First-Year M. Phil Experimental Economics Lectures, Hilary Term 2010

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(with heavy debts to Miguel Costa-Gomes)

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Motivation and overview

● Role of experiments: to fully test economic theories requires control of preferences, information, institutions and/or the structure of interactions, often more than possible in the field

● Econometrician Guy Orcutt (quoted in Smith (1982 *AER*)):

"…the econometrician as being in the same predicament as that of an electrical engineer who has been charged with the task of deducing the laws of electricity by listening to a radio play."

(But contrast the use of simple statistical tests in psychology and early economics experiments, versus heavier contemporary use of econometrics as substitute for unattainable full control with regard to complex of phenomena.)

● Importance of scientific culture, respect for but innovation within culture, replicability

● Importance of ethics in treatment of subjects

● Importance of ethics in generating and reporting data, unbiased design and choice of data to analyze and report (Sir Arthur Evans's model of Knossos)

Advantages of experimental methods:

- Control

- Replicability

Limitations:

- Subjects may not be representative of relevant decision makers in the field (but can use different kinds of subjects, realistic framing, field experiments with more control than "natural experiments")

- Simplicity of laboratory environments may limit transfer of results to parallel field environments (theories should work in simple settings too, but need to explore range applicability, transfer)

- Technical difficulties in establishing control may also limit effectiveness, e.g. when seeking to elicit information about individual preferences via hypothetical contingent valuation choices

Experimental Designs

There are three approaches to a given theoretically relevant aspect of an environment: control, measure, or assume

Most economic experiments try to control preferences, information, institutions and/or the structure of interactions

Control of preferences is accomplished via significant, salient marginal rewards to decisions in money or (to control for risk preferences when necessary) "binary lotteries"; neutral framing (usually more abstract than in the field, but concrete framing can be neutral too); conducting experiments so participants don't perceive any behavior as being correct or expected (unless this is a treatment variable; e.g. avoid statements like "You will be doing us a favor if you maximize your money payoffs."); and avoiding face-to-face or nonanonymous interactions (unless this is a treatment variable; subjects should be anonymous to the experimenters as well as each other, to minimize "social" effects on preferences)

Control of knowledge is accomplished via public announcements, practice runs, and understanding tests); "public knowledge" is experimenter's analog of game-theoretic notion of common knowledge; special problems arise in controlling beliefs, especially about stochastic processes

Control of institutions and/or the structure of interactions is accomplished via standardized, fully reported procedures; in game experiments, often need to give subjects experience and observe or control the effects of learning, usually solved by a repeated-game design with measures to eliminate or at least discourage the use of repeated-game strategies

Competition in partial-equilibrium markets (with help from Al Roth and Miguel Costa-Gomes)

Edward H. Chamberlin, "An Experimental Imperfect Market," *Journal of Political Economy* 56 (1948), 95-108

Vernon Smith, “An Experimental Study of Competitive Market Behavior,” *Journal of Political Economy* 70 (1962), 111-137.

Questions: What does "perfect competition" require? How well do competitive markets aggregate participants' private information? How do market institutions affect market performance?

(Before Smith's paper, most people thought that perfect competition required dozens, if not hundreds, or people on each side of the market, and that they had to be perfectly informed.

But one cannot directly address this set of questions with field data, because traders' preferences and information are unobservable.

Can address questions indirectly by *assuming* perfect competition and seeing how well the econometrics works, but that's not very convincing.)

History: Edward Chamberlin, “An Experimental Imperfect Market,” *Journal of Political Economy* 56 (1948), 95-108.

Chamberlin's Design: Chamberlin initiated the design of laboratory markets with controlled supply and demand, which he induced by giving each buyer (seller) a redemption value (cost) for her or his single unit. Each trader knew his own redemption value or cost, but not others’. Here, equilibrium price = 56-58, equilibrium quantity = 15.

TABLE 1

Transactions B	Transactions S	Transactions P
56	18	55
54	26	40
72	30	50
84	34	45
44	44	44
102	42	42
80	20	40
60	28	55
48	40	45
76	36	45
94	52	55
68	58	62
66	46	55
82	32	58
90	72	72
104	54	54
52	50	50
86	64	64
74	62	69

Left Over

B	S
38	68
50	66
28	82
32	88
18	90
26	84
22	104
24	78
30	80
20	98
34	74
58	70

Market Schedules B	Market Schedules S
104	18
102	20
94	26
90	28
86	30
84	32
82	34
80	36
76	40
74	42
72	44
68	46
66	50
60	52
58	54
56	58
54	62
52	64
50	66
48	68
44	70
38	72
34	74
32	78
30	80
28	82
26	84
24	88
22	90
20	98
18	104

Equilibrium sales 15
Actual sales 19

Equilibrium price 57 (56–58)
Average of actual prices ... 52.63

dicated by the demand and supply curves, is explained by the fact that imperfections introduce prices above and below the equilibrium figure. At prices above it some “normally” excluded sellers may make bargains; at prices below it

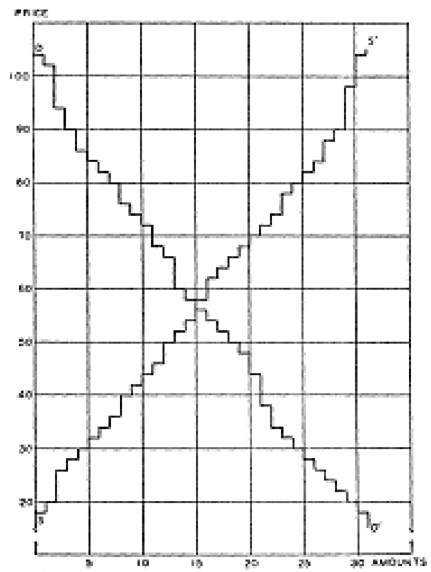


FIG. 1

some “normally” excluded buyers may make bargains. For instance, the seller whose limit is 58—and who would not make a sale if the market were perfect—has a good chance to make a bargain

Chamberlin's Institution: Subjects walk around (in classroom), bargain in pairs or groups. Once a buyer and seller reach a deal, they drop out. Transaction price is recorded on the blackboard (in fact, not always…). Market operated for a single trading period. The competitive equilibria in this market have an equilibrium quantity of 15, and an equilibrium price of 56-58.

Chamberlin's Results: Chamberlin found too much trading (19 on average, versus 15 in equilibrium), average prices usually lower than in equilibrium, "No tendency for prices to move toward equilibrium during the course of the market." (But why should they?)

Of course, after each trade, the remaining supply and demand curves shift as traders leave the market, so he graphed the "moving equilibrium."

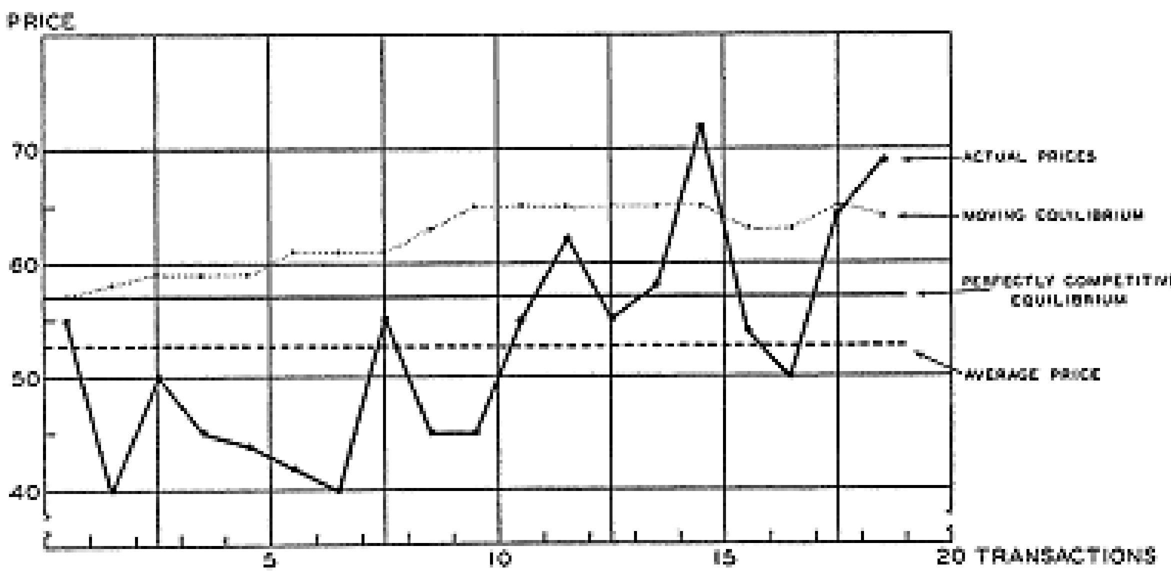
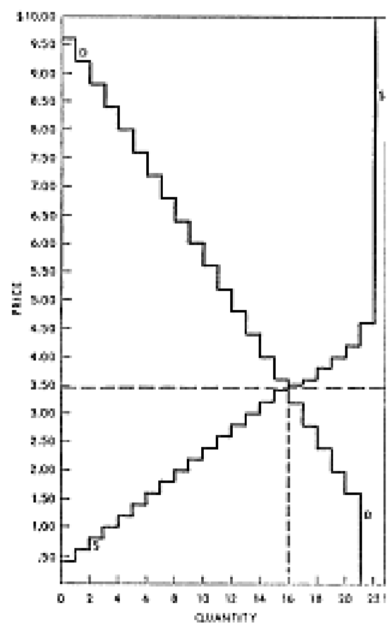


FIG. 3

Smith continued this line of work by replacing Chamberlin's trading institution with a repeated double auction, finding very different results.

Smith's Design: Supply and demand again induced by giving each buyer (seller) a redemption value (cost) for her or his one or more units. Each trader knows own redemption value or cost, not others'.



