Rituals of Reason: Experimental Evidence on the Social Acceptability of Lotteries in Allocation Problems*

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Abstract

Procedures designed by economists to optimally allocate scarce resources such as school places for students typically rely on the use of lotteries as a tie breaker. This feature is often criticized by the rest of society and has blocked the implementation of many of those procedures. In this paper, we study collective preferences towards the use of random procedures in allocation mechanisms. We report the results of two incentivized experiments in which subjects choose a procedure to allocate a reward to half of them. The first possibility is an explicitly random device: the result of a lottery. The second is an equally unpredictable procedure with identical rate of success, but not involving any explicit randomization. We identify an aversion to lotteries, in particular against procedures that are reminiscent of meritocratic ones. In line with the literature, we also find evidence of a control premium in most procedures. Our results provide a simple guidance to make random tie-breakers more socially acceptable: use equally unpredictable procedures that are not explicitly lotteries, and give participants control on the procedure.

Keywords: lotteries, mechanism design, allocation problems, procedures, tie-breaking rule

JEL-Code: D01, D78, D91

A frequent issue in allocation problems is the choice of a procedure to break ties when no better criterion is available. While many of the solutions provided by economists involve a role for randomization and the use of lotteries,[1] random tie-breakers are often

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[1]Randomization is widely accepted by economists as the fairest way to deal with ties in allocation problems, albeit there is a debate on the best way to implement it (see Budish et al., 2013 for a review, and Erdil and Ergin, 2008; Kesten and Ünver, 2015; Abdulkadiroglu et al., 2019 for the case of school choice). Lotteries can also have efficiency benefits: Basteck et al. (2021) show for instance that the introduction of a lottery quota strengthens truth-telling in the deferred acceptance mechanism.

unpopular in practice. In USA law, the Administrative Procedure Act (APA) forbids the use of random devices in administrative decisions.[2] Several European countries have witnessed a large opposition to school choice mechanisms involving randomization.[3] The UK government's "school admission code (2014)" explicitly states (p.14) that "local authorities must not use random allocation as the principal oversubscription criterion".[4] Medical researchers often report difficulties to run random control trials (RCT), as doctors oppose allocating a treatment at random. They only see randomization as ethical if a state of *equipoise* is reached: a consensus that none of the possible outcomes of the lottery is *ex ante* better than the others (Lilford and Jackson, 1995). It is however very difficult for a doctor not to seek an additional rationalization instead of declaring two possible treatments to be *ex ante* equivalent (Donovan et al., 2014). Even economists are reluctant to use lotteries for themselves. To allocate funding, academics spend a lot of time ranking research projects of similar quality and reach conclusions that are as good as random (Cole et al., 1981; Graves et al., 2011; Pier et al., 2018).[5]

The goal of this paper is to understand the social acceptability of lotteries in allocation problems. In two incentivized experiments using a neutral framing, we ask subjects to choose between two unpredictable procedures, one clearly random and the other not, to allocate a reward within their group. We document an aversion to the explicit use of lotteries, in particular when the alternative follows the *rituals of reason* (Elster, 1989, p.37):[6] albeit unpredictable, it is reminiscent of meritocratic procedures and subjects may be able to interpret it as such. We also replicate the result that subjects have a preference for control over most procedures (Owens et al., 2014; Bartling et al., 2014; Bobadilla-Suarez et al., 2017; Ferreira et al., 2020). This concept is related to the idea of *illusion of control* (Langer, 1975; Sloof and von Siemens, 2017), which explains this preference by overconfidence. By running treatments with and without control on all procedures, we however show that control alone does not explain the aversion to lotteries we identify.

Our subjects win a prize if they rank among the top half in the procedure chosen by their group. Experiment 1 is pre-registered and uses a representative sample of the

[2]The APA [requires court to] set aside agency action that is "arbitrary, capricious, [or] an abuse of discretion." (Vermeule, 2015, p.475) The US Supreme court (Judulang v. Holder, 132 S. Ct. 476, 485 [2011] [Kagan, J.]) rules that the use of a random device is arbitrary and thus illegal (p.11), regardless of how costly it is to provide a rationalization (p.21).

[3]In France, the system of allocation of students to universities (APB) used lotteries to break ties when capacity was reached. It was criticized and replaced in 2018 by another mechanism with a lot more criteria. In French-speaking Belgium, a 2009 attempt to randomly allocate students in oversubscribed high schools lasted only a year after being dubbed by some parents and the media the "lottery law" and replaced by a set of criteria still in use today.

