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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. Our data support this prediction, although this result attenuates with contest experience. Theory also predicts more cooperation in indefinitely repeated contests compared to finitely repeated contests of the same expected length, and we find empirical support for this. Finally, theory predicts no difference in cooperation across indefinitely repeated *winner-take-all* and *proportional-prize* contests, yet we find evidence of less cooperation in the latter, though only in longer treatments with more contests played. 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.

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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 (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 repeated and have an indefinite time horizon. 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 end time. Another example involves network neutrality lobbying. In recent years, internet service providers (Comcast, Verizon, AT&T) and content providers (Netflix, Google, Facebook, Amazon) have repeatedly contested the legality of termination fees—payments from content providers to service providers—over a changing sequence of regulatory regimes.

This study examines repeated contests of both known and, especially, unknown length.[1] Our methodology is experimental, because the opportunity costs of contest expenditures are difficult—often impossible—to parse from field data. While one-shot and finitely repeated contests have received much experimental attention, to the best of our knowledge, there is no extant experimental research on indefinitely repeated contests.[2] Such contests are worthy of study as important economic phenomena, but they also deserve attention because they have quite large strategy spaces, and 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. Beyond better knowledge of contest behavior vis-à-vis game theory, we are simply interested in the extent of cooperation in

[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] Dechenaux et al. (2015) survey experimental contest research while Dal Bó and Fréchette (2018) review the experimental supergame literature. The experimental indefinite supergame literature has largely focused on the Prisoner's Dilemma. For example, see Murnighan and Roth (1983); Dal Bó (2005); Duffy and Ochs (2009); Dal Bó and Fréchette (2011); Dal Bó and Fréchette (2018). 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), Holt (1985); Feinberg and Husted (1993) (oligopoly), Camera and Casari (2014); Duffy and Puzzello (2014) (monetary exchange), Engle-Warnick and Slonim (2006a, b) (trust), McBride and Skaperdas (2014) (conflict), and Kloosterman (2020) (coordination).

indefinite contests. Changes in the (expected) length of interaction can affect how firms collude on research and development, how politicians cooperate on legislation, or whether litigants sustain suits or settle them. Examining cooperation in indefinite contest experiments sheds light on cooperation in many real-world contests.

In focusing on the extent of cooperation in indefinite contests, we track a number of existing studies of indefinitely repeated games. The great majority of this work examines the Prisoner's Dilemma (PD). It typically focuses on testing two theoretical predictions: (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 comparable finite supergames*. Many, though not all, studies confirm these predictions.[3]

Like the PD, contests are social dilemmas. In both games, the stage game equilibrium is not socially optimal, but the socially optimal or "cooperative" outcome can be supported in an indefinite supergame when players are sufficiently patient. This being said, there are several important 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 (PGGs) that can serve as extended strategy space analogs of PDs. Contests have a nonmonotone (typically, single-peaked) best response; that is, relatively low expenditure levels are best responses to low rival expenditure *and* to 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, 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 et al. 2015). Such overbidding increases one's chances of winning and imposes negative externalities on other players, which is, by construction, 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 indefinite PDs and other previously studied games.

We conduct indefinitely repeated contest experiments using the well-established continuation probability approach.[4] Following the seminal setups by Engle-Warnick and Slonim (2004) (trust games) and by Dal Bó (2005) (PDs), 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.

[3] Some studies mostly confirm theory (Dal Bó 2005; Duffy and Ochs 2009; Dal Bó and Fréchette 2011; Fréchette and Yuksel 2017), while others report more mixed support for theory (Roth and Murnighan 1978; Murnighan and Roth 1983; Normann and Wallace 2012; Lugovskyy et al. 2017; Kloosterman 2020).

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

As do existing indefinite 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 *proportional-prize* allocation rule.

In a *winner-take-all* (WTA) setting (Tullock 1980), the entire contest prize is awarded stochastically, according to probabilities equal to each player's 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* (PP) setting (Van 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 find evidence of greater cooperation in indefinitely repeated contests of longer expected length. Both *WTA* and *PP* indefinite contests feature lower expenditures when the continuation probability is larger compared to when it is smaller. However, this difference is only statistically significant when participants have played relatively few contests; as participants experience more and more rounds of play, expenditure remains lower in high continuation probability treatments, but the difference loses its statistical significance. When we compare indefinitely repeated contests to finitely repeated contests of the same expected length, we find significantly more cooperation in both indefinitely repeated *WTA* and *PP* contests. Finally, we report evidence that expenditure is greater (less cooperative) in *PP* contests relative to *WTA* contests, but only in longer treatments with more contests played.

Our results are particularly interesting vis-à-vis results from indefinitely repeated PD experiments. At least some support for repeated game theory is typically found in these experiments, but the sparse experimental evidence on cooperation in indefinitely repeated games with larger action spaces is mixed. In line with theory, Holt (1985) reports both collusive and non-cooperative behavior in indefinitely repeated Cournot duopolies and only non-cooperative behavior in one-shot Cournot duopolies.[5] However, Lugovskyy et al. (2017) find no systematic differences in cooperation across indefinitely and finitely repeated public goods games.[6]

In a contest setting, we find qualified support for the predictions of the theory of repeated games. Our results are equivocal because in our data the effect of the shadow of the future appears to depend on participant experience. We think that much experimental work remains to be done on indefinite supergames with "large" strategy spaces, since the very early returns suggest that standard predictions for

[5] Examining cooperation across continuation probabilities is not the main concern of Holt (1985).

[6] Kloosterman (2020) reports low cooperation in an indefinite 3×3 coordination game—a game with a slightly larger strategy space than the PD.

social dilemmas *may* have less bite in more complex indefinite supergames than in relatively simpler indefinite supergames such as the Prisoner's Dilemma.

Our paper is organized as follows: In Sect. 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 Sect. 4. In Sect. 5 we analyze the results of experiments, and we discuss our results and conclude in Sect. 6.

2 Theory

We consider two contest settings. The first setting is the *winner-take-all* contest (*WTA*) where the entire prize revenue is allocated to one winner. 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, own-payoff maximizers, 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.[7] Without loss of generality, we provide details of a dynamic contest model assuming that each stage game is a *WTA* contest.

2.1 The stage game

Consider a contest with two risk-neutral players, indexed by $i \in \{1, 2\}$, competing for a positive integer prize V by independently and simultaneously choosing expenditure levels $x_i \in \{0, 1, \ldots, V\}$.[8] 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, therefore,

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

[7] Relaxing the risk-neutrality assumption and considering, say, symmetrically risk-averse players exhibiting CARA preferences changes predictions quantitatively, but preserves our comparative statics, with the exception that expenditure levels in *PP* and *WTA* contests no longer coincide. In Sect. 4, we elaborate on the behavior of non-risk-neutral players in each contest setting.

[8] Contests are typically defined with continuous, unbounded strategy spaces. In this paper, we define strategy spaces as discrete and bounded because that is the way they are implemented in our experiment. This discrete expenditure level restriction is also useful because it allows for the existence of the best deviation from socially optimal play. For a sufficiently large V, this modified, discrete game is a good approximation of the original, continuous game and has the same equilibrium, socially optimal expenditures, and payoffs as the original game, provided that V is divisible by 4.

It is easy to see that, provided V is divisible by 4, both players choosing expenditure levels $x^* = \frac{V}{4}$ is a symmetric Nash equilibrium (NE).[9] The expected payoffs in equilibrium are $\pi^* = \frac{V}{4}$. Socially optimal, cooperative play (SO) is characterized by both players choosing expenditure $x^{so} = 0$, and earning expected payoffs $\pi^{so} = \frac{V}{2}$.

2.2 The supergame

Consider an infinitely (or indefinitely) repeated game where the contest described in Sect. 2.1 is the stage game. Both players discount future payoffs by factor $\delta \in [0, 1]$ (or the probability of continuation in each round is δ). 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:

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

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 $x^{dev} = 1$ leads to stage game payoff $V - 1$ for the deviating player, provided that the other player spends $x^{so} = 0$). 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 the cooperative solution *can* be sustained as an equilibrium, under the specific trigger strategy we defined above. There are many other trigger strategies, not to mention other, alternative strategies that can be supported as equilbria in the indefinitely repeated game. For example, always playing the stage game NE is obviously an equilibrium for any δ, including δ satisfying Condition (4). Thus, Condition (4) extends the set of available equilibria enough to include the cooperative outcome, but it does not pin this outcome down in any stronger sense. It is an empirical question as to what extent—if at all—behavior will shift in the direction of cooperation when Condition (4) is satisfied, compared to when it is not.

