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Indefinitely Repeated Contests: An Experimental Study

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Abstract

We experimentally explore indefinitely repeated contests. Theory predicts more cooperation, in the form of lower expenditures, in indefinitely repeated contests with a longer expected time horizon, yet our data do not support this prediction. Theory also predicts more cooperation in indefinitely repeated contests compared to finitely repeated contests of the same expected length, but we find no significant difference empirically. When controlling for risk and gender, we actually find significantly *higher* long-run expenditure in some indefinite contests relative to finite contests. Finally, theory predicts no difference in cooperation across indefinitely repeated *winner-take-all* and *proportional-prize* contests. We find significantly less cooperation in the latter, because female participants expend more on average than their male counterparts in our data. Our paper extends the experimental literature on indefinitely repeated games to contests and, more generally, contributes to an infant empirical literature on behavior in indefinitely repeated games with "large" strategy spaces.

Keywords: contest, repeated game, cooperation, experiment
JEL classification codes: C72, C73, C91, D72

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1 Introduction

Contests are frequently-observed strategic situations where players devote costly and irreversible resources (such as time, money, or effort) to increase their chances of winning a reward (e.g., a prize, rent, or patent). Research and development races, advertising wars, political campaigns, lobbying efforts, legal battles, sports tournaments, and employee-of-the-month challenges are all examples of contests.

A defining characteristic of many contests is that they are *dynamic*. For example, Coca-Cola and Pepsi have targeted aggressive advertising campaigns at each other since the 1950s. Both firms continue to engage in a series of monthly, weekly, and even daily contests for soft drink market share, and their ongoing feud has no well-defined time horizon.

This study focuses on a particular class of dynamic contests in which the contest length is unknown. Specifically, we experimentally explore behavior in two-player repeated contests of indefinite length.[1] The experimental indefinite supergame literature has largely focused on the Prisoner's Dilemma (PD).[2] To the best of our knowledge, no study has yet experimentally examined behavior in indefinitely repeated contests.

Indefinitely repeated contests are interesting not only because contests are important economic phenomena, but also because contests have (relatively) large strategy spaces, and very little is known about behavior in indefinitely repeated games with large strategy spaces. Our paper both extends the existing experimental indefinite supergame literature to contests *and* adds to our general understanding of behavior in indefinite supergames with many feasible actions.

Existing experimental studies of the indefinitely repeated PD focus on two predictions from the theory of repeated games: (i) *Cooperation increases in the expected length of an indefinite supergame*, and (ii) *Cooperation in indefinite supergames should be at least as high as cooperation in finite supergames.* Many, though not all studies confirm these predictions.[3]

[1] Games that are repeated a known number of times are termed *finite supergames* or *finitely repeated games*, while games with an unknown time horizon are *indefinite supergames* or *indefinitely repeated games* (Friedman, 1971).

[2] For example, see Murnighan and Roth (1983); Dal Bó (2005); Duffy and Ochs (2009); Dal Bó and Fréchette (2011). Dal Bó and Fréchette (Forthcoming) survey of the experimental supergame literature. Non-prisoner's dilemma indefinite supergame experiments include Palfrey and Rosenthal (1994); Sell and Wilson (1999); Tan and Wei (2014); Lugovskyy et al. (2017) (public goods), Engle-Warnick and Slonim (2006*a*,*b*) (trust), Holt (1985); Feinberg and Husted (1993) (oligopoly), McBride and Skaperdas (2014) (conflict), and Camera and Casari (2014); Duffy and Puzzello (2014) (monetary exchange).

[3] Some studies mostly confirm theory (Dal Bó, 2005; Duffy and Ochs, 2009; Dal Bó and Fréchette, 2011;

Contests and PDs are social dilemmas. In both games, the equilibrium of the stage game is not socially optimal, but the socially optimal or "cooperative" outcome can be supported in an indefinite supergame when players are sufficiently patient. However, there are critical differences between contests and PDs.

First, as already mentioned, relative to the two-strategy PD, there are many more feasible strategies in contests. Moreover, contests do not have a dominant strategy. In this respect they are not only more complex than PDs, but also more complex than linear public good games (PGG) that can serve as extended strategy space analogs of the PD. Contests have a nonmonotone (typically, single-peaked) best response; that is, relatively low expenditure levels are best responses to both low rival expenditure *and* high rival expenditure. This contrasts with PDs and linear PGGs which have a dominant strategy, and to coordination games and supermodular games which have unidirectional best responses.

Finally, behaviorally, contests are rife with "overbidding"—an almost ubiquitous experimental finding that average expenditure exceeds the risk-neutral Nash equilibrium expenditure and that a sizable fraction of participants choose strictly dominated expenditures (Sheremeta, 2013; Dechenaux, Kovenock and Sheremeta, 2015). By construction, such overbidding is impossible in PDs, PGGs, or supermodular games. It is thus an open empirical question as to whether the comparative statics of cooperation in indefinitely repeated contests are similar to those in indefinitely repeated PDs and other previously studied games.

We conduct indefinitely repeated contest experiments using the well-established continuation probability approach.[4] Following Dal Bó (2005)'s seminal indefinitely repeated PD study, our experimental design lets us compare cooperation across indefinite contests of *different* expected length and between finitely and indefinitely repeated contests of the *same* expected length.

As do existing indefinitely repeated PD studies, we ask two main questions: (i) *Does cooperation increase in the expected length of indefinite contest supergames?*, and (ii) *Is cooperation greater in indefinite contest supergames compared to finite contest supergames?* We also consider whether contest outcomes depend on the allocation rule for distributing the contested prize. Specifically, we examine a *winner-take-all* allocation rule and a

Fréchette and Yuksel, 2017) while others report more mixed support for theory (Roth and Murnighan, 1978; Murnighan and Roth, 1983; Normann and Wallace, 2012).

[4]See Roth and Murnighan (1978). For comparisons of supergame termination rules, see Normann and Wallace (2012) and Fréchette and Yuksel (2017).

proportional-prize allocation rule.

In a *winner-take-all* setting (Tullock, 1980), the entire contest prize is awarded stochastically, according to probabilities equal to each players' share of total expenditure. As in a patent race, this setting is extreme because one player receives the prize, while all other players receive zero revenue. In a *proportional-prize* setting (Long and Vousden, 1987), each player's share of the contest prize is their share of total expenditure. As in our Cola Wars example, this "smooth" allocation rule implies that a firm's market share is increasing in its own advertising expenditure, but decreasing in its rivals' expenditures. For risk-neutral players, the equilibrium expenditure is the same across both settings, but empirically: *Is cooperation the same across indefinitely repeated winner-take-all and proportional-prize contests*?

We observe little evidence of greater cooperation in indefinitely repeated contests of larger expected length. Comparing across two expected lengths, there is no difference in our *winner-take-all* data. There is some difference in our *proportional-prize* data, but when we control for our participants' risk preferences and gender—characteristics previously found to significantly affect contestant behavior—this result vanishes. We also do not observe more cooperation in indefinitely repeated contests compared to finitely repeated contests of the same expected length in either contest setting. In fact, when we control for risk and gender, we find evidence of *less* long-run cooperation in indefinite contests relative to finite contests. Finally, we find strong evidence of greater cooperation in *winner-take-all* contests relative to *proportional-prize* contests. This result is driven by differences in average expenditure across gender.

Our results are particularly interesting vis-à-vis results from indefinitely repeated prisoner's dilemma experiments where at least some support is typically found for predictions of the theory of repeated games. As in a recent study of indefinitely repeated public good games (Lugovskyy et al., 2017), we find less support for these predictions in our contest environments. While much experimental work remains to be done on indefinite supergames with "large" strategy spaces, the early returns suggest that standard theory has less predictive power in more complex indefinite supergames than in relatively simpler indefinite supergames such as the prisoner's dilemma.

Our paper is organized as follows: In Section 2 we briefly describe a model of indefinitely repeated contests. Section 3 details our experimental design and procedures. We outline our three testable hypotheses in Section 4. In Section 5 we analyze the results of experiments, and we discuss our results and conclude in Section 6.

2 Theory

We consider two contest settings. The first setting is the *winner-take-all* contest (*WTA*) where one player earns all of the prize revenue and all other players earn zero prize revenue. The second setting is the *proportional-prize* contest (*PP*) where each player earns a share of the prize revenue equal to their expenditure divided by total expenditure. Because we assume that all players are risk-neutral, and because we use the standard lottery contest success function (CSF) of Tullock (1980), both settings are strategically equivalent and the equilibria for both settings coincide. Without loss of generality, we provide details of a dynamic contest model assuming each that stage game is a *WTA* contest.

2.1 The Stage Game

Consider a contest with two risk-neutral players, indexed by $i = 1, 2$, competing for a prize $V > 0$ by independently and simultaneously choosing expenditure levels $x_i \geq 0$. The probability of Player 1 winning the contest is given by the CSF:

$$p(x_1, x_2) = \begin{cases} \frac{x_1}{x_1+x_2} & \text{if } x_1 + x_2 > 0 \\ \frac{1}{2} & \text{if } x_1 + x_2 = 0 \end{cases}, \tag{1}$$

and the probability of Player 2 winning is $1 - p(x_1, x_2)$. The expected payoffs of the players are:

$$\pi_1 = Vp(x_1, x_2) - x_1, \quad \pi_2 = V[1 - p(x_1, x_2)] - x_2. \tag{2}$$

In the unique symmetric Nash equilibrium (NE), both players choose expenditure levels $x^* = \frac{V}{4}$ and earn expected payoffs $\pi^* = \frac{V}{4}$. Socially optimal, cooperative play (SO) is characterized by both players choosing expenditure $x^{\text{so}} = 0$ and earning expected payoffs $\pi^{\text{so}} = \frac{V}{2}$.

In what follows, we use a modified version of the contest game where expenditure levels x_i are restricted to nonnegative integers. For a sufficiently large V, this modified game is a good approximation of the original game and has the same equilibrium and socially optimal expenditures and payoffs as the original game, provided that V is divisible by 4.

2.2 The Supergame

Consider an infinitely repeated, dynamic game where the modified contest described in Section 2.1 is the stage game. Both players discount future payoffs by factor $\delta \in [0, 1]$.

Fully cooperative play (both players choosing x^{so} forever) can be supported as a subgame perfect Nash equilibrium (SPNE) in this dynamic game if both players use a Nash reversion, grim trigger strategy and if δ satisfies the condition:

$$\frac{\pi^{\mathrm{so}}}{1-\delta} > V - 1 + \frac{\delta \pi^*}{1-\delta}. \tag{3}$$

The left-hand side of (3) is the expected payoff from full cooperation, and the right-hand side of (3) is the payoff from the best deviation (expenditure $x^{\mathrm{dev}} = 1$ leads to stage game payoff $V - 1$ for the deviating player, provided that the other player spends $x^{\mathrm{so}} = 0$). Rearranging Condition (3) yields:

$$\delta > \bar{\delta} \equiv \frac{\frac{V}{2} - 1}{\frac{3V}{4} - 1}. \tag{4}$$

Thus if the players are sufficiently patient, a cooperative solution of $(x_1, x_2) = (0, 0)$ can be sustained indefinitely.

