Nudge Me: Preferences over Default-Setting Rules*

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Abstract

We study how choices are influenced by the procedure used to select the default option. In a within-subject lottery choice experiment, we compare four different default-setting rules: Random defaults, Custom defaults based on an individual's own past choices, Social defaults based on others' choices, and Expert-set defaults. Using tests that control for default quality and heterogeneous preferences, we find that default bias is higher for non-random default-setting rules. We find a similar effect with a designated option that is not pre-selected, demonstrating that default bias is primarily driven by the message contained in default-setting rules. We then elicit subjects' preferences over default-setting rules and find that the majority (~80%) prefer to have non-random defaults. These results support the endorsement effect as a driver of default bias, and suggests that communicating the way the default option was selected can strengthen default bias.

Keywords: Default Bias, Choice Architecture, Endorsement Effect

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1 Introduction

In many decisions one option is pre-selected as the "default" and will be selected unless the decision-maker actively changes to another option. Examples range from selecting a health plan, to setting retirement contributions, to choosing a shipping method for online purchases. Behavioral economists have documented that people are more prone to select an option when it is the default option, exhibiting so-called "default bias" [Samuelson and Zeckhauser, 1988, Madrian and Shea, 2001, Johnson and Goldstein, 2003, Handel, 2013, Ericson, 2014, Choi et al., 2004, Beshears et al., 2009, Thaler and Benartzi, 2004]. 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]. The existence of default bias motivates firms and governments to intentionally set defaults, often with the aim of improving decisions. But there are many different ways to select a default option from a choice set and, when people have heterogeneous preferences, any selected default option will be undesirable for some people. The goal of this paper is to understand how decision makers behave when they are informed of how defaults are intentionally set. To do so, we experimentally study how people respond to, rank, and report feeling about different "default-setting rules" that select a default option from a given choice set.

Our experiment involves choice among lotteries under five default-setting rules used in practice and studied in the choice architecture literature [Thaler and Sunstein, 2003, Johnson et al., 2012, Madrian, 2014, Jachimowicz et al., 2019, Beshears and Kosowsky, 2020]: (i) defaults with no information about how they are set ("Uninformative"), (ii) defaults set randomly ("Random"), (iii) defaults based on the decisions of others ("Social"), (iv) defaults selected by an expert ("Expert") based on a normative criterion, and (v) defaults custom-selected for each person based on their past choices ("Custom"). In our within-subject design, each subject faces the same choice sets both without a default option ("No Default") and with a default option selected according to each of the five different default-setting rules, and is informed of which rule selected the default. This design allows us to measure default bias under each rule while using choices in No Default to control for differences in default quality across rules at the individual level. We then elicit each subject's incentivized ranking of the default-setting rules (ii)-(v) and No Default, and conduct a follow-up survey on unincentivized attitudes towards defaults. We thus provide a unified experimental framework to measure and compare both how people respond to defaults and how they want defaults to be set.

We chose default-setting rules that mimic real-world examples. 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]. Social defaults are another possibility suggested by Thaler and Sunstein [2003], are analogous to a seller that makes the most popular option the default. The discussion of choice architecture tends to view 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 was 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].

We find substantially more default bias under non-random default setting rules — Expert, Social, and Custom — than with Random defaults, although default bias is statistically significant in each case. This suggests that decision-makers respond to how the default is set, and not just the default itself. We find further support by comparison with subjects in a "Message" group, for whom the rule each round picked a "designated" option that was not pre-selected. For each rule, we find no significant difference in bias between the "Message"and "Default" groups. This suggests that default bias is primarily driven by the message and not by the fact that one option is pre-selected. In the Uninformative default-setting rule — where no information about how the default or designated option was provided — default-bias is closer to that in the non-random default-setting rules than in Random, after controlling for order effects. This suggests that in our Uninformative treatment, people respond to the default option as if it provides a recommendation akin to the explicit endorsement provided in Expert, Social, and Custom default-setting rules [Madrian and Shea, 2001].

In an incentivized ranking, we find that the vast majority of subjects prefer non-random default setting rules. In the Default treatment group, 77% of subjects rank of Expert, Social, or Custom number one and only 17% rank No Default number one. Expert is the most popular, ranked top two by 74% of all subjects. In our unincentivized survey, 62%

report "liking" having a default, though 17% remain unsure. This shows that most people want intentionally-set defaults. Moreover, default bias is higher for subjects who report in the survey that they like having defaults as well as for those who report being influenced by the default option. Curiously, we also find default bias for subjects who report not liking defaults and for those who report that they were not influenced by defaults. This suggests that people may be partially unaware when they are influenced by choice architecture.

We make several contributions. First, our study provides the first incentivized evidence on people's preferences over choice architecture, specifically default setting rules. Our revealed preference approach complements philosophical work (e.g. Bovens, 2009, Hausman and Welch, 2010) and surveys on attitudes towards choice architecture (e.g. Arad and Rubinstein, 2018). People's rankings clearly demonstrate that most people want intentionally-designed choice architecture. Second, our comparison of the level of default bias across default-setting rules complements work that demonstrates more default bias under better default-setting rules in settings with objectively ranked choice objects [de Haan and de Linde, 2018, Caplin and Martin, 2017, Altmann et al., 2022]. However, because we use lotteries as choice objects, the alignment between the default-setting rule and a person's preferences is subjective and heterogeneous, which qualitatively changes our interpretation and also requires us to introduce different approaches to control for differences in default quality across rules. Finally, our findings support the endorsement effect [Madrian and Shea, 2001, Bernheim et al., 2015, Madrian, 2014] as a driver of default bias, noting that our setting minimizes adjustment costs and suppresses procrastination as a possible mechanism [Carroll et al., 2009, Blumenstock et al., 2018]. This finding buttresses a main conclusion of Jachimowicz et al.'s [2019] meta-analysis that defaults tend to be more "effective when they operate through endorsement".

This paper is organized follows: In Section 2, we present our approach for measuring and comparing default bias across default-setting rules while controlling for default quality at the individual level. In Section 3, we describe our experimental design and procedure. Our results are presented in Section 4 and Section 5 concludes with a brief discussion of related topics.

2 Defining and Comparing Default Bias across Default-setting Rules

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 information about the rule used to select the default from the choice set.

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.[1] Let $T = \text{ND}$ 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, a decision-maker may exhibit default bias under some rules but not others.

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. This allows us to define an measure of default bias that can be estimated from choice frequencies.

Definition. The default bias score for p under rule T is given by the average of $p(x|A, x, T)-$

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

$p(x|A, \emptyset, ND)$ across (A, x) pairs such that observations $p(x|A, x, T)$ and $p(x|A, \emptyset, ND)$ can be estimated.

While in some models, the quantity $p(x|A, x, T) - p(x|A, \emptyset, ND)$ may be independent of which (A, x) pairs are observed for T, this will not generally be true. Thus any measure of the default bias score can depend on which choice sets are observed. When we calculate default bias scores for a rule T in our data, we sum across the fraction of times the default was chosen in treatment T and subtract the fraction of times the same person chose T-default option when they faced the same choice set in ND.

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.

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'.

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. Alternatively, we use a multinomial logit model with a default-bias coefficient for each default-setting rule and controls for option quality so as to control for default quality, and use clustered standard errors for inference.

3 Experimental Design

Our individual choice experiment consists of 72 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 for which experimental literature shows that individuals exhibit heterogeneous preferences (e.g. Hey and Orme 1994, Holt and Laury 2005, Bruhin et al. 2010).

The experiment includes choices from 18 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 18 rounds – the No Default treatment – subjects make choices from each of the 18 choice sets without any option being designated as the default option. In the next 9 rounds (rounds 19-27), subjects are presented with a default (preselected) option without any explanation regarding how this option was chosen. We label this treatment "Uninformative". In the next 36 rounds, subjects proceed through the four default-setting rules (Random, Social, Expert, and Custom), completing 9 rounds for each.

Treatment	Description	Rounds
No Default	"In each of the next 18 decisions, no option will be selected as the default option."	1 - 18
w/ Defaults	"In each of the next 9 decisions, one option will be selected as the default option."	
Uninformative	A default option is chosen but nothing is said regarding how it was selected	19 - 27
Random	"The default was selected **randomly** from the available lotteries."	28 - 36*
Expert	"The default was selected **by an expert** from the available lotteries."	37 - 45*
Social	"The default is the option that was most often selected by **a group of previous participants**."	46 - 54*
Custom	"The default was **custom-selected** for you based on your past choices."	55 - 63*
Choice	Default regime is determined by incentive compatible ranking by subject	64 - 72
* The order of the Random, Expert, Social, and Custom treatments was shuffled.		

Table 1: Summary of Treatments

After this is complete, we elicit each subject's ranking of the four default-setting rules and No Default (excluding Uninformative). 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 default options in Rounds 64 through 72. Table 1 summarizes the work-flow as well as the exact wording of how the default rules are communicated to the participants.[2]
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 student participants in October 2019. We used this group's modal choice in the No Default treatment as the default lottery for each choice set.

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.[3]

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

[2]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.

[3]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.

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

Figure 1: Sample Interface

(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 each subject 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 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. The default option is automatically implemented unless the subject clicks another option before the 60 second timer displayed on the top of a decision screen runs out. Figure 1 includes sample choice screens for the

No-Default and Custom treatments.

To disentangle the impact of the message associated with a default from the default itself, we assigned a group of participants a "Message" group in which no option is pre-selected in any round. Instead, this group receives only a message with analogous content to those in Table 1. In this case, "The default ..." is replaced by the "The designated option ..." for all rounds where such a message is present in the original. This variation permits a between-subjects test of the difference between the effect of default and messages together and the effect of messages only which allows us to potentially isolate the impact of each intervention separately.

