THE TIMES THEY ARE A-CHANGING: DYNAMIC ADVERSE SELECTION IN THE LABORATORY

FELIPE A. ARAUJO, STEPHANIE W. WANG, AND ALISTAIR J. WILSON

ABSTRACT. Across a variety of contexts decision-makers exhibit a robust failure to understand the interaction of private information and strategy. Such failures have generally been observed in static settings, where participants fail to think through a future hypothetical, with closer response to theory in sequential settings. We use a laboratory experiment to examine a common-value matching environment where strategic thinking is entirely backward looking, and adverse selection is a dynamic, non-stationary process. While a minority of subjects do condition on time, reflecting an introspective rather than learned solution to the problem, the majority of subjects use a sub-optimal stationary response, even after extended experience and feedback. Though unreactive to time, stationary subjects’ responses do exhibit strong learning effects. After outlining a misspecified model of the world that describes these subjects’ steady-state behavior, we construct two further treatments that validate this learning model out of sample.

1. INTRODUCTION

In many situations of economic interest the passage of time carries with it important strategic implications. In labor markets, good workers are hired and retained at higher rates, and so frequent spells of unemployment can serve as a negative signal to future employers. For durable goods such as houses, long periods of market availability can provide prospective buyers with a stronger bargaining position to push down the price. In health-insurance markets, long prior spells without insurance may signal adverse selection for customers newly seeking a policy. These forces can be present in simple consumer settings such as shopping for food, where the quality of produce on offer at a farmer’s market would be lower later in the afternoon than in the morning, with earlier shoppers having picked through the best offerings. With an inward focus, such forces are even present in the market for academic papers, where the length of time a paper has been around in working paper form can act as signal for the likelihood that referees at other outlets have found substantial flaws.

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While we believe situations with dynamically accruing adverse selection are commonplace, evidence for how decision makers respond to these forces is predominantly derived from behavior in static situations, such as sealed-bid common-value auctions. While the experimental literature certainly demonstrates failures of Bayes-Nash equilibrium predictions, there is evidence that experience moves behavior towards equilibrium in the static setting. For example, though many fall for the winner’s curse early on, they learn to bid less with experience. As such the Bayes-Nash equilibrium might still be viewed as a good long-run predictor, an “as if” outcome, without the need for subjects to understand the environment introspectively. However, similar “as ifs” in richer settings require greater sophistication. In order for behavior to converge towards equilibrium, agents need to have a flexible enough model of the environment to admit conditional responses to the strategically relevant variables. If their model is not flexible enough, long-run behavior can differ substantially from equilibrium, potentially reversing policy relevant comparative statics.

In this study, we experimentally examine behavior in a dynamic common-value matching environment where the strategically relevant variable is the passing of time. In our main experimental treatment, subjects are formed into groups with each member initially assigned to an object with an independently drawn value. Each subject is informed on their assigned object’s value at a random point in time and given a chance to exchange it for an unknown rematching option. In our dynamic setting exchange results in the given-up object becoming the rematching option for subsequent movers, leading to adverse selection. So long as participants are monotone, keeping high-value objects and giving up low-value ones, this adverse selection will accrue over time.

Given the accruing selection, equilibrium behavior is highly sensitive to the passage of time, reflecting the increasing likelihood that others observed their object’s value. Moreover, so long as others are believed to be monotonic, best-response in our environment is relatively insensitive to the beliefs over the levels of others’ response. The core understanding required for an introspectively reached equilibrium outcome is a qualitative model of others’ behavior: that they will keep good items and exchange bad ones. Outside of an introspectively reached solution, another possibility is that equilibrium behavior is reached in the long run through experience. The simple requirement for convergence to equilibrium in our setting is that subjects’ models of the world allows for a time-dependent response, where bad (good) past outcomes that occurred later (earlier) are treated distinctly.

Our experimental results find aggregate behavior is qualitatively in line with equilibrium response: a significantly negative response to time. However, there are substantial deviations from the point predictions. More important, looking at individual-level data, a large proportion of subjects fail to respond to the passage of time at all, exhibiting a stationary response even after extensive experience. Four additional treatments check the robustness of these results: varying the feedback on others’ strategic play both across and within supergames; providing subjects with peer advice;
and making the dynamic selection explicit. Mirroring the baseline results, a large proportion of participants fail to condition their responses on informative observables (in most cases the time at which their decision is made).

In our discussion section we go on to explore the extent to which boundedly rational equilibrium models might explain the outcomes. In particular, we show that a steady-state learning model where subjects use past play to form expectations can rationalize the distinctly different behavior from equilibrium. However, as this model was formulated post hoc, we conduct two further treatments as out-of-sample tests that explicitly test the model’s long-run mechanism. In a simplified version of our initial environment we vary the role assignment across subjects. Similar to our original sessions, in our first treatment we assign the time of choice randomly in each new supergame, so subjects gain experience across the strategically relevant variable. In our second treatment, subjects roles are fixed, with a player always making choices at the same time period in each new supergame. As such their observed outcomes more closely mirror the conditional experiences required for a learned equilibrium response. Despite an identical equilibrium prediction in the two settings, we find stark differences in the long-run response. Moreover, the observed differences across our two new treatments provide a strong out-of-sample validation of our behavioral model, qualitatively and quantitatively.

We make contributions to several strands of literature. First, our experimental setting provides a novel test tube for examining conditional responses under uncertainty. While the setting is economically interesting in its own right, it has a number of useful features for diagnosing failures in strategic thinking.

Second, we provide experimental evidence on the extent to which human decision makers can correctly condition in sequential settings with uncertainty. Our results feed into a growing literature on failures to account for how others’ private information affects one’s best response. Thus far, the theoretical and experimental studies have mostly focused on static common-value settings (for example, auctions and voting) where the key conditional events are future hypotheticals. Our study makes clear that similar failures occur when conditioning is backward looking on others’ expected past play. This is in tension with a recent experimental literature that finds behavior closer to equilibrium in perfect-information sequential play than strategically equivalent simultaneous environments. Our findings suggest that it is not sequentiality itself that is the sufficient condition for equilibrium play to emerge. Rather, by subtraction, our results suggest that it is the interaction between uncertainty and features of the extensive form, as in Li (2017).

Third, we show that boundedly rational learning models (see for example, Fudenberg and Levine, 1993, Espinosa and Pouzo, 2016, and Spiegler 2016 for a corresponding graph-theoretic language) can be successful at organizing behavior, in our case with a very simple maintained model of the
world. Variations in our experimental setting can be fertile ground for examining related questions, such as what regularities exist in the maintained models.

Finally, our experimental results speak directly to the theoretical literatures on dynamic adverse selection in asset, labor and insurance markets, sounding a note of caution over models that require substantial strategic sophistication for convergence to equilibrium. However, while there is a glass-half-empty interpretation from our result that the majority of participants in our experiments do not condition on time, we should make clear the glass-half-full complement to this. A large minority (approximately a third of the participants) do condition on time, and their behavior is well-explained by the Bayes-Nash equilibrium. What’s more, the minority’s behavior exhibits time-conditioning even in the very early rounds, and their written statements in our peer-advice treatment indicates a clear understanding of the mechanics of the dynamic selection. When we consider the self-selection effects likely to be present for workers in finance, human resource, and actuarial disciplines, equilibrium predictions in these professionals settings seem much more likely to hold.

The paper is structured as follows. Section two reviews the related literature. Section three contains the experimental design and procedures, and section four presents the model and hypotheses. The main results are presented in sections five, and in section six we discuss the heterogeneity in response, outline a behavioral model and provide an out-of-sample, out-of-context test. Finally, section seven concludes.

2. Literature Review

Our study contributes to the growing theoretical (Eyster and Rabin, 2005; Jehiel, 2005; Jehiel and Koessler, 2008; Esponda, 2008) and experimental (Esponda and Vespa, 2014, 2015) literature on peoples’ failures to account for how others’ private information will affect them in strategic settings. Experimental and empirical studies have primarily focused on two settings: auctions (see Kagel and Levin, 2002 for a survey) and voting. One well-documented case is the winner’s curse, the systematic overbidding found in common-value auctions. A leading theoretical explanation for this effect is that bidders fail to infer decision-relevant information on the value for the item they are bidding on, conditional on a very relevant hypothetical scenario: their bid being successful. For example, Eyster and Rabin (2005) predicts that subjects only best respond to others’ expected actions, failing to incorporate (or imperfectly incorporating, if partially cursed) how others’ actions are correlated with their private information.

A number of experimental studies have focused on determining the extent to which the winner’s curse can be explained by this conjecture. Charness and Levin (2009) ask participants to solve an individual decision-making problem with an adverse-selection component. In Ivanov et al. (2010)
players bid in a common-value second-price auction where the value of the object is the highest signal in the group (the maximal game), thereby controlling for beliefs about their opponents’ private information. Both studies continue to find deviations from the standard predictions, suggesting that incorrect beliefs about other players’ information are one source, but not the only one, of failure to respond optimally. For cursed behavior in voting, Esponda and Vespa (2014) find that most participants in a simple voting decision (with other voters played by robots) with minimal computational demands are unable to think hypothetically. That is, they do not condition their votes on the event (and subsequent information on others’ behavior) of their vote being pivotal. Moreover, a smaller fraction of subjects is also unable to infer the other (computerized) voters’ information from their actual votes. Similarly, Esponda and Vespa (2015) found that most participants were not able to correctly account for sample selection driven by other players’ private information.\(^1\) Our experimental setup expands this literature by offering a novel setting that can be easily modified to explore various bounded rational models of learning to detect regularities in people’s misspecified perceptions of the strategic setting.

Thus far, the experimental literature has focused on the importance of sequential rather than simultaneous play in reaching closer to equilibrium behavior in these strategic settings. For example, a significant share of participants who received explicit feedback about the computerized players’ choices in the sequential treatment of Esponda and Vespa (2014) were able to correctly extract information from those observed choices. Similarly, players are more likely to adjust their thresholds to account for the selection problem if they were actually pivotal in the previous round (Esponda and Vespa, 2015). A number of experiments on sealed-bid vs. clock auctions have found closer to equilibrium bidding behavior when bidders are able to observe the decisions of other bidders (Levin et al., 1996; Kagel, 1995). Carrillo and Palfrey, 2009 find that second movers in the sequential version of their two-sided adverse selection setup behave more in line with equilibrium predictions than the first movers or players in the simultaneous version. Ngangoué and Weizsäcker (2017) is another recent example where traders neglect the information contained in the hypothetical value of the price when they submit bids before the price is realized in the simultaneous market. However, in sequential markets where the price is known before the bid, traders’ reaction to price are in line with standard theory. However, while the literature has identified sequentiality as the key to subjects understanding the equilibrium thinking, our paper suggests that it is not sequentiality on its own, but the interaction between sequentiality and the resolution of uncertainty. In a complementary result to our negative result for sequentiality under uncertainty, Martínez-Marquina et al. (2017) indicate that participants better understand the adverse selection features in the simultaneous environment once uncertainty has been eliminated.

\(^1\)See also Enke (2017) and Jin et al. (2015) for further work on subjects’ failures to understand the complexity of the environment.
Our study also speaks to the substantial theoretical literature interested in dynamic adverse selection environments (Hendel et al., 2005; Daley and Green, 2012; Gershkov and Perry, 2012; Chang, 2014; Guerrieri and Shimer, 2014; and Fuchs and Skrzypacz, 2015). One focus has been on asset markets where sellers have private information about the quality of the asset (Chang, 2014; Guerrieri and Shimer, 2014). Similarly, the current and past owners of an object in our setup could know the value of the object, while those who have never held the object do not. Although our players only make a binary choice on whether to keep the object or trade it for another in the early rounds, they state a cutoff value for trading the object in later rounds, much like the price setting done by sellers and buyers in the asset markets. Our experimental results suggest that these models should take seriously behavioral agents with misspecified models of the dynamic adverse selection environment.

3. DESIGN

We conducted 28 experimental sessions with a total of 480 undergraduate subjects. The experiments were all computer-based and took place at the Pittsburgh Experimental Economics Laboratory (PEEL). Sessions lasted approximately 90 minutes and payment averaged $25.60, including a $6 participation fee. In total we have eight different treatments, but for the next two sections we will focus on describing just two: i) our Selection treatment, which induces a dynamic adverse-selection environment; and ii) our No Selection (Control) treatment that removes the adverse selection and has a stationary best response.

Selection and No Selection sessions both consist of 21 repetitions of the main supergame, broken up into: part (i) (supergames 1–5), which introduces subjects to the environment; part (ii) (supergames 6–20) and part (iii) (supergame 21), which add strategy methods; and part (iv), which elicits information on risk preferences and strategic thinking. Before each part, instructions were read aloud to the subjects, alongside handouts and an overhead presentation.2

The environment in both treatments has a similar sequential structure, with one key difference: in Selection supergames three randomly chosen subjects are matched together into a group to play a game; in No Selection supergames an individual subject makes choices in an isolated decision problem. We next describe the Selection environment in more detail before coming back to describe the No Selection setting.

Selection. The primary uncertainty in each of our supergames is generated by drawing four numbered balls, labeled as Balls A–D. Each ball is assigned a value through an independent draw over the integers 1–100 (with proportionate monetary values from $0.10 to $10.00) according to a fixed

2Detailed instructions, presentation slides, and screenshots of the experimental interface are in the technical appendix.
distribution $F$, which has an expected value of 50.5.\(^3\) A group of three players are randomly assigned a mover position, which we refer to as first, second and third mover. Each group member takes one of the four balls in turn, randomly and without replacement. As the three players each hold a different ball, a single ball remains unheld. This unheld ball is the rematching population in our game.

An example matching is illustrated in Figure 1, where the first line shows an example initial matching. In the illustrated example the first mover is matched to Ball B, the second mover to Ball A and the third mover to Ball D, or \(\langle 1B, 2A, 3D \rangle\) for short. In this example the leftover unheld ball is Ball C.

Though players know which of the four balls they have been assigned at the start of the supergame, they do not start out knowing the assigned ball’s value, nor the balls (or values) held by other group members. In each round, the three players flip fair coins. If it lands heads they learn their held ball’s value, and if the coin lands tails they do not learn the value and must wait to flip again. However, if a player has not seen their held ball’s value in rounds one or two (flipping tails in both), then the value is always revealed to them in round three.

In the period when they see their ball’s value, the player makes one and only payoff relevant decision:

**Either:** Keep the currently held known-value ball as the final supergame outcome.

**Or:** Take the unknown rematching ball (the currently unheld ball) as the final supergame outcome, where the currently held ball is released and becomes the rematching ball for subsequent movers.

To make clear the process and intuition of the game, consider the example illustrated in Figure 1. The figure here takes the point of view of the first mover, where figure elements in black represent information that is known to the first mover at each point in time, while elements in grey represent unknowns. In the example, though the first mover knows she is holding Ball B in the first round \((t = 1)\), its value remains unknown to her as she fails the coin flip. The first mover does not know which balls the other two players are initially holding, nor their coin flip outcomes, nor their decisions. She only knows that they are present and that their decisions are potentially affecting the rematching ball.

In the illustrated example, the initial matching is \(\langle 1B, 2A, 3D \rangle\) and Ball C is initially unheld. However, unknown to the first mover, the second mover flips a head and sees his held ball’s value

\(^3\)The distribution used in our experiments is a discrete uniform with additional point masses at the two extreme points. Precisely, the probability mass function puts a $\frac{1}{200}$ mass on the two values 1 and 100 and a $\frac{1}{200}$ weight on each of the integers 2–99. This distribution was chosen to make the selection problem more salient, and to generate sharper predictions for the Bayes-Nash equilibrium.
is 1, and he decides to switch, while the third mover flips tails and does not learn her value. The interim matching is therefore $\langle 1B, 2C, 3D \rangle$ where the rematching ball is now the released Ball A. In the second round, the first mover flips a head, and sees that her held ball’s value is 34. She decides to release this ball and rematches to the currently unheld Ball A, and the matching becomes $\langle 1A, 2C, 3D \rangle$. After her round-two decision (and again, unknown to the first mover) the second mover does not act as he has already made a decision, while the third mover flips a head and decides to give up her 13-ball, rematching to the Ball B that was just given up by the first mover, shifting the match to $\langle 1A, 2C, 3B \rangle$. By round three, all three participants have made a decision, and thus the final matching is $\langle 1A, 2C, 3B \rangle$. At the end of the supergame all four balls’ values are made common knowledge—though which balls other players are assigned to is not—and the first mover learns that the ball she rematched to has a value of one.

Supergames one to five exactly mirror the procedure above. Subjects make a binary decision to keep or switch only in the round where their ball value is revealed. The second part of each session then adds a partial strategy method. Specifically, in supergames 6–20 participants are asked to provide a cutoff in each round, indicating the lowest value for which they would keep their held ball contingent on seeing its value that round. If they receive information, the decision to keep or switch is resolved according to the stated cutoff; if they do not, they must wait until the next round, when they will provide another cutoff. Finally, in part (iii) we use a complete strategy method in which subjects are not informed about whether or not information was received in each round, and we collect their minimum-acceptable cutoff values in all three rounds of supergame 21 with certainty.⁴⁵

⁴In expectation one-quarter of subject data in supergames 6–20 will have data from all three round cutoffs, one quarter with cutoffs from rounds one and two only, and one half of the data only has an elicited first-round cutoff.
⁵In part (iv) at the end of each session we collect survey information, and incentivize the following elicitations: (a) risk preferences (using a version of the Dynamically Optimized Sequential Experimentation, see Wang et al. 2010); (b)
Strategic feedback on the other participants is purposefully limited in our baseline Selection game. At the end of each of our Selection supergames, each group member sees the values of the four drawn balls, as well as the particular ball he/she is holding at the end and (if relevant) the identity and value of the ball they were initially matched to. Participants do not see strategic feedback. That is, they observe neither the identity of the balls held by the other two group members at the end of the supergame, nor the balls others were initially holding, nor their choices.

Subjects’ final payments for the session are the sum of: a $6 show-up fee; $0.10 times the value of their final held ball ($0.10 to $10.00) from two randomly selected supergames from 1–20; and $0.10 times the value of their final held ball in supergame 21. Excluding the part (iv) payments the experiment therefore has a minimum possible payment of $6.30 and a maximum of $36.00.

No Selection. Our No Selection games are designed to have the same structure as the Selection game, except that we turn off the dynamic adverse selection. This is achieved by making a single change to the environment: each group has just one member. As such, each supergame is a decision problem with a single participant in the role of first mover. As there are four balls, and only one of them is held by the agent, there are three unheld balls. In whichever round the first-mover sees their held ball’s value, if they decide to switch their ball, they receive one randomly selected ball of the three unheld balls. Our No Selection sessions therefore replicate the same incentives and timing as the Selection sessions, but without the other group members. We illustrate a parallel example supergame for the No Selection environment in Figure 1(B).

4. MODEL AND HYPOTHESIS

The games described above are dynamic assignment problems over a finite set of common-value objects. The objects (the long-side) are initially assigned randomly to the short-side of the market (the game’s participants). Private information on the held object’s long-run value arrives randomly over time, according to an exogenous process (in the experiment, the coin flips).

With a single decision maker, the rematching pool is never affected by other participants’ decisions. As such, the risk-neutral prediction in our No Selection treatment is that subjects are stationary and use a minimal acceptable cutoff of 51 for retaining a ball. That is, the cutoff rule gives up balls valued 50 or below (beneath the expected value of 50.5) and keeps balls valued 51 or higher (above the expected value).

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6We examine the effects of alternative feedback in Section 5.
Though risk-aversion or risk-lovingness might lead to alternative cutoff rules, the passing of time conveys no information on the expected value of rematching, and decision makers are predicted to be stationary across supergame rounds.

**Hypothesis 1 (Control Stationarity). Subjects use stationary decision-making cutoffs in the No Selection (Control) treatment**

In contrast to the control, when there are multiple players, each making self-interested decisions over common-value objects, the arrival of private information leads to adverse selection on the rematching pool. Whenever other players give up objects with (privately) observed low values, and keep objects with high values, the rematching pool will become selected. As private information arrives stochastically, adverse selection accrues over time. In early periods, it is less likely that others have received private information, so the rematching pool is less likely to be selected. In later periods, it is more likely that others have received private information, which leads to greater and greater likelihoods that the rematching pool is adversely selected, a picked over item by others.

Because the environment is sequential and involves each player making a single decision, the equilibrium predictions can be solved inductively, where best-response calculations are entirely backward looking. This is in contrast to many other situations that examine “cursed” behavior over hypothetical forward events. For example, in common-value auctions, optimal decision-making requires the bidder to act as if concentrating solely on the hypothetical event that their chosen bid will win the auction, and then inferring the information contained in this hypothetical on the object’s value. Similarly, in common-value voting, the voter has to reason as if focused on the hypothetical event that her chosen vote is pivotal. In our environment, the optimal response is conditioned on time, where the hypothetical thinking relates to how other participants have acted in previous periods.

The optimal response for a risk-neutral player on seeing her held object’s value at time $t$ is to give up objects with values lower than the expected value of rematching conditional on the available information, and to keep objects with values higher than the expected value. The best response is therefore summarizable by a time-indexed cutoff $\mu^*_t$, the expected value of the rematching pool value distribution $G_t$ at time $t$. Both the distribution $G_t$ and the policy cutoff $\mu^*_t$ can be calculated inductively from the first-mover seeing her object’s value in the first round ($t = 1$). For the base case the rematching pool is an iid draw from the generating distribution $F$ with certainty, as no other participant has had a chance to exchange their object yet. Hence the distribution for the rematching pool is $G_1 = F$, the initial value distribution. The policy for a risk-neutral first-mover

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$^7$In our experiments the action set is discrete as the ball values are in $\Theta = \{1, \ldots, 100\}$, and so the cutoff can be summarized instead by $\min\{\theta \in \Theta : \theta \geq \mu^*_t\}$, the minimal acceptable ball value.

$^8$For the theory, instead of indexing time by the round number, we do it by round-mover. So the first mover in round 1 is $t = 1$; the second mover in round 1 is $t = 2$; the third mover in round 1 is $t = 3$; etc.
in the first round is a cutoff equal to the minimum integer in \(\{1, \ldots, 100\}\) higher than or equal to expected-value of a single draw from \(F\) (\(\theta^* = 51, \mu_1^* = 50.5\)).

