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

Elias Bouacida[†] Renaud Foucart[‡]

October 11, 2024

Abstract

We study collective preferences towards the use of random procedures in allocation mechanisms. We report the results of two 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 preference for control in most procedures.

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

JEL-Code: D01, D78, D91

1 Introduction

The solutions provided by economists to allocation problems typically use lotteries to break ties when no better criterion is available (Abdulkadiroglu et al. 2022; Basteck, Klaus, and Kübler 2021; Budish et al. 2013; Erdil and Ergin 2008; Kesten and Ünver

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[†]Université Paris 8, elias.bouacida@univ-paris8.fr

[‡]Lancaster University Management School, r.foucart@lancaster.ac.uk

2015). Random tie-breakers are however often unpopular in practice. In the United States, the Administrative Procedure Act (APA) effectively prohibits the use of random devices in administrative decisions.¹ Several European countries have witnessed significant opposition to school choice mechanisms involving randomization.² The UK government’s “School Admissions Code (2014)” explicitly states (p.14) that “local authorities must not use random allocation as the principal oversubscription criterion”.³ Medical researchers often report difficulties in running randomized controlled trials (RCTs), as doctors oppose allocating treatment at random. Even economists are reluctant to use lotteries for themselves. To allocate funding, academics spend significant time ranking research projects of similar quality, often reaching conclusions that are as good as random (Cole, Simon, et al. 1981; Graves, Barnett, and Clarke 2011; Pier et al. 2018).⁴

Simple modifications can, however, make random allocations more socially acceptable. For instance, the Republic of Ireland adjusted their university admission system in 2015. In Ireland, students are ranked using their CAO score, a metric composed of the sum of their leaving certificate scores over 6 subjects. A lottery decides the allocation among tied students.⁵ Following complaints that lotteries were unfair and too frequent, the Irish government commissioned a reform to reduce the number of ties by slightly modifying the marking system to make it less likely that two students have the same score.⁶ Despite having no other meaning than reducing the use of lotteries, this more “reasonable” procedure successfully ended the controversy.

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

¹The APA requires courts 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.]) ruled 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).

²In France, the system for allocating students to universities (APB) used lotteries to break ties when capacity was reached. It was criticized and replaced in 2018 by another mechanism with more criteria. In French-speaking Belgium, a 2009 attempt to randomly allocate students in oversubscribed high schools lasted only a year after being dubbed the “lottery law” by some parents and the media and was replaced by a set of criteria still in use today.

³This 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.”

⁴Proposals 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 Foundation in Germany, and the small grants from the British Academy.

⁵Central Application Office, “Random number - How it works” on <https://www.cao.ie/>, retrieved August 2, 2021.

⁶Before the reform, possible marks on a given subject were all multiples of 5. After the reform, the possible marks are $\{0,37,46,56,66,77,88,100\}$, resulting in fewer ties when summed, Central Application Office, The New Common Point Scale, on cao.ie, retrieved August 2, 2021.

choose between two unpredictable procedures, one is an explicit lottery while the other is not, to allocate a reward within their group. We document an aversion to the use of lotteries, particularly when the alternative follows the *rituals of reason* (Elster 1989, p.37):⁷ although unpredictable, it is reminiscent of meritocratic procedures, and subjects may interpret it as such. We also replicate the result that subjects have a preference for control over most procedures (Owens, Grossman, and Fackler 2014; Bartling, Fehr, and Herz 2014; Bobadilla-Suarez, Sunstein, and Sharot 2017; Ferreira, Hanaki, and Tarroux 2020). This concept is related to the idea of *illusion of control* (Langer 1975; Sloof and Siemens 2017), which explains this preference by overconfidence. By running treatments with and without control over all procedures, we show that control alone does not explain the aversion to lotteries we identify.

Both experiments consist of subjects choosing between an explicitly random procedure (lottery) and an equally unpredictable and competitive but not explicitly random one (non-lottery).

In Experiment 1,⁸ we use a large online non-representative sample. Our explicitly random procedure is based on the results of a public lottery in Washington DC. Subjects bet on whether the next day’s numbers are odd or even. The first non-lottery procedure is a variant of a rock-paper-scissors (*RPS*) game, designed to be unpredictable and devoid of psychological thrill: we ask subjects to provide in advance a list of five moves, which we then play against the moves of all other participants. In the second, *Paintings*, subjects have to guess, from pairs of paintings selected by the second author of this paper, the ones preferred by the first author. These two procedures follow the rituals of reason, as it is possible to make sense of them and interpret them as meritocratic. This is not true of the third non-lottery procedure, *Time*, in which we ask subjects to provide a time of the day, and then implement an intractable algorithm. We run 12 treatments, so that each procedure is tested with and without control. In treatments without control, we make the choice for the subjects: the bets on the lottery, the sequence of RPS, the painting guesses and the time of the day. Our subjects win a prize if they rank among the top half in the procedure chosen by their group.

