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

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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.

Date: April, 2018.

We would like to thank the following for their very helpful comments and input: Ignacio Esponda, David Huffman, Alessandro Lizzeri, Dan Levin, Muriel Niederle, Ariel Rubinstein, Emanuel Vespa, Lise Vesterlund and Georg Weizsäcker; as well as seminar and conference audiences at New York University, Princeton, University of California Santa Barbara, University of California San Diego, University of East Anglia, University of Virginia, Norwegian School of Economics, and SITE for helpful comments.
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 occured 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, Esponda 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.\footnote{See also Enke (2017) and Jin et al. (2015) for further work on subjects’ failures to understand the complexity of the environment.} 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.
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

\(^2\)Detailed 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.4,5

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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 Hypotheses

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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a three-question Cognitive Reflection Test (Frederick, 2005); and (c) a version of the standard Monty Hall problem. One participant per session was selected for payment in the part (iv) elicitations.

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 ($t = 1$).\footnote{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.} 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

\footnote{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 \( \mathcal{I}_t \), and the joint event that they both see their value and choose to switch as \( \mathcal{S}_t \). 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:

\[
G_{t+1}(x | \mathcal{I}_{t+1}) = \Pr \{ \mathcal{S}_t; \mu^*_t | \mathcal{I}_{t+1} \} \cdot F(x | x < \mu^*_t) + \Pr \{ \text{not } \mathcal{S}_t; \mu^*_t | \mathcal{I}_{t+1} \} \cdot G_1(x | \mathcal{I}_{t+1}, \text{not } \mathcal{S}_t).
\]

The optimal policy cutoff \( \mu^*_{t+1} \) for the player at the inductive step is simply the expected value of \( G_{t+1}(x | \mathcal{I}_{t+1}) \). 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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9For example, given the base case the first two rounds of the induction are: second-mover sees their value and infers that \( \Pr \{ \mathcal{S}_1; \mu^*_1 | \mathcal{I}_2 \} = \Pr \{ \mathcal{S}_1 \} = \Pr \{ \mathcal{I}_1 \} \cdot F(\mu^*_1) = \frac{1}{2} \), 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 \( G_2(x) = \frac{1}{2} \cdot F(x | x < 50.5) + \frac{1}{4} \cdot F(x) \), with expected value \( \mu^*_2 \). The third-mover therefore faces the distribution \( G_3(x) = \Pr \{ \mathcal{I}_3 \} \cdot F(\mu^*_2) \cdot F(x | x < \mu^*_2) + (1 - \Pr \{ \mathcal{I}_3 \}) \cdot F(\mu^*_2) \cdot G_2(x) \).

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

11The 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.

12The 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 \( \mathcal{I}_4 \) in the induction) knows that they did not switch in round one. So in the language of the induction, \( \Pr \{ \mathcal{S}_1 | \mathcal{I}_4 \} = 0 \) as \( \mathcal{S}_1 \subset \mathcal{I}_1 \), but \( \Pr \{ \mathcal{I}_3 \cap \mathcal{I}_4 \} = 0 \) given our information structure. Note, if players were different for each decision, the cutoffs would be strictly decreasing, as conditioning on \( \mathcal{I}_{t+1} \) 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.\(^\text{15}\) 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.\(^\text{16}\) 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.
Figure 3. First-Mover Cutoffs (Supergame 6–21)

Note: Bars depict 95 percent confidence intervals from a random-effects estimation across all cutoffs in supergames 6–21.

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. Aggregate estimates are produced by regressing the chosen first-mover cutoff $\mu_{ist}$ (subject $i$, supergame $s$, round $t$, and session type $j \in \{\text{NoSel, Sel}\}$) on a set of treatment-round dummies. The estimated aggregate cutoff $\hat{\mu}_j^t$ 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 $H_0: \hat{\mu}_j^t = \mu_j^{\text{NS}}$ column. Individually,