For the inductive step we define the event that the player who moves at time \(t\) sees their value as \(I_t\), and the joint event that they both see their value and choose to switch as \(S_t\). Given the value distribution faced by the player in period \(t\), \(G_t\), and the policy cutoff \(\mu_t^*\), the conditional distribution for the player making a choice in period \(t + 1\) (such that \(t = 2\) would be the second mover in round one, etc.) is:  

\[
(1) \quad G_{t+1}(x | I_{t+1}) = \Pr \{S_t; \mu_t^* | I_{t+1}\} \cdot F(x | x < \mu_t^*) + \Pr \{\text{not } S_t; \mu_t^* | I_{t+1}\} \cdot G_t(x | I_{t+1}, \text{not } S_t).
\]

The optimal policy cutoff \(\mu_{t+1}^*\) for the player at the inductive step is simply the expected value of \(G_{t+1}(x | I_{t+1})\). Given the induction in (1) it is clear the solution to the model is entirely backward looking.

The risk-neutral Bayes-Nash Equilibrium predictions for the Selection treatment vary from a predicted cutoff of 51 for the first mover in the first round, to a cutoff of 23 for the third mover in the third. This represents a substantial response to adverse selection by the end of the supergame, reducing the expected value of rematching by almost half. To put this in context, if the other two agents were fully informed on the other three balls’ values and perfectly sorted so the remaining unheld ball was the worst of the three, its expected value would be \(\mu(3) = 16.4\). That is, by the end of the last round over 75 percent of the adverse selection possible under full information and perfect sorting has occurred. Figure 2 expresses the risk-neutral Bayes-Nash Equilibrium predictions as the degree of the possible selection, graphing the transformation \((\mu_t^* - \bar{\mu})\), where \(\bar{\mu}\) is the expected value of the original distribution \(F\). Within each round the cutoffs are decreasing as the different roles take turns to move, but across rounds there is a slight increase from the third mover in round one to the first mover in round two. Importantly, within each role the PBE predictions indicate strictly decreasing cutoffs, reflecting the increased adverse selection as the game unfolds.

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9 For example, given the base case the first two rounds of the induction are: second-mover sees their value and infers that \(\Pr \{S_1; \mu_1^* | I_2\} = \Pr \{S_1\} = \Pr \{I_1\} \cdot F(\mu_1^*) = \frac{1}{4}\), given a half probability the first mover observes their value, and a half probability that their held ball’s value is lower than the average. The effective CDF for the rematching pool in period two is therefore \(G_2(x) = \frac{1}{4} \cdot F(x | x < 50.5) + \frac{1}{4} \cdot F(x)\), with expected value \(\mu_2^*\). The third-mover therefore faces the distribution \(G_3(x) = \Pr \{\bar{I}_3\} \cdot F(\mu_3^*) \cdot F(x | x < \mu_2^*) + (1 - \Pr \{\bar{I}_3\}) \cdot F(\mu_3^*) \cdot G_2(x)\).

10 We condition on the information \(I_{t+1}\) throughout here, as a player moving in later periods knows they personally did not get information in previous periods.

11 The risk-neutral PBE cutoffs for rounds 1, 2, and 3 are, respectively: 51, 42, and 35 for the first-mover; 35, 31, 28 for the second-mover; and 28, 25, 23 for the third-mover.

12 The reason for the non-decreasing parts is the conditioning in equation (1): the first mover who sees their value in round two (the fourth mover, so the event \(I_4\) in the induction) knows that they did not switch in round one. So in the language of the induction, \(\Pr \{S_1; I_4\} = 0\) as \(S_1 \subset I_1\), but \(\Pr \{I_3 \cap I_4\} = 0\) given our information structure. Note, if players were different for each decision, the cutoffs would be strictly decreasing, as conditioning on \(I_{t+1}\) would be uninformative to prior periods.
While the equilibrium cutoffs are unique under risk neutrality, the decreasing pattern holds in equilibrium for both risk-loving and risk-averse preferences. Moreover, decreasing cutoffs will be predicted even without sophisticated equilibrium beliefs on others’ behavior. For example, a simple belief that other participants use a stationary (non-boundary) cutoff rule that gives up low-valued objects and keeps high ones yields best-response cutoffs with quantitatively very similar predictions to the equilibrium.\footnote{Using a differing interior cutoff $\mu'$ from the equilibrium one has two offsetting effects. On the one hand increasing the cutoff increases the likelihood of selection, $\Pr\{S_i; \mu'\}$. On the other hand, it decreases how bad the selection is when it does occur, $F(x \mid x < \mu')$. In our experimental parameterization, the best-response cutoffs are quantitatively similar to the equilibrium cutoffs for a large set of beliefs on others’ behavior. See Figure A.1 in the appendix for a graphical depiction of the invariance to other subjects’ (interior) cutoffs.}

This robustness to the actual behavior of others is a useful feature of our dynamic environment, where despite substantial deviations from equilibrium, the empirical best response in our sessions is essentially identical to the PBE. Instead of needing to form accurate beliefs on the cutoffs used by others, the strategic sophistication required to used a decreasing cutoff across time is to understand that others information arrives over time and that they will give up low-valued objects and keep high valued ones. Given the similarity between the agents in the supergame, understanding this can be achieved by projecting one’s own experiences and behavior onto others.

While the levels in Figure 2 are calculated using the risk neutral PBE, the following hypotheses are independent of the subjects’ risk aversion, and are robust to subjects’ beliefs on others’ strategic behavior. As the majority of our results elicit subjects’ precise cutoff rules, we specify our hypotheses over such cutoffs.\footnote{We examine the behavior in supergames one to five in the appendix, where we show that subjects act as if they are using a monotone rule that keeps high value balls and gives up on low values.}

**Hypothesis 2 (Adverse Selection in Treatment).** Subjects use strictly decreasing decision-making cutoffs in the Selection Treatment.

![Figure 2: Predicted Adverse-Selection Accruing over Supergame](image)
In addition to the qualitative direction of cutoffs within treatments, we can also make comparisons across treatments, as the first-mover in the first round of our Selection settings faces an identical problem to the No Selection one.

**Hypothesis 3** (First decision equivalence). *The distribution of first-round first-mover decision-making cutoffs in the Selection Treatment is identical to the cutoffs used in No Selection (Control)*

While our experimental environment is formulated for tractability and simplicity in the dynamic effect, the fundamental strategic tension we examine is relevant to a number of economic scenarios where selection occurs over time, where the prime examples are markets for labor, insurance, and durable goods. While many applied settings have features that alter the mechanics—such as the information available on others’ decisions, observable signals on the degree of selection in the rematching pool, etc.—the main economic idea is frequently that the pool gets worse over time, and thus a non-stationary response is necessary.\(^{15}\) Models of environments like this using standard solution concepts will therefore require that participants’ either introspectively solve the model, or alternatively that behavior adapts to the observables *as if* they understand how the selection is occurring.

Below we first outline our aggregate experimental results, where our focus will be on the late-session play after subjects have acquired extensive experience with the environment. After outlining the main results, and checking their robustness, we come back to the “as if” in section five where we examine two behavioral models of steady-state learning.

5. **Aggregate Results**

We now describe the main experimental results, comparing the behavior in the decision environments with and without adverse selection, examining the three hypotheses above. The aggregate results for the *Selection* and *No Selection* treatments are illustrated in Figure 3, where the figure presents all data from subjects in the first-mover role where a cutoff is elicited. The focus on first movers provides the cleanest comparison across treatments because (i) the PBE prediction is identical for the first-mover in the first round, and (ii) the changes in the optimal cutoffs across rounds are largest for first-movers.\(^{16}\) The figure indicates the first-mover subjects’ responses relative to the expected cutoff without selection of \(\mu^*_1 = 51\). While the equilibrium theory predicts no adverse

\(^{15}\)In particular, many settings will allow for observable signals of others’ choices (such as a CV listing employment stints, an open box or refurbished status, knowledge of previous marriages, etc.). Observing signals of others’ choices along the path of play may help subjects understand and learn about the adverse selection accruing over time. We tackle this idea in one of our robustness treatments in Section 5.

\(^{16}\)Results and conclusions are statistically and numerically similar with a focus on all rounds and mover roles. In the appendix we provide evidence from the first five supergames where we did not explicitly elicit cutoffs; subjects behavior in these rounds with the binary keep/switch action is consistent with the use of a cutoff.
selection in No Selection (the white triangles), the prediction in the Selection treatment is for selection to accrue across the three rounds (the gray circles), with much of the predicted selection accruing by round two.

Three patterns emerge from Figure 3: (i) aggregate subject behavior does respond to the passage of time in Selection supergames, but the adjustment to the adverse selection falls short of the equilibrium predictions; (ii) behavior is markedly different between treatments; and (iii) while aggregate behavior in the No Selection treatment is statistically indistinguishable from the risk-neutral theory, behavior in the Selection treatment is significantly different.

Table 1 provides random-effect regression results to complement the figure. The table reports estimated (absolute) cutoffs for first movers across rounds one to three, where we separately estimate first-mover behavior in supergames 11 to 20 and in the full-strategy method supergame 21.\footnote{We focus here on results in the latter half of the session. Results for supergames 6 to 20 are in the appendix’s Table A1; results for subjects in the second- and third-mover roles are in Table A2. Qualitative results are similar to those using supergames 11 to 20 for first-movers only.}

Aggregate estimates are produced by regressing the chosen first-mover cutoff $\mu_{ist}^j$ (subject $i$, supergame $s$, round $t$, and session type $j \in \{\text{NoSel}, \text{Sel}\}$) on a set of treatment-round dummies. The estimated aggregate cutoff $\hat{\mu}_i^j$ for session type $j$ and supergame round $t$, allows us to make statistical inference over the equilibrium hypotheses.

Hypothesis 1 is a basic check for the control environment: given the stationary No Selection environment, are the aggregate cutoffs in this treatment stationary across the supergame? Inspecting the No Selection coefficients in Table 1, we verify that the first-round cutoffs are just under 55. This decreases slightly over the course of each supergame, to 54 in rounds two and three. Examining each coefficient in turn we test whether the average cutoffs used in each treatment-round are equal to the coefficients in round one, reporting the $p$-values in the $H_0 : \hat{\mu}_i^j = \hat{\mu}_i^{NS}$ column. Individually,
### Table 1. Average Cutoff per Round for No Selection and Selection Treatments, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 11 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}$</td>
<td>$p$-values</td>
</tr>
<tr>
<td><strong>No Selection</strong></td>
<td></td>
<td></td>
<td>$H_0 : \hat{\mu}_j^1 = \hat{\mu}_1^{NS}$</td>
<td>0.119</td>
</tr>
<tr>
<td>(Control)</td>
<td>Round 1, $\hat{\mu}_1^{NS}$</td>
<td>[51]</td>
<td>54.81 (2.45)</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}_2^{NS}$</td>
<td>[51]</td>
<td>54.06 (2.47)</td>
<td>0.213</td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^{NS}$</td>
<td>[51]</td>
<td>53.94 (2.52)</td>
<td>0.268</td>
</tr>
<tr>
<td><strong>Selection</strong></td>
<td></td>
<td></td>
<td>$\mu^*$</td>
<td>$\hat{\mu}$</td>
</tr>
<tr>
<td>(Treatment)</td>
<td>Round 1, $\hat{\mu}_1^S$</td>
<td>[51]</td>
<td>46.63 (1.23)</td>
<td><strong>0.003</strong></td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}_2^S$</td>
<td>[35]</td>
<td>42.99 (1.28)</td>
<td><strong>0.000</strong></td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^S$</td>
<td>[28]</td>
<td>39.12 (1.35)</td>
<td><strong>0.000</strong></td>
</tr>
<tr>
<td><strong>Joint Tests:</strong></td>
<td></td>
<td></td>
<td>0.335$^\dagger$</td>
<td>0.238$^§$</td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 170/137/33 Total/Selection/No Selection first-mover subjects across supergames 11-20, and 55/22/33 in supergame 21. Selection treatment exclude subjects in the second- and third-mover roles (these figures given in the appendix). $^\dagger$–Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0 : \hat{\mu}_j^1 = \hat{\mu}_1^{NS}$ for treatment $j$, round $t$) or the theoretical prediction ($H_0 : \hat{\mu}_j^1 = \mu^*_j$). $^\dagger$–Joint test of stationary cutoffs across the supergame ($H_0 : \hat{\mu}_j^1 = \hat{\mu}_j^2 = \hat{\mu}_j^3$ for treatment $j$); $^\dagger$–Joint test of PBE cutoffs in supergame ($H_0 : 0 = \hat{\mu}_1^j - \mu^*_j = \hat{\mu}_2^j - \mu^*_j = \hat{\mu}_3^j - \mu^*_j$).
neither the second- nor third-round’s No Selection coefficients are significantly different from the first round. Examining Hypothesis 1 directly with a Wald test on $H_0 : \hat{\mu}_1^{NS} = \hat{\mu}_2^{NS} = \hat{\mu}_3^{NS}$ (same cutoff in the control for all three rounds) we fail to reject with $p = 0.355$ for supergames 11 to 20 and $p = 0.875$ for supergame 21.

Beyond just stationarity in the No Selection cutoffs, we also fail to reject the stronger hypothesis that aggregate behavior in No Selection is both stationary and equal to the risk-neutral prediction. Examining each coefficient separately, we fail to reject the risk-neutral predictions for all No Selection round coefficients (the $H_0 : \hat{\mu}_t^j = \mu_t^j$ column). Jointly we fail to reject the Wald test that all three coefficients are at the risk-neutral PBE prediction ($p = 0.238$).

**Result 1** (Control Stationarity). *We cannot reject the hypothesis that average behavior in No Selection sessions is stationary nor that it is at the risk-neutral PBE predictions.*

Given that aggregate behavior in our control is well-behaved, we turn to an examination of aggregate behavior in the environment with adverse selection. The bottom half of Table 1 provides the average Selection cutoffs $\hat{\mu}_1^S$, $\hat{\mu}_2^S$ and $\hat{\mu}_3^S$, again breaking the estimates up into those obtained in supergames 11–20 and supergame 21. Our coarsest prediction for the Selection treatment is that the cutoffs decrease, indicating that subjects respond to the adverse selection accruing over time (Hypothesis 2). The hypothesis is tested by examining a Wald test for stationary cutoffs, $H_0 : \hat{\mu}_1^S = \hat{\mu}_2^S = \hat{\mu}_3^S$. Unlike the control where we fail to reject stationarity, we strongly reject it in the Selection treatment ($p = 0.000$) in favor of the PBE prediction of strictly decreasing cutoffs.

Though qualitative behavior is in line with the theory, the aggregate levels in Selection are far from the PBE predictions. As illustrated in Figure 3, subjects’ behavior does not fully internalize the predicted degree of adverse selection. For supergames 11 to 20, the relative level is just over half the predicted magnitude at $\frac{\hat{\mu}_3^S - \hat{\mu}_1^S}{\mu_3^S - \mu_1^S} = -0.517$. For supergame 21 on its own, the relative magnitude is closer to the prediction, representing 64 percent of the predicted adverse effect. Moreover, the attenuated response relative to theory becomes even more pronounced when you consider that subjects start out with lower cutoffs in the very first round. While the behavioral shift across the three rounds is significantly less than zero ($\hat{\mu}_3^S - \hat{\mu}_1^S = -7.65$ in supergame 21, $p = 0.000$), the size of the difference is a third of the theoretical prediction ($\mu_3^S - \mu_1^S = -23.0$).

The relative drops in willingness to rematch across the supergame are therefore less pronounced than the equilibrium predictions, but the different behavior in the first-round of the Selection treatment also jumps out as an anomaly. Despite an equivalent decision for first movers in the very first round, the provided cutoffs in the Selection supergames ($\hat{\mu}_1^S$) are significantly lower than both the No Selection cutoffs ($p = 0.002$) and the risk-neutral prediction ($p = 0.004$). Moreover, this effect becomes more pronounced if we focus just on play at the end of the session in Supergame 21.

We summarize the aggregate findings in the adverse selection environment:
Result 2 (Treatment Dynamics). *We reject that aggregate behavior in the Selection treatment is stationary, as the cutoffs have a significant and strictly decreasing trend. However, the dynamic reaction is significantly different from the theoretical prediction.*

Result 3 (First Round Non-Equivalence). *Average first-round cutoffs in the Selection treatment are significantly lower than both the No Selection results and the risk-neutral prediction.*

In Section 6 we show that these two aggregate patterns from the Selection treatment (a negative but shallow slope, with a lower intercept) are a product of individual heterogeneity. Two behavioral types emerge: (a) Sophisticated subjects who change their cutoffs across supergames, starting close to 51 in the first round; and (b) Coarse-reasoning subjects that use a constant response across the supergame, but where the level of this response does respond to the unconditional selection forces. When mixed, the aggregate behavior is shifted downward with an attenuated response across time.

Before analyzing the results from individual heterogeneity though, we briefly outline results from six further treatments that demonstrate the robustness of our results, both to the type of strategic feedback that subjects receive and to changes in the environment.

**Summary of Robustness Treatments.** Previous studies have found that the structure and timing of feedback players get in these strategic situations can make a big difference in how well they are able to extract information. In the adverse-selection experiment of Fudenberg and Peysakhovich (2014) subjects appear to react more to extreme outcomes in the most-recent round compared to earlier rounds, a feedback recency effect. Huck et al. (2011) find that when presenting the aggregate distribution of play across all games in a multi-game environment, the feedback spillover induces long-run behavior that converges to an analogy-based expectations equilibrium (Jehiel, 2005).

In addition to our main treatment/control comparison, we conducted four further treatments that manipulate the information subjects receive in environments with dynamic adverse selection. Details of these treatments are included in the appendix for interested readers, where the main findings above are replicated. Here we provide the reader with a concise summary of the treatments and the qualitative results.

Robustness Treatment 1 (S-Across). *This treatment provides additional strategic feedback across the Selection supergames.*

Here we replicate the Selection treatment, but the subjects are now completely informed on all players’ actions at the end of each supergame. Looking back to Figure 1(A) in the design, where Selection only informed subjects on their own choices (the elements in black), in S-Across treatments subjects are informed of all elements in the figure once the supergame has ended. The treatment results mirror those in the Selection treatment.
Robustness Treatment 2 (S-Within). This treatment provides additional strategic feedback within the Selection supergame as it proceeds.

Where S-Across provided feedback at the end of each supergame, this treatment modifies the information structure within the supergame so that subjects are informed about switches along the path of play. Rather than time, the relevant conditioning variable for cutoffs is observing a switch by the other participants. In the Figure 1(A) example, the first mover would know that the second-mover had switched when they made their choice in round 2. We come back to this treatment to talk about some of the individual-level results in the next section, but we find qualitatively similar effects to the Selection treatment at the aggregate level. Subjects respond to the appropriate signal (here an observed move, not the passage of time), but the size of the response is attenuated.

Robustness Treatment 3 (S-Explicit). This treatment adds adverse selection across time to the No Selection decision environment.

In the No Selection decision problem, a single agent makes choices over time, and, because the rematching pool is held constant, there is no adverse selection. In this modification, we provide the same rematching pool in the first round (an equal chance of each of the three unheld balls). In round two, the rematching pool has the highest-value ball removed, and becomes selected. In round three, the second-highest rematching ball is also removed, so the only rematching ball is the worst of the three. This treatment exhibits similar effects to the Selection setting, with a non-stationary response that under-reacts to the adverse selection present.18

Robustness Treatment 4 (S-Peer). This treatment adds peer advice to the final choice (supergame 21) in the Selection environment.

These sessions are identical to the Selection treatments, except for the final part, supergame 21.19 In the final supergame (which is paid with certainty), subjects are first matched into chat groups of three. After chatting, each member is matched with members from other chat groups for the final Selection supergame. Crucially, one of the three chat-group members is selected at random and that participant’s supergame 21 outcomes determines the payoff for the entire chat team. As such, each team member has an incentive to explain the environment to others.

---

18One difference relative to Selection is that we do not find significant differences to the first-round cutoffs in No Selection. In the Appendix we provide a reinforcement learning model that shows that variation in the subjects’ exposure to bad/good outcomes across the session is a potential driver for these differences, which will relate to the behavioral models we later construct.

19Results from this treatment were included in Table 1 for the columns examining Supergames 11–20 as the treatment is identical up to this point, but not for the results examining Supergame 21.
Even though several groups do have chat members who explain the underlying tensions in the game to the other participants,20 the end behavior in supergame 21 is not significantly different from that observed in the Selection environment.

Robustness Conclusion. As the results from our robustness treatments mirror the findings in our Selection treatment, we do not provide more extensive documentation on the results here (shifting these additional findings to the Appendix for interested readers). Instead, we focus on breaking down behavior at the individual level and show that the aggregate results obscure substantial heterogeneity. Given the qualitatively and quantitatively similar results, we use subject-level data from the robustness treatments introduced above to demonstrate that this heterogeneity has substantial stability across different treatments.

6. Subject Heterogeneity, Learning and Behavioral Models

In Section 5 we showed that subjects’ average cutoffs are decreasing across the Selection supergames, but are stationary in the No Selection control. To some extent, this represents a victory for the theory as a qualitative prediction. However, in this section, we show that relying on the averages mask an important heterogeneity in behavior. While a substantial fraction of subjects do use strictly decreasing cutoffs in Selection, a majority use stationary cutoffs that are entirely unresponsive to the relevant conditioning variable. In this section, we dive into the individual-level results to better understand the within-subject response.

In order to describe individual behavior, we first define a very simple type-scheme based on each subject’s choices in the final supergame.21 Specifically, we dichotomize subjects as either Decreasing or Non-Decreasing, where for the Non-Decreasing types we further break out the total fraction that are Stationary. An exact Decreasing-type subject is one whose final supergame cutoffs satisfy $\mu^i_1 > \mu^i_2 \geq \mu^i_3$, where an exact Stationary-type satisfies $\mu^i_1 = \mu^i_2 = \mu^i_3$. In addition to the knife-edge type definitions, we create a parallel family of definitions for $\epsilon > 0$, such that an $\epsilon$-Decreasing type satisfies $\mu^i_1 \geq \mu^i_2 + \epsilon$ and $\mu^i_2 \geq \mu^i_3$, and an $\epsilon$-Stationary type is one that satisfies $|\mu^i_1 - \mu^i_2|, |\mu^i_1 - \mu^i_3| < \epsilon$.22

20Every team chats from all S-Peer sessions are included in Appendix D for interested readers. Example explanations: “As the rounds go on, the chances that the ball the computer is holding has a really small value increases[...] because in previous rounds, if someone had a small value they probably switched and gave it to the computer”; “So here are my thoughts: The chance of you getting a low # that someone else switched out is based on which mover you are and what round it is. Typically I go with ~50 if I am mover 1 or 2 on the first round[...] Then drop down for each subsequent round. Because you get stuck with what you switch too and as time goes on that is much more likely to be a low #”.

