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

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September 12, 2018

[a]This project was supported by the Network for Integrated Behavioural Science (Economic and Social Research Council Grant ES/K002201/1). We thank Catherine Eckel, Aidas Masiliunas, and Henrik Orzen, as well as participants at the Contests: Theory and Evidence Conference, Eastern ARC Workshop, Jadavpur University Annual Conference 2016, and the Visions in Methodology Conference for helpful comments. We thank Tim Cason and the Vernon Smith Experimental Economics Laboratory at Purdue University for allowing us to use their facilities. Any errors are the sole responsibility of the authors.

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Abstract

Most laboratory experiments studying Tullock contest games find bids significantly exceed risk-neutral equilibrium predictions. We test the generalisability of these results by comparing a typical experimental implementation of a contest against the familiar institution of a lottery. We find that in the lottery (1) initial bid levels are significantly lower and (2) bids adjust more rapidly towards expected-earnings best responses. We demonstrate the robustness of our results by replicating them across two continents at two universities with contrasting student profiles. Care is therefore required in mapping the features of the experimental protocol to the features of the target field application(s) of Tullock contests.

JEL classifications: C72, C91, D72, D83.

Keywords: lottery, learning, mechanism equivalence, 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)

There is an active literature studying contest games using laboratory experiments. (Dechenaux et al., 2015) The workhorse game form in these experiments has been the model of Tullock (1980). Under the Tullock contest success function (CSF), the ratio of the chances of victory of any two contestants is given by the ratio of their bids, raised to some exponent. Most experimental studies using the Tullock form report that bids in the contests exceed the levels predicted by the Nash equilibrium for risk-neutral contestants, both initially and after repeated experience with the game.

The maturity of this literature on experimental contests is reflected by the standardisation of papers in the area. Most papers follow a standard formula of motivating the interest of the experimental design and the Tullock CSF to applications in one or more areas such as rent-seeking, electoral competition, advertising, research and development, or sports. This versatility of potential applications is indeed one of the reasons for the interest in behaviour in contests. However, in the real world, the contest success function is rarely observable, and most experimental papers do not explore in detail the link between their motivating example(s) and the choice of the Tullock CSF in the experiment. The external validity of these experiments can be supported by an implicit argument that the Tullock CSF, for sufficiently small exponent, represents a generic environment in which success in the contest depends significantly *both* on the choices (hereinbelow *bids*) of the contestants, and on luck. The resulting game is analytically tractable; for example in symmetric setting with risk-neutral contestants, no spillovers, and the exponent set to one, there is a unique Nash equilibrium in which the contestants play pure strategies (Szidarovszky and Okuguchi, 1997; Chowdhury and Sheremeta, 2011).

Laboratory studies using the Tullock CSF also vary in their use of instructions, examples, and choice architecture in the experimental protocol. In Section 2 we summarise a review of recent protocols. Experimenters tend to design protocols with features parallel to other recent papers. This increases the comparability of results with the existing literature, at the potential risk of converging on one set of conventions for the protocol design. However, the Tullock CSF is interesting precisely because of its wide range of applicability. Although the game form generated by the Tullock CSF abstracts away from implementation detail, it is well-established that implementation details can matter because human decision-makers do attend to details of language and process. Careful empirical support is required for confident claims about robust behavioural findings across "contests" in all their domains of application.

In the literature on auctions, there is a long tradition of studying the equivalence or non-equivalence of institutions. For example, although they have distinct game forms, first-price sealed-bid auctions for a single object are equivalent strategically to Dutch descending-clock auctions, under standard assumptions, when bidders have independent private values. An extensive literature of experiments have found the sealed-bid implementation generally results in higher revenues (e.g. Coppinger et al., 1980; Cox et al., 1982, 1983; Lucking-Reiley, 1999). Subsequently, attention turned to understanding how the features of experimental auctions, *not* modeled in the game form, influence bidding behaviour. Such features include the structure of information about the outcome of the auction, even when that information should be irrelevant under standard assumptions (e.g. Filiz-Ozbay and Ozbay, 2007; Engelbrecht-Wiggans and Katok, 2008), frequency of interaction and amount of experience (e.g. Deck and Wilson, 2018), and the contextual embedding of the auction (e.g. Turocy and Watson, 2012), among others. Although there are many other points of contact between the literatures on auctions and on contests, as yet the contests literature has not developed the same body of evidence on the role of implementation in influencing behaviour. This paper is a first step in establishing such an understanding.