Smith's Institution: Double oral auction, buyers and sellers can freely enter limit orders (bids or asks) and accept others' asks or bids. No restrictions on messages. Prices of completed transactions always recorded on blackboard. Traders with no more units drop out. Market operated for 3-6 periods, each lasting 5-10 minutes.

Smith on his differences from Chamberlin:

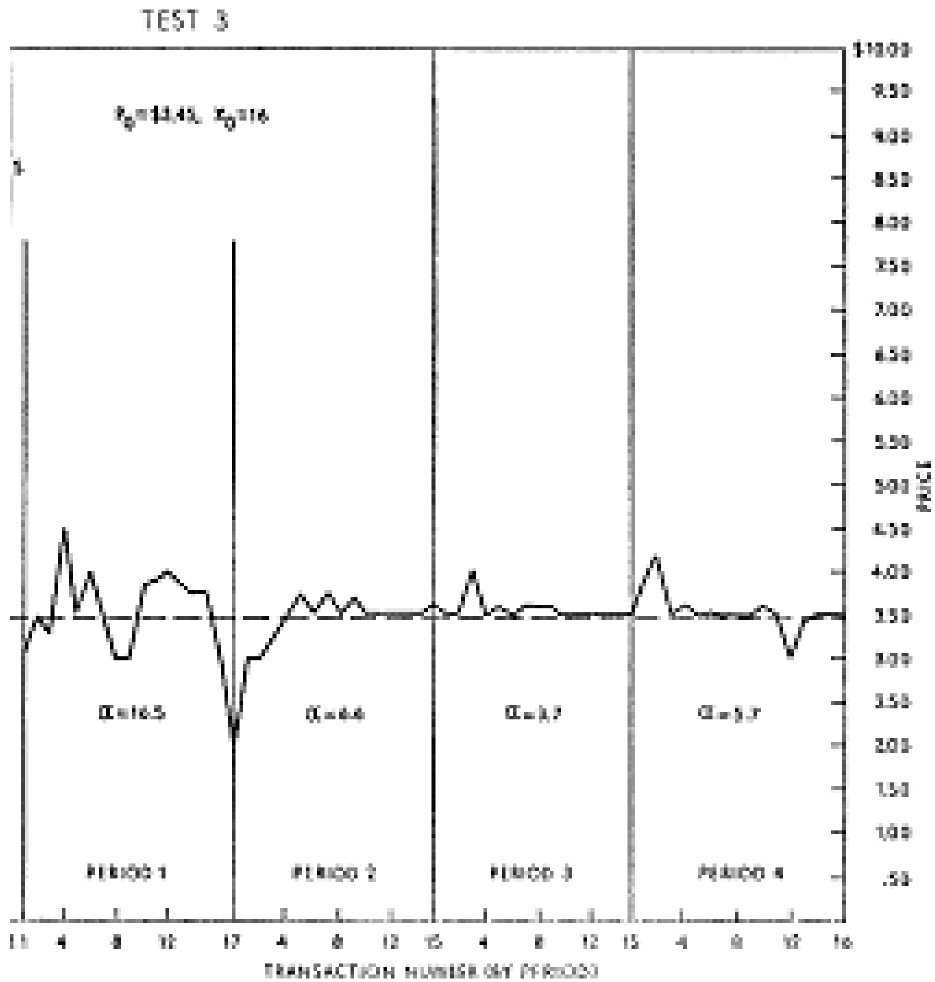
“The design of my experiments differs from that of Chamberlin in several ways. In Chamberlin’s experiment the buyers and sellers simply circulate and engage in bilateral haggling and bargaining until they make a contract or the trading period ends. As contracts are made the transaction price is recorded on the blackboard (in fact, not always…). Each trader’s attention is directed to the one person with whom he is bargaining, whereas in my experiment each trader’s quotation is addressed to the entire trading group one quotation at a time.”

“[Also] Chamberlin’s experiment constitutes a pure exchange market operated for a single trading period. There is, therefore, less opportunity for traders to gain experience and to modify their subsequent behavior in the light of such experience. It is only through some learning mechanism of this kind that I can imagine the possibility of equilibrium being approached in any real market.”

“One important condition operating in our experimental markets is not likely to prevail in real markets. The experimental conditions of supply and demand are held constant over several successive trading periods in order to give any equilibrating mechanisms an opportunity to establish an equilibrium over time. Real markets are likely to be continually subjected to changing conditions of supply and demand.”

Smith's Results:

Smith showed that for a wide range of supply and demand schedules, the double auction tended to quickly converge in repeated markets to the competitive equilibrium price and quantity.



Sometimes subjects traded too little, sometimes too much.

But overall quantity traded was close to equilibrium, and the average price tended to approach the equilibrium price over time.

Prices of trades initiated by buyers were generally lower than prices of trades initiated by sellers.

The sequence of price changes was typically negatively autocorrelated, around -0.5.

Traders with higher expected surplus usually traded earlier than traders with lower expected surplus.

The outcomes were robustly competitive even with three or four people on each side of the market.

The results even closer to competitive equilibrium when traders were *not* informed about others' values.

Powerful but not unlimited aggregation of private information for some market institutions.

Smith's paper may still be the most striking illustration of the power of experiments to change how we think.

Discrimination in labor markets

Marianne Bertrand and Sendhil Mullainathan, "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination," *American Economic Review* 94 (2004), 991-1013.

Question: Is observed racial discrimination explained by models of statistical discrimination, or is it the product of racial preferences or beliefs?

Design: Create resumes that are identical in every aspect, except the name and address of the applicant. In one case the name and address suggest an African-American applicant, in the other they suggest a Caucasian applicant. Then use these resumes to apply for jobs and set up phone lines with African-American and Caucasian voices to receive callbacks from interested employers.

Results: The rate of callbacks is much lower for the African-American resumes, and, in contrast to most models of statistical discrimination, the gap in callback rates grows with the skill level of the paired ("African-American" and "Caucasian") applicants.

A simple and powerful example of a field experiment that measures something that is normally impossible to untangle from other correlated factors that may affect labor market discrimination in naturally occurring data, or to measure credibly in the laboratory.

Measuring trust

Edward Glaeser, David Laibson, Jose Scheinkman, and Christine Soutter, "Measuring Trust," *Quarterly Journal of Economics* 115 (2000), 811-846.

Question: What is "social capital"?

Design: First survey 258 Harvard undergraduates, subpopulation of 196 plays two trust games (Berg, Dickhaut and McCabe, 1995 *GEB*).

In the first of these trust games, individuals are randomly paired and meet their partners. They are then separated and one member (the sender) has the opportunity to send between 0 and 15 dollars to the other (the recipient). The experimenter matches each dollar that is sent to the recipient. After the recipient the transfer and the matching amount, s/he may send money back to the sender.

(With purely self-interested preferences, in subgame-perfect equilibrium the recipient will not send anything back, and therefore the sender will not send anything. Thus the game provides simple measures of subjects' altruism and reciprocity, and of their beliefs about others' altruism and reciprocity.)

In the second trust game, subjects report their willingnesses to pay for an envelope containing $10 that is addressed to them and dropped in several different public places (e.g. Harvard Square) under various conditions (e.g. sealed and stamped).

(Again, with self-interested preferences, the person who finds the envelope will not send it back, so the game provides simple measures of subjects' beliefs about how others' altruism and reciprocity are affected by expected demographics proxied by location.)

Results:

In the first game, the degree of social connection between sender and recipient (number of friends in common, membership in the same race or nationality, duration of acquaintanceship) predict the level of trust and trustworthiness well.

High status individuals elicit more trustworthiness in others.

Standard attitudinal survey questions about *trust* predict *trustworthy* behavior much better than they predict trusting behavior. (Survey questions about how much you trust others predict the extent to which you yourself can be trusted.)

Trusting behavior is predicted by past trusting behavior outside of the experiments.

Only children are much less trustworthy than subjects with siblings.

Thus, in this setting, individual variables meant to capture social capital really do produce individual financial returns, just as one would expect of any form of "capital". However, these variables do not enhance everyone's welfare, e.g. people who are playing against high status subjects end up with less earnings: some individual social capital produces negative externalities.

A clever field experiment, seeking to operationalize two key components of "social capital".

Unstructured bargaining (again with help from Al Roth)

Alvin Roth, "Bargaining Phenomena and Bargaining Theory," Chapter 2 (pp. 14-41) in Roth (ed.), *Laboratory Experimentation in Economics: Six Points of View*, Cambridge, 1987

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

Alvin Roth, "Toward a Focal-Point Theory of Bargaining," Chapter 12 in Roth, (ed.), *Game-Theoretic Models of Bargaining*, Cambridge, 1985

Question: Does the Nash (1950 *Econometrica*) bargaining solution predict the outcome of unstructured bargaining?

Design:

One key design problem is that the Nash solution depends on bargainers' vN-M utility functions, which are unobservable.

We could just assume risk-neutrality, or try to measuring utilities without interfering with bargaining process; but Roth found a clever way to *induce* risk-neutrality:

Subjects bargained over the division of 100 lottery tickets, with each subject's share determining his probability of winning the larger of two possible monetary prizes, specific to him.

Thus under standard assumptions, a subject who wants to raise the probability of winning the larger prize must maximize the expected number of lottery tickets, without regard to his risk preferences.

Another key design problem is that the actual "rules" of bargaining are complex and unobservable. Roth and collaborators found a tractable way to study unstructured bargaining:

Subjects bargained, for 10 or 12 minutes, exchanging messages via monitored email.

If subjects could agree on how to share the lottery tickets (by sending back the same proposal they just heard) by the deadline, the agreement was enforced; otherwise they got zero probabilities.

Subjects could make any binding proposal they wished, or accept their partner's latest proposal, at any time.

They could also send nonbinding messages at any time, except that they could not identify themselves or, in some treatments, reveal their prizes.

The environment was public knowledge, except subjects' prizes or information about prizes in some treatments.