Note that Condition (4) only ensures that players prefer cooperation to immediately deviating and suffering the consequences of competitive NE play under the specific trigger strategy we defined above. There are (uncountably) many other trigger strategies, not to mention other, alternative strategies for playing the indefinitely repeated game. So the condition ensuring that players prefer the cooperative solution need not always be Condition (4).

3 Experimental Design and Procedures

To explore how contest expenditure is affected by the discount factor (continuation probability) and by the contest setting, we utilized a 2×2, between-participant experimental design. Along one dimension we varied the continuation probability (*Low* δ or *High* δ) and along the other we varied the contest setting (*WTA* or *PP*). Additionally, for each combination of discount factor and contest setting, we followed Dal Bó (2005) and conducted finitely-repeated "control" sessions of the same expected length as our indefinitely-repeated sessions. The resulting eight treatments are summarized in Table 1.

All of our experiments were conducted at the Economic Science Institute of Chapman University. A total of 240 participants (58.8% female) took part. Each participant was in exactly one session, and none of our participants were experienced in our environment.

Table 1: Summary of Experimental Treatments

Treatment Name	Setting	δ-Value	Control	Sessions	Participants
WTA-Low δ-Indefinite	WTA	0.5		2	40
WTA-Low δ-Finite	WTA	0.5	✓	1	20
WTA-High δ-Indefinite	WTA	0.8		2	40
WTA-High δ-Finite	WTA	0.8	✓	1	20
PP-Low δ-Indefinite	PP	0.5		2	40
PP-Low δ-Finite	PP	0.5	✓	1	20
PP-High δ-Indefinite	PP	0.8		2	40
PP-High δ-Finite	PP	0.8	✓	1	20
				12	240

The experiment was implemented in z-Tree (Fischbacher, 2007). *Low δ* sessions lasted approximately 40 minutes, while *High δ* sessions took roughly 80 minutes to complete.

After participants entered the computer lab, they were randomly assigned to visually-isolated computer carrels. Instructions were read out loud and a printed, reference copy was distributed to participants (see Appendix A for sample instructions). After the instruction phase, participants completed an unpaid practice stage where they entered hypothetical expenditures for themselves and a "paired participant" three times to generate three practice contest outcomes. This practice stage familiarized participants with the underlying contest environment.

All monetary figures in the experiments were denominated in Experimental Currency Units, or ECUs. For all treatments, participants made integer expenditures in the range $[0, 120]$ in each stage game contest. The stage game contest prize was $V = 120$, so that the NE effort was $x^* = 30$ and the payoff assuming NE expenditure was $\pi^* = 30$. Because $x^{SO} = 0$, under our parameterization, the payoff assuming SO expenditure was $\pi^{\text{so}} = 60$, or twice the payoff under NE expenditure.

By Condition (4) from Section 2.2, a prize of $V = 120$ implies a threshold discount factor of $\bar{\delta} = 0.663$. We chose our two discount factors so that the socially optimal, cooperative outcome was supportable with the Nash reversion, grim trigger strategy discussed in Section 2.2 in our *-High* treatments ($\delta = 0.8$), but not in our *-Low* treatments ($\delta = 0.5$). For all treatments, each experimental session consisted of 10 supergames whose stage games are outlined in theory in Section 2.1. We will refer to a stage game as a 'round.'

Prior to the start of the first supergame, participants were randomly paired and instructed that they would only interact with their current "paired participant" during the

Table 2: Supergame Lengths

Treatment Name	Sequence	Supergame 1	2	3	4	5	6	7	8	9	10	Total
Low δ-Indefinite	*A*	2	4	1	1	5	1	1	2	1	2	20
	B	1	2	1	1	5	1	1	2	2	4	20
Low δ-Finite	*C*	2	2	2	2	2	2	2	2	2	2	20
High δ-Indefinite	*D*	5	13	1	2	8	2	2	12	5	1	51
	E	5	1	1	2	8	2	2	12	5	13	51
High δ-Finite	*F*	5	5	5	5	5	5	5	5	5	5	50

Note: The values in the table are the number of rounds (stage games) per supergame.

current supergame. Between supergames, participants were randomly re-paired according to a zipper matching protocol (Cooper et al., 1996). Participants were instructed that they would interact with every other participant in their session during one, and only one, supergame.[5]

In the *Indefinite* treatments, discounting was implemented through random supergame termination.[6] Different pre-drawn realizations of supergame length were used across sessions with the same value of δ. The supergame lengths are shown in Table 2. We constructed Sequence B [E] from Sequence A [D] by swapping the first two supergames (1 and 2) with the last two supergames (9 and 10). For continuation probability δ, the expected supergame length is $\frac{1}{1-\delta}$, or 2 periods when $\delta = 0.5$ and 5 periods when $\delta = 0.8$. All of our *Finite* sessions used either Sequence C or Sequence F in Table 2.

The first round of each supergame was always played.[7] Once participants submitted their expenditures, the outcome of the stage contest was randomly or non-randomly determined in accordance with CSF (1) from Section 2.1. In the *WTA* sessions, one participant received the entire 120 ECU prize; in the *PP* sessions, the prize was split according to the expenditure shares. At the end of a round, participants were shown their own expenditure, their rival's expenditure, and their payoff for the round.

After each round in the *Indefinite* treatments, a random integer $T \in [1, \frac{1}{1-\delta}]$ was

[5]Many indefinite supergame experiments use this procedure (Dal Bó, 2005; Dal Bó and Fréchette, 2011; McBride and Skaperdas, 2014). As noted in Dal Bó (2005), zipper matching precludes direct contagion effects.

[6]See Fréchette and Yuksel (2017) for additional ways to implement discounting in indefinitely repeated games and comparisons between them.

[7]We did not use the words 'supergame' or 'round' in the experimental instructions. Instead, we refereed to a supergame as a 'period' and rounds within the supergame as a 'decision.'

drawn and shown to participants. If $T = 1$ was shown, the current supergame ended; if any other number was shown, another round was played.[8] During each round, participants were reminded that there was a $(1 - \delta) \times 100\%$ chance that they were playing the last round of the current supergame.

The total payoff in a supergame was calculated as the sum of payoffs from all of the rounds in that supergame. At the end of the experiment, participants were paid their earnings for one of the supergames, selected at random (Azrieli, Chambers and Healy, Forthcoming). The exchange rate was 25 ECU to 1 US Dollar. Participants earned $23.22 on average, including a $7.00 show-up fee.

4 Hypotheses

We examine the following hypotheses:

Hypothesis 1. *In indefinite supergames, expenditure is lower (more cooperative) with* $\delta = 0.8$ *than with* $\delta = 0.5$.

Hypothesis 2. *(a) Expenditure is lower (more cooperative) in indefinite supergames than in finite supergames of the same expected length with* $\delta = 0.8$.
(b) Expenditure is at least as low (at least as cooperative) in indefinite supergames than in finite supergames of the same expected length with $\delta = 0.5$.

Hypothesis 3. *Expenditure is identical across* winner-take-all *and* proportional-prize *contest settings.*

Hypothesis 1 is a standard result from the theory of repeated games and is a direct consequence of the analysis presented in Section 2. Holding the contest setting constant, expenditure should be lower in our *High δ-Indefinite* treatments relative to our *Low δ-Indefinite* treatments. This should be true under either contest setting.

Hypothesis 2(a) results from the fact that the Nash reversion, grim trigger strategy from Section 2.2 can support socially optimal cooperation in our *High δ-Indefinite* treatments but not in our *High δ-Finite* treatments. In theory, it does so under either contest setting.

Hypothesis 2(b) follows from the fact that a Nash reversion strategy cannot support socially optimal cooperation in *any* of our *Low δ* treatments (whether *Indefinite*, *Finite*,

[8] The draws of T were consistent with the pre-drawn sequences shown in Table 2.

WTA, or *PP*). However, other cooperative outcomes where gains from deviation are not as large can be supported. For example, consider the following strategy: Choose expenditure $\bar{x} < x^*$ as long as the other player chooses expenditure $\bar{x}$ or lower; otherwise, choose x^* forever. As $\bar{x}$ increases from zero to $\frac{V}{4}$, the threshold value of δ necessary to support cooperation decreases monotonically to zero.[9] We thus hypothesize that at least some cooperation (not necessarily the socially optimal level) will be observed in the Low δ *Indefinite* treatments.

Hypothesis 3 is a direct consequence of our risk-neutral equilibrium characterization. There is mixed empirical evidence related to this hypothesis. Fallucchi, Renner and Sefton (2013) report similar expenditure in *WTA* and *PP* settings when participants receive complete post-round feedback. However, when feedback is limited to players' own information, expenditure is greater in *WTA* contests. Cason, Masters and Sheremeta (2010) find similar expenditures across *WTA* and *PP* settings irrespective of whether entry into the contest is exogenous or endogenous. Shupp et al. (2013) report greater expenditure in *PP* contests than in *WTA* contests. Finally, assuming individual output is a noisy function of individual investments, Cason, Masters and Sheremeta (Forthcoming) report greater expenditure in *WTA* contests relative to *PP* contests. Note that none of these studies examine indefinitely repeated contests.

While our experimental instructions are as neutral as possible, the perceived "competitiveness" of the contest may be greater in the *WTA* setting than in the *PP* setting. This is plausible *ex ante* because one player receives the entire contest prize in the *WTA* setting, whereas players can "split" the prize in the *PP* setting. Thus an alternative, "behavioral" version of Hypothesis 3 predicts less cooperative expenditure (i.e. higher expenditure) in the *WTA* setting than in the *PP* setting.

This behavioral hypothesis can be formalized with a joy of winning model where players receive a relatively large non-monetary utility from winning a *WTA* contest (Goeree, Holt and Palfrey, 2002; Sheremeta, 2013; Boosey, Brookins and Ryvkin, 2017). In a *PP* contest, the concept of winning is not as sharply defined as in a *WTA* contest. It is plausible that some participants think they "win" a contest when their share of the prize exceeds one half, but the utility of winning is less salient in the *PP* setting.

[9] Formally, the stage game best response to expenditure $\bar{x} < \frac{V}{4}$ is $\hat{x} = \sqrt{V\bar{x}} - \bar{x}$ (ignoring the integer problem) and the payoff from optimal deviation is $\hat{\pi} = (\sqrt{V} - \sqrt{\bar{x}})^2$, whereas the payoff from the cooperative strategy profile $(\bar{x}, \bar{x})$ is $\frac{V}{2} - \bar{x}$. A derivation similar to the one in Section 2.2 produces a threshold value of the discount factor $\hat{\delta} = \frac{(\sqrt{V}-\sqrt{\bar{x}})^2 - \frac{V}{2} + \bar{x}}{(\sqrt{V}-\sqrt{\bar{x}})^2 - \frac{V}{4}}$. It can be shown that $\hat{\delta}$ decreases monotonically in $\bar{x}$ for $\bar{x} \in [0, \frac{V}{4}]$.