There is a familiar and naturally-occurring institution which implements the Tullock CSF with exponent one in a transparent way: the lottery or raffle. We conduct a series of experiments contrasting this institution with a protocol based on typical features of the experiments reported in our survey. We demonstrate that both the levels and the dynamics of bids differ significantly as a function of the institutions. In the lottery, initial bids are significantly lower, and adjustment towards (risk-neutral) best-responses are faster, with median group bids approaching the risk-neutral prediction. Although our two treatments implement the same Tullock CSF and have the same financial incentives as a function of the bids of the players, they give rise to visibly different patterns of behaviour. We replicate our results in two participant pools in two countries, at two universities with undergraduate student bodies with contrasting profiles.

We begin by surveying the details of recent experiments using the Tullock CSF in Section 2. We present the formal description of the Tullock contest game, our experimental design, and hypotheses in Section 3. The summary of the data and the results are included in Section 4. We conclude in Section 5 with further discussion.

2 Survey of protocols

Table 1 updates and extends the list of Tullock contest studies compiled in Sheremeta (2013). The rightmost column in this table shows the ratio of observed bids in the experiment to the risk-neutral Nash equilibrium bid. Most studies report bids to be above the Nash prediction. Sheremeta (2013) reports that the median experiment generates bids 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; Cason et al., 2012a; Cohen and Shavit, 2012; Mago et al., 2013) report average bids more than double the Nash level. However, bids relative to the Nash prediction do vary substantially, with some studies finding bids below or close to the Nash benchmark. This variability, which has not been carefully studied, could be attributable to differences in parameters, in terminology and choice architecture, or participant pools.

We augment the survey of Sheremeta by collecting information on the experimental protocols used for presenting the game.[1] We focus on two qualitative features of these protocols: (1) whether the instructions refer to a ratio of bids mapping into probabilities of winning the prize, and (2) what concrete randomisation mechanism, if any, is mentioned in the instructions.

[Table 1 about here.]

The column "ratio rule" 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 bid to the total bids of all contestants. A majority of studies do discuss this ratio, with many, including Fallucchi et al. (2013), Ke et al. (2013), and Lim et al. (2014) explicitly using a displayed mathematical formula similar to (1).[2]

Many designs supplement mention of a ratio or probability with a concrete description of a mechanism capable of generating the probability. For example, instructions may state that it is "as if" bids translate into lottery tickets, or other objects such as balls or tokens (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 "Lottery" in the "Example" column of Table 1. However, in carrying out the experiment itself, experimenters rarely resolve the outcome of each period using an explicit lottery draw presentation.

An alternative approach used in a minority of studies (Herrmann and Orzen, 2008; Morgan et al., 2012; Ke et al., 2013; Shupp et al., 2013) is a spinning lottery wheel. On this wheel, bids are mapped proportionally onto wedges of 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 Shupp et al. (2013) find bids below the equilibrium prediction.

Based on this survey, the emerging standard of recent years, which we refer to as the *conventional* approach, is for a Tullock contest experiment to

1. introduce the game in terms of a probability or ratio;

[1] We were unable to obtain instructions for a few 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.

2. give a lottery as an example mechanism, but

3. not to play out the lottery explicitly when determining the winner of the prize in each game.

The introduction of the conceit of the lottery mechanism suggests that the total *counts* or frequencies of tickets are helpful for participants to understand and process the strategic situation. Differences in processing probabilistic (ratio) versus frequency (count) information have been studied extensively in psychology. For example, Gigerenzer and Hoffrage (1995) proposed humans are well-adapted to manipulating frequency-based information as this is the format in which information arises in nature.

However, the lottery is precisely the institution which occurs in the field, with which participants will typically be familiar, and which implements exactly the Tullock CSF. Our *ticket* treatment implements this institution throughout the instructions and play of the game. The instructions introduce the game only as a lottery, in which bids translate into individually-numbered tickets. When playing the game, ticket numbers are assigned, and in realising the outcome of the game the number of the winning ticket is revealed. The play of the game therefore matches the description in the instructions. It likewise corresponds closely to the implementation of real-world lotteries, in which tickets are often collected into a container such as a large drum, with one selected at random to determine the winner.

3 Experimental design

The Tullock lottery contest game we study is formally 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 receives the prize is given by

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

If players are risk-neutral, they maximise their expected payoff $u_i(b) = vp_i(b) + (\omega - b_i)$. The unique Nash equilibrium is in pure strategies, with $b_i^{NE} = \min\left\{\frac{n-1}{n^2}v, \omega\right\}$ for all players i.

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. Formally, therefore, the 4 participants in a group play a repeated game of 30 periods, with a common public history.[3] By standard arguments, the unique subgame-perfect equilibrium of this supergame interaction is to play the $b_i^{NE} = 30$ in all periods irrespective of the history of play.[4]

We contrast two treatments, the *conventional* treatment and the *ticket* treatment, in a between-sessions design. The instructions for both treatments introduce the game as "bidding for a reward."[5] In the conventional treatment, the instructions explain the relationship between bids and chances of receiving the reward using the mathematical formula first, with a subsequent sentence mentioning that bids could be thought of as lottery tickets. Our explanation follows the most common pattern found across the studies surveyed in Table 1. In the ticket treatment, each penny bid purchases an individually-numbered lottery ticket. To determine the winner of the prize, the number of one of those lottery tickets is selected and displayed on the screen.

