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Information feedback and contest structure in rent-seeking games

by

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10 September 2012

Abstract

We investigate the role of information feedback in rent-seeking games with two different contest structures. In the stochastic contest a contestant wins the entire rent with probability equal to her share of rent-seeking expenditures; in the deterministic contest she receives a share of the rent equal to her share of rent-seeking expenditures. Information feedback has very different effects depending on the contest structure. We observe the highest rent dissipation in stochastic contests when players only get feedback on own choices and earnings. In these contests aggregate expenditures usually exceed the value of the rent. We find that giving additional feedback about rivals' choices and earnings moderates average expenditures. In contrast, in deterministic contests average expenditures converge to equilibrium levels when subjects only get feedback about own choices and earnings. In these contests additional feedback about rivals' choices and earnings has the opposite effect of raising average expenditures.

Keywords: contests, rent-seeking, information, learning, imitation, experiments

JEL classification: C72; C92; D72

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1. Introduction

Tullock's (1980) seminal model of rent-seeking is widely used to model a variety of contests in economics and political science. For example, in a recent review Konrad (2009) discusses applications ranging from lobbying and patent races to litigation lawsuits and sporting contests. Typically, applications of the model use equilibrium analysis to examine how outcomes depend on underlying structural features. However, in numerous recent experiments the outcomes of Tullock contests diverge quite markedly from equilibrium predictions.

As we discuss in Section 2, laboratory rent-seeking expenditures typically exceed equilibrium levels, even when subjects have ample learning opportunities, and often exceed the value of the rent that is being sought. We note, however, that there is substantial variation in both design features and outcomes across various studies. In this paper we examine one hitherto neglected design feature: information feedback to contestants. In some previous experiments subjects are informed of the choices and earnings of all players after each contest; we refer to this design feature as 'full information'. In other studies subjects are informed only of own earnings ('own information'). Many studies use some form of partial information between these two extremes.

Our motivation for studying information feedback is that different forms of information feedback facilitate different kinds of learning, and in particular, as we discuss in Section 3, full information conditions allow subjects to employ imitative learning rules that can have sharp implications for the outcomes of Tullock contests. In other settings varying information feedback has been shown to significantly affect behavior in experiments, and several experimental oligopoly studies find that subjects adjust their decisions in a way consistent with imitative learning models (see Offerman et al. 1997, Huck et al. 1999, Huck et al. 2000, Apesteguia et al. 2007, Apesteguia et al. 2010).

We study two types of rent-seeking contest – a "deterministic" contest and a "stochastic" contest – and show that the way in which imitative learning affects outcomes depends crucially on contest structure. In the deterministic contest contestants compete for a rent and each receives a share of the rent equal to the share of rent-seeking expenditures. In this setting standard imitative learning dynamics imply that aggregate expenditures converge to full dissipation of the rent. The stochastic contest is the more commonly-used formulation where one contestant wins the entire rent, and each contestant's probability of winning is her expenditure divided by aggregate expenditures. In this setting imitative learning rules imply quite different dynamics whereby expected expenditures increase as long as individual expenditures do not exceed the rent.

We discuss our design in Section 4. In order to study learning dynamics in environments where imitative dynamics may take time to converge (if at all), we have participants play a sequence of 60 contests. This distinguishes our study from previous contest experiments, which have used shorter horizons. We use a simple 2x2 design varying information condition (full or own) and type of contest (deterministic or stochastic).

We present our results in Section 5. In deterministic contests expenditures start out at levels substantially exceeding equilibrium levels, but subjects learn to temper their rent-seeking expenditures with experience. In the own feedback treatment average expenditure levels in later periods are remarkably close to equilibrium levels, while in the full feedback treatment average expenditures are significantly higher – about 20% higher than equilibrium levels. Analysis of individual level data finds support for imitative learning to explain this difference between treatments. In stochastic contests the effect of information feedback is even more marked, and is *reversed*. With full feedback expenditures again begin at high levels and decrease with experience before stabilizing around 13% above equilibrium levels. When participants only get information about own payoffs expenditures begin high and remain high. Even in later periods average group expenditures are 67% above equilibrium levels and exceed the rent in the majority of games.

In Section 6 we discuss these findings in the light of related literature and offer concluding comments.

2. Related Rent-Seeking Experiments

Numerous experiments have been conducted using the framework of Tullock's (1980) rent-seeking model (for an extensive survey of these and related contest experiments see Dechenaux et al., 2012). These experiments usually consist of multiple periods where, in each period, participants take part in a simple version of a Tullock contest as follows. N contestants compete for a rent of size R. Each contestant i has an endowment of e and simultaneously chooses a level of rent-seeking expenditure $x_i \in [0, e]$. Let aggregate rent-seeking expenditures be denoted by X:

$X = \sum_{j=1}^{N} x_j$. The probability that contestant i wins the rent is her expenditure relative to aggregate expenditure, $p_i = x_i/X$. Thus, i's payoff function can be expressed as:

$$\pi_i = \begin{cases} e - x_i + R & \textit{with probability } p_i \\ e - x_i & \textit{otherwise} \end{cases}$$

Because an individual's payoff is random given the profile of expenditures, we refer to this as a stochastic contest structure. Assuming risk-neutrality, and that the endowment is non-binding, contestant i invests $x_i = R(N-1)/N^2$ in the unique equilibrium.

Substantial departures from this equilibrium prediction are often observed. For example Potters et al. (1998) found that over a thirty-period experiment in which groups of two played a contest in each period the average expenditures were 68% greater than the equilibrium prediction. Even focusing on the last ten periods expenditures were more than 50% above the equilibrium. In fact, the vast majority of studies find excessive expenditure relative to the risk-neutral equilibrium, sometimes more than double the equilibrium predictions (Fonseca, 2009, Abbink et al., 2010).[1] Although excessive rent-seeking is most commonly observed in experimental studies, expenditure levels as a percentage of equilibrium vary widely across studies even after controlling for differences in the number of contestants, the size of the rent, and other factors that affect equilibrium expenditures. Table 1 compares the results from experiments using the Tullock contest as described above.[2]

[1] A variety of potential explanations for deviations from equilibrium have been discussed in the literature. Risk aversion can account for departures from risk-neutral predictions. Konrad and Schlesinger (1997) show that, theoretically, risk aversion can either increase or decrease contest expenditures. However, empirical findings suggest more risk averse subjects spend less (e.g. Millner and Pratt, 1991), and so it is unlikely that risk aversion can account for the observed excess expenditure. Collusive behaviour might also create deviations from the equilibrium, although we would expect collusion to lead to lower than equilibrium expenditures. Herrmann and Orzen (2008) show that, theoretically, inequality aversion can lead to excessive expenditures, but patterns in their experimental data do not support this explanation, and in fact their subjects act as if they get additional utility from earning more than an opponent. They speculate that a "joy of winning" motive may explain excessive expenditures. Sheremeta (2010) introduces a method for measuring the joy of winning and finds support for this explanation. Some models of mistakes can also predict excessive expenditures. As shown by Lim et al. (2011), McKelvey and Palfrey's (1995) model of Quantal Response Equilibrium predicts excessive expenditures when the equilibrium expenditure is less than half the endowment (as is commonly the case in experiments).

[2] Many other studies are excluded that vary in more or less minor ways from that described above. For example, the pioneering studies of Millner and Pratt (1989, 1991) employ a design in which expenditures are made continuously during a period with real-time updating of information about all contestants' purchases, while Shogren and Baik (1991) use a design in which subjects receive an initial endowment to cover expenditures for the entire sequence of contests.