5 Results

Following Dal Bó (2005), we mostly focus on expenditure levels in the first round of each supergame. This is the only round where the expected number of rounds is the same across *Indefinite* and *Finite* treatments. To see this, compare our *WTA-High* δ*-Indefinite* treatment to our *WTA-High* δ*-Finite* treatment. In Round 1, the expected number of rounds is 5 in both treatments. In Round 2, it is still 5 in *WTA-High* δ*-Indefinite*, but it is only 4 in *WTA-High* δ*-Finite*. By examining Round 1 behavior, we can assess whether indefiniteness matters. We pool across our two *Indefinite* sessions for each discount factor and contest setting because rank sum tests suggest that there are no Round 1 session effects ($p > 0.345$ for all tests).

5.1 Does continuation probability affect expenditure?

Figure 1 shows average expenditure across all rounds (top panel) and average Round 1 expenditure across all 10 supergames (bottom panel), both by treatment. The time series for the *WTA* treatments are on the left of the figure, and the time series for the *PP* treatments are on the right. The round (stage game) Nash equilibrium expenditure of $x^* = 30$ is included in the figures as a dashed line for reference.

Figure 1a does not suggest a difference in average expenditure across *WTA-Low* δ*-Indefinite* and *WTA-High* δ*-Indefinite*. However, Figure 1c hints at a possible difference in Round 1 expenditure across these two treatments. Both Figures 1b and 1d also indicate a potential difference in expenditure across *PP-Low* δ*-Indefinite* and *PP-High* δ*-Indefinite*.

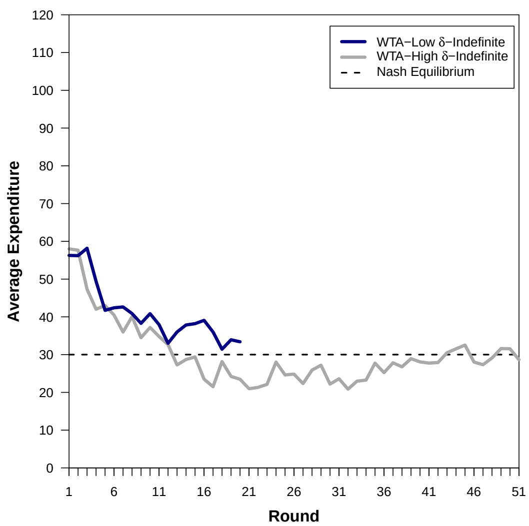
To determine if there are any statistically significant differences in expenditure across the *Low* δ*-Indefinite* and *High* δ*-Indefinite* treatments, we estimate several ordinary least squares (OLS) regressions. Our primary specification is:

$$Expnd_{i,t} = \beta_0 + \beta_1 HighDelta_i + \beta_2 Round_t + \beta_3 (HighDelta_i \times Round_t) + \epsilon_{i,t} \quad (5)$$

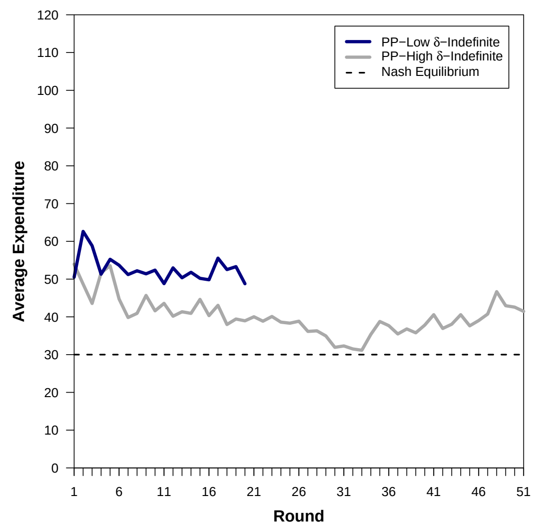
The sample only includes expenditures from the first round of supergames. The dependent variable is Participant i's expenditure in the actual experimental round t. We control for the overall effect of experience with variable $Round_t$, which is the actual round in which the expenditure occurs (1-20 in *Low* δ treatments, 1-51 in *High* δ treatments). $HighDelta_i$ is an indicator variable equal to 1 if the participant is in a *High* δ treatment, and 0 otherwise.

Table 3 shows regression results. Standard errors, clustered at the participant level, are shown in parenthesis. Our specification yields "slope" and "intercept" estimates for

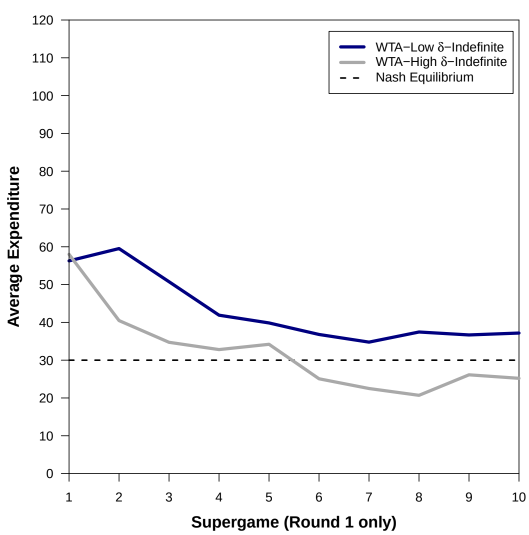
Figure 1: Expenditure on Time, δ Comparisons



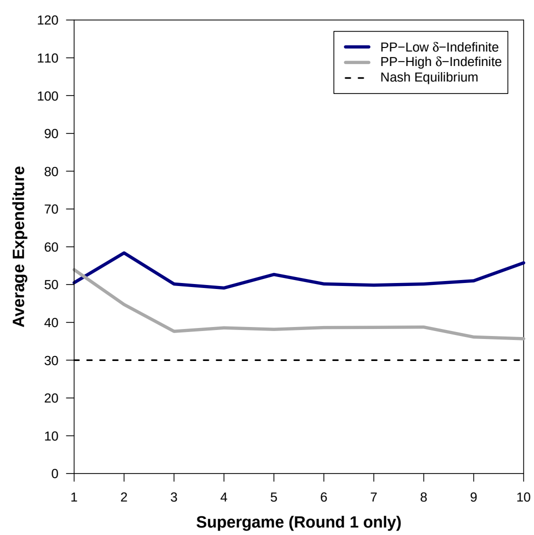
(a) *WTA*-δ Comparison



(b) *PP*-δ Comparison



(c) *WTA*-δ Comparison



(d) *PP*-δ Comparison

Table 3: Regression Results, δ Comparisons

	WTA-Indefinite		PP-Indefinite	
Expnd	(1)	(2)	(3)	(4)
Constant	43.12***	54.70***	51.77***	53.34***
	(4.08)	(5.34)	(3.07)	(4.10)
HighDelta	−11.14**	−12.59*	−11.69**	−6.54
	(5.42)	(7.05)	(4.53)	(6.02)
Round		−1.18***		−0.16
		(0.33)		(0.29)
HighDelta×*Round*		0.71*		−0.15
		(0.36)		(0.32)
R^2	0.03	0.07	0.04	0.06
Observations	800	800	800	800

*Note: Standard errors clustered at the participant level. Estimating sample is Round 1 expenditure of each supergame for each participant. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

each treatment. The *Low δ-Indefinite* intercept estimate is *Constant*, and the *High δ-Indefinite* intercept estimate is the sum of *Constant* and the coefficient on *HighDelta*. The partial effect of round (the slope estimate) is the coefficient on *Round* for *Low δ-Indefinite* and the sum of coefficients on *Round* and *HighDelta*×*Round* for *High δ-Indefinite*. For *WTA-High δ-Indefinite* this estimate is -0.48, which is significantly different from zero ($p = 0.003$). The estimate for *WTA-Low δ-Indefinite* is -0.32, which is also statistically significant ($p = 0.014$).

The estimates in Table 3 can be summarized as follows. When experience is controlled for—see column (2)—expenditure is initially lower in *WTA-High δ-Indefinite* relative to *WTA-Low δ-Indefinite*. However, this gap narrows with time, because expenditure in *WTA-High δ-Indefinite* falls by 0.32 each round, but it falls by 1.18 in *WTA-Low δ-Indefinite* with each passing round. For the *PP* setting—see column (4)—there is no initial difference in expenditure across *PP-High δ-Indefinite* and *PP-Low δ-Indefinite*. In *PP-High δ-Indefinite*, expenditure falls by 0.32 each round, and there is no statistically significant time trend in *PP-Low δ-Indefinite*. These conclusions correspond to the visuals in Figures 1c and 1d.

We can use the estimates in Table 3 to predict expenditure in Round t. Specifically:

$$\begin{aligned}(Low\delta)\ \widehat{Expnd}_t &= \hat{\beta}_0 + \hat{\beta}_2 Round_t \\ (High\delta)\ \widehat{Expnd}_t &= \hat{\beta}_0 + \hat{\beta}_1 + (\hat{\beta}_2 + \hat{\beta}_3) Round_t \\ (High\delta - Low\delta)\ \widehat{Expnd}_t &= \hat{\beta}_1 + \hat{\beta}_3 Round_t\end{aligned} \tag{6}$$

Table 4: Estimated Expenditure Differences, by Round

	Highδ − Lowδ		*Indefinite − Finite*		*PP − WTA*		*PP − WTA*	
Round	**WTA–Indefinite** (1)	**PP–Indefinite** (2)	**WTA–High δ** (3)	**PP–High δ** (4)	**High δ–Finite** (5)	**Low δ–Finite** (6)	**High δ–Indefinite** (7)	**Low δ–Indefinite** (8)
1	−11.89*	−6.70	−5.92	−1.02	−0.06	13.35	4.85	−0.34
2	−11.18*	−6.85	−5.68	−0.72	0.05	12.90	5.01	0.69
3	−10.47	−7.01	−5.44	−0.42	0.16	12.45	5.17	1.71
4	−9.77	−7.16	−5.19	−0.12	0.26	11.99*	5.33	2.73
5	−9.06	−7.32	−4.95	0.18	0.37	11.54*	5.49	3.75
6	−8.35	−7.47	−4.70	0.47	0.48	11.09*	5.65	4.77
7	−7.65	−7.63	−4.46	0.77	0.58	10.64*	5.81	5.79
8	−6.94	−7.78	−4.22	1.07	0.69	10.18*	5.97	6.82
9	−6.24	−7.94	−3.97	1.37	0.79	9.73*	6.13	7.84
10	−5.53	−8.09*	−3.73	1.66	0.90	9.28*	6.29	8.86*
11	−4.82	−8.25*	−3.49	1.96	1.01	8.83	6.45	9.88*
12	−4.12	−8.40*	−3.24	2.26	1.11	8.38	6.61	10.90**
13	−3.41	−8.56*	−3.00	2.56	1.22	7.92	6.78	11.92**
14	−2.70	−8.71*	−2.76	2.85	1.33	7.47	6.94	12.95**
15	−2.00	−8.87*	−2.51	3.15	1.43	7.02	7.10	13.97**
16	−1.29	−9.02*	−2.27	3.45	1.54	6.57	7.26	14.99**
17	−0.58	−9.18*	−2.02	3.75	1.64	6.11	7.42	16.01***
18	0.12	−9.33*	−1.78	4.04	1.75	5.66	7.58	17.03***
19	0.83	−9.49*	−1.54	4.34	1.86	5.21	7.74	18.05***
20	1.54	−9.64*	−1.29	4.64	1.96	4.76	7.90	19.08***

Note: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The estimated difference in expenditure across *High δ-Indefinite* and *Low δ-Indefinite* treatments is shown in Table 4 for the *WTA* sessions—see column (1)—and the *PP* sessions—see column (2). Estimates are only reported for Rounds 1-20, because above Round 20, estimates are "out of sample" for the *Low δ-Indefinite* treatments. According to our estimated model, average expenditure only differs significantly across *High δ* and *Low δ* in the first two rounds of *WTA-Indefinite*. In *PP-Indefinite*, average expenditure is significantly different ($p < 0.1$) in Rounds 10-20.