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.
### 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></th>
<th>Supergame 21</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimate</td>
<td>p-values</td>
<td></td>
<td>p-values</td>
</tr>
<tr>
<td>No Selection</td>
<td>Round 1, $\mu^*_1$</td>
<td>[51]</td>
<td>54.81</td>
<td>(2.45)</td>
<td>0.119</td>
<td>52.76</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\mu^*_2$</td>
<td>[51]</td>
<td>54.06</td>
<td>(2.47)</td>
<td>0.213</td>
<td>0.216</td>
</tr>
<tr>
<td></td>
<td>Round 3, $\mu^*_3$</td>
<td>[51]</td>
<td>53.94</td>
<td>(2.52)</td>
<td>0.268</td>
<td>0.244</td>
</tr>
<tr>
<td></td>
<td>Joint Tests:</td>
<td></td>
<td>0.335(\dagger)</td>
<td>0.238(\§)</td>
<td></td>
<td>0.875(\dagger)</td>
</tr>
<tr>
<td>Selection</td>
<td>Round 1, $\mu^*_1$</td>
<td>[51]</td>
<td>46.63</td>
<td>(1.23)</td>
<td>0.003</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Round 2, $\mu^*_2$</td>
<td>[35]</td>
<td>42.99</td>
<td>(1.28)</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Round 3, $\mu^*_3$</td>
<td>[28]</td>
<td>39.12</td>
<td>(1.35)</td>
<td>0.000</td>
<td>0.000</td>
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<tr>
<td></td>
<td>Joint Test:</td>
<td></td>
<td>0.000(\dagger)</td>
<td>0.000(\§)</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 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 ($H_0: \hat{\mu}_1 = \mu^*_1$ for treatment \(j\), round \(t\)) or the theoretical prediction ($H_0: \hat{\mu}_1 = \mu^*_1$). \(\dagger\)–Joint test of stationary cutoffs across the supergame ($H_0: \hat{\mu}_1 = \hat{\mu}_2 = \hat{\mu}_3$ for treatment \(j\)); \(\§\)–Joint test of PBE cutoffs in supergame ($H_0: 0 = \hat{\mu}_1 - \mu^*_1 = \hat{\mu}_2 - \mu^*_2 = \hat{\mu}_3 - \mu^*_3$).
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 $\hat{\mu}_3^S - \hat{\mu}_1^S = -0.517$. 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.\(^\text{18}\)

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.\(^\text{19}\) 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 members has an incentive to explain the environment to others.

---

\(^{18}\)One 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.

\(^{19}\)Results 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,\textsuperscript{20} the end behavior in supergame 21 is not significantly different from that observed in the \textit{Selection} environment.

\textbf{Robustness Conclusion.} As the results from our robustness treatments mirror the findings in our \textit{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.

\section*{6. Subject Heterogeneity, Learning and Behavioral Models}

In Section 5 we showed that subjects’ average cutoffs are decreasing across the \textit{Selection} supergames, but are stationary in the \textit{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 \textit{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.\textsuperscript{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 $\mu_{i1} > \mu_{i2} \geq \mu_{i3}$, where an exact Stationary-type satisfies $\mu_{i1} = \mu_{i2} = \mu_{i3}$. 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 $\mu_{i1} \geq \mu_{i2} + \epsilon$ and $\mu_{i2} \geq \mu_{i3}$, and an $\epsilon$-Stationary type is one that satisfies $|\mu_{i1} - \mu_{i2}|, |\mu_{i1} - \mu_{i3}| < \epsilon$\textsuperscript{22}

\textsuperscript{20}Every 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 #.”

\textsuperscript{21}The 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.

\textsuperscript{22}Figure 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></th>
<th>Non-Decreasing</th>
<th></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>
<td></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>
<td>57.6%</td>
<td>75.8%</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>
<td>36.4%</td>
<td>47.0%</td>
</tr>
<tr>
<td>Robustness:</td>
<td></td>
<td></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>
<td>45.0%</td>
<td>58.3%</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>
<td>33.3%</td>
<td>50.0%</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>
<td>47.2%</td>
<td>55.6%</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>
<td>40.3%</td>
<td>51.1%</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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23 Given 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.

24 In 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, \( \mu^i_{1,1} \) 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, \( \Delta \mu^i := \mu^i_{j,1} - \mu^i_{j,2} \), 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.\(^{25}\)

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.\(^{26}\) (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.\(^{27}\) 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.\(^{28}\) Indeed, evidence from the S-Peer

\(^{25}\)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.

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

\(^{27}\)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 \((p = 0.194)\).