Study	Year	Treatment	N	e	R	Equilibrium group expenditure	Periods	Matching	Subjects	Expenditure as % of equilibrium	Expenditure as % of equilibrium (later periods)	Feedback
Potters et al.	1998	r=1	2	15	13	6.5	30	Random	66	**168.3**	150 (last 10)	Full
Schmitt et al.	2004	Static	2	15	12	6	5	Random	98	**175.7**		Full
Shupp	2004	low info	4	40	144	108	15	Random	12	**67.9**		Own
		high info	4	40	144	108	15	Random	24	**70.6**		Full
Herrmann and Orzen	2008	Direct, repeated	2	16	16	8	15	Random	46	**216.2**		Partial
Kong	2008	more loss averse	3	300	200	133.3	30	Fixed	30	**127.9**	135.5 (last 10)	Full
		less loss averse	3	300	200	133.3	30	Fixed	30	**156.2**	151.6 (last 10)	Full
Fonseca	2009	simultaneous – symmetric	2	300	200	100	30	Random	30	**200.2**	170.8 (last 10)	Full
Abbink et al.	2010	1:1	2	1000	1000	500	20	Fixed	28	**205.2**	179 (last 5)	Partial
Sheremeta	2010	Single	4	120	120	90	30	Random	84	**151.5**		Partial
Sheremeta and Zhang	2010	Individual	4	120	120	90	30	Random	36	**194.7**		Partial
Lim et al.	2011	2	2	1200	1000	500	10	Random	50	**130**		Full
		3	3	1200	1000	666	10	Random	39	**127.4**		Partial
		4	4	1200	1000	752	10	Random	52	**160.6**		Partial
		5	5	1200	1000	800	10	Random	50	**201.3**		Partial
		9	9	1200	1000	891	10	Random	54	**329.3**		Partial
Price and Sheremeta	2011	P	4	120	120	90	30	Random	48	**232**		Partial
Sheremeta	2011	GC	4	60	120	90	30	Random	48	**133.3**		Partial
		GC (40)	4	40	120	90	30	Random	12	**96**		Partial
		SC	2	60	60	30	30	Random	48	**131.3**		Full
Cason et al.	2012	Individual-NC	2	60	60	30	30	Fixed	16	**126.4**		Full
Faravelli and Stanca	2012	LOT	2	800	1600	400	20	Random	32	**110.2**	105.5 (last 5)	Own
Mago et al.	2012	NP-NI	4	80	80	60	20	Fixed	60	**194**		Own
		NP-I	4	80	80	60	20	Fixed	60	**188.7**		Full

Table 1. Summary of previous Tullock contest treatments

Note that the studies listed in Table 1 use a variety of forms of information feedback. We categorize feedback as "full" if participants are told, or can infer, the choices and earnings of all other group members at the end of the period. (Even within this category studies vary in the way feedback was given. For example, in some of the N=2 cases participants are given the effort of the rival and own earnings, from which they can infer the rival's earnings, whereas in other cases they are informed about earnings directly.) At the other extreme subjects are informed only of their own choices and earnings (e.g. Faravelli and Stanca, 2012). Most studies fall between these two extremes, giving different sorts of partial information; in many cases the experimenter reveals aggregate expenditure (e.g. Sheremeta, 2010) while in others information was not conveyed in numerical terms (e.g. Abbink et al., 2010).

It should also be noted that the studies in Table 1 vary considerably in numerous dimensions, making it difficult to disentangle the effect of information feedback from other factors that vary across studies. This is why we introduce a new design that varies information feedback conditions while holding other factors constant.[6]

In all of the above studies the contest winner earns the entire rent. An alternative version of a Tullock contest can be employed in which each contestant receives a share of the rent equal to her share of rent-seeking expenditures. Because an individual's payoff is completely determined by the profile of expenditures, we refer to this as a deterministic contest structure. In the deterministic contest i's payoff is given by

$$\pi_i = e - x_i + Rx_i/X.$$

This can be interpreted as a Tullock contest in which contestants are paid their expected earnings. Since stochastic and deterministic contests have the same expected payoff function equilibrium predictions (assuming risk-neutrality) are the same in both contests.

A small number of recent studies have examined deterministic contests. Schmidt et al. (2006) conduct one-shot contests and find no significant differences between stochastic and deterministic versions (although, somewhat unusually relative to other studies, expenditures are below equilibrium predictions). Chowdhury et al. (2012) also compare stochastic and deterministic contests where participants play over 30 periods against randomly changing opponents and are

[6] Information feedback *within contests* has been extensively studied in experiments on dynamic contests and tournaments (see the discussion in Dechenaux et al., 2012). Our focus is different since we study information feedback *between contests*. Mago et al. (2012) also vary between-contest feedback holding other variables constant. We discuss their experiment and how our results relate to theirs in Section 6.

informed of own earnings and aggregate group expenditure at the end of each period. They also find no significant difference between the two treatments.[7] Cason et al. (2010) implement a deterministic contest using a real effort task, although they do not study a comparable stochastic contest and it is difficult to compare efforts with equilibrium predictions without making restrictive assumptions about the effort cost function. Sheremeta, Masters and Cason (2012) compare 20-period stochastic and deterministic contests (albeit with a somewhat different contest structure than that defined above), giving feedback on own earnings and aggregate choices at the end of each period. In both contests they find excess expenditures relative to equilibrium, with expenditures significantly lower, and hence closer to equilibrium, in the deterministic contest.

None of these studies using deterministic contests have examined the effects of alternative information feedback. Information feedback determines the extent to which individuals can employ different learning rules. In the next section we show that imitative learning, which requires information feedback on others' choices and earnings, has important implications for deterministic contests. In addition, we show that the implications for deterministic and stochastic contests are very different.

3. Imitation Theory and Experiments

Why should information feedback make any difference? One reason is that different sorts of information feedback may facilitate different sorts of learning. It is very unlikely that participants in an experiment will calculate the equilibrium of a game and use the equilibrium strategy from the outset. Instead, participants are more likely to follow boundedly rational decision processes that draw on the information they receive about past choices and associated payoffs. When information about others' choices and payoffs is available, participants may employ learning rules that condition on this, such as imitating successful contestants, while absent this information these learning rules cannot be used.

Evidence of imitative behavior is found in a number of studies based on Cournot oligopoly settings (Offerman et al. 1997, Huck et al. 1999, Huck et al. 2000, Apesteguia et al. 2007, Apesteguia et al. 2010). These studies were motivated by Vega-Redondo's (1997) theoretical result that, in a dynamic market where agents can observe competitors choices and payoffs, the tendency to imitate the most successful agents leads to convergence to the Walrasian

[7] They do find significant differences when the cost functions are convex. In this case the stochastic contest results in excess expenditures relative to equilibrium, while the deterministic contest results are closer to equilibrium.

outcome. Although experimental outcomes usually do not converge to the Walrasian outcome, there is evidence that outcomes move in the direction of the Walrasian outcome when subjects are given information about the choices and earnings of opponents. That is, aggregate output increases when subjects are informed of opponents' choices and earnings in the previous period, compared to the case where they are only informed of aggregate output in the previous period.

Imitating successful others has important implications in our rent-seeking contests. For the deterministic contest the payoff function can be rewritten as

$$\pi_i = e + \frac{x_i}{X}(R - X).$$

From this it is easily seen that if the rent is less than fully dissipated ($R - X > 0$) the contestant who invests the most has the highest payoff, while if the rent is over-dissipated ($R - X < 0$) the contestant who invests the least has the highest payoff. Thus, if contestants imitate the contestant who received the highest payoff, choices will lock-in on the highest (lowest) initial choice if initial group expenditure is less than (more than) the rent. Imitation dynamics that include a small perturbation about imitating the best converge to full-dissipation: $X = R$. For example, suppose (as will be the case in our experiment) that three contestants compete for a rent of 1000. Let x_{it} denote contestant *i*'s expenditure in period *t*, and suppose $x_{it} \in \{0, 1, \dots, 1000\}$ (also as will be the case in our experiment). Let initial choices be independent uniform draws from $\{0, 1, \dots, 1000\}$ and let

$$x_{it} = \begin{cases} 1000 & if \quad x'_{it} > 1000 \\ x'_{it} & if \quad 0 \le x'_{it} \le 1000 \\ 0 & if \quad x'_{it} < 0 \end{cases}$$

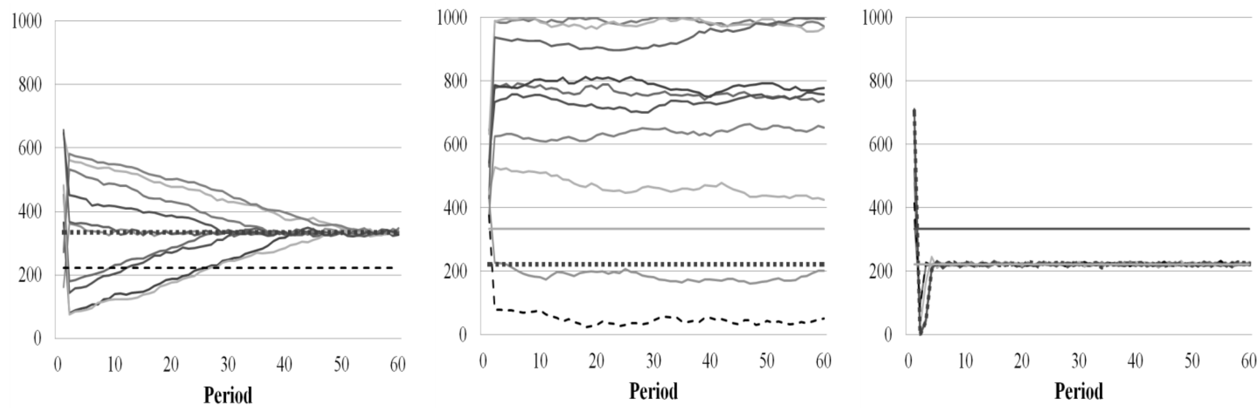
where $x'_{it} = x^*_{t-1} + \varepsilon_{it}$, x^*_{t-1} is the choice in the previous period that received the highest payoff and ε_{it} are independent uniform draws from $\{-10, -9, \dots, 10\}$.[8] Figure 1 (panel a) shows the results of a simulation with ten groups.

