

VINCENT CRAWFORD

AND

BRUNO BROSETA

DISCUSSION PAPER 98-22
SEPTEMBER 1998

COGNITION AND BEHAVIOR IN NORMAL-FORM GAMES: AN EXPERIMENTAL STUDY[1]

Miguel Costa-Gomes (University of California, San Diego, and Harvard Business School)

Vincent Crawford (University of California, San Diego)

Bruno Broseta (University of Arizona)

October 12, 1998

This paper reports experiments designed to measure *strategic sophistication*, the extent to which players' behavior reflects attempts to predict others' decisions, taking their incentives into account. Subjects played normal-form games with various patterns of iterated dominance and unique pure-strategy equilibria without dominance, using a computer interface that allowed them to look up hidden payoffs as often as desired, one at a time, while automatically recording their look-ups. Monitoring information search allows tests of game theory's implications for cognition as well as decisions, and subjects' deviations from search patterns suggested by equilibrium analysis help to predict their deviations from equilibrium decisions.

Keywords: noncooperative games, experimental economics, strategic sophistication, cognition (*JEL* C72, C92, C51)

1. Introduction

[1]Thanks are due Colin Camerer, Aaron Cicourel, John Conlisk, Daniel Friedman, David Grether, Eric Johnson, Mark Machina, Amnon Rapoport, Stanley Reynolds, Alvin Roth, Larry Samuelson, Jason Shachat, Joel Sobel, and Dale Stahl for helpful advice; to Mary Francis Luce for software; to Bill Janss and Dirk Tischer for research assistance; and to the National Science Foundation (Costa-Gomes, Crawford, and Broseta), the Russell Sage Foundation and the University of California, San Diego (Costa-Gomes and Crawford), the Alfred P. Sloan Foundation, the Banco de Portugal, the Luso-American Development Foundation, and the Fundacao da Ciencia e Tecnologia (Costa-Gomes), the John Simon Guggenheim Memorial Foundation (Crawford), and the University of Arizona (Broseta) for research support. We are grateful for the assistance of the University of Arizona's Economic Science Laboratory, where we ran the experiments.

Recent years have seen great progress in the analysis of strategic interaction, much of it fueled by a dialog between game theory and economics. To date this dialog has consisted mainly of conversations among theorists, with introspection and casual empiricism the main sources of information about behavior. Although this approach has plainly been productive, it has also revealed the limits of what can be learned by theory alone. Purely theoretical analyses of strategic behavior—traditional or adaptive—yield specific predictions only under strong assumptions. Those assumptions are reasonable for some applications but potentially misleading for others, and most applications raise questions about the principles that govern strategic behavior that are not adequately resolved by theory. Further progress is likely to require systematic empirical work.

Many unresolved questions about strategic behavior concern the extent to which it reflects players' analysis of their environment as a game, taking the structure and other players' incentives and likely responses into account. This notion, which we shall call *strategic sophistication*, is a multidimensional concept, which takes different forms in different settings. In games in which other players are likely to play according to a given equilibrium, sophistication requires only that a player identify and play his part of that equilibrium, but in other games a sophisticated player may need to anticipate other players' deviations from equilibrium. Thus, in general, sophistication may require at least an implicit understanding of equilibrium analysis and its predictive success in games with different strategic structures.[2] Sophistication is the main difference between the behavioral assumptions of the leading alternative theories of strategic behavior: Traditional noncooperative and cooperative game theory take it to be unlimited, while evolutionary game theory and adaptive learning models take it to be nonexistent or severely limited. These diametrically opposed assumptions about sophistication highlight the need for empirical work on this issue.[3]

In this paper we study sophistication in experiments designed to test the predictions of noncooperative game theory in normal-form games with various patterns of dominance, iterated dominance, and unique pure-strategy equilibria without dominance. Experiments have two advantages for this purpose. They allow the control needed to test game-theoretic predictions, which are

[2]In dominance-solvable games, for instance, sophistication requires an understanding of the extent to which other players' beliefs and decisions reflect iterated dominance. In other games it may require an understanding of the principles that influence others' beliefs and their effectiveness in coordinating behavior as required for equilibrium.
[3]The importance of sophistication is sometimes downplayed on the grounds that learning can lead even unsophisticated players to converge to an equilibrium. However, sophistication is still likely to have important influences on convergence and limiting outcomes through its effects on players' initial beliefs and the structures of their learning rules.

notoriously sensitive to details of the environment.[4] And—although sophistication is an aspect of cognition, which it is often assumed can only be studied indirectly, by inference from the model that best describes players' decisions—experimental techniques developed by Camerer et al. (1993) and Johnson et al. (1998) ("C&J" below) make it possible to study sophistication more directly.

C&J's designs are based on two-person alternating-offers bargaining games in which the size of the "pie" to be divided varies across periods to simulate discounting at a common rate. With complete information these games have unique subgame-perfect equilibria, easily computed by backward induction. Yet previous experiments have yielded large, systematic deviations from the subgame-perfect equilibrium offer and acceptance decisions. These deviations have usually been attributed to subjects' lack of sophistication, to a failure to maximize monetary payoffs, or both.

C&J studied the cognitive underpinnings of these results by presenting three-period alternating-offers bargaining games in a way that allowed them to observe subjects' searches for hidden payoff information. Subjects played a series of such games, with different partners and pie sizes each period. Each game was presented to subjects in extensive form, as a sequence of pie sizes and decisions, using a computer interface called Mouselab, which normally concealed the pie sizes but allowed subjects to look them up as often as desired, one at a time, automatically recording their look-up sequences along with their decisions.[5] The rest of the structure was publicly announced, so that with their unrestricted access to the pie sizes subjects could evaluate their own and their partners' payoffs for any decision combination. This made the structure *public knowledge*, coming as close as possible to common knowledge in the laboratory. Varying the games and pairings allowed C&J to elicit subjects' initial responses to well defined games, each played as if in isolation; to focus sharply on subjects' strategic thinking by making it hard for them to learn about their partners' likely responses from previous plays; and to maintain control over subjects' information searches by making it impossible for them to remember current pie sizes from earlier plays.

---

[4]There is a growing experimental literature that studies the principles that govern strategic behavior, surveyed in Kagel and Roth (1995) and Crawford (1997). See, among others, Beard and Beil (1994), Brandts and Holt (1993), Cachon and Camerer (1996), Camerer and Ho (1998), Cooper, DeJong, Forsythe, and Ross (1990, 1994), Friedman (1996), Ho and Weigelt (1996), Ho, Camerer, and Weigelt (1998), McKelvey and Palfrey (1992), Nagel (1995), Palfrey and Rosenthal (1994), Roth (1987), Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991), Schotter, Weigelt, and Wilson (1994), Selten (1998), Stahl (1996), Stahl and Wilson (1995), Straub (1995), and Van Huyck, Battalio, and Beil (1990, 1991, 1993).

[5]Subjects were not allowed to record the pie sizes, and the frequencies with which they looked up payoffs repeatedly made clear that they did not memorize them. Mouselab can be viewed as an automated way of doing "eye-movement" studies like those used to study cognition via information search in experimental psychology.

C&J argued that in their design backward induction has a characteristic information search pattern, in which subjects first look up the last-period pie size, then the second-last (possibly re-checking the last), and so on, with most transitions from later to earlier periods. They supported this plausible claim empirically by showing that a control group, trained in backward induction but not in information search, and rewarded only for correctly identifying their subgame-perfect equilibrium decisions, came to exhibit just such a search pattern while succeeding in their task. By contrast, C&J's baseline subjects, who were untrained and were rewarded according to their payoffs playing the games with each other, deviated systematically from both the backward-induction search pattern and their subgame-perfect equilibrium decisions, with subjects whose search patterns were closer to backward induction tending to make and/or accept offers closer to the subgame-perfect equilibrium. These results add a valuable cognitive dimension to the evidence from alternating-offers bargaining games, which is helpful in discriminating among alternative explanations of subjects' decisions.

Our experiments adapt C&J's methods to study cognition via information search in two-person normal-form games, using Mouselab to present games as payoff tables in which subjects can look up their own and their partners' payoffs for each decision combination, as often as desired, but only one at a time.[6] Each subject faces a series of games with different partners, payoffs, and strategic structures, so that subjects' decisions and information searches can reveal their strategic thinking in initial responses to well defined games. Our goals are to use game theory's cognitive implications to conduct more powerful tests of the theory, to give a more precise classification of subjects by the rules that govern their decisions in games with a variety of strategic structures, and to learn whether subjects' deviations from the information search implications of equilibrium analysis help to predict the occurrence and nature of their deviations from equilibrium decisions.

Studying cognition via information search in normal-form games is a powerful complement to C&J's extensive-form analysis. There are close connections between game-theoretic analyses of decisions in extensive- and normal-form games, but their cognitive foundations are very different. While C&J's subjects searched for three pie sizes arrayed along one dimension, in our simplest (2x2) games subjects search for eight payoffs in patterns that can vary in several dimensions. We argue below that the leading theories of strategic behavior have different implications for subjects' information search

[6]The only precedent of which we are aware is Algaze [Croson] (1990), who briefly discussed the results of two trials using a normal-form Mouselab design apparently quite close to the one we independently developed.

patterns in our design, so that those patterns can reveal a great deal about their strategic thinking. This allows us to study several aspects of sophistication that do not come into play in alternating-offers bargaining games.

Our results can be summarized as follows. We find considerable heterogeneity in subjects' decision rules, as in Stahl and Wilson (1995), with still more heterogeneity in their information searches. As in previous experiments (Crawford (1997, Section 4), compliance with equilibrium is high in games that can be solved by one or two rounds of iterated dominance; but compliance is much lower in our more complex games in which it depends on three rounds of iterated dominance or the circular logic of pure-strategy equilibrium without dominance. Most of our subjects reveal some strategic sophistication by their decisions, and many even seem to have anticipated the pattern of noncompliance in more complex games; but sophistication is neither extensive nor widespread enough to justify full reliance on equilibrium analysis. Our analysis of subjects' information search patterns generally confirms the interpretation of their behavior suggested by their decisions, but there are systematic relationships between subjects' deviations from the search patterns suggested by equilibrium analysis and their deviations from equilibrium decisions, which allow better estimates of their decision rules and predictions of their decisions than an analysis based on decisions alone.

The rest of the paper is organized as follows. Section 2 reviews the traditional theory of noncooperative games and the leading alternative theories we compare it with. Section 3 introduces our design, discusses the use of Mouselab to present games in normal form, and describes the games we study. Section 4 conducts a preliminary econometric analysis of subjects' decisions alone, using a maximum likelihood error-rate model in the style of El-Gamal and Grether (1995) and Harless and Camerer (1994, 1995) to classify each subject by the theory of strategic behavior, or *decision rule*, that best describes his decisions over the series of games he played. Section 5 introduces the model of cognition and information search we use to derive the implications of alternative decision rules for subjects' look-up patterns. Section 6 generalizes Section 4's analysis by conditioning the error rates on the degree of compliance with the information search implications of alternative decision rules, reevaluates Section 4's estimates of subjects' decision rules, and assesses the extent to which observing information search helps to predict decisions. Section 7 is the conclusion.

2. Noncooperative Game Theory and Leading Alternative Theories

This section reviews the relevant parts of noncooperative game theory and the leading alternative theories of strategic behavior with which it will be compared, focusing on two-person normal-form games of complete information like those studied in our experiments.

The starting point of a noncooperative analysis is the *structure* of the game, which consists of its players, their sets of feasible *strategies* or *decisions*, and their payoffs for each combination of decisions.[7] We assume that the structure is *common knowledge* in that all players know it, all know that all know it, and so on ad infinitum. The players are assumed to be *rational* in the standard decision-theoretic sense that their expectations about uncertain events, including other players' decisions, can be represented by probability distributions called *beliefs*; and their preferences over uncertain outcomes can be described by assigning *payoffs* to each possible outcome so that each player's decision maximizes his expected payoff, given his beliefs. Rationality is assumed to be *mutual knowledge* in the sense that all players know it, and this is sometimes strengthened to higher levels of knowledge, knowledge about knowledge, etc., up to common knowledge.

A player's decision *dominates* (respectively, *is dominated by*) another of his decisions if it yields a strictly higher (respectively, lower) payoff for any of the other player's decisions. A decision is *iteratively undominated* if it survives iterated elimination of dominated decisions. A game is *dominance-solvable* if it has a unique iteratively undominated decision combination. A rational player plainly never chooses a dominated decision, but beyond this, rationality—even if common knowledge—yields only weak restrictions on behavior. Assuming common knowledge of the structure, common knowledge of rationality implies that each player must choose an iteratively undominated decision.[8] This yields unique predictions in dominance-solvable games, but not other games; and in many games common knowledge of rationality yields no behavioral restrictions at all.

To derive restrictions on behavior in non-dominance-solvable games, rationality is often supplemented by the assumption that players' decisions are in *Nash equilibrium*, in that each player's decision maximizes his expected payoff, given the others' decisions. The iteratively undominated

---

[7]Brandenburger (1992) and Aumann and Brandenburger (1995) discuss the theory outlined here in more detail. We use "decision" and "strategy" interchangeably because we avoid games with mixed-strategy equilibria, and the distinction is otherwise irrelevant for normal-form games. In some theories of strategic behavior (and in many real environments) players' decisions are also influenced by *contextual* factors that are not part of the structure as game theorists define it, such as how the game is presented or the social setting (Crawford (1997, Section 2)). Here we follow traditional game theory in focusing on structural factors, using a design that suppresses contextual effects as much as possible.

[8]Here, common knowledge of rationality can be weakened to approximate common knowledge, or to mutual knowledge up to the number of rounds of iterated elimination of dominated decisions the game allows; but it is otherwise necessary.

decisions in a dominance-solvable game are necessarily in equilibrium, but in non-dominance-solvable games equilibrium depends on more than rationality, or even common knowledge of rationality. The weakest general sufficient conditions for pure-strategy equilibrium require both rationality and mutual knowledge of players' decisions (Brandenburger (1992, Proposition 2), Aumann and Brandenburger (1995, Section 4, Preliminary Observation)).

The mutual knowledge of players' decisions required by this rationale for equilibrium can be justified either deductively or inductively. From the deductive point of view, common knowledge of rationality is all that is needed if the game is dominance-solvable, but in non-dominance-solvable games the deductive rationale for equilibrium may require common knowledge of a complete theory of strategic behavior that always yields unique predictions, such as Harsanyi and Selten's (1988) general theory of equilibrium selection.[9] In general this involves an extreme form of sophistication, in which players have complete models of the structure and of other players' decision processes, with the latter models taking into account other players' models of their own decision processes.

From the inductive point of view, mutual knowledge of players' decisions can emerge as the limiting outcome of a learning process, in which players suppose that other players' decisions in previous plays of analogous games are representative of their decisions in the current game. Inductive rationales for equilibrium can be cognitively less demanding than deductive rationales because players may avoid the need to model other players' decision processes by basing their beliefs on direct observations of others' decisions.[10] Learning models vary greatly in their assumptions about sophistication, ranging from reinforcement learning (Roth and Erev (1995)), in which players need not even know that they are playing a game, to models in which players have complete models of the structure but simplified models of others' decisions (Fudenberg and Kreps (1993), Crawford (1995), Crawford and Broseta (1998), Ho, Camerer, and Weigelt (1998), Camerer and Ho (1998)) and models whose cognitive requirements approach those of the deductive rationale for equilibrium (Kalai and Lehrer (1993), Stahl (1996)). Even the least sophisticated among these learning models have a strong tendency to converge to equilibrium in many environments.

[9]The theory must be *common* knowledge to make players' decisions *mutual* knowledge because otherwise a player might doubt whether the others know that he knows the theory, or know that he knows that they know it, etc. This is also why common knowledge of rationality is needed to support the analogous deduction in dominance-solvable games.
[10]Inductive players do need to decide which games are analogous, and how to translate their experience to the current game; except in theoretical models, which tend to trivialize these issues, this may require considerable sophistication.

The fact that unsophisticated learning can mimic sophisticated deduction in this way has important implications for our design. In general sophistication influences both subjects' initial responses to games and the form of their learning rules. The level of sophistication can sometimes be inferred from learning rules (e.g. Shachat and Walker (1997)), but it appears in its purest form in initial responses. Our experiments seek to measure sophistication by eliciting subjects' initial responses to a series of games with different partners and strategic structures, suppressing learning and repeated-game effects as much as possible. The deductive rationale for equilibrium is therefore the relevant one for our purposes. Studying sophistication this way is of considerable interest in its own right, and should yield results that complement analyses of sophistication via learning rules.

One of our main goals is to search for useful relationships between subjects' deviations from the information searches suggested by equilibrium analysis and their deviations from equilibrium decisions. We structure our search for such relationships by comparing the cognitive requirements of equilibrium analysis with those of alternative theories of strategic behavior, limiting attention to a total of six decision rules, or *strategic types*. We chose these types, before our first pilots were run, for the important roles they have played in the theoretical literature and the clarity and separation of their cognitive implications. They allow us below to describe our subjects' information searches and decisions in a comprehensible way, without overfitting or artificially restricting the data analysis.

Our *Equilibrium* type always makes its equilibrium decision (which was unique in all of our games, although subjects were not told this). Our *Naive* type makes decisions that are best responses to beliefs that assign equal probabilities to the other player's decisions. Our *Optimistic* type makes "maximax" decisions that maximize its maximum payoff over the other player's decisions. Our *Pessimistic* type makes "maximin" or "secure" decisions that maximize its minimum payoff. Our *Altruistic* type makes decisions that maximize the sum of its own and the other player's payoffs over all possible combinations of decisions. Finally, to capture the idea of a rational player who can accurately predict how other players will respond to the environment, our *Sophisticated* type makes decisions that are best responses to the probability distributions of his partners' decisions.[11]

[11]In environments that yield high frequencies of Equilibrium play, Sophisticated and Equilibrium players make the same decisions, but they differ in other settings. We operationalize our definition of Sophistication by estimating probability distributions of subjects' decisions in each game from the observed population frequencies in our experiment (Section 4.C). Our approach resembles the notions used to test for sophisticated behavior in experiments by Roth and Murnighan (1982) and Roth et al. (1991). Stahl and Wilson (1995) give a good discussion of the issues that arise in formalizing the idea of sophistication in support of more subtle notions (their "worldly" and "rational expectations" types).

3. Experimental Design

This section discusses our experimental design. First we describe the overall structure, then the use of Mouselab to present games in normal form, and finally the games to be studied.

A. Overall structure

Our experiment consisted of two sessions of a Baseline treatment, B1 on 22 April 1997 and B2 on 21 July 1997, and one session each of two control treatments, OB (for "Open Boxes") on 24 July 97 and TS (for "Trained Subjects") on 22 July 97. These were preceded by one session each of three pilot treatments, P1 on 24 February 1997, P2 on 25 February 1997, and P3 on 27 February 1997. All seven sessions were run in the Economic Science Laboratory at the University of Arizona, using its local area network of Pentium PCs. We now describe those treatments, beginning with the Baseline and then explaining how the others differed. Appendix A, which reproduces the Baseline and TS instructions, is available from the authors on request, as are the OB and P1-P3 instructions.

The main features of our design are dictated by our goal of studying sophistication. To test game-theoretic predictions, the design must clearly identify the games to which subjects are responding. Most experiments accomplish this by having subjects repeatedly play the same stage game, suppressing repeated-game effects by randomly matching then from a "large" population each period. The results are then used to test theories of behavior in the stage game. Although the learning that such designs allow often greatly reduces the noisiness of subjects' responses over time, our purposes are better served by a design in which subjects play a series of games with different strategic structures.[12] Varying the games this way helps to prevent subjects from developing preconceptions about their structures, avoids confounding sophisticated deduction with learning (Section 2), and more precisely identifies subjects' strategic decision rules. It also enhances our control of subjects' information by making it impossible for them to remember their current payoffs from previous plays, which is important in a study that examines cognition via information search.

Subjects were recruited from undergraduate and graduate students at the University of Arizona, with a separate subject population for each session. In order to avoid noisiness due to unfamiliarity with computers or abstract decision problems, we sought subjects whose course enrollments suggested that they had strong quantitative backgrounds. However, we disqualified all subjects who revealed that they had ever studied game theory or participated in game experiments.

[12]Such designs have been used successfully before by Beard and Beil (1994), Roth (1987), and Stahl and Wilson (1995).

In our Baseline treatment, after an instruction and screening process described below, the subject population was randomly divided into subpopulations of Row and Column players, as nearly equal in size as possible. Subjects were then anonymously and randomly paired, with generally different partners each period, to play a common series of 18 two-person normal-form games in roles determined by their subpopulations.[13] The games were 2x2, 2x3, 3x2, 2x4, and 4x2 matrix games with various patterns of dominance, iterated dominance, and unique pure-strategy equilibria without dominance, described in Section 3.C.[14] The order of the games was the same for all subjects, randomized to avoid bias except that the single 2x4 and 4x2 games in the series were placed at the end.[15] Subjects were given no feedback about their own or their partners' payoffs or decisions while they played these games.[16] They could proceed independently, at their own paces, but they were not allowed to reconsider their decisions once they were made and confirmed.

At the end of the session subjects were asked to fill out a brief exit questionnaire, in which they were asked to give their year and major and to describe how they thought about their decisions and information searches in the games, and given an opportunity to comment on the experiment.

To ensure that subjects were well motivated and to maintain control over their preferences, they were paid according to their game payoffs as follows. After the session each subject returned to the lab individually, at which time he was shown the number of points he earned in each round, given his partners' decisions. He then drew a number from a bag containing the numbers 1-18 and was paid in proportion to his payoff in the game whose number he drew, at the rate of $0.40 per point.[17] With payoffs ranging from 12-98 points, this made the average payment about $21; adding $5 for showing up and passing the test made subjects' average earnings approximately $15 per hour over the 1½ to 2 hours

---

[13]The pairings were repeated, usually once a session, in a period unknown to the subjects. The 18 games took subjects an average of 1-2 minutes each. Adding an hour or more for signing up, seating, instructions, and screening yielded sessions of 1½ - 2 hours, which we judged to be near the limit of subjects' attention spans for their difficult tasks.
[14]We omitted games with mixed-strategy or multiple equilibria, because they seemed unnecessary to prevent preconceptions about the structure, and they each raise issues interesting enough for a separate investigation.
[15]We put the 2x4 and 4x2 games at the end to preserve comparability with the pilots, which omitted them, and because we feared (incorrectly) that they would confuse subjects, contaminating the data for any subsequent games. Row and Column subjects faced different orders of strategic structures because most of the games had asymmetric structures.
[16]We had no choice about feedback because Mouselab cannot be "networked" and manual feedback would have taken too long, but on balance we believe that not giving feedback during the main part of the session furthered our goals.
[17]Iterated dominance and pure-strategy equilibrium predictions can be tested without controlling for risk preferences, but they might affect the results when subjects face significant strategic uncertainty. It is theoretically possible to control for risk preferences using the *binary lottery* procedure (Roth and Malouf 1979), in which a subject's probability of winning a given monetary prize is proportional to his or her payoff (as in Cooper et al. (1990) and Stahl and Wilson (1995)). We avoid this complexity because subjects often appear approximately risk-neutral in money for payoffs like ours, and

of the session.[18] Subjects never interacted directly, and their identities were kept confidential by requiring them to sign up via identifying numbers and paying them in private.[19] These procedures appeared to motivate most subjects to try to maximize their expected payoffs.

The structure of the environment, except the game payoffs, was made public knowledge at the start by presenting the instructions via handouts and subjects' computer screens and announcing that all subjects received the same instructions. The instructions avoided suggesting decisions or strategic principles. Subjects were given unlimited access to the payoffs during the session via Mouselab, and received training in the mechanics of looking up their payoffs (Section 3.B).

After reading the instructions, subjects were given ample opportunity to ask questions and required to pass an Understanding Test before they were allowed to continue. Subjects were paid an additional $5 for showing up on time, following instructions, and passing the test; and subjects who failed the test were dismissed.[20] To enhance subjects' understanding of how their payoffs would be determined, subjects who passed the test were required to participate in four unpaid practice rounds before the main part of the session, in which Row and Column subjects each faced a balanced mix of games of different sizes and strategic structures, with various patterns of dominance, iterated dominance, and unique pure-strategy equilibria. To further reinforce their understanding, they also received *feedback* from the first and third of these rounds (also roughly balanced in strategic structures) in the form of summaries of Row and Column subjects' decisions in their session.[21]

---

results using the binary lottery procedure are usually close to those using direct payment (Cooper et al. 1990, fn. 5).

[18]To avoid effects of differences in preferences for gains and losses, all games had all positive payoffs. The analogous earnings figures were $23 and $16 for OB subjects and $27 and $21 for TS subjects, who were paid an extra $4 for correctly answering the questions in a quiz they were required to take, as described in Appendix A.

[19]After all subjects had checked in, each picked an identification number from 1 to 32 from a basket. They were then told to seat themselves at the terminal in the lab with that number. To receive their payment, they only needed to show this identification number. This made it clear (and they were told) that we would never know their identities.

[20]The dismissal rates were 25%, 16%, and 53% for the Baseline, OB, and TS treatments respectively. Following standard practice at the Economic Science Laboratory, subjects who were dismissed were also paid $5.

[21]Round 1 was a dominance-solvable 2x2 game in which Column but not Row had a dominant decision, which could be solved by two rounds of iterated dominance. In Round 1, 75%, 45%, and 67% of Column subjects chose their dominant decision and 25%, 40%, and 25% of Row subjects chose their iteratively undominated decision in B1, B2, and OB respectively. Round 3 was a dominance-solvable 3x2 game with a dominated decision for Row but no dominance for Column, which could be solved by three rounds of iterated dominance. In round 3, 50%, 27%, and 75% of Column subjects chose their iteratively undominated decision; 33%, 30%, and 25% of Row subjects chose their iteratively undominated decision; and 17%, 0%, and 0% of Row subjects chose their dominated decision in B1, B2, and OB respectively. Viewing these results in the light of Section 4's analysis suggests that the noisiness and variation across runs and treatments in the results from practice rounds had little effect on subjects' decisions in the actual games.

In the data analysis below, we maintain the following assumptions about the effects of our design. The fact that the structure was publicly announced except for the game payoffs, to which subjects were given unlimited access, made the structure public knowledge, so that the results can be used to test theories of strategic behavior in games of complete information. The sizes of our subject populations and the lack of identifiable repeated interaction or feedback made the effects of current decisions on future payoffs small and unpredictable. Previous experiments suggest that this made repeated-game effects negligible, so that subjects viewed each of the 18 games they played as strategically independent. Finally, our practice rounds, the fact that subjects never played the same game twice, the varying strategic structures, and the lack of feedback minimize learning effects, suggesting that subjects' behavior can be analyzed without modeling the dynamics of their responses. We take these features to justify separate, static analyses of each subject's behavior in each of our games, using the results to test theories of behavior at the individual level.