\(^{28}\)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 (6-10)</th>
<th>(11-15)</th>
<th>(16-20)</th>
<th>$\triangle$Session</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>No Selection</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>52.2</td>
<td>54.7</td>
<td>54.0</td>
<td>1.8</td>
</tr>
<tr>
<td></td>
<td>(2.5)</td>
<td>(2.5)</td>
<td>(2.8)</td>
<td></td>
</tr>
<tr>
<td><strong>Selection</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decreasing</td>
<td>49.2</td>
<td>47.1</td>
<td>48.8</td>
<td>0.4</td>
</tr>
<tr>
<td></td>
<td>(2.2)</td>
<td>(2.6)</td>
<td>(2.4)</td>
<td></td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>50.2†</td>
<td>47.4</td>
<td>46.6†</td>
<td>-3.8†</td>
</tr>
<tr>
<td></td>
<td>(1.9)</td>
<td>(1.8)</td>
<td>(1.9)</td>
<td></td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th>Type</th>
<th>Supergame (6-10)</th>
<th>(11-15)</th>
<th>(16-20)</th>
<th>$\Delta$Session</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>No Selection</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>-0.2</td>
<td>1.2</td>
<td>0.1</td>
<td>0.3</td>
</tr>
<tr>
<td></td>
<td>(0.4)</td>
<td>(0.7)</td>
<td>(0.7)</td>
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<tr>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>6.3***</td>
<td>8.5***</td>
<td>9.2***</td>
<td>2.9*</td>
</tr>
<tr>
<td></td>
<td>(1.4)</td>
<td>(1.3)</td>
<td>(1.1)</td>
<td></td>
</tr>
<tr>
<td>Non-Decreasing</td>
<td>0.2</td>
<td>0.2</td>
<td>0.8**</td>
<td>0.7</td>
</tr>
<tr>
<td></td>
<td>(0.4)</td>
<td>(0.3)</td>
<td>(0.4)</td>
<td></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 $\mathbb{E}(\theta_R | t)$. If this conditional expectation is learned, a risk-neutral subject would use a cutoff that switches out any initial assignment $\theta_0 < \mu_t := \mathbb{E}(\theta_R | t)$. In the steady-state, any subject that switches out their assigned ball with some probability must build an accurate estimate of $\mathbb{E}(\theta_R | t)$ 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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29 Given 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.

30 Every 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
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 con-
ditioning 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.

Our first behavioral model for simplicity assumes that all participants ignore time and use a
stationary cutoff \( \bar{\mu} \) 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 condition-
ing 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 treatments)
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

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\(^{31}\)The 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 supersgames 1–10 and 11–20 as $\dot{\nu}_{G,10}$ and $\dot{\nu}_{G,20}$, 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):}

$$\mu_{21} = 35.7 \ (4.1) *** + 0.267 \ (0.087) ** \cdot \dot{\nu}_{G,10} + 0.036 \ (0.074) * \cdot \dot{\nu}_{G,20}. $$

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 behav-
ioral model.\footnote{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.} 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).\footnote{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.}

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.\footnote{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.} If all participants use a stationary cutoff $\bar{\mu}$, the final supergame outcome is drawn from the following CDF:

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

$\Pr \{V \leq x; \bar{\mu}\}$ 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.

$\Pr \{\text{Switch}; \bar{\mu}\}$ 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.
The referant 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.\(^{38}\) 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 \textit{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.\(^{39}\)

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 \textit{Selection} treatments, while the dashed line indicates the prediction in the \textit{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 \textit{Selection}, \textit{No Selection} and \textit{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 \textit{Selection} and \textit{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 \textit{No Selection} treatments. At the median level of elicited risk aversion, our value-based behavioral model predicts a steady-state cutoff of \(\bar{\mu}_V = 53\)

\(^{38}\)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.

\(^{39}\)Analogously, the risk-neutral prediction in the \textit{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.\(^{40}\)

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.

\(^{40}\)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,

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.
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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**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>$(N = 144)$</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>$(N = 87)$</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) $\mu^* = \hat{\mu}$; (ii) $\bar{\mu}_V^{\text{Rdm.}} = \hat{\mu}$; (iii) $\bar{\mu}_V^{\text{Fix.}} = \hat{\mu}$; 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.

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44 We 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 mechan-
ic 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.\footnote{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.} 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 condition-
ing 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 re-
sponses 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.
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Enke, Benjamin, “What you see is all there is,” July 2017.


