Robo-Advice for Household Debt Repayment

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Abstract

Debt repayment is an ideal application for robo-advice because it depends on neither risk preferences nor beliefs. In a pre-registered RCT with a representative UK sample observed before, during, and after robo-advice is removed, we start by verifying that it improves repayment choices, especially for less sophisticated consumers. Willingness to pay exceeds expected monetary benefits, perhaps by eliminating the psychological costs of solving debt repayment problems. Non-adopters report less trust in algorithms. Bundling robo-advice with education reduces reliance on suboptimal repayment heuristics after removal, yet controls improve, suggesting learning-by-doing that automation may limit.

JEL Codes: D14, G51, G53.

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The origins and consequences of growing wealth inequality are among the most debated issues in today's society (Keister (2000); Zucman (2019)). A large literature links the lack of wealth accumulation by low-income households to a combination of low saving rates and low financial returns (Campbell, Ramadorai, and Ranish (2019)). Differences in wealth accumulation also depend, however, on how much debt households accumulate and how (in)efficiently they manage it (Bogan (2022)). The likelihood of suboptimal debt management is especially high for households with lower levels of income and financial literacy (Kaiser et al. (2022), Jørring (2024), Lusardi, Schneider, and Tufano (2011), Drexler, Fischer, and Schoar (2014)), who are less likely to understand the implications of annual percentage rates (APRs), minimum payments, and late fees (Agarwal and Mazumder (2013); Keys and Wang (2019); Kuchler and Pagel (2021)), and more likely to lack access to cost-effective financial advice (Lusardi and Mitchell (2014), Chalmers and Reuter (2020)). Reliance on heuristics and biases in debt repayment can lead to debt spirals, where debt piles up, further reducing wealth (Agarwal et al. (2008); Agarwal and Zhang (2015); Argyle, Nadauld, and Palmer (2020); Gathergood et al. (2019); Ponce, Seira, and Zamarripa (2017); Stango and Zinman (2023); Zinman (2015)).

Whereas the amount of debt that households decide to accumulate depends on their preferences and beliefs, optimal debt repayment strategies do not, which makes debt repayment a promising application for robo-advice (D'Acunto, Prabhala, and Rossi (2019); D'Acunto and Rossi (2021b); Olafsson and Pagel (2018); Wang (2024)). In contrast to debt counseling and other forms of in-person advice, robo-advising is an easily scalable technology that can be delivered at low cost through smartphones and other personal devices, including dedicated applications and generative AI, making it viable to consumers regardless of income or financial literacy (Philippon (2016), Reher and Sokolinski (2024), Reher and Sun (2019), D'Acunto and Rossi (2021a, 2023), Bianchi and Brière (2026)). At the same time, algorithmic aversion, which is especially common among the least sophisticated consumers (Niszczota and Kaszás (2020)), might limit demand for robo-advice by those that would most benefit from it. Reliance on robo-advice might also eliminate learning-by-doing, thereby increasing consumers' vulnerability when faced with financial decisions for which robo-advice is unavailable.[2]

Tackling questions about the value of robo-advice for debt repayment using observational data would require information on the individual-level usage and outcomes of robo-advice tools for debt repayment, about whose availability we are not aware,

[2]Recent studies of the potential complementarity and substitutability of financial education programs and learning-by-doing deliver results showing either no complementarity or effects of learning-by-doing or complementary effects. For instance, see Oberrauch and Kaiser (2024) and Bu et al. (2022).

plus plausibly exogenous variation in access to robo-advice. Therefore, to study how different types of consumers adopt robo-advice for debt repayment, how its perceived value relates to its expected monetary value, and whether consumers learn about optimal debt repayment strategies from exposure to robo-advice, we conduct a laboratory-in-the-field experiment on a representative sample in the United Kingdom.[3]

In our experiment, subjects allocate pre-specified budgets across debt accounts in nine different repayment scenarios. Each scenario includes two, three, or four loans, which vary with respect to interest rates and balances. In the medium and hard versions, some loans include minimum payments and late fees. The experiment consists of three phases, each featuring three randomly chosen scenarios. In the *pre-intervention* phase, we assess the mistakes that subjects make when managing multiple debt accounts unassisted. This phase establishes our benchmark by confirming that, in our setting as in prior work, loan repayment mistakes are common and can have sizable economic consequences for debtors' wealth: consumers fail to minimize interest and late fees in 68.5% of the trials they tackle unassisted. The average percentage of savings forgone relative to the optimal choices is 21.9%, which translates into 29 basis points of interest and fees per month and compounds to 3.5% higher payments per year. For the average US family with $6,270 in credit card debt around the time of our experiment,[4] mistakes of this magnitude would result in an overpayment of $222.42 per year.[5] The estimates increase to 37 basis points per month and $285.39 per year when we focus on the hardest versions of the problems. Adapting Gathergood et al. (2019)'s classification of repayment strategies to our setting, we find that, among those making mistakes, dividing payments equally across loans ("1/N") is slightly more popular than making payments proportional to loan balances ("balance matching"). Consistent with earlier research (e.g., Lusardi (2012)), higher financial literacy and greater numeracy are associated with fewer and less costly mistakes.