The OB treatment was identical to the Baseline treatment (including the practice rounds and feedback, to preserve comparability) except that the games were presented via Mouselab with all payoffs continuously visible, in "open boxes." Comparing the OB and Baseline results allows us to test the hypothesis that subjects' responses are unaffected by looking up their payoffs via Mouselab. If so, our Baseline results should be representative of results obtained by standard methods.

The goal of the TS treatment was to learn what subjects' information searches would have been like in the Baseline under the leading hypothesis that they were Equilibrium players, allowing us to check the model of information search we use to draw inferences about cognition. The TS treatment was identical to the Baseline treatment (including the presentation of payoff information via Mouselab, in "closed boxes") except that subjects were trained and rewarded differently. First, via instructions on their computer screens (Appendix A), TS subjects were taught the relevant parts of noncooperative game theory, including dominance, iterated dominance, dominance-solvability, and pure-strategy equilibrium. Although TS subjects, like Baseline subjects, received training in the mechanics of looking up their payoffs, they were *not* trained in information search patterns to identify the predictions of any theory. TS subjects were then rewarded only for correctly identifying their equilibrium decisions (or, equivalently, their iteratively undominated decisions in dominance-solvable games) in the 18 games, without regard to other subjects' responses.[22] To the extent that TS subjects correctly identified their equilibrium decisions, we can be confident that any failures of game-theoretic predictions in the Baseline treatment are not due to subjects' cognitive limitations.

We close this section by describing pilots P1-P3 and how they influenced our designs for the main treatments. All three pilots had 16 games, with structures like the first 16 games in the main treatments, but with weaker incentives and payoff separation of equilibrium and alternative theories.[23] Pilot P1 paid subjects for their total payoffs over all games, at a lower rate but with the same expected total payment, while P2 and P3 paid them for their payoffs in one randomly selected game as in the Baseline and OB treatments. Pilots P1 and P2 gave subjects no feedback from practice rounds, while P3 gave feedback as in the Baseline and OB treatments. Pilots P1 and P2 framed the payoff matrix as in the main treatments (Section 3.B), while P3 interchanged the locations of subjects' own and their partners' payoffs, still with all subjects framed as Row players.

The results for the pilots, which are available on request, were similar to the results for the Baseline but noisier, with P1's and P2's much noisier than P3's. The noisiness seemed due mainly to subjects' lack of comprehension and weak incentives. Pilot P1's alternative payment scheme made little difference, so from then on we followed the common practice of paying each subject a larger amount for his payoff in one randomly selected game. Pilot P3's alternative framing also made little difference, so we returned to P1's and P2's framing for the main treatments. Pilot P3's feedback did reduce the noise significantly, but in our judgment not enough. To reduce the noise further, we combined this feedback with the strengthened incentives of our Baseline design.

B. Using Mouselab to study cognition in normal-form games

We now describe how Mouselab was used to present games to subjects. Each subject was framed as Row player and called "You" in all games, without regard to his player role as described here.

[22]For TS subjects the practice rounds were replaced by a supplementary test of their understanding of dominance, iterated dominance, and equilibrium, with no feedback because other subjects' responses were not relevant to their tasks. All TS subjects were Row players because only 15 of 32 recruits passed the Understanding Test, and splitting them would have yielded too small a sample. This difference is inessential because TS subjects were paid for correct answers, not game payoffs, and the mix of strategic structures is similar for Row and Column players. To encourage TS subjects to search for equilibria without preconceptions, so their searches would resemble those of "real" Equilibrium players, they were *not* told that all games had unique equilibria. Instead they were told that a game might have more than one equilibrium, and that to receive credit for a game they had to identify their decision in the equilibrium that gave them the highest payoff of any equilibrium. This made it necessary to identify all equilibria to be sure of receiving payment.

We believe that framing subjects as Row players eases comprehension. It also makes it impossible for subjects to condition their behavior on observable differences in their roles, which increases the effective size of our samples by creating strategic equivalences between some games in the series that differ essentially only by interchanging player roles (Section 3.C).

A typical game appeared on a subject's screen as in Figure 1. There, each player has two decisions (# and * for Row and & and @ for Column). Row's ("Your") payoffs are in the two columns on the left, here identified as r#&, r#@, etc., and Column's ("Her/His") are in the two columns on the right, here identified as c#&, c#@, etc.[24] The actual display had two-digit numbers of "points" instead of these abstract payoff symbols. The visual separation of Row and Column players' payoffs, emphasized by the legends at the bottom of the matrix, helps subjects to distinguish them. Different labels were always used for Row and Column players' decisions. The order and labeling of decisions was the same for all subjects in a given player role, but was jointly randomized across games. These features and our framing of all subjects as Row players suppress contextual effects, allowing us to focus on the structural principles studied in noncooperative game theory.

	S/He: &	**S/He: @**	**S/He: &**	**S/He: @**
You: #	**r#&**	**r#@**	**c#&**	**c#@**
You: *	**r*&**	**r*@**	**c*&**	**c*@**
	Your Points		**Her/His Points**	
	You: #		**You: ***	

Figure 1: Screen Display

In the Baseline, TS, and P1-P3 treatments all payoffs were normally hidden, in "closed boxes." A subject could look up the payoffs as often as desired, one at a time, by using his mouse to move the cursor into its box and left-clicking. Before he could open another box, or record his decision, he had to close the box by right-clicking, which could be done even after the cursor had been moved out of the

[23]The weak incentives and separation were unintended by-products of our attempt to use variations in out-of-equilibrium payoffs, as in Beard and Beil's (1994) extensive-form game experiments, to detect subtle aspects of sophistication. In a design like ours this severely constrains the strength of incentives and separation of alternative theories.
[24]In pilot P3 "Your points" appeared in the right side of the display and "Her/His" points in the left side. P3 subjects started looking in the left side 65% of the time, and their subsequent look-ups and decisions resembled those in P1 and P2, where the locations were as in Figure 1. Comparing the 65% starting in the left side in P3 with the 73% observed in the Baseline suggests that for initial look-ups, many more subjects favored the left side than favored their own payoffs. Some such framing bias seems inevitable in any normal-form design with a two-dimensional display.

box. Thus both opening and closing a box required conscious choices. At any time, a subject could record and confirm his decision in the current game by using the mouse to move the cursor into one of the boxes at the bottom of the display and left-clicking.[25] Mouselab automatically records subjects' decisions and look-up sequences, including look-up durations.[26] The OB treatment used Mouselab in exactly the same way, but with all payoffs continuously visible, so that subjects used the mouse only to record and confirm their decisions.

Our display makes subjects' information processing somewhat simpler than in CJ's alternating-offers bargaining games, by revealing their payoffs directly rather than requiring them to deduce them from pie sizes; but it also makes their information searches much more complex, with 8-16 independent payoffs that can be searched multidimensionally rather than three pie sizes along one dimension. This complexity could be reduced by limiting the number of independent payoffs subjects need to look up, either by leaving some payoffs continuously visible or by creating simple relationships between payoffs and making them public knowledge (e.g. by announcing that the game is a pure coordination game or a zero-sum game, and simplifying the display accordingly). But this simplification would come at the cost of losing some of the information in subjects' look-ups, significantly reducing the power of our method to discriminate among alternative theories.

Most of the cognitive processes required by noncooperative game theory and the leading alternatives involve sequences of binary payoff comparisons. As explained in Section 5, there is good reason to expect subjects to make such comparisons via adjacent look-ups, but even so their look-up sequences will necessarily include many adjacent look-ups that are not comparisons. One can easily imagine Mouselab-like software that requires subjects to observe payoffs in pairs, which could then be interpreted as binary comparisons, but we are not aware of any software that allows this.[27] Instead we develop a theory of cognition and information search that addresses this issue by deriving restrictions

[25]A subject could move on to the next game only by confirming his decision. The cursor always started at the top-center.
[26]See Payne, Bettman, and Johnson (1993, Appendix) for more on Mouselab. In the commercially available version C&J used, a box remains open as long as the cursor is in it, and closes when the cursor leaves it. In preliminary trials with this version, subjects often compared payoffs in nonadjoining cells by rolling the mouse across the intervening cells in our two-dimensional display, which takes longer than 0.017 seconds, the minimal look-up duration Mouselab records. The resulting "accidental" look-ups add a great deal of noise to the look-up data. The ease of opening boxes with this version also yields very large numbers of look-ups (100 or more in a 2x2 game), decreasing the discriminatory power of subjects' look-up patterns. Following C&J, the noise could be reduced by screening out look-ups shorter than subjects' minimum perception time of approximately 0.18 seconds (Card, Moran, and Newell (1983)). However, the "click" version of Mouselab we use, supplied to us by Mary Francis Luce, gives a good solution to both problems at once.
[27]Even if this were possible, it might not be desirable. In a 2x2 game there are 28 (= 7x8/2) possible payoff pairs, and

that do not depend on precise identification of comparisons (Sections 5-6).

C. The Games

Table I summarizes the strategic structures of the 18 games subjects played in our main treatments and our types' predicted decisions in them; the payoff matrices are given in Appendix B. For clarity in the table, we use mnemonic names for players' roles (Row and Column) and decisions (Top, Middle, and Bottom or Left, Middle, and Right) and present the games in an order that highlights the relationships among them.[28] As Table I shows, the games are chosen to explore the limits of strategic sophistication in a various strategic structures, including games with dominance, dominance-solvable games, and games with unique pure-strategy equilibria without dominance. These games separate our Sophisticated and Equilibrium types' predicted decisions from those of our other types as sharply as possible, while providing strong incentives for a subject of a given type to make his type's predicted decisions. In conjunction with our avoidance of salient payoffs and the artificial clarity of overly simple payoff structures, this separation allows us to "stress-test" noncooperative game theory, minimizing the chance that subjects will choose equilibrium decisions for reasons other than those contemplated by the theory. Because the cognitive requirements of our Sophisticated and Equilibrium types are sharply separated from those of our other types (Section 5), this separation facilitates our search for relationships between decisions and information search.

Games 2A and 2B are 2x2 games with a dominant decision for Row but not for Column, solvable via two rounds of iterated dominance.[29] The dominance for Row occurs with overlapping payoff ranges, so that he can reliably identify his dominant decision only by looking up all of his payoffs (Appendix B). In our design dominance always occurs this way, so that subjects who only sample their payoffs will violate it with nonnegligible probabilities. Games 3A and 3B are 2x2 games, which are *isomorphic* to games 2A and 2B, obtained from them by transposing players' roles and reducing all payoffs by 4 and 2 points respectively. Games 3A and 3B have dominant decisions for Column but no dominance for Row, and are solvable via two rounds of iterated dominance.

---

deciding which ones to look at is a nontrivial task, which might distract subjects from thinking about the game.

[28]In the experiment the games were presented to each subject as Row player, with abstract decision labels, random orderings of all games but 9A and 9B (3A, 6B, 2A, 8B, 8A, 5A, 4A, 7A, 4C, 7B, 4B, 3B, 2B, 6A, 5B, 4D, 9A, 9B) and decisions, and with relationships between games disguised by the random orderings and small payoff variations. In Table I, dominance-solvability for games 7A, 7B, 8A, and 8B is defined excluding mixed strategies.

[29]Here and below, the number of rounds of iterated dominance is defined as the number of dominance relationships it takes for the player in question to identify his own equilibrium decision. Thus playing a dominant decision is one round, best responding to another's dominant decision is two rounds, etc.

Game 4A is a 2x3 game and game 4B is a 3x2 game, each with a dominant decision for Column but no dominance for Row, and solvable by two rounds of iterated dominance. Game 4C is a 3x2 game and game 4D is a 2x3 game, which are isomorphic to games 4A and 4B, obtained from them by transposing roles and reducing all payoffs by 1 or 4 respectively; each has a dominant decision for Row but no dominance for Column, and is solvable by two rounds of iterated dominance. Games 5A and 5B are 3x2 games with a dominated decision for Row but no dominance for Column, and solvable by three rounds of iterated dominance. Games 6A and 6B are 2x3 games isomorphic to 5A and 5B, obtained from them by transposing roles and increasing all payoffs by 2 or decreasing them by 6 respectively; each has a dominated decision for Column but no dominance for Row, and is solvable by three rounds of iterated dominance.

Games 7A and 7B are 2x3 games, each with a unique, pure-strategy equilibrium but no decisions dominated by pure decisions.[30] Games 8A and 8B are isomorphic 3x2 games, obtained from 7A and 7B by transposing roles and decreasing all payoffs by 4 or 7 respectively; each has a unique, pure-strategy equilibrium but no decisions dominated by pure decisions.

Games 9A and 9B are a 4x2 and a 2x4 game, each with a dominant decision for one player but no dominance for the other, and each solvable by two rounds of iterated dominance.

4. Analysis of Subjects' Decisions

This section conducts an analysis of subjects' decisions without considering their information searches, in preparation for the analysis of decisions and information search in Sections 5-6. In Section 4.A we test for differences in subjects' decisions in the aggregate across runs in the Baseline treatment, treatments, and player roles in isomorphic games. In Section 4.B we study their aggregate compliance with dominance, iterated dominance, and equilibrium in games without dominance. In Section 4.C we conduct a maximum likelihood error-rate analysis in the style of Harless and Camerer (1994, 1995) ("H&C") and El-Gamal and Grether (1995) ("EG&G") at the individual level, estimating the strategic type that best describes each subject's decisions over the games he played, and the error rates for subjects estimated to be of each type.

[30]Column's decision Middle is dominated by a mixture of Left and Right, so the games are actually dominance-solvable, a necessary feature with unique pure-strategy equilibria when at least one player has only two pure strategies. Mixed-strategy dominance has the same effect as pure-strategy dominance in the deductive justification for equilibrium, but we expect subjects to find it far less salient and it seemed a small price to pay for the simplicity of these 2x3 and 3x2 games.

A. Preliminary statistical tests

In this section we test for aggregate differences in subjects' decisions across Baseline runs, treatments, and player roles in isomorphic games; subjects' decision data are in Appendix C. These tests check the consistency of subjects' responses across related games, confirm simplifying restrictions suggested by theory, and answer questions that are helpful in evaluating our methods. Because the tests compare categorical data from independent samples with no presumption about how they differ, we use Fisher's exact probability test, which requires no distributional assumptions. We conduct the tests separately for each game, pooling the data for all subjects in each player role, and in some cases pooling the data for subjects with isomorphic player roles in different games. [31]

Table II reports *p*-values for tests of the hypotheses that Column and Row subjects' decisions were drawn from the same distributions in: (i) the two runs of the Baseline treatment, B1 and B2; (ii) the Baseline (with B1 and B2 pooled, as B1+B2) and OB treatments; and (iii) the TS (which had only Row subjects) and Baseline treatments. For (iii) we report *p*-values for the full TS sample and "TS (ex.)," which excludes the 3 out of 15 TS subjects (TSR17, TSR18, and TSR21 in Appendix C) who revealed by their comments or exit questionnaires that they did not try to identify equilibria.

Table II's tests reveal no differences in subjects' decisions in B1 and B2 that are significant at the 5% level except in game 4C for Column subjects, a result within the limits of chance for 36 comparisons under the null hypothesis. Given these results, we pool the data from B1 and B2 from now on. These tests also reveal no differences between B1+B2 and OB that are significant at the 5% level, except in game 6A for Column subjects, again within the limits of chance. Accordingly, we pool the data from B1+B2 and OB when necessary to obtain adequate sample sizes, even though there is some indication in the table, discussed below, that presentation via Mouselab has a small but systematic effect on subjects' decisions. The number of the subject's or his partner's decisions appears to make little difference to the results of these tests. Finally, as expected, there are large differences between the Baseline and TS treatments in most games. These differences are noticeable in 16 of our 18 games, significant at the 5%

[31] Conducting tests separately for each game is fully justified only if subjects' decisions are statistically independent across games, which is unlikely because some games are related. However, the correct test without independence (comparing the distributions of the $2^{11}3^{6}4 \sim 6$ million possible *histories* of decisions in 18 games) is impractical, and testing game by game gives us a metric in which to gauge the differences across games and treatments. Fisher's exact test is usually discussed only for 2x2 contingency tables (e.g. Davis and Holt (1993)), but it is straightforward to extend it to 2x3 and 2x4 contingency tables. Computer codes to compute p-values for 2x2, 2x3, and 2x4 contingency tables are available from the authors on request.

level in 6 games for TS and 9 for TS (ex.), and significant at any reasonable level in 4 of the 6 games where the subject had three decisions.

Our design includes several pairs of isomorphic games, identical for Row and Column players except for uniform shifts in payoffs that never exceed 7 (out of a maximum of 98) points (Section 3.C, Appendix B). Game 2A for Row subjects, for instance, is the same as game 3A for Column subjects except for a payoff shift of 4 points. There are good theoretical reasons to expect behavior not to vary systematically across isomorphic games, because any theory that ignores presentation effects predicts the same decisions in them, and our design controls for presentation effects except for the order and labeling of decisions across Row and Column players and the payoff shifts, which we made small and nonsalient in the hope that they would not alter subjects' decisions.

Table III reports *p*-values for tests of the hypotheses that Row and Column subjects' decisions in isomorphic games were drawn from the same distributions. These tests are conducted separately for B1+B2 and OB. Each pair of isomorphic games appears twice in the table, once for each player role. [32] These tests reveal no differences between subjects' decisions in isomorphic games that are significant at the 5% level except in 4B, 4D for B1+B2 and 9B, 9A for OB, about what would be expected by chance in 18 comparisons. Accordingly, we pool the data for Row and Column subjects across isomorphic games when necessary to obtain adequate sample sizes.

Because these tests include several pairs of isomorphic games that were widely separated in the sequence but revealed no significant differences (5B, 6B, by 12 games; 5A, 6A, by 7 games; and 7B, 8B, by 5 games), they give some evidence that our design successfully suppressed learning. [33]

The far right-hand column in Table III reports *p*-values for tests for differences between B1+B2 and OB like those in Table II, but with the data pooled across isomorphic games. These more powerful tests reveal differences that are significant at the 5% level only for 4D, 4B and 5A, 6A, slightly more than would be expected by chance in 18 comparisons.

We close this section by using the observed frequencies of subjects' decisions in the B1+B2 and OB treatments to estimate the strength of subjects' ex ante incentives. Table IV reports the payoffs our Row and Column subjects would have received over all 18 games by following the decision rules of

[32] For example, "2A, 3A" compares Row subjects' decisions in game 2A with Column subjects' decisions in game 3A, while "3A, 2A" compares Row subjects' decisions in game 3A with Column subjects' decisions in game 2A.
[33] Because we did not control for the effects of decision order and labeling across isomorphic games, the test results also suggest that the labeling and order of decisions had little effect on subjects' decisions.

each of our types exactly, as point totals and as percentages of a Sophisticated subject's payoffs, with the latter normalized to 100% because it is necessarily the largest. The expected monetary value of a point was $0.022 (= $0.40/18), so a Sophisticated subject's points are worth about $25 on average. The payoffs for Row and Column subjects are remarkably similar. An Equilibrium subject gives up 2.2-3.2% of this total, or about $1. Naïve/Optimistic, Pessimistic, and Altruistic subjects, respectively, give up about 14.1-14.4%, 15.7-16.3%, or 31.3-31.4%, or about $3.50, $4.75, or $7.75. Thus the incentives to behave as a Sophisticated rather than an Equilibrium player were weak, but the incentives relative to our other types were substantial.

B. Aggregate compliance with dominance, iterated dominance, and equilibrium

We now examine subjects' decisions in the aggregate, first for significant deviations from randomness, with no presumption about their nature, and then for compliance with dominance, iterated dominance, and equilibrium in the different kinds of games we study. This reveals the overall patterns in subjects' aggregate responses to games with different strategic structures.

In testing the hypothesis that subjects' decisions were generated by uniform randomization over the possible decisions, we use exact $?^2$ tests (Pierce (1970, chapter 11)), which are appropriate for comparing categorical sample data with the theoretical distribution under the null hypothesis . Table V reports *p*-values for these tests, first for Row and Column subjects separately, pooling the Baseline and OB data; then for Row and Column subjects in isomorphic games, again pooling the Baseline and OB data; and finally using the TS data (which includes only Row subjects). For the TS treatment two *p*-values are reported: one for the full sample ("TS") and one ("TS (ex.)") excluding the three TS subjects who did not try to identify equilibria. Large *p*-values indicate randomness.

For the Baseline and OB treatments there are significant deviations from randomness whenever the subject had three or four decisions, and in most cases when he had two decisions. The most powerful test, with data pooled across isomorphic games, rejects randomness in all but 7A, 8A and 7B, 8B, the games with unique pure-strategy equilibria but no dominance in which the subject had two decisions. For the TS treatment, there are significant deviations from randomness across the board, with the sole exceptions again in 7A, 8A and 7B, 8B for the pooled data.

Table VI reports the percentages of subjects' decisions that comply with equilibrium in the B, OB, B+OB, TS, and TS (ex.) treatments, pooling the data from Row and Column subjects in isomorphic games whenever possible. In each case the compliance percentages are reported by type of game, with the population fractions in parentheses. The games are grouped by the complexity of the strategic reasoning needed to identify the subject in question's equilibrium decision, from games in which he has a dominant decision to games solvable by two or three rounds of iterated dominance and games with unique pure-strategy equilibria but no (pure-strategy) dominance.

In the Baseline and OB treatments, subjects played dominant decisions with frequencies averaging close to 90%, and best responses to partners' dominant decisions with frequencies averaging close to 70%.[34] In other games solvable by two rounds of iterated dominance, they played equilibrium decisions with frequencies averaging about 65%. However, in games solvable by three rounds of iterated dominance they played equilibrium decisions with frequencies averaging only about 15%, and in games with unique pure-strategy equilibria but no dominance they played equilibrium decisions with frequencies averaging about 35%. In most cases equilibrium compliance was slightly higher in OB than in the Baseline. Although this is unlikely to be due entirely to chance, the difference is too small to be significant in our samples (Section 4.A, Table II).

Overall, equilibrium compliance is quite high, bearing in mind that it reflects subjects' initial responses to games. It is remarkably consistent across games with similar levels of strategic complexity, and the number of the subject's or his partner's decisions has little effect, holding complexity constant. Compliance stays well above random for most games, and most of the rejections of randomness in Table V reflect systematic deviations in the direction of equilibrium. However, compliance falls steadily as complexity increases, eventually dropping below random in 3x2 games that are dominance-solvable in three rounds (11.1%-22.2% in 5A, 5B for Rows and 6A, 6B for Columns, versus 33.3% for random decisions) and in 3x2 games with unique equilibria but no dominance (17.8%-27.8% in 8A, 8B for Rows and 7A, 7B for Columns, again versus 33.3%).

These results are generally consistent with the results of other experiments that study subjects' initial responses to games, except that the below-random compliance in our most complex games differs from Stahl and Wilson's (1995) findings for closely related games.[35] They found compliance rates of

[34]Interestingly, subjects played dominated decisions with comparably low frequencies in 8A,8B for Rows and 7A,7B for Columns, the 3x2 games with unique pure-strategy equilibria and dominance only via mixed strategies. In those games decisions dominated by mixed strategies were played with frequency 10% in the Baseline and 3.7% in the OB treatment.
[35]Our findings for games that can be solved by three rounds of iterated dominance are consistent with previous evidence on dominance-solvable games (Crawford 1997, Section 4.1). Our findings for games with unique equilibria but no dominance are broadly consistent with the evidence from other settings summarized by Selten (1998, Section 5), which tends to favor principles whose implications can be identified by step-by-step reasoning (such as iterated dominance in our setting) rather than what Selten calls "circular concepts" (such as equilibrium in non-dominance-solvable games).

68% and 57%, respectively, for symmetric 3x3 games solvable by three rounds of iterated dominance and for similar games with unique pure-strategy equilibria but no dominance. This difference may stem from our avoidance of salient payoffs and simple payoff structures, including symmetry, and our attempt to separate equilibrium decisions from those suggested by other decision rationales, so that any alternative rationale tends to yield non-equilibrium decisions.

In the TS and TS (ex.) treatments, subjects played dominant strategies with frequencies well above 90%. In striking contrast to the Baseline and OB results, TS subjects' equilibrium compliance rates fell off only slightly in more complex games, averaging well over 80% (90% in TS (ex.)) even in those games in which compliance fell below random in the Baseline and OB treatments. This suggests that Baseline and OB subjects' noncompliance in those games was due mainly to factors other than the difficulty of looking up payoffs via Mouselab or cognitive limitations.

C. Maximum likelihood error-rate analysis of individual subjects' decision rules

In this section we use a simple maximum likelihood error-rate model to estimate the decision rule that best describes each subject's decisions over the 18 games he played.

To describe the possibilities for subjects' decisions (and ultimately, their information searches) in a tractable way, avoiding overfitting or excessively constraining the data analysis, we restrict attention to six decision rules or strategic types: Equilibrium, Sophisticated, Naïve, Optimistic, Pessimistic, and Altruistic. We assume that each subject is one of our six types, constant over all games, with probability one; but we allow for random decision errors as explained below.[36]

Recall that our Sophisticated type is defined as a player who always makes decisions that are best responses to the probability distributions of others' decisions. Because those distributions are unobservable, we need a way to operationalize our definition in the Baseline and OB treatments.[37] If there were an empirically reliable theory of players' responses to our games, we could make our Sophisticated type a purely theoretical construct, like our Equilibrium type. But games like ours evoke significant frequencies of non-equilibrium decisions, which depend on the strategic structure of the game in ways that at present cannot be well predicted by theory (Section 4.B, Table VI). We therefore

[36]Section 2 gives the definitions of our types and discusses the rationale for this approach. One could construct an "ad hoc" type for each subject to mimic his decision history exactly, but this would have little explanatory power. And since there are multiple rationales for any given decision history, it would leave us unable to derive the cognitive implications of such types, which is an essential part of our analysis of information search. Similar problems are encountered in "data mining" approaches to determining types, such as El-Gamal and Grether (1995).

[37]In the TS treatment the Sophisticated type is excluded *a priori* as meaningless, because TS subjects were rewarded

define Sophistication empirically, first estimating the probability distributions of subjects' decisions, game by game, from the population frequencies in our experiment, and then computing subjects' best responses to those estimated distributions.[38] Although one can imagine attractive alternatives using more complex designs and larger datasets, this seems a natural way to operationalize the notion of Sophistication within our design.

Each of our types implies a unique *predicted decision* for each player role in each game (Table I). We chose not to separate the predicted decisions of our Naïve and Optimistic types, due to a conflict with the need to provide strong incentives; we therefore lump those types together in this section's analysis. With this exception, any two of our types have different predicted decisions in at least 3 of our 18 games for each player role.[39] This separation allows us to identify individual subjects' types by comparing their decision histories with the predicted decisions for each type.