Imitation dynamics in a stochastic contest are very different. In this case the contestant who receives the highest payoff is the winner of the rent.[9] If contestants imitate the choice that led to the highest payoff in the previous period, then expenditures lock-in on the expenditure of the initial winner. If initial choices have an average expenditure of $\bar{x}_0$ and a variance of σ_0^2, the

[8] That is $x^*_{t-1} = \max\{x_{1\,t-1}, x_{2\,t-1}, x_{3\,t-1}\}$ if $x_{1\,t-1} + x_{2\,t-1} + x_{3\,t-1} < 1000$ or $x^*_{t-1} = \min\{x_{1\,t-1}, x_{2\,t-1}, x_{3\,t-1}\}$ if $x_{1\,t-1} + x_{2\,t-1} + x_{3\,t-1} > 1000$.

[9] More precisely, this is the case as long as individual expenditures do not exceed the rent. As long as $x_i \le R$ it follows that the winner's payoff is $e - x_i + R \ge e$, and a loser's payoff is $e - x_i \le e$.

expected choice next period can be shown to be $\bar{x}_0 + \sigma_0^2/\bar{x}_0$.[10] Thus, in expectation, expenditures lock-in at a higher level than the initial average. If the imitation dynamic includes a small perturbation, as before, then the dynamic process resembles a random walk with upward drift. However, if all contestants imitate the winner, variability is low and the adaptive process is very slow. Figure 1 (panel b) shows a ten-group simulation.



(a) Deterministic-Imitation (b) Stochastic-Imitation (c) Best Reply

Figure 1. Simulated contest expenditures: Imitation and Best Reply dynamics. Each panel displays expenditure per group member for ten groups of three contestants. Equilibrium (dash line) and Full Dissipation (dotted line) expenditures per group member also shown.

Alternative dynamic processes exhibit different patterns. Figure 1 (panel c) shows a simulation of ten groups following a best reply adjustment process. The process is identical to the imitation process described above except that now x^*_{t-1} is the best response to the opponents' choices in the previous period (and so may vary across contestants). In both deterministic and stochastic contest settings the best reply dynamic converges on the equilibrium.[11]

4. Experimental Design and Procedures

The experiment consisted of eight sessions with either 15 or 18 subjects each. Sessions were conducted at the University of Nottingham in December 2011 using the software z-tree (Fischbacher, 2007). We recruited 123 students from a wide range of disciplines through the online recruiting system ORSEE (Greiner 2004) and no participant took part in more than one session. None of the participants had taken part in previous contest experiments.

[10] Formally, $E(x_{it}|x_{1\,t-1},\dots,x_{n\,t-1}) = x_{1\,t-1}\frac{x_{1\,t-1}}{X_{t-1}} + \cdots + x_{n\,t-1}\frac{x_{n\,t-1}}{X_{t-1}} = \frac{1}{n\bar{x}_{t-1}}\sum_{i=1}^{n} x_{i\,t-1}^2 = \bar{x}_{t-1} + \frac{\sigma_{t-1}^2}{\bar{x}_{t-1}} \geq \bar{x}_{t-1}$.

[11] In fact with risk-neutrality best replies, and hence best reply dynamics, are identical for stochastic and deterministic contests.

At the beginning of each session participants were randomly matched into groups of three that remained the same for the whole experiment. Participants did not know the identities of the other subjects in the room with whom they were grouped. They were given instructions for the experiment (reproduced in Appendix A) and these were read aloud by the experimenter. Any questions were answered by the experimenter in private, and no communication between participants was allowed. No information passed across groups during the entire session.

We used a 2x2 design where our four treatments differed by the contest payoff function (DETERMINISTIC or STOCHASTIC) and the information provided to subjects at the end of each period (OWN or FULL). We conducted two sessions with each treatment, resulting in 33 observations on eleven independent groups in the FULL-DETERMINISTIC treatment and ten independent groups in each of the other treatments.

In all sessions the decision-making part of the experiment consisted of 60 periods. In each period subjects were endowed with 1000 points and competed for a prize of 1000 points. Subjects simultaneously chose how many contest tokens to purchase, at a price of one point per contest token, and any points not used to purchase tokens were added to their total balance. At the end of the period each subject also received contest earnings which were added to their total balance. In the deterministic contest each subject received a share of the prize in accordance with their relative token expenditures, while in the stochastic contest one subject per group won the entire prize.[12] With these parameters and assuming risk-neutrality equilibrium group (individual) expenditure is approximately 667 (222) points in both contests.

At the end of each period subjects in the OWN information treatments were reminded of their own choice and informed of their own earnings. In the FULL information treatments subjects were additionally informed about the choices and earnings of the other two members of the group to which they belong. Those were listed according to contest tokens purchased in descending order. Subjects could recognize their choices in the screen by the label “OWN”, while information about the other participants were labeled as “OTHER”. This was done to prevent the possibility of tracking the choice of a particular member of the group.[13]

Subjects accumulated points across the 60 periods and at the end of each session were paid 0.015 pence per point. Earnings averaged £9.40 for a session lasting about 60 minutes.

[12] If none of the subjects bought any tokens the prize was not shared or assigned.
[13] Screenshots of the feedback screens are included in the instructions, reproduced in Appendix A.

5. Results

5.1 Deterministic Contests

We begin with an analysis of results from our DETERMINISTIC treatments. Figure 2 shows the average group expenditures across periods. In both treatments expenditures decrease from initially high levels. The decrease is particularly marked in the first half of the session, while average expenditures are more stable in the second half. Comparing expenditures in periods 1-30 with 31-60 we see a significant decrease in the OWN (p=0.009) but not in the FULL (p=0.286) information treatments.[14] However, comparing periods 31-45 with 46-60 we fail to find significant differences in either treatment (FULL: p=0.328, OWN: p=0.575), supporting the observation that expenditures are stable within the second half of the experiment.

