1 Introduction

The ability to recover preferences from choice data, and subsequently predict choices from preferences, is fundamental for economic analysis. The revealed preference approach (Samuelson, 1938, 1948; Houthakker, 1950; Arrow, 1959; Richter, 1966) essentially views preferences as nothing more than organizing schemes reflecting both observed and predicted choices. Accordingly, choice data is universally used to estimate latent and derived concepts ranging from utility functions and risk attitudes to demand functions and social welfare (e.g., Harsanyi, 1955; Koopmans, 1960; Afriat, 1967; Varian, 1982; Andreoni and Miller, 2002; Cox et al., 2008; Deb et al., 2014, among many others). The use of choice data, however, entails an implicit but rarely-discussed assumption: stability. Predicting future choices from preferences which themselves are estimated from past choices is only warranted as long as economic agents display well-defined and stable choice patterns (or, equivalently, stable preferences) in the relevant time frame.

Worryingly, the assumption of stable preferences is at odds with fundamental theories in psychology, which postulate that choices can create and alter preferences (Festinger, 1957; Bem, 1967a,b; Simon et al., 2004; Ariely and Norton, 2008; Slovic, 1995). That is, the mere act of choice, even when no new information is revealed after the choice, can lead to fundamental changes in preferences, so that we do not only "choose what we like," but also "like what we choose." Empirical support for such feedback loops between choices and preferences appears to be widespread. Past choices can causally increase the desirability of chosen objects (e.g., Brehm, 1956; Shultz et al., 1999; Jarcho et al., 2011; Alós-Ferrer et al., 2012), and a number of studies even suggest that the mere act of choosing something unconditionally increases its desirability (Egan et al., 2010; Sharot et al., 2010; Nakamura and Kawabata, 2013; Johansson et al., 2014). These alleged preference changes occur within the time span of a few minutes and in the absence of any new, choice-relevant information. They are therefore fundamentally problematic for economics. If such effects extend to economic choices, every choice-based preference elicitation procedure bears the potential to interfere with the very concept it ought to measure. Observed economic choices may then permanently lag behind current preferences, and standard economic applications estimating utilities, demand, and social welfare may be systematically biased.

In light of its potential consequences, it is of paramount importance to investigate the economic validity and significance of this *mere choice effect*. Evidence from psychology is insufficient to settle the question, due to difficulties with the experimental paradigms applied in that literature (see next section), the hypothetical nature of choices in such studies, and the non-economic nature of the alternatives they study. This paper undertakes the endeavor of establishing the validity of the mere choice effect (preference change due purely to the act of choice) *in economics*. We develop a novel, parsimonious experimental design that, for the first time ever, allows researchers to isolate the effect of mere, uninformative choices on future choices in an economically valid domain (binary monetary gambles or lotteries). In essence, our experimental design presents participants with two choice options (lotteries). The experimenter randomly determines whether a certain choice option is transparently inferior or superior (through stochastic dominance). As choices are incentivized, it is in the best interest of participants to choose the objectively superior option and hence follow the pre-determined, randomized choice patterns. That is, the design effectively randomizes uninformative (mere) choices. We hereby solve typical issues encountered in the existing literature: unreliable preference measures, hypothetical bias, and deception (we will elaborate on these issues in the next section).

This work will report the results of a large-scale, preregistered online experiment relying on the basic design described above. The mere choice effect will be assessed by measuring whether merely-chosen options are *subsequently* chosen more often than merely-rejected ones. The results will allow us to establish whether or not the mere choice effect is relevant for economics and whether or not it is warranted to maintain a unidirectional link between choices and preferences in the domain we study. If supporting evidence for the mere choice effect is found, our work will pave the way to develop better preference elicitation methods and to improve their predictive accuracy. For example, if preference change follows regular patterns, standard elicitation procedures could be corrected by taking into account quantitative predictions about the expected magnitude of preference change. If no supporting evidence is found, and since our study will have sufficient power, we will conclude that the effects reported in psychology are likely to be too small for economic choices to merit sparking a major reevaluation of economic methods.

The remainder of the paper is structured as follows. Section 2 reviews the existing literature on choice-induced preference change and briefly discusses the main theory underlying the effect. Section 3 presents our experimental design, including the derivation of our main hypothesis and the power analysis. Section 4 presents the planned statistical analyses and discusses the interpretation of the results (conditional on whether evidence is found or not) and section 5 concludes. Supplementary experimental materials (experimental instructions and screenshots) are presented in the online appendix.

2 Literature Review: Choice-Induced Preference Change

Psychological theories that explain how choices can create preferences often draw an analogy between how we make inferences about others' preferences and how we make inferences about our own preferences (Bem, 1967a,b; Ariely and Norton, 2008). As we cannot fathom what others feel and think, we infer their preferences and beliefs by what we can observe: their behavior. If we observe a stranger on the street giving money to a homeless person, we infer that the stranger is altruistic. Analogously, if our own preferences are vague, imprecisely formulated, or incomplete, we cannot fathom what we ourselves feel and think. Thus, we infer our own preferences from what we can observe: our own past behavior. Imagine a consumer standing in front of a drug-store shelf filled with many shampoo brands. One particular brand catches her eye. She is not quite certain of whether she likes the brand or not, but remembers buying it in the past. She deduces that there must have been a good reason for that decision. Being a rational consumer, the shampoo must have fulfilled her needs. She infers that she likes the shampoo and buys it again. This line of reasoning can lead us astray because memory often inaccurately captures hedonic experiences.

For example, it is well understood that unrelated situational factors can impact behavior and that we are not always aware of their influence (Slovic, 1995;Ariely et al., 2003;Ariely and Norton, 2008; see, however, Fudenberg et al., 2012 and Maniadis et al., 2014). Maybe the consumer correctly remembers buying the shampoo, but forgets having been in a rush that day, or that the shampoo was part of a promotional deal. In that case, her self-inference process was based on an inaccurate recollection of a past event. This is the logic behind the mere choice phenomenon, with the only caveat that, in psychology, processes of preference change are assumed to happen subconsciously. Uninformative (mere) choices can serve as input factors for the self-inference process, which itself may then lead to wrongly imputed preferences.[1]

Most of the relevant evidence on preference change in psychology has been collected using the following three-stage setup. In stage 1, participants rate or rank certain objects, like artistic paintings, on their desirability. In stage 2, they are asked to make a choice between two previously-rated objects. Participants are led to believe that they have made a free choice, but, in reality, researchers use some form of deceptive technique to manipulate choice and randomly determine what was chosen and rejected, e.g. alleged subliminal choice (Sharot et al., 2010). In the third and final stage, objects are rated or ranked again. Preference change is measured by comparing how much chosen objects have increased in self-reported desirability relative to rejected objects. The typical finding is that chosen objects are reevaluated upwards and non-chosen ones are reevaluated downwards, even if choices were randomly assigned. If preferences are stable, one should have observed no changes in desirability.

