Defaults and Cognitive Effort*

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October 26, 2022

Abstract

We explore experimentally a cognitive-effort channel through which defaults might influence behavior in an insurance market setting where there is uncertainty in the benefits offered by different potential plans. We find that defaults can strongly influence purchasing behavior when participants can make decisions at their own pace and we document a positive correlation between the time subjects spend making a decision and the probability that they adjust away from the offered default. By contrast, we observe no significant impact of defaults in a treatment where we fix the decision time so that participants must spend 45 seconds on each decision screen without the possibility of moving faster. Our fixed-deliberation manipulation lowers the opportunity cost of decision time and suggests that defaults operate by influencing decisions of individuals who find the cognitive costs of active decision-making prohibitively high.

Keywords: default effect, cognitive costs, cognitive-effort channel, insurance, experiment
JEL Codes: C91, D91, G22

*We thank Associate Editor and two anonymous referees for their comments, and Timothy Johnson, Chengbin Feng and Lunzheng Li for programming the experiment in oTree. We gratefully acknowledge the financial support of the Australian Research Council through ARC Discovery Award DP19013475 and the programming support offered by the BizLab at the UNSW Business School.

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What progress individuals could make, and what progress the world would make, if thinking were given proper consideration.
Thomas A. Edison

1 Introduction

There is considerable evidence that non-binding defaults can influence peoples' decisions. Default effects have been documented in individually and socially important decisions such as retirement-savings contribution rates (Madrian and Shea, 2001), the selection of retirement-savings plans (Dobrescu et al., 2016), organ donation (Adabie and Gay, 2006; Johnson and Goldstein, 2009), the selection of auto insurance (Johnson et al., 1993), and the choice of vehicles (Levav et al., 2010). In the hands of benevolent "choice architects," defaults are the leading example of behavioral policies based on the ideas of Libertarian Paternalism (Thaler and Sunstein, 2003; Sunstein, 2014; Jachimowicz et al., 2019); whereas, when used by profit-maximizing firms, defaults can be exploitative (Armstrong, Vickers and Zhou, 2009; Altmann, Falk and Grunewald, 2022).

Prior research has identified three distinct channels by which defaults might influence choice (Johnson and Goldstein, 2009; McKenzie, Liersch and Finkelstein, 2006; Dinner et al., 2011; Smith, Goldstein and Johnston, 2013). The first channel is endorsement: in selecting a default, the choice architect is revealing information to the decision maker regarding the choice architect's preferred outcome. If the incentives of both parties are at least partially aligned, this information may provide an endorsement to the decision maker regarding the best outcome. The second is endowment: decision makers may treat the default as the status quo and may evaluate their utility in terms of the gains and losses associated with switching away from this default. Finally, the third is cognitive effort: the decision maker may find evaluating different outcomes cognitively costly and may adopt the default rather than exploring more broadly if the cost of effort is high or the expected gain from switching away from the default is low.

In this paper, we report the results of a laboratory experiment with which we seek to isolate the cognitive-effort channel by using a deliberation time manipulation that influences the marginal cost of effort but leaves the other aspects of the decision problem unchanged. We see the identification of this channel as important for three reasons. First, although the cognitive-effort channel is implied in the literature that relates defaults to heuristics (Gigerenzer, Todd and the ABC Research Group, 1999; Anderson, 2003; Gigerenzer, 2008; Johnson and Goldstein, 2009), prior research has not been able to cleanly identify it. In particular, in an extensive meta-analysis that casts a wide net across many domains, Jachimowicz et al. (2019) find evidence only for the endorsement and endowment effects. We provide an easy manipulation for modifying cognitive effort costs and use it to provide clean evidence that defaults can operate through a cognitive-effort channel.

Second, we believe that the cognitive-effort channel is likely important for understanding how defaults could be used by self-interested firms. As shown in Altmann, Falk and Grunewald (2022), decision makers take into consideration the incentives of the choice architect when evaluating the likely quality of the default option. Thus, defaults that rely on the endorsement effect may function less well in market settings where the preferences of the decision maker and choice architect are misaligned.[1] By contrast, defaults that are set to reduce cognitive effort do not rely on beliefs about the choice architect and require only that the choice architect selects a default that is sufficiently good to prevent the decision maker from searching (Ortmann et al., 2022). Thus, default effects based on cognitive effort are likely to be less sensitive to the intentions of the choice architect and are, therefore, likely to operate in market settings.

Third, from a policy standpoint, the cognitive-cost channel has different implications for the distributional aspects of default policies relative to the other two channels. In particular, if defaults operate through cognitive costs, then default policies are likely to operate on the portions of the population whose costs of cognitive effort are high. Often, these are vulnerable segments of the population for whom choice is difficult (Byrne and Martin, 2021), such as the older population whose cognitive functions have declined (Besedeš et al., 2012), individuals with limited numeracy training (Cokely and Kelley, 2009), or individuals who are inexperienced in a particular market (Steffel, Williams and Pogacar, 2016). Understanding the way in which defaults impact different segments of the population is important to evaluating the potential welfare consequences of such interventions.

We study defaults in an insurance choice context based loosely on the Australian private health insurance market. In our experiment, subjects face a series of individual decision problems where they are asked to choose an insurance contract from a menu of four available options. Similar to the real market, these options are layered on top of each other, with a higher "tier" of coverage insuring all the states that are covered in a lower tier.[2] The underlying risks are based on a randomly generated 10×10 grid where each of the 100 squares is randomly assigned one of five colors. White squares are the most likely and represent states without loss. The other four colors represent states where the participant may incur a loss.

[1] See also Brown and Krishna (2004) and Campbell (2007), who find that decision makers adjust their behavior and response to the default based on their beliefs about the default setter.

[2] In the real market there are three tiers and plans are described as "bronze," "silver," and "gold." A silver plan covers all the issues covered in the bronze plan plus some additional categories. The gold plan covers all possible categories.

In each round, participants choose which colors to insure, and then the computer randomly draws one of the 100 states. If the state is a color that has not been insured, the participant incurs a loss.

Our interest is to induce an environment where active deliberation requires careful evaluations of both the state and one's own preferences so that defaults might be used to economize on cognitive effort. As such, we introduce risk, uncertainty, and complexity into our design by (i) showing participants only a random sub-sample of 10 states (keeping the remaining 90 states hidden),[3] (ii) constructing the menu of insurance contracts based on the risks observed in the sub-sample, and (iii) pricing the insurance contracts based on the risk observed in the sub-sample and an exogenous price floor. Due to potential sampling error, our setup is one where it is difficult to calculate the risk premium associated with each insurance contract and to make optimal decisions.

We use a 2×2 between-subject design to identify a behavioral response to defaults and to isolate a cognitive-effort channel by which defaults might operate. In the first dimension of the design, we explore whether participants can be induced to change their purchasing behavior based on the default insurance offered. To maximize contrast, we compare behavior between a treatment where the default contract is no insurance to one where the default contract is full insurance. As hypothesized, we find the basic default effect: there are significant differences in the amount of insurance purchased by participants in the two treatments. Furthermore, these differences are driven primarily by an increased number of choices that correspond to the default assigned in the treatment.

In the second dimension of the design, we explore the cognitive-effort channel hypothesis by running two additional treatments where we do not allow individuals to proceed through the experiment at their own pace. Instead, individuals are forced to spend exactly 45 seconds on each decision screen, without the possibility of moving faster. We refer to these treatments as having a *fixed deliberation time* because individuals are free to make a choice, or revise their choice, at any point in the 45-second deliberation window, but cannot continue until the deliberation window ends.

