That's the ticket: Explicit lottery randomisation and learning in Tullock contests[a]

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Abstract

We experimentally contrast mathematical versus operational explanations of Tullock lottery contests. We contrast a protocol explaining the contest in terms of probability of winning, with an operational approach that carries out the random component of the contest as an explicit lottery each period. Initial expenditure levels are significantly lower when using the operational approach. In addition, using the operational approach, groups far from equilibrium in a given period move more rapidly towards approximate mutual best response. We find these results in sessions conducted in the UK and in the US. The implications that can be drawn from experiments on contest games therefore depend on the approach used to present the game to the players.

JEL classifications: C72, C91, D72, D83.

Keywords: lottery contest, learning, framing, experiment.

1 Introduction

> *"The hero ... is condemned because he doesn't play the game. [...] But to get a more accurate picture of his character, [...] you must ask yourself in what way (the hero) doesn't play the game".*
>
> — Albert Camus, in the afterword of *The Outsider* (Camus, 1982, p. 118)

Economic agents have ample opportunities to behave strategically in expending resources to attempt to win valuable prizes. Examples of such contests can be found in diverse settings such as rent-seeking, electoral competition, advertising, research and development, and sports. Ultimate success in these competitions is often a product of both a contestant's bid, which represents their effort or investment, and luck or happenstance.

The model of Tullock (1980) is the workhorse used when the impact of luck is assumed to be sufficiently large relative to that of the bid. In Tullock's specification of a contest, for any two contestants, their relative chances of victory are given by the ratio of their bids, raised to some exponent. This exponent is frequently set to one in applications, and it is this case we will focus on in the current study. With an exponent of one, the influence of chance is large enough that a contestant's payoff, as a function of their effort investment, is single-peaked, and the corresponding best response changes smoothly as the conjectured investments of other competitors is changed. In a symmetric setting with risk-neutral contestants and no spillovers, there is a unique Nash equilibrium in which the contestants play pure strategies (Szidarovszky and Okuguchi, 2008; Chowdhury and Sheremeta, 2011).

The Tullock contest has been a component in many experimental studies, either standing alone or as part of a broader research question. In Table 1 we provide a listing based on and updating the survey of Sheremeta (2013). The rightmost column in this table shows the ratio of observed expenditures in the experiment to the risk-neutral Nash equilibrium expenditure. Most studies report expenditures to be above the Nash prediction. Sheremeta (2013) reports that the median experiment generates expenditures 1.72 times that of the Nash prediction, on the basis of his meta-analysis of 30 different contest experiments involving 39 experimental treatments. Several papers (Herrmann and Orzen, 2008; Abbink et al., 2010; Cohen and Shavit, 2012; Cason et al., 2012a; Mago et al., 2013) report average expenditures more than double the Nash level. However, expenditures relative to the Nash prediction do vary substantially, with some studies finding expenditure levels below or close to the Nash benchmark.

In this paper, we explore the hypothesis that how the game is explained and operationalised has a systematic and significant effect on the strategic behaviour of experimental participants. From the perspective of standard game theory, the Tullock contest is a well-defined strategic game with

Study	Treatment	Ratio rule	Example	Actual-to-Nash
Millner and Pratt (1989)	Lottery	Yes	Lottery	1.12
Millner and Pratt (1991)	Less risk averse	Yes	Lottery	1.23
Shogren and Baik (1991)	Lottery	No (payoff table)	—	1.01
Davis and Reilly (1998)	Lottery	—	—	1.46
Potters et al. (1998)	Lottery	Yes	Lottery	1.68
Anderson and Stafford (2003)	One-shot (n=2)	No	Lottery	1.79
	One-shot (n=3)	No	Lottery	1.50
	One-shot (n=4)	No	Lottery	1.87
	One-shot (n=5)	No	Lottery	3.00
	One-shot (n=10)	No	Lottery	2.46
Schmitt et al. (2004)	Static	Yes	Lottery	1.76
Schmidt et al. (2006)	Single-prize (one-shot)	Yes	Wheel	0.70
Herrmann and Orzen (2008)	Direct repeated	No	Wheel	1.05
Kong (2008)	Loss aversion	Yes	Lottery	1.81
Fonseca (2009)	Simultaneous	No	Lottery	2.00
Abbink et al. (2010)	1:1	No	Lottery	2.05
Sheremeta (2010)	Single	Yes	Lottery	1.52
Sheremeta and Zhang (2010)	Individual	Yes	Lottery	1.95
Ahn et al. (2011)	1V1	—	—	1.37
Price and Sheremeta (2011)	P treatment	Yes	—	1.90
Sheremeta (2011)	GC	Yes	Lottery	1.33
	SC	Yes	Lottery	1.31
Cason et al. (2012b)	Individual	No	—	1.26
Faravelli and Stanca (2012)	Standard Lottery	Yes	Lottery	1.10
Morgan et al. (2012)	Small Prize	No	Wheel	1.45
	Large Prize	No	Wheel	1.19
Cohen and Shavit (2012)	One-shot (w/o refund)	Yes	—	1.52
Mago et al. (2013)	High r	Yes	Lottery	1.90
	High r + IP	Yes	Lottery	1.51
	Low r + IP	Yes	Lottery	1.73
Ke et al. (2013)	Share/fight	Yes	Wheel	1.50
Kimbrough and Sheremeta (2013)	Baseline	Yes	Lottery	1.95
Cason et al. (2012a)	Blue & Green	Yes	Lottery	2.23
Shupp et al. (2013)	Single-prize (real lottery)	Yes	Lottery	0.73
Brookins and Ryvkin (2014)	Complete symmetric	Yes	—	1.42
Chowdhury et al. (2014)	PL	Yes	—	0.75
Lim et al. (2014)	n=2	Yes	Lottery	1.30
	n=4	Yes	Lottery	1.61
	n=9	Yes	Lottery	3.30
Fallucchi et al. (2013)	Lottery-Full	Yes	Lottery	1.25
Kimbrough et al. (2014)	Baseline unbalanced	Yes	Lottery	1.41
	Baseline balanced	Yes	Lottery	1.15
	Random unbalanced	Yes	Lottery	1.17
	Random balanced	Yes	Lottery	1.25
Masiliunas et al. (2014)	N1S1	Yes	Lottery	1.25
Deck and Jahedi (2015)	Experiment 1	Yes	—	1.64
Sheremeta (2015)	One-shot	Yes	Lottery	1.81
Price and Sheremeta (2015)	Gift / Yardstick	Yes	Lottery	1.92
Godoy et al. (2015)	Partner-Random	No	Lottery	0.66
	Partner-No allocation	No	Lottery	1.37
Baik et al. (2015)	Medium	Yes	Lottery	1.73
Baik et al. (2016)	Partner (n=3)	Yes	Lottery	1.53
	Partner (n=2)	Yes	Lottery	1.20
Mago et al. (2016)	NP-NI	Yes	Lottery	1.94
	NP-I	Yes	Lottery	1.89