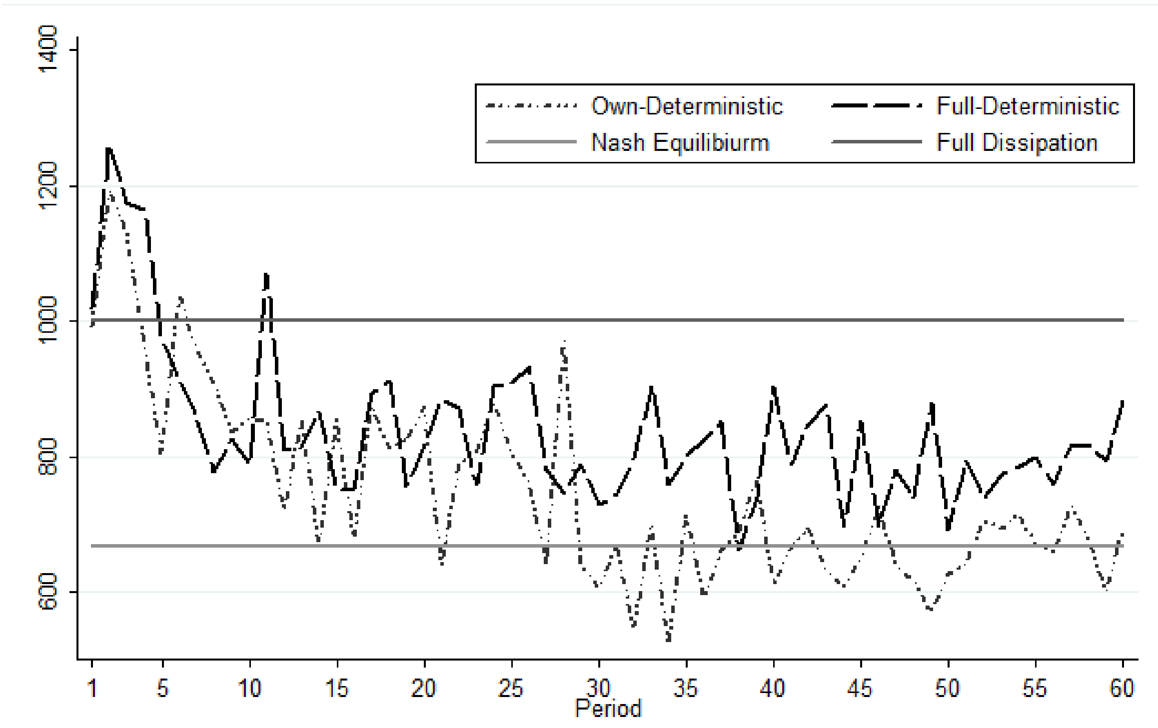


Figure 2. Average group expenditures in Deterministic treatments

Table 2 summarizes average group expenditures. Taking all periods together, group expenditure is lower with OWN than with FULL information, although the difference is not significant (p=0.121). However, if we consider only the last 30 periods, the difference is significant (p=0.024). While the average group expenditure of 794 in FULL is 20% higher than

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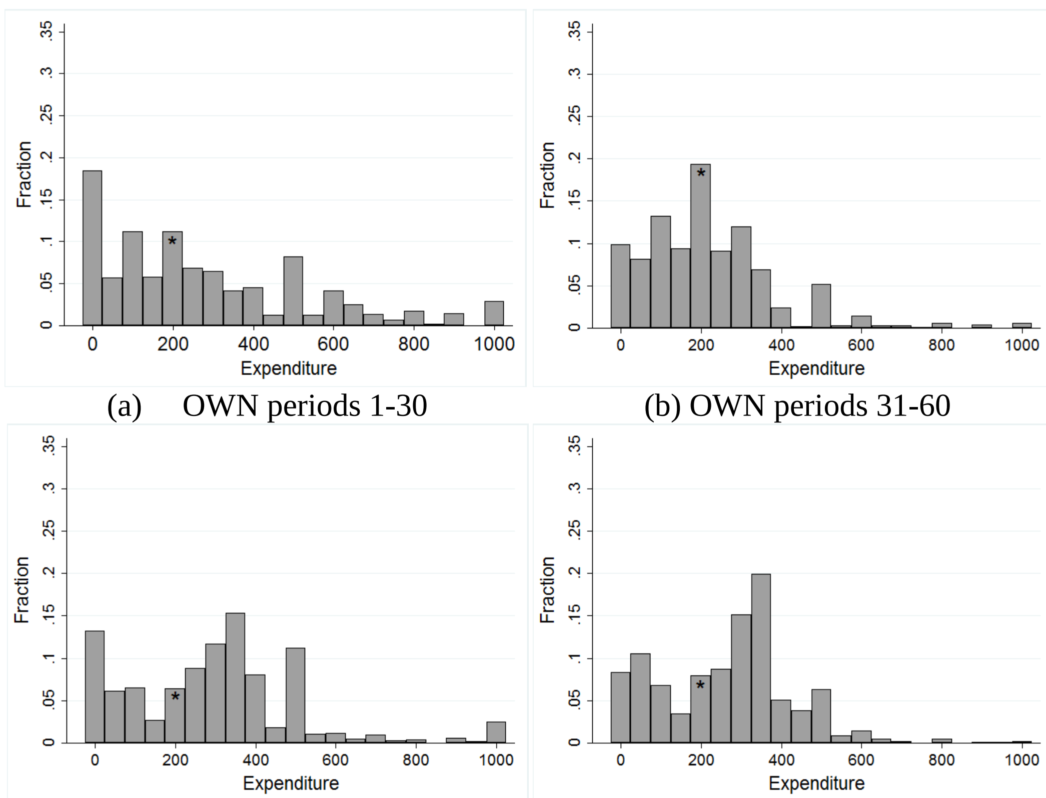
[14] Unless otherwise noted within-group comparisons are based on two-sided Wilcoxon matched-pairs signed-rank tests and between-group comparisons are based on two-sided Wilcoxon rank-sum tests, in both cases treating each group as a single independent observation. Raw group data are reported in Appendix B.

the equilibrium prediction, the average group expenditure of 657 in OWN is remarkably close to the equilibrium level.[15]

Average Expenditures	OWN	FULL	Difference	p-value
Overall	749.26	838.66	-89.4	0.121
Period 1-30	841.79	883.79	-42	0.481
Period 31-60	656.73	793.54	-136.81	0.024

Table 2. Average group expenditure in Deterministic treatments

A closer look at the distribution of the choices reveals more information about changes in behavior over time and across treatments. In Figure 3, for each treatment, we compare the distribution of choices in the first and second half of the experiment.



(a) OWN periods 1-30 (b) OWN periods 31-60

(c) FULL periods 1-30 (d) FULL periods 31-60

Figure 3. Distributions of individual expenditures in Deterministic treatments.
Intervals containing Nash Equilibrium indicated by asterisks.

[15] Note however, that we do not observe convergence to the equilibrium at the individual group level, and in fact there is substantial dispersion in expenditures within groups. Taking the difference between the highest and lowest expenditure in a group in a period as a measure of dispersion, dispersion averaged across all groups and periods is 363.22 in OWN, and significantly lower, 249.48, in FULL (p = 0.067). Appendix B reports the raw group data.

In the first half of the experiment choices in the OWN information treatment are widely dispersed with a mode at the lowest expenditure interval (panel a). In the second half the distribution shifts with a mode at the equilibrium interval (panel b). In the FULL information treatment the distributions in the two halves are more similar. Note however the differences between panels b and d. In the second half of the experiment choices in FULL are mainly above equilibrium while in OWN choices are more symmetrically distributed about the mode at the equilibrium. The difference between panel b and d is qualitatively consistent with the hypothesis of imitative learning.

This result resembles findings from Cournot experiments discussed earlier in section 3. To further examine how different learning rules drive behavior changes we follow Huck et al. (1999). They estimate how adjustments in individual behavior depend on the adjustments that would be required to i) imitate the best, ii) best respond, and iii) imitate the average. The most general adjustment model is as follows:

$$x_{i\,t} - x_{i\,t-1} = \alpha + \beta(x^{*}_{i\,t} - x_{i\,t-1}) + \lambda\left(x^{B}_{i\,t} - x_{i\,t-1}\right) + \gamma\left(x^{A}_{i\,t} - x_{i\,t-1}\right) + \varepsilon_{i\,t}$$

where $x_{i\,t-1}$ and $x_{i\,t}$ are the expenditures of subject i in the previous and current period, $x^{*}_{i\,t}$ is subject i's best response to rivals' expenditures in t – 1, $x^{B}_{i\,t}$ is the expenditure of group member with the highest payoff in t – 1, and $x^{A}_{i\,t}$ is the average expenditure of rivals in t – 1. This model is estimated when subjects have sufficient information to calculate the relevant regressors. When the information feedback does not allow subjects to calculate a regressor that regressor is dropped from the estimation. In Table 3 we report OLS estimates using data from the last 30 periods. In all regressions we use standard errors clustered at the group-level.

