Defaults with Misaligned Incentives: The Role of Cognitive Effort*

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Abstract

We explore experimentally a cognitive-effort channel through which defaults might influence behavior in an environment where the choice architect has misaligned incentives. Our experimental setting is an insurance market where the firm is better informed about the aggregate statistical risk associated with potential buyers of a policy than the buyers themselves. Since buyers' perceived risk and actuarially fair risk differ, the firm has incentives to exploit buyers' informational disadvantage. We find that defaults can strongly influence purchasing behavior in this environment. Further, by using a decision-time manipulation that lowers the opportunity cost of decision-making time, we provide channel-specific evidence 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

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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

Defaults have been used extensively by Behavioral Economics programmes to nudge individuals into socially beneficial actions such as increasing their retirement savings, joining organ donation lists, and giving blood. They are one of the primary tools of Behavioral Insight Units worldwide (Sanders, Snijders and Hallsworth, 2018; DellaVigna and Linos, 2021). 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). Indeed, setting a default option (i) does not restrict the choices of consumers who are active in their decision making process but (ii) might improve the choices made by individuals who are overwhelmed by the choice process and who make bad (or no) decisions as a result.

While defaults can be socially beneficial, there is a potential dark side to them when the interests of the choice architect and consumers are misaligned. Such misalignment of incentives can arise if the choice architect is a profit-maximizing firm or when it is a not-for-profit firm or government department that has objectives beyond the consumers' best interests.[1] Defaults in these settings are a concern because they might lead to socially inefficient outcomes or could be used to exploit the more vulnerable portions of the population. Often, these are 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). Defaults are also a concern because they are likely to be used in markets where decision makers feel unequipped to make decisions and the consequences of their choices may be significant, e.g., in private insurance, superannuation, mortgages, investment funds, electricity, and telephony (DellaVigna, 2009).

We explore how decision makers respond to defaults in an environment with misaligned incentives and seeks to identify a channel by which these defaults operate. Despite defaults

[1] Gigerenzer (2015) provides a number of examples of potentially misaligned incentives in the health-care environment related to screening for breast and prostate cancer (Gøtzsche and Jørgensen, 2013; Woloshin and Schwartz, 2012; Gigerenzer, Mata and Frank, 2009). Dobrescu et al. (2016) show that default settings (e.g., plan type, contribution levels, and investment allocations) strongly influence wealth accumulation in a superannuation setting. In the same setting, Deetlefs et al. (2018) shows that movement away from the default is influenced both by interest in financial decision making and trust in the company.

being used ubiquitously by firms selling complex goods, there is surprisingly little exploration of how they shape behavior in such environments.[2] Specifically, we study the effect of defaults in an insurance-market context where the firm is better informed about aggregate statistical risks associated with the potential buyer of a policy than the buyer herself.[3] Since buyers' perceived risk and actuarially fair risk differ, insurance companies have incentives to offer contracts that might exploit buyers' informational disadvantage.

In our experiment, subjects face a series of individual decision problems where they are asked to choose an insurance contract from a menu of available options. 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. 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. To induce an asymmetry between the participants' information and that of the computerized firm, participants are only shown a random subsample of 10 states, and the remaining 90 states are hidden.[4] The subsample allows the participant to estimate the expected frequency of each color without bias, but introduces systematic errors in the perceived risk of different potential states that can be profitably reflected in the premiums associated with each insurance contract. Due to sampling error, this setup implies that the firms' expected profits from participants choosing various contracts differ based on the underlying true distribution. Thus, our environment is one where the firm may have an incentive to nudge participants into different insurance contracts based on the differences between the true and sampled risks.

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 behav-

[2]As discussed below, the channels by which defaults operate in environments with misaligned incentives may differ from those where the default is set by a benevolent planner.

[3]The assumption that the firm has more aggregate information than consumers is based on the medical, health, and insurance literatures that find that individuals have strong misperceptions of potential risks and that these misperceptions can differ with observable characteristics (Ridde and Hales, 2018; Solomon, 2021), geographic area (Ropeik, 2010; Pennycook et al., 2021), or social group (Lee, Choi and Britt, 2021). We abstract away from common issues in the insurance market such as adverse selection and moral hazard to concentrate directly on firm-based exploitation of superior aggregate information. For a discussion of how adverse selection and moral hazard interact with belief misperceptions, see Spinnewijn (2013) and the subsequent literature.