Table 1: Summary of Tullock contest experiments. “Ratio rule” indicates whether the study gives chances of winning in terms of ratios of bids. “Example” describes whether and how the game is additionally explained in terms of a (pseudo-)physical mechanism. “Actual-to-Nash” is the reported ratio of average bids to the Nash baseline prediction.

simultaneous moves. When we analyse it formally, we may express it using functions (as in, for example, equation (1) below), or payoff tables, or other mathematical tools. We also, less formally, may describe an operational implementation of the game. For example, a game with the structure of the Tullock contest is generated by a lottery or raffle. In such a lottery, participants may purchase some number of tickets, at a constant cost per ticket; all tickets are then collected into a drum, and one is selected at random to determine the winner of the prize.

The maintained assumption in standard game theory is that behaviour is only a function of the strategic form representation of the game. Any methods used to describe the game to the players, the labeling of choices or objects, or other considerations are deemed strategically irrelevant and should not have any effect on the predicted outcome. The work of Schelling (1960) already demonstrated that a theory which assumed away the content communicated by strategy labels could not account for the way that people (successfully) solved coordination games. Our experiment focuses on how the alternative explanations and implementations of the game affect behaviour.

The "Ratio rule" column in Table 1 indicates whether the experiment's instructions made an explicit mention that the probability of winning is given by the *ratio* of the contestant's own expenditure to the total expenditures of all contestants. A majority[1] of the studies do discuss this probability, with many, including Fallucchi et al. (2013); Lim et al. (2014); Ke et al. (2013) using a simplified but explicit form of equation (1).[2]

There is a literature on mathematical thinking and learning that asserts that feelings of uncertainty, anxiety, or discomfort may arise in individuals when presented with mathematical displays or terminology (Tobias and Weissbrod, 1980; Molina et al., 2016; Aiken, 1970; Goldin, 2002). Kapeller and Steinerberger (2013) argue that mere mathematical representation of an argument may hinder understanding among a significant proportion of people. Xue et al. (2015) show that people who self-report that they are not "good at math" make earnings-maximising choices substantially less often in a riskless decision task. On the basis of tasks involving basic numerical additions, they propose their results are consistent with the theory of math anxiety.

Many experimental instructions who mention the ratio supplement the description with a more concrete explanation of the game. (For some examples, see Chowdhury et al. (2014); Baik et al. (2015, 2016).) Many experiments say something along the lines that it is as if expenditures translate into lottery tickets (sometimes balls or tokens as in Potters et al. (1998); Fonseca (2009); Masiliunas et al. (2014); Godoy et al. (2015)) which are then placed together in a container, with one drawn at random to determine the winner. We record this practice as "Example" in the "Lottery" column of Table 1. However, in carrying out the experiment, rarely, if ever, do experimenters

[1]We were not able to obtain instructions for all of the studies listed; cells with entries marked — indicate studies we were not able to classify.

[2]Another alternative, giving a full payoff table as used by Shogren and Baik (1991), is a rather rare device.

resolve the outcome of each period using an explicit lottery draw presentation. The lottery is only an "as if" fable used for explanation purposes in the instruction, and then discarded.

An alternative approach used in a minority of studies (Schmidt et al., 2006; Herrmann and Orzen, 2008; Morgan et al., 2012; Ke et al., 2013) is a spinning lottery wheel, in which expenditures are mapped proportionally onto wedges on a circle. Morgan et al. (2012) and Ke et al. (2013) report bids around 1.5 times that predicted by the equilibrium, while Herrmann and Orzen (2008) report bids just above equilibrium and Schmidt et al. (2006) finds bids below the equilibrium prediction.

Our experiment draws a clean contrast between the mathematical and operational approaches to explaining the contest game. In our *probability* treatment, we use instructions which explain the game in terms of probabilities of winning. When resolving the contest in each period, we carry out the randomisation implicitly, reporting only the winning participant. In our *operational* treatment, we explain the game as purchasing tickets in a lottery. To resolve the outcome of the contest, each ticket purchased is given an individualised number, and when the randomisation is carried out each period, the winning ticket number is revealed to participants alongside the identifier of the winning participant.

We find that this manipulation has significant effects on behaviour. The operational treatment results in significantly lower expenditures in the first period. We interpret this as showing the mere explanation of the game in operational lottery terms, rather than in terms of mathematical probability, is important. In addition, learning and adaptation is faster in the operational treatment; expenditures move more quickly towards being approximately mutual best responses when outcomes are determined by the ticket-based randomisation. We interpret our results as showing that learning is also enhanced when experimenters follow through and carry out the lottery to determine outcomes, rather than leaving the lottery as a hypothetical "as if" aside in the instructions. We report on sessions conducted at two sites, one in the UK and one in the US, and do not find subject pool effects, confirming that our results are not the result of the peculiarities of the participants who attend sessions at a particular laboratory.

We present the formal description of the game, the experimental implementation, and the hypotheses in Section 2. The summary of the data and the results are included in Section 3. We conclude in Section 4 with further discussion.

2 Experimental design

Formally, the Tullock lottery contest we study is an n-player simultaneous-move game. There is one indivisible prize, which each player values at $v > 0$. Each player i has an endowment $\omega \geq v$, and chooses a bid $b_i \in [0, \omega]$. Given a vector of bids $b = (b_1, \ldots, b_n)$, the probability player i wins the prize is given by

$$p_i(b) = \begin{cases} \frac{b_i}{\sum_{j=1}^n b_j} & \text{if } \sum_{j=1}^n b_j > 0 \\ \frac{1}{n} & \text{otherwise} \end{cases} \tag{1}$$

Players are assumed to be risk-neutral, and so their expected payoff function is

$$u_i(b) = vp_i(b) + (\omega - b_i)$$

The unique Nash equilibrium is in pure strategies, with $b_i^{NE} = \frac{n-1}{n^2}v$ for all players.