Choice sets

We constructed 18 choice sets of five lotteries each (Appendix Section A.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). Six 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 three, where each block included one simple, one intermediate, and one complex choice set.

Each subject first faced all 18 choice sets in the No Default treatment. Then they faced each choice set again under two of the four default-setting rules, facing 9 choice sets per rule. Choice sets were arranged so that there were exactly three 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.

Procedures

In July 2022, we recruited 295 subjects from the United States using Prolific, an online marketing panel also used for economic experiments. The sample was representative of the US population according to sex, ethnicity, and age. Experiments took place online with an average time of completion (including instructions) of 35 minutes. The experiment was conducted using a designed computerized interface based on oTree [Chen et al., 2016]. 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 \$27.01 USD, including a \$7.50 USD participation payment. Full details of the experimental procedure including instructions and screenshots are provided in the Appendix.

Student sample

We conducted our original experiment in the SFU Experimental Economics Lab, using 113 student participants in 22 sessions between November 2019 and March 2020. The average session lasted approximately 65 minutes. The experimental design and interface were the same as the Default group in the Prolific experiment, with some minor differences. There was no Uninformative default-setting rule. Instead, subjects first completed 24 choices in ND, then 12 choices in Random, then 12 choices in each of Expert, Social, and Custom, with the order varied between the intentional default-setting rules. Participants then ranked default-setting rules and faced 12 rounds with one of the ranked rules, incentivized as with our default group. The average payment was a \$27.40 CAD, including a \$7 participation payment.

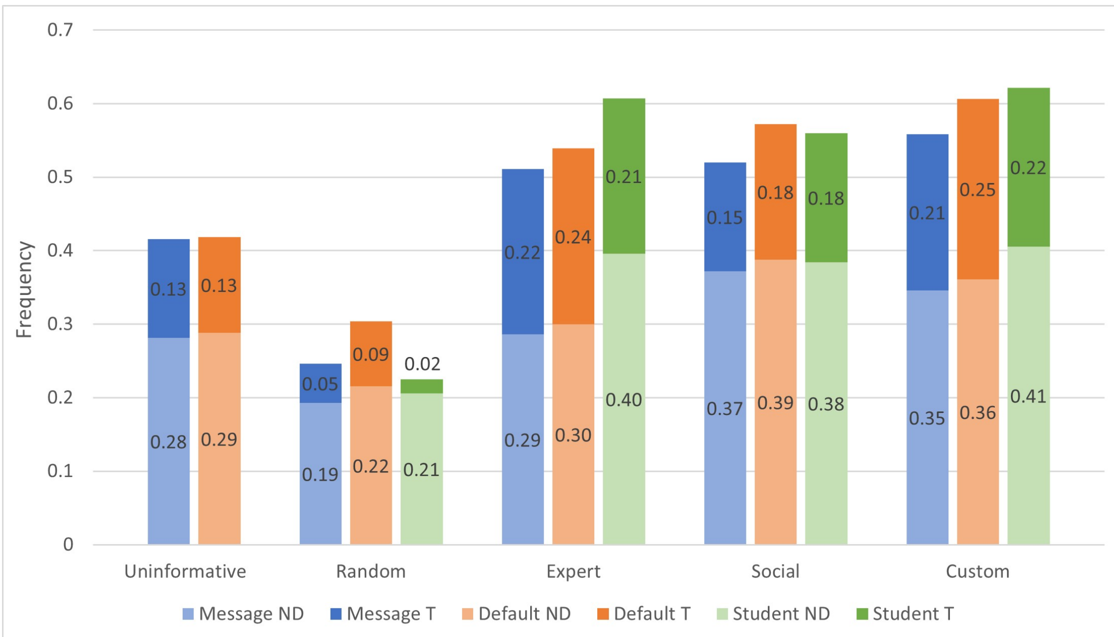
4 Results

Result 1: Default bias is significant for all default-setting rules except with Random defaults in the student sample.

Figure 2 compares the frequency at which each default-setting rule's default lottery was chosen to the frequency at which this option was chosen when the same choice set was faced with in No Default. We call this difference the "default bias score" and it is given by:

$$\text{Default Bias Score}_T = \sum_{i=1}^{n} \sum_{A \in T} c_i(x|A, x, T) - c_i(x|A, \emptyset, ND)$$

where $c_i(x|A, x, T) = 1$ if subject i chose the default option from choice set A in treatment T and zero otherwise, and n is the number of subjects. The differing heights of the lighter bars in Figure 2 indicate that the default-setting rules systematically differ in how frequently they select options that were commonly-chosen in No Default. A natural interpretation is that intentional default-setting rules selected higher-quality defaults that tended to better



* The lightly colored bars represent the frequency with which default option was chosen in the No Default rounds. The darkly colored bars represent the frequency with which the default is chosen in the Treatment rounds. The height of the lightly colored bars in embedded in the lightly colored bars and the difference in height between the drakly colored and lightly colored bars is embedded in the darkly coloured bars. We call the latter quantity quantity the default bias score. Frequencies are reported separately for the two groups, Default and Message, from the Prolific Sample as well as the Student Sample.

Figure 2: Default Bias by Rule and Group

align with subjects' preferences than did the Random rule. 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 (statistically) for Random in the student sample. 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 for each of these default-setting rules.

	Uninformative	Random	Expert	Social	Custom	# subjects
Prolific Message	<0.001	<0.001	<0.001	<0.001	<0.001	101
Prolific Default	<0.001	<0.001	<0.001	<0.001	<0.001	193
Student Default	N/A	0.178	< 0.001	< 0.001	< 0.001	113

p-values for an Obuchowski test for default bias

Each Student Default test uses 1356 choices from 113 subjects.

Each Prolific test uses 1542, 1733, 1516, 1673, 1681 choices respectively for 193 subjects in Default (1737)[4]

Each Prolific test uses 806, 909, 803, 871, 884 choices respectively for 101 subjects in Message (909)

Table 2: Obuchowski tests for default bias

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 in our student sample ($p = 0.178$), but significant in all other cases.

We also estimate multinomial logit specifications with a default $\times$ default-setting rule indicator variable . In our preferred specification (Table 3, column 5), we use option-level coefficients to estimate population average utilities, and a "participant chose x in No Default" dummy to further control for preferences at the individual level:

$$\begin{aligned} y^i(x|A,d,T) &= u(x) + \delta_T I(x=d) + \eta I(i \text{ chose } x \text{ in } ND), \\ p^i(x|A,d,T) &= \frac{\exp\left(y^i(x|A,d,T)\right)}{\sum\limits_{w\in A} \exp\left(y^i(w|A,d,T)\right)}. \end{aligned}$$

Specifications (1)-(4) do not include option fixed effects, that is, the $u(x)$ terms; (3) and (4) instead include a variable that gives the frequency the option was chosen in No Default; this cardinal variable will be ordinally equivalent to a population utility function u estimated only from No Default data. Specifications (1) and (3) do not include the "participant chose

[4]Variation in observations across rules is the result of a small coding error. Observations are removed for instances of repeated choice sets within the same rule or repeated rules for the same subject, or missing choices.

x in No Default" variable. Specifications (7) and (8) control for order effects that may affect the strength of default bias; (5) adds an additional rule-independent default interacted with round term, allowing default bias to get stronger over the experiment linearly with round number; (6) counts rounds after round 36 as late, and interacts late round with default option, allowing default bias to be stronger after round 36. In all specifications, default bias is significant for every default-setting rule.

	(1)	(2)	(3)	(4)	(5)	(6)
Random, Message	0.27	0.33	0.36	0.41	0.32	0.33
	(0.09)	(0.10)	(0.10)	(0.10)	(0.21)	(0.13)
Expert, Message	1.44	1.49	0.98	1.05	0.96	0.92
	(0.10)	(0.10)	(0.11)	(0.12)	(0.19)	(0.15)
Social, Message	1.49	1.38	0.74	0.77	0.69	0.66
	(0.09)	(0.09)	(0.09)	(0.10)	(0.15)	(0.12)
Custom, Message	1.62	1.59	1.04	1.06	0.98	0.96
	(0.09)	(0.10)	(0.10)	(0.10)	(0.16)	(0.13)
Uninformative, Message	1.05	1.05	0.68	0.72	0.71	0.72
	(0.08)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)
Random, Default	0.56	0.61	0.69	0.73	0.49	0.53
	(0.07)	(0.08)	(0.08)	(0.09)	(0.14)	(0.10)
Expert, Default	1.54	1.58	1.10	1.17	0.96	0.86
	0.08	0.09	0.09	0.09	0.13	0.12
Social, Default	1.66	1.55	0.93	0.96	0.75	0.71
	0.07	0.08	0.08	0.08	0.13	0.11
Custom, Default	1.82	1.79	1.24	1.26	1.05	1.00
	(0.08)	(0.08)	(0.08)	(0.09)	(0.12)	(0.10)
Uninformative, Default	1.06	1.04	0.67	0.70	0.66	0.70
	(0.07)	(0.08)	(0.08)	(0.08)	(0.09)	(0.08)
ownND		1.33		0.95	0.95	0.95
		(0.04)		(0.04)	(0.04)	(0.04)
Alternative Dummies	N	N	Y	Y	Y	Y
Default in Round >36						0.35
						(0.08)
Designated Option in Round >36						-0.14
						(0.11)
Default * Round/45					0.35	
					(0.16)	
Designated * Round/45					0.14	
					(0.23)	
LL	-26615.58	-24355.93	-22825.43	-21831.78	-21827.37	-21817.64
AIC	53251.15	48733.85	45814.86	43829.56	43824.74	43805.28
N	91380	91380	91380	91380	91380	91380

