Default-Setting and Default Bias: Does the Choice Architect Matter?*

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Abstract

This paper studies how choices are influenced by the procedure used to select the default option. We develop an approach to test and compare default bias across different default-setting rules while controlling for heterogeneous preferences. We apply it to a within-subjects experimental design lottery choice experiment to compare four different default-setting rules: Random defaults, Custom defaults selected based on an individual's own past choices, Social defaults selected based on others' choices, and Expert-set defaults. We find that the content of default-setting rules matters: default bias is present for all non-random default-setting rules we study, but not for randomly-set defaults.

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1 Introduction

In a variety of decisions ranging from choice of health plan or retirement contributions to shipping methods for online shopping, one option is pre-selected as the "default alternative" and thus will be selected unless the decision-maker actively changes to another option. While pre-selecting a default alternative does not in itself affect the set of alternatives available to the decision-maker, behavioral economists have documented that it can have a substantial impact on actual choices — biasing decision-makers towards the default [Samuelson and Zeckhauser, 1988, Madrian and Shea, 2001, Johnson and Goldstein, 2003, Handel, 2013, Ericson, 2014]. Default bias is a classic example of how "choice architecture" that does not influence the set of available options can influence decisions (Thaler and Sunstein 2003).

This observation motivates firms and governments to intentionally set defaults — often with the aim of improving decisions. In principle and in practice, there are many different ways to select a default option from a choice set. However, when people's preferences are heterogeneous, any selected default option will be undesirable for some people. In such a setting, it is unclear whether and, if so, how people's response to defaults depends on the rule used to select the default option. It is also not obvious how people would want defaults to be set if they were given the choice.

This paper provides the first systematic study of how people both respond to and rank different rules for selecting a default option from a choice set in a setting where the desirability of choice alternatives is not objectively ranked. We consider four default-setting rules: (i) defaults set randomly ("Random"), (ii) defaults based on the decisions of others ("Social"), (iii) defaults selected by an expert ("Expert") set based on a normative criterion, and (iv) defaults custom-selected for each person based on their past choices ("Custom"). These four default-setting rules are motivated from discussions of choice architecture in the behavioral economics literature and real-world examples of default-setting in practice [Thaler and Sunstein, 2003, Johnson et al., 2012, Madrian, 2014, Jachimowicz et al., 2019, Beshears and Kosowsky, 2020].

To compare these default-setting rules in a setting where preferences are heterogeneous, we thus conduct an experiment in which each subject makes choices among risky lotteries. Each subject faces both choices without a default option ("No Default") and choices with a default option selected according to each of the four different default-setting rules. Comparing choices under a default-setting rule versus choices without a default allows us to measure and test for the presence of absolute default bias separately for each rule. To examine whether the default-setting rule itself affects adherence to defaults, we also compare the strength of default bias across different default-setting rules. Finally, we ask each subject to rank these four default-setting rules as well as choice without defaults, thereby observing their preferences over default-setting rules.

An important contribution of our paper is to provide a generally applicable approach to measure and test for absolute and comparative default bias that controls for subjective default quality at the individual level. Such control is necessary because people tend to choose options they prefer. Thus a decision-maker will tend to choose the default option more often under a default-setting rule that tends to pick options they prefer. This confounds the measurement of default bias and its comparison across default-setting rules that sometimes select different defaults. Our experiment has each subject face each choice set under multiple default rules, allowing us to use choice from the same set under No Default as a control for measuring absolute default bias under each rule. Similarly, we compare the strength of default bias across rules using choice sets in which two rules assign the same default option.

We find significant evidence of default bias under each of the Social, Expert, and Custom default-setting rules but do not detect default bias with Random defaults. This suggests that decision-makers respond not just to defaults themselves, but also how they are set. That said, we do not find statistically significant differences in the strength of bias between the three rules that set defaults intentionally. Our data additionally confirms the importance of controlling for default quality, as subjects are substantially more likely to choose a default option that they had also chosen when there was no default. Finally, subjects' rankings of default-setting rules reveals that they tend to prefer rules that set subjectively higher quality defaults, with a noticeable preference for the Expert default-setting rule – but there is also a notable minority of subjects who most prefer No Default.

We chose our default-setting rules to mimic real-world default-setting rules. Randomly selected defaults are a useful theoretical benchmark since defaults will be transparently uncorrelated with individual preferences and control for any possible endorsement implied by the default. Randomly selected defaults have been used in past experiments like Samuelson and Zeckhauser [1988] and in the United States of America for assigning a default health insurance plan under the Medicare Part D program [Ericson, 2014]. More broadly, we view randomly selected defaults as representing real-world cases where a historically-set default is unlikely to be correlated with a person's preferences. Social defaults are another possibility suggested by Thaler and Sunstein [2003]. In some situations, sellers may indicate the most popular choices among a list of available alternatives, for example when the most popular color is pre-selected for cars or the most popular configuration is pre-selected for personal computers. The discussion of choice architecture tends to views defaults as something that experts can intentionally set. Our design intentionally provides no information on how the expert chooses defaults – which we view as consistent with real-world examples. For example, the default allocation for pension plan contributions at Simon Fraser University is to a "balanced" fund designed by the pension fund trustees and pension administrator to be a good and balanced option for a large number of plan members. Customized defaults have been suggested as one way to improve choices [Smith et al., 2013], including health plan choices [Zhang et al., 2015]; "sensory defaults" on a website based on cookies or other information are an example of where custom defaults are used in practice [Johnson et al., 2012].

Following our discussion of related literature, we present our approach for measuring and comparing default bias across default-setting rules while controlling for default quality at the individual level (Section 2), our experimental design and procedure (Section 3), our results (Section 4), and discussion (Section 5).

Related Literature

Our paper builds on the broad literature on choice architecture [Thaler and Sunstein, 2003] and default bias [Samuelson and Zeckhauser, 1988, Madrian and Shea, 2001, Johnson and Goldstein, 2003, Handel, 2013, Ericson, 2014].

A related line of experiments study the effects of default quality on subjects' propensity to choose the default option in settings where the quality of choice options can be objectively ranked and ought to be the same across all subjects. This literature consistently finds that when default quality is manipulated, subjects are more prone to choose the default option when the default-setting rule has a greater tendency to select higher quality defaults [Caplin and Martin, 2017, de Haan and de Linde, 2018, Altmann et al., 2019a,b].[1] In each of these papers, each choice alternative is a monetary payment, but is presented so that computing each monetary payment is difficult [de Haan and de Linde, 2018, Altmann et al., 2019b] or requires a probabilistic inference about the default setter's information [Altmann et al., 2019a]. Unlike these papers, in most settings of interest (as in our experiment), preferences are subjective, heterogeneous, and not directly observed, thus the decision to follow or abandon a default takes on another dimension. Our study measures and compares default bias and decision quality in and across default-setting rules while controlling for subjective preferences.[2]

Our paper also complements the literature on the effect of defaults in the field. For

[1] Altmann et al. [2019b] study how the presence of a background tasks affects choice of the default option, and find that subjects tend to choose the default option more often when faced a more difficult background task. Altmann et al. [2019a] study a game where the default is set by a partially informed player whose incentives are either aligned, misaligned, or partially aligned with the partially informed decision-maker, and find that the default was chosen more often when incentives were more aligned. de Haan and de Linde [2018] study how the quality of defaults in earlier decisions affects subsequent default bias and find that it does.

[2] In a related line of work, Arad and Rubinstein [2018] study people's attitudes to soft interventions that have been proposed in the behavioral economics literature, and find that a substantial fraction of respondents are averse to government-mandated default savings rates and other interventions. However, they only study attitudes and do not elicit actual choice behavior.

example, a long literature following Madrian and Shea [2001] documents that automatic enrollment in retirement savings plans affects savings due to default adherence. This literature has suggested various possible reasons for why people exhibit default bias, including small adjustment costs and present biased procrastination [Carroll et al., 2009, Blumenstock et al., 2018] – factors suppressed in our experimental setting. Another leading explanation from this literature is an "endorsement effect" – that the decision-maker interprets the default as a recommendation from the choice architect [Madrian and Shea, 2001, Bernheim et al., 2015, Madrian, 2014]. In our setting, the nature of the endorsement varies across default setting rules. Hence our finding that we only observe default bias with intentionally-set defaults broadly supports the endorsement effect as a driver of default bias – and as an effect that can be varied by the choice architect by how they communicate their default setting rule. Our analysis suggests that a choice architect should strive not just to set good defaults but also to consider how to communicate the rule they use to select defaults.

2 Defining and Comparing Default Bias across Default-setting Rules

2.1 Definitions

We study decision-makers who make choices with and without default options under different default-setting rules. Our conceptual framework extends the existing work on default bias in choice (e.g. Masatlioglu and Ok 2005) to allow choice to depend on the rule used to select the default from the choice set, which we assume is known by the decision-maker.

Formally, let X be the set of all possible options, let $\mathcal{A}$ denote the set of all choice sets, which are non-empty subsets of X. Let $\mathcal{T}$ denote the set of functions, called default-setting rules, that select a probability distribution over defaults (or no default) for each set in $\mathcal{A}$; $\emptyset$

denotes "no default". A triple $(A, d, T) \in \mathcal{A} \times X \cup \{\emptyset\} \times \mathcal{T}$ defines a choice problem whenever d is in the support of $T(A)$.