[4]This debate followed the introduction in 2008 of a random tie-breaking rule in Brighton and Hove, after a criterion of distance to the school. Lotteries are also used as last-resort criteria in cities such as Birmingham. The issue remains a political talking point, and the 2017 Conservative Party manifesto explicitly committed the government to (p.50): "never introduce a mandatory lottery-based school admissions policy."

[5]Proposals to save time and money by adding random tie breakers to the criteria used for peer review (Greenberg, 1998; Brezis, 2007; Fang and Casadevall, 2016; Roumbanis, 2019; Avin, 2019) are largely ignored (Barnett, 2016). Notable exceptions are limited to small or exploratory grants, such as the Health Research Council of New Zealand "Explorer" grants, The Volkswagen fundation in Germany and the small grants from the British Academy.

[6]"We have a strong reluctance to admit uncertainty and indeterminacy in human affairs. Rather than accept the limits of reason, we prefer the rituals of reason."

US population.[7] We run two treatments, each with a different alternative non-random procedure. The explicitly random procedure uses the results of a public lottery in Washington DC. The first non-random procedure is a variant of a rock-paper-scissors (*RPS*) game, designed to be unpredictable and devoid of psychological thrill: we ask subjects to give us in advance a list of five moves, that we then play against the moves of all other participants. To ensure no difference in the entertainment value of our procedures, all subjects play RPS regardless of the chosen procedure, and learn the result alongside the lottery one. Despite having to trust that we implement the procedure correctly, 78% of the subjects have a preference for *RPS* over the more transparent lottery. In the second non-random procedure, *Time*, we ask subjects to provide a time of the day, and then implement an intractable and unnecessarily complicated algorithm. Despite being unpredictable, non-transparent and not reminiscent of any existing meritocratic procedure, a significant majority (55.9%) still chooses *Time* over the lottery. These preferences are strict: subjects are willing to pay in order to choose the non-random procedure.

In Experiment 2, we use a large online non-representative sample. Subjects still choose between our lottery and a non-explicitly random procedure, and we run all treatments – including the lottery – with and without control. We also use a third non-random procedure reminiscent of meritocratic ones, *Paintings*, in which subjects have to guess, from pairs of paintings selected by the second author of this paper, the ones preferred by the first author.

The results of our non-representative sample are consistent with the representative one of Experiment 1 for the two treatments used in both, albeit with a smaller preference for non-random treatments. The results for *Painting* are not significantly different from those of *RPS*. Perhaps surprisingly, we find that while subjects have a preference for control in most procedures, the largest impact is for the lottery (an 11pp difference in the share of subjects choosing that procedure). This result is particularly policy-relevant as adding control on the lottery is something we are not aware exists in any real-world allocation mechanism.

Our paper focuses on a difficulty with the social acceptability of lotteries in the context of the collective choice of a tie-breaking rule in an allocation procedure. There is widespread evidence however that lotteries are perceived, in general, as fair, and help make unequal outcomes socially acceptable (Bolton et al., 2005; Schmidt and Trautmann, 2019). Individuals also often choose to rely on a randomization device to make their choice when indifferent or indecisive between two alternatives (see Agranov and Ortoleva, 2021 for a recent example). This *preference for randomization* has for instance been observed in university choice (Dwenger et al., 2018), an allocation problem in which many subjects seem however reluctant for the mechanism itself to use lotteries.

Lotteries appear easier to implement when they apply to allocation problems where the decision-maker is not a subject of the allocation. For instance in the green card lottery or the random tie-breaking rule of the short-term skilled migration programs H-1B in the USA (Pathak et al., 2020), the participants are, by definition, not US citizens. Their preferences have no direct impact on the social acceptability of the procedure by the public voting for it. In the allocation of social housing, the decision to randomize is mostly made by people unlikely to apply for one. When economists run RCTs, they expect the treatment to benefit a share of the population, and to have no effect or even a

[7] Both our experiments have received IRB approval from Lancaster University and the code and data will be made available in a public repository before publication. Our pre-registration is available at https://aspredicted.org/3yy5p.pdf

negative one on those who were not treated (Aldashev et al., 2017; Deaton and Cartwright, 2018; Heckman, 2020). Whether the subjects of the RCTs would have preferred another allocation mechanism is generally not discussed. In other famous cases were lotteries are implemented, it is not as a final tie-breaking rule. The military draft in the USA offers a large number of exemptions after the lottery numbers are drawn, and those were widely used during the Vietnam war (Bailey and Chyn, 2020). In Rotating and Saving Credit Associations (ROSCAs), the lottery (when there is one) typically happens at the beginning of the process (Anderson and Baland, 2002).