In light of Figure 1 and Table 3, it is clear that contest experience matters. There are declining time trends in average expenditure in Figure 1 until approximately Supergame 5. While the estimates in Table 3 suggest a treatment effect across *WTA-High δ-Indefinite* and *WTA-Low δ-Indefinite*, this difference is not statistically significant over time (i.e., when the effect of the intercept *and* slope estimates are both accounted for as in Table 4). On the other hand, Tables 3 and 4 both suggest a difference in expenditure across *PP-High δ-Indefinite* and *PP-Low δ-Indefinite* over time.

Result 1. *Experience affects average expenditure in indefinitely repeated contests with both $\delta = 0.8$ and $\delta = 0.5$, and under both* WTA *and* PP *allocation rules.*

Table 5: Regression Results, *Indefinite* Comparisons

	WTA-High δ		**PP-High δ**	
Expnd	(1)	(2)	(3)	(4)
Constant	31.38***	48.27***	33.71***	48.11***
	(4.38)	(7.25)	(3.74)	(5.28)
Indefinite	0.61	−6.17	6.37	−1.31
	(5.65)	(8.59)	(5.02)	(6.88)
Round		−0.72***		−0.61***
		(0.20)		(0.15)
Indefinite×*Round*		0.24		0.30
		(0.26)		(0.19)
R^2	0.00	0.07	0.01	0.07
Observations	600	600	600	600

*Note: Standard errors clustered at the participant level. Estimating sample is Round 1 expenditure of each supergame for each participant. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Result 2. *There is no significant difference in average expenditure across δ in indefinitely repeated* WTA *contests. However, after several rounds of contest experience, average expenditure is lower in indefinitely repeated* PP *contests with $\delta = 0.8$ relative to indefinitely repeated* PP *contests with $\delta = 0.5$.*

In summary, we find mixed support for Hypothesis 1. We now consider evidence related to Hypothesis 2.

5.2 Does indefiniteness affect expenditure?

Figure 2 compares expenditure across *High δ-Indefinite* and *High δ-Finite* treatments for both contest settings. The figure shows average expenditure across all rounds (top panel) and average Round 1 expenditure across all 10 supergames (bottom panel), by treatment. As before, the time series for the *WTA* treatments are on the left of the figure, and the time series for the *PP* treatments are on the right.

There is no evidence in either Figure 2a or Figure 2b suggesting a difference in average expenditure across *WTA-High δ-Finite* and *WTA-High δ-Indefinite*. On the other hand, Figures 2c and 2d appear to show that with contest experience average expenditure is actually *higher* in *PP-High δ-Indefinite* relative to *PP-High δ-Finite*. This is surprising because Hypothesis 2 implies lower expenditure in indefinitely repeated games relative to finitely repeated games of the same expected length.

As before, we examine expenditure across treatments formally with regression analysis. Table 5 contains estimates from specifications that are analogous to Specification (5) with $HighDelta_i$ replaced by $Indefinite_i$, an indicator variable equal to 1 if the participant was in an *Indefinite* treatment, and 0 otherwise.

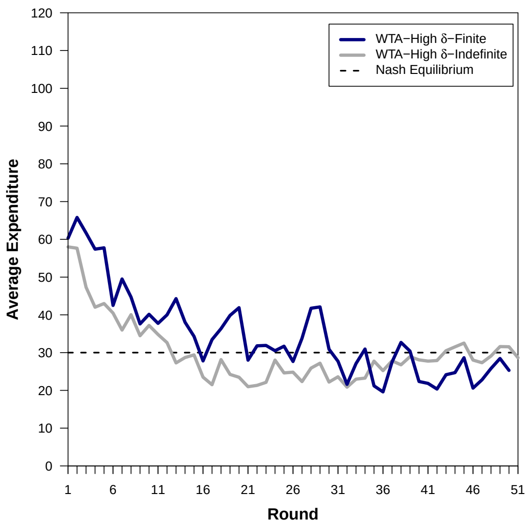
The "intercept" coefficient estimates in Table 5 do not indicate any differences in initial average expenditure across either *WTA-High δ-Indefinite* and *WTA-High δ-Finite* or *PP-High δ-Indefinite* and *PP-High δ-Finite*. The partial effect of time on expenditure is significant and negative for *Low δ* treatments in both contest settings. For the *WTA* treatments, the partial effect of time on expenditure is -0.48 ($p = 0.004$), and it is -0.32 ($p = 0.015$) for the *PP* treatments.

As we did for our δ comparisons, we use the model estimates to estimate expenditures for Rounds 1-20. These are shown in columns (3) and (4) of Table 4. None of the differences across *Indefinite* and *Finite* are statistically significant.

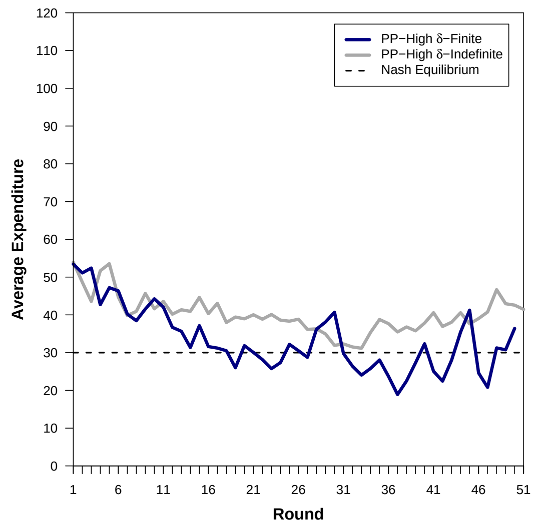
Result 3. *There is no significant difference in average expenditure across indefinitely repeated contests and finitely repeated contests of the same expected length, for either the* WTA *allocation rule or the* PP *allocation rule.*

We focus on the *High δ* treatments in this subsection, but the results for the *Low δ* treatments are qualitatively the same. For *Low δ*, regression analysis suggests no difference in intercept estimates across *WTA-Low δ-Indefinite* and *WTA-Low δ-Finite*. For *WTA-Low δ-Finite*, the coefficient estimate for *Round* is -0.79 ($p = 0.047$) and the corresponding estimate is -1.18 ($p = 0.001$) for the *WTA-Low δ-Indefinite* session.

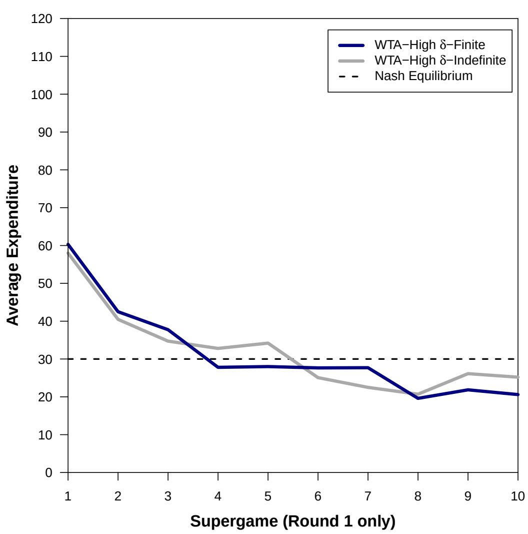
Figure 2: Expenditure on Time, *Indefinite* Comparisons



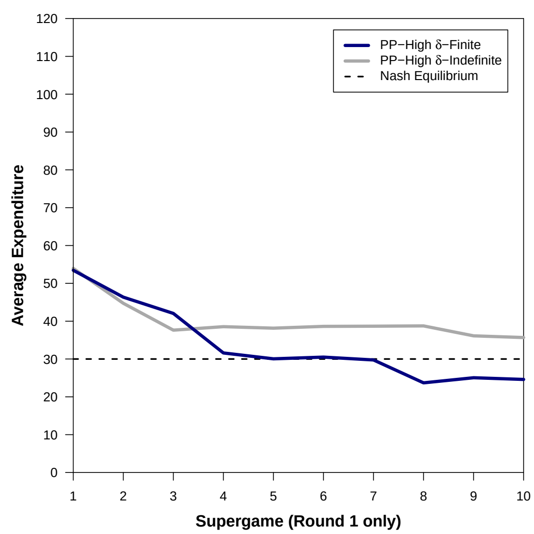
(a) *WTA-Indefinite* Comparison



(b) *PP-Indefinite* Comparison



(c) *WTA-Indefinite* Comparison



(d) *PP-Indefinite* Comparison

There is also no difference in intercept estimates across *PP-Low δ-Indefinite* and *PP-Low δ-Finite*. For the *PP-Low δ-Finite* session, the slope estimate is -1.24 ($p = 0.012$) and for *PP-Low δ-Indefinite* the slope estimate is -0.16, but the latter is not statistically different from zero ($p = 0.586$). Thus, over time average expenditure in the *PP-Low δ-Indefinite* treatment may exceed that in the *PP-Low δ-Finite* treatment.

The conclusions in the preceding paragraphs are supported by expenditure estimates for Rounds 1-20. There are no significant differences is estimated expenditure across *WTA-Low δ-Indefinite* and *WTA-Low δ-Finite*. In *PP-Low δ-Indefinite*, average expenditure is estimated to be statistically significantly larger than average expenditure in *PP-Low δ-Finite* for Rounds 16-20 ($p < 0.10$ or lower in these rounds).

We now examine differences across the *WTA* and *PP* settings directly and thereby assess Hypothesis 3.

