# Selection Pressure in Repeated Contests

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Competition for scarce resources under birth and death (the struggle for survival) has long shaped social and economic interaction. This study is a first effort to *induce* selection pressure in controlled $N$-player strategic decision-making experiments via performance-based participant replacement. We compare behavior in repeated Tullock contests with and without selection pressure. While prior experiments without selection pressure reveal excess competition inconsistent with profit maximization, we find that adding selection pressure *decreases* competitive intensity. Behavior under selection aligns closely with the finite-population evolutionarily stable strategy (ESS), as many participants adapt quickly to survive at the expense of new entrants. Without selection, behavior is more erratic and overly competitive. Selection pressure thus disciplines decision-making, lengthens contestant lifespans, and raises average round payoffs across the entire population.

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## 1 Introduction

Much of human history has been governed by the "perpetual struggle for room and food" (Malthus, 1798, p. 84), i.e., the competition among individuals or groups for scarce resources in the face of birth and death. In Economics, Alchian (1950) was first to point out that competition between firms and other agents in modern society involves similar selection pressures. In particular, just like the struggle for "biological survival", the struggle for "economic survival" features repeated contests in which unsuccessful agents (e.g., bankrupted firms) are replaced by "newly birthed" contestants (e.g., entrants) whereas successful agents (e.g., incumbent firms) continue operating. Friedman (1953) echos this analogy arguing that "natural selection" arguments can be used to justify the notion that firms maximize their expected returns.

In this paper, we investigate how human participants adapt their behavior to the presence of evolutionary selection pressures in an incentivized decision-making experiment. Specifically, we consider repeated Tullock contests, which are known to feature a conflict between profit maximization- and survival-incentives (Schaffer 1989, Hehenkamp et al. 2004, Alós-Ferrer and Ania 2005).

Our experiment has subjects play repeated contests where, as in previous experiments, they earn money based on their decisions. Differently from previous experimental studies, we introduce stochastic "birth and death" during sessions: contestants play an indefinitely repeated (randomly terminating) supergame, and following the final round of each supergame a *selection event* takes place in which one incumbent contestant is replaced by a fresh subject from a waiting room. Specifically, at each selection event the single lowest-payoff contestant from the preceding round (the last round of the supergame) is replaced, with ties broken at random. We compare this **Pressure** treatment to a **No Pressure** control treatment in which, after the final round of each indefinitely repeated supergame, one active participant is also replaced by a new entrant, but the participant who is replaced is determined *at random*, independently of their payoff in the last round of the repeated game.

Our **Pressure**-condition thus resembles the settings of *evolutionary game theory* (Weibull 1997, Nowak 2006, Sandholm 2010), which has successfully modeled both economic- and biological-selection pressures. As in evolutionary game theory, the relative payoffs of subjects serve as a *fitness basis* for determination of their (expected) lifespan in the experiment. While evolutionary game theory typically assumes fixed strategies, our experimental design allows human participants to adapt their strategies over time in response to selection pressure as the repeated contest unfolds over time. In particular, stage-game payoffs in the Tullock contest lead to a *mixed-motive game*, where the chance of surviving to play additional contests has to be traded off against the expected payoffs that can be earned in each contest round. This allows us to study how the pressure for survival faced by actual human decision-makers interacts with individual payoff maximization-incentives as traditionally considered in theoretical- and experimental-economics.

Indeed, the setting we study has real-world counterparts. For instance, it is similar to "rank and yank" programs at technology firms (e.g., General Electric, Microsoft, Amazon and Tesla), where employees are ranked based on their performance and the lowest-performing employees – typically the bottom 10% – are either let go or reassigned to less critical roles. Similarly, the dictum "publish or perish" associated with university tenure decisions, has an element of performance–based selection of talent.

Surprisingly, and contrary to theoretical predictions, we find that selection pressure *decreases* participants' competitiveness as measured by average investment levels across contests, relative to the **No Pressure**-control treatment. This happens because the pressure to survive exerts a strong, disciplining influence on participants' decision-making in the **Pressure**-treatment, with average investments closely approximating the survival-optimal level. In particular, a specifically simple behavioral rule (which we refer to as an "average unbeatable strategy" in Section 3) allows incumbent contestants under **Pressure** to all but ensure survival. By contrast, the absence of any connection between decision making and survival chances in the **No Pressure**-treatment leads to large fluctuations in investment levels, as well as to widespread and substantial over-investment beyond both profit- and survival-maximizing levels.

Strikingly, selection pressure does not merely redistribute profits to successful contestants. Rather, the average round payoffs for the entire population of contestants in the **Pressure**-treatment are increased relative to the **No Pressure**-treatment. Indeed, an important takeaway from this paper is the need to consider the survival pressure that agents may be operating under as an important payoff-relevant factor.

The remainder of the paper is organized as follows. Section 2 reviews related experimental work, including the studies most relevant to our selection-pressure design. Section 3 outlines the theoretical background on profit maximization and survival incentives germane to the repeated Tullock contest game that we study. Section 4 describes the experimental design. Section 5 presents the main findings, both at the aggregate and individual levels of analysis. Finally, section 6 concludes with a summary and suggestions for future research.

## 2 Related Literature

While experimental economists have studied zero-sum games and contests (see e.g., Dechenaux et al. 2015), implemented relative performance-based incentives (e.g., Andreoni 1995), and examined decision-making under induced time pressure (e.g., Kocher and Sutter 2006), under stress (e.g., Starcke and Brand 2012), and under cognitive load (e.g., Deck and Jahedi 2015), we are not aware of any experimental work involving explicit, pre-announced replacement of participants based on their performance relative to others.

The only prior experiment that explicitly examines survival versus payoff-maximization incentives is by Oprea (2014). However, his study involves a non-strategic environment, in which participants repeatedly withdraw funds from a stochastically replenished account, balancing the potential for additional earnings against the risk of bankruptcy from depleting the account. We suspect that the lack of experiments studying selection pressure in an $n$-player setting stems from the logistical challenges of implementing subject replacement in traditional laboratory environments. These difficulties have recently been mitigated by the feasibility of conducting online sessions with participants, as implemented in this study.

The closest *contest* experiment to ours is by Brookins et al. (2021), who compare indefinitely repeated two-player contests to finitely repeated contests of the same expected duration. In their indefinitely repeated contest, cooperative behavior (both players invest 0) is sustainable via trigger strategies, whereas it unravels by backward induction in the finitely repeated game. In our study, selection events are randomly determined so that both our **Pressure** and **No Pressure** treatments resemble indefinitely repeated games. However, unlike standard random termination, in our approach, only a subset of players exit after a terminal round (selection event), while others continue, complicating the use of history-dependent strategies such as grim trigger.

A number of experimental studies have implemented closely related overlapping generations structures in the laboratory (see, e.g., Cadsby and Frank 1990, Marimon and Sunder 1993, Lim et al. 1994, van der Heijden et al. 1998, Offerman et al. 2001, and Duffy and Lafky 2016). In these studies, new, inexperienced "young" subjects periodically replace "old" experienced subjects. However, the birth-and-death processes in these overlapping generation studies are all fully *deterministic* and are *not* based on relative payoffs as in our design. Moreover, in some of these studies, subjects are "reincarnated" (the old become young again) due to the logistical challenges of recruiting new participants and dismissing others. We do not allow for such reincarnation in our approach.

Lastly, several experiments (e.g., Huck et al. 1999, Offerman et al. 2002, Friedman et al. 2015) have examined selection by imitation in settings where the underlying payoff structures are unknown, partially known, or complex. In contrast, our study introduces a novel form of selection pressure by explicitly and transparently pre-announcing the *replacement* of participants based on their relative performance—an approach that, to our knowledge, has not been implemented in prior experimental work.

## 3 Theory

Many competitive interactions in biological and socioeconomic environments share the incentive structure of rent-seeking contests (Tullock 1980). Examples include mate selection, military conflict, R&D races among firms, promotion tournaments within organizations, litigation, political lobbying, fundraising, and charitable auctions. In this section, we outline the theoretical framework that underlies our experimental design which makes use of an indefinitely repeated contest. We show that, in the finite-population setting we study, the evolutionarily stable strategy (ESS) prediction differs from the Nash equilibrium prediction. Moreover, under our assumption that rewards are allocated deterministically in proportion to relative investment, we show that the ESS is uniquely unbeatable on average.

Formally, consider a population of $n$ active players competing for a resource of common value $V > 0$. Each player $i$ chooses an effort or investment amount $x_i \geq 0$. Given an individual investment $x_i$ and a vector of opponents' investments $x_{-i} = (x_j)_{j \neq i}$, players are deterministically awarded shares in the resource,[1] with $i$'s share following:

$$p_i(x_i, x_{-i}) = \begin{cases} \frac{x_i}{\sum_j x_j}, & \text{if } \sum_j x_j \neq 0, \\ \frac{1}{n}, & \text{otherwise.} \end{cases}$$

Player $i$'s payoffs are thus given by $\pi_i(x_i, x_{-i}) = p_i(x_i, x_{-i})V - x_i$. Given an endowment of $X$, the period payoff maximization problem is:

$$\max_{x_i \leq X} \{\pi_i(x_i, x_{-i})\}.$$

Maximization yields the best-response function:

$$x_i^{\pi}(x_{-i}) = \sqrt{\sum_{j \neq i} x_j V} - \sum_{j \neq i} x_j.$$

In the unique Nash equilibrium of this symmetric $n$-player game,

$$x_i^{NE} = \frac{n-1}{n^2} V.$$

Tullock contests belong to a class of games for which Alchian's (1950) intuition that evolutionary pressure changes the predictions of economic models holds true in a radical way (also see Hehenkamp et al. 2004): That is, a player seeking to maximize their chance of survival and, hence, maximizing their payoff relative to the group average will choose a strictly higher investment level than does a player who is simply maximizing period profits. In particular, the equilibrium among players maximizing relative payoffs– Schaffer's (1988) finite population ESS – implies strictly higher investment levels than the standard Nash equilibrium.[2]

[1] A commonly used alternative implementation stochastically awards the full resource to one player. In this "winner-take all" implementation, $p_i(x_i, x_{-i})$ is the probability with which player $i$ wins the resource. In our experiment, deterministic proportional allocation of resource shares based on relative investments was chosen over the stochastic allocation of the full resource to make the connection between investment decisions and survival chances in the repeated contest less noisy and more easy for subjects to comprehend. Theoretically, assuming risk neutral preferences, there should be no difference in equilibrium outcomes between these two methods of allocating the prize.

[2] As seen below, the difference between Nash equilibrium and finite-population ESS is decreasing in $n$. In particular, for an infinite number of players, each with vanishing influence on aggregate profits, the difference disappears altogether. This corresponds to the more widely known analysis of evolutionary stability for a *continuum* of players (Weibull 1997), in which ESS becomes a *refinement* of Nash equilibrium with further stability properties with respect to mutants making up a "small" share of the infinite population.

There also exists a class of games for which ESS and Nash equilibrium coincide for arbitrary finite numbers of players, see Hehenkamp et al. (2010).

To see this, consider the maximization problem

$$\max_{x_i}\left\{\pi_i(x_i,x_{-i})-\frac{1}{n}\sum_i \pi_i(x_i,x_{-i})\right\}\sim \max_{x_i}\left\{\pi_i(x_i,x_{-i})-\frac{1}{n-1}\sum_{j\neq i}\pi_j(x_j,x_{-j})\right\}.$$

Noting that $x_i \neq 0$ is not optimal at $\sum_{j\neq i} x_j = 0$,[3] $i$'s relative-payoff best-response function $x_i^s$ follows from taking first-order conditions:[4]

$$x_i^s(x_{-i})=\sqrt{\frac{n}{n-1}\sum_{j\neq i} x_j V}-\sum_{j\neq i} x_j.$$

Under relative payoff maximization, investment choices are uniformly higher than under profit maximization, i.e., $x_i^s(x_{-i}) > x_i^\pi(x_{-i})$ for all $x_{-i}$. And, in particular, the finite-population ESS $x^{ESS}$ (equilibrium among relative payoff-maximizing players), implies strictly higher investment levels than under the standard (profit-maximizing) Nash equilibrium:

$$x^{ESS}=\frac{V}{n}>x^{NE}=\frac{n-1}{n^2}V.$$

Hence, Tullock contests provide an ideal incentive structure to observe experimental subjects trading off profit maximization- against survival-incentives.[5]

In addition, ESS-play in the $n$-player Tullock contest has an extra, previously unstudied, stability property. That is, strategy $x^{ESS}$ is uniquely "unbeatable on average":[6]

**Proposition 3.1.** *(ESS is uniquely unbeatable on average)*
$x^{ESS}=\frac{V}{n}$ *is the unique strategy in the Tullock contest such that*
$\pi_i(x^{ESS},x_{-i})-\frac{1}{n-1}\pi_j(x^{ESS},x_{-i})\geq 0$ *for all* $x_{-i}$.

*Proof.* To see that $x^{ESS}$ is unbeatable on average, note that

$$\pi_i(x^{ESS},x_{-i})-\frac{1}{n-1}\sum_{j\neq i}\pi_j(x^{ESS},x_{-i})=\left(\frac{V}{n}-\frac{\sum_{j\neq i}x_j}{n-1}\right)^2\frac{n-1}{V/n+\sum_{j\neq i}x_j}\geq 0.$$

To see that $x^{ESS}$ is *uniquely* unbeatable on average, let $x_{-i}^{ESS}$ be the $n-1$-row vector of ones

[3] This follows from observing that $i$'s relative payoff for $\sum_i x_i = 0$ is 0, whereas investing $0<\varepsilon<V$ would yield a relative payoff of $V-\varepsilon$. A similar argument shows that $\sum_i x_i = 0$ never occurs among profit-maximizing contestants.

[4] The value function's second derivative is negative everywhere, showing that a local optimum derived in this way is, in fact, global. This is also true for standard profit maximization in the Tullock contest.

[5] More generally, such incentives obtain for quasisubmodular aggregative games as in Alós-Ferrer and Ania (2005). Next to more general versions of Tullock contests (allowing for Tullock's-$r$ in the range $0<r<\frac{n}{n-1}$), this includes popular games featuring strategic substitutes such as Cournot competition, and the Tragedy of the Commons. While all of these games would be suitable in terms of featuring the trade-off described above, $x^{ESS}$ is not necessarily "unbeatable on average" as described below.

[6] This property was first suggested by Possajenikov (2023). He proves that the finite-population ESS is unbeatable in 2-player Cournot games with decreasing demand function. In particular, this includes 2-player Tullock contests with Tullock $r\leq\frac{n}{n-1}$. Possajenikov's results do extend to average unbeatability in some $n>2$-player versions of these games. In particular, the finite-population ESS is unbeatable on average for $n>2$-player Tullock contests with $r\leq 1$.

multiplied by $x^{ESS}$, and note

$$\pi_i(x_i, x_{-i}^{ESS}) - \frac{1}{n-1}\sum_{j\neq i}\pi_j(x_i, x_{-i}^{ESS}) = -\frac{(x^{ESS}-x_i)^2}{x} \leq 0$$

with equality iff $x_i = x^{ESS}$. □

Thus, independent of other contestants' behavior, playing $x^{ESS}$ guarantees a (weakly) positive relative payoff. While this is not sufficient to guarantee survival in a general stochastic setting, it *does* the job in our **Pressure**-treatment described below, where incumbent contestants face a risk of replacement iff their payoffs are below the population average. Thus, in our repeated contest experiment described below, we expect that $x^{ESS}$ will be a salient choice for participants focusing on surviving in our repeated contest experiment. Furthermore, to the extent that participants pick up the average unbeatability of $x^{ESS}$, we expect widespread investments at that level and little change of investment levels around $x^{ESS}$ in reaction to other participants' investment behavior.

# 4 Experimental Design

The experimental design implements selection pressure in indefinitely repeated Tullock contests. We consider a constant population of $n = 4$ active participants who repeatedly compete for a resource of value $V = 100$. The contest success function is the same as in Section 3, with each contestant choosing an investment amount from a fixed per-round budget of $X = V$. Any unused portion of this budget, as well as any contest winnings, contribute to per-round profits.[7] Investment budgets are non-transferable across rounds. Since the endowment is the same, constant amount for all participants, it does not affect equilibrium calculations.

Given our setup and parameterization, the evolutionarily stable strategy (ESS) investment in the one-shot contest is $x^{ESS} = 25$, while the Nash equilibrium investment is $x^N = 18.75$. Under the ESS, total expenditures equal the prize value so dissipation is complete, $\frac{nx^{ESS}}{V} = \frac{4\cdot 25}{100} = 1$. By contrast, the Nash equilibrium implies *under*dissipation, $\frac{nx^N}{V} = \frac{4\cdot 18.75}{100} = \frac{3}{4}$. Taking the endowment into account, each player would earn a payoff of 100 if all four invested at the ESS level, compared with 106.25 if all four invested at the Nash equilibrium level.

In our design, the Tullock contest is played repeatedly by the $n = 4$ participants. The interaction always lasts for at least one round and continues for an indefinite number of additional rounds. After each round, there is a fixed and commonly known probability, $\rho = 0.1$, that a replacement event occurs. When such an event takes place, one incumbent contestant is eliminated and replaced by a new subject drawn from the waiting room who has not previously participated in the experiment.

Replacement events also govern the termination of the game. Specifically, we draw game lengths from a negative binomial distribution with 11 replacement events in total using probability $\rho = .1$,

[7] Specifically, the profit to player $i$ in our experiment is: $\pi_i(x) = V - x_i + V\cdot\frac{x_i}{\sum_j x_j}$, with $\frac{x_i}{\sum_j x_j} := \frac{1}{n}$ if $\sum_j x_j = 0$.

where the 11th replacement event simply terminates the game. Hence, each of our sessions features 10 replacements of a contestant (replacement events) before the game's termination, with a random number of intermittent rounds between any two replacement events. The number of replacement events is not disclosed to participants.[8] All game configurations (i.e., numbers of intermittent rounds between replacement events) are pre-drawn and balanced across treatments.

We employ two distinct treatments, corresponding to the extremes of no selection pressure and deterministic selection based on relative payoffs. In our first **No Pressure** control treatment, contestant replacement is fully random. Conditional on a replacement event, each of the four participants faces the same probability of being replaced, $\frac{1}{4}$, regardless of their prior decisions or payoffs, and this feature is common knowledge. Because replacement is not related to relative performance, subjects have no incentive to adjust their behavior in response to the threat of elimination, so this treatment provides a natural benchmark without selection pressure.

In our second **Pressure** treatment, replacement is deterministic and explicitly based on relative performance. Conditional on a replacement event, the worst performing active participant is selected for replacement with certainty, and this fact is also common knowledge. Performance is measured by relative payoffs in the contest round immediately preceding the replacement event. To avoid portfolio effects across contests, we use the outcome of a single contest round rather than the sum or average of multiple rounds. In cases of multiple worst-performers, random tie-breaking is used to determine which contestant is replaced.[9]

Figure 1 below shows the decision interface used in the experiment. Participants used a slider to submit investment amounts in lab dollars L$ from their budget of L$100 in each contest round. They used a second slider (above the investment choice slider) to specify their expectation of the average investment amount of their opponents. As an alternative to the sliders, contestants could directly input investments and predictions in a field above the respective slider. A dynamically updating pie chart showed the expected split of the resource given a subject's investment and their prediction of their opponents' average investment amounts. The expected share of the resource and the resulting expected payoff for the players and his opponents was displayed at the bottom of the decision screen. Subjects could experiment with different predictions and investment choices and the calculator would display the consequences of those choices (see the bottom of Figure 1 for their own expected earnings as well as those of their opponents. Prediction in puts and investment choices were not final until the subject clicked on the "Send" button. By using this prediction tool,

[8]From the contestant's perspective, the game features an unknown number of replacement events arriving at rate $\rho = .1$. Statistically, the game's unknown length from the participant's perspective can be modeled as a geometric process with an additional, uncertain parameter $\delta \in (0, 1)$ determining termination.

[9]Since only the last round before a replacement event is relevant, and replacement events arrive randomly, incentives are equivalent to a procedure where a single round is drawn at random for evaluation. Using the last round has the additional benefit that the feedback participants receive about payoffs is identical whether or not a replacement occurs.

it is possible for players to discover the unbeatable on average strategy.