Adopting the classification of Spiliopoulos and Ortmann (2018), our deliberation time manipulation contrasts *endogenous choice* of decision time in the baseline *endogenous-deliberation time* treatments with *time delay* in the fixed-deliberation time treatments.[4] The fixed-deliberation time environment reduces the opportunity cost of decision-making time, which has been shown in the psychology literature to be an important cognitive cost (Otto and

[3] The use of grids and blackout to generate information asymmetries was inspired by Cooper and Rege (2011), who use a similar approach to generate decision problems with both risk and ambiguity.

[4] The latter has been shown to improve decision making in various domains through mandatory "cooling-off" periods (e.g., Rekaiti and Van den Bergh, 2000; Lee, 2013).

Daw, 2019). To fix ideas, suppose an individual spends time t on a choice from a set of available options and let $r(t)$ be the corresponding expected payoff benefit from the resulting choice.[5] If c is the individual's (constant) marginal opportunity cost of time and $r(t)$ is an increasing function, the endogenous decision time, $t^*(c)$, solves $\max_{t\geq 0}[r(t) - ct]$, and is a decreasing function of c. In the timed treatments, subjects instead face a fixed deliberation time, T, and if $T > t^*(c)$ for a given individual, her choice will (weakly) improve.[6]

We find the median decision time to be 11 seconds in the endogenous-deliberation time treatments—substantially less than $T = 45$ seconds in the fixed-deliberation time treatments. We predicted that the introduction of the (generous) timer would cause the default to be less important for decision making.[7] Consistent with our hypothesis, we observe no significant differences in decisions between participants whose default contract is no insurance and those whose default contract is full insurance in the treatments with a fixed timer. We also find that individuals are less likely to stick with their default in the treatments with the timer as compared to the corresponding treatments without a timer.

The cognitive-cost channel is also supported by the relationship between defaults and decision times. In a subset of our fixed-deliberation sessions, we measured a lower bound for the amount of time subjects spent making active decisions by identifying the last point in time at which they actively switch their selected insurance contract. We find that subjects spend more time actively making decisions in the fixed-deliberation treatments relative to the endogenous-deliberation time treatments. Further, deliberation time is strongly correlated with an increase in insurance in the no-insurance default treatment and a decrease in insurance in the full-insurance treatment in both the fixed- and endogenous-deliberation time treatments. These results are supportive of the hypothesis that moving away from the default is cognitively costly and that our fixed-deliberation treatments reduce the opportunity cost of this effort.

Our decision-time manipulation is different from those explored in most of the existing experimental literature. Typically, decision-making costs are manipulated directly by varying the complexity of the task (Wilcox, 1993; Kalayci, 2016; Kalayci and Serra-Garcia, 2016) or by rushing individuals with a binding time constraint (Sutter, Kocher and Strauß, 2003;

[5]Function $r(t)$ can be micro-founded, for example, via a search model.

[6]It may happen that $T < t^*(c)$ for some individuals. As long as this is a rare event, it does not invalidate our argument in the aggregate.

[7]Note that we predicted that default effects are attenuated in the fixed-deliberation time treatment relative to the endogenous-deliberation treatments rather than predicting no default effect since both endorsement and endowment effects may exist in both of our treatments. In a different environment, de Haan and Linde (2018) explore defaults in a setting with fixed decision time but where (i) decision makers receive a large time-dependent bonus for making quick decisions and (ii) decision makers cannot change their answer after making a choice. In their setting, the default effect exists and can be reinforced over time by initially offering good defaults.

Kocher and Sutter, 2006; Kocher, Pahlke and Trautmann, 2013).[8] One notable study of the delay effect is Grimm and Mengel (2011) who find that "sleeping on it" reduces rejection rates in the ultimatum game. Their result is consistent with ours in that in both cases a delay helps participants move away from the initial hasted response.

Taken together, our decision time treatment changes the opportunity cost of time (an important cognitive cost) but does not change potential cues related to endowment or endorsement. As such, we are the first to provide channel-specific evidence that defaults operate by allowing decision makers to economize on cognitive effort. Our paper suggests a tight connection between defaults, search, and decision-making strategies that may have a variety of implications on the use and distributional consequences of defaults.

The rest of our paper is organized as follows. We discuss our experimental design and hypotheses in Section 2. Next, we report on the results from the experiment in Section 3. We further discuss the results and conclude in Section 4.

2 Experimental Design

The main section of the experiment consisted of 12 computerized choice tasks in which the participant chose between potential insurance contracts that insure against losses in different states of the world.

Each choice task began by assigning the participant a 10×10 grid of potential states. As seen in the example grid on the left-hand side of Figure 1, each square in the grid was color coded and corresponded to one of five potential states. White squares were the most frequent and represented states without loss ($k = 0$). The remaining four colors—red, orange, yellow, and green—represented states where the decision maker incurred a loss if the state was drawn and the color was not insured.

We generated the state grids as follows: Each square in the grid was first independently assigned a state $k \in \{0, 1, 2, 3, 4\}$ via a random draw with respective probabilities $\{0.37, 0.18, 0.18, 0.14, 0.13\}$.[9] Squares that were assigned state 0 were colored white. Squares that were assigned one of the other four states were assigned colors using a random permutation of {red, orange, yellow, green}. The permutation implied that the most frequent

[8] We chose against these alternatives because it is possible that they directly alter the deliberation strategy individuals use to make a decision in addition to altering the opportunity cost of cognition. For instance, there is evidence that individuals are more likely to use heuristic reasoning under time pressure (Spiliopoulos, Ortmann and Zhang, 2018). Since our focus is on defaults, there is also a purely mechanical effect as it takes time to select alternative options.

[9] We assign a state to each square independently, as opposed to generating a grid with color frequencies that exactly match these probabilities. This ensured that the elements of the sub-sample had the same probabilities *ex ante* as the elements of the full sample.

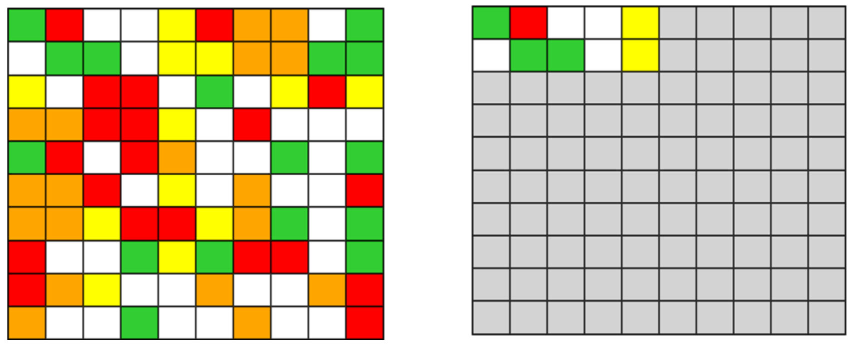

Figure 1: *Left*: A 10×10 grid representing a full sample. *Right*: The same grid with 10 cells revealed, representing a sub-sample.

non-white color in one task was likely to be different from the most frequent non-white color in another task. The grids for all 12 tasks, and one practice task at the beginning, were pre-drawn and re-used in the same order for all participants in all sessions of the experiment.

Participants were not informed about the grid that was allocated to them. However, a 2×5 subset of the grid was revealed to participants in each decision problem. Thus, we revealed a sub-sample of 10 potential states that could be used to infer the probability of each state being drawn. Subjects were informed in the instructions that each square was filled in independently using the same assignment process and that we randomly selected the block of squares that are revealed.

Similar to the Australian private health care market, the insurance contracts consisted of four potential tiers, where each tier insured the colors offered in the previous tier plus an additional color. To generate this set of insurance contracts, we first ranked the non-white colors based on the observed frequency of occurrence in the sub-sample and broke ties randomly. Next, we constructed four insurance contracts, C_1 through C_4, where each contract C_k insures the states with k highest ranks.