In spite of an apparently-overwhelming body of evidence, economists should be skeptical about the relevance of the mere choice phenomenon as currently established. First, the extant literature typically studies the effect of past choice on future desirability measures, e.g., liking ratings or rankings (Nakamura and Kawabata, 2013; Sharot et al., 2010). In economics, the most relevant data source is actual choices, and preferences are just binary relations organizing those choices, which decision makers might or might not have conscious access to. Whether (typically unincentivized) desirability measures proxy choice data sufficiently well is not self-evident (Cason and Plott, 2014). Hence, it is important to establish the mere choice effect on actual, subsequent choices and not only self-reported desirability scales. Second, the available experimental evidence exclusively investigates preferences in hypothetical choice scenarios over ill-defined options, which do not reference all preference-relevant option dimensions (Egan et al., 2010). Examples include hypothetical holiday destinations described by their destination names only, or the attractiveness of human faces (Sharot et al., 2010; Johansson et al., 2014). In such cases, behavior might be extremely noisy and easily swayed by irrelevant factors (Murphy et al., 2005; Fudenberg et al., 2012). The hypothetical bias identified in related domains casts doubts on whether observed behavior is actually indicative of preferences (Hertwig and Ortmann, 2001; Murphy et al., 2005; Harrison and Rutstrom, 2008). Third, the existing literature has adopted research designs that deceive participants

[1] The described self-inference process is related to a recent stream of literature on motivated reasoning in economics. Motivated reasoning can be as a forceful driver of people's shifts in beliefs and attitudes. Bénabou and Tirole (2016) provides an overview. The mere choice phenomenon suggests that (pressumably unconscious) motivated reasoning may also apply to the domain of preferences.

to achieve experimental control. For example, experimenters give wrong feedback about past choice using card tricks (swapping choices) or present cover stories about subliminal decision making and have a computer prompt a random choice (Sharot et al., 2010; Nakamura and Kawabata, 2013; Johansson et al., 2014). Deception is obviously inappropriate in experimental economics and, through lab reputation, would render any incentivized design ineffective. In summary, it remains unresolved whether actual choices, in contrast to perceived and make-believe choices, lead to preference change.

It needs to be pointed out that a large part of the literature on choice-induced preference change in psychology has studied a related but different question, namely whether and how choices involving some sort of tradeoff change preferences (Brehm, 1956; Harmon-Jones and Mills, 1999; Alós-Ferrer et al., 2012; Izuma and Murayama, 2013). The dominant theory behind such effects is cognitive dissonance (Festinger, 1957; Akerlof and Dickens, 1982). In a nutshell, the hypothesis is that any choice involving tradeoffs creates dissonance (psychological discomfort) because the chosen option has some negative characteristics and the rejected option has some positive ones, and decision makers unconsciously reduce this dissonance by adjusting their preferences, hereby reevaluating chosen options up and rejected ones down. However, it has been recently shown that the experimental paradigm which has guided the development of this literature for over 50 years is regrettably flawed. It contains a statistical bias that can result in apparent preference change even if participants have stable preferences (Chen and Risen, 2010; Izuma and Murayama, 2013; Alós-Ferrer and Shi, 2015). Although some improved designs have been proposed (e.g., Alós-Ferrer et al., 2012), how the effect of trade-off choices in economically-relevant domains could be studied remains an unresolved issue at the time of writing. Although beyond the scope of the current paper, it would of course also be valuable for economics to understand if and when trade-off choices change preferences. This work, however, concentrates on the mere choice effect, which more clearly isolates the possible effects of the act of choice on preferences.

3 Experimental Design and Procedures

3.1 Design and Main Hypothesis

We developed a novel experimental design that bypasses all of the critiques and difficulties mentioned above. First, we study the impact of past choices on subsequent ones, and hence our dependent variable are choices, the most relevant preference measure in economics. Second, we do so using lotteries. Lotteries have well-defined, objective, and economically-relevant characteristics (probabilities and monetary outcomes). This allows us to induce monetary incentives, which eliminates any potential hypothetical bias. Finally, we achieve control over initial choices without using any form of deception. To this end, we exploit the well-defined structure of lotteries. In our design, initial choices are made between a fixed target lottery, a, and a new lottery, c, which is constructed on the spot. We randomly determine whether the constructed lottery c is transparently inferior or superior monetary-wise to the target lottery a.

Assuming only that participants prefer more money over less money, they should follow the randomly pre-determined choice patterns. If c is inferior, participants should choose the target lottery a. If c is superior, participants should reject a. We call these predicted choices *mere choices*, as they do not reveal any new information about the underlying preferences over lotteries. After mere choices, we subsequently elicit choices between the target lottery a and a fixed, not-previously-encountered third lottery b. Call this choice the *preference choice* (a,b). Crucially, preference choices involve trade-offs and a is neither superior nor inferior to b in a dominance sense. The mere choice effect can now be measured precisely. If mere choices change the desirability of lottery a, we can expect lotteries a that were merely-chosen to be more attractive than comparable lotteries a that were merely-rejected. This in turn should impact the choice frequencies in preference-choices (a,b). Merely-chosen lotteries a should be chosen more often than merely-rejected lotteries a in preference-choices (a,b). We can formulate our main research hypothesis as follows:

H1: *Frequency*(a chosen over b | a is merely-chosen) >
Frequency(a chosen over b | a is merely-rejected)

3.2 Procedures

To facilitate the exposition, we describe the experimental procedures as if we had already conducted the experiment, with placeholders for the statistics.

We conducted an online experiment to investigate the economic validity of the mere choice effect and test our main research hypothesis (H1). Participants were recruited via the research platform Prolific and sampled from a U.S. general population.[2] Table 1 presents the descriptive statistics on the sample demographics. The sample shows the typical characteristics of an online panel (mean age was YY, SD = ZZ).

Each participant made eight choices between two lotteries with two monetary outcomes and two probabilities each. Lotteries were presented as icon arrays and we used a colored-balls-in-a-box framing (Garcia-Retamero and Galesic, 2010; Dambacher et al., 2016). All relevant design aspects of the presentation format were counterbalanced (colors, position on screen, order of presentation within stages). Figure 1 summarizes the experimental design using sample screenshots and Table 2 shows the lotteries used in the experiment.

All participants first went through a standard attention screening, a typical procedure to reduce noise in online experiments (Oppenheimer et al., 2009).[3] After passing the attention check, participants received detailed instructions on our lottery presentation format. They were then required to answer a small control quiz ensuring that they understood the lottery presentation format. After passing the quiz, each participant faced two decision stages, a mere-choice task in stage 1 and a preference-choice

[2] Prolific is a well-established research platform and is increasingly gaining popularity in economics (Palan and Schitter, 2018; Kong et al., 2019).

[3] We used an instructional attention check, see Figure A.2 in the online appendix. Participants were instructed to ignore the question text and to simply answer the question in a specific way by entering the word ‘clear’ into a text-field.

Table 1 Sample demographics. N and $N\%$ represent absolute and relative frequencies, respectively. Percentages in $N\%$ columns were calculated excluding "Prefer not to disclose" (PNTD) answers. Percentages in U.S.% columns represent 2017 U.S. adult population figures, taken from the U.S. Census Bureau (Current Population Survey).

	N	$N\%$	U.S.%
Household income			
Less than $24,999	YY	YY	20.3
$25,000 to $49,999	YY	YY	21.5
$50,000 to $84,999	YY	YY	22.2
$85,000 to $149,999	YY	YY	21.2
$150,000 or more	YY	YY	14.8
Prefer not to disclose (PNTD)	YY	YY	-
Total	450	100.0	100.0
Highest education level			
No formal educational credential	YY	YY	11.0
High school diploma or equivalent	YY	YY	28.9
Some college, no or less than 4-yr degree	YY	YY	28.6
Bachelor's degree	YY	YY	20.0
Master's degree	YY	YY	8.4
Doctoral or professional degree	YY	YY	3.0
PNTD	YY	YY	-
Total	450	100.0	100.0
Student status			
Yes	YY	YY	7.5
No	YY	YY	92.5
PNTD	YY	YY	-
Total	450	100.0	100.0
Gender			
Female	YY	YY	51.6
Male	YY	YY	48.4
Trans*	YY	YY	-
PNTD	YY	YY	-
Total	450	100.0	100.0
Employment status			
Full-time	YY	YY	NA
Part-time	YY	YY	NA
Not in paid work	YY	YY	NA
PNTD	YY	YY	-
Total	450	100.0	NA

task in stage 2. In both choice tasks, participants were presented with pairs of lotteries sequentially. They were instructed to choose the lottery they preferred in each pair.