In-principle observable behavior can be described by a stochastic choice function $p : X \times \mathcal{A} \times X \cup \{\emptyset\} \times \mathcal{T} \to [0, 1]$, where $p(x|A, d, T)$ denotes the probability that the decision-maker chooses x from choice set A when the default is d and was selected according to rule T.[3] Let $T =$ ND denote the rule that always assigns no default, $\emptyset$.

Next, we define default bias as a higher probability of choosing x from A when x is the default than when A is faced with no default option present. Note that in our definition, default bias is evaluated under specific default-setting rules – a decision-maker may exhibit default bias under one rule but not another.

Definition. p exhibits default bias under rule T if $p(x|A, x, T) \geq p(x|A, \emptyset, ND)$ for every $A \in \mathcal{A}$ and x in the support of $T(A)$, with strict inequality for at least one such choice problem.

This definition of absolute default bias compares the frequency with which the default option is chosen under rule T to the frequency with which the same option is chosen when no default option is present. To compare the strength of default bias across different default-setting rules, we adapt the main idea from our definition of default bias to control for default quality by only comparing choice under different rules when they prescribe the same defaults.

Definition. p exhibits a stronger default bias under default-setting rule T than under T' if $p(x|A, x, T) \geq p(x|A, x, T')$, for all choice problems (A, x) such that x is the default for A under both T and T', with strict inequality for at least one such choice problem.

[3]There exists substantial evidence that behavior has a random element, even when studied at the individual level (e.g. Hey 1995).

This definition of *comparative* default bias compares the frequency with which the default option is chosen under rule T to the frequency with which the same default option is chosen under rule T'.

2.2 Statistical tests of absolute and comparative default bias

In most settings, including our experiment, we only observe a finite number of choices per person and not their entire stochastic choice function. Thus we seek methods of testing for absolute and comparative default bias that allow us to aggregate data from all subjects while still carefully controlling for heterogeneity. Our definitions of absolute and comparative default bias are based on the comparisons of pairs of choice problems where each paired comparison controls for heterogeneity across individuals and choice sets.

To test for the presence of absolute default bias, we test the null hypothesis of "no default bias under T", stated formally as: $p(x|A, x, T) = p(x|A, \emptyset, ND)$ for every $A \in \mathcal{A}$ and x in the support of $T(A)$.For testing absolute default bias, each pair consists of an observed choice under a default-setting rule and a choice made when no default was present. Similarly, the null hypothesis of "equal default bias under T and under T'" is stated formally as: $p(x|A, x, T) = p(x|A, x, T')$ for every $A \in \mathcal{A}$ and every x that is in the support of both $T(A)$ and $T'(A)$. For comparative default bias tests, each pair consists of two observed choices, each with the same default, but where the default was selected under different default-setting rules in each case.

With data from many different choice sets from a single individual, or from one choice set each for many individuals, we could apply a McNemar's test to non-parametrically test the null hypothesis without having to estimate an entire stochastic choice function. However, when we observe many different choice sets for each individual under both T and ND, as we do here, we must use an Obuchowski [1998] test to aggregate across individuals and choice sets. Like a McNemar's test, the Obuchowski non-parametric test uses paired data to compare the estimated proportions with which the default is chosen. Yet, the Obuchowski test additionally includes an adjustment for intra-subject correlations to account for the fact that pairs of observations from the same subject cannot be viewed as independent. This adjustment is analogous to the use of clustered standard errors in a panel regression.

3 Experimental Design

Our individual choice experiment consists of 84 rounds of lottery choice tasks with monetary outcomes and no feedback between decisions. We use lotteries as simple-to-implement choice objects whose values are subjective and, based on the experimental literature on decision-making under risk, we expect substantial variation in tastes between individuals (e.g. Hey and Orme 1994, Holt and Laury 2005, Bruhin et al. 2010).

The experiment includes choices from 24 unique choice sets each comprised of five lotteries. Each of these choice sets is seen by the subject at least three times across different default-setting rules. In the first 24 rounds – the No Default (ND) treatment – subjects make choices from each of the 24 choice sets without any option being designated as the default option. In the following rounds, subjects proceed through the four default-setting rules (Random, Social, Expert, and Custom), completing 12 rounds for each. We then elicit each subject's ranking of the four default-setting rules and ND. To incentivize this ranking, rules that a subject ranked #1 through #4 were implemented with probabilities 90%, 7%, 2%, and 1% respectively to determine the defaults in Rounds 73 through 84. Table 1 summarizes the work-flow as well as the exact wording of how the default rules are communicated to the participants.[4]

[4] To address potential concerns about treatment order effects, we varied the order of three treatments with defaults - Social, Expert, and Custom, at the session level. Subjects went through No Default, Random, one of the six treatment orders, then finally Choice of Default. We also varied starting choice sets and the order of choice sets in each treatment to control for order effects.

We set defaults for each choice set according to the following procedures.

Random. A lottery was randomly selected from each choice set. Each lottery in the choice set was equally likely to be selected.

Social. We ran a pilot experiment with nine participants in October 2019 with the same 24 choice sets. We used this group's modal choice in the No Default treatment as the default lottery.

Expert. We evaluated each lottery in the choice set according to an expected utility functional with constant relative risk averse utility-for-income function $u(x) = \frac{x^{1-\gamma}}{1-\gamma}$ and $\gamma = \frac{3}{4}$. The lottery with the highest expected utility was selected.[5]

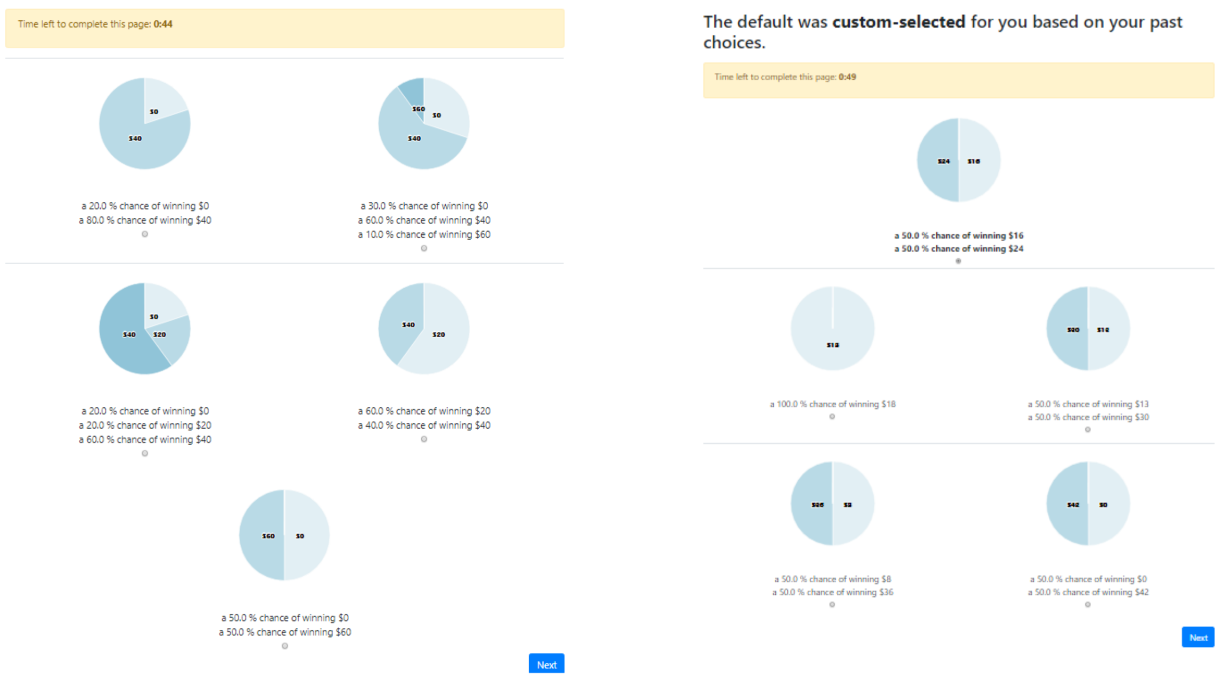
Custom. We coarsely scored each subject's risk aversion based on their choices in the No Default treatment using the three Eckel and Grossman [2002] style choice problems with a riskless option. In each of these choice sets, lotteries were scored from 1 (safest) to 5 (riskiest) and we added these scores to obtain a final score S between 3 (i.e. always choosing the safest option) and 15 (i.e. always choosing the riskiest option). Based on the score S, we assigned all subjects to one of three groups, each associated with a different constant relative risk aversion parameter γ that would generate a score in that range. Specifically, we assigned $\gamma = 2, 1.25$, and 0.5 respectively for the cases $S \leq 6$, $7 \leq S \leq 9$, and $S \geq 10$. Then, for each choice set, the expected utility maximizing lottery was selected as the default option.

Prior to making choices under a particular default-setting rule, the rule was described to

[5] Expected utility is a normatively appealing model, and expected utility with constant-relative risk aversion is widely used by experimental economists to describe choices involving risks. This parameter value was chosen to be consistent with the median choices in Holt and Laury [2005] and we thus felt that this was a good default for most subjects.

Treatment	Description	Rounds
No Default	“In each of the next 24 decisions, no option will be selected as the default option.”	1 - 24
w/ Defaults	“In each of the next 12 decisions, one option will be selected as the default option.”	
Random	“The default was selected **randomly** from the available lotteries.”	25 - 36
Expert	“The default was selected **by an expert** from the available lotteries.”	37 - 48*
Social	“The default is the option that was most often selected by **a group of previous participants**.”	49 - 60*
Custom	“The default was **custom-selected** for you based on your past choices.”	61 - 72*
Choice	Default rule is determined by incentive compatible ranking by subject	73 - 84
* The order of the Expert, Social, and Custom treatments was randomized across subjects.		