[9] This is the unique NE of the contest game with continuous strategy spaces (see, e.g., Szidarovszky and Okuguchi 1997). For our parameterization, we verify numerically that it is also the unique NE in the discrete game.

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 Engle-Warnick and Slonim (2004) and 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 XS/FS laboratory at Florida State University. A total of 480 participants took part, 60 per treatment.[10] Participants were recruited using ORSEE (Greiner 2015), and the experiment was implemented in z-Tree (Fischbacher 2007). Each participant was in exactly one session, and none of our participants were experienced in our environment. *Low* δ sessions lasted 45−60 minutes, while *High* δ sessions took 80−100 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 the supplementary material for the instructions). Participants were told that they could refer to their hard copy of the instructions at any point during the actual experiment. After the instructions phase, in order to better familiarize themselves with the contest environment, 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. Participants did not interact with each other during this practice stage.

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 the NE expenditure was $x^* = 30$ and the payoff assuming NE expenditure was $\pi^* = 30$. Because $x^{so} = 0$, the payoff assuming SO expenditure was $\pi^{so} = 60$, or twice the payoff under the NE expenditure.

By Condition (4) from Sect. 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 Sect. 2.2 in our *High* δ treatments ($\delta = 0.8$), but not in our *Low* δ treatments ($\delta = 0.5$).[11] For all treatments, each experimental session consisted of 10 supergames whose stage games are outlined in theory in Sect. 2.1. We will refer to a stage game as a "Round."

[10] With this sample size, average expenditure comparisons between any two treatments have power 29%, 86%, and 99% for effect sizes (Cohen's *d*) 0.2, 0.5 and 0.8, respectively, at $\alpha = 0.10$. Thus, the conventional 80% power level would be reached for medium effect sizes and beyond.

[11] While the grim trigger strategy involving $x^{so} = 0$ cannot be supported under $\delta = 0.5$, other symmetric strategy pairs $(\bar{x}, \bar{x})$, with $\bar{x} \in (0, x^*]$, can be supported. The most cooperative outcome that can be supported is the grim trigger strategy involving cooperation at $\bar{x} = 4$ until defection.

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 current supergame. Between supergames, participants were randomly re-paired according to a zipper matching protocol (Cooper et al. 1996).[12] Participants were instructed that they would not interact with any other participant in more than one supergame.[13] Strictly speaking, using a zipper matching protocol does not remove the possibility of dynamic session effects (Fréchette 2012). But it does resolve many contagion-related issues that are observed in experimental designs featuring fixed pairings or random re-matching within small-sized matching groups, such as reciprocity or strategic teaching from round to round.

In the *Indefinite* treatments, discounting was implemented through random supergame termination.[14] In line with previous studies (Lugovskyy et al. 2017; Fréchette and Yuksel 2017), we employed a seed procedure to determine supergame length. With ten supergames per session, and three sessions each for *High* δ and *Low* δ treatments, we used sixty seeds to generate six unique sequences. These seeds generated pseudo-random supergame lengths, and we used seeds that are themselves (arguably) random.

Our seeds were ten digit ISBN numbers for ten *New York Times* best sellers from six years: 1985, 1990, 1995, 2000, 2005, and 2010.[15] Different random realizations of supergame length were used across sessions with the same value of δ (as in, e.g., Lugovskyy et al. 2017). The supergame lengths are shown in Table 2. For continuation probability δ, the expected supergame length is $\frac{1}{1-\delta}$, or 2 rounds when $\delta = 0.5$ and 5 rounds when $\delta = 0.8$. Conditional on $\delta \in \{0.5, 0.8\}$, all of our *Finite* sessions used the same sequence lengths. The *Finite* sequences are also presented in Table 2.

The first round of each supergame was always played.[16] Once participants submitted their expenditures, the outcome of the stage contest was randomly or non-randomly determined in accordance with CSF (1) from Sect. 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

[12] The zipper (turnpike) matching protocol was implemented with 20 participants over 10 supergames. Before the first supergame, participants were divided into two groups (A and B) of 10 participants. Within each group, participants were assigned unique ID numbers, $i_A, i_B \in \{1, \dots, 10\}$. For the first supergame, participant i_A from group A was matched with participant i_B from group B so that $i_A = i_B$. In subsequent supergames, group B participants were rotated into novel matches with group A participants. Specifically, in supergame S, participant i_A was always matched with participant $i_B = 10 \cdot \mathbb{1}\{S > i_A\} + (i_A + 1) - S$.

[13] Many indefinite supergame experiments use this procedure (see, e.g., Dal Bó 2005; McBride and Skaperdas 2014).

[14] See Fréchette and Yuksel (2017) for additional ways to implement discounting in indefinitely repeated games and for comparisons between them. We briefly considered, but ultimately rejected having participants play for infinite time due to IRB concerns.

[15] Please see Table 2 in the supplementary material for the specific seeds. For *Low* δ sequences we used the years 1985, 1995, and 2005; for the *High* δ sequences we used the years 1990, 2000, and 2010.

[16] We did not use the words "supergame" or "round" in the experimental instructions. Instead, we referred to supergames as "periods" and to rounds within the supergame as "decisions."

Table 1 Summary of experimental treatments

Treatment name	Setting	δ-Value	Sessions	Participants
WTA-Low δ-Indefinite	WTA	0.5	3	60
WTA-Low δ-Finite	WTA	0.5	3	60
WTA-High δ-Indefinite	WTA	0.8	3	60
WTA-High δ-Finite	WTA	0.8	3	60
PP-Low δ-Indefinite	PP	0.5	3	60
PP-Low δ-Finite	PP	0.5	3	60
PP-High δ-Indefinite	PP	0.8	3	60
PP-High δ-Finite	PP	0.8	3	60
			24	480

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 $T \in [1, 100]$ was shown to participants. The draws of T were consistent with the pre-drawn sequences shown in Table 2. If $T > \delta \cdot 100$ was shown, the current supergame ended; if any other number was shown, another round was played. The relevant portion of the instructions (with $\delta = 0.8$) reads:

> Each Period will contain some number of Decisions. The actual number of Decisions in each Period was randomly determined by a computer prior to today's session. (...) After each Decision, the computer will display a randomly generated number between 1 and 100 on your screen. This number was randomly determined by a computer prior to today's session. If the number is less than or equal to 80, the current Period will continue for at least one more Decision. If the number is greater than 80, the current Period will end.

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 et al. 2018). The exchange rate was 25 ECUs to 1 US Dollar. Participants earned \$29.93 on average, including a \$10.00 show-up fee and earnings from an incentivized risk task.[17]

[17] Please see Table 1 in the supplementary material for treatment-specific mean earnings. Following the main portion of the experiment, participants completed an incentivized risk task where they chose a single "switch point" from a menu of 21 choices between the same lottery—(\$2.00, \$0.00;0.5, 0.5)—and different sure amounts of money—from \$0.00 to \$2.00 in increments of \$0.10. They then answered a qualitative risk question ["risk_qual" from the Preference Survey Module introduced by Falk et al. (2016)] and a few demographic questions before being paid. Instructions for the incentivized risk task are in the supplementary material.

Table 2 Supergame lengths

Treatment	Sequence	Supergame										Total
		1	2	3	4	5	6	7	8	9	10	
Low δ -Indefinite	*i*	2	1	1	3	1	1	2	1	1	1	14
	ii	2	1	2	2	2	1	2	1	1	1	15
	iii	1	2	1	1	1	1	1	1	1	4	14
Low δ -Finite	*i*	2	2	2	2	2	2	2	2	2	2	20
	ii	2	2	2	2	2	2	2	2	2	2	20
	iii	2	2	2	2	2	2	2	2	2	2	20
High δ -Indefinite	*i*	4	10	5	1	12	2	5	20	2	2	63
	ii	18	1	2	9	1	5	4	6	1	4	51
	iii	7	11	1	6	2	6	6	3	5	1	48
High δ -Finite	*i*	5	5	5	5	5	5	5	5	5	5	50
	ii	5	5	5	5	5	5	5	5	5	5	50
	iii	5	5	5	5	5	5	5	5	5	5	50

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

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 prediction for repeated social dilemmas and follows, albeit in a "soft" way, from the analysis presented in Sect. 2. An increase in δ extends the set of available cooperative equilibria, and we hypothesize that this shifts behavior into the extended set. In other words, we expect expenditure to be lower in our *High δ-Indefinite* treatments relative to our *Low δ-Indefinite* treatments, under either contest setting.