In addition to controlling for subjects' unobservable risk preferences, the binary lottery procedure creates invariances that allow sharp tests of cooperative and noncooperative theories of bargaining.

Cooperative game theory summarizes the implications of a structure by the payoffs players can obtain acting alone or in coalitions, which payoffs can be taken to equal their expected numbers of lottery tickets.

This makes bargaining over a fixed total of lottery tickets, even when risk preferences are unobservable, equivalent to a *complete-information* Divide the Dollar game with risk-neutral players, whose symmetry leads cooperative theories to predict equal division of the lottery tickets.

These conclusions are independent of players' risk preferences, prizes, or information about prizes, so that cooperative theories can be tested by observing the effects of varying those factors.

Noncooperative theories are harder to test this way because their predictions may depend on the details of the structure, but the binary lottery procedure also makes it possible to create invariances that allow such tests.

Each treatment paired a subject whose prize was low (typically $5) with one whose prize was high (typically $20).

A subject always knew his own prize.

The first experiment compared two information conditions: "full," in which a subject also knew his partner's prize; and "partial," in which a subject knew only his own prize.

The second experiment created a richer set of information conditions using an intermediate commodity, chips, which subjects could later exchange for money in private.

A subject always knew his own chip prize and its value in money.

There were three information conditions: "high," in which a subject also knew his partner's chip prize and its value; "intermediate," in which a subject knew his partner's chip prize but not its value; and "low," in which a subject knew neither his partner's chip prize nor its value.

Subjects were prevented from communicating the missing information, and the information condition was public knowledge.

Partial and low information induce games with identical structures, given that players cannot send messages about chip or money prizes, because their strategy spaces are isomorphic (with chips in the latter treatment playing the role of money in the former) and isomorphic strategy combinations yield identical payoffs (in lottery tickets).

For the same reasons full and intermediate information also induce games with identical structures, given that players in the latter cannot send messages about money prizes.

Any structural theory, cooperative or noncooperative, predicts identical outcomes in these pairs of treatments.

A third experiment explored the strategic use of private information by giving subjects the option of communicating missing information about prizes.

There were no chips, and a subject always knew his own money prize.

There were four basic information conditions:

(i) neither subject knew both prizes;

(ii) only the subject whose prize was $20 knew both prizes;

(iii) only the subject whose prize was $5 knew both prizes; and

(iv) both subjects knew both prizes.

Some treatments made the basic information condition public knowledge, while in others subjects were told only that their partners might or might not know what information they had.

Thus there were eight information conditions in all.

Results:

I first describe the observed patterns of agreements, and then discuss disagreements.

With partial information almost all subjects agreed on a 50-50 division of the lottery tickets.

With full information, agreements averaged about halfway between 50-50 and equal expected money winnings, with much higher variance (Roth 1987b, Table 2.2). This violates the predictions of Nash's solution and other standard cooperative theories of bargaining.

With low and high information, respectively, agreements averaged close to 50-50 and roughly halfway between 50-50 and equal expected money winnings, again with higher variance.

With intermediate information, agreements averaged close to 50-50 (Roth 1987b, Figure 2.1).

Thus partial and low information yielded similar outcomes; but with full and intermediate information, strategically equivalent information about money and chips affected the outcomes in very different ways. This violates the predictions of standard noncooperative theories of bargaining.

Frequency of Agreements in Terms of the Percentage of Lottery
Tickets Obtained by the $20 Player

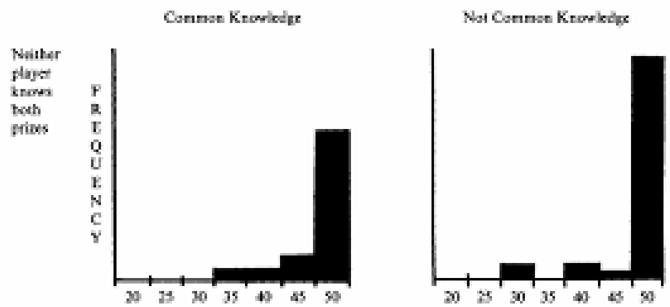


FIGURE 1.

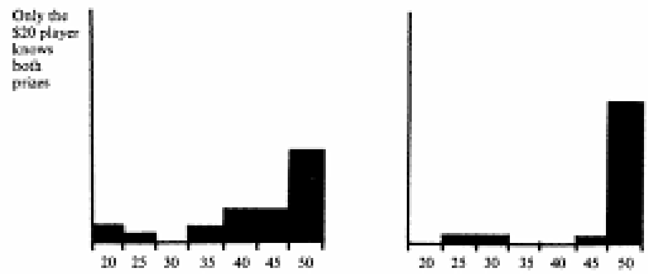


FIGURE 2.

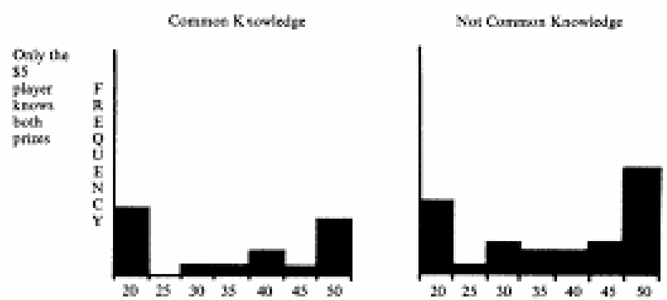


FIGURE 3.

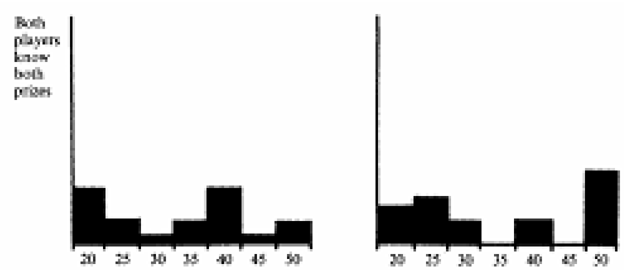


FIGURE 4.

MEAN OUTCOMES TO THE \$20 AND \$5 PLAYERS IN EACH INFORMATION/COMMON KNOWLEDGE CONDITION OVER ALL INTERACTIONS (DISAGREEMENTS INCLUDED AS ZERO OUTCOMES)

	Common Knowledge		Not Common Knowledge	
Information	\$20 Player	\$5 Player	\$20 Player	\$5 Player
Neither player knows both prizes	41.6_{ab}	43.3_{c}	43.5_{a}	48.2
Only the \$20 player knows both prizes	34.9_{bc}	45.1_{bc}	40.9_{a}	42.4
Only the \$5 player knows both prizes	27.2_{c}	53.6_{ab}	25.0_{b}	42.0
Both players know both prizes	27.2_{c}	56.4_{a}	25.5_{b}	48.8

NOTE: Within a column, means with common subscripts are *not* significantly different from one another using the Mann–Whitney U test ($\alpha = .01$); none were significantly different in the Not-Common-Knowledge conditions for the \$5 player.

The authors attributed the strong influence of subjects' prizes and information about prizes, which are irrelevant in traditional analyses, to the different meanings subjects assigned to chips and money outside the laboratory.

Their agreements can be summarized by postulating a commonly understood hierarchy of contextual equal-sharing norms in which subjects implemented the most "relevant" norm their public knowledge allowed, with money most relevant, then lottery tickets, and then chips.
In the third experiment agreements were largely determined by whether the $5 subject knew both prizes, clustering around 50-50 when he did not, and shifting more than halfway toward equal expected money winnings when he did (Roth 1987b, Table 2.4).

In effect these agreements were determined by the most relevant norm in the above hierarchy that subjects could implement, using their public knowledge plus whatever private information they had incentives to reveal, on the anticipation that it would be used this way.

Subjects' revelation decisions were approximately in equilibrium in beliefs in a restricted game, in which they could either reveal the truth or nothing at all, when their beliefs are estimated from the mean payoffs in related treatments (Roth 1987b, pp. 27-32).

There was a subtle interplay between the use of norms and the revelation of private information.

In the public-knowledge version of condition (ii) in the third experiment, for instance, the $5 subject knew that his partner knew which agreement gave them equal expected money winnings, but the $20 subject usually refused to reveal his prize.

This left the 50-50 division the only norm that could be implemented using public knowledge.

Although many $5 subjects voiced suspicions (in transcripts) that they were being treated unfairly, in the end most settled for the 50-50 division.

In all three experiments disagreements occurred, with frequencies ranging from 8-33%.

Disagreements were most common when both subjects knew enough to implement more than one norm, or when the information condition was not public knowledge.

Because the set of feasible divisions of lottery tickets and subjects' preferences over them were public knowledge, under standard assumptions, it is natural to assume complete information in modeling the bargaining game.

The nonnegligible frequency of disagreements is then incompatible with explanations based on Nash's bargaining solution or the subgame-perfect equilibrium of an alternating-offers model, as is the strong influence of context on the agreements subjects reached.

The manipulation of norms by withholding private information is inconsistent with nonstrategic explanations in which subjects "try to be fair."

However, most of the results can be understood using a simple strategic model, with players' shared ideas about fairness as coordinating principles.

The model summarizes the strategic possibilities of unstructured bargaining using Nash's (1953 *Econometrica*) demand game, in which players make simultaneous demands, in this case for lottery tickets. If their demands are feasible they yield a binding agreement; if not there is disagreement.

To see how this simple game can describe the dynamics of unstructured bargaining, assume that delay costs are negligible before the deadline, so that the timing of an agreement is irrelevant. (This is a good approximation for the experiments and many applications to bargaining in the field.)

Then, if equilibrium is assumed, all that matters about a player's strategy is the lowest share it can be induced to accept by the deadline. These lowest shares determine the outcome like players' demands in the demand game (Schelling 1960, pp. 267-290; Harsanyi and Selten 1988, pp. 23-24).