In the *intervention* phase, we introduce robo-advice for debt repayment. Subjects solve three more debt repayment scenarios after being randomly assigned to one of five experimental arms: a control group for whom the task is identical to the pre-intervention

[3]The trial was preregistered in the AEA RCT registry (AEARCTR-0006447) and implemented by the Financial Conduct Authority (FCA) of the United Kingdom.

[4]Source: Federal Reserve Survey of Consumer Finances (Link).

[5]In Gathergood et al. (2019), who study reliance on debt-repayment heuristics within a similar UK population, average annualized interest savings from optimizing credit card repayments range from 64.82 pounds for those with 2 cards to 247.65 pounds for those with 5 cards (see their Table 2 Panel B). Because their sample only includes individual-months in which all minimum payments are made, they focus solely on possible interest savings. In contrast, the medium and hard versions of our experiments introduce minimum payments and late fees, which may push subjects away from balance matching.

phase; two groups for whom robo-advice is available for free, one with and one without explanations of the financial principles behind optimal strategies ("robo-advice bundled with education"); and two groups for whom robo-advice can be obtained for a fee, one bundled with education and one without education. In all four treatment arms, subjects are told the maximum interest and fees they could save using the robo-advisor. In the treatments with paid robo-advice, we elicit subjects' willingness to pay (WTP) for the robo-advice tool in an incentive-compatible way.[6] The *post-intervention* phase mirrors the pre-intervention phase, in which subjects solve three debt repayment problems of varying difficulty levels without access to robo-advice. At no point do subjects receive feedback on how their proposed allocations, which are used to determine incentive payments, compare to the optimal allocations.

Apart from the starting point of whether and by how much exposure to robo-advice improves loan repayment, the five experimental arms allow us to investigate a series of economic questions related to automated advice: (i) which observables help explain the (un)willingness to adopt robo-advice; (ii) how much consumers value robo-advice relative to the expected monetary savings; (iii) which characteristics relate to compliance with the robo-advisor's recommendations, and (iv) whether consumers learn from exposure to robo-advice, allowing them to implement optimal repayment strategies when the tool is no longer available to them.

Stressing what robo-advice means in our setting is important, because the same term often refers to different applications (D'Acunto and Rossi, 2023). Our robo-advisor is an automated tool that knows the features of a consumer's debt accounts—balances, interest rates, minimum payments, and late fees—and returns the repayment allocation that minimizes interest and fees. This design aims to mimic field applications whereby consumers share access to their accounts with robo-advisors, which automatically retrieve the relevant data.[7] This design isolates the margin on which field evidence documents costly consumer mistakes—the translation of known account terms into repayment allocations (Gathergood et al. (2019); Keys and Wang (2019))—from the separate frictions

[6]As we describe in Section *1.1.*, the fee was drawn randomly from a uniform distribution based on the maximum potential savings from relying on the robo-advice tool after subjects were explained the procedure and provided their WTP. This method allows us to elicit an individual-level WTP in an incentive-compatible fashion, which would not be possible if we simply assigned a price to the service common to all subjects.

[7]For instance, in the UK, open banking allows automatic retrieval of balances, rates, and fees once the consumer grants access. Relatedly, ChatGPT launched a "connected finance" feature on May 15, 2026, which can access account-level data from more than 12,000 financial institutions. To the extent that users are willing to grant access to their accounts, ChatGPT can answer questions about balances, spending, investment, and debt repayment (but not move money across accounts or pay bills).

involved in gathering and correctly inputing account information.

We build a benchmark by finding that subjects exposed to free robo-advice improve their repayment strategies significantly relative to the control group. Because all of our debt repayment problems have unambiguous ex ante solutions, any subject who implements the recommended repayment strategy rather than overriding it will minimize interest and late fees. Overriding is uncommon (5.5% of all subjects-trials),[8] and hence the Treatment on the Treated (TOT) effect (the effect of free robo-advising on the subjects who decide to consult it when offered) is large. Average losses decline by 19.6 percentage points, the average savings forgone being 21.9% in the pre-intervention stage. In the cross-section, TOT effects are heterogeneous and disproportionally benefit subjects with lower financial literacy and numeracy, suggesting that (free) robo-advice tools can help to level the playing field when it comes to household debt repayment.