We can also consider whether playing the cooperative equilibrium (SO) is risk-dominant compared to playing the stage game NE.[18] Suppose that Players i and j can only choose between two strategies. They can both either play the grim trigger strategy described in Sect. 2.2, or they can play (x^*, x^*) indefinitely.[19] Let ρ denote the probability that Player j plays grim trigger, and let $\pi(x_i^{so};\rho)$ and $\pi(x_i^*;\rho)$ denote the expected payoffs to Player i from playing the grim trigger strategy and the stage game NE, respectively.

Ignoring our experimental restriction of integer investments, it is easily verified that $\pi(x_i^{so};\rho) = \pi(x_i^*;\rho)$ at $\rho^* = \frac{1-\delta}{2\delta-1}$, for $\delta \in [\frac{2}{3}, 1]$. Thus, the grim trigger strategy will be played for any beliefs on grim trigger play satisfying $\rho \geq \rho^*$. When $\delta = 0.8$, $\rho^* = \frac{1}{3}$, which implies that the grim trigger strategy is risk dominant since $\rho^* < 0.5$. We can also find the threshold level of discounting, $\hat{\delta}$, such that, for $\delta < \hat{\delta}$, playing the grim trigger strategy is no longer risk dominant ($\rho^* > 0.5$). It is easily verified that $\hat{\delta} = 0.75$ in our experiment, so the grim trigger strategy is risk dominant in our *High δ-Indefinite* treatments where $\delta = 0.8$. In sum, Hypothesis 1 can also be motivated through consideration of risk dominance.

Hypothesis 2a results from the fact that the Nash reversion, grim trigger strategy from Sect. 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 2b follows from the fact that a Nash reversion strategy cannot support socially optimal cooperation in *any* of our *Low δ* treatments (whether *Indefinite*, *Finite*, *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.[20] 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 et al. (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 et al. (2010) find similar expenditures across *WTA* and *PP* settings irrespective of whether entry into the

[18] The analysis of risk-dominant equilibria has shed light on the role of expectations in cooperative behavior in indefinitely repeated PD experiments (Blonski et al. 2011; Dal Bó and Fréchette 2011; Kloosterman 2020).

[19] We consider these two strategies to succinctly highlight the trade-off between risk and cooperation.

[20] Formally, the stage game best response to expenditure $\bar{x} < V/4$ 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 $V/2 - \bar{x}$. A derivation similar to the one in Sect. 2.2 produces a threshold value of the discount factor $\tilde{\delta} = \frac{(\sqrt{V}-\sqrt{\bar{x}})^2 - \frac{V}{2} + \bar{x}}{(\sqrt{V}-\sqrt{\bar{x}})^2 - \frac{V}{4}}$. It can be shown that $\tilde{\delta}$ decreases monotonically in $\bar{x}$ for $\bar{x} \in [0, \frac{V}{4}]$.

contest is exogenous or endogenous. Shupp et al. (2013) report greater expenditure in *PP* contests than in *WTA* contests. Finally, assuming that individual output is a noisy function of individual expenditures, Cason et al. (2018) report greater expenditure in *WTA* contests relative to *PP* contests. Note that none of these studies examine indefinitely repeated contests.

If we assume that agents have a symmetric and publicly known degree of risk aversion, the stage game equilibrium expenditure is *higher* in *PP* contests than in *WTA* contests. However, we do not propose an alternative to Hypothesis 3 based on risk-aversion, because of the aforementioned mixed empirical results on the relationship between risk aversion and expenditure in the experimental contest literature.

Expenditure differences across the *PP* and *WTA* environments may be driven by unobserved behavioral forces that are stronger than risk aversion. For example, while our experimental instructions are neutral, 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 (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 et al. 2002; Sheremeta 2013; Boosey et al. 2017). In a *PP* contest, the concept of winning is not as sharply defined as in a *WTA* contest. It is plausible that some *PP* participants think they "win" a contest when their share of the prize exceeds one half, but winning is less salient in the *PP* setting.

5 Results

We now report expenditure results using data from all rounds (All Rounds) and from the first round of each supergame (Round 1). Round 1 is interesting because it is the only round where the expected number of rounds is the same across the *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. While we always report results from both All Rounds and Round 1, when examining Hypotheses 1 and 3, we focus on the analysis using data from All Rounds. By contrast, when examining Hypothesis 2, we focus on the analysis using Round 1 data.

Table 3 presents average expenditures for each treatment (for all ten supergames and for the last five supergames only). While these averages indicate that expenditure fell with experience, we caution the reader that they obscure exactly how participant experience affects expenditure. We also report round-specific expenditure averages in Table 4, though they do not suggest any obvious expenditure pattern within a supergame.[21] Table 3 [4] and Figure 1 [2] in the supplementary material show average expenditure for each session of each *WTA* [*PP*] treatment.[22] Visual inspection suggests possible session effects in *WTA-Low δ-Indefinite* and *WTA-High δ-Finite*. To account for these session differences, we report regression results with session fixed effects as discussed below.

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 horizontal axis in the All Rounds panel is labeled ‘Contests’ for easy correspondence with the regression results presented below. 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 and c suggest differences in average expenditure across *WTA-Low δ -Indefinite* and *WTA-High δ-Indefinite*, both in All Rounds and in Round 1 only. Likewise, Fig. 1b and d 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 regressions.

Note from Fig. 1 (and from Figs. 2, 3, and 4 below) that the time series of average expenditure is sometimes quite linear, but usually displays some curvature in the cumulative number of contests played. This observation motivates our regression specification; we use a straightforward model, with separate “intercepts” for each session, and different “slopes” for each of the two treatments being compared. Our quadratic specification allows for curvature in the “slope” estimate.[23] Formally, to test the effect of continuation probability on expenditure, our specification is:

$$\begin{aligned} Expenditure_{i,s,t} = \beta_1 Contests_t &+ \beta_2 (HighDelta_i \times Contests_t) \\ &+ \beta_3 Contests_t^2 + \beta_4 (HighDelta_i \times Contests_t^2) \\ &+ \alpha_s' Session + \epsilon_{i,s,t} \end{aligned} \tag{5}$$

We estimate the coefficients in (5) using data from both All Rounds and from Round 1 only. The dependent variable is Participant i’s expenditure in the actual experimental contest t. We control for the overall effect of experience with the variable $Contests_t$, which is the cumulative round (across supergames) in which the

[21] Tables 5, 6, and 7 in the supplementary material contain average expenditures for every five contests, by supergame using data from All Rounds, and by supergame using data from Round 1 only, respectively.
[22] Figure 3 in the supplementary material shows mean expenditure by round, by treatment and session, for each of the empirical comparisons we make in this section.
[23] Tables 26 and 27 in the supplementary material indicate that our analysis with quadratic specifications is robust to using linear specifications instead. Table 8 in the supplementary material has regression estimates from a specification with only session fixed effects. As we discuss in detail in this section, this very simple specification fails to capture important experience effects.

Table 3 Average expenditure, by treatment

Supergames		WTA Low δ Indefinite	WTA High δ Indefinite	PP Low δ Indefinite	PP High δ Indefinite	WTA Low δ Finite	WTA High δ Finite	PP Low δ Finite	PP High δ Finite
All	*Mean*	53.91	33.48	57.87	39.83	53.23	40.42	53.32	49.06
	S.E.	(29.49)	(28.36)	(25.20)	(22.62)	(33.60)	(30.51)	(22.95)	(25.28)
	N	860	3240	860	3240	1200	3000	1200	3000
Last five	*Mean*	50.97	29.57	55.57	36.42	47.59	37.28	51.39	45.51
	S.E.	(28.96)	(24.89)	(24.20)	(20.39)	(32.09)	(27.79)	(20.67)	(23.49)
	N	400	1440	400	1440	600	1500	600	1500

The values in the rows labeled *Mean* are averages over all rounds and all supergames

expenditure occurs (Contests 1-15 in *Low δ-Indefinite* treatments, Contests 1-63 in *High δ-Indefinite* treatments). $HighDelta_i$ is an indicator variable equal to 1 if the participant is in a *High δ* treatment, and 0 otherwise. Finally, *Session* is a vector of session fixed effects (indicator variables for session).