The mere-choice task in stage 1 consisted of four pairs of lotteries. Each mere-choice pair displayed one target lottery of type a (see Table 2) and a new lottery c constructed on the spot. Lotteries c were constructed to induce predetermined choice patterns and did not replicate any of the lotteries from Table 2. For the construction of c, we relied on transparent first-order stochastic dominance (FOSD). A lottery a first-order stochastically dominates another lottery c if for any monetary outcome x, a

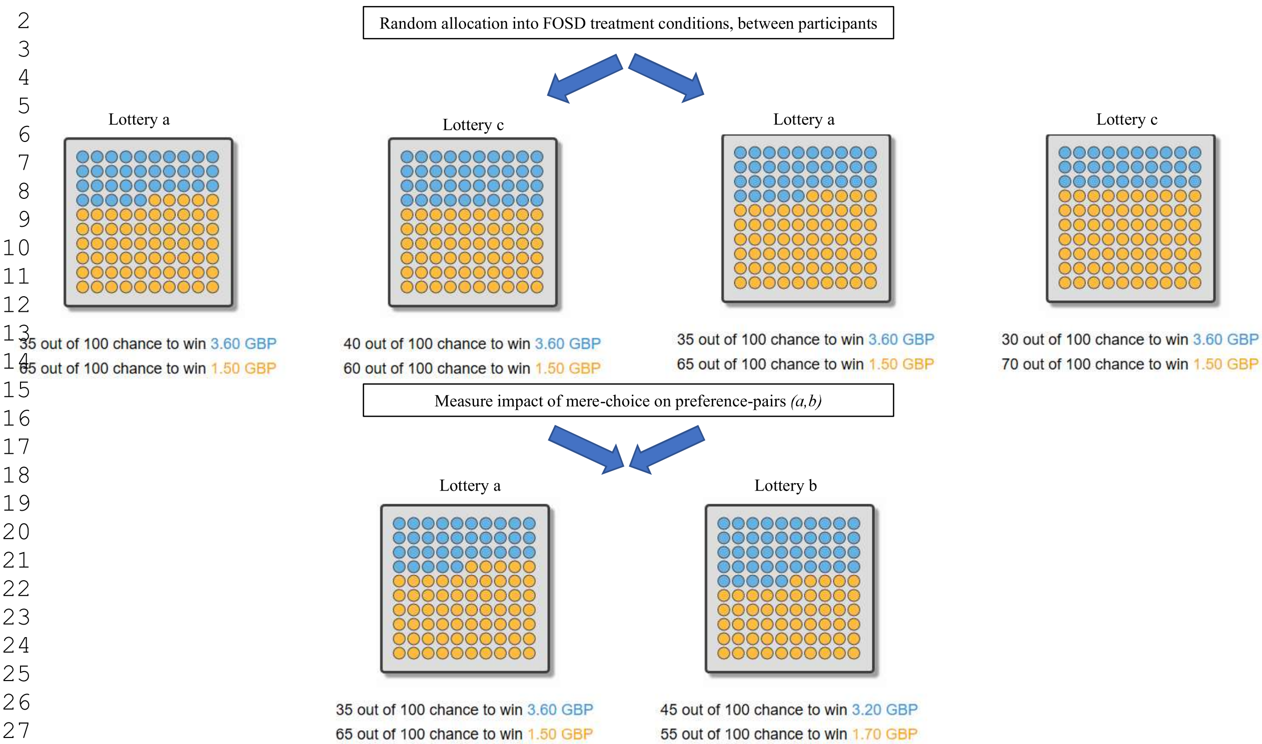

Fig. 1 Schematic overview experimental design, including screenshots from actual decision screens. Lottery labels *a*, *b*, and *c* are for illustrative purposes only, and were not shown to participants.

Table 2 Lotteries used in experiment. Lotteries pay amount Outc. 1 with probability p, and Outc. 2 with probability $1-p$. EV denotes expected values. All outcomes are in British Pounds (£), as Prolific is UK-based and compensates participants in £.

Lottery a	p	Outc. 1	Outc. 2	EV	Lottery b	p	Outc. 1	Outc. 2	EV
1	0.18	4.80	1.30	1.93	5	0.24	4.10	1.50	2.12
2	0.35	3.60	1.50	2.24	6	0.45	3.20	1.70	2.38
3	0.55	2.80	1.00	1.99	7	0.65	2.40	1.20	1.98
4	0.75	1.90	0.90	1.65	8	0.85	1.60	1.10	1.53

gives at least as high a probability of receiving at least x as does c, with strictly higher probability for some x. If a lottery first-order stochastically dominates another lottery, the former is objectively superior, independently of underlying risk preferences, as long as participants prefer larger amounts of money over smaller ones (the same remains true if decision makers are described correctly by cumulative prospect theory or rank-dependent utility instead of expected utility theory).

In the experiment, participants were randomly assigned to one of two possible treatments. In the CHOOSE treatment, lottery a dominated lottery c. Participants who obeyed FOSD thus 'merely-chose' a. In the REJECT treatment, the FOSD relationship was reversed so that participants obeying FOSD 'merely-rejected' a. To obtain transparent FOSD relationships, we changed one lottery attribute keeping the other one constant. For robustness reasons, half of the cases changed the probabilities and the other half changed the monetary outcomes (more details are provided below). These cases were counterbalanced between participants, yielding a 2×2 between-participants design (mere-choice vs. FOSD domain manipulation). Figure 1 includes a schematic overview of our FOSD construction for the probability domain. Randomization into treatments occurred after passing the control quiz.

The preference-choice task in stage 2 followed a setup analogous to the mere-choice task. It consisted of four pairs of lotteries. Each preference pair presented one target lottery a and the corresponding lottery b given in the same row in Table 2. We thus had four fixed preference pairs of the form (a,b): lottery pairs (1,5), (2,6), (3,7), and (4,8), as given in Table 2.

To incentivize decisions, we implemented a random lottery incentive system (Cubitt et al., 1998). A participant's payment for the experiment was derived by selecting one of the eight lottery pairs from stage 1 and stage 2 at random. The participant then received the lottery she had chosen and that lottery was played out. This was done after all decision-relevant data was collected. On the basis of past experience with comparable experiments, the experiment was expected to last about 7 minutes and yield an average remuneration of £2.61.[4] Actual average duration was XX minutes and actual average remuneration was £YY.

The lotteries in Table 2 were designed such that no FOSD relation obtains among any preference pair (a,b); lotteries of type c do not duplicate any of the existing lotteries a or b; all lotteries are non-degenerate, i.e., no certainty is involved; and the expected average payment of the experiment meets the current standards in experimental economics. The online appendix contains screenshots of all phases of the experiment. The complete experiment is accessible via:

`http://bit.ly/mere-choice`

3.3 Measuring and testing the mere choice effect

In our design, the mere choice effect on future choices can be measured precisely. In preference-choices (a,b), merely-chosen target lotteries a should be chosen more often than merely-rejected target lotteries a. This effect is causal, because it was randomly determined whether the target lottery was merely-chosen or merely-rejected. Statistical significance is assessed via a Mann-Whitney-U (MWU) test, one-tailed as our hypothesis is directional. For the test, we count for each participant how often she chose lottery a in preference choices (a,b) (from 0 to 4). Let x_{CHOOSE} and x_{REJECT}

[4] In Prolific, participants are paid a flat completion fee of £0.60. Assuming that all choices comply with FOSD, the expected value of our random lottery incentive system is £2.01. Hence, expected earnings are £0.60 + £2.01 = £2.61. This is more than twice as high as the current highest minimum wage rate in the U.S.

denote one randomly drawn choice-count observation from each of the two treatments CHOOSE and REJECT, respectively. The MWU tests the following statistical hypotheses:

H0: Probability$[x_{CHOOSE} > x_{REJECT}] \leq \frac{1}{2}$
Ha: Probability$[x_{CHOOSE} > x_{REJECT}] > \frac{1}{2}$.

We commit to have found supportive evidence of a mere choice effect if and only if the MWU test is significant at the 5% level.