Apart from overriding observed recommendations, users may avoid consulting robo-advice altogether, for instance, due to distrust in algorithms or confidence that they can identify the best course of action without assistance. We find evidence of this possibility by estimating the Intention-to-Treat (ITT) effect—the difference in loan repayment performance between subjects offered free robo-advice and those in the control group, regardless of whether they use the tool. Although economically and statistically significant, the ITT estimates are about 25% smaller than the TOT estimates. Consistently, subjects forgo free robo-advice in about 25% of the problems for which it is offered.

While free robo-advice is unambiguously beneficial to consumers, paid robo-advice is only beneficial if the fee consumers pay is lower than the benefits they perceive from using it, which include monetary components (the counterfactual savings they would have forgone absent the advice) as well as potential non-monetary benefits, such as avoiding the cognitive and psychological costs of making choices unassisted, or the expectation that they will learn optimal debt repayment strategies from the tool, which will benefit them in the future. We find that, for the average subject, WTP is *higher* than the expected monetary benefit from using it in our setting. This willingness to "overpay" for robo-advice beyond the immediate monetary gains motivates overlooked potential benefits of providing robo-advice to the general public.[9]

From a policy perspective, it would be optimal if demand for robo-advice were

[8] The share reduces to 5.2% for the sample of subjects for whom we observe all covariates in our regression analysis.

[9] As discussed above, a substantial minority of consumers refuses robo-advice even when free, which stresses that WTP is highly heterogeneous in the general population.

greatest among the least financially literate and numerate, since they make costlier mistakes in the pre-intervention phase. At the same time, research finds that distrust in algorithms tends to be higher for this category of consumers (e.g., see Niszczota and Kaszás (2020)). We find that demand for paid robo-advice is inversely related to our measures of financial literacy and financial confidence. Everything else equal, demand for free and paid robo-advice are both positively related to trust in robo-advice.[10] More financially literate subjects have a lower WTP, while men and more trustful subjects are willing to pay more. Low trust in algorithms is strongly correlated with overriding robo-advice, which is never optimal in our setting, and with the stated desire to interact with a human advisor.

We conclude by exploring whether robo-advice helps consumers learn about optimal debt repayment or, instead, limits learning-by-doing by eliminating subjects' first-hand experience solving debt repayment problems. While we find evidence of learning-by-doing effects in the control group, which has to solve six debt repayment problems on its own before reaching the post-intervention phase, we also find some evidence of learning in the treatment groups. Those participants who see optimal allocations computed by the robo-advisor are more likely to pursue an optimal allocation strategy in the post-intervention phase, when robo-advice is no longer available, and less likely to rely on heuristics such as 1/N and balance matching. Moreover, we find that the movement towards optimal repayment strategies and away from these heuristics is largest for subjects who were exposed to robo-advice bundled with education, which in our setting consists of simply stating the principles underlying the robo-advisor's recommendations in a few words before showing the recommendations to users.

While one may question the external validity of an experiment in which subjects solve debt repayment scenarios, our setting has two advantages over observational data. First, account-level data from a robo-advice provider would capture a population that self-selects into robo-advice.[11] Our representative UK sample, irrespective of ex-ante interest in robo-advice, allows us to study both selection into advice and willingness to pay. Second, the experiment lets us observe the same consumer solving problems with varying interest rates, loan amounts, minimum payments, and late fees, providing richer evidence on repayment strategies and how consumers abstractly think about debt

[10]We elicited trust in robo-advice after the experiment to avoid priming subjects before measuring demand for robo-advice and WTP. As a result of this choice, it is unclear whether pre-existing levels of trust increase WTP or whether higher WTP increases reported trust through an increased likelihood of exposure to robo-advice.

[11]While D'Acunto, Prabhala, and Rossi (2019) and Chalmers and Reuter (2020) exploit time-series variation in advice availability, they still need to model selection into advice.

repayment relative to field data, where liability structures change infrequently and are less heterogeneous. The fact that our representative sample also includes consumers who do not deal with debt repayment regularly in their daily lives does not appear to be a relevant external-validty concern: among subjects with recent credit card debt experience in the field (66.8% of the sample), pre-intervention performance is only slightly better, and TOT and ITT estimates remain close to those for the full sample. Results are even more similar for the 37.1% with experience across multiple debt types.