Typically, *Supergame* is employed as a regressor that controls for experience in the analysis of repeated game experiments. We use *Contests* (i.e., the cumulative round or stage game, across supergames) instead. In our context, using a coefficient estimate on *Supergame* as a proxy for "experience" is potentially misleading because there were large contest experience differences both *between* treatments (51 contests compared to 14 contests, e.g.) and *within* treatments across sessions (48 compared to 63 contests, e.g.). For example, the first round of Supergame 3 was the fourth contest for some participants, but the nineteenth contest for others.[24] We also do not estimate regressions using only data from the second half of supergames (6–10) for the same reason.

To compare two treatments, we first estimate "intercept" coefficients for six sessions (three for each treatment). To generate an estimate of the *HighDelta* treatment effect (relative to *LowDelta*), we subtract the average of the three session effect estimates from the *HighDelta* treatment (Sessions 4, 5, and 6) from those for the *LowDelta* treatment (Sessions 1, 2, and 3):

$$\widehat{HighDelta} = \frac{\hat{\alpha}_4 + \hat{\alpha}_5 + \hat{\alpha}_6}{3} - \frac{\hat{\alpha}_1 + \hat{\alpha}_2 + \hat{\alpha}_3}{3} \tag{6}$$

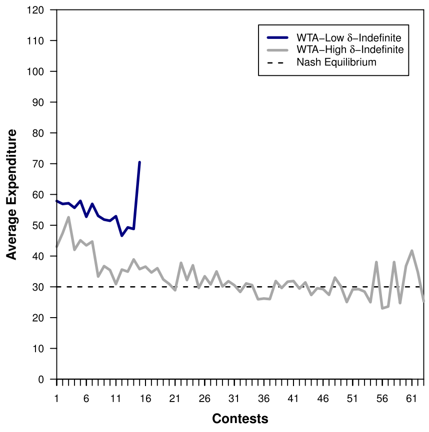
$\widehat{HighDelta}$ thus mirrors the coefficient estimate on a *HighDelta* treatment dummy in a specification with a constant term and a *HighDelta* treatment dummy. To consider initial (Round 1, Supergame 1) expenditure levels, the reader should examine this *HighDelta* estimate in Table 5. The other estimates in Table 5 are "raw" regression estimates. Due to our quadratic specification, we cannot report a constant estimate of

[24] We do present regression results with *Supergame* instead of *Contests* in the supplementary material. Our results are robust to using supergame (*Supergame*) instead of cumulative round (*Contests*).

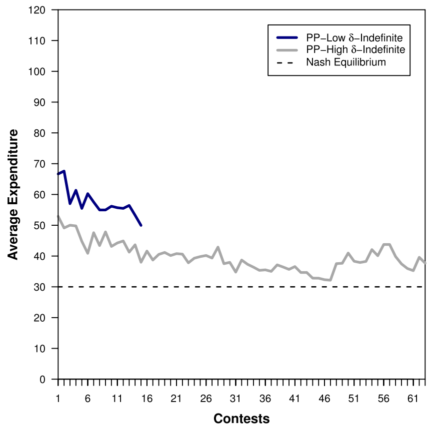
Table 4 Average expenditure by round, by treatment

Treatment	Round																			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
WTA-Low δ *-Indefinite*	53	57	51	44																
WTA-High δ *-Indefinite*	33	34	34	32	34	33	41	34	35	38	28	30	34	28	29	40	28	29	38	25
PP-Low δ *-Indefinite*	57	60	55	58																
PP-High δ *-Indefinite*	38	40	40	40	41	40	45	43	41	40	39	38	34	37	37	37	36	40	40	37
WTA-Low δ *-Finite*	53	53																		
WTA-High δ *-Finite*	40	39	41	40	41															
PP-Low δ *-Finite*	52	54																		
PP-High δ *-Finite*	49	48	49	49	51															

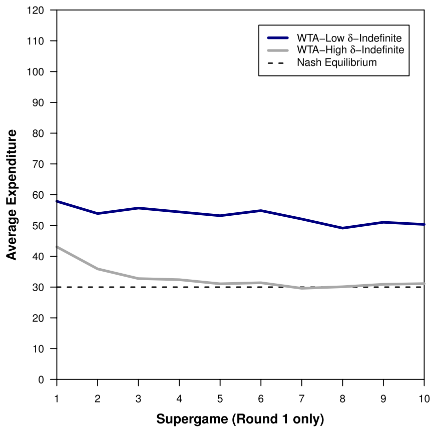
The values in the table are (rounded) averages by round (stage game), with the average taken over all supergames



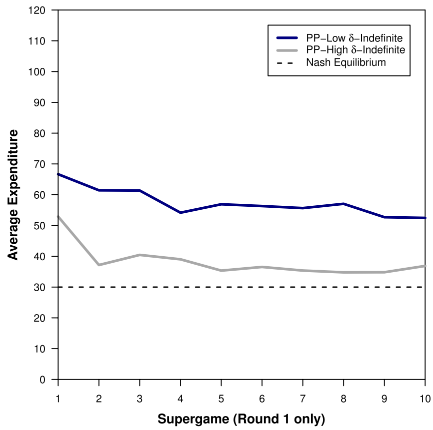
(a) *WTA-δ* Comparison



(b) *PP-δ* Comparison



(c) *WTA-δ* Comparison



(d) *PP-δ* Comparison

Fig. 1 Expenditure on time, δ comparisons

the effect of an additional contest (round) of experience on expenditure.[25] Because treatments that initially have different expenditure levels (their "intercept" estimates are significantly different) may converge with time (if their "slope" estimates differ), we report estimated expenditure differences as described in detail below.

Table 5 shows regression results from four models.[26] For each model, standard errors clustered at the session level are shown in parenthesis. As discussed above,

[25] The partial effect of an additional contest (round) of experience on expenditure is, itself, a function of *Contests*. For example, the estimated difference in the effect of an additional contest (round) of experience in the *HighDelta* treatment compared to the *LowDelta* treatment is $\hat{\beta}_2 + 2\hat{\beta}_4 Contests$.

[26] These results are robust to the inclusion of regressors capturing participant age, gender, and risk preferences. See the supplementary material for these robustness results.

Table 5 Regression results, δ comparisons

Expenditure	WTA-Indefinite		PP-Indefinite	
	All rounds	Round 1	All rounds	Round 1
	(1)	(2)	(3)	(4)
HighDelta	-14.15^{***}	-14.96^{**}	-16.83^{**}	-16.42^{**}
	(3.07)	(4.63)	(5.26)	(5.16)
Contests	−0.92	−0.79	−1.95	−1.59
	(0.97)	(1.06)	(1.04)	(1.22)
HighDelta × *Contests*	0.22	0.11	1.26	0.78
	(0.98)	(1.09)	(1.06)	(1.24)
$Contests^2$	0.02	0.02	0.07	0.04
	(0.07)	(0.09)	(0.05)	(0.07)
HighDelta × $Contests^2$	−0.01	−0.01	−0.06	−0.03
	(0.07)	(0.09)	(0.05)	(0.07)
R^2	0.67	0.73	0.79	0.81
Observations	4100	1200	4100	1200

Standard errors clustered at the session level. *HighDelta* is calculated from session effect estimates as described in the text. Significance levels: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

our specification allows for session "intercepts" and treatment "slopes." The estimates in Table 5 can be summarized as follows. For both contest settings (*WTA* and *PP*), initial expenditure is significantly lower in *High* δ relative to *Low* δ and the difference is meaningful in magnitude whether using data from All Rounds or Round 1 only. For the All Rounds estimates, initial expenditure in ECUs is 59.45 in *WTA-Low* δ*-Indefinite* versus 45.30 in *WTA-High* δ*-Indefinite*. Likewise, initial expenditure is 67.35 in *PP-Low* δ*-Indefinite* compared to 50.52 in *PP-High* δ*-Indefinite*.