[4]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.

ior 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.

A working hypothesis is that defaults operate in the insurance market by allowing the firm to extract rents from individuals who find the cognitive costs of active decision making prohibitively high. This form of passive decision making 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), but is a channel where there is limited direct empirical support (Jachimowicz et al., 2019). In the second dimension of the design, we explore this 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 a possibility to move 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 decision time manipulation contrasts *endogenous choice* of decision time in the baseline *endogeneous-deliberation time* treatments with *time delay* in the fixed-deliberation time treatments.[5] 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 Daw, 2019). To fix ideas, suppose an individual spends time t on choice from a set of available options and $r(t)$ is the corresponding expected payoff benefit from the resulting choice.[6] If c is the individual's (constant) marginal opportunity cost of time, the endogenous decision time, $t^*(c)$, solves $\max_{t\geq 0}[r(t)-ct]$, and is a decreasing function of c. When c is large enough, $t^*(c) = 0$, which in our setting would correspond to the individual simply following the default. 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.[7]

We, therefore, predicted that the introduction of a (generous enough) timer would cause

[5] 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).

[6] Function $r(t)$ can be microfounded, for example, via a search model.

[7] It may happen that in a particular decision problem, by chance, the default turns out to be a globally optimal choice. As long as it does not happen all the time, it does not invalidate our argument in the aggregate. It is also possible that in the fixed-deliberation time treatments subjects do not spend the entire allotted time T on active decision making. However, it is still rather plausible that at least some fraction of subjects deliberate longer than their $t^*(c)$ in these treatments.

the default to be less important for decision making. On average, our subjects spend about 16 seconds on decisions in the endogenous-deliberation time treatments—substantially less than $T = 45$ seconds in the fixed-deliberation time treatments. 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.

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; 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 results highlight the potential for defaults to be used in an exploitative way by firms and provide evidence that they operate (in part) by allowing some individuals to economize on cognitive effort. In previous work, Smith, Goldstein and Johnston (2013) identify endorsements, endowment effects, and cognitive effort as potential channels upon which defaults can operate across settings. However, in an extensive meta-analsysis, Jachimowicz et al. (2019), casting a wide net across many domains, find evidence only for the endorsement and endowment effects.[9] 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 in the way that defaults are used by firms and their impact on social welfare.

The rest of our paper is organized as follows. In Section 2 we provide a framework for the insurance market where competitive firms have an incentive to nudge less informed participants into different types of insurance contracts. Our experimental design is discussed

[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] Jachimowicz et al. (2019) use the term "ease" to describe the cognitive effort channel and use reaction time to evaluate this channel.

in Sections 3 while the results are provided in Section 4. We further discuss the results and conclude in Section 5.

2 The decision environment

Our experimental design explores defaults in the following decision environment. A decision maker (DM) faces a lottery with $K+1$ possible states, indexed by $k \in \mathcal{K} = \{0, 1, \ldots, K\}$, with the corresponding probabilities $q = (q_0, q_1, \ldots, q_K)$. The decision maker incurs no loss if state 0 is realized, but incurs a loss of $L > 0$ if any other state is realized.

Before the lottery is realized, an insurance agent (seller) offers a menu of simple insurance contracts C_k ($k \in \mathcal{K}$) structured as follows: contract C_0 provides no insurance, and is priced at zero; each contract C_k fully insures the DM against losses in states $1, \ldots, k$ and is priced at p_k. Thus, C_K provides full insurance, while contracts C_k with $1 \leq k \leq K-1$ provide partial insurance, with coverage increasing in k.

We assume that the seller knows the true probabilities q and operates as an intermediary with access to a competitive market where insurance is available at actuarially fair prices. That is, the seller's cost of contract C_k is its actuarially fair price $c_k = L \sum_{i=1}^{k} q_i$. Therefore, if the DM buys contract C_k, the seller's profit is $\Pi_k = p_k - c_k$.