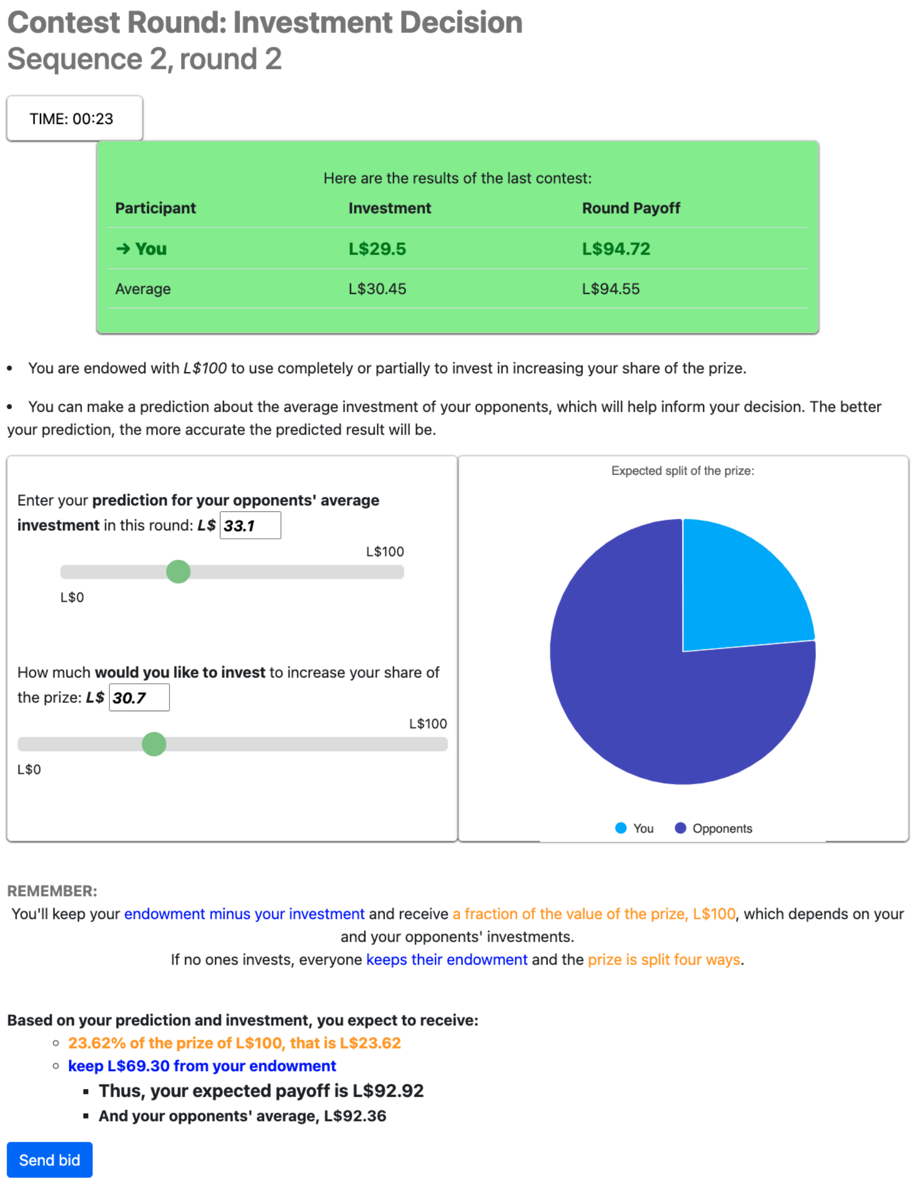



Figure 1: **Decision screen used in the experiment**

Beginning with the second round of each repeated game, the top of the decision screen displayed a green summary box reporting the prior round's outcomes: each contestant's own investment (bid) and payoff, together with the group-average investment and payoff. In rounds without replacement, the prior round's inputs were pre-populated as default entries for the current round prediction and investment choices. The decision screen also included a countdown timer that reset to 30 seconds at the start of each round and ticked down to zero. This time limit was intentionally *soft*: exceeding it carried no penalty, and default entries were *never* submitted automatically; instead, a choice was recorded only when the contestant actively clicked the "Send" button. We adopted these interface features to reduce frictions and decision errors and thereby focus participants' attention on the strategic problem they faced.

Note that the prediction of the other contestants' average investment was unincentivized (to avoid complicating instructions and incentives). This prediction feature was added to assist subjects in making their investment decisions. Although we did not pay participants separately for their predictions, as noted earlier, expected earnings were based on these predictions, so prediction accuracy was *endogenously incentivized*: more accurate beliefs improved expected-earnings assessments and thus supported better investment decisions, which did have payoff consequences.

As noted, after the initial round and every round thereafter, continuing contestants in both treatments are shown their own investment and payoff, as well as the average investment and payoff of the other contestants in the previous round. Importantly, we did not furnish new entrants with historical data on prior gameplay. This design choice serves two purposes. First, it ensures that each entrant begins with the same information set they would have upon entering an ongoing contest: they observe the contemporaneous feedback available from their first round onward, but do not receive a curated summary of what transpired before they arrived. Second, it preserves a clear experiential channel through which beliefs evolve, so that any differences in behavior between incumbents and entrants can be attributed to differential *experience within the environment* rather than to information transfers or "second-hand" histories.[10] In addition to the previous round's results, contestants were informed each time a replacement event occurred, including when they were or were not selected for replacement.

Subjects were recruited from UC Irvine's ESSL student subject pool and from UC Santa Cruz's LEEPS student subject pool to remotely participate in our study.[11]

We have data for 12 sessions of each treatment (24 sessions in total) with 14 subjects per session, or 336 subjects in total. As described above, sessions were run online with a constant number of 11 replacement cycles (with the final replacement event terminating the game) and random numbers of rounds in between replacement events. Sessions lasted around 2 hours, though subjects were instructed that a session could last up to 3 hours (180 minutes).[12] Twelve allocations of rounds (supergame lengths) were pre-drawn, with each allocation being implemented for one session of the **Pressure**- and one session of the **No Pressure**-treatment. Table 1 reports the number of rounds between each replacement cycle in the twelve allocations. This design allows for stochastic variation in the number of rounds between selection events, while the pairing of treatments to

[10]This approach also aligns with evidence that individuals' expectations are disproportionately shaped by their own *lived* experiences; see, e.g., Malmendier and Nagel (2011).

[11]We used subjects from both schools as the number of subjects needed for this study exceeds the typical supply available at a single campus. We find no statistical differences in investment behavior between these two samples.

[12]We chose to have just 10 replacement events (11 cycles) as we did not want to make the duration of the experiment too burdensome for subjects who began their participation in the first cycle, and who may have survived many or all 10 replacement events. A total of 13 subjects in the experiment started in the first cycle and went on to survive the entire experiment (4 subjects in **No Pressure** and 9 subjects in **Pressure**).

each allocation ensures that the randomness is balanced between the two treatments. The average number of rounds over all 24 session-pairs was 96.

The experiment was computerized and programmed using oTree (Chen et al., 2016). At the starat of each session, subjects first signed in using the Zoom software to verify their identities. Then subjects were sent remote links to the study. After opening these links, the first page asked subjects permission to enable notifications. Each session started with 4 active subjects making choices in a 4-player contest. Subjects yet to enter the session were instructed to wait patiently; a timer on their screens indicated the estimated time until their participation, and they remained connected to the experimenter via Zoom (with chat among participants other than the experimenter disabled). At some time prior to their entrance into the study, a notification and sound went off on these subjects' computer screens to alert them that their participation would begin soon. At that stage, subjects were presented experimental instructions for their treatment and had to complete a related comprehension quiz. The full instructions and quizzes are shown in Appendix B. After completing the quiz, a subject entered the game following a replacement event. In this manner, all subjects read instructions at the same time, that is just prior to their participation in the study. The subject who was removed from the session at each replacement event either randomly (**No Pressure**) or based on relative payoff (**Pressure**) was informed of their removal. They then had to answer an exit questionnaire that is shown in Appendix section B.11. In this questionnaire, subjects provided demographic information, their educational background and self-assessed measures of risk, patience, and competitiveness. They also answered four cognitive reflection test (CRT) questions. After that, they were told their payoff from the study, and they were no longer able to make decisions in the experiment; once participation was over, subjects were free to close their study links and leave the session. They were paid their earnings electronically shortly after the session had concluded. In this manner, the population of active subjects was kept constant at 4 in every round of a session. At the end of the final cycle, instead of another replacement event, the session was declared over and all four remaining subjects were informed of this outcome (they did not know in advance which replacement cycle would result in termination of the session). They then answered the exit questionnaire and received payoffs in the same manner as the subjects who left before them.

Subjects were given a budget of L\$100 in every round of every cycle they participated in and could not invest more than that amount, so it was not possible for any subject to lose money in our experiment. Subjects were instructed that every L\$20 earned in a Tullock contest selected for payment amounted to \$1 in earnings. In addition to a \$10 show-up fee, participants received their earnings from the last round of *each* replacement cycle in which they were active players. Since subjects did not know when the final round of a cycle would occur, this payment rule was equivalent to randomly selecting one round of each cycle for payment.[13] Thus, subjects in the

[13]Paying only the *last round* of each randomly terminated cycle is theoretically robust, as it preserves dynamic

Table 1: Number of contest rounds within each of the 10 replacement cycles, Sessions 1–24. Cycle 11 was the final cycle in each session.

| | Cycle | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | $\sum$ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Sessions | 1, 2 | 2 | 2 | 12 | 7 | 6 | 24 | 17 | 3 | 3 | 3 | 3 | 82 |
| | 3, 4 | 8 | 8 | 12 | 4 | 13 | 4 | 1 | 6 | 8 | 35 | 3 | 102 |
| | 5, 6 | 5 | 3 | 30 | 9 | 2 | 6 | 5 | 23 | 7 | 12 | 5 | 107 |
| | 7, 8 | 2 | 6 | 8 | 10 | 10 | 16 | 4 | 7 | 1 | 13 | 5 | 82 |
| | 9, 10 | 4 | 2 | 4 | 21 | 2 | 9 | 16 | 13 | 4 | 9 | 1 | 85 |
| | 11, 12 | 11 | 1 | 8 | 2 | 26 | 5 | 1 | 10 | 3 | 17 | 2 | 86 |
| | 13, 14 | 4 | 11 | 6 | 8 | 5 | 30 | 1 | 11 | 17 | 16 | 2 | 111 |
| | 15, 16 | 6 | 6 | 3 | 12 | 19 | 6 | 4 | 31 | 4 | 8 | 10 | 109 |
| | 17, 18 | 9 | 7 | 2 | 3 | 6 | 9 | 31 | 3 | 1 | 8 | 12 | 91 |
| | 19, 20 | 5 | 1 | 8 | 8 | 2 | 15 | 12 | 3 | 28 | 7 | 20 | 109 |
| | 21, 22 | 9 | 5 | 3 | 4 | 6 | 24 | 2 | 8 | 21 | 4 | 2 | 88 |
| | 23, 24 | 4 | 3 | 15 | 22 | 9 | 13 | 5 | 2 | 14 | 1 | 12 | 100 |

**Pressure** treatment had a strong incentive to survive, as doing so enabled them to participate in more cycles and thus receive more payments. Participants were paid electronically, using the platform of their choice (Paypal, Venmo or Zelle) and earned an average of $25.44 including the $10 show-up fee, with a minimum payoff of $12.82 and a maximum of $68.02. We will discuss further the payoff differences between the two treatments in the next section.

# 5 Main Results

In this section, we present our main experimental findings. We start with aggregate comparisons between the **No-Pressure** and **Pressure** treatments, and then examine individual-level behavior within each treatment.

## 5.1 Aggregate Investment

The left panel of Figure 2 reports the average investment levels for the two treatments, **No Pressure** and **Pressure**, considering all rounds of all 11 cycles. For reference purposes, the figures include both the Nash and ESS predictions. We further include a reference to the findings of the empirical literature ('Lit') regarding the mean investment fraction of endowment bid in a four player contest experiment by Sheremeta (2010) without selection pressure or random replacement. [14] While the average investment level is not significantly different between the **Pressure** and the

incentives under any risk preferences and avoids distortions from paying all rounds or a randomly chosen subset of rounds–see (Sherstyuk et al., 2013) for details.

[14](Sheremeta, 2010, Table 4.1) reports that in a one-shot, four player contest, the mean subject bid was 34.1 out of an endowment of 120, or 28.4%.

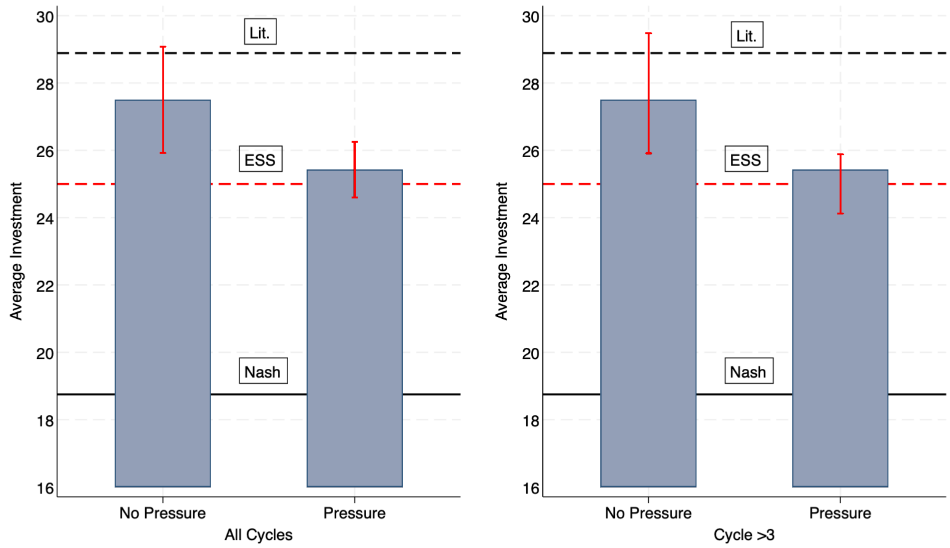



Figure 2: Average Investment Levels by Treatment. Bars: 95% confidence interval.

**No Pressure** treatments using all cycles, when we eliminate the first 3 cycles and look only at average investment in cycles 4-11, the difference between the two treatments is significant at the 5% level, with investments being lower under **Pressure** as compared to **No Pressure**. Starting the analysis at cycle four is justified because, assuming each player has a 3/4 chance of surviving a given cycle, this is the earliest point at which a majority of initial players are expected to have been replaced and the replacement mechanism can meaningfully influence group composition. Note further that for both treatments, investments significantly exceed $x^{NE}$ and they significantly exceed $x^{ESS}$ in the **No Pressure** treatment.

Table 2 reproduces these investment comparisons numerically and reports the corresponding standard deviations. The table reveals that the dispersion of investments in the **No Pressure** treatment, as measured by the standard deviation, is substantially greater than in **Pressure**. This difference remains robust when restricting the analysis to the "stable" stage, defined as excluding the first three cycles.

Figure 3 plots average investments across the 10 sessions in each treatment by replacement cycle, in the left panel and by supergame/contest round in the right panel. While cycle investment levels are not very distinct across treatments in the first three cycles, they become more distinct in the later cycles. Further, investment levels in **Pressure** tend to display low variance and closely track the ESS-investment level. By contrast, investments in **No Pressure** are higher and more volatile, rarely falling below the ESS-level. The right panel of Figure 10 reveals that over the course of a cycle, investment levels become more similar between the two treatments, so the cycle differences stem largely from investment level differences in the first few rounds (of which there are many more observations, given our choice of $\rho = .10$ for the replacement event probability.)

Table 2: Investment Levels, Pressure vs. No Pressure

| | | Pressure | CI[1] | No Pressure | CI[1] | Nash | ESS |
|---|---|---|---|---|---|---|---|
| All Cycles | Mean | 25.43 | [24.60, 26.26] | 27.50 | [25.92, 29.08] | 18.75 | 25 |
| | Std.dev. | 12.40 | [11.61, 13.26] | 19.41 | [18.66, 20.21] | | |
| Stable stage[2] | Mean | 25.00 | [ 24.12, 25.87] | 27.70 | [25.92, 29.47] | 18.75 | 25 |
| | Std.dev. | 10.86 | [9.97, 11.84] | 19.00 | [18.15, 19.90] | | |

[1] Mean: 95% confidence interval with errors clustered at the subject-cycle level; Std.dev.: 99% Bonett confidence interval.
[2] Replacement cycles 4–11.

However, the variance of investments at the round level remains higher in **No Pressure** compared to **Pressure**.

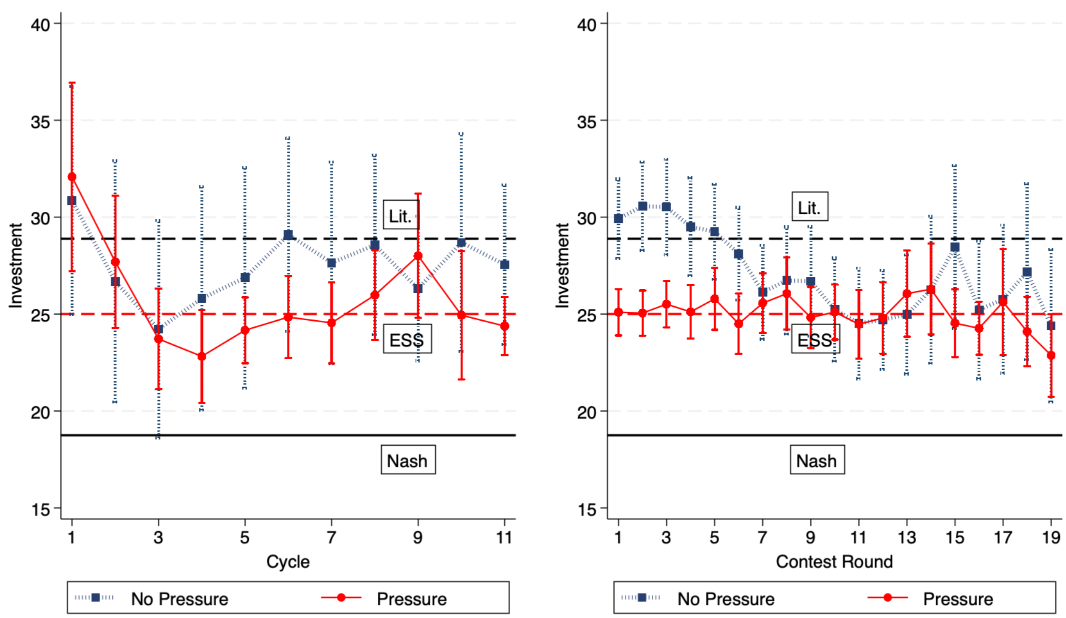
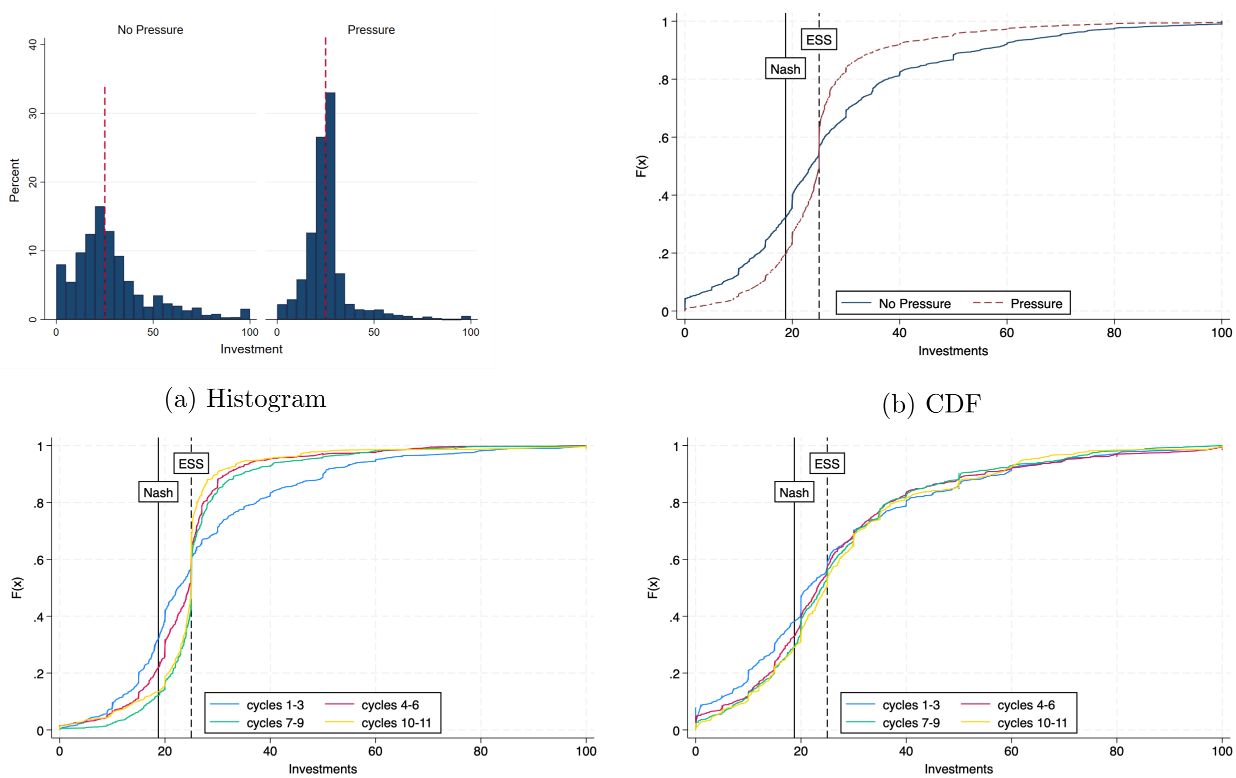



Figure 3: Average Investment Levels by Replacement Cycle and Contest Round. Grey line: Expected number of rounds per cycle.