Combining evidence from different kinds of devi ations from predicted decisions requires an error structure. In specifying the error structure we impose as few restrictions as possible, adapting the maximum likelihood error-rate analyses of El-Gamal and Grether (1995) ("EG&G") and Harless and Camerer (1994, 1995) ("H&C"), who give good discussions of the philosophy of this approach. Our analysis is also related to that of Stahl and Wilson (1995), who to our knowledge were the first to estimate heterogeneous strategic decision rules for experimental subjects.

Let $i = 1, \ldots, N$ index the subjects in a given treatment , and let $k = 1,\ldots,K$ index our types. Let $c = 2$, 3, or 4 be the number of decisions a given subject has in a given game. We assume that a subject normally makes his type's predicted decision, but that in each game, for each type k, there is a given probability e_k ? [0, 1], type k's *error rate*, that the subject makes an error, in which case he makes each of his c decisions with probability $1/c$. The probability that a subject of type k makes type k's predicted decision is then $(1 - e_k) + e_k/c = 1 - (c - 1)\, e_k/c$, and the probability that he makes any single unpredicted decision is e_k/c. Note that since e_k may equal 1, the model implicitly allows a random type, which makes each of the c decisions with probability $1/c$.[40] We assume that errors are independently and identically distributed ("i.i.d.") across games and subjects, so that the e_k are constant. The resulting

---

only for identifying equilibria, and never played the games with other subjects .

[38]To reduce sampling error, we base our definition on the pooled Baseline and OB frequencies, which differ only slightly (Table II). The resulting definition differs from one using only the Baseline data in only 1 of 18 games for each player role, and has the additional advantage of making the definition uniform across the Baseline and OB treatments.

[39]With 2-4 decisions per game and six types, some overlap in predicted decisions is inevitable. Overlaps are even more frequent because our types all favor higher own payoffs, and all but the Altruistic type always play dominant strategies.

model extends EG&G's and H&C's (1994) analyses to allow type-dependent error rates, which is important, given our types' different informational and cognitive requirements, and to allow $c = 2$ to vary in the sample. We follow EG&G in estimating a separate decision rule from each subject's decision history because we wish to study cognition at the individual level, and in discounting predicted decisions for the probability they were made in error.[41]

Let T_c denote the number of games in a given treatment for which subject i has c decisions; in our designs $T_2 = 11$, $T_3 = 6$, and $T_4 = 1$ for all i. Let x_{ck}^i denote the number of subject i's decisions that equal type k's predicted decisions in the T_c games in which he has c decisions. Because our error structure distinguishes neither different unpredicted decisions nor different predicted decisions for a given i, c, and k, the x_{ck}^i are sufficient statistics. Let $x_k^i \equiv (x_{2k}^i, x_{3k}^i, x_{4k}^i)$, $x^i \equiv (x_1^i,...,x_K^i)$, and $x \equiv (x^1,...,x^N)$. The probability of observing x_k^i when subject i is of type k is:

$$L_k^i(e_k \mid x_k^i) = \prod_{c=2,3,4}[1-(c-1)e_k/c]^{x_{ck}^i}[e_k/c]^{T_c - x_{ck}^i}. \tag{4.1}$$

Let $e \equiv (e_1,...,e_K)$ and $d \equiv (d^1,...,d^N)$, where $d^i \equiv (d_1^i,...,d_K^i)$ is a vector of type indicator variables, with $d_k^i = 1$ if subject i is of type k, $d_k^i = 0$ otherwise, and $\sum_{k=1}^K d_k^i = 1$. Subject i's contribution to the likelihood function can be written:

$$L^i(e, d^i \mid x^i) = \prod_{k=1}^K L_k^i(e_k \mid x_k^i)^{d_k^i}. \tag{4.2}$$

Taking logarithms and summing over i yields the log-likelihood function for the entire sample:

$$\ln L(e,d \mid x) = \sum_{i=1}^N \ln L^i(e,d^i \mid x^i) = \sum_{i=1}^N\sum_{k=1}^K d_k^i \sum_{c=2,3,4}\{x_{ck}^i[\ln(1-(c-1)e_k/c) - \ln(e_k/c)] + T_c\ln(e_k/c)\}. \tag{4.3}$$

(4.3) implies that the maximum likelihood estimate of d_k^i identifies the type k for subject i that maximizes a weighted sum of the x_{ck}^i for c = 2, 3, and 4. In this section's model, if c were constant in

---

[40]Our specification constrains the probability of the predicted decision to be at least $1/c$, but this is never binding.
[41]H&C estimate population frequencies or probabilities of subjects' types from aggregate data, ignoring individual histories. Assuming $c = 2$, H&C write the probability of a predicted decision as $1 - e$ while EG&G write it as $1 - e/2$. When c is constant (even if $c > 2$) this difference is only one of notation; but when c varies it affects the relative log-likelihood weights of predicted decisions for different values of c. We briefly considered a model that nests these specifications, in which the probability of each decision conditional on an error is d/ c for some d ? [0, 1]. In our dataset

the sample subject i's estimated type would always be the k that maximizes x^i_{ck}, independent of the weights and therefore of the error rates. But when c varies the weights matter, and the maximum likelihood estimates of types and error rates are simultaneously determined.[42] The weight of x^i_{ck} is [ln(1-(c-1)e_k/c) - ln (e_k/c)], the difference between the log-probabilities of a predicted decision and of any single unpredicted decision, given the estimated e_k. This weight is positive for e_k ? [0, 1) and decreasing in e_k, approaching 0 as $e_k \to 1$.[43] Thus, a predicted decision is evidence in favor of any type that predicts it, but only to the extent that the type's estimated error rate indicates that it wa s more likely than an unpredicted decision.

Our model has N + 5 parameters: one type for each of N subjects, and one error rate for each of the 5 distinguished types. We computed maximum likelihood estimates of these parameters separately for the Baseline, OB, B+OB, TS, and TS (ex.) treatments, u sing standard grid search algorithms.[44] The results are summarized in Table VII, which gives the frequencies and numbers of subjects estimated to be of each type in each treatment, the estimated error rates, and the implied probabilities of the estimated types' predicted decisions in games with c = 2, 3, or 4.

In Section 4.A we could not reject the hypothesis that subjects' decisions were drawn from the same distribution in the Baseline and OB treatments. Moreover, a likelihood ratio test cannot reject the hypothesis that their type-dependent error rates are the same (p-value 0.476), and pooling the Baseline and OB data and reestimating leaves every subject's type estimate unchanged. We therefore discuss the Baseline and OB results together in the rest of this section.

In the pooled Baseline and OB treatments, the most frequ ent estimated type is Sophisticated with 51% (37) of the 72 (= 45+27) subjects, followed by Naïve/Optimistic with 25% (18), Equilibrium with 14% (10), Altruistic with 8% (6), and Pessimistic with 1% (1). The estimated error rate for Altruistic subjects is 0.66, implying that they made Altruistic predicted decisions with probabilities that range from 0.50-0.67 for c = 2, 3, 4. This error rate is well below the value of 1.0 that implies complete

---

d is weakly identified, and we can reject neither d = 0 nor d = 1 in any treatment. We set d = 1 for simplicity.

[42]The maximum likelihood estimate of e_k is a kind of weighted average of the sample error frequencies when c = 2, 3, 4 for subjects estimated to be of type k, with the frequency for each c adjusted for the associated probability that a predicted decision was made by chance. If c = 2 for all games the model gives the probability of a predicted decision as 1 - e_k/2, and the estimated e_k is just twice the observed error frequency. When c varies in the sample the estimates make similar corrections for each c, but nonlinearities preclude a closed-form solution for the estimate of e_k.

[43]The weight of x^l_{ck} also increases in c for any e_k? [0, 1], so a predicted decision is stronger evidence in favor of types that predict it when a subject has more decisions, because the decision is then less likely to have been made in error.

randomness, but it indicates that the model explains only about a third of the variation in Altruistic subjects' decisions. The other types' error rates range from 0.20-0.29, similar to those EG&G and H&C found in other settings. These error rates are quite low, given that they reflect subjects' initial responses to unfamiliar, abstractly framed tasks , and that they are discounted for the probability that a predicted decision was made by chance (fn. 42); they imply that subjects made their types' predicted decisions with probabilities from 0.78-0.90 for c = 2,3,4. Overall, for all subjects but those estimated to be Altruistic the model explains a large fraction of the variation in decisions, and the model seems to do a good job of describing our subjects' highly heterogeneous decisions. The Naïve/Optimistic and Sophisticated types alone capture most of the variation in subjects' decisions, and the low estimated frequencies for our other three types give reason for confidence that no non-ad hoc alternative type would enter significantly if added to our list.

As expected, subjects' decisions in the TS treatment were very different. The most frequent estimated type (recalling that the Sophisticated type is excluded a priori) is Equilibrium with 80% (12) of the 15 subjects, followed by Naïve/Optimistic with 13% (2), Altruistic with 7% (1), and Pessimistic with 0% (0). The estimated error rate for Equilibrium subjects is 0.07, implying that they made Equilibrium decisions with probabilities that range from 0.95-0.96 for c = 2, 3, 4. The estimated error rates for Naive/Optimistic and Altruistic subjects were very high, 0.79 and 0.57 respectively, implying that they made their types' predicted decisions with probabilities that range from 0.41-0.71. The three TS subjects not estimated to be Equilibrium were the same three who revealed by their comments or in our exit survey that they did not try to identify equilibria (Section 4.A). Excluding them, in TS (ex.), yields 100% (12) Equilibrium types, still with error rate 0.07 because the error rates are type-dependent and they are the same subjects as in TS. These subjects were remarkably proficient at identifying equilibria, better after 40 minutes of programmed learning on the screen than most of our undergraduate students who have completed a course in game theory.

We also tested our assumption that error rates are type-dependent within each treatment, but not idiosyncratic (i.e. subject-specific). Likelihood ratio tests reject the hypothesis of type-independent in favor of type-dependent error rates in the Baseline (p-value 0.0033) and TS treatments (p-value 1.732E-09), but not in OB (p-value 0.481) or in TS (ex.) (trivially, because all 12 TS (ex.) subjects were Equilibrium). They also reject the hypothesis of type-dependent in favor of idiosyncratic error

[44]The Gauss computer code and intermediate computations are available from the authors on request.

rates, with p-values of 0.033 or less in every treatment. We focus on the model with type-dependent error rates despite these last rejections because in Section 6's analysis we cannot reject type-dependence in favor of idiosyncrasy, and as a simplifying restriction it reduces the large number of parameters in that model and brings it closer to the spirit of error-rate analysis. [45]

To sum up, Baseline and OB subjects' decision rules are highly heterogeneous, and no theory that assumes homogeneity is likely to describe their behavior adequately. Their compliance with equilibrium is uniformly high in simple games, but falls off as complexity increases, eventually falling below random in games in which it depends on equilibrium logic or three rounds of iterated dominance (Table VI). But despite low compliance in our most complex games, Baseline and OB subjects reveal considerable strategic sophistication, with 14% estimated to be Equilibrium and 51% estimated to be Sophisticated (Table VII).

Perhaps the most surprising aspects of our findings so far are that Sophisticated subjects outnumber Equilibrium subjects by nearly four to one, and that *non*compliance with equilibrium in complex games far exceeds our estimates of the numbers of unsophisticated subjects: 35% of our Baseline and OB subjects are estimated to be types other than Equilibrium or Sophisticated, while noncompliance in our most complex games (those solvable by three rounds of iterated dominance or with unique equilibria but no dominance) ranges from 48-89% (= 11-52% compliance in Table VI).

It is instructive to examine these results more closely. Our Equilibrium and Sophisticated types' predicted decisions are separated only in games 5A, 5B, 8A, and 8B for Row players and in games 6B, 7A, and 7B for Column players (Table I). These games are essentially all of our most complex games in which the player in question has three decisions, and essentially all of the games for which equilibrium compliance was worse than random, which is what yields the separation. [46] Speaking approximately, our Equilibrium subjects were those who chose equilibrium decisions in those games despite the low compliance rates, while our Sophisticated subjects were those who chose best responses to their partners' prevailing non-equilibrium decisions. Thus, the predominance of Sophisticated over Equilibrium subjects suggests that many of our subjects anticipated the pattern of compliance and

[45]Reestimating with type-independent error rates changes three Baseline subjects' estimated types (one from Altruistic to Naïve/Optimistic and two from Altruistic to Sophisticated), no OB subjects', and one TS subject's (from Naïve/Optimistic to Equilibrium). Reestimating with idiosyncratic error rates yields exactly the same type estimates as with type-independent error rates. These relatively minor changes suggest that our type estimates are quite robust.
[46]"Essentially" in each case because game 6A for Columns is excluded. The observed B+OB decision frequencies were such that for this game, a Sophisticated Column subject only slightly preferred his Equilibrium decision.

noncompliance in our games—an anticipation that required a better understanding of their partners' decisions than equilibrium analysis alone can supply.[47]

Why don't the high frequencies of Equilibrium or Sophisticated subjects in the Baseline and OB treatments yield high frequencies of equilibrium—and therefore self-justifying—decisions in our most complex games? Given Baseline and OB subjects' uniformly high compliance with dominance, this is unlikely to be due to failures of decision-theoretic rationality, and given TS subjects' success identifying equilibrium decisions in our most complex games, it is unlikely to be due to cognitive limitations or difficulties looking up payoffs via Mouselab. We believe our results are best explained by a failure of the common knowledge of rationality and/or mutual knowledge of strategies assumptions that underlie the deductive justification for equilibrium (Section 2).

In this view, many of our subjects are sophisticated, in the sense that they can predict others' decisions in games with a variety of strategic structures, and they choose best responses to their predictions. However, those subjects do *not* base their predictions on the assumption that all other subjects are sophisticated, and will therefore analyze the games the way they themselves do.[48] Instead they expect a certain proportion of unsophisticated subjects in the population, who deviate from equilibrium in certain kinds of games in predictable ways, and they also expect a certain proportion of sophisticated subjects, who anticipate those deviations.[49] These responses by sophisticated subjects raise the rate of equilibrium noncompliance above what one might expect, given the estimated frequencies of Equilibrium and Sophisticated subjects.

More generally, even a high frequency of sophisticated players may not justify the use of equilibrium analysis to describe behavior in all games: To do well a player must both respect the limitations of equilibrium analysis in describing unsophisticated players' decisions and take into account the likely heterogeneity of his partners' levels of sophistication.[50]

---

[47]The alternative explanation that the subjects estimated to be Sophisticated were following another decision rule that happened to produce Sophisticated decisions in those games is less plausible because our model allows for, and rejects, the leading alternative hypotheses that those subjects were Naïve, Optimistic, Pessimistic, or Altruistic.

[48]If it were common knowledge (and therefore correct) that all subjects are Sophisticated, their strategies would be mutual knowledge, and therefore, given their rationality, necessarily in equilibrium.

[49]In this respect our Sophisticated type resembles Stahl and Wilson's (1995) "worldly" and, especially, their "rational expectations" types. They estimate a high frequency (about 40%) of worldly subjects and a moderate frequency (about 20%) of "Naïve Nash" (like our Equilibrium) subjects, but, in contrast to our results, no "rational expectations" subjects.

[50]This reconfirms a point that has been made by many other experimental studies, including Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991), McKelvey and Palfrey (1992), Beard and Beil (1994), Nagel (1995), Stahl and Wilson (1995), Stahl (1996), Ho and Weigelt (1996), and Ho, Weigelt, and Camerer (1998).

5. Modeling Cognition and Information Search

Our analysis has so far been confined to subjects' decisions, but our main goal was to gain a deeper understanding of strategic behavior by studying subjects' underlying cognitive processes. Although we cannot observe cognition directly, the look-up data recorded via Mouselab in our Baseline and TS treatments allow us to study cognition indirectly, via subjects' information searches. However, any inferences we draw about cognition depend on its relationship to information search. Modeling this relationship raises difficult issues that to our knowledge have been considered only by C&J and Algaze [Croson] (1990).[51] In this section we discuss those issues and introduce the model of cognition and information search used in Section 6's analysis.

Recall that we view a subject's decisions in the 18 games he faces as determined by a single decision rule or *type* as defined in Section 2, subject to errors as modeled in Section 4.C. We imagine that a subject implements his type's predicted decision in each game via a cognitive process, during which he searches for the needed payoff information. More precisely, we suppose that his type first determines his information search, possibly with error, and then his type and information search, including any errors, jointly determine his decision, again possibly with error.

In modeling this process, we face two main problems. First, the relationships between a subject's type, cognition, information search, and decisions are not yet well understood; many of the factors that influence them are unobservable; and little theory exists to guide their specification. Second, the space of possible look-up sequences in our design is enormous, and the observed sequences are noisy and highly heterogeneous. Almost any aspect of subjects' information searches might be related to their decisions—Section 6.A's analysis and Table IX below suggest several possibilities—but an analysis that took their full richness into account would be intractable.

These circumstances make it difficult to specify a complete parametric model of subjects' information searches and decisions with any confidence, and suggest that any such specification is more than usually likely to introduce distortions. We therefore take a more conservative approach, which promises to be robust to the things we cannot specify with confidence.

[51]Related issues have been discussed in the computational complexity literature, particularly for iterated dominance, by Knuth, Papadimitriou, and Tsitsiklis (1988) and Gilboa, Kalai, and Zemel (1993), among others; but their analyses focus on identifying ways to "solve" a game in a number of operations that is polynomial in its size, which yields algorithms that seem to us too sophisticated to be descriptive of real players' cognitive processes.

We first introduce several hypotheses about how cognition influences information search, suggested by the results of C&J's control treatment and our TS treatment. We then derive the implications of our most basic hypotheses for our types' look-up patterns, showing that they are strongly separated across types. In Section 6.B we generalize Section 4.C's model of decisions by conditioning each type's error rate on the level of compliance with the implications for that type's look-up patterns. The resulting model allows us to use the information in individual subjects' look-up patterns, as well as their decisions, to estimate their types, while imposing only minimal restrictions on the process that determines subjects' look-up patterns. Finally, we define 13 additional measures on look-up sequences and use our hypotheses to derive our types' implications for those measures, showing that they too are separated across types. In Section 6.A we use those measures to conduct an aggregate analysis, showing how subjects' information searches vary, on average, across their types as estimated from decisions alone in Section 4.C. This approach complements Section 6.B's econometric analysis while imposing less structure, conveying more of the information in the look-up data, and indicating the possibilities for further analyses.

We begin our discussion with some background and terminology. Identifying one of our types' predicted decision in one of our games requires a set of *comparisons*, or sometimes one of several alternative sets whose elements may not be interchangeable. For instance, depending on the structure of the game, our Equilibrium type's predicted decision can be identified by checking for dominance among its own and/or the other player's decisions, checking for iterated dominance, checking directly for pure-strategy equilibria, or some combination of those methods. [52] Referring to Figure 1, those operations involve the following kinds of comparisons:

1. Checking for dominance among one's own decisions requires a set of up-down comparisons of the payoffs labeled "Your points" in each of the left-hand columns.

2. Checking for dominance among the other player's decisions requires a set of left-right comparisons of the payoffs labeled "Her/His points" in the right-hand portions of each row.

3. Checking for iterated dominance requires alternating searches as in 1 and 2, in either order, possibly omitting the rows and/or columns of decisions previously identified as dominated.

[52] Because subjects had no reason to believe equilibrium was unique, our Equilibrium and Sophisticated types may need to be sure they have identified all equilibria (Section 3.A). Our discussion is simplified using the facts that our designs avoid ties in comparisons, multiple equilibria, and games for which plausible predictions involve mixed strategies.

4. Checking directly for pure-strategy equilibria requires one of two alternative sets of comparisons. One can either check each possible decision combination separately or use what we shall call *best-response dynamics*, which rules out some combinations using the fact that only decisions that are best responses can be part of an equilibrium. Checking a particular combination, say (#, &), requires an up-down comparison of the payoffs r#& and r*& and a left-right comparison of the payoffs c#& and c#@. Checking via best-response dynamics requires only a subset of the look-ups and comparisons required to check each combination separately, described below.

The comparisons in 1-4 are all pairwise ordinal payoff comparisons. As explained below, identifying our Sophisticated, Naïve, Optimistic, Pessimistic, and Altruistic types' predicted decisions also requires only pairwise ordinal payoff comparisons, with minor exceptions. [53] We identify each type's cognitive process with the sets of look-ups and comparisons it requires in our games. Our inferences about cognition depend on how the required look-ups and comparisons are realized in look-up sequences. We presume that a subject's look-up sequence is determined by his type's required look-ups and comparisons, his costs of memorization and search, his memory limitations, and how efficiently he searches. Instead of modeling this relationship, we structure our inferences about cognition via five hypotheses about how cognition influences information search.

Each of our hypotheses has separate implications for each of our types, which differ across games with different strategic structures. Our two most basic hypotheses, A and B, reflect our types' minimal look-up and comparison requirements, to avoid arbitrarily imputing inconsistency to subjects whose cognitive processes we cannot observe. Our presumption that a subject's type first determines his information search, possibly with error, and then his type and information search jointly determine his decision, again with error, suggests that a subject's error rate should decrease as his compliance with his type's hypotheses A and B increases, other things equal. Accordingly, hypotheses A and B play leading roles in Section 6.B's individual-level econometric analysis, where we test this implication of our theory. Our three subsidiary hypotheses, A', A'', and B', are stochastic counterparts of hypotheses A and B, used only to derive benchmark predictions about how our look-up measures vary across types, in

[53] This is true of most notions in normal-form noncooperative game theory, but not all (e.g. risk-dominance). The exceptions are that a Sophisticated or Naive subject may compute his decisions' expected payoffs via left-right look-ups in own payoffs, which requires ternary or quaternary payoff comparisons when his partner has three or four decisions, and an Altruistic subject needs to add his and his partner's payoffs and compare the totals across decision combinations.

preparation for Section 6.A's aggregate analysis. [54] Hypotheses A, A', and A'' restrict the look-ups that appear in the look-up sequence of a subject of a given type in a given game, while hypotheses B and B' restrict the adjacent look-up pairs that appear in the sequence, in each case otherwise without regard to the order of look-ups.

Hypothesis A: For a given type, in a given game, each look-up needed to identify the type's predicted decision must appear at least once in the look-up sequence for that game.

We call the look-ups in the set (or in any of the alternative sets, if there are more than one) required by hypothesis A for a type in a game the type's *relevant look-ups* for that game.

Hypothesis A should be uncontroversial, because a subject who does not make all of the look-ups it requires for a type cannot identify the type's predicted decision with certainty—although a clever subject might be able to identify it with high probability, using our publicly announced bounds on possible payoffs. Hypothesis A's discriminatory power is limited, however, because it is likely to be satisfied by chance even for moderately long look-up sequences. For instance, our TS subjects satisfied hypothesis A for the Equilibrium type in 98% of the game-subject pairs: 97% of those in which they made the equilibrium decision and 100% of the few pairs in which they did not.

Hypothesis B: For a given type, in a given game, hypothesis A must be satisfied, and each comparison needed to identify the type's predicted decision must be represented by an adjacent look-up pair at least once in the look-up sequence for that game.

We call the comparisons required by hypothesis B for a type in a game the type's *relevant comparisons* for that game.

Hypothesis B would be a poor approximation if subjects simply scanned and memorized all of a game's payoffs before analyzing it, because the order of their look-ups would then be unrelated to their relevant comparisons. But hypothesis B is a good approximation if repeated look-ups are less costly than memory, so that subjects usually perform comparisons one at a time, acquiring the information for each comparison by adjacent look-ups, storing the results in the simplest form that suffices for the rest

[54] Our original proposal for these experiments discussed a hypothesis C, which combined hypothesis B with the requirement that the look-up policy is *efficient*, in that it minimizes the expected total number of look-ups (and, in particular, avoids unnecessary look-ups). This hypothesis was also suggested by C&J's control subjects, whose look-ups were usually in the last-period-first order that minimized the total number needed to identify their subgame-perfect equilibrium offers. Hypothesis C implies potentially useful restrictions on the order of subjects' comparisons, but we omit it because its implications for our types are subtle, and seem unlikely to be satisfied often enough to be useful.

of their analysis, and otherwise relying on repeated look-ups rather than memory. [55] The results of C&J's control treatment and our TS treatment suggest that hypothesis B is a good approximation for our Baseline subjects.[56] Our TS subjects, whose environment was less conducive to memorization than C&J's (with 8-16 payoffs versus three pie sizes), looked up most of the payoffs in each game repeatedly, and 50% of the adjacent pairs in their look-up sequences corresponded to comparisons relevant for the Equilibrium type, close to the maximum possible number given that each look-up (except the first and last) belongs to two adjacent pairs.

Hypothesis B is more controversial than hypothesis A, because memory might allow a subject to identify his type's predicted decision with certainty even if his relevant comparisons are not all represented by adjacent look-ups. Accordingly, we treat it more conservatively in Section 6.B's econometric analysis. Hypothesis B is important because it is less likely than hypothesis A to be satisfied by chance, and therefore has greater discriminatory power. Our TS subjects, for instance, satisfied Equilibrium hypothesis B in 89% of the game-subject pairs: 94% of those in which they made the equilibrium decision and 47% of those in which they did not.

We supplement hypotheses A and B with three subsidiary hypotheses.

Hypothesis A': For a given type, in a given game, the type's relevant look-ups appear more frequently in the look-up sequence, on average, than other look-ups, hence more frequently than in a random sequence with the same total number of look-ups.

Hypothesis A": For a given type, in a given game, the type's relevant look-ups have longer average look-up durations, or *gaze times*, than other look-ups.

[55]Hypothesis B reflects our inability to use Mouselab to distinguish look-up pairs that are adjacent by coincidence from adjacent pairs associated with comparisons (Section 3.B). We interpret *simplicity* as follows: The ordinal ranking(s) of a pair (group) of payoffs is (are) simpler than the numerical payoffs, and a dominance relationship between two decisions or the fact that a decision combination is an equilibrium are simpler than the corresponding sets of payoff comparisons.

[56]Recall that C&J's subjects played three-period alternating-offers bargaining games whose subgame-perfect equilibria can be computed by backward induction, and that their control subjects were trained in backward induction and paid for correctly identifying their subgame-perfect equilibrium offers, at which they enjoyed great success. This task requires only pairwise ordinal payoff comparisons involving simple functions of the pie sizes, and is therefore similar to our Equilibrium type's task. (It is somewhat more complex because it involves calculating payoffs from the pie sizes and one's partners' offers, and then "folding back" the results.) C&J's control subjects usually looked up the third-period pie size first, then the second-period pie size, sometimes returning to the third period, and only then the first-period pie size, with most transitions from later to earlier periods. C&J's baseline subjects' look-up patterns are also consistent with this view, under a plausible interpretation of their behavior as influenced by fairness (Crawford (1997, Section 4.1)).