Our results are related to the concept of *outcome bias*, a tendency to interpret success by merit and effort and ignore the role of luck (Loewenstein and Issacharoff, 1994; Frank, 2016; Brownback and Kuhn, 2019). Hence, subjects may be willing to interpret any device that is not explicitly a lottery as more meritocratic, even if it is in practice completely unpredictable. Our results are also a form of *source uncertainty*: individuals treat uncertainty differently depending on the mechanism generating it (see, for instance, Heath and Tversky, 1991; Fox and Tversky, 1995; Abdellaoui et al., 2011).

Finally, our work is related to the survey evidence of Keren and Teigen (2010) on preferences for lotteries on determining the outcome of hypothetical decisions involving other people. Oberholzer-Gee et al. (1997) also report an opinion poll suggesting that a market mechanism is the only less acceptable procedure than a lottery for the allocation of a nuclear waste facility.

In Section 1, we present the design of the two experiments. Section 2 outlines the main results. We conclude and develop two main suggestions on how to make random tie-breaking rules socially acceptable in Section 3: to make a larger use of unpredictable but "reasonable" procedures, and to give participants as much control as possible on the chosen one.

1 Experimental design

In both experiments, all subjects receive a fixed payment ($1 in Experiment 1 and $0.80 in Experiment 2), and half of them receive an additional payment ($2.00 and $1.60).[8] We also offer additional smaller incentives for specific questions such as the elicitation of beliefs and of the intensity of preferences. 577 participants completed Experiment 1 and 1,324 Experiment 2. The median time spent in Experiment 1 is 5 minutes and 11 seconds, and 4 minutes and 41 seconds in Experiment 2, for a median hourly payment of $20.87 and $19.71 respectively. Following the results of Snowberg and Yariv (2021) who found no impact of doubling the rewards for a battery of experimental games, we do not expect our results to depend on their exact level.

The experiment is composed of three parts. In the first part, we introduce two procedures to allocate the additional payment: one is explicitly random, and the other is not. Subjects also choose their strategies for the procedure(s) in which they have control (if any). In the second part, we ask them to vote on which procedure to use to allocate a reward to half of the members of their group. We focus on preferences for allocation problems in which the decision makers are a subject of the mechanism, but not the only one. We do so in contrast to spectator settings in which subjects make decisions for the others without monetary consequence for themselves, and with individual preferences in which

[8] The higher nominal amounts in Experiment 1 (August 2023) than in Experiment 2 (July 2021) reflect the inflation during the period.

Table 1: Size of the sample for each treatment.

		Experiment 1	Experiment 2	
		Lottery		
Non-random procedure	Control?	No	No	Yes
RPS	No	-	152	140
	Yes	291	98	99
Paintings	No	-	123	114
	Yes	-	97	89
Time	No	-	106	91
	Yes	286	102	113

Note: When yes for control, subjects chose the sequence used in the corresponding procedure.

a subject's choice only apply to themselve. In Experiment 2, we implement the choice of the majority. In Experiment 1, we also measure the intensity of preferences. To do so, we ask subjects for their preferred procedure and the smallest amount we should pay them to change their choice. We then implement the choice of a subject chosen at random.[9] In the third part, we ask them incentivized questions on their beliefs about their ranks in the mechanism, and one incentivized measure of their ambiguity aversion (Experiment 2 only). Finally, we ask non-incentivized demographic and feedback questions. We provide screenshots of the complete instructions in Online Appendix F.

The first experiment in chronological order is Experiment 2. We ran it on Amazon Mechanical Turk (AMT), on June 22, July 7 and August 31, 2021.[10] In this experiment, we randomly allocate subjects among 12 treatments of on average 110 participants. The number of treatments corresponds to all possible choices between one of our three non-random procedures and the random one, with or without control. We then pre-registered Experiment 1 and ran it on August 14, 2023, using Prolific on a representative sample of 600 USA residents.[11] We ran two treatments of this experiment, one with *RPS* and one with *Time* as a non-random procedure (with control), offered alongside the lottery without control. We excluded subjects who did not complete the experiment, in line with our pre-registration. We detail the sample in Table 1. We ran both experiments using oTree (Chen et al., 2016).