3.4 Power calculations

We expected a small effect size and hence set $d = 0.2$ for power calculations (Cohen, 1988, 1992); for example, the related literature on choice-induced preference change in psychology reports an average effect size of $d = 0.26$ (Izuma and Murayama, 2013). Setting $\alpha = 0.05$, $1-\beta = 0.8$, and $d = 0.2$, the *a priori* required sample size for a one-tailed MWU test is 650 participants, equally split between treatments. Hence, the research question is best tackled by a large-sample but rather short experiment, and hence on online platform is ideal.

3.5 FOSD and exclusion criteria

To ensure that the FOSD manipulation induced behavior as expected, independently of other factors, we aimed to maximize the transparency of FOSD relationships. We therefore changed one lottery attribute keeping the other one constant. In the probability domain, FOSD relationships were established by adding or subtracting five percentage points in probabilities for the higher outcome. In the outcome domain, we added or subtracted 20 pence to or from the high outcome. For example, let (p ; x, y) denote a lottery that pays x with probability p and y with the complementary probability $1-p$. Let the target lottery be $a = (0.25; 12, 2)$. Suppose we wish to construct a lottery c so that a is to be chosen in the pair (a, c). In the probability domain, we would construct $c = (0.20; 12, 2)$. In the outcome domain, we would set $c = (0.25; 11.8, 2)$. In the former case, c pays the same amounts as a, but entails a lower probability to win the higher amount. In the latter case, c simply pays less money, but the probabilities are the same as in a. If behavior follows FOSD, participants are expected to choose a in both cases.

However, it is possible that some participants violate FOSD, e.g. due to lack of attention. We committed to excluding participants who violate FOSD in at least one of the four mere-choice pairs from the analysis. Alós-Ferrer et al. (2016) conducted a laboratory experiment with a standard student population. The authors included FOSD-choice pairs similar to ours as a basic rationality check in their experiment, which was designed to test an unrelated phenomenon (the preference reversal phenomenon). The authors report extremely low FOSD violation rates (around 2%). As in Alós-Ferrer et al. (2016), we use incentivized choice, and our lottery presentation format relies on icon-arrays which communicate risk understandably to lay audiences (Garcia-Retamero and Galesic, 2010; Dambacher et al., 2016). Taking into account the noisier online environment, we therefore expect FOSD violations rates of 5%. We conservatively set to obtain the required number of 650 observations after a 5% of exclusions, leading to a required number of participants of 682, which we conservatively rounded up to 700. We committed to performing our tests with all remaining participants after excluding those who violated FOSD.

This exclusion is based on objective criteria and does not compromise a causal interpretation of our results. First, the two treatments only differ with respect to whether c is objectively better or worse than a. Otherwise, they are identical. Participants are blind with regard to the identities of the lotteries, they do not know which lottery is of type a, c, or b. Hence, FOSD violations are pure noise and we do not foresee any plausible reason why FOSD violation rates should vary across treatments. Second, mere choices do not carry any *a priori* relevant information for preference pairs (a,b). Hence, our exclusion criterion does not condition on any relevant information with regard to the measurement of the mere choice effect. Admittedly, one can take the position that excluding participants limits the generalizability of our conclusions, and that all results stated hold only for the subset of participants who obey FOSD in the mere-choice task (or actually pay attention to the task). However, we expected this subset to be large.

4 Results (analysis plan)

To ease the exposition, we first explicitly spell out our expected results and then simply report the analyses we plan to conduct. Expectations are spelled out in italics and will be deleted if accepted to the final paper stage.

4.1 FOSD violations

As argued above, we do not expect any differences between treatments CHOOSE and REJECT in terms of FOSD violation rates. The treatment assignment process happened after the control quiz and participants were blind with respect to the identities of the lotteries. We also do not expect any difference in FOSD violations rates between the domains we manipulated to obtain FOSD relationships. It should be equally easy/difficult to detect higher probabilities or higher outcomes, everything else being equal. Based on Alós-Ferrer et al. (2016), we also expect a low rate of FOSD violations.

In total we observed YY decisions in which one lottery dominated the other one in the FOSD sense. Only a small fraction of these decisions violated FOSD in the CHOOSE and REJECT treatments, respectively YY (ZZ%) and YY (ZZ%). We run a 2×2 Boschloo test[5] assuming under the null that FOSD exclusion rates are equal

[5] With proportions close to one or zero, the Fisher's exact test for 2×2 contingency tables can be too conservative. This is because the test conditions on the margins to calculate what is a more extreme observation. The Boschloo test uses the Fisher's exact p-value to unconditionally calculate more extreme

between mere-choice treatments. With a p-value of YY we cannot reject the null. We further split up the data across manipulation domains, i.e., outcomes and probabilities. FOSD violation rates are again low, respectively YY (ZZ%) and YY (ZZ%). We run a 2×2 Boschloo test assuming under the null that FOSD exclusion rates are equal between domains of manipulation. With a p-value of YY we cannot reject the null. Violations of FOSD were rare events and did not undermine the validity of the mere-choice randomization procedure of our experimental protocol.

4.2 Mere choice effect

We expect our main hypothesis to be supported. Mere choices impact preferences and increase the choice frequency of merely-chosen lotteries. In accordance with our statistical hypothesis, we therefore expect the MWU test to be significant at the 5% level. Any deviation from that will be reported here, but only explored in the subsequent subsection.

The left-hand side of Figure 2 plots the average number of times that lottery a was chosen across participants (0 to 4) for the CHOOSE and REJECT treatments. With YY in the CHOOSE treatment vs. ZZ in the REJECT treatment, the participant-average count of choices for a in (a,b) was higher in treatment CHOOSE than in treatment REJECT (medians were YY and YY, respectively). These observations are corroborated by a one-sided MWU test on differences in the distribution of a-choices between treatments ($z =$XX, $p = .$YY), see Section 3.3. Uninformative mere choices significantly increased the choice frequencies of merely-chosen lotteries. For illustrative purposes, the right-hand side of Figure 2 also plots the choice frequencies for lottery a in preference pairs (a,b) for the CHOOSE and REJECT treatments, for each of the four preference pairs (a,b) separately. In accordance with our main hypothesis, we observe that merely-chosen lotteries a were chosen more often than comparable but merely-rejected lotteries a in preference pairs X, Y, ...

4.3 Robustness analysis

In this subsection we detail our planned robustness analysis. We will rely on regression analyses, which are well-suited to study individual-level behavior. Our data structure dictates the use of panel regressions. Independent observations are taken at the individual-preference-pair level. Our dependent variable is a dummy, taking the value 1 if a participant chose a in (a,b). We have two goals in this subsection, both related to the robustness of our findings from Section 4.2.

First, we plan to confirm our results from Section 4.2. To this end, we will run regressions with different specifications of control variables. Our main independent variable is the mere-choice treatment dummy. We plan to successively include period fixed effects (dummies capturing the round of the preference-choice task), lottery pair fixed-effects (dummies capturing the different lottery pairs), a control dummy for

observations. It can be shown that the Boschloo test is uniformly more powerful than Fisher's exact test (Boschloo, 1970; Mehrotra et al., 2003).

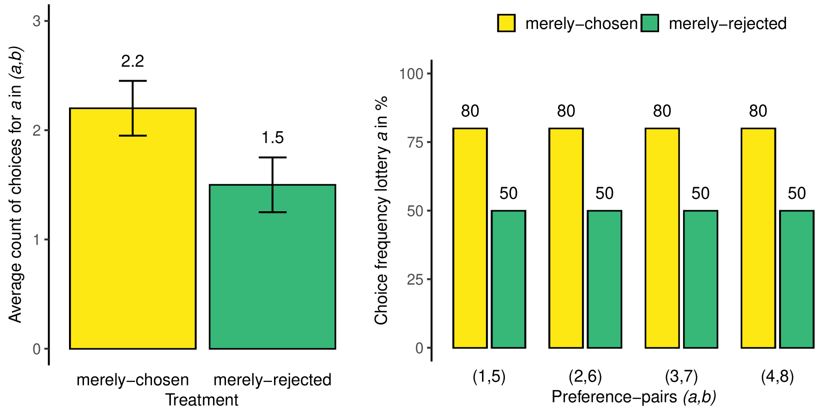

Fig. 2 Left-hand panel: Average count of choices for lottery a in (a,b) across mere-choice treatments. Right-hand panel: Choice frequencies for lottery a across preference pairs (a,b) and mere-choice treatments.

the FOSD manipulation domain, presentation variables (position on screen, winning color), and demographics (gender, age, income, education level, student status, employment status). We will run the regressions on both the sub-set of participants who obey FOSD and all collected data treating FOSD violations as regular mere choices. We will clearly point out any contingency of our main mere-choice results.