Table 1: Summary of treatments



(a) No Default (ND) (b) Custom

Figure 1: Sample Interface

subjects on a screen (as in Table 1); it was also described on each waiting screen between successive choices and on the top of each decision screen (see Figure 1b). In each of these rounds, five lotteries are presented on the decision screen, and one available lottery is selected by the default-setting rule to be the default lottery. This lottery is prominently displayed at the top of the screen and appears pre-selected. Figure 1 includes sample choice screens for the No-Default and Custom treatments.

Choice sets

We constructed 24 choice sets of five lotteries each (Appendix Section 5.3). Choice sets qualitatively varied. Six choice sets consisted of five two-outcome lotteries with the same probabilities of the higher and lower outcome for all lotteries, or four such lotteries and a sure payment option (as in Eckel and Grossman 2002). Twelve choice sets consisted of five one-to-three outcome lotteries with common support (as in Hey and Orme 1994). Finally, six choice sets consisted of five lotteries where all but at most one had support on three or four outcomes. This mix of qualitatively different choice sets that varied in choice complexity precluded construction of simple common heuristics. We label these qualitatively different choice sets as Simple, Intermediate, and Complex respectively. Furthermore, choice sets were grouped into six blocks of four, where each block included one simple, two intermediate, and one complex choice set.

Each subject first faced all 24 choice sets in the No Default treatment. Then they faced each choice set again under two of the four default-setting rules, facing 12 choice sets per rule. Choice sets were arranged so that there were exactly four choice sets common to any two default-setting rules. We varied the order of choice sets across subjects to control for possible order effects and interactions. All possible orders are listed in Appendix A.

Procedures

In November 2019 and February-March 2020, we recruited 113 subjects in 22 sessions from the SFU Experimental Economics Lab participation pool. Each session took place in the Lab and lasted approximately 65 minutes. The experiment was conducted using an oTree [Chen et al., 2016] designed computerized interface. To determine payment, one round was randomly selected, and the subject's chosen lottery in that round was played out. The average payment was a $27.40 CAD, including a $7 participation payment. Full details of the experimental procedure including instructions and screenshots are provided in the Appendix.

4 Results

Result 1: Intentionally-set defaults are better on-average than randomly-selected defaults

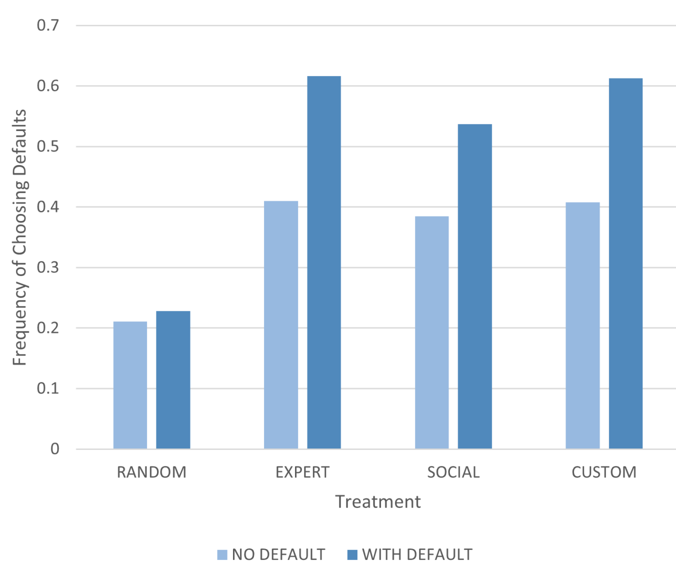


Figure 2: Frequency of choosing the default lottery in each default-setting rule versus in No Default

The frequency with which the defaults assigned by a rule are chosen in the No Default treatment provides us with an aggregate measure of subjective default quality. The default options assigned by the Random, Social, Expert and Custom rules are respectively chosen 21%, 39%, 40%, and 41% of the time in No Default (Figure 2, light bars). That is, for the intentionally-set defaults, subjects tended to choose the default option more frequently than by chance alone when the same choice was faced in No Default. This indicates that the quality of default options tends to be better than that of randomly-selected options. Thus, if people tend to choose options that they like, we cannot use the frequency of choosing the default in a given default-setting rule as a measure of default bias unless we control for subjective default quality. Our first result shows this concern is empirically relevant in our setting.

Result 2: Default bias is significant in the Social, Expert, and Custom default-setting rules, but not with Random defaults.

Figure 2 compares the frequency at which each default-setting rule's default lottery was chosen in that treatment to the case when the same choice set was faced with in No Default. If there were no default bias under a given default-setting rule, its dark-blue bar would be the same height as its light-blue bar. We only observe this equality for Random. In each of Social, Expert, and Custom, the rule's assigned default lottery was chosen more frequently when it was the default than in No Default -- indicating default-bias in each of these default-setting rules.

Table 2: Obuchowski tests for default bias

	Random	Social	Expert	Custom
p-value	0.178	< 0.001	< 0.001	< 0.001

p-values for an Obuchowski test for default bias
Each test uses 1356 choices from 113 subjects.

We apply an Obuchowski test of absolute default bias to assess statistical significance while controlling for subjective default quality (Table 2). Default bias is insignificant with Random defaults ($p = 0.178$), but significant under the Social, Expert, and Custom default-setting rules ($p < 0.001$ for each of the three tests).

Result 3: Default bias is stronger with intentionally-set defaults than with randomly-set defaults.

While we find evidence of default bias under the Social, Expert, and Custom rules, but not under the Random rule, this does not tell us how the strength of default bias differs across these rules. Even for the comparison between the Random rule and the remaining rules, Result 1 notes significant differences in the quality of defaults between the Random rule and the rules with intentionally-set defaults – Social, Expert, and Custom – thus necessitating the use of our comparative Obuchowski test to compare the strength of default bias while controlling for differences in default quality.

We find that default bias is significantly stronger under each of the Social, Expert, and Custom rules than under Random defaults ($p = 0.003$, 0.03, 0.04, respectively) when applying our test of comparative default bias in each case. When comparing default bias under the Social, Expert, and Custom rules, we find no detectable differences in the strength of default bias between any pair of these rules ($p = 0.07$ for Social vs. Expert, $p = 0.51$ for Social vs. Custom, and $p = 0.83$ for Expert vs. Custom). These tests formally confirm that default bias is stronger when defaults are intentionally set, as in the Social, Expert, and Custom rules, than when defaults are randomly selected.

Result 4: Subjects tended to rank Expert $\succ$ Custom $\succ$ Social $\succ$ No Default $\succ$ Random. A notable minority ranked the No Default rule as most-preferred. Subjects' rankings reflect a tendency to prefer rules that set defaults with higher subjective quality.

Table 3 counts the fraction of subjects who ranked each rule in each position. From this, we construct a Borda count for each default-setting rule and obtain the aggregate ranking Expert $\succ$ Custom $\succ$ Social $\succ$ No Default $\succ$ Random. Additionally, we find that 45% of subjects divided their top three ranks among the three informative default-setting rules (Social, Expert, and Custom). Expert was the most preferred default-setting rule for 43% of subjects and was the most commonly first-ranked rule; 23% of subjects ranked the Custom rule first, while the Social default-setting rule was most preferred by only 9% of subjects. Random defaults and No Default were the two least favored rules and were ranked last by 40% and 34% of subjects, respectively. However, a noticeable proportion of our subjects exhibit some preference for choosing without defaults – 21% rank No Default as their first choice, and 44% of subjects put No Default in their top three rules.

Table 3: Ranking of default-setting rules

Default type	#1	#2	#3	#4	#5
No Default	0.212	0.115	0.115	0.221	0.336
Random	0.044	0.080	0.133	0.345	0.398
Expert	0.425	0.301	0.168	0.044	0.062
Social	0.088	0.230	0.327	0.248	0.106
Custom	0.230	0.274	0.257	0.142	0.097

$n = 113$ subjects

To measure how preferences over default-setting rules relate to measures of default quality, we run a rank-ordered logit model with a subject's rank of a default-setting rule on the left hand side and a default quality measure on the right hand side:

$$\text{rank}_{iT} \quad = \quad \beta \text{defaultquality}_{iT} + \epsilon_{iT}$$

where i denotes subject i, $T \in \{\text{Random}, \text{Social}, \text{Expert}, \text{Custom}\}$[6] denotes a default-setting rule, and $\text{defaultquality}_{iT}$ is the number of times (out of 12) the T default option was chosen when the same option was faced with in No Default. We estimate $\beta = 0.208$ (s.e. = 0.038, $p < 0.001$). This indicates a significant association between a subject's ranking of a rule and the quality of that rule's defaults for that subject.Result 5: The strength of subjects' default bias is unaffected by their act of ranking default-setting rules.

It might be the case that the mere act of choosing a default-setting rule affects a person's degree of default bias – a possibility we now test. After subjects had ranked the five default-setting rules, one was implemented – and their first-choice rule was implemented with a 90% chance. Among subjects whose first choice was implemented (excluding those whose first choice was No Default), we find no significant difference when we compare their default bias in their top-ranked rule before as compared to after they ranked it, using an Obuchowski test of comparative default bias ($p = 0.48$). Thus we conclude that a subject's mere act of choosing to rank a default-setting rule highly does not lead to any detectable strengthening of their default bias.

Result 6: Neither complexity nor rational inattention help explain our results.