While the picture of higher investment levels and higher variance absent pressure is generally corroborated by average investment patterns for individual session pairs, there is significant session-level heterogeneity in investments patterns. In particular, significant movement and volatility in investment levels for both treatments tends to coincide with the entry of new contestants, suggesting a destabilizing influence of replacements especially in **No Pressure**. See Appendix C for average investment patterns across the 12 session pairs.

## 5.2 Investments, Lifespans and Profits

Figure 4 shows data on the distribution of investments across treatments. The top panels a and b show investments using a histogram and cumulative distribution function. **Pressure** displays a high percentage of bids within 5 points of $x^{ESS}$, with close to 35% of investments in that bin. By contrast, less than 20% of **No Pressure**-participants' investments lie in that bin. A two-sample Kolmogorov-Smirnov test rejects equality of investment distributions across treatments at all conventional levels ($p = .000$), while indicating that **No Pressure**-investments are significantly less concentrated than **Pressure**-investments. The bottom panels c and d of Figure 4 show how investment in early cycles of **Pressure** (panel c) are as scattered as in **No Pressure** (panel d) but that as these sessions progress, investment more clearly clusters around the ESS level under **Pressure** while this is not the case under **No Pressure**.




(a) Histogram

(b) CDF

(c) CDF by Cycle - Pressure

(d) CDF by Cycle - No pressure

Figure 4: Distribution of Investment by Treatment. Vertical Dotted line: ESS Investment

Figure 5 shows the distributions of selection events survived by participants in the two treatments. For comparison purposes, red dashes in the Figure indicate the theoretical asymptotic distribution of survived events absent any selection pressure, that is, based on random replacement with probability $\rho = .1$. As Figure 5 reveals, the **No Pressure**-realizations in our sample closely approximate this asymptotic distribution. By contrast, in **Pressure** we observe an increased share of participants surviving entire sessions. On the flip side, the share of participants surviving zero selection events in **Pressure** rises by a whopping 15% over **No Pressure**. This comparison suggests that **Pressure** enables a small number of experienced players to survive most rounds of selection at the expense of less experienced participants.

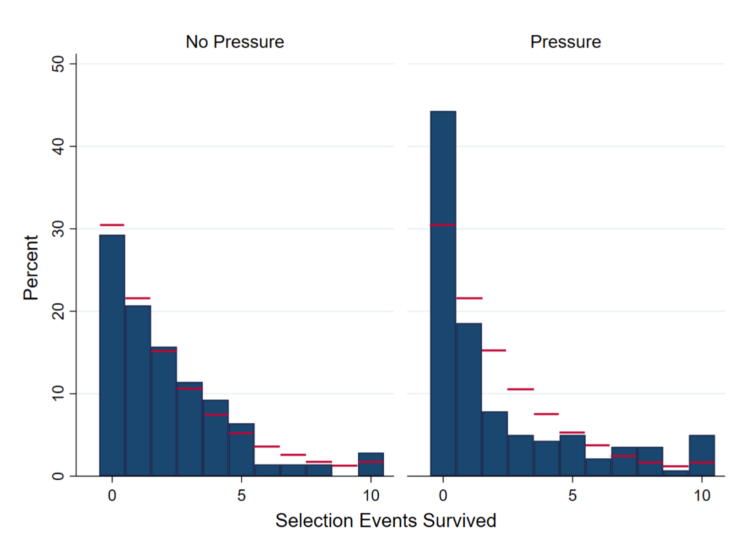



Figure 5: Selection events survived by treatment. Red lines indicate the asymptotic distribution under random replacement ($p = 0.1$).

Since each additional survived replacement event opens up the chance for additional earnings, it is clear that the increased lifespan of some participants in **Pressure** also tends to increase their payoffs relative to the average earned by **No Pressure**-participants. Indeed, Table 3, which reports on mean final payoffs net of the showup payment, reveals that **Pressure** increases the variance of final payoffs among participants, while both lowering minimum- and increasing maximum-earnings.

Table 3: Final Payoffs, US$ (Net of Show-up Fee)

| | Mean | Std.Dev. | CI$^{(1)}$ | Min | Max |
|---|---|---|---|---|---|
| No Pressure | 15.29 | 11.12 | [13.61, 16.98] | 3.06 | 57.31 |
| Pressure | 15.62 | 14.77 | [13.39, 17.86] | 2.82 | 58.02 |

[1] Mean: 95% confidence interval with errors clustered at subject-cycle level.

However, it turns out that **Pressure**-participants are also doing better than **No Pressure**-contestants when looking at round payoffs. Specifically, **Pressure**-participants' round payoffs average 99.88 points (95% CI: $[99.58, 100.18]$) – significantly more than their **No Pressure**-counterparts average round payoffs of 96.57 (99% CI: $[96.07, 97.08]$). This finding is further corroborated in Figure 6, which shows the cumulative distribution of round payoffs by treatment. The distribution of round payoffs in **Pressure** is significantly more concentrated around the ESS-payoff, with **No Pressure**-round payoffs displaying a more pronounced left tail. Indeed, a two-sample Kolmogorov-Smirnov test supports the observation that round payoffs under **No Pressure** tend to be smaller than payoffs under **Pressure** ($p < .001$).

Figure 7 compares average round payoffs by replacement cycle across treatments. As seen in this figure, average round payoffs are initially indistinguishable across treatments, whereas **Pressure**-round payoffs tend to significantly exceed **No Pressure**-round payoffs in later cycles. Furthermore, **No Pressure**-round payoffs generally lie below the ESS-payoff benchmark and (hence) imply negative average returns on investments in the contest. By contrast, **Pressure**-round payoffs tend to equal or exceed the ESS-payoff benchmark implying mildly positive (but sub-Nash) average returns on investment in the contest for most replacement cycles.

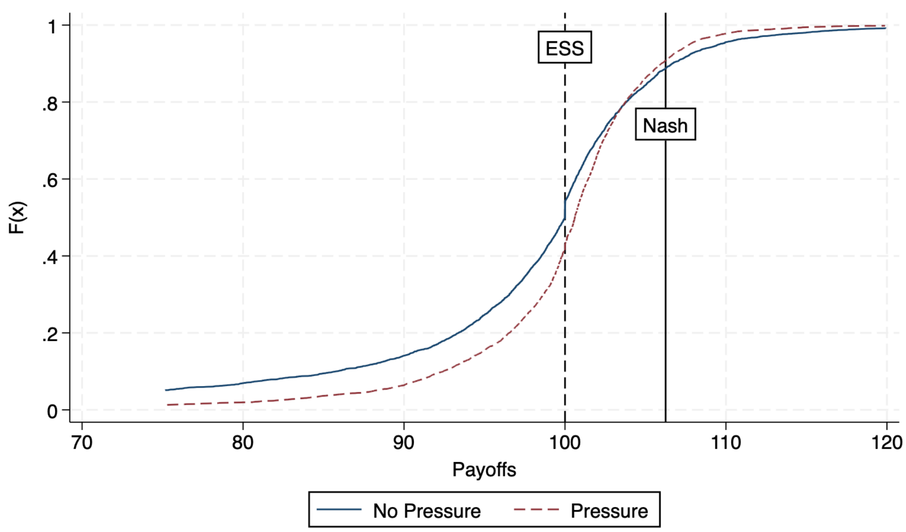



Figure 6: Cumulative Distribution of Round Payoffs by Treatment. Vertical dashed (solid) line: Round payoff at ESS (Nash).

In summary, selection pressure has a moderating effect on participants' investments in the contest, and this significantly improves round payoffs in **Pressure** as compared to **No Pressure**. In the next section, we will zoom in on the behavioral channels that lead to these differences in investment- and payoff-patterns across treatments.

### 5.3 Dissipation

The experimental literature studying Tullock contests (Dechenaux et al. 2015) *absent selection pressure* documents results that seem eerily supportive of our theoretical predictions *under selection pressure*. Starting with Millner and Pratt (1989), experiments have typically found substantial levels of *over-dissipation*. That is, contestants' aggregate investment in the contest $\sum_i x_i$ typically

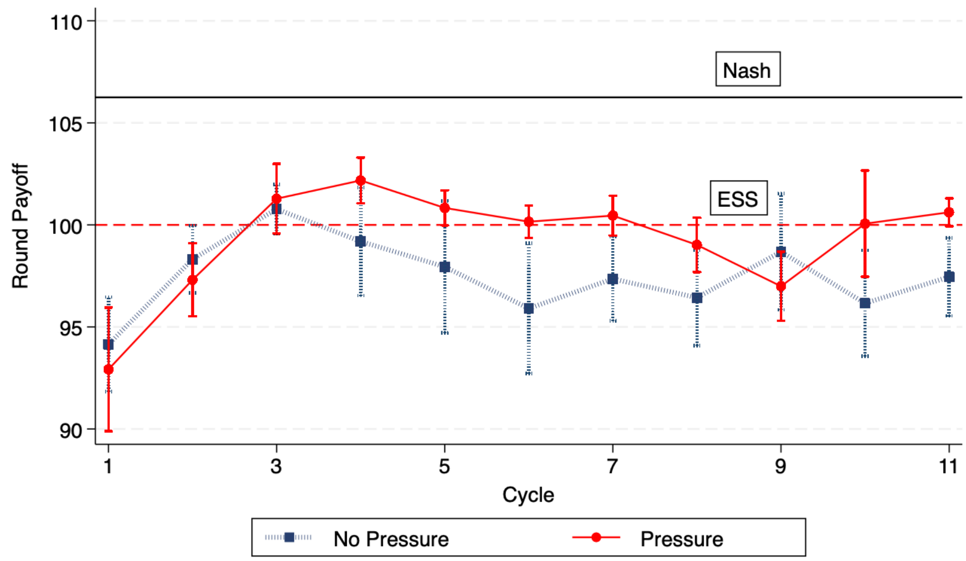



Figure 7: Distribution of Round Payoffs by Replacement Cycle.

exceeds the value of the resource.

Remarkably, significant shares of participants invest in a manner consistent with evolutionarily stable behavior.[15] At the same time, there also tends to be a significant minority of subjects that go beyond what is supported by ESS-play, sometimes even exceeding the resource's monetary value (Sheremeta 2013, Mago et al. 2016, Gneezy and Smorodinsky 2006). In particular, over-dissipation rates average 15.55% across a large number of previous contest experiments surveyed by Sheremeta (2013), whereas the ESS would predict zero overdissipation (or, equivalently, full dissipation of the resource's value).

Figure 8 demonstrates that our **No Pressure** treatment generally replicates the amount of over-dissipation found in prior research (marked as 'Lit' in the figure), despite participants engaging in more complex, repeated contests in our study. By contrast, over-dissipation is drastically lower and essentially absent in our **Pressure** treatment approximating theoretical predictions under ESS-play and remaining significantly above Nash-equilibrium levels (labeled ESS and Nash in the figure).

As seen in Figure 9, these findings generally hold up at the cycle level once incumbent contestants are sufficiently experienced, that is, following the first three cycles.

These findings regarding excess dissipation suggest that the large rates of over-dissipation reported in previous contest experiments are at least partially driven by participants' singular focus on monetary earnings in those experimental settings. By contrast, the threat of performance-based replacement faced by contestants in our **Pressure**-treatment leads to more modest investments. Moreover, **Pressure**-investments are well-approximated by ESS-play that would theoretically maximize a decision-makers' survival chances.

[15]See, e.g., Figure 2 in Dechenaux et al. (2015), which reproduces findings from Sheremeta (2011). The Figure shows a high concentration of investments at the ESS-investment level of 30 points which is not further discussed by the authors.

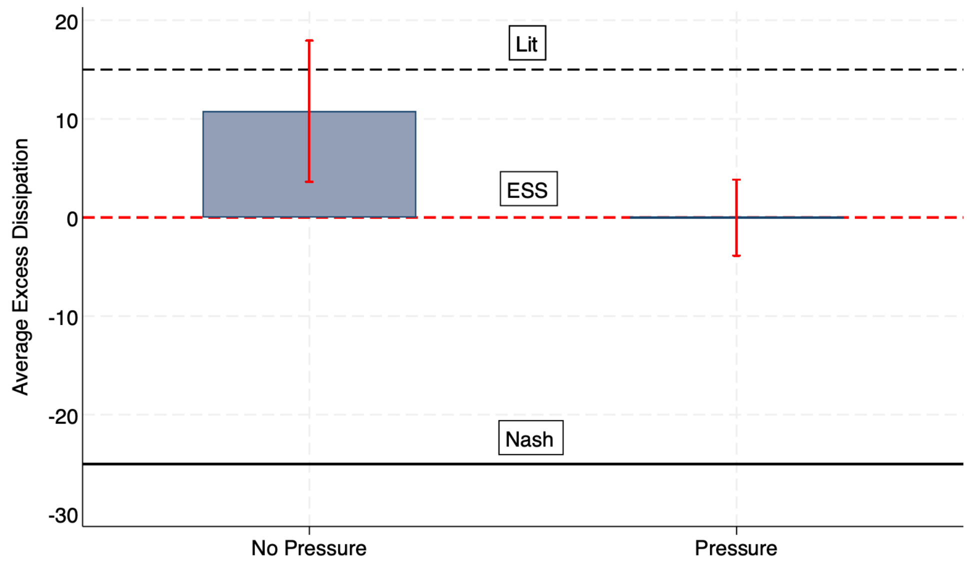



Figure 8: Average Excess Dissipation by Treatment. Bars: 95% confidence interval.

### 5.4 Individual Behavior and Decision-Making

The previous sections reported on results at the aggregate level. In this section, we look more closely at what subjects were doing at the *individual* level.

Figure 10 reports on the stability of subjects' investment decisions, as measured by the standard deviation of each subject's investments across replacement cycles. As this figure reveals, the within-cycle standard deviations of individual investments are large for **No Pressure**, generally ranging between one fifth and one fourth of the resource's value. By contrast, individual investments show less variation in **Pressure** ranging between 10-15% of the resource's value and sometimes even less. This finding indicates that selection pressure has a disciplining effect on individual behavior, causing **Pressure**-subjects to make more consistent investment decisions within a given replacement cycle.

Next, Figure 11 considers the possible *drivers* of individual investment patterns. This figure examines the relationship between individual lifespans in the experiment, as measured by selection events survived (on the horizontal axis) and three summary measures of individual decision-making: average distance to the ESS-investment level (panel a), average distance to the best response (panel b), and average absolute prediction error (panel c). The cumulative distributions by treatment of these same three measures are included in panels a, b and c of Figure 12.

As seen in the top panel (a) of Figure 11, there is a direct relationship between longevity (selection events survived) in **Pressure** and increased ESS-play in that treatment, as indicated by the declining distance to the ESS strategy. By contrast, the average distance of investments from the ESS in the **No Pressure** treatment are unrelated to the number of selection events survived. The CDFs in panel (a) of Figure 12 reveal that the **Pressure** treatment exhibits first-

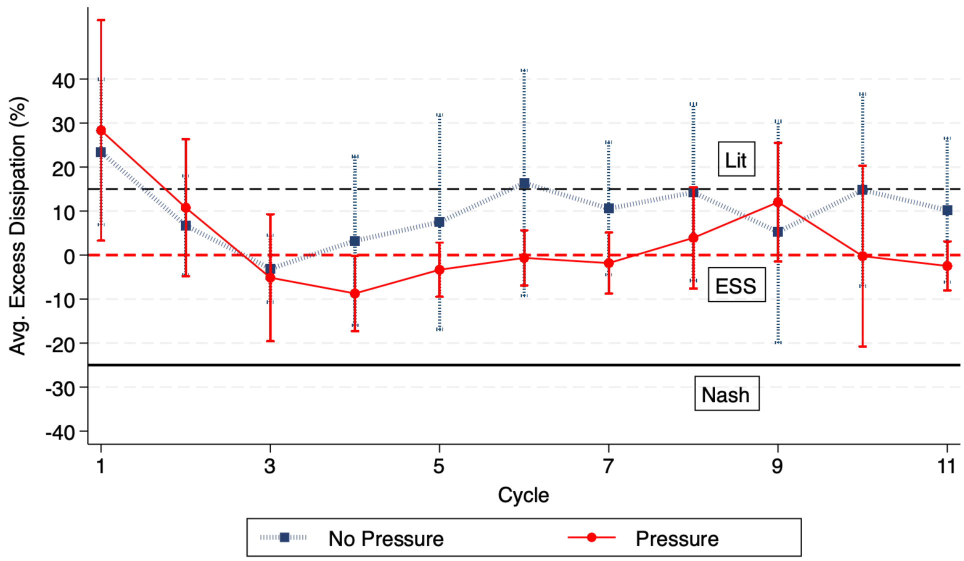



Figure 9: Dissipation by Replacement Cycle.

order stochastic dominance over the No Pressure treatment in terms of proximity to the ESS: for every percentile, subjects under Pressure are closer to the ESS. This finding suggest that the higher incidence of near-ESS play in **Pressure** is driven by subjects adapting to the presence of selection pressure in that treatment.

Next, as seen in the middle panel (b) of Figures 11–12, **Pressure** also improves players' decision making as measured by their average distance to best-response play. Under **Pressure**, subjects' distance to best-response behavior decreases with the number of selection events survived, whereas under **No Pressure** this distance is slightly increasing.

However, as panel (b) of Figure 12 shows, the **Pressure** treatment does not exhibit first-order stochastic dominance in reducing the distance to best-response play.

Finally, as shown in panel (c) of Figures 11–12, the accuracy of successful players' predictions regarding the average investments of others is also greater under **Pressure** relative to **No Pressure**. However, a joint regression of selection events survived under **Pressure** on distance to ESS, distance to best response, and predictive accuracy shows that only the distance to ESS is a significant predictor of lifespan at the 1% level ($p < 0.0001$), whereas distance to best response and predictive accuracy do not explain significant variation in lifespans ($p = 0.042$ and $p = 0.896$, respectively). None of these three variables explain significant variation in lifespans under **No Pressure**.

The Figure 11-patterns and the related regression results indicate that subjects in **Pressure** display a tendency to choose ESS-level investments that are independent of best-response behavior. To examine this further, Figure 13a investigates individual play-dynamics relative to the ESS. As seen in the left panel, average investments across replacement cycles are lower in **Pressure** than in **No Pressure** and they exhibit a significant decrease towards the ESS investment level, with no

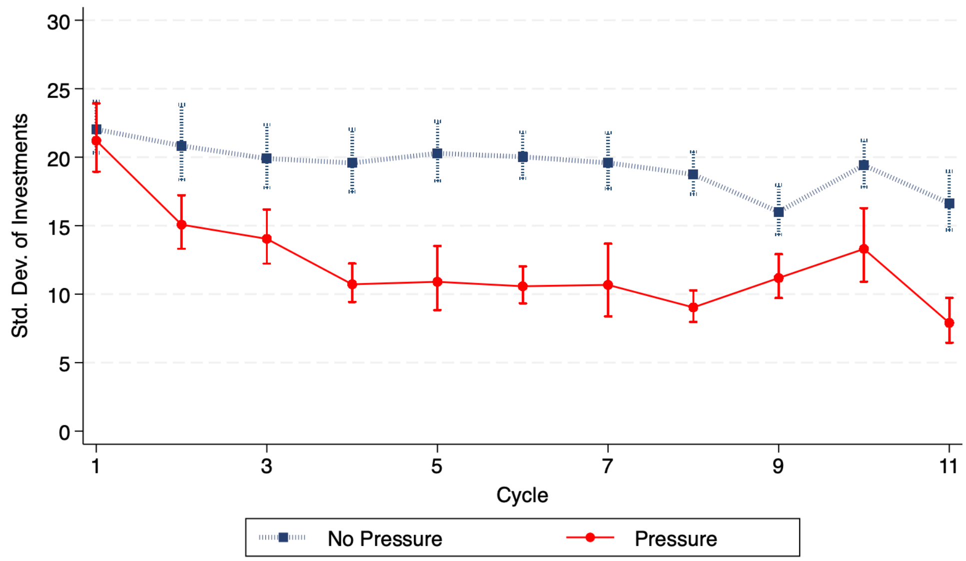



Figure 10: Individual-level Standard Deviations of Investments by Replacement Cycle.

comparable decreasing pattern absent pressure.