21The final supergame represents the point where subjects have maximal experience with the task and where we ramp up the incentive by an order of magnitude, as the final supergame is paid for sure.

22Figure A4.1 in the appendix provides the type fractions as we vary $\epsilon$ from 0 to 10 to provide context on the robustness.
Table 2. Type Proportions

<table>
<thead>
<tr>
<th></th>
<th>$N_S$</th>
<th>Decreasing</th>
<th></th>
<th>Non-Decreasing</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Exact $\epsilon = 2.5$</td>
<td>Exact $\epsilon = 2.5$</td>
<td>Exact $\epsilon = 2.5$</td>
<td></td>
</tr>
<tr>
<td>No Selection</td>
<td>33</td>
<td>3.0%</td>
<td>3.0%</td>
<td>97.0%</td>
<td>97.0%</td>
</tr>
<tr>
<td>Selection</td>
<td>66</td>
<td>42.0%</td>
<td>37.9%</td>
<td>58.0%</td>
<td>62.1%</td>
</tr>
<tr>
<td>Robustness:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>S-Across</td>
<td>60</td>
<td>28.3%</td>
<td>23.3%</td>
<td>71.7%</td>
<td>76.7%</td>
</tr>
<tr>
<td>S-Explicit</td>
<td>36</td>
<td>33.3%</td>
<td>33.3%</td>
<td>66.7%</td>
<td>66.7%</td>
</tr>
<tr>
<td>S-Peer</td>
<td>72</td>
<td>34.7%</td>
<td>30.6%</td>
<td>65.3%</td>
<td>69.4%</td>
</tr>
<tr>
<td>Selection +Robustness</td>
<td>174</td>
<td>37.4%</td>
<td>33.9%</td>
<td>62.6%</td>
<td>66.1%</td>
</tr>
</tbody>
</table>

Table 2 provides the type composition in our No Selection, Selection, and comparable robustness treatments. Focusing on the type definitions with an error band of $\epsilon = 2.5$, we find that all but one subject in our No Selection treatment uses non-decreasing cutoffs, with a large (slim) majority being close to (exactly) stationary. In contrast, pooling across all of our treatments with adverse selection, we find that only a third of subjects use a strongly decreasing cutoff profile. Instead, a slight majority of subjects in our selection treatments are better classified as stationary. Comparing the fraction of decreasing types between Selection and No Selection suggest that about 30 percent of subjects are responsive to the theoretic predictions. However, a much larger fraction of the participants are qualitatively invariant to the key theoretical predictions, and are better classified as using a stationary cutoff across the supergame.

For the S-Simple treatments we collect cutoffs for all three roles in supergame 41. For those treatments, the proportions of Decreasing and Non-Decreasing types are, respectively, 40.3 and 59.7 percent. For consistency with the other treatments, for the S-Simple-Random treatment we perform the type classification closer to supergame 21 using subject-average cutoffs across roles in supergames 18-24. The proportions of Decreasing and Non-Decreasing types in this case are 43.1 and 56.9 percent, respectively. This suggests that the additional supergames do not lead to more Decreasing subjects.

Though we create our type-dichotomy based on behavior in the last supergame (under increased monetary incentives) the assigned types are highly predictive of the dynamic responses in earlier supergames.

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23Given a tendency to select exact multiples of five, an error-band of $\epsilon = 2.5$ has a similar effect to rounding all responses to the nearest multiple of five.

24In Table A.4.1 in the appendix we repeat this exercise for all other treatments, classifying participants using data from the last five supergames excluding the last one. Results are broadly similar to the type classification using only the last supergame.
supergames. In supergames 6 to 20 we elicit cutoffs from subjects using a partial strategy method. For each block of five supergames we can expect to elicit: (i) at least one measurement of the subjects’ cutoff as a first mover, $\mu_{i,1}^j$, from 87 percent of subjects; and (ii) at least one measurement of the subjects dynamic response (regardless of mover role $j$) between rounds 1 and 2, $\Delta \mu_i^j := \mu_{j,1}^i - \mu_{j,2}^i$, from 97 percent of subjects. Collecting these two pieces of choice information (and taking averages where we have multiple data points for a subject in any block of five) we regress each on dummies interacting the subject’s type (classified based on supergame 21 behavior) and an indicator for each block of five supergames. The results are provided in Table 3, where each panel represents a separate regression with standard errors clustered at the subject level.\textsuperscript{25}

Within Table 3, panel (A) provides data on the initial cutoffs, while panel (B) provides data on the changes in cutoffs. Examining panel (A) first, which provides data on the initial response where there is no adverse selection in any treatment, we find that: (i) subjects in the No Selection treatment use cutoffs that are consistent with slightly risk-loving preferences, where the cutoffs increase slightly across the session.\textsuperscript{26} (ii) In contrast to the No Selection results, non-decreasing subjects in Selection treatments use initial first-mover cutoffs that are consistent with risk-neutral preferences, which decrease significantly as the session proceeds. (iii) First-round first-mover cutoffs of the decreasing-type subjects are not significantly different from the risk–neutral PBE prediction in any of the Selection supergame blocks.

Turning to panel (B), which examines the response across the supergame, we find that: (iv) Subjects classified as non-decreasing based on supergame 21 behavior do not have significant changes in their relative cutoffs prior to supergame 21.\textsuperscript{27} This is true in environments both with and without adverse selection. (v) Subjects classified as decreasing in supergame 21 show significant within-supergame decreases in prior supergames, even in blocks 6–10.

From the two panels we first conclude that the type classifications based on supergame 21 are useful for understanding subjects’ behavior in prior supergames. While this result may not be surprising for the non-decreasing subjects, it does speak to the stability of their behavior. For the decreasing types, the results indicate that these subjects understand the qualitative component of the game early on, rather than through extensive experience.\textsuperscript{28}

\textsuperscript{25}For the selection treatments we pool subjects from both Selection and S-Across as the supergames are theoretically identical. We do not include data from S-Explicit, S-Within or S-Simple as the extensive-form games here are distinct, nor do we include data from S-Peer as the type classification is done after the chat rounds.

\textsuperscript{26}The increasing trend across the session is not significant as we have less power in this treatment, where the $\Delta Session$ column provides the estimated change between the first and third block of cutoff-eliciting supergames.

\textsuperscript{27}Non-Decreasing subjects in Selection supergames 16-20 do have a significant decrease in the cutoffs of 0.8. We ignore this as it is both quantitatively small, and insignificant when we look at joint behavior in all three blocks ($p = 0.194$).

\textsuperscript{28}Looking just at the decreasing types, 75-80 percent have a cutoff-difference in excess of 2.5 in each of the prior supergame blocks. Of the 29 subjects with data in all three blocks, 21 are consistently negative in all three.
**Table 3. Behavior by Type and Supergame Block**

(A) Initial Cutoff, $\mu_1$

<table>
<thead>
<tr>
<th>Treatment-Type</th>
<th>Supergame</th>
<th>$\Delta$Session</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>6–10</td>
<td>11–15</td>
</tr>
<tr>
<td><strong>No Selection</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>52.2</td>
<td>54.7</td>
</tr>
<tr>
<td></td>
<td>(2.5)</td>
<td>(2.5)</td>
</tr>
<tr>
<td><strong>Selection</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decreasing</td>
<td>49.2</td>
<td>47.1</td>
</tr>
<tr>
<td></td>
<td>(2.2)</td>
<td>(2.6)</td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>50.2</td>
<td>47.4</td>
</tr>
<tr>
<td></td>
<td>(1.9)</td>
<td>(1.8)</td>
</tr>
</tbody>
</table>

(B) Change in Cutoff, $\Delta\mu$

<table>
<thead>
<tr>
<th>Type</th>
<th>Supergame</th>
<th>$\Delta$Session</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>6–10</td>
<td>11–15</td>
</tr>
<tr>
<td><strong>No Selection</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>-0.2</td>
<td>1.2</td>
</tr>
<tr>
<td></td>
<td>(0.4)</td>
<td>(0.7)</td>
</tr>
<tr>
<td><strong>Selection</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decreasing</td>
<td>6.3 ***</td>
<td>8.5 ***</td>
</tr>
<tr>
<td></td>
<td>(1.4)</td>
<td>(1.3)</td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>0.2</td>
<td>0.2</td>
</tr>
<tr>
<td></td>
<td>(0.4)</td>
<td>(0.3)</td>
</tr>
</tbody>
</table>

*Note: Figures in parentheses represent standard errors (from 417/463 total observations in panels A/B, with 159/153 subject clusters). Significance at the following confidence levels: * - 90 percent; ** - 95 percent; *** - 99 percent. Significance tests for the initial cutoff $\mu_{1,1}$ are assessed against the PBE prediction of $H_0 : \mu_{1,1} = 51$. Significance tests for the change in cutoff $\Delta\mu_{1,2}$ are assessed against the stationary null $H_0 : \Delta\mu_{1,2} = 0$. (N.B. The appropriate null for the PBE prediction is $H_0 : \Delta\mu_{1,2} = 11.33$, as subjects have an equal chance of being a first/second/third mover. Except for Decreasing types in 16–20 every single coefficient is significantly different from this level with at most $p = 0.04$.)

The treatment suggest that approximately a quarter of subjects are capable of explaining the game’s dynamic adverse selection mechanics to others. The conclusion is that the minority of decreasing subjects introspectively understand the environment.
Second, the results suggest that the non-decreasing subjects become more pessimistic over their stationary cutoffs as the Selection sessions proceed. But this pattern does not show up in the No Selection treatments without adverse selection. Indeed, if anything, they act as if becoming more optimistic about the outside option as the No Selection treatment proceeds. What then is going on?

Before diving into our behavioral models, we shift briefly to standard explanations for how an equilibrium “as if” could work without an introspective understanding of the dynamic game. Through repeated exposure to the environment, a subject should be able to build a direct understanding of how their resulting value $V$ depends upon their decision to keep or switch based on the cutoff $\mu$, the value of their initially held ball $\theta_0$, on the rematching outcome $\theta_R$, and on the time $t$ at which they make a decision. Through access to a limitless amount of data, the agent would be able to choose $\mu$ to decide on whether or not to keep or switch out $\theta_0$ at time $t$ by considering the joint probability for $(\theta_0, \theta_R, \mu, t)$ as:

$$\Pr\{\theta_0\} \cdot \Pr\{t\} \cdot \Pr\{\mu|t\} \cdot \Pr\{\theta_R|t\} \cdot \Pr\{V|\mu, \theta_R\}.$$ 

Given our game’s simple structure (where the final outcome $V$ is either $\theta_0$ or $\theta_R$, depending on the cutoff $\mu$) the optimal strategy can be learned solely by obtaining a good estimate of $E(\theta_R|t)$. If this conditional expectation is learned, a risk-neutral subject would use a cutoff that switches out any initial assignment $\theta_0 < \mu_t := E(\theta_R|t)$. In the steady-state, any subject that switches out their assigned ball with some probability must build an accurate estimate of $E(\theta_R|t)$ through sample analogs. Given that participants will always want to switch out the lowest-value balls (and making a choice at each time period $t$ is independent) all participants should converge to the PBE strategy in the steady-state. If the subject understands (or is open to the possibility) that values should be conditioned on an informative observable (here time), their end actions will be as if they fully internalize the initial distributions, strategies of others, etc.

While each conditional expectation can be learned by looking at a large enough subsample of past play, understanding that conditioning is required is less easy to learn. In some ways this is analogous to a failure to include a relevant regressor in an econometric model, with which many economists can probably exhibit more sympathy. One of the main findings from our experiments is that a majority of subjects are entirely insensitive to time after 20 supergames. In stark contrast to this, those subjects that do condition on time seem to do so from an introspective understanding,

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29Given the between-subject identification across the sessions, we should clarify that about 30 percent of the subjects classified as non-decreasing in No Selection would be expected to be decreasing types were they counterfactually placed in our Selection environment. However, we cannot identify whether these sophisticated subjects end up being risk-averse/neutral/loving in their No Selection decision.

30Every participant arrives at every information set in the game with positive probability, the distribution has full support over $\Theta$ at each information set, where it is a strictly dominant strategy to switch out 1 balls.
as their responses are decreasing very early on within the session, where what changes across
the session is the extent of the decrease. Moreover, beyond their choice behavior, our S-Peer
treatment provides evidence that decreasing-type subjects can explain to others why they need to
condition on time.

While our experiments do indicate that many subjects are entirely unresponsive to time, we do
find evidence that the non-decreasing subjects are responsive to their past experience (details be-
low). Subjects who experienced many good rematching outcomes seem more optimistic in their
supergame 21 cutoffs than those that experienced many bad outcomes. Below, we outline two
alternative behavioral predictions in our environment, under the assumption that subjects maintain
a model of the world that is stationary. The two stationary models each remove time as a condi-
tioning variable in forming expectations. The difference across the two models is the referent
feedback variable that is used to form expectations. In our first model we have participants learn
the unconditional value of a draw from the rematching pool. In our second model, participants are
instead learning about the final supergame outcome, and using the learned expectation of this to
make their decisions. Below, we describe the predictions made by both models in the steady state,
and go on to pick among these two models for the stationary types. After doing so, we test the
resulting behavioral model out-of-sample with two additional experimental treatments designed to
isolate our learning channel.

Maintained (but Wrong) Models of the World: Two Alternative Boundedly Rational Mod-
el.

Our first behavioral model for simplicity assumes that all participants ignore time and use a
stationary cutoff $\mu$ across the supergame, giving up objects of lower value and keeping objects of
equal or higher value. Given our type results, and in the spirit of models like Cursed Equilibrium,
the exportable behavioral model we are describing would ideally allow for a $\lambda$ proportion of so-
phisticated types, and a $1 - \lambda$ proportion of behavioral types that are invariant to the conditioning
variable. However, in our particular setting, the proportion $\lambda$ does not have any substantial effect
on the cutoffs used by the behavioral types in our specific setting, nor vice versa. For simplicity of
presentation we therefore focus on the simple cases with $\lambda = 0$.

Though the rematching value will vary if the subject (correctly in the Selection treat-
ments) conditions on time, the unconditional value of rematching has a marginal probability distribution
$\Pr \{ \theta_R; \mu \}$ that varies with the cutoff $\mu$. This marginal probability factors in the degree of selection

31The best predictor for being classified as a decreasing type in the selection treatments is score on the cognitive
reflection task (CRT). Marginal effects from a probit estimate suggest each correct answer on the 3-question CRT
increases the likelihood of being classified as a decreasing type by 23 percent ($p = 0.000$). In contrast, while there are
predictive effects from other covariates—gender (lower probability for females), risk-aversion (increased probability
with greater risk aversion) and behavior in a Monty Hall problem (increased probability for those with a sophisticated
response)—these covariates are only marginally significant with $p$-values in the $0.069 - 0.079$ range.
at each point in time weighted by the likelihood the agent moves at that point.\footnote{Given the symmetry from all subjects using a stationarity cutoff strategy $\mu$, the rematching outcome in the Selection environment can be written through its CDF as $\Pr \{ \theta_R \leq x; \mu \} = \pi(\mu) \cdot F(x) + (1 - \pi(\mu)) \cdot F(x | x < \mu)$ where $\pi(\mu)$ is the ex-ante likelihood a participant faces an unselected rematching ball.} Defining $\nu_{\theta R}(\mu)$ as the expected value of $\theta_R$ fixing $\mu$, the steady-state theoretic cutoff for a stationary type slowly learning about the value of rematching can be found by solving for a fixed point $\bar{\mu}_R = \nu_{\theta R}(\bar{\mu}_R)$.

For our Selection treatment the stationary equilibrium cutoff is $\bar{\mu}_R = 36$. A subject believing the rematching pool was entirely stationary would give up objects of value 35 and below, and would on average get back rematched objects of value 35.\footnote{Given our finite qualities the exact equilibrium solution actually requires a mixture that randomizes between a cutoff of 36 nine-tenths of the time and 35 one-tenth of the time. The effective difference with a pure-strategy cutoff is minimal, and so we simply report the modal cutoff where mixing is required.} In contrast, for the No Selection treatments the steady-state cutoff for the first behavioral model is exactly the PBE prediction, where the stationary model is correctly specified in this setting. A cutoff of 51 is predicted, where objects of value 50 and below are given up, and replaced by objects with an average value of 50.5.

The predictions for this boundedly rational model rely on (i) subjects holding constant a belief that the rematching pool does not change within the supergame; and (ii) that the decision-relevant learning is focused on understanding the expected value of rematching. Our experiments provide supportive evidence for both points. First, the majority of subjects are stationary, using a static response where they would do better with a decreasing cutoff. Second, there is evidence that subjects do respond to their idiosyncratic rematching experiences. Using the non-decreasing subjects’ final supergame cutoffs as the dependent variable, we find a significant relationship to the subjects’ idiosyncratic rematching experiences.\footnote{For each subject classified as non-decreasing in supergame 21, we compile their average experienced rematching outcome in supergames 1–10 and 11–20 as $\hat{\nu}_{G,10}^1$ and $\hat{\nu}_{G,20}^1$, respectively. Using the 87 $\epsilon$-non-decreasing subjects in Selection and S-Across we find the following regression prediction for their first cutoff in supergame-21 (standard errors in parentheses):}

\begin{equation}
\mu_{21} = 35.7_{(4.1)}^{***} + 0.267_{(0.087)}^{**} \cdot \hat{\nu}_{10}^G + 0.036_{(0.074)}^{*} \cdot \hat{\nu}_{20}^G.
\end{equation}

Despite this qualitative evidence, there are two reasons to be cautious about the stationary model based on the expectation of rematching. First, while the fact that subjects’ later behavior is related to their experienced rematching outcomes points towards the learning mechanics in the behavioral model, the precise channel is not pinned down. In particular, another correlated measure of experience, namely the final outcome instead of the rematching outcome, has a much stronger statistical
relationship to end-of-session choices, a point we come back to when we present the second behavioral model. Second, and perhaps more important, the overall cutoff levels chosen by subjects in the experiments are quantitatively distinct from the model’s predictions.

While subjects *experience* rematching values of 35.7 in the later half of the *Selection* sessions, the average supergame-21 cutoff chosen by the stationary-type subjects was 44.6. Similarly, the average experienced rematching outcome was 49.5 in the *No Selection* treatment, but the average final cutoff of the non-decreasing types was 54.7 (where Table 3 shows that this response *increases* across the session). Both treatments therefore indicate that behavior is significantly *above* the risk-neutral prediction made by the stationary-rematching model.

Risk-loving preferences could rationalize this behavior. However, in a separate elicitation our non-decreasing subjects are revealed to be risk averse. The median non-decreasing subject’s elicited CRRA coefficient is $\hat{\rho}_{0.5} = 0.73$ (with an interquartile range of $[0.44, 0.93]$) indicating substantial risk aversion. The steady state prediction for the expected rematching value (median risk parameter) is therefore even lower at 16 (with predictions of 24 and 11 for the respective interquartile points).

A possible explanation is that the non-decreasing subjects (or a large fraction of them) are responding to a different expectation. An alternative model (and misspecified for all of our treatments) is that participants lump together the distributions both of the objects they are initially assigned to and the objects to which they are rematched. Instead of comparing the expected outcome from rematching to their current object, they instead compare it to their expected outcome from the overall game.

To make clear the distinction, in the first behavioral model the decision-maker compares the held object $\theta_0$ to a draw of $\theta_R$, and gives up objects that have lower values than their experienced rematching value. In the second misspecified model, the decision-maker continues to believe the world is stationary, but instead compares their held object to their experienced supergame values $V$. Agents under this model give up objects that have lower value than the average supergame value, keeping all others. If all participants use a stationary cutoff $\bar{\mu}$, the final supergame *outcome* is drawn from the following CDF:

\[
\Pr \{ V \leq x; \bar{\mu} \} = \Pr \{ \text{Switch}; \bar{\mu} \} \cdot \Pr \{ \theta_R \leq x; \bar{\mu} \} + \Pr \{ \text{Keep}; \bar{\mu} \} \cdot F(x \mid x \geq \bar{\mu}).
\]

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35 Appendix Table A5.1 contains regressions results using both rematching and final outcomes as independent variables, as well as subjects’ risk aversion parameters. As mentioned in the text, the results point to a more prominent role of final outcomes.

36 Similarly, for the *No Selection* treatment the elicited risk aversion predicts a steady-state cutoff of 30 (40 to 22 for the interquartile range), where we instead observe cutoffs significantly greater than the risk-neutral prediction.

37 Again, the exportable model to other settings would allow for a fraction $\lambda$ of sophisticated types that condition on time. We again omit this for simplicity of presentation as there are no quantitative effects for the behavioral types in our setting.
The referent distribution is therefore a mixture between the stationary rematching distribution from our first behavioral model and the outcome distribution for objects that were found acceptable.\textsuperscript{38}

The steady-state prediction from this model is the fixed-point solution $\bar{\mu}_V$ to the equation $\bar{\mu}_V = \nu_V(\bar{\mu}_V)$, where $\nu_V(\bar{\mu})$ is the expected value of $V$ under $\bar{\mu}$.

Given that supergame outcomes reflect a choice process that retains good initial outcomes and replaces bad ones, the steady-state predictions using $V$ as the referent instead of $\theta_R$ are distinct. In the Selection game, the risk-neutral solution to the rematching model is to use a stationary cutoff of 36; but in the second outcome model the stationary cutoff is instead 61.\textsuperscript{39}

The risk-neutral prediction for the boundedly rational model where $V$ is the referent against which $\mu$ is chosen also misses the observed aggregate levels at the end of our experimental sessions. However, subjects’ elicited risk aversion now provides a compatible explanation. In Figure 4, we provide the steady-state prediction for the final-outcome behavioral model as we increase risk aversion. Parameterizing risk with a CRRA specification, Figure 4 varies the coefficient of relative risk aversion $\rho$ on the horizontal axis from risk neutral ($\rho = 0$) to very risk averse ($\rho = 1$, logarithmic preferences). The vertical axis then indicates the predicted cutoff from the stationary model where the unconditional distribution of $V$ is the referent used to optimize $\mu$. The solid line indicates the stationary cutoff prediction in our Selection treatments, while the dashed line indicates the prediction in the No Selection treatment.