On top of testing pre-registered hypothesis, the objective of Experiment 1 was to estimate the level of support among the general US population for the most and least popular non-random procedures. We can then combine the results of Experiment 1 with

[9]We incentivize truthful revelation of the intensity of preferences in the following way. Denote by T the smallest amount a subject reports to be willing to accept to change her choice. If the subject is selected, we draw a random number $D \in (0, \$2)$. If $D < T$ we keep the initial choice of the subject. If $D \geq T$ we transfer an amount D to the subject and implement the other procedure. We could not use the standard voting procedure of Experiment 1, because measuring the intensity of preferences would then imply measuring the perceived probability of being pivotal.

[10]We also ran a pilot experiment on April 29 and May 5, 2020, detailed in a previous version of this work, Lancaster Economics Working Papers Series, vol. 2022/005.

[11]The pre-analysis plan report is available in a separate online appendix.

Experiment 2 to understand the marginal impact of adding or removing control.

For all the treatments, we provide subjects with a personal ID code. We then posted all the strategies and results on a website owned by Lancaster University, to allow subjects to check their strategies, as well as our procedures (see screenshots in Online Appendix F). In Experiment 1, we also sent an email to all subjects with the detail of their payments, as well as their results in the two procedures.

We first describe the explicitly random procedure and then the three non-random ones. Each procedure consists in ranking subjects based on a sequence of five actions.

Our explicitly random procedure, the *Lottery*, is a bet on whether each of the 5 numbers in the next-day results of a state lottery, the DC-5 lottery, are odd or even. As the lottery is run by a third party, our subjects should in theory trust it as being the most transparent we offer. It also satisfies the five properties identified by Eliaz and Rubinstein (2014) as characterizing a fair random procedure. In the treatment without control, we draw a sequence for them, as public authorities do in actual school choice lotteries.[12]

The following three procedures *RPS*, *Paintings* and *Time* do not exhibit explicit randomness in the allocation. We however argue (and show in Section 2.2) that they are in practice unpredictable. The objective of *RPS* and *Paintings* is to respect Elster (1989)'s rituals of reason: subjects make decisions that can be interpreted as meaningful and are easy to understand, although they have no predictable influence on the outcome. In contrast, *Time* is designed to appear arbitrary and meaningless.

In *RPS*, each action is taken from the set {Rock, Paper, Scissors}. In the treatment without control, we provide the subjects with a sequence of actions. In the treatment with control, we let them choose their actions. We play the actions of each subject against all other subjects in their treatment. As in the traditional game, Rock wins against Scissors, Scissors against Paper, and Paper against Rock. A subject wins if they win more rounds than their opponent.[13] We then rank all participants by their number of wins. The main difference with a traditional RPS game is that all strategies are chosen in advance and a subject uses the same strategies against all other subjects. The objective is to ensure people do not choose RPS for its entertainment value: regardless of the chosen procedure, they get to "play" RPS in the same way.

In *Paintings*, we explain to the subjects that one of the two experimenters, Renaud, has chosen 5 pairs of paintings, each pair by the same artist, and that the second experimenter, Elias, has chosen in each pair his favourite painting. To win, subjects have to guess the paintings chosen by Elias. We rank participants according to the number of paintings they have guessed correctly. The first half gets the reward. In case of tie, we use the first pair of paintings, then the second and so on. In the treatment without control, we provide the subjects with a sequence of actions. In the treatment with control, we let them choose their actions. Subjects could download a password protected PDF copy of Elias' choices and we revealed the password alongside the results of the experiments.

We designed the third non-random procedure, *Time*, in a way to make it look as unreasonable as possible: an arbitrary, complicated, and intractable alogrithm. In the treatment without control, we provide subjects with a code corresponding to the last five

[12]For instance, the official website https://www.schools.nyc.gov reports that a real example of random number generated by the NYC school lottery is B51920AF-F1C6-40EC-8E9A-3E1E50CB13BB. A student or parent can log into their account and see the number, but has no influence on it.

[13]In case of a tie, the first winner of a round wins the game. In the rare event where both players have chosen exactly the same strategy, we consider it as neither a win nor a loss.

Table 2: The influence of control on the percentage of participants choosing the non-random procedure.

