Appendix

Power Planning for Rutström and Wilcox paper:
"Stated beliefs versus inferred beliefs"

As we examine learning in simple two player games where each player has two strategies to pick, we limit our exposition of the RL and CFP models to this case. Our version of the RL model is the three-parameter model examined by Erev and Roth (1998). Suppose two players n and m $\in$ {1,2} both have two pure strategies j $\in$ {1,2} to choose from in some normal form game. Let $q_{nj}(t)$ be player n's propensity to play her strategy j at the beginning of period t of this repeated 2X2 normal form game. This version of the RL model supposes that $P_{nj}(t)$ (the probability that player n chooses strategy j in period t) is a simple function of these propensities:

(A1) $P_{nj}(t) = q_{nj}(t)/[q_{n1}(t) + q_{n2}(t)]$.

Let $\pi_n[i,k]$ be the payoff be the payoff received by player n when she chooses strategy i in period t and her partner player m chooses strategy k in period t. To simplify our exposition, we assume that the game in question has a minimum payoff of zero for both players, as is true in our experiment. In this case, the RL model assumes that propensities at the outset of period t+1 are defined recursively as follows:

(A2) $q_{nj}(t+1) = \beta q_{nj}(t) + (1-\varepsilon)\pi_n[i,k]$ if $j = i$, for j = 1 and 2; and
$q_{nj}(t+1) = \beta q_{nj}(t) + \varepsilon\pi_n[i,k]$ if $j \neq i$, for j = 1 and 2.

The parameter $\beta \in [0,1]$ discounts propensities; $1-\beta$ is called a forgetting or recency effect by various authors. The parameter $\varepsilon \in [0,0.5]$ provides for experimentation: It spreads reinforcement to strategies that were not chosen in period t.

To complete Erev and Roth's (1998) 3-parameter model, we need to specify propensities at the beginning of repeated play of the game. We assume $P_{n1}(1) = P_{n2}(2) = 1/2$, implying that $q_{n1}(1) = q_{n2}(1)$; and also assume that these initial propensities are proportional to player n's average possible payoff Π_n in the game matrix. That is, let $q_{nj}(1) = S = Q\Pi_n$ for all j, where $S > 0$, the strength parameter, represents how strongly players hold on to their initial propensities to play strategies. Erev and Roth found that the best-fitting values of the three parameters of this model across a variety of mixed strategy games were $\beta = 0.9$, $\varepsilon = 0.2$ and $Q = 9$, so we use these values in our power planning simulations.

The cautious fictitious play model of Fudenberg and Levine (1998) can also be specified as a three parameter model. Let $w_{nk}(t)$ be the belief weight that player n attaches to the event that the player m will play his kth strategy in period t. These belief weights then determine player n's subjective expected payoff from playing strategy j at time t, as follows:

(A3) $E_{nj}(t) = [\pi_n(j,1)w_{n1}(t) + \pi_n(j,2)w_{n2}(t)]/ [w_{n1}(t) + w_{n2}(t)]$, for j = 1 and 2.

The CFP model assumes that a logit model determines $P_{nj}(t)$ (the probability that player n chooses strategy k in period t) as a function of these subjective expected payoffs:

(A4) $P_{nj}(t) = \exp[\lambda E_{nj}(t)]/\{\exp[\lambda E_{n1}(t)] + \exp[\lambda E_{n2}(t)]\}$, for j = 1 and 2.

The parameter λ has been called sensitivity by Camerer and Ho (1999). As $\lambda \rightarrow \infty$, the subject best-responds to her subjective expected payoffs and chooses her strategy yielding the highest subjective expected payoff with certainty. By contrast, if $\lambda = 0$, the subject is completely insensitive to expected payoff differences and chooses all strategies with equal probability.

As in the case of the RL model, the CFP model requires period-to-period updating, but here it is updating of beliefs rather than propensities. If player n's partner player m chooses his ith strategy in period t, player n's belief weights for period t+1 will be:

(A5) $w_{nk}(t+1) = \mu w_{nk}(t) + 1$ if k = i, for k = 1 and 2; and
$w_{nk}(t+1) = \mu w_{nk}(t)$ otherwise, for k = 1 and 2.

The parameter μ in the CFP model is much like the parameter β in the RL model. $1-\mu$ is thought of as a rate of forgetting; past belief weights depreciated at rate μ. Finally, to initialize the CFP in a manner that is similar to the RL model, we assume that $w_{n1}(1) = w_{n2}(1) = W$, where $2W = w_{n1}(1) + w_{n2}(1)$ is called the strength of initial belief weights.