At the same time, there is no trend towards ESS-play in either treatment as the Tullock contest gets repeated *within* cycles. This is seen in the right panel where average distance to the ESS is plotted by within-cycle contest rounds, averaged across all cycles.[16]

Taken together, these findings suggest that increased ESS-play is chiefly driven by subjects reacting to the selection pressure intervention, rather than by learning- or imitation-dynamics, which had been found to support ESS-play in previous experiments absent selection pressure (see, e.g., Friedman 1996, Huck et al. 1999, Offerman et al. 2002, Friedman et al. 2015).

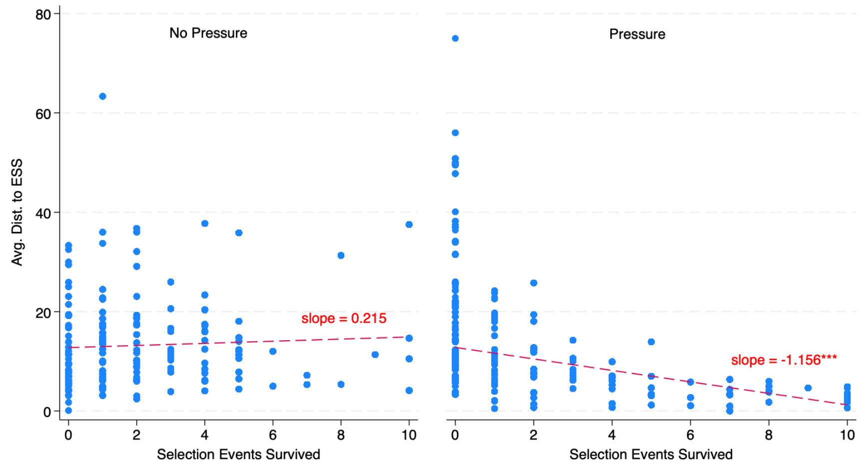
Figure 13b below redoes the Figure 13a-analysis with respect to **best response**-dynamics. As seen in this figure, while subjects in **Pressure** are closer to best-responding in most cycles and for most contest rounds within a cycle, no tendency towards increased best-response behavior is observed within or across cycles for either treatment. This is a first piece of evidence supporting the idea that subjects tend to select the ESS-investment level not for optimizing reasons but based on a more rigid behavioral rule such as average unbeatability (see also see Section 3).

To better understand the decision rules followed by subjects, Figure 14 compares the distance of investments to the ESS between new entrant subjects starting to play in a given replacement cycle and incumbent subjects that have survived the previous replacement cycle.[17]

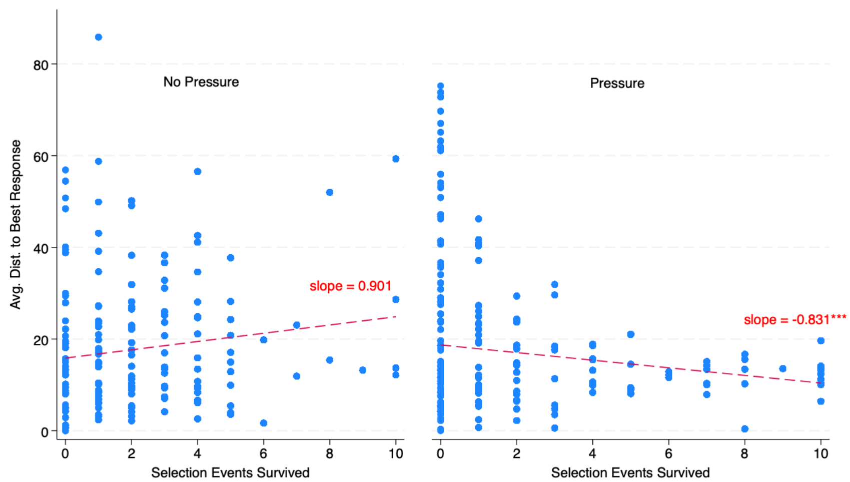
Strikingly, while there are no notable differences across treatments with respect to entrant

[16]As the number of contest rounds increases, there are fewer and fewer cycles for which we observe that many rounds, with no cycle having more than 31 rounds in our sample. The figure therefore reports results up to contest round 19. Given the negative binomial distribution that we use to draw game lengths, this corresponds to the expected number of contest rounds for each cycle plus one standard deviation.

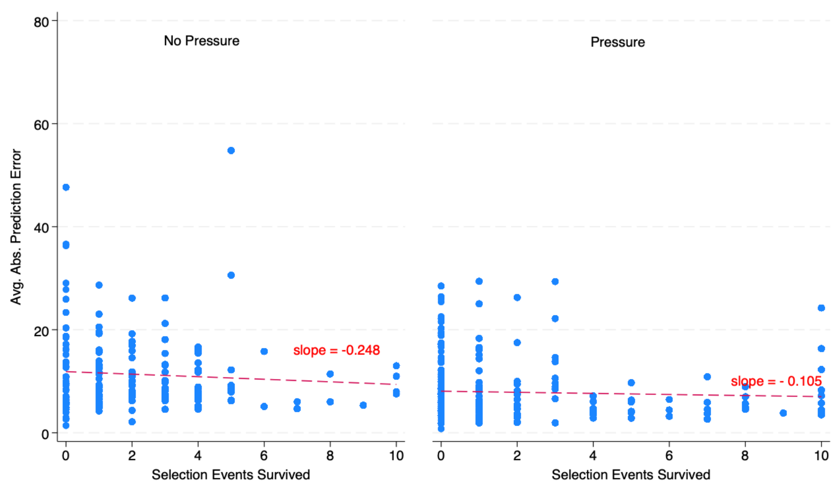
[17]Note that the Cycle 1-data is fully included in the Figure 14-top panels as all subjects in Cycle 1 are entrants. The results regarding new entrant behavior across contest rounds reported in the top right panel are robust to excluding Cycle 1-data.




(a) Average Distance to ESS

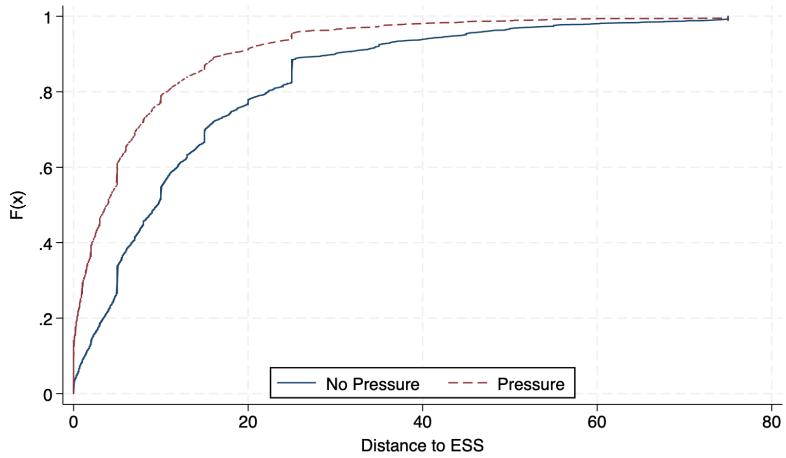



(b) Average Distance to Best Response

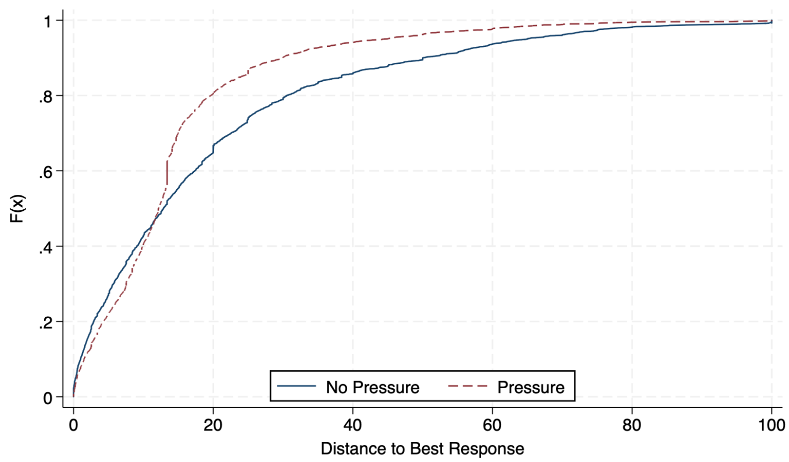



(c) Average Absolute Prediction Error

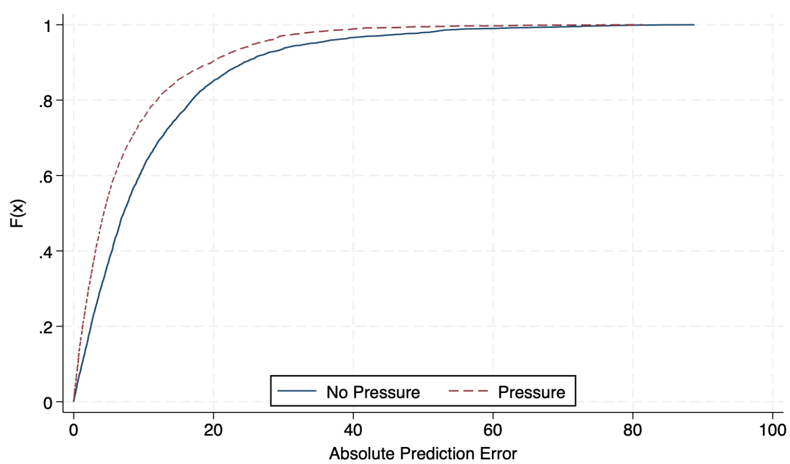
Figure 11: Decision-Making v. Lifespan.




(a) Distance to ESS

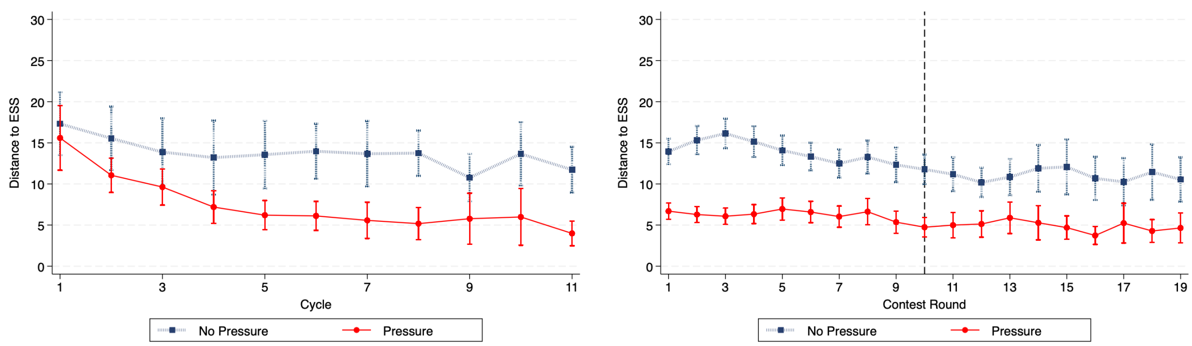



(b) Distance to Best Response

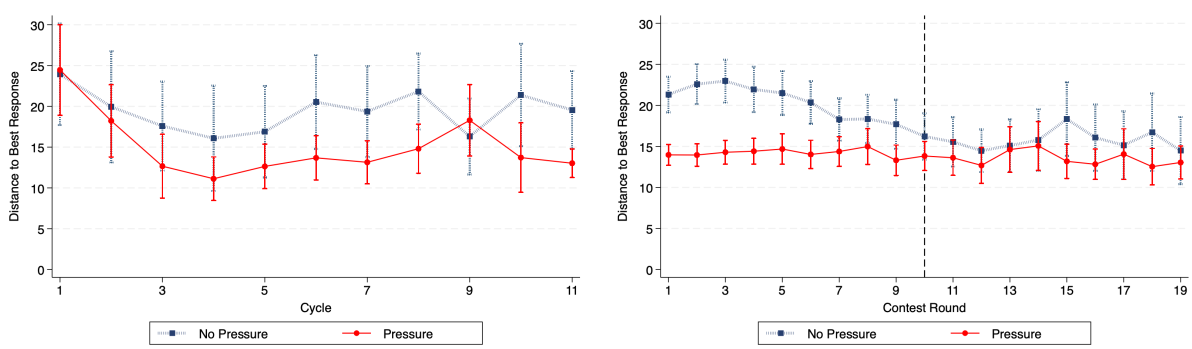



(c) Absolute Prediction Error

Figure 12: Decision-Making CDF.



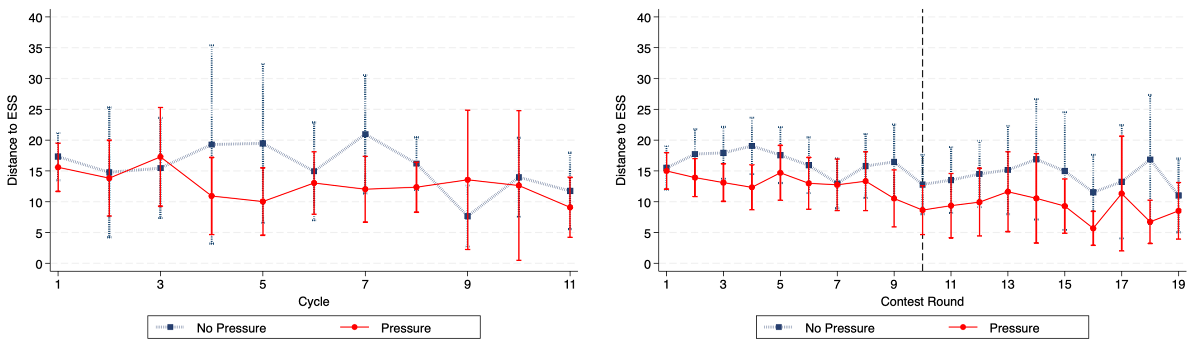

(a) Dynamics of play vs. ESS



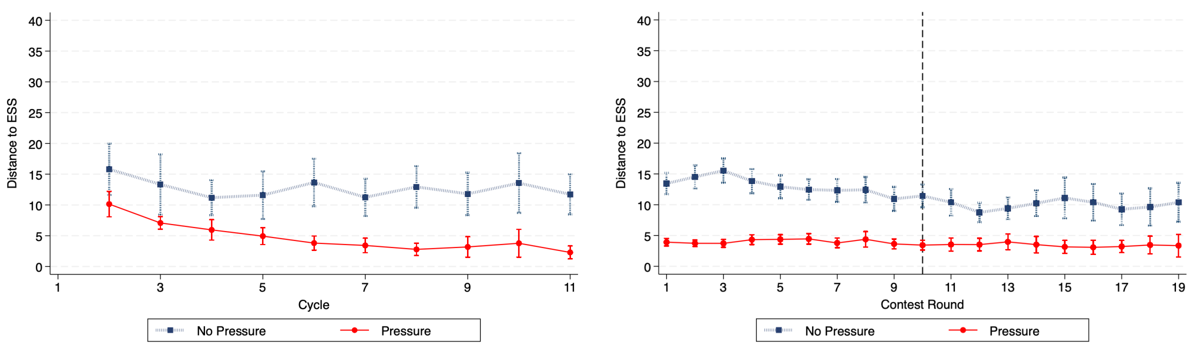

(b) Dynamics of play vs. Best Response

Figure 13: Dynamics of play. Grey line: Expected number of rounds per cycle.

behavior (top panels), incumbent subjects in **Pressure** display both a tendency to approach ESS-investment levels *across* cycles and essentially constant investments in close proximity to the ESS *within* cycles. By contrast, **No Pressure**-incumbent subjects do not approach ESS-investments in later replacement cycles, and their within-cycle investments are significantly higher than the ESS-benchmark and incumbent investment in **Pressure**.




(a) Entrants




(b) Incumbents

Figure 14: Dynamics of play vs. ESS. Grey line: Expected number of rounds per cycle.

This demonstrates that selection pressure causes surviving subjects to adopt a rather rigid behavioral rule of investing close to the ESS-level, with fluctuations in investment for **Pressure** being chiefly driven by entrants and their decision-making. This rigid incumbent behavior is very much in line with the average unbeatability-property of the ESS which was demonstrated in Section 3. That is, investing at the ESS-level guarantees survival in the **Pressure**-treatment while at least one other player invests at a different level. **Pressure**-incumbent subjects appear to exploit this property to maximize their lifespan in the experiment without much regard to other, potentially conflicting, objectives such as maximizing their round payoffs.

This behavioral rule "works" as long as other less experienced subjects do not pick up on the ESS's unbeatability quickly enough, leading them to face disproportionately higher odds of being eliminated from the repeated contest. And indeed, as seen in figure 15 below, the share of entrants among eliminated subjects[18] is significantly higher under **Pressure** than under **No Pressure**, and it significantly exceeds the 25%-theoretical asymptotic share of entrants eliminated under random replacement that would obtain with an infinite sample of **No Pressure** sessions.

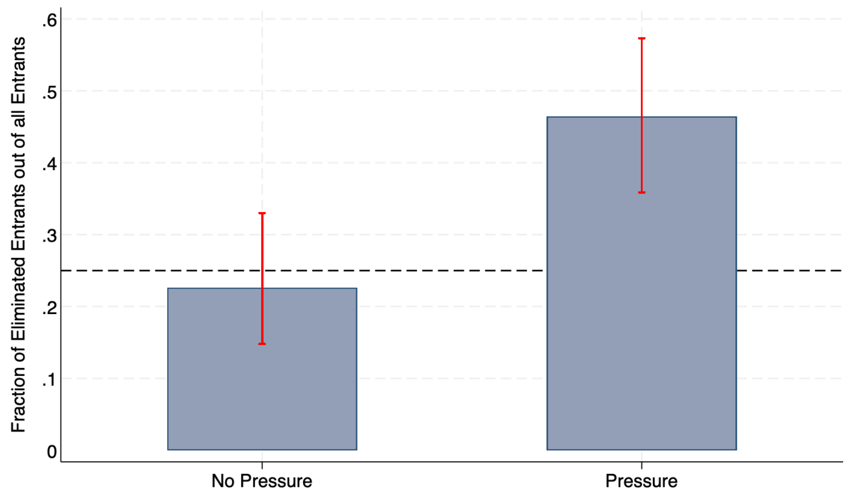



Figure 15: Share of entrants among eliminated contestants. Grey line: Asymptotic share of entrants replaced under random replacement.

### 5.5 Long-Lived vs. Short-Lived Subjects

The **Pressure** mechanism alters the evolution of the population by selectively eliminating lower-performing subjects. We therefore compare the behavior of subjects who were repeatedly selected to survive with those who were not. We classify a subject as "long-lived" if they survive at least two selection events, and as "short-lived" otherwise.[19]

The cutoff at two selection events is motivated by the empirical hazard pattern: under **Pressure**, the per-cycle hazard rate is high in a subject's first two personal cycles (average $\approx 0.33$) but then drops sharply and stabilizes from the third personal cycle onward (average $\approx 0.13$; See Table 8 in Appendix D). Thus, surviving two selection events marks passage out of the early high-attrition phase and into the stable survival regime.

After removing censored cases, 70 of 163 participants (43%) in the **Pressure** treatment are classified as long-lived, compared to 84 of 148 (57%) under **No Pressure**. This difference is statistically significant ($\chi^2 = 5.38$, $p = 0.020$), consistent with the higher early-cycle hazard under

[18]For both treatments, we exclude Cycle 1 as there are no incumbents. Cycle 11 is excluded for **No Pressure**, whereas for **Pressure** we use round payoffs in the final round of Cycle 11 to determine which subjects would have survived had there been a 12th cycle. The results in Figure 15 are robust to excluding Cycle 11 data for both treatments.