	Experiment 1	Experiment 2			
Control in					
Non-Random	Yes	Yes	Yes	No	No
Lottery	No	No	Yes	No	Yes
Non-random procedure					
RPS	78.0%	74.5%	58.6%	65.1%	51.4%
Paintings	-	66.0%	59.6%	62.6%	51.8%
Time	55.9%	46.1%	41.6%	48.1%	37.4%

Note: Share over all subjects in the sample.

digits of a time, in hours, minutes and seconds. In the treatment with control, we asked subjects to choose their time, which is then transformed into their five digits code. We rank the code of all subjects using a long list of rules loosely related the number of odd digits. The full algorithm is given in Online Appendix E.

2 Results

2.1 Choice of Procedure

We report the share of subjects choosing the non-random procedure in each treatment in Table 2. First, we look at the two treatments of the pre-registered Experiment 1, based on a representative sample of the US population. When *RPS* is offered with control, and *Lottery* without, 78% choose the former. This serves as a baseline of the collective preference for a non-random procedure following the rituals of reason. When *Time* is offered, 56% choose it over the lottery. In both treatments, we can reject the hypothesis that this share is equal to 50% (with $p < 0.001$, using a one sample two-sided t-test). As predicted, *RPS* is chosen by a majority of subjects and the share of subjects choosing *RPS* over the lottery is significantly higher than the share choosing *Time*.[14] The fact that *Time* is chosen by a majority of subjects however goes against our predictions, showing stronger than expected aversion to lotteries.

The results are not statistically different when removing the small number of subjects (3.1% of our sample) who revealed being indifferent between the two procedures by asking for a minimum amount of $0 to change their choice. We detail the intensity of preferences and run robustness checks without the indifferent subjects in the Online Appendix A.

As expected from the literature, preferences for the different procedures change with control. The two first columns of Experiment 2 in Table 2 correspond to the treatments in which subjects have control over the non-random procedure. The second and fourth columns correspond to the treatments in which they have control over the lottery. The results show that control indeed matters for the lottery, as on average subjects choose the lottery with control 50% of the time, compared to 39% without in Experiment 2.[15]

[14] The p-value of the Fisher test of the proportion of subjects choosing the non-random procedure in both treatments being different is <0.001.

[15] The p-value of the Fisher Exact test of equality of the proportion is < 0.001.

It is not the case in general for the non-random procedures, with a p-value of the Fisher test of 0.27.

When we separate the procedures, control does not matter for *Time* (p-value of 0.92) but matters for the procedures following the rituals of reason with an increase in choice of these by 7pp (p-value of the Fisher test of 0.046). This is nonetheless lower than the 11pp increase in the choice of the *Lottery*. A possible explanation for the higher preference for control in lotteries than in non-random procedures is that subjects may misunderstand the probability of some sequences in a lottery, such as five Even for instance. We however show in the Online Appendix D that the actual sequence has no influence on the choice of procedure in the treatments without control over the lottery.

The results of Experiment 2 therefore suggest that roughly half of the social preference for *RPS* observed in Experiment 1 - the difference between the share of the subjects choosing that procedure and 50% - can be explained by control, and the other half by following the rituals of reason. While keeping the same treatment conditions on control, preference for *RPS* over *Lottery* is roughly similar in Experiment 2 and 1 (the p-value of the Fisher test is 0.48). Similarly, the share of subjects choosing *Time* is higher in Experiment 1, but not significantly so (p-value of the Fisher test is 0.083). We should thus interpret the results of Experiment 2 as providing an upper bound for the social acceptability of lotteries in the USA.

2.2 All our procedures are unpredictable

To verify our conjecture that the results of all our procedures are unpredictable, we compare our incentivized measures of beliefs with the actual performance of subjects in Table 3. We find no significant correlation. Each row corresponds to a different treatment, denoted by the name of the corresponding non-random procedure.

For *RPS* in particular, we were concerned that some subjects could exploit a well-known bias of RPS: too many players choose Rock (37.3% in our sample), and not enough Scissors (27.9%), in particular in the first round (44.5% and 19.8% respectively), so that Paper (chosen by 34.8% of subjects) gives a higher probability of winning.[16] While we cannot rule out that some participants correctly guessed they were going to win, on aggregate their predictions are as good as random.