Hypothesis B': For a given type, in a given game, adjacent look-up pairs associated with the type's relevant comparisons appear more frequently, on average, than other adjacent pairs, hence more frequently than in a random sequence with the same total number of look-ups.

Hypotheses A' and B' are stochastic counterparts of hypotheses A and B. Although they are not logically weaker—a subject might, for example, make all of the look-ups required by hypothesis A while making many more irrelevant look-ups, thus violating the relative frequency requirement of hypothesis A'—they seem more likely to be satisfied in noisy data. Hypothesis A'' extends the intuition of hypothesis A' from look-up frequencies to gaze times, on the theory that relevant payoffs may evoke longer look-up durations as well as more frequent look-ups. [57]

We now discuss the implications of our hypotheses. We begin by describing the implications of hypotheses A and B, characterizing the minimal sufficient set or sets of look-ups or adjacent pairs consistent with those hypotheses for each of our types and each kind of strategic structure in our design. Some additional details and illustrations are given in Appendix D.

Equilibrium type: As noted above, depending on the strategic structure, the Equilibrium type can identify its predicted decision by checking for dominance or iterated dominance; checking directly for pure-strategy equilibrium, either decision combination by decision combination or via best-response dynamics; or some combination of those methods. For our games, the minimal set or sets of sufficient look-ups or adjacent pairs have a simple characterization that depends only on whether or not the game is dominance-solvable.

In our dominance-solvable games there is only one way to perform iterated dominance, and the sets of look-ups or comparisons it requires are always contained in the sets that checking directly for equilibrium (by either method) requires. We can therefore identify the implications of Equilibrium hypotheses A and B with the sets of look-ups and comparisons required for iterated dominance. Equilibrium hypothesis A then requires that the look-up sequence include all look-ups associated with the dominance and iterated dominance relationships by which the game can be solved, namely all payoffs for the own or other player's decisions being compared except those that can be eliminated using dominance relationships identified elsewhere in the iteration. Equilibrium hypothesis B then

[57]Standard decision-theoretic notions have no implications for gaze time because they focus on the information look-ups reveal, which is independent of gaze time provided that (as here) it suffices for comprehension. However, subjects might make irrelevant look-ups out of curiosity, and these may have shorter gaze times than relevant look-ups. Irrelevant look-ups could also have longer gaze times, if subjects make relevant comparisons via brief, frequently repeated look-ups.

requires that each of the associated comparisons is represented by an adjacent look-up pair in the sequence, namely all up-down pairs of own payoffs or left-right pairs of other's payoffs for the decisions being compared except pairs that can be eliminated using dominance relationships identified elsewhere in the iteration.

In our non-dominance-solvable games, there is never any pure-strategy dominance, and the sets of look-ups or comparisons required to check for equilibrium via best-response dynamics are always contained in the sets required to check decision combination by decision combination. We can therefore identify the implications of Equilibrium hypotheses A and B with the sets of look-ups and comparisons required for best-response dynamics. Equilibrium hypothesis A then requires that the look-up sequence include all of the look-ups associated with up-down comparisons of own payoffs or left-right comparisons of other's payoffs starting from each possible decision combination, except those that can be eliminated as never best responses. Equilibrium hypothesis B then requires that each of the associated comparisons is represented by an adjacent look-up pair in the sequence, namely all up-down pairs of own payoffs or left-right pairs of other's payoffs for each possible decision combination, except those that can be eliminated as never best responses.

Sophisticated type: For the Sophisticated type, the characterization depends only on whether or not the player has a dominant decision.

If the player has a dominant decision, and he happens to choose look-ups (for hypothesis A) or comparisons (for hypothesis B) that identify it, they identify his predicted decision. Sophisticated hypothesis A then requires only that his look-up sequence include all look-ups needed to identify his dominant decision, and Sophisticated hypothesis B requires only that each of the associated comparisons is represented by an adjacent look-up pair in the sequence, namely all up-down pairs of own payoffs for the decision comparisons needed to identify his dominant decision.

If the player does not have a dominant decision (whether or not the game is dominance-solvable), the player needs to form his beliefs and compare the expected payoffs of (at least) his undominated decisions. Although *we* estimate those beliefs from the observed decision frequencies in our experiment (Section 4.C), a Sophisticated player must deduce them from his knowledge of the structure of the game and other players' typical responses to games with that structure. We take the position that such deductions require him to identify all of the game's dominance and iterated dominance relationships and its set of equilibria, because results in the literature (and our own results) make it clear that subjects' responses generally depend on them (Crawford (1997, Sections 4-5); Table VI). The player can then make the expected-payoff comparisons required to identify his best response via running totals, either by up-down comparisons column by column or left-right comparisons row by row; we allow either method, but rule out hybrids. Sophisticated hypothesis A then requires that the look-up sequence include all of the player's own and the other player's payoffs, because they are all relevant to forming his beliefs and/or comparing the expected payoffs of his decisions. Sophisticated hypothesis B then requires that his look-up sequence include the same adjacent pairs as Equilibrium hypothesis B, plus any additional adjacent pairs needed to identify the dominance relationships among his own decisions.[58] His look-up sequence must also include a complete set of the adjacent pairs associated either with all up-down comparisons of his own payoffs for his undominated decisions, or with all such left-right comparisons.

Naïve, Optimistic, and Pessimistic types: To identify his predicted decision, a Naïve player needs only to compare the expected payoffs of his decisions, given a uniform prior over the other player's decisions. This can be done via running expected-payoff totals, either by up-down comparisons column by column or left-right comparisons row by row; we allow either method, but rule out hybrids. [59] An Optimistic or Pessimistic player needs only to identify his maximax or maximin decision, respectively. The maximax decision can be identified by scanning all own payoffs in any desired order, with no restrictions on comparisons, keeping a record of the highest payoff found so far. The maximin decision, however, must be identified by left-right comparisons, as there is no reliable way to identify it by up-down comparisons.

For all three of these types, hypothesis A requires that the look-up sequence incl ude all the player's own payoffs and only those payoffs, with two exceptions: if an Optimistic player's look-ups include all of his payoffs for all but one of his own decisions and an own payoff for the remaining decision that is higher than his maximum payoff for the former decisions, then his look-up sequence need not include any more payoffs for the latter decision; and if a Pessimistic player's look-ups include all of his own payoffs for one of his decisions and an own payoff for another decision that is lower than

[58]Note that, for non-dominance-solvable games, the Equilibrium type might not need these additional pairs.

[59]A Naïve player may be able to avoid some of these expected-payoff comparisons by identifying dominance among his own decisions; this must be done by up-down comparisons, which have the same look-up requirements as comparing expected payoffs of own decisions by up-down comparisons. He may also be able to avoid some look-ups or comparisons by eliminating decisions that can be seen, using look-ups (for hypothesis A) or comparisons (for hypothesis B) that are in the sequence, to have lower expected payoffs than some other decision.

his minimum payoff for the former decision, then his look-up sequence need not include any more payoffs for the latter decision. The Naïve Hypothesis B requires that a complete set either of up-down comparisons or of left-right comparisons sufficient to compare the expected payoffs of the player's undominated decisions and/or to identify his dominated decisions is represented by adjacent pairs in the sequence. The Optimistic hypothesis B implies no restrictions beyond the Optimistic hypothesis A. The Pessimistic hypothesis B requires that a set of left-right comparisons sufficient to identify the maximin decision be represented by adjacent look-up pairs in the sequence.

Altruistic type: An Altruistic player needs only to compare the totals of his own and the other's payoffs for each of the possible decision combinations. The Altruistic hypothesis A therefore requires that his look-up sequence include all own and other's payoffs, and the Altruistic hypothesis B requires that each of the above totals be represented by an adjacent look-up pair in the sequence.

We now define 13 additional measures on subjects' look-up sequences and derive our types' implications for them under hypotheses A and B and our subsidiary hypotheses A', A'', and B'. The implications are derived game by game, then averaged over games because they are approximately the same for all games. The measures are: the average total numbers of look-ups per game in own and other's payoffs; the average numbers of consecutive look-ups, or *string lengths*, in own and other's payoffs; the average look-up durations, or *gaze times*, in own and other's payoffs, in seconds; the frequencies with which own payoffs are inspected first, and last; the frequencies of look-up transitions from own to own payoffs, and from other's to other's payoffs; the frequencies of up-down transitions in own payoffs and left-right transitions in other's payoffs, conditional on remaining in own or other's payoffs, respectively; and the frequency of *altruistic* transitions—those from a given decision combination in own payoffs to the same combination in other's payoffs, or vice versa.

Table VIII lists the 13 measures and our types' theoretical implications for them, with the relevant hypothesis indicated at the top of each column and the implications of *random* look-ups as a benchmark.[60] Table VIII shows that the measures can be expected to differ systematically across types, separating them into three main groups, with differences across groups large enough to have a chance of showing up even in aggregate data. The first group includes our two "game-theoretic" types,

[60]*Random* look-ups are defined as independently and uniformly distributed given their total number, which is set equal to the observed total for each game-subject pair and then treated as exogenous.

Equilibrium and Sophisticated. The second includes our solipsistic types, Naïve, Optimistic, and Pessimistic. The third consists of our other-regarding but non-strategic Altruistic type.

We close this section by sketching the arguments for the implications in Table VIII. Readers uninterested in the details can skip ahead to Section 6 without loss of continuity.

The implications about the minimal numbers of look-ups in own and other's payoffs follow from hypothesis A. The Sophisticated type has a higher minimal number for own payoffs than the Equilibrium type because a Sophisticated player, in forming his beliefs, may need to check for dominance relationships among his own strategies that may influence his partner's response but are irrelevant to an Equilibrium player. The differences in the Sophisticated and Equilibrium types' minimal numbers of look-ups across own and other's payoffs stem from the different implications of dominant strategies for eliminating required look-ups. [61] The minimal numbers of own look-ups for the Naïve and Optimistic types are the same as for the Sophisticated type because all three types must look up all own payoffs unless they happen to discover that they have a dominant decision, with a minor exception for the Optimistic type concerning bounds based on payoff information available in the look-up sequence, noted above. The minimal number of own look-ups for the Pessimistic type is lower because that type can avoid the need for some look-ups more often, using bounds like those just noted. There are no implications for other's look-ups for the Naïve, Optimistic, and Pessimistic types because other's look-ups are irrelevant for those types.

Table VIII's implications for minimal average string lengths follow from hypothesis B'. Average string length would approach two for long sequences of random look-ups if transitions to the same payoff were as likely as to other payoffs, because there are as many own as other's payoffs. Our subjects, however, almost never returned immediately to the same payoff. [62] If we elevate this empirical regularity to an assumption, average string length approaches a limit less than two, which depends on the numbers of decisions, as the total number of look-ups increases. An easy calculation assuming equal numbers of look-ups in each game and averaging over games yields a limiting average string length of 1.82 for random look-ups. Our subjects' look-up sequences were long enough to make this an appropriate benchmark. Hypothesis B' implies average string lengths at least as long as random for the Equilibrium, Sophisticated, Naïve, Optimistic, and Pessimistic types, for which no relevant comparisons

[61] Random look-ups have no implications for the minimal numbers of look-ups, because they take the total numbers as given. As explained in Appendix D, hypothesis B implies no further restrictions on minimal numbers of look-ups.

cross the boundary between own and other's payoffs; and at most as long as random for the Altruistic type, for which all relevant comparisons do so.

Table VIII's implications for gaze times follow immediately from hypothesis A". The implications about the frequencies of inspecting own payoffs first and last follow from hypothesis A', on the assumption that first and last look-ups are, like other look-ups, more likely than not to be relevant. Random first and last look-ups are of course equally likely to be of own and other's payoffs. For the Equilibrium, Sophisticated, and Altruistic types, hypothesis A' implies no presumption about first look-ups, because both own and other's payoffs are usually relevant for those types. However, for the Equilibrium and particularly the Sophisticated type, last look-ups are more likely than not to be of own payoffs: For the Equilibrium type, the last piece of information relevant to identifying his predicted decision via iterated dominance is an own payoff, while other methods are neutral on this point; and for the Sophisticated type, the last piece of relevant information is always from an expected-payoff or dominance comparison of own decisions. For the Naïve, Optimistic, and Pessimistic types, hypothesis A' implies that first and last look-ups are more likely than not to be of own payoffs, because only own payoffs are relevant to those types. For the Altruistic type, hypothesis A' implies no presumption about first or last look-ups.

Table VIII's implications for the frequencies of transitions from own to own and other's to other's payoffs, of up-down transitions in own payoffs and left-right transitions in other's payoffs, and of altruistic own-to-other's transitions, all follow from hypothesis B'. For random look-ups the expected frequencies of those transitions, averaged across games, can be shown to be 45.0%, 45.0%, 30.6%, 30.6%, and 10.0%, respectively, again assuming that subjects never return immediately to the same payoff. Just as for string lengths, hypothesis B' implies that relevant comparisons are represented by adjacent pairs with frequencies at least as great as random.

It follows that the frequencies of own to own and other's to other's payoff transitions for the Equilibrium and Sophisticated types; of up-down in own and left-right in other's payoff transitions for the Equilibrium and Sophisticated types; of own to own transitions for the Naïve, Optimistic, and Pessimistic types; and of Altruistic own to other's transitions for the Altruistic type should all be at least random, because the associated comparisons are all more likely than not to be relevant. The frequencies of up-down in own transitions for the Pessimistic type should be at most random, because left-right in

[62]We can distinguish "returning" from "staying" because clicking was required to open *and* close boxes (Section 3.B).

own transitions are more likely than not to be relevant comparisons for that type. The frequencies of Altruistic own to other's transitions should be at most random for all types other than Altruistic, because the associated comparisons are irrelevant for those types. The frequencies of up-down in own transitions for the Naïve type should be approximately random because Naïve predicted decisions can be identified equally well by left-right or up-down comparisons in own payoffs. Finally, there is no presumption about the frequencies of up-down in own transitions for the Optimistic type; left-right in other's transitions for the Naïve, Optimistic, or Pessimistic types; or up-down in own and left-right in other's transitions for the Altruistic type, because there are no relevant comparisons of those kinds for those types.

6. Analysis of Subjects' Decisions and Information Search

The look-up data recorded via Mouselab in our Baseline and TS treatments allow us to evaluate subjects' compliance with the information search implications of each type's hypotheses A and B, and to compute the other measures defined in Section 5. [63] In this section we analyze the relationship between subjects' decisions and information searches in the light of the theory of cognition and information search outlined in Section 5. In Section 6.A we examine aggregate patterns in subjects' information searches, and in Section 6.B we generalize Section 4.C's econometric model of decisions to study the relationship between decisions and information search at the individual level. The generalized model allows more stringent tests of theories of strategic behavior, yields improved estimates of subjects' types, and allows us to assess the extent to which conditioning on their information searches allows better predictions of their decisions.

A. Aggregate information search patterns

Table IX summarizes Section 5's information search measures for the Baseline and TS treatments; definitions of the measures are given in Section 5's discussion of Table VIII, whose column headings are the same as Table IX's. (Aggregate compliance with our types' hypotheses A and B is reported in Table X, discussed below.) The measures are aggregated across all 18 games, first with all subjects in each treatment pooled and then with Baseline subjects disaggregated by their types as estimated from their decisions alone (Section 4.C, Table VII). [64]

[63]The look-up data from the Baseline and TS treatments are available from the authors on request.
[64]The measures were computed by first computing an (unweighted) average for each subject over all 18 games and then

Several interesting patterns are apparent even at this level of aggregation. In the top part of Table IX, we note that TS subjects have more own and other's look-ups, longer string lengths, and shorter gaze times than Baseline subjects, all of which suggest that TS subjects perform more systematic analyses. TS subjects also have many more own up-down and other's left-right transitions, both characteristic (under hypothesis B) of most algorithms for identifying equilibrium or sophisticated decisions, and more generally of strategic thinking in the normal form (Section 5, Appendix D). These differences suggest that the methods theorists use to analyze normal-form games may not emerge spontaneously without training in game theory. We also note the predominance of other's left-right over own up-down transitions for both TS and Baseline subjects, which suggests that our display generates some bias in favor of left-right look-ups (Section 3.B).

In the bottom part of Table IX, we note that the information search measures for Baseline subjects disaggregated by estimated type are often several times higher than Table VIII's theoretical bounds (Section 5), but that the two vary across types roughly in proportion. Altruistic and Sophisticated subjects have systematically more own and other's look-ups than Equilibrium, Naïve/Optimistic, and Pessimistic subjects, with a particularly striking difference between Sophisticated and Equilibrium subjects. Altruistic and Equilibrium subjects have shorter string lengths and fewer own-to-own and other's-to-other's transitions than other subjects. Not surprisingly, Altruistic subjects excel in Altruistic own-to-other's transitions. For every type, there are more own than other's look-ups and (except for Altruistic) longer own than other's gaze times, with the largest look-up differences for Naïve/Optimistic and Pessimistic and the largest gaze time differences for Equilibrium and Naïve/Optimistic. Own payoff first exceeds 60% for every type, and own payoff last exceeds 70% for all types but Pessimistic and Altruistic. Curiously, Equilibrium and Sophisticated subjects have similar frequencies of own up-down transitions, both higher than the frequencies of Naïve/Optimistic, Pessimistic, and Altruistic subjects; but they have very different frequencies of other's left-right transitions, with Equilibrium subjects more closely resembling TS (and Pessimistic and Altruistic) subjects than Sophisticated subjects in this measure.

In describing subjects' compliance with hypotheses A and B, both here and in Section 6.B's econometric analysis, we use discrete categories for tractability. For each subject, type, and game, we first compute the percentages of the type's look-ups required by hypothesis A, and of the type's adjacent

---

computing an (unweighted) average over subjects.

look-up pairs required by hypothesis B, that appear at least once in the subject's look-up sequence for the game.[65] We then sort those percentages into mutually exclusive and collectively exhaustive categories as follows. For the Optimistic type, whose hypothesis B is vacuous, we use two categories: A and ~A, meaning 100% and anything less than 100% compliance with Optimistic hypothesis A for the game and subject in question. For the other types we use five categories: B_1, 100% compliance with the type's hypotheses A and B; B_H, 100% compliance with hypothesis A and 67-99% compliance with hypothesis B; B_M, 100% compliance with hypothesis A and 34-66% compliance with hypothesis B; B_L, 100% compliance with hypothesis A and 0-33% compliance with hypothesis B; and ~A, anything less than 100% compliance with hypothesis A.[66]

Our categorization uses a coarser grid for hypothesis A than for hypothesis B, in effect assuming that compliance with a type's hypothesis B is meaningless without 100% compliance with its hypothesis A. This is a reasonable simplification because a subject who does not make all of the look-ups a type's hypothesis A requires cannot identify that type's predicted decision with certainty, while memory may allow a subject to identify a type's predicted decision with certainty by comparisons that are not all represented by the adjacent pairs hypothesis B requires (Section 5).

Table X summarizes TS and Baseline subjects' aggregate rates of compliance with our types' hypotheses A and B. Compliance rates are first calculated and categorized for each subject, type, and game as just described, and then aggregated across games and subjects as indicated in the table. The top part of the table gives overall compliance in the TS, TS (ex.), and Baseline treatments; and the bottom part gives compliance in the Baseline with subjects disaggregated by types as estimated from decisions alone in Section 4.C (Table VII). In reading the table, it may help to focus on the first and fifth numbers in each entry, the compliance rates for B_1 and ~A, subtracting the ~A entry from 100% to get the rate for hypothesis A (without regard to the level of compliance with B).

The overall results in the top part of Table X show that TS and Baseline subjects differ very sharply in compliance with Equilibrium hypotheses A and B (and to some extent with Sophisticated hypothesis B), but they differ hardly at all in compliance with other types' hypotheses A and B.

[65]We verify compliance game by game because this is more robust to noise in the look-up data. Few subjects satisfied any type's hypothesis B perfectly in all 18 games, although many satisfied hypothesis A for most types in all games.
[66]100% compliance with a type's hypothesis B implies 100% compliance with its hypothesis A, but less than 100% compliance with hypothesis B implies no simple restrictions on compliance with hypothesis A. Although we describe compliance as a continuous percentage, it too is discrete. Hypothesis B, for instance, requires between 0-8 comparisons for different games and types. This discreteness makes the precise locations of the boundaries between our categories

The Baseline results with subjects disaggregated by estimated type in the bottom part of the table suggest that hypothesis A by itself doesn't discriminate very well, mainly because most subjects comply with it for most types in most games. By contrast, category B_1 (100% compliance with both hypotheses A and B) discriminates well even at this aggregate level. The 23 Baseline subjects estimated to be Sophisticated, for instance, satisfy Sophisticated hypotheses B_1 and A in 26% and 81% (= 100% - 19%) of game-subject pairs, respectively. These compliance rates are higher than compliance with Sophisticated hypotheses B_1 and A for any other estimated type but Altruistic (six subjects), with 26% and 82%, and (for A) Pessimistic (one subject), with 6% and 100%. Similar comparisons show that subjects whose estimated types are Naïve/Optimistic, Pessimistic, and Altruistic each have higher compliance with their type's hypotheses A and B_1 than subjects of any other estimated type, with isolated exceptions involving the one Pessimistic subject. The discriminatory power of category B_1 is weakest for subjects estimated to be Equilibrium, who satisfy Equilibrium hypotheses B_1 and A in 32% and 84% of game-subject pairs, respectively, slightly less than Sophisticated subjects, with 35% and 86%; Altruistic subjects, with 35% and 85%; and (for A) the one Pessimistic subject, with 11% and 100%.[67]

B. Generalized error-rate analysis of decision rules and information search

In this section we generalize Section 4.C's maximum likelihood error-rate analysis to study the relationship between decisions and information search at the individual level. The generalized model differs from Section 4.C's by conditioning each type's error rate on the level of compliance with that type's implications for look-up patterns under our hypotheses A and B. Compliance is evaluated for each subject, type, and game and categorized discretely as explained in Section 6.A. The model is otherwise identical to Section 4.C's, and ignores all other aspects of subjects' look-up sequences. It allows us to use the information in subjects' look-up patterns along with their decisions to estimate their types, while imposing minimal structure on subjects' information searches. Like Section 4.C's model, it provides a coherent framework in which to combine information from observations that are consistent with predicted behavior for more than one type.

---

unimportant. It also happens to make the Pessimistic type's category B_H vacuous, as indicated in Tables X and XI.

[67]The contrast between these results and the fact that Equilibrium category B_1 sharply separates TS from Baseline subjects in the top part of Table X suggests that there are some important differences between TS subjects and "naturally occurring" (Baseline) Equilibrium subjects, which are worth investigating further.

As before, let $i = 1, \ldots, N$ index the subjects in a given treatment; let $c = 2, 3$, or 4 be the number of decisions a subject has in a given game; and let $k = 1,\ldots,K$ index our types. We assume that each subject is one of the types used in Section 4.C's analysis, defined in Section 2, but we now separate our Naïve and Optimistic types, which have different implications for information searches (though not for decisions). K, the number of distinct types, is therefore now six instead of five .

We index a subject's level of compliance with a given type's hypotheses A and B in a given game by j, where $j = 1, H, M, L$, or 0 as his look-up sequence is in category B_1, B_H, B_M, B_L, or ~A. All summations over j are taken over the values 1, H, M, L, and 0. A subject whose compliance with type k's hypotheses A and B in a given game is in category j will be said to have *type-k compliance j* in that game. For the Optimistic type, whose hypothesis B is vacuous, we identify hypothesis A (logically the union of B_1, B_H, B_M, and B_L) with B_L, so compliance with Optimistic hypothesis A is coded as $j = L$. This choice is arbitrary, but it has substantive implications only when we test the restriction that error rates are type-independent, and those implications are minor even then.

We again assume that a subject normally makes his type's predicted decision, but we now assume that in each game, there is a given probability, e_{kj} ? [0, 1], that a subject of type k with type-k compliance j makes an error, in which case he makes each of his c decisions with probability $1/c$. The probability that such a subject makes type k's predicted decision is then $(1 - e_{kj}) + e_{kj}/c = 1 - (c - 1)e_{kj}/c$, and the probability that he makes any single unpredicted decision is e_{kj}/c. We assume that errors are i.i.d. across games and subjects, so that each e_{kj} is constant. As in Section 4.C's model we allow type-dependent error rates, which is important given our types' very different cognitive and informational requirements. The only innovation in the present model is that type k's error rate is now also allowed to depend on the level of type-k compliance, j. This reflects the implication of our theory of cognition and information search that a subject's error rate should decrease as his compliance with his type's hypotheses A and B increases, other things equal (Section 5). The model nests Section 4.C's model, for which $e_{kj} \overset{k}{\equiv} e_k$. Most of our types have five error rates, corresponding to the categories $j = 1$, H, M, L, or 0. But because the Optimistic type's hypothesis B is vacuous it has only two error rates, e_{kL} for $A = B_L$ and e_{k0} for ~A; and because the Pessimistic type's category B_H is vacuous (fn. 66) it has only four error rates, e_{k1}, e_{kM}, e_{kL}, and e_{k0} for B_1, B_M, B_L, and ~A.