[19]That is, a long-lived subject survives two selection events: one after entering and at least one subsequent event as an incumbent. Under **Pressure**, payoffs in Cycle 11 determine whether a subject would survive a hypothetical twelfth cycle. We exclude subjects whose long-lived status is censored. Examples include: under **Pressure**, subjects who enter in Cycle 11 and would have survived; and under **No Pressure**, subjects who enter in Cycle 10 and survive, as well as all subjects who enter in Cycle 11.

**Pressure**. The proportion of long-lived subjects under **No Pressure** closely matches the theoretical baseline survival probability, $(3/4)^2 = 56.25\%$, consistent with random attrition in the absence of performance pressure.

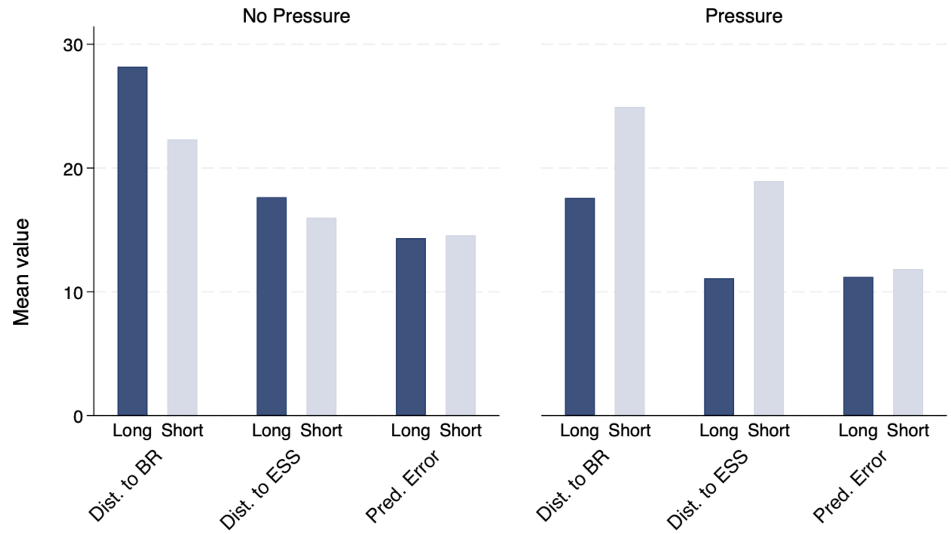



Figure 16: Mean Performance by Treatment and Group (First Cycle)

To compare behavioral differences between long-lived and short-lived participants, we restrict attention to subjects' decisions in their first personal cycle in order to equalize experience across the two groups. Table 4 reports the average distance from several performance benchmarks, using (i) all decisions in a subject's first personal cycle and (ii) only the first two contest rounds, which minimizes learning effects while still permitting a lagged measure. Under **Pressure**, long-lived subjects' initial decisions (long) are significantly closer to all performance benchmarks—except prediction error—than those of short-lived subjects (short), both when using the full first cycle and when restricting to the first two rounds. Long-lived subjects in **Pressure** also begin closer to these benchmarks than long-lived subjects in **No Pressure**, indicating a systematic difference in the composition of survivors across treatments. This pattern indicates that long-lived subjects do not succeed merely by learning or adapting more effectively over time; rather, they enter the experiment already making systematically better *initial* choices, even though their prediction accuracy is no better than that of short-lived subjects.

In addition to starting out closer to the performance benchmarks, long-lived subjects under **Pressure** also display modestly more effective early adjustments than their short-lived counterparts. Table 5 reports the mean change in distance to each benchmark during a subject's first personal cycle, and Figure 17 visualizes these dynamics. Across most measures, long- and short-lived subjects adjust similarly, both under **Pressure** and under **No Pressure**. The one clear exception is movement toward the ESS: long-lived subjects in **Pressure** reduce their distance to

Table 4: Comparison of Mean Distance Measures between Long- and Short-Lived Subjects

| | | First Personal Cycle | | | | First Two Iterations | | | |
|---|---|---|---|---|---|---|---|---|---|
| Distance Measure | Treatment | Mean (Long) | Mean (Short) | $t$-stat | $p$-value | Mean (Long) | Mean (Short) | $t$-stat | $p$-value |
| $BR_{dist}$ | No Pressure | 26.2 | 19.2 | 2.72 | 0.007** | 28.2 | 22.3 | 1.73 | 0.086 |
| | Pressure | 16.6 | 25.0 | -3.94 | 0.000*** | 17.6 | 24.9 | -2.77 | 0.006** |
| $ESS_{dist}$ | No Pressure | 17.6 | 13.8 | 1.93 | 0.056 | 17.6 | 16.0 | 0.63 | 0.531 |
| | Pressure | 9.0 | 18.5 | -5.48 | 0.000*** | 11.1 | 18.9 | -3.79 | 0.000*** |
| $pred\_error$ | No Pressure | 12.2 | 12.2 | 0.02 | 0.987 | 14.3 | 14.6 | -0.12 | 0.903 |
| | Pressure | 10.4 | 11.2 | -0.48 | 0.636 | 11.2 | 11.8 | -0.37 | 0.714 |

**Notes:** Distance measures are constructed as follows. $BR_{dist}$: absolute difference between a subject's bid and the payoff-maximizing best response to the *contemporaneous* bids of others; $ESS_{dist}$: absolute difference between a subject's bid and the ESS investment level (25); $pred\_error$: absolute prediction error (difference between a subject's predicted and actual mean bid of others).
All statistics are computed at the individual level: for each subject, we compute their mean distance in the relevant period (first personal cycle or first two contest rounds). Two-sample $t$-tests compare these subject-level means between long- and short-lived groups, separately by treatment. Because each observation is aggregated to the subject level, standard errors are implicitly clustered by subject.
$^{\dagger}p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

the ESS significantly more than short-lived subjects, whereas no such pattern appears in **No Pressure**. This limited evidence of differential adjustment is consistent with the ESS serving as the principal adaptive target when selection pressure is present, but it is small relative to the large differences in initial decision quality documented above.

Table 5: Differences in Learning Across Long- and Short-Lived Participants in their First Cycle

| | No Pressure | | | | Pressure | | | |
|---|---|---|---|---|---|---|---|---|
| Measure | Mean (Long) | Mean (Short) | t-stat | $p$ | Mean (Long) | Mean (Short) | t-stat | $p$ |
| $\Delta BR\ Distance$ | 0.009 | -0.720 | 0.60 | 0.552 | -0.283 | -1.010 | 0.65 | 0.518 |
| $\Delta ESS\ Distance$ | -0.246 | 0.100 | -0.36 | 0.717 | 1.290 | -0.381 | 2.07 | 0.040* |
| $\Delta Prediction\ Error$ | 0.074 | 0.956 | -1.06 | 0.293 | -0.354 | 0.595 | -1.19 | 0.236 |

**Notes:** For each subject $i$ and benchmark $B$, let $d_{i,t}(B) = |\text{bid}_{i,t} - B_{i,t}|$ denote the absolute distance to the benchmark in round $t$ of the subject's first personal cycle. The learning measure is the average round-to-round change in this distance:

$$\Delta_i(B) = \frac{1}{T_i - 1} \sum_{t=2}^{T_i} \left( d_{i,t-1}(B) - d_{i,t}(B) \right),$$

where $T_i$ is the number of rounds in subject $i$'s first personal cycle. Positive values indicate improvement (movement *toward* the benchmark). Reported values are group means for long- and short-lived subjects. T-statistics and $p$-values are from two-sample tests of equal means. $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

We also examine how subjects responded to performance feedback, again restricting the analysis to each participant's first personal cycle. In each round, subjects observed their own investment and payoff as well as the average investment and payoff from the previous round[20]; see Figure 1 for a screenshot of the feedback screen. We classify bid adjustments as "No Change" if the new bid differed by less than 0.5 from the previous bid; otherwise, changes are coded as moving toward or away from the previous-round average. Figure 18 plots adjustment patterns when subjects were performing above or below the average, separately by longevity and treatment. Under **Pressure**, short-lived subjects exhibit little systematic responsiveness to feedback. By contrast,

[20]Unless they had just joined and did not participate in the prior round.

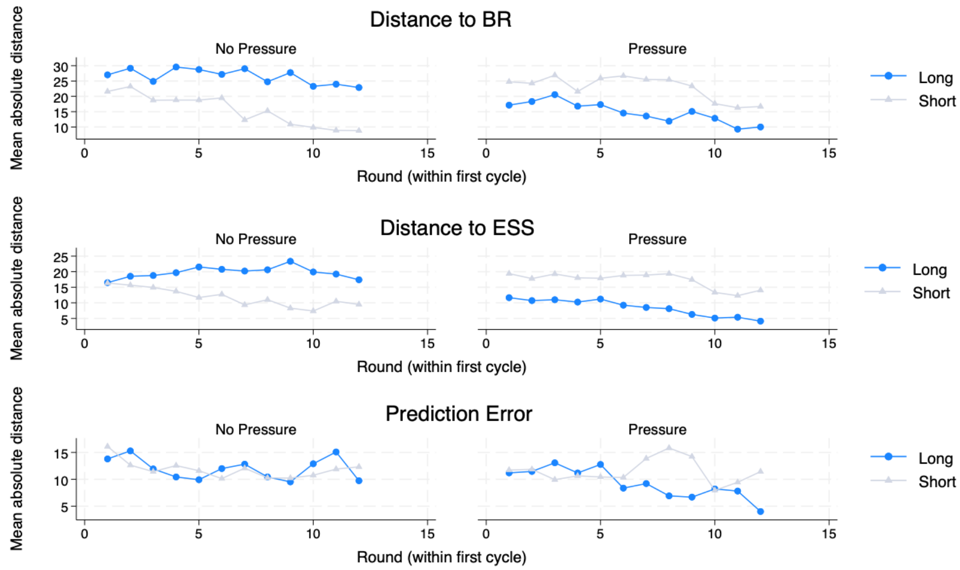



Figure 17: First-Cycle Distance Measures by Group and Treatment

long-lived subjects display risk-sensitive adjustments: they move their bids toward the average when underperforming—when the risk of elimination is highest—and maintain their bids when performing above average and at lower risk. This pattern does not appear under **No Pressure**, where feedback-driven adjustments are weaker and do not systematically differ between long- and short-lived subjects.

### 5.6 Behavioral vs Mechanical Effects

The preceding sections document large population-level differences between the **Pressure** and **No Pressure** treatments, as well as systematic differences between subjects who ultimately survive many selection events and those who do not. A remaining question is whether the observed divergence in population outcomes is driven primarily by mechanical selection—where higher-performing subjects are retained but behave no differently than they would in the absence of selection pressure—or whether subjects' behavior itself changes in response to the presence of selection pressure. In other words, does the treatment affect outcomes solely by reshaping population composition, or does it also alter how surviving subjects play the game?

To address this question, we regress long-lived status among **Pressure** subjects on a set of distance and learning measures from their first cycle of play.[21] We then apply the estimated model to **No Pressure** subjects to generate counterfactual predictions of how likely each subject would have been to survive under selection pressure based on their initial behavior. Model performance, evaluated on the **Pressure** sample, yields a log loss of 0.5521 and a ROC value of 0.7828, indicating reasonable predictive accuracy.

[21]See Table **??** for regression results.

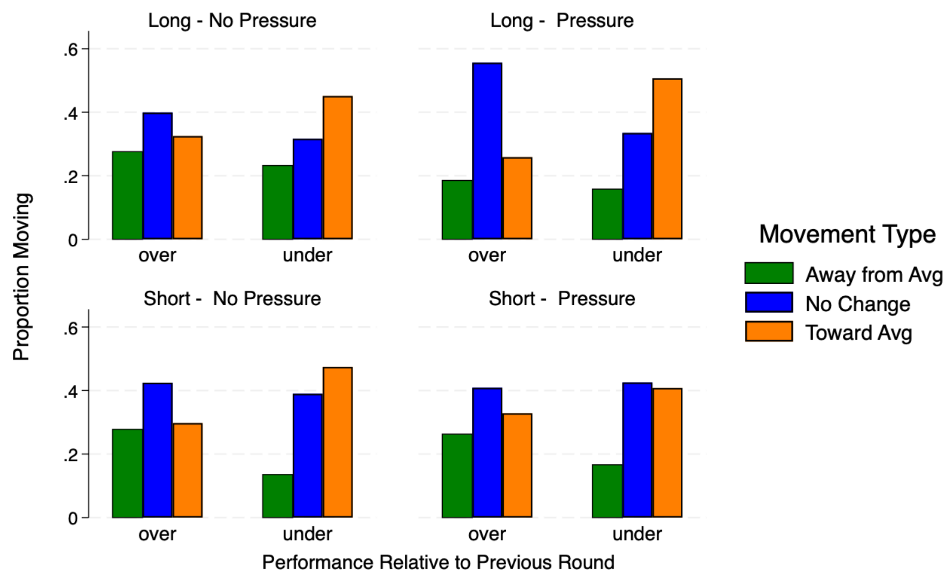



Figure 18: Direction of bid adjustments by relative payoff in subjects' first cycle

Figure 19 plots the cumulative distribution functions (CDFs) of subjects' predicted probabilities of being long-lived under **Pressure**, separately by treatment. If subjects' initial behavior differed systematically across treatments, these predicted survival distributions would diverge. Instead, the distributions are nearly identical: a two-sample Kolmogorov–Smirnov test yields $D = 0.0742$ with $p = 0.8427$, indicating no statistically detectable difference in predicted long-lived probabilities across treatments.

These results indicate that the observed differences in population evolution are primarily driven by the selection mechanism rather than by differences in individual behavior. When survival is predicted solely from subjects' first-cycle behavior under **Pressure**, the implied probabilities of being long-lived are nearly identical across treatments for new entrants (Figure 21), suggesting little systematic difference in initial behavior between **Pressure** and **No Pressure**. Selection pressure instead operates through differential retention: subjects whose early behavior predicts longer survival are increasingly overrepresented among incumbents under **Pressure**, leading to a widening divergence in population composition over time (Figure 20). As these surviving subjects converge toward the ESS, incumbents become increasingly entrenched (Figure 22).

### 5.7 Individual Characteristics and Effects of Selection Pressure

Having shown that population-level differences are driven primarily by selection rather than differential entry behavior, this section considers whether individual characteristics can account for the remaining treatment effects. We examine demographics and post-experiment survey measures and explore their relationship to decision-making in the two treatments.

To begin, Table 6 summarizes subjects' responses to the demographic- and survey-questions and

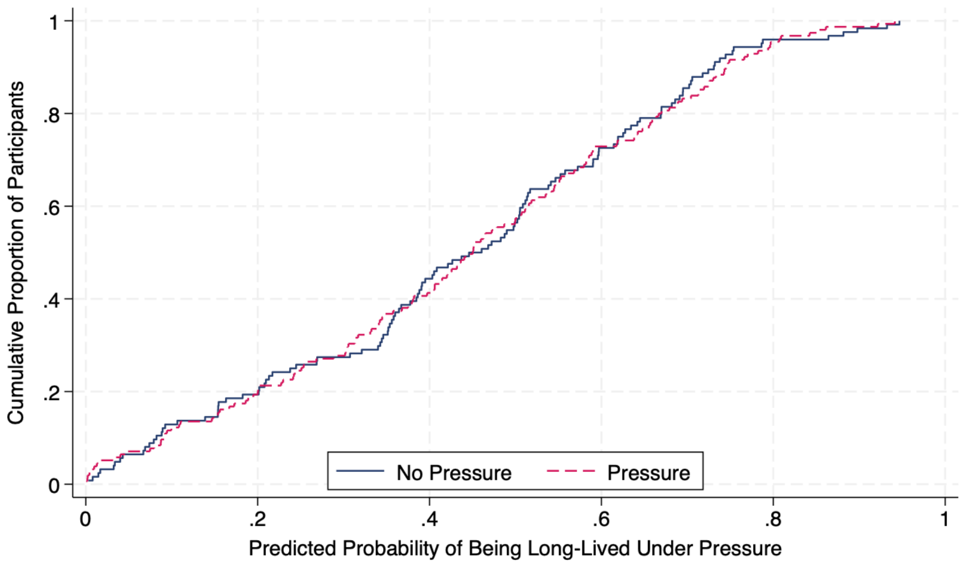



Figure 19: CDF of Predicted Long-Lived Probabilities by Treatment

compares the means and standard deviations of their responses across treatments. No significant differences are found between subjects assigned to the two treatment conditions.

Table 6: Overview of Survey and Demographics

| | Total | | No Pressure | | | Pressure | | |
|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std.Dev. | Mean | Std.Dev. | 95% CI | Mean | Std.Dev. | 95% CI |
| *Age* | 20.82 | 2.90 | 20.96 | 2.90 | [20.52, 21.40] | 20.69 | 2.90 | [20.25, 21.13] |
| *Gender* | .67 | .47 | .64 | .48 | [.56, .71] | .70 | .46 | [.63, .77] |
| *GPA* | 3.52 | .46 | 3.46 | .51 | [3.38, 3.54] | 3.59 | .38 | [3.53, 3.65] |
| *Quant* | 6.49 | 6.16 | 6.90 | 6.87 | [5.85, 7.95] | 6.08 | 5.34 | [5.26, 6.89] |
| *CRT* | 2.31 | 1.28 | 2.29 | 1.30 | [2.09, 2.48] | 2.33 | 1.25 | [2.14, 2.52] |
| *Risk* | 4.17 | 1.46 | 4.03 | 1.52 | [3.80, 4.26] | 4.32 | 1.50 | [4.09, 4.54] |
| *Time Pref.* | 4.86 | 1.46 | 4.83 | 1.55 | [4.60, 5.07] | 4.89 | 1.37 | [4.68, 5.10] |
| *Competitiveness* | 4.51 | 1.83 | 4.45 | 1.80 | [4.18, 4.73] | 4.57 | 1.87 | [4.29, 4.86] |

**Notes:** *CI* is 95% normal confidence interval; *Gender* takes value 1 for female subjects, value .5 for non-binary subjects, and value 0 for male subjects; *Quant* is number of economics-, statistics-, and math-courses taken; *CRT* is score out of 4 on cognitive reflection test; *Risk* is 7-point Likert scale measure of willingness to take risks; *Time Pref.* is 7-point Likert scale measure of willingness to incur costs today for future benefits; *Competitiveness* is 7-point Likert scale measure of willingness to compete.

Next, to investigate whether variations in demographic and survey responses influence behavior across treatments, Table 7 reports on a regression of: (1) investment amounts, (2) round payoffs, and (3) distance to the ESS on a treatment dummy for **Pressure** and on the demographic and survey responses we collected from subjects at the end of the experiment. As this table reveals, the selection pressure treatment effect remains significant at the .01 level of significance for all three

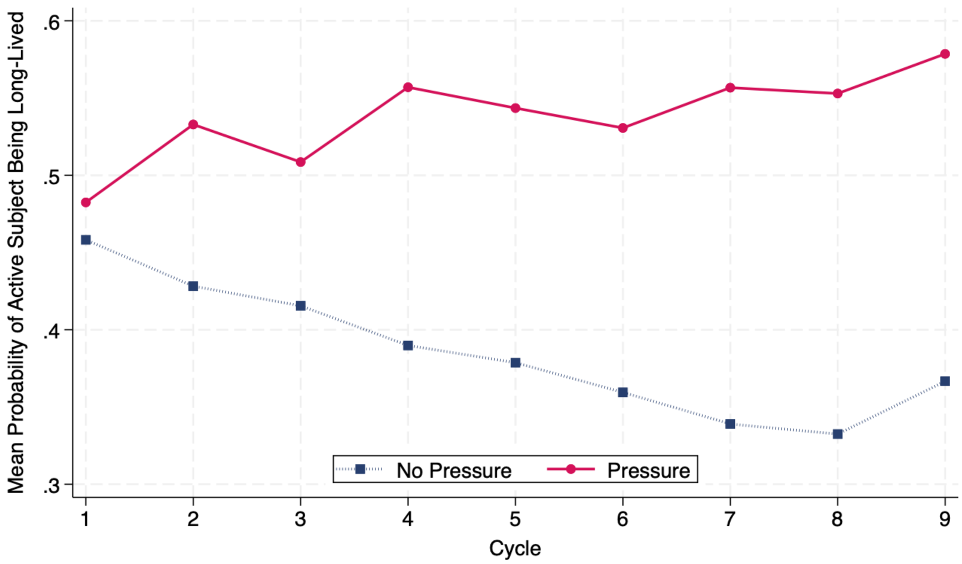



Figure 20: Predicted Concentration of Long-Lived Participants in Population by Cycle and Treatment

dependent variables when controlling for demographics and survey responses. Furthermore, there is a significant and consistent effect of subjects' self-reported willingness to take risks across all three dependent variables; a greater willingness to take risks increases investments and the distance to the ESS while lowering round payoffs. None of the other individual characteristics provide significant and consistent effects on investments, payoffs and distance to ESS. The gender dummy variable is significant for investments, but not for payoffs or distance to the ESS. These results suggest that our treatment interventions are the primary driving force behind the results that we obtain.