	Coefficient (standard error)			
	Constant	**Best Response**	**Imitate the Best**	**Imitate the Average**
FULL	6.23	0.19***	0.27***	0.06**
(n = 33, T = 30)	(12.41)	(0.06)	(0.05)	(0.03)
OWN	7.40	0.45***		0.04
(n = 30, T = 30)	(7.34)	(0.06)		(0.05)

Table 3. Adjustment Model Estimates for Deterministic treatments.
* significant at 10%; ** significant at 5%; *** significant at 1%

For the FULL information treatment we find that all regressors are significant, but the largest coefficient is that on "imitate-the-best". Thus, when subjects can imitate successful rivals there is a significant tendency to do so. For the OWN information treatment subjects cannot imitate the best (although in principle they can infer the average choice of others and the best response) and so we omit the imitate-the-best variable from the regression. Here, estimation results show a stronger effect of best response learning, while the coefficient of imitate-the-average is not significant.[16]

5.2 Stochastic Contests

Figure 4 shows expenditures across periods in the STOCHASTIC treatments. In both treatments expenditure levels are high in early periods. Expenditures in the FULL-STOCHASTIC treatment then exhibit a decreasing trend: expenditures in periods 31-60 are significantly lower than in periods 1-30 ($p = 0.022$). In contrast, the OWN-STOCHASTIC treatment does not show any decreasing trend: the difference in expenditures between the two halves is insignificant ($p = 0.575$). Expenditure levels are stable in the second half of both treatments.[17]

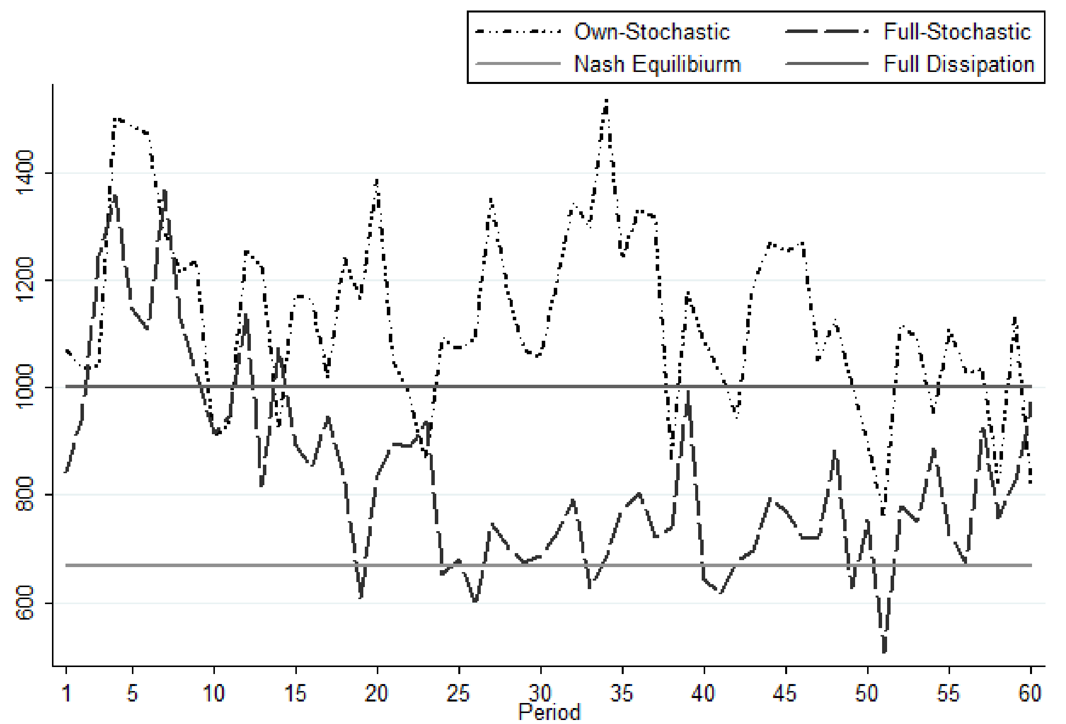


Figure 4. Average group expenditures in Stochastic treatments

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[16] Including the imitate-the-best regressor in the adjustment model for the OWN information treatment does not affect the results, and the coefficient is not significant.

[17] Expenditures in periods 31-45 and 46-60 do not differ significantly in either FULL ($p=0.878$) or OWN ($p=0.114$).

Table 4 summarizes average group expenditures. Group expenditures are significantly higher in the OWN than FULL information treatment, based on either all periods or the early or later periods separately.[18] Across all periods the average expenditure in the FULL information treatment falls from the initially high levels to a level about 13% above equilibrium in the second half of the experiment. In contrast, expenditures in the OWN information treatment remain higher than the value of the prize even in later periods. The difference between the two treatments is substantial: expenditures in OWN are 26% higher than in FULL in the first 30 periods and 48% higher in the last 30 periods.[19]

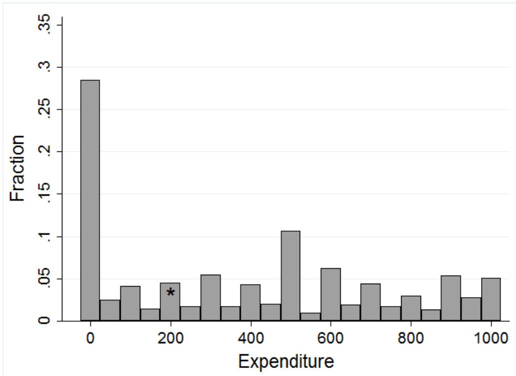
Average Expenditures	OWN	FULL	Difference	p-value
Overall	1131.03	834.22	296.81	0.041
Period 1-30	1151.90	916.14	235.76	0.041
Period 31-60	1110.17	752.30	357.87	0.023

Table 4. Average group expenditure in Stochastic treatments

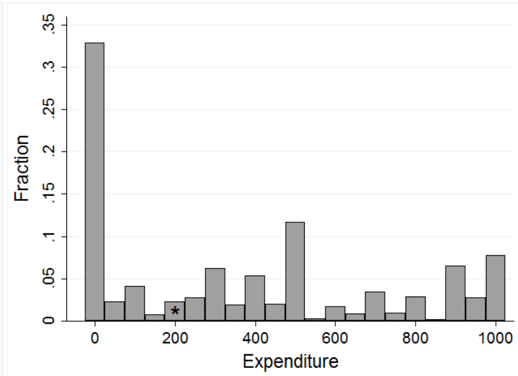
Figure 5 shows the distributions of individual choices in the STOCHASTIC treatments. The upper panels show the OWN treatment and the distributions are similar in earlier and later periods. There is a pronounced mode at the lowest expenditure interval and a less pronounced one in the interval containing 500. There are also a non-negligible number of choices in the 900-1000 range. The distribution of choices in the first thirty periods of the FULL-STOCHASTIC treatment (panel c) is similar to that in previous experiments (e.g. Sheremeta 2010, Lim et al. 2011 and Chowdhury et al. 2012). In the second half there are lower frequencies of choices at the extreme intervals of the strategy space, and somewhat more choices in the 50-350 range.

[18] There is also a clear treatment effect in terms of dispersion. As for the deterministic treatments, within-group dispersion of expenditures is significantly lower in FULL-STOCHASTIC, where it averages 402.85, compared to OWN-STOCHASTIC, where it averages 628.19 (p = 0.002).

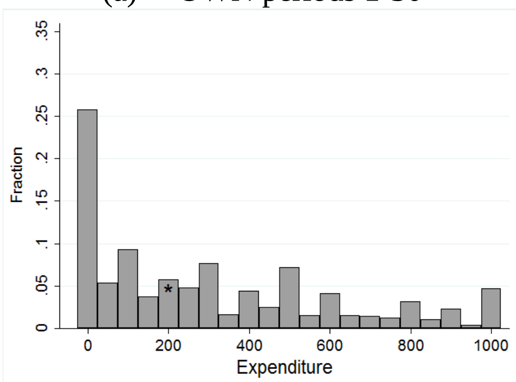
[19] It is also interesting to compare stochastic and deterministic contests in a given information condition. Expenditures are significantly higher in OWN-STOCHASTIC than OWN-DETERMINISTIC (periods 1-30: p = 0.004; periods 31-60: p = 0.001), but expenditures in FULL-STOCHASTIC and FULL-DETERMINISTIC are not significantly different (periods 1-30: p = 0.833; periods 31-60: p = 0.778). The latter result contrasts with Sheremeta, Masters and Cason (2012) who report substantial differences in rent dissipation between stochastic and deterministic contests with full information feedback. Note, however, that in addition to numerous other design differences, their results are based on a twenty-period experiment whereas ours is based on sixty periods. In fact, in the first twenty periods of our experiment we also observe substantially higher dissipation rates in our FULL-STOCHASTIC treatment (150% of equilibrium levels) compared to our FULL-DETERMINISTIC treatment (136% of equilibrium levels), although this difference is not significant in our data.



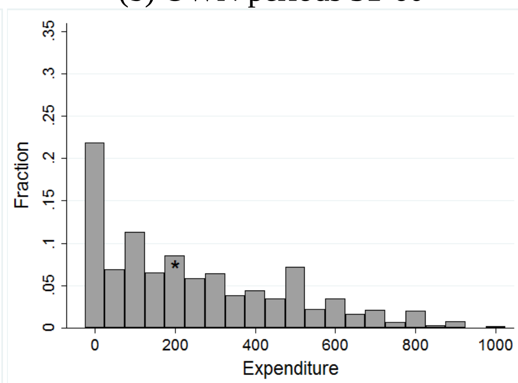
(a) OWN periods 1-30



(b) OWN periods 31-60



(c) FULL periods 1-30



(d) FULL periods 31-60

Figure 5. Distributions of individual expenditures in Stochastic treatments.
Intervals containing Nash Equilibrium indicated by asterisks.