The four contracts were priced in a two-step process. For each color, we calculated a naive expected event frequency $\hat{q}_i$ by multiplying the number of observed occurrences of each state by $100/10 = 10$. We next set the price of insuring color i to $a_i = \max\{12, L\hat{q}_i\}$. The price of contract $k = 1, \ldots, 4$ was equal to the sum of the prices of its insured colors: $p_k = \sum_{i=1}^{k} a_i$. Note that a minimum price of 12 was set for insuring an additional color. This *price floor* ensured that a firm never lost money in a contract when a color was under-represented in the observed sub-sample.[10]

[10] In our setting, the floor tended to bind only for the highest two tiers of insurance. Thus, the floor can also be thought of as a market premium for high levels of insurance. Such premiums exist in competitive insurance markets due to moral hazard, an issue we do not consider in this paper.

For example, suppose a participant observed the grid in the right panel of Figure 1 with one red square, zero orange squares, two yellow squares, and three green squares. Based on these draws, the participant would be able to purchase four insurance contracts: C_1 that insures green, priced at 30; C_2 that insures green and yellow, priced at 50; C_3 that insures green, yellow, and red, priced at 62; and C_4 that insures all colors, priced at 74. Note that the last two prices are based on $a_3 = a_4 = 12$ for red and orange since fewer than two cells are observed for these two colors.

Participants were offered a *default* insurance contract that they could purchase or modify. We refer to this contract as the "pre-selected" contract in the instructions. Participants could purchase a contract by clicking on a Confirm button, or modify it by clicking on an Add or Remove button to cycle through the other possible contracts in both directions. Note that the defaults have no impact on the set of contracts offered in any decision problem. On the results screen at the end of each round, participants were again shown the partially revealed grid, reminded which insurance contract they selected, and shown the square drawn and the payoff for the round. With the exception of a trial round at the beginning, the full grid was never shown.

All amounts were denominated in tokens. Participants were endowed with 200 tokens at the start of each decision problem. A participant's payoff for a decision was thus equal to 200 minus the price paid for insurance and minus 100 if an uninsured color was drawn. One decision was chosen randomly for actual payment at the end, at the exchange rate of 1 AUD = 10 tokens.

The main part of the experiment was followed by three tasks where subjects' risk aversion, loss aversion, and ambiguity aversion were elicited using list methods. During each task, subjects were presented with a list of 21 choices between a lottery and a sure amount of money, constructed in such a way that a subject preferring more money to less would have a unique point at which they are willing to switch from the draw to the sure amount. In the risk task, the lottery was $(0, \$2.00; 0.5, 0.5)$, and the sure amounts of money increased from zero to \$2.00, in 10 cent increments. In the loss task, the lotteries were $(-\$x, \$2.00; 0.5, 0.5)$, where x changed from 0 to 2.00 in 10 cent increments, and the sure amount of money was always 0. Finally, in the ambiguity task the lottery was $(0, \$2.00; p, 1 - p)$, where, unbeknownst to subjects, p was generated randomly from the uniform distribution on $[0, 1]$, and the sure amounts were the same as in the risk task. The three tasks were presented to subjects in a random order, without feedback, and one of them was randomly selected for actual payment.

2.1 Cognitive Costs in the Decision Environment

Similar to Cooper and Rege (2011), decision makers in our experiment face an environment where there is both risk and ambiguity regarding the underlying state. Further, while prices are reflective of observable information, they provide very little additional information and it is hard to assess the risk premium associated with different contracts. As such, we conjecture that deliberation in our environment is costly and that active decision making requires a careful evaluation of both the state and one's own preferences. This creates a strong rationale for following the default and minimizing deliberation costs.

To get a better sense of the deliberation costs, consider again the example in Figure 1 and suppose that the decision maker is trading off between full insurance (C_4) and a contract that only insures the green states (C_1). Calculating the expected value of full insurance is easy: under full insurance, the decision maker always receives the high payoff of 200 and must pay the insurance cost of 74. Thus, the expected (and true) value of the contract is 126. The sub-sample of 10 states is an unbiased estimate of the total amount of risk in the environment. Thus, the decision maker could estimate that there are 60 uninsured states in the environment. This estimate would be close to the true average of 61.7 bad states that exists in problems where one red square, zero orange squares, two yellow squares and three green squares are observed.[11]

Assessing the value of partial insurance is more difficult. Recall that the four states are ordered according to the probabilities realized in the sample rather than the true probabilities. Similar to how order statistics can create a winner's curse problem in common-value auctions, this will cause states that occur frequently in the sub-sample to be overpriced in expectation. Since insurance contracts are sold in tiers, any contract other than full insurance (C_4) and no insurance (C_0) will be overpriced, in expectation, even if we did not impose a price floor. In the current example, the expected value of contract C_1 is 125.05, and 17.75 states are insured by the contract on average. However, to estimate this expected value, the decision maker would need to know the underlying distribution of risks or estimate it based on the sub-sample observed. Calculating the value of partial-insurance contracts is computationally hard and requires intermediate estimates that are prone to error.

The above discussion suggests that assessing the trade-off between insurance contracts is costly and that exact comparisons are computationally difficult. In expectation, the decision maker faces a risk premium for all contracts and thus choosing no insurance (C_0)

[11]This average and other calculations in the section were obtained by first forming posteriors over all 24 possible permutations of the assigned colors based on the observed sample and the true probability distribution used to assign colors to squares. We then calculate the expected number of insured squares for each possible insurance contract using these posteriors as weights.

maximizes expected earnings. Conditional on selecting an insurance contract, the decision maker's expected returns tend to be maximized by selecting either full insurance (C_4) or insuring only the most commonly observed state (C_1).

2.2 Treatments

We use a 2×2 between-subject design. Along the first dimension, we vary the default insurance contract offered to subjects at the beginning of each round: a default of no insurance (Blank, C_0) or a default of full insurance (Full, C_4). Along the second dimension, we vary whether subjects have endogenous deliberation time where they can proceed at their own pace with no timer (NT), or whether they have a fixed deliberation time where they must spend 45 seconds on the respective decision screen, without a possibility to move faster (T). We abbreviate the resulting four treatments as BlankNT, FullNT, BlankT and FullT.

In the fixed-deliberation time treatments, individuals were still required to actively lock in their choice in a round by pressing the Confirm button. However, making a choice did not end the round and individuals could revise and update their choice as often as they like in the 45 second deliberation window. Thus, the deliberation time was fixed, but decision time within the deliberation window could be endogenous.

In our original four sessions of BlankT and two sessions of FullT, we did not capture clicks of the Confirm button and it was not possible to assess how long individuals actively took to make decisions. To better capture this process data, we ran two additional sessions of the BlankT and FullT treatments where we recorded the points when the Confirm button was clicked. We use the last of these points as a measure of active decision making time in our analysis in Section 3.3. Our original sessions of BlankT and FullT also used slightly different instructions, which included the word "default" in one of the explanations. As a robustness check, we removed this language in the additional sessions. We show in Appendix B that there is no significant difference between the two samples and have hence pooled them in the analysis below.

2.3 Protocol

We conducted two pilot sessions—one for treatment BlankNT (13 subjects) and one for treatment FullNT (10 subjects)—to assess the expected effect size and perform power analysis. For each subject, we calculated the average number of states insured over 12 rounds. With these averages as the unit of observation, the effect size (Cohen's d) between the two treatments was 0.532. At $\alpha = 0.05$ and power $1 - \beta = 0.8$, $N = 57$ observations per treatment are called for. We, therefore, targeted roughly 60 subjects per treatment.

Treatments	Sessions	Subjects per session	Total
BlankNT	4	16,11,16,19	62
FullNT	4	13,18,10,7	48
BlankT	6	12,14,9,13,25,23	96
FullT	4	13,31,22,26	92
Total	18		298

Table 1: Treatments, sessions, and the number of subjects.