The DM is not informed about the true probabilities q but instead observes an unbiased signal $\hat{q} = (\hat{q}_0, \hat{q}_1, \ldots, \hat{q}_K)$ about q. Signal $\hat{q}$ is constructed as follows. First, a random sample $(X_1, \ldots, X_n)$ of size n is generated, where each element X_i takes values in $\mathcal{K}$ with probabilities q. Second, a sub-sample $(X_1, \ldots, X_m)$ of size $m \leq n$ is used to generate $\hat{q}_k = \frac{\#\{X_i \colon 1 \leq i \leq m,\, X_i = k\}}{m}$. In other words, $\hat{q}_k$ is the empirical frequency of state k in the sub-sample. This signal is unbiased but becomes increasingly noisy as m decreases.

The seller also observes $\hat{q}$ and structures prices p_k as follows. First, the seller re-orders states $1, \ldots, K$ according to their realized probabilities $\hat{q}$, such that $\hat{q}_1 \geq \ldots \geq \hat{q}_K$. Second, each state $k = 1, \ldots, K$ is assigned a price $a_k = \max\{\hat{q}_k L, f\}$, where $f \geq 0$ is a floor price. Third, contracts C_k are priced at $p_k = \sum_{i=1}^{k} a_i$.

These prices allow the seller to extract rents from the buyer in two ways. First, the K risky 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 cumulatively—that is, it is possible to insure state k only after states $1, \ldots, k-1$ have been insured—any contract other than full insurance (C_K) and no insurance (C_0) will be overpriced, in expectation, even when $f = 0$.

	k				
	0	1	2	3	4
π_k	137.0	125.4	122.8	124.1	125.4
Π_k	0	11.5	14.2	12.9	11.6
$\mathbb{P}(k = \arg\max_i \Pi_i)$	0.04	0.26	0.37	0.20	0.13

Table 1: Average DM's payoff, seller's profit, and probability that contract C_k is profit-maximizing for the seller, obtained via simulations of 10,000 samples.

Second, the presence of a positive floor price makes states with low realizations of $\hat{q}_k$ overpriced relative to actuarially fair prices $\hat{q}_k L$. The floor price ensures that the expected price of full insurance is above the actuarially fair amount.[10]

In the experiment, we use $K = 4$ and probabilities $q = (0.37, 0.18, 0.18, 0.14, 0.13)$. The full sample size is $n = 100$, and the size of the sub-sample is $m = 10$. The DM starts with an initial endowment $E = 200$ and incurs a loss of $L = 100$ in all states $k > 0$ that are not insured. The floor price is $f = 12$. We conducted simulations to calculate the expected payoff of the DM, π_k, and the expected profit of the seller, Π_k, from each contract C_k. We also calculated the proportion of times each contract maximizes the seller's profit. The results from these simulations are shown in Table 1.

Although the DM does not observe the data generating process, Table 1 shows that the DM's expected payoff for buying insurance is actually worse on average under partial insurance contract C_2 than when buying full insurance. This is due to the order statistic issue being particularly severe when one and two categories of insurance are purchased.

Table 1 also reveals that the seller prefers the DM to select contract C_2 on average, but can do better if it can induce different defaults across different decision problems. In particular, the seller would prefer the buyer to select full insurance in 13% of decision problems, and partial insurance contract C_2 in 37% of decision problems. Thus, the environment is one where it is natural for the seller to want to use defaults to encourage the DM to select different amounts of insurance.

3 Experimental design

The experiment consists of 12 computerized choice tasks in which the participant chooses between potential insurance contracts that insure against losses in different states of the world.

[10] In real insurance markets, this is likely to be the case due to the existence of adverse selection and moral hazard. In our setting, we use f to ensure that not all risk averse participants prefer full insurance.