Let T_{ck}^{ij} denote the total number of games in a given treatment for which subject i has c decisions and type-k compliance j, and let x_{ck}^{ij} denote the total number of such games in which subject i makes type k's predicted decision. Because our error structure distinguishes neither different unpredicted nor different predicted decisions for a given c, i, j, and k, the T_{ck}^{ij} and x_{ck}^{ij} are sufficient statistics. Letting $x_k^i \equiv [x_{ck}^{ij}]$, $x^i \equiv (x_1^i,...,x_K^i)$, $x \equiv (x^1,...,x^N)$, $T_k^i \equiv [T_{ck}^{ij}]$, $T^i \equiv (T_1^i,...,T_K^i)$, $T \equiv (T^1,...,T^N)$, $e \equiv [e_{kj}]$, and $e_k \equiv (e_{k1}, e_{kH}, e_{kM}, e_{kL}, e_{k0})$, the probability of observing a sample with T_{ck}^{ij} and x_{ck}^{ij} when subject i is of type k is:

(6.1) $$L_k^i(e_k \mid x_k^i, T_k^i) = \prod_j \prod_{c=2,3,4} \{[1-(c-1)e_{kj}/c]^{x_{ck}^{ij}} [e_{kj}/c]^{T_{ck}^{ij}-x_{ck}^{ij}}\}.$$

Again letting $d \equiv (d^1,...,d^N)$, where $d^i \equiv (d_1^i,...,d_K^i)$, with d_k^i = 1 if subject i is of type k and d_k^i = 0 otherwise, subject i's contribution to the likelihood function is:

(6.2) $$L^i(e, d^i \mid x^i, T^i) = \prod_{k=1}^{K} L_k^i(e_k \mid x_k^i, T_k^i)^{d_k^i}.$$

Taking logarithms and summing over i yields the log-likelihood function for the entire sample:

(6.3) $$\ln L(e, d \mid x, T) = \sum_{i=1}^{N} \ln L^i(e, d^i \mid x^i, T^i)$$

$$= \sum_{i=1}^{N} \sum_{k=1}^{K} d_k^i \sum_j \sum_{c=2,3,4} \{x_{ck}^{ij}[\ln(1-(c-1)e_{kj}/c) - \ln(e_{kj}/c)] + T_{ck}^{ij} \ln(e_{kj}/c)\}.$$

(6.3) implies that the maximum likelihood estimate of d_k^i identifies the type k for subject i that maximizes a weighted sum of the x_{ck}^{ij}, with the weights for different c, j combinations determined by the estimated error rates e_{kj}.[68] The weights of the x_{ck}^{ij} are again nonnegative and increasing in c. As in Section 4.C, they reflect the extent to which a predicted decision is estimated to have been more likely

[68]The estimate of e_{kj} is again a kind of weighted average of the sample error frequencies when c = 2,3, 4 for subjects estimated to be of type k who have type-k compliance j, with weights reflecting the probabilities that a predicted decision was made by chance, given c.

than an unpredicted decision, approaching 0 as e_{kj} approaches one, the value for which the probability of a predicted decision equals that of a single unpredicted decision.

In Section 4.C's analysis, the estimated error rates affected the type estimates only by determining the relative weights of predicted decisions for different values of c. Here the estimated e_{kj} play a much more important role, determining the relative weights of predicted decisions that occur with different levels of compliance with their types' information search implications. Other things equal, a type receives more credit for a correctly predicted decision when the estimated error rate given the observed level of compliance for that type is low. This feature of the model allows us simultaneously to estimate which aspects of subjects' look-up patterns are relevant in predicting their decisions, and to use this information in estimating subjects' types, in effect discounting correctly predicted decisions for a given type when they occur with the "wrong" kind of look-up pattern for that type. We stress that our specification allows but does not assume this, in that the "right" and "wrong" look-up patterns are determined endogenously by the estimated error rates. Except for sampling error the estimated error rates will be independent of aspects of compliance that do not affect subjects' decisions, so that our type estimates will effectively ignore such aspects. This is an important advantage of our approach, given how little is known about information search.

In general, our types' implications for decisions and information search work together to distinguish them, with decisions doing most of the work when types' predicted decisions are separated and information search taking up the slack when they are not. Even when predicted decisions are separated, information search plays an important role by determining the relative weights on predicted decisions that occur with different levels of compliance.

To see how this works more concretely, consider our Naïve and Optimistic types, whose decisions are not separated in our games, so that no analysis of decisions alone can distinguish them. The present analysis can distinguish them because their information search implications are separated, with a vacuous Optimistic hypothesis B but a restrictive Naïve hypothesis B. It may appear that the Naïve type must always be at a disadvantage in the estimation, relative to the Optimistic type, because the restrictive Naïve hypothesis B is harder to satisfy. However, that restrictiveness also makes it possible for compliance with Naïve hypothesis B to be statistically related to the frequency of a subject's Naïve/Optimistic predicted decisions. To the extent that this allows a subject's frequency to be higher for compliance levels that have lower estimated error rates, it is advantageous for the Naïve type, essentially because compliance with its hypothesis B is more useful than compliance with the vacuous Optimistic hypothesis B in predicting his decisions. [69]

Suppose, for simplicity, that both the Naïve and Optimistic types' hypotheses A are always satisfied in the sample, and therefore useless in distinguishing them. [70] A subject will then be estimated to be Naïve if, roughly speaking, his Naïve/Optimistic predicted decisions occur with lower Naïve than Optimistic error rates, on average. In our empirical analysis, this possibility is realized for one of the 10 Baseline subjects classified as Naïve/Optimistic in Section 4.C, who is reclassified as Naïve. However, three subjects not previously classified as Naïve/Optimistic are also reclassified as Naïve. Those subjects happened to make fewer Naïve/Optimistic predicted decisions than those of the 10 previously Naïve/Optimistic subjects for whom compliance with Naïve hypothesis B is weakly related to the frequency of Naïve/Optimistic decisions. As a result, the estimated error rates are higher, on average, for the Naïve type than for the Optimistic type. This yields lower weights on most Naïve/Optimistic decisions for the Naïve type, so that eight of the 10 previously Naïve/Optimistic subjects are reclassified as Optimistic (and one as Pessimistic).

The model has $N + 26$ parameters: one type for each of the N subjects plus five error rates (one for each compliance level j) for each of our six types except Optimistic and Pessimistic, which have two and four, respectively, as explained above. We computed maximum likelihood estimates of these parameters separately for the Baseline, TS, and TS (ex.) treatments, u sing standard algorithms . The results are summarized in Table XI, whose entries give the frequencies and numbers of subjects estimated to be of each type, the estimated error rates for each type and level of compliance, and, as a rough measure of the reliability of the error rate estimates, the percentages of the total number of observations (45 subjects x 18 games = 810) on which they are based.

In the Baseline treatment, the most frequent estimated type is again Sophisticated with 47% (21) of 45 subjects, and the next most frequent is now Optimistic with 18% (8), followed by Equilibrium, Naïve, Pessimistic, and Altruistic with 9% (4) each. Individual subjects ' estimated types are the same as

[69] It can be shown that maximum likelihood type and error-rate estimates are such that predicted decisions for a subject's estimated type tend to occur with levels of compliance for which that type's estimated error rate is low. This is only a tendency because in our model with type-dependent (but not idiosyncratic) error rates, the error rate estimates are determined by the error frequencies for *all* subjects of a given estimated type. A subject who is sufficiently unlike other subjects of his estimated type may have more frequent predicted decisions when his type's estimated error rate is high.

[70] This is not quite true in our analysis, where overall Baseline compliance with hypothesis A was 96% for both types. Although there are some differences between their hypothesis A implications that might be used to distinguish them, we ignore this possibility in favor of the much sharper difference in hypothesis B implications for clarity.

Section 4.C's, with the following exceptions: One of the five subjects estimated in Section 4.C to be Equilibrium, one of the 10 then estimated to be Naïve/Optimistic, and one of the six then estimated to be Altruistic are now estimated to be Pessimistic. Two of the 23 then estimated to be Sophisticated and one of the six then estimated to be Altruistic are now estimated to be Naïve. Finally, eight of the 10 then estimated to be Naïve/Optimistic are now estimated to be Optimistic, and one of those 10 is now estimated to be Naïve. Overall, six of our 45 subjects are reclassified by taking information search into account, and another nine are uniquely identified, resolving ambiguities in Section 4.C 's analysis of decisions alone.

The Baseline error rate estimates are generally consistent with the implications of Section 5's theory of information search, which suggests that higher compliance with a type's information search implications is associated with a lower error rate. There is one major anomaly, in that the error rate of 0.10 for Sophisticated subjects with the lowest level of compliance is much lower than the error rates of 0.24-0.29 for Sophisticated subjects with higher compliance, and is based on 9% of the sample, a nonnegligible fraction. There are also four minor anomalies, one each for the Equilibrium, Naïve, Pessimistic, and Altruistic types, based on 1%, 0%, 0%, and 1% of the sample. Otherwise, each type 's estimated error rates decrease with higher compliance as expected.

Comparing Tables VII and XI, this section's estimated error rate conditional on the highest level of compliance (B_1, or A for the Optimistic type) is higher than Section 4.C 's unconditional estimated error rate for two types, representing 8 out of 45 Baseline subjects: Naïve, at 0.61 versus 0.28 for Naïve/Optimistic; and Pessimistic, at 0.38 versus 0.20. The error rate conditional on high compliance is lower for the other four types, representing 37 Baseline subjects: Equilibrium, at 0.12 versus 0.35, Altruistic, at 0.14 versus 0.66, Optimistic, at 0.22 versus 0.28 for Naïve/Optimistic, and Sophisticated, at 0.24 versus 0.26.[71] The error rates for our Equilibrium, Altruistic, Optimistic, and Sophisticated subjects are very low, given that they reflect initial responses to games and are discounted for the probability that a predicted decision was made by chance (fn. 42); they imply that those subjects made their types' predicted decisions with probabilities from 0.82-0.94 for c = 2,3,4.

In the TS treatment the most frequent estimated type is again Equilibrium with 80% (12) of the 15 subjects, followed by Naïve with 13% (2) and Altruistic with 7% (1). In TS (ex.) 100% of the 12

[71]Except for changes in subjects' estimated types, these error rate estimates are based on the same frequencies of predicted decisions as Section 4.C's unconditional estimates, hence must approximately equal them, on average. Thus,

subjects are again estimated to be Equilibrium. **S**ubjects' type estimates are the same as Section 4.C's, except that two Naïve/Optimistic TS subjects are now identified as uniquely Naïve. Each type's estimated error rates decrease with higher compliance, with minor exceptions involving 0%, 2%, 1%, and 1% of the sample in TS, and 1% and 3% of the sample in TS (ex.)). The error rate for the highest level of compliance is lower than Section 4.C's unconditional error rate for each type: 0.05 versus 0.07 for Equilibrium, 0.75 versus 0.79 for Naive, and 0.47 versus 0.57 for Altruistic.

As in Section 4.C, likelihood ratio tests strongly reject the hypothesis of type-independent versus type-dependent error rates in the Baseline (p-value < 0.001) and TS (p-value < 0.001) treatments.[72] But likelihood ratio tests can no longer reject the hypothesis of type-dependent error rates versus error rates that are idiosyncratic across subjects in the Baseline (p-value 0.066), TS (p-value 0.196), or TS (ex.) (p-value 0.429) treatments.[73]

The level of compliance with our types' information search implications is statistically highly significant in both the Baseline and TS treatments. Allowing error rates to be type-dependent, likelihood ratio tests strongly reject the hypothesis that for each type they do not vary with the level of compliance in the Baseline (p-value 0.002), TS (p-value 0.017), and TS (ex.) (p-value 0.006) treatments. Together with the fact that our estimated error rates generally decrease with higher compliance, this shows that incorporating the cognitive implications of our types into an error rate analysis allows a coherent unified account of subjects' decisions and information searches.

Overall, this section's analysis confirms the view of subjects' decisions suggested by Section 4.C's analysis, with some adjustments. Conditioning on information search changes six of 45 Baseline subjects' type estimates, and allows us to identify another nine subjects previously estimated to be Naïve/Optimistic as uniquely Naïve (one) or Optimistic (eight). For other subjects conditioning on compliance yields unchanged type estimates but higher error rates when compliance is low, moving them toward the "random" type that our model implicitly allows. The error rate estimates also generally

---

part of the difference is an automatic consequence of the fact that the error rates are generally decreasing in compliance.

[72]In the TS (ex.) treatment this restriction is vacuously satisfied because all subjects were estimated to be Equilibrium. In the Baseline treatment, reestimating with type-independent error rates changes three subjects' estimated types, one from Altruistic to Sophisticated, one from Optimistic to Sophisticated, and one from Altruistic to Optimistic. In the TS Treatment, reestimating changes one subject's estimated type, from Naïve to Equilibrium.

[73]In the Baseline treatment, reestimating with idiosyncratic rather than type-dependent error rates changes 13 subjects' estimated types: six from Optimistic to Naïve, two from Naive to Sophisticated, one from Naïve to Altruistic, one from Altruistic to Sophisticated, two from Pessimistic to Naive, and one from Pessimistic to Equilibrium.

satisfy the restrictions suggested by our theory of information search, confirming the cognitive implications of our interpretation of subjects' decisions.

7. Conclusion

This paper reports the results of experiments in which subjects play a series o f 18 two-person normal-form games with varying strategic structures, using a computer interface that records their searches for hidden payoff information along with their decisions. The experimental design is structured to allow tests of deductive theories of behavior in games, including equilibrium analysis. We conclude by summarizing the lessons we think can be drawn from our analysis.

With regard to subjects' decisions, we find high rates of compliance with equilibrium in games that can be solved by one or two rounds of iterated dominance, as in previous experiments, but low compliance in more complex games in which it depends on equilibrium logic or three rounds of iterated dominance. The decision rules that best describe subjects' decisions over the games they played are highly heterogeneous. A substantial majority of subjects exhibited some strategic sophistication, in that their decisions appeared to reflect an analysis of their partners' incentives; and many subjects even seem to have anticipated the pattern of noncompliance with equilibrium in our more complex games. However, sophistication was neither extensive nor widespread enough to justify full reliance on equilibrium analysis. Our results suggest that this was due mainly to the sophisticated subjects' failure to assume that all other subjects are sophisticated.

Subjects' information searches were even more heterogeneous than their decision rules, providing an additional lens through which to examine the cognitive process that underlies their strategic thinking. We find systematic relationships between subjects' deviations from the search patterns suggested by equilibrium analysis and their deviations from equilibrium decisions. To structure our analysis of those relationships, we develop a unified theory of decisions and information search, based on six alternative decision rules with different implications for subjects' information searches and decisions. This theory provides the foundation for an econometric analysis of our results, which allows tests of game theory's cognitive implications along with its implications for decisions, and uses the same principles to explain subjects' decisions and information searches. The results lend credence to the view of strategic behavior that underlies our approach.

More generally, our analysis suggests that behavior in other kinds of games and decision problems might be better understood by searching for simple rules that describe subjects ' decisions in a variety of settings, developing their cognitive implications, and using them to construct a unified explanation of subjects' information searches along with their decisions. We hope that the theory and the tools for measurement and data analysis discussed here will be useful in such efforts.

Table I

Games Classified by Strategic Structure

Game	Size	Dominant Decision for Row/Column	Dominance-Solvable	Equilibrium	Sophisticated	Naive/ Optimistic	Pessimistic	Altruistic
2A	2x2	Yes/No	Yes	Top-Left	Top-Left	Top-Right	Top-Right	Bottom-Right
2B	2x2	Yes/No	Yes	Top-Left	Top-Left	Top-Right	Top-Right	Bottom-Right
3A	2x2	No/Yes	Yes	Top-Left	Top-Left	Bottom-Left	Bottom-Left	Bottom-Right
3B	2x2	No/Yes	Yes	Top-Left	Top-Left	Bottom-Left	Bottom-Left	Bottom-Right
4A	2x3	No/Yes	Yes	Top-Left	Top-Left	Bottom-Left	Bottom-Left	Bottom-Right
4B	3x2	No/Yes	Yes	Top-Left	Top-Left	Bottom-Left	Middle-Left	Bottom-Right
4C	3x2	Yes/No	Yes	Top-Left	Top-Left	Top-Right	Top-Right	Bottom-Right
4D	2x3	Yes/No	Yes	Top-Left	Top-Left	Top-Right	Top-Middle	Bottom-Right
5A	3x2	No/No	Yes	Top-Left	Bottom-Left	Bottom-Right	Bottom- Right	Middle-Right
5B	3x2	No/No	Yes	Top-Left	Bottom-Left	Bottom-Right	Bottom-Right	Middle-Right
6A	2x3	No/No	Yes	Top-Left	Top-Left	Bottom-Right	Bottom-Right	Bottom-Middle
6B	2x3	No/No	Yes	Top-Left	Top-Right	Bottom-Right	Bottom-Right	Bottom-Middle
7A	2x3	No/No	No	Top-Left	Top-Right	Bottom-Right	Bottom-Middle	Bottom-Middle
7B	2x3	No/No	No	Top-Left	Top-Right	Bottom-Right	Bottom-Middle	Bottom-Middle
8A	3x2	No/No	No	Top-Left	Bottom-Left	Bottom-Right	Middle- Right	Middle-Right
8B	3x2	No/No	No	Top-Left	Bottom-Left	Bottom-Right	Middle-Right	Middle-Right
9A	4x2	Yes/No	Yes	Top-Left	Top-Left	Top-Right	Top-Right	Bottom-Right
9B	2x4	No/Yes	Yes	Top-Left	Top-Left	Bottom-Left	Bottom-Left	Bottom-Right

Table II

***p*-Values, Fisher's Exact Tests for Treatment Effects**

(* *p*-value less than 0.05)

Game	Size	B1 vs. B2 Columns	(B1+B2) vs. OB Columns	B1 vs. B2 Rows	(B1+B2) vs. OB Rows	(B1+B2) vs. TS Rows	(B1+B2) vs. TS (ex.) Rows
2A	2x2	1.000	0.096	1.000	1.000	0.204	0.069
2B	2x2	1.000	0.270	1.000	0.267	0.257	0.294
3A	2x2	0.217	1.000	1.000	0.142	0.028*	0.061
3B	2x2	0.478	0.525	1.000	0.706	0.028*	0.070
4A	2x3	0.217	1.000	0.675	0.467	0.056	0.030*
4B	3x2	0.093	0.274	1.000	0.867	0.086	0.020*
4C	3x2	0.036*	0.493	0.221	0.060	0.633	0.537
4D	2x3	0.317	0.487	0.096	0.141	1.000	0.272
5A	3x2	1.000	0.159	1.000	0.795	0.0001*	0.0001*
5B	3x2	0.640	0.153	0.594	0.863	0.00002*	0.00000*
6A	2x3	1.000	0.024*	1.000	0.467	0.153	0.030*
6B	2x3	1.000	1.000	1.000	1.000	0.141	0.013*
7A	2x3	1.000	0.217	0.391	0.322	0.176	0.011*
7B	2x3	0.784	0.847	0.231	1.000	0.314	0.053
8A	3x2	0.680	0.730	0.841	0.435	0.0002*	0.0003*
8B	3x2	0.680	0.177	1.000	0.855	0.0002*	0.0001*
9A	4x2	0.379	0.150	0.221	0.267	1.000	0.537
9B	2x4	0.590	0.070	1.000	0.370	1.000	0.137

Table III

***p*-Values, Fisher's Exact Tests, Rows versus Columns in Isomorphic Games**

(* *p*-value less than 0.05)

Games Row, Column	Decisions	B1+B2	OB	(B1+B2) vs. OB, Rows+Columns
2A, 3A	2	0.134	0.596	1.000
2B, 3B	2	0.665	1.000	0.150
3A, 2A	2	0.766	0.385	0.077
3B, 2B	2	1.000	1.000	0.277
4A, 4C	2	1.000	0.120	1.000
4D, 4B	2	1.000	1.000	0.046*
6A, 5A	2	0.373	1.000	0.077
6B, 5B	2	0.208	0.704	0.450
7A, 8A	2	1.000	0.704	0.330
7B, 8B	2	1.000	0.252	0.466
9B, 9A	2	1.000	0.013*	0.783
4B, 4D	3	0.003*	0.320	0.693
4C, 4A	3	1.000	0.222	0.701
5A, 6A	3	0.596	0.420	0.040*
5B, 6B	3	0.887	1.000	1.000
8A, 7A	3	0.739	1.000	0.100
8B, 7B	3	0.881	1.000	0.710
9A, 9B	4	1.000	0.481	0.244

Table IV

Payoff Incentives for Strategic Types, B+OB

Player role	Sophisticated	Equilibrium	Naïve/Optimistic	Pessimistic	Altruistic
Row	1127.8	1092.0	965.5	943.2	773.5
	(100.0%)	(96.8%)	(85.6%)	(83.6%)	(68.6%)
Column	1115.5	1091.4	958.4	940.5	766.5
	(100.0%)	(97.8%)	(85.9%)	(84.3%)	(68.7%)

Table V

***p*-Values, Exact ?^{2} Tests for Random Decisions**

(+ *p*-value greater than 0.05)

Games	Decisions	B+OB Rows	B+OB Columns	B+OB Rows+Cols.	TS Rows	TS (ex.) Rows
2A, 3A	2	0.004	2.3e-07	8.1e-09	0.0010	0.0005
2B, 3B	2	2.3e-07	1.9e-08	8.1e-09	6.1e-06	0.0005
3A, 2A	2	0.029	0.405+	0.024	0.0010	0.0006
3B, 2B	2	0.011	0.011	0.0002	6.1e-06	0.0005
4A, 4C	2	0.029	0.618+	0.044	0.0010	0.0005
4D, 4B	2	1.9e-06	1.9e-06	5.8e-12	0.0074	0.0005
6A, 5A	2	0.029	0.405+	0.024	0.0074	0.0005
6B, 5B	2	0.243+	0.029	0.013	0.0074	0.0005
7A, 8A	2	0.868+	0.868+	1.000+	0.118+	0.0006
7B, 8B	2	0.618+	0.868+	0.906+	0.118+	0.0006
9B, 9A	2	7.0e-05	0.132+	7.6e-06	0.0352	0.0005
4B, 4D	3	0.0007	3.5e-08	1.2e-07	0.0003	0.0009
4C, 4A	3	3.4e-13	3.5e-11	0.04437	6.5e-06	5.6e-06
5A, 6A	3	5.4e-09	6.9e-08	9.2e-16	0.0003	0.0009
5B, 6B	3	1.2e-06	5.1e-06	1.0e-11	0.0009	0.0001
8A, 7A	3	4.8e-05	3.0e-06	3.2e-10	8.0e-05	0.0001
8B, 7B	3	1.3e-05	3.5e-07	1.2e-11	0.0009	0.0001
9A, 9B	4	2.3e-17	5.2e-15	1.2e-07	0.035	0.0005

Table VI

Percentages of Decisions that Comply with Equilibrium by Type of Game

Type of Game (rounds of dominance)	B1+B2	OB	B+OB	TS	TS (ex.)
2x2 with dominant decision (1)	85.6%	92.6%	88.2%	96.7%	100.0%
(2A, 2B for Rows; 3A, 3B for Cols.)	(77/90)	(50/54)	(127/144)	(29/30)	(24/24)
2x3 with dominant decision (1)	82.2%	100.0%	88.9%	86.7%	100.0%
(4D for Rows; 4B for Cols.)	(37/45)	(27/27)	(64/72)	(13/15)	(12/12)
3x2 with dominant decision (1)	86.7%	92.6%	88.9%	93.3%	100.0%
(4C for Rows; 4A for Cols.)	(39/45)	(25/27)	(64/72)	(14/15)	(12/12)
4x2 with dominant decision (1)	88.9%	96.3%	91.7%	86.7%	100.0%
(9A for Rows; 9B for Cols.)	(40/45)	(26/27)	(66/72)	(13/15)	(12/12)
2x2, partner has dominant decision (2)	61.1%	79.6%	68.1%	96.7%	95.8%
(3A, 3B for Rows; 2A, 2B for Cols.)	(55/90)	(43/54)	(98/144)	(29/30)	(23/24)
2x3, partner has dominant decision (2)	62.2%	63.0%	62.5%	93.3%	100.0%
(4A for Rows; 4C for Cols.)	(28/45)	(17/27)	(45/72)	(14/15)	(12/12)
3x2, partner has dominant decision (2)	60.0%	55.6%	58.3%	80.0%	83.3%
(4B for Rows; 4D for Cols.)	(27/45)	(15/27)	(42/72)	(12/15)	(10/12)
2x4, partner has dominant decision (2)	73.3%	70.4%	72.2%	80.0%	100.0%
(9B for Rows; 9A for Cols.)	(33/45)	(19/27)	(52/72)	(12/15)	(12/12)
2x3 with 2 rounds of dominance (2)	62.2%	68.5%	64.6%	86.7%	100.0%
(6A, 6B for Rows; 5A, 5B for Cols.)	(56/90)	(37/54)	(93/144)	(26/30)	(24/24)
3x2 with 3 rounds of dominance (3)	11.1%	22.2%	15.3%	80.0%	87.5%
(5A, 5B for Rows; 6A, 6B for Cols.)	(10/90)	(12/54)	(22/144)	(24/30)	(21/24)
2x3, unique equilibrium, no dominance	50.0%	51.9%	50.7%	73.3%	91.7%
(7A, 7B for Rows; 8A, 8B for Cols.)	(45/90)	(28/54)	(73/144)	(22/30)	(22/24)
3x2, unique equilibrium, no dominance	17.8%	27.8%	21.5%	83.3%	91.7%
(8A, 8B for Rows; 7A, 7B for Cols.)	(16/90)	(15/54)	(31/144)	(25/30)	(22/24)

Table VII

Type Distributions and Error Rates Estimated from Subjects' Decisions

(— vacuous category; percentages may not sum to 100% due to rounding error)

Treatment (# subs.)	B (45)	OB (27)	B+OB (72)	TS (15)	TS(ex.) (12)
Log-likelihood	-423.72	-237.64	-662.61	-84.22	-40.99
Type					
Sophisticated (# subs.)	51% (23)	52% (14)	51% (37)	—	—
Error rate	0.26	0.29	0.27	—	—
Pr{pred. dec.}|c=2,3,4	0.87,0.83,0.80	0.86,0.81,0.79	0.86,0.82,0.80	—	—
Equilibrium (# subs.)	11% (5)	19% (5)	14% (10)	80% (12)	100% (12)
Error rate	0.35	0.21	0.28	0.07	0.07
Pr{pred. dec.}|c=2,3,4	0.83,0.77,0.74	0.89,0.86,0.84	0.86,0.81,0.79	0.96,0.95,0.95	0.96,0.95,0.95
Naïve/Opt. (# subs.)	22% (10)	30% (8)	25% (18)	13% (2)	0% (0)
Error rate	0.28	0.31	0.29	0.79	—
Pr{pred. dec.}|c=2,3,4	0.86,0.81,0.79	0.84,0.79,0.76	0.85,0.80,0.78	0.61,0.48,0.41	—
Pessimistic (# subs.)	2% (1)	0% (0)	1% (1)	0% (0)	0% (0)
Error rate	0.20	—	0.20	—	—
Pr{pred. dec.}|c=2,3,4	0.90,0.87,0.85	—	0.90, 0.87,0.85	—	—
Altruistic (# subs.)	13% (6)	0% (0)	8% (6)	7% (1)	0% (0)
Error rate	0.66	—	0.66	0.57	—
Pr{pred. dec.}|c=2,3,4	0.67,0.56,0.50	—	0.67,0.56,0.50	0.71,0.62,0.57	—

Table VIII

Implications of Types for Look-up Measures (— vacuous)