In our experiment, we choose $n = 4$ and $\omega = v = 160$. We restrict the bids to be drawn from the discrete set of integers, $\{0, 1, \ldots, 159, 160\}$. With these parameters, the unique Nash equilibrium has $b_i^{NE} = 30$.

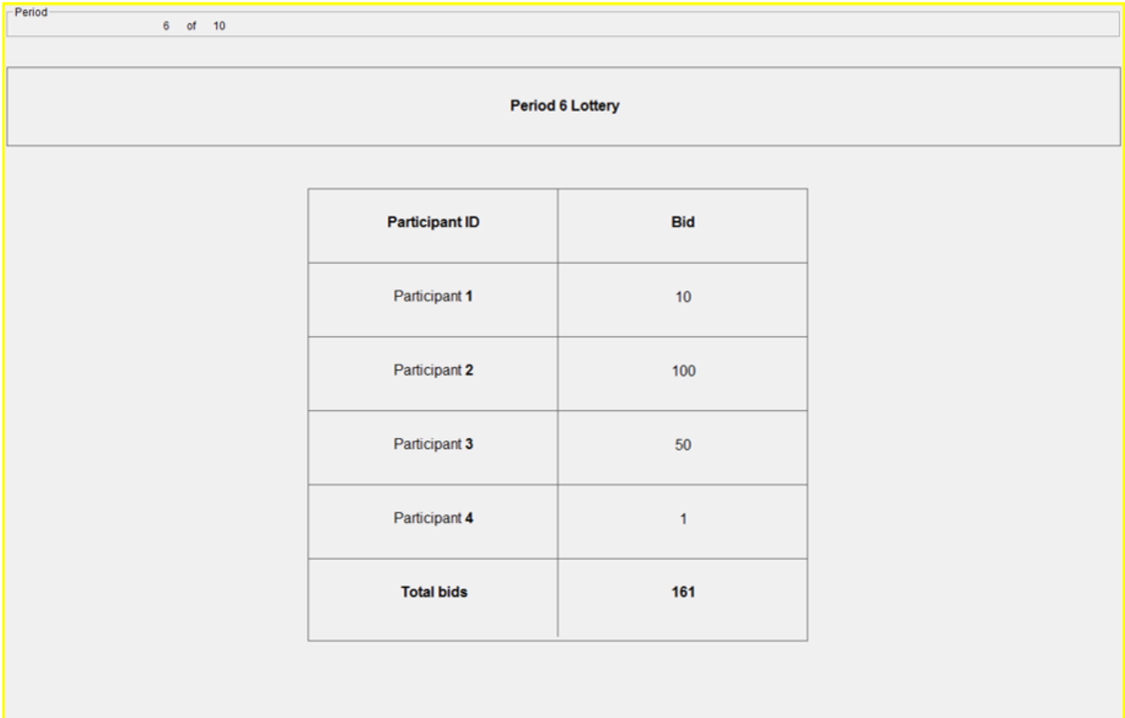
Participants played 30 contest periods, with the number of periods announced in the instructions. The groups of $n = 4$ participants were fixed throughout the session. Within a group, members were referred to anonymously by ID numbers 1, 2, 3, and 4; these ID numbers were randomised after each period. All interaction was mediated through computer terminals, using zTree (Fischbacher, 2007). A participant's complete history of their own bids and their earnings in each period was provided throughout the experiment.

We contrast two treatments, across sessions. In sessions using the *probability treatment*, the instructions[3] presented the game using a description of how choices determined the probability of obtaining the prize. After the bids were made but before realising the outcome of the contest, participants saw a screen of the form in Figure 1, detailing the bids of each of the participants in the group.

In sessions using the *operational treatment*, the instructions described the game as a lottery. The relationship between bids and the chance of receiving the prize was explained in terms of a lottery implementation. Bids translated one-to-one into tickets, and each ticket was equally likely to be the one purchased. After the bids were made, participants saw a screen of the form in Figure 2, which numbered the tickets purchased and indicated the number of the selected ticket, as well as the identifier of the successful participant.

We conducted a total of 14 experimental sessions. Seven of the sessions took place at the Centre for Behavioural and Experimental Social Science at University of East Anglia in the United Kingdom, using the hRoot recruitment system (Bock et al., 2012), and seven at the Vernon Smith Experimental Economics Laboratory at Purdue University in the United States, using ORSEE (Greiner, 2015). We refer to the samples as UK and US, respectively. In the UK, there were four sessions of each treatment with 12 participants (3 fixed groups) per session; in the US, there were three sessions of each treatment with 16 participants (4 fixed groups) per session. We therefore

[3]See Appendix A for the full text of the instructions.