To estimate the likely power of designs, we need estimates of the parameters μ, λ and W of this CFP model. We set out to estimate them from Ochs' (1995) data on the games we call Ochs1, Ochs4 and Ochs9. We noticed that the CFP model, as specified above, had a good deal of trouble fitting data for both the row and column players across those games: In particular, the best-fitting values of the parameters result in overpredictions of row player's choices of "Up" and underpredictions of column player's choices of "Right," especially in Ochs9.

We conjecture that the problem with the CFP model, as specified above, is that sensitivity λ operates on absolute levels of subjective expected payoff. As Ho et al. (2001) note, psychophysical considerations suggest that this may be a poor assumption. We conjecture instead that sensitivity should operate on subjective expected payoffs as a proportion of some payoff reference statistic defined by each players' possible payoffs in a game. In particular, let $RNG_n = \max_{i,j}\{\pi_n[i,k]\} - \min_{i,j}\{\pi_n[i,k]\}$ be the range of player n's payoffs. We will use this modification of the CFP model, where equation A4 is replaced by the following equation:

(A6) $P_{nj}(t) = \exp[\lambda E_{nj}(t)/RNG_n]/\{\exp[\lambda E_{nj}(t)/RNG_n] + \exp[\lambda E_{nj}(t)/RNG_n]\}$, for j = 1 and 2,

We offer two observations on this model, which we call the CFP^* model. First, choice probabilities in Erev and Roth's (1998) RL model, as given by equation (1), are invariant to such linear transformations of payoff. Therefore, the psychophysical point that prompts the CFP^* model is moot for this specification of the RL model (see also Camerer and Ho 1999). Second, we note that many experiments with nontrivial mixed-strategy games are based on either symmetric or zero-sum games (or both): In such games, the range of possible payoffs of row and column players are equivalent. In such games one cannot identify the difference between the

CFP^* and CFP models. Ochs' (1995) data is somewhat special in this regard: Even if our CFP^* model is the "true" model,[1] one wouldn't see the patterned distortion of predicted choice probabilities under the CFP model except when looking at a game (like the OchsX games with large X) that is neither symmetric nor zero-sum. Of course, such distortion could be observed across different symmetric or zero-sum games if those games had varying payoff ranges.

Using Ochs' (1995) data, then, we find that the parameters of the best-fitting CFP^* model are $\mu = 0.92$, $\lambda = 2.80$ and $W = 0.5$. We used these parameters in the CFP^* model, and Erev and Roth's (1998) preferred parameters for the RL model, to search for an OchsX game with good power characteristics. To estimate design power, we assume that the "true" model without belief elicitation procedures is the RL model, and that the "true" model with a belief elicitation procedure is the CFP^* model. Assuming between-pair treatment variation, as well as fixed pairings of subjects over the experiment,[2] we searched over a variety of games (including games other than OchsX games), number of subject pairs in each treatment (N), and total periods of play (T) to find a design with acceptable power and interesting predicted differences in behavior.

As in the text, we let $I_e(p,t,s,DGP)$ be an indicator function that equals 1 when event e occurs in period $t \leq T$ in subject pair $p \leq N$ of sample s, using the "data-generating process" DGP. The data-generating process DGP is either the RL or CFP^* models specified above. The event e is either "Row player chooses Up" (denoted by ru) or "Column player chooses Left" (denoted by cl). As in the text (in equations 1-a to 1-d), we define four summary measures of the occurrence of the events e in each subject pair, as follows:

(A7-a) $F_e(p,s,DGP) = \Sigma_{t=1 \text{ to } T} I_e(p,t,s,DGP)/T$

(A7-b) $F_{e,1}(p,s,DGP) = \Sigma_{t=1 \text{ to } (1/3)T} I_e(p,t,s,DGP)/(T/3)$,

(A7-c) $F_{e,3}(p,s,DGP) = \Sigma_{t=(2/3)T \text{ to } T} I_e(p,t,s,DGP)/(T/3)$ and

(A7-d) $\Delta_e(p,s,DGP) = F_{e,1}(p,s,DGP) - F_{e,3}(p,s,DGP)$.

Assuming that two hypothetical treatments NB (no belief elicitation procedure) and B (belief elicitation procedure) result in play that exactly matches the data-generating processes RL and CFP^*, respectively, the simulations calculate the fraction of 10,000 such pairs of samples, each containing N subject pairs, in which we would reject the following four hypotheses using a 2-sample Signed-Rank test. As mentioned in the text, then, the simulations calculate the power of these tests, using various values of N and T and various versions of the OchsX game, against these null hypotheses at the 5% level:

(I) The distributions of $F_e(p,s,RL)$ and $F_e(p,s,CFP^*)$ do not differ;

(II) The distributions of $F_{e,1}(p,s,RL)$ and $F_{e,1}(p,s,CFP^*)$ do not differ;

(III) The distributions of $F_{e,3}(p,s,RL)$ and $F_{e,3}(p,s,CFP^*)$ do not differ; and

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[1] Other reference statistics of profit are possible, of course. We have tried to use Π_n (player n's mean possible payoff in the game) and find that RNG_n results in a better fit to Ochs' (1995) data. Another possibility is M_n, player n's maximum payoffs in the game, but this is identical to RNG_n in OchsX games.