Type	Own Look-Ups (A)	Other Look-Ups (A)	Own String Length (B')	Other String Length (B')	Own Gaze Time (A")	Other Gaze Time (A")	Own Payoff First (A')	Own Payoff Last (A')	Own-Own Trans. (B')	Other-Other Trans. (B')	Own Up-Dn. Trans. (B')	Other L.-Rt. Trans. (B')	Altr. Own-Oth. Trans. (B')
Equilibrium	=4.1	=3.6	=1.82	=1.82	Long	Long	—	=50%	=45%	=45%	=31%	=31%	=10%
Sophisticated	=5.8	=4.2	=1.82	=1.82	Long	Long	—	=50%	=45%	=45%	=31%	=31%	=10%
Naïve	=5.8	—	=1.82	—	Long	Short	=50%	=50%	=45%	—	˜ 31%	—	=10%
Optimistic	=5.8	—	=1.82	—	Long	Short	=50%	=50%	=45%	—	—	—	=10%
Pessimistic	=3.9	—	=1.82	—	Long	Short	=50%	=50%	=45%	—	=31%	—	=10%
Altruistic	=5.8	=5.8	=1.82	=1.82	Long	Long	—	—	=45%	=45%	—	—	=10%
Random	—	—	1.82	1.82	—	—	50.0%	50.0%	45.0%	45.0%	30.6%	30.6%	10.0%

Table IX

Aggregate Look-up Measures for TS and Baseline Subjects, and for Baseline Subjects by Type Estimated from Decisions Alone

Treatment or Type	Own Look-Ups	Other Look-Ups	Own String Length	Other String Length	Own Gaze Time	Other Gaze Time	Own Payoff First	Own Payoff Last	Own-Own Trans.	Other-Other Trans.	Own Up-Dn. Trans.	Other L.-Rt. Trans.	Altr. Own-Oth. Trans.
All TS	20.5	17.6	6.27	6.57	0.60	0.47	68.3%	79.4%	80.2%	77.8%	57.1%	66.1%	7.9%
TS (ex.)	19.0	15.7	6.88	7.33	0.60	0.45	68.3%	83.9%	84.2%	81.6%	63.3%	69.3%	5.1%
All Baseline	16.8	14.6	5.46	5.95	0.67	0.60	72.8%	78.5%	79.7%	77.5%	31.6%	42.9%	8.5%
Equilibrium	12.9	12.5	3.43	4.26	0.81	0.57	71.1%	71.1%	67.6%	67.0%	31.4%	69.6%	12.3%
Sophisticated	18.6	17.3	5.81	7.00	0.58	0.52	61.7%	87.2%	84.4%	82.5%	37.6%	30.0%	6.4%
Naïve/Opt.	14.1	8.4	6.86	6.09	0.81	0.66	95.6%	81.1%	85.0%	79.8%	21.0%	46.1%	5.1%
Pessimistic	9.8	7.6	5.68	5.50	0.97	0.93	100.0%	38.9%	84.0%	87.0%	22.0%	68.0%	2.7%
Altruistic	18.2	17.5	3.53	3.29	0.66	0.73	72.2%	43.5%	61.8%	61.7%	28.2%	61.2%	19.8%

Table X

Aggregate Rates of Compliance with Each Type's Hypotheses B_1, B_H, B_M, B_L, and A (~A) for TS and Baseline Subjects, and for Baseline Subjects by Type Estimated from Decisions Alone

(— vacuous category; percentages may not sum to 100% due to rounding error)

Treatment (# subjects)	Equilibrium B_1,B_H,B_M,B_L,~A	Sophisticated B_1,B_H,B_M,B_L,~A	Naïve B_1,B_H,B_M,B_L,~A	Optimistic A,~A (B —)	Pessimistic B_1,B_H,B_M,B_L,~A	Altruistic B_1,B_H,B_M,B_L,~A
All TS (15)	89%,3%,3%,3%,2%	61%,12%,5%,2%,20%	83%,1%,2%,2%,13%	88%,12%	50%,—,8%,30%,12%	8%,3%,12%,44%,33%
TS (ex.) (12)	95%,1%,1%,1%,3%	66%,9%,1%,1%,24%	82%,1%,2%,0%,15%	86%,14%	44%,—,7%,36%,13%	1%,2%,10%,50%,27%
All Baseline(45)	30%,12%,23%,19%,16%	22%,17%,21%,20%,21%	78%,1%,4%,4%,14%	86%,14%	74%,—,2%,11%,14%	9%,5%,11%,59%,24%
Baseline subjects by type estimated from decisions (# subjects)						
Equilibrium (5)	32%,9%,23%,20%,16%	23%,11%,17%,20%,22%	62%,0%,10%,6%,22%	78%,22%	61%,—,3%,13%,22%	7%,6%,12%,52%,23%
Sophist. (23)	35%,14%,24%,13%,14%	26%,20%,23%,13%,19%	79%,1%,3%,0%,17%	83%,17%	74%,—,2%,8%,16%	4%,5%,14%,56%,21%
Naïve/Opt. (10)	17%,7%,22%,29%,25%	10%,12%,21%,28%,29%	88%,1%,5%,3%,4%	96%,4%	84%,—,1%,10%,6%	8%,2%,5%,48%,37%
Pessimistic (1)	11%,33%,39%,17%,0%	6%,33%,44%,17%,0%	94%,0%,0%,6%,0%	100%,0%	94%,—,6%,0%,0%	0%,0%,6%,94%,0%
Altruistic (6)	35%,11%,13%,26%,15%	26%,18%,12%,27%,18%	68%,0%,3%,17%,13%	87%,13%	63%,—,5%,20%,12%	30%,8%,16%,26%,20%

Table XI

Type Distributions and Error Rates Estimated from Decisions and Information Searches

(—vacuous category; percentages may not sum to 100% due to rounding error)

Treatment (# subs.) / Log-likelihood / Type	B (45) -401.14	TS (15) -74.16	TS(ex.) (12) -33.74
Sophisticated (#subs.)	47% (21)	—	—
Error rate|B_1,B_H,B_M,B_L,~A	0.24, 0.25, 0.28, 0.29, 0.10	—	—
(sample frequency of Sophisticated subject with Soph. B_1,B_H,B_M,B_L,~A)	(12%, 9%, 10%, 6%, 9%)	—	—
Equilibrium (#subs.)	9% (4)	80% (12)	100% (12)
Error rate|B_1,B_H,B_M,B_L,~A	0.12, 0.25, 0.50, 0.00, 0.93	0.05, 1.0, 1.0, 0.0, 0.0	0.05, 1.0, 1.0, 0.0, 0.0
(sample frequency of Equilibrium subject with Eq. B_1,B_H,B_M,B_L,~A)	(4%, 1%, 2%, 1%, 1%)	(76%, 0%, 2%, 0%, 2%)	(94%, 1%, 2%, 1%, 3%)
Naïve (#subs.)	9% (4)	13% (2)	0%
Error rate|B_1,B_H,B_M,B_L,~A	0.61, 0.00, 1.00, 1.00, 1.00	0.75, —, 0.0, —, 1.0	—
(sample frequency of Naïve subject with Naïve B_1,B_H,B_M,B_L,~A)	(7%, 0%, 0%, 0%, 1%)	(11%, 0%, 1%, 0%, 0%)	—
Optimistic (#subs.)	18% (8)	0%	0%
Error rate|A,~A	0.22, 0.69	—	—
(sample frequency of Optimistic subject with Optimistic A,~A)	(17%, 1%)	—	—
Pessimistic (#subs.)	9% (4)	0%	0%
Error rate|B_1,B_H,B_M,B_L,~A	0.38, —, 0.0, 0.93, 1.00	—	—
(sample frequency of Pessimistic subject with Pessimistic B_1,B_H,B_M,B_L,~A)	(6%, —, 0%, 1%, 2%)	—	—
Altruistic (#subs.)	9% (4)	7% (1)	0%
Error rate|B_1,B_H,B_M,B_L,~A	0.14, 1.00, 0.66, 0.85, 0.87	0.47, 0.0, 1.0, 1.0,—	—
(sample frequency of Altruistic subject with Altruistic B_1,B_H,B_M,B_L,~A)	(3%, 1%, 2%, 2%, 1%)	(6%, 1%, 0%, 0%, 0%)	—

References

Algaze [Croson], Rachel (1990): "A Test of Presentation Effects on Strategy Choice," B. A. Honors Thesis, University of Pennsylvania

Aumann, Robert, and Adam Brandenburger (1995): "Epistemic Conditions for Nash Equilibrium," *Econometrica*, 63, 1161-1180.

Beard, T. Randolph, and Richard Beil (1994): "Do People Rely on the Self-interested Maximization of Others?: An Experimental Test," *Management Science*, 40, 252-262.

Brandenburger, Adam (1992): "Knowledge and Equilibrium in Games," *Journal of Economic Perspectives*, 6, 83-101.

Brandts, Jordi, and Charles Holt (1993): "Dominance and Forward Induction: Experimental Evidence," 119-136 in R. Mark Isaac (editor), *Research in Experimental Economics*, vol. 5. Greenwich, Connecticut: JAI Press.

Cachon, Gerard, and Colin Camerer (1996): "Loss Avoidance and Forward Induction in Experimental Coordination Games," *Quarterly Journal of Economics*, 111, 165-194.

Camerer, Colin, and Teck-Hua Ho (1998): "Experience-weighted Attraction Learning in Normal Form Games," *Econometrica*, in press.

Camerer, Colin, Eric Johnson, Talia Rymon, and Sankar Sen (1993): "Cognition and Framing in Sequential Bargaining for Gains and Losses," 27-47 in Kenneth Binmore, Alan Kirman, and Piero Tani (editors), *Frontiers of Game Theory*. Cambridge: MIT Press.

Card, S. K., T. P. Moran, and A. Newell (1983): *The Psychology of Human-Computer Interaction.* Hillsdale N. J.: Lawrence Erlbaum.

Cooper, Russell, Douglas DeJong, Robert Forsythe, and Thomas Ross (1990): "Selection Criteria in Coordination Games: Some Experimental Results," *American Economic Review*, 80, 218-233.

Cooper, Russell, Douglas DeJong, Robert Forsythe, and Thomas Ross (1994): "Alternative Institutions for Resolving Coordination Problems: Experimental Evidence on Forward Induction and Preplay Communication," 129-146 in James Friedman (editor), *Problems of Coordination in Economic Activity*. Boston: Kluwer.

Crawford, Vincent (1995): "Adaptive Dynamics in Coordination Games," *Econometrica*, 63, 103-143.

Crawford, Vincent (1997): "Theory and Experiment in the Analysis of Strategic Interaction," 206-242 in David Kreps and Ken Wallis (editors), *Advances in Economics and Econometrics, Seventh World Congress: Theory and Applications, Vol I*. Econometric Society Monographs No. 27. Cambridge, U.K.: Cambridge University Press.

Crawford, Vincent, and Bruno Broseta (1998): "What Price Coordination? The Efficiency-enhancing Effect of Auctioning the Right to Play," *American Economic Review*, 88, 198-225.

Davis, Douglas, and Charles Holt (1993): *Experimental Economics*. Princeton, New Jersey: Princeton University Press.

El-Gamal, Mahmoud, and David Grether (1995): "Are People Bayesian? Uncovering Behavioral Strategies," *Journal of the American Statistical Association*, 90, 1137-1145.

Gilboa, Itzhak, Ehud Kalai, and Eitan Zemel (1993): "The Complexity of Eliminating Dominated Strategies," *Mathematics of Operations Research*, 18, 553-565.

Friedman, Daniel (1996): "Equilibrium in Evolutionary Games: Some Experimental Results," *Economic Journal*, 106, 1-25.

Fudenberg, Drew, and David Kreps (1993): "Learning Mixed Equilibria," *Games and Economic Behavior*, 5, 320-367.

Harless, David, and Colin Camerer (1994): "The Predictive Utility of Generalized Expected Utility Theories," *Econometrica*, 62, 1251-1289.

Harless, David, and Colin Camerer (1995): "An Error Rate Analysis of Experimental Data Testing Nash Refinements," *European Economic Review*, 39, 649-660.

Harsanyi, John, and Reinhard Selten (1988): *A General Theory of Equilibrium Selection in Games*. Cambridge: MIT Press.

Ho, Teck Hua, and Keith Weigelt (1996): "Task Complexity, Equilibrium Selection, and Learning: An Experimental Study," *Management Science*, 42, 659-679.

Ho, Teck Hua, Colin Camerer, and Keith Weigelt (1998): "Iterated Dominance and Learning in Experimental 'Beauty Contest' Games," *American Economic Review*, in press.

Johnson, Eric, Colin Camerer, Sankar Sen, and Talia Rymon (1998): "Detecting Failures of Backward Induction: Monitoring Information Search in Sequential Bargaining," *Games and Economic Behavior*, in press.

Kagel, John, and Alvin Roth, editors (1995): *Handbook of Experimental Economics*. Princeton University Press: Princeton, New Jersey.

Kalai, Ehud, and Ehud Lehrer (1993): "Rational Learning Leads to Nash Equilibrium," *Econometrica*, 61, 1019-1045.

Knuth, Donald, Christos Papadimitriou, and John Tsitsiklis (1988): "A Note on Strategy Elimination in Bimatrix Games," *Operations Research Letters*, 7, 103-107.

McKelvey, Richard and Thomas Palfrey (1992): "An Experimental Study of the Centipede Game," *Econometrica*, 60, 803-836.

Nagel, Rosemarie (1995): "Unraveling in Guessing Games: An Experimental Study," *American Economic Review*, 85, 1313-1326.

Palfrey, Thomas, and Howard Rosenthal (1994): "Repeated Play, Cooperation and Coordination: An Experimental Study," *Review of Economic Studies*, 61, 545-565.

Payne, John, James Bettman, and Eric Johnson (1993): *The Adaptive Decision Maker*. Cambridge, U.K.: Cambridge University Press.

Pierce, Albert (1970): *Fundamentals of nonparametric statistics*. Belmont, California: Dickenson Publishing Co.

Roth, Alvin (1987): "Bargaining Phenomena and Bargaining Theory," 14-41 in Alvin Roth (ed.), Laboratory Experimentation in Economics: Six Points of View. New York: Cambridge University Press.

Roth, Alvin, and Ido Erev (1995): "Learning in Extensive-Form Games: Experimental Data and Simple Dynamic Models in the Intermediate Term," *Games and Economic Behavior*, 8, 164-212.

Roth, Alvin and Michael Malouf (1979): "Game-Theoretic Models and the Role of Information in Bargaining," *Psychological Review*, 86, 574-594.

Roth, Alvin and J. Keith Murnighan (1982): "The Role of Information in Bargaining: An Experimental Study," *Econometrica*, 50, 1123-1142.

Roth, Alvin, Vesna Prasnikar, Masahiro Okuno-Fujiwara, and Shmuel Zamir (1991): "Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An Experimental Study," *American Economic Review*, 81, 1068-1095.

Schotter, Andrew, Keith Weigelt, and Charles Wilson (1994): "A Laboratory Investigation of Multiperson Rationality and Presentation Effects," *Games and Economic Behavior*, 6, 445-468.

Selten, Reinhard (1998): "Features of Experimentally Observed Bounded Rationality," *European Economic Review*, 42, 413-436.

Shachat, Jason, and Mark Walker (1997): "Unobserved Heterogeneity and Equilibrium: An Experimental Study of Bayesian and Adaptive Learning in Normal Form Games," Discussion Paper 97-33, University of California, San Diego.

Stahl, Dale (1996): "Boundedly Rational Rule Learning in a Guessing Game," *Games and Economic Behavior*, 16, 303-330.

Stahl, Dale and Paul Wilson (1995): "On Players' Models of Other Players: Theory and Experimental Evidence," *Games and Economic Behavior*, 10, 218-254.

Straub, Paul (1995): "Risk Dominance and Coordination Failures in Static Games," *Quarterly Review of Economics and Finance*, 35, 339-363.

Van Huyck, John, Raymond Battalio, and Richard Beil (1990): "Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure," *American Economic Review*, 80, 234-248.

Van Huyck, John, Raymond Battalio, and Richard Beil (1991): "Strategic Uncertainty, Equilibrium Selection, and Coordination Failure in Average Opinion Games," *Quarterly Journal of Economics*, 106, 885-910.

Van Huyck, John, Raymond Battalio, and Richard Beil (1993): "Asset Markets as an Equilibrium Selection Mechanism: Coordination Failure, Game Form Auctions, and Tacit Communication," *Games and Economic Behavior*, 5, 485-504.

APPENDIX A: BASELINE AND TS INSTRUCTIONS [1]

BASELINE INSTRUCTIONS

[1ST SCREEN] {Introduction}

WELCOME!

PLEASE WAIT UNTIL THE EXPERIMENTER TELLS YOU TO START

You are about to participate in an experiment in interdependent decision making. Universities and research foundations have provided the funds for this experiment. If you follow the instructions and pass the Understanding Test, you will be allowed to continue in the experiment. If you make good decisions, you may then earn a considerable additional amount of money, between $4 and $40. This additional amount will be determined both by your decisions and by those of other participants in the experiment. Before making your decisions, you will have the opportunity to gather information about how your earnings and the other participants' earnings depend on your and their decisions. All that you earn is yours to keep, and will be paid to you in private, in cash, after today's session.

(Click on the bar at the bottom of this screen to move on to the next screen)

[2ND SCREEN] {Silence}

It is important to remain silent and not to look at other people's work. If you have any questions or need assistance of any kind, please raise your hand, and an experimenter will come to you. Otherwise, if you talk, laugh, exclaim out loud, etc., YOU WILL BE ASKED TO LEAVE. Thank you.

(Click on the bar at the bottom of this screen to move on to the next screen)

[3RD SCREEN] {Games}

The experiment consists of 18 rounds. In each round, you will be anonymously matched with one of the other participants, a different one in each round. We will refer to the other participant as "s/he". In each round, you and s/he will be presented with a decision problem. Each of you, separately and independently, will make a DECISION. Together, the two decisions will determine the numbers of POINTS each of you earn that round, which may be different. Earning more points increases your payment at the end of the experiment, as explained below.

Once a round is over, you will not be able to change your decision in that round. Neither you nor the other participants will learn anyone else's decisions in any round until the entire session is over.

The next screen displays an illustrative decision problem and its table of points. IT IS ONLY AN ILLUSTRATION; the decision problems you will face in the 18 rounds will be different from this one, and will change each round. AS YOU LOOK AT THIS PROBLEM, READ THE FIRST PAGE OF THE PRINTED HANDOUT.

(Click on the bar at the bottom of this screen to move on to the next screen)

[4TH SCREEN] {2x2 table display}

[The following Table for Row subjects (or a variant for Column subjects, as explained below) will be shown to the subject on the computer screen with the boxes open. **]**

[1]The OB instructions, which differ from the Baseline instructions only in the parts that pertain to opening boxes to look up payoffs, are available from the authors on request. Everything between [ ] or { } is not shown to subjects.

	S/He: &	S/He: @	S/He: &	S/He: @
You: #	53	21	84	22
You: *	87	49	38	65
	YOUR POINTS		HER/HIS POINTS	
	I am done			

[The following is the first page of the handout. **]**

<u>DO NOT START READING THIS PAGE UNTIL INSTRUCTED ON THE SCREEN TO DO SO</u>

In the actual decision problems, you will be shown a table like this (but with different numbers of points) on your screen, and asked to choose one of your decisions, here labeled # and *. The other participant with whom you are matched will be asked, independently, to choose one of her/his decisions, here labeled & and @.

The combination of your decision and her/his decision is called an OUTCOME. The number of points you and s/he receive for an outcome will be whole numbers from 0 to 99. Your points appear in the boxes on the left side of the table, labeled "YOUR POINTS" underneath. Her/His points appear in the boxes on the right side of the table, labeled "HER/HIS POINTS" underneath. To interpret the table, consider the results of the possible outcomes (that is, combinations of decisions):

- If you choose # and s/he chooses @, **s/he** earns 22 points.
- If you choose * and s/he chooses @, **you** earn 49 points.
- If you choose # and s/he chooses &, **you** earn 53 points.
- If you choose * and s/he chooses &, **s/he** earns 38 points.
- If you choose # and s/he chooses &, **s/he** earns 84 points.
- If you choose # and s/he chooses @, **you** earn 21 points.
- If you choose * and s/he chooses &, **you** earn 87 points.
- If you choose * and s/he chooses @, **s/he** earns 65 points.

In each round of the actual decision problems, you will see a new table and you will be matched with a different participant. As in this problem, the points that you and s/he earn will depend on both your decisions.

Please be sure you understand the table. Raise your hand if you would like further ex planation. Otherwise, move to the next screen by clicking on the box "I am done".

<u>DO NOT TURN TO THE NEXT PAGE BEFORE INSTRUCTED ON THE SCREEN TO DO SO</u>.

[This completes the first page of the handout. **]**

[5TH SCREEN] {Covered-boxes explanation}

In the actual experiment, the points in a table, like the one on the previous screen, will not be openly displayed. Instead they will be "hidden" in the boxes, as if the boxes were covered. However you will be able to open any box, just by POINTING AT it with the mouse (that is, moving the cursor into the box by sliding the mouse) and CLICKING the LEFT button of the mouse. You may open as many or as few boxes as you wish, as often and as long as you wish, and in any order. However, you will be able to have only one box open at a time. If you want to open a new box, you will have to close the previous box first by CLICKING the RIGHT button of the mouse. YOU ARE NOT ALLOWED TO WRITE DOWN THE NUMBERS IN THE BOXES. If you would like to know the number of points in a box that you do not remember, just open that box again.

The points will also be hidden on her/his screen, and s/he will be able to open the boxes in the same way, subject to the same restrictions.

The next screen displays the same illustrative table as before, b ut with the boxes covered. Use the mouse to practice opening boxes until you feel comfortable with the procedure. Then move on to the following screen by CLICKING the box "Next Screen". For further explanation, raise your hand.

(Click on the bar at the bottom of this screen to move on to the next screen)

[6TH SCREEN] {2x2 table display}

[The table displayed below will be shown on the computer screen with the boxes covered. **]**

	S/He: &	S/He: @	S/He: &	S/He: @
You: #	53	21	84	22
You: *	87	49	38	65
	YOUR POINTS		HER/HIS POINTS	
	Next Screen			

[7TH SCREEN] {Different table formats}

In some rounds of the experiment, you will be asked to choose one of THREE possible decisions, labeled #, *, and ^; while the other participant with whom you are matched will be asked to choose from TWO decisions, as before. Thus, the table of points will have an extra row of boxes. In some other rounds, s/he will be asked to choose one of THREE possible decisions, labeled &, @, and %; while you will be asked to choose from TWO decisions. Thus, the table of points will have two extra columns of boxes (one for your points, one for her/his points). The following screen illustrates the case when you have three possible decisions and s/he has two. Please raise your hand if you have any questions. Otherwise move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[8TH SCREEN] {Example of a 3x2 table}

[The Table below will be shown to the subject with the boxes closed. **]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: *	54	67	64	42
You: ^	89	21	35	13
You: #	35	82	91	68
	YOUR POINTS		HER/HIS POINTS	

Next Screen

[9TH SCREEN] {Making decisions}

To complete a given round, you must make a decision. (Remember that the number of points you obtain in any given round will depend on both your AND her/his decisions). Your possible decisions will be displayed in decision boxes below the table of points. To make a decision, use the mouse to point at the chosen decision, then click any button on the mouse. The screen will then ask you to confirm your decision. At that time, you can change your mind by pointing to and clicking on a different decision box, after which you will again be asked to confirm your decision. Once you confirm a decision, you cannot change it.

The next screen displays the same illustrative table as before, but with decision boxes. Notice the boxes in which the possible decisions are displayed, under the table of points. For practice, point at one of them and click on the mouse. Then either confirm the decision or, if you wish, change the decision and then confirm. Raise your hand if you have questions.

(Click on the bar at the bottom of this screen to move on to the next screen)

[10TH SCREEN] {Practicing decisions in a 3x2 table}

[The Table below will be shown to the subject with the boxes closed. **]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: *	54	67	64	42
You: ^	89	21	35	13
You: #	35	82	91	68
	YOUR POINTS		HER/HIS POINTS	

You: *	You: ^	You: #

[11TH SCREEN] {Different table formats}

In some rounds of the experiment, you will be asked to choose one of FOUR possible decisions, labeled #, *, ^, and <>; while the other participant with whom you are matched will be asked to choose from TWO decisions, as before. Thus, the table of points will have four rows of boxes. In some other rounds, s/he will be asked to choose one of FOUR possible decisions, labeled &, @, %, and /\; while you will be asked to choose from TWO decisions. Thus, the table of points will have eight columns of boxes

(four for your points, four for her/his points). Please raise your hand if you have any questions. Otherwise move on to the next screen.
(Click on the bar at the bottom of this screen to move on to the next screen)

[12TH SCREEN] {Understanding Test}

UNDERSTANDING TEST

You will now take a short UNDERSTANDING TEST. After you finish the TEST it will be graded and you will only be allowed to continue in the experiment if you have answered ALL the QUESTIONS CORRECTLY.

Turn to the SECOND page of the handout, which contains th e test questions, and move on to the next screen.
(Click on the bar at the bottom of this screen to move on to the next screen)

[13TH SCREEN] {Test using a 2x3 table}

[The Table below will be shown to the subject with the boxes closed. **]**

	S/He: %	S/He: @	S/He: &	S/He: %	S/He: @	S/He: &
You: *	25	64	63	35	29	53
You: #	97	41	32	19	68	12
	YOUR POINTS			HER/HIS POINTS		
	You: *			You: #		

[The following questions will be written on the 2nd page of the handout. **]**
Please write your identification number just above. Then, by reference to the table of points on the screen, answer the following questions. Note that the screen allows two choices for you and three for her/him.

<u>Questions</u>:

1. If you choose * and s/he chooses @, how many points will **you** earn? _______.
2. If you choose # and s/he chooses &, how many points will **you** earn? ________.
3. If you choose # and s/he chooses @, how many points will **s/he** earn? ______.
4. If you choose # and s/he chooses %, how many points will **s/he** earn? _______.

After you have answered the questions, please point at one of the decision boxes, click on it, and confirm it. In the actual experiment you will click on the box corresponding to the decision you want to choose.

YOU HAVE JUST COMPLETED THE TEST.

Please raise your hand until the experimenter sees you. He will come to collect your test.

DO NOT TURN TO THE NEXT PAGE BEFORE INSTRUCTED ON THE SCREEN TO DO SO.

[This sentence completes the second page of the handout. **]**

[14TH SCREEN] {Matching protocol}

In each round of the experiment, you will be anonymously matched with a different participant and both of you will face the same interdependent decision problem. Your decisions in a round will not influence the matching of participants or assignment of decision problems in later rounds. Your identity and the identities of the other participants will never be revealed. Each participant you are matched with will receive the same instructions as you and will face the same kind of screen display.

At the conclusion of each round, you will see a screen asking you to proceed to the next round. If you wish, you may rest before proceeding. However, we ask you not to rest during a round. This screen will also tell you how many possible decisions you and the participant assigned to you in the next round will have to choose from.