The randomisation in each period was presented to participants in line with the explanations in the instructions. In conventional treatment sessions, after bids were made but before realising the outcome of the lottery, participants saw a summary screen (Figure 1a), detailing the bids of each of the participants in the group. In sessions using the ticket treatment, the explicit ticket metaphor was played out by providing the identifying numbers for each ticket purchased (Figure 1b).

[Figure 1 about here.]

There are two channels through which the implementation of the ticket treatment could lead to behaviour different from that in the conventional treatment.

1. While both mention the lottery ticket metaphor, the conventional treatment instructions discuss the chances of receiving the prize, whereas the ticket treatment uses counts of tickets purchased. An effect due to this change would be identifiable in the first-period bids, which are taken when participants have not had any experience with the mechanism or information about the behaviour of others.

[3] That is, all players in the group share the same history of which bids were submitted in each period. Because of the re-assignment of IDs in each period, the private histories differ, because each participant knows which of the four bids in each period was the one they submitted, and whether or not they were the winner in each period.

[4] Table 1 in Fallucchi et al. (2013) shows that both fixed-groups and random-groups designs are common in the literature. We choose a fixed-groups design to facilitate the focus in our analysis on the dynamics of behaviour as the supergame is played out.

[5] We provide full text of the instructions in Appendix A.

2. Feedback is structured in terms of the individually-identifiable tickets. The number of the winning ticket conveys no additional payoff-relevant information beyond the identity of its owner. An effect due to this feedback structure will be identifiable by looking at the evolution of play within each fixed group over the course of the 30 periods of the session.

We structure our analysis to look for treatment effects via both of these possible channels.

We conducted a total of 14 experimental sessions. Eight 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., 2014), and six 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 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 for simplicity.

Participants received payment for 5 of the 30 periods. The five periods which were paid were selected publicly at random at the end of the experiment, and were the same for all participants in a session.[6] Sessions lasted about an hour, and average payments were approximately £10 in the UK and $15 in the US.

4 Results

We begin with an overview of all 5,760 bids in our sample. Figure 2 displays dotplots for the bids made in each period, broken out by subject pool and treatment. Table 2 provides summary statistics on the individual bids for each treatment and subject pool. Both the figure and table suggest a treatment difference. Aggressive bids at or near the maximum of 160 are infrequent in the ticket treatment after the first few periods, but persist in the conventional treatment. Figure 3 summarises the distribution of mean bids by group over time. The aggregate patterns of behaviour are similar in the UK and US.

[Table 2 about here.]

[Figure 2 about here.]

[6] The US participants also received a $5 participation payment on top of their contingent payment, to be consistent with conventions at Purdue.

[Figure 3 about here.]

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 (MWW) rank-sum test does not reject the null hypothesis of equal distributions of these group means between the UK and US pools ($p = 0.86$, $r = 0.479$ for the conventional and $p = 0.91$, $r = 0.486$ for the ticket treatment).[7] Similarly, the MWW test does not reject the null hypothesis if the group means are computed based only on periods 1-10, 11-20, or 21-30. □

This result speaks against a hypothesis that a significant amount of the variability observed in Table 1 is attributable to differences across subject pools. In view of the similarities between the data from our 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 ticket treatment than in the conventional treatment. Both treatments significantly exceed the Nash equilibrium prediction.*

Support. For each group we compute the mean bid over the course of the experiment. The mean over groups is 51.7 in the conventional treatment (standard deviation 14.8) and 40.7 in the ticket (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 MWW rank-sum test, we reject the hypothesis that the distribution under the ticket treatment is the same as that under the conventional treatment ($p = 0.0036$, $r = 0.255$). □

[Figure 4 about here.]

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. We can look for evidence of the latter by considering only the first-period bids.

[7] The MWW test statistic between samples A and B is equivalent to the probability that a randomly-selected value from sample A exceeds a randomly-selected value from sample B. We report this effect size probability as r, with the convention that the sample called A is the one mentioned first in the description of the hypothesis in the text. Under the MWW null hypothesis of the same distribution, $r = 0.5$.

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

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 conventional treatment is 71.1, versus 56.8 in the ticket 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 MWW rank-sum test, we reject the hypothesis that the distribution under the ticket treatment is the same as that under the conventional treatment ($p = 0.020$, $r = 0.403$). □

[Figure 5 about here.]

First-period bids on average are 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. Bid levels are higher in the conventional treatment in the first period, and both treatments exhibit a trend of average bids decreasing towards the Nash equilibrium prediction.