Second, we plan to explore whether the FOSD manipulation domain had any impact on the mere choice effect. To this end, we will add an interaction term between the mere-choice treatment dummy and the domain manipulation dummy in the most comprehensive specification of our econometric model. We will run a post-hoc hypothesis test to check whether the mere choice effect differs between FOSD manipulation domains.

Detailed analysis plan. We ran panel probit regressions with participant random-effects to confirm our main analysis on the mere choice effect. Independent observations were taken at the participant-preference-pair level. Our dependent variable is the *Choice* dummy, taking the value 1 if a participant chose a in (a,b). Reported are average marginal effects with standard errors in parentheses. The corresponding results are presented in Table 3, Models (1) to (5).

Our regression analysis confirms our main findings from Section 4.2. The Merely-Chosen dummy, taking value 1 if lottery a was merely-chosen, is positive and significant. The choice frequency for merely-chosen lotteries a was between YY and YY percentage points higher than for merely-rejected lotteries a. These results are robust with regard to preference-pair-specific features and period effects, demographic controls, and presentation controls. Models (1) to (4) utilize data after excluding participants violating FOSD. Model (5) is equivalent to model (4) in terms of specification, but includes participants who violate FOSD (with FOSD violations treated as regular mere choices).

Table 3 Panel probit regressions on Choice dummy (choose a in (a,b)) with participant random-effects. Model (1) to (5) report average marginal effects with standard errors in parentheses. Model (6) reports raw coefficient estimates with standard errors in parentheses. Significance codes: * - 5%, ** - 1%.

Dependent variable	Choice dummy, choose a in (a,b)					
Model	(1)	(2)	(3)	(4)	(5)	(6)
Merely-Chosen	YY**	YY**	YY**	YY**	YY**	YY**
	(YY)	(YY)	(YY)	(YY)	(YY)	(YY)
FOSD Manipulation Domain: Probabilities		YY**	YY**	YY**	YY**	YY**
		(YY)	(YY)	(YY)	(YY)	(YY)
Merely-Chosen × FOSD Manipulation Domain: Probabilities						YY
						(YY)
Position screen: Right			YY	YY	YY	YY
			(YY)	(YY)	(YY)	(YY)
Winning color: Orange			YY	YY	YY	YY
			(YY)	(YY)	(YY)	(YY)
Gender (indicators)				YY	YY	YY
				(YY)	(YY)	(YY)
Age				YY	YY	YY
				(YY)	(YY)	(YY)
Student status				YY	YY	YY
				(YY)	(YY)	(YY)
Education level (indicators)				YY	YY	YY
				(YY)	(YY)	(YY)
Employment status (indicators)				YY	YY	YY
				(YY)	(YY)	(YY)
Income (indicators)				YY	YY	YY
				(YY)	(YY)	(YY)
Constant	YY	YY	YY	YY	YY	YY
	(YY)	(YY)	(YY)	(YY)	(YY)	(YY)
Number of participants	665	665	665	665	700	665
Number of observations	2,660	2,660	2,660	2,660	2,800	2,660
FOSD violations	No	No	No	No	Yes	No
Period fixed-effects	No	Yes	Yes	Yes	Yes	Yes
Lottery fixed-effects	No	Yes	Yes	Yes	Yes	Yes
Interaction treatments	No	No	No	No	No	Yes

Demographic control variables are included for robustness purposes, but we have no specific hypotheses about them, as the existing literature has typically reported no such associations. We will simply state which coefficients are significant and provide some interpretations. For the presentation variables, one could expect the typical left-hand-side bias encountered in survey experiments. We have no prior hypothesis regarding the winning color (orange or blue color for the higher amount to win).

Finally, we analyze whether the mere choice effect differs between FOSD manipulation domains. We expect no differences between the manipulations domains, which were included for robustness purposes. To this end, we included an interaction term between the mere-choice treatment dummy and the domain manipulation dummy in our fully specified econometric model in our probit estimations. The results are presented in Model (6) of Table 3. It is, however, not possible to estimate the marginal effect of an interaction term in non-linear models like probit. Model (6) therefore reports the raw panel probit coefficient estimates. The average marginal mere-choice effects in the two manipulation domains were estimated via STATA's margins commands specifying the probability domains via the *at* option. The corresponding estimates are YY and YY percentage points for the probability domain and outcome domain, respectively. We then used STATA's built-in contrast functionality of the margins command to assess the statistical significance in differences in mere-choice effects between manipulation domains. The corresponding *p*-value was YY. To conclude, we found no influence of the manipulation domain on the mere-choice effect. Figure 3 plots the corresponding unconditional choice frequencies data. The top panel displays average number of times that lottery *a* was chosen across participants (0 to 4) for the CHOOSE and REJECT treatments, disentangling manipulations. The bottom panel presents the analogous data disentangled by choice pairs.

5 Conclusion

Using a novel, parsimonious experimental design, we have presented the first conclusive evidence on the economic validity of the mere-choice induced preference change phenomenon.

The following two subsections present a "strategy-method" conclusion. We will include the relevant one depending on results.

5.1 If evidence is found as expected

Our results establish that experimentally controlled past choices can causally shape subsequent risk preferences with fully-incentivized decisions. Quasi-randomly assigned, and therefore uninformative, 'chosen' and 'rejected' labels consistently impacted participants' future choices over simple monetary lotteries. These results demonstrate, for the first time, the existence and relevance of a bi-directional link between behavior and preference for the domain of decision making under risk.

From predicting consumer behavior to cost-benefit analyses of medical treatments to welfare comparisons of alternative market institutions, many applications of standard theories of decision making under risk are built on the possibility to organize observed choices through underlying stable preferences. We have shown that the latter view misses out a fundamental aspect of human behavior, as choice may actively alter preferences. Our results offer an opportunity to stimulate further research into this topic, as they highlight the need to gain a deeper understanding of the mere choice effect. A better understanding of decision makers' need for logical and cognitive consistency with their own past choices will help develop better preference elicitation methods and improve the predictive accuracy of formal models of behavior.