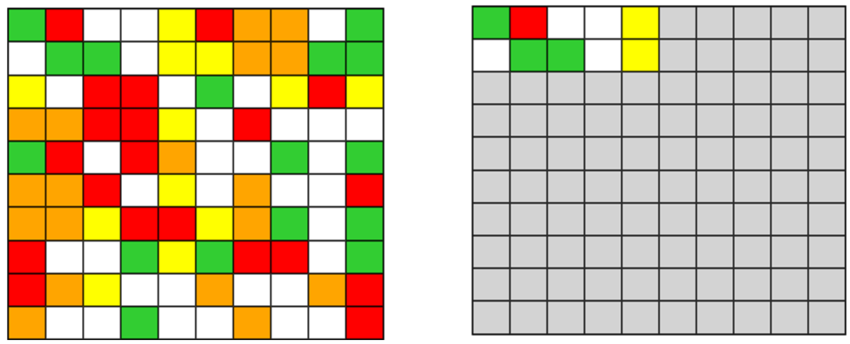

Figure 1: *Left*: A 10×10 grid representing a full sample. *Right*: The same grid with 10 cells revealed, representing a sub-sample.

Each choice task begins 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 is color coded and corresponds to one of five potential states. White squares are the most frequent and represents states without loss ($k = 0$). The remaining four colors—red, orange, yellow, and green–represent states where the DM incurs a loss if the state is drawn and the color is not insured.

We generate the state grids as follows: Each square in the grid is first independently assigned a state $k \in \{0, 1, \ldots, 4\}$ via a random draw with the respective probabilities $\{0.37, 0.18, 0.18, 0.14, 0.13\}$.[11] Squares that are assigned state 0 are colored white. Squares that are assigned one of the other four states are assigned colors using a random permutation of {red, orange, yellow, green}. The permutation implies that the most frequent non-white color in one task is 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.[12]

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 $m = 10$ 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

[11] We assign a state to each square independently, as opposed to generating a grid with color frequencies that exactly match probabilities q, in order to ensure that the elements of the sub-sample have the same probabilities *ex ante* as the elements of the full sample.

[12] Within the pre-drawn set, the firm's profit is maximized if the DM selects full insurance (C_4) in 6 decision problems and is maximized if the DM selects partial insurance contract C_2 in the other 6 decision problems. Thus our setup is one where the optimal default differs across decision problems. As discussed below, we are primarily interested in identifying a behavioral response to defaults and to identify the channel by which it operates. As such, we use full insurance (C_4) and blank insurance (C_0) as the defaults to maximize power.

the block of squares that are revealed.

To generate the set of insurance contracts, we ranked the colors based on the observed frequency of occurrence 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 are priced in a two step process. For each color, we calculate a naive expected event frequency $\hat{q}_i$ by multiplying the number of observed occurrences of each state by $n/m = 10$. We next set the price of insuring color i to $a_i = \max\{12, L\hat{q}_i\}$. The price of contract $k = 1, \ldots, 4$ is equal to the sum of the prices of its insured colors: $p_k = \sum_{i=1}^{k} a_i$.

For example, suppose a participant observes 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 will 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 whose estimated actuarially fair prices—10 and 0, respectively—fall below the floor price of 12.

Participants are offered a *default* insurance contract that they can purchase or modify. Participants can 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 are 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 is never shown.

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

The main part of the experiment is followed by three tasks where subjects' risk aversion, loss aversion and ambiguity aversion are elicited using list methods. During each task, subjects are 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 will have a unique point at which they are willing to switch from the draw to the sure amount. In the risk task, the lottery $(0, \$2.00; 0.5, 0.5)$, and the sure amounts of money increase from zero to \$2.00, in 10 cent increments. In the loss task, the lotteries are $(-\$x, \$2.00; 0.5, 0.5)$, where x changes from 0 to 2.00 in 10 cent increments, and the sure amount of money is always

0. Finally, in the ambiguity task the lottery is $(0, \$2.00; p, 1-p)$, where, unbeknownst to subjects, p is generated randomly from the uniform distribution on $[0,1]$, and the sure amounts are the same as in the risk task. The three tasks are presented to subjects in a random order, without feedback, and one of them is randomly selected for actual payment.

3.1 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).[13] We abbreviate the resulting four treatments as BlankNT, FullNT, BlankT and FullT.

3.2 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.[14]

We ran 14 sessions of the experiment proper 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 202 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 2.

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.