Since choice sets varied in complexity (by design), we can separately apply the Obuchowski test for absolute default bias separately for low, intermediate, and high complexity choice sets. In each case, we obtain the same direction of effects, and (with one exception) pattern of statistical significance. While complexity may have a role in default bias, this

[6]We drop choices under the No Default rule in this specification.

indicates that it was, at least, not the driving force behind our results.

Another possible explanation for default bias is rational inattention. This approach posits that a person must exert costly attention to evaluate each option. If they believe that the default option (under a given rule) tends to be better than a randomly-selected option then then the default provides costless information to the decision-maker. In Matějka and McKay's [2015] model of rationally inattentive choice, the decision-maker's use of this information leads them to have a higher probability of selecting an option when it is specified as the default compared to when the same choice set is faced without a default option. This approach correctly predicts that there is no default bias in Random.

This approach also predicts that subjects with higher attention costs will, ceteris paribus, exhibit more stochasticity in their choices without a default – which can be measured from our experiment by the number of times they make the same choice in Random as they did in No Default. Under our assumptions, such higher attention cost subjects will also, ceteris paribus, exhibit more default bias, since a tendency to choose default options that tend to be good can lead to good choices without requiring costly attention – a strategy that is less advantageous for people for whom attention is less costly. Yet we see essentially no correlation between an individual's number of common choices between No Default and Random on one hand and the number of additional times they chose the default option but did not do so in No Default on the other hand. This is confirmed by a point biserial correlation test (corr.=-0.07 and p=0.768). While complexity and rational inattention may be partly responsible for default bias in our experiment and/or other settings, we do not find these approaches particularly useful for explaining the behavior we observe.

5 Conclusion

We study the effectiveness of different default-setting rules in a setting where preferences are heterogeneous, and there are no objective ranking of options. Our experimental design and statistical approach allow us to disentangle the subjective quality of defaults that a rule assigns from the amount of default bias that that same rule induces. We find a significant increase in the probability of choosing default options for intentionally-set defaults, but not for randomly-set defaults. Our experimental results indicate that the mere presence of a default option is not enough to affectchoice, but intentionally-set defaults can induce a significant amount of default bias. Our results thus suggest that policymakers need to set defaults that are, on average, good for most people and to communicate this if they wish to harness default bias to nudge decisions in a particular direction.

We find that most participants prefer such intentional default-setting rules (Expert, Custom, and Social) to choosing without a default or to randomly-set defaults – providing evidence for a preference to be nudged. Our analysis shows that subjects tended to assign rankings of default-setting rules consistent with both the subjective default quality and their strength of default bias.

In our experimental design, the three rules with intentionally-set defaults tended to select options with similar quality, measured by the concordance of defaults with choices in the No Default treatment. The similar strength of default bias across the three rules reflects a similar willingness to follow defaults according to the rules as described – without any evidenced aversion to any of them. However, we suspect that in other settings where decision-makers make a large number of repeated decisions such as in de Haan and de Linde [2018], the actual quality of the defaults prescribed by a rule will eventually matter more than the initial framing of the default.

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Appendix: Complete Experimental Procedure

5.1 Instructions

At the beginning of each session, paper instructions (Figure 3) were distributed and read aloud; subjects followed along and could use a pen or pencil to write notes on the instructions, however, they were not allowed to use a pen or pencil once the experiment started. After the instructions, subjects completed a comprehension quiz (Figure 4). The experimenter checked the answers privately, and when they encountered incorrect answers, the experimenter pointed the subject to the relevant part of the instructions and gave the subject the opportunity to revise their answers. After all subjects had answered all questions correctly, the experiment commenced. The experiment was programmed using the oTree web-based platform [Chen et al., 2016] and completed by subjects in their web browser.

Figure 3: Instructions

Instructions

Overview

- You are going to take part in an experimental study of decision making.
- During the experiment, you are not allowed to talk or communicate with other participants.
- Also, please turn off your smart phones and put them away for the duration of the experiment.
- If at any time you have any questions, please raise your hand and the experimenter will come to your desk to answer it.
- Your earnings in the experiment will depend on your choices and an element of chance. By following the instructions and making decisions carefully, you may earn a considerable amount of money.
- The lottery you chose in one round of the experiment will be played out for real to determine your earnings from the experiment: thus you should make each choice as though it will be played out "for real" to determine your payment.
- Your earnings and a $7 participation payment will be paid to you in cash at the end of this experiment.

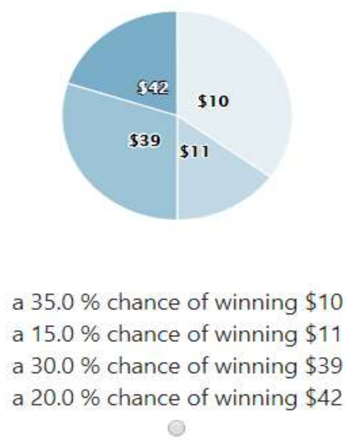
Choice task

- In each round you will choose one from among five lotteries.
- The decision screen (see the following page) will list the five lotteries, where each lottery consists of a set of possible payoffs and a probability of attaining each payoff.
- You should choose the lottery that you most prefer.
- There are no right or wrong answers and your responses may differ from other participants.
- You will have 60 seconds to make each decision.
- You finalize your choice by clicking the “Next” button or by allowing the time to run out.

Interface

The following screenshot shows how each choice task will be displayed on each decision screen.

Below is an example of how the information about the set of possible payoffs and the corresponding probabilities are presented to you.

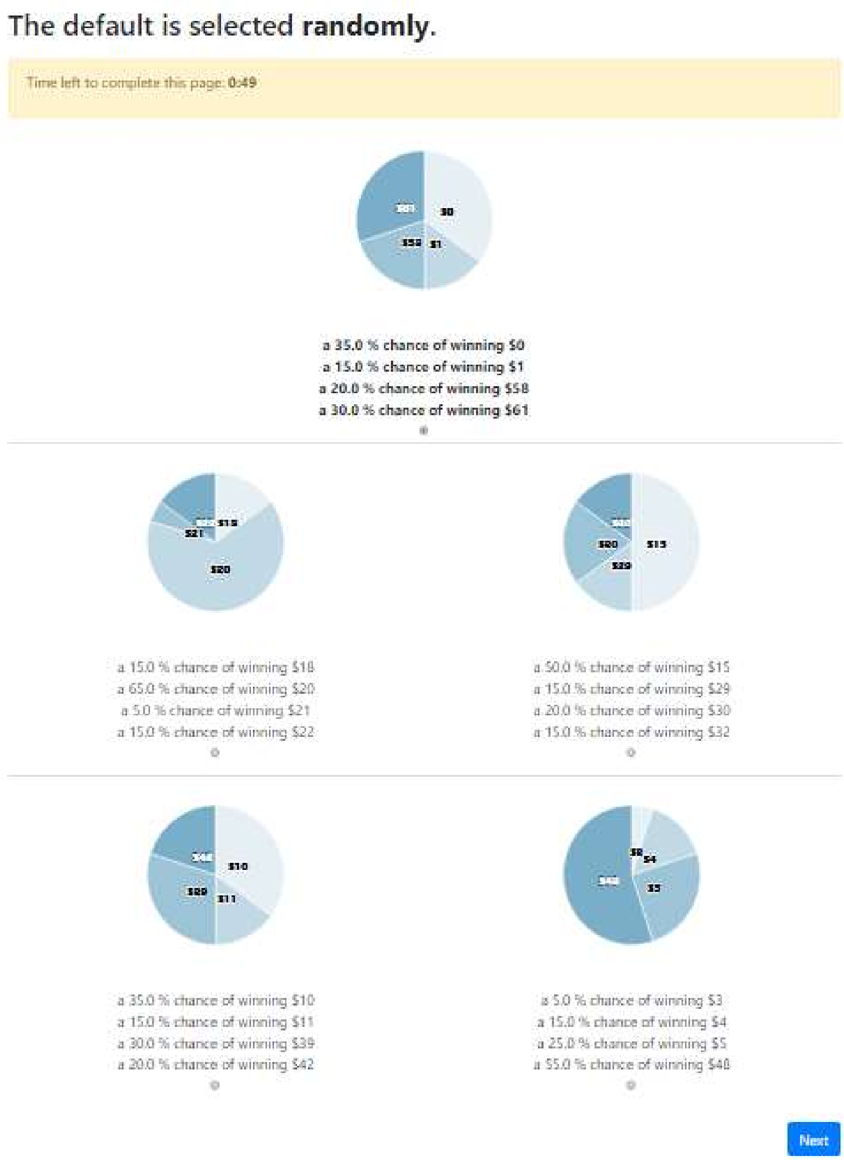


- The payoffs are ordered in increasing order.
- The size of a portion of the pie represents the probability of attaining the corresponding payoff in that portion.

Default options

- In some rounds, one lottery is selected as a “default” option.
- In such a round, the procedure used to select the default will be described at the top of the decision screen.
- When present, a default option is initially selected for you and will be presented at the top of the screen and in bold.
- For each task, you will have 60 seconds to make a decision.
- If you do not select another option before the time runs out, then the default option will automatically become your choice.
- You are always free to select the default or to choose another option: it’s your decision.

The following screenshot is an example of a round with default option.



Payment

- You will complete a total of 84 rounds in the experiment.
- One (and only one) round will be randomly selected to be the round that counts to determine your payment.
- Since any round could be the round that counts, you should treat your choice in each round of the experiment as though it will determine your earnings for the experiment.