In the complete model, players first decide simultaneously how much private information to reveal.

They then bargain, with ultimate acceptance decisions described by the demand game, in which there is effectively complete information.

The demand game has a continuum of efficient equilibria, in which players' demands are just feasible and no worse than disagreement for both. (There is also a continuum of mixed-strategy equilibria, in which disagreement occurs with positive probability.) Thus, in this model bargaining is in essence a coordination problem, with players' beliefs the dominant influence on outcomes.

Players' beliefs are focused, if at all, by the most relevant norm their public knowledge (including any revealed private information) allows them to implement.

Pure-strategy equilibria, selected this way, yield agreements that closely resemble those observed in the various treatments.

From this point of view, it is the desire to avoid a risk of disagreement due to coordination failure that explains $5 subjects' willingness to settle on the "unfair" 50-50 division in condition (ii) of the third experiment, a phenomenon that is difficult to explain any other way.

Finally, mixed-strategy equilibria in which players' beliefs in each treatment are focused on the norms subjects' public knowledge allowed them to implement yield disagreement frequencies close to those observed in the various treatments (Roth 1985).

The results demonstrate the importance of coordination in unstructured bargaining and the strategic use of private information to manipulate bargainers' use of fairness notions that depend on dispersed information.

A beautiful example of the use of the use of design to create theoretical invariances that allow sharp tests of theories.

These experiments are of particular interest because leaving the bargaining process unstructured comes closer to bargaining in the field, where rules like those in noncooperative models of bargaining are seldom encountered. The use of monitored electronic communication to mimic "no rules" bargaining is now easy with NetMeeting software as in Moreno and Wooders (1998 *GEB*).

Equilibrium selection via adaptive learning

Vincent Crawford, "Learning Dynamics, Lock-in, and Equilibrium Selection in Experimental Coordination Games," in Ugo Pagano and Antonio Nicita, editors, *The Evolution of Economic Diversity*, London and New York: Routledge, 2001, 133-163; UCSD Discussion Paper 97-19; at http://dss.ucsd.edu/~vcrawfor/ucsd9719.pdf

Vincent Crawford, "Adaptive Dynamics in Coordination Games," *Econometrica* 63 (1995), 103-143; http://www.jstor.org/stable/2951699 or http://dss.ucsd.edu/~vcrawfor/Crawford95EMT.pdf)

Vincent Crawford and Bruno Broseta, "What Price Coordination? The Efficiency-enhancing Effect of Auctioning the Right to Play," *American Economic Review* 88 (March 1998), 198-225; http://www.jstor.org/stable/116825 or http://dss.ucsd.edu/~vcrawfor/CrawBro98AER.pdf

Question: What determines equilibrium selection via learning in a class of coordination games whose structures are common in applications?

Van Huyck, Battalio, and Beil's (1990 *AER*, 1991 *QJE*) designs

Repeated play of player-role-symmetric coordination games in populations of subjects, interacting all at once ("large groups") or in pairs drawn randomly ("random pairing").

Subjects chose simultaneously among 7 efforts, with payoffs and ex post optimal choices determined by own efforts and an order statistic, the population median or minimum effort in large groups or the current pair's minimum with random pairing.

There were five leading treatments, varying the order statistic (minimum in 1990, median in 1991), the size of the subject population, and the patterns in which they interact (minimum games were played either by the entire population of 14-16 or by random pairs, median games were played by the entire population of 9).

Explicit communication was prohibited throughout, the order statistic was publicly announced after each play (with random pairs told only pair minima), and the structure was publicly announced at the start, so subjects were uncertain only about others' efforts.

The subject populations were large enough that subjects treated own influences on order statistic as negligible (the smallest "large" number in behavioral game theory is around four or five).

PAYOFF TABLE Γ

		Median value of X chosen						
		7	6	5	4	3	2	1
Your choice of X	7	1.30	1.15	0.90	0.55	0.10	−0.45	−1.10
	6	1.25	1.20	1.05	0.80	0.45	0.00	−0.55
	5	1.10	1.15	1.10	0.95	0.70	0.35	−0.10
	4	0.85	1.00	1.05	1.00	0.85	0.60	0.25
	3	0.50	0.75	0.90	0.95	0.90	0.75	0.50
	2	0.05	0.40	0.65	0.80	0.85	0.80	0.65
	1	−0.50	−0.05	0.30	0.55	0.70	0.75	0.70

PAYOFF TABLE A

		Smallest Value of X Chosen						
		7	6	5	4	3	2	1
Your Choice of X	7	1.30	1.10	0.90	0.70	0.50	0.30	0.10
	6	–	1.20	1.00	0.80	0.60	0.40	0.20
	5	–	–	1.10	0.90	0.70	0.50	0.30
	4	–	–	–	1.00	0.80	0.60	0.40
	3	–	–	–	–	0.90	0.70	0.50
	2	–	–	–	–	–	0.80	0.60
	1	–	–	–	–	–	–	0.70

VHBB’s Leading Median and Minimum Payoff Tables

The random-pairing and large-group minimum games are larger versions of two-effort Stag Hunts.

Other Player

	Stag	**Rabbit**
Stag	2, 2	0, 1
Rabbit	1, 0	1, 1

Two-Person Stag Hunt

All Other Players

	All-Stag	**Not All-Stag**
Stag	2	0
Rabbit	1	1

***n*-Person Stag Hunt**

The stage games all have seven strict, symmetric, Pareto-ranked equilibria, with players' best responses an order statistic of the population effort distribution.

The games are like a meeting that can't start until a given quorum is achieved—100% in the large-group minimum game, 50% in the large-group median games.

Intuitively, efficient coordination is more difficult, the larger the quorum or the larger the group, other things equal; but traditional equilibrium analysis and refinements don't fully reflect this.

Van Huyck, Battalio, and Beil's (1990 *AER*, 1991 *QJE*) results:

The five leading treatments all evoked similar initial responses (table from Crawford (1991 *GEB*)).

TABLE I

		Minimum treatment				
		A (%)	B (%)	A′ (%)	C_d (%)	C_f (%)
Subject's	7	33 (31)	76 (84)	23 (25)	11 (37)	13 (42)
initial	6	10 (9)	1 (1)	1 (1)	1 (3)	0 (0)
effort	5	34 (32)	2 (2)	2 (2)	2 (7)	6 (19)
	4	18 (17)	5 (5)	7 (8)	5 (17)	2 (6)
	3	5 (5)	1 (1)	7 (8)	3 (10)	1 (3)
	2	5 (5)	1 (1)	17 (19)	1 (3)	1 (3)
	1	2 (2)	5 (5)	34 (37)	7 (23)	8 (26)
Totals		107 (101)	91 (99)	91 (100)	30 (100)	31 (99)

		Median treatment		
		Γ, Γdm (%)	Ω (%)	Φ (%)
Subject's	7	8 (15)	14 (52)	2 (7)
initial	6	4 (7)	1 (4)	3 (11)
effort	5	15 (28)	9 (33)	9 (33)
	4	19 (35)	3 (11)	11 (41)
	3	8 (15)	0 (0)	2 (7)
	2	0 (0)	0 (0)	0 (0)
	1	0 (0)	0 (0)	0 (0)
Totals		54 (100)	27 (100)	27 (99)

Inexperienced subjects' initial strategic thinking doesn't react strongly to order statistic or group size.

Thus the strong treatment effects in subsequent outcomes are due to the dynamics of learning.

Subjects almost always converged to some equilibrium.

But the dynamics varied with the treatment variables (order statistic, group size, interaction pattern), with large differences in drift, history-dependence, rate of convergence, and equilibrium selection:

- In 12 out of 12 large-group median trials, there was near-perfect “lock-in” on the initial median (even though it varied across runs and was usually inefficient)

- In 9 out of 9 large-group minimum trials, there was very strong downward drift, with subjects always approaching the least efficient equilibrium

- In 2 out of 2 random-pairing minimum trials, there was very slow convergence, no discernible drift, and moderate inefficiency

Comparing the first two reveals an “order statistic” or “robustness” effect, with coordination less efficient the smaller the groups that can disrupt desirable outcomes.

Comparing the last two reveals a “group size” effect, in which coordination is less efficient in larger groups (holding the order statistic constant, measured from the “bottom”).

TABLE III

MEDIAN CHOICE FOR THE FIRST TEN PERIODS OF ALL EXPERIMENTS

Treatment	Period 1	2	3	4	5	6	7	8	9	10
Gamma										
Exp. 1	4	4	4	4	4	4	4*	4	4*	4*
Exp. 2	5	5	5	5	5	5	5	5	5	5
Exp. 3	5	5	5	5	5	5	5	5	5	5*
Gammadm										
Exp. 4	4	4	4	4	4	4*	4*	4*	4*	4*
Exp. 5	4	4	4	4*	4*	4*	4*	4*	4*	4*
Exp. 6	5	5	5	5	5	5	5	5*	5*	5*
Omega										
Exp. 7	7	7	7	7*	7*	7*	7*	7*	7*	7*
Exp. 8	5	5	5	5	5*	5*	5*	5*	5*	5*
Exp. 9	7	7	7*	7*	7*	7*	7*	7*	7*	7*
Phi										
Exp. 10	4	4	4	4	4*	4*	4*	4*	4*	4*
Exp. 11	5	5	5	5*	5*	5*	5*	5*	5*	5*
Exp. 12	5	5	5	5*	5*	5*	5*	5*	5*	5*

Notes. Exp. = experiment. * = indicates a mutual best response outcome.