Finally, while financial services firms that benefit from interest payments and late fees have limited incentives to create robo-advice tools like ours, generative AI technologies such as ChatGPT can now solve the debt repayment problems in our experiment at near-zero marginal cost. Why, then, measure WTP for debt repayment advice? The value of advice depends not only on availability but also on reliability and consumers' willingness to trust and follow recommendations. Because those who benefit most from advice are often least able to verify its quality, our WTP estimates capture demand for trustworthy, debt-specific advice—a service that general-purpose LLMs may not always provide reliably, as their outputs can vary with prompts, model versions, and user inputs (Choukhmane et al. (2026)). Whether LLM-generated repayment advice aligns with standard economic models and whether consumers follow such advice sufficiently to realize savings remain open questions. Our estimates provide a benchmark for this emerging research agenda by quantifying the value consumers place on verified, debt-specific advice delivered at the point of decision, before accounting for the costs of prompting, verifying, or acting on alternative sources.

Importantly, our design also defines the scope of the questions our estimates address. The experiment isolates the allocation stage of debt management. Our estimates abstract from a set of frictions, such as awareness (recognizing poor debt management or the availability of advice), inaccurate information (being aware of correct account data), and intertemporal trade-offs (trading off debt repayment with consumption/saving). The TOT, ITT, and WTP estimates should thus be interpreted as the value of automating the allocation decision conditional on these margins. They provide a lower bound on the frictions a full-service tool would address, which we discuss in more detail in the conclusion.

1 Experimental Design and Procedure

In this section, we describe the design of the experimental tasks and the experimental procedure, including the recruitment of subjects and the sequence of actions in the experiment.

1.1. Experimental Design

The experiment consisted of three ordered phases, executed sequentially within the same session. We summarize the phases and the sequence of subjects' actions in Figure 1. To implement our experimental design, we created 27 loan repayment scenarios ("trials"), which we summarize in the Online Appendix. In each trial, subjects were asked to allocate a hypothetical amount of money across multiple loans with the goal of minimizing the sum of monthly interest payments or, in problems that include minimum payment amounts and late fees, of minimizing the sum of monthly interest payments and late fees.

We first designed nine scenarios that differed based on the number of loans, the mixture of APRs, and loan balances, which we label A to I.[12] We then designed three versions of each scenario: easy, medium, and high difficulty. The easy version focuses on loan amounts and APRs; the medium and hard difficulty versions introduce minimum payments, which can trigger late fees when left unpaid. The most difficult version mimics real-world scenarios by including information not needed to calculate optimal repayment amounts. To illustrate these differences, we report the three versions of "Trial E" in Figure 2.

Throughout the experiment, debt repayment scenarios A through I are presented to each subject in random order. Within each of the three phases, subjects are randomly presented with one easy scenario, one medium scenario, and one hard scenario. In the *pre-intervention phase*, each subject solves three scenarios without any external inputs or information beyond the scenario description. Subjects do not receive any feedback on whether or how their choices differed from those under the optimal repayment strategy.

In the *intervention phase*, subjects are randomly assigned to one of five experimental arms: a control group for whom the task was identical to the pre-intervention phase (group 1); an arm in which robo-advice was available for free (group 2); an arm in which

[12] In the United Kingdom, the "APR" is an effective annual interest rate. According to the FCA: "if you borrowed £100 and the loan APR is 56%, after a year, you would pay back £156 in total." Therefore, to calculate the monthly interest payment for a given loan, we use $r_{monthly} = (1 + APR)^{(1/12)} - 1$.

robo-advice was available for free and bundled with education on why the robo-advisor's proposed allocation was optimal (group 3); an arm in which robo-advice could be obtained for a fee (group 4); and, an arm in which robo-advising bundled with education could be obtained for a fee (group 5). In all four treatment arms, subjects were told the maximum interest and fees they could save by using the robo-advisor (i.e., the maximum possible interest and fees to be paid over the next month minus the minimum possible interest and fees).