Pooling all treatments together, we find a preference for our two “reasonable” procedures (RPS and Paintings) over the lottery, but not for the “obscure” Time procedure. We find that subjects have a preference for control in most procedures, in particular in the lottery. This result is particularly relevant as adding control to the lottery is something we are not aware exists in any real-world allocation mechanism.

We then pre-registered and ran Experiment 2.⁹ We wanted to see if our results of an

⁷“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.”

⁸Both our experiments have received IRB approval from Lancaster University, and the code and data for replication are available at <https://rituals-of-reasons-reproduction-40ab0b.gitpages.huma-num.fr/>

⁹Our pre-registration is available at <https://aspredicted.org/3yy5p.pdf>

aversion to lotteries when the alternative follows the rituals of reason replicated, using a representative sample of the US population. We added to the protocol a measure for the intensity of preferences. To do this, we reduced the number of treatments and only compared the Lottery (without control) to RPS and Time (both with control). We find a slightly higher but not significantly different aversion to lotteries, with 78% of the subjects choosing RPS over the lottery, and (in contrast with Experiment 1), a significant majority (55.9%) still choosing *Time* over the lottery. These preferences are strict: subjects are willing to pay to choose the non-lottery procedure.

A challenge for both experiments was to ensure that subjects would not dislike the lottery for a reason unrelated to its explicit randomness. By running treatments with and without control over all procedures, we tried to give a similar entertainment value: arguably, actively betting on lottery numbers is more thrilling than “playing” with an exogenous sequence in a large anonymous game of rock-paper-scissors. The competitive aspect was the same in all procedures: only the relative ranking of subjects mattered to get the reward. We also made sure the lottery was the most transparent of our procedures. While our subjects had to trust us to implement non-lottery procedures correctly, they could check with their anonymous ID and the actual results of the DC lottery that we did not deceive them. Moreover, all drawings of the DC-5 lottery are open to the public, which is not true for any of our other procedures.

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, Brandts, and Ockenfels 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. This *preference for randomization* (Agranov and Ortoleva 2017) has, for instance, been observed in university choice (Dwenger, Kübler, and Weizsäcker 2018), an allocation problem in which many subjects seem 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 program H-1B in the USA (Pathak, Rees-Jones, and Sönmez 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 negative one on those who were not treated (Aldashev, Kirchsteiger, and Sebald 2017; Deaton and Cartwright 2018; Heckman 1991). Whether the subjects of the RCTs would have preferred another allocation mechanism is generally not discussed.

In other famous cases where lotteries are implemented, it is not as a final tie-breaking rule. The military draft in the USA offers many exemptions after the lottery numbers are drawn, and these were widely used during the Vietnam War (Bailey and Chyn 2020). In Rotating and Savings 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. It is also related to *causal responsibility* (Engl 2022), and the idea that subjects prefer to feel responsible for outcomes they judge as good – even if their actions are not predictive of the said outcome. Our results are 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). They are also reminiscent of a result found in a “spectator” setting, in which subjects are more willing to accept inequality when it is the result of a forced action than of an explicit lottery (Cappelen et al. 2022).

Finally, our work is related to the survey evidence of Keren and Teigen (2010) on preferences for lotteries in determining the outcome of hypothetical decisions involving other people. Oberholzer-Gee, Bohnet, and Frey (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.

2 Experiment 1

The objective of Experiment 1 was to test whether subjects have an aversion to lotteries, whether the answer depends on the *reasonable* nature of the non-lottery alternative, and on whether subjects have control over the procedure.

2.1 Experimental design

2.1.1 Practicalities

The first experiment was run on Amazon Mechanical Turk (AMT), on June 22, July 7, and August 31, 2021.¹⁰ We ran the experiment using oTree (Chen, Schonger, and Wickens 2016). All subjects received a fixed payment of \$0.8, and half of them received an additional payment of \$1.60. We also offered additional smaller incentives for specific questions, 20 cents for one of the belief elicitation questions and 10 cents for the ambiguity

¹⁰We 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.

attitude question. 1,324 participants completed the experiment, for a median hourly payment of \$20.46.¹¹ For all the treatments, we provide subjects with a personal identifier. 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 C).

We randomly allocated subjects among 12 treatments of on average 110 participants. The number of treatments corresponds to all possible choices between one of our three non-lottery procedures and the lottery one, with or without control. We detail the sample in Table 1 and provide demographic information in Appendix E.

Table 1: Size of the sample for each treatment in the two experiments.

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

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

2.1.2 Procedures

We first describe the explicitly random lottery procedure and then the three non-lottery ones. Each procedure consists in ranking subjects based on a sequence of five actions. All procedures were in the form of contest in which the top half of the performers received a reward.