[13]In the fixed deliberation time treatment, individuals still must actively lock in their choice in a round by pressing the Confirm button. However, making a choice does not end the round and individuals can revise and update their choice as often as they like in the 45 second deliberation window. Thus, the deliberation time is fixed, but decision time within the deliberation window may be endogenous.

[14]Our actual samples ended up being slightly smaller than 60 due to low attendance in our last three planned sessions as a result of changing COVID-19 conditions. We did not add additional sessions due to concerns of potential changes in the composition of the subject pool post outbreak.

Treatments	Sessions	Subjects per session	Total
BlankNT	4	16,11,16,19	62
FullNT	4	13,18,10,7	48
BlankT	4	12,14,9,13	48
FullT	2	13,31	44
Total	14		202

Table 2: Treatments, sessions, and the number of subjects.

3.3 Hypotheses

Our 2×2 design is designed 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, there will be less insurance 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 has identified endorsement, endowment effects, and cognitive effort as channels by which defaults operate when set by benevolent choice architects. Our working hypothesis is that in an environment with misaligned incentives the endorsement channel is less important, and that the cognitive-effort channel is likely to be a primary channel by which defaults operate. To test for this, 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 fixed. 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 would thus predict the following.

Hypothesis 2 *Behavior in the endogenous deliberation time treatment 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.

We note that although we have strong predictions between the endogeneous deliberation time treatment 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.

4 Results

4.1 Treatment Comparisons

Result 1 *Consistent with Hypothesis 1, there is significantly more insurance chosen in the treatment with full insurance default and endogenous deliberation times (FullNT) compared to the treatment with no insurance default and endogenous deliberation times (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 ($p = 0.002$, the Wald test with clustering at the subject level; $p = 0.002$, 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, $p = 0.007$), and significantly more instances of four states insured in FullNT (11 p.p. difference, $p = 0.045$). The difference is only marginally significant for one state (5.5 p.p., $p = 0.062$) and not significant for two and three states.

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

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*

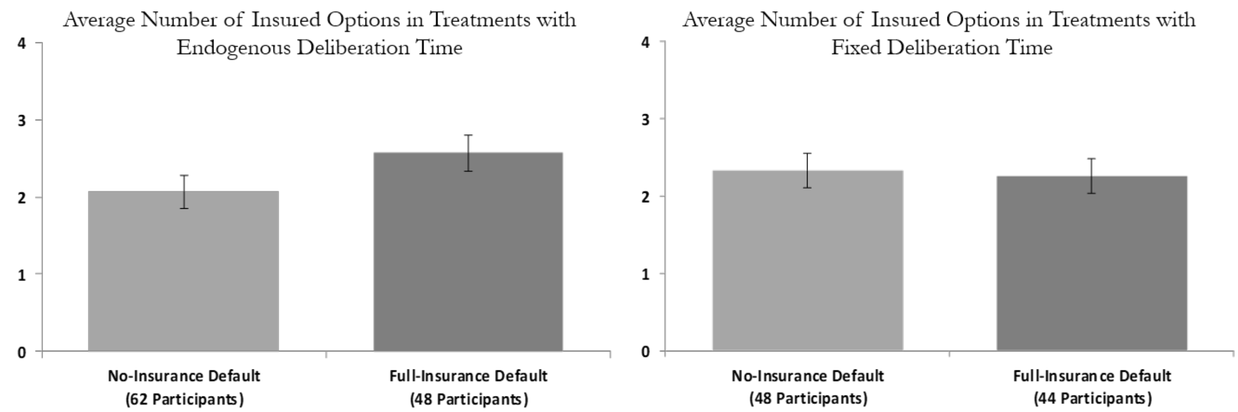

Figure 2: Average number of items insured by treatment. Error bars are 95% confidence intervals clustered at the individual level.