Figure 4: Quiz

Quiz

In each round, I can choose any lottery I wish.

True / False

In rounds with “default” option, if I don’t choose another option before the time runs out, the default option will be chosen for me.

True / False

5.2 Experimental flow

Once the experiment started, each subject went through the experiment in the following order.

1. No Default treatment (24 rounds)

    (a) Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 5

    (b) Reminder/ waiting page between choices: Empty for 2 seconds for No Default treatment

    (c) Choice page as in Figure 6

2. Random treatment (12 rounds)

    (a) Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 7

    (b) Reminder/ waiting page between choices for 2 seconds as in Figure 8

    (c) Choice page as in Figure 9

3. Depending on the session, subjects went through one of the six possible orders for Expert, Social, and Custom treatments – this was varied across subjects. Below is an example where the order is Social, Expert, and Custom.

(a) Social treatment (12 rounds)

   i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 10
   ii. Reminder/ waiting page between choices for 2 seconds as in Figure 11
   iii. Choice page as in Figure 12

(b) Expert treatment (12 rounds)

   i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 13
   ii. Reminder/ waiting page between choices for 2 seconds as in Figure 14
   iii. Choice page as in Figure 15

(c) Custom treatment (12 rounds)

   i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 16
   ii. Reminder/ waiting page between choices for 2 seconds as in Figure 17
   iii. Choice page as in Figure 18