[Figure 6 about here.]

We are interested in determining whether the ticket-based implementation also has an effect on the dynamics of behaviour over the experiment. Under the maintained assumption that participants are interested in the earnings consequences of their actions, we organise our analysis of dynamics in terms of payoffs, rather than bids themselves. Consider a group g in session s of treatment $c \in \{\text{conventional}, \text{ticket}\}$. 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[8]

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

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. The marginal earnings consequences of an incremental change in bid depends on both b_{it} and B_{it}, so a solely 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 about here.]

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 ticket treatment throughout the experiment, while any convergence in the conventional 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 ticket 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 ε to decrease among the groups in the ticket 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.[9] Consider two groups, one in the conventional treatment and one in the ticket 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 ticket treatment will be lower, that is,

[8] 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.

[9] 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.

they will move further towards an approximate mutual (expected-earnings) best response.[10]

[Figure 8 about here.]

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

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

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

where $\mathbf{1}_{c=\text{ticket}}$ is a dummy variable which takes on the value 1 for groups in sessions using the ticket treatment and 0 otherwise. 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 ticket treatment are significantly lower than for the conventional treatment, indicating a faster rate of convergence for groups in the ticket treatment from any given initial value of ε. □

[Table 3 about here.]

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 conventional treatment, and 22.5 for the ticket treatment. This observation is consistent with previous studies (Chowdhury et al., 2014) 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 in part 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.

5 Discussion

We provide a first investigation into behaviour in theoretically-equivalent implementations of Tullock contests. We demonstrate a significant difference between two implementations with features

[10]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.

drawn from the substantial existing literature on experimental contests. This literature commonly takes the risk-neutral Nash equilibrium as a benchmark and defines "overbidding" to be bids in excess of this amount. The lottery implementation lowers overbidding by approximately one-third in initial bids, and one-half over the course of 30 periods, measured relative to the conventional implementation. Although we may identify a variety of laboratory environments as implementing a Tullock CSF, the prediction that these environments are behaviourally equivalent is not supported by our data.

Our literature survey summarised in Table 2 illustrates that while greater-than-Nash average bids are the norm, there is substantial variation reported across studies. These studies have been conducted over years, with different implementation details as well as in different participant pools. Our design focuses on two institutional implementations and explicitly compares behaviour across two universities in two countries. Usefully these universities have distinct academic profiles and reputations. Purdue University is noted for its strengths in particular in engineering and other quantitative disciplines, whereas University of East Anglia has no engineering programme but is strong in areas such as creative writing.

Our results therefore suggest that much of the variation observed in Table 2 may be attributable to features of experimental implementation. To address this research question our experimental design compared two existing implementations, a typical one drawn from the literature and a lottery-based implementation which we designed to be similar to the explanation and implementation of a real-world raffle. Our study does not go on to construct additional new auxiliary implementations as additional ex-post treatments not previously observed in an attempt to identify detailed mechanisms for the differences. Nevertheless, our results do allow for some partial conclusions, and some questions for further research.[11]

We observe differences both in initial bids and in the dynamics of adjustment over the course of the experiment. The concrete feedback mechanism of a lottery with individually-numbered tickets is indeed different from a more vague mechanism in which only a winner is determined. Both mechanisms appear in applications of contest theory. The winner of a real-world lottery holds a real ticket with a specific number, and this is "why" that person is the winner, while it is not possible to identify "why" one participant defeated another in a sporting contest modeled with a Tullock CSF. The concreteness of the feedback mechanism may influence the faster dynamics of adjustment we observe in the lottery.

[11] The ex-post addition of treatments to a study risks introducing a number of confounds. In particular, we alternated sessions of each treatment to try to make unobservable participant characteristics as similar as possible between the two treatments. To add a treatment ex-post means tapping into a potentially different segment of the participant pool. For example, Xue et al. (2017) find that participants who responded to a treatment conducted later than the others were significantly less likely to report they considered themselves "good at math." A valid exploration of new hypotheses arising from our results must await a new design with a fresh selection of participants.

However, we likewise observe significantly different and lower bids in the initial period, when participants have not had any concrete feedback experience. Unless the mere anticipation of getting the feedback influences participants' introspection in their initial bids, other candidate explanations for this difference are required. The ticket institution uses a frequency-based representation, which Gigerenzer (1996) and others suggest leads to systematically different judgments relative to the probability-based representation in the conventional treatment. Experiments using an implementation like the conventional treatment often present the probability of winning using a displayed formula patterned after (1). Kapeller and Steinerberger (2013) argue that the mere introduction of a mathematical representation of an argument may hinder understanding among a significant proportion of people. Xue et al. (2017) 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. As quantitative researchers, experimental economists may suppose that the precision of a mathematical display encourages comprehension and improves control, whereas the opposite may occur.