After each round the computer will record the number of points you earned, but will not report the number to you. Your point earnings for each individual round will be reported to you (and only to you) at the end of the experiment. Your point earnings will then be used to determine your payment, as described next.

(Click on the bar at the bottom of this screen to move on to the next screen)

[15TH SCREEN] {Payment}

PAYMENT

After you have made your decisions for all 18 rounds, your payment will be determined according to the number of points you earned, as follows:

One of the 18 rounds will be selected at random, and you will be paid $.40 (forty cents) per point for your points on that round. The selection of the round will take place as follows. Tokens numbered 1 to 18 will be placed in a container and shaken. You will draw a token at random, and the number you draw will be the round for which your points determine your earnings.

You will be paid your earnings in cash, in priva te, after the experiment.

To illustrate the payment we will now work through two examples so that you fully understand how the payment is determined.

Suppose that in the round randomly chosen one of the two following outcomes happened:

- You chose # and s/he chose &, and you earned 20 points and s/he earned 50 points. At $0.40 per point you will receive $8 and s/he will receive $20.
- You chose * and s/he chose @, and you earned 70 points and s/he earned 30 points. At $0.40 per point you will receive $28 and s/he will receive $12.

(Click on the bar at the bottom of this screen to move on to the next screen)

[16TH SCREEN] {Practice rounds}

PRACTICE ROUNDS

Before you start playing the actual decision problems for money, you will have the opportunity to practice for **4** rounds. During the practice games we will use different labels for the decisions (**T**, **V**, and **W** for you, and **X**, **Y**, and **Z** for her/him) from the ones that will be used throughout the experiment and that you have already seen (#, *, ^, and <> for you, and &, @, %, and ~ for her/him).

Turn to the THIRD page of the handout, where YOU will WRITE DOWN YOUR DECISION for each of the practice rounds and move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[17TH SCREEN] {Practice round 1 - Introductory screen}

In this first practice round (Round 1):

You will have TWO decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY

(Click on the bar at the bottom of this screen to move on to the next screen)

[18TH SCREEN] {Practice round 1}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He: X	S/He: Y	S/He: X	S/He: Y
You: T	78	32	54	81
You: V	21	56	23	49
	YOUR POINTS		HER/HIS POINTS	
	You: T		You: V	

[19TH SCREEN] {Practice round 2 - Introductory screen}

In this practice round (Round 2):

You will have TWO decisions to choose from.

S/he will have THREE decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[20TH SCREEN] {Practice round 2}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He: Z	S/He: Y	S/He: X	S/He: Z	S/He: Y	S/He: X
You: V	54	21	98	85	43	21
You: T	79	45	67	45	76	52
	YOUR POINTS			HER/HIS POINTS		
	You: V			You: T		

[21ST SCREEN] {Practice round 3 - Introductory screen}

In this practice round (Round 3):

You will have THREE decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[22ND SCREEN] {Practice round 3}

[The table displayed below will be shown on the computer screen with the boxes closed.]

	S/He: X	S/He: Y	S/He: X	S/He: Y
You: W	87	43	53	82
You: V	39	76	21	44
You: T	17	54	95	12
	YOUR POINTS		HER/HIS POINTS	

You: W	You: V	You: T

[23RD SCREEN] {Practice round 4 - Introductory screen}

In this practice round (Round 4):

You will have TWO decisions to choose from.

S/he will have THREE decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[24TH SCREEN] {Practice round 4}

[The table displayed below will be shown on the computer screen with the boxes closed.]

	S/He: X	S/He: Y	S/He: Z	S/He: X	S/He: Y	S/He: Z
You: T	81	35	42	67	25	92
You: V	23	74	68	39	61	32
	YOUR POINTS			HER/HIS POINTS		

You: T	You: V

(The screens 16th to 24th for the Column subjects are displayed in **APPENDIX COLUMN**)

[25TH SCREEN] {Last Screen of the Instructions input file}

You have now completed all practice rounds. Please raise your hand unti l the experimenter sees you, and please remain in your seat. An experimenter will come to collect your handout with your decisions for the practice rounds. You will then take a short break before starting the actual decision problems. Please remain in your seat.

THE END

(Click on the bar at the bottom of this screen to exit the program)

[VARIATION FOR COLUMN SUBJECTS]

[16TH SCREEN] {Practice rounds}

PRACTICE ROUNDS

Before you start playing the actual decision problems for money, you will have the opport unity to practice for **4** rounds. During the practice games we will use different labels for the decisions (**X**, **Y**, and **Z** for you, and **T**, **V**, and **W** for her/him) than the ones that will be used throughout the experiment and that you have already seen (#, *, ^, and <> for you, and &, @, %, and ~ for her/him).

Turn to the THIRD page of the handout, where YOU will WRITE DOWN YOUR DECISION for each of the practice rounds and move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[17TH SCREEN] {Practice round 1 - Introductory screen}

In this practice round (Round 1):

You will have TWO decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[18TH SCREEN] {Practice round 1}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He: T	S/He: V	S/He: T	S/He: V
You: X	54	23	78	21
You: Y	81	49	32	56
	YOUR POINTS		HER/HIS POINTS	
	You: X		You: Y	

[19TH SCREEN] {Practice round 2 - Introductory screen}

In this practice round (Round 2):

You will have THREE decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[20TH SCREEN] {Practice round 2}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He: V	S/He: T	S/He: V	S/He: T
You: Z	85	45	54	79
You: Y	43	76	21	45
You: X	21	52	98	67
	YOUR POINTS		HER/HIS POINTS	

You: Z	You: Y	You: X

[21ST SCREEN] {Practice round 3 - Introductory screen}

In this practice round (Round 3):

You will have TWO decisions to choose from.

S/he will have THREE decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[22ND SCREEN] {Practice round 3}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He: W	S/He: V	S/He: T	S/He: W	S/He: V	S/He: T
You: Y	53	21	95	87	39	17
You: X	82	44	12	43	76	54
	YOUR POINTS			HER/HIS POINTS		

You: Y	You: X

[23RD SCREEN] {Practice round 4 - Introductory screen}

In this practice round (Round 4):

You will have THREE decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE TABLE OF POINTS ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[24TH SCREEN] {Practice round 4}

[The table displayed below will be shown on the computer screen with the boxes closed. **]**

	S/He T:	S/He: V	S/He T:	S/He: V
You: X	67	39	81	23
You: Y	25	61	35	74
You: Z	92	32	42	68
	YOUR POINTS		HER/HIS POINTS	

You: X	You: Y	You: Z

TS INSTRUCTIONS

[1ST SCREEN] {Introduction}

WELCOME!

PLEASE WAIT UNTIL THE EXPERIMENTER TELLS YOU TO START

You are about to participate in an experiment in interdependent decision making. Univers ities and research foundations have provided the funds for this experiment. If you follow the instructions and pass the Understanding Test, you will be allowed to continue in the experiment, and you may earn a considerable additional amount of money, up to $35.50. All that you earn is yours to keep, and will be paid to you in private, in cash, after today's session.

(Click on the bar at the bottom of this screen to move on to the next screen)

[2ND SCREEN] {Silence}

It is important to remain silent and not to look at other people's work. If you have any questions or need assistance of any kind, please raise your hand, and an experimenter will come to you. Otherwise, if you talk, laugh, exclaim out loud, etc., YOU WILL BE ASKED TO LEAVE. Thank you.

(Click on the bar at the bottom of this screen to move on to the next screen)

[3RD SCREEN] {Games}

The experiment consists of 18 rounds. Each round concerns an INTERDEPENDENT DECISION PROBLEM. In an interdependent decision problem two people ("You", and someone else, henceforth "S/He"), separately and independently make DECISIONS. Together, the two decisions determine the numbers of POINTS you and s/he earn that round, which may be different. Each person is assumed to want to maximize her/his own number of points, given her/his expectations about the other's decision.

In each round, you will be asked to select a decision, using a criterion explained below. Once a round is over, you will not be able to change your selection in that round. You will be paid only for the number of rounds in which you correctly select the decision that satisfies the criterion, as explained below.

The next screen displays a sample decision problem and its table of points. THIS PROBLEM IS ONLY AN ILLUSTRATION; the problems you will face in the 18 rounds will be different from this one, and will change each round. AS YOU LOOK AT THIS PROBLEM, READ THE FIRST PAGE OF THE PRINTED HANDOUT.

(Click on the bar at the bottom of this screen to move on to the next screen)

[4TH SCREEN] {2x2 table display}

[The table below will be shown (screen) to the subject with the boxes <u>open</u>.**]**

	S/He: &	S/He: @	S/He: &	S/He: @
You: #	53	21	84	22
You: *	87	49	38	65
	YOUR POINTS		HER/HIS POINTS	
	I am done			

[The following explanation will be given to subjects on the first page of a two-page handout. **]**

<u>DO NOT START READING THIS PAGE UNTIL INSTRUCTED ON THE SCREEN TO DO SO</u>

In the actual decision problems, you will have a table like this (but with different numbers of points) on your screen, and you will be asked to select a decision from decisions like # and *. S/He will be asked, independently, to choose one of her/his decisions from decisions like & and @.

The combination of your decision and her/his decision is called an OUTCOME. The number of points you and s/he receive for an outcome will be whole numbers from 0 to 99. Your points appear in the boxes on the left side of the table, labeled "YOUR POINTS" underneath. Her/His points appear in the boxes on the right side of the table, labeled "HER/HIS POINTS" underneath. To interpret the table, consider the results of the possible outcomes (combinations of decisions):

- If you choose # and s/he chooses @, **s/he** earns 22 points.
- If you choose * and s/he chooses @, **you** earn 49 points.
- If you choose # and s/he chooses &, **you** earn 53 points.
- If you choose * and s/he chooses &, **s/he** earns 38 points.
- If you choose # and s/he chooses &, **s/he** earns 84 points.
- If you choose # and s/he chooses @, **you** earn 21 points.
- If you choose * and s/he chooses &, **you** earn 87 points.
- If you choose * and s/he chooses @, **s/he** earns 65 points.

In each round of the actual decision problems, you will have a new table. As in this problem, the points that you and s/he earn will depend on both your decisions.

Please be sure you understand the table. Raise your hand if you would like further explanation. Otherwise, move to the next screen by clicking on the box "I am done".

<u>DO NOT TURN TO THE NEXT PAGE BEFORE INSTRUCTED ON THE SCREEN TO DO SO</u>.

[This sentence completes the first page of the handout. **]**

[5TH SCREEN] {Different table formats}

In some rounds of the experiment, you will be asked to choose one of THREE or FOUR possible decisions, labeled #, *, ^, and (if there are four) <>; while s/he will have TWO possible decisions, as before. Thus, the table of points will have one or two extra rows of boxes. In some other rounds, s/he will have THREE or FOUR possible decisions, labeled &, @, %, and (if there are four) ~; while you will be asked to choose from TWO decisions. Thus, the table of points will have two or four extra columns of boxes, one or two for your points, and one or two for her/his points. The following screen illustrates the case when you have three possible decisions and s/he has two. Please raise your hand if you have any questions. Otherwise move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[6TH SCREEN] {Example of a 3x2 table}

[The Table below will be shown to the subject with the boxes <u>open</u>.**]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: *	54	67	64	42
You: ^	89	21	35	13
You: #	35	82	91	68
	YOUR POINTS		HER/HIS POINTS	

Next Screen

[7TH SCREEN] {Making decisions}

To complete a given round, you must select a decision. Your possible decisions will be displayed in decision boxes below the table of points. To select a decision, use the mouse to point at the chosen decision, then click any button on the mouse. The screen will then ask you to confirm your decision. At that time, you can change your mind by pointing to and clicking on a different decision box, after which you will again be asked to confirm your decision. Once you confirm a decision, you cannot change it.

The next screen displays the same table as before, but with boxes in which the possible decisions are displayed under the table of points. For practice, point at one of them and click on the mouse. Then either confirm the decision or, if you wish, change the decision and then confirm. Raise your hand if you have questions.

(Click on the bar at the bottom of this screen to move on to the next screen)

[8TH SCREEN] {Practicing decisions in a 3x2 table}

[The table below will be shown to the subject with the boxes <u>open</u>.**]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: *	54	67	64	42
You: ^	89	21	35	13
You: #	35	82	91	68
	YOUR POINTS		HER/HIS POINTS	

You: *	You: ^	You: #

[9TH SCREEN] {Understanding Test - 1st part}

UNDERSTANDING TEST - (PART 1)

You will now take the 1st part of an UNDERSTANDING TEST. After you finish this part of the test, it will be graded. YOU WILL ONLY BE ALLOWED TO CONTINUE IN THE EXPERIMENT IF YOU HAVE ANSWERED ALL THE QUESTIONS CORRECTLY.

Turn to the SECOND page of the handout, which contains the questions, and move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[10TH SCREEN] {Test using a 2x3 table}

[The Table below will be shown to the subjects with the boxes open.**]**

	S/He: %	S/He: @	S/He: &	S/He: %	S/He: @	S/He: &
You: *	25	64	63	35	29	53
You: #	97	41	32	19	68	12
	YOUR POINTS			HER/HIS POINTS		
	You: *			You: #		

[The following questions will be written on the 2nd page of the handout. **]**

UNDERSTANDING TEST Identification number**:** _____.

Please write your identification number in the upper right hand corner of this page. Then, by using the table of points on the screen, answer the following questions. Note that the screen allows two choices for you and three for her/him.

Questions:

1. If you choose * and s/he chooses @, how many points will **you** earn? __________.
2. If you choose # and s/he chooses &, how many points will **you** earn? __________.
3. If you choose # and s/he chooses @, how many points will **s/he** earn? __________.
4. If you choose # and s/he chooses %, how many points will **s/he** earn? __________.

After you have answered the questions, please point at one of the decision boxes, click on it, and confirm it. In the actual experiment you will click on the box corresponding to the decision you want to select.

YOU HAVE JUST COMPLETED THE TEST.

Please raise your hand until the e xperimenter sees you. Do not proceed until he has collected your test.

DO NOT TURN TO THE NEXT PAGE BEFORE INSTRUCTED ON THE SCREEN TO DO SO.

[This sentence completes the 2nd page of the handout.**]**

[11TH SCREEN] {Explanation of an equilibrium}

We now describe the criterion you are asked to use to select your decisions in the decision problems you will face. This description is repeated on your handout, which you will be allowed to keep for reference (but not to write on) during the experiment. The criterion is based on the idea that each person wants to get as many points as possible, taking into account the other person's decision as well as her/his own. A combination of decisions, one for each person, such that each person's decision gives her/him the largest number of points possible, given the other's decision, is called an EQUILIBRIUM. Your decision in an equilibrium is called your EQUILIBRIUM DECISION, and her/his decision in an equilibrium is called her/his EQUILIBRIUM DECISION. You are asked to select your EQUILIBRIUM DECISION in each round.

Turn to the next (THIRD) page of the handout, and read ONLY the FIRST paragraph, while you look at the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[The following will be written on the 3rd page of the handout.**]**

For example, in the decision problem displayed on the screen the combination of your decision * and her/his decision @ is an EQUILIBRIUM. This is because, if s/he chooses @, you would earn more points by choosing * (49) than by choosing # (21). And if you choose *, s/he would earn more points by choosing @ (65) than by choosing & (38).

[12TH SCREEN] {2x2 table display - Same as 2nd screen}

[The table below will be shown to the subjects with the boxes open.**]**

	S/He: &	S/He: @	S/He: &	S/He: @
You: #	53	21	84	22
You: *	87	49	38	65
	YOUR POINTS		HER/HIS POINTS	
	You: #		You: *	

[13TH SCREEN]

{Explanation of an equilibrium - continuation}

Some decision problems may have more than one equilibrium, so you may have more than one equilibrium decision. In such problems you are asked to select your decision in the equilibrium that gives you the most points of any equilibrium. Suppose, for example, that a decision problem (not shown on your screen) has two equilibria, (*,@) with 66 points for you and 34 points for her/him, and (#,&) with 41 points for you and 76 points for her/him. Then you must select *, your decision in the first equilibrium, because it gives you 66 points and the other equilibrium gives you only 41 points. Note that to be sure you have applied the criterion correctly, you may need to find all of the equilibria in the decision problem.

You will be paid only for the number of rounds in which you correctly select the decision that satisfies this criterion, NOT for the number of points your selected decision would earn you in the decision problem. Remember that once a round is over, you will not be able to change your selection in that round.

(Click on the bar at the bottom of this screen to move on to the next screen)

[14TH SCREEN] {Mentioning the existence of different methods to find equilibrium}

To correctly identify the decisions that satisfy the criterion, you will need to know how find the equilibria of the decision problems. There are several methods you may find helpful, which we will now explain. In each round, you should feel free to use whichever method or combination of methods is best for you. Your payment will not depend on which method you use, but only on the number of rounds in which you correctly select the decision that satisfies the criterion.

Please read the rest of the (THIRD) page of the handout now. DO NOT move on to the next screen until instructed on the handout to do so.

(Click on the bar at the bottom of this screen to move on to the next screen)

[The following will be also written on the 3rd page of the handout.**]**

We now restate the criterion you will be asked to use to select your decisions, and describe some methods you may find helpful in applying it.

You will be allowed to keep this handout for reference during the experiment. HOWEVER, IT IS IMPORTANT TO READ IT CAREFULLY AND UNDERSTAND IT NOW, BECAUSE YOUR UNDERSTANDING WILL BE TESTED NEXT, AND YOU WILL ONLY BE ALLOWED TO PARTICIPATE IN THE EXPERIMENT IF YOU PASS THE TEST.

Recall that a combination of decisions, one for each person, such that each person's decision gives her/him the largest number of points possible, given the other's decision, is called an EQUILIBRIUM. Your decision in an equilibrium is called your EQUILIBRIUM DECISION, and her/his decision in an equilibrium is called her/his EQUILIBRIUM DECISION. You are asked to select your EQUILIBRIUM DECISION in each round.

Some decision problems may have more than one equilibrium, so that you have more than one equilibrium decision. In such problems you are asked to select your decision in the equilibrium that gives you the most points of any equilibrium. Suppose, for example, that a decision problem (not shown on your screen) has two equilibria, (*,@) with 66 points for You and 34 points for Her/Him, and (#,&) with 41 points for You and 76 points for Her/Him. Then, you must select *, your decision in the first equilibrium, because it gives you 66 points and the other equilibrium gives you only 41 points. Note that to be sure you have applied the criterion correctly, you may need to find all of the equilibria in the decision problem.

To correctly identify the decisions that satisfy the criterion, you will need to know how to find the equilibria of the decision problems. There are several methods you may find helpful, which we will now explain. In each round, you should feel free to use whichever method or combination of methods is best for you. Your payment will not depend on which method you use, but only on the number of rounds in which you correctly select the decision the criterion yields.

TURN TO THE NEXT PAGE AND READ IT.

PLEASE DO NOT MOVE ON TO THE NEXT SCREEN

[This sentence completes the 3rd page of the handout.**]**

[The following will be written on the 4th page of the handout.**]**

The methods are called: Dominance for Yourself, Dominance for Her/Him, Dominance Solvability (a combination of the previous two methods), and Equilibrium Checking.

DOMINANCE FOR YOURSELF: We say that one of your decisions is DOMINATED by another of your decisions if the other decision gives you more points no matter which decision s/he chooses. For example, in the decision problem displayed on the next screen (PLEASE MOVE ON TO THE NEXT SCREEN) your decision # is dominated by your decision *, because 87 is greater than 53 and 49 is greater than 21.

If eliminating decisions that are dominated for yourself reduces the decision problem to one in which you have only one decision left, then that decision (for example, * in the decision problem on your screen) is your only equilibrium decision.

(Make sure you understand the explanation of Dominance for Yourself before proceeding. Then read the next method, but DO NOT move to the next screen until instructed to do so.)

[15TH SCREEN] {2x2 table display - Same as 2nd screen}

[The table below will be shown to the subjects with the boxes open.**]**

	S/He: &	S/He: @	S/He: &	S/He: @
You: #	53	21	84	22
You: *	87	49	38	65
	YOUR POINTS		HER/HIS POINTS	
	You: #		You: *	

DOMINANCE FOR HER/HIM: We say that one of her/his decisions is DOMINATED by another of her/his decisions if the other decision gives her/him more points no matter which decision you choose.

For example, in the decision problem displayed on the next screen (PLEASE MOVE ON TO THE NEXT SCREEN) her/his decision & is dominated by her/his decision @, because 43 is greater than 21, and 76 is greater than 52.

(Make sure you understand the of Dominance for Her/Him above before proceeding. Then read the next method, but DO NOT move to the next screen until instructed to do so.)

[16TH SCREEN] {2x3 table display}

[The Table below will be shown to the subject with the boxes open.**]**

	S/He: %	S/He: @	S/He: &	S/He: %	S/He: @	S/He: &
You: *	54	21	98	85	43	21
You: #	79	45	67	45	76	52
	YOUR POINTS			HER/HIS POINTS		
	You: *			You: #		

DOMINANCE SOLVABILITY: We say that a decision problem is DOMINANCE SOLVABLE if it can be reduced to a problem with only one decision for each decision-maker by eliminating decisions that are dominated for yourself and/or her/him, decisions that become dominated once dominated decisions are eliminated, and so on.

For example, in the decision problem displayed on this screen (PLEASE DO NOT MOVE FROM THIS SCREEN) her/his decision & is dominated by her/his decision @, because 43 is greater than 21, and 76 is greater than 52.

TURN TO THE NEXT PAGE AND READ IT.

[This sentence completes the <u>4th</u> page of the handout.**]**

[The following will be written on the <u>5th</u> page of the handout.**]**

Once her/his decision & is eliminated, your decision * is dominated by #, because 79 is bigger than 54 and 45 is bigger than 21. Once your decision * is eliminated, her/his decision % is dominated by @, because 76 is bigger than 45. So, eliminating her/his decision &, then eliminating your decision *, and then eliminating her/his decision % reduces this decision problem to one in which her/his only decision is @ and your only decision is #. This means that the decision problem is dominance-solvable. As in the previous example, the only decision you have left, in this case #, is your only equilibrium decision.

(Make sure you understand the explanation of Dominance Solvability before proceeding. Then read the next method, but do not move to the next screen until instructed to do so.)

If eliminating dominated decisions, decision s that become dominated once dominated decisions are eliminated, and so on, does not reduce the decision problem to one with only one decision for each person, then you can find the equilibria by Equilibrium Checking.

EQUILIBRIUM CHECKING: As explained earlier, a combination of decisions, one for you and one for her/him, is an <u>EQUILIBRIUM</u> if your decision gives you the largest number of points possible, given her/his decision, AND her/his decision gives her/him the largest number of points possible, given your decision.

For example, in the decision problem displayed on the next screen (PLEASE MOVE ON TO THE NEXT SCREEN) there are no dominated decisions for yourself or for her/him, but the combination of your decision # and her/his decision & is an EQUILIBRIUM. This is because, if s/he chooses &, you would earn more points by choosing # (61) than by choosing ^ (39) or * (32). And if you choose #, s/he would earn more points by choosing & (74) than by choosing @ (35).

The combination of your decision * and he r/his decision @ is NOT an EQUILIBRIUM, because if you choose *, s/he would earn more points by choosing & (68) than by choosing @ (42), and so s/he would choose & instead of @. By making similar comparisons, you can check that there are no other equilibria in this decision problem, so that (#,&) is the only equilibrium and # is your only equilibrium decision.

[17TH SCREEN] {3x2 table display}

[The Table below will be shown to the subjects with the boxes <u>open</u>.**]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: ^	67	39	81	23
You: #	25	61	35	74
You: *	92	32	42	68
	YOUR POINTS		HER/HIS POINTS	

You: ^	You: #	You: *

(Make sure you understand the explanation of Equilibrium Checking before proceeding. Then move on to the next screen.)

DO NOT TURN TO THE NEXT PAGE BEFORE INSTRUCTED ON THE SCREEN TO DO SO.

[This sentence completes the 5th page of the handout.**]**

[18TH SCREEN] {Covered-boxes explanation}

In the actual experiment, the points in a table, like the one on the previous screen, will not be openly displayed. Instead they will be "hidden" in the boxes, as if the boxes were covered. However you will be able to open any box, just by POINTING AT it with the mouse (that is, moving the cursor into the box by sliding the mouse) and CLICKING the LEFT button of the mouse. You may open as many or as few boxes as you wish, as often and as long as you wish, and in any order. However, you will be able to have only one box open at a time. If you want to open a new box, you will have to close the previous box first by CLICKING the RIGHT button of the mouse. YOU ARE NOT ALLOWED TO WRITE DOWN THE NUMBERS IN THE BOXES. If you would like to know the number of points in a box that you do not remember, just open that box again.

The next screen displays the same table as before, but with the boxes covered. Use the mouse to practice opening boxes until you feel comfortable with the procedure. Then move on to the following screen by CLICKING on one of the decision boxes. For further explanation, raise your hand.

(Click on the bar at the bottom of this screen to move on to the next screen)

[19TH SCREEN] {3x2 table display - Same as 16th screen}

[The Table below will be shown to the subjects with the boxes closed.**]**

	S/He: @	S/He: &	S/He: @	S/He: &
You: ^	67	39	81	23
You: #	25	61	35	74
You: *	92	32	42	68
	YOUR POINTS		HER/HIS POINTS	

You: ^	You: #	You: *

[20TH SCREEN] {Explanation of intermediate screen}

At the conclusion of each round, you will see a screen asking you to proceed to the next round. If you wish, you may rest before proceeding. However, we ask you not to rest DURING a round. This screen will also tell you how many possible decisions you and s/he will have to choose from.

(Click on the bar at the bottom of this screen to move on to the next screen)

[21ST SCREEN] {Payment}

PAYMENT

You will be asked to select your equilibrium decision, using the criterion described above, in each of 18 interdependent decision problems. After you have selected your decisions for all 18 rounds, you will be paid $1.75 for each round in which you correctly selected the decision that satisfies the criterion.

You will be paid your earnings in cash, in private, after today's session.

[22ND SCREEN] {Understanding Test - 2nd part}

UNDERSTANDING TEST - PART 2

Before you start selecting your actual decisions for money, you must take the 2nd part of the understanding test (where you will demonstrate your understanding of the methods explained above). After you finish this part of the test it will be graded. YOU WILL ONLY BE ALLOWED TO CONTINUE IN THE EXPERIMENT IF YOU HAVE ANSWERED ALL THE QUESTIONS CORRECTLY, IN WHICH CASE YOU WILL EARN AN ADDITIONAL $4 JUST FOR PASSING THE TEST.

Turn to the SIXTH page of the handout, which contains the test questions, and move on to the next screen.

(Click on the bar at the bottom of this screen to move on to the next screen)

[23RD SCREEN] {Test Round 1 (3x2) - Introductory screen}

In this decision problem (Number 1):

You will have THREE decisions to choose from.