4. Ranking of default rules at the beginning of round 73 as in Figure 19

5. One treatment is implemented for the last 12 rounds.

Figure 5: Instruction for No Default treatment

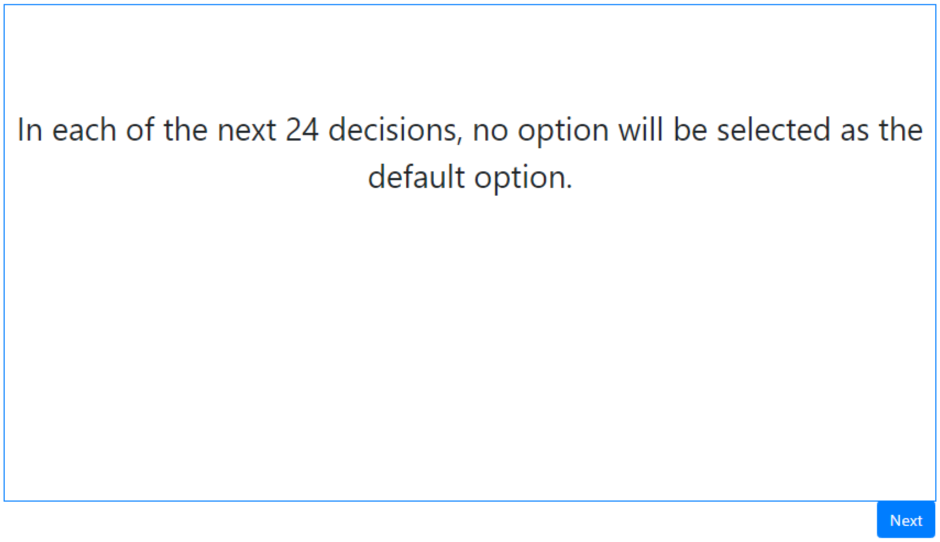


Figure 6: Choice screen for No Default treatment

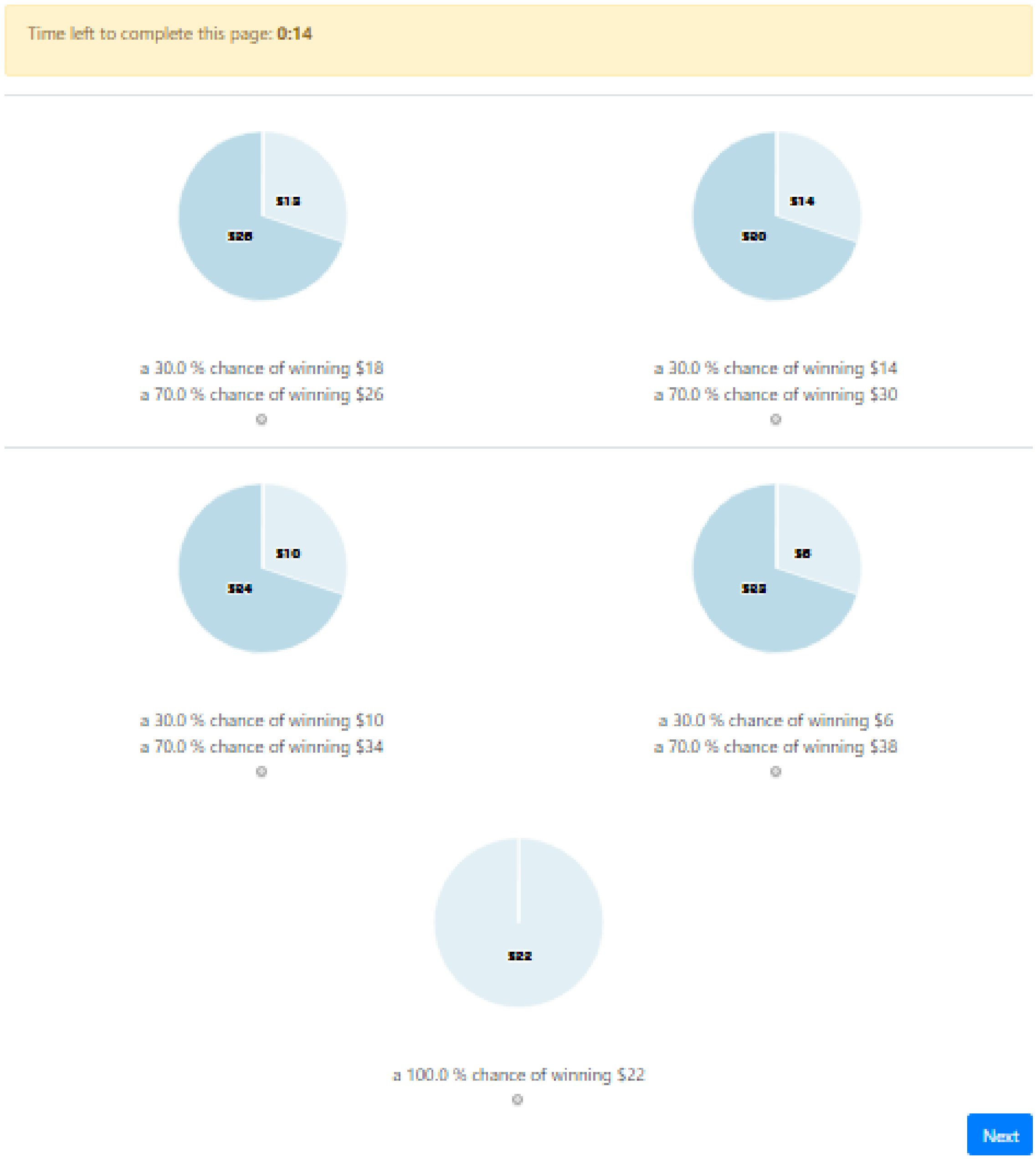


Figure 7: Instruction for Random treatment

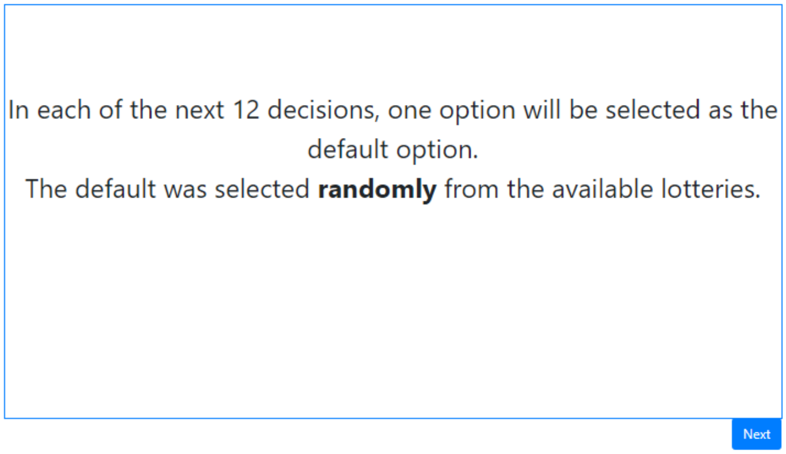


Figure 8: Waiting page for Random treatment

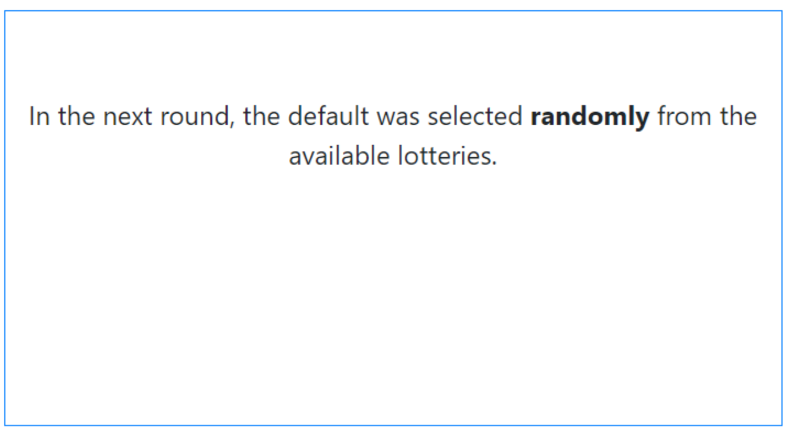


Figure 9: Choice screen for Random treatment

Figure 10: Instruction for Social treatment

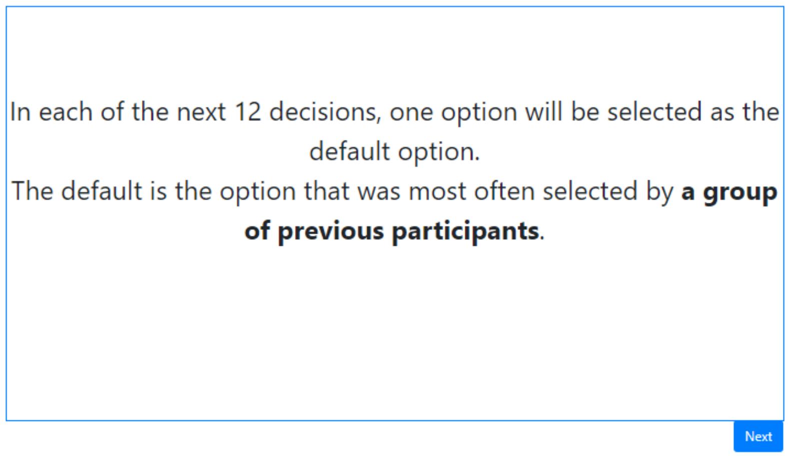


Figure 11: Waiting page for Social treatment

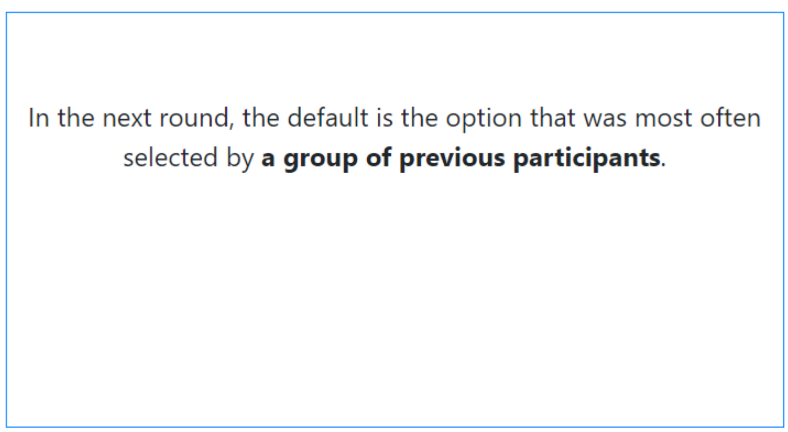


Figure 12: Choice screen for Social treatment

Figure 13: Instruction for Expert treatment

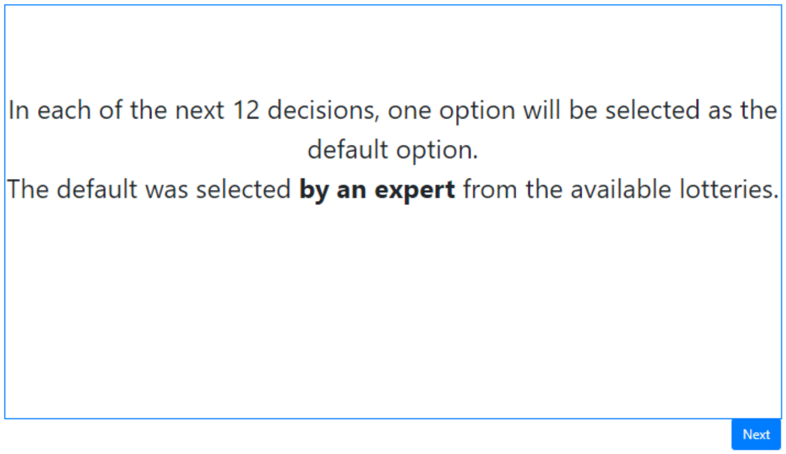


Figure 14: Waiting page for Expert treatment

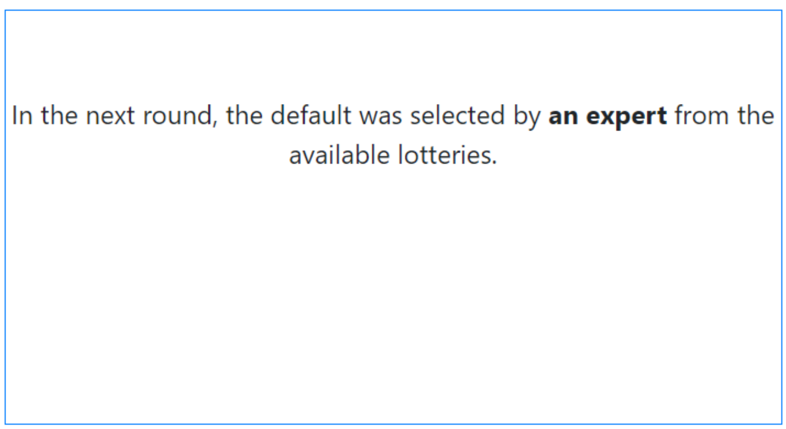


Figure 15: Choice screen for Expert treatment

Figure 16: Instruction for Custom treatment

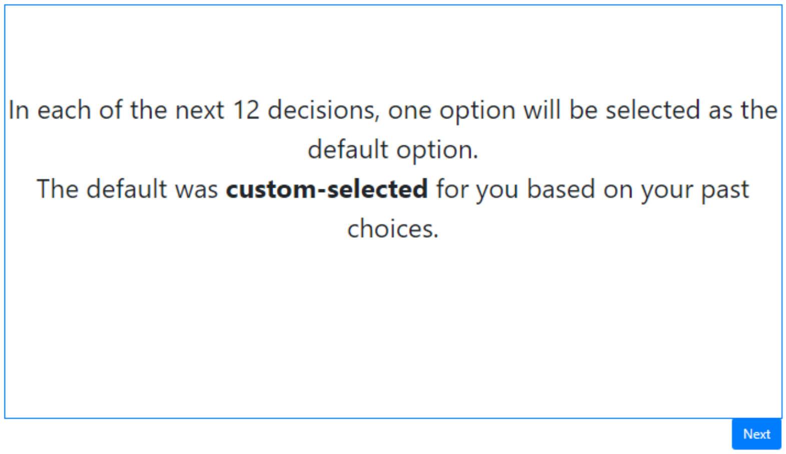


Figure 17: Waiting page for Custom treatment

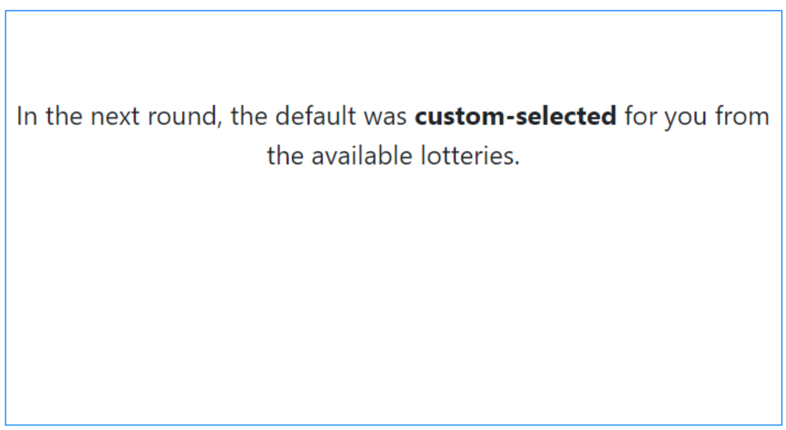


Figure 18: Choice screen for Custom treatment

Figure 19: Ranking of default rules

Ranking defaults

Please rank the default-setting rules used thus far from #1 (most preferred) to #5 (least preferred).
The default-setting rule used in the next 12 rounds of the experiment will be determined from your ranking.
The default-setting rules you indicate that you prefer more are more likely to be implemented, as follows:

Rank		Implemented with probability
Most preferred	#1	90%
	#2	7%
	#3	2%
	#4	1%
Least Preferred	#5	0%

Number 1 (most preferred):

Number 2:

Number 3:

Number 4:

Number 5 (least preferred):

Next

5.3 Choice sets

The Table lists 24 unique choice sets[7] which we organized into six blocks of four lotteries (A to F). Each choice set comprises of five lotteries (1 to 5). Each lottery has at most four outcomes. We use pa, pb, pc, pd and xa, xb, xc, xd to denote probabilities and payoffs of outcomes a, b, c, and d, respectively.

We use * to mark the default option for a choice set, under a default-setting rule. Default-setting rules are explained in the Experimental Design section. For the Random treatment, a lottery was randomly selected from each choice set, for each subject, so we drop a column for this treatment. For Expert and Custom treatment, we used an expected utility model with constant relative risk averse utility-for-income function $u(x) = \frac{x^{1-\gamma}}{1-\gamma}$. For the Expert treatment, we used $\gamma = \frac{3}{4}$ to select a default lottery from each choice set. For the Custom treatment, we assigned each participant into one of three groups based on their choices in three Eckel-Grossman style choice sets in No Default treatment. We then used the parameters $\gamma = 2$,1.25, 0.5 to select a default lottery for groups C_H, C_M, and C_L, respectively.

[7] We constructed 84 rounds from 24 choice sets using an overlapping structure outlined in the next section.