Our non-equivalence result parallels those in the literature on first-price auctions. Just as in Tullock contests, the standard result in first-price auctions is that bids exceed the baseline risk-neutral equilibrium prediction. However, that literature has shown that levels of bids depend both on the game form and the details of the implementation of the auction in the experiment. However, the literature on contests does differ from that in winner-pay auctions in its application. The standard game forms for auctions are drawn directly from mechanisms which exist in the real world, and bidding behaviour in them is in and of itself of direct economic interest. The literature on contest experiments is inspired by the many and varied potential applications of contest models, and the Tullock model specifically, to the real world. One of our treatments draws on the lottery institution, which like auctions appears frequently in practice. However, Tullock's model is also applied more metaphorically, in settings like electoral competition or sports, in which the success function is a convenient and tractable approximation in settings in which the randomness or uncertainty which contributes to the outcome of the contest is more opaque. Our results suggest that, behaviourally, the "Tullock contest" is not a single mechanism, but one in which implementation features play a non-negligible role in shaping behaviour.

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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 conventional 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 ticket 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:

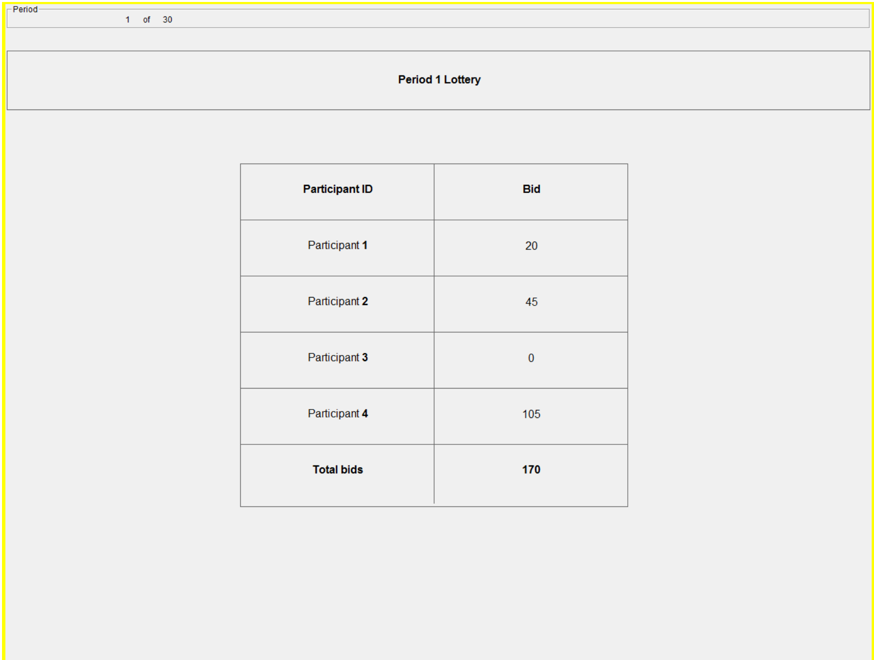
Participant ID	Bid	Ticket Type	Total tickets	Ticket number(s)
Participant 1	80	Type 1	80	1 - 80
Participant 2	6	Type 2	6	81 - 86
Participant 3	124	Type 3	124	87 - 210
Participant 4	45	Type 4	45	211 - 255

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.

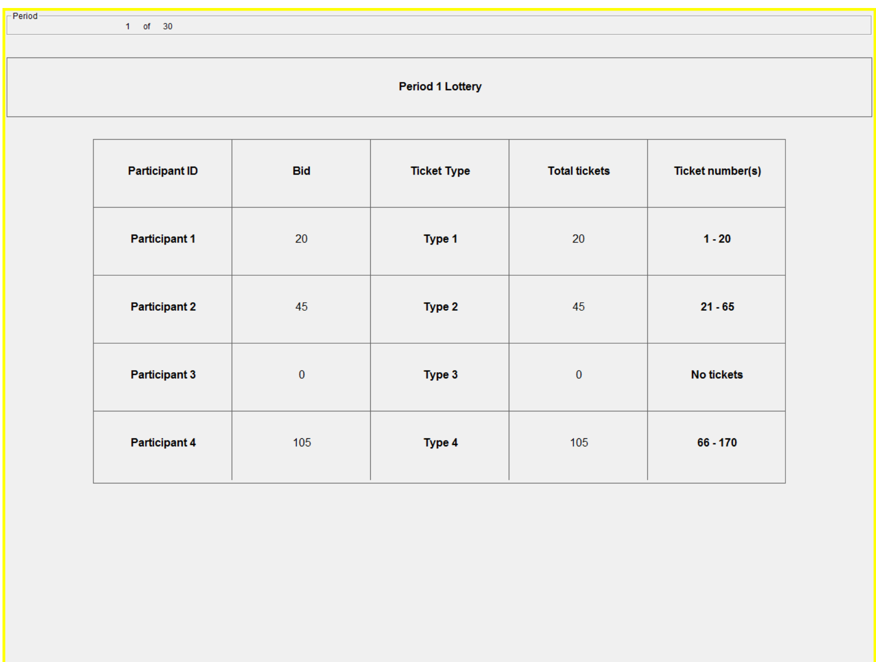


Period
1 of 30

Period 1 Lottery

Participant ID	Bid
Participant 1	20
Participant 2	45
Participant 3	0
Participant 4	105
Total bids	170