Our lottery procedure 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. We then rank subjects by the number of correct guesses they made. As the lottery is run by a third party, our subjects should in theory trust this procedure as being the most transparent one we offer. It also satisfies the five properties identified by Eliaz and Rubinstein (2014) as characterizing a fair random procedure. In the treatments with control, subjects make

¹¹Following 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 bets themselves. In those 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 4.1) 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 those of all other subjects in their session. 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, where a tie is half a win. The main difference with a traditional RPS game is that all actions are chosen in advance and a subject uses the same actions 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 must guess (with control) the paintings chosen by Elias. We rank participants according to the number of paintings they have guessed correctly. In case of tie, we use the first pair of paintings, then the second and so on. In the treatment without control, we make the guesses for the subjects. 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-lottery procedure, *Time*, in a way to make it look as unreasonable as possible: an arbitrary, complicated, and intractable algorithm. In the treatment without control, we provide subjects with a code corresponding to the last five 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 a five digits code. We rank the codes of all subjects according to the algorithm, which is available in Online Appendix B.

¹²For instance, the official website <https://www.schools.nyc.gov> (consulted September 26, 2024) 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 over it.

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

2.1.3 Protocol

The experiment is composed of four parts.

In the first part, we introduce two procedures to allocate the additional payment: one is a lottery, and the other is not. Subjects also choose their actions 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 one of the many subjects of the mechanism. 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 a subject's choice only apply to themselves. We then implement the choice of the majority.

In the third part, we ask them incentivized questions on their beliefs about their ranks in the procedures, and one incentivized measure of their ambiguity aversion. The belief elicitation was a simple binary choice, where we asked participants to pick whether they expected to win or lose in each procedure, and they received a payment of \$0.20 for a correct answer. The ambiguity task was a simple choice between two urns, one with known probabilities and the other without. Finally, we ask non-incentivized demographic and feedback questions. We provide screenshots of the complete instructions in Online Appendix C.

In the last part, one or two days later, subjects could check their results and others' using their personal identifier. Screenshots are available in Online Appendix C.

2.2 Results

The main result of Experiment 1 is displayed in Figure 1 and aggregates all the different treatments with and without control. We find that RPS and Paintings are both chosen by significantly more than 50% of subjects, and significantly more than Time. We report the results for each treatment in Appendix A.

We perform a linear regression analysis of the choice of the non-lottery procedure in Table 2. The coefficients represent variations (in percentage points) of the share of subjects who chose a non-lottery procedure. The baseline is given with *RPS* with control and *Lottery* without control. We used the same treatment in Experiment 2 and it is also the treatment with the lowest share of subjects choosing the lottery. We find that the procedures following the rituals of reason (*RPS* and *Paintings*) are statistically indistinguishable from each other, whereas the arbitrary one (*Time*) is significantly less chosen by 19 percentage points, *ceteris paribus*. Control matters more for the lottery than for the non-lottery procedures. It increases the choice of the lottery by around 10 percentage points, compared to 5 percentage points in the non-lottery procedures. For some subjects, the belief that they will win in one of the procedures and not the other

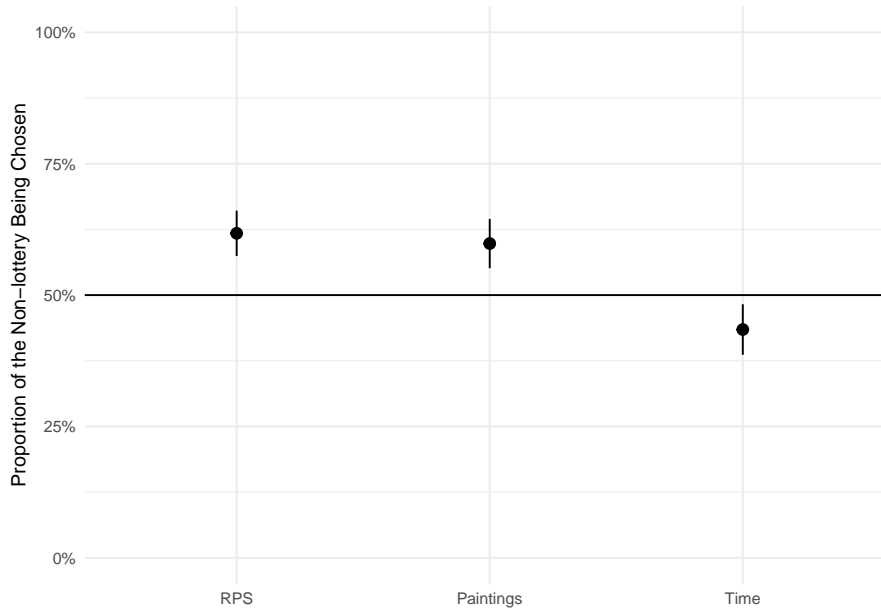


Figure 1: Share of the subjects voting for a non-lottery procedure in Experiment 1, pooling treatments with and without control. The error bars represent the 95% confidence interval and the point the estimate for a two-sided one sample t-test of equality with 50%.

drives their choices. It represents a fraction of around 20 percent of the sample.