The intervention phase introduced three features absent in the pre-intervention phase: access to robo-advice, education regarding the optimal allocations proposed by the robo-advisor, and elicitation of subjects' willingness to pay (WTP) for the robo-advice tool. For example, in the easy version of trial E (see Figure 2), the subject has £1,000 to allocate between three loans: a £1,040.55 balance with an APR of 22.5%, a £466.74 balance with an APR of 45.9%, and a £879.04 balance with an APR of 49.5%.[13] In the absence of minimum payments, the robo-advisor would allocate £879.04 to the loan charging 49.5%, extinguishing the loan subject to the highest APR, and would allocate the remaining £120.96 to the loan charging 45.9%. This allocation results in the best-case monthly interest payment of £28.81. (Allocating everything to the loan with an APR of 22.5% would have resulted in the worst-case monthly interest payment of £45.57.) Subjects within treatment groups 2 and 3 who chose to receive free robo-advice and subjects within treatment groups 4 and 5 who chose to receive paid robo-advice and whose WTP exceeded the cost of the tool found the suggested best-case allocation automatically filled in on their screens. Subjects were told the robo-advisor's allocations across debts were optimal, and could either accept or modify/override the allocations before advancing to the next screen.[14]

Education consists of brief explanations of the principles the robo-advisor uses before the proposed allocations are displayed. For example, in the easy version of trial E, subjects who see the robo-advised allocations are shown the educational material in Figure A.1. On the one hand, education provides generalized financial literacy about optimal debt

[13]The APRs in our scenarios are designed to match the rates on the main debt instruments available to UK consumers in 2020. Credit cards and store cards charge 0–39.9%, spanning average credit card borrowing costs and higher-risk products (Bank of England (2020); StepChange Debt Charity (2019)). Overdrafts charge 34.9–48.9%, consistent with pricing following the FCA's 2020 reforms, while revolving and flexible loans charge 39.9–79.5% (Financial Conduct Authority (2020)). Payday loans charge 148% or 292% APR, the latter corresponding to the FCA's price cap on high-cost short-term credit (Financial Conduct Authority (2015)).

[14]Of course, consumers do not necessarily believe that an automated allocation is optimal, even if told so by the application. Their distrust in algorithms and/or conviction about rules of thumb from which the robo-advised allocations depart could lead them to modify and/or override the proposed allocations.

repayment that subjects could use in subsequent unassisted choices—providing rationales for robo-suggestions has been shown to limit users' algorithmic aversion (Ben David, Resheff, and Tron (2021)). On the other hand, subjects may not want to engage with the educational material, especially if they are already inclined to trust automated advice.

In the treatments with paid robo-advice, we elicited WTP in an incentive-compatible fashion. Rather than being asked whether they were willing to pay a pre-specified fee, subjects were asked to state their WTP for access to the robo-advice tool, using a slider that ranged from 0 pounds to the maximum possible savings in interest and fees for their loan repayment problem (which varied across trials and, in many cases, across the easy and not-easy versions of a given trial). Subjects were explained that the actual price would be picked at random and compared to their WTP to assess the outcome: "You should respond truthfully: after you say how much you are willing to pay, the actual price of the assistant is picked randomly. If what you said you're willing to pay is higher than the random price, you'll get automated assistance at that price. If what you said you're willing to pay is lower than the price, you won't get the automated assistance and will pay nothing."

Our price-setting mechanism is purposefully designed to elicit truthful WTP and deaprts from how prices would be set in field applications, i.e. a common pre-set fee charged to any interested user. Knowing individual-level WTP allows us to understand the average perceived value of robo-advice as well as any cross-sectional heterogeneity, which is our objective. Note also that eliciting WTP with incentive-compatible mechanisms rather than proposing pre-set fees can be an implementable and effective price-setting mechanism for tools that lead to technology adoption (Berry, Fischer, and Guiteras (2020)).

Overall, 36.6% of the subjects who sought paid robo-advice received it. This is because the average WTP (scaled by the maximum possible savings) was 34.7% while the average random price of robo-advice (scaled by the maximum potential savings) was 47.9%.[15] Crucially, subjects could see the problem *before* entering their WTP for the robo-advice tool, which is a realistic feature since borrowers in the field can seek advice based on the perceived difficulty of managing their debt. Moreover, because subjects should have chosen their willingness to pay based on the expected difference between the savings they could have made on their own and the maximum savings they could have made when using the robo-advice tool (which the problem told them explicitly), it was important to allow them to observe the debt repayment problem and assess its complexity

[15]This fraction is not exactly 50% because the cost of paid robo-advice in two of the 27 debt repayment scenarios was erroneously drawn from a uniform distribution that slightly understated the difference between the maximum and minimum possible interest and fee payments.

before deciding whether and how much to bid.

The *post-intervention phase* followed the same design as the pre-intervention phase. Subjects had to solve three problems of different difficulty levels without access to robo-advice. Before the end of the experiment, subjects answered a short debriefing survey, which we describe in the next section.