(a) Conventional treatment



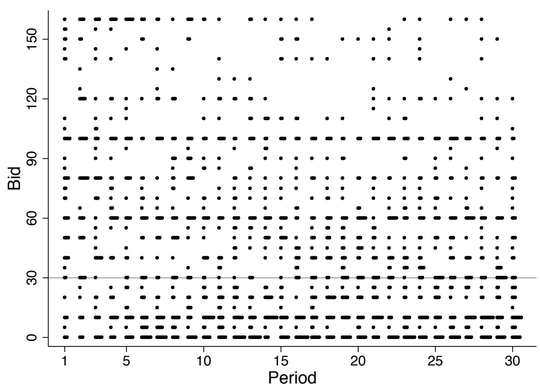
Period
1 of 30

Period 1 Lottery

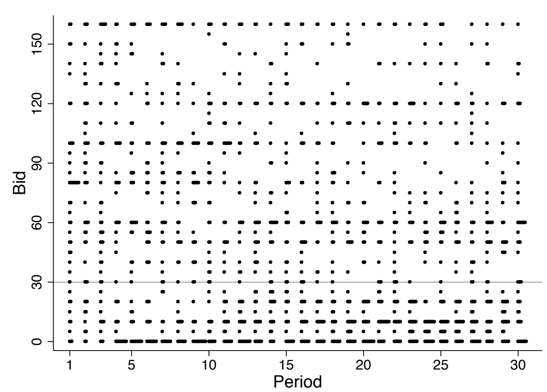
Participant ID	Bid	Ticket Type	Total tickets	Ticket number(s)
Participant 1	20	Type 1	20	1 - 20
Participant 2	45	Type 2	45	21 - 65
Participant 3	0	Type 3	0	No tickets
Participant 4	105	Type 4	105	66 - 170

(b) Ticket treatment

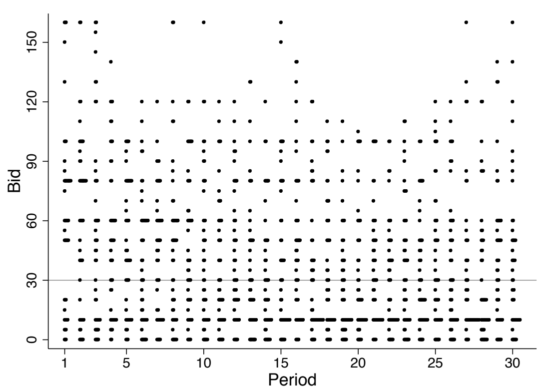
Figure 1: Comparison of bid summary screens