Table 2—Experimental Results for Treatment *A*

	Period									
	1	2	3	4	5	6	7	8	9	10
Experiment 1										
No. of 7's	8	1	1	0	0	0	0	0	0	1
No. of 6's	3	2	1	0	0	0	0	0	0	0
No. of 5's	2	3	2	1	0	0	1	0	0	0
No. of 4's	1	6	5	4	1	1	1	0	0	0
No. of 3's	1	2	5	5	4	1	1	1	0	1
No. of 2's	1	2	2	4	8	7	8	6	4	1
No. of 1's	0	0	0	2	3	7	5	9	12	13
Minimum	2	2	2	1	1	1	1	1	1	1
Experiment 2										
No. of 7's	4	0	1	0	0	0	0	0	0	1
No. of 6's	1	0	1	0	0	1	0	0	0	0
No. of 5's	3	3	2	1	0	0	1	1	0	1
No. of 4's	4	6	2	3	3	0	0	0	0	0
No. of 3's	1	4	2	5	0	1	1	0	1	0
No. of 2's	3	2	6	5	5	9	3	4	3	1
No. of 1's	0	1	2	2	8	5	11	11	12	13
Minimum	2	1	1	1	1	1	1	1	1	1
Experiment 3										
No. of 7's	4	4	1	0	1	1	1	0	0	2
No. of 6's	2	0	2	0	0	0	0	0	0	0
No. of 5's	5	6	1	1	1	0	0	0	0	0
No. of 4's	3	3	2	1	2	1	0	0	0	1
No. of 3's	0	0	7	6	0	2	3	0	0	0
No. of 2's	0	1	1	4	5	3	6	3	2	2
No. of 1's	0	0	0	2	5	7	4	11	12	9
Minimum	4	2	2	1	1	1	1	1	1	1
Experiment 4										
No. of 7's	6	0	1	1	0	0	1	0	0	0
No. of 6's	0	6	2	0	0	1	0	0	0	0
No. of 5's	8	5	5	5	0	1	0	0	0	0
No. of 4's	1	1	4	6	7	1	2	1	1	0
No. of 3's	0	2	3	2	4	3	2	2	1	0
No. of 2's	0	1	0	0	2	3	7	4	2	2
No. of 1's	0	0	0	1	2	6	3	8	11	13
Minimum	4	2	3	1	1	1	1	1	1	1

TABLE 2—EXPERIMENTAL RESULTS FOR TREATMENT A, Continued

	Period									
	1	2	3	4	5	6	7	8	9	10
Experiment 5										
No. of 7's	2	2	3	1	1	1	1	0	0	0
No. of 6's	1	3	1	0	0	0	0	0	0	0
No. of 5's	9	3	0	4	1	0	2	0	0	0
No. of 4's	3	4	6	2	1	2	0	2	1	1
No. of 3's	1	2	2	4	6	0	0	0	0	1
No. of 2's	0	2	2	3	4	6	5	2	5	3
No. of 1's	0	0	2	2	3	7	8	12	10	11
Minimum	3	2	1	1	1	1	1	1	1	1
Experiment 6										
No. of 7's	5	3	1	1	1	1	2	2	2	3
No. of 6's	2	0	0	0	1	0	0	0	0	0
No. of 5's	5	1	0	0	0	1	0	0	0	0
No. of 4's	2	3	4	0	0	0	0	0	0	0
No. of 3's	1	5	4	2	2	2	1	0	2	0
No. of 2's	0	2	4	5	3	3	6	4	5	5
No. of 1's	1	2	3	8	9	9	7	10	7	8
Minimum	1	1	1	1	1	1	1	1	1	1
Experiment 7										
No. of 7's	4	3	1	1	1	1	1	1	1	1
No. of 6's	1	0	0	0	0	0	0	0	0	0
No. of 5's	2	3	0	0	0	0	0	0	0	0
No. of 4's	4	0	1	2	1	0	0	0	0	0
No. of 3's	1	3	2	1	1	0	0	0	0	0
No. of 2's	1	3	2	2	4	4	4	4	5	3
No. of 1's	1	2	8	8	7	9	9	9	8	10
Minimum	1	1	1	1	1	1	1	1	1	1

TABLE 5—DISTRIBUTION OF ACTIONS FOR TREATMENT *C*: RANDOM PAIRINGS

	Period				
	21	22	23	24	25
Experiment 6					
No. of 7's	5	5	4	10	8
No. of 6's	0	1	3	0	0
No. of 5's	2	5	3	3	4
No. of 4's	3	1	1	1	1
No. of 3's	1	1	1	0	0
No. of 2's	1	1	2	2	2
No. of 1's	4	2	2	0	1
Experiment 7					
No. of 7's	–	–	6	5	5
No. of 6's	–	–	1	0	1
No. of 5's	–	–	0	3	0
No. of 4's	–	–	2	1	4
No. of 3's	–	–	2	0	0
No. of 2's	–	–	0	0	1
No. of 1's	–	–	3	5	3

Van Huyck, Battalio, and Beil's (1993 *GEB*) design

VHBB's (1993 *GEB*) design was the same as their 1991 design, with repeated play of one of the 1991 median games, but with the right to play auctioned each period to the highest 9 bidders in a population of 18 (an English clock auction, with the same price paid by all winning bidders).

The market-clearing price was publicly announced after each period's auction, the median was publicly announced after each period's play, and the structure was publicly announced at the start.

Van Huyck, Battalio, and Beil's (1993 *GEB*) results

The stage game has a range of symmetric equilibria, in which all bid the payoff of some equilibrium of the median game and play that equilibrium, unless others bid differently.

In 8 of 8 trials, subjects quickly bid the price to a level that could only be recouped in the most efficient equilibrium and then converged to that equilibrium: strong, precise selection among a wide range of equilibria.

Auctioning the right to play had a strong efficiency-enhancing effect via focusing subjects' beliefs on more efficient ways to coordinate—a new and potentially important mechanism by which competition promotes efficiency.

TABLE V

DISTRIBUTION OF ACTIONS FOR GAME Γ(9): EC AUCTION

	Period														
	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Exp. 10															
Price	1.24	1.24	1.28	1.29	1.30	1.30	1.30	1.30	1.30	1.30	—	—	—	—	—
Undom. actions	≥6	≥6	7	7	7	7	7	7	7	7	—	—	—	—	—
# of 7s	7	8	9	8	9	9	9	9	9	9	—	—	—	—	—
# of 6s	2	1	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 5s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 4s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 3s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 2s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 1s	0	0	0	1	0	0	0	0	0	0	—	—	—	—	—
Median	7	7	7*	7	7*	7*	7*	7*	7*	7*	—	—	—	—	—
Exp. 11															
Price	1.00	1.20	1.29	1.30	1.29	1.30	1.29	1.29	1.30	1.30	—	—	—	—	—
Undom. actions	≥4	≥6	7	7	7	7	7	7	7	7	—	—	—	—	—
# of 7s	4	5	9	9	9	9	9	9	9	9	—	—	—	—	—
# of 6s	1	3	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 5s	2	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 4s	2	1	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 3s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 2s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
# of 1s	0	0	0	0	0	0	0	0	0	0	—	—	—	—	—
Median	6	7	7*	7*	7*	7*	7*	7*	7*	7*	—	—	—	—	—
Exp. 12															
Price	.95	1.04	1.08	1.10	1.15	1.20	1.25	1.25	1.30	1.30	1.30	1.30	1.30	1.30	1.30
Undom. actions	≥3	≥4	≥5	≥5	≥5	≥6	≥6	≥6	7	7	7	7	7	7	7
# of 7s	1	0	1	3	2	5	8	9	9	9	9	9	9	9	9
# of 6s	0	3	5	6	7	4	0	0	0	0	0	0	0	0	0
# of 5s	6	2	2	0	0	0	0	0	0	0	0	0	0	0	0
# of 4s	1	4	1	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	1	0	0	0	0	0	1	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	5	5	6	6	6	7	7	7	7*	7*	7*	7*	7*	7*	7*
Exp. 13															
Price	1.05	1.14	1.18	1.25	1.29	1.25	1.25	1.30	1.25	1.30	1.30	1.30	1.30	1.30	1.30
Undom. actions	≥4	≥5	≥6	≥6	7	≥6	≥6	7	≥6	7	7	7	7	7	7
# of 7s	2	2	4	6	9	9	9	9	9	9	9	9	9	9	9
# of 6s	1	6	5	3	0	0	0	0	0	0	0	0	0	0	0
# of 5s	3	1	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 4s	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	5	6	6	7	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*
Exp. 14															
Price	1.05	1.15	1.27	1.25	1.25	1.30	1.30	1.25	1.30	1.30	1.25	1.30	1.30	1.30	1.30
Undom. actions	≥4	≥5	7	≥6	≥6	7	7	≥6	7	7	7	7	7	7	7
# of 7s	0	5	8	8	9	9	9	9	9	9	9	9	9	9	9
# of 6s	7	4	0	1	0	0	0	0	0	0	0	0	0	0	0
# of 5s	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0
# of 4s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	6	7	7	7	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*

Notes. * indicates mutual best response outcome. ___ Partitions actions into $FI(P)$ and the complement of $FI(P)$.

TABLE VI

Distribution of Actions for Game Γ(9): EC Auction and Experienced Subjects

	Period														
	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Exp. 7 (M = 5)															
Price	1.09	1.09	1.10	1.19	1.29	1.29	1.30	1.29	1.30	1.30	1.30	1.29	1.30	1.25	1.29
Undom. actions	≥5	≥5	≥5	≥6	7	7	7	7	7	7	7	7	7	≥6	7
# of 7s	0	0	2	5	9	9	9	9	9	9	9	9	9	5	9
# of 6s	2	1	5	4	0	0	0	0	0	0	0	0	0	[illegible]	0
# of 5s	6	8	2	0	0	0	0	0	0	0	0	0	0	[illegible]	0
# of 4s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	5	5	6	7	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*
Exp. 8 (M = 5)															
Price	1.09	1.25	1.28	1.29	1.30	1.29	1.30	1.30	1.29	1.30	1.29	1.30	1.29	1.30	1.30
Undom. actions	≥5	≥6	7	7	7	7	7	7	7	7	7	7	7	7	7
# of 7s	3	7	9	9	9	9	9	9	9	9	9	9	9	9	9
# of 6s	2	2	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 5s	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 4s	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	6	7	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*
Exp. 9 (M = 6)															
Price	1.15	1.21	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29	1.29
Undom. actions	≥5	≥6	7	7	7	7	7	7	7	7	7	7	7	7	7
# of 7s	0	7	9	9	9	9	9	9	9	9	9	9	9	9	9
# of 6s	8	1	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 5s	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 4s	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 3s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 2s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
# of 1s	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Median	6	7	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*	7*

Notes. * indicates mutual best response outcome. ___ Partitions actions into $FI(P)$ and the complement of $FI(P)$.