A possible explanation for the higher preference for control in lotteries than in non-lottery procedures is that subjects may misunderstand the real probability of winning of some sequences in a lottery, such as five Even for instance. We however show in the Appendix D that the actual sequence has no influence on the choice of procedure in the treatments without control over the lottery.

3 Experiment 2

The objective of Experiment 2 was to test if our result of an aversion to lotteries when the alternative follows the rituals of reason replicates when we use a representative sample of the US population.

3.1 Experimental design

We pre-registered Experiment 2 and ran it on August 14, 2023, using Prolific on a representative sample of 577 USA residents. We ran two treatments of this experiment, one with *RPS* and one with *Time* as a non-lottery 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. Our pre-registration is available at <https://aspredicted.org/3yy5p.pdf>. The Pre-analysis Plan Report is available

Table 2: Regression analysis: share of subjects choosing a non-lottery procedure in Experiment 1, where the baseline is RPS with control and Lottery without control.

	Non-Lottery Chosen	
	Simple	Controls
(Intercept)	0.695 (0.032)	0.699 (0.078)
Time	-0.190*** (0.033)	-0.191*** (0.033)
Paintings	-0.028 (0.032)	-0.024 (0.032)
Win only in Non-lottery ¹	0.114*** (0.033)	0.112*** (0.033)
Win only in Lottery ¹	-0.099** (0.035)	-0.098** (0.035)
No Control on Non-lottery	-0.053* (0.027)	-0.050+ (0.027)
Control on Lottery	-0.103*** (0.027)	-0.107*** (0.027)
Male		0.052+ (0.027)
Age Controls ²	No	Yes
Num.Obs.	1324	1317
R2	0.059	0.065
R2 Adj.	0.055	0.058

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001
¹ Win only in XX is a dummy for when a subjects believe they win the XX procedure and NOT in the alternative one.
² Age controls: Age categories, no category was significant.

in Online Appendix A.

All subjects received a fixed payment of \$1, and half of them received an additional payment of \$2. The higher payment as compared to Experiment 1 reflects the inflation that occurred during the period. 577 participants completed the experiment, for a median hourly payment of \$18.75.

The main difference with Experiment 1 is that in Experiment 2, 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.¹⁴ We then implement the

¹⁴We 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.

choice of a subject chosen at random. 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. In Experiment 2, we also sent an email to all subjects with the detail of their payments, as well as their results in the two procedures. We detail the sample in Table 1 and provide demographic information in Appendix E.

3.2 Results

We report (in black) the share of subjects choosing the non-lottery procedure in each treatment in Figure 2. When *RPS* is offered with control, and *Lottery* without, 78% choose the former. 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 pre-registered, *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*.¹⁵ The fact that *Time* is chosen by a majority of subjects however goes against our predictions, showing stronger aversion to lotteries in the general population.

We then plot in grey the share of subjects choosing the non-lottery procedure in the equivalent treatments of Experiment 1. This number is therefore different from Figure 1 where we pool all treatments. We find that preference for *RPS* over *Lottery* is roughly similar in Experiment 2 and 1 (the p-value of the Fisher test is 0.49). Similarly, the share of subjects choosing *Time* is higher in Experiment 1, but not significantly so (p-value of the Fisher test is 0.11). We should thus interpret the results of Experiment 1 as providing an upper bound for the social acceptability of lotteries in the USA.

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 Appendix D.

Combining the results of Experiment 1 and Experiment 2 therefore suggests that roughly half of the social preference for *RPS* observed in Experiment 2 - 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. In Appendix B, we provide a regression analysis similar to the one given in Table 2 for Experiment 1. When the treatments coincides, the results are comparable.

¹⁵The p-value of the Fisher test of the proportion of subjects choosing the non-lottery procedure in both treatments being different is <0.001 .