The estimates in Table 5 correspond to the visuals in Fig. 1.[27] We note that by Supergame 5 of *WTA-High* δ*-Indefinite*, average expenditure converges to the stage game NE expenditure of 30 ECUs and stays at that level for the remainder of the experiment (see Fig. 1a). Expenditure also converges in *PP-High* δ*-Indefinite*, but to a level slightly above the stage game NE (see Fig. 1b).[28]

We can use the coefficient estimates in Table 5 to predict the expenditure level after X contests of experience, which is easier to examine and interpret than are the "raw" coefficient estimates in Table 5. Specifically, we calculate the following:

[27] They also match empirical cumulative density functions (CDFs) for expenditure. See Fig. 5 in the supplementary material.

[28] Figure 4 in the supplementary material plots "overbidding" (deviations from the stage game NE) directly. There is always significant overbidding in *WTA-Low* δ*-Indefinite* and *PP-Low* δ*-Indefinite*. After the first few rounds, there is no significant overbidding in *WTA-High* δ*-Indefinite*. In *PP-High* δ*-Indefinite*, overbidding converges to a level slightly above 0 ECUs.

$$(High\delta - Low\delta) \qquad \widehat{Expenditure}_t = \widehat{HighDelta} + \hat{\beta}_2 Contests_t + \hat{\beta}_4 Contests_t^2 \tag{7}$$

The estimated differences in expenditure across *High δ-Indefinite* and *Low δ-Indefinite* treatments using data from All Rounds are shown in Table 6 for the *WTA* sessions—see column (1)—and the *PP* sessions—see column (2). The related estimated expenditure differences using Round 1 data only are presented in Table 7.[29] Estimates in Tables 6 and 7 are only reported for Contests 1-20, because above Contests 14/15, estimates are already "out of sample" for the *Low δ-Indefinite* treatments.

According to our estimated model, average expenditure in both contest settings is lower in *High δ* than in *Low δ* for between 1 and 20 contests of experience. However, the statistical significance of these differences does trail off with more contests of experience. Table 7 shows the estimated expenditure differences using only Round 1 data (which should be interpreted as the estimated difference in Round 1 expenditure assuming that a supergame's Round 1 occurs after $X \in \{1, \ldots, 20\}$ contests of experience). Note that, relative to Table 6, the estimated differences are qualitatively similar.

We summarize our results pertaining to Hypothesis 1 in the following way. Our estimates of the difference in expenditure across otherwise-identical treatments with a continuation probability of $\delta = 0.8$ versus a continuation probability of $\delta = 0.5$ suggest that contest expenditure is lower in both *WTA* and *PP* contests with the larger continuation probability. However, there is evidence that contest experience matters; the number of contests of experience is highly relevant. Our data suggest that with experience in contests, the difference between expenditure in "high" continuation probability treatments and "low" continuation probability treatments narrows somewhat. We state this summary as our first result.[30]

Result 1 *When only a few contests have been played, average expenditure in indefinitely repeated WTA and PP contests is significantly lower with* $\delta = 0.8$ *than with* $\delta = 0.5$. *After a number of contests have been played, this difference in expenditure remains negative, but loses its statistical significance.*

In summary, we find qualified 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

[29] Tables 24 and 35 in the supplementary material show that the results from Tables 6 and 7 are robust to including participant-level controls for age, gender, and risk.

[30] The interested reader can find analysis comparing *High δ-Finite* contests to *Low δ-Finite* contests in the supplementary material, in Fig. 9 and in Tables 19 and 20.

Table 6 Estimated expenditure differences, by contests experienced

Contests Experienced	*Highδ − Lowδ*		*Indefinite − Finite*		*PP − WTA*		*PP − WTA*	
	WTA	PP	WTA	PP	High δ	Low δ	High δ	Low δ
	Indefinite	Indefinite	High δ	High δ	Finite	Finite	Indefinite	Indefinite
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	−13.94***	−15.64**	−8.53**	−8.45**	5.15**	0.47	5.23	6.93
2	−13.74***	−14.56**	−8.21**	−8.52**	5.55**	−1.00	5.24	6.06*
3	−13.57***	−13.61***	−7.91**	−8.59**	5.93**	−2.23*	5.26	5.30*
4	−13.41***	−12.79***	−7.63**	−8.65**	6.30***	−3.22***	5.28	4.66**
5	−13.27***	−12.09***	−7.36**	−8.71**	6.65***	−3.97***	5.30*	4.12*
6	−13.14***	−11.52***	−7.10***	−8.77***	6.99***	−4.49***	5.32*	3.70*
7	−13.04***	−11.07***	−6.86***	−8.83***	7.31***	−4.77***	5.34*	3.38*
8	−12.95***	−10.75***	−6.64***	−8.88***	7.62***	−4.81***	5.37**	3.17*
9	−12.88***	−10.55***	−6.43***	−8.94***	7.90***	−4.61***	5.40**	3.07*
10	−12.83***	−10.48***	−6.24***	−8.98***	8.18***	−4.17***	5.44**	3.09*
11	−12.80***	−10.53***	−6.07***	−9.03***	8.43***	−3.50***	5.47**	3.21*
12	−12.78***	−10.71***	−5.91***	−9.07***	8.67***	−2.59**	5.51***	3.44
13	−12.79***	−11.02***	−5.77***	−9.11***	8.90***	−1.44*	5.55***	3.78
14	−12.81**	−11.45***	−5.64***	−9.15***	9.10***	−0.06	5.59***	4.23
15	−12.85**	−12.00***	−5.53***	−9.19***	9.29***	1.57**	5.64***	4.79
16	−12.90*	−12.68***	−5.43***	−9.22***	9.47***	3.43***	5.68***	5.46
17	−12.98	−13.48**	−5.35***	−9.25***	9.63***	5.53***	5.73***	6.24
18	−13.07	−14.41**	−5.29***	−9.27***	9.77***	7.86***	5.79***	7.13
19	−13.18	−15.47**	−5.24***	−9.30***	9.90***	10.44***	5.84***	8.13
20	−13.31	−16.65*	−5.21***	−9.32***	10.01***	13.25***	5.90***	9.23

Estimating sample contains data from All Rounds. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

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.

As noted above, when testing “indefiniteness,” Round 1 results are crucial.[31] Visually, Figs. 2c and d indicate that average Round 1 expenditure is always lower in the *High δ-Indefinite* treatments relative to *High δ-Finite* treatments. When participants chose their expenditures in Round 1 of Supergame 10, the disparity in average experience across these two treatments was not too great: 51.7 contests (*Indefinite*)

[31] While we focus on Round 1 results in the main text, the reader may be curious about Round 5 expenditures—the last round in the *High δ-Finite* treatments. Figure 7a in the supplementary material shows there is no significant final round effect (increase in expenditure) in either *High δ-Finite* treatment. However, Figure 7b, which compares mean expenditure in Round 5 across *Indefinite* and *Finite* treatments, reveals that Round 5 expenditure was significantly lower in the *Indefinite* treatments relative to their *Finite* counterparts. This latter result is consistent with theory.