Explaining Van Huyck et al.'s (1990, 1991) results

Vincent Crawford, "Adaptive Dynamics in Coordination Games," *Econometrica* 63 (1995), 103-143; http://www.jstor.org/stable/2951699 or http://dss.ucsd.edu/~vcrawfor/Crawford95EMT.pdf)

Vincent Crawford and Bruno Broseta, "What Price Coordination? The Efficiency-enhancing Effect of Auctioning the Right to Play," *American Economic Review* 88 (March 1998), 198-225; http://www.jstor.org/stable/116825 or http://dss.ucsd.edu/~vcrawfor/CrawBro98AER.pdf

see also

Vincent Crawford, "Learning Dynamics, Lock-in, and Equilibrium Selection in Experimental Coordination Games," in Ugo Pagano and Antonio Nicita, editors, *The Evolution of Economic Diversity*, London and New York: Routledge, 2001, 133-163; UCSD Discussion Paper 97-19; at http://dss.ucsd.edu/~vcrawfor/ucsd9719.pdf

show that Van Huyck et al.'s results can (only) be explained by an adaptive learning model in which players' beliefs and decisions are heterogeneous.

Unless the heterogeneity of beliefs is eliminated very slowly, the learning dynamics converge, with probability 1, to one of the symmetric equilibria of the coordination game.

The model's implications for equilibrium selection can be summarized by the prior probability distribution of the limiting equilibrium, which is normally nondegenerate due to the persistent effects of strategic uncertainty.

The limiting outcome is determined by the cumulative drift before learning eliminates strategic uncertainty (faculty meeting example with varying quorum and group size).

Overall, the analysis yields the following conclusions:

- Perfect history-dependence in 1991 median treatments is due to no drift and small variance; but convergence to initial median in 12 of 12 trials may overstate history-dependence: initial median "explains" 46-81% of variance of final median.

- Lack of history-dependence in large-group minimum treatment is due to strong downward drift, which yields convergence to lower bound with very high probability; but convergence in 9 of 9 trials may understate the difficulty of coordination: in simulations it occurred in 500 of 500 trials.

- Slow convergence, weak history-dependence, and lack of trend in the random-pairing minimum treatment are due to no drift and subjects' observation of only their current pair's minimum, which is a very noisy estimate of the population median that determined their best responses.

The analysis yields qualitative comparative dynamics conclusions about the direct effects of changes in treatment variables, holding the behavioral parameters constant:

- Coordination is less efficient the lower the order statistic (the smaller the subsets of the population that can adversely affect the outcome), because small numbers of deviations are more likely than large numbers.

- Coordination is less efficient in larger groups (holding the order statistic constant, measured from the bottom) because it requires coherence among more independent decisions.

Explaining Van Huyck et al.'s (1993) results

Crawford and Broseta (1998 *AER*) show that the results can be understood as following from effects that formalize "order statistic," "optimistic subjects," and "forward induction" intuitions.

The optimistic subjects and order statistic effects together have approximately the same magnitude in VHBB's environment (where the right to play a nine-person median game was auctioned in a group of 18) as the order statistic effect in an 18-person coordination game without auctions in which payoffs and best responses are determined by the fifth highest (the median of the nine highest) of all 18 players' efforts.

Auctioning the right to play a 9-person median game in a group of 18 effectively turns the game into a "75^{th} percentile" game ($0.75 = 13.5/18$), whose order statistic effect contributes large upward drift as Crawford's (1995) analysis suggests there would have been without auctions.

Crawford and Broseta's analysis attributes the other half of the efficiency-enhancing effect of auctions in VHBB's environment to a strong forward induction effect.

The analysis shows that coordination is more efficient with more intense competition for the right to play, because it yields higher prices for a given level of dispersion in bidding strategies, and it increases the optimistic subjects effect.

This effect should extend to related environments, but may not always yield full efficiency.

Strategic communication of private information

Joseph Wang, Michael Spezio, and Colin Camerer, "Pinocchio's Pupil: Using Eyetracking and Pupil Dilation To Understand Truth-telling and Deception in Sender-Receiver Games," *American Economic Review* 101 (2011), in press; http://homepage.ntu.edu.tw/~josephw/pinocchio_final.pdf

Question: How do people behave in initial responses to simple sender-receiver games?

Design:

Sender observes state S = 1, 2, 3, 4, or 5, sends message M = 1, 2, 3, 4, or 5. Receiver observes message, chooses action A = 1, 2, 3, 4, or 5.

The Receiver's choice of A determines the welfare of both:

- The Receiver's ideal outcome is A = S.
- The Sender's ideal outcome is A = S + b.

The Receiver's von Neumann-Morgenstern utility function is $110 - 20|S - A|^{1.4}$, and the Sender's is $110 - 20|S + b - A|^{1.4}$.

The difference in preferences varied across treatments: b = 0, 1, or 2.

Eyetracking is used to monitor subjects' information search along with their decisions, as a way to get a handle on their cognitive processes.

Crawford and Sobel's (1982 *Econometrica*) theoretical analysis characterized the possible equilibrium relationships between Sender's observed S and Receiver's choice of A, which determines the informativeness of communication.

They showed, for a class of models with continuous state and action spaces that generalizes Wang et al.'s examples (except for discreteness), that all equilibria are "partition equilibria", in which as illustrated below, the Sender partitions the set of states into contiguous groups and tells the Receiver, in effect, only which group his observation lies in.

For any given difference in Sender's and Receiver's preferences (b), there is a range of equilibria, from a "babbling" equilibrium with one partition element to more informative equilibria that exist when b is small enough.

Under reasonable assumptions there is a "most informative" equilibrium, which has the most partition elements and gives the Receiver the highest ex ante (before the Sender observes the state) expected payoff.

As the preference difference decreases, the amount of information transmitted in the most informative equilibrium increases (measured either by the correlation between S and A or the Receiver's expected payoff).

The unambiguous part of Crawford and Sobel's characterization of equilibrium concerns the possible relationships between S and A.

Because messages have no direct effect on payoffs ("cheap talk"), there is nothing to tie down their meanings in equilibrium.

As a result, any equilibrium relationship between S and A can be supported by any sufficiently rich language, with the meanings of messages determined by players' equilibrium beliefs.

(By contrast, in Tom Stoppard's play "Dogg's Hamlet", the actors speak a language called "Dogg", which consists of ordinary English words but with meanings completely different from their normal meanings. This creates a lot of amusing confusion when they interact with true English speakers—confusion that would not arise if Dogg did not sound so much like English.)

Results:

Behaviorally, however, in experiments like Wang et al.'s with a clear correspondence between state and message—S = 1, 2, 3, 4, or 5 and M = 1, 2, 3, 4, or 5—or where communication is in a common natural language, the interpretations of messages are dictated by their literal meanings.

Thus messages are always understood—even if not always believed.

Wang et al.'s data analysis therefore fixes the meanings of Sender subjects' messages at their literal values.

Even with this restriction, when b = 0 or 1 in their design (Sender's and Receiver's preferences are close enough) there are multiple equilibria.

Wang et al.'s analysis then focuses on the "most informative" equilibrium.

When b = 0, the most informative equilibrium has M = S and A = S: perfect truth-telling, credulity, and information transmission, as is intuitively plausible when Sender and Receiver have identical preferences.

When b = 2, the most informative equilibrium has Senders sending a completely uninformative message M = {1, 2, 3, 4, 5} for any value of S; and Receivers ignoring it, hence choosing A = 3, which is optimal given their prior beliefs, for any value of M.

(A babbling equilibrium also exists when b = 0 or 1, but then it is not the most informative equilibrium.)

When b = 1, the most informative equilibrium has Senders sending M = 1 when S = 1 but M = {2, 3, 4, 5} when S = 2, 3, 4, or 5; and Receivers choosing A = 1 when M = 1 and A = 3 or 4 when M = {2, 3, 4, 5}.

(The Sender’s message M = {2, 3, 4, 5} is the simplest way to implement the intentional vagueness of this partition equilibrium. Another way would be for the Sender to randomize M uniformly on {2, 3, 4, 5} when S = 1.)

Thus, when b = 1 the difference in preferences causes noisy information transmission even in the most informative equilibrium.