Superimposed on the theoretical predictions for the cutoff, the figure illustrate the distribution of risk parameters among the non-decreasing type subject population (obtained from our DOSE elicitation, pooled across Selection, No Selection and S-Across). The interquartile range for stationary subjects’ risk parameters, $[\hat{\rho}_{0.25}, \hat{\rho}_{0.75}]$, is shown as the gray band, where the median $\hat{\rho}_{0.5}$ is indicated with a vertical line. The average final cutoffs for non-decreasing subjects in supergame 21 is then shown as the horizontal lines labeled $\hat{\mu}_{\text{Sel}}$ and $\hat{\mu}_{\text{NoSel}}$ for the Selection and No Selection treatments, respectively.

Using the median risk coefficient of $\hat{\rho}_{0.5} = 0.73$, the behavioral model where learning occurs on the final outcome $V$ squares the conflict between highly risk-averse preferences in our risk elicitations with the risk-loving behavior in the No Selection treatments. At the median level of elicited risk aversion, our value-based behavioral model predicts a steady-state cutoff of $\bar{\mu}_V = 53$

\textsuperscript{38}As a metaphor consider a worker considering quitting her current employer to look for a new job. The rational agent would look for information on the wage outcomes for similar workers to her (years of experience, field, education, etc) that switched. The rematching model removes the conditioning on the similar situation, having the worker look at all outcomes for everyone that quit and rematched. Our final model has the agent looks to the total population for the referant outcome, with the worker comparing her current situation to everyone else.

\textsuperscript{39}Analogously, the risk-neutral prediction in the No Selection treatment increases from 51 in the stationary rematching model (in this case correctly specified) to 69 in the alternative (misspecified) model where subjects are learning about and comparing to average supergame outcomes.
in the No Selection treatment and $\bar{\mu}_V = 43$ in the Selection treatments, which compares well with the observed average supergame-21 cutoff of 53 and 45.7, respectively.\footnote{For the interquartile range of risk parameters $[0.44, 0.93]$ the steady-state prediction of the behavioral model predicts values of $\mu_V$ between 60 and 46 for the No Selection treatment (subject data is 60.0 to 47.8) and between 52 and 36 for Selection (subject data is 51 to 40).}

The stationary behavioral model that has subjects comparing their drawn objects to a referent distribution based on final outcomes does well at predicting aggregate outcomes in both treatment and control, once risk aversion is taken into account. However, while this behavioral model fits both treatments, we should note that we did not consider this model prior to conducting our main experiments. Instead our arrival at it came about as an ex-post description of the aggregate behavior. The model requires two distinct features to accurately predict levels: that the subjects’ form expectations over final outcomes, not rematching outcomes (this was non-obvious to us at the start of the study); and that risk aversion modifies the cutoff from the risk-neutral level (this was anticipated, hence the risk elicitations).

One way of addressing the post hoc nature of the model is to look at subject-level variation. The model was formed from an examination of aggregate behaviors, and so individual-level variation can help identify the two main mechanics. In the appendix (Table A.5.1) we provide regressions that do just that, where we show that non-decreasing subject’s final supergame 21 cutoffs vary in the predicted directions both with their idiosyncratic experienced final outcomes in prior supergames, and with their elicited risk aversion.

\footnote{For the interquartile range of risk parameters $[0.44, 0.93]$ the steady-state prediction of the behavioral model predicts values of $\mu_V$ between 60 and 46 for the No Selection treatment (subject data is 60.0 to 47.8) and between 52 and 36 for Selection (subject data is 51 to 40).}
Individual heterogeneity therefore corroborates the second behavioral model’s two required components. However, a strong test requires us to test the model out of sample. An even stronger test has us move out of sample to a distinct environment.

**Out-of-sample Tests.** To test our post hoc behavioral model we conducted two further robustness treatments, labeled *S-Simple-Random* and *S-Simple-Fixed*. These treatments were planned and conducted after formulating the model (more specifically, after circulating our first working paper). The treatments’ three aims were to: (i) simplify the environment; (ii) double the number of supergame repetitions (made feasible through the simplification); and (iii) vary the feedback on *V* that specific subjects obtained, altering the behavioral but not the PBE prediction. In achieving these aims the new treatments provide a strong examination not only of the behavioral model’s predictive power, but also a validation of the key learning mechanism underlying the model.

The *S-Simple* environment has three movers act sequentially, each knowing for sure that previous movers made a decision (effectively making the probability of observing the held ball’s value certain). The risk-neutral PBE prediction for the environment has the first mover use a cutoff of 51, the second mover 32, and the third mover 22. The total reduction in the expected rematching outcome across the entire supergame is therefore quantitatively similar to our *Selection* environment, but with the decrease happening more quickly.

In the *S-Simple-Random* treatment 72 subjects play 40 supergames where the first/second/third mover role is randomly assigned in each new supergame. In the *S-Simple-Fixed* treatment a further 72 subjects are randomly assigned one of the three roles at the start of the session, playing that mover’s role 40 times (always the first/second/third mover). In supergame 41 they are then asked to provide cutoffs for each role using a similar strategy method to supergame 21 in our previous sessions. Dichotomizing subjects into *Decreasing/Non-decreasing* by their supergame 41 behavior we find that 46 percent are decreasing in the random-role treatment, and 33.3 percent given fixed roles.

Our out-of-sample behavioral prediction relates to the behavior of the non-decreasing types. Specifically, the boundedly rational model based on a supergame referent predicts a cutoff of 43 (under $\rho = 0.73$) for all participants in the *S-Simple-Random* treatment. This prediction balances out the (equally likely) chance of being a first, second or third mover in this treatment. In contrast, for the fixed-roles treatment the subject only experiences supergame outcomes *V* from a single role.

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41 A cutoff of 51 for a first mover in the first round; 31 for a second-mover in round two, and 21 for a third mover in the last round.

42 See also Fudenberg and Vespa (2018) for an investigation of the effect of experiencing different roles in a signalling game.

43 Note the slight asymmetry in supergame 41: in *S-Simple-Random* we ask for strategies over roles the subject experienced; in *S-Simple-Fixed* this question asks them to make decisions out of their previous experimental context.
### Table 4. Supergame 40 Cutoffs: Out-of-Sample Model Test

<table>
<thead>
<tr>
<th>Data</th>
<th>Treatment</th>
<th>Mover</th>
<th>Joint Tests</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>First, $\hat{\mu}_1$</td>
<td>Second, $\hat{\mu}_2$</td>
</tr>
<tr>
<td>All Subjects</td>
<td>Fixed</td>
<td>50.2</td>
<td>43.0 ***</td>
</tr>
<tr>
<td></td>
<td>Random</td>
<td>44.9 **</td>
<td>41.6 ***</td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>Fixed</td>
<td>49.1 **</td>
<td>41.5 **</td>
</tr>
<tr>
<td></td>
<td>Random</td>
<td>41.5 **</td>
<td>38.8</td>
</tr>
</tbody>
</table>

Note: Significantly different from PBE prediction $\mu^* = (51, 32, 22)$ at the following confidence levels: * - 90 percent; ** - 95 percent; *** - 99 percent. Significantly different from relevant behavioral prediction $\bar{\mu}_{V}^{\text{Fix.}} = (53, 42, 37)$ or $\bar{\mu}_{V}^{\text{Rdm.}} = (43, 43, 43)$ at the following confidence levels: * - 90 percent; ** - 95 percent; *** - 99 percent. Joint tests are Wald tests of, respectively: (i) $\mu^* = \bar{\mu}$; (ii) $\bar{\mu}_{V}^{\text{Rdm.}} = \bar{\mu}$; (iii) $\bar{\mu}_{V}^{\text{Fix.}} = \bar{\mu}$; and stationarity $\hat{\mu}_1 = \hat{\mu}_2 = \hat{\mu}_3$.

As such the same equilibrium model predicts different cutoffs: 53 for the first mover, 42 for the second mover and 37 for the third mover.

In Table 4 we examine supergame 40 cutoffs of subjects in the two treatments through a joint regression, where we include dummies for all interactions of role type and treatment (and so exclude the constant). In the first two rows we report the regression results for all 144 subjects. In the last two rows we present the results from a separate regression where we only include the non-decreasing subjects (based on their supergame 41 cutoffs, with $\epsilon = 2.5$). For each estimated coefficient we test whether it is significantly different from the PBE prediction, and from the relevant behavioral prediction $\bar{\mu}_{V}$. Moreover, in the last four columns we present joint tests that the three coefficients represent: (i) the PBE prediction; (ii) the behavioral prediction based on supergame outcomes for the random-role treatment; (iii) the same behavioral prediction for the fixed-role treatment; and (iv) a stationary response across roles.

The results validate the final-outcome behavioral model both quantitatively and qualitatively. For the quantitative levels, and focusing on the joint tests, we reject the PBE prediction with 99 percent confidence in all tests. Similarly, we can reject the other treatment’s behavioral prediction with at least 95 percent confidence. In contrast, we cannot reject the relevant behavioral prediction $\bar{\mu}_{V}$ at conventional significance levels. Moreover, focusing just on the non-decreasing subjects for whom we are targeting the behavioral prediction, we fail to reject in all six individual tests.

Qualitatively, our two treatments are predicted to have distinct outcomes. Though all of the non-decreasing subjects exhibit a failure to understand how the roles affect the expected value in the final supergame, we observe distinct behavior across roles in the two treatments. In the fixed role treatments, the non-decreasing subjects (randomly assigned to each role) have significantly

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44We focus here on supergame 40 as the last-round behavior as supergame 41 is used to classify participants.
different cutoffs from one another, and we can reject stationarity across roles with 99 percent confidence. In contrast, in the treatment with random roles, subjects taking on each role in supergame 40 (again, randomly assigned) do not have distinct cutoffs from one another, and we fail to reject stationarity. As such, this represents a strong test of the model’s mechanism. We summarize the result as follows:

**Result 4 (Behavioral Model).** Long-run behavior for participants who do not introspectively understand the game can be modeled with a boundedly rational steady state model that uses unconditional final outcomes as the referent expectation.

### 7. Conclusion

In this study, we use a novel experimental design that implements a common-value matching environment. The environment sets up a dynamic adverse selection problem, similar in its strategic tensions to those present in labor markets, housing markets, and mating markets, among others. The main prediction is that subjects adapt to the dynamic adverse selection by conditioning their responses on time, where the particulars of our environment make this prediction robust to risk preferences and the quantitative response of others. Moreover, as our environment is sequential, conditioning variables are experienced rather than hypothetical, and the strategic thinking in our environment is entirely backwards-looking, previous work would make us optimistic for equilibrium. However, while a substantial minority do respond to the adverse selection, the majority fail to adjust their valuations over time, maintaining a stationary response in an evolving setting.

A number of additional treatments show the results are robust to changes in the environment. Three robustness treatments increase the provision of feedback (providing strategic information both across and within supergames; making the selection channel explicit), while a fourth treatment allows for peer feedback. These treatments further underscore the result that moving from a simultaneous setting to a sequential one where the conditioning event is experienced is not sufficient to attain equilibrium-like behavior. Taken together with the previous experimental literature, our results suggest that sequentiality must also remove uncertainty about the correct conditioning (see Martínez-Marquina et al. 2017 for a complementary result in a simultaneous setting without uncertainty). Further, while our sophisticated minority are able to clearly explain the adverse-selection mechanism to their peers, very few of the stationary subjects actually understand their advice, and instead stick with their previous strategy.

While the modal stationary response do place a cloud over equilibrium predictions for our dynamic environment, we do see some silver-lining. Our sophisticated minority seem to understand the equilibrium introspectively. Their valuations are decreasing with time from the first supergames
that we can observe this response; and the fact that they can explain the game’s selection mechanism to others speak to their deeper understanding. When we think of professionals operating in dynamic markets—for example in finance, insurance, and labor markets—selection forces would seem to make the behavior of our sophisticated minority more representative.

Outside professional settings where expert decision-makers are likely to introspectively understand the strategic forces, our results point to the need for alternative theoretical models.\(^\text{45}\) and/or the potential for informational nudges on the need to condition (for example, ). For example, in non-professional setting such as finding a mate in matching markets, or consumers in durable-good markets. In our discussion section we consider a boundedly rational model that can help explain long-run outcomes. In this steady-state learning model, subjects learn the overall expectation of the supergame, which they use to make their (stationary) decisions. Two new treatments provide an out-of-sample test for this behavioral model’s long-run prediction, where the results provide a strong validation.

In two simplified version of our original setup, we vary whether subjects experience all conditioning outcomes, or just one. Consistent with the predictions of the behavioral learning model, we find that subjects who only experience outcomes at a fixed point in time have a starkly different responses to those who experience outcomes at a random point in time, matching both the directions and levels predicted by the behavioral model. Future work using variations of our experimental setting can continue to probe for patterns and regularities in subjects’ maintained models, thereby sharpening the power of behavioral theoretic predictions as a complement to classical ones.

\(^{45}\)The results also point to the power of informational nudges. For example, Hanna et al. (2014) show that seaweed farmers fail to optimize production even when given all the relevant data, but pointing out the relevant conditioning variable corrects the problem. Behavioral models are useful here too, as they can help us understand whether the correcting nudge produces societal benefits. For example, in a CV audit study Bellemare et al. (2018) document a failure to condition on unemployment durations when selecting candidates. Correcting this non-response might have a social cost if planners aims are the reduction of persistant unemployment.
REFERENCES


Enke, Benjamin, “What you see is all there is,” July 2017.


Gershkov, Alex and Motty Perry, “Can Relaxation of Beliefs Rationalize the Winner’s Curse?: An Experimental Study,” Econometrica, 2010, 78 (4), 1435–1452.


A.1. **Invariance to quantitative beliefs.** Figure A1.1 shows the effect of the cutoffs in round one for the second and third mover on the best-response cutoffs for the first mover in round two. In particular the figure indicates the difference in best response between the first and second round, so $51 - \mu_{1.2}(\mu_{2.1}, \mu_{2.3})$. For most second- and third-mover responses close to the equilibrium the best response is identical. Even with substantially different beliefs about the cutoffs, the first mover should still have a difference in their first and second cutoffs of at least 10. The sole exceptions are those cases where the second and third movers are believed to use boundary cutoffs, either always or never switching.

**Figure A1.1.** Differences for First Mover Cutoffs (Round one to two) as a Function of Beliefs on Others’ Cutoffs
A.2. **Experimental Design.** Overall allocation of subjects to session is given in Table A2.1.

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Type</th>
<th>Sessions</th>
<th>Subjects</th>
<th>Supergames</th>
</tr>
</thead>
<tbody>
<tr>
<td>Selection</td>
<td>Game</td>
<td>4</td>
<td>66</td>
<td>1,386</td>
</tr>
<tr>
<td>No Selection</td>
<td>Decision</td>
<td>2</td>
<td>33</td>
<td>693</td>
</tr>
<tr>
<td>S-Across</td>
<td>Game</td>
<td>4</td>
<td>60</td>
<td>1,260</td>
</tr>
<tr>
<td>S-Within</td>
<td>Game</td>
<td>4</td>
<td>69</td>
<td>1,446</td>
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<tr>
<td>S-Explicit</td>
<td>Decision</td>
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<td>36</td>
<td>756</td>
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<td>S-Peer</td>
<td>Game</td>
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<tr>
<td>S-Simple-Random</td>
<td>Game</td>
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<td>72</td>
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<tr>
<td>S-Simple-Fixed</td>
<td>Game</td>
<td>4</td>
<td>72</td>
<td>2,952</td>
</tr>
<tr>
<td><strong>All</strong></td>
<td></td>
<td>28</td>
<td>480</td>
<td>12,960</td>
</tr>
</tbody>
</table>

**Figure A2.1.** Experimental Design Table
A.3. **Robustness.** This appendix contains tables and figures that extend the analysis in the main text. Table A3.1 reports the same regressions as in Table 1 in the main text, but using data from supergames 6 to 20 instead of supergames 11 to 20. Table A3.2 extends the same analysis for players in the role of second- and third-movers, and finds very similar results. Table A3.3 replicates the analysis from Table 1 for the *S-Across* treatment, and Table A3.4 extends the analysis to include data from supergames 6 to 20. Tables A3.5 and A3.6 do the same for the *S-Explicit* treatment.

Table A3.7 reports results from the *S-Within* treatment, in which subjects are informed about the actions taken by other players during the course of the supergame. In the *S-Within* treatment, the relevant conditioning variable should be the information that other people switched, and the passing of time per se does not convey direct actionable information. Table A3.8 extends the analysis to include data from supergames 6 to 20.

Tables A3.9 and A3.10 report results of supergame 21 behavior for subjects in the *S-Within* and *S-Peer* treatments, respectively. For both cases, we cannot reject the hypothesis that cutoffs are similar to the *Selection* treatment. We conclude that neither receiving strategic feedback during the path of play, nor discussing the optimal strategy with peers significantly affects behavior in supergame 21.
Table A3.1. Average Cutoff per Round for No Selection and Selection Treatments, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 6 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td>Test (p-Value)</td>
</tr>
<tr>
<td>No Selection</td>
<td>Round 1, $\hat{\mu}_1^{NS}$ [51]</td>
<td>$\mu^*$</td>
<td>$\hat{\mu}$</td>
<td>$\hat{\mu}_1^{NS} = \hat{\mu}_2^{NS} = \hat{\mu}_3^{NS}$</td>
</tr>
<tr>
<td>(Control)</td>
<td>Round 2, $\hat{\mu}_2^{NS}$ [51]</td>
<td></td>
<td>54.13</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.40)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^{NS}$ [51]</td>
<td></td>
<td>53.53</td>
<td>0.249</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.42)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td></td>
<td>0.197$^\dagger$</td>
<td>0.201$^\S$</td>
</tr>
<tr>
<td>Selection</td>
<td>Round 1, $\hat{\mu}_1^S$ [51]</td>
<td></td>
<td>46.92</td>
<td>0.007</td>
</tr>
<tr>
<td>(Treatment)</td>
<td>Round 2, $\hat{\mu}_2^S$ [35]</td>
<td></td>
<td>(1.20)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>43.28</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(1.23)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^S$ [28]</td>
<td></td>
<td>38.92</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(1.30)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td></td>
<td>0.000$^\dagger$</td>
<td>0.000$^\S$</td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 171/138/33 Total/Selection/No Selection first-mover subjects across supergames 6-20, and 55/22/33 in supergame 21. Selection treatment exclude subjects in the second- and third-mover roles. $^\dagger$—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0: \hat{\mu}_1^j = \hat{\mu}_1^{NS}$ for treatment $j$, round $t$) and the theoretical prediction ($H_0: \hat{\mu}_1^j = \mu_1^j$). $^\S$—Joint test of stationary cutoffs across the supergame ($H_0: \hat{\mu}_1^j = \hat{\mu}_2^j = \hat{\mu}_3^j$ for treatment $j$); $^\S$—Joint test of PBE cutoffs in supergame ($H_0: 0 = \hat{\mu}_1^j - \mu_1^j = \hat{\mu}_2^j - \mu_2^j = \hat{\mu}_3^j - \mu_3^j$).
Table A3.2. Average Cutoff per Round for *Selection* Treatments, Second- and Third-Movers

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 11 to 20</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td>Test (p-Value)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Selection, Second-Movers</td>
<td>Round 1, $\hat{\mu}_1^S$</td>
<td>[42]</td>
<td>$\hat{\mu}$</td>
<td>44.64</td>
<td>0.000</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_1^N$</td>
<td>46.09</td>
<td>0.066</td>
<td>0.145</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_2^N$</td>
<td>40.36</td>
<td>0.001</td>
<td>0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_3^N$</td>
<td>35.18</td>
<td>0.000</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td>$\hat{\mu}$</td>
<td>0.000$^\dagger$</td>
<td>0.000$^\dagger$</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Selection, Third-Movers</td>
<td>Round 1, $\hat{\mu}_1^S$</td>
<td>[35]</td>
<td>$\hat{\mu}$</td>
<td>43.79</td>
<td>0.000</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_1^N$</td>
<td>45.50</td>
<td>0.030</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_2^N$</td>
<td>41.95</td>
<td>0.001</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}_3^N$</td>
<td>36.82</td>
<td>0.000</td>
<td>0.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td>$\hat{\mu}$</td>
<td>0.000$^\dagger$</td>
<td>0.000$^\dagger$</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 133/138 second-/third-mover subjects across supergames 11-20, and 22 second- and third-movers in supergame 21. $^\dagger$—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0: \hat{\mu}_1^j = \hat{\mu}_1^N$ for treatment $j$, round $t$) and the theoretical prediction ($H_0: \hat{\mu}_1^j = \mu_1^j$). $^\ddagger$—Joint test of stationary cutoffs across the supergame ($H_0: \hat{\mu}_1^j = \hat{\mu}_2^j = \hat{\mu}_3^j$ for treatment $j$); $^\S$—Joint test of PBE cutoffs in supergame ($H_0: \hat{\mu}_1^j - \mu_1^j = \hat{\mu}_2^j - \mu_2^j = \hat{\mu}_3^j - \mu_3^j$).
### Table A3.3. Average Cutoff per Round for S-Across Treatment, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 11 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td>Test (p-Value)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\mu^*$</td>
<td>$\hat{\mu}$</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>S-Across</td>
<td>Round 1, $\hat{\mu}_1^{S(A)}$</td>
<td>[51]</td>
<td>49.26</td>
<td>0.082</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}_2^{S(A)}$</td>
<td>[35]</td>
<td>45.37</td>
<td>0.034</td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^{S(A)}$</td>
<td>[28]</td>
<td>41.30</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td></td>
<td>0.000$^\dagger$</td>
<td>0.000$^\dagger$</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 60 first-mover subjects across supergames 11-20, and 20 first-movers in supergame 21. $^\dagger$—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0: \hat{\mu}_j^i = \hat{\mu}_{11}^{NS}$ for treatment $j$, round $t$), the first-mover coefficients from the Selection treatment ($H_0: \hat{\mu}_j^i = \hat{\mu}_{12}^{S}$ for treatment $j$, round $t$) and the theoretical prediction ($H_0: \hat{\mu}_j^i = \mu_j^{*i}$). $^\dagger$—Joint test of stationary cutoffs across the supergame ($H_0: \hat{\mu}_1^i = \hat{\mu}_2^i = \hat{\mu}_3^i$ for treatment $j$); $^\dagger$—Joint test of PBE cutoffs in supergame ($H_0: 0 = \hat{\mu}_1^i - \mu_j^{*i} = \hat{\mu}_2^i - \mu_j^{*i} = \hat{\mu}_3^i - \mu_j^{*i}$).
TABLE A3.4. Average Cutoff per Round for S-Across Treatment, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 6 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td>Test (p-Value)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>$\hat{\mu}$</td>
<td>$\hat{\mu}<em>1 = \hat{\mu}</em>{NS}$</td>
</tr>
<tr>
<td>S-Across</td>
<td>Round 1, $\hat{\mu}_1^{S(A)}$</td>
<td>[51]</td>
<td>49.59 (1.90)</td>
<td>0.150</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}_2^{S(A)}$</td>
<td>[35]</td>
<td>46.40 (1.94)</td>
<td><strong>0.015</strong></td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^{S(A)}$</td>
<td>[28]</td>
<td>41.66 (2.02)</td>
<td><strong>0.000</strong></td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td></td>
<td><strong>0.000</strong></td>
<td><strong>0.000</strong></td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 60 first-mover subjects across supergames 6-20, and 20 first-movers in supergame 21. †—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0: \hat{\mu}_1^j = \hat{\mu}_{NS}^j$ for treatment $j$, round $t$), the first-mover coefficients from the Selection treatment ($H_0: \hat{\mu}_1^j = \hat{\mu}_{S}^j$ for treatment $j$, round $t$) and the theoretical prediction ($H_0: \hat{\mu}_1^j = \mu_1^j$). ‡—Joint test of stationary cutoffs across the supergame ($H_0: \hat{\mu}_1^j = \hat{\mu}_2^j = \hat{\mu}_3^j$ for treatment $j$); §—Joint test of PBE cutoffs in supergame ($H_0: 0 = \hat{\mu}_1^j - \mu_1^j = \hat{\mu}_2^j - \mu_2^j = \hat{\mu}_3^j - \mu_3^j$).
### Table A3.5. Average Cutoff per Round for S-Explicit Treatment