1.2. Experimental Procedure and Subject Pool

We pre-registered the experimental design and procedure, including details about subject recruitment and target sample sizes, in the AEA RCT Registry (trial ID: AEARCTR-0006447).[16] Subject recruitment aimed to obtain a UK nationally representative sample of adults aged 18 and above.[17] The initial target sample size was 4,500 subjects, but because of changes in FCA's budget and the timeline for completing the experiment phase, the delivered sample size was 3,423. Importantly, no analysis was performed prior to the delivery of this sample.

The experimental data were collected in the Summer of 2020 using the third-party online platform Qualtrics, which engaged in quota control on several demographic characteristics to maintain the sample's representativeness overall and within each arm. In addition to recording subjects' choices in the experimental tasks, Qualtrics provided us with the demographic characteristics that we control for in the analysis. The randomized assignment of subjects across experimental arms should ensure that observable and unobservable characteristics are uncorrelated with the assignment rule.

After accessing and signing the consent form, subjects read the instructions describing the task of solving a loan repayment problem. The instructions specified that the goal of the research was to better understand how people manage their debts. The instructions briefly described the notion of debt and interest, indicated that subjects would need to work on nine loan repayment scenarios, and provided them with a screenshot of an example problem. Subjects were told that an optimal repayment strategy always existed. Subjects assigned to treatment arms also read an additional line of instructions stating that they might be offered help in the form of an automated assistant in some scenarios. Finally, the instructions indicated how the incentive-compatible component of subjects' payment would be computed based on subjects' choices. Subjects received two forms of compensation: a participation fee, which was independent of their choices, and a

[16]The proposed procedures were approved by the Financial Conduct Authority (FCA) Internal Ethics Review Board on May 6, 2020.

[17]Individuals below 18 years of age were excluded due to the low likelihood of direct access to credit.

performance-based bonus. The instructions stated: "You will get the standard Qualtrics payment for taking part. On top of this, you can win an additional bonus of up to £2. For every hypothetical £8 you save in the task, you'll win 10p." The £2 maximum corresponded to making no mistakes rather than to a binding cap: subjects were also told that incorrect choices could generate up to £160 in hypothetical interest and fees, so avoiding the full £160 yielded exactly the maximum £2 bonus. Thus, the marginal incentive of 10p per £8 of hypothetical savings applied throughout the entire payoff range.

The section "Survey Instrument" of the Online Appendix reproduces the instructions and the key screens of the experiment as they were displayed to participants, including the general instructions, the screen through which robo-advice was offered, and the script used to elicit the willingness to pay for robo-advice. After reading the instructions, subjects proceeded to execute the three phases described above sequentially across nine subsequent screens. Figure 2 reproduces the screens for the three difficulty versions of trial E as they appeared to subjects. Each screen displayed a table with one row per debt account, reporting the nature of the loan (e.g., credit card, overdraft, or revolving loan), its current balance, its interest rate, and any minimum payment and fee for a missed minimum payment. The final column of the table, labeled "This month I will pay off...," contained the entry fields in which subjects typed the amount to allocate to each account. These fields were initialized at £0.00; for subjects receiving robo-advice, they were instead pre-filled with the recommended allocations, which subjects were free to modify. (For illustration, the screenshots in Figure 2 display the fields populated with the optimal allocations.) Subjects were never shown the total interest and fees payable under the best or worst possible allocation for a scenario. The only related information subjects received was in the treatment arms, where the offer of robo-advice stated the maximum amount the subject could save in the trial at hand—the difference between the worst and best possible outcomes (which equals the denominator of equation (1) below). The only mechanical assistance was a pair of running counters at the bottom of the table—"Amount still to allocate" and "Total"—that updated as subjects typed, helping them respect the budget constraint without informing them about the quality of their allocation. The instructions stated that "You must allocate the whole amount to paying off debts (i.e. you cannot leave any money unallocated)." [18]

In the debriefing survey administered after the post-intervention phase, we elicited a variety of individual-level characteristics that earlier research suggests may help to

[18]The interface did not always prevent subjects from advancing with allocations that did not exhaust the budget. We describe how we deal with the small number of subject-trials observations that do not sum to the available budget in Section 2.

predict financial decision-making: financial literacy, numeracy, risk tolerance, patience, and generalized trust. Subjects also were asked to describe positive and negative aspects of the robo-advice tool, what types of loans they had used over the past 12 months (including credit cards), whether they had fallen behind on any of these accounts, whether they had a preference for working with human advisors or "automated assistants," and whether they were willing to pay more, less, or the same to work with a human advisor.

Experiments like ours might raise concerns that modest payment incentives induce shirking. Completion times provide a useful check: subjects allocating random amounts across accounts and answering the debriefing questions at random could have finished the experiment in under 3 minutes. Table A.1 reports completion times for the control group by performance. Subjects spent an average of 24.72 minutes on the experiment, consistent with reading the instructions and deliberating over their choices.