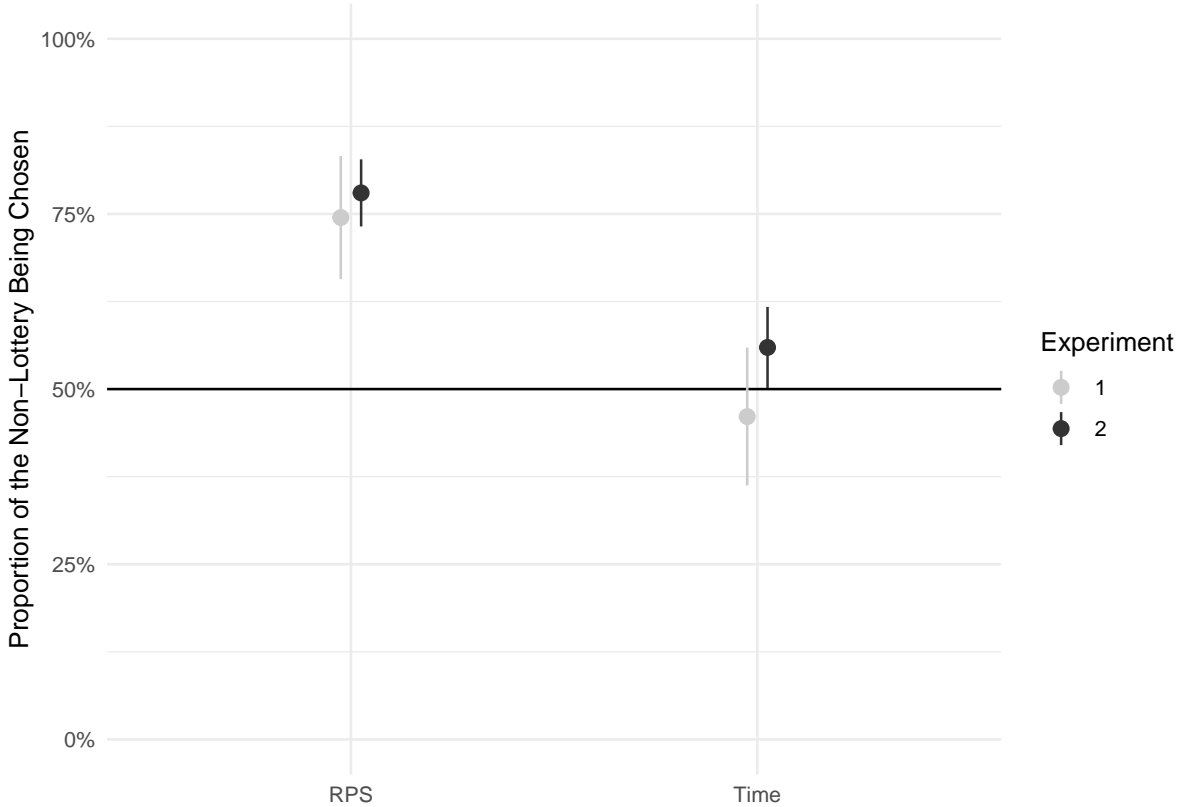


Figure 2: Share of the subjects voting for a non-lottery procedure in Experiment 2, and in the corresponding treatment of Experiment 1. In Experiment 2, subjects had control over RPS and Time, but not over Lottery. Error bars indicate the 95% confidence interval of a two-sided one-sample t-test for equality with 50%, while the central point represents the point estimate. The p-values of the Fisher test of equal proportions between the two experiments are 0.49 and 0.11 for RPS and Time, respectively.

4 Robustness

4.1 All our procedures are unpredictable

Over all participants in our experiments, the correlation between the belief of winning in the non-lottery procedure and actually winning is 0.006, with a p-value for the correlation of being significantly different from 0 of 0.79. The similar figure for the lottery is -0.003 and the p-value is 0.91. Our procedures are unpredictable. For *RPS* in particular, we were concerned that some subjects could exploit a well-known bias of RPS: too many players choose Rock (37.5% in our sample), and not enough Scissors (27.7%), in particular in the first round (44.7% and 19.6% 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

¹⁶The 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

correctly guessed they were going to win, on aggregate their predictions are as good as random, as shown in Table 3. In Appendix C, we show that additionally controlling for control does not improve the belief accuracy.

Table 3: Correlation between real and believed performance in each procedure, for both experiments.

Non-lottery Procedure	Correlation (p-value)	
	Non-lottery	Lottery
Experiment 1		
RPS	0.010 (0.825)	0.035 (0.44)
Paintings	-0.031 (0.522)	-0.013 (0.783)
Time	-0.047 (0.344)	0.033 (0.511)
Experiment 2		
RPS	0.110 (0.061)	-0.032 (0.582)
Time	0.036 (0.545)	-0.071 (0.234)

Note: P-value of the correlation test of the correlation being equal to 0 in parenthesis.

4.2 Beliefs and overconfidence

We now look at the role of beliefs and overconfidence in determining individual choices. In both experiments, what matters to the payment is whether a subject is among the first half of the participants of their treatment group in the chosen procedure. In Experiment 1, where all procedures are offered with and without control, we see no significant difference between the expectations of success in *RPS* and the *Lottery* and *Time* and the *Lottery*, respectively.¹⁷ In Experiment 2, where subjects have control over non-lottery 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.¹⁸ If we look at the same treatment in Experiment 1, we have no evidence of “illusion of control”. 53.1% of subjects expect to win in *RPS* against 56.1% in the *Lottery* and the figures are 65.7% versus 69.6% in *Time* against the *Lottery*.¹⁹

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-lottery procedure but not in the lottery, and, conversely, of those who only expect to win the lottery. The numbers for Experiment 1 aggregate the different levels of control.

five choices in advance. They are therefore more likely to play regularly and have a notion of the best strategies.

¹⁷The p-value of the Fisher tests are 0.74 and 0.53 respectively.

¹⁸The differences are significant according to Fisher tests of equal proportions, with a p-value < 0.001 in the first case and of 0.022 in the second.