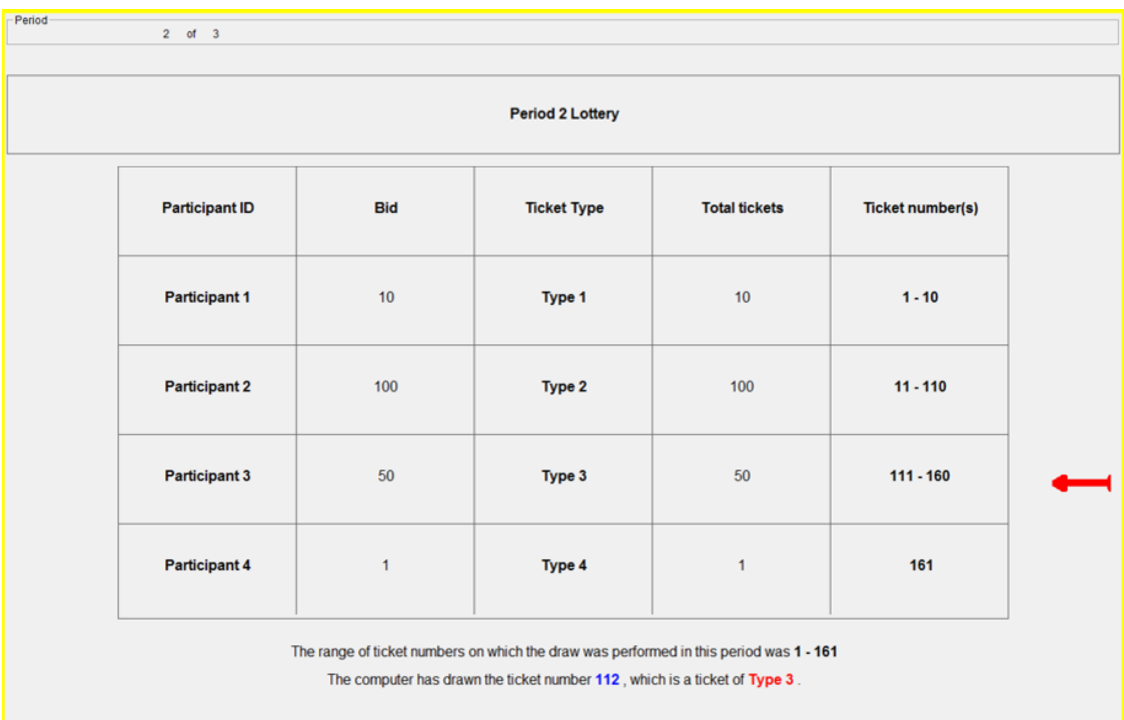
Period

6 of 10

Period 6 Lottery

Participant ID	Bid
Participant **1**	10
Participant **2**	100
Participant **3**	50
Participant **4**	1
Total bids	**161**

Figure 1: Screenshot of bid summary in probability treatment



Period

2 of 3

Period 2 Lottery

Participant ID	Bid	Ticket Type	Total tickets	Ticket number(s)
Participant 1	10	**Type 1**	10	**1 - 10**
Participant 2	100	**Type 2**	100	**11 - 110**
Participant 3	50	**Type 3**	50	**111 - 160**
Participant 4	1	**Type 4**	1	**161**

The range of ticket numbers on which the draw was performed in this period was **1 - 161**

The computer has drawn the ticket number **112** , which is a ticket of **Type 3** .

Figure 2: Screenshot of bid summary and lottery in operational treatment

	UK	US	All
Probability	49.85	53.56	51.70
	(43.77)	(48.41)	(46.18)
	$N = 1440$	$N = 1440$	$N = 2880$
Operational	40.35	41.08	40.72
	(35.96)	(35.36)	(35.65)
	$N = 1440$	$N = 1440$	$N = 2880$

Table 2: Descriptive statistics on individual bids. Each cell contains the mean, standard deviation (in parentheses), and total number of bids. The column All pools the bids from the two sites.

have data on a total of 48 participants (12 fixed groups) in each treatment at each site.

The units of currency in the experiment were pence. In the UK sessions, these are UK pence. In the US sessions, we had an exchange rate, announced prior to the session, of 1.5 US cents per pence. We selected this as being close to the average exchange rate between the currencies in the year prior to the experiment, rounded to 1.5 to make the rate easy to understand.

Participants received payment for 5 of the 30 periods, which were selected in public at random at the end of the experiment.[4] Sessions lasted about an hour, and average payments were approximately £10 in the UK and $15 in the US.

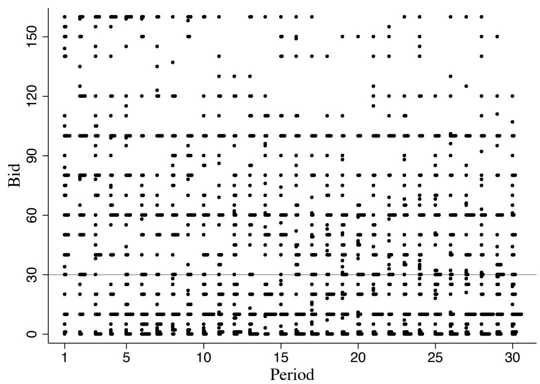
3 Results

We begin with a graphical overview of all 5,760 bids in our sample. Figure 3 displays dotplots for the bids made in each period, broken out by subject pool and treatment, and Table 2 provides summary statistics on the individual bids for each treatment and subject pool. Both the figure and table indicate a treatment difference. Aggressive bids at or near the maximum of 160 are infrequent in the operational treatment after the first few periods, but persist in the probability treatment. Looking separately at each treatment, the aggregate patterns of bidding are similar in the UK and US.

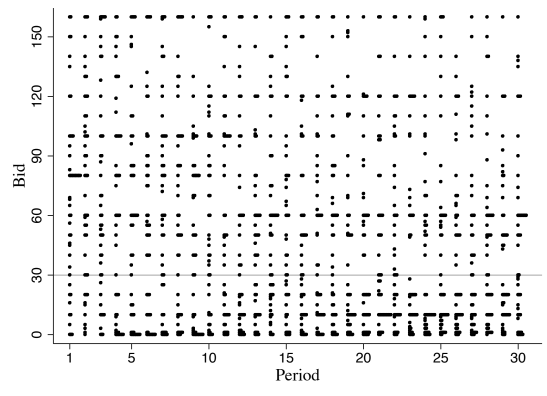
Result 1. *In each treatment, there are no significant differences between the distributions of bids in the UK versus in the US.*

Support. We use the group as the unit of independent observation, and compute, for each group, the average bid over the course of the experiment. The Mann-Whitney-Wilcoxon rank-sum test does not reject the null hypothesis of equal distributions of these group means across the subject

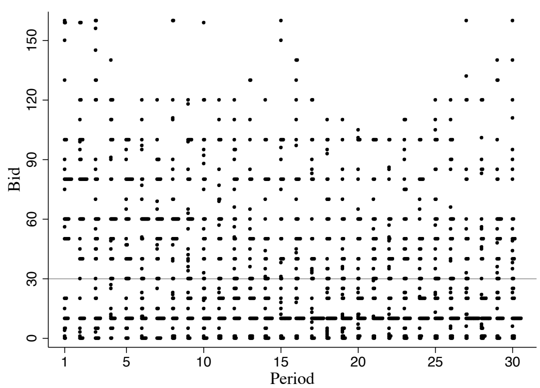
[4]The US participants also received a USD 5.00 participation payment on top of their contingent payment, to be consistent with conventions at Purdue.