2 Debt Repayment and Free Robo-Advice

To quantify the performance of each subject in each loan repayment problem, we compute the *percentage of savings forgone* as follows:

$$\%\,Savings\ Forgone_{i,p} = \frac{Interest\ Paid_{i,p} - Minimum\ Interest\ Payable_p}{Maximum\ Interest\ Payable_p - Minimum\ Interest\ Payable_p}, \tag{1}$$

where $Interest\ Paid_{i,p}$ is the actual amount of interest and late fees that subject i would have paid over the next month when solving loan repayment problem p; $Minimum\ Interest\ Payable_p$ is the amount of interest and late fees that subjects would have paid had they chosen the optimal loan repayment strategy; and $Maximum\ Interest\ Payable_p$ is the amount of interest and late fees that subjects would have paid had they chosen the strategy that resulted in the highest possible interest and late fee payment. The range in possible interest and late fee payments varies across problems A through I; in six of the problems, it also varies between the easy version and the medium- and high-difficulty versions.[19]

We focus on % *Savings Forgone* as the main outcome of interest because it does not depend on the specific design of each scenario, allowing cross-scenario comparisons. As the subject's repayment strategy improves from worst case to best case,

[19] In the medium- and high-difficulty versions of problems A, F, and I, there are no feasible allocations of money across the loans that result in subjects incurring late fees.

% *Savings Forgone*$_{i,p}$ improves from 100% (worst case) to 0% (optimal repayment).[20] During the pre-intervention phase, we also focus on the fraction of subjects that do not correctly specify the optimal allocation and the fraction of subjects whose performance is worse than the expected performance when allocating money at random. We summarize the performance on the loan repayment problems during the pre-intervention phase in Table 1. In 68.5% of the subject-problem combinations, the subject fails to minimize interest and late fees. Average % *Savings Forgone* is 21.9% within the full sample and 32.0% within the subset of trials that contain suboptimal allocations. While both statistics are slightly lower (19.9% and 30.8%) among the subset of participants who report having used a credit card in the past 12 months, the statistics for the 79.8% of subject-problem combinations that report having any debt over the past 12 months are similar to those for the full sample.

2.1. Repayment Strategies

To avoid arbitrariness, we use off-the-shelf debt repayment strategies from Gathergood et al. (2019). In addition to the *Optimal Strategy*, which minimizes % *Savings Forgone*, we consider four strategies based on heuristics: an *Equal Split (1/N)* strategy, whereby consumers split their available budget equally across all the debt accounts; a *Balance Matching* strategy, whereby consumers allocate their available budget across accounts proportionally to the beginning balance in each account; a *High Balance* heuristic, whereby consumers begin by allocating payments to the loan with the highest balance, before turning to the loan with the second highest balance, etc.; and a *Low Balance* heuristic, whereby consumers begin by allocating payments to the loan with the lowest balance, before turning to the loan with the second lowest balance, etc.[21] For each strategy, we determine a corresponding benchmark allocation across the loans within each of our scenarios. In scenarios that do not involve late fees, the benchmark allocations associated with each of the six strategies are easily calculated. When late fees are present, we adjust

[20] Across the 30,807 subject-trials, there are 551 observations (limited to trial numbers 4 through 9) where all of the allocations are reported as being equal to zero pounds. In these cases, where the actual interest payments and late fees exceed the maximum possible when allocating the full budget across loans, we set % *Savings Forgone* equal to 100%. We also note that for 76 subject-trial observations, the overall allocated amount is neither 0 nor the full budget allocated for the problem.

[21] Gathergood et al. (2019) also consider *High Credit Limit* and *Low Credit Limit* strategies in which the allocation is proportional to the difference between an account's credit limit and its balance. We cannot include these strategies, however, because our experiment does not provide subjects with any information on credit limits.

the benchmark allocations to avoid late fees.[22] In addition, we include *Worst Strategy*, based on the allocations that maximize interest payments and late fees.[23]

To assign the actual allocations in a given trial to one of the six strategies, we also follow Gathergood et al. (2019). For each potential strategy, we compute the sum of absolute deviations from the actual allocations and the benchmark allocations implied by the strategy, and we classify the subject as following the debt repayment strategy that minimizes this sum.[24] This approach allows us to map every actual set of allocations into a single strategy.[25] To be clear, we do not know whether the subject consciously sought to implement the assigned strategy, only that their allocations are closer to the benchmark allocations of the assigned strategy than to any of the other strategies that we consider.