5.3 Does contest setting affect expenditure?

Figure 3 contains cumulative distribution function (CDF) comparisons of Round 1 expenditure across *WTA* and *PP* settings. Figures 3a and 3b do not suggest large differences across contest setting in our *Finite* treatments. On the other hand, Figures 3c and 3d hint at possible differences across contest setting in our *Indefinite* treatments.

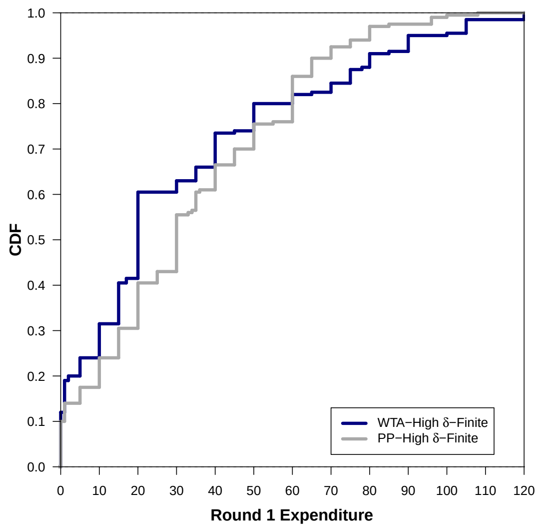
Interestingly, all four of the *WTA* and *PP* CDFs exhibit a jump at the expenditure level of $x = 20$. In all of the comparisons, and particularly in the *Indefinite* treatments, this expenditure level appears focal for *WTA* participants but not for *PP* participants. All of the CDFs, especially those for the *Indefinite* treatments, suggest higher average expenditure in the *PP* setting relative to the *WTA* setting.

We do not conduct statistical tests on these cumulative distributions because the data include repeat observations from each participant and thus are not independent within each CDF. Rather, we now report separate regression analysis for our *Finite* treatments and for our *Indefinite* treatments.

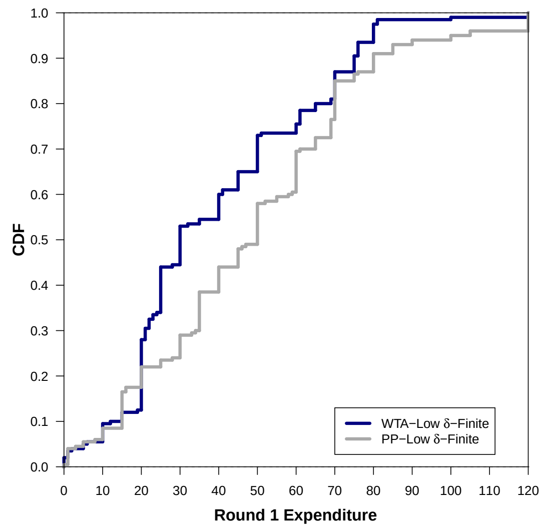
5.3.1 Finitely Repeated Contests

Figure 4 shows comparisons across *WTA* and *PP* settings for finitely repeated contests. No differences in average expenditure are apparent in the 5 round *High δ* supergames. But Figures 4c and 4d suggest that average expenditure may be higher in the *PP* setting compared to the *WTA* setting in the 2 round *Low δ* supergames.

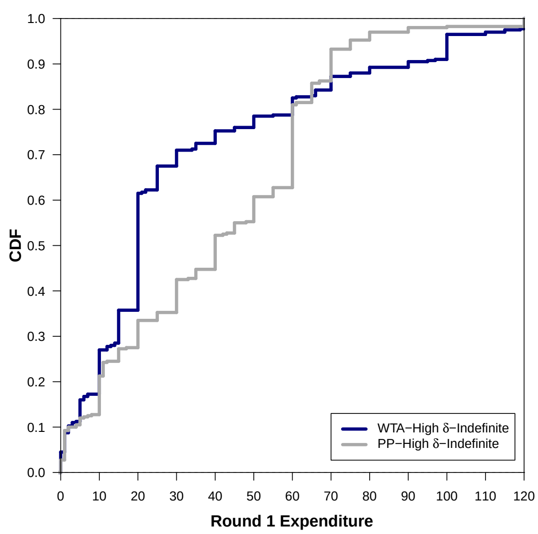
Figure 3: Empirical Cumulative Distribution Functions



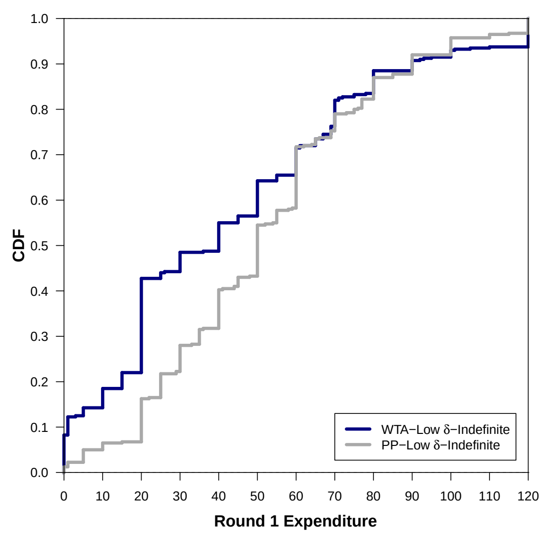
(a) *High δ–Finite* Comparison



(b) *Low δ–Finite* Comparison



(c) *High δ–Indefinite* Comparison



(d) *Low δ-Indefinite* Comparison

Table 6: Regression Results, *Finite* Contest Setting Comparisons

	High δ-Finite		Low δ-Finite	
Expnd	(1)	(2)	(3)	(4)
Constant	31.37***	48.27***	39.25***	47.13***
	(4.40)	(7.29)	(3.74)	(5.24)
PP	2.34	−0.16	9.28*	13.80
	(5.79)	(9.02)	(5.38)	(8.79)
Round		−0.72***		−0.79*
		(0.20)		(0.39)
PP×*Round*		0.11		−0.45
		(0.25)		(0.62)
R^2	0.00	0.12	0.03	0.08
Observations	400	400	400	400

Note: Standard errors clustered at the participant level. Estimating sample is Round 1 expenditure of each supergame for each participant. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

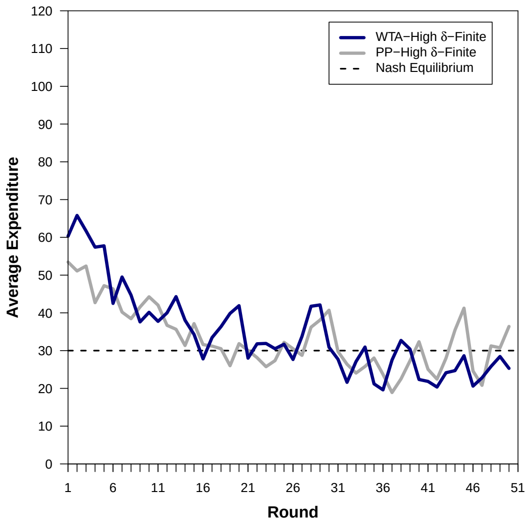
However, the eyeball analysis of Figure 4 is not confirmed by the regressions in Table 6. Our analysis in this section mirrors our earlier regression analysis. The variable *PP* is an indicator equal to 1 if the participant's expenditure was made in a proportional-prize setting, and 0 otherwise. The coefficient estimates on *PP* are not significantly different from zero in either of the two specifications that control for experience—models (2) and (4).

The estimates on *Round* (which apply to the *WTA* setting) are significant and negative for both values of δ. By adding the coefficient estimates on *Round* to those on the interaction term, we can calculate the slope estimates for the *PP* setting. For *PP-High δ-Finite*, the slope estimate is -0.61 ($p = 0.000$). The corresponding estimate for *PP-Low δ-Finite* is -1.24 ($p = 0.014$).

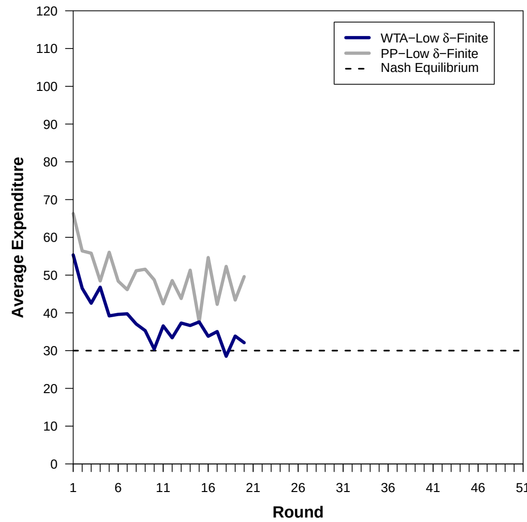
These estimates suggest that average expenditure in *High δ* stays close over time across the two contest settings. This is confirmed by Figure 4c. However, for *Low δ*, average expenditure drops less quickly over time in the *PP* setting relative to the *WTA* setting.

We also calculate estimated expenditures for the *High δ-Finite* and *Low δ-Finite* treatments. The differences between estimated expenditure in *PP* contests and *WTA* contests are presented in Table 4 for Rounds 1-20. Positive differences indicate greater expenditure in *PP* contests relative to *WTA* contests. There are no significant differences in the *High δ* data. In the *Low δ* data, while a number of early rounds contain significantly

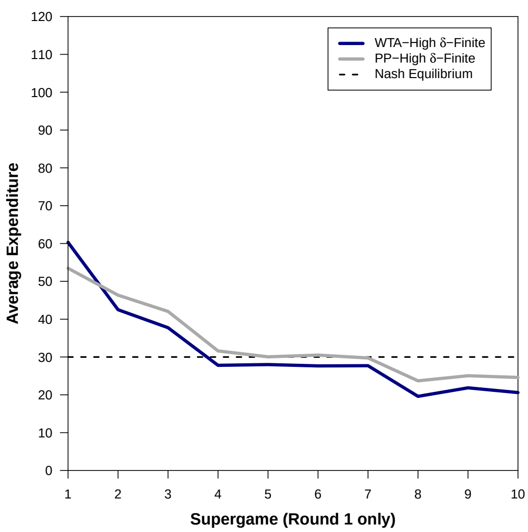
Figure 4: Expenditure on Time, *Finite* Contest Setting Comparisons



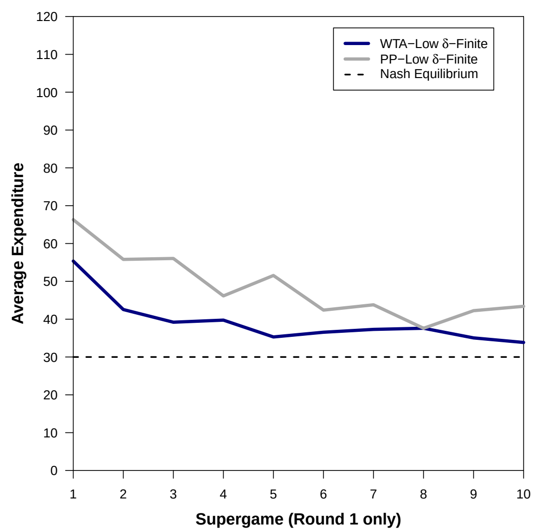
(a) *High δ–Finite* Comparison



(b) *Low δ–Finite* Comparison



(c) *High δ–Finite* Comparison



(d) *Low δ-Finite* Comparison

Table 7: Regression Results, *Indefinite* Contest Setting Comparisons

	High δ-Indefinite		**Low δ-Indefinite**	
Expnd	(1)	(2)	(3)	(4)
Constant	31.98***	42.11***	43.12***	54.70***
	(3.56)	(4.59)	(4.08)	(5.34)
PP	8.11	4.69	8.65*	−1.36
	(4.88)	(6.36)	(5.10)	(6.73)
Round		−0.48***		−1.18***
		(0.16)		(0.33)
PP×*Round*		0.16		1.02**
		(0.20)		(0.44)
R^2	0.02	0.06	0.02	0.04
Observations	800	800	800	800

*Note: Standard errors clustered at the participant level. Estimating sample is Round 1 expenditure of each supergame for each participant. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

larger expenditure predictions for *PP* contests relative to *WTA* contests, there are no long-run differences.