¹⁹The p-value of the Fisher tests are 0.67 and 0.55 respectively.

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

Treatment	Subjects expect to win only in		P-value ¹
	Non-lottery	Lottery	
Experiment 1			
RPS	79.1%	46.2%	<0.001
Paintings	67.0%	43.7%	0.003
Time	50.6%	45.3%	0.530
Experiment 2			
RPS	84.9%	63.2%	0.010
Time	63.8%	45.2%	0.117

Note: The first column represents subjects expecting to win in the non-lottery 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-lottery procedure being equal.

In both experiments, we see that some subjects act 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 2, we see for instance that a significant majority of subjects (63.2%) prefer *RPS* even when they expect to only win in the *Lottery*. This preference is a strict one: when we restrict to subjects who are willing to pay in order to choose a procedure in which they nonetheless think they are less likely to win, the proportion rises to 66.7%.²⁰ Given the simplicity of our belief elicitation - we asked subjects to bet on whether or not they would win the prize - we cannot rule out however the possibility that some chose to use the elicitation to hedge their risk, which may explain why some subjects act against their elicited beliefs. As subjects were not aware of the belief elicitation task at the time of choosing their preferred procedure, this possibility does not affect the main result of the paper.

Finally, we looked at whether the sequence assigned to our subjects in the Lottery procedure (for instance five odd numbers in a row) had any impact on the preferences of our subjects. We show in Appendix F that we did not find any such effect.

²⁰As we offered subjects a binary choice of a bet on whether they expect to win or lose in each 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.

5 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. 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 lotteries but look reasonable enough to be more socially acceptable. We gave in the introduction the example of the Republic of Ireland tweaking its university admission rules to reduce the number of tie-breakers. More generally, evaluation standards typically place competitors in broad categories. One of the reasons for such a standard is that it allows conveying information about 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, it may prove useful to avoid requiring an additional criterion 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 or merits of two projects, it nonetheless provides a non-lottery procedure following the rituals of reason on which participants had some control and avoids the need for random tie-breakers – or lengthy deliberations in committees to rank two equivalent projects.

Our second recommendation is that, when the only choice is to have a lottery, it may be worth giving participants some control over it. As shown with our arbitrary procedure *Time*, subjects do not prefer an obscure algorithm over a clear lottery once they have control over the latter. 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, Agarwal, and Pathak 2017), and the entire procedure goes by the name of “lottery.” While official websites inform parents that the algorithm generates a random number, there is however no way to check how the number is generated, and certainly no involvement of the subjects in the lottery procedure. We are not aware of any real-world mechanism offering a lottery with 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 only have a nationally representative sample for a single country, the USA. 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.

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Appendix

A Detailed results of Experiment 1

As expected from the literature, preferences for the different procedures change with control. The two first columns in Table 5 correspond to the treatments in which subjects have control over the non-lottery. 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 1.²¹ It is not the case in general for the non-lottery procedures, with a p-value of the Fisher exact test of equal proportions is 0.27.

Table 5: Percentage of participants choosing the non-lottery procedure in Experiment 1, for each treatment.

Control in				
Non-Lottery	Yes	Yes	No	No
Lottery	No	Yes	No	Yes
Non-lottery procedure				
RPS	74.5%	58.6%	65.1%	51.4%
Paintings	66.0%	59.6%	62.6%	51.8%
Time	46.1%	41.6%	48.1%	37.4%

Note: Share over all subjects in their respective sample.

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*. Those results are consistent with the regression analysis presented in the main part of the paper.

B Detailed Results of Experiment 2

We show in Table 6 the results for a regression in Experiment 2 similar to the one given in Table 2 for Experiment 1.

The specification is however simpler than the one of Experiment 1, as we do not vary control in our treatments. The regression analysis confirms the result that subjects treat our obscure procedure *Time* differently than RPS. We also see some evidence that beliefs

²¹The p-value of the Fisher test of equal proportions is < 0.001 .

explain part of the results, as subjects are more likely to choose the lottery if they expect to win in that procedure, but not in the non-lottery one.

Table 6: Share of subject choosing the non-lottery procedure in Experiment 2. The baseline is RPS (with control).

	Non-Lottery Chosen	
	Simple	Controls
(Intercept)	0.780 (0.032)	0.848 (0.070)
Time	-0.221*** (0.038)	-0.223*** (0.038)
Win only in Non-Lottery ¹	0.077+ (0.045)	0.076+ (0.046)
Win only in Lottery ¹	-0.126* (0.060)	-0.132* (0.060)
Indifferent	-0.127 (0.109)	-0.120 (0.110)
Male		-0.044 (0.038)
Demographic Controls ²	No	Yes
Num.Obs.	577	577
R2	0.073	0.079
R2 Adj.	0.066	0.064

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

¹ Win only in XX is a dummy for when a subjects believe they win the XX procedure and NOT in the alternative one.