We ran 18 sessions of the experiment at the UTS Behavioural Laboratory of the University of Technology Sydney. The experiment was run online using oTree (Chen, Schonger and Wickens, 2016). A total of 298 subjects were recruited via ORSEE (Greiner, 2015) from a population of undergraduate students at UTS. The numbers of sessions and subjects in each treatment are summarized in Table 1.

On average, sessions without timer lasted 41 minutes, while sessions with timer lasted 50 minutes. Subjects earned \$18.33 on average, including a \$5 participation payment.

2.4 Hypotheses

By way of our 2×2 design, we aim to test for a behavioral response to defaults and to isolate the cognitive-effort channel by which it might operate. To first test for a behavioral response to defaults, we compare behavior in the endogenous-deliberation treatments (BlankNT vs FullNT). We make the following prediction.

Hypothesis 1 *When decision time is unconstrained, less insurance will be purchased in the treatment where no insurance is the default compared to the treatment where full insurance is the default.*

Conditional on our first hypothesis being established, we then use the combination of all four treatments to differentiate between potential channels. As noted in the Introduction, previous work identified endorsement and endowment effects, as well as cognitive effort as channels by which defaults operate when set by benevolent choice architects. Notably, Jachimowicz et al. (2019), in their extensive meta-analysis, found evidence for endorsement and endowment effects only. Our fixed-deliberation time treatment varies the opportunity cost of decision time but leaves other aspects of the problem that relate to endorsement and endowment unchanged. If cognitive costs are a channel by which defaults operate, we would predict that defaults have a greater impact on behavior in the endogenous-deliberation time treatments than in the corresponding fixed-deliberation time treatments. We thus predict the following:

Hypothesis 2 *Behavior in the endogenous-deliberation time treatments is more sensitive to changes in the default than behavior in the fixed-deliberation time treatments.*

Hypothesis 2 calls for a difference-in-difference specification where we compare the difference in the average number of categories insured in the FullNT and BlankNT treatments to the difference in the average number of categories insured in the FullT and BlankT treatments. We would predict that the difference-in-difference coefficient is positive.

Although we have strong predictions between the endogenous-deliberation time and the fixed deliberation time treatments, we do not have an *a priori* prediction as to whether there is a default effect in the fixed deliberation time treatments. If, for instance, defaults operate through loss aversion and an endowment effect, then a default effect may still exist in the treatments with a fixed deliberation time.

3 Results

3.1 Treatment Comparisons

Result 1 *Consistent with Hypothesis 1, significantly more insurance was chosen in the treatment with full-insurance default and endogenous deliberation time (FullNT) compared to the treatment with no-insurance default and endogenous deliberation time (BlankNT). The differences between the two treatments is driven primarily by a large number of instances where subjects followed the default assigned to them.*

Support for Result 1 is provided in the left panel of Figure 2, which shows the average number of items insured in both of the endogenous deliberation time treatments. The error bars are the 95% confidence intervals of each treatment average with errors clustered at the individual level.

As seen in the figure, the average number of states insured in the no-insurance default treatment is 2.07 while the number of states insured in the full-insurance default treatment is 2.57. The difference is statistically significant (p-value $= 0.002$, the Wald test with clustering at the subject level; p-value $= 0.002$, the Mann-Whitney test with subject-level average as the unit of observation).

Figure 3 shows the histograms of the number of states insured. As seen from the left panel, there is a clear difference in the two treatments without the timer. The effect is driven mostly by a larger mass of choices at the corresponding default: significantly more instances of zero states insured in BlankNT (9 p.p. difference, p-value $= 0.007$), and significantly more instances of four states insured in FullNT (11 p.p. difference, p-value $= 0.045$). The difference is only marginally significant for one state (5.5 p.p., p-value $= 0.062$) and not significant for two and three states.[12]

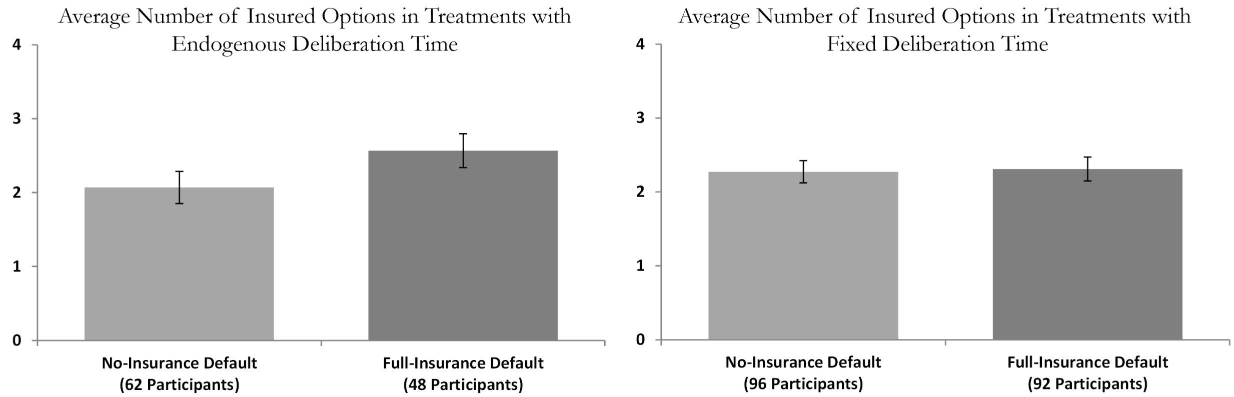

Figure 2: Average number of items insured by treatment. Error bars are 95% confidence intervals clustered at the individual level.

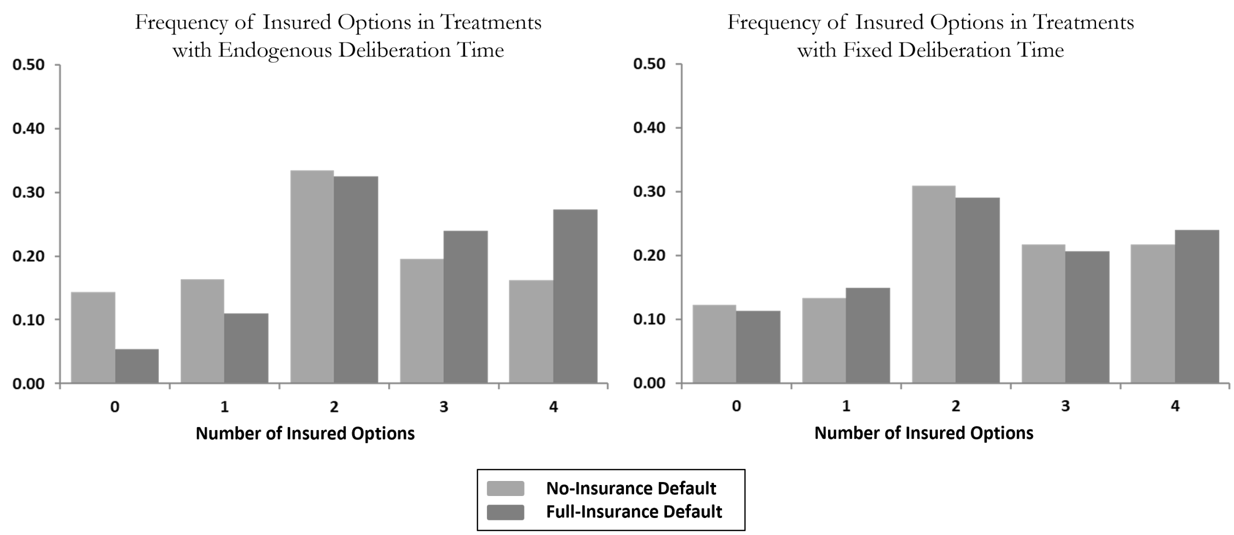

Figure 3: Histograms of the number of items insured by treatment.