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 11 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>μ*</td>
<td>Estimate</td>
<td>Test (p-Value)</td>
</tr>
<tr>
<td>S-Explicit</td>
<td>Round 1, (\hat{\mu}_{1}^{S(E)}) [51]</td>
<td>55.52 (2.47)</td>
<td>0.844</td>
<td><strong>0.001</strong></td>
</tr>
<tr>
<td></td>
<td>Round 2, (\hat{\mu}_{2}^{S(E)}) [34]</td>
<td>48.46 (2.49)</td>
<td>0.076</td>
<td><strong>0.054</strong></td>
</tr>
<tr>
<td></td>
<td>Round 3, (\hat{\mu}_{3}^{S(E)}) [17]</td>
<td>42.57 (2.55)</td>
<td><strong>0.000</strong></td>
<td>0.238</td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td><strong>0.000</strong></td>
<td><strong>0.000</strong></td>
<td><strong>0.000</strong></td>
</tr>
</tbody>
</table>

**Note:** Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 36 subjects in the S-Explicit treatment across supergames 11-20, and 12 first-movers in supergame 21. †—Univariate significance tests columns examine differences from either the first-round coefficient from the control (\(H_0 : \hat{\mu}_1 = \hat{\mu}_{NS}^1\) for treatment \(j\), round \(t\)), the first-mover coefficients from the Selection treatment (\(H_0 : \hat{\mu}_1 = \hat{\mu}_{S}^1\) for treatment \(j\), round \(t\)) and the theoretical prediction (\(H_0 : \hat{\mu}_1 = \mu_1^{\star}\)). ‡—Joint test of stationary cutoffs across the supergame (\(H_0 : \hat{\mu}_1 = \hat{\mu}_{1}^S = \hat{\mu}_3^S\) for treatment \(j\)); §—Joint test of PBE cutoffs in supergame (\(H_0 : 0 = \hat{\mu}_1^S - \mu_1^{\star} = \hat{\mu}_2^S - \mu_2^{\star} = \hat{\mu}_3^S - \mu_3^{\star}\)).
### Table A3.6. Average Cutoff per Round for S-Explicit Treatment

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Supergame 6 to 20</th>
<th>Supergame 21</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate Test (p-Value)</td>
<td>Estimate Test (p-Value)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>µ*</td>
<td>µ̂</td>
<td>µ̂ = µ̂_NS</td>
</tr>
<tr>
<td>S-Explicit</td>
<td>Round 1, ̂μ₁^{S(E)}</td>
<td>[51]</td>
<td>55.49</td>
<td>0.698</td>
</tr>
<tr>
<td></td>
<td>Round 2, ̂μ₂^{S(E)}</td>
<td>[34]</td>
<td>48.88</td>
<td>0.134</td>
</tr>
<tr>
<td></td>
<td>Round 3, ̂μ₃^{S(E)}</td>
<td>[17]</td>
<td>43.84</td>
<td>0.004</td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td>0.000 †</td>
<td>0.000 ‡</td>
</tr>
</tbody>
</table>

**Note:** Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 36 subjects in the S-Explicit treatment across supergames 6-20, and 12 first-movers in supergame 21. †–Univariate significance tests columns examine differences from either the first-round coefficient from the control (H₀: ̂μ̂₁ = ̂μ̂_NS for treatment j, round t), the first-mover coefficients from the Selection treatment (H₀: ̂μ̂₁ = ̂μ̂_S for treatment j, round t) and the theoretical prediction (H₀: ̂μ̂₁ = ̂μ̂^*). ‡–Joint test of stationary cutoffs across the supergame (H₀: ̂μ̂₁ = ̂μ̂₁ = ̂μ̂_2 = ̂μ̂_3 for treatment j); †‡–Joint test of PBE cutoffs in supergame (H₀: 0 = ̂μ̂₁ - ̂μ̂^* = ̂μ̂₂ - ̂μ̂^* = ̂μ̂₃ - ̂μ̂^*).
<table>
<thead>
<tr>
<th>Treatment</th>
<th>In-Group Switches</th>
<th>Theory</th>
<th>Supergame 11 to 20</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimates</td>
</tr>
<tr>
<td></td>
<td></td>
<td>$\mu^*$</td>
<td>Round 1, $\hat{\mu}_{1s}^{S(W)}$</td>
</tr>
<tr>
<td>Zero</td>
<td>[51]</td>
<td></td>
<td>42.91 [0.000]$^\dagger$</td>
</tr>
<tr>
<td>S-Within</td>
<td></td>
<td></td>
<td>35.66 [0.000]$^\dagger$</td>
</tr>
<tr>
<td>One</td>
<td>[14]</td>
<td></td>
<td>30.77 [0.000]$^\dagger$</td>
</tr>
<tr>
<td>Two</td>
<td>[3]</td>
<td></td>
<td>(1.80)</td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td>0.000$^\dagger$</td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 69 subjects across supergames 11-20. $^\dagger$–P-Value of univariate significance tests of the theoretical predictions ($H_0: \hat{\mu}_{ts} = \mu_{ts}^\ast$). $^\ddagger$–Join test: Cutoffs are stationary across the treatment ($H_0: \hat{\mu}_{1s}^{S(W)} = \hat{\mu}_{2s}^{S(W)} = \hat{\mu}_{3s}^{S(W)}$ across rounds, and $H_0: \hat{\mu}_{t1}^{S(W)} = \hat{\mu}_{t2}^{S(W)} = \hat{\mu}_{t3}^{S(W)}$ across number of in-group switches).
Table A3.8. Average Cutoff per Round and Number of In-Group Switches, S-Within Treatment

<table>
<thead>
<tr>
<th>Treatment</th>
<th>In-Group Switches</th>
<th>Theory</th>
<th>Supergame 6 to 20</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimates</td>
</tr>
<tr>
<td></td>
<td>µ*</td>
<td>Round 1, µ₁s</td>
<td>Round 2, µ₂s</td>
</tr>
<tr>
<td>Zero</td>
<td>[51]</td>
<td>43.42 [0.000]†</td>
<td>42.15 [0.000]†</td>
</tr>
<tr>
<td>One</td>
<td>[14]</td>
<td>37.04 [0.000]†</td>
<td>37.04 [0.000]†</td>
</tr>
<tr>
<td>Two</td>
<td>[3]</td>
<td>34.67 [0.000]†</td>
<td>31.26 [0.000]†</td>
</tr>
</tbody>
</table>

Joint Tests: 0.000† 0.000† 0.000‡

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 69 subjects across supergames 6-20. †=P-Value of univariate significance tests of the theoretical predictions (H₀: ˆµ₁s = ˆµ₂s). ‡=Joint test: Cutoffs are stationary across the treatment (H₀: ˆµ₁s = ˆµ₂s = ˆµ₃s) across rounds, and H₀: ˆµ₁s = ˆµ₂s = ˆµ₃s across number of in-group switches.)
### Table A3.9. Average Cutoff per Round in Supergame 21 for S-Within Treatment, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Estimate</th>
<th>Test (p-Value)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>µ*</td>
<td>(\hat{\mu})</td>
<td>(\hat{\mu}<em>1 = \hat{\mu}</em>{1}^{NS})</td>
</tr>
<tr>
<td>S-Within</td>
<td>Round 1, (\hat{\mu}_1^{S(W)})</td>
<td>[51] 39.57</td>
<td><strong>0.001</strong></td>
<td>0.413</td>
</tr>
<tr>
<td></td>
<td>Round 2, (\hat{\mu}_2^{S(W)})</td>
<td>[35] 37.00</td>
<td><strong>0.004</strong></td>
<td>0.540</td>
</tr>
<tr>
<td></td>
<td>Round 3, (\hat{\mu}_3^{S(W)})</td>
<td>[28] 34.09</td>
<td><strong>0.001</strong></td>
<td>0.639</td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td><strong>0.005</strong> ‡</td>
<td><strong>0.000</strong> §</td>
</tr>
</tbody>
</table>

**Note:** Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 23 first-mover subjects in supergame 21. †—Univariate significance tests columns examine differences from either the first-round coefficient from the control \((H_0 : \hat{\mu}_1 = \hat{\mu}_1^{NS} \text{ for treatment j, round } t)\), the first-mover coefficients from the Selection treatment \((H_0 : \hat{\mu}_1 = \hat{\mu}_1^{S} \text{ for treatment j, round } t)\) and the theoretical prediction \((H_0 : \hat{\mu}_1 = \mu_1^{*} \text{ for treatment j})\). ‡—Joint test of stationary cutoffs across the supergame \((H_0 : \hat{\mu}_1 = \hat{\mu}_2 = \hat{\mu}_3 \text{ for treatment j})\); §—Joint test of PBE cutoffs in supergame \((H_0 : 0 = \hat{\mu}_1 - \mu_1^{*} = \hat{\mu}_2 - \mu_2^{*} = \hat{\mu}_3 - \mu_3^{*})\).
TABLE A3.10. Average Cutoff per Round for S-Peer Treatment on Supergame 21, First-Movers Only

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Estimate</th>
<th>Test (p-Value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>S-Peer</td>
<td>Round 1, $\hat{\mu}_1^{S(A)}$</td>
<td></td>
<td>46.13</td>
<td>0.187 0.577 0.140</td>
</tr>
<tr>
<td></td>
<td>(51)</td>
<td></td>
<td></td>
<td>(3.31)</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}_2^{S(A)}$</td>
<td></td>
<td>41.5</td>
<td>0.035 0.739 0.049</td>
</tr>
<tr>
<td></td>
<td>(35)</td>
<td></td>
<td></td>
<td>(3.31)</td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}_3^{S(A)}$</td>
<td></td>
<td>37.17</td>
<td>0.005 0.859 0.001</td>
</tr>
<tr>
<td></td>
<td>(28)</td>
<td></td>
<td></td>
<td>(3.31)</td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td>0.000‡</td>
<td>0.000§</td>
</tr>
</tbody>
</table>

Note: Figures derived from a single random-effects least-squares regression for all chosen cutoffs against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). There are 24 first-mover subjects in supergame 21. †—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0 : \hat{\mu}_1^j = \hat{\mu}_1^{NS}$ for treatment j, round t), the first-mover coefficients from the Selection treatment ($H_0 : \hat{\mu}_1^j = \hat{\mu}_1^S$ for treatment j, round t) and the theoretical prediction ($H_0 : \hat{\mu}_1^j = \mu_j^\star$). ‡—Joint test of stationary cutoffs across the supergame ($H_0 : \hat{\mu}_1^j = \hat{\mu}_2^j = \hat{\mu}_3^j$ for treatment j); §—Joint test of PBE cutoffs in supergame ($H_0 : 0 = \hat{\mu}_1^j - \mu_j^{\star \star} = \hat{\mu}_2^j - \mu_j^{\star \star} = \hat{\mu}_3^j - \mu_j^{\star \star}$).
### Table A3.11. Average Cutoff in Last Round for Within and Between Treatments (Supergame 40)

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Game Round</th>
<th>Theory</th>
<th>Between</th>
<th>Within</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$\mu^*$</td>
<td>$p$-values</td>
<td>$p$-values</td>
</tr>
<tr>
<td></td>
<td></td>
<td>$\hat{\mu}$</td>
<td>$H_0 : \hat{\mu}^W_j = \mu^W_j$</td>
<td>$H_0 : \hat{\mu}_t^j = \mu^*_j$</td>
</tr>
<tr>
<td>Within</td>
<td>Round 1, $\hat{\mu}^{NS}_1$</td>
<td>[51]</td>
<td>44.88</td>
<td>$-$</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.85)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Round 2, $\hat{\mu}^{NS}_2$</td>
<td>[32]</td>
<td>41.62</td>
<td>0.421</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.85)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Round 3, $\hat{\mu}^{NS}_3$</td>
<td>[22]</td>
<td>38.00</td>
<td><strong>0.090</strong></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.85)</td>
<td></td>
</tr>
<tr>
<td>Joint Tests:</td>
<td></td>
<td></td>
<td><strong>0.000$^\dagger$</strong></td>
<td><strong>0.000$^\ddagger$</strong></td>
</tr>
</tbody>
</table>

**Note:** Figures derived from a single OLS regression ($N = 144$ subjects each making a single choice) against treatment-round dummies. Standard errors in parentheses, risk-neutral predicted cutoffs in square brackets (switch ball if value is lower than cutoff). $^\dagger$—Univariate significance tests columns examine differences from either the first-round coefficient from the control ($H_0 : \hat{\mu}_1^j = \hat{\mu}^{NS}_1$ for treatment $j$, round $t$), the first-mover coefficients from the Selection treatment ($H_0 : \hat{\mu}_1^j = \hat{\mu}^S_j$ for treatment $j$, round $t$) and the theoretical prediction ($H_0 : \hat{\mu}_t^j = \mu^*_j$). $^\ddagger$—Joint test of stationary cutoffs across the supergame ($H_0 : \hat{\mu}_1^j = \hat{\mu}_2^j = \hat{\mu}_3^j$ for treatment $j$). $^\dagger$—Joint test of PBE cutoffs in supergame ($H_0 : 0 = \hat{\mu}_1^j - \mu^*_j = \hat{\mu}_2^j - \mu^*_j = \hat{\mu}_3^j - \mu^*_j$).
A.4. **Type Classification Robustness.** In Figure A4.1 we indicate the three type categories as we vary the bandwidth parameter $\epsilon$ from 0 to 10.

Table A4.1 reports on the proportion of types as classified in the last five partial strategy supergames for each treatment. Rather than the full strategy method final supergames, we here take averages across supergames to assess the strategy cutoffs.

Following the paper we focus on the type specifications with an error bandwidth $\epsilon = 2.5$ (though we also provide data on $\epsilon = 0$ and $\epsilon = 5$).

For treatment *S-Simple-Random* a subject is *decreasing* if the difference in average cutoffs for *all* pairwise comparisons (first minus second-mover, first minus third-mover, and
second minus third-mover) are strictly positive; likewise, subject is \( \epsilon \)-decreasing if all such differences are (weakly) greater than \( \epsilon \). For treatment S-Within subject is decreasing if the difference in average cutoffs for all pairwise comparisons (no switches minus 1 switch, no switches minus 2 switches, and 1 switch minus 2 switches) are strictly positive; and subject is \( \epsilon \)-decreasing if all such differences are (weakly) greater than \( \epsilon \). For all other treatments subject is decreasing if minimum of (round 1 - round 2) cutoffs, irrespective of type, is strictly positive, and is \( \epsilon \)-decreasing if minimum is larger than \( \epsilon \). Treatment Selection includes data from S-Deliberation as both treatment are identical up until cycle 21.
### Table A4.1. Type Classifications on Last Five Cycles

<table>
<thead>
<tr>
<th>NS</th>
<th>Decreasing</th>
<th>Non-Decreasing</th>
<th>Total</th>
<th>Stationary</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Exact</td>
<td>$\epsilon = 2.5$</td>
<td>$\epsilon = 5$</td>
<td>Exact</td>
</tr>
<tr>
<td>No Selection</td>
<td>33</td>
<td>51.5%</td>
<td>30.3%</td>
<td>15.2%</td>
</tr>
<tr>
<td>Selection</td>
<td>138</td>
<td>30.4%</td>
<td>26.1%</td>
<td>24.6%</td>
</tr>
<tr>
<td>S-Across</td>
<td>60</td>
<td>61.7%</td>
<td>50%</td>
<td>36.7%</td>
</tr>
<tr>
<td>S-Explicit</td>
<td>36</td>
<td>80.5%</td>
<td>61.1%</td>
<td>41.7%</td>
</tr>
<tr>
<td>S-Within</td>
<td>69</td>
<td>69.6%</td>
<td>55.1%</td>
<td>50.7%</td>
</tr>
<tr>
<td>S-Simple-Random</td>
<td>72</td>
<td>48.6%</td>
<td>40.3%</td>
<td>34.7%</td>
</tr>
</tbody>
</table>

*Note: Type classification based on cutoffs chosen in cycles 36-40 for S-Simple-Random and cycles 16-20 for all other treatments.*
### Table A5.1. Individual Regressions

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Experienced Rematching</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\hat{\nu}_{1,10}^G$</td>
<td>0.037</td>
<td>0.063</td>
<td></td>
</tr>
<tr>
<td>$\hat{\nu}_{11,20}^G$</td>
<td>0.055</td>
<td>0.051</td>
<td></td>
</tr>
<tr>
<td>Experienced Outcomes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\hat{\nu}_{1,10}^H$</td>
<td>0.363 **</td>
<td>0.277 **</td>
<td>0.341 ***</td>
</tr>
<tr>
<td>$\hat{\nu}_{11,20}^H$</td>
<td>0.015 0.030</td>
<td>0.018</td>
<td>0.003</td>
</tr>
<tr>
<td>Risk Aversion</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\hat{\rho}$</td>
<td>-2.24 **</td>
<td>-2.12 **</td>
<td>-2.11 **</td>
</tr>
<tr>
<td>No Selection</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\delta_{\text{NoSel}}$</td>
<td>-</td>
<td>5.52 0.046</td>
<td>7.23 ** 0.017</td>
</tr>
<tr>
<td>Constant</td>
<td>21.6</td>
<td>25.8</td>
<td>26.9</td>
</tr>
<tr>
<td>$N$</td>
<td>66</td>
<td>91</td>
<td>91</td>
</tr>
<tr>
<td>$\bar{R}^2$</td>
<td>0.141</td>
<td>0.198</td>
<td>0.214</td>
</tr>
</tbody>
</table>

Note: Statistically significant variables indicated by stars (⋆−10%; ⋆⋆−5%; ⋆⋆⋆−1%) with p-values beneath them. $\bar{R}^2$ is the adjusted $R$-squared measure of fit.

### A.5. Effects from Experience and Risk Aversion.

For each $\epsilon$-stationary subject we generate their experienced rematching ($\hat{\nu}_{i}^G$) and final ($\hat{\nu}_{i}^H$) outcomes, averaging across supergames 1–10 and supergames 11–20. Despite facing the same generating process in each treatment there is substantial subject variation in rematching and final outcomes in these blocks of ten supergames. For each individual subject variation in the average experiences can be used to validate the mechanic in the hypothesized learning model. In Table A5.1, we regress each stationary subject’s supergame-21 cutoff choice on the observed experiences ($\hat{\nu}_{i}^G$ and $\hat{\nu}_{i}^H$) and elicited risk preference ($\hat{\rho}_i$). In the first regression (Column I), we present results for the 66 stationary-type subjects in our Selection treatments (the baseline and S-Across). In the second (Column II) we pool in the additional 25 stationary subjects from the No Selection treatment (including a treatment dummy). In the third regression, we remove the experienced rematching (both) and late-session experienced final outcomes as independent variables (which can be motivated by the adjusted $R$-squared measures provided in the last row).

Across specifications we find that variation in final outcomes in the early-session supergames, $\hat{\nu}_{1,10}^H$, has a strong effect in the predicted direction. However, subjects’ learning seems to occur quite quickly, as we do not find a significant final cutoff response to final-outcome variation in the second block of ten supergames. While the signs on both rematching variables and the late-session final-outcome variable are positive, the size of the estimated effects are far smaller than for the early-session final outcomes. Separate from experience, subject-variation in elicited risk preferences is a significant predictor for
subjects’ final supergame cutoffs in all of the regressions, reflecting the other requirement for the aggregate match made by the behavioral model.
Appendix B. Theoretical Model

We model a dynamic setting with adverse selection occurring over time. To do this we set up a finite population of objects $O = \{o_1, \ldots, o_M\}$ (with the interpretation of long-side participants or durable goods, etc.). Each object has a common value, an iid draw over $V \subset \mathbb{R}$ according to a commonly known distribution $F$ so that the average value is $\bar{v}$. These objects are initially matched to a group of individuals $I = \{1, \ldots, N\}$ (with the interpretation of short-side participants, consumers, etc.). Each individual is randomly assigned to one of the objects at the beginning of the game through a one-to-one matching function, so that individual $i$ has the initial object $\mu_i^0$. In our setting there are more objects than individuals ($N < M$) and so some of the objects $\hat{O}_0 \subset O$ are initially unassigned. The set of unassigned objects will function here as a rematching population, and it is adverse selection over this population that is our main focus.

Choices take place over a time variable $t \in \{1, 2, \ldots, T \cdot N\}$, where individuals take turns receiving an opportunity to see their object’s value. In particular each individual sees their object’s value in period $t_i^* = i + \tau_i N$, where $\tau_i$ is an iid random variable from 1 to $T$.

At the exogenously determined time $t_i^*$ the individual $i$ faces a choice: keep the initially assigned object $\mu_i^0$ which has a known value $v_0$, or instead rematch to an object $\mu_i^t$ from the unmatched population $\hat{O}_t$. Importantly, rather than the rematching population being some fixed outcome or a draw from a stationary distribution, the rematching population after agent $i$’s choice is given by $\hat{O}_{t+1} = \{\mu_0^0\} \cup \hat{O}_t \setminus \{\mu_i^t\}$.