² Demographic controls: Age categories, Being White, No demographic control was significant.

C Winning and Believing to win

In Table 7, we show that taking into account the control over the non-lottery procedures or the lottery does not change the accuracy of beliefs: subjects do not, on aggregate, correctly predict their performance.

D Intensity of Preferences in Experiment 2

We only elicited our subjects' willingness to accept to change their choice in Experiment 2. We estimate that a subject is indifferent if they state a \$0 minimal value for changing

Table 7: Correlation between the belief of subjects they will win in a procedure and whether they actually won in that procedure, by control over the procedures.

Non-Lottery Procedure	Control on		Correlation (p-value)	
	Non-Lottery	Lottery	Non-Lottery	Lottery
Experiment 1				
RPS	No	No	0.11 (0.177)	-0.027 (0.743)
RPS	No	Yes	-0.118 (0.164)	0.161 (0.058)
RPS	Yes	No	-0.041 (0.689)	-0.021 (0.841)
RPS	Yes	Yes	0.091 (0.372)	0.008 (0.938)
Paintings	No	No	0.023 (0.799)	-0.016 (0.863)
Paintings	No	Yes	-0.197 (0.036)	-0.033 (0.726)
Paintings	Yes	No	0.180 (0.0770)	0.031 (0.766)
Paintings	Yes	Yes	-0.115 (0.284)	-0.036 (0.740)
Time	No	No	-0.066 (0.505)	0.018 (0.856)
Time	No	Yes	0.010 (0.924)	-0.089 (0.401)
Time	Yes	No	-0.185 (0.063)	0.159 (0.111)
Time	Yes	Yes	0.033 (0.731)	0.033 (0.731)
Experiment 2				
RPS	Yes	No	0.110 (0.061)	-0.032 (0.582)
Time	Yes	No	0.036 (0.545)	-0.071 (0.234)

Note: In parentheses are the p-value of the correlation test of the correlation being different from 0.

their choices.

Table 8: Number of subjects and strength of their preference.

Non-lottery Procedure	Procedure Chosen	Indifferent	Strict Preferences
RPS	Lottery	4	60
RPS	RPS	10	217
Time	Lottery	3	123
Time	Time	1	159

Note: A preference is considered strict if the WTA is non-null.

Table 8 shows that a very large majority of subjects have strict preferences. These preferences are quite strong, as implied by the distribution of the minimal values asked for in Figure 3. We show when reporting the pre-registered analysis in Online Appendix, as you would expect with such a small proportion of indifferent subjects, that adding or removing them has no influence on the results.

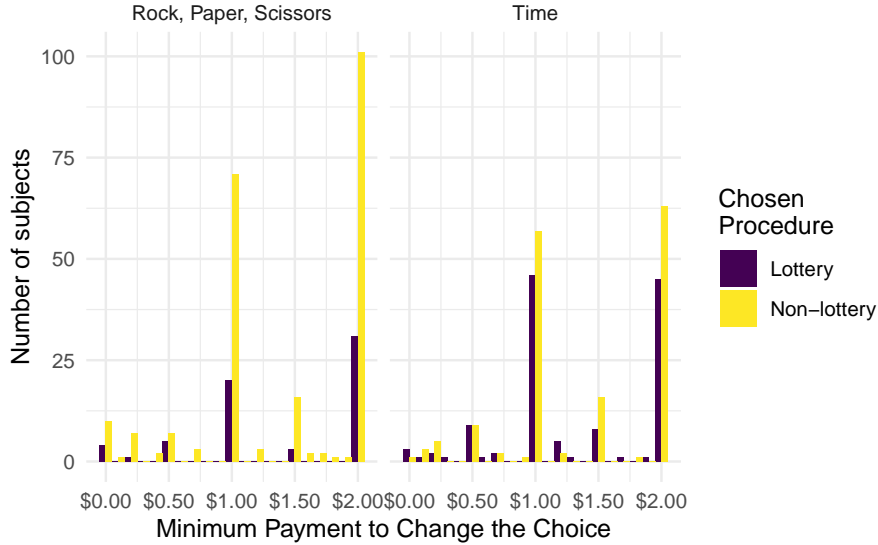


Figure 3: Minimum amount we would need to pay subjects for them to change their choice. The reward for ranking among the first half of the chosen procedure is \$2.

E Demographics

Table 9 shows the gender balance in Experiment 1 and 2. There are a lot more males in Experiment 1, whereas Experiment 2 is representative in terms of gender, as expected. Table 10 shows the repartition by age group in Experiment 1 and 2. As Experiment 2 is made on a representative sample of the USA population, it shows that Experiment 1 is far from representative in terms of age. Table 11 shows the repartition of ethnicity using Prolific stratification strategy.

Table 9: Proportion of subjects of each gender in both experiments.

Gender	Experiment 1	Experiment 2
Female	38.8%	52.5%
Male	61.2%	47.5%

Table 10: Proportion of subjects in each age group for each experiment.