In Table 5 we report estimates of the adjustment model described in the previous sub-section for the FULL-STOCHASTIC treatment, again based on the last 30 periods of data.[20] Note that now imitating the contestant who earned the most in the previous period means imitating the winner of the contest and so in the imitate-the-best regressor $x_{i\,t}^{B}$ denotes the expenditure of the contestant who won the prize in the previous period. Also, since subjects were informed of all choices in the previous period they could, in principle, calculate the expected earnings of each, and so another possibility is that subjects imitate the choice from the previous period that implied the highest *expected* earnings. Thus, we included another regressor representing the choice in the previous period that received the highest expected payoff. We refer to this as the imitate-the-expected-best learning rule. The results in Table 5 show that although the coefficient on imitate-the-best is significant, it is small in magnitude relative to the coefficients from the DETERMINISTIC treatments. Moreover, it is small in magnitude relative

[20] We did not estimate the model for the OWN-STOCHASTIC treatment since subjects could not observe any of the variables.

to the coefficient on the best response regressor. Thus, in our stochastic contest setting best response learning plays a more important role than imitative learning.

	Coefficient (standard error)				
	Constant	**Best Response**	**Imitate the Best**	**Imitate the Average**	**Imitate the (expected) Best**
FULL	9.48	0.37***	0.18***	0.08*	0.02
(n = 30, T = 30)	(20.01)	(0.07)	(0.05)	(0.04)	(0.05)

Table 5. Adjustment Model Estimates for Stochastic treatment.
* significant at 10%; ** significant at 5%; *** significant at 1%

5.3 Implications for rent-dissipation

Our results show that information feedback has a significant effect on behavior in rent-seeking contests. Contestants adjust their choices based on what they observe about the choices and earnings of others in previous periods. However, adjustment patterns vary across the different contest settings. The implications of this for rent-dissipation in the last thirty periods are summarized in Table 6. Average expenditure levels vary considerably across treatments. Expenditures are lowest, equal to 98% of the Nash Equilibrium level, in the OWN-DETERMINISTIC treatment and highest, 166% of Nash equilibrium level, in the OWN-STOCHASTIC treatment, with the expenditures of the two FULL treatments in between.

Treatment	**Expenditure as % of equilibrium expenditure**	**% of contests with group expenditure exceeding the rent**	**% of subjects earning less than their endowment**
OWN-DETERMINISTIC	98	6	0
FULL-DETERMINISTIC	119	23	12
OWN-STOCHASTIC	166	59	70
FULL-STOCHASTIC	113	26	27

Table 6. Implications for rent-dissipation. All percentages based on last 30 periods.

Revealing information about opponents’ choices increases rent-seeking expenditures in deterministic contests, but mitigates over-expenditure in stochastic contests. Remarkably, of the contests played in OWN-STOCHASTIC in the last thirty periods, 59% of them ended up with aggregate expenditures exceeding the rent. Thus, most contests in this treatment led to more than full-dissipation of the rent. By comparison, this happened only 6% of the time in the OWN-DETERMINISTIC treatment. As a consequence of excessive rent-seeking, in the OWN-STOCHASTIC treatment 70% of subjects earned less than their endowment. Relative to spending zero and earning their endowment, they consistently made losses throughout the experiment.

6. Discussion and Conclusion

In our experiment we find that information feedback has very different effects depending on the type of rent-seeking contest. In deterministic contests our results nicely complement those from oligopoly experiments, where feedback on the choices and earnings of others facilitates imitative learning, and in our setting leads to higher rent-seeking expenditures. Our deterministic treatments can be compared with two of the treatments used by Huck et al. (1999) to analyze learning in Cournot triopolies: their BEST (similar to our OWN) and FULL treatments. Consistent with their results, we find that revealing information about opponents' choices and earnings leads to more competitive behavior.

In stochastic contests, however, we find that this result is reversed. When information on the choices and earnings of others is withheld, as in our OWN-STOCHASTIC treatment, subjects’ expenditures remain high throughout the experiment and result in low group earnings. When subjects are given information on the choices and earnings of others they seem to place less weight on the choices of previously successful contestants than they do in deterministic contests. This perhaps reflects recognition on the part of subjects that past choices of successful rivals are less exemplary when success depends on luck as well as the profile of choices. Instead, we find that the main effect of adding information is to mitigate overly aggressive rent-seeking expenditures.

We find the results from our OWN-STOCHASTIC treatment particularly interesting because in many natural settings contestants easily observe own effort and whether or not they win, but do not easily observe the efforts and payoffs of rivals (e.g., consider grant-seeking competitions). Of course, in natural repeated contest environments the ease with which contestants can observe rival's expenditures and payoffs is likely to depend on a variety of institutional factors, such as legal disclosure rules, the costs/benefits of secrecy/transparency, and the intrinsic observability of different forms of expenditure (e.g., effort versus monetary expenditures). Our results suggest that one important avenue for further research would be to identify more systematically factors of the institutional environment that enhance informational feedback.

Our finding of excessive expenditures and limited learning in this low information setting is also reminiscent of findings from experiments using the "Buying a Company" task to investigate the winner's curse phenomenon (Samuelson and Bazerman, 1985). In these experiments the value of a company to a potential buyer is 1½ times the value to an incumbent owner, but the potential buyer does not know the exact value. She only knows that the incumbent's value is uniformly distributed between 0 and 100, and the incumbent will accept any offer at least equal to her value. Subjects typically bid in the range between the expected value of the company to the incumbent, 50, and the (unconditional) expected value to the potential buyer, 75. This results in expected losses relative to the risk-neutral optimal bid of zero. Moreover, persistent excessive bidding is observed even when the task is repeated with own-earnings information at the end of each task (see, for example, Selten et al. 2005). Bereby-Meyer and Grosskopf (2008) show that one reason for persistent over-bidding is the stochastic link between bids and outcomes: even when bidders overbid, they sometimes make positive profits, and this makes it far from transparent that excessive bids lead to an expected loss. When subjects bid for ten companies and then receive their average earnings from the ten separate outcomes the variability in earnings associated with a given bid is reduced, and subjects learn to avoid the winner's curse. Our experiment is somewhat different as our OWN information treatments are strategic settings, but nevertheless we observe excessive bidding and expected losses in stochastic contests, which is reduced by paying subjects their expected payoffs in deterministic contests.

We are only aware of three other experiments that include a treatment similar to our OWN-STOCHASTIC treatment. First, Mago et al. (2012) compare own and full information treatments in a twenty period game. They find high expenditures in both treatments, and no significant differences between treatments. Although there are many design differences between the two experiments we suspect that the difference between their results and ours reflects the different durations of the experiments. Based on the first twenty periods of our experiment the difference between our treatments is also insignificant at conventional levels ($p = 0.131$). Second, Faravelli and Stanca (2012) report a LOT treatment in which two subjects compete for a rent and subjects' endowments are set at half of the rent. Thus, the set of permissible choices is quite different from our setting, and in equilibrium a subject should spend half of her endowment on rent-seeking. As in our experiment, Faravelli and Stanca find that average expenditures change very little with experience. However, while initial expenditures as a fraction of the endowment are similar in the two experiments, this corresponds to excessive expenditure relative to equilibrium in our experiment and close-to-equilibrium expenditures in their experiment, due to the different endowments. Thus, in their experiment expenditures start and stay close to equilibrium levels, whereas in ours expenditures start and remain above equilibrium levels. Third, Shupp (2004) gives subjects an even lower endowment, equal to less than one third of the rent. The expenditure level in his treatment with low information is around 70% of the Nash Equilibrium. A reconciliation of the differing results from these experiments is possible if i) initial choices are sensitive to the set of permissible choices, and ii) the path of average expenditures is sensitive to initial expenditures. An interesting avenue for further research would be to investigate more systematically the determinants of rent-seeking in such low information environments.

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Appendix A

Below are the instructions given to experimental subjects. Differences between treatments are indicated in square brackets.

Instructions

Welcome! You are about to participate in an experiment in the economics of decision making. Please do not talk to any of the other participants until the experiment is over. If you have a question at any time please raise your hand and an experimenter will come to your desk to answer it.