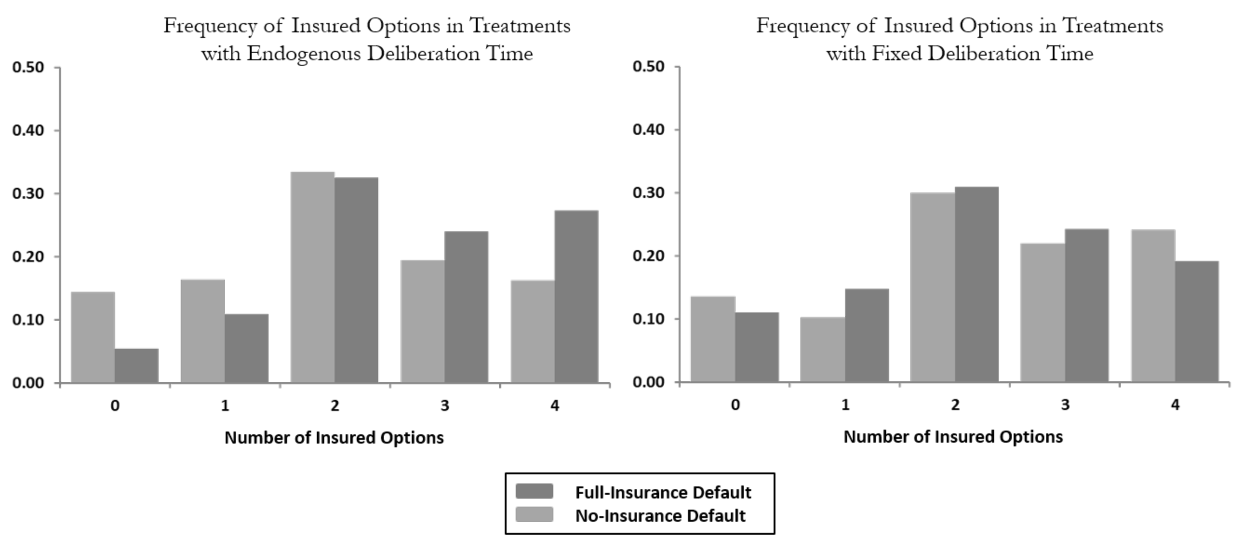

Figure 3: Histograms of the number of items insured by treatment.

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 above, 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 hand panel, the average number of states insured in FullT is 2.26 while the average number of states insured in FullNT is 2.33. Thus, the difference in these treatments is -0.07 categories. The difference-in-difference estimate of 0.57 $(0.5 - (-0.07)))$ is significant in a simple linear 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 ($p = 0.010$, errors clustered at the individual level).

A further comparison of the treatments with a fixed deliberation time suggests that there is no differences 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 ($p = 0.641$, the Wald test; $p = 0.725$, the Mann-Whitney test). Likewise, there is no significant difference in the proportion of cases where no insurance is chosen ($p = 0.452$) nor in the proportion of cases where full insurance is chosen ($p = 0.313$).

Figure 4 shows how the average numbers of states insured varied over time. There are no obvious time trends, which is confirmed by linear regressions of the number of states insured on the period number producing $p = 0.289$, 0.959, 0.139 and 0.723 in BlankNT, FullNT, BlankT and FullT treatments, respectively. Subjects consistently insured more states in FullNT as compared to BlankNT, while there is no consistent ordering of states in the BlankNT and FullNT treatments.

4.2 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 ($p = 0.046$ 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