To see that there is adverse-selection in our environment it suffices to solve the first few periods of the model. Define the event that the participant who moves in period $t$ observes their value that period as $I_t$ and the event that they observe their value and switch as $S_t$, with complementary event $\overline{S_t}$.

$t = 1$: Suppose individual 1 observes their object’s value in period 1 (the event $I_1 = \{t_1^* = 1\}$). Because no-one else can have moved yet, each object in the rematching pool is an iid draw from the distribution $G_1(v | I_1) = F(v)$. The optimal choice is therefore to rematch if $\mu_1^0 < \bar{v} =: v_1^*$, which happens with probability $p_1^* = \Pr\{t_1^* = 1\} \cdot \Pr\{\mu_1^0 < v_1^*\}$.

$t = 2$: Suppose 2 observes their object’s value. A draw from the rematching pool is therefore distributed as $G_2(v | I_2) = G_2(v) = \lambda \Pr\{S_1 \mid I_2\} \cdot F(v \mid v < v_1^*) + (1 - \lambda p_1^*) \cdot G_1(v)$ where $\lambda = \frac{1}{M-N}$ is the probability of drawing any particular object from the rematching pool. The distribution $G_2(v)$ therefore has an expected value $v_2^* < v_1^*$, and the optimal choice by 2 is therefore to rematch if $\mu_2^0 < v_2^*$, which happens with probability $p_2^* = \Pr\{t_2^* = 2\} \cdot \Pr\{\mu_2^0 < v_2^*\}$

---

1The interpretation given is that there are $T$ periods, but agents move in turns within the period, so 1 is the first-mover, 2 the second-mover, etc. An alternative interpretation is that draws where two agents choose at the same time are resolved in preference to the first-mover, then the second-mover, and so on.
\[ G_t(v | \mathcal{L}_t) = \lambda \mathcal{P}_{t-1} \cdot F(v | v < v^*_t) + (1 - \lambda \mathcal{P}_{t-1}) \cdot G_{t-1}(v | S_{t-1}, \mathcal{L}_t) \] which has expectation \( v^*_t \). Individual \( i \) therefore switches with probability \( p^*_t = \Pr \{ \mathcal{L}_t \} \cdot \Pr \{ \mu_0^* < v^*_t \} \).

The optimal solution proceeds inductively, with the additional complication that after period \( N \) agents must condition their choices on their own information, and that of other agents.\(^2\)

Define the two sets of time periods
\[
\mathcal{X}_t(s, s') = \{ s, s + 1, \ldots, s' - 1, s' \} \cap \{ r | J(r) = J(t) \},
\]
\[
\mathcal{Y}_t(s, s') = \{ s, s + 1, \ldots, s' - 1, s' \} \cap \{ r | J(r) \neq J(t) \},
\]
the time periods between \( s \) and \( s' \) where the player who moves at \( t \) is or is not the relevant decision maker, respectively. The optimal decision rules:

**Proposition 1.** The optimal decision rules can be calculated inductively according to
\[
v^*_t := \sum_{s \in \mathcal{Y}_t(1,t-1)} q_s \cdot F(v^*_s) \cdot \prod_{r \in \mathcal{X}_t(s+1,t-1)} (1 - q_r \cdot F(v^*_r)) \mathbb{E} [ v | v < v^*_s ] + \prod_{s \in \mathcal{Y}_t(1,t-1)} (1 - q_s \cdot F(v^*_s)) \mathbb{E} [ v ].
\]

This forms a decreasing sequence \( \{ v^*_t \}_{i=1}^{(j+1)N} \) across each sequence of player turns \( j \in \{1, \ldots, T\} \), while for any individual \( i \) the optimal decision rule \( \{ v^*_{jN+i} \}_{j=1}^T \) is decreasing.

That the first sequence of turns has a decreasing optimal decision rule is illustrated above, as each participant faces a rematching pool that is a convex combination of the previous participant’s distribution \( F_{t-1} \), but with a positive probability of \( F(v \mid v < \mathbb{E}v_{t-1}) \) mixed in. For the first result we need to nest information. Define the event that a participant \( i \) observes the object at time \( t = j \cdot N + i \) and switches as \( S_t \) and its complement of not switching as \( N_t \). Essentially this event encodes information that \( j \) did not switch in periods \((j-1)N+i, (j-2)N+i, \ldots, 0 \), etc. The rematching distribution of a participant who is thinking about switching at time \( t \) is given by:
\[
G_t(v) = \lambda \cdot \Pr \{ S_{t-1} \} \cdot F(v \mid v < v^*_t) + (1 - \lambda \cdot \Pr \{ S_t \}) \cdot G_{t-1}(v | N_{t-1}).
\]

For the recursive calculation, the event \( N_{jN+i} \) only contains information that is relevant to the probability that person \( i \) saw their object in a previous period, and so for any fixed sequence of turns \( \{ v^*_t \}_{i=1}^{(j+1)N} \) the same reasoning as before holds, given any distribution at the start of the sequence \( G_{jN}(v) \).

---

\(^2\)For instance, agent \( i \) observing the value of \( \mu^*_0 \) in period \( N + i \) knows that they did not see their value in period \( i \). Similarly, the inductive step must incorporate the nested condition that player \( i - 1 \) did not switch when working backwards.
C.1. Instructions to Part 1 [Supergames 1-5].

C.1.1. No Selection (Control) and S-Explicit.

Introduction. Thank you for participating in our study. Please turn off mobile phones and other electronic devices. These must remain turned off for the duration of the session. This is an experiment on decision making. The money you earn will depend on both your decisions and chance. The session will be conducted only through your computer terminal; please do not talk to or attempt to communicate with any other participants during the experiment. If you have a question during this instruction phase please raise your hand and one of the experimenters will come to where you are sitting to answer your question in private.

During the experiment, you will have the opportunity to earn a considerable amount of money depending on your decisions. At the end of the experiment, you will be paid in private and in cash. On top of what you earn through your decisions during the experiment, you will also receive a $6 participation fee.

Outline. Your interactions in this experiment will be divided into “Cycles”.

- In each cycle you will be holding one of four balls, called Balls A to D.
- Each ball has a value between 1 and 100, and your payoff in each cycle will be determined by the value of the ball you are holding at the cycle’s end.
- Initially you will not know any of the four ball’s values, and will only know which of the four balls you are holding. Each cycle is divided into three rounds, and in one of these rounds you will see the value of your ball.
- At the point when you see your ball’s value you will be asked to make your only choice for the cycle:
  - either keep the ball you are holding.
  - or instead let go of your current ball and take hold of one of the other three balls.

Main Task. In more detail, a cycle proceeds as follows:

- In each new cycle and for each participant, the computer randomly draws four balls. Each ball’s value is chosen in an identical manner:
  - With 50% probability the computer rolls a fair hundred-sided die: so the ball has an equal probability of being any number between 1 and 100.
  - With 25% probability the ball has value 1.
– With 25% probability the ball has value 100.

• After drawing the values for the four balls, the computer randomly shuffles them into positions A to D. Once in place, the four balls’ positions are fixed for the entire cycle. So whatever the value on Ball A, this is its value for the entire cycle. Only at the end of each cycle are four new balls drawn for your next cycle.

• Once the balls are in position, the computer randomly matches you to a ball, so you start out holding one of the balls A to D. There are therefore three leftover balls which are held by the Computer.
  – For example, one possible initial match might be that you hold Ball B. So because Ball B is held in this example, the Computer starts the cycle holding Balls A, C and D.

• Your outcome each cycle will depend only on the ball you are holding at the end of the cycle.
  – In each cycle you will know which of the four balls you start out holding.
  – You will not know any of the four balls’ values to start with.
  – Every cycle you will make just one decision. At some random point in the cycle you will be told your ball’s value. You will then be asked to make a choice after learning your ball’s value: either keep holding your ball or give your ball to the computer, and instead take one of the three balls the computer is holding.

• The point in the cycle when you see your ball’s value is random. Each cycle is divided into three rounds where you are given a chance to see your ball.

• The round in which you will see your held ball’s value and make your choice for the cycle is random:
  – In round one, the computer flips a fair coin. If the coin lands Heads, you will see your ball’s value. So you have a 50% chance of seeing your ball’s value in the first round.
  – In round two, if you did not see you ball in round one you get another 50% chance of seeing its value: another coin flip.
  – Finally, in round three, if you did not see your ball’s value in either round one or round two you will see its value for sure in round three and make your choice.

• Whenever you do see your held ball’s value—either in round one, two or three—you will make your only decision for the cycle. The two options you have are:
  (1) Keep hold of your ball until the end of the cycle.
  (2) Switch balls: Give your ball to the computer to hold, and instead take one of the balls it is currently holding.
• If you do choose to switch balls with the computer, the procedure the computer uses to select a ball to give you in exchange varies with the round:
  – In round one, the computer will randomly select one of the three available balls, choosing between each of the three balls it is holding with equal probability.
  – In round two, the computer will randomly select a ball from the two lowest value balls of the three it is holding, choosing each of the two lowest-value balls with equal probability. So, in round two the computer will never offer you the highest-value ball of the three.
  – In round three, the computer will only offer you the lowest-value ball of the three it is holding.

• Your cycle payoff is $0.10 multiplied by the number on the ball you are holding at the end of the cycle. So a ball with value 1 at the end of a cycle has a payoff of $0.10, a ball with value 50 has a payoff of $5.00, while a ball with value 100 has a payoff of $10.00.

Cycle Summary.

(1) Each participant is given four balls A to D, where each ball has a random value between 1 and 100.
(2) Each participant is then assigned one of the four balls to hold, with the leftover balls held by the computer.
(3) Across three rounds the participants are given the chance to see the value of the balls they are holding.
  • Whenever you see the value of the ball you are holding you must decide whether to keep holding it, or trade it with the computer.
  • In rounds one and two you have a 50% probability of seeing the held ball’s values. Any participant that reaches round three without seeing their ball’s value will always see its value in round three, and are then given the option to trade it for one of the computer balls.

[S-Explicit]

• The procedure the computer uses to choose the ball it is willing to exchange with you changes across the cycle. In round one it will randomize across all three balls. In round two, it randomizes over the two lowest-value balls. In round three, it will offer the lowest-value ball with certainty.

Experiment Organization. There will be three parts to this experiment. The first part will last for 5 cycles. After this you will get instructions for the second part which will last for another 15 cycles, where the task is very similar. Part 3 will last for a single cycle.
Following part 3, we will conclude the experiment with a number of survey questions for which there is the chance for further payment.

**Payment.**

- Monetary payment for Parts 1 and 2 will be made on two randomly chosen cycles, where each of the 20 cycles in the first two parts are equally likely to be selected for payment.
- You will be given the opportunity for further earnings in Part 3 and the survey at the end of the experiment (which we will explain once the preceding parts end).
- All participants will receive a $6 participation fee added to total earnings from the other parts of the experiment.

C.1.2. *Selection, S-Across, S-Within, and S-Peer*.  

**Introduction.** Thank you for participating in our study. Please turn off mobile phones and other electronic devices. These must remain turned off for the duration of the session.

This is an experiment on decision making. The money you earn will depend on both your decisions and chance. The session will be conducted only through your computer terminal; please do not talk to or attempt to communicate with any other participants during the experiment. If you have a question during this instruction phase please raise your hand and one of the experimenters will come to where you are sitting to answer your question in private.

During the experiment, you will have the opportunity to earn a considerable amount of money depending on your decisions. At the end of the experiment, you will be paid in private and in cash. On top of what you earn through your decisions during the experiment, you will also receive a $6 participation fee.

**Outline.** Your interactions in this experiment will be divided into “Cycles”.

- In each cycle you will be in a group of three, with each participant holding one of four balls, called Balls A to D.
- Each ball has a value between 1 and 100, and your payoff in each cycle will be determined by the value of the ball you are holding at the cycle’s end.
- At the start of each cycle you will see which ball each of the three participants are holding. However, you will NOT know any of the four ball’s values. Each cycle is divided into three rounds, and in one of these rounds you will see the value of your ball.
- At the point when you see your ball’s value you will be asked to make your only choice for the cycle:
  - either keep the ball you are holding.

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5Some passages are different depending on treatment, and are indicated by highlighted text and the name of the treatment in brackets. Everything else is the same.
– or instead let go of your current ball and take hold of whichever ball is not being held by another group member.

**Main Task.** In more detail, a cycle proceeds as follows:

- At the start of each cycle the computer randomly divides all of the participants in the room into groups of three. Each player will randomly be given one of three roles: either First Mover, Second Mover or Third Mover.
  - The groups of three and specific roles assigned are fixed for each cycle.
  - In each new cycle you will be randomly matched into a new group of three.
  - In each new cycle you will be randomly assigned to either be the First, Second or Third Mover.
- In each new cycle and for each separate group of three, the computer randomly draws four balls. Each ball’s value is chosen in an identical manner:
  - With 50% probability the computer rolls a fair hundred-sided die: so the ball has an equal probability of being any number between 1 and 100.
  - With 25% probability the ball has value 1.
  - With 25% probability the ball has value 100.
- After drawing the values for the four balls, the computer randomly shuffles them into positions A to D. Once in place, the four balls’ positions are fixed for the entire cycle. So whatever the value on Ball A, this is its value for the entire cycle. Only at the end of each cycle are four new balls drawn for your next cycle and next group of three.
- Once the balls are in position, the computer randomly gives a different ball to each of the three group members. Each group member therefore starts out holding one of the balls A to D. But because the three group members are each holding one of the four balls there is one leftover ball. This leftover ball is held by the Computer.
  - For example, one possible initial match might be that Ball A is held by the Third Mover; Ball B by the First Mover; and Ball D by the Second Mover. So because Balls A, B and D are all held in this example, the Computer starts the cycle holding the leftover Ball C.

[Selection, S-Across, and S-Peer]

- **Your outcome each cycle will depend only on the ball you are holding at the end of the cycle.**
  - **In each cycle you will know which of the four balls you start out holding**
  - **You will not know any of the four balls’ values to start with, nor which balls the other two group members are holding.**

[S-Within]

- **Your outcome each cycle will depend only on the ball you are holding at the end of the cycle.**
– You will not know any of the four ball’s values to start with.
– You will know which ball is being held by each participant, and will also know if a ball was previously held by another participant.
– Every cycle you will make just one decision. At some random point in the cycle you will be told your ball’s value. You will then be asked to make a choice after learning your ball’s value: either keep holding your ball or give your ball to the computer, and instead take whichever ball the computer is holding.

• The point in the cycle when you see your ball’s value is random. Each cycle is divided into three rounds where each group member is given a chance to see their ball. Each round is further divided into a sequence of turns, dictated by your role:
  (1) The first mover gets the first opportunity to see their ball’s value. If they see it, they make their one choice for the cycle, if not they must wait until the next round for another opportunity to see their ball’s value.
  (2) After the first mover, the second mover gets an opportunity to see their ball. Again, if they see it, they make their one choice for the cycle, otherwise they must wait until the next round.
  (3) Finally, after both the first and second mover, the third mover gets an opportunity to see their ball. As before, if they see their ball they make their one choice for the cycle, otherwise they must wait until the next round.

• The round in which you will see your held ball’s value and make your choice for the cycle is random:
  – In round one, the computer flips a fair coin once for each group member. If the coin lands Heads, the group member sees their ball’s value. So each group member has a 50% chance of seeing their ball’s value in the first round.
  – In round two, any group members who did not see their ball in round one get another 50% chance of seeing its value: another coin flip.
  – Finally, in round three, any group members who did not see their ball in either round one or round two see their ball’s value for sure in round three and make their choice.

• Whenever you do see your held ball’s value—either in round one, two or three—you will make your only decision for the cycle. The two options you have are:
  (1) Keep hold of your ball until the end of the cycle.
  (2) Switch balls: Give your ball to the computer to hold, and instead take the ball it is currently holding.

• The cycle ends after every participant within a group sees the ball’s value and makes a decision.
- At the end of the cycle you will get feedback on what happened. You will be told:
  - The balls each group member and the computer started with.
  - Choice 1/2/3: The identity of the group member who was 1st/2nd/3rd to see their ball’s value; the round they saw their ball’s value; their choice (keep or switch); and which ball the computer was holding after their choice.
- Your cycle payoff is $0.10 multiplied by the number on the ball you are holding at the end of the cycle. So a ball with value 1 at the end of a cycle has a payoff of $0.10, a ball with value 50 has a payoff of $5.00, while a ball with value 100 has a payoff of $10.00.

Cycle Summary.

1. The computer randomly forms the participants in the room into groups of three.
2. Each group is given four balls A to D, where each ball has a random value between 1 and 100.
3. Each of the three participants are given one of the four balls to hold, with the leftover ball held by the computer.
4. Across three rounds the group members move in sequence according to their roles, and each are given the chance to see the value of the ball they are holding.
   - Whenever you see the value of the ball you are holding you must decide whether to keep holding it, or trade it with the computer.
   - In rounds one and two each group member has a 50% probability of seeing their held ball’s value. Any group members that reach round three without seeing their ball’s value will always see it in round three, and are then given the option to trade it for the current computer ball.

Experiment Organization. There will be three parts to this experiment. The first part will last for 5 cycles. After this you will get instructions for the second part which will last for another 15 cycles, where the task is very similar. Part 3 will last for a single cycle. Following part 3, we will conclude the experiment with a number of survey questions for which there is the chance for further payment.

Payment.

- Monetary payment for Parts 1 and 2 will be made on two randomly chosen cycles, where each of the 20 cycles in the first two parts are equally likely to be selected for payment.
- You will be given the opportunity for further earnings in Part 3 and the survey at the end of the experiment (which we will explain once the preceding parts end).
- All participants will receive a $6 participation fee added to total earnings from the other parts of the experiment.
C.2. **Instructions to Part 2 [Supergames 6-20]**

We will now pause briefly before continuing on to the second part of the experiment. The task for the next 15 cycles of the experiment is very similar to the last 5. In fact, there is only one difference from part one. So far, if you flipped a head you have been told the value of the ball you are holding prior to deciding whether or not to trade it for the computer’s ball. For the remaining cycles you instead will be asked to provide a cutoff rule in case you see your ball.

This cutoff is the minimum value you would need to keep the ball you are holding. In every round of a cycle, you will be asked to provide a cutoff for trading your ball should you see its value that round.

You will be asked to choose your cutoff value by clicking on the horizontal bar at the bottom of your screens per the projected slide. You can click anywhere on the bar to change your cutoff, and you can always adjust your minimum cutoff by plus or minus one by clicking on the two buttons below the bar.

In the projected example I selected a minimum cutoff of 80.

After you submit your cutoff the computer will then flip the coin if you are in rounds one or two to determine if you see your ball’s value, similar to part one.

If the coin flip is tails, nothing happens, and you will have to wait to decide until at least the next round, where you will repeat this procedure and provide another minimum cutoff.

If instead the coin flip is Heads, or you are making your decision in round three where you are guaranteed to see your ball, the computer will show you the value of your ball. The computer will automatically keep it or trade your ball according to the minimum cutoff you selected.

If your ball’s value is LOWER than your selected minimum, you will automatically trade your ball for the computer’s ball, which you will keep until the end of the cycle. In the projected example I had selected 80 as my minimum cutoff. In the above example, it shows what would happen if I saw my held ball, and its value was 75. Because this is lower than my selected minimum value of 80, the computer uses my selected cutoff to automatically trade my ball for the computer’s ball, rather than keeping it.

The next example shows what happens if the coin flip is heads, and your held ball is equal to or greater than your selected minimum cutoff. In this case, because the ball’s value is greater than my selected minimum value of 80, the computer uses my selected cutoff to automatically keep my ball until the end of the cycle, rather than trading it. The projected example illustrates what would happen if my held ball had a value of 85. Because 85 is above my selected cutoff of 80, I would keep my ball until the end of the cycle.

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The experimenter read the instructions aloud after Part 1 had ended. Slides were used to show screen-shots and emphasize important points (see section C.3). The text was identical for all treatments, and the accompanying slides differ only for treatment S-Within.
Because of this procedure, you will maximize your potential earnings by selecting the chosen cutoff value to answer the following question: What is the smallest value X for which I would keep my ball right now, where for any balls lower than X, I would rather trade them for the computer’s ball?

The computer will now ask you three questions to make sure you understand this cutoff. At the top of your screens the computer will indicate a ball you are holding. Just for these question we will also tell you the ball the computer is holding. For each question we will give you a selected cutoff, and the value of the ball you are holding. Given this information, we would like you to select what happens.

You must answer all three questions correctly for the experiment to proceed.
You are the first mover in this cycle.
You are waiting for the second and third movers to act in the current round.
Below is a summary of your previous outcome:

The outcome of the coin flip in round 1 was HEADS. Therefore, you saw your ball's value in round 1.
The value of your initial ball was 75. This value was lower than your cutoff of 80.
Therefore, you switch it. You are now assigned to Ball D.

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Ball A: 0
Ball B: 75
Ball D: 60

HEADS
Your ball's value is 75 and your cutoff is 80.
You now have Ball D.

You are the first mover in this cycle.
You are waiting for the second and third movers to act in the current round.
Below is a summary of your previous outcome:

The outcome of the coin flip in round 1 was HEADS. Therefore, you saw your ball's value in round 1.
The value of your initial ball was 85. This value was greater than your cutoff of 80.
Therefore, you keep it.

---

Ball A: 0
Ball B: 85
Ball D: 60

HEADS
Your ball's value is 85 and your cutoff is 80.
You keep your original ball.
C.4. **Handouts for Part 2 [Supergames 6-20]**

C.4.1. **No Selection (Control) and S-Explicit**

**Part Two.**

- Everything in part two cycles will be the same as part one, with one exception.
- In every round, you will be asked to provide a *cutoff*.
- The *cutoff* you provide is the *minimum* value required for you to keep your ball.
- After you have confirmed your cutoff $X$ between 1 and 100 the computer will determine if you see your ball’s value this round:
  - If you see your ball’s value this round a choice will be made according to your minimum-value cutoff.
  - If you do not see your ball’s value this round, you will provide another minimum-value cutoff in the next round.
- In the round where you see your ball, the computer will use your minimum cutoff $X$ as follows:
  - If your ball’s value is equal to or greater than the minimum cutoff $X$, then you will choose to keep your ball.
  - Otherwise if your ball’s value is less than the minimum cutoff $X$, the computer will choose to trade your ball for one of the computer’s balls.

**[No Selection (Control)]**

- Otherwise if your ball’s value is less than the minimum cutoff $X$, the computer will choose to trade your ball for the one chosen by the computer that round.