# 6 Conclusion

Economists have long argued that selection pressure, – the struggle to survive – is an important driving force underlying a variety of economic decisions. Examples include investments in research and development, the acquisition of new skills or education, the adoption of new technologies, and competitive pricing in oligopolistic markets. In this paper, we have presented causal evidence from a controlled experimental setting – a first of its kind – regarding how individuals respond to such selection pressure. Our design allows us to directly observe the impact of selection pressure on decision-making in repeated Tullock contests. Our Pressure treatment is analogous to real-world mechanisms like rank-and-yank policies and high-stakes performance evaluations.

We find that selection pressure due to performance-based replacement (mimicking evolutionary selection pressure) significantly impacts on contest investments and payoffs. Contestants in our Pressure treatment exhibit more moderated and survival-oriented investment strategies compared to contestants in the No Pressure control, where replacement is random. In particular, the Pressure group's investments align closely with the finite-population ESS of the Tullock contest. By contrast,

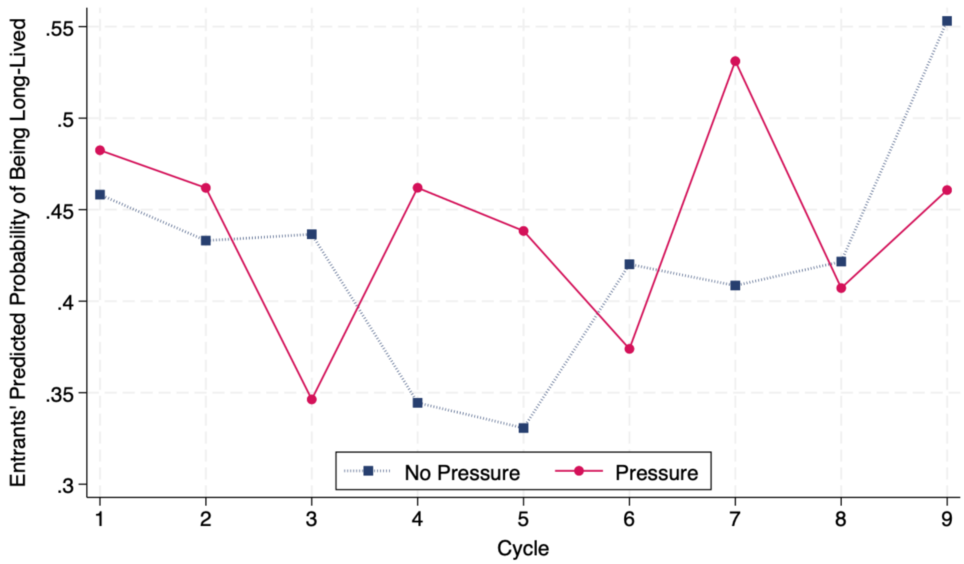



Figure 21: Entrants' Predicted Probability of Being Long-Lived by Cycle and Treatment

Table 7: Regression Analysis of the Impact of Demographics and Survey Responses on Investments, Payoffs and Distance to ESS

| | (1) Investments | | (2) Payoffs | | (3) Distance to ESS | |
|---|---|---|---|---|---|---|
| | Coeff. | 95% CI | Coeff. | 95% CI | Coeff. | 95% CI |
| *Pressure* (*d*) | -1.91 | [ -3.68, -0.14] | 2.18 | [ 1.21, 3.14] | -6.60 | [ -7.90, -5.31] |
| *Gender* | 3.50 | [ 1.56, 5.45] | -1.23 | [ -2.23, -.22] | 2.47 | [ 1.06, 3.87] |
| *GPA* | .62 | [ -1.27, 2.52] | -0.51 | [ -1.57, 0.55] | 1.48 | [ -.10, 3.07] |
| *Quant* | .21 | [ .00, .42] | -.05 | [ -.17, .06] | .06 | [ -.07, .19] |
| *CRT* | -.11 | [ -.80, .58] | .02 | [ -.34, .38] | -.03 | [ -.56, .50] |
| *Risk* | 1.76 | [ 1.07, 2.45] | -.71 | [ -1.12, -.31] | .72 | [ .20, 1.24] |
| *Time Pref.* | -.29 | [ -.89, .31] | .21 | [ -.11, .53] | -.30 | [ -.82, .21] |
| *Competitiveness* | -.45 | [ -.98, .08] | .07 | [ -.26, .39] | -.07 | [ -.47, .32] |
| *Constant* | 18.11 | [ 9.20, 27.01] | 101.90 | [ 97.27, 106.53] | 5.38 | [ -2.15, 12.90] |
| *N* | 9,216 | | 9,216 | | 9,216 | |
| $R^2$ | .03 | | .02 | | .08 | |

(d) denotes dummy variable, effect of change from 0 to 1 reported.
Standard errors clustered at subject-cycle level.

investments in the No Pressure group are much higher, and they result in dissipation rates similar to what is found across a large number of previous experiments studying finitely repeated contests without selection pressure.

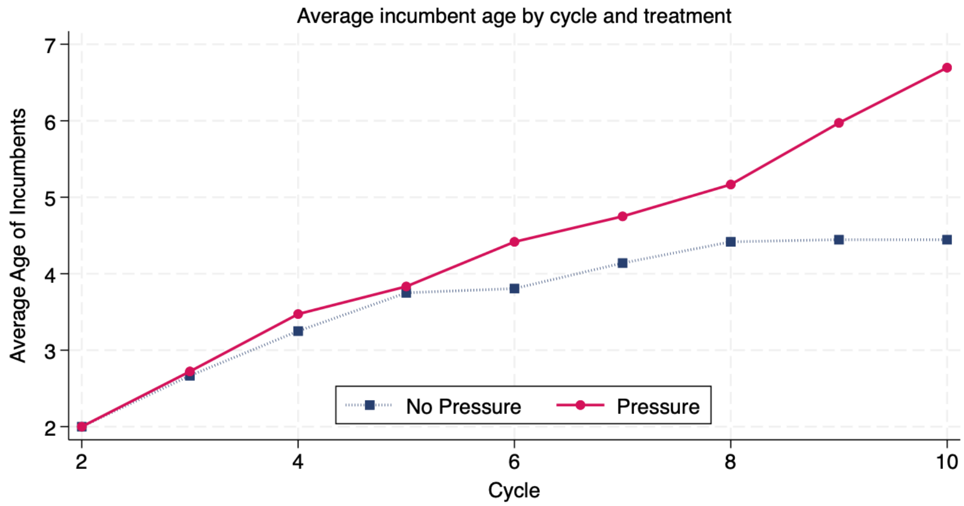



Figure 22: Average Incumbent Age by Cycle and Treatment

Furthermore, selection pressure is found to cause a notable decrease in the variability of investments within the Pressure treatment, suggesting that the threat of replacement can induce a more disciplined and consistent strategy among participants. Specifically, successful subjects in the Pressure treatment appear to adopt a rather rigid behavioral rule, which implements near ESS-investment levels independent of other contestants' behavior. This behavior appears reminiscent of recent theoretical results establishing an *unbeatability property* of the ESS in certain finite-population games. Disentangling unbeatability as a motivator for economic decision-making under selection pressure from other potential drivers such as (e.g.) the maximization of absolute or relative payoffs is an interesting avenue for future research.

Our findings are particularly relevant for understanding behaviors in high-stakes economic environments, such as corporate settings or competitive markets, where survival and success are closely tied to consistently outperforming benchmarks and peers. Our experiment shows that explicitly accounting for selection pressures in such settings leads to qualitatively different predictions.

Finally, it is important to note that our Pressure treatment results in higher overall payoffs and more efficient resource allocation among participants. This aligns with theories suggesting that competition and selection pressures can lead to more efficient outcomes in economic and biological systems. By contrast, the No Pressure condition, which lacks performance-based replacement, is characterized by greater fluctuations in investments and often suboptimal decision-making, mirroring less competitive economic environments or biological systems with low intensity of selection.

Our approach can be applied to many settings besides contests. For instance, we are currently applying our selection pressure design to investigate competition and collusion in oligopolistic industries. Other possible applications are to bargaining games or financial asset markets.

More broadly, our findings highlight an important limitation of traditional laboratory designs that abstract from selection. In standard experiments, population composition is fixed by construction, obscuring the role of survival and persistence in shaping aggregate outcomes. Our results demonstrate that even when individuals' behavior is similar across environments, introducing performance-based selection fundamentally alters how populations evolve over time. Selection pressure need not induce different behavior at entry to matter; instead, it determines which behaviors persist, spread, and ultimately dominate the population. By endogenizing population composition, the Pressure treatment captures a central feature of natural and economic systems—namely, that poorly performing strategies are gradually eliminated—thereby generating dynamics and outcomes that static, no-selection designs are unable to reproduce.

In conclusion, this research not only reaffirms the relevance of evolutionary theory to economic analysis, but it also expands our understanding of how survival pressures can shape competitive strategies in significant ways. The experimental methods introduced here open up new avenues for research into the adaptive behaviors of real-life economic agents under different types of selection pressures. In this manner, they enhance our understanding of the complex interplay between economic incentives and evolutionary dynamics.

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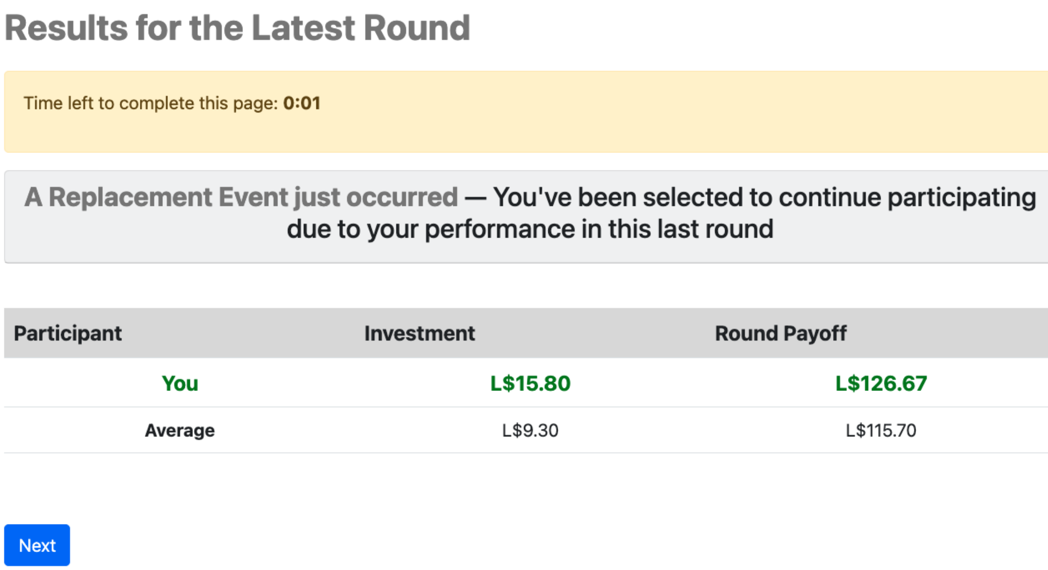
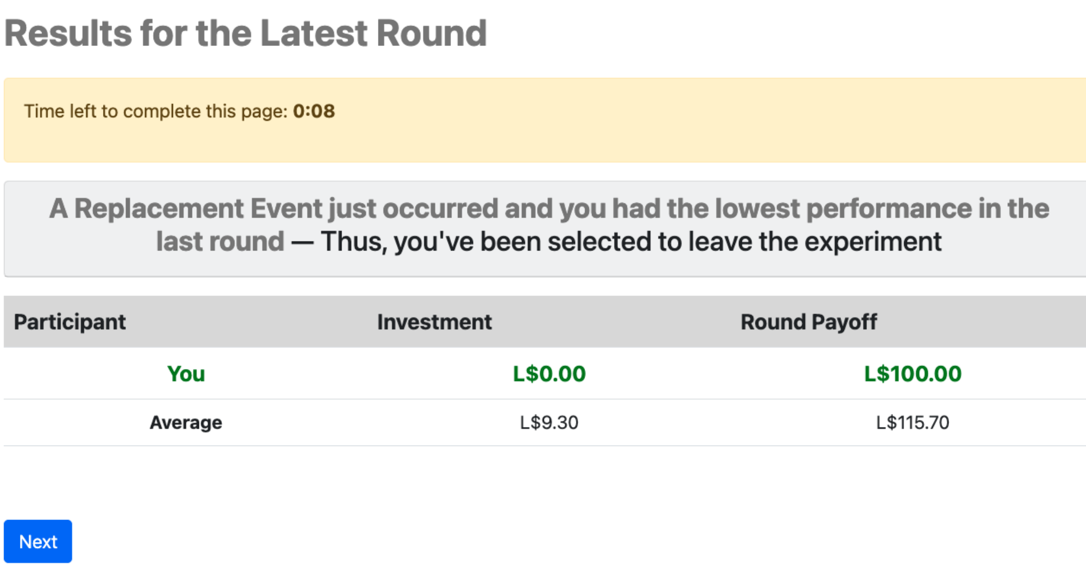
# Appendix (For Online Publication)

## A Additional Interface Screenshots

### A.1 Selection event results screen, Pressure Treatment



## A.2 Selection event results screen, Random Treatment

### Results for the Latest Round

Time left to complete this page: **0:10**

**A Replacement Event just occurred** — You've been randomly selected to continue participating

| Participant | Investment | Round Payoff |
|---|---|---|
| **You** | **L$30.50** | **L$104.48** |
| **Average** | L$21.79 | L$103.20 |

Next

### Results for the Latest Round

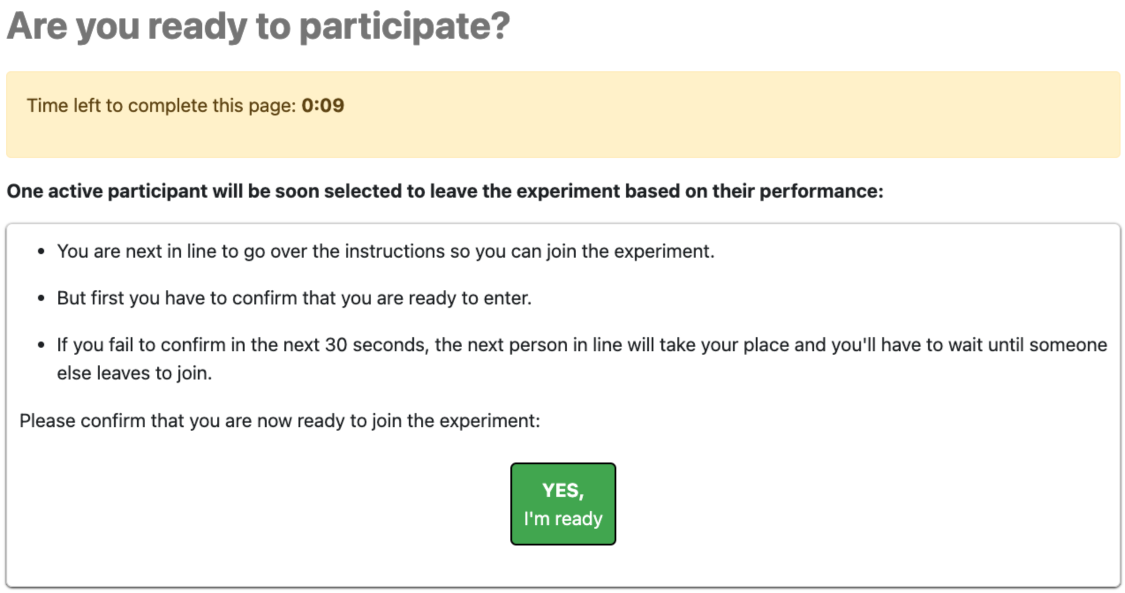
Time left to complete this page: **0:06**

**A Replacement Event just occurred** — You've been randomly selected to leave the experiment

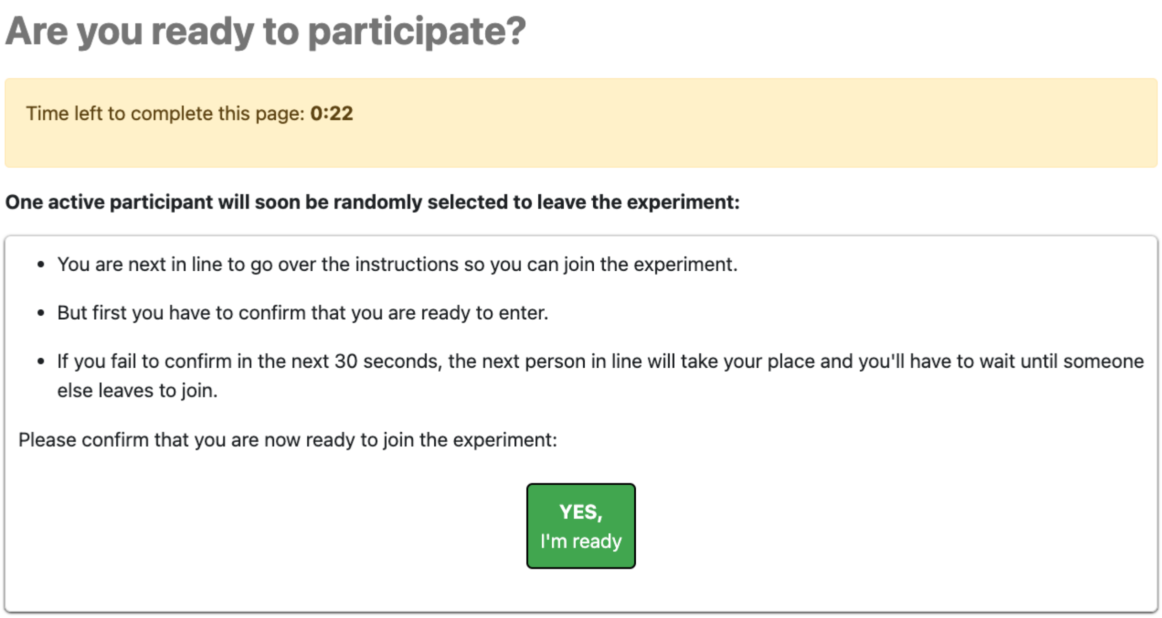
| Participant | Investment | Round Payoff |
|---|---|---|
| **You** | **L$17.40** | **L$98.91** |
| **Average** | L$26.67 | L$98.32 |

Next

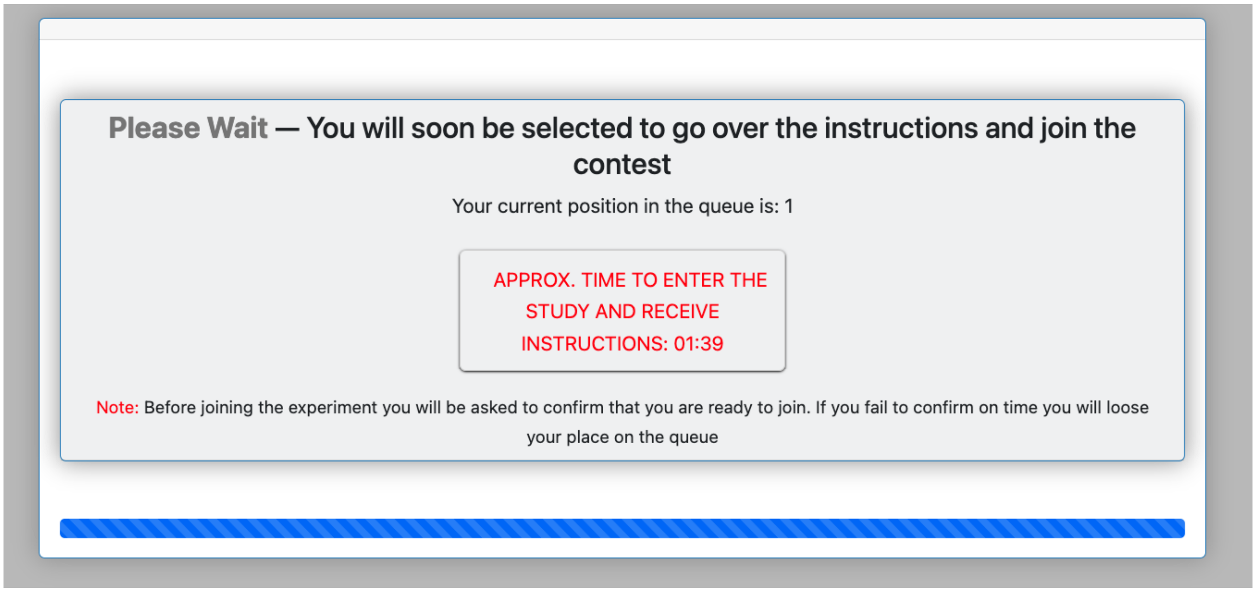
### A.3 Ready to enter screen, Pressure Treatment

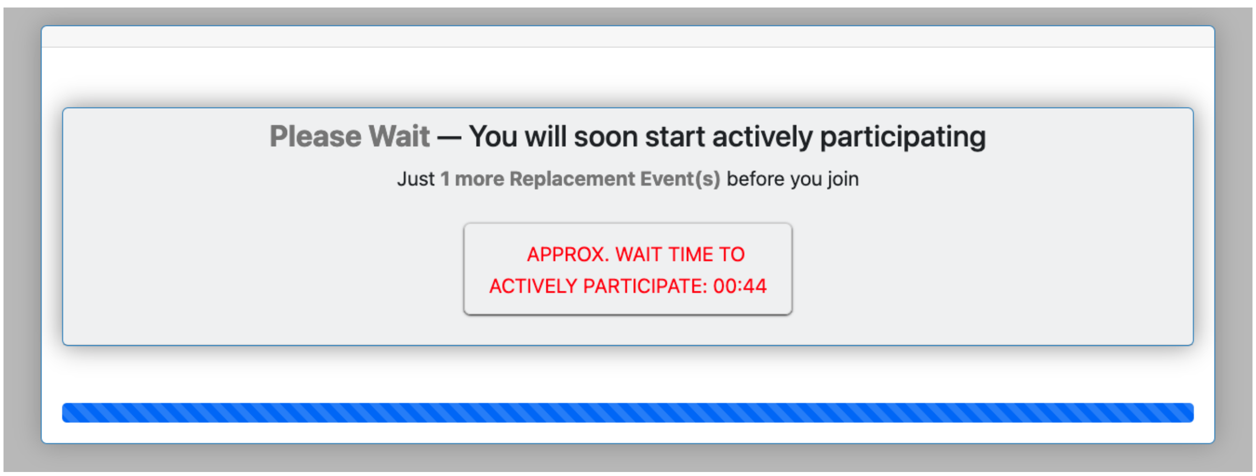



### A.4 Ready to enter, Random Treatment



### A.5 Wait screens



# B Experimental Instructions

The experiment was programmed using oTree (Chen et al., 2016). The following screenshots show the complete instructions for both treatment and related comprehension quizzes, as well as the post-experiment questionnaire.