Having established the existence of a default effect in the endogenous deliberation time treatments, we now turn to our second hypothesis.

[12]These comparisons are performed by running pooled OLS regressions of a binary variable equal to 1 if the corresponding number of states is insured, and 0 otherwise, on the FullNT treatment dummy, with errors clustered at the subject level.

Result 2 *Consistent with Hypothesis 2, the endogenous-deliberation time treatments are more sensitive to defaults than the fixed-deliberation time treatments. Further, there is no significant difference in the amount of insurance chosen in the fixed-deliberation time treatments.*

Support for Result 2 comes from comparing the difference in the treatments in the left panel of Figure 2 to the difference in the treatments in the right panel. As noted, the average number of states insured in FullNT is 2.57 while the average number of states insured in BlankNT is 2.07. Thus, the difference in these treatments is 0.5 categories. As seen in the right panel, the average number of states insured in FullT is 2.31 while the average number of states insured in FullNT is 2.26. Thus, the difference in these treatments is 0.05 categories. The difference-in-difference estimate of $0.5 - 0.05 = 0.45$ is significant in a pooled OLS regression where the insurance selected by individual is regressed on a dummy variable for the full information treatment, a dummy for the endogenous time treatments, and the interaction of these treatments ($p = 0.016$, errors clustered at the individual level).[13]

A further comparison of the treatments with a fixed-deliberation time suggests that there is no difference between subjects assigned to the full-insurance default and those assigned to a no-insurance default. As seen in Figure 3, the number of states insured in the two treatments is very similar ($p = 0.726$, the Wald test; $p = 0.719$, the Mann-Whitney test). Likewise, there is no significant difference in the proportion of cases where no insurance is chosen (p-value $= 0.664$) nor in the proportion of cases where full insurance is chosen (p-value $= 0.539$).

Figure 4 shows how the average numbers of states insured varied over time. As seen in the left panel, there is no obvious time trend in BlankNT or FullNT, which is confirmed by linear regressions of the number of states insured on the period number producing p-value $= 0.289$, 0.959 in BlankNT and FullNT, respectively. Subjects consistently insured more states in FullNT as compared to BlankNT.

As seen in the right panel, there is a small but significant positive time trend in BlankT (p-value $= 0.036$) and no significant time trend in FullT (p-value $= 0.616$). In general, there is no consistent ordering in the average number of states insured in these two treatments.

One might wonder whether the difference in defaults is economically meaningful. To study this question, we also calculated the expected profits of the firm and the expected

[13]We also tested for the interaction effect non-parametrically using a synchronized permutation test developed in Pesarin (2001) and Salmaso (2003), which restricts permutations to the same level of a factor to generate test statistics that can separate main factors from interaction effects. The permutation test on the interaction term is significant (p-value $= 0.007$). See Appendix D of Burfurd and Wilkening (2022) for the implementation details of this test.

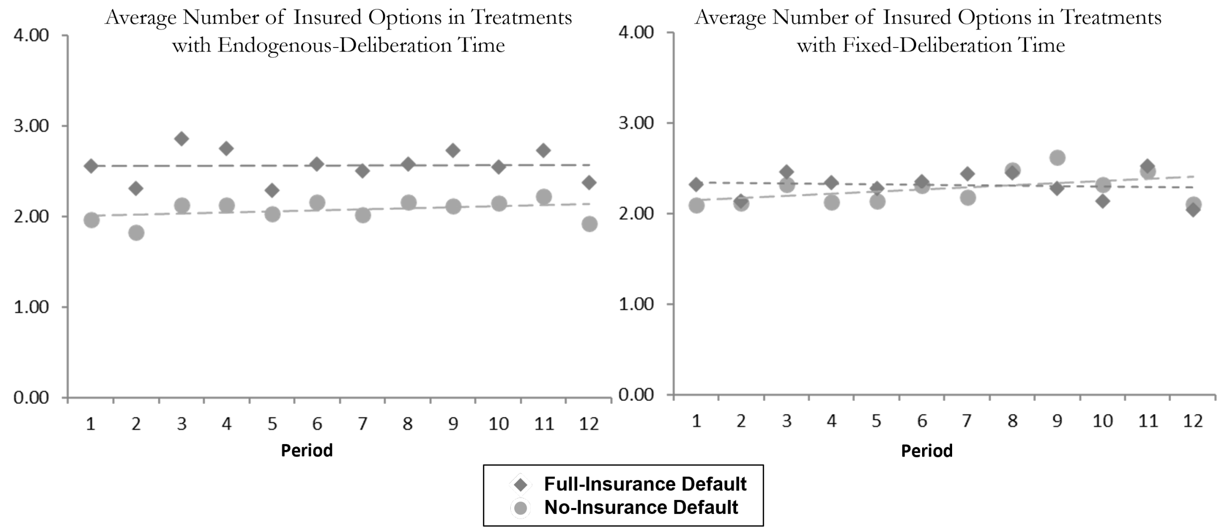

Figure 4: Average number of items insured by treatment over time.

earnings of the decision makers in each of our treatments.[14] The firm earns 1.77 points (12%) more in FullNT as compared to BlankNT, but only 0.12 points (0.8%) more in FullT as compared to BlankT. The former effect is statistically significant ($p = 0.010$, the Wald test; $p = 0.030$, the Mann-Whitney test), and the latter is not ($p = 0.457$, the Wald test; $p = 0.506$, the Mann-Whitney test). Decision makers' expected earnings are 1.58 points (7.8%) less in the FullNT treatment as compared to the BlankNT treatment, but only 0.13 points (0.12%) less in in the FullT treatment as compared to the BlankT treatment. The former effect is statistically significant in parametric tests ($p = 0.025$, the Wald test) but not in non-parametric tests ($p = 0.138$, the Mann-Whitney test), and the latter is not significant in either specification ($p = 0.793$, the Wald test; $p = 0.532$, the Mann-Whitney test). Overall, these results suggest that specific forms of defaults do matter.

Result 3 *On average, firms' expected profit and decision makers' expected earnings are influenced by the default in the endogenous-deliberation time treatments but are not influenced by the default in the fixed-deliberation time treatments.*

Results 2 and 3 show that the default effect is large and economically meaningful in treatments where subjects' decisions are not timed. However, in the treatments where subjects (are induced to) think more about the decision problem, the default effect disappears.

[14]To calculate the expected earnings of the decision makers, we used the sample observed by the individual to generate the posterior distribution over all possible color permutations starting from the true prior distribution used to assign colors to states. This method takes into account the information embedded in the observed sample but does not account for errors that occur when trying to estimate the prior distribution itself. We do not have sufficient beliefs and preference data to be able to generate reasonable estimates of decision makers' prior distribution.

3.2 Individual-level Analysis

In this section, we look deeper into individual behavior to identify regularities underlying the default effect in the endogenous-deliberation time treatments.

We start by analyzing how consistently subjects followed the default. For each subject, we calculated the number of times the subject insured zero states (N_0) and the number of times the subject insured all four states (N_4). The empirical CDFs of the two variables are shown in Figure 5. The first-order stochastic dominance in each case is apparent, and the distributions are different (p-value $= 0.033$ and 0.059, respectively, the Mann-Whitney test).

Although first order stochastic dominance is established in the data, the existence of the default did not cause many individuals to fully disengage from active decision making over the entire experiment. Very few participants chose no insurance or full insurance in every period. As seen in the left panel of Figure 5, only five subjects (8.1%) in BlankNT insured zero states in 6 periods or more.[15] As seen in the right panel, only 12 subjects (25%) in FullNT insured all states in 6 periods or more. Thus, while the default influenced decision making, it did not fully eliminate the sensitivity of modification of insurance strategies to either insurance prices or outcomes.