2.3 Beliefs and overconfidence

We now look at the role of beliefs and overconfidence in determining individual choices. In both experiments, what matters is a subject's expectation of being in the first half of the participants of their treatment group. In Experiment 1, where subjects have control over non-random procedures only, we observe some "illusion of control:" 72.5% of our subjects expect to win *RPS*, versus 56.0% the *Lottery* in one treatment, and 62.9% expect to win *Time* versus 53.5% the *Lottery* in the other.[17] In Experiment 2 however, where all

[16] The bias is similar to the choices of players on the website https://roshambo.me (We thank Lasse Hassing for giving us access to their data). They choose Rock 35.3% of the time in the first round, and Scissors 29.1% of the time.The difference in magnitude is likely to arise because players on the website chose to play a game of RPS, contrary to our subjects, and play actual games instead of giving a set of five choices in advance. They are therefore more likely to play regularly and have a notion of the best strategies, at least for some of them.

[17] The difference are significant according to a Fisher exact test of equal proportions, with a p-value < 0.001 in the first case and of 0.027 in the second.

Table 3: Proportion of subject believing that they will win or lose in a procedure who win in that procedure.

	Non-Random			Lottery		
Treatment	Win	Lose	P-value[1]	Win	Lose	P-value[1]
Experiment 1						
RPS	41.2%	53.6%	0.067	52.3%	49.1%	0.637
Time	49.1%	52.8%	0.624	54.1%	47.1%	0.239
Experiment 2						
RPS	49.5%	50.5%	0.853	48%	51.5%	0.462
Paintings	52.7%	49.5%	0.541	52.6%	51.2%	0.843
Time	53.5%	48.8%	0.369	48.9%	52.2%	0.552

Note: We have a similar table for subjects losing in the procedure.
[1] P-value of the Fisher test of the proportion of subjects saying they will win or lose in a procedure predicting if they win or lose.

procedures are offered with and without control, we see no significant difference between the expectations of success *RPS* and the *Lottery* and *Time* and the *Lottery*, respectively.[18]

To explore further to what extent choices are driven by preferences or beliefs, Table 4 reports the difference between the choices of subjects who expect to win in the non-random procedure but not in the lottery, and, conversely, of those who only expect to win the lottery. The numbers for Experiment 2 aggregate the different levels of control.

In both experiments, we see that subjects act to a certain extent according to their beliefs, as they are significantly more likely to pick *RPS* if they expect to win in that procedure only, than if they expect to only win in the *Lottery*. This is not the case for *Time*, a procedure for which it is arguably more difficult to convince yourself of your ability to win.

Beliefs are however far from explaining everything. In Experiment 1, we see for instance that a significant majority of subjects (66.7%) prefer *RPS* even when they expect to only win in the *Lottery*. This preference is a strict one: they are willing to pay in order to choose a procedure in which they nonetheless think they are less likely to win.[19]

In the Online Appendix C, we confirm all these results in a regression analysis. We also provide additional information on the subjects' demographics and relative preferences, as well as more detail on our sample characteristics in Online Appendix B.

3 Discussion and Conclusion

We start this section by providing two recommendations on the implementation of random tie breakers based on the results of our experiment and the previous literature. They are aimed at any designer willing to implement a random (explicitly or not) tie-breaking rule.

[18] P-value of the same Fisher tests are 0.74 and 0.53 respectively.

[19] As we offered subjects a binary choice of a bet on whether they expect to win or lose in a given procedure, we cannot rule out that some subjects expect to win in both procedures with an identical probability. However, if they reveal a strictly positive willingness to pay to keep their chosen procedure, we can conclude that they have a strict preference for one procedure over the other that is not explained by differences in expectations of winning.

Table 4: Percentage of subjects who choose the non-random procedure conditional on their beliefs of winning only in one procedure but not the other.

Treatment	Subjects expect to win only in		
	Non-random	Lottery	P-value[1]
Experiment 1			
RPS	84.9%	63.2%	0.010
Time	63.8%	45.2%	0.117
Experiment 2			
RPS	79.1%	46.2%	<0.001
Paintings	67%	43.7%	0.003
Time	50.6%	45.3%	0.530

Note: The first column represents subjects expecting to win in the non-random procedure but not in the lottery, the second column the reverse.
[1] P-value of the Fisher test of the proportions of subjects who believe they will win in one procedure but not the other who choose the non-random procedure being equal.

We then conclude by discussing limitations of the paper and avenues for future research.