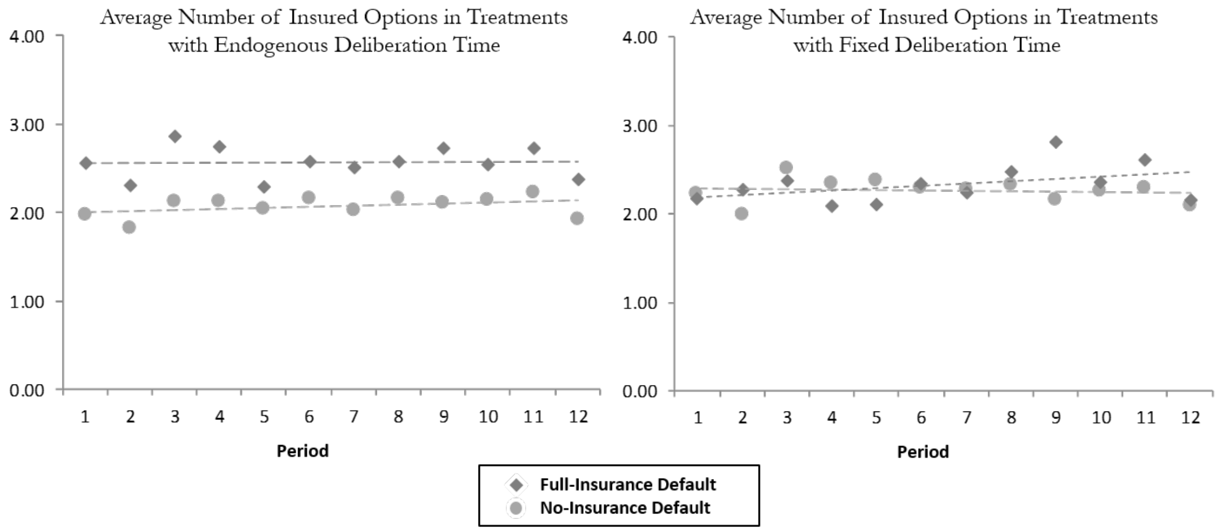

Figure 4: Average number of items insured by treatment over time.

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%) insured zero states in 6 periods or more.[15] As seen in the right panel, only 12 subjects (25%) insured all states in 6 periods or more. Thus, while the default influenced decision making, it does not appear to have fully eliminated the sensitivity of modification of insurance strategies to either insurance prices or outcomes.

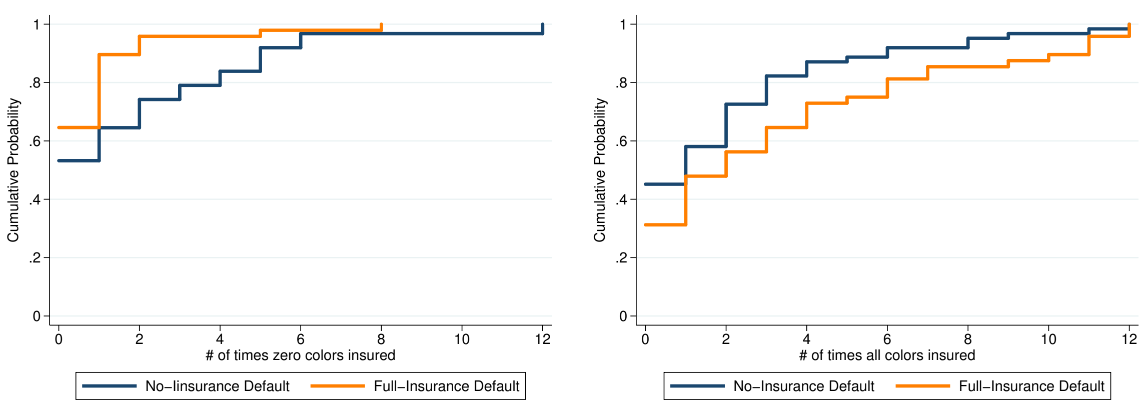

Figure 5: Empirical CDFs of the number of times a subject insured zero states (left) and all four states (right), in the treatments with endogenous deliberation time.

[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$_{t-1}\times$FullNT					0.024	0.012
					(0.119)	(0.117)
Loss$_{t-1}$				0.25***	0.21**	0.22**
				(0.07)	(0.09)	(0.10)
Loss$_{t-1}\times$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			-0.0069
		(0.024)	(0.019)			(0.0113)
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 3: Pooled OLS regressions using data from treatments BlankNT and FullNT, robust standard errors clustered by subject in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 3. 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 to uncertainty—risk-aversion (RA), loss-aversion (LA) and ambiguity-aversion (AA). Each of the measures is constructed as explained in Section 3. 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. 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 3. A no-loss event can be of two types: a white cell is drawn or an insured colored cell is drawn. A boundedly rational subject may infer she has too much insurance in the former case, and the "right" amount of insurance in the latter, leading her to reduce the number of states insured after a white cell is drawn. Alternatively, a subject 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 significant (0.16, $p = 0.028$), 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.