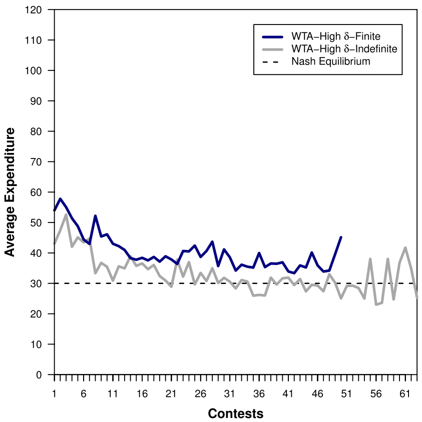
Table 7 Estimated round 1 expenditure differences, by contests experienced

Contests Experienced	*High*δ – *Low*δ		*Indefinite – Finite*		*PP – WTA*		*PP – WTA*	
	WTA	PP	WTA	PP	High δ	Low δ	High δ	Low δ
	Indefinite	Indefinite	High δ	High δ	Finite	Finite	Indefinite	Indefinite
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	−14.86**	−15.67**	−8.31*	−13.67**	12.27***	−0.03	6.91	7.72*
2	−14.78**	−14.98***	−8.16*	−13.41**	12.04***	−1.84	6.79	6.99*
3	−14.70**	−14.35***	−8.01*	−13.16**	11.81***	−3.35**	6.67	6.31**
4	−14.65***	−13.77***	−7.87*	−12.91***	11.59***	−4.58***	6.56	5.68**
5	−14.60***	−13.25***	−7.74**	−12.66***	11.37***	−5.51***	6.45	5.09*
6	−14.57***	−12.78***	−7.61**	−12.42***	11.15***	−6.16***	6.34	4.56*
7	−14.55***	−12.38***	−7.49**	−12.18***	10.94***	−6.52***	6.24*	4.06
8	−14.55***	−12.03***	−7.37**	−11.95***	10.72***	−6.59***	6.14*	3.62
9	−14.56***	−11.74***	−7.26**	−11.72***	10.51***	−6.37***	6.05*	3.23
10	−14.58***	−11.50***	−7.15**	−11.50***	10.31***	−5.86***	5.96*	2.88
11	−14.62***	−11.33***	−7.06***	−11.29***	10.10***	−5.07**	5.87**	2.58
12	−14.67***	−11.21***	−6.96***	−11.07***	9.90***	−3.98**	5.79**	2.33
13	−14.74***	−11.15***	−6.88***	−10.87***	9.70***	−2.61*	5.71**	2.12
14	−14.81**	−11.14***	−6.79***	−10.66***	9.50***	−0.94	5.64**	1.96
15	−14.90*	−11.19***	−6.72***	−10.47***	9.31***	1.01	5.56***	1.85
16	−15.01	−11.30**	−6.65***	−10.27***	9.12***	3.25***	5.50***	1.79
17	−15.13	−11.47**	−6.59***	−10.08***	8.93***	5.79***	5.43***	1.78
18	−15.26	−11.69*	−6.53***	−9.90***	8.75***	8.61***	5.37***	1.81
19	−15.40	−11.98	−6.48***	−9.72***	8.56***	11.71***	5.32***	1.89
20	−15.56	−12.32	−6.43***	−9.55***	8.38***	15.11***	5.27***	2.02

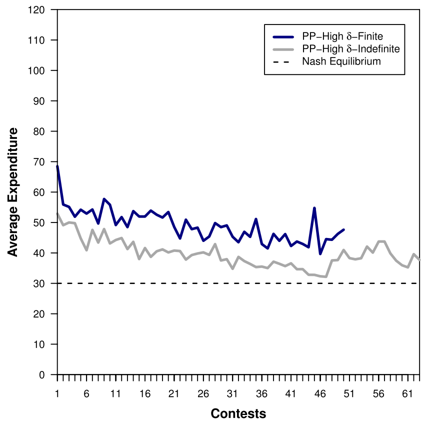
Estimating sample contains Round 1 data only. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

compared to 45.0 contests (*Finite*). Average expenditure in that round and supergame (in ECUs) was 31.13 in *WTA-High* δ*-Indefinite* and 35.90 in *WTA-High* δ *-Finite*. In *PP-High* δ*-Indefinite* average expenditure was 36.87 compared to 39.65 in *PP-High* δ*-Finite*.

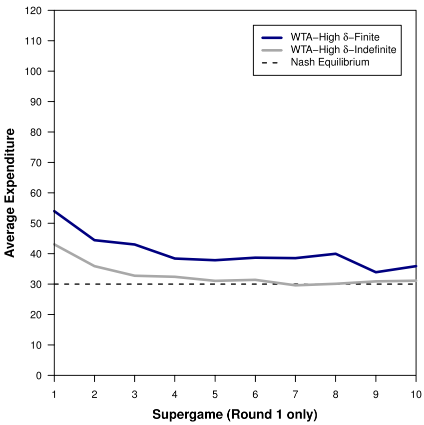
As before, we formally examine expenditure across treatments with regression analysis. Table 8 contains estimates from specifications that are analogous to Specification (5) with $HighDelta_i$ replaced by $Indefinite_i$, an indicator variable equal to 1 if the participant was in an *Indefinite* treatment, and 0 otherwise. We focus on the



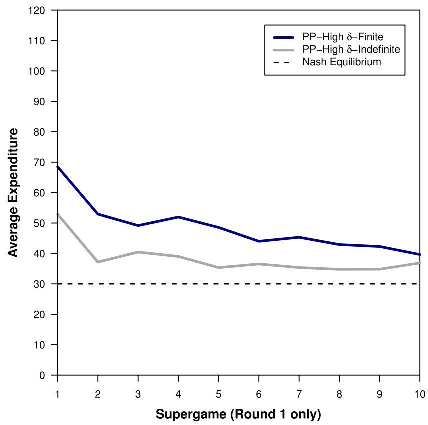
(a) *WTA-Indefinite* Comparison



(b) *PP-Indefinite* Comparison



(c) *WTA-Indefinite* Comparison



(d) *PP-Indefinite* Comparison

Fig. 2 Expenditure on time, *Indefinite* comparisons

Round 1 results, that is, columns (2) and (4) of Table 8. The "intercept" coefficient estimates indicate a significant difference in initial average expenditure across the two *WTA* treatments (43.12 versus 51.59) and also *PP-High δ-Indefinite* and *PP-High δ-Finite* (50.15 versus 64.09).

As we did for our δ comparisons, we use the raw coefficient estimates to calculate estimated expenditures for Contests 1-20. For Round 1 data, these are presented in columns (3) and (4) of Table 7. The estimated differences between *Indefinite* and *Finite* are negative and significant in each case, suggesting lower expenditure in the *Indefinite* treatments compared to the *Finite* treatments.[32] In addition, following several contests of experience, there were no significant differences in Round 1

[32] Our conclusions are robust to participant-level control variables or to using *Supergame* instead of *Round* to account for experience. Please see the supplementary material for robustness analysis.

Table 8 Regression results, *Indefinite* comparisons

Expenditure	WTA-High δ		PP-High δ	
	All Rounds	Round 1	All Rounds	Round 1
	(1)	(2)	(3)	(4)
Indefinite	-8.86^{**}	-8.47^{*}	-8.38^{**}	-13.94^{**}
	(2.70)	(4.00)	(3.13)	(4.10)
Contests	-1.04^{***}	-0.85^{***}	-0.61^{***}	-1.08^{***}
	(0.13)	(0.06)	(0.05)	(0.20)
Indefinite × *Contests*	0.34	0.16	−0.07	0.27
	(0.22)	(0.25)	(0.21)	(0.27)
Contests^2	0.01^{***}	0.01^{***}	0.01^{**}	0.01^{**}
	(0.00)	(0.00)	(0.00)	(0.00)
Indefinite × *Contests*^{2}	−0.01	−0.00	0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.65	0.65	0.78	0.77
Observations	6240	1200	6240	1200

Standard errors clustered at the session level. *Indefinite* is calculated from session effect estimates as described in the text. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

expenditure across *Low δ-Indefinite* and *Low δ-Finite* for either allocation rule. See Fig. 8 and Tables 17, 18, and 23 in the supplementary material for this analysis.