Result 4. *There is no evidence that long-run expenditure is different in* proportional-prize *and* winner-take-all *contests for finitely repeated contests.*

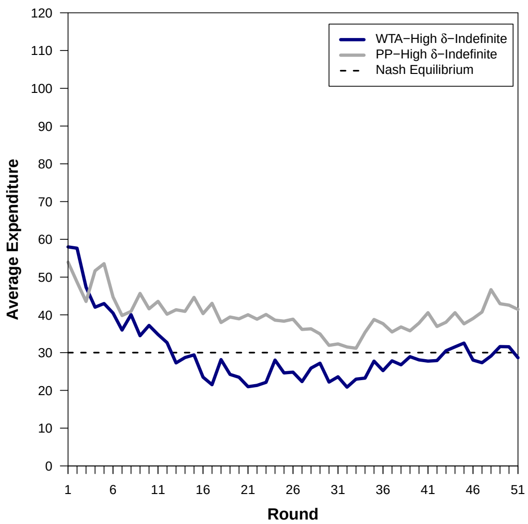
We now report our final comparison on expenditure differences across contest settings in indefinitely repeated contests.

5.3.2 Indefinitely Repeated Contests

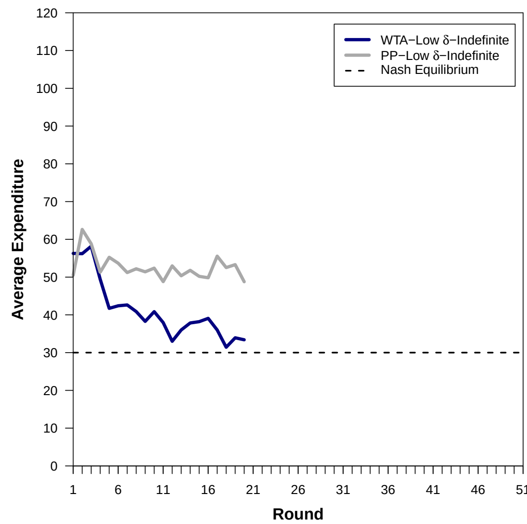
Figure 5 shows average expenditure across all rounds (top panel) and average Round 1 expenditure across all 10 supergames (bottom panel), both by treatment. The time series for the *High* δ treatments are on the left of the figure, and the time series for the *Low* δ treatments are on the right. In both cases, there appears to be a difference in average expenditure across contest settings. Namely, average expenditure appears greater in the proportional-prize indefinite contests.

Table 7 shows regression results for different contest settings in indefinitely repeated contests. The variable *PP* is as described above. Notice that in specifications (2) and (4), there are no significant intercept estimates. However, both slope estimates are significant and negatively signed.

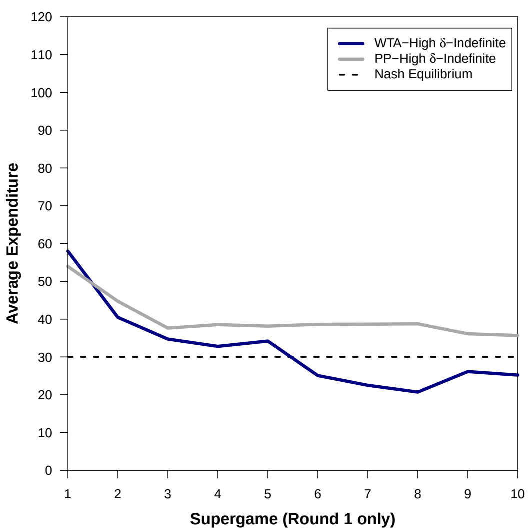
Figure 5: Expenditure on Time, *Indefinite* Contest Setting Comparisons



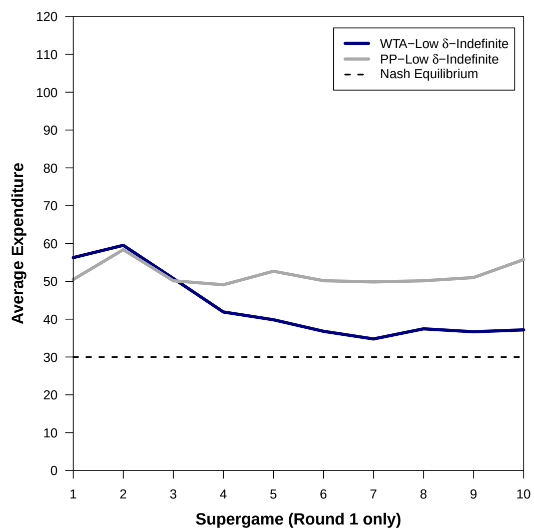
(a) *High δ–Indefinite* Comparison



(b) *Low δ–Indefinite* Comparison



(c) *High δ–Indefinite* Comparison



(d) *Low δ-Indefinite* Comparison

For the *High* δ data, the partial effect of time on expenditure in *WTA* contests (i.e. *Round*) is -0.48 and significant. The comparable partial effect for *PP* contests is -0.32 ($p = 0.014$). Thus, average expenditure declines slightly less with each round of experience in the *PP* setting compared to the *WTA* setting. This is in line with Figure 5c. The partial effect of time on expenditure for *WTA-Low* δ is -1.18. For *PP-Low* δ, the estimate is -0.16 ($p = 0.584$). In other words, there is no time trend in average Round 1 expenditure in the *PP* setting (see Figure 5d).

In Table 4, column (8) shows that the difference between estimated *PP* expenditure and estimated *WTA* expenditure is positive and significant for Rounds 10-20 for the *Low* δ data.

Result 5. *Long-run average expenditure in proportional-prize indefinitely repeated contests is higher than in winner-take-all indefinitely repeated contests for* $\delta = 0.5$.

In the next section, we examine this result and Result 2 in more detail.

5.4 Do risk preferences and gender affect expenditure?

Our two statistically significant treatment differences are: Experience interacts with the *PP* setting to produce lower average expenditure in $\delta = 0.8$ indefinite contests relative to $\delta = 0.5$ indefinite contests (Result 2), and experience interacts with the contest setting to produce higher average expenditure in *PP-Low* δ indefinite contests relative to *WTA-Low* δ indefinite contests (Result 5). In this section, we re-examine our data to see if these results hold up when controlling for risk preferences and gender, which have both previously been found to significantly affect participant investment behavior.[10]

Our first treatment comparison illustrates how we extend specification (5) to control

[10]For *WTA* contests, more risk-averse participants have been found to make significantly lower investments (Millner and Pratt, 1991; Anderson and Freeborn, 2010; Sheremeta and Zhang, 2010; Sheremeta, 2011; Mago, Sheremeta and Yates, 2013; Shupp et al., 2013; Cason, Masters and Sheremeta, Forthcoming), and women have been found to make significantly higher investments (Mago, Sheremeta and Yates, 2013; Brookins and Ryvkin, 2014; Price and Sheremeta, 2015), although these findings are not universal. For *PP* contests, Cason, Masters and Sheremeta (Forthcoming) report lower investments by risk-averse participants, but there has yet to be a reported association between gender on contest investment in this setting.

for risk and gender:

$$\begin{aligned} Expnd_{i,t} = \beta_0 + &\beta_1 HighDelta_i + \beta_2 Round_t + \beta_3 (HighDelta_i \times Round_t) + \\ &\beta_4 Risk_i + \beta_5 (HighDelta_i \times Risk_i) + \\ &\beta_6 Female_i + \beta_7 (HighDelta_i \times Female_i) + \\ &\beta_8 (Risk_i \times Female_i) + \beta_9 (HighDelta_i \times Risk_i \times Female_i) + \epsilon_{i,t} \end{aligned} \tag{7}$$

where our estimating sample only contains Round 1 data, and where $HighDelta_i$ is replaced with $Indefinite_i$ or PP_i for our other treatment comparisons.

$Risk_i \in \{1, 10\}$ is a cardinal measure of risk aversion equal to the switch point in the Holt and Laury (2002) risk elicitation mechanism. This variable was elicited from our participants prior to data collection for this paper. A value of 5 is consistent with risk-neutrality, while values below 5 are consistent with risk-attraction, and values above 5 are consistent with risk-aversion. We are missing $Risk_i$ for 55 of our 240 participants because we only use data for participants who "switched" one time (or zero times). Our other new regressor is $Female_i$, an indicator variable equal to 1 if the participant reported being female (and 0 otherwise). We are missing $Female_i$ for one participant (from *PP-High δ-Finite*).

Table 8 shows re-estimates of the expenditure differences in Table 4 that account for risk and gender. We illustrate how these new expenditure estimates are calculated using our first treatment comparison:

$$\begin{aligned} (High\delta - Low\delta)\ \widehat{Expnd}_t = &\hat{\beta}_1 + \hat{\beta}_3 Round_t + \\ &(\hat{\beta}_4 + \hat{\beta}_5)\overline{Risk_{High\delta}} - \hat{\beta}_4 \overline{Risk_{Low\delta}} + \hat{\beta}_7 + \\ &(\hat{\beta}_8 + \hat{\beta}_9)\overline{Risk_{High\delta,Female}} - \hat{\beta}_8 \overline{Risk_{Low\delta,Female}} \end{aligned} \tag{8}$$

where $\overline{Risk_{High\delta}}$ is the mean of $Risk$ across all participants in *High* δ, and $\overline{Risk_{High\delta,Female}}$ is the mean of $Risk$ over all female participants in *High* δ. We discuss the results in Table 8 in the following subsections.