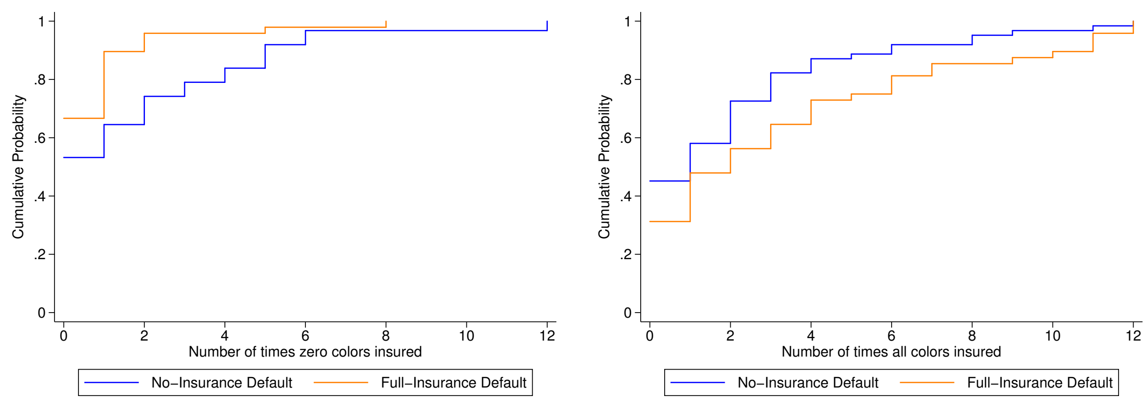

Figure 5: Empirical CDFs of the number of times a subject insured zero states (left) and all four states (right), in the treatments with endogenous-deliberation time.

To study how individuals' behavior responded to experience, we ran a series of exploratory OLS regressions to measure how learning and preferences influenced choices. The results are shown in Table 2. Specifications (1) and (2) consider data from the first period only. As seen from both regressions, a large and statistically significant default effect is present from the start. Specification (2) additionally controls for three measures of attitudes

[15] Of these five subjects, two chose to have no insurance in all 12 periods and three subjects chose to have no insurance in six periods.

States insured	(1)	(2)	(3)	(4)	(5)	(6)
FullNT	0.59***	0.55***	0.44***	0.28***	0.20	0.20
	(0.22)	(0.21)	(0.15)	(0.10)	(0.30)	(0.30)
States insured$_{t-1}$				0.46***	0.45***	0.45***
				(0.06)	(0.08)	(0.07)
States insured$_{t-1}$×FullNT					0.024	0.012
					(0.12)	(0.12)
Loss$_{t-1}$				0.25***	0.21**	0.22**
				(0.07)	(0.09)	(0.10)
Loss$_{t-1}$×FullNT					0.10	0.11
					(0.15)	(0.15)
RA		0.073***	0.035**			0.018*
		(0.025)	(0.016)			(0.010)
LA		0.023	0.020			0.013
		(0.018)	(0.013)			(0.008)
AA		-0.041*	-0.013			-0.007
		(0.024)	(0.019)			(0.011)
Intercept	1.97***	1.45***	1.67***	1.04***	1.07***	0.85***
	(0.15)	(0.33)	(0.24)	(0.14)	(0.17)	(0.23)
Subjects	110	110	110	110	110	110
Periods	1	1	12	11	11	11
Observations	110	110	1,320	1,210	1,210	1,210
R^2	0.063	0.14	0.066	0.22	0.22	0.22

Table 2: Pooled OLS regressions using data from treatments BlankNT and FullNT, robust standard errors clustered by subject in parentheses. Significance levels: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

to uncertainty—risk-aversion (RA), loss-aversion (LA), and ambiguity-aversion (AA). Each of the measures is constructed as explained in Section 2. More risk-averse subjects tend to insure more states.

Specifications (3)-(6) use data from all periods. In (3), we simply measure the difference in the average number of states insured controlling for uncertainty attitudes. The treatment effect confirms Result 1, and risk aversion continues to play a role, although the effect is lower in magnitude than in the first period alone. Specification (4) looks at two dynamic effects—persistence in decisions (States insured$_{t-1}$), and the effect of a loss in the last period (Loss$_{t-1}$). Subjects' decisions are persistent: about 45% of decision at $t-1$ contributes to decision at t.[16] Reactions to losses follow the expected pattern of reinforcement learning: subjects increase the number of states insured by 0.25 after experiencing a loss. In the absence of trends, this also implies a reduction in insurance by roughly the same amount following a period without a loss. This behavior may be moderated by whether the subject drew a no-loss event (a white cell) or an insured event (a colored cell), which we explore in detail below.

Finally, specifications (5) and (6) control for possible differences in dynamics between BlankNT and FullNT by including the interactions of persistence and reaction to losses with the treatment. Neither of the interactions is significant, implying that learning patterns are similar in the two treatments. Controls for uncertainty attitudes in (6) do not reveal strong effects, which is likely caused by all time-independent individual differences being subsumed by States insured$_{t-1}$.

As mentioned above, subjects tend to increase (respectively, reduce) insurance following periods with (respectively, without) a loss, see specification (4) in Table 2. A no-loss event can be of two types: a white cell is drawn or an insured colored cell is drawn. Boundedly-rational subjects may infer they have too much insurance in the former case, and the "right" amount of insurance in the latter, leading them to reduce the number of states insured after a white cell is drawn. Alternatively, subjects may believe in negative auto-correlation in luck, i.e., that having drawn a white (respectively, colored) cell at $t-1$ makes it more likely that a colored (respectively, white) cell will be drawn at t. In this case, subjects may decide to buy more insurance after a white cell is drawn. To verify these conjectures, we ran a regression similar to specification (4) where Loss$_{t-1}$ is replaced with Draw white$_{t-1}$—an indicator equal 1 if the subject drew a white cell at $t-1$—restricting the data to cases where no loss occurred at $t-1$. The coefficient estimate on Draw white$_{t-1}$ is positive and

[16]Similar persistence or inertia, consistent with the status quo bias, is observed by Agnew, Balduzzi and Sunden (2003) in the context of 401(k) portfolio choices, and by Besedeš et al. (2015) in a laboratory experiment studying choice overload.

significant (0.14, p-value = 0.05), indicating that subjects purchased more insurance after drawing a white cell relative to drawing an insured colored cell, which is consistent with beliefs in negative auto-correlation.

3.3 Decision Time and Defaults

The cognitive-effort channel of defaults would suggest that individuals who use the default to guide choices are doing so to actively reduce the cost of cognitive effort. As such, we would predict that active decision making is correlated with longer deliberation times. We explore whether this correlation exists both at a treatment level and in the analysis of individual decision making.

We note two important caveats in our measurement of decision time. First, in the endogenous-time treatment, 95% (99%) of decisions are made in under 45 (90) seconds, but there are a small number of decision times between 90 and 600 seconds. It is likely that many of these observations are from individuals who took a break from the experiment and treating these observations as active choices is hence problematic. To reduce the importance of these outliers, we use non-parametric analysis when comparing decision times across treatments and report specifications where these outliers are winsorized to 45 when correlating decision times with individual insurance decisions.[17]

Second, decision makers in the fixed-deliberation time treatments may change their insurance multiple times in a period. As a measure of active decision making, we use the last time in a period the Confirm button is clicked. Individuals who never clicked Confirm and receive the default option at the end of the period are assigned an active deliberation time of zero.[18] We captured this process data only in our additional sessions (48 subjects in the FullT treatment and 48 subjects in the BlankT treatment). As such, we report results based only on this sub-sample in this section.

Looking first at the difference in deliberation times across treatments, we predicted that the fixed-deliberation time treatments reduced the opportunity cost of deliberation time. As such, we predicted that deliberation time is longer in these treatments. The result is summarized as follows.