[2] Our simulations indicate that across a moderate number of total periods, fixed pairings of subjects result in noticeably greater power of tests than do designs with random rematching at the beginning of each new period. Partly for this reason—but also because Nyarko and Schotter's (2000) test uses fixed pairings, too—our experimental design uses fixed pairings rather than random rematching.

(IV) The distributions of $\Delta_e(p,s,RL)$ and $\Delta_e(p,s,CFP^*)$ do not differ.

After a search to make power large for most of these hypotheses, for one or both of the subjects (that is, for at least one of the events $e \in \{ru,cl\}$), we settled on the game Ochs19, an extreme version of Ochs' original games (Ochs1, Ochs4 and Ochs9). This design involves N = 40 subject pairs in each treatment and T = 36 periods in each game. The first row of Table A1 below shows the results of our simulations for this design, for the four hypotheses for each of the events $e \in \{ru,cl\}$, using the Wilcoxon two-sample test. As can be seen, the power of the test is expected to be quite high for six of the eight possible hypotheses, the exceptions being average plays of "Up" by row players over all 36 periods, and late period plays of "Left" by column players (as suggested by Figure 1). These power figures are for each test in isolation. Thus, the probability that at least one test rejects at least one null is much higher, and the probability that all six tests that appear to be of high power in isolation would all reject their null hypotheses is low. Thus our expectation for this design is that we should get significant results on some, but not necessarily all six, of the null hypotheses that appear to have high power in Table A1. Also, these simulations assume that a "no belief elicitation" treatment results in perfect RL model play, and that a "belief elicitation" treatment results in perfect CFP^* play. Clearly, if belief elicitation procedures simply alter the mix of play in the direction of more CFP^* play, then the power of all the tests will be somewhat lower than shown in Table A1, and this is what we expect.

TABLE A1

POWER OF WILCOXON TESTS AGAINST THE HYPOTHESIS OF NO TREATMENT DIFFERENCE (SIMULATIONS—SEE NOTES BELOW)

	Power of test (at 5% significance level) to detect a treatment difference in the proportion of times each player plays the indicated strategy.						Power of test to detect change in proportion (between first third and last third of periods)	
Game	All periods		First third of all periods		Last third of all periods			
	Row plays Up	Column plays Left	Row plays Up	Column plays Left	Row plays Up	Column plays Left	Row plays Up	Column plays Left
Ochs19	0.18	0.99	0.84	0.99	0.85	0.11	0.98	0.79
N&S	0.04	0.04	0.04	0.14	0.04	0.07	0.04	0.14

For comparison, we have also subjected the design of Nyarko and Schotter (2002) to an identical power analysis. Here is their game:

Payoffs to (Row,Column) player as a Function of their Strategy Choices		Column Player's Strategies	
		"Left"	"Right"
Row Player's Strategies	"Up"	(6,2)	(3,5)
	"Down"	(3,5)	(5,3)

This game, too, is played with fixed pairings of subjects. In Nyarko and Schotter's design, 14 and 13 subject pairs in "Belief Elicitation" and "No Belief Elicitation" treatments, respectively, play 60 periods of this game. The last row of Table A1, labeled as "N&S" shows the power of this design against the various hypotheses. As can be seen, Nyarko and Schotter's design has almost no chance of detecting a difference in strategy choices between the "Beliefs" and "No Beliefs" treatments—assuming, of course, that in the Beliefs treatment behavior is in accord with the CFP^* model, and that in the No Beliefs treatment behavior is in accord with the RL model. Our point is that Nyarko and Schotter's failure to find this treatment difference is understandable, and that there is certainly room to continue looking for one in an appropriately designed game.

The Nyarko and Schotter design has low power in part because of the game itself, and not simply because of the relatively small number of subject pairs: Increasing this from 14 to 40 pairs (our own planned number of pairs) does not yield power approaching the conventional level of 0.80 at 5% significance. Let us be clear that we are not criticizing Nyarko and Schotter's (2002) design. Their primary purpose seems to have been a study of subject behavior within their treatment that uses belief elicitation; it seems to us that their comparison of treatments with and without belief elicitation procedures may well have been a robustness check conceived of well after they had chosen a game for their primary purposes. Rather, our point is that their results probably leave our primary question unanswered.