5.4.1 *PP-High δ-Indefinite* and *PP-Low δ-Indefinite* comparison

Relative to Table 4, there is no longer a statistically significant difference between *PP-High δ-Indefinite* and *PP-Low δ-Indefinite* in Table 8. How does controlling for risk and gender translate into a null result? Table 9 presents the mean risk and mean expenditure values by treatment and by gender. The raw risk averages in the table do not suggest a

Table 8: Estimated Expenditure Differences with Risk and Gender Controls, by Round

	Highδ − *Low*δ		*Indefinite* − *Finite*		*PP* − *WTA*		*PP* − *WTA*	
Round	**WTA–Indefinite** (1)	**PP–Indefinite** (2)	**WTA–High δ** (3)	**PP–High δ** (4)	**High δ–Finite** (5)	**Low δ–Finite** (6)	**High δ–Indefinite** (7)	**Low δ–Indefinite** (8)
1	−13.04*	−7.71	1.00	7.61	6.19	24.09***	12.80	7.48
2	−12.14*	−7.62	1.12	7.93	6.20	23.55***	13.01	8.48
3	−11.25	−7.52	1.24	8.25	6.22	23.01***	13.22	9.49
4	−10.36	−7.42	1.36	8.56	6.23	22.48***	13.44*	10.50
5	−9.46	−7.32	1.48	8.88	6.25	21.94***	13.65*	11.51*
6	−8.57	−7.22	1.60	9.20	6.27	21.41***	13.86*	12.51**
7	−7.67	−7.12	1.72	9.51	6.28	20.87***	14.07*	13.52**
8	−6.78	−7.02	1.84	9.83	6.30	20.33***	14.28*	14.53**
9	−5.88	−6.92	1.96	10.15	6.31	19.80***	14.50*	15.53***
10	−4.99	−6.82	2.08	10.46	6.33	19.26***	14.71*	16.54***
11	−4.10	−6.72	2.20	10.78	6.34	18.72**	14.92**	17.55***
12	−3.20	−6.63	2.32	11.10	6.36	18.19**	15.13**	18.56***
13	−2.31	−6.53	2.44	11.41*	6.38	17.65**	15.34**	19.56***
14	−1.41	−6.43	2.56	11.73*	6.39	17.12**	15.56**	20.57***
15	−0.52	−6.33	2.68	12.05*	6.41	16.58**	15.77**	21.58***
16	0.38	−6.23	2.81	12.36*	6.42	16.04*	15.98**	22.58***
17	1.27	−6.13	2.93	12.68**	6.44	15.51*	16.19**	23.59***
18	2.16	−6.03	3.05	13.00**	6.45	14.97	16.40**	24.60***
19	3.06	−5.93	3.17	13.31**	6.47	14.43	16.62**	25.61***
20	3.95	−5.83	3.29	13.63**	6.49	13.90	16.83**	26.61***

*Note: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

difference across genders, and there was no significant, *ex ante* difference in *Risk* across men and women in *High* δ ($p = 0.264$) or in *Low* δ ($p = 0.911$).[11] The table does indicate that female participants had higher average expenditure than their male counterparts in both *PP-High* δ*-Indefinite* and *PP-Low* δ*-Indefinite*.[12]

Table 9 also gives partial effect estimates of *Risk* on *Expnd* and of *Female* on *Expnd*. The former partial effect can be interpreted as the change in expenditure (in ECUs) of a participant switching at one lottery further down the list in the Holt and Laury (2002) risk elicitation mechanism (of being more risk averse). Risk aversion has a significant and negative effect on expenditure in three of the four treatment and gender combinations. However, the partial effect of *Risk* is not significantly different across females and males in *High* δ ($p = 0.374$) or in *Low* δ ($p = 0.234$).

The two partial effects of *Female* in Table 9 indicate that female participants had

[11]A rank sum test ($n_m = 13, n_f = 17$) and a robust rank test ($n_m = 12, n_f = 18$) were used, respectively. The robust rank test was used because the variance in *Risk* was not equal across *PP-High* δ*-Indefinite* and *PP-Low* δ*-Indefinite*.

[12]To test this we regressed expenditure on gender and treatment dummies with standard errors clustered at the participant level and then tested the appropriate combination of coefficient estimates. Female participants had higher average expenditure in *High* δ ($p = 0.029$) and in *Low* δ ($p = 0.011$).

Table 9: Partial Effects, *PP-High δ-Indefinite* and *PP-Low δ-Indefinite*

	Male		Female		Female − Male		
Partial Effect	*High δ*	*Low δ*	*High δ*	*Low δ*	*High δ*	*Low δ*	*High δ−Low δ*
Risk	−1.80	−4.76*	−4.03***	−9.76***	−2.23	−5.00	
Female					14.62**	10.55*	4.07
Mean of *Risk*	5.69	6.33	6.12	6.22	0.43	−0.11	
	(1.75)	(1.56)	(2.00)	(0.94)			
Mean of *Expnd*	31.43	43.36	45.28	57.99	13.85**	14.62**	−0.77
	(4.58)	(3.97)	(4.24)	(3.99)			
Participant %	37.5%	42.5%	62.5%	57.5%	25.0%	15.0%	

*Note: Partial effects on Female evaluated at the treatment averages of Risk. Standard errors for means in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

significantly larger expenditures than their male counterparts in both *PP-High δ-Indefinite* and in *PP-Low δ-Indefinite*.[13] However, there is not a significant difference in the partial effect of *Female* across the two treatments ($p = 0.656$). This conclusion is supported by Figure 6 which shows kernel densities for Round 1 expenditure in the *PP* indefinite contests, by gender.

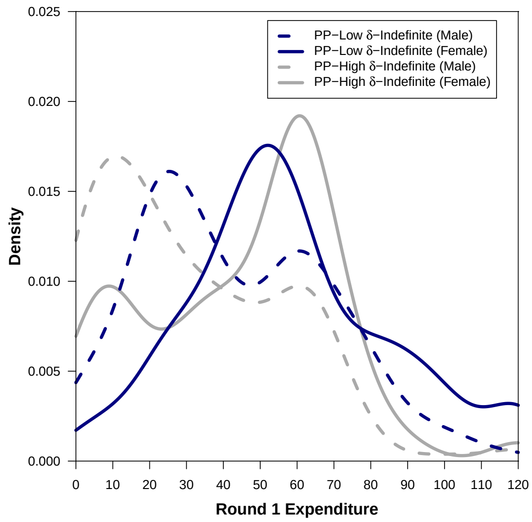
So why *do* we observe significantly lower expenditure in *PP-High δ-Indefinite* than in *PP-Low δ-Indefinite* when we do not control for risk and gender (as in Section 5.1)? The difference across genders in average expenditure was -0.77 ECUs. However, this figure does not account for the gender composition of the two treatments, and there was a higher percentage of females in *High δ* than in *Low δ*.

The difference in the partial effect of gender on expenditure across treatments was 4.07 ECUs. While this figure is not significantly different from zero, coupled with the difference in gender composition across *High δ* and *Low δ*, it suggests that *High δ* had a few more female participants who had a little higher expenditure. This being the case, when we control for gender (Table 8) we do not see a significant effect across treatments, whereas when we do not control for gender, we do see a significant effect (Table 4).

Result 6. *When we do not control for risk and gender, we find significantly less expenditure in* PP-High δ-Indefinite *relative to* PP-Low δ-Indefinite. *Females had higher average expenditure than males in both treatments, and the gender composition was slightly more skewed towards females in* High δ *than in* Low δ. *When we control for risk and gender we find no significant difference in expenditure across the treatments. We conclude that the specific gender composition of our participant sample explains Result 2.*

[13] These partial effects are calculated using the treatment average of *Risk*.

Figure 6: Kernel Densities, *PP* Indefinite Contests



Expenditure, by Treatment and Gender

5.4.2 *PP-High δ-Indefinite* and *PP-High δ-Finite* comparison

In column (4) of Table 8, there is a significant difference in average expenditure across *PP-High δ-Indefinite* and *PP-High δ-Finite* in the long-run. In contrast, in Table 4 there is no significant difference across these two treatments. When we control for risk and gender, expenditure is significantly *higher* in the *Indefinite* treatment relative to the *Finite* treatment. This finding accords with Figure 2d, but it is very surprising in light of Hypothesis 2a, which predicts the opposite.

Result 7. *Controlling for risk and gender, and after several rounds of contest experience, average expenditure is higher in indefinitely repeated* PP *contests with* $\delta = 0.8$ *relative to finitely repeated* PP *contests with* $\delta = 0.8$.

5.4.3 *WTA* and *PP* comparisons

In Table 8, short-run expenditure is higher in *PP-Low δ-Finite* relative to *WTA-Low δ-Finite*, but this difference disappears over time. However, there are long-run expenditure differences in the indefinite contests. Over time, estimated expenditure is significantly larger in the *PP* setting relative to the *WTA* setting in both the *High δ* and the *Low δ* indefinite contest data. Table 10 contains the same information as does Table 9, but for the *PP* and *WTA* indefinite contest comparisons.

Table 10: Partial Effects, *WTA* and *PP* Indefinite Contests

	Male		Female		Female − Male		
Partial Effect	*WTA-High δ*	*PP-High δ*	*WTA-High δ*	*PP-High δ*	*WTA-High δ*	*PP-High δ*	*WTA−PP*
Risk	−0.17	−1.80	3.42	−4.03***	3.59	−2.23	
Female					−0.90	15.37**	−16.27*
Mean of *Risk*	5.00	5.69	6.38	6.12	1.38*	0.43	
	(1.84)	(1.75)	(1.50)	(2.00)			
Mean of *Expnd*	35.14	31.43	29.87	45.28	−5.27	13.85**	−19.13*
	(6.37)	(4.58)	(4.10)	(4.24)			
Participant %	40.0%	37.5%	60.0%	62.5%	20.0%	25.0%	

	Male		Female		Female − Male		
Partial Effect	*WTA-Low δ*	*PP-Low δ*	*WTA-Low δ*	*PP-Low δ*	*WTA-Low δ*	*PP-Low δ*	*WTA−PP*
Risk	3.75	−4.76*	−3.59***	−9.76**	−7.34	−5.00	
Female					−10.63*	10.55*	−21.18**
Mean of *Risk*	6.65	6.33	6.56	6.22	−0.09	−0.11	
	(1.14)	(1.56)	(2.19)	(0.94)			
Mean of *Expnd*	48.93	43.36	35.25	57.99	−13.68*	14.62**	−28.30***
	(5.83)	(3.97)	(4.88)	(3.99)			
Participant %	57.5%	42.5%	42.5%	57.5%	−15.0%	15.0%	

Note: Partial effects on Female evaluated at the treatment averages of Risk. Standard errors for means in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the *High δ* treatments, females were significantly more risk averse *ex ante* than males in the *WTA* setting (rank sum test, $n_m = 11, n_f = 16$, $p = 0.059$), but the *ex post* partial effect of *Risk* on expenditure was not significant ($p = 0.286$). There was no significant difference in risk *ex ante* in the *PP* setting (rank sum test, $n_m = 13, n_f = 17$, $p = 0.264$), and the partial effect of *Risk* across genders was not significant in *PP-High δ-Indefinite* ($p = 0.375$)

Table 10 also contains information on gender and expenditure. Female and male participants had statistically indistinguishable expenditures in *WTA-High δ-Indefinite* ($p = 0.488$), but female participants had higher expenditures than their male counterparts to the tune of 13.85 ECUs in *PP-High δ-Indefinite* ($p = 0.029$). The magnitude of the partial effect of *Female* on expenditure is similar (15.37 ECUs), and is also significantly different ($p = 0.077$) across contest settings for *High δ*.