Our first recommendation is that mechanism designers looking for a random tie-breaking rule should aim first for unpredictable procedures that are not explicitly random, but look reasonable enough to be more socially acceptable. In the case of university admission, tie-breakers are often needed between students with similar high school grades. In the United States, as in the United Kingdom and the Republic of Ireland, the high school marking standard indeed places students in broad categories, usually in letter form. One of the reasons for such a standard is that it allows conveying information about student achievement without putting the burden on markers to provide an impossible level of precision (Schneider and Hutt, 2014). While providing more precision may be seen as unfair or arbitrary in the context of high school results, it may actually prove useful to avoid requiring an additional tie-breaking rule for the mechanisms using these results. In other words, if the difference between a 787/1000 and a 788/1000 is not meaningful and does not tell us anything about the relative abilities of two students, it nonetheless provides a non-random procedure following the rituals of reason on which students had some control.

Simple tweaks can also be used to reduce the probability of ties without increasing the burden put on markers. In the allocation mechanism of high school students to university in the Republic of Ireland, students are allocated a random number to break ties among those who have an identical CAO score, a metric composed of the sum, over 6 subjects, of their leaving certificate, composed of letter grades translated into marks out of 100.[20] Following complaints that random tie-breaking procedures were unfair and too frequent, the Irish government revised that metrics in 2015 with as objective to reduce the number of ties. Before the reform, possible marks on a given subject where all multiple of 5. After the reform, the possible marks are {0,37,46,56,66,77,88,100}, so that summing them up leads to much fewer ties. The government does not hide that those numbers have been

[20] Central Application Office, "Random number - How it works" on cao.ie, retrieved August, 2, 2021.

chosen with the main objective of reducing ties, and have no particular meaning.[21]

Our second recommendation is that, when the only choice is to have a lottery, it may be worth trying to give participants some control on it. In line with Eliaz and Rubinstein (2014) subjects need to understand what is happening, the procedure must be familiar. As shown with our arbitrary procedure *Time*, subjects do not prefer an obscure algorithm over a clear lottery once they have control on the latter. For instance, in France, the algorithm allocating students to university between 2009 and 2017 was simply not public. Citizens could find out it was a modified deferred acceptance mechanism but were not provided any information on the details. In 2017, the French public authority overseeing data privacy law (CNIL) summoned the French government to explain to participants to the mechanism the precise algorithm used to rank them (decision 201-053, 30 August 2017).[22] The current procedure features a transparent central algorithm, but the algorithms used by universities to rank students are not disclosed. Simply put, citizens have a general idea of what the mechanism aims to achieve and are aware that the choice is made by algorithms, but know little more than that.

It would also help to let people take an active part to the process. The Washington DC "school lottery" is a prime example of a mechanism that publicly embraces the concept of lottery. It is a deferred acceptance mechanism using an individual randomly generated number as a tie-breaker (Abdulkadiroğlu et al., 2017). However, instead of trying to hide the random tie-breaker, the entire procedure goes by the name of "lottery." How random number are being assigned to each parent is however often strikingly non-transparent. For the DC lottery, as well as for the NYC and Denver deferred acceptance mechanisms, official websites simply inform parents that the algorithm generates a random number, but there is no way to actually check how the number is generated, and certainly no involvement of the subjects in the random procedure. We are not aware of any real world mechanism making the random tie-breaking rule transparent and offering some form of control to participants. Our results however suggest that doing so may increase the social acceptability of such lotteries.

We see two main limitations to our approach. The first is inherent to every incentivized study aiming at understanding major choices in life: the experimental stakes we can offer will never be sufficiently high to mimic the real life incentives. We cannot think of any experimental reward one could reasonably offer that would approach the importance for a parent of putting their child in their preferred school, or for a researcher to get a major grant. We however believe that our incentivized approach offers a step in the right direction, allowing to better understand individual choices than what surveys on stated preferences do. The second is that our data on cultural dimensions between subjects is limited, and we would certainly learn from running a similar experiment in different countries.

Finally, while making our subjects vote on the choice of procedures aims at replicating democratic processes, we could learn more by adding an important feature of actual democracies: debate and consensus building. Observing how smaller groups of subjects manage to agree on a procedure would clearly improve our understanding of collective preferences.

[21]Central Application Office, The New Common Point Scale, on cao.ie, retrieved August, 2, 2021.

[22]See also the description of the algorithm of Admission Post Bac (APB) provided by matching in practice, https://www.matching-in-practice.eu/university-admission-practices-france/.

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