S/he will have TWO decisions to choose from.

PROCEED TO THE NEXT SCREEN ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[24TH SCREEN] {Test Round 1 - 3x2}

[The Table below will be shown to the subjects with the boxes <u>closed</u>.**]**

	S/He: &	S/He: @	S/He: &	S/He: @
You: *	87	43	53	82
You: ^	39	76	21	44
You: #	17	54	95	12
	YOUR POINTS		HER/HIS POINTS	

You: *	You: ^	You: #

In the decision problem shown on the screen:

A) Identify a DOMINATED decision for yourself, if there is one; otherwise write "None". _______.

B) Identify a DOMINATED decision for her/him, if there is one; otherwise write "None". _______.

C) Is the decision problem DOMINANCE SOLVABLE? Answer "Yes" or "No". ____________.

D) If so, reduce the decision problem to one decision for each decision-maker by eliminating DOMINATED decisions and so on, and write down the remaining decision for each decision-maker.

Your Decision ________ Her/His Decision _________

E) Do the decisions * and @ form an EQUILIBRIUM? If not, explain. _______________.

(Please move on to the next screen where you will find the next decision problem)

[25TH SCREEN] {Test Round 2 (2x3) - Introductory screen}

In this decision problem (Number 2):

You will have TWO decisions to choose from.

S/he will have THREE decisions to choose from.

PROCEED TO THE NEXT SCREEN ONLY WHEN YOU FEEL READY.

(Click on the bar at the bottom of this screen to move on to the next screen)

[26TH SCREEN] {Test Round 2 (2x3)}

[The Table below will be shown to the subjects with the boxes closed.**]**

	S/He: %	S/He: @	S/He: &	S/He: %	S/He: @	S/He: &
You: #	81	35	42	67	25	92
You: *	23	74	68	39	61	32
	YOUR POINTS			HER/HIS POINTS		
	You: #			You: *		

In the decision problem shown on the screen:

A) Identify an EQUILIBRIUM combination of decisions.

Your Decision _________________. Her/His Decision _________________.

B) Identify a DOMINATED decision for yourself, if there is one; otherwise write "None". ____________________________.

C) Identify a DOMINATED decision for her/him, if there is one; otherwise write "None". ____________________________.

D) Is the decision problem DOMINANCE SOLVABLE? Answer "Yes" or "No". _____________.

[27TH SCREEN] {Last Screen of the Instructions input file}

You have now completed the second part of the test. Please raise your hand until the experimenter sees you, and please remain in your seat. An experimenter will come to collect your handout with your answers for the test. You will then take a short break before starting the actual decision problems. Please remain in your seat. **THE END**

(Click on the bar at the bottom of this screen to exit the program)

APPENDIX B: GAMES

Here we display the games ordered as in Table 1, but with decisions ordered as they appeared to Row and Column subjects; the equilibrium is identified by underlining its payoffs.

Game	To Row subject						To Column subject					
2A	72	31		93	46		93	52		72	84	
	84	$\underline{55}$		52	$\underline{79}$		46	$\underline{79}$		31	$\underline{55}$	
2B	94	$\underline{38}$		23	$\underline{57}$		23	89		94	45	
	45	14		89	18		$\underline{57}$	18		$\underline{38}$	14	
3A	$\underline{75}$	42		$\underline{51}$	27		$\underline{51}$	80		$\underline{75}$	48	
	48	89		80	68		27	68		42	89	
3B	21	87		92	43		92	$\underline{36}$		21	$\underline{55}$	
	$\underline{55}$	16		$\underline{36}$	12		43	12		87	16	
4A	59	46	85	58	83	61	58	29		59	38	
	38	$\underline{70}$	37	29	$\underline{52}$	23	83	$\underline{52}$		46	$\underline{70}$	
							61	23		85	37	
4B	31	$\underline{68}$		32	$\underline{46}$		32	43	65	31	72	91
	72	47		43	61		$\underline{46}$	61	84	$\underline{68}$	47	43
	91	43		65	84							
4C	28	57		37	58		37	36	$\underline{69}$	28	22	$\underline{51}$
	22	60		36	84		58	84	45	57	60	82
	$\underline{51}$	82		$\underline{69}$	45							
4D	$\underline{42}$	57	80	$\underline{64}$	43	39	$\underline{64}$	27		$\underline{42}$	28	
	28	39	61	27	68	87	43	68		57	39	
							39	87		80	61	
5A	53	24		86	19		86	57	23	53	79	28
	79	42		57	73		19	73	$\underline{50}$	24	42	71
	28	$\underline{71}$		23	$\underline{50}$							
5B	76	25		93	12		93	40	16	76	43	94
	43	$\underline{74}$		40	$\underline{62}$		12	$\underline{62}$	37	25	$\underline{74}$	59
	94	59		16	37							
6A	21	$\underline{52}$	75	26	$\underline{73}$	44	26	55		21	88	
	88	25	59	55	30	81	$\underline{73}$	30		$\underline{52}$	25	
							44	81		75	59	

Game	To Row subject								To Column subject						
6B	42	95	18	|	45	78	96		45	<u>76</u>	|	42	<u>64</u>		
	<u>64</u>	14	39	|	<u>76</u>	27	61		78	27	|	95	14		
									96	61	|	18	39		
7A	87	18	<u>63</u>	|	32	37	<u>76</u>		32	89	|	87	65		
	65	96	24	|	89	63	30		37	63	|	18	96		
									<u>76</u>	30	|	<u>63</u>	24		
7B	67	95	31	|	91	64	35		91	49	|	67	89		
	89	23	<u>56</u>	|	49	53	<u>78</u>		64	53	|	95	23		
									35	<u>78</u>	|	31	<u>56</u>		
8A	<u>72</u>	26	|	<u>59</u>	20				<u>59</u>	14	83	|	<u>72</u>	33	28
	33	59	|	14	92				20	92	61	|	26	59	85
	28	85	|	83	61										
8B	46	57	|	16	88				16	<u>49</u>	82	|	46	<u>71</u>	42
	<u>71</u>	28	|	<u>49</u>	24				88	24	60	|	57	28	84
	42	84	|	82	60										
9A	22	57	|	14	55		14	42	60	<u>66</u>	|	22	30	15	<u>45</u>
	30	28	|	42	37		55	37	88	31	|	57	28	61	82
	15	61	|	60	88										
	<u>45</u>	82	|	<u>66</u>	31										
9B	56	38	89	32	|	58	29	62	86		58	23	|	56	15
	15	43	61	<u>67</u>	|	23	31	16	<u>46</u>		29	31	|	38	43
											62	16	|	89	61
											86	<u>46</u>	|	32	<u>67</u>

APPENDIX C: SUBJECTS' DECISIONS IN B, OB, AND TS TREATMENTS

The first two characters in the subject code identify the treatment and run, the third identifies the subject's role (Row or Column), and the fourth and fifth are the subject's identification number (gaps in the number sequence correspond to subjects who were dismissed for failing the Understanding Test).

	GAMES																	
SUBJECTS	2A	2B	3A	3B	4A	4B	4C	4D	5A	5B	6A	6B	7A	7B	8A	8B	9A	9B
B1R01	T	T	T	T	T	T	T	T	B	B	T	B	B	T	B	B	T	T
B1R02	T	T	T	T	B	T	T	T	B	B	T	T	B	T	B	B	T	T
B1R03	T	T	B	B	T	T	B	B	T	M	B	T	B	B	T	T	B	B
B1R04	B	B	B	B	T	B	T	B	B	M	B	B	B	B	M	B	B	B
B1R05	T	T	B	T	B	B	T	T	B	B	B	B	B	B	B	B	T	T
B1R06	B	T	T	B	B	B	B	T	B	B	T	T	T	B	B	B	T	T
B1R09	T	T	T	T	T	M	T	T	B	T	T	T	B	T	B	B	T	T
B1R10	T	T	T	T	T	T	T	B	B	B	T	T	T	T	B	B	T	T
B1R21	B	B	B	B	B	B	B	B	M	T	B	B	B	B	M	M	B	B
B1R22	T	T	T	T	T	B	T	T	B	B	T	T	T	B	B	B	T	T
B1R23	T	T	T	T	T	B	T	T	T	B	T	B	B	B	T	T	T	T
B1R24	T	T	B	T	B	B	T	T	B	B	T	T	T	T	B	B	T	T
B1C11	R	R	L	L	L	L	R	L	R	L	R	R	M	R	R	R	L	L
B1C12	R	L	L	L	R	R	L	L	L	L	R	M	M	R	R	L	L	R
B1C13	L	L	L	L	L	L	L	L	L	L	R	R	R	R	L	L	L	L
B1C14	L	L	L	L	L	L	L	L	R	L	R	R	R	R	R	L	L	L
B1C15	R	L	L	R	R	R	L	L	R	L	R	L	L	L	L	R	L	L
B1C16	L	L	L	L	L	L	L	L	L	L	R	R	R	R	R	R	L	L
B1C18	R	L	L	L	L	L	L	L	L	L	R	R	R	R	L	L	L	L
B1C20	L	L	L	L	R	R	L	L	R	R	R	R	R	R	L	L	L	R
B1C25	L	L	L	L	L	L	L	L	L	L	L	R	L	L	L	L	L	L
B1C27	R	R	L	R	L	R	L	L	L	L	M	M	R	R	R	R	L	R
B1C28	R	R	L	L	L	L	R	R	R	R	R	R	R	R	R	R	R	L
B1C30	L	R	L	L	L	L	L	L	R	L	R	L	R	R	L	R	L	L
B2R01	T	T	B	T	B	B	T	T	B	B	T	T	B	T	B	B	T	T
B2R03	T	T	T	T	T	T	T	T	B	M	T	T	T	T	B	B	T	T
B2R05	B	T	B	T	T	B	T	T	B	B	T	B	B	B	B	B	T	B
B2R06	T	T	T	B	T	B	T	T	B	B	B	B	B	B	T	T	T	T
B2R07	T	B	B	B	B	B	T	T	B	B	B	B	B	B	M	M	T	B
B2R08	T	T	T	B	T	T	T	T	B	B	T	B	T	T	B	B	T	T
B2R09	T	T	T	T	T	T	T	T	T	B	B	T	T	T	T	T	T	T
B2R10	B	T	B	T	T	B	T	T	B	B	T	T	T	T	T	B	T	T
B2R21	T	T	T	T	T	T	T	T	B	B	T	T	T	T	B	B	T	T
B2R22	B	T	T	T	B	B	T	T	B	B	B	T	T	T	B	B	T	T
B2C11	L	L	L	L	L	L	L	L	L	L	R	R	R	R	R	L	L	L
B2C12	L	L	L	L	L	L	L	L	L	L	R	R	R	R	L	L	L	R
B2C14	R	L	L	L	L	L	R	R	R	L	R	L	R	R	R	R	R	L
B2C16	L	L	L	L	L	L	L	L	L	L	R	R	R	R	L	L	L	L
B2C17	R	R	R	L	L	L	R	R	R	R	R	L	R	M	R	L	R	L
B2C18	L	L	L	L	L	L	L	L	L	R	R	R	R	L	L	L	L	L
B2C19	R	R	L	L	L	L	R	L	R	L	R	R	R	R	R	R	L	L
B2C26	L	L	L	L	L	L	R	L	L	L	R	R	L	L	L	L	L	L
B2C27	L	R	R	L	L	L	R	L	R	L	M	M	M	R	R	R	R	L
B2C29	R	R	L	L	L	L	R	R	R	R	R	R	R	R	R	L	R	L
B2C30	R	L	L	L	L	L	R	L	R	L	R	R	R	R	R	R	R	L

	GAMES																	
SUBJECTS	**2A**	**2B**	**3A**	**3B**	**4A**	**4B**	**4C**	**4D**	**5A**	**5B**	**6A**	**6B**	**7A**	**7B**	**8A**	**8B**	**9A**	**9B**
OBR01	T	T	T	T	T	B	T	T	B	B	B	B	B	B	B	B	T	T
OBR02	T	T	T	T	B	T	T	T	B	B	T	T	B	B	B	B	T	T
OBR03	B	T	T	T	T	B	T	T	B	B	T	T	T	B	B	T	T	T
OBR04	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	B	T	T
OBR05	T	T	T	T	T	M	T	T	T	B	T	B	B	B	T	T	T	T
OBR06	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
OBR07	T	T	T	T	T	T	T	T	B	B	T	T	T	T	B	B	T	T
OBR09	B	T	T	T	T	B	T	T	B	B	T	T	T	T	B	B	T	T
OBR21	T	T	B	B	B	B	T	T	B	B	B	B	B	B	B	B	T	B
OBR22	B	T	B	B	B	B	T	T	B	M	B	B	B	T	B	M	T	T
OBR23	T	T	T	T	T	T	T	T	B	B	T	T	T	T	B	B	T	T
OBR24	T	T	T	T	T	T	T	T	B	B	T	T	T	T	T	T	T	T
OBR25	T	T	T	T	T	B	T	T	B	B	T	T	T	T	B	B	T	T
OBR26	T	T	T	B	T	B	T	T	B	M	T	B	T	B	B	B	T	T
OBC11	L	L	L	L	L	L	L	L	L	L	R	R	R	R	R	R	L	L
OBC12	L	L	L	L	R	L	R	L	L	L	R	R	R	M	L	R	L	L
OBC14	L	L	R	L	R	L	L	R	L	R	R	R	R	R	L	L	R	R
OBC15	L	L	L	L	L	L	L	L	R	L	L	M	L	L	R	L	L	L
OBC16	L	L	L	L	L	L	L	M	L	R	R	R	R	R	L	L	R	L
OBC17	L	L	L	L	L	L	L	L	L	L	R	R	R	L	L	R	L	L
OBC20	R	L	L	L	L	L	R	L	R	R	L	R	L	R	R	R	R	L
OBC27	L	L	L	L	L	L	R	L	L	R	R	R	R	R	R	R	R	L
OBC28	R	R	L	L	L	L	R	R	L	R	L	R	R	R	L	R	R	L
OBC29	L	L	L	L	L	L	R	L	L	L	L	L	L	L	L	R	L	L
OBC30	R	R	L	L	L	L	R	R	R	R	R	L	R	R	R	L	R	L
OBC31	R	L	L	L	L	L	R	L	L	L	R	R	R	R	R	R	R	L
OBC32	L	L	L	L	L	L	L	L	L	L	L	R	L	R	L	R	L	L
TSR02	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR03	T	T	T	T	T	T	T	T	T	T	T	T	T	T	B	B	T	T
TSR05	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR07	T	T	T	T	T	T	T	T	T	T	T	T	B	B	T	T	T	T
TSR12	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR14	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR16	T	T	B	T	T	B	T	T	B	T	T	T	T	T	T	T	T	T
TSR17	B	T	T	T	B	B	B	B	T	M	T	B	B	B	T	M	B	B
TSR18	T	T	T	T	T	T	T	T	T	M	B	B	B	B	M	B	T	B
TSR19	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR20	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR21	T	T	T	T	T	T	T	B	B	B	B	T	B	B	T	T	B	B
TSR23	T	T	T	T	T	B	T	T	B	T	T	T	T	T	T	T	T	T
TSR29	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T
TSR31	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T	T

T = Top
M = Middle
B = Bottom

L = Left
M = Middle
R = Right

APPENDIX D: CHARACTERIZATION OF TYPES' INFORMATION SEARCH IMPLICATIONS UNDER HYPOTHESES A AND B

We now describe the implications of hypotheses A and B for each of our types, with reference to example games from our design, reproduced in Appendix B. All games are discussed from the point of view of the Row player.

Equilibrium type: An equilibrium subject can use equilibrium-checking to identify his equilibrium decision in any of our games, and that is the only method he can use in games that are not dominance-solvable. Equilibrium-checking can be done either decision combination by decision combination or by "best-response dynamics," which rules out some combinations using the fact that only decisions that are best responses can be part of an equilibrium. But in games that are dominance-solvable he can alternatively use iterated dominance or a combination of iterated dominance and equilibrium-checking. For our games, the minimal set or sets of sufficient look-ups or adjacent pairs have a simple characterization that depends only on whether or not the game is dominance-solvable. We now illustrate how this works in each case, using games from our design.

In our dominance-solvable games, there is only one way to perform iterated dominance, and this always yields a minimal set of look-ups or comparisons over all possible methods. Given this, Equilibrium hypothesis A requires that the look-up sequence include all look-ups associated with the dominance and iterated dominance relationships by which the game can be solved, namely all payoffs for the own or other player's decisions being compared except those that can be eliminated using dominance relationships identified elsewhere in the iteration. In Game 3A for the Row player, for instance, the Equilibrium type needs to identify that his partner has a dominant decision, Left, which he can do only by looking up all of his partner's payoffs, 51, 27, 80, and 68. To identify his best response to Column's dominant decision, he needs to look up only two of his own payoffs, 75 and 48. Thus, Equilibrium Hypothesis A implies that the payoffs 51, 27, 80, 68, 75, and 48 appear at least once somewhere in the look-up sequence. Equilibrium hypothesis B requires that each of the comparisons required to identify that his partner has a dominant decision and to identify his best response to it is represented by an adjacent look-up pair somewhere in the sequence. In Game 3A, Equilibrium hypothesis B implies that the payoff pairs (51, 27), (80, 68), and (75, 48) each appear (in either order) as an adjacent pair at least once somewhere in the sequence.

In our non-dominance-solvable games, there is never any pure-strategy dominance, and checking for equilibrium via best-response dynamics always yields a minimal set of look-ups or comparisons over all possible methods. Equilibrium hypothesis A then requires that the look-up sequence include all of the look-ups associated with up-down comparisons of own payoffs or left-right comparisons of other's payoffs starting from each possible decision combination, except those that can be eliminated because they are never best responses. Equilibrium hypothesis B then requires that each of the associated comparisons is represented by an adjacent look-up pair in the sequence, namely all up-down pairs of own payoffs or left-right pairs of other's payoffs for each possible decision combination, except those that can be eliminated as never best responses. In Game 7A, for example, the Row player can start by identifying that if he played Bottom, his partner's best response would be Left. This requires him to look up three of his partner's payoffs, 30, 63, and 89, and to perform the payoff comparisons (89, 63) and (89, 30). Thus, Equilibrium hypothesis B requires that (89, 63) and (89, 30) appear as adjacent pairs in the sequence. Row's best response to Left is Top. This is identified by looking up the payoffs 87 and 65 and comparing them. Thus, Equilibrium hypothesis A requires that the payoffs 87 and 65 appear in the sequence, and Equilibrium hypothesis B requires that they appear at least once as an adjacent pair. Column's best response to Top is Right, which can be identified by looking up three of Column's payoffs, 32, 37, and 76 and performing the payoff comparisons (76, 37) and (76, 32). Thus, hypothesis A requires that the payoffs 32, 37 and 76 appear somewhere in the sequence, and hypothesis B requires that (76, 37) and (76, 32) appear at least once as adjacent pairs. Row's best response to Right is Top, which he can identify by looking-up the payoffs 63 and 24 and comparing them. Thus, Equilibrium hypothesis A requires that the payoffs 63 and 24 appear somewhere in the sequence, and Equilibrium hypothesis B requires that they appear at least once as an adjacent pair. At this point Row has identified that the decision combination Top, Right is an equilibrium, and that it is the only equilibrium in the game. To summarize, in game 7A for the Row player, Equilibrium hypothesis A requires that the look-up sequence include the payoffs 87, 65, 63, 24, 32, 37, 76, 89, 63, and 30, and Equilibrium hypothesis B requires that the look-up sequence include (89, 63), (89, 30), (63, 24), (87, 65), (76, 37), and (37, 32) as adjacent pairs.

Sophisticated type: For the Sophisticated type, the characterization of the minimal set or sets of sufficient look-ups or adjacent pairs for our games depends only on whether or not the player has a dominant decision. If so, he can identify his predicted decision simply by identifying his dominant decision. If not, he needs to form his beliefs, deducing them from his knowledge of the game's dominance and iterated dominance relationships and its set of equilibria, and of other players' typical responses to games with that structure, and then to compare the expected payoffs of (at least) his undominated decisions. He can make these expected-payoff comparisons via running totals, either by up-down comparisons column by column or left-right comparisons row by row; we allow either kind, but rule out mixtures of the two, so his look-up sequence must include a complete set of one or the other kind of comparison.

When the player has a dominant decision, Sophisticated hypothesis A requires only that his look-up sequence include all look-ups needed for the payoff comparisons needed to identify his dominant decision. In Game 2A, for instance, Sophisticated hypothesis A implies that the payoffs 72, 31, 84, and 55 appear at least once somewhere in the look-up sequence. Sophisticated hypothesis B then requires only that each of the associated comparisons is represented by an adjacent look-up pair somewhere in the sequence, namely all up-down pairs of own payoffs needed to identify his dominant decision. In Game 2A, Sophisticated hypothesis B requires that the payoff pairs (72, 84) and (31, 55) appear as adjacent pairs (in either order) somewhere in the sequence.

When the player does not have a dominant decision, forming his beliefs and comparing the expected payoffs of his undominated decisions involves at least the same look-ups and comparisons as the Equilibrium type, and in our games some additional look-ups and comparisons. Sophisticated hypothesis A then requires that the look-up sequence include all of his own and the other player's payoffs, because they are all relevant to forming his beliefs and/or comparing the expected payoffs of his decisions. In Game 3A, for instance, Sophisticated hypothesis A implies that the payoffs 51, 27, 80, 68, 75, 48, 42, and 89 appear at least once somewhere in the sequence. Sophisticated hypothesis B requires that the look-up sequence include the same adjacent pairs as Equilibrium hypothesis B, plus any additional adjacent pairs needed to identify the dominance relationships among his own and his partner's decisions. In addition, the look-up sequence must include a complete set either of the adjacent pairs associated with all up-down comparisons of his own payoffs for his undominated decisions, or of the adjacent pairs associated with all such left-right comparisons. In Game 3A, Sophisticated hypothesis B implies that the comparisons (51, 27), (80, 68), (75, 48), and (42, 89) appear at least once as adjacent pairs somewhere in the sequence. In Game 7A, Sophisticated hypothesis A implies that the payoffs required by Equilibrium hypothesis A plus the payoffs 18 and 96 appear at least once somewhere in the sequence. Sophisticated hypothesis B requires that the sequence include the same adjacent pairs as Equilibrium hypothesis B, plus the adjacent pairs (30, 63), (32, 37), and (18, 96), so as to identify all possible dominance relationships among the player's own and his partner's decisions.

Naïve type: A Naïve player needs only to compare the expected payoffs of his decisions, given a uniform prior over his partner's decisions. He can do this for any two possible decisions via running expected-payoff totals, either by up-down comparisons column by column, or by left-right comparisons row by row. As before, we allow either kind, but rule out mixtures of the two, so his look-up sequence must include a complete set of one or the other kind of comparison. With regard to hypothesis B, he can avoid some comparisons by eliminating decisions that can be seen to have lower expected payoffs than some other decision, using adjacent payoff pairs that appear in the sequence. Naïve hypothesis A then requires that the look-up sequence include all the player's own payoffs and only those payoffs. In Game

3A, for example, Naïve hypothesis A requires that the payoffs 75, 48, 42, and 89 appear at least once somewhere in the sequence. Naïve hypothesis B requires that a complete set either of up-down comparisons or of left-right comparisons sufficient to compare the expected payoffs of his undominated decisions and/or identify his dominated decisions is represented by adjacent pairs in the sequence. In Game 3A, Naïve hypothesis B requires that either the two comparisons (75, 42) and (48, 89) or the two comparisons (75, 48) and (42, 89) appear at least once (in either order) somewhere in the sequence. In Game 5A, Naïve hypothesis A requires that the payoffs 53, 24, 79, 42, 28, and 71 appear at least once somewhere in the sequence. In Game 5A, if the player compares the expected payoffs of his undominated decisions via left-right comparisons, Naïve hypothesis B requires that (53, 24), (79, 42), and (28, 71) appear at least once somewhere in the sequence. If instead the player uses up-down comparisons two decisions at a time (remembering the best decision so far, and comparing it with each remaining decision), then either the set of comparisons (53, 79), (24, 42), (79, 28), and (42, 71), or the set of comparisons (53, 28), (24, 71), (79, 28), and (42, 71) must be represented by adjacent pairs somewhere in the sequence.

Optimistic type: An Optimistic player needs only to compare the maximal payoffs of his own decisions, which can be done by scanning all own payoffs in any desired order, hence with no restrictions on comparisons, keeping a record of the highest payoff found so far. He can eliminate some of these look-ups if he finds a payoff for a new decision higher than the maximum for all previously checked decisions. Thus, Optimistic hypothesis A requires that the look-up sequence include all the player's own payoffs and only those payoffs, except that if his look-ups include all of his payoffs for all but one of his own decisions and an own payoff for the remaining decision that is higher than his maximum payoff for the former decisions, then his look-up sequence need not include any more payoffs for the latter decision. In Game 3A, for example, Optimistic hypothesis A requires that the payoffs 75, 42, and 89 appear at least once somewhere in the sequence. Optimistic hypothesis B implies no restrictions beyond those implied by Optimistic hypothesis A in any of our games.

Pessimistic type: A Pessimistic player needs only to compare the minimal payoffs of his own decisions. This requires a set of left-right comparisons of his own payoffs, as there is no reliable way to identify it with up-down comparisons. He can eliminate some look-ups if he finds a payoff for a new decision lower than the previously identified minimum for another decision. Thus, Pessimistic hypothesis A requires that the look-up sequence include all the player's own payoffs and only those payoffs, except that if his look-ups include all of his own payoffs for one of his decisions and an own payoff for another decision that is lower than his minimum payoff for the former decision, then his look-up sequence need not include any more payoffs for the latter decision. In Game 3A, for example, Pessimistic hypothesis A requires that the payoffs 48, 89, and 42 appear somewhere in the sequence. Pessimistic hypothesis B requires that a set of left-right comparisons sufficient to identify the maximin decision be represented by adjacent look-up pairs in the sequence. In Game 3A, Pessimistic hypothesis B requires that the comparison (48, 89) appear (in either order) somewhere in the sequence.

Altruistic type: An Altruistic player needs only to compare the totals of his own and the other's payoffs for each feasible decision combinations. Altruistic hypothesis A therefore requires that his look-up sequence include all of his own and the other player's payoffs, and Altruistic hypothesis B requires that each of the above totals be represented by an adjacent look-up pair in the sequence. In Game 3A, for example, Altruistic hypothesis A requires that the payoffs 75, 51, 42, 27, 48, 80, 89, and 68 appear somewhere in the sequence, and Altruistic hypothesis B requires that the comparisons (75, 51), (42, 27), (48, 80), and (89, 68) appear (in either order) somewhere in the sequence.