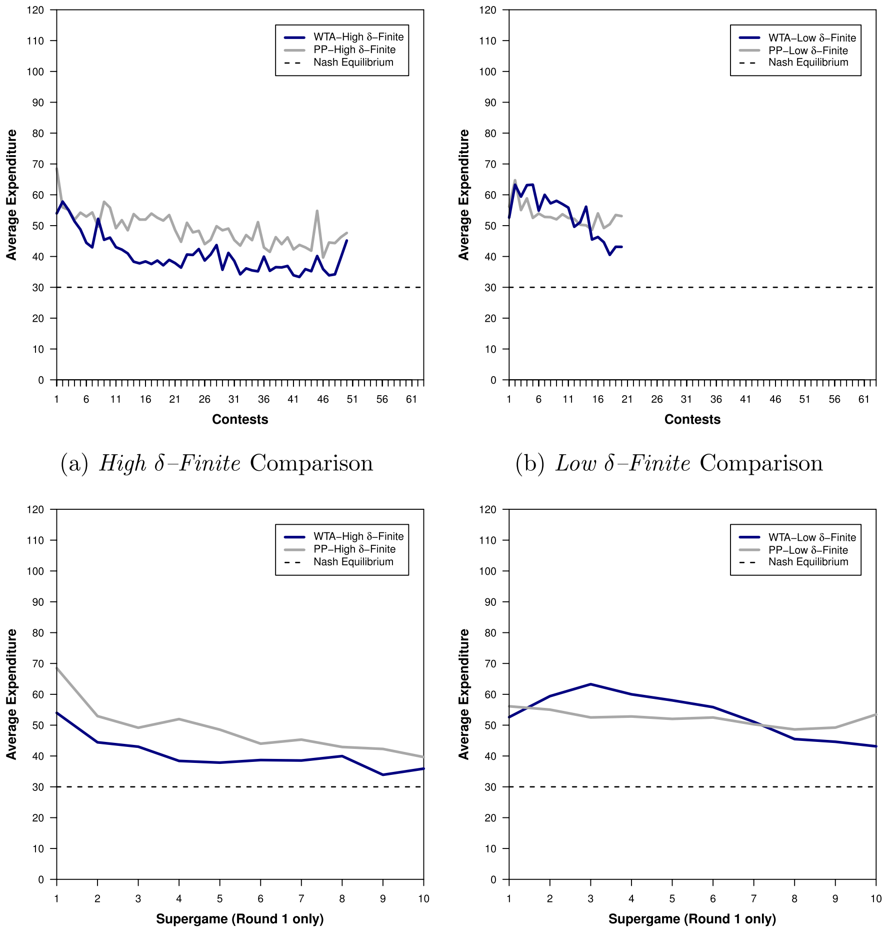
These conclusions motivate our second result concerning Hypothesis 2.

Result 2 *For both the WTA and the PP allocation rule, average expenditure is estimated to be significantly lower in indefinitely repeated contests than in finitely repeated contests of the same expected length in our High δ treatments. We find no significant differences in expenditure for either allocation rule in our Low δ treatments.*

We now examine differences across the *WTA* and *PP* settings (Hypothesis 3).

5.3 Does contest setting affect expenditure?

We report separate regression analysis for our *Finite* treatments and for our *Indefinite* treatments. In this section we will focus on analysis using All Rounds, but as the reader can readily verify from our tables, results are similar whether using data from All Rounds or from Round 1 only.



(a) *High δ–Finite* Comparison

(b) *Low δ–Finite* Comparison

(c) *High δ–Finite* Comparison

(d) *Low δ-Finite* Comparison

Fig. 3 Expenditure on time, *Finite* contest setting comparisons

5.3.1 Finitely repeated contests

Figure 3 shows comparisons across *WTA* and *PP* settings for finitely repeated contests. No differences in average expenditure are apparent in the two round, *Low* δ supergames. But Fig. 3a and c suggest that average expenditure may be higher in the *PP* setting compared to the *WTA* setting in the five round, *High* δ supergames. Table 9 contains coefficient estimates from ordinary least squares regressions. Our analysis in this section mirrors our earlier 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.

Table 9 Regression results, *Finite* contest setting comparisons

Expenditure	High δ-Finite		Low δ-Finite	
	All rounds	Round 1	All rounds	Round 1
	(1)	(2)	(3)	(4)
PP	4.73**	12.50***	2.17	2.06
	(1.74)	(2.91)	(2.02)	(3.15)
Contests	−1.04***	−0.85***	0.36	1.25
	(0.13)	(0.06)	(0.38)	(0.81)
PP × *Contests*	0.43**	−0.23	−1.82***	−2.24*
	(0.14)	(0.21)	(0.45)	(0.89)
Contests^2	0.01***	0.01***	−0.07***	−0.11**
	(0.00)	(0.00)	(0.02)	(0.04)
PP × *Contests*^{2}	−0.01**	0.00	0.12***	0.14**
	(0.00)	(0.00)	(0.02)	(0.04)
R^2	0.75	0.75	0.78	0.78
Observations	6000	1200	2400	1200

Standard errors clustered at the session level. *PP* is calculated from session effect estimates as described in the text. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

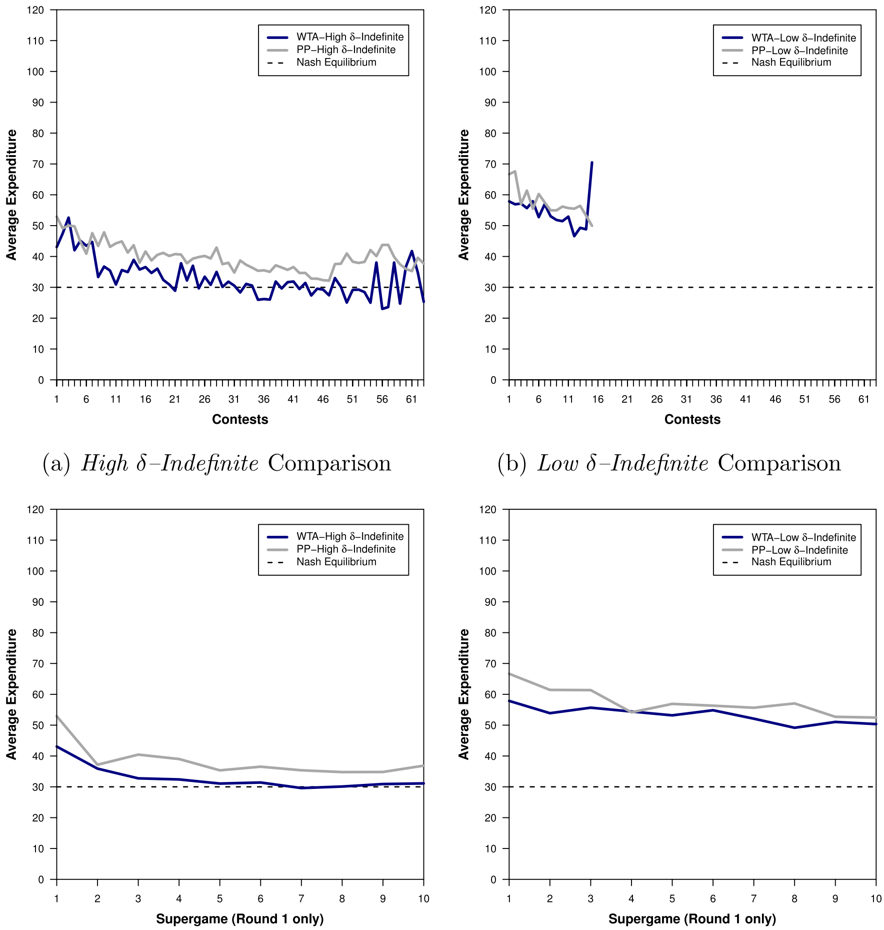
The initial expenditure, or "intercept" estimates for All Rounds are 54.17 for *WTA-High δ-Finite* and 58.90 for *PP-High δ-Finite*. For the *Low δ-Finite* treatments, this difference is 59.10 for *WTA* compared to 61.27 for *PP*. The Round 1 data estimates are similar. To more easily compare these estimates, consider columns (5) and (6) of Table 6, and note that the *High δ* estimates are far less ambiguous than the *Low δ* estimates.

To summarize our results on finitely repeated contests with different allocation rules, in the *High-δ* comparison, we find clear statistically significant differences across the *WTA* and *PP* rules. But in the *Low-δ* comparison, the expenditure difference flips from negative to positive with experience.[33] As such, we state the following result.

Result 3 *There is evidence that expenditure is greater in proportional-prize contests than in winner-take-all contests for finitely repeated contests, but only in longer treatments with more contests played.*

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

[33] These conclusions are robust to the inclusion of participant-level control variables or to using *Supergame* to account for experience. Please see the supplementary material. *Ex post* power analysis indicates an effect size of $d < 0.01$ (very small) for the *Low-δ-Finite* comparison. A sample size of 1,445,116 participants per treatment would be needed for 80% power (assuming $\alpha = 0.10$).



(a) *High* δ*–Indefinite* Comparison

(b) *Low* δ*–Indefinite* Comparison

(c) *High* δ*–Indefinite* Comparison

(d) *Low* δ*-Indefinite* Comparison

Fig. 4 Expenditure on time, *Indefinite* contest setting comparisons

5.3.2 Indefinitely repeated contests

Figure 4 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. Especially in the latter comparison, there appear to be either small differences or no difference in average expenditure across contest settings.