Importantly, however, the Receiver’s beliefs on hearing the Sender’s message M are necessarily an unbiased—though noisy—estimate of S:

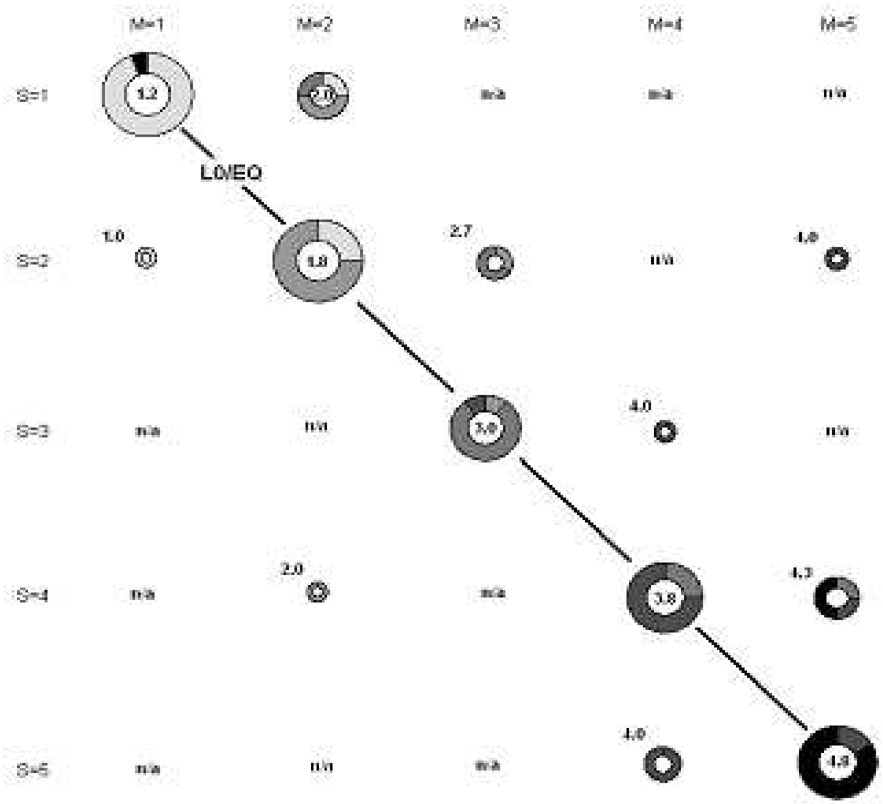
In equilibrium there is no lying or deception, only intentional vagueness.

(When b = 1, there’s another, more informative equilibrium, found by David Eil of UCSD, in which Senders send M = {1, 2} when S = 1 or 2 but M = {3, 4, 5} when S = 3, 4, or 5; and Receivers choose A = 2 when M = {1, 2} and A = 4 when M = {3, 4, 5}. But this equilibrium is not “robust”, in that Senders who observe S = 2 are indifferent between M = {1, 2} and M = {3, 4, 5}.)

When b = 0 Senders almost always set M = S and Receivers almost always set A = M: The result is near the perfect information transmission predicted by the most informative equilibrium.

Figure 1 shows the Sender’s message frequencies and the Receiver’s action frequencies as functions of the observed state S: A circle’s size shows the Sender’s message frequencies. A circle’s darkness and the poorly visible numbers inside show the Receiver’s action frequencies.

Figure 1: Raw Data Pie Charts (b=0)
(Hidden Bias-Stranger)



As b increases to b = 1 or b = 2, the amount of information transmitted decreases as predicted by Crawford and Sobel's equilibrium comparative statics, but there are also systematic deviations from the most informative (or any) equilibrium, and lying and successful deception occur.

In Figure 3 (next slide; b = 2 omitted from Wang et al.'s label by accident), in the essentially unique, most informative equilibrium M = {1, 2, 3, 4, 5}, so equilibrium message distributions would look the same for all five rows; and equilibrium actions would be concentrated on A = 3.

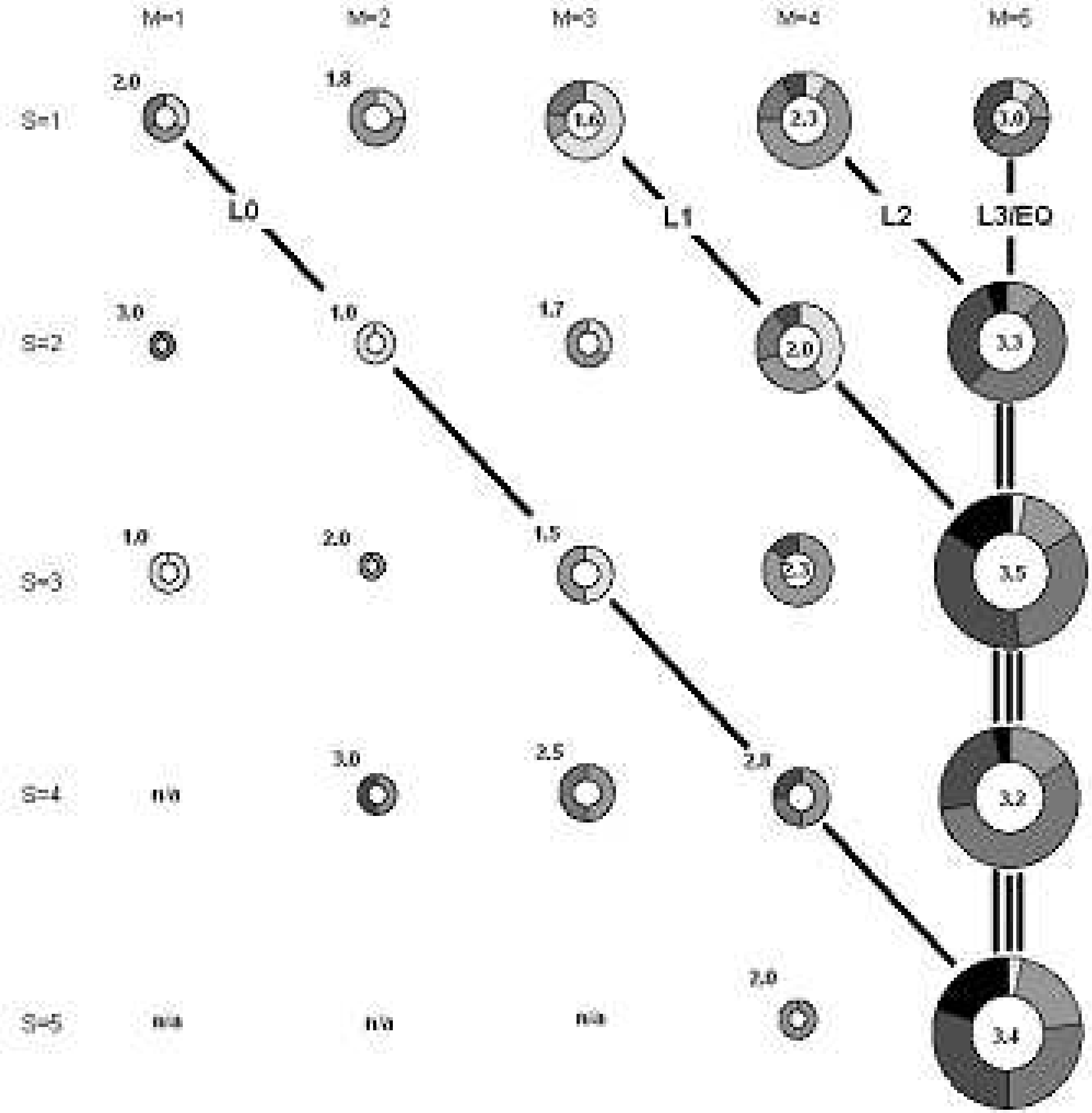
However, although the observed actions are fairly close to A = 3, message distributions shift rightward as S increases (going down in the table); thus:

● Most Senders exaggerate the truth (most messages above the diagonal), apparently trying to move Receivers from Receivers' ideal action A = S toward Senders' ideal action A = S + 2 (or 5, whichever is smaller).

● Even so, there is some information in Senders' messages (message distributions shift rightward going down in the table, so messages are positively correlated with the state).

● Receivers are usually deceived to some extent (average A usually > S).

Figure 3: Raw Data Pie Chart, (Hidden Bias-Stranger)



When b = 1, in the most informative robust equilibrium, the Sender's message is M = 1 when S = 1 and M = {2, 3, 4, 5} when S = 2, 3, 4, or 5; and the Receiver chooses A = 1 when M = 1 and A = 3 or 4 when M = {2, 3, 4, 5}. Thus, in equilibrium the distributions of messages and actions would be the same for S = 2, 3, 4, or 5.

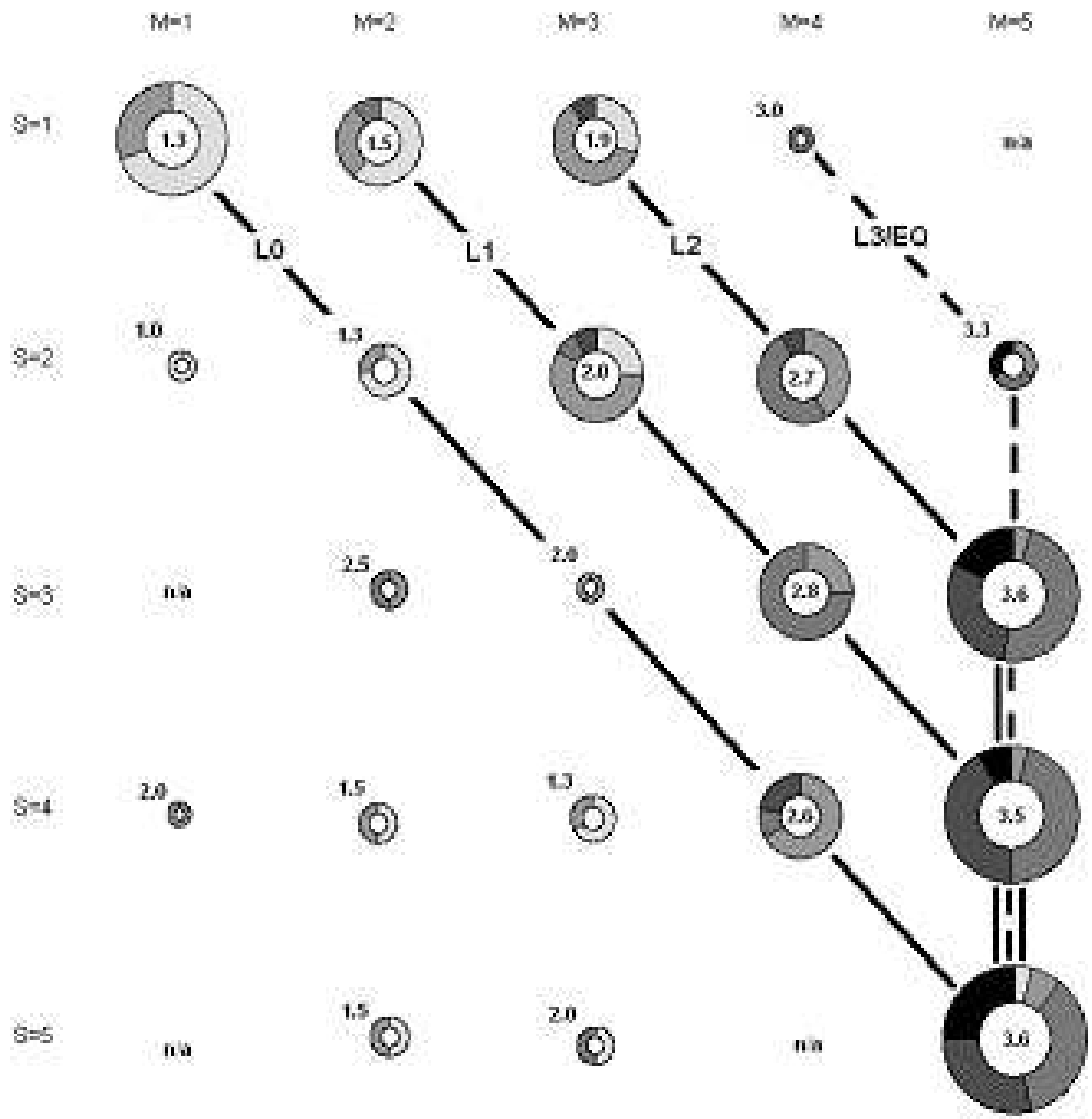
By contrast, turning to Figure 2 (b = 1; next slide):