Cluster-robust standard errors in parentheses

Table 3: Multinomial Logit Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Random, Default	0.15	0.15	0.16	0.16	0.13	0.16
	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)
Expert, Default	1.82	1.71	1.00	1.00	0.88	0.90
	(0.09)	(0.09)	(0.08)	(0.09)	(0.15)	(0.11)
Social, Default	1.63	1.51	0.79	0.79	0.67	0.69
	(0.09)	(0.09)	(0.09)	(0.09)	(0.14)	(0.11)
Custom, Default	1.89	1.77	1.14	1.12	1.00	1.02
	(0.09)	(0.09)	(0.08)	(0.09)	(0.15)	(0.11)
ownND		1.23		0.89	0.89	0.89
		(0.06)		(0.06)	(0.06)	(0.06)
Alternative Dummies	N	N	Y	Y	Y	Y
Default in Round >48						0.15
						(0.09)
Default * Round/48					0.19	
					(0.20)	
LL	-11537.51	-10776.28	-9901.82	-9534.83	-9534.16	-9532.7
AIC	23083.02	21562.57	20003.63	19271.66	19272.31	19269.41
N	40615	40615	40615	40615	40615	40615

Cluster-robust standard errors in parentheses

Table 4: Multinomial Logit Estimates, Student Sample

Result 2: Default bias is stronger with intentionally-set defaults than defaults with Uninformative or Random defaults.

Figure 2 shows that people choose the default option more often with Expert, Social, and Custom defaults than with Uninformative or Random defaults, but they also chose those options more frequently in No Default. To control for differences in default quality, we perform comparative Obuchowski tests and also test for differences in the default bias coefficients in our multinomial logit specifications.[5] In all tests using the multinomial logit specifications, we find significant differences between Random and each of Expert, Social, and Custom. We find a consistent significant difference between Social and Custom, and (in most specifications) between Social and Expert, but most specifications do not find a significant difference between Expert and Custom.

In all specifications in Table 3 (except for (3) and (4), where the difference is tiny), the estimated default bias parameter for Uninformative is higher than that for Random but lower

[5]Obuchowski tests provide the cleanest control, but cannot control for order effects and drop many observations. This latter factor is especially relevant for comparisons involving Random, since these comparisons discard approximately 80% of choices by design. In contrast, intentional default setting rules often pick the same defaults, so comparisons between them have more power.

		Rule				
	Rounds	Uninformative	Random	Expert	Social	Custom
Order	18 - 27	0.128				
	28 - 36		0.057	0.144	0.177	0.126
	37 - 45			0.244	0.195	0.276
	46 - 54			0.251	0.185	0.300
	55 - 63		0.111	0.239	0.164	0.240

Table 5: Default Group - Prolific Sample

than for all intentional default setting rules. In specifications (1) to (4) we find a significant difference between default bias in Uninformative and in each of Expert, Social, and Custom. However, Uninformative is always the first default setting rule faced by participants, so this may be driven by an order effect. Specifications (5) and (6) control for order effects in the strength of default bias. Both specifications (5) and (6) find significant differences between default bias in Uninformative and Custom, neither find significant differences between default bias in Uninformative and Social, and (5) but not (6) finds a significant difference between default bias in Uninformative and Expert. We find a lack of a significant difference between Uninformative and Random defaults in all specifications except (1) and (2). These suggest that default bias in the Uninformative treatment lies somewhere between the levels of default bias in the Random and the intentional default setting rules, but our analysis suggests that the effect size makes our sample insufficiently large to establish statistical significance.[6] This indicates that subjects do respond to the informational content of defaults, exhibiting more default bias with intentionally set defaults than with random defaults or when no information is given about how the default was selected.

A limited non-parametric analysis finds similar results as well as substantial order effects. Table 5 shows the default bias score broken down by round and rule.[7] First, for Random, Expert, and Custom default bias increases substantially in later rounds indicating possible fatigue. Second, focusing only on earlier rounds, default bias is substantially higher for Uninformative, Expert, Social, and Custom than for Random which suggests that our Prolific subjects recognized the lack of information inherent in the randomly set defaults. Finally, in early rounds, default bias is similar for Uniformative, Expert, Social, and Custom. This suggests that even when subjects are not told explicitly how default options are chosen they

[6] This finding is consistent with previous work that shows that some people view a default as an implicit recommendation in the absence of explicit information on how the default was set [McKenzie et al., 2006].

[7] Order effects for the Message and Student groups can be found in Appendix B.

nevertheless infer some normative content. Together with the results of Altmann et al. [2022], this supports an endorsement effect interpretation even when defaults are not explicitly framed as recommendations.

Result 3: Bias towards the designated option in the Message treatment is quantitatively similar to default bias in the Default treatment.

Figure 2 shows that the propensity to choose the designated option in the Message treatment is nearly identical to the propensity to choose the default option in the comparable default-setting rule in the Default treatment. In two-sample t-test, we find no statistically significant differences. This further demonstrates that subjects respond to the message behind the default and not just the default itself. Indeed, if we take the lack of quantitative and statistical differences between treatments seriously, it indicates that almost all of the default bias we find comes from the message itself.[8]

Our multinomial logit specifications similarly estimate the bias towards the designated option in our Message group. We compare each of these coefficients to the default bias coefficient for the same rule to test, between subjects, whether providing a message explaining a rule for designating an option has the same effect as when that option is also made the default. We find a significant difference for Random in specifications that do not control for order effects, with slightly more bias towards the default in the Default group than towards the designated option in the Message group. In all other tests, default bias in the Default group is not significantly different from bias towards the designated option in the Message group. This suggests that the message contained in the default is the primary driver of default bias in our setting.

Result 4a: Most people prefer intentional default setting rules.

Table 6a counts the fraction of people who ranked each default setting rule in each position. We find that 74.3% of people most prefer one of the three intentional default setting rule, whereas only 4.4% most prefer the Random rule and 17.0% prefer No Default. Expert is the most preferred rule for 44.6% of people, and the second most preferred for another 28.8%, making it the most popular rule. Custom is a clear second, with 23.2% ranking it first

[8]These findings are consistent with Altmann et al. (Experiment 2) who make a similar between-subjects comparison between messages versus defaults, but obtain statistical significance. In their setting, values are entirely objective, and a participant whose preferences over the decision-maker's action are transparently misaligned with the decision-maker acts as a choice architect and selects an option as a message or default. They find that decision-makers follow defaults in 65.3% of the time, but only follow a comparable message 61.5% of the time. We find quantitatively similar difference between our treatments in the same direction. While we have roughly similar sample sizes, our effective statistical power is lower because we must control for unobserved quality when measuring bias towards a default or a designated option.

and 29.4% ranking it second. Performing a Borda count, we obtain an aggregate ranking Expert $\succ$ Custom $\succ$ Social $\succ$ Random $\succ$ No Default. We obtain similar results with our student sample (Table 6b). However, we note that in both samples, a substantial minority of people rank No Default first, indicating that they prefer not to have defaults.

Default type	#1	#2	#3	#4	#5
No Default	0.171	0.040	0.085	0.205	0.500
Random	0.051	0.125	0.165	0.400	0.261
Expert	0.450	0.290	0.165	0.063	0.034
Social	0.097	0.250	0.335	0.216	0.102
Custom	0.233	0.296	0.250	0.119	0.102

$n = 176$ subjects who faced all default-setting rules.

(a) Prolific Default Group

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

(b) Student Sample

Default type	#1	#2	#3	#4	#5
No Default	0.215	0.043	0.022	0.280	0.441
Random	0.032	0.140	0.065	0.387	0.376
Expert	0.559	0.258	0.140	0.022	0.022
Social	0.032	0.344	0.355	0.183	0.086
Custom	0.161	0.215	0.419	0.129	0.075

$n = 93$ subjects who faced all rules; rule chooses

(c) Prolific Message Group, Delegated Choice

Table 6: Ranking of default-setting rules

Result 4b: People rank rules similarly when they are committed to follow choices from the selected rule.

Subjects in the Message group perform the same ranking exercise across all rules, but instead of the rule simply selecting a default, subjects are told that the rule will make the choice for the subject except in the No Default case. Thus, a subject who prefers to retain autonomy to choose should rank No Default first to maximize their odds of being able

	Default Bias Score by Default-Setting Rule					
Difficulty	Uninformative	Random	Expert	Social	Custom	Choice
Easy	0.127	0.085	0.249	0.227	0.278	0.305
Intermediate	0.133	0.099	0.257	0.152	0.249	0.299
Hard	0.125	0.083	0.201	0.170	0.185	0.240

Table 7: Default Bias by Difficulty (Prolific, Default Group)

to choose. The ranking of all remaining rules ought to represent the perceived alignment between the rule and one's own preferences. Strikingly, we obtain essentially similar rankings of rules (Table 6c) and the same aggregate rating using the Borda count. Only 21.5% of subjects rank No Default first and 44.1% of subjects rank it last.

Result 5: Neither complexity nor rational inattention seem to explain default bias.

Choice sets varied in complexity (by design). Thus, we can separately measure and 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 pattern of statistical significance. While complexity may have a role in default bias, this indicates that it was not the driving force behind our results.