Age Group	Experiment 1	Experiment 2
<25	3.5%	13.9%
25-40	64.6%	28.9%
40-55	23.2%	25.8%
>55	8.2%	31.4%
Prefer not to say	0.5%	-

Note: Age groups used are those elicited in Experiment 1.

Table 11: Declared ethnicity in Experiment 2

Declared Race	Count
Asian	36
Black	76
Mixed	12
Other	6
White	447

We report the self-declared country of residence of the subjects in Experiment 1 and 2 in Table 12. As expected, all subjects are coming from the United States in Experiment 2. In Experiment 1, the majority of our subjects report coming from the United States (79.3%). The second highest country of residence is India (12.2%).

F Influence of the Given Sequence in the Lottery

Participants may be influenced in their choices by the strategies we chose for them when they had no control over a procedure. For instance, they may believe that the sequences Even, Even, Even, Even, Even or Odd, Odd, Odd, Odd, Odd are less likely to happen than the sequence Even, Odd, Even, Odd, Even. This incorrect understanding of probabilities is more likely to happen when facing the *Lottery*, as there is no particular order for the *Paintings* or for the *Time* procedure. In *RPS*, subjects would have to be sophisticated in their strategies to form a belief about it, and it does not appear to be the case as beliefs do not explain results.

To investigate this issue, we restrict ourselves to the sample of subjects without control on the lotteries, which is all participants of Experiment 2 and 678 participants in Experiment 1. We first count the number of Even in the sequence we gave them. Table 13 shows the proportions of subjects choosing each non-lottery procedure depending on the number of Even in their sequence. The sample sizes vary widely: subjects are much more likely to have a sequence with 3 Even than 5 (even if any given sequence is equally likely).

First, in Table 13, we find no difference between the treatment of Odd and Even. There is no significant difference between the share of subjects choosing the non-lottery procedure when awarded a sequence of 0 or 5 Even. The same holds between 1 and 4 or 2 and 3, respectively, according to a Fisher test of equal proportions.

We can therefore group up sequences of 0 and 5 Even together, as well as sequences of 1 and 4 and 2 and 3. It yields to Table 14. The proportions of each non-lottery procedure being chosen are never significantly different between 0 and 1 or 2 Even in the sequence, according to a Fisher test of equal proportions.

Table 12: Number of subjects from each country in both experiments.

Country	Experiment 1 ¹	Experiment 2
Asian	0.2%	-
Brazil	4.2%	-
Bulgaria	0.1%	-
Canada	0.5%	-
Columbia	0.1%	-
France	0.3%	-
Germany	0.3%	-
India	12.2%	-
Italy	1.5%	-
Portugal	0.1%	-
Spain	0.2%	-
Sweden	0.1%	-
The Netherlands	0.1%	-
Turkey	0.1%	-
UAE	0.2%	-
USA	79.3%	100%
Ukraine	0.1%	-
United Kingdom	0.5%	-

¹ In Experiment 1, the country of residence is self-declared. We reconstructed the intended country as best as we could.

Table 13: Choices of the non-lottery procedure when subjects have no control over the lottery, by the number of Even in the sequence given to them.

	Number of evens						P-values		
	0	1	2	3	4	5	0~5	1~4	2~3
Experiment 1									
RPS	40.0%	63.6%	65.0%	77.5%	76.9%	50.0%	>0.999	0.234	0.108
Paintings	66.7%	63.4%	70.0%	62.2%	52.2%	75.0%	>0.999	0.433	0.375
Time	28.6%	39.3%	49.2%	54.3%	34.4%	62.5%	0.315	0.791	0.604
Experiment 2									
RPS	88.9%	86.5%	78.6%	78.3%	71.9%	60.0%	0.505	0.139	>0.999
Time	37.5%	53.2%	60.4%	51.7%	56.5%	85.7%	0.119	0.836	0.290
Aggregate	52.5%	61.9%	65.1%	64.9%	61.3%	67.6%	0.237	0.918	>0.999

Note: P-values of the Fisher test of equal proportion in both samples.

Table 14: Choices of the non-lottery procedure when subjects have no control over the lottery, by the number of Even/odd in the sequence given to them.

Non-lottery procedure	Number of evens/odds			P-values		
	0	1	2	0~1	0~2	1~2
Experiment 1						
RPS	43.8%	69.9%	70.9%	0.081	0.045	0.882
Paintings	71.4%	59.4%	65.5%	0.548	0.773	0.436
Time	46.7%	36.7%	51.9%	0.558	0.789	0.062
Experiment 2						
RPS	78.6%	77.2%	78.4%	>0.999	>0.999	0.881
Time	60.0%	54.8%	56.2%	0.785	>0.999	0.898
Aggregate	59.5%	61.6%	65.0%	0.795	0.374	0.251

Note: P-values of the Fisher test of equal proportion for each procedure between each number of sequence.