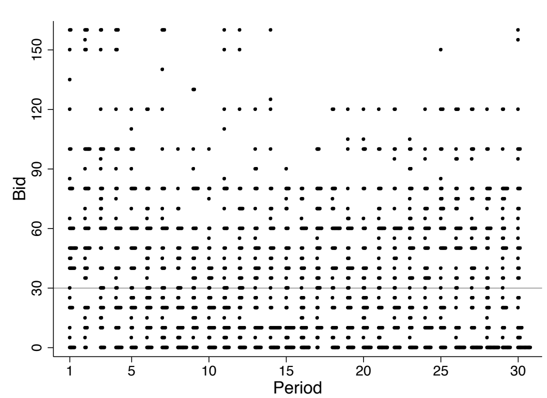
(a) Conventional, UK



(b) Conventional, US

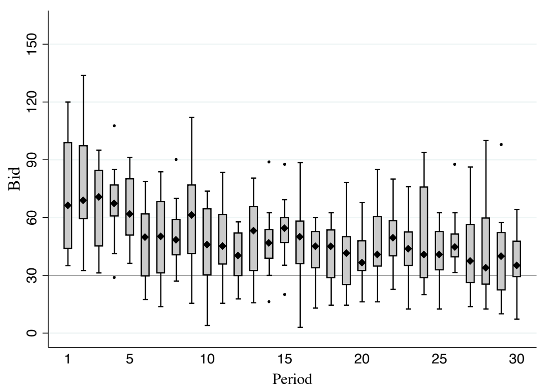


(c) Ticket, UK

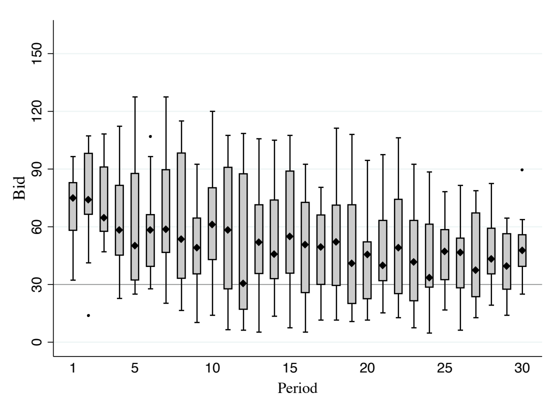


(d) Ticket, US

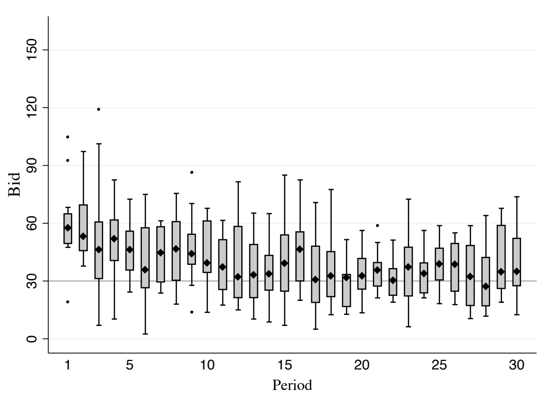
Figure 2: All bids by period, grouped by subject pool and treatment. Each dot represents the bid of one participant in one period. The Nash equilibrium bid of 30 is indicated by a horizontal line.



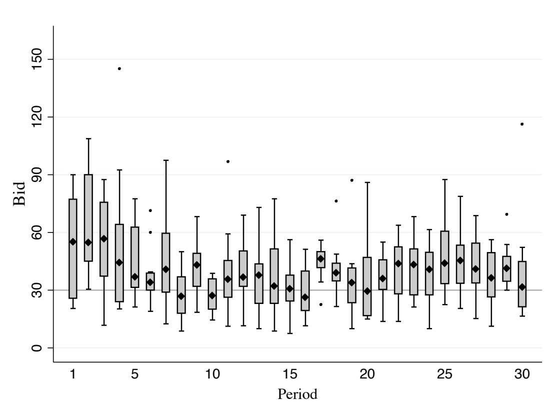
(a) Conventional, UK



(b) Conventional, US



(c) Ticket, UK



(d) Ticket, US

Figure 3: Distribution of group mean bids, by subject pool and treatment. 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.

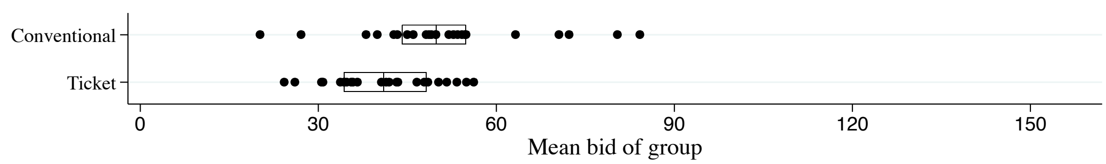


Figure 4: Distribution of mean bids for each group over the experiment. Each dot represents the mean bid of one group.

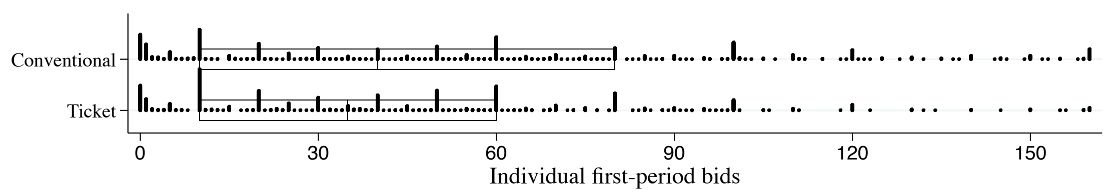
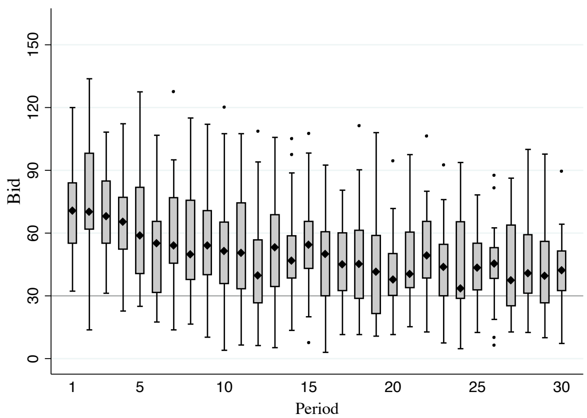
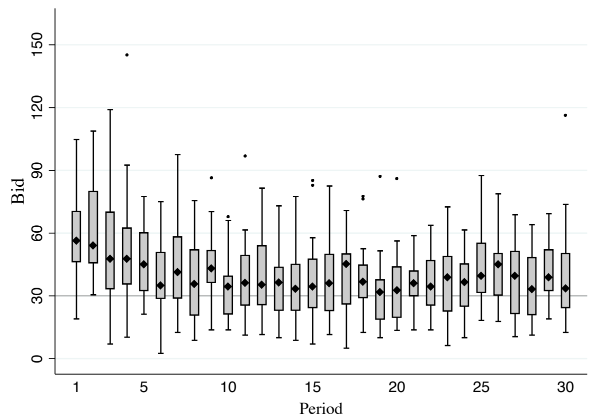