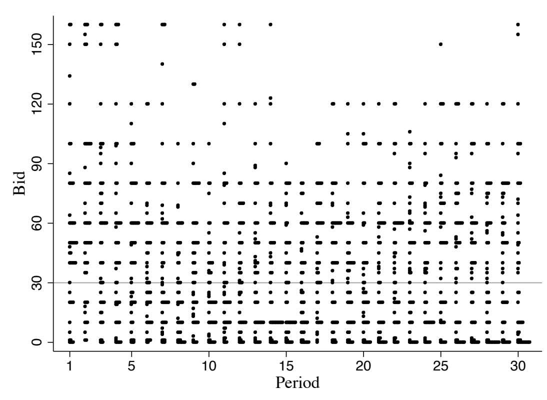
(a) Probability, UK



(b) Probability, US



(c) Operational, UK



(d) Operational, US

Figure 3: All bids by period, grouped by subject pool and treatment. Each dot represents the bid of one participant in one period. The Nash equilibrium bid of 30 is indicated by a horizontal line.

pools ($p = 0.86$ for the probability and $p = 0.91$ for the operational treatment). Similarly, the Mann-Whitney-Wilcoxon test does not reject the null hypothesis if the group means are computed based only on periods 1-10, 11-20, or 21-30.[5] □

In view of the similarities between the data from the two subject pools, we continue by using combined sample for our subsequent analysis. Our next result treats the full 30-period supergame as a single unit for each group, and compares behaviour to the benchmark of the unique subgame-perfect Nash equilibrium in which the stage game equilibrium is played in each period.

Result 2. *Bids are significantly lower over the course of the experiment in the operational treatment than in the probability treatment. Both treatments significantly exceed the Nash equilibrium prediction.*

[5]For the probability treatment, the p-values for the M-W-W test are $p = 0.69$ for periods 1-10, $p = 0.77$ for periods 10-21, and $p = 0.91$ for periods 21-30. For the operational treatment the corresponding p-values are $p = 0.39$, $p = 0.95$, and $p = 0.29$, respectively.

Support. For each group we compute the mean bid over the course of the experiment. The mean over groups is 51.7 in the probability treatment (standard deviation 14.8) and 40.7 in the operational (standard deviation 9.1). Figure 4 plots the full distribution of these group means; the boxes indicate the locations of the median and upper and lower quartiles of the distributions. Using the Mann-Whitney-Wilcoxon rank-sum test, we reject the hypothesis that the distributions are equal ($p = 0.0036$). □

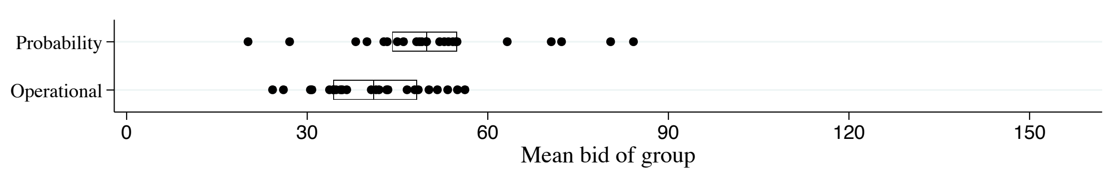


Figure 4: Distribution of mean bids for each group over the experiment. Each dot represents the mean bid of one group.

The difference between the treatments could be attributable to some difference in how experiential learning takes place because of the feedback mechanism in playing out the lottery explicitly, or simply because participants process the explanation of the game differently when facing the explicit lottery frame. We can look for evidence of the latter by considering only the first-period bids.

Result 3. *First-period bids are significantly lower, and therefore closer to the Nash equilibrium prediction, in the operational treatment.*

Support. Figure 5 displays the distribution of first-period bids for all 192 bidders (96 in each treatment). Because at the time of the first-period bids participants have had no interaction, we can treat these as independent observations. The mean first-period bid in the probability treatment is 71.1, versus 56.8 in the operational treatment. Put another way, as a point estimate approximately 35% of the observed overbidding relative to the Nash prediction is explained in the first-period by the treatment difference. Using the Mann-Whitney-Wilcoxon rank-sum test, we reject the hypothesis that the distributions are equal ($p = 0.0197$). □

Although there is a significant treatment effect in the first period, bids remain significantly above the equilibrium prediction in both treatments. We therefore turn to the dynamics of bidding over the course of the session. Returning to the group as the unit of independent observation, Figure 6 displays boxplots of the distribution of group average bids period-by-period for each treatment. The probability treatment begins at a higher level of bids in the first period, and both treatments exhibit a trend of average bids decreasing towards the Nash equilibrium prediction.

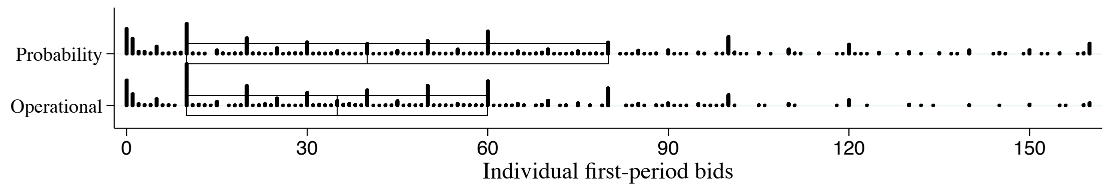
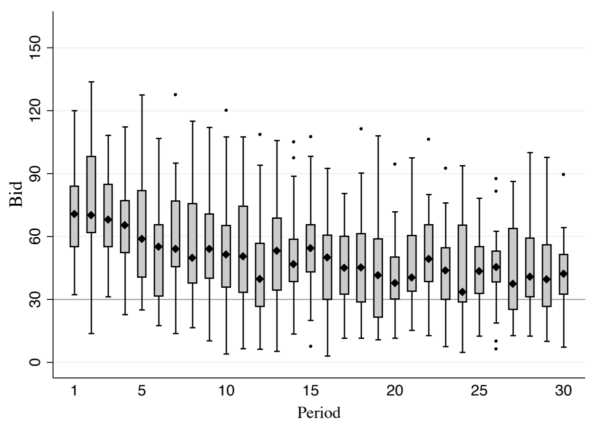
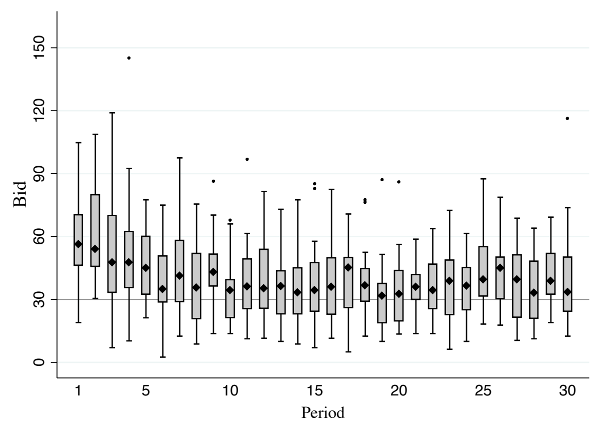


Figure 5: Distribution of first-period bids for all participants. Each dot represents the bid of one participant. For each distribution, the superimposed box indicates the median and the lower and upper quartiles.