Finally, we analyze the amount of time subjects spent on decisions in the endogenous-deliberation-time treatments. On average, subjects spent about 16 seconds per decision screen, with virtually no difference between treatments (16.69 sec in BlankNT, 16.35 sec in FullNT). Moreover, in 48% of cases subjects spent 10 seconds or less. This is a drastic reduction compared to the 45 seconds subjects had to spend on decisions in the treatments with timer.

As expected, the number of states insured is positively correlated with decision time in BlankNT, and negatively correlated with decision time in FullNT, although these effects are small and marginally significant at best ($p = 0.074$ and 0.427, respectively). We also do not find a significantly negative association between decision time and the likelihood of accepting the default. Importantly, these effects cannot be interpreted as causal since decision times are endogenous and chosen simultaneously with insurance. Part of the correlation can be due to a purely mechanical effect: In order to modify the default contract, subjects need to spend time adding options in Blank and removing options in Full.

5 Discussion and Conclusion

We explored the potential of defaults that are designed to benefit the choice architect rather than the target of the choice architecture. In an insurance-market context where the firm wishes to exploit its informational advantages, we showed experimentally that defaults can strongly influence purchasing behavior. By using a decision-time manipulation that lowers the opportunity cost of time, we provided channel-specific evidence that defaults operate by influencing decisions of individuals who find the cognitive costs of active decision-making prohibitively high.

Our results suggest that defaults may operate differently in environments with misaligned incentives than they do in environments where the choice architect is known to be benevolent. While Smith, Goldstein and Johnston (2013) identify implied endorsements, endowment effects, and cognitive effort as possible drivers of the acceptance of defaults, Jachimowicz et al. (2019) find limited direct evidence that the cognitive-effort channel drives defaults. In our paper, we find that cognitive effort is important to the establishment of defaults and find no default effect when the opportunity cost of time is strongly reduced. As such, we provide evidence of the cognitive-effort channel for defaults in an environment with misaligned incentives.

Our research opens up additional questions related to defaults and their impact on consumers. In principle, a firm that extracts rents in exchange for reducing the cognitive costs may or may not be exploitative. For instance, defaults may be socially beneficial if the cognitive effort saved by consumers is greater than the distortions caused in the products selected. In our setting, the preferences of the firm directly relate to rents that can be gained from informational biases, which would suggest that defaults are likely exploitative. However, in other settings where both cognitive and tangible search costs exist, it is an open question as to how defaults influence social welfare. 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.*

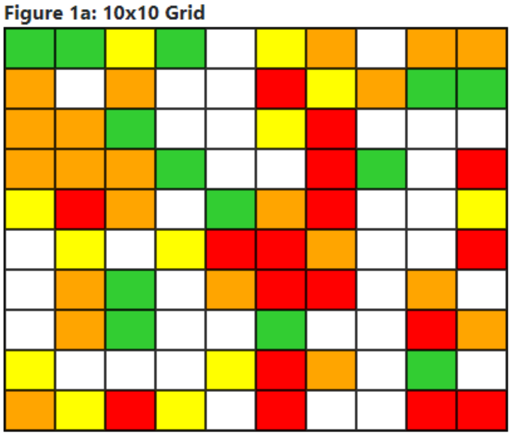
Next

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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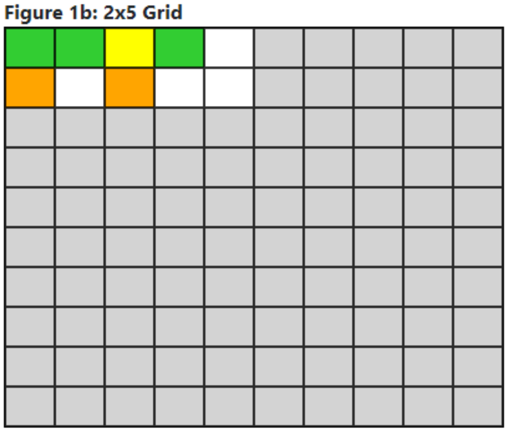
Instructions (3 of 4)

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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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:

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

If the colour drawn is white or insured:

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

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

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