Figure 5: Distribution of first-period bids for all participants. Each dot represents the bid of one participant. For each distribution, the superimposed box indicates the median and the lower and upper quartiles.

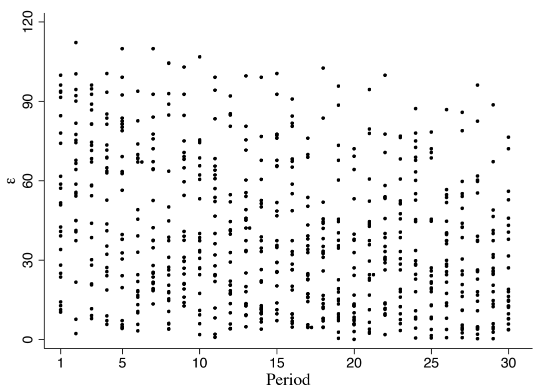


(a) Conventional

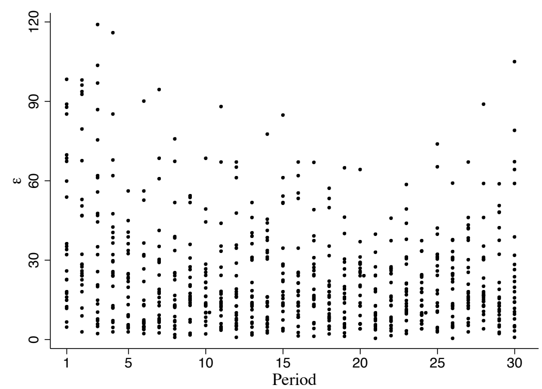


(b) Ticket

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.



(a) Conventional



(b) Ticket

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.

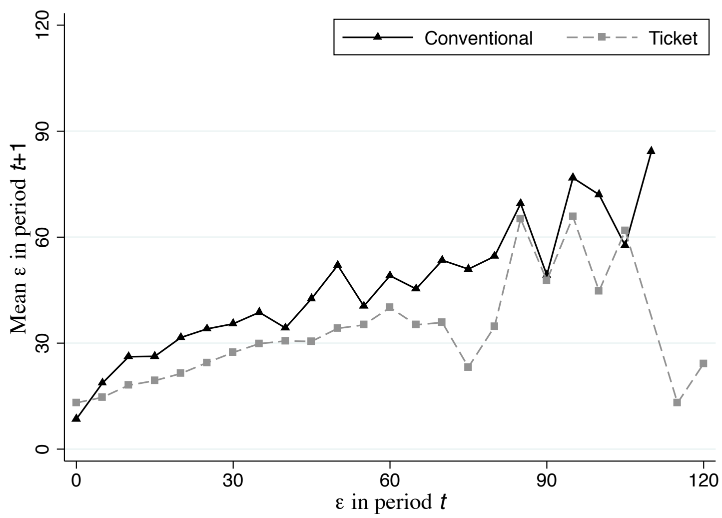


Figure 8: Expected value of disequilibrium measure ε in next period, as a function of a group's current ε.

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	3.46
Schmitt et al. (2004)	Static	Yes	Lottery	1.76
Herrmann and Orzen (2008)	Direct repeated	No	Wheel	2.05
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	—	2.52
Mago et al. (2013)	High r	Yes	Lottery	1.90
	High r + IP	Yes	Lottery	1.51
	Low r + IP	Yes	Lottery	2.73
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	Wheel	0.73
Brookins and Ryvkin (2014)	Complete symmetric	Yes	—	1.42
Chowdhury et al. (2014)	PL	Yes	—	1.75
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.

	UK	US	All
Conventional	49.85	53.56	51.70
	(43.77)	(48.41)	(46.18)
	$N = 1440$	$N = 1440$	$N = 2880$
Ticket	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.

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