**[Selection, S-Across, S-Within, and S-Peer]**

- Otherwise if your ball’s value is less than the minimum cutoff $X$, the computer will choose to trade your ball for the one held by the computer that round.

- Because of this procedure, you should choose your cutoff value to answer the following question:

**[No Selection (Control)]**

*What is the smallest value $X$ from 1 to 100 for which I would like to keep my ball right now, rather than give it to the computer in exchange for one of the balls the computer is holding?*

**[S-Explicit]**

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7 The experimenter distributed the handouts before showing the slides and reading the script. They served as a consultation material for subjects.

8 Some passages are different depending on treatment, and are indicated by highlighted text and the name of the treatment on brackets. Everything else is the same.
What is the smallest value $X$ from 1 to 100 for which I would like to keep my ball right now, rather than give it to the computer in exchange for its selected ball for this round?

[Selection, S-Across, S-Within, and S-Peer]

What is the smallest value $X$ from 1 to 100 for which I would like to keep my ball right now, rather than give it to the computer in exchange for the ball the computer is holding?

C.5. Instructions to Part Three$^9$.

C.5.1. No Selection, Selection, S-Across, S-Explicit, and S-Within. We will now pause briefly before continuing on to the third part of the experiment. The task for the final cycle of the experiment is very similar to the last 20. However, where we paid two random cycles from the last 20, we will pay you whatever you earn in this last cycle for sure. So for this one cycle you will earn between $0.10$ and $10.00$ depending on your final ball.

In this cycle we will randomly assign you to be either a first, second or third mover, and will tell you your role. Like the preceding rounds, we will ask you for a minimum cutoff value to keep your ball, and the computer will automatically keep or switch your ball depending on your selected cutoff if you see your balls value that round.

[No Selection, Selection, S-Explicit, and S-Across]

The only difference in part 3 is that we will only tell you which round you saw your ball’s value in at the end of the cycle.

[S-Within]

There are two differences in Part 3. First, we have removed the information on which balls the other participants are holding. All you will know is which ball you are currently holding. Second, we will now only tell you which round you saw your ball’s value and how you decided at the very end of this cycle.

You will submit cutoffs in rounds 1 to 3, as before. In the round where you see your ball’s value, you will use your selected cutoff for that round to make a decision.

[No Selection, Selection, S-Across, and S-Within]

If your balls’ value is lower than your minimal cutoff you will exchange your ball with the one the computer is holding at that point.

[S-Explicit]

If your ball’s value is lower than your minimal cutoff you will exchange your ball with the one the computer has selected to exchange for that round.

If your balls’ value is equal to or greater than your minimum cutoff you will keep your ball for the cycle. Because of this procedure, nothing in the structure of the task has changed from Part 2. So you should make your decisions exactly as before. The new

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$^9$The experimenter read the instructions aloud after Part 2 had ended. Slides were used to show screen-shots and emphasize important points (see section C.6).
procedure allows us to collect information on the cutoffs you would select in all three rounds of the cycle.

[No Selection, Selection, S-Across, and S-Explicit]

Effectively, the only thing that has changed from part two is the point at which we tell you you have made your choice, and that your payoff from this cycle will always be added to your final cash payoff for the experiment.

[S-Within]

Recall that, in contrast to the previous cycles, (i) you will now not receive any information about which balls the other two participants and the computer are currently holding in any period, and (ii) you will choose a cutoff in all three rounds.

In terms of payment, remember that the outcome from this cycle will always be added to your final cash payoff for the experiment, so that the ball you are holding at the end of this cycle will add between $0.10 and $10.00 to your final payoff, depending on the ball you are holding at the end.

Please now make your choices for the final cycle.

C.5.2. S-Peer.
We will now pause briefly before continuing to Part 3 of the experiment. Part 3 will last for just one cycle, and this cycle will be paid with certainty. So you will earn between $0.10 and $10.00 for Part 3.

The task for the final cycle of the experiment is very similar to the last 20. Like the preceding rounds, we will randomly assign you to be either the first, second, or third mover, and we will ask you for a minimum cutoff to keep your ball. The computer will then automatically keep or switch your ball depending on your selected cutoff and the value of your ball. The first difference in Part 3 is that we will only tell you which round you saw your ball’s value at the very end of the cycle.

That is, you will submit cutoffs for rounds 1 to 3, as before. In the round where you see your ball’s value, we will use your selected cutoff for that round to make a decision.

(1) If your ball’s value is lower than your minimum cutoff, you will exchange your ball with the one the computer is holding at that time.
(2) If your ball’s value is equal to or greater than your minimal cutoff, you will keep your ball for the cycle.

Because of this procedure, nothing in the structure of the task has changed from Part 2. The new procedure is chosen to allow us to collect information on the cutoffs you would select in all three rounds of the cycle.

The second difference from the first 20 cycles is the determination of payment for Part 3. Before you go on to Part 3, you will be matched into a team of three participants. This team will be given the chance to communicate with each other, prior to making their choices in Part 3. After the chat is completed, each team member will be assigned to a different role (first, second, or third mover), and each of the three team members will be
assigned to different groups with participants from other teams. Each team-member will then make their choices in the final cycle.

Payment for Part 3 for your entire team (the participants you will chat with) will be determined by the actions of one randomly selected team member. The chat window allows you to discuss your possible cutoff choices with your other team members before the cycle begins.

This is what the chat screen looks like. You will have 5 minutes to discuss with your team members what to do in cycle 21.

You may not use the chat to discuss details about your previous earnings, nor are you to provide any details that may help other participants in this room identify you. This is important to the validity of the study and will not be tolerated. However, you are encouraged to use the chat window to discuss the choices in the upcoming cycle. In particular, because of our modification to the cutoff procedure, all three team members will make three cutoff choices: the cutoff decision for each round one, two and three.

Whatever advice you can provide your matched team-members that leads them to a better outcome in Part 3 will also benefit you, as there is a two-in-three chance that one of the other team member’s choices will define your earnings for this part. Similarly, the advice of others can also help you, as there is a one-in-three probability that your choices will define both your earnings for Part 3 as well as the earnings of the other two team-members.

After the Part 3 cycle is completed, one of the three team members will be randomly selected and that participant’s final held ball will determine the payment for all three members of the team. We will then show you the outcome of the chosen cycle, as in the projected slide. We will pay you for whatever you earn in this last cycle for sure. So for Part 3 you will earn $0.10 times the value of the ball the selected team member is holding at the end of the cycle.


- Part Three will consist of a single cycle
- Whatever you earn in this cycle will be added to the two random cycles selected from Parts 1 and 2
  - So you have the chance to earn between $0.10 and $10.00 for this cycle depending on your final ball

- You will be told whether you are the First, Second or Third mover
- We will ask you for your minimal cutoff in each new round as before
- If your coin flip is heads, you will choose an action according to your cutoff as before
- The only difference is that we will not tell you when and if you have made a choice until the end of the cycle
• You will submit cutoffs in rounds 1 to 3, as before
• In the round where you see your ball’s value, you will use your selected cutoff to make a decision
  – If your balls’ value is lower than your minimal cutoff you will exchange your ball with the one the computer is holding
  – If your balls’ value is equal to or greater than your minimal cutoff you will keep your ball
• Because of this procedure, nothing in the structure of the task has changed
  – The new procedure allows us to collect information on the cutoff you would select in all three rounds
  – All that has changed from part two is the time at which we inform you on your choice for the cycle
C.6.2. S-Peer.

- Part Three will consist of a single cycle
- Whatever you earn in this cycle will be added to the two random cycles selected from Parts 1 and 2
  - So you will earn between $0.10 and $10.00 for this cycle
- We will ask you for your minimal cutoff in each new round as before
- If your coin flip is heads, you will choose an action according to your cutoff as before
- The only difference is that we will not tell you when and if you have made a choice until the end of the cycle

- You will submit cutoffs in rounds 1 to 3, as before
- In the round where you see your ball’s value, you will use your selected cutoff to make a decision
  - If your balls’ value is lower than your minimal cutoff you will exchange your ball with the one the computer is holding
  - If your balls’ value is equal to or greater than your minimal cutoff you will keep your ball
- Because of this procedure, nothing in the structure of the task has changed.
  - The new procedure allows us to collect information on the cutoff you would select in all three rounds
  - All that has changed from part two is the time at which we inform you on your choice for the cycle
Use the space below to communicate with the other team members.
You have 5 minutes to talk to each other.
After the chat is over, each team member will be assigned a different role (first, second or third mover) and be matched to a different group.
One of the team members will be randomly selected and this participant's earnings will determine the Cycle 21 payment for all members of the chat team.
Use the space below to communicate with the other team members.

You have 5 minutes to talk to each other.
After the chat is over, each team member will be assigned a different role (first, second or third mover) and be matched to a different group.
One of the team members will be randomly selected and this participant's earnings will determine the Cycle 21 payment for all members of the chat team.
You were Team Member C of your chat team. The participant randomly selected to determine the team payment was:
Team Member B

Team Member B was the second mover in this cycle and was initially holding Ball A, which had a value of 100. The coin flip was HEADS in round 1, and Team Member B's cutoff for round 1 was 80. Since the value of the ball was greater than the cutoff, Team Member B kept Ball A.
Hence, this cycle will pay $10,00
C.7. **Instructions for Part 4.** Finally we will conduct a number of survey questions for which there will be the chance of an additional payment.

Part four consists of three sets of questions, which will have a series of possible prizes. One participant in the room will be randomly selected for payment on these additional questions.

The first question is a decision making task. You will be presented with three balls. One of these balls is worth $10, while the other two are worth $0. The computer has shuffled the three balls, and fixed their locations.

1. We will ask you to choose one of the three balls.
2. After you have chosen a ball, we will reveal one zero dollar ball from the two balls that you did not choose.
3. We will then make you an offer:
   - Would you like the ball you initially chose plus $5?
   - Or would you instead like to switch to the remaining ball plus X times $0.10?

   X will vary between 1 and 100.

   We would like you to tell us the minimum value of X for which you would like to swap.
   - If you swap you get the value of the remaining unchosen ball (either 0$ or $10) plus $0.10 times X (so between $0.10 and $10)
   - If you keep your ball, you get its value (either 0$ or $10) plus $5.

Please make your choices for this task now.

[Wait while subjects complete task]

The next task will ask you to answer three numerical questions within a 15 second time-limit. Whoever is selected for payment in part 4 will receive $1 per correct answer.

[Wait while subjects complete task]

Finally, we would like you to make a series of choices between lotteries. In each choice you will be asked to pick either Lottery A or Lottery B, where each offers a probability over two monetary prizes.

One of your four choices from these lotteries will be selected for payment, and the outcome added to your total earnings if you are selected for payment in part four.
APPENDIX D. SCREENSHOTS

This is round 1 of cycle 1, and you are holding Ball C. Use the bar below to choose your cutoff value, then flip the coin. Heads, you see your ball’s value. Tails, you don’t.

If you see the value of your ball, and if:
(1) value is greater than or equal to 80, keep it; or
(2) value is lower than 80, switch it.

It’s TAILS!
Therefore, you don’t see your ball’s value in this round.

TAILS!
You do not get to see the value of your ball.
It's HEADS!
The value of your ball is 75, which is lower than your cutoff of 80.
Therefore, you switch it. You are now assigned to Ball D.

Ball A  Ball B  75  Ball D

HEADS!
Your ball's value is 75 and your cutoff is 80.
You now have Ball D.

It's HEADS!
The value of your ball is 85, which is higher than your cutoff of 80.
Therefore, you keep it.

Ball D  Ball A  Ball B  85

HEADS!
Your ball's value is 85 and your cutoff is 80.
You keep your original ball.
<table>
<thead>
<tr>
<th><strong>FINAL RESULT FOR CYCLE 1</strong></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>START</strong></td>
<td>Third mover</td>
</tr>
<tr>
<td></td>
<td>79</td>
</tr>
<tr>
<td><strong>CHOICE 1</strong></td>
<td>Third mover</td>
</tr>
<tr>
<td></td>
<td>79</td>
</tr>
</tbody>
</table>
| Who: 1st Mover  
When: Round 1  
Choice: Keep |
| **CHOICE 2** | COMPUTER | Second mover (you) | First mover | Third mover |
| | 79 | 100 | 65 | 100 |
| Who: 3rd Mover  
When: Round 1  
Choice: Switch |
| **CHOICE 3**  
(FINAL) | COMPUTER | Second mover (you) | First mover | Third mover |
| | 79 | 100 | 65 | 100 |
| Who: 2nd Mover  
When: Round 2  
Choice: Keep |
## Session-17, Group-1

A: Hey guys (t=6)
C: hello (t=14)
B: Hi, ideally we want the bot to have the lowest value (t=29)
A: true (t=36)
C: i think a decent cutoff is somewhere between 20-50 (t=51)
B: this way when one of us 3 get selected at random we will always not have the lowest value chosen (t=56)
B: I think a cut off between those values is ideal as well (t=69)
A: i usually put my cut-offs lower if i’m third mover, around 5-20 (t=91)
A: because the people before me are likely switching out a 1 value (t=107)
C: very true (t=116)
B: (t=127)
A: but i agree with 20-50 for first or second mover (t=138)
C: so if we are a first mover go between 20-50 and lower it if we are the later mover (t=144)
A: progressively getting lower with each round (t=144)
B: I think that is a good plan (t=144)
B: Also don’t forget the coin flip (t=150)
A: but we won’t know the outcome of it :( (t=158)
B: we can communicate if we get heads or tails (t=167)
B: for example (t=171)
B: if you are first mover (t=181)
B: quickly select your cutoff value at 40 or something around there (t=190)
B: oh shit nevermind (t=204)
A: my understanding is that this round, we won’t be told which round we’re making the decision, therefore we won’t know where we’re making our decision (t=216)
C: yeah, i think we just have to go in with the cutoffs we want (t=236)
B: Yea I made a mistake, I guess we will just have to follow the formula of putting in low cutoffs later on (t=247)
C: sounds good (t=251)
A: sounds good (t=253)
B: sounds good (t=262)
A: hey (t=9)
C: hello there (t=18)
B: hello (t=22)
A: so honestly i’ve just been making like 60 my cut-off? (t=37)
C: i’ve been doing about 60-65 for the first one (t=51)
B: i’ve been using 40 just to be safe (t=61)
B: what are u guys going to use on this round (t=74)
C: and then once i’ve gotten to the 2nd and 3rd rounds i’ve decreased it to like 40 and then 20 just to be safer (t=79)
A: uhh, i’m probably going to stick to 60ish (t=95)
A: idk really haha (t=99)
C: i think i’m going to do 60 too fr the first round (t=111)
B: lets hope it gives us all 100s (t=116)
A: yeah, forreal lol (t=121)
B: *all get 1s’ line up for dimes (t=141)
A: i’ve been just trying to calculate it so i atleast come out with $20 when i can (t=148)
A: wow, no don’t even joke (t=155)
A: haha (t=158)
B: lol (t=159)
C: did you guys decrease your cutoff as the rounds go on or no? (t=166)
A: the lowest i’ve gone is 50 (t=175)
B: i left mine at 40 worked good, either get 60+ or 1 (t=184)
A: because we’ve had some rounds where the values were really low, but not too many (t=191)
A: i usually got pretty lucky i think (t=199)
C: yea i havent had too many in the 20-50 range (t=205)
A: all right, 60s it is haha (t=255)
B: if we all have all put a high cut off do you think its better for this round (t=258)
A: uhhh I think that’s why I like being in the middle because it’s kinda a catch all. (t=281)
A: like you might not make a lot but you probs won’t get 0.01 either (t=291)
A: or .10 (t=295)

Session-17, Group-3

C: Hi! (t=9)
B: Hello! (t=11)
A: hi! (t=16)
C: How is everyone? (t=18)
A: kinda tired (t=23)
C: I hear ya. Me too (t=30)
B: Yeah (t=33)
C: Anyways, what would you guys like to do? (t=46)
A: Im not really sure (t=71)
C: Does anyone have any opinions about the study cut offs. (t=75)
B: my cutoff was 50, what are yours? (t=95)
C: I am not really sure either. I had lower cut off during my game (1-10) (t=100)
A: mine was 35 (t=109)
B: 1-10 was kinda low , i think (t=118)
C: How did 50 work? Or 35? (t=119)
B: 50 give a lot of chance to get 100 (t=127)
C: Did it give you higher chances of having a better ball? (t=132)
B: yeah, like i have 5 times 100 (t=151)
A: I dont necessarily think so (t=155)
C: Good. My lower numbers did not work well for me (t=166)
B: how many 100 did 35 get? (t=172)
A: mm maybe like 5 or 6 (t=186)
C: I did it because at the beginning of the experiment I had several 1s going back to 1s (t=197)
C: Yeah, I had like 4 100s, and a few 80s (t=211)
C: You did better (t=214)
C: So it sounds like a slightly higher cut off gives better odds (t=238)
C: Thanks for the good information (t=252)
A: okay so you want to go with the 50 (t=253)
C: I do (t=259)
C: Want to go with 50, sorry (t=264)
A: lol its cool (t=279)
C: It seems like it yields better probability in the outcome (t=280)
C: Cool (t=282)
C: Good luck to everyone n the rest of the study (t=297)

Session-17, Group-4
C: I’ve been using 60 as my minimum cutoff (t=22)
B: 50 seems to be the safest bet. Worked well so far. (t=28)
A: I’ve used 48 (t=33)
B: so lets averaged them? (t=53)
A: 49???? (t=64)
C: Around 52 or 53? (t=96)
C: If we average the 3 (t=102)
B: yeah it would be 52 and change (t=108)
C: Ok so how about 52 (t=117)
B: works for me (t=122)
A: hmmmm (t=130)
A: what about a little lower (t=161)
B: why lower? (t=169)
B: 51? (t=184)
A: what if it’s 50 (t=186)
B: ok (t=189)
C: Works for me (t=192)
A: and then we get a 1 (t=193)
A: ok so 50 (t=198)
C: soo what should we make the cutoff \( t=14 \)

B: As the rounds go on, the chances that the ball the computer is holding has a really small value increases \( t=31 \)

C: yea \( t=38 \)

B: because in previous rounds, if someone had a small value they probably switched and gave it to the computer \( t=55 \)

B: so what I’ve been doing is decreasing my cutoff values each round \( t=66 \)

C: so as the rounds go on we should increase the cutoff \( t=67 \)

A: go down by 5 for cutoff in each round? \( t=67 \)

B: decrease \( t=74 \)

C: decrease yea \( t=80 \)

C: my bad haha \( t=83 \)

B: I’ve been doing 50/40/20 \( t=93 \)

B: idk what you guys think haha \( t=99 \)

C: ive been doing 30/25/20 \( t=107 \)

C: because im cool with $3 \( t=121 \)

C: instead of potentially giving up close to $5 \( t=144 \)

B: yeah that’s true \( t=147 \)

A: ive been 45/35/25 \( t=148 \)

A: doesn’t matter to me \( t=156 \)

C: how about we do 40/30/20? \( t=163 \)

B: yeah sounds good \( t=169 \)

A: perfect \( t=173 \)

C: alright cool \( t=176 \)

C: lets make some money \( t=182 \)

B: we get matched with other people though anyway right haha \( t=185 \)

C: oh idk what the point of this then \( t=300 \)

B: just to share strategies I guess lol \( t=213 \)
C: true haha (t=220)
A: yeah it says above each team member will be assigned a different group (t=241)
C: well good luck (t=242)
B: yep to you guys too haha (t=258)
C: 's-.+( (t=278)

Session-17, Group-6
A: Hello World! (t=12)
C: Hello (t=16)
B: hello (t=18)
B: what cutoff number do you guys feel is best/ (t=64)
A: What have you folks been using as your cutoff? (t=65)
A: I've been doing 20 (t=82)
B: ive been using everything above 60 (t=85)
C: So here are my thoughts: The chance of you getting a low # that someone else switched out is based on which mover you are and what round it is. Typically I go with ~50 if I am mover 1 or 2 on the first round (t=96)
B: so is 50 the best option then? (t=129)
A: Alright so mover 1 and 2 we want to do 50 plus (t=131)
B: that sounds good (t=152)
C: Then drop down for each subsequent round. Because you get stuck with what you switch too and as time goes on that is much more likely to be a low # (t=154)
A: So thats round 1 (t=161)
A: Do the same thing for round 2?? (t=169)
B: yeah we should (t=191)
A: Alright want to drop to 40? (t=197)
B: it feels better biting doin (t=202)
B: yeah we should drop to 40 (t=218)
C: I'd say if you are mover 1 or 2, you do 50/40/20. Mover 3 40/30/20 (t=230)
A: Follow that exactly (t=230)
A: Do the same thing for round 2?? (t=239)
B: ok gotcha (t=255)
**Session-18, Group-1**

<table>
<thead>
<tr>
<th>User</th>
<th>Message</th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
<td>wazzzzzzzzzzzzzzzup any ideas? (t=9)</td>
</tr>
<tr>
<td>C</td>
<td>nooosee (t=16)</td>
</tr>
<tr>
<td>B</td>
<td>ive just put 40 everytime haha (t=33)</td>
</tr>
<tr>
<td>C</td>
<td>i did 49 everytime (t=46)</td>
</tr>
<tr>
<td>A</td>
<td>i did 10 everytime (t=58)</td>
</tr>
<tr>
<td>B</td>
<td>soooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooooo should we all put 10 (t=101)</td>
</tr>
<tr>
<td>C</td>
<td>hmmmnm (t=104)</td>
</tr>
<tr>
<td>C</td>
<td>my brain hurts i just wanna leave (t=129)</td>
</tr>
<tr>
<td>A</td>
<td>haha (t=136)</td>
</tr>
<tr>
<td>B</td>
<td>if i walk outta here with 20 bucks ill be happy (t=153)</td>
</tr>
<tr>
<td>C</td>
<td>ya if it’s less i’ll feel like i wasted my time (t=177)</td>
</tr>
<tr>
<td>C</td>
<td>but does 10 work? (t=185)</td>
</tr>
<tr>
<td>B</td>
<td>i think imma stick with 40, it worked for me nice (t=216)</td>
</tr>
<tr>
<td>C</td>
<td>same w 49 hehe (t=228)</td>
</tr>
<tr>
<td>B</td>
<td>alright so basically we all just wasted 5 min here (t=245)</td>
</tr>
<tr>
<td>B</td>
<td>GO TEAM (t=248)</td>
</tr>
<tr>
<td>C</td>
<td>other people r typin so much like damn what are they plannin out here (t=257)</td>
</tr>
<tr>
<td>B</td>
<td>world domination (t=266)</td>
</tr>
<tr>
<td>A</td>
<td>no idea (t=267)</td>
</tr>
<tr>
<td>C</td>
<td>may the odds be ever in your favor (t=288)</td>
</tr>
<tr>
<td>C</td>
<td>good buck yallllll (t=294)</td>
</tr>
</tbody>
</table>