Set	Name	Lottery	pa	pb	pc	pd	xa	xb	xc	xd	Social	Expert	C_H	C_M	C_L
1	A1	1	1	0	0	0	18	0	0	0	*				
1	A1	2	0.5	0.5	0	0	16	24	0	0			*	*	
1	A1	3	0.5	0.5	0	0	13	30	0	0		*			*
1	A1	4	0.5	0.5	0	0	8	36	0	0					
1	A1	5	0.5	0.5	0	0	0	42	0	0					
2	A2	1	1	0	0	0	12	0	0	0	*	*	*	*	*
2	A2	2	0.25	0.375	0.375	0	0	12	24	0					
2	A2	3	0.375	0.625	0	0	0	24	0	0					
2	A2	4	0.375	0.25	0.375	0	0	12	36	0					
2	A2	5	0.5	0.5	0	0	0	36	0	0					
3	A3	1	0.5	0.5	0	0	0	60	0	0					
3	A3	2	0.2	0.8	0	0	0	40	0	0					
3	A3	3	0.3	0.6	0.1	0	0	40	60	0					
3	A3	4	0.2	0.2	0.6	0	0	20	40	0					
3	A3	5	0.6	0.4	0	0	20	40	0	0	*	*	*	*	*
4	A4	1	1	0	0	0	20	0	0	0					
4	A4	2	0.25	0.25	0.25	0.25	11	13	31	33					
4	A4	3	0.5	0.25	0.25	0	12	24	42	0					
4	A4	4	0.25	0.25	0.5	0	4	22	32	0					
4	A4	5	0.25	0.5	0.25	0	12	23	42	0	*	*	*	*	*
5	B1	1	1	0	0	0	18	0	0	0	*				
5	B1	2	0.1	0.9	0	0	15	19	0	0					
5	B1	3	0.1	0.9	0	0	12	20	0	0			*		
5	B1	4	0.1	0.9	0	0	9	21	0	0				*	
5	B1	5	0.1	0.9	0	0	6	22	0	0		*			*
6	B2	1	1	0	0	0	20	0	0	0					
6	B2	2	0.25	0.5	0.25	0	10	20	30	0					
6	B2	3	0.5	0.5	0	0	10	30	0	0					
6	B2	4	0.25	0.375	0.375	0	0	20	30	0					
6	B2	5	0.875	0.125	0	0	20	30	0	0	*	*	*	*	*

Set	Name	Lottery	pa	pb	pc	pd	xa	xb	xc	xd	Social	Expert	C_H	C_M	C_L
7	B3	1	0.6	0.4	0	0	0	60	0	0					
7	B3	2	0.5	0.5	0	0	0	40	0	0					
7	B3	3	0.2	0.6	0.2	0	0	20	40	0	*				
7	B3	4	0.3	0.5	0.2	0	0	20	60	0					
7	B3	5	0.1	0.9	0	0	0	20	0	0		*	*	*	*
8	B4	1	1	0	0	0	18	0	0	0			*		
8	B4	2	0.2	0.3	0.05	0.45	2	20	34	36					
8	B4	3	0.25	0.35	0.15	0.25	2	21	35	37					
8	B4	4	0.05	0.45	0.2	0.3	9	11	25	45		*		*	*
8	B4	5	0.35	0.15	0.3	0.2	9	12	25	46	*				
9	C1	1	1	0	0	0	24	0	0	0					
9	C1	2	0.5	0.5	0	0	23	30	0	0					
9	C1	3	0.5	0.5	0	0	21	35	0	0	*	*	*	*	*
9	C1	4	0.5	0.5	0	0	18	39	0	0					
9	C1	5	0.5	0.5	0	0	14	43	0	0					
10	C2	1	0.5	0.5	0	0	0	60	0	0					
10	C2	2	0.3	0.7	0	0	0	40	0	0					
10	C2	3	0.1	0.4	0.5	0	0	20	40	0	*				*
10	C2	4	0.2	0.6	0.2	0	0	20	60	0					
10	C2	5	1	0	0	0	20	0	0	0		*	*	*	
11	C3	1	0.5	0.125	0.375	0	0	20	30	0					
11	C3	2	0.125	0.5	0.375	0	0	10	30	0					*
11	C3	3	0.375	0.125	0.5	0	0	10	20	0					
11	C3	4	0.125	0.75	0.125	0	0	10	20	0					
11	C3	5	0.75	0.125	0.125	0	10	20	30	0	*	*	*	*	
12	C4	1	0.15	0.45	0.15	0.25	22	24	25	26			*	*	
12	C4	2	0.05	0.45	0.4	0.1	17	18	33	34		*			*
12	C4	3	0.15	0.35	0.05	0.45	10	13	41	42	*				
12	C4	4	0.5	0.05	0.1	0.35	6	50	51	52					
12	C4	5	0.45	0.05	0.1	0.4	0	1	51	62					

Set	Name	Lottery	pa	pb	pc	pd	xa	xb	xc	xd	Social	Expert	C_H	C_M	C_L
13	D1	1	1	0	0	0	20	0	0	0	*				
13	D1	2	0.2	0.8	0	0	16	22	0	0			*		
13	D1	3	0.2	0.8	0	0	12	24	0	0		*		*	
13	D1	4	0.2	0.8	0	0	8	26	0	0					*
13	D1	5	0.2	0.8	0	0	4	28	0	0					
14	D2	1	0.5	0.5	0	0	0	60	0	0					
14	D2	2	0.3	0.7	0	0	0	40	0	0					
14	D2	3	0.1	0.4	0.5	0	0	20	40	0					*
14	D2	4	0.2	0.6	0.2	0	0	20	60	0					
14	D2	5	1	0	0	0	20	0	0	0	*	*	*	*	
15	D3	1	0.25	0.375	0.375	0	0	10	30	0					
15	D3	2	0.125	0.625	0.25	0	0	10	30	0					
15	D3	3	0.125	0.25	0.625	0	0	10	20	0					
15	D3	4	0.5	0.5	0	0	10	20	0	0	*	*	*	*	*
15	D3	5	0.375	0.5	0.125	0	0	20	30	0					
16	D4	1	0.35	0.25	0.15	0.25	17	18	19	20	*				
16	D4	2	0.3	0.2	0.35	0.15	15	17	25	26		*	*	*	*
16	D4	3	0.4	0.2	0.15	0.25	13	14	29	31					
16	D4	4	0.5	0.15	0.25	0.1	8	35	36	37					
16	D4	5	0.35	0.05	0.1	0.5	0	1	2	42					
17	E1	1	1	0	0	0	15	0	0	0					
17	E1	2	0.5	0.5	0	0	12	21	0	0	*	*	*	*	
17	E1	3	0.5	0.5	0	0	9	26	0	0					*
17	E1	4	0.5	0.5	0	0	6	30	0	0					
17	E1	5	0.5	0.5	0	0	3	33	0	0					
18	E2	1	1	0	0	0	20	0	0	0	*		*	*	
18	E2	2	0.125	0.5	0.375	0	0	20	30	0					
18	E2	3	0.375	0.625	0	0	10	30	0	0		*			*
18	E2	4	0.25	0.375	0.375	0	10	20	30	0					
18	E2	5	0.25	0.75	0	0	0	30	0	0					

Set	Name	Lottery	pa	pb	pc	pd	xa	xb	xc	xd	Social	Expert	C_H	C_M	C_L
19	E3	1	0.5	0.2	0.3	0	0	20	40	0	*				*
19	E3	2	0.6	0.1	0.3	0	0	20	60	0					
19	E3	3	0.6	0.4	0	0	0	40	0	0					
19	E3	4	0.7	0.1	0.2	0	0	40	60	0					
19	E3	5	0.4	0.6	0	0	0	20	0	0		*	*	*	
20	E4	1	0.15	0.45	0.25	0.15	15	16	17	18					
20	E4	2	0.5	0.15	0.1	0.25	12	20	21	24					
20	E4	3	0.2	0.25	0.1	0.45	12	13	15	25	*	*	*	*	*
20	E4	4	0.35	0.1	0.25	0.3	2	10	33	35					
20	E4	5	0.45	0.1	0.2	0.25	2	32	34	35					
21	F1	1	1	0	0	0	22	0	0	0	*				
21	F1	2	0.3	0.7	0	0	18	26	0	0			*		
21	F1	3	0.3	0.7	0	0	14	30	0	0				*	
21	F1	4	0.3	0.7	0	0	10	34	0	0		*			
21	F1	5	0.3	0.7	0	0	6	38	0	0					*
22	F2	1	0.375	0.375	0.25	0	0	10	20	0					
22	F2	2	0.5	0.125	0.375	0	0	10	20	0					
22	F2	3	0.5	0.375	0.125	0	0	10	30	0					
22	F2	4	0.75	0.25	0	0	0	30	0	0					
22	F2	5	0.25	0.75	0	0	0	10	0	0	*	*	*	*	*
23	F3	1	0.2	0.8	0	0	0	40	0	0					
23	F3	2	0.3	0.6	0.1	0	0	40	60	0					
23	F3	3	0.2	0.2	0.6	0	0	20	40	0					
23	F3	4	0.6	0.4	0	0	20	40	0	0	*	*	*	*	*
23	F3	5	1	0	0	0	20	0	0	0					
24	F4	1	0.35	0.45	0.15	0.05	22	24	25	27					
24	F4	2	0.25	0.2	0.35	0.2	22	23	28	31					
24	F4	3	0.5	0.15	0.05	0.3	21	34	35	36		*	*	*	*
24	F4	4	0.3	0.2	0.15	0.35	17	19	38	40	*				
24	F4	5	0.1	0.2	0.25	0.45	14	15	16	46					

Overlapping structure and Order of choice tasks

In our experiment, each subject first faced all 24 unique choice sets in the No Default treatment, and then faced each choice set twice more under the four default-setting rules, three blocks for each rule. These were arranged so that there was exactly one block of overlap between any two default-setting rules. After default-setting preference elicitation, three blocks were repeated again in the last 12 rounds where the default-setting rule was selected based on the subject's ranking. In total, we have 84 rounds.