One 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 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 predicts that the strength of default bias in each rule should be positively related to how well its defaults are correlated with preferences, which are noisily revealed in No Default. Thus there should be no default bias in Random unless a person decides to completely exert no decision-making effort.[9] This prediction is consistent with our finding that there is less default bias in Random. However, only 3 participants in the Prolific sample always choose the default option in Random. Thus, the default bias we find in Random cannot be explained by the drop out effect and is inconsistent with rational inattention.

[9]The approach of avoiding all attentional effort by always picking the default is a version of what Caplin and Martin [2017] term the "drop out effect".

Since the defaults in Social overlap with No Default choices more than defaults in Expert, under plausible assumptions, defaults Social are relatively more correlated with underlying preferences. Thus, in the rational inattention model, people should exhibit weakly more default bias in Social than in Expert, and be more likely to rank these rules highly. Yet people tend to rank Expert #1 in all treatments, and most rank Social below Expert. Additionally, all of our point estimates suggest that people exhibit weakly more default bias in Expert than in Social, though this difference is not statistically significant. This is inconsistent with the rational inattention account of default bias, and suggests that people prefer expert guidance over guidance that is more in line with their own choices.

Result 6a: Most but not all people said they liked having a default.

After completing all choices, participants in the Prolific groups were asked "In general, did you like having a default/designated option?" For the default group, 62% of participants answered "Yes". In the Message group, only 40% answered "Yes". That is, most people mostly liked having defaults, and relatively more people said they liked defaults than said they liked having a designated option. We interpret this as a general, but not universal endorsement of defaults, and of defaults versus mere information even though they have similar effects on choice in our data.

We also compute default bias scores for each group, separately for all default-setting rules. Unsurprisingly, the group that reports generally liking defaults exhibits "more" default bias, on average, including for Uninformative and Random defaults. Surprisingly, the group that reports disliking defaults also exhibits default bias, on average, for all default-setting rules. This conflicts with our inclination to view default-bias as a revealed-preference measure of whether people like defaults.

Result 6b: More people said their choices were influenced by the default in intentional default-setting rules than for Random. Yet a majority also said the explanation describing how the default option was chosen did not affect their decisions.

Our survey asked each participant in the Prolific groups whether or not, for each rule, the default/designated option influenced their choices (Table 9). Only 17% of participants said it did in Random, compared to 59% for Expert, 44% for Social, and 47% for Custom. While we see the "Yes" group for each rule has a positive default bias score, as we would expect, we observe the same for the "No" group for each rule as well. On the face of it, this suggests that many people either fail to recognize or deny their own default bias.

Next, we look at the correspondence whether a person reports that they liked defaults

			Default Bias				
		n	Uninformative	Random	Expert	Social	Custom
Default	Yes	119	0.141	0.073	0.259	0.206	0.274
	No	41	0.101	0.106	0.169	0.144	0.150
	Unsure	32	0.118	0.129	0.150	0.221	0.223
Message	Yes	40	0.148	0.022	0.307	0.190	0.258
	No	36	0.132	0.071	0.159	0.153	0.136
	Unsure	25	0.130	0.080	0.182	0.095	0.237
Prolific sample Default bias column reports the average default bias for the Default group and the bias towards the designated option for the Message group.							

Table 8: Did you like having a default/designated option?

		Random	Expert	Social	Custom
Yes	$N =$	33	104	84	101
	Default Bias	0.189	0.271	0.263	0.281
No	$N =$	159	72	108	91
	Default Bias	0.069	0.186	0.121	0.191
When the default was selected [according to the given rule], did the default option influence your choices?					

Table 9: Survey: Did the default option influence your choices?

in general against whether they report being influenced by defaults in each rule (Table 10). We find that 78% of those who answered that they liked defaults answered that they were influenced by them in at least one default-setting rule. We also find that 56% of those who answered that they did not like defaults reported that they were influenced by defaults in at least one default-setting rule. We find roughly similar frequencies for the analogous Message subgroups. Just as we found an imperfect correspondence between self-reports of liking defaults and our choice-based measure of default bias, we found a similarly imperfect

		Influenced?							Influenced?				
Like?		Random	Expert	Social	Custom	Any?	Like?		Random	Expert	Social	Custom	Any?
Yes	119	27	74	66	74	93	Yes	40	7	24	23	21	30
No	41	2	16	9	13	23	No	36	3	19	11	14	21
Unsure	32	4	14	9	14	18	Unsure	25	2	14	10	11	15
Default							Message						

Table 10: Survey: Did you like having a default vs. did the default option influence

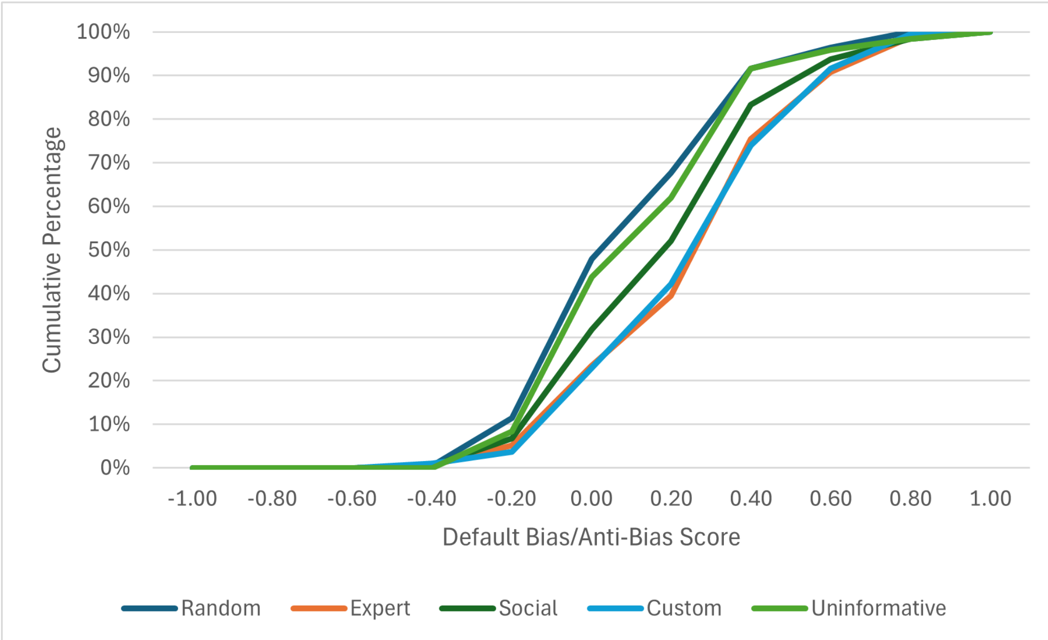


Figure 3: Distribution of Default Bias

correspondence with the survey indicator of default bias.

Result 7: Default bias is heterogeneous for all default-setting rules

Figure 3 reports the distribution of default bias and anti-bias for subjects in the Default treatment. The statistic differs slightly from the default bias score reported previously but it matches the way in which the Obuchowski test is calculated. For each subject and default setting rule this statistic is equal to the fraction of choice sets (typically out of 9) for which subjects did not choose the default in No Default and chose the default for a given rule minus the fraction of choice sets for which subjects did choose the default in No Default but did not choose the default for a given rule. Hence, this is a number bounded between -1 and 1 where positive numbers are associated with relative bias and negative numbers are associated with relative anti-bias. This subject-level measure is given by:

$$\text{Net Default Bias Score}_{iT} = \frac{1}{|A \in T|} \sum_{A \in T} [c_i(x|A, x, T) - c_i(x|A, \emptyset, ND)]$$

The shape of the CDFs illustrate the dispersion in default bias for all default-setting

	Random	Expert	Social	Custom	Uninformative
Random	1				
Expert	0.285	1			
Social	0.234	0.414	1		
Custom	0.301	0.514	0.507	1	
Uninformative	0.347	0.307	0.3677	0.300	1

Table 11: Correlation between Default-Setting Rules

	Uninformative	Random	Expert	Social	Custom
Always	4	3	7	15	13
Never	11	17	9	10	9

Table 12: Number of Subjects in Default Treatment who Always/Never Pick the Default

rules. The CDFs also indicate that default bias is driven primarily by many subjects with moderate bias rather than few subjects with extreme bias while, at the same time, a non-trivial proportion of subject-rule pairs exhibit small degrees of anti-bias. Additionally, we find statistically significant positive subject-level correlation across any pair of rules (Table 11).[10]

Finally, Table 12 reports the number of subjects who always pick the default and who never pick the default by rule. Few subjects — less than 10% and often much less — always pick the default option for all rules. This indicates that the vast majority of subjects remained engaged throughout the experiment in spite of what may be inferred from the order effects in Table 5. Overall, we do not see strong evidence that default bias in our experiment is driven by a heuristic where the default option is always chosen. As well, few subjects never choose the default option, with the largest number for the Random rule. We might expect these numbers to be even smaller, however this is consistent with the moderate degree of anti-bias observed (see Figure 3). In fact, 4 subjects never selected the default option in any round.

5 Conclusion

We study the effectiveness of different default-setting rules in a setting where preferences are heterogeneous, and there is no objective ranking of options. Our experimental design

[10] 18 subjects are dropped due to missing values for one or more default-setting rule.

and statistical approach allow us to disentangle the subjective quality of defaults that a rule assigns from the amount of default bias that the same rule induces. We find a significant increase in the probability of choosing default options for intentionally-set defaults, but less so for randomly-set defaults and for uninformative defaults. We find a similar effect on choice of designating an option and communicating how it was chosen without pre-selecting it as a default. This suggests that the message associated with the default is the primary driver of default bias in our setting.