(a) Probability



(b) Operational

Figure 6: Evolution of group average bids over time. For each period, the vertical boxes plot the interquartile range of average bids across groups. The black diamond indicates the median of the group averages.

We are interested in determining whether the explicit implementation of the lottery also has an effect on the dynamics of behaviour over the experiment. Carrying out the lottery explicitly may make the payoff implications of bids more transparent; perhaps in part because it communicates that the more tickets are purchased, the less valuable each individual ticket becomes. Assuming that participants are interested in trying to increase their earnings in the experiment, this feedback would lead to adjustment towards bids with better expected earnings potential.

Therefore, we organise our analysis of dynamics in terms of payoffs, rather than bids themselves. Consider a group g in session s of treatment $c \in \{\text{probability}, \text{operational}\}$. We construct for this group, for each period t, a measure of disequilibrium based on ε-equilibrium. (Radner, 1980) In each period t, each bidder i in the group submitted a bid b_{it}. Given these bids, bid b_{it} had an expected payoff to i of

$$\pi_{it} = \frac{b_{it}}{\sum_{j \in G} b_{jt}} \times 160 + (160 - b_{it}).$$

For comparison, we can consider bidder i's best response to the other bids of his group. Letting $B_{it} = \sum_{j \in G: j \neq i} b_{jt}$, the best response, if bids were permitted to be continuous, would be given by

$$\tilde{b}^{\star}_{it} = \max\{0, \sqrt{160 B_{it}} - B_{it}\}.$$

Bids are required to be discrete in our experiment; the quasiconcavity of the expected payoff function ensures that the discretised best response $b^{\star}_{it} \in \{\lceil \tilde{b}^{\star}_{it} \rceil, \lfloor \tilde{b}^{\star}_{it} \rfloor\}$. This discretised best response then generates an expected payoff to i of

$$\pi^{\star}_{it} = \frac{b^{\star}_{it}}{b^{\star}_{it} + B_{it}} \times 160 + (160 - b^{\star}_{it}).$$

We then write[6]

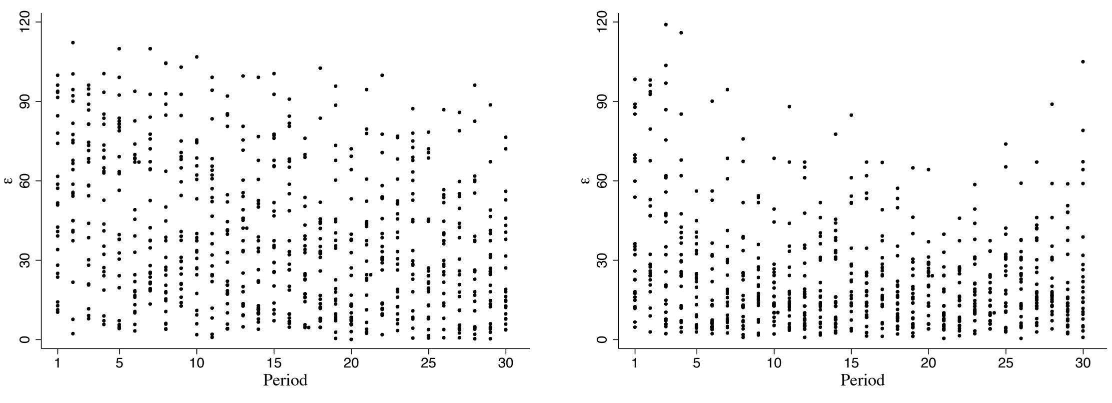
$$\varepsilon_{csgt} = \max_{i \in g} \{\pi^{\star}_{it} - \pi_{it}\}.$$

By construction, $\varepsilon_{csgt} \geq 0$, with $\varepsilon_{csgt} = 0$ only at the Nash equilibrium.

Conducting the analysis in the payoff space measures behaviour in terms of potential earnings, which is what we assume motivates our participants' behaviour, at least in some part. The marginal earnings consequences of an incremental change in bid depends on both b_{it} and B_{it}, so a bid-based analysis would not adequately capture incentives. In addition, although in general bids are high enough that the best response in most groups in most periods is to bid low, there are many instances in which the best response for a bidder would have been to bid higher than they actually did. A focus on payoffs allows us to track the dynamics without having to account for directional learning in the bid space.

Figure 7 shows the evolution of the disequilibrium measure ε over the experiment. The clustering of this measure at lower values, especially below about 30, is evident in the operational treatment throughout the experiment, while any convergence in the probability treatment is slower. While suggestive, these dot plots alone are not enough to establish whether the evolution of play differs between the treatments, because it does not take into account the dynamics of each individual group. Result 3 implies that values of ε in Period 1 are lower in the operational treatment. Therefore, the difference seen in Figure 7 could be attributable to the different initial conditions rather than different dynamics, as there is simply less room for improvement among the groups in

[6] Taking the maximum to define the metric ε_{csgt} gives the standard definition of ε-equilibrium. Our results about the treatment effect on dynamics also hold if ε_{csgt} is defined as the average or the median in each group.



(a) Probability

(b) Operational

Figure 7: Ex-post measure of disequilibrium ε within groups, by period. Each dot corresponds to the value of the measure for one group in one period.

the operational treatment given their first-period decisions.