- Senders almost always exaggerate the truth (messages above the diagonal), apparently trying to move Receivers from Receivers' ideal action A = S toward Senders' ideal action A = S + 1.

- Even so, there is some information in Senders' messages (message distributions shift rightward going down in the table, so messages are positively correlated with the state).

- Receivers are usually deceived to some extent (average A usually > S).

Figure 2: Raw Data Pie Chart (b=1)
(Hidden Bias-Stranger)



What kind of model can explain results like this? Wang et al., following Cai and Wang (2006 *GEB*), propose a level-*k* explanation based on Crawford's (2003 *AER*) analysis of preplay communication of intentions (see also Kartik et al. (2007 *JET*)):

Anchor beliefs in a truthful Sender *L0*, which sets M = S; and a credulous Receiver *L0* (which also best responds to an *L0* Sender), setting A = M.

L1 Senders best respond to *L0* Receivers by inflating their messages by b: M = S + b (up to M = 5), so that *L0* Receivers will choose S + b, yielding the Sender's ideal action given S.

L1 Receivers (as defined by Wang et al.; the numbering is a convention) best respond to *L1* Senders by discounting the message, normally setting A = M – b, yielding Receivers' ideal action given M = S + b of S.

The qualification "normally" reflects Wang et al.'s assumption that *L1* Receivers take into account that when b = 2, *L1* senders with S = 3, 4, or 5 all send M = 5, with the result that *L1* Receivers, knowing that S is equally likely to be 3, 4, or 5, choose A = 4 instead of A = M – 2b = 3.

L2 Senders best respond to *L1* Receivers by inflating their messages by 2b: M = S + 2b (up to M = 5), so that *L1* Receivers will set A = M – b = S + b, yielding Senders' ideal action given S.

L2 Receivers best respond to *L2* Senders by discounting the message, normally setting A = M – 2b, yielding Receivers' ideal action given M = S + 2b of S.

The qualification "normally" reflects Wang et al.'s assumption that *L2* Receivers take into account that when b = 1, *L2* senders with S = 3, 4, or 5 all send M = 5, with the result that *L2* Receivers, knowing that S is equally likely to be 3, 4, or 5, choose A = 4 instead of A = M – 2b = 3.

L2 Receivers also take into account that when b = 2, *L2* senders with S = 2, 3, 4, or 5 send M = 5, with the result that *L2* Receivers, knowing that S is equally likely to be 2, 3, 4, or 5, choose A = 4 instead of A = M – 2b = 3.

Figure 2: Raw Data Pie Chart (b=1)
(Hidden Bias-Stranger)

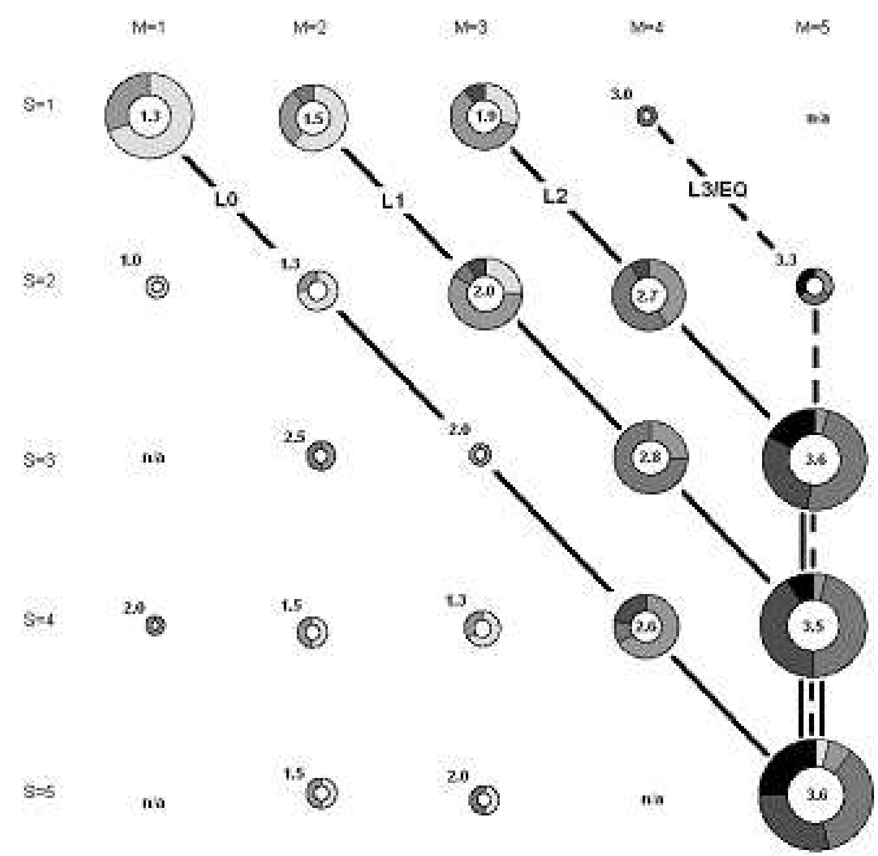
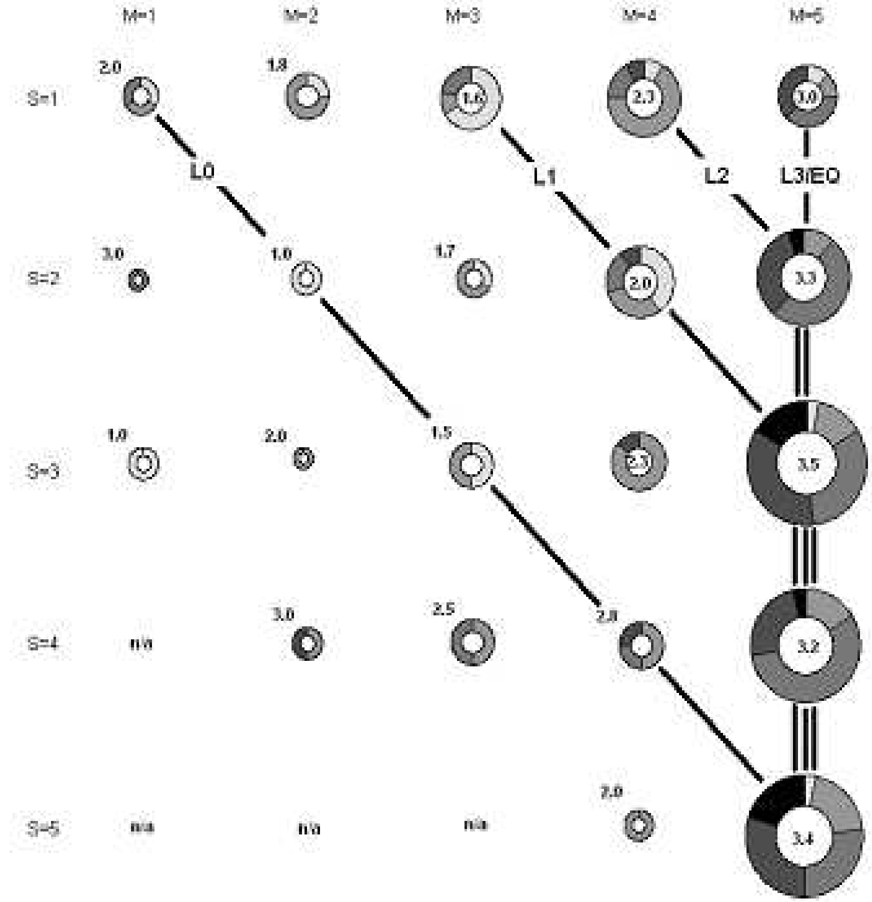


Figure 3: Raw Data Pie Chart, (Hidden Bias-Stranger)



Note that when b = 1, *L1*, *L2*, and *Eq* all predict M = 5 when S = 4 or 5; and when b = 2, *L1*, *L2*, and *Eq* all predict M = 5 when S = 3, 4, or 5.

Econometric estimation classifies 18% of 16 Sender subjects as *L0*, 25% as *L1*, 25% as *L2*, 14% as *Sophisticated*, and 18% as *Equilibrium* (not implausible, but note different type definitions).