Our experimental results indicate that it is not the mere presence of a default option, but also the endorsement it provides that drives default bias. As such, defaults in our setting are like message nudges (e.g. Milkman et al., 2021). This adds to a long literature on endorsement as one rationale for default bias [Blumenstock et al., 2018, Jachimowicz et al., 2019]. Bovens [2009] conjectured that nudges "work better in the dark". While Loewenstein et al. [2015] found that default bias persists when people are pre-informed that a default has been selected, our results go further and show default bias is stronger with intentional defaults when the choice architect communicates how they selected defaults.

In the incentivized ranking, most participants prefer intentional default-setting rules (Expert, Custom, and Social) to choosing without a default or to randomly-set defaults. This provides clear evidence that most people prefer a good nudge. However, while Social and Custom defaults tended to coincide with No Default choices more often than Expert defaults, the Expert default-setting rule was the most popular when people ranked rules, suggesting that the preference for intentional default rules does not merely arise from a desire to reduce random decision error, but is a stronger endorsement of libertarian paternalism.

We hope our results provide a small bridge to the large branch of the literature doing field studies where alternatives are often, if not always, subjective and preferences heterogeneous as it is when selecting savings rates or insurance premia.[11] One key difference, however, between our experiment and real-world settings is that our interface clearly informs and saliently reminds decision-makers how defaults are set for each decision. This is not analogous to most real world settings listed above, where decision-makers are likely to hold incorrect beliefs about how defaults are set. Even so, our findings suggest that communicating the way defaults are set may be a potentially effective policy channel for enhancing adherence to intentionally-set defaults and promoting active choice when defaults are random.

[11] See, for example, Choi et al. [2004], Beshears et al. [2009], Madrian and Shea [2001], Samuelson and Zeckhauser [1988], Thaler and Benartzi [2004].

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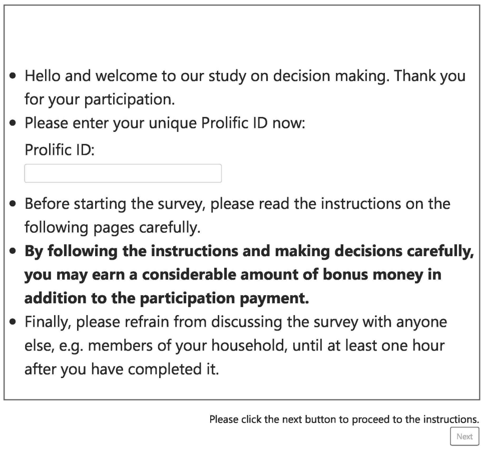
Supplemental Appendix



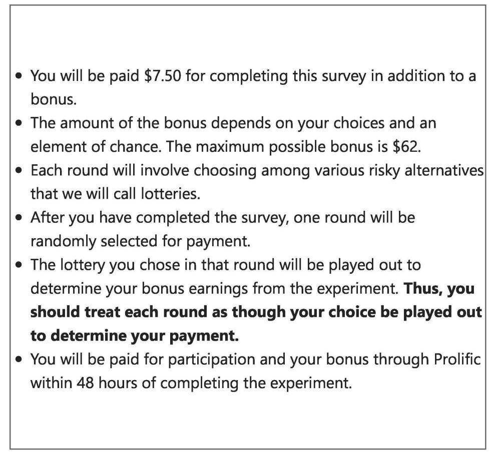
- Hello and welcome to our study on decision making. Thank you for your participation.
- Please enter your unique Prolific ID now:

  Prolific ID:
- Before starting the survey, please read the instructions on the following pages carefully.
- **By following the instructions and making decisions carefully, you may earn a considerable amount of bonus money in addition to the participation payment.**
- Finally, please refrain from discussing the survey with anyone else, e.g. members of your household, until at least one hour after you have completed it.

Please click the next button to proceed to the instructions.

Next

(a) Introduction



- You will be paid $7.50 for completing this survey in addition to a bonus.
- The amount of the bonus depends on your choices and an element of chance. The maximum possible bonus is $62.
- Each round will involve choosing among various risky alternatives that we will call lotteries.
- After you have completed the survey, one round will be randomly selected for payment.
- The lottery you chose in that round will be played out to determine your bonus earnings from the experiment. **Thus, you should treat each round as though your choice be played out to determine your payment.**
- You will be paid for participation and your bonus through Prolific within 48 hours of completing the experiment.

(b) Instructions 1

Figure 1

A Complete Experimental Procedure - Prolific Sample

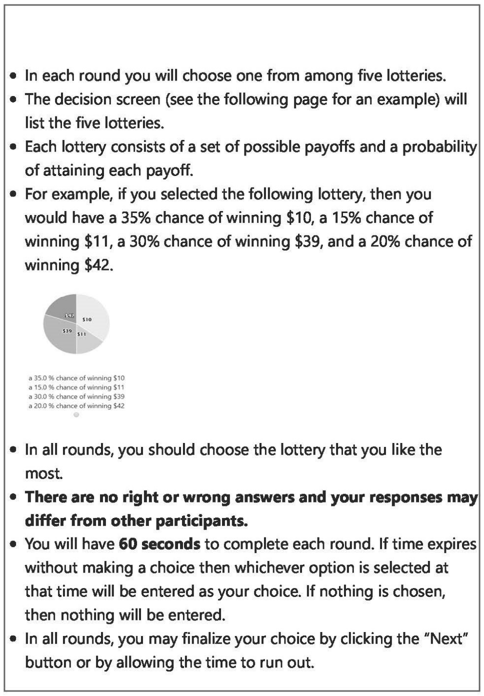
A.1 Instructions

The experiment was programmed using the oTree web-based platform [Chen et al., 2016] and completed by subjects in their web browser. After accessing the link provided by Prolific, subjects read the instructions independently before consenting to the use of their data and beginning the experiment. The screenshots in Figures 1a through 5b contain the text of the instructions for both the message-default and message-only sessions.