We would like to suggest that new formal models need to take into account the reality of preference change on the face of previous, uninformative choices. For ex-

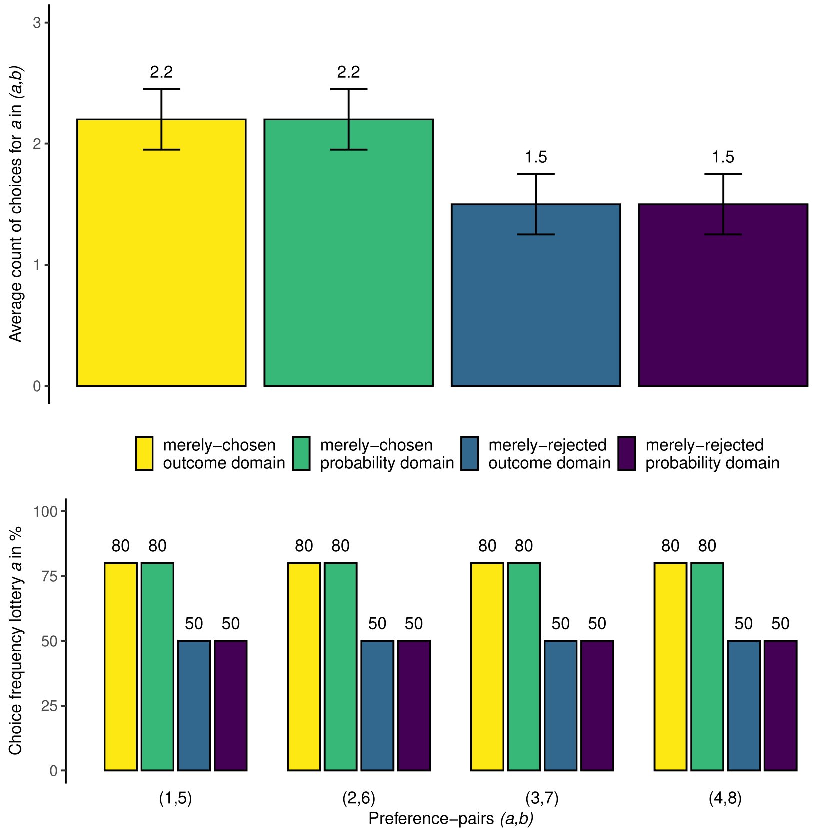

Fig. 3 Top panel: Average count of choices for lottery a in (a,b) across treatments. Bottom panel: Choice frequencies for lottery a across preference pairs (a,b) and treatments.

ample, Alós-Ferrer and Mihm (2019) consider a general model of updating in models of stochastic choice, which in particular includes the possibility that a decision maker updates his or her preferences following the own, previous choices. Also, further experimental and empirical research on this topic is needed to identify expected preference-change patterns, which will both discipline theoretical developments and allow applied researchers to correct for them in elicitation methods.

5.2 If only a null result is found

We do not find evidence which could be interpreted as mere-choice-induced preference change. Of course, absence of evidence is not evidence of absence, but, given the power analysis underlying our analysis, the simplest explanation for our results at this point is that mere-choice-induced preference change in economic domains does not exist or is of a negligible magnitude.

From predicting consumer behavior to cost-benefit analyses of medical treatments to welfare comparisons of alternative market institutions, many applications of standard theories of decision making under risk are built on the possibility to organize observed choices through underlying stable preferences. We have shown that the latter view seems appropriate with regard to mere-choice-induced preference changes.

However, we remark that we have studied the pure effect of uninformative choice on preference. A related stream of literature in psychology, which regrettably used a flawed design (see Alós-Ferrer and Shi, 2015, for details), can be seen as incorporating some form of trade-off in choice. If trade-offs are a necessary precondition for the phenomenon to emerge then appropriate experimental designs will have to be developed, with an eye on separating this potential source from the pure effect of choice. At this point, however, we have to conclude that economics can safely ignore the phenomenon of mere-choice-induced preference change.

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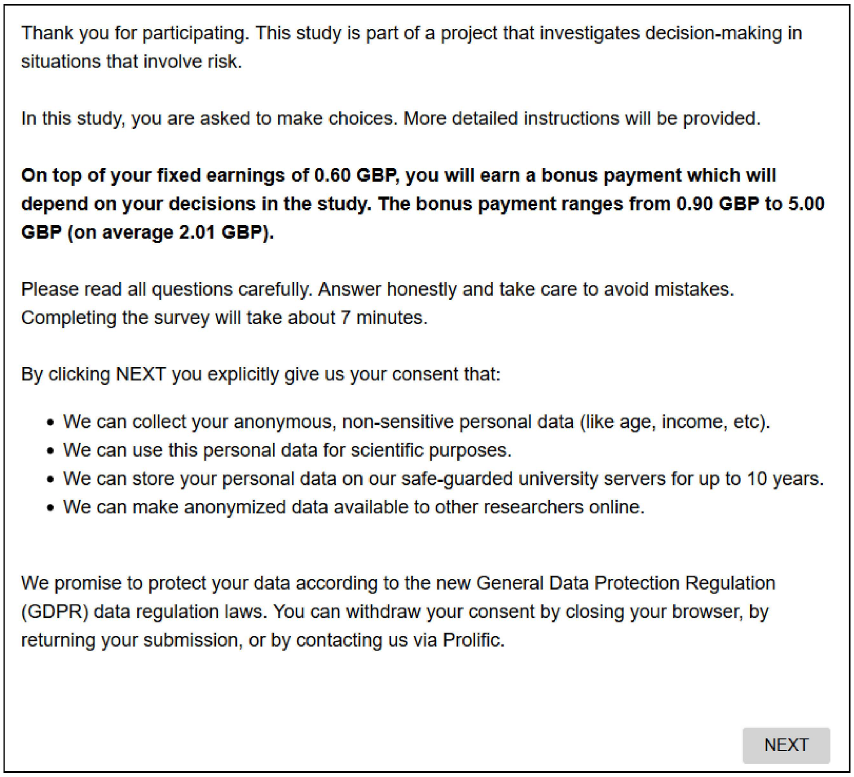
A Online appendix



Thank you for participating. This study is part of a project that investigates decision-making in situations that involve risk.

In this study, you are asked to make choices. More detailed instructions will be provided.

On top of your fixed earnings of 0.60 GBP, you will earn a bonus payment which will depend on your decisions in the study. The bonus payment ranges from 0.90 GBP to 5.00 GBP (on average 2.01 GBP).

Please read all questions carefully. Answer honestly and take care to avoid mistakes.
Completing the survey will take about 7 minutes.

By clicking NEXT you explicitly give us your consent that:

- We can collect your anonymous, non-sensitive personal data (like age, income, etc).
- We can use this personal data for scientific purposes.
- We can store your personal data on our safe-guarded university servers for up to 10 years.
- We can make anonymized data available to other researchers online.

We promise to protect your data according to the new General Data Protection Regulation (GDPR) data regulation laws. You can withdraw your consent by closing your browser, by returning your submission, or by contacting us via Prolific.

NEXT

Fig. A.1 Screen 1 online experiment: General introduction and consent.

Your bonus payment today depends on the decisions you are about to make. At the end of the survey, we will randomly pick one of your decisions. This particular decision will then be paid out according to the rules specified in later screens.

Each decision could be the one that counts for your bonus. It is therefore in your best interest to consider all your answers carefully.

Before you proceed, please answer the sports test. The test is simple, when asked for your favorite sport you must enter the word ***clear*** in the text box below.

Based on the text you read above, what favorite sport have you been asked to enter in the text box below?

Please click on NEXT to proceed.

NEXT

Fig. A.2 Screen 2 online experiment: Random lottery incentives and attention check.

Unfortunately you failed our attention check. You have been asked to enter the word 'clear'.

You entered: 'failed attention check'.

The study will now be terminated. Please return your submission on Prolific by selecting the 'Stop without completing' button.

Fig. A.3 Screen 2a online experiment: Failed attention check.