Result 4 *Consistent with a cognitive-effort channel, participants spend significantly more time making active decisions in the fixed-deliberation time treatments.*

[17]Results are similar if we use a log transformation of decision time to control for outliers or drop the three largest decision times (which all exceed 180 seconds).

[18]The decision maker does not make an active choice in 4.1% of observations in the fixed-deliberation treatment. On average, the confirmation button was clicked 1.61 times in this treatment and 85% of individuals click the confirm button one or two times.

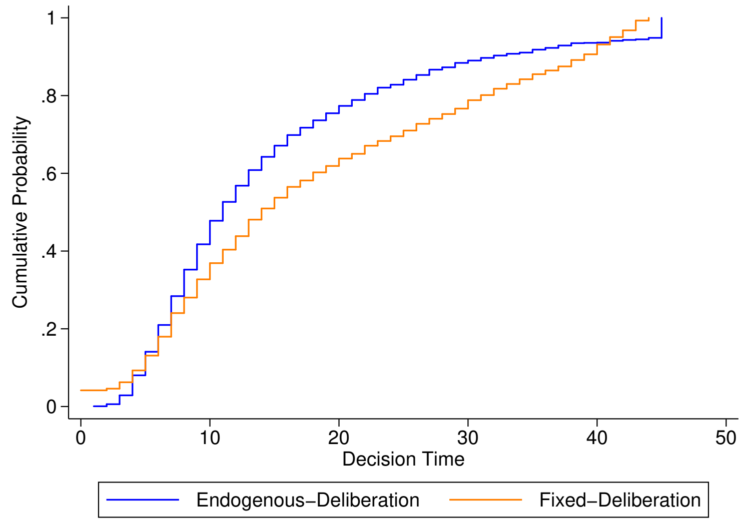

Figure 6: Empirical CDFs of decision times in the Endogenous-Deliberation treatments (blue) and the Fixed-Deliberation treatments (orange). Decision times greater than 45 seconds are winsorized to 45 seconds for clarity.

Support for Result 4 is provided in Figure 6, which shows the cumulative density functions of decision times in the two fixed-deliberation time treatments and the two endogenous-deliberation time treatments. As can be seen, deliberation times are longer in the fixed-deliberation time treatment at the median (11 vs. 14 seconds) as well as the 20th, 40th, 60th, and 80th percentile. The two distributions cross on the left hand side due to the 4.1% of observations where the decision maker does not make an active choice in the fixed-deliberation treatment; the distributions cross again on the right hand side due to decision times being truncated at 45 seconds in the fixed-deliberation treatment. Overall, the two distributions are significantly different in a Mann-Whitney test where an observation is the average deliberation time of an individual over all 12 decisions (p-value < 0.012).[19]

Having shown that deliberation times are indeed greater in the fixed-deliberation time treatment, one might wonder whether deliberation time influences decision making in similar ways in the two types of treatments. To explore this conjecture, we ran additional OLS regressions that extended specification (4) in Table 2 in the previous section by including an interaction term between deliberation time and the treatment. Specification (1) in Table

[19]The average decision time in the fixed-deliberation treatments is 18.1. This is not significantly different from the deliberation time of 16.5 in a simple linear regression with a dummy for the fixed-deliberation treatment if all observations in the endogenous-deliberation treatments are used (p-value $= 0.148$, errors clustered by subject). However, the difference is significant if we winsorize the decision time data in the endogenous-decision time treatments to 45 seconds (winsorized mean: 14.9; p-value < 0.001, errors clustered by subject).

3 shows this OLS regression for the endogenous-deliberation time treatments with decision times above 45 seconds winsorized to 45 seconds. Specification (2) in Table 3 shows the same regression for the fixed-deliberation time treatments.

States insured	Endogenous Deliberation (1)	Fixed Deliberation (2)
Full	0.262**	0.052
	(0.098)	(0.117)
(Decision Time)×Blank	0.017***	0.023***
	(0.006)	(0.005)
(Decision Time)×Full	−0.011**	−0.004
	(0.005)	(0.005)
States insured$_{t-1}$	0.455***	0.309***
	(0.057)	(0.053)
Loss$_{t-1}$	0.252***	0.161**
	(0.074)	(0.099)
Intercept	1.076***	1.52***
	(0.135)	(0.137)
Subjects	110	96
Periods	11	11
Observations	1,210	1,056
R^2	0.23	0.11

Table 3: Pooled OLS regressions using data from treatments BlankNT and FullNT in the endogenous-deliberation specification and from the follow-up sessions of BlankT and FullT in the fixed-deliberation specification. Decision time data winsorized at 45 in the endogenous-deliberation specification and demeaned by treatment in all regressions. Robust standard errors clustered by subject in parentheses. Significance levels: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

As can be seen in the regressions, longer decision times are predictive of decisions that move away from the default: in both the BlankNT and BlankT treatments, decision times are associated with a significant increase in the number of colors insured. Similarly, in the FullNT treatment, decision time is associated with a significant decrease in the number of colors insured. Decision time is also associated with a decrease in the number of colors insured in the FullT treatment, but the relationship is not significant.

Based on these patterns, we conclude the following.

Result 5 *Consistent with a cognitive-effort channel, longer decision times are associated with an increase in insurance in cases where no-insurance was the default and a decrease in insurance in cases where full insurance is the default.*

4 Discussion and Conclusion

We explored a cognitive-effort channel through which defaults might influence behavior in an insurance market setting where there is uncertainty in the benefits offered by different potential plans. We showed experimentally that defaults can strongly, and consequentially, influence purchasing behavior when participants can make decisions at their own pace. By contrast, we observed no significant impact of defaults in treatments where we fix the deliberation time. Our fixed deliberation manipulation was hypothesized to lower the opportunity cost of decision time and to influence decisions of individuals who find the cognitive costs of active decision-making prohibitively high. Our analysis of decision time is consistent with this cognitive-effort channel both at the treatment and individual decision level.

Our research opens up additional questions related to defaults and their impact on decision making. The cognitive-effort channel is an interesting one since it does not rely on trust and may continue to be relevant in environments where the choice architect is a self-interested entity whose incentives are potentially misaligned, such as a profit-maximizing firm. In principle, the welfare implications of defaults in these settings are ambiguous. In particular, defaults may be socially beneficial if the cognitive effort saved by consumers is greater than the distortions caused in the products selected. It is also an open question as to how defaults interact with vulnerable populations such as the poor or the old. Answering these questions is important for understanding the value of potential remedies, such as cooling-off periods or forced decision-making, which might improve decision-making but could, in principle, impose additional cognitive costs on the decision maker.

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A Experimental Instructions

Instructions (treatments with a fixed deliberation time)

Instructions (1 of 4)

Welcome and thank you for participating in today's experiment. Please turn off your phone now and put it away. Please do not talk during the experiment. If you have a question, please type it in the Chat box and send it only to the experimenter who will answer it.

Your earnings in this experiment will depend on your decisions, the decisions of others, and chance events. Understanding the instructions is likely to increase your earnings.

Earnings are private. You will be paid in cash at the end of the experiment. The exchange rate used in the experiment is **$1** for every **10 tokens**. There is a **$5.00 participation fee**. You will be using the computer for the entire experiment, and all interaction between you and others will be through computer terminals.