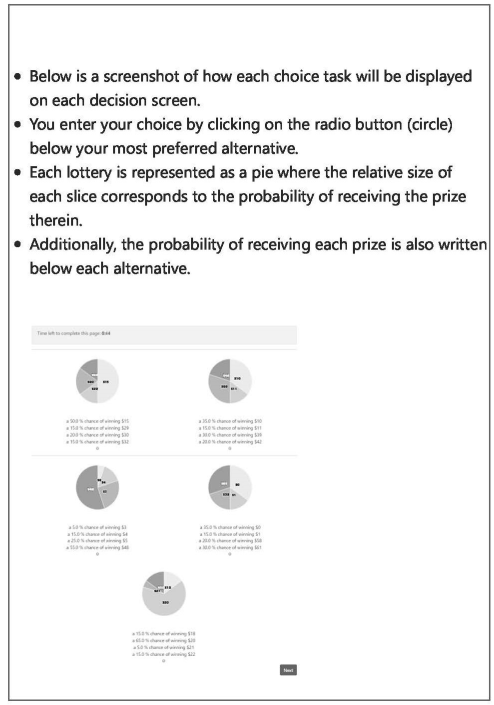
A.2 Experimental flow

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

1. No Default treatment (18 rounds)

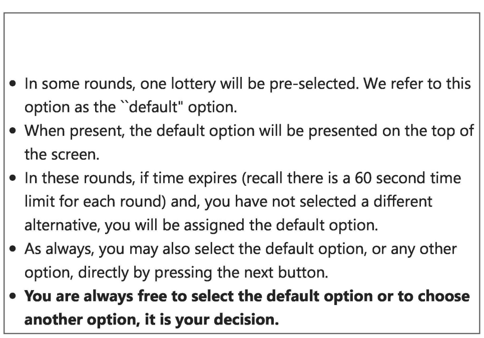


(a) Instructions 2

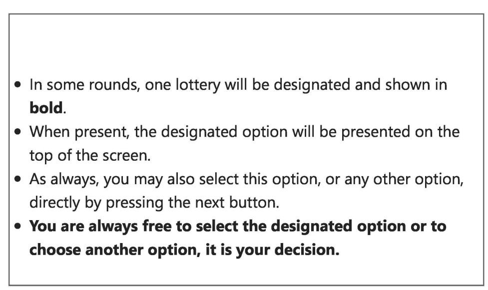


(b) Instructions 3

Figure 2

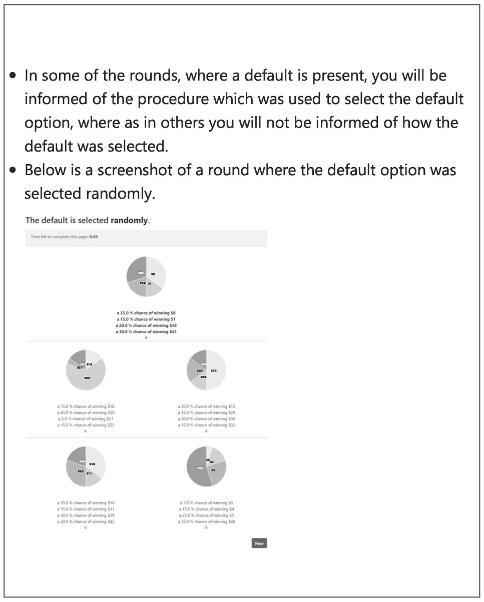


(a) Instructions 4

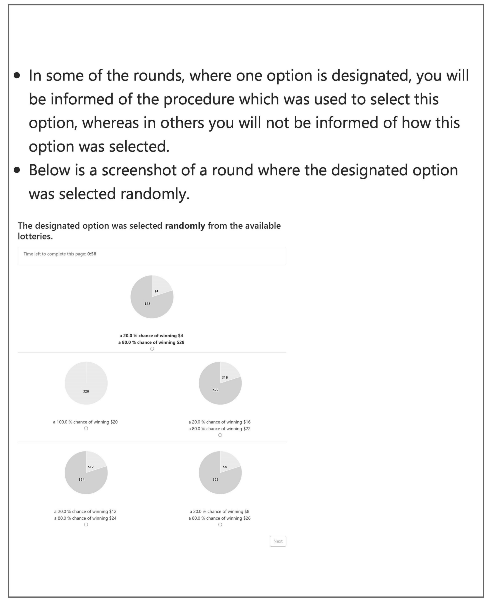


(b) Instructions 4m

Figure 3

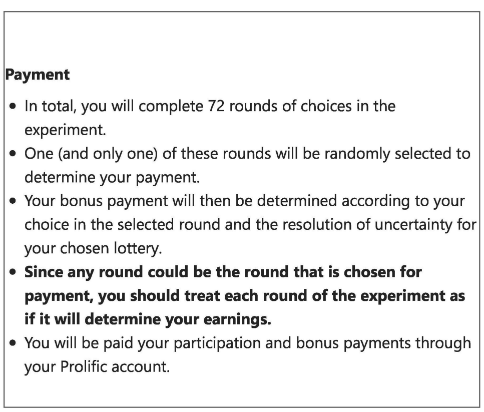


(a) Instructions 5

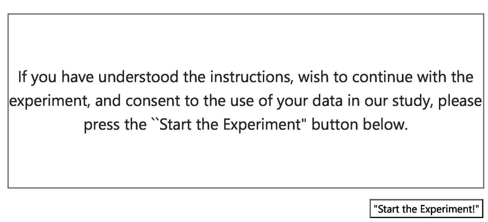


(b) Instructions 5m

Figure 4



(a) Instructions 6



(b) Consent

Figure 5

(a) Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 6aChoice page as in Figure 6c

2. Uninformative treatment (9 rounds)

    (a) Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 7a
    (b) Choice page as in Figure 7cDepending on the session, subjects went through one of the twelve possible orders for Random, Expert, Social, and Custom treatments – this was varied across subjects. Below is an example where the order is Random, Social, Expert, and Custom.
    (c) Random treatment (9 rounds)

        i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 7a
        ii. Choice page as in Figure 7c

    (d) Social treatment (9 rounds)

        i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 7b
        ii. Choice page as in Figure 7d

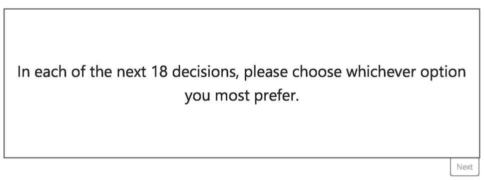
    (e) Expert treatment (9 rounds)

        i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 8a
        ii. Choice page as in Figure 8c

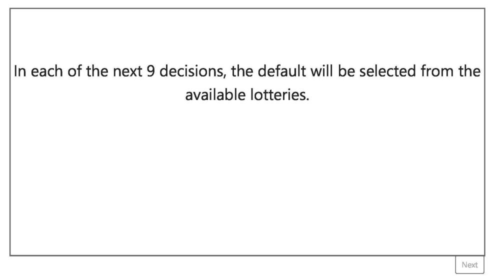
    (f) Custom treatment (9 rounds)

i. Instruction for the treatment (shown once at the beginning of the treatment) as in Figure 8b
ii. Choice page as in Figure 8d

3. Ranking of default rules at the end of round 63 as in Figure 9a
4. One treatment is implemented for the last 9 rounds.

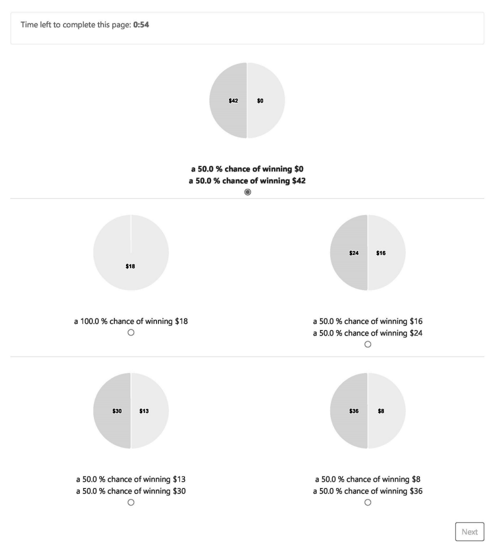


(a) Instruction for No Default treatment



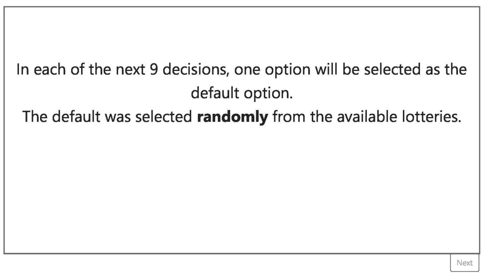
(b) Instruction for Ininformative treatment