We control for this by investigating the evolution of ε within-group over the experiment. As a first graphical investigation, we plot the average value of $\varepsilon_{csg(t+1)}$ as a function of ε_{csgt} for both treatments in Figure 8.[7] Consider two groups, one in the probability treatment and one in the operational treatment, who happen to have the same ε in some period. Figure 8 says that in the subsequent period, on average, the ε measure of the group in the operational treatment will be lower, that is, they will move further towards an approximate mutual best response.[8]

Result 4. *Convergence towards equilibrium, as measured by ε-equilibrium, is significantly faster in the operational treatment than in the probability treatment.*

Support**.** To formalise the intuition provided by Figure 8, we estimate a random-effects panel regression

$$\varepsilon_{csg(t+1)} = \alpha + \beta_0 \varepsilon_{csgt} + \beta_1 \mathbf{1}_{c=\text{operational}} + \beta_2 \mathbf{1}_{c=\text{operational}} \times \varepsilon_{csgt},$$

where $\mathbf{1}_{c=\text{operational}}$ is a dummy variable which is true for groups in sessions using the operational treatment. The resulting parameter estimates, with standard errors clustered at the session level, are reported in Table 3. Both the intercept and slope parameters for the operational treatment are significantly lower than for the probability treatment, indicating a faster rate of convergence for groups in the operational treatment from any given initial value of ε. □

[7] For the purposes of Figure 8 we aggregate observations by rounding ε_{csgt} to the nearest multiple of five, and taking the average over all observations with the same rounded value.

[8] There are very few groups in either treatment with values of ε above about 75, accounting for the instability in the graph for large ε.

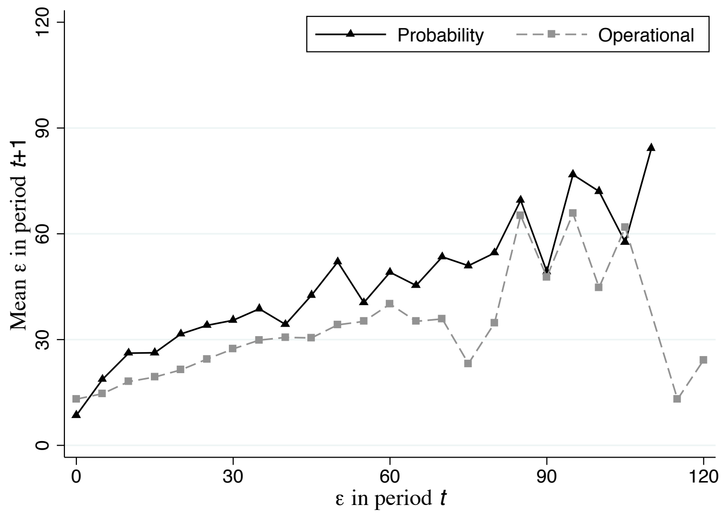


Figure 8: Expected value of disequilibrium measure ε in next period, as a function of a group's current ε.

	Coefficient	Standard error	p-value
Intercept	18.77	1.39	<0.001***
ε_{csgt}	0.50	0.03	<0.001***
$\mathbf{1}_{c=\text{operational}}$	-4.90	1.85	0.008**
$\mathbf{1}_{c=\text{operational}} \times \varepsilon_{csgt}$	-0.11	0.05	0.024*

Table 3: Random-effects panel regression of evolution of disequilibrium measure ε over time. Dependent variable is $\varepsilon_{csg(t+1)}$; standard errors clustered at the session level. Overall $R^2 = 0.2955$. *** denotes significantly different from zero at 0.1%; ** at 1%, * at 5%.

Figure 8 and Table 3 show that in both treatments, groups that have very small values of ε in one period tend to increase ε in the subsequent period; that is, they move away from equilibrium. The fixed point for ε using the point estimates is about 37.5 for the probability treatment, and 22.5 for the operational treatment. This observation is consistent with previous studies which have demonstrated a difference between the Tullock contest with a random outcome, as studied here, and a version in which the prize is shared deterministically among the contestants in proportion to their bids. When behaviour is close to equilibrium, small deviations in bids have small consequences in terms of expected payoffs, leaving open the door for other behavioural factors to come into play. For example, although the outcome of the randomisation contains no new information for participants, they may nevertheless base their bids in subsequent periods on the outcomes of previous random draws. The presence of this or similar heuristics would introduce an underlying level of noise in play consistent with the positive intercepts obtained in Table 3.

4 Discussion

Our experiment shows that a consistent use of an operational implementation of the Tullock contest as a lottery game, both in the instructions and in realising the outcome of each period's contest, has a significant effect on behaviour. Measured relative to the risk-neutral Nash equilibrium benchmark, roughly one-third of the overbidding in the first period, and one-half of the overbidding over the course of the 30 periods of our experiment, can be attributed to the use of a mathematical exposition of the game in terms of probabilities, compared to the lottery implementation.

Although the lottery-based treatment is significantly closer to the Nash benchmark, we do not interpret our results as saying this is universally a "better" way to implement the game for the purposes of experiments. The heterogeneity in design features in the studies listed in Table 1 can be attributed at least in part to the research goals of each individual study. Both of our treatments imply the same strategic game representation (under standard assumptions). But the Tullock model is very versatile in its application. For example, in a sports application, the random component of the outcome is not in general as transparently observable as it is in the ticket-based lottery. Our results highlight that while standard game theory considers them the same game, a satisfactory behavioural game theory will need to distinguish between them.

We conduct both of our treatments in two participant pools, one each in the UK and US, placing our design as one of very few in the contest literature to investigate directly possible differences across participant pools. The data in both treatments is very similar across sites, providing initial evidence that results in contest experiments are portable across at least some participant pools. We use two participant pools drawn from university students in English-speaking countries, which allows us to use identical instruction texts across the two sites, but samples only a particular part of the population. We might hypothesise that using a broader, non-student population might lead to an even larger effect of using the lottery implementation, as current university students are likely to be relatively more comfortable, at least on average, with a mathematical explanation than people from the general population. Equally, there may be cultural differences, especially in cultures in which skill in mathematical calculation is valued more highly, which might lead to a smaller treatment effect.

Our study is by no means exhaustive in exploring implementations of the Tullock contest. There are other possibilities for expressing and realising the random component of the game. Instead of opting for the lottery approach, which presents counts of pseudo-physical objects (tickets or balls), there are possibilities for graphical representations such as the lottery wheel used by a handful of studies listed in Table 2. The implications for visualisations on individual and strategic choice is a small but growing area of research. The lottery implementation would extend in a straightforward way to the case of a nonlinear cost of bidding, simply by having a non-linear pricing scheme for purchasing tickets. Like probability, non-linearity is a concept which, while it can be expressed concisely in mathematical terms, could benefit from an operational expression in the implementation of certain experimental designs.