We have six treatment orders based on six possible orders of Expert, Social, and Custom treatments. In addition, we mixed starting blocks and interweaved blocks so that subjects would rarely see two choice sets in the same order, to avoid subjects recognizing the block order. Treatment variations were assigned at the session level. Table 4 lists the choice sequence that we used in our experiment.

Table 4: Choice sequence

Session	No. of subjects	Treatment order	Choice set order
S1	9	No Default	A1, B2, C3, A4, B1, C2, A3, B4, C1, A2, B3, C4, D1, E2, F3, D4, E1, F2, D3, E4, F1, D2, E3, F4
		Random	A1, A2, B4, A3, B2, A4, C1, B3, C2, B1, C3, C4
		Expert	D1, D2, E4, D3, E2, D4, A1, E3, A2, E1, A3, A4
		Social	F1, F2, B4, F3, B2, F4, D1, B3, D2, B1, D3, D4
		Custom	E1, E2, C4, E3, C2, E4, F1, C3, F2, C1, F3, F4
		Choice of Default	B1, B2, D4, B3, D2, B4, E1, D3, E2, D1, E3, E4
S2	8	No Default	A4, A3, A2, A1, B4, B3, B2, B1, C4, C3, C2, C1, D4, D3, D2, D1, E4, E3, E2, E1, F4, F3, F2, F1
		Random	A4, A3, B1, A2, B3, A1, C4, B2, C3, B4, C2, C1
		Custom	D4, D3, E1, D2, E3, D1, A4, E2, A3, E4, A2, A1
		Expert	F4, F3, B1, F2, B3, F1, D4, B2, D3, B4, D2, D1
		Social	E4, E3, C1, E2, C3, E1, F4, C2, F3, C4, F2, F1
		Choice of Default	B4, B3, D1, B2, D3, B1, E4, D2, E3, D4, E2, E1
S5	12	No Default	B4, B3, B2, B1, C4, C3, C2, C1, D4, D3, D2, D1, E4, E3, E2, E1, F4, F3, F2, F1, A4, A3, A2, A1
		Random	B1, B2, C4, B3, C2, B4, D1, C3, D2, C1, D3, D4
		Social	E1, E2, F4, E3, F2, E4, B1, F3, B2, F1, B3, B4
		Expert	A1, A2, C4, A3, C2, A4, E1, C3, E2, C1, E3, E4
		Custom	F1, F2, D4, F3, D2, F4, A1, D3, A2, D1, A3, A4
		Choice of Default	C1, C2, E4, C3, E2, C4, F1, E3, F2, E1, F3, F4
S6	11	No Default	B1, C2, B3, C4, C1, B2, C3, B4, D1, E2, D3, E4, E1, D2, E3, D4, F1, A2, F3, A4, A1, F2, A3, F4
		Random	B4, B3, C1, B2, C3, B1, D4, C2, D3, C4, D2, D1
		Custom	E4, E3, F1, E2, F3, E1, B4, F2, B3, F4, B2, B1
		Social	A4, A3, C1, A2, C3, A1, E4, C2, E3, C4, E2, E1
		Expert	F4, F3, D1, F2, D3, F1, A4, D2, A3, D4, A2, A1
		Choice of Default	C4, C3, E1, C2, E3, C1, F4, E2, F3, E4, F2, F1
S8	8	No Default	C1, D2, E3, C4, D1, E2, C3, D4, E1, C2, D3, E4, F1, A2, B3, F4, A1, B2, F3, A4, B1, F2, A3, B4
		Random	C4, C2, D1, C3, D2, C1, E4, D3, E2, D4, E3, E1
		Custom	F4, F2, A1, F3, A2, F1, C4, A3, C2, A4, C3, C1
		Expert	B4, B2, D1, B3, D2, B1, F4, D3, F2, D4, F3, F1
		Social	A4, A2, E1, A3, E2, A1, B4, E3, B2, E4, B3, B1
		Choice of Default	D4, D2, F1, D3, F2, D1, A4, F3, A2, F4, A3, A1
S9	8	No Default	C1, D2, C3, D4, D1, C2, D3, C4, E1, F2, E3, F4, F1, E2, F3, E4, A1, B2, A3, B4, B1, A2, B3, A4
		Random	C1, C2, D4, C3, D2, C4, E1, D3, E2, D1, E3, E4
		Social	F1, F2, A4, F3, A2, F4, C1, A3, C2, A1, C3, C4
		Custom	B1, B2, D4, B3, D2, B4, F1, D3, F2, D1, F3, F4
		Expert	A1, A2, E4, A3, E2, A4, B1, E3, B2, E1, B3, B4
		Choice of Default	D1, D2, F4, D3, F2, D4, A1, F3, A2, F1, A3, A4

Session	No. of subjects	Treatment order	Choice set order
S10	11	No Default	D1, E2, D3, E4, E1, D2, E3, D4, F1, A2, F3, A4, A1, F2, A3, F4, B1, C2, B3, C4, C1, B2, C3, B4
		Random	D4, D3, E1, D2, E3, D1, F4, E2, F3, E4, F2, F1
		Expert	A4, A3, B1, A2, B3, A1, D4, B2, D3, B4, D2, D1
		Custom	B4, B3, F1, B2, F3, B1, C4, F2, C3, F4, C2, C1
		Social	C4, C3, E1, C2, E3, C1, A4, E2, A3, E4, A2, A1
		Choice of Default	E4, E3, A1, E2, A3, E1, B4, A2, B3, A4, B2, B1
S11	11	No Default	D1, E2, F3, D4, E1, F2, D3, E4, F1, D2, E3, F4, A1, B2, C3, A4, B1, C2, A3, B4, C1, A2, B3, C4
		Random	D1, D3, E4, D2, E3, D4, F1, E2, F3, E1, F2, F4
		Social	A1, A3, B4, A2, B3, A4, D1, B2, D3, B1, D2, D4
		Expert	C1, C3, E4, C2, E3, C4, A1, E2, A3, E1, A2, A4
		Custom	B1, B3, F4, B2, F3, B4, C1, F2, C3, F1, C2, C4
		Choice of Default	E1, E3, A4, E2, A3, E4, B1, A2, B3, A1, B2, B4
S13	6	No Default	E1, F2, E3, F4, F1, E2, F3, E4, A1, B2, A3, B4, B1, A2, B3, A4, C1, D2, C3, D4, D1, C2, D3, C4
		Random	E1, E2, F4, E3, F2, E4, A1, F3, A2, F1, A3, A4
		Expert	B1, B2, C4, B3, C2, B4, E1, C3, E2, C1, E3, E4
		Social	D1, D2, F4, D3, F2, D4, B1, F3, B2, F1, B3, B4
		Custom	C1, C2, A4, C3, A2, C4, D1, A3, D2, A1, D3, D4
		Choice of Default	F1, F2, B4, F3, B2, F4, C1, B3, C2, B1, C3, C4
S15	10	No Default	E4, E3, E2, E1, F4, F3, F2, F1, A4, A3, A2, A1, B4, B3, B2, B1, C4, C3, C2, C1, D4, D3, D2, D1
		Random	E1, E3, F4, E2, F3, E4, A1, F2, A3, F1, A2, A4
		Social	B1, B3, C4, B2, C3, B4, E1, C2, E3, C1, E2, E4
		Custom	D1, D3, F4, D2, F3, D4, B1, F2, B3, F1, B2, B4
		Expert	C1, C3, A4, C2, A3, C4, D1, A2, D3, A1, D2, D4
		Choice of Default	F1, F3, B4, F2, B3, F4, C1, B2, C3, B1, C2, C4
S16	11	No Default	F1, A2, F3, A4, A1, F2, A3, F4, B1, C2, B3, C4, C1, B2, C3, B4, D1, E2, D3, E4, E1, D2, E3, D4
		Random	F4, F2, A1, F3, A2, F1, B4, A3, B2, A4, B3, B1
		Expert	C4, C2, D1, C3, D2, C1, F4, D3, F2, D4, F3, F1
		Custom	E4, E2, A1, E3, A2, E1, C4, A3, C2, A4, C3, C1
		Social	D4, D2, B1, D3, B2, D1, E4, B3, E2, B4, E3, E1
		Choice of Default	A4, A2, C1, A3, C2, A1, D4, C3, D2, C4, D3, D1
S18	8	No Default	F4, F3, F2, F1, A4, A3, A2, A1, B4, B3, B2, B1, C4, C3, C2, C1, D4, D3, D2, D1, E4, E3, E2, E1
		Random	F4, F3, A1, F2, A3, F1, B4, A2, B3, A4, B2, B1
		Custom	C4, C3, D1, C2, D3, C1, F4, D2, F3, D4, F2, F1
		Social	E4, E3, A1, E2, A3, E1, C4, A2, C3, A4, C2, C1
		Expert	D4, D3, B1, D2, B3, D1, E4, B2, E3, B4, E2, E1
		Choice of Default	A4, A3, C1, A2, C3, A1, D4, C2, D3, C4, D2, D1

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