(c) Choice screen for No Default treatment

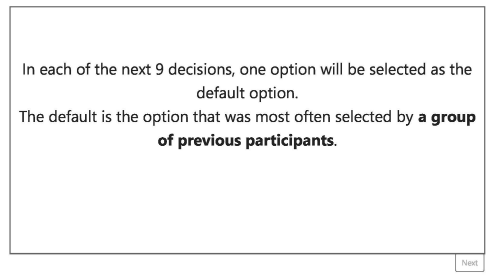


(d) Choice screen for Uninformative treatment

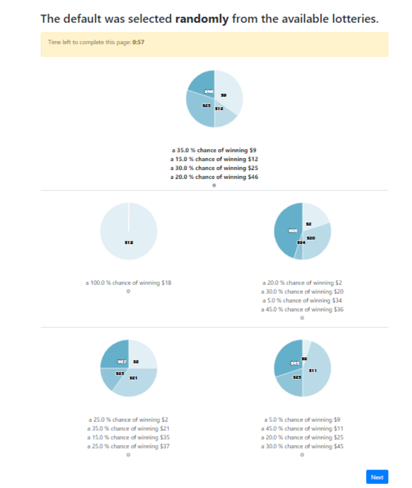
Figure 6



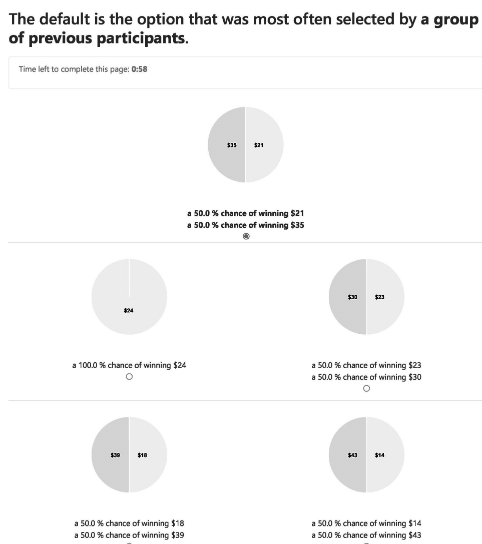
(a) Instruction for Random treatment



(b) Instruction for Social treatment

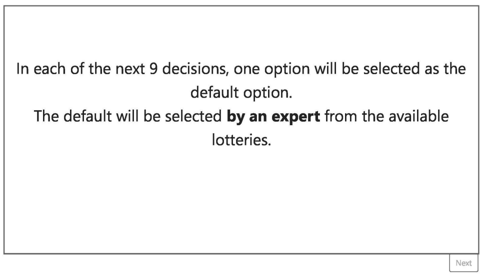


(c) Choice screen for Random treatment

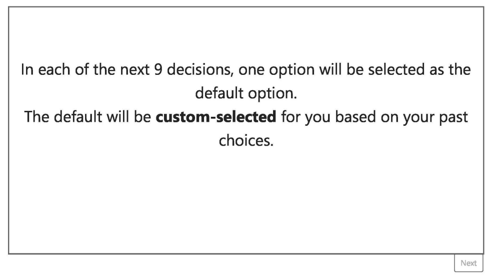


(d) Choice screen for Social treatment

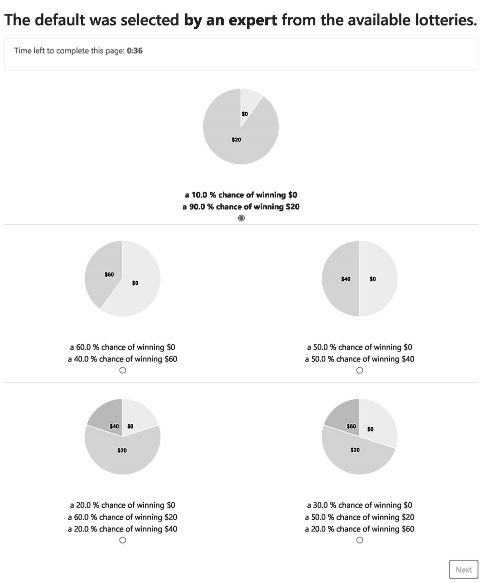
Figure 7



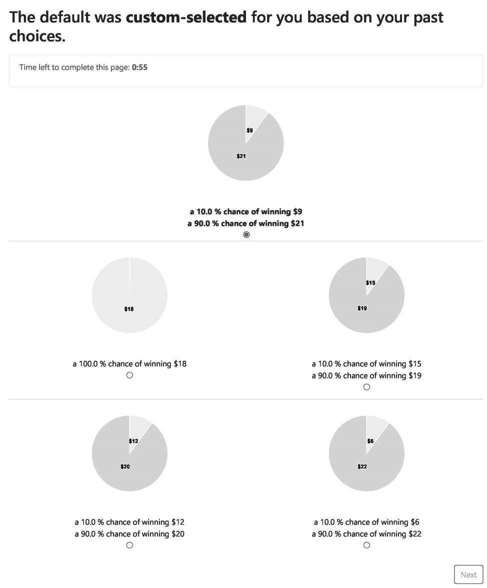
(a) Instruction for Expert treatment



(b) Instruction for Custom treatment

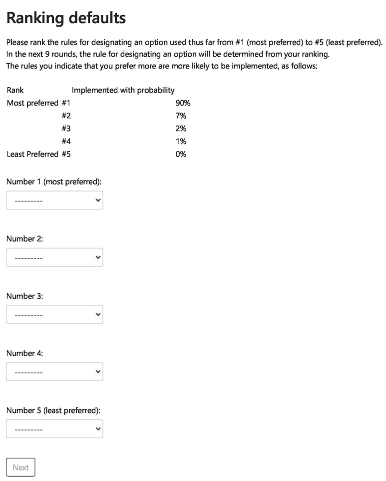


(c) Choice screen for Expert treatment

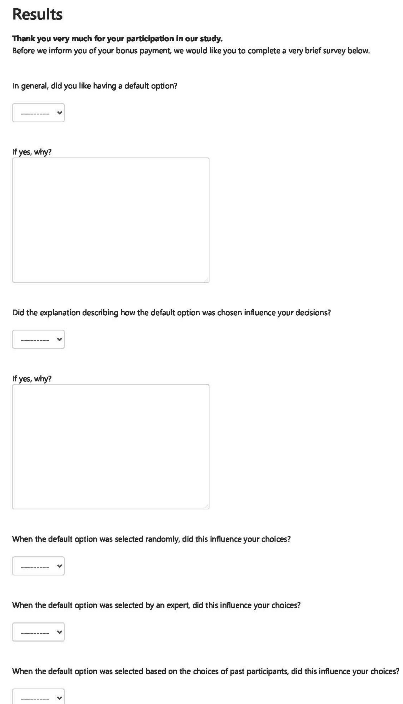


(d) Choice screen for Custom treatment

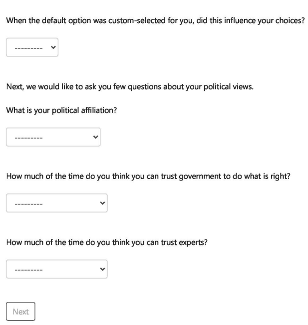
Figure 8



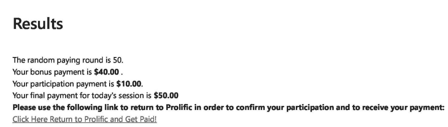
(a) Ranking of default rules



(b) Survey - Page 1

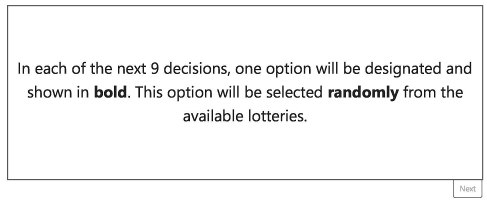


(c) Survey - Page 2

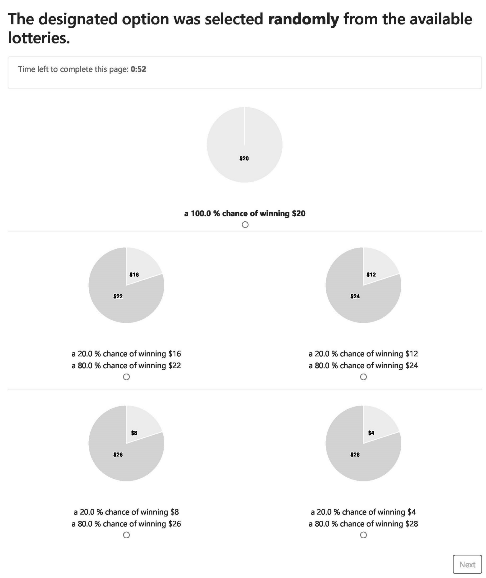


(d) Results Screen

Figure 9



(a) Instruction for Random treatment - Message only



(b) Choice screen for Random treatment - Message Only

Figure 10

A.3 Choice sets

The Table lists 18 unique choice sets[12] which we organized into six blocks of three lotteries (1 to 6). 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.

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

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.

Set	Name	Lottery	pa	pb	pc	pd	xa	xb	xc	xd	Social	Expert	C_H	C_M	C_L
1	11	1	100	0	0	0	18	0	0	0	*				
1	11	2	50	50	0	0	16	24	0	0			*	*	
1	11	3	50	50	0	0	13	30	0	0		*			*
1	11	4	50	50	0	0	8	36	0	0					
1	11	5	50	50	0	0	0	42	0	0					
2	12	1	100	0	0	0	12	0	0	0	*	*	*	*	*
2	12	2	25	37.5	37.5	0	0	12	24	0					
2	12	3	37.5	62.5	0	0	0	24	0	0					
2	12	4	37.5	25	37.5	0	0	12	36	0					
2	12	5	50	50	0	0	0	36	0	0					
3	13	1	100	0	0	0	20	0	0	0					
3	13	2	25	25	25	25	11	13	31	33					
3	13	3	50	25	25	0	12	24	42	0					

3	13	4	25	25	50	0	4	22	32	0					
3	13	5	25	50	25	0	12	23	42	0	*	*	*	*	*
4	21	1	100	0	0	0	18	0	0	0	*				
4	21	2	10	90	0	0	15	19	0	0					
4	21	3	10	90	0	0	12	20	0	0			*		
4	21	4	10	90	0	0	9	21	0	0				*	
4	21	5	10	90	0	0	6	22	0	0		*			*
5	22	1	60	40	0	0	0	60	0	0					
5	22	2	50	50	0	0	0	40	0	0					
5	22	3	20	60	20	0	0	20	40	0	*				
5	22	4	30	50	20	0	0	20	60	0					
5	22	5	10	90	0	0	0	20	0	0		*	*	*	*
6	23	1	100	0	0	0	18	0	0	0			*		
6	23	2	20	30	5	45	2	20	34	36					
6	23	3	25	35	15	25	2	21	35	37					
6	23	4	5	45	20	30	9	11	25	45		*		*	*
6	23	5	35	15	30	20	9	12	25	46	*				
7	31	1	100	0	0	0	24	0	0	0					
7	31	2	50	50	0	0	23	30	0	0					
7	31	3	50	50	0	0	21	35	0	0	*	*	*	*	*
7	31	4	50	50	0	0	18	39	0	0					
7	31	5	50	50	0	0	14	43	0	0					
8	32	1	37.5	37.5	25	0	0	10	20	0					
8	32	2	50	12.5	37.5	0	0	10	20	0					
8	32	3	50	37.5	12.5	0	0	10	30	0					
8	32	4	75	25	0	0	0	30	0	0					

8	32	5	25	75	0	0	0	10	0	0	*	*	*	*	*
9	33	1	15	45	15	25	22	24	25	26			*	*	
9	33	2	5	45	40	10	17	18	33	34		*			*
9	33	3	15	35	5	45	10	13	41	42	*				
9	33	4	50	5	10	35	6	50	51	52					
9	33	5	45	5	10	40	0	1	51	62					
10	41	1	100	0	0	0	20	0	0	0	*				
10	41	2	20	80	0	0	16	22	0	0			*		
10	41	3	20	80	0	0	12	24	0	0		*		*	
10	41	4	20	80	0	0	8	26	0	0					*
10	41	5	20	80	0	0	4	28	0	0					
11	42	1	50	50	0	0	0	60	0	0					
11	42	2	30	70	0	0	0	40	0	0					
11	42	3	10	40	50	0	0	20	40	0					*
11	42	4	20	60	20	0	0	20	60	0					
11	42	5	100	0	0	0	20	0	0	0	*	*	*	*	
12	43	1	35	25	15	25	17	18	19	20	*				
12	43	2	30	20	35	15	15	17	25	26		*	*	*	*
12	43	3	40	20	15	25	13	14	29	31					
12	43	4	50	15	25	10	8	35	36	37					
12	43	5	35	5	10	50	0	1	2	42					
13	51	1	100	0	0	0	15	0	0	0					
13	51	2	50	50	0	0	12	21	0	0	*	*	*	*	
13	51	3	50	50	0	0	9	26	0	0					*
13	51	4	50	50	0	0	6	30	0	0					
13	51	5	50	50	0	0	3	33	0	0					

14	52	1	50	20	30	0	0	20	40	0	*				*
14	52	2	60	10	30	0	0	20	60	0					
14	52	3	60	40	0	0	0	40	0	0					
14	52	4	70	10	20	0	0	40	60	0					
14	52	5	40	60	0	0	0	20	0	0		*	*	*	
15	53	1	15	45	25	15	15	16	17	18					
15	53	2	50	15	10	25	12	20	21	24					
15	53	3	20	25	10	45	12	13	15	25	*	*	*	*	*
15	53	4	35	10	25	30	2	10	33	35					
15	53	5	45	10	20	25	2	32	34	35					
16	61	1	100	0	0	0	22	0	0	0	*				
16	61	2	30	70	0	0	18	26	0	0			*		
16	61	3	30	70	0	0	14	30	0	0				*	
16	61	4	30	70	0	0	10	34	0	0		*			
16	61	5	30	70	0	0	6	38	0	0					*
17	62	1	20	80	0	0	0	40	0	0					
17	62	2	30	60	10	0	0	40	60	0					
17	62	3	20	20	60	0	0	20	40	0					
17	62	4	60	40	0	0	20	40	0	0	*	*	*	*	*
17	62	5	100	0	0	0	20	0	0	0					
18	63	1	35	45	15	5	22	24	25	27					
18	63	2	25	20	35	20	22	23	28	31					
18	63	3	50	15	5	30	21	34	35	36		*	*	*	*
18	63	4	30	20	15	35	17	19	38	40	*				
18	63	5	10	20	25	45	14	15	16	46					

Overlapping structure and Order of choice tasks

In our experiment, each subject first faced all 18 unique choice sets in the No Default treatment, and then faced each choice set at least twice more under various 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 9 rounds where the default-setting rule was selected based on the subject's ranking. In total, we have 72 rounds.