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A Instructions

The session consists of 30 decision-making periods. At the conclusion, any 5 of the 30 periods will be chosen at random, and your earnings from this part of the experiment will be calculated as the sum of your earnings from those 5 selected periods.

You will be randomly and anonymously placed into a group of 4 participants. Within each group, one participant will have ID number 1, one ID number 2, one ID number 3, and one ID number 4. The composition of your group remains the same for all 30 periods but the individual ID numbers within a group are randomly reassigned in every period.

In each period, you may bid for a reward worth 160 pence. In your group, one of the four participants will receive a reward. You begin each period with an endowment of 160 pence. You may bid any whole number of pence from 0 to 160; fractions or decimals may not be used.

If you receive a reward in a period, your earnings will be calculated as:

Your payoff in pence = your endowment – your bid + the reward.

That is, your payoff in pence = 160 – your bid + 160.

If you do not receive a reward in a period, your earnings will be calculated as:

Your payoff in pence = your endowment – your bid.

That is, your payoff in pence = 160 – your bid.

Portion for probability treatment only

The more you bid, the more likely you are to receive the reward. The more the other participants in your group bid, the less likely you are to receive the reward. Specifically, your chance of receiving the reward is given by your bid divided by the sum of all 4 bids in your group:

$$\text{Chance of receiving the reward} = \frac{\text{Your bid}}{\text{Sum of all 4 bids in your group}}.$$

You can consider the amounts of the bids to be equivalent to numbers of lottery tickets. The computer will draw one ticket from those entered by you and the other participants, and assign the reward to one of the participants through a random draw.

An example. Suppose participant 1 bids 80 pence, participant 2 bids 6 pence, participant 3 bids 124 pence, and participant 4 bids 45 pence. Therefore, the computer assigns 80 lottery tickets to participant 1, 6 lottery tickets to participant 2, 124 lottery tickets to participant 3, and 45 lottery tickets for participant 4. Then the computer randomly draws one lottery ticket out of 255 (80 + 6 + 124 + 45). As you can see, participant 3 has the highest chance of receiving the reward: 0.49 = 124/255. Participant 1 has a 0.31 = 80/255 chance, participant 4 has a 0.18 = 45/255 chance and participant 2 has the lowest, 0.05 = 6/255 chance of receiving the reward.

After all participants have made their decisions, all four bids in your group as well as the total of those bids will be shown on your screen.

Participant ID	Bid
Participant 1	80
Participant 2	6
Participant 3	124
Participant 4	45
Total bids	255

Interpretation of the table: The horizontal rows in the left column of the above table contain the ID numbers of the four participants in every period. The right column lists their corresponding bids. The last row shows the total of the four bids. The summary of the bids, the outcome of the draw and your earnings will be reported at the end of each period.

At the end of 30 periods, the experimenter will approach a random participant and will ask him/her to pick up five balls from a sack containing 30 balls numbered from 1 to 30. The numbers on those five balls will indicate the 5 periods, for which you will be paid in Part 2. Your earnings from all the preceding periods will be throughout present on your screen.

Portion for operational treatment only

The chance that you receive a reward in a period depends on how much you bid, and also how much the other participants in your group bid. At the start of each period, all four participants of each group will decide how much to bid. Once the bids are determined, a computerised lottery will be conducted to determine which participant in the group will receive the reward. In this lottery draw, there are four types of tickets: Type 1, Type 2, Type 3 and Type 4. Each type of ticket corresponds to the participant who will receive the reward if a ticket of that type is drawn. So, if a Type 1 ticket is drawn, then participant 1 will receive the reward; if a Type 2 ticket is drawn, then participant 2 will receive the reward; and so on.

The number of tickets of each type depends on the bids of the corresponding participant:

- Number of Type 1 tickets = Bid of participant 1
- Number of Type 2 tickets = Bid of participant 2

- Number of Type 3 tickets = Bid of participant 3
- Number of Type 4 tickets = Bid of participant 4

Each ticket is equally likely to be drawn by the computer. If the ticket type that is drawn has your ID number, then you will receive a reward for that period.

An example. Suppose participant 1 bids 80 pence, participant 2 bids 6 pence, participant 3 bids 124 pence, and participant 4 bids 45 pence. Then:

- Number of Type 1 tickets = Bid of participant 1 = 80
- Number of Type 2 tickets = Bid of participant 2 = 6
- Number of Type 3 tickets = Bid of participant 3 = 124
- Number of Type 4 tickets = Bid of participant 4 = 45

There will therefore be a total of 80 + 6 + 124 + 45 = 255 tickets in the lottery. Each ticket is equally likely to be selected.

In each period, the calculations above will be summarised for you on your screen, using a table like the one in this screenshot:

Participant ID	Bid	Ticket Type	Total tickets	Ticket number(s)
Participant 1	80	Type 1	80	1 - 80
Participant 2	6	Type 2	6	81 - 86
Participant 3	124	Type 3	124	87 - 210
Participant 4	45	Type 4	45	211 - 255

Interpretation of the table: The horizontal rows in the above table contain the ID numbers of the four participants in every period. The vertical columns list the participants' bids, the corresponding ticket types, total number of each type of ticket (second column from right) and the range of ticket numbers for each type of ticket (last column). Note that the total number of each ticket type is exactly same as the corresponding participant's bid. For example, the total number of Type 1 tickets is equal to Participant 1's bid.

The last column gives the range of ticket numbers for each ticket type. Any ticket number that lies within that range is a ticket of the corresponding type. That is, all the ticket numbers from 81 to 86 are tickets of Type 2, which implies a total of 6 tickets of Type 2, as appears from the 'Total Tickets' column. In case a participant bids zero, there will be no ticket that contains his or her ID number. In such a case, the last column will show 'No tickets' for that particular ticket type.

The computer then selects one ticket at random. The number and the type of the drawn ticket will appear below the table. The ID number on the ticket type indicate the participant receiving the reward.

At the end of 30 periods, the experimenter will approach a random participant and will ask him/her to pick up five balls from a sack containing 30 balls numbered from 1 to 30. The numbers on those five balls will indicate the 5 periods, for which you will be paid in Part 2. Your earnings from all the preceding periods will be throughout present on your screen.