**Session-18, Group-2**

<table>
<thead>
<tr>
<th>User</th>
<th>Message</th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
<td>hi (t=10)</td>
</tr>
<tr>
<td>C</td>
<td>I always put 42 (t=14)</td>
</tr>
<tr>
<td>C</td>
<td>every time (t=17)</td>
</tr>
</tbody>
</table>
A: I went 70 pretty much everytime (t=26)
B: im cool w/ 42 (t=35)
A: It was pretty successful (t=42)
C: 42 was also successful (t=52)
A: I got 100 a good amount of times (t=54)
C: i think they had higher values for the last part than the first (t=66)
C: is this the team or do we get new teams? (t=78)
A: We are collectively deciding on one value right? (t=79)
B: i also went in the 40’s and got 100 6-10 times (t=82)
C: me too (t=92)
B: this is our team (t=94)
B: idt we all have to say the same amount tho (t=111)
A: I know but are we deciding on one value collectively? (t=113)
B: i dont belive so, just talking about a gameplan (t=128)
B: i just dont want .10 tbh (t=167)
A: ok so who ever gets to decide, you guys want to go in the 40s? (t=171)
A: I would prefer to go a little higher (t=185)
A: like 60s (t=193)
A: ???? (t=226)
C: i guess the question is, if we all put the same number, will we get the lower thing that was given up by someone if they accepted the higher one (t=230)
B: i think we all play the round with other people like normal and then one of our values in chosen as our payment (t=261)
B: so we dont all have to say the same # (t=272)
A:Whats the point of the chat then? (t=387)
C: if we were all successful, should we just play as we had been (t=292)

Session-18, Group-3

B: any of you know what we’re supposed to be discussing? (t=47)
C: What the minimum should be? (t=57)
B: Awesome. Thanks (t=70)
C: maybe 60? (t=83)
A: so what value are you planning to set (t=85)
A: i guess it can be lower, around 50? (t=128)
C: that sounds good (t=137)
A: ~ (t=153)
C: so we all set 50 as minimum (t=212)
A: yes, i would do this (t=237)
B: we’ll all be matched to different groups. I’m not quite sure why it would matter whether or not we all picked the same (t=253)
A: oh (t=267)
A: i thought we will be in the same group... sry (t=284)
C: same (t=288)
A: good luck (t=299)
B: 50 would work if we were all together, but in separate groups, we should probably just do as before (t=309)

Session-18, Group-4

C: hello (t=56)
A: hello (t=64)
B: hey hows it going (t=81)
C: oh you know pretty solid (t=91)
A: i really have to pee (t=98)
C: thanks for the info (t=107)
B: Im getting sleepy (t=110)
C: (t=121)
C: lets all just get 100 on this last level so we’re gtd the $10 (t=121)
A: are we supposed to be deciding anything (t=136)
C: not really just talking about what our cutoffs will be (t=153)
A: mine has been 50 the whole time (t=163)
C: i set 50 for first mover and less for second & third (t=178)
C: but its mostly luck (t=193)
B: Mine are 20 if Im the first or second mover; after that I use 2 (t=229)
C: the key is to just get a 100 ball (t=266)
C: gl (t=298)
Session-18, Group-5

C: What’s good team (t=15)
A: hey team! My strategy has been to choose a low cutoff (between 10 and 25) since there seem be a lot of 1s in each round and it seems better to keep anything above 1 even if it’s not that high of a number (t=58)
A: sorry that was super detailed (t=95)
C: I’ve been going around 40-50. Mine have had a lot of 100s (t=121)
A: i think it is good to choose a higher cutoff if you are mover 1 (t=154)
A: i have just by chance been mover 3 a lot, which means I am more likely to get someone’s discarded 1 (t=170)
B: I choose a cutoff value of 50. Only because there seem to be more numbers above 50 than below (t=183)
C: I would agree with that. I sort of assume the later you move the worse the remaining ball is (t=195)
B: that’s true. It’s better to find out early what the value of your ball is. And keep it if it is high. (t=243)
A: I guess I am not sure if I should change my strategy of choosing a lower-ish cutoff to choosing a slightly higher one (t=268)
C: May the force be with you (t=298)

Session-18, Group-6

B: k, so is this supposed to be like economics omegle? (t=30)
C: basically (t=46)
B: anyone had an econ course yet? (t=56)
B: I’m minoring in it (t=62)
A: My strategy is to start my cutoff highest in the first round, usually around 50. Then I decrease it as the rounds go on, to 40 then to 30 or something like that. (t=78)
B: I figure this: (t=92)
B: 25% for both 1 and 100 (t=98)
A: But sorry no I have not taken any econ class in college. (t=98)
B: after that, 50% for above 50 (t=116)
C: Is it just me or is there more like a 75% chance of a 1 (t=121)
B: there is a 26% chance for a 1 (t=154)
B: 26% for 100 (t=144)
C: Right i get that but there’s been like 3 ones in each group i’ve been in (t=151)
B: the rest is random (t=153)
B: Yea, and I’ve been denied seeing my number like 30 times (t=171)
C: Like multiple 1’s so even when I switch a 1 I get a 1 haha (t=177)
B: it’s just bad luck haha (t=181)
B: happened to me too (t=188)
B: too* (t=190)
A: Same. (t=194)
B: yo who’s tryna get some food after? lol (t=213)
B: okay but just go 50 and then u have a 2/3rds chance of getting above your cutoff (t=240)
C: okie will do (t=256)
B: econ minor, business major, had stats, those are my qualifications xDD (t=274)
A: v hungry but I have class after :( But basically if you haven’t seen the number by the third time but like 2 as your cutoff so we dont get 10 cents plz (t=283)

Session-19, Group-1

B: So... does anybody have any strategies? (t=29)
B: Or is this all up to chance? (t=49)
C: i haven’t found any strategic way to go about this... just been picking a number i would be ok with getting (t=95)
B: Same (t=115)
A: ive been picking low numbers and it’s been turning out well for me. (t=128)
B: How low? (t=137)
A: like single digits (t=149)
A: but i feel like theres an element of teamwork here (t=167)
A: maybe two people pick low and one pick high? (t=174)
A: honestly i have no idea. (t=181)
B: We could try that (t=187)
C: ya works for me (t=196)
B: So, i’ll pick low I guess (t=206)
A: haha ok (t=213)
C: ill go high (t=226)
A: i can pick low \(t=228\)
C: how high do you guys want me to go? \(t=239\)
A: have you guys been picking high or low and how has that been working out for you? \(t=252\)
B: I've been picking 40 and have gotten mixed results \(t=266\)
C: i've been going around 30-40 and it's been going pretty good \(t=269\)
A: ok so maybe two high and one low \(t=279\)
C: when we say high what are we talking about \(t=294\)
A: because low has been real good \(t=294\)

Session-19, Group-2

C: What have you all been making your cutoffs? \(t=42\)
B: 30 fam \(t=66\)
A: I have been making mine at 50 \(t=71\)
C: i have been doing 50 too \(t=82\)
B: amateurs lol \(t=276\)

Session-19, Group-3

C: what cutoff's have you guys been using \(t=37\)
A: i used 30 for every one \(t=50\)
C: i used 60 \(t=57\)
B: I've been using 40 every time and it's been working pretty well \(t=60\)
A: yea i feel like the lowerish ones have pretty good outcomes \(t=82\)
C: alrighty so what do you guys suggest we use? 30,40? \(t=116\)
A: 35? \(t=126\)
C: sweet sounds good to me \(t=143\)
C: me* \(t=149\)
B: yeah that works \(t=155\)
C: lets make some cash $$$ \(t=290\)

Session-19, Group-4

C: hello \(t=25\)
B: hi (t=28)
A: hi (t=30)
B: i say we make cut off 55 (t=36)
B: or 60 (t=56)
B: idk (t=57)
A: i have been doing 60 or 65 (t=64)
C: ive been going lower (t=73)
B: like what (t=84)
C: like 40's (t=89)
A: has it been working well (t=94)
C: yeah (t=98)
C: but we could do 55 (t=113)
C: that makes sense (t=118)
A: same here so i say 55 (t=125)
B: okay perfect (t=136)
B: good talk (t=141)
A: good back ppl (t=150)
B: thx u too (t=155)
B: hopefully we all make $$$$$$$$$ (t=167)
A: in desperate need of it always (t=182)
B: #collegelife (t=191)

Session-19, Group-5

A: Hi (t=19)
B: hey (t=23)
C: Sup (t=27)
C: I usually go 75 50 25 (t=39)
B: i do 57, 54, 50 (t=60)
B: #risky (t=70)
C: i guess the goal is to get the lowest possible value into the computers hands (t=75)
A: I’ve been doing (t=76)
A: 50 (t=84)
A: how can we do that (t=122)
B: pray (t=148)
C: Basically lol (t=158)
C: There’s usually one ball thats pretty low (Like sub 10) right? (t=162)
A: yeah, but i have seen a couple low balls in one cycle (t=216)
B: yeah. i can only remember one instance where it was like 100 100 99 68 (t=220)
B: one for me was 1 1 1 1 (t=240)
A: i guess the smaller the cutoff we do the better for all of us for the most part? (t=276)
B: well, do good guys! maybe 30 as cutoff? (t=287)
B: or 20? (t=295)
C: 31 (t=297)
A: sounds good to me! (t=298)
C: :P (t=299)

Session-19, Group-6
C: Any ideas? (t=34)
B: Initial thoughts? (t=37)
A: 50? (t=67)
B: 35? (t=78)
C: Yeah 35 sounds good, so a lower value is switched out (t=121)
A: Cool (t=129)
B: It has worked well for me in previous cycles (t=155)
A: I’ve been doing well with 65 (t=176)
A: But 35 is more safe (t=188)
B: How many .10 vs 100? (t=193)
C: I agree with 35 (t=213)
B: 35 it is (t=240)
A: Sounds good (t=248)

60
A: hi guys (t=18)
B: hello (t=23)
C: hi (t=24)
A: what do we want to do for cutoffs (t=25)
B: what have u guys been doing prior (t=37)
B: ive been doing 70 (t=42)
C: i've been keeping mine pretty low, around 40 (t=53)
A: i think we should start out higher and then go lower in the rounds (t=61)
C: yeah i agree (t=71)
B: that would work (t=77)
A: so first round we should do 70? (t=98)
B: im good with that (t=111)
C: okay (t=113)
B: then have like 40 be our lowest (t=126)
A: ok (t=130)
C: sounds good (t=134)
A: so never go lower than 40? (t=162)
C: yeah maybe 40 for the last round? (t=176)
B: how many rounds are there again? (t=185)
C: 3 (t=194)
A: three (t=194)
A: so 70 for the first, 40 for the last (t=206)
A: what about the middle round? (t=217)
B: what about the second round (t=220)
B: 55?? (t=228)
C: somewhere in the middle (t=232)
A: that works (t=232)
C: yeah (t=234)
B: okay lol (t=236)
A: let’s hope were lucky haha (t=245)
<table>
<thead>
<tr>
<th>timestamp</th>
<th>user</th>
<th>message</th>
</tr>
</thead>
<tbody>
<tr>
<td>12</td>
<td>A</td>
<td>Hello world!</td>
</tr>
<tr>
<td>37</td>
<td>B</td>
<td>hey</td>
</tr>
<tr>
<td>43</td>
<td>C</td>
<td>Sup</td>
</tr>
<tr>
<td>48</td>
<td>A</td>
<td>Does anyone have a good strategy for this?</td>
</tr>
<tr>
<td>72</td>
<td>B</td>
<td>i’m just going to keep doing it how i did before tbh</td>
</tr>
<tr>
<td>85</td>
<td>A</td>
<td>Yeah thats how I feel too</td>
</tr>
<tr>
<td>92</td>
<td>C</td>
<td>same</td>
</tr>
<tr>
<td>108</td>
<td>B</td>
<td>cool cool</td>
</tr>
<tr>
<td>126</td>
<td>A</td>
<td>since we do not know the order of the chosers, we cannot really make a</td>
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<td></td>
<td></td>
<td>succinct gameplan</td>
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<tr>
<td>141</td>
<td>A</td>
<td>did anyone watch south park on wednesday?</td>
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<tr>
<td>159</td>
<td>B</td>
<td>nah i don’t like that show</td>
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<tr>
<td>162</td>
<td>C</td>
<td>No was it a good episode?</td>
</tr>
<tr>
<td>178</td>
<td>A</td>
<td>yeah it was pretty funny</td>
</tr>
<tr>
<td>193</td>
<td>C</td>
<td>I’ll have to watch it over the weekend</td>
</tr>
<tr>
<td>194</td>
<td>A</td>
<td>Im sorry for you member B</td>
</tr>
<tr>
<td>203</td>
<td>C</td>
<td>Anyone into AHS</td>
</tr>
<tr>
<td>227</td>
<td>B</td>
<td>nope guess i just live under a rock</td>
</tr>
<tr>
<td>228</td>
<td>A</td>
<td>I was invited over someones house tonight to watch it but ive never seen</td>
</tr>
<tr>
<td></td>
<td></td>
<td>it before</td>
</tr>
<tr>
<td>251</td>
<td>B</td>
<td>t-minus 1 minute thank god</td>
</tr>
<tr>
<td>256</td>
<td>C</td>
<td>It’s really good kind of a creepy show tho</td>
</tr>
<tr>
<td>258</td>
<td>A</td>
<td>yeah this sucks</td>
</tr>
<tr>
<td>264</td>
<td>C</td>
<td>yeah I just wanna nap tbh</td>
</tr>
<tr>
<td>273</td>
<td>A</td>
<td>#dicksoutforharambe</td>
</tr>
<tr>
<td>284</td>
<td>C</td>
<td>RIP</td>
</tr>
<tr>
<td>289</td>
<td>B</td>
<td>fuck penn state</td>
</tr>
<tr>
<td>294</td>
<td>C</td>
<td>****</td>
</tr>
</tbody>
</table>
Session-20, Group-3

C: greetings (t=8)
A: hello (t=27)
B: hey (t=44)
A: what are you thinking (t=49)
A: im not really sure how we help each other (t=66)
C: My thought process is to set your first cutoff highest, second cutoff lower than that, and third
cutoff lower than that because each successive round there is a greater chance that someone
else switched with the computer ball and therefore gave (t=89)
A: yeah thats a good idea (t=109)
C: so i usually set my fist at 50, second 40, and third about 45 (t=120)
C: third about 35*** (t=126)
B: well thats good with me (t=128)
A: yeah ive been always doing 50 pretty much but thats a better idea (t=138)
A: i will do that (t=150)
C: have you guys found a different successful strategy (t=168)
B: I just kind of eyeball it but If thats been working for you (t=176)
C: i dont think we’re allowed to say specifically but yes it’s been working for me (t=200)
A: okay good (t=208)
C: best of luck (t=215)
A: thanks you too (t=220)

Session-20, Group-4

C: if you’re the first mover set your threshold pretty high (t=42)
C: like around 60-75 (t=50)
C: if you’re second mover set it around 40-50 (t=63)
A: and go down depending on what mover you are (t=65)
C: and if you’re third set it at like 25 (t=74)
C: ¦what team member A said (t=85)
C: Team Member B you got it? (t=113)
B: yes (t=119)
C: alright cool (t=126)
<table>
<thead>
<tr>
<th>Session-20, Group-5</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A:</strong> hi (t=9)</td>
</tr>
<tr>
<td><strong>B:</strong> hey (t=29)</td>
</tr>
<tr>
<td><strong>C:</strong> hi (t=50)</td>
</tr>
<tr>
<td><strong>A:</strong> does anyone have a strategy (t=54)</td>
</tr>
<tr>
<td><strong>C:</strong> what do you think our cutoffs should be (t=59)</td>
</tr>
<tr>
<td><strong>A:</strong> I’ve just been using 50 (t=76)</td>
</tr>
<tr>
<td><strong>B:</strong> same (t=87)</td>
</tr>
<tr>
<td><strong>A:</strong> do you want to use that or something else (t=118)</td>
</tr>
<tr>
<td><strong>B:</strong> sounds good to me, C are you good with it? (t=144)</td>
</tr>
<tr>
<td><strong>C:</strong> I’ve actually used a cutoff of 2 for a lot of the rounds, seeing as that anything is better than a $0.10 payoff (t=166)</td>
</tr>
<tr>
<td><strong>C:</strong> but I’m good with 50 if you guys want to do that (t=179)</td>
</tr>
<tr>
<td><strong>A:</strong> has 2 worked for you for the most part (t=197)</td>
</tr>
<tr>
<td><strong>C:</strong> yeah for the most part, because when you use 50 I feel like you end up losing a lot of opportunities to get more than $0.10 (t=239)</td>
</tr>
<tr>
<td><strong>B:</strong> using 50 I don’t think I got .10 once and I got about 5 $10.00 (t=246)</td>
</tr>
<tr>
<td><strong>C:</strong> up to you guys though (t=252)</td>
</tr>
<tr>
<td><strong>A:</strong> doesn’t matter to me (t=270)</td>
</tr>
<tr>
<td><strong>B:</strong> how did 50 work for you A? (t=273)</td>
</tr>
<tr>
<td><strong>A:</strong> I only got .10 twice (t=292)</td>
</tr>
<tr>
<td><strong>C:</strong> okay we can do 50 (t=297)</td>
</tr>
<tr>
<td><strong>A:</strong> so good I guess (t=297)</td>
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<thead>
<tr>
<th>Session-20, Group-6</th>
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</thead>
<tbody>
<tr>
<td><strong>C:</strong> how should we choose (t=41)</td>
</tr>
<tr>
<td><strong>B:</strong> I have no idea (t=65)</td>
</tr>
</tbody>
</table>
C: i guess just make the cut off higher rather than lower? (t=88)
A: just make sure we get a number larger than 1 (t=97)
B: yeah thats what i was thinking. Make the cutoff a bit higher than usual. (t=122)
C: okay sounds good (t=134)
C: i feel like its all random anyway (t=210)
A: yeah theres no way to predict anything (t=227)
B: Lets just hope the person selected gets 100 off the bat (t=269)
C: yeah (t=278)
Appendix F. One-Parameter Reinforcement Learning Model

In this section we use a one-parameter reinforcement learning model, based on Erev and Roth (1998) and Luce (1959), to explain the differences in behavior across some of our treatments for subjects classified as stationary. Figure F.1 plots the average round-1 cutoff for supergames 6-20 for treatments Selection, S-Across, S-Deliberation, and S-Explicit. As is clear from the graphs, there is a marked difference in the cutoff choices between S-Explicit and the other treatments. Figure F.2 plots the same averages, but combining the treatments Selection, S-Across and S-Deliberation.

Figure A0.1. Average Cutoff for Stationary Subjects in Round 1

Note: Subjects are classified as stationary if their cycle-21 cutoffs satisfy $|\mu_1 - \mu_2| \leq 2.5$ and $|\mu_1 - \mu_3| \leq 2.5$, where $\mu_t$ is the cutoff value chosen in round $t$.

One possible reason for the observed difference is the type of learning available for subjects in each case. In the S-Explicit treatment, the adverse selection was implemented through a time-dependent rule, with the rematching pool comprised of all three balls in round 1, the two lowest-value balls in round 2, and the lowest-value ball in round 3. For this reason, there were no mover types in treatment S-Explicit, and, since half of the time subjects make a decision in round 1, there is an overall lower chance of observing a bad outcome after switching in S-Explicit compared to other treatments with first, second, and third movers.
In what follows, we estimate a simple one-parameter reinforcement model that best fit the data for treatments Selection, S-Across, and S-Deliberation using data only for subjects classified as stationary. Later, we use the optimal parameter estimate to predict round-1 cutoff values for the S-Explicit treatment, again restricting the analysis to the stationary subjects.

**The Model.** In supergame $s = 6$, each player $n$ has an initial propensity to choose the $k$-th cutoff value, where $k \in [1, 100]$, given by a non-negative number $q_{nk}(1)$. We assume that each player has an equal propensity for each of the possible cutoffs, such that:

\begin{equation}
q_{nk}(1) = q_{nj}(1) \quad \forall n \text{ and } \forall k
\end{equation}

The reinforcement of receiving payoff $x$ is given by the identity function:

\begin{equation}
R(x) = x
\end{equation}

Suppose that player $n$ chooses cutoff $k$ in supergame $s$. For supergame $s+1$, he adjusts the propensity of his $j^{th}$-cutoff according to:
\( q_{nj}(s + 1) = \begin{cases} q_{nj}(s) + R(x), & \text{if } j = k \\ q_{nj}(s), & \text{otherwise} \end{cases} \)

Finally, the probability that player \( n \) chooses the \( k^{th} \) cutoff in supergame \( s \) is given by:

\[
p_{nk}(s) = \frac{q_{nk}(s)}{\sum_{j=1}^{100} q_{nj}(s)}
\]

Equation (4) is Luce’s linear probability response rule. Note that, even though we assume that every cutoff has the same propensity at \( s = 6 \), we made no assumptions about the sum of propensities, which appear in the denominator of equation (4). This is the only parameter of the model, and, together with the size of the payoffs, it determines the speed of learning.

Let \( X \) be the average payoff of all players in all four treatments. The parameter \( s(1) \), which is assumed to be the same for all players, is given by:

\[
s(1) = \frac{\sum_{j=1}^{100} q_{nj}(1)}{X}
\]

which, together with (4), implies that the initial propensities are:

\[
q_{nj}(1) = p_{nj}(1)s(1)X
\]

Both \( p_{nj}(1) \) and \( X \) are known: the first by assumption, and the second from the data. All we need is an estimate for \( s(1) \). The value that minimizes the Mean Squared Deviation (MSD) using data from Selection, S-Across, and S-Deliberation is \( s(1)=0.162 \), which indicates an extremely fast learning speed.

Figure F.3 presents the same graph as in Figure F.2, but also plots the fitted values for Selection, S-Across, and S-Deliberation and the predicted values for S-Explicit.
Figure A0.3. Average Cutoff for Stationary Subjects in Round 1, Data and Prediction ($s(1) = 0.162$)

Note that both in S-Explicit and in the other treatments, subjects start choosing very similar cutoffs. The subsequent experience with realized payoffs, however, is different, which accounts for the increasing profile of S-Explicit cutoff choices and the decreasing profile of cutoff choices in the other treatments.