## B.1 First screen, Pressure Treatment

# Instructions

## Welcome to this experiment in decision-making.

- **General Information:**
  - Please plan on staying on your computer for up to 180 minutes.
  - This study consists of a random number of **Contest Rounds** in which 4 active subjects compete by bidding over the fraction of a prize they will receive.
  - At the end of each **Contest Round** with 10% probability a **Replacement Event** occurs. When such an event happens, the participant with the lowest payoff in that round is replaced with a new inexperienced participant.
  - The 4 participants in today's first **Contest Round** will be selected at random among a larger pool of enrolled participants and immediately receive detailed instructions. Instructions will remain the same for the entire session.
  - The rest of the enrolled subjects must wait and queue to later enter the study, and will receive the same detailed instructions, once their turn approaches. Information on the expected wait time to enter the study will be individually provided.
  - Participants waiting to later join the study are free to keep this window in the background and engage in other leisure activities on their device, but it is crucial that they keep an eye on the timer and queue position and also "Allow Notifications" in this page (button below) so that they can be informed when their turn approaches.
  - Before actively joining the study, waiting participants will receive a notification (even if they minimize this window) and hear (if they keep this window open in the background) the following sound ▶ ♫ , after which they will have 45 seconds to confirm they are ready to join. Failure to confirm will relegate participants to the end of the queue.
- **Participants Compensation:**
  - After the end of your participation in the experiment, a US$10 show-up fee, plus your earnings from one random **Contest Round** for each **Replacement Event** you reached transformed to US dollars (US$) at a rate of US$1 per L$20, will be paid to you.
  - If you leave before being given permission by the experimenter, you will only be eligible for the US$10 show-up fee.

**Please note**: Throughout the experiment, endowments, investments and payoffs will always be expressed in Laboratory Dollars, L$.

→ Please Allow Notifications to Continue

## B.2 First screen, No Pressure Treatment

# Instructions

## Welcome to this experiment in decision-making.

- **General Information:**
  - Please plan on staying on your computer for up to 180 minutes.
  - This study consists of a random number of **Contest Rounds** in which 4 active subjects compete by bidding over the fraction of a prize they will receive.
  - At the end of each **Contest Round** with 10% probability a **Replacement Event** occurs. When such an event happens, one participant is randomly replaced with a new inexperienced participant.
  - The 4 participants in today's first **Contest Round** will be selected at random among a larger pool of enrolled participants and immediately receive detailed instructions. Instructions will remain the same for the entire session.
  - The rest of the enrolled subjects must wait and queue to later enter the study, and will receive the same detailed instructions, once their turn approaches. Information on the expected wait time to enter the study will be individually provided.
  - Participants waiting to later join the study are free to keep this window in the background and engage in other leisure activities on their device, but it is crucial that they keep an eye on the timer and queue position and also "Allow Notifications" in this page (button below) so that they can be informed when their turn approaches.
  - Before actively joining the study, waiting participants will receive a notification (even if they minimize this window) and hear (if they keep this window open in the background) the following sound (▶ ♫), after which they will have 45 seconds to confirm they are ready to join. Failure to confirm will relegate participants to the end of the queue.
- **Participants Compensation:**
  - After the end of your participation in the experiment, a US$10 show-up fee, plus your earnings from one random **Contest Round** for each **Replacement Event** you reached transformed to US dollars (US$) at a rate of US$1 per L$20, will be paid to you.
  - If you leave before being given permission by the experimenter, you will only be eligible for the US$10 show-up fee.

**Please note**: Throughout the experiment, endowments, investments and payoffs will always be expressed in Laboratory Dollars, L$.

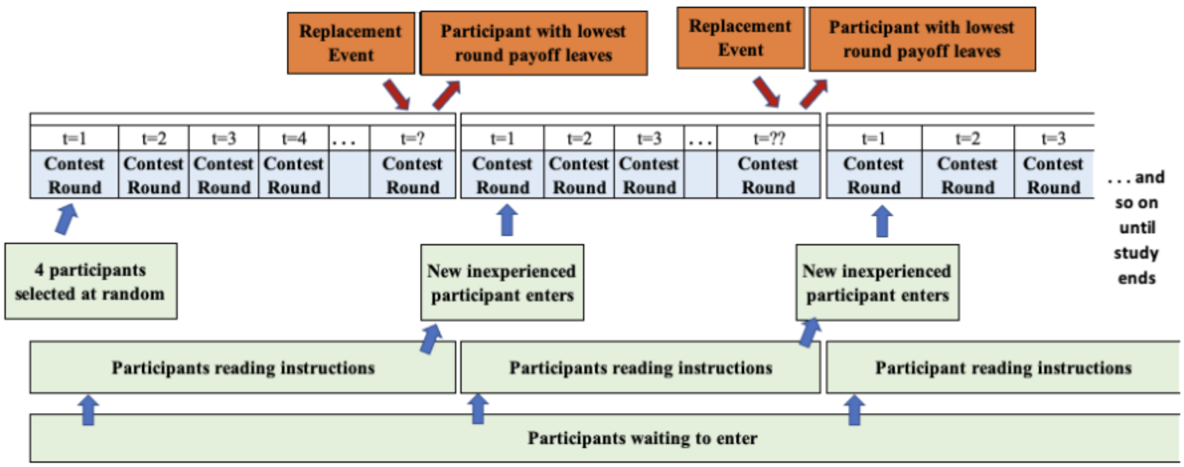
→ Please Allow Notifications to Continue

## B.3 Second screen, Pressure Treatment

# Instructions: Structure of the Study

**Read these instructions carefully.** You will have to successfully answer some questions that check your understanding of the instructions before you can proceed on to the study. If you have any questions let the instructor know via the zoom chat.

- **The structure of the study is illustrated below:**




- **Some Important Reminders**:
  - In each **Contest Round** the 4 active compete over the split of a prize.
  - At the end of each **Contest Round** with 10% probability a **Replacement Event** occurs, the total number of **Replacement Events** before the end of the study is unknown to participants.
  - When such an event happens, the participant with the lowest payoff in that round is replaced with a new inexperienced participant.
- **Participant Compensation**:
  - After the end of your participation in the experiment, a US$10 show-up fee, plus your earnings from one random **Contest Round** for each **Replacement Event** you reached transformed to US dollars (US$) at a rate of US$1 per L$20, will be paid to you.
  - Thus, the more **Replacement Events** you reach, the more payments you'll receive. Since you do not know which rounds will be chosen for payment, you will want to do your best in every round.
  - If you leave the session before being given permission by the experimenter, you will only be eligible for the US$10 show-up fee.

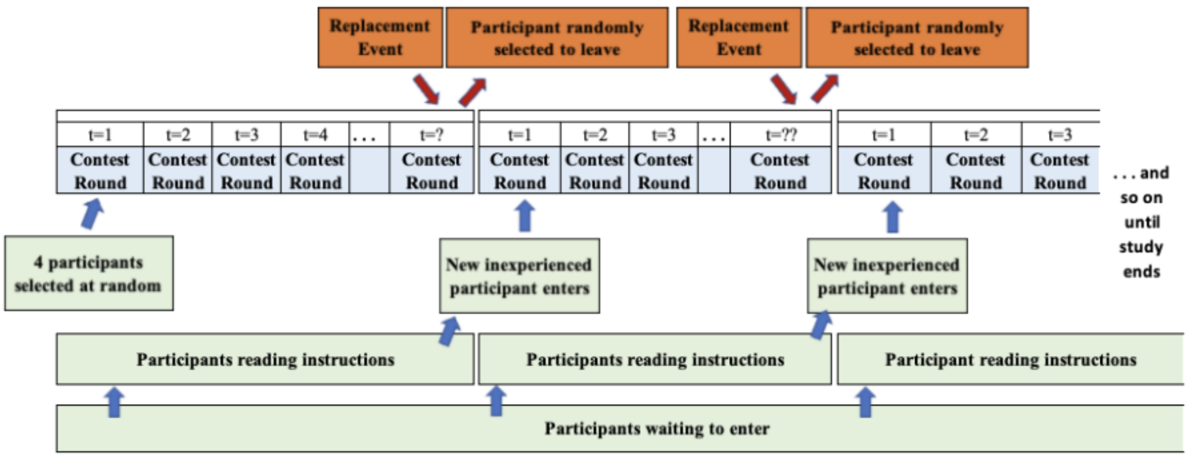
Next

## B.4 Second screen, No Pressure Treatment

# Instructions: Structure of the Study

**Read these instructions carefully.** You will have to successfully answer some questions that check your understanding of the instructions before you can proceed on to the study. If you have any questions let the instructor know via the zoom chat.

- **The structure of the study is illustrated below:**



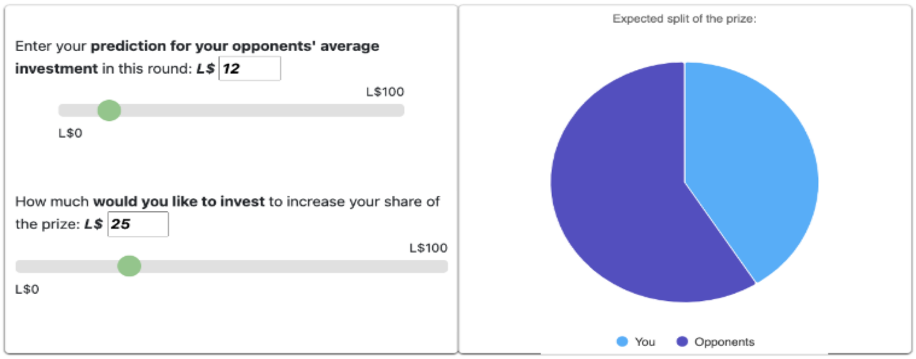

- **Some Important Reminders**:
  - In each **Contest Round** the 4 active compete over the split of a prize.
  - At the end of each **Contest Round** with 10% probability a **Replacement Event** occurs, the total number of **Replacement Events** before the end of the study is unknown to participants.
  - When such an event happens, one participant is randomly replaced with a new inexperienced participant.
- **Participant Compensation**:
  - After the end of your participation in the experiment, a US$10 show-up fee, plus your earnings from one random **Contest Round** for each **Replacement Event** you reached transformed to US dollars (US$) at a rate of US$1 per L$20, will be paid to you.
  - Thus, the more **Replacement Events** you reach, the more payments you'll receive. Since you do not know which rounds will be chosen for payment, you will want to do your best in every round.
  - If you leave the session before being given permission by the experimenter, you will only be eligible for the US$10 show-up fee.

Next

## B.5 Third screen, Pressure Treatment

### Instructions: Contest Rounds

- In each **Contest Round** that you participate in, you wll have to use the following interface to make your decisions (figure below):




**REMEMBER:**
You'll keep your endowment minus your investment and receive a fraction of the value of the prize, L$100, which depends on your and your opponents' investments.
If no ones invests, everyone keeps their endowment and the prize is split four ways.

**Based on your prediction and investment, you expect to receive:**

- **40.98% of the prize of L$100, that is L$40.98**
- **keep L$75.00 from your endowment**
  - **Thus, your expected payoff is L$115.98**
  - **And your opponents' average, L$107.67**

- In each **Contest Round** you and the other 3 active participants (your opponents) compete in the following way:
  - You and your opponents are each endowed with *L$100* to use completely or partially to invest in increasing your share of a *L$100* prize.
  - The more you invest relative to your opponents, the higher the fraction of the prize you'll receive. In particular, if only one participant invests, they would take the whole prize independent of the invested amount. If all participants invest the same, the prize is equally split.
  - Formally, if participant *i*, *j*, *k* and *l* invest $x_i$, $x_j$, $x_k$ and $x_l$, respectively, then the fraction, $f_i$ of the prize that participant *i* would take would be equal to:

$$f_i(x_i, x_j, x_k, x_l) = \begin{cases} \frac{x_i}{x_i+x_j+x_k+x_l}, & \text{if } x_i + x_j + x_k + x_l \neq 0 \\ \frac{1}{4}, & \text{otherwise} \end{cases}$$

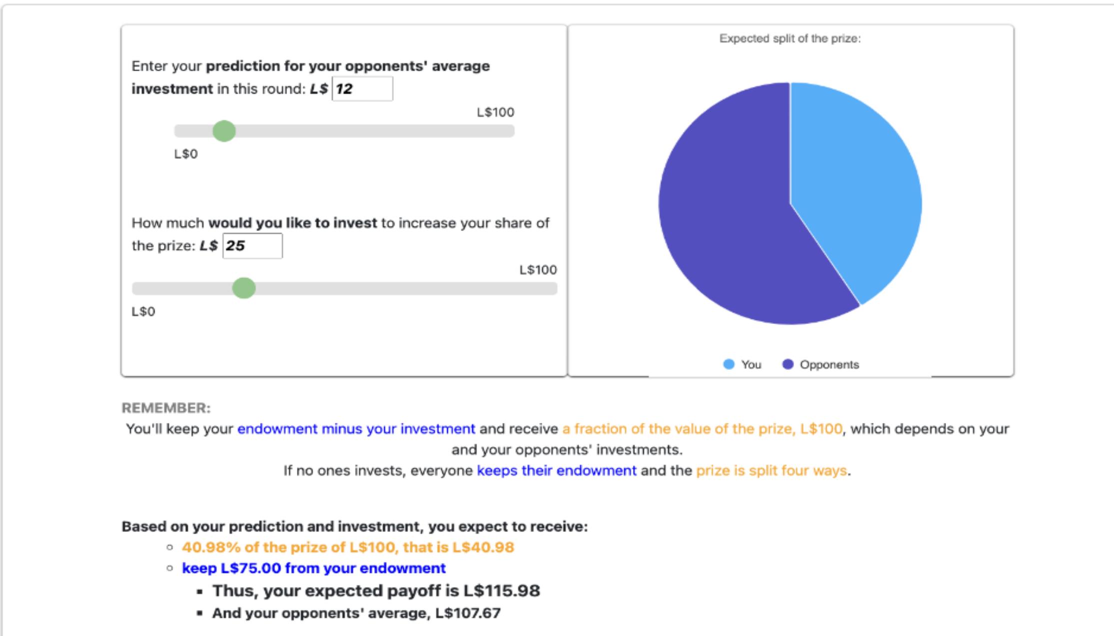
  - In addition to choosing your investment, you can make a prediction about the average investment of your opponents. This will help inform your decision. The better your prediction, the more accurate the predicted result will be.
  - On the left-hand side of your screen you'll have 2 sliders (and corresponding input boxes): one at the top that you can use for your prediction and one at the bottom which corresponds to your desired investment. Once you are happy with your prediction and your investment, you can press the "Submit" button.
  - On the right-hand side of your screen you'll see a pie chart that illustrates the expected outcome given your prediction and investment.
  - Whatever part of your endowment you do not invest is yours to keep and will be included in your round payoff along the fraction of the prize you receive. But note that you cannot save investment budgets across rounds: For each contest you will have *L$100* to invest, independent of what you invested in previous rounds.
  - **REMEMBER:** At the end of each **Contest Round** the probability of a **Replacement Event** is 10%. If a replacement event happens, the participant with the lowest payoff in that round will be asked to leave the study and replaced with an inexperienced participant.

Next

## B.6 Third screen, No Pressure Treatment

# Instructions: Contest Rounds

- In each **Contest Round** that you participate in, you wll have to use the following interface to make your decisions (figure below):




- In each **Contest Round** you and the other 3 active participants (your opponents) compete in the following way:
  - You and your opponents are each endowed with *L$100* to use completely or partially to invest in increasing your share of a *L$100* prize.
  - The more you invest relative to your opponents, the higher the fraction of the prize you'll receive. In particular, if only one participant invests, they would take the whole prize independent of the invested amount. If all participants invest the same, the prize is equally split.
  - Formally, if participant *i*, *j*, *k* and *l* invest $x_i$, $x_j$, $x_k$ and $x_l$, respectively, then the fraction, $f_i$ of the prize that participant *i* would take would be equal to:

$$f_i(x_i, x_j, x_k, x_l) = \begin{cases} \frac{x_i}{x_i + x_j + x_k + x_l}, & \text{if } x_i + x_j + x_k + x_l \neq 0 \\ \frac{1}{4}, & \text{otherwise} \end{cases}$$

  - In addition to choosing your investment, you can make a prediction about the average investment of your opponents. This will help inform your decision. The better your prediction, the more accurate the predicted result will be.
  - On the left-hand side of your screen you'll have 2 sliders (and corresponding input boxes): one at the top that you can use for your prediction and one at the bottom which corresponds to your desired investment. Once you are happy with your prediction and your investment, you can press the "Submit" button.
  - On the right-hand side of your screen you'll see a pie chart that illustrates the expected outcome given your prediction and investment.
  - Whatever part of your endowment you do not invest is yours to keep and will be included in your round payoff along the fraction of the prize you receive. But note that you cannot save investment budgets across rounds: For each contest you will have *L$100* to invest, independent of what you invested in previous rounds.
  - **REMEMBER:** At the end of each **Contest Round** the probability of a **Replacement Event** is 10%. If a replacement event happens, one participant at random is asked to leave the study and replaced with a new inexperienced participant.

Next

## B.7 Quiz screen 1, Pressure Treatment

# Instructions Quiz Part I

Before proceeding to the study, please answer the following questions regarding the instructions you just read:

1.How many **Contest Rounds** are there in between **Replacement Events** ?