WTA-Low δ-Indefinite and *PP-Low δ-Indefinite* can also be compared via Table 10. There is neither a significant *ex ante* risk difference across gender in the *WTA* setting ($p = 1.000$), nor in the *PP* setting ($p = 0.895$).[14] *Risk* has a negative and significant partial effect on expenditure in three of the four gender and treatment cases, but the

[14] To reach this conclusion, a rank sum test ($n_m = 20, n_f = 9$) and a rank sum test ($n_m = 12, n_f = 18$) were used, respectively.

partial effect of $Risk$ is not significantly different across men and women in *WTA* contests ($p = 0.206$) or in *PP* contests ($p = 0.234$).

On the other hand, there are significant differences in expenditure across females and males. Both the mean expenditure and the partial effect of $Female$ on expenditure are negative and significant in *WTA-Low δ-Indefinite*, while both are positive and significant in *PP-Low δ-Indefinite*. The two mean expenditures are different from one another ($p = 0.004$), as are the two partial effects of $Female$ on expenditure ($p = 0.016$).

Result 8. *Female participants had higher average expenditure than male participants in* PP *indefinite contests with* $\delta = 0.5$ *or* $\delta = 0.8$. *Female participants had lower average expenditure than male participants in* WTA *indefinite contests when* $\delta = 0.5$. *Controlling for risk and gender, average expenditure is higher in indefinitely repeated* PP *contests than in indefinitely repeated* WTA *contests in the long-run.*

We now discuss our results and conclude.

6 Discussion and Conclusion

Despite the recent surge in experimental contest work (Dechenaux, Kovenock and Sheremeta, 2015), no research has yet examined indefinitely repeated contests. Such contests are interesting in their own right because of their prevalence in economic life, but also because they shed empirical light on indefinitely repeated games with (relatively) large strategy spaces and complex payoff structures. This paper experimentally examines three primary questions related to indefinitely repeated contests.

First, *is cooperation increasing in the expected length of indefinite contest supergames*? There is no significant difference in average expenditure across *WTA-High δ-Indefinite* and *WTA-Low δ-Indefinite*, even when we control for risk and gender. We find some evidence of lower average expenditure (more cooperation) in *PP-High δ-Indefinite* compared to *PP-Low δ-Indefinite*. But after controlling for risk and gender, we attribute this difference to a gender composition effect across these two treatments.

Second, *is there more cooperation in indefinite contest supergames compared to finite contest supergames*? Surprisingly, we find no support for more cooperation in indefinite contests compared to finite contests. If anything, when we control for risk and gender, there is evidence of greater average expenditure in the long-run in *PP-Low δ-Indefinite* relative to *PP-Low δ-Finite*.

Our third and final question is: *Is cooperation the same across indefinite winner-take-all and indefinite proportional-prize contests*? In our indefinite contests, there is strong evidence of less cooperation in the *PP* setting relative to the *WTA* setting when $\delta = 0.5$, whether we control for risk and gender or not. When we control for risk and gender, there is also evidence of less cooperation in *PP-High* δ indefinite contests than in *WTA-High* δ indefinite contests.

Our *WTA-PP* results contrast with the results in Cason, Masters and Sheremeta (Forthcoming). They report higher average expenditure in *WTA* contests compared to *PP* contests. We find the opposite to be true in both finitely repeated and indefinitely repeated contests. However, there are a number of important differences between our experiments and theirs. In particular, their contest success function has a random noise component and they use quadratic expenditure costs.

We find that the *WTA-PP* expenditure difference in our indefinite contests is driven by female participants with relatively high expenditures. This is a novel result in the indefinitely repeated games literature and in the finitely repeated contests literature. Dal Bó and Fréchette (Forthcoming), focusing on the PD, report that the literature has "not found a robust relationship between gender and cooperation in infinitely repeated games." In the contest literature, Sheremeta (2013) and Dechenaux, Kovenock and Sheremeta (2015) report that women overbid more relative to equilibrium than men in finitely repeated *WTA* contests. Our interesting results on gender in indefinite contests deserve further examination, but we leave such a study for future research and a targeted experimental design that is better suited than ours for such an exploration.

Perhaps our most surprising result is that indefiniteness does not affect expenditure in our repeated contests. This conclusion is surprising because indefiniteness *does* matter in repeated prisoner's dilemmas. Here we can only speculate as to why this is so. It is possible that in indefinite prisoner's dilemmas, players can focus on the length uncertainty because payoffs are certain. By contrast, perhaps players in indefinite contests cannot focus on the length uncertainty because they must contend with payoff uncertainty. We observe similar time trends in average expenditure in all of our treatments: Average expenditure falls over the first several supergames. This is consistent with participants grappling with payoff uncertainty, and the idea that participants need experience to realize that mutual expenditure reductions (cooperation) can be beneficial in contests.

To the best of our knowledge, our paper is only the third experimental examination of indefinitely repeated games with "large" strategy spaces. Tan and Wei (2014) and Lugovskyy et al. (2017) both investigate indefinitely repeated public good games. Like us, they report less support for predictions from the theory of repeated games than have experimental examinations of indefinite prisoner's dilemmas. More empirical work on indefinite supergames with large strategy spaces is clearly needed, but the early returns from papers examining indefinite supergames with many feasible strategies suggest that participant behavior in such supergames does not conform well to the theory of repeated games.

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A Experimental Instructions

Due to the similarity between all instructions, we only reproduce the instructions for the *WTA-High δ-Indefinite* treatment. All of the instructions are available upon request.

Introduction

Thank you for participating in today's experiment. Your earnings will depend on your own decisions and the decisions of other participants. Your earnings, including your $7.00 show-up fee, will be paid to you privately in cash at the end of the experiment.

Please remain quiet and do not communicate with other participants. If you have a question, raise your hand and wait for a proctor to come answer your question privately.

In the experiment, your payoffs will be denominated in experimental currency units (ECUs for short). At the end of the experiment, ECUs will be converted into cash at an exchange rate of **25 ECUs to $1**.

Timeline

Today's experiment will last **10 Periods**.

Each Period consists of an uncertain number of Decisions. The actual number of Decisions in a given Period will be randomly determined. So any two Periods *may or may not* contain the same number of Decisions.

In each Decision you will make an allocation. After you've made your allocation, the computer will randomly determine if there will be another Decision in the current Period. There will be a **80%** chance of another Decision (and a **20%** chance that the Period ends).

After each Decision, the computer will randomly select one of the following integers: {1, 2, 3, 4, 5}. The computer is equally likely to select any of these integers. If the computer selects "1," there will be no further allocation decisions, and the current Period will conclude. If the computer selects "2," "3," "4," or "5," the Period will continue for another Decision. Note that the odds that the computer randomly selects "1" are 1/5 or 20%.

Pairing Procedure

Within a Period, regardless of the number of Decisions it contains, you'll be paired with the same participant during each Decision.

Between Periods, you'll be randomly paired with another participant with whom you have never been previously paired.

Decisions

In each Decision, you will have **120 ECUs** at your disposal.

You will decide how many of these 120 ECUs you wish to allocate towards possibly receiving **120 additional ECUs**.

You can allocate any integer amount between 0 ECUs and 120 ECUs.

What determines if you receive the 120 additional ECUs? You will allocate a certain number of ECUs as will the participant you are paired with. Suppose that you allocate X ECUs and that the participant you are paired with allocates Y ECUs. In this example, you have a

$$\mathbf{X/(X+Y)}$$

chance of receiving the 120 additional ECUs.

In other words, your chance of receiving the 120 additional ECUs is equal to the number of ECUs you allocate divided by the total number of ECUs allocated by both you and the participant you are paired with.

In each Decision, either you *or* the participant you are paired with will receive all the 120 additional ECUs.

Note that if both X and Y are 0 (zero), your chance of receiving the 120 additional ECUs is 1/2 or 50%.

Payoffs

Your payoff in each Decision will be calculated as follows:

- If you do receive the 120 additional ECUs, you will earn

$$PAYOFF = (120 - ALLOCATED\ ECUs) + 120$$

- If you don't receive the 120 additional ECUs, you will earn

$$PAYOFF = (120 - ALLOCATED\ ECUs) + 0$$

Your payoff in a given Period will be the sum of your payoffs for all the Decisions that make up that Period (recall that the number of Decisions in a Period is uncertain).

Note that any of the 120 ECUs at your disposal that are not allocated towards possibly receiving the additional ECUs are included in your payoff.

The participant you are paired with has exactly the same payoff equation that you do, but you may have different payoffs in a given Decision, depending upon who receives the additional ECUs.

All participants in today's session will be paid for 1 randomly selected Period. The computer will randomly select the Period that you will be paid for after the experiment concludes.

Practice

Before the actual experiment begins, you will make some Practice Decisions. By practicing, you will become familiar with how your chance of receiving the additional ECUs is determined.

You will not interact with other participants during this practice, and your practice decisions won't be shown to anyone. You won't earn anything for Practice Decisions; they are simply to help you understand the experiment.

Just like in Decisions in the actual experiment, you will practice allocating ECUs towards possibly receiving 120 additional ECUs. During these Practice Decisions you will also choose a hypothetical allocation for the participant you are paired with. **In the actual experiment, you will be paired with a real participant who will make their own allocation decision.**

In just a moment, you will be able to practice making three allocation decisions.

Summary

- This experiment will last **10 Periods**
- Periods consist of Decisions
  - each Period will have *at least* 1 Decision
  - the total number of Decisions in a Period is uncertain
  - after each Decision, there is a **80%** chance that another decision will occur in the current Period (there is a **20%** chance that the current Period ends)
- You will be paired with the same participant *during* each Period, but randomly paired with another participant with whom you have never been previously paired *between* each Period
- Each Decision, you will have **120 ECUs** at your disposal
- Each Decision, you can receive **120 additional ECUs** depending on your own allocation and the allocation of the participant you are paired with
- Each Decision, either you *or* the participant you are paired with will receive all the 120 additional ECUs
- Each Decision, your payoff will be:

$$\mathbf{120 - ALLOCATED\ ECUs + \underset{\text{(if you receive them)}}{ADDITIONAL\ ECUs}}$$

- Your Period payoff is the sum of your payoffs for all the Decisions in that Period

- You will be paid for 1 randomly selected Period at an exchange rate of **25 ECUs to $1**
- You may review these instructions at any point during the experiment
- Please remain quiet and do not communicate with other participants. Failure to do so will result in you being asked to leave the lab without being paid
- Are there any questions before we begin?