*Please use Alt+Tab to switch between the **"Instructions - A summary"** and the experiment screen. Please refer to it during the experiment as you see fit.*

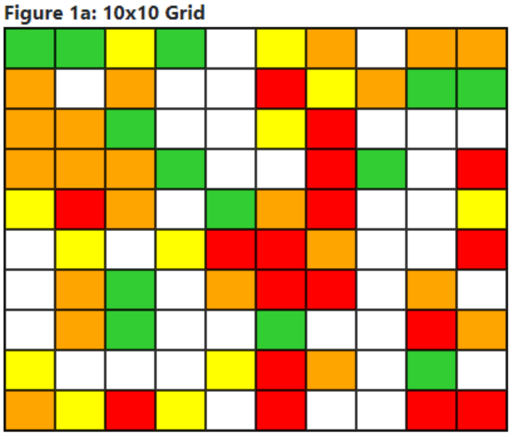
Next

Instructions (2 of 4)

The Choice Tasks and Payoffs: There are **1 trial round** and **12 decision rounds** in this experiment. In each round, you will be shown a 10x10 randomly generated grid like the one shown in **Figure 1a** below. Each of the 100 squares contained in the grid has been coloured one of **five** colours: white, red, orange, yellow or green. Each square is filled in independently using the same assignment process.

The grids differ from round to round so you will always want to look carefully at the grids being offered. The grid shown below is an example only.

Figure 1a: 10x10 Grid



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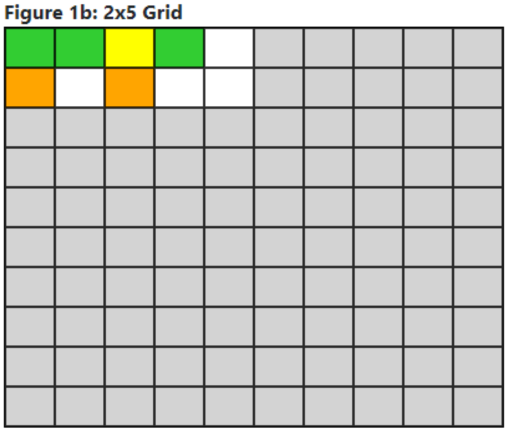
Next

Instructions (3 of 4)

Moreover, only a subset of the grid is revealed to you in each round.

Figure 1b: 2x5 Grid



For the trial round and each of the 12 decision rounds, you will be only shown a **2x5** portion of the grid as in **Figure 1b**.

Each colour represents an event that could result in a loss of **100 tokens** to you. You can choose to purchase an insurance contract to cover the loss. One of the **100 squares** in the grid will be randomly selected in each round, with all squares being equally likely. If the colour of the selected square (hidden or revealed) is non-white and not insured, you will lose **100 tokens**. If it is white or insured, you won't.

An example of an insurance contract is shown on the next page.

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Next

Instructions (4 of 4)

The contract might include pre-set options such as shown in the example contract below.

	Colour	Option			
					Modify
	Red	20	✓		Remove
	Orange	18	✓		Remove
	Yellow	16			Add
	Green	10			Add
	Total Price	**38**			

Confirm

You will be able to modify contracts to include more or less insurance by clicking on one or several of the Add or Remove buttons in the **Modify** column. Note that options can only be added or removed sequentially. That is, in the example above you will not be able to remove Red until you remove Orange, and you will not be able to add Green until you add Yellow. When you add or remove options, new buttons will appear. The **Total Price**, which is the sum of the prices of the insured colours, will be automatically updated in the decision rounds. In the current example, the **Total Price** is **38** because colour Red and Orange have been insured.

Once you are happy with your decision, hit the Confirm button to confirm your chosen level of coverage.

You will have **45** (60) **seconds** seconds to make your decision in the **decision** (trial) **rounds**. The time left will be indicated by a clock in the upper corner of the decision screen. The computer will not advance to the next round until these seconds have elapsed, so there is no need to rush your decision. That said, if you have not **Confirmed** your decision within the time limit, you will be defaulted into the previously confirmed level of coverage.

At the beginning of each round, you will be endowed with **200** tokens. Your payoff in a given round will be calculated as follows:
If the colour drawn is non-white and not insured:

$$\text{Payoff} = 200 - \text{loss} - \textbf{total price for the insurance, where } \mathbf{100} \textbf{ is the loss incurred}$$

If the colour drawn is white or insured:

$$\text{Payoff} = 200 - 0 - \textbf{total price for the insurance}$$

After you complete the **12** decision rounds, **one** round will be randomly chosen for payment. *Are there any questions?*

Previous Next

Note: in the endogenous-deliberation time treatments, the paragraph starting from "You will have 45(60) seconds" is removed. Everything else stays the same.

B Comparison of Instruction Variants in the Fixed Deliberation Time Treatments

The BlankT and FullT sessions include two different sets of instructions that varied in the way we described this confirmation process. In the original instructions (used for the first 48 subjects in BlankT and the first 44 subjects in BlankNT), we stated

> "You will have **45** (60) seconds to make your decision in the **decision** (trial) **rounds**. The computer will not advance in the next round until these seconds have elapsed, so there is no need to rush your decisions. That said, if you have not **Confirmed** your decision within the time limit, you will be defaulted into the previously confirmed level of coverage."

There was some concern that the words "defaulted" and "no need to rush your decision" could prime participants in these sessions. As such, we ran new sessions that avoided potential priming language and instead stated:

> "You will have **45** (60) seconds to make your decision in the **decision** (trial) **rounds**. The computer will not advance to the next round until these seconds have elapsed.
>
> Note that changes to your insurance policy will only be recorded when you hit the confirm button and your policy does not automatically update at the end of the round. Thus, if you wish to adjust your policy, you need to use the add or remove buttons to change the policy and then press the confirm button."

We ran both non-parametric tests and parametric tests to determine whether there were any observable differences in the samples prior to pooling the data for the main analysis. Figure 7 below shows the cumulative density functions for each of the fixed-deliberation treatments with the data divided into the sample that uses the original instructions and the sample that uses the new instructions. It is the basis for our non-parametric analysis: there is no statistically significant difference in the two samples using a Mann-Whitney test on the data from the two BlankT subsamples (p-value = 0.472) nor when using the data from the two FullT subsamples (p-value = 0.608).

There is also no significant difference in the samples using parametric tests where we regress a dummy for the new instructions on (i) the number of insured options (BlankT: p-value = 0.457; FullT: p-value = 0.532) or (ii) a binary variable that is one if the default is chosen and zero otherwise (BlankT: p-value = 0.361; FullT: p-value = 0.077). As with the rest of the paper, these parametric tests use errors clustered at the individual level.

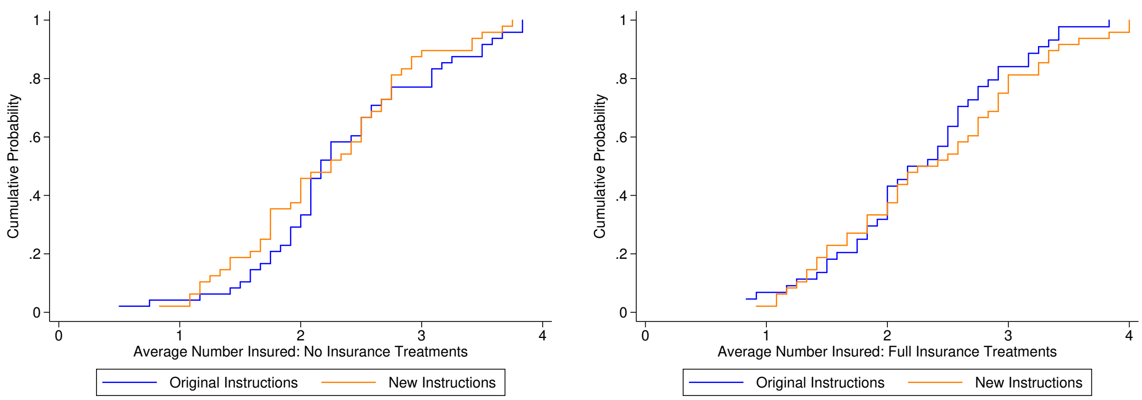

Figure 7: Empirical CDFs of the average number of states insured by subject in the no-insurance default treatments (left) and full-insurance default treatments (right) using the original and new instructions.