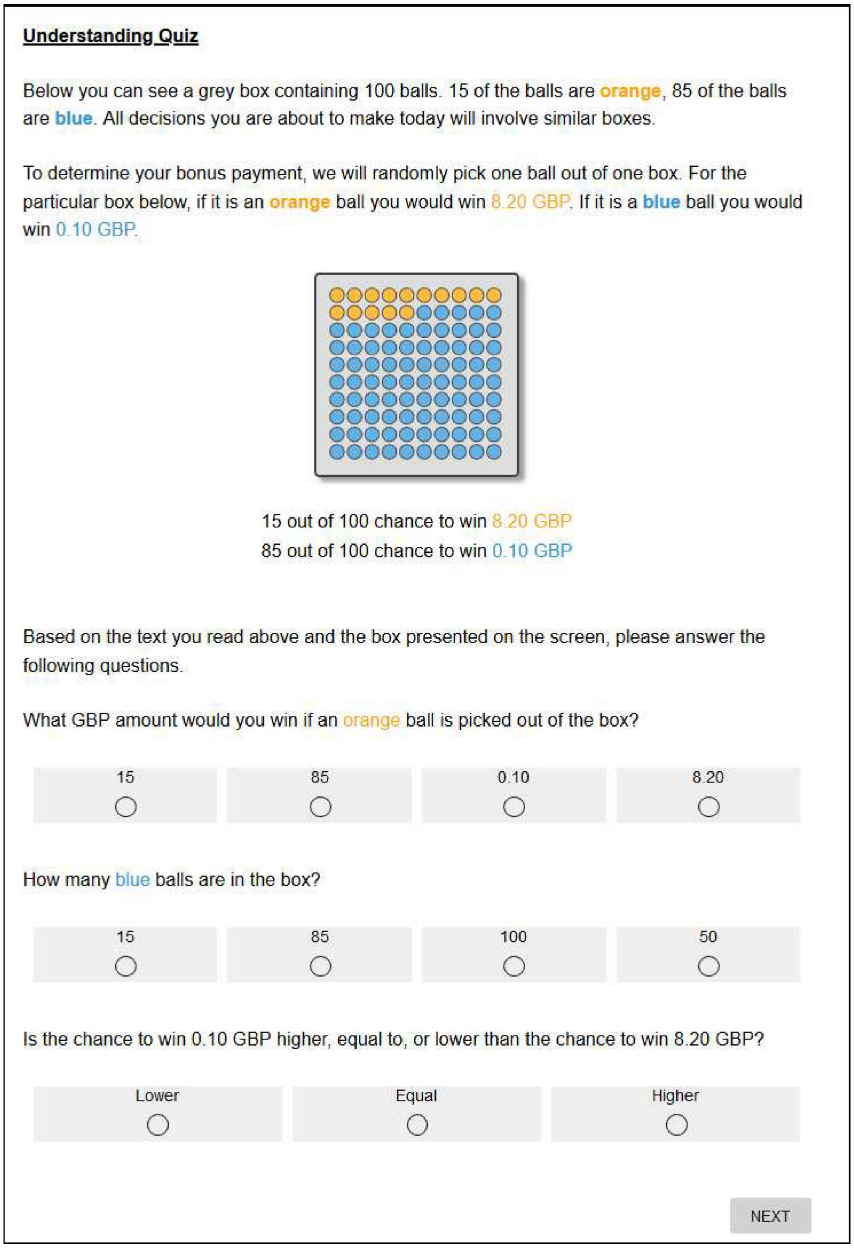


Fig. A.4 Screen 3 online experiment: Understanding quiz.

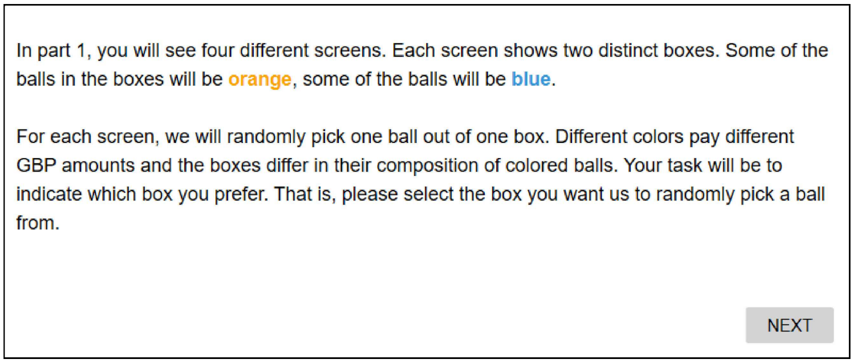


Fig. A.5 Screen 4 online experiment: Mere-choice task introduction.

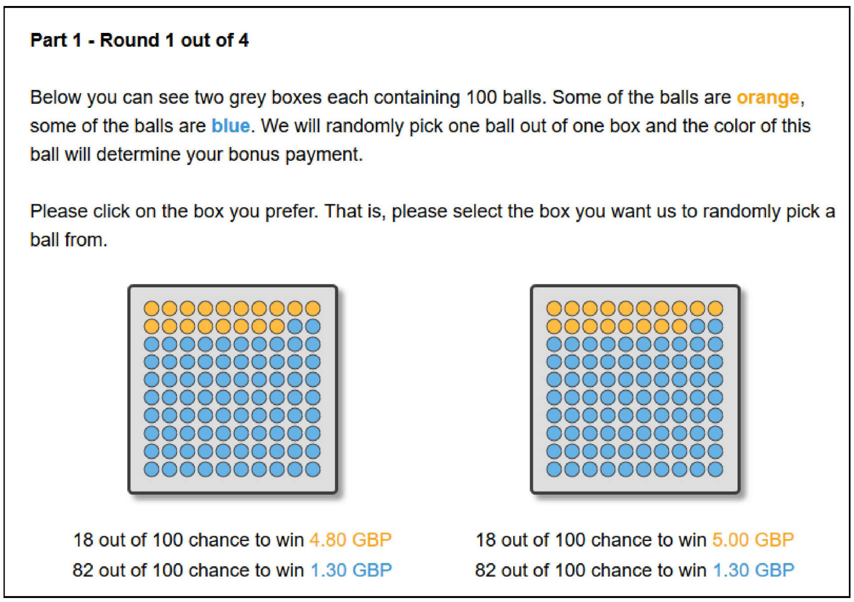


Fig. A.6 Screen 5 online experiment: Mere-choice task, REJECT treatment, lottery a on left-hand side of screen ($ID = 1$), FOSD on outcomes.

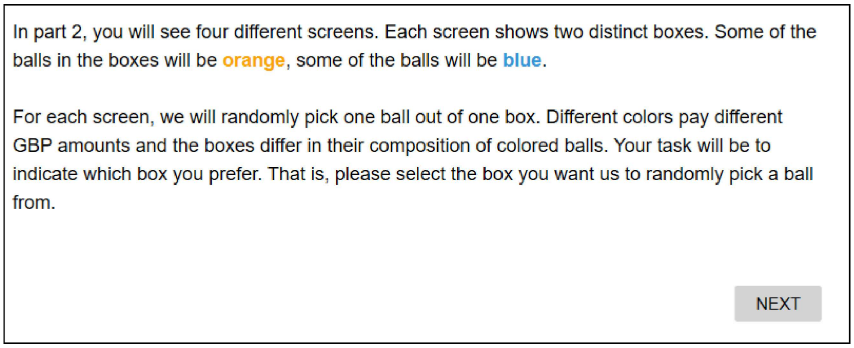


Fig. A.7 Screen 6 online experiment: Preference-choice task introduction.

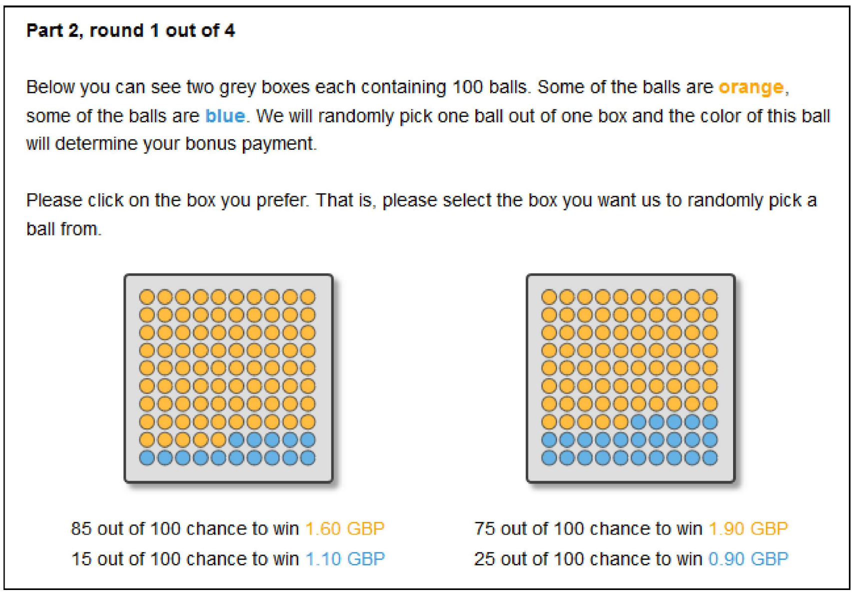


Fig. A.8 Screen 7 online experiment, preference pair $(4,8)$, lottery a on right-hand side of screen.