The experiment will consist of 60 periods. In each period you will have the chance to earn points. At the end of the experiment each participant's accumulated point earnings from all periods will be converted into cash at the exchange rate of 0.015 pence per point. Each participant will be paid in cash and in private.

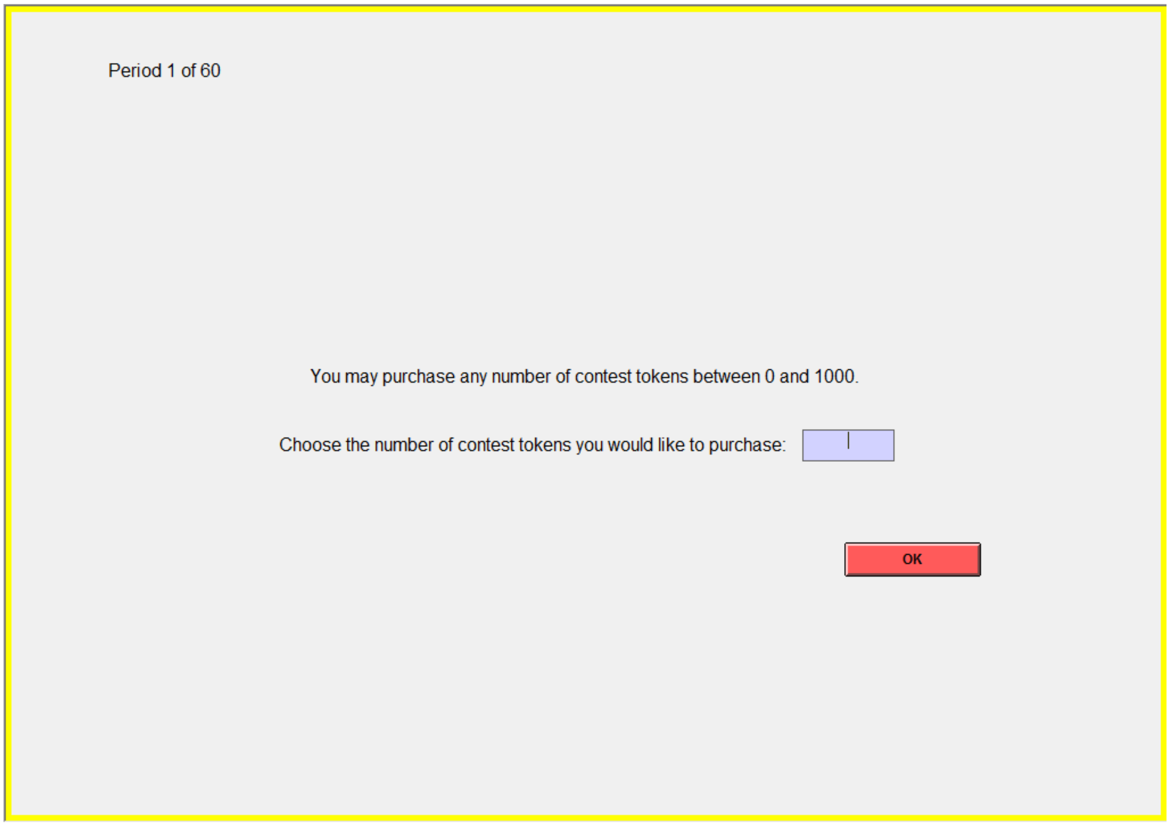
At the beginning of the experiment you will be matched with two other people, randomly selected from the participants in this room, to form a group of three. The composition of the group will stay the same throughout the experiment, i.e. you will form a group with the same two other participants during the whole experiment. Your earnings will depend on the decisions made within your group, as described below. Your earnings will not be affected by decisions made in other groups.

All decisions are made anonymously and you will not learn the identity of the other participants in your group.

Decision task in each period

Each period has the same structure. In each period the three participants in each group will be competing for a prize of 1000 points.

At the beginning of the period each participant will be given **an endowment of 1000 points**. Each participant has to decide how many of these points they want to use to buy "contest tokens". Each contest token costs 1 point, so each participant can purchase up to 1000 of these tokens. **Any part of the endowment that is not spent on contest tokens is kept by the participant**. Each participant must enter his or her decision via the computer. An example screenshot is shown below.



[STOCHASTIC: Once everybody has chosen how many contest tokens to purchase, the computer will determine which participant in your group wins the prize of 1000 points. Your chances of winning the prize will depend on how many contest tokens you have purchased and the total number of contest tokens purchased in your group.

If nobody in your group purchases any contest tokens, none of you will win the prize. Otherwise, the computer will determine which participant wins the prize in a way that will ensure that **the probability that you will win the prize is equal to the number of contest tokens that you have purchased divided by the total number of contest tokens purchased in your group**. That is, if you buy a number of *X* contest tokens and if the other two participants in your group buy *Y* and *Z* contest tokens each, then the probability that you win the prize will be *X/(X+Y+Z)*. **Your contest earnings will be either 0 (if you do not win the prize), or 1000 (if you win the prize).]**

[DETERMINISTIC: Once everybody has chosen how many contest tokens to purchase, the computer will calculate each participant's share of the prize of 1000 points. Your share of the prize will depend on how many contest tokens you have purchased and the total number of contest tokens purchased in your group.

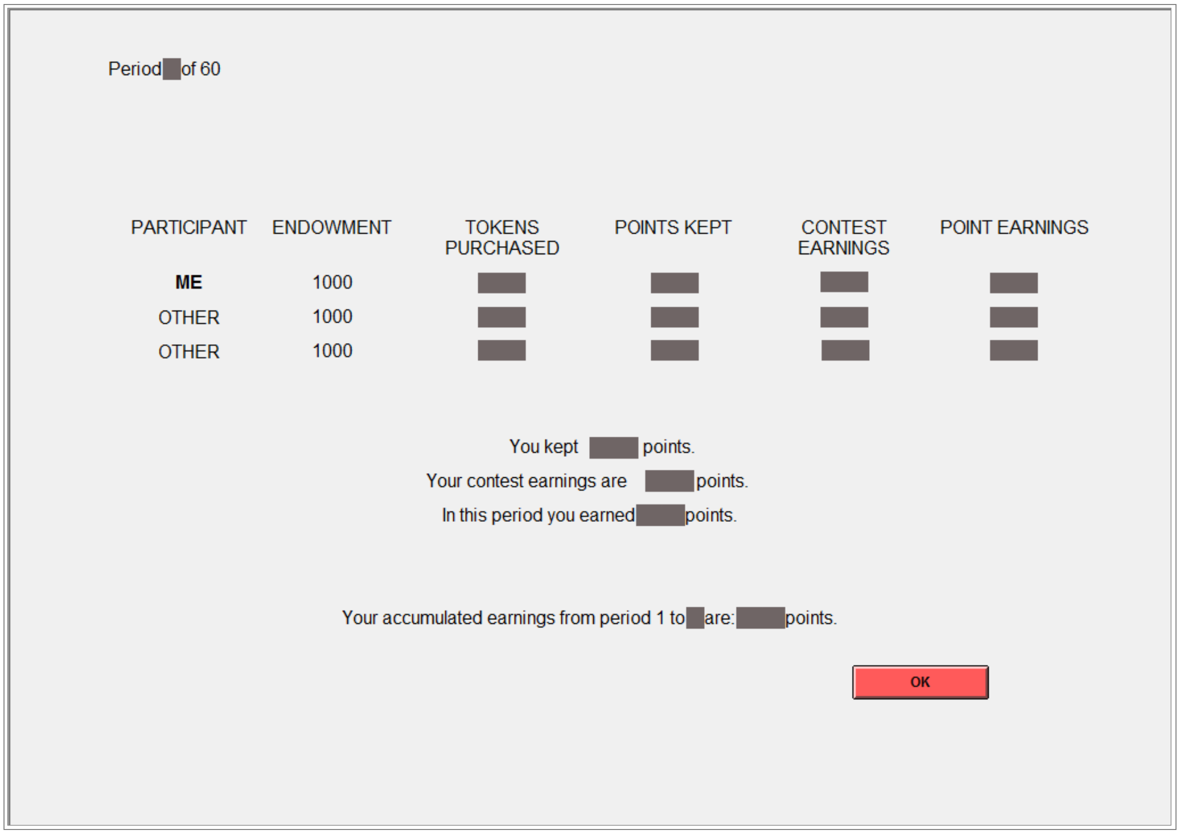
If nobody in your group purchases any contest tokens, none of you will receive a share of the prize. Otherwise, the computer will calculate each participant's share of the prize so that **your share of the prize will be equal to the number of contest tokens that you have purchased divided by the total number of contest tokens purchased in your group**. That is, if you buy a number of *X* contest tokens and if the other two participants in your group buy *Y* and *Z* contest tokens each, then your share of the prize will be *X/(X+Y+Z)*. Your **contest earnings will be your share times 1000 points (rounded to the nearest point)**.]