Table 10 shows regression results for different contest settings in indefinitely repeated contests, where the variable *PP* is as described above. In *High* δ*-Indefinite*,

Table 10 Regression results, *Indefinite* contest setting comparisons

Expenditure	High δ-Indefinite		Low δ-Indefinite	
	All rounds	Round 1	All rounds	Round 1
	(1)	(2)	(3)	(4)
PP	5.22	7.04	7.90	8.50
	(3.75)	(4.93)	(4.81)	(4.87)
Contests	−0.69**	−0.68**	−0.92	−0.79
	(0.18)	(0.24)	(0.97)	(1.06)
$PP \times Contests$	0.01	−0.13	−1.03	−0.80
	(0.27)	(0.30)	(1.42)	(1.62)
$Contests^2$	0.01*	0.01**	0.02	0.02
	(0.00)	(0.00)	(0.07)	(0.09)
$PP \times Contests^2$	0.00	0.00	0.05	0.02
	(0.00)	(0.00)	(0.09)	(0.11)
R^2	0.69	0.67	0.82	0.83
Observations	6480	1200	1720	1200

Standard errors clustered at the session level. *PP* is calculated from session effect estimates as described in the text. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

initial expenditure in ECUs is estimated at 45.30 in *WTA* contests and at 50.52 in *PP* contests. These expenditure levels are 59.45 in *WTA* contests and 67.35 in *PP* contests for the *Low δ-Indefinite* treatments.

Estimated expenditure differences for these contest setting comparisons using data from All Rounds are shown in columns (7) and (8) of Table 6. The *Low δ* differences always indicate greater relative expenditure in *PP* contests but are only sometimes statistically significant.[34] On the other hand, *PP* contests are estimated to have larger relative expenditure than *WTA* contests in *High δ*, and significantly higher relative expenditure for Contests 5-20. These conclusions are similar when examining data from Round 1 only (see Table 7).[35] We summarize these remarks with our final result.

Result 4 *There is evidence that expenditure is greater in proportional-prize contests than in winner-take-all contests for indefinitely repeated contests, but only in longer treatments with more contests played.*

We now discuss all of our results and conclude.

[34] *Ex post* power analysis indicates an effect size of $d = 0.14$ (small), and that 592 participants per treatment would be needed for 80% power (with $\alpha = 0.10$).

[35] They are also robust to controlling for participant-level variables or to using *Supergame* to account for experience. Please see the supplementary material.

6 Discussion and conclusion

Despite the recent surge in experimental contest research, no existing studies examine indefinitely repeated contests. Such contests are interesting in their own right because of their prevalence and importance 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 indefinite contests.

First, *Is cooperation increasing in the expected length of indefinite contest supergames*? There is a significant difference in average expenditure across both *WTA-High δ-Indefinite* and *WTA-Low δ-Indefinite* contests, and across *PP-High δ-Indefinite* and *PP-Low δ-Indefinite* contests. For both comparisons, we find significantly less expenditure in the *High δ* treatments, though the difference does taper off with experience. This result is robust to using the supergame as a control for experience instead of cumulative round (*Contests*), or to including participant-level controls in the analysis.

We consider our first result equivocal, since the statistical significance of the estimated differences attenuates with contest experience. As we discuss in Sect. 5.1, the "experience gap" between *Low δ* and *High δ* treatments presents an identification challenge. Researchers want to ensure that their results are due to δ (or to indefiniteness) as opposed to experience differences. Experience matters even in relatively simple games like the PD (Dal Bó and Fréchette 2018), and should matter even more in the relatively more complex contests we study. If participants need time to learn about the game they play, perhaps the standard indefinitely repeated experimental design—including ours—can be improved. For studying repeated games with a complex stage game, an improved design might involve many initial rounds of one-shot play, followed by a smaller number of indefinitely-repeated games. This design would aim to equalize experience *before* testing repeated game theory.[36]

Second, *Is there more cooperation in indefinite contest supergames compared to finite contest supergames*? We find significantly less expenditure both in *WTA-High δ-Indefinite* contests relative to *WTA-High δ-Finite* contests, and also across *PP-High δ-Indefinite* contests and *PP-High δ-Finite* contests. These conclusions hold up when participant-level controls are used, or when supergame is employed to control for experience.

Our third and final question is: *Is cooperation the same across indefinite winner-take-all and indefinite proportional-prize contests*? In our shorter treatments—whether *Finite* or *Indefinite*, there is either no significant difference in cooperation in the *PP* setting relative to the *WTA* setting, or the difference is significant but changes from negative to positive over time. On the other hand, in our longer treatments we do find that, after several rounds of experience, average expenditure in *PP-High δ*

[36] We came to this idea independently, but we wish to acknowledge discussions with Gabriele Camera and Guillaume Fréchette who expressed similar ideas related to this topic.

contests is significantly greater than average expenditure in *WTA-High δ* contests. These statements are robust to participant-level controls or to using supergame instead of the number of contests played to control for experience.

Our contest setting results contrast with the conclusions in Cason et al. (2018). They report higher average expenditure in *WTA* contests compared to *PP* contests. While we find either no expenditure differences across *WTA* and *PP* contests, or lower expenditures in *WTA* contests compared to *PP* contests, there are a number of important differences between our experiment and theirs. In particular, their contest success function has a random noise component and they use quadratic expenditure costs.

The coordination burden of cooperating (colluding) should be lower in *PP* contests than in *WTA* contests, because the *PP* prize is split deterministically according to expenditures. We do see evidence of greater coordination in *PP* contests. Participants in *PP* contests simultaneously select the same expenditure more frequently than their *WTA* counterparts, but this does not translate into lower average expenditures.[37] Thus, greater coordination in *PP* contests does not transfer into greater cooperation.

Given the all-or-nothing nature of *WTA* contests, one possible explanation for lower average expenditures in *WTA* contests is that participants take turns alternating between having the higher expenditure in a pair and the lower expenditure in a pair, across the rounds of a supergame. This strategy gives each participant a high likelihood of winning the prize in every other round, and it could imply low mean expenditures across both participants. However, we do not observe differences in the frequency of alternating expenditures across *WTA* and *PP* contests.[38]

Overbidding (relative to the non-cooperative expenditure) is a near omnipresent result in the experimental contest literature. With the caveat that most previous contest studies feature repeated, one-shot contests, our overbidding results are somewhat unique and quite interesting. In all of our contests, we find very significant downwards trends in overbidding over time (i.e., with experience). Moreover, we actually find no significant overbidding in the long run in *WTA-High δ* contests.

Solely on the basis of our *Low δ* data, one would be tempted to conclude that there is extreme overbidding in contests. However, over time our *High δ* contests all trend *towards* or *right to* the stage game Nash equilibrium. For this reason, our data suggest that future research on overbidding in contests should allow for many cumulative rounds of play. It is also interesting that we never observe average expenditures that are significantly below the stage game equilibrium level. Our data offer no evidence of cooperation (collusion) beyond—that is, below—the non-cooperative Nash level.[39]

[37] Table 28 in the supplementary material contains data on equal expenditure choices. In addition to *PP* contests having a greater percentage of equal expenditure than *WTA* contests, the percentage of equal expenditure is always greater in *High δ* contests than in *Low δ* contests (as would be expected).

[38] See Fig. 6 in the supplementary material.

[39] This contrasts somewhat with Cournot duopoly experiments (without communication). For example, Holt (1985) finds some evidence of collusion in indefinite Cournot duopolies, while Huck et al. (2004) observe some collusion in finitely repeated, one-shot Cournot duopolies.

This paper is one of the very first experimental examinations of indefinitely repeated games with "large" strategy spaces. Notably, Holt (1985) examines indefinitely repeated Cournot duopolies and Lugovskyy et al. (2017) investigate indefinite public goods games. The latter study finds less support for standard predictions from the theory of repeated social dilemmas than have experimental examinations of indefinite Prisoner's Dilemmas. In Lugovskyy et al. (2017), coarsening the strategy space leads to more first round cooperation in both finite and indefinite games, supporting the conjecture that the size of the strategy space matters. While our paper is generally supportive of the standard predictions of repeated game theory, our results highlight the critical role of experience in large strategy space games and clearly suggest the need for more empirical work on indefinite supergames with large strategy spaces.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10683-021-09703-0.

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