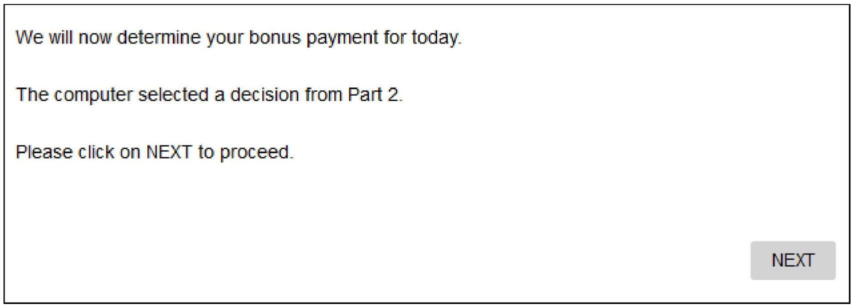


Fig. A.9 Screen 8 online experiment: Payment introduction.

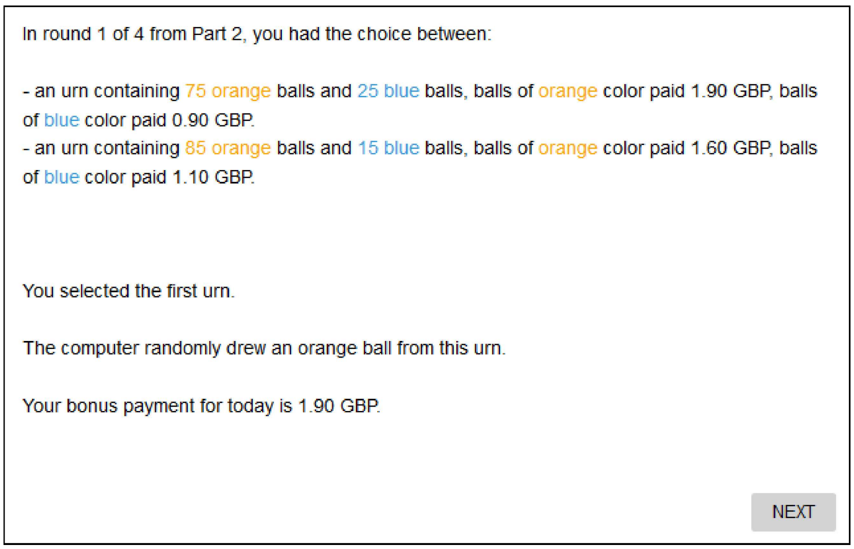
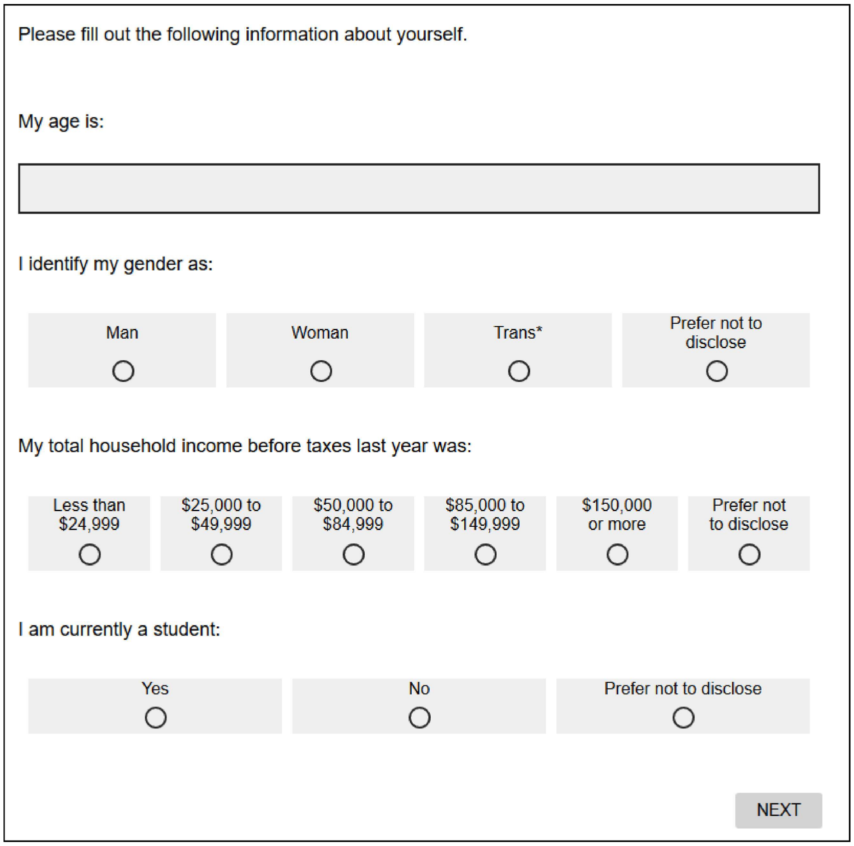


Fig. A.10 Screen 9 online experiment: Payment feedback for participant selecting lottery a in preference pair $(4,8)$.

Fig. A.11 Screen 10 online experiment: Payment information.



Please fill out the following information about yourself.

My age is:

I identify my gender as:

Man | Woman | Trans* | Prefer not to disclose

My total household income before taxes last year was:

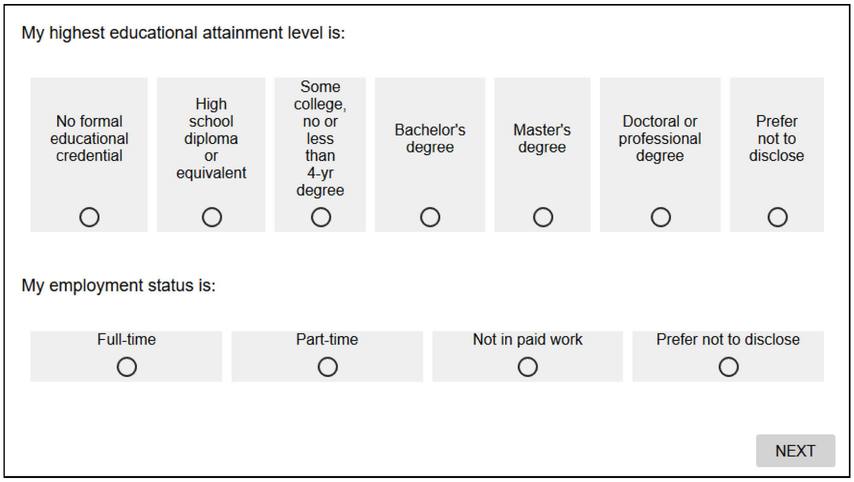
Less than $24,999 | $25,000 to $49,999 | $50,000 to $84,999 | $85,000 to $149,999 | $150,000 or more | Prefer not to disclose

I am currently a student:

Yes | No | Prefer not to disclose

NEXT

Fig. A.12 Screen 11 online experiment: Final questionnaire part 1.


My highest educational attainment level is:

No formal educational credential
High school diploma or equivalent
Some college, no or less than 4-yr degree
Bachelor's degree
Master's degree
Doctoral or professional degree
Prefer not to disclose

My employment status is:

Full-time
Part-time
Not in paid work
Prefer not to disclose

NEXT

Fig. A.13 Screen 12 online experiment: Final questionnaire part 2.