Your point earnings for the period will be calculated as follows:

point earnings = 1000 – contest tokens purchased + contest earnings

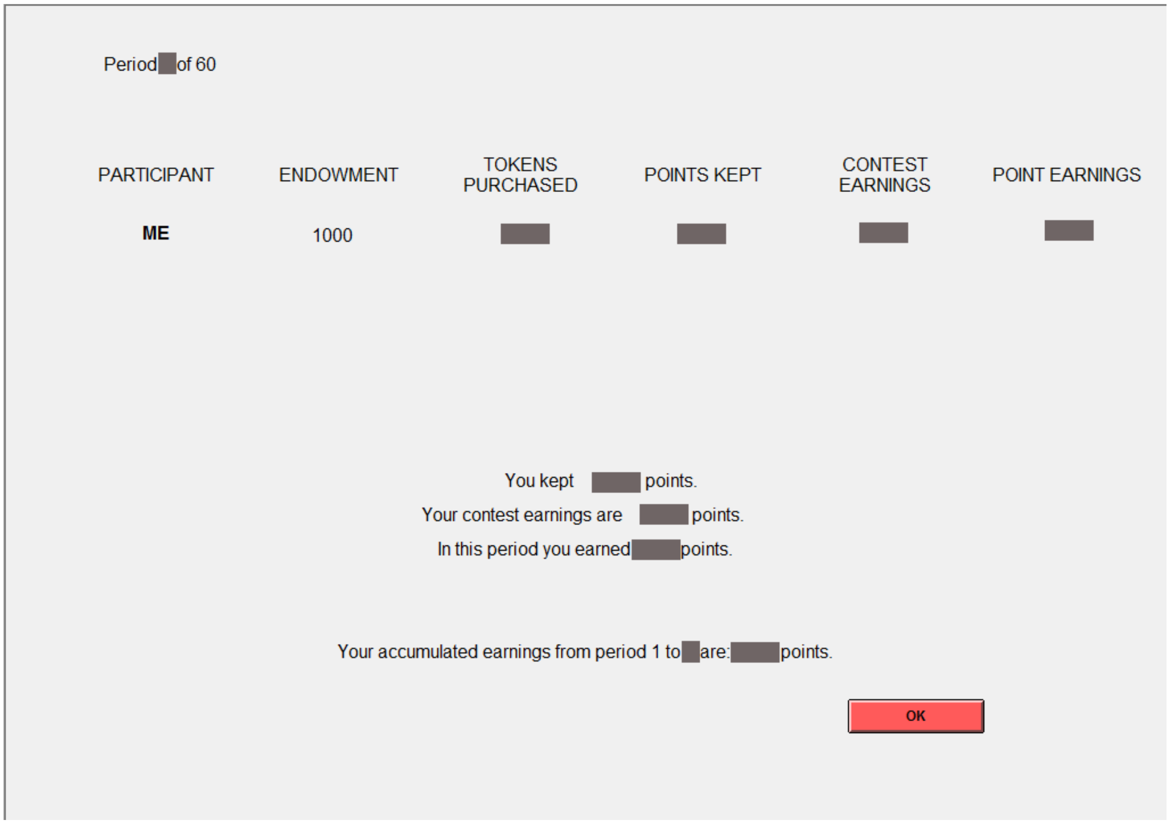
After all participants have made a decision, a result screen will appear. An example screenshot is shown below. This is like the screen you will see during the experiment except that the blacked out fields will be filled in according to the decisions made and the outcome of the contest in that round.

[FULL:



Each participant will be informed of the number of contest tokens they and the other two participants have purchased, the points remaining from their respective endowments, their respective contest earnings, and their respective point earnings for the period. The information is listed according to contest tokens purchased in descending order (with the participant who purchased most contest tokens listed first). Thus a participant's information may be listed on different lines in different periods.**]**

[OWN:



Each participant will be informed of the number of contest tokens they have purchased, the points remaining from their endowment after making their purchase, their contest earnings, and their point earnings for the period.**]**

In addition, the results screen will inform each participant of his or her accumulated points from all periods so far.

Beginning the experiment

If you have any questions please raise your hand and an experimenter will come to your desk to answer it.

We are now ready to begin the decision-making part of the experiment. Please look at your computer screen and begin making your decisions.

Appendix B. Group-level Data

Treatment	Group	Group Expenditure per period			Expenditure Dispersion per period*		
		All periods	Periods 1-30	Periods 31-60	All periods	Periods 1-30	Periods 31-60
OWN-D	1	693.90	855.97	531.83	442.82	564.30	321.33
OWN-D	2	924.75	1054.17	795.33	381.02	489.27	272.77
OWN-D	3	720.90	783.30	658.50	326.33	481.17	171.50
OWN-D	4	836.13	846.50	825.77	402.82	413.63	392.00
OWN-D	5	687.12	766.90	607.33	141.38	176.07	106.70
OWN-D	6	497.05	472.03	522.07	253.16	261.33	245.00
OWN-D	7	783.40	947.83	618.97	510.30	616.87	403.73
OWN-D	8	639.20	755.27	523.13	313.68	350.97	276.40
OWN-D	9	779.88	843.67	716.10	295.98	372.47	219.50
OWN-D	10	930.30	1092.27	768.33	564.70	651.20	478.20
FULL-D	1	677.37	607.10	747.63	264.23	286.73	241.73
FULL-D	2	853.42	839.67	867.17	105.83	158.00	53.67
FULL-D	3	986.57	987.07	986.07	444.97	425.03	464.90
FULL-D	4	909.40	866.43	952.37	390.20	423.43	356.97
FULL-D	5	966.82	1007.53	926.10	399.83	455.77	343.90
FULL-D	6	386.13	605.27	167.00	82.75	141.17	24.33
FULL-D	7	917.48	968.97	866.00	199.22	197.27	201.17
FULL-D	8	554.78	820.33	289.23	143.00	165.40	120.60
FULL-D	9	989.10	999.93	978.27	284.73	456.77	112.70
FULL-D	10	1016.45	1091.57	941.33	122.03	167.57	76.50
FULL-D	11	967.85	927.90	1007.80	307.45	289.73	325.17
OWN-S	1	810.88	793.50	828.27	524.52	546.37	502.67
OWN-S	2	1157.07	1304.00	1010.13	590.30	646.23	534.37
OWN-S	3	1253.83	1130.00	1377.67	775.75	698.83	856.67
OWN-S	4	1082.43	1286.10	878.77	626.75	783.20	470.30
OWN-S	5	842.73	1009.97	675.50	457.38	508.03	406.73
OWN-S	6	1335.72	1452.43	1219.00	809.50	772.80	846.20
OWN-S	7	1329.83	1204.23	1455.43	794.38	724.43	864.33
OWN-S	8	1379.02	1333.70	1424.33	663.80	614.63	712.97
OWN-S	9	1058.17	1058.47	1057.87	491.18	487.20	495.17
OWN-S	10	1060.67	946.60	1174.73	546.37	484.03	608.07
FULL-S	1	1067.95	1020.33	1115.57	470.78	436.20	505.37
FULL-S	2	1012.50	1121.63	903.37	442.05	467.07	417.03
FULL-S	3	376.70	394.57	358.83	212.68	227.57	197.80
FULL-S	4	671.75	745.63	597.87	470.67	546.50	394.83
FULL-S	5	853.03	850.60	855.47	336.73	264.90	408.57
FULL-S	6	525.37	646.13	404.60	255.87	327.47	184.27
FULL-S	7	735.88	890.10	581.67	346.20	479.90	212.50
FULL-S	8	581.20	692.57	469.83	397.38	504.57	290.20
FULL-S	9	1412.83	1679.47	1146.20	642.22	692.80	591.63
FULL-S	10	1105.00	1120.37	1089.63	458.88	497.80	409.97

* Expenditure dispersion within a period is calculated as $\max\{x_{1t}, x_{2t}, x_{3t}\} - \min\{x_{1t}, x_{2t}, x_{3t}\}$.