We have twelve treatment orders based on twelve arrangements of Random, 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 according to log in order. Any difference in subject counts is due to some subjects dropping out after starting the experiment. We had 33 drop-outs out of 328 subjects. The table below lists the choice sequence that we used in our experiment. The number of subjects per sequence is reported in the table for both treatments with the Message treatment in brackets.

Order	N	Treatment	Choice Set	Order	N	Treatment	Choice Set
S1	16 (10)	No Default	11 12 13 61 22 43 31 32 33	S2	18 (8)	No Default	11 12 13 61 22 43 31 32 33
			41 62 23 51 52 53 21 42 63				41 62 23 51 52 53 21 42 63
		Uninformative	11 22 33 21 32 13 31 12 33*			Uninformative	11 22 33 21 32 13 31 12 33*
		Random	41 52 13 51 12 43 11 41 53			Expert	41 52 13 51 12 43 11 41 53
		Expert	61 22 43 21 42 63 41 62 23			Social	61 22 43 21 42 63 41 62 23
		Social	31 53 63 51 62 33 61 32 63			Custom	31 53 63 51 62 33 61 32 63
		Custom	21 42 33 41 32 23 31 22 43			Random	21 42 33 41 32 23 31 22 43
		Choice	11 52 23 51 22 13 21 12 53			Choice	11 52 23 51 22 13 21 12 53
S3	17 (8)	No Default	31 32 33 21 42 63 51 52 53	S4	17 (8)	No Default	31 32 33 21 42 63 51 52 53
			61 22 43 11 12 13 41 62 23				61 22 43 11 12 13 41 62 23

		Uninformative	31 42 53 41 52 33 51 32 53 *			Uninformative	31 42 53 41 52 33 51 32 53*
		Random	61 12 33 11 32 63 31 62 13			Social	61 12 33 11 32 63 31 62 13
		Expert	21 42 63 41 62 23 61 22 43			Custom	21 42 63 41 62 23 61 22 43
		Custom	51 12 23 11 22 53 21 52 23			Social*	51 12 23 11 22 53 21 52 23
		Social	41 62 53 61 52 43 51 42 63			Random	41 62 53 61 52 43 51 42 63
		Choice	31 12 43 11 42 33 41 32 13			Choice	31 12 43 11 42 33 41 32 13
S5	16 (8)	No Default	51 52 53 41 62 23 11 12 13	S6	15 (8)	No Default	51 52 53 41 62 23 11 12 13
			21 42 63 31 32 33 61 22 43				21 42 63 31 32 33 61 22 43
		Uninformative	51 62 13 61 12 53 11 52 13*			Uninformative	51 62 13 61 12 53 11 52 13*
		Random	21 32 53 31 52 23 51 22 33			Social	21 32 53 31 52 23 51 22 33
		Social	41 62 23 61 22 43 21 42 63			Expert	41 62 23 61 22 43 21 42 63
		Expert	11 32 43 31 42 13 41 12 43			Custom	11 32 43 31 42 13 41 12 43
		Custom	61 22 13 21 12 63 11 62 23			Random	61 22 13 21 12 63 11 62 23
		Choice	51 32 63 31 62 53 61 52 33			Choice	51 32 63 31 62 53 61 52 33
S7	16 (9)	No Default	31 32 33 21 42 63 51 52 53	S8	17 (9)	No Default	11 12 13 61 22 43 31 32 33
			61 22 43 11 12 13 41 62 23				41 62 23 51 52 53 21 42 63
		Uninformative	31 42 53 41 52 33 51 32 53			Uninformative	11 22 33 21 32 13 31 12 33
		Custom	61 12 33 11 32 63 31 62 13			Social	41 52 13 51 12 43 11 42 53
		Expert	21 42 63 41 62 23 61 22 43			Custom	61 22 43 21 42 63 41 62 23
		Social	51 12 23 11 22 53 21 52 23			Expert	31 52 63 51 62 33 61 32 63
		Random	41 62 53 61 52 43 51 42 63			Random	21 42 33 41 32 23 31 22 43
		Choice	31 12 43 11 42 33 41 32 13			Choice	11 52 23 51 22 13 21 12 53
S9	17 (7)	No Default	31 32 33 21 42 63 51 52 53	S10	15 (8)	No Default	31 32 33 21 42 63 51 52 53
			61 22 43 11 12 13 41 62 23				61 22 43 11 12 13 41 62 23
		Uninformative	31 42 53 41 52 33 51 32 53*			Uninformative	31 42 53 41 52 33 51 32 53*
		Random	61 12 33 11 32 63 31 62 13			Custom	61 12 33 11 32 63 31 62 13
		Custom	21 42 63 41 62 23 61 22 43			Expert	21 42 63 41 62 23 61 22 43
		Expert	51 12 23 11 22 53 21 52 23			Social	51 12 23 11 22 53 21 52 23

		Social	41 62 53 61 52 43 51 42 63			Random	41 62 53 61 52 43 51 42 63
		Choice	31 12 43 11 42 33 41 32 13			Choice	31 12 43 11 42 33 41 32 13
S11	14 (9)	No Default	51 52 53 41 62 23 11 12 13	S12	15 (9)	No Default	51 52 53 41 62 23 11 12 13
			21 42 63 31 32 33 61 22 43				21 42 63 31 32 33 61 22 43
		Uninformative	51 62 13 61 12 53 11 52 13*			Uninformative	51 62 13 61 12 53 11 52 13*
		Random	21 32 53 31 52 23 51 22 33			Custom	21 32 53 31 52 23 51 22 33
		Custom	41 62 23 61 22 43 21 42 63			Social	41 62 23 61 22 43 21 42 63
		Social	11 32 43 31 42 13 41 12 43			Expert	11 32 43 31 42 13 41 12 43
		Expert	61 22 13 21 12 63 11 62 23			Random	61 22 13 21 12 63 11 62 23
		Choice	51 32 63 31 62 53 61 52 33			Choice	51 32 63 31 62 53 61 52 33

* denotes a repeated observation that was dropped. The inclusion of these was an unintentional error.

B Robustness

Order effects are a concern for all within-subjects designs. Table 15 reports the default bias score by order and default setting rule. The default bias scores appear to increase, sometimes dramatically, after Round 36 for all default-setting rules except Social. As well, note that the default bias score for the Uniformative rounds (which occur always in Rounds 19-27) is quantitatively similar to the scores of the intentionally-set defaults and greater than the score for the randomly set defaults in Rounds 28 - 36. This suggests that subjects infer some normative content from the Uniformative defaults.

Another potential concern is that our results could be mechanically driven by a heuristic. For example, subjects tend to choose options in the No Default rounds which are located either at the bottom of the screen (the third row), or on the right side of the second row, and then also choose the default option when available. Table 16 shows the distribution of choices in the No Default rounds for all subjects. For the default sessions 62.3% of choices are from these two locations and for the message sessions it is 59.1%, with the remainder being roughly evenly distributed across the remaining three options. In order to address

		Rule				
	Rounds	Uninformative	Random	Expert	Social	Custom
Order	18 - 27	0.128				
	28 - 36		0.057	0.144	0.177	0.126
	37 - 45			0.244	0.195	0.276
	46 - 54			0.251	0.185	0.300
	55 - 63		0.111	0.239	0.164	0.240

(a) Default

		Rule				
	Rounds	Uninformative	Random	Expert	Social	Custom
Order	18 - 27	0.138				
	28 - 36		0.056	0.183	0.165	0.175
	37 - 45			0.225	0.125	0.256
	46 - 54			0.242	0.174	0.173
	55 - 63		0.053	0.161	0.141	0.216

(b) Message

Table 15: Default Bias Score by Order and Rule

	Upper Left	Upper Right	Middle Left	Middle Right	Bottom
Default (n = 3466)	0.137	0.118	0.123	0.248	0.375
Message (n = 1815)	0.129	0.149	0.131	0.235	0.356

Table 16: Distribution of Choices in No Default by Screen Position

this concern, we create a “Filtered” sampling by dropping all observations where the choice in ND was in the Middle Right or Bottom position. Table 17 reports the default bias and designated option bias scores for the Default and Message groups, both for the usual and for the filtered samples. Note that the Choice column of Messages corresponds gives the maximal bias possible given ND choices since those choices are made mechanically by the chosen rule.

		Uninformative	Random	Expert	Social	Custom	Choice
Default	Filtered	0.145	0.076	0.171	0.239	0.220	0.291
	Unfiltered	0.128	0.089	0.236	0.182	0.237	0.281
Message	Filtered	0.159	0.073	0.200	0.239	0.221	0.625
	Unfiltered	0.138	0.054	0.220	0.154	0.209	0.683

Table 17: Default/Designation Option Bias, Filtered versus Unfiltered Sample