- It is randomly determined and there are always the same number of rounds in between **Replacement Events**
- Always 4 rounds in between **Replacement Events**

--> That is incorrect, please try again

- It is randomly determined and there is possibly a different number of rounds in between **Replacement Events**

--> Correct! After each round there is a 90% probability of participating in a new round before a new Replacement Event

2. Are instructions going to be changing throughout the experiment?

- No, instructions stay the same throughout the experiment

--> Correct! Instructions stay the same. A summary of the instructions is provided in the decision screens

- Yes, new instructions will be provided every other **Replacement Event**
- Yes, instructions change in the middle of the session

3. If I am the only participant who invests in a **Contest Round**:

- I would take 75% of the prize.
- I would take a 100% of the prize.

--> Correct! If you are the only participant investing you will certainly receive the whole prize

- It depends on how much I invested.

4. What happens if I have the lowest round payoff and a **Replacement Event** occurs at the end of that round?

- It will depend on my average payoff since the last **Replacement Event**
- I will be asked to leave the study because of my low round payoff

--> Correct! Whenever a Replacement Event occurs, the participant with the lowest payoff in that round must leave

- I will be able to keep playing for just one more round

5. What are the expected round payoffs if all my opponents and I decide to invest exactly *L$30* in a **Contest Round**:

- We would each receive *L$50* of the prize and keep *L$70* of our respective endowments
- We would each receive *L$70* and keep *L$25* of our respective endowments
- We would each receive *L$25* and keep *L$70* of our respective endowments

--> Correct! Each participant would get a 25% of the prize, that is L$25 each

6.What percentage of the prize would you take if you and one other participant invest the same amount and everyone else invests zero?

- 50%

--> Correct! The prize would be equally split between the 2 participants that invest the same positive amount

- 25%
- 100%

Next

## B.8 Quiz screen 1, No Pressure Treatment

# Instructions Quiz Part I

Before proceeding to the study, please answer the following questions regarding the instructions you just read:

1.How many **Contest Rounds** are there in between **Replacement Events** ?

- It is randomly determined and there are always the same number of rounds in between **Replacement Events**
- Always 4 rounds in between **Replacement Events**

--> That is incorrect, please try again

- It is randomly determined and there is possibly a different number of rounds in between **Replacement Events**

--> Correct! After each round there is a 90% probability of participating in a new round before a new Replacement Event

2. Are instructions going to be changing throughout the experiment?

- No, instructions stay the same throughout the experiment

--> Correct! Instructions stay the same. A summary of the instructions is provided in the decision screens

- Yes, new instructions will be provided every other **Replacement Event**
- Yes, instructions change in the middle of the session

3. If I am the only participant who invests in a **Contest Round**:

- I would take 75% of the prize.
- I would take a 100% of the prize.

--> Correct! If you are the only participant investing you will certainly receive the whole prize

- It depends on how much I invested.

4. What happens if I have the lowest round payoff and a **Replacement Event** occurs at the end of that round?

- It will depend on my average payoff since the last **Replacement Event**
- One participant is randomly selected to leave regardless of payoffs

--> Correct! Whenever a Replacement Event occurs one participant is randomly selected to leave regardless of payoffs

- I will be able to keep playing for just one more round

5. What are the expected round payoffs if all my opponents and I decide to invest exactly *L$30* in a **Contest Round**:

- We would each receive *L$50* of the prize and keep *L$70* of our respective endowments
- We would each receive *L$70* and keep *L$25* of our respective endowments
- We would each receive *L$25* and keep *L$70* of our respective endowments

--> Correct! Each participant would get a 25% of the prize, that is L$25 each

6.What percentage of the prize would you take if you and one other participant invest the same amount and everyone else invests zero?

- 50%

--> Correct! The prize would be equally split between the 2 participants that invest the same positive amount

- 25%
- 100%

Next

## B.9 Quiz screen 2, Pressure Treatment

### Instructions Quiz Part II

**Please use the sliders or input boxes (same interface you will use in each Contest Round ) to answer the following questions regarding the instructions you just read:**

Enter your **prediction for your opponents' average investment** in this round: ***L$*** *27*

L$100

L$0

How much **would you like to invest** to increase your share of the prize: ***L$*** *9*

L$100

L$0

Expected split of the prize:


**REMEMBER:**

You'll keep your endowment minus your investment and receive a fraction of the value of the prize, L$100, which depends on your and your opponents' investments.

If no ones invests, everyone keeps their endowment and the prize is split four ways.

**Based on your prediction and investment, you expect to receive:**

- **10.00% of the prize of L$100, that is L$10.00**
- **keep L$91.00 from your endowment**
  - **Thus, your expected payoff is L$101.00**
  - **And your opponents' average, L$103.00**

### Questions:

7. If you expect your opponents to invest an average of *L$25* and you invest *L$55*, then what percentage of the prize would you expect to receive?

- 22.56% of the prize
- 82.25% of the prize
- (selected) 42.31% of the prize
- --> Correct! You are using the sliders properly

8. If you expect your opponents to invest an average of *L$25* and you invest *L$25*, then what is the payoff you would expect to receive?

- (selected) *L$100*
- --> Correct! Your payoff would be L$100: L$75 coming from your original endowment plus 25% of the prize
- *L$125*
- *L$75*

9. If you expect your opponents to invest an average of *L$10*, which of the following investment choice would result in the highest percentage of the prize for you?

- Invest *L$25*
- Invest *L$50*
- (selected) Invest *L$100*
- --> Correct! Invest L$100 if you only care about maximizing your share of the prize

10. If you expect your opponents to invest an average of *L$27*, which of the following investment choice would result in the highest payoff for you?

- (selected) Invest *L$9*
- --> Correct! This would maximize your payoff, which includes what you keep from your endowment
- Invest *L$25*
- Invest *L$60*

Next

## B.10 Quiz screen 2, No Pressure Treatment

### Instructions Quiz Part II

**Please use the sliders or input boxes (same interface you will use in each Contest Round ) to answer the following questions regarding the instructions you just read:**

Enter your **prediction for your opponents' average investment** in this round: ***L$*** 27

L$100

L$0

How much **would you like to invest** to increase your share of the prize: ***L$*** 9

L$100

L$0

Expected split of the prize:


**REMEMBER:**

You'll keep your endowment minus your investment and receive a fraction of the value of the prize, L$100, which depends on your and your opponents' investments.

If no ones invests, everyone keeps their endowment and the prize is split four ways.

**Based on your prediction and investment, you expect to receive:**

- **10.00% of the prize of L$100, that is L$10.00**
- **keep L$91.00 from your endowment**
  - **Thus, your expected payoff is L$101.00**
  - **And your opponents' average, L$103.00**

### Questions:

7. If you expect your opponents to invest an average of *L$25* and you invest *L$55*, then what percentage of the prize would you expect to receive?

- ○ 22.56% of the prize
- ○ 82.25% of the prize
- ◉ 42.31% of the prize

--> Correct! You are using the sliders properly

8. If you expect your opponents to invest an average of *L$25* and you invest *L$25*, then what is the payoff you would expect to receive?

- ◉ *L$100*

--> Correct! Your payoff would be L$100: L$75 coming from your original endowment plus 25% of the prize

- ○ *L$125*
- ○ *L$75*

9. If you expect your opponents to invest an average of *L$10*, which of the following investment choice would result in the highest percentage of the prize for you?

- ○ Invest *L$25*
- ○ Invest *L$50*
- ◉ Invest *L$100*

--> Correct! Invest L$100 if you only care about maximizing your share of the prize

10. If you expect your opponents to invest an average of *L$27*, which of the following investment choice would result in the highest payoff for you?

- ◉ Invest *L$9*

--> Correct! This would maximize your payoff, which includes what you keep from your endowment

- ○ Invest *L$25*
- ○ Invest *L$60*

Next

## B.11 Post-Experiment Survey screen 1

# Exit Questionnaire

The following questionnaire is for research purposes only. Your answers will stay anonymous. Your payment will be prepared while you answer this questionnaire. We thank you for your cooperation.

1. How old are you?

2. What is your sex?

○ Male ○ Female ○ Non-Binary

3. What is (closest) to your major field of study? If more than one major, or major not represented, please select "other STEM" or "non-STEM" as appropriate

--------

4. Which one of the following best describes you?

--------

5. What is your current GPA ?

6. Choose the scale that best describes how you see yourself: are you a person who is generally willing to take risks, or do you try to avoid taking risks?

Completely unwilling ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 Very willing

7. Choose the scale that best describes how willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?

Completely unwilling ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 Very willing

8. Choose the scale to which the following statements describe you: "Competition brings the best out of me"

Not at all like me ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 Exactly like me

9. How many math classes have you taken in college?

10. How many statistics classes have you taken in college?"

11. How many economics classes have you taken in college?"

Next

## Post-Experiment Survey screen 2 and 3

# Exit Questionnaire

For the next four questions, please provide the answer that you think is better.

11. If John can drink one barrel of water in 6 days, and Mary can drink one barrel of water in 12 days, how many days would it take them to drink one barrel of water together?

12. Jerry received both the 15th highest and the 15th lowest mark in the class. How many students are in the class?

13. A man buys a pig for $60, sells it for $70, buys it back for $80, and sells it finally for $90. How much ($) has he made?

14. Simon decided to invest $8,000 in the stock market one day early in 2008. Six months after he invested, on July 17, the stocks he had purchased were down 50%. Fortunately for Simon, from July 17 to October 17, the stocks he had purchased went up 75%. At this point, Simon has:

- broke even in the stock market
- is ahead of where he began
- has lost money

Next

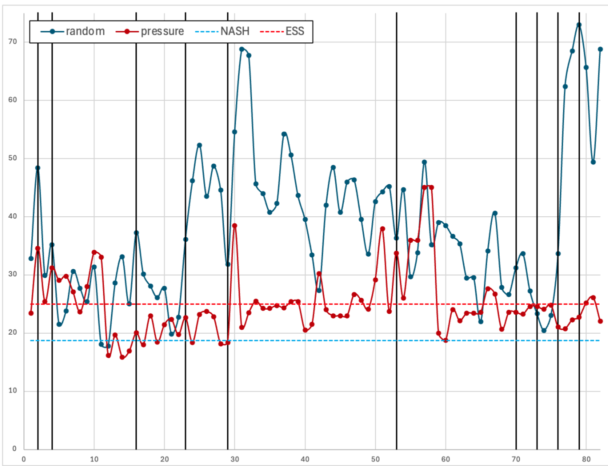
# Exit Questionnaire

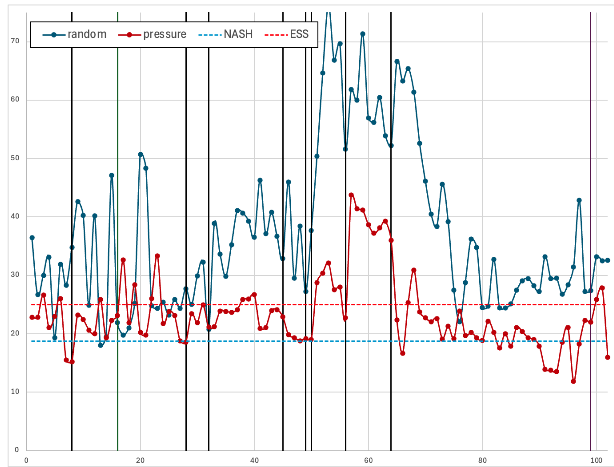
15. Please briefly describe your strategy/approach when deciding your investment.

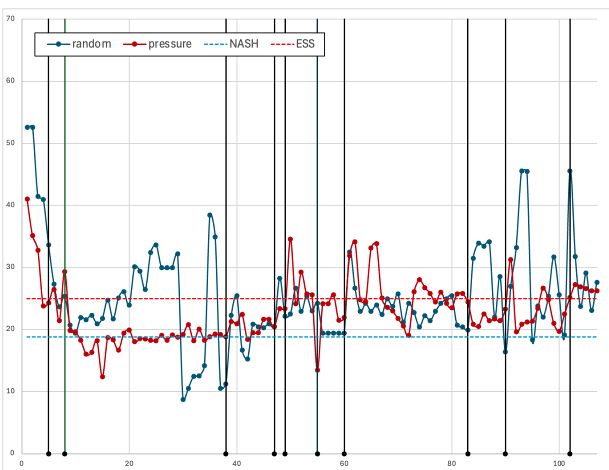
Next

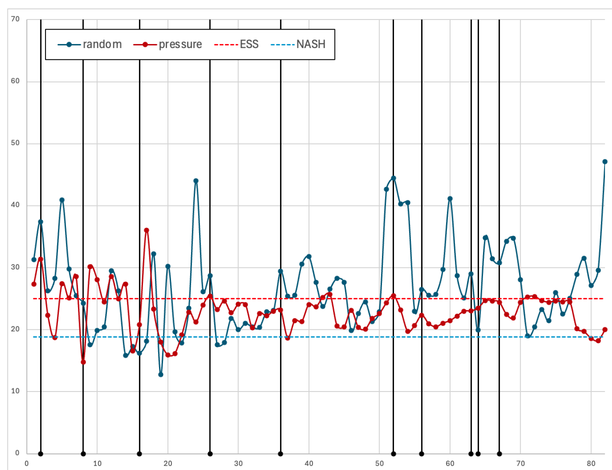
# C Session-Level Investment Patterns

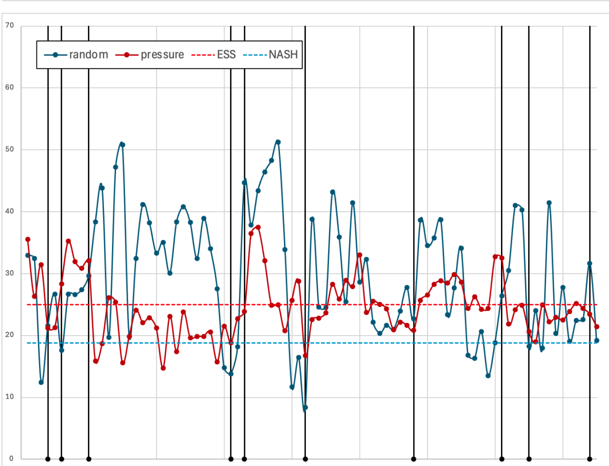
The Figures below display average Investment Patterns by session pair for the 12 sessions conducted of each treatment (24 sessions in total) Recall that pairs of sessions use the same allocations of replacement cycles. Most **Pressure**-sessions display lower levels and variance of investments. In **No Pressure**-sessions, investment levels are highly volatile and rise significantly above both Nash- and ESS-levels.

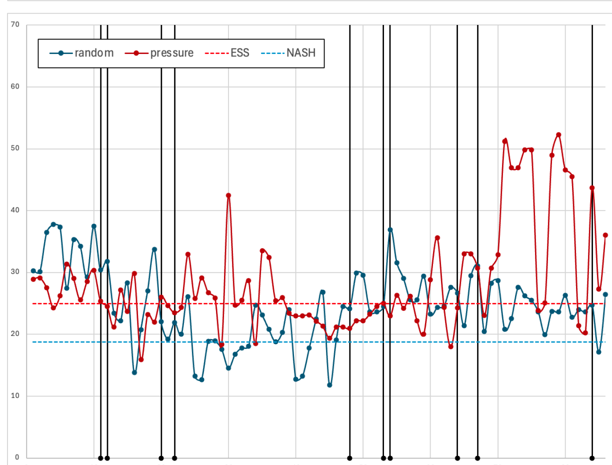


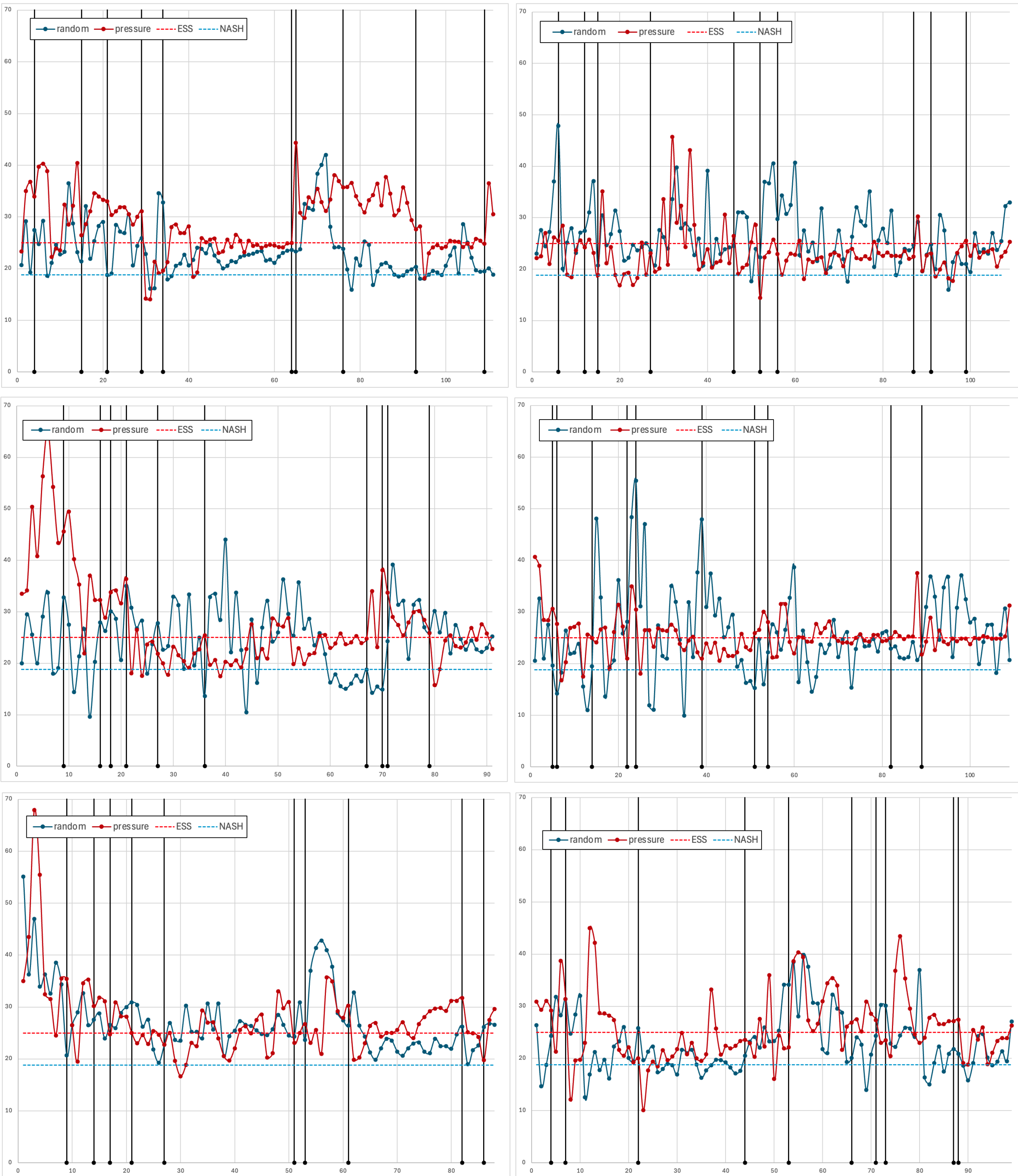




random
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# D Hazard Rates by Personal Cycle

Table 8: Hazard Rates by Personal Cycle Under the Pressure Treatment

| Personal Cycle $x$ | Alive at Start | Eliminated at $x$ | Hazard | Conditional Survival |
|---|---|---|---|---|
| 1 | 168 | 66 | 0.3929 | 0.6071 |
| 2 | 102 | 27 | 0.2647 | 0.7353 |
| 3 | 70 | 11 | 0.1571 | 0.8429 |
| 4 | 54 | 9 | 0.1667 | 0.8333 |
| 5 | 40 | 5 | 0.1250 | 0.8750 |
| 6 | 34 | 6 | 0.1765 | 0.8235 |
| 7 | 25 | 1 | 0.0400 | 0.9600 |
| 8 | 23 | 3 | 0.1304 | 0.8696 |
| 9 | 18 | 3 | 0.1667 | 0.8333 |
| 10 | 12 | 1 | 0.0833 | 0.9167 |
| 11 | 9 | 0 | 0.0000 | 1.0000 |
| 12 | 9 | 0 | 0.0000 | 1.0000 |

*Note*: Personal cycle $x$ indexes the number of consecutive cycles a subject has survived since first entering the environment. Because subjects enter and exit at different times, personal cycles are distinct from global rounds and represent each subject's own survival sequence. Differences between successive "Alive at Start" values need not equal the corresponding "Eliminated at $x$" values because some subjects are *right-censored*, meaning the experiment ends before they face another elimination opportunity.