Within the pre-intervention phase, when subjects make repayment decisions without access to robo-advice, subjects are classified as following an optimal strategy in 49.9% of the trials.[26] In 31.5% of all trials, $\%$ *Savings Forgone* is equal to 0% and in another 18.4%, the subject's allocations are closest to those of the optimal allocation despite a positive $\%$ *Savings Forgone*. The most popular heuristic is "1/N" (20.1%), followed by balance matching (16.4%), low balance first (8.3%), and high balance first (4.4%). In 0.8% of the trials, the subject's allocations are actually closest to the allocations that maximize interest and fees.[27]

[22]Gathergood et al. (2019) limit their sample to household-month observations for which all minimum payments are made.

[23] *Worst Strategy* is a useful benchmark, even if subjects have no reason to adopt it.

[24]Classifications are very similar if we instead choose the strategy that minimizes the sum of squared deviations.

[25]When the allocations under the *Optimal Strategy* coincide with the allocations under the *High Balance* or *Low Balance* heuristic, we classify the subject as following the *Optimal Strategy*.

[26]It is not surprising that the share of optimal allocations is considerably higher than Gathergood et al. (2019) find in their credit card data from the field. First, because many of our debt management scenario extend beyond credit cards, the ranges of APRs in our scenario are significantly wider than the ranges of APRs that Gathergood et al. (2019) document in their sample of credit card accounts, making it easier to realize which debts should be paid down first. Second, our subjects receive a pre-specified amount that they must allocate across debt accounts, whereas in the field setting of Gathergood et al. (2019), credit card holders need to decide how to allocate available resources between repaying debt, consumption, and investment opportunities. We abstract from this decision in order to focus on subjects' understanding of debt repayment.

[27]We describe how repayment strategies vary across different samples of subjects, including those with different levels of financial literacy, in Section 5; see Table 5 and Table A.6.

2.2. Baseline Variation in Borrower Repayment Performance

We begin by determining the demographic characteristics that correlate with the quality of loan repayment skills, focusing on the control group, who lack access to robo-advice:

$$\%\ Savings\ Forgone_{i,p} = \alpha_p + X_i\beta + \epsilon_{i,p}, \tag{2}$$

where % *Savings Forgone*$_{i,p}$ reflects the performance on loan repayment problem p by control subject i, X_i is a vector of individual characteristics, and α_p is a problem-specific fixed effect (easy version of problem A, medium or hard version of problem A, easy version of problem B, etc.). We estimate the equation via OLS regression and cluster the standard errors by subject.

In the first column of Table 2, X_i includes financial literacy fixed effects (terciles based on the number of correct answers to five financial literacy questions), educational attainment fixed effects (six categories), and age category fixed effects (seven categories, ranging from "18-24" to "75+").[28] As expected, greater financial literacy is associated with better loan repayment skills. For subjects in the middle tercile of financial literacy, the estimated effect is -9.5 percentage points; for those in the top tercile, it is -14.2 percentage points. Relative to the average member of the control group (mean of the dependent variable is 19.7%), those in the middle tercile of financial literacy forgo 48% less savings due to loan repayment mistakes and those in the top tercile forgo 72% less. Note that the estimated coefficients on financial literacy are incremental to the effects of formal general education, for which we also control. Unreported coefficients on the education fixed effects reveal that the most educated outperform the least educated by 6.8 percentage points. Unreported coefficients on the age range fixed effects reveal a non-monotonic relation, with the best performance by subjects aged 45-54 and 55-64.

We expand the set of investor characteristics in the second column of Table 2 to include self-assessed numerical literacy fixed effects (terciles), self-assessed financial confidence fixed effects (terciles), self-assessed patience fixed effects (terciles), self-assessed risk aversion fixed effects (terciles), general knowledge test score fixed effects (terciles), a dummy variable for whether the investor trusts algorithms at least as much as human advisors, a dummy variable for whether the subject identifies as male, and a dummy variable for whether the individual has incurred any debt over the past 12 months. To measure general knowledge, we ask subjects a set of five questions—three questions on geography, one about population sizes, and one about when the iPhone first launched.

[28] All variables and categories are defined in Table A.2.

While the coefficients on the financial literacy fixed effects are slightly attenuated when we include additional proxies for problem-solving skills, they remain economically and statistically significant. In addition, the estimate for high numerical literacy suggests that having greater familiarity with performing computations—above and beyond financial literacy and general education—leads to better repayment performance. Greater patience and higher general knowledge are also associated with better performance. However, we find little evidence that recent debt experience or financial confidence is associated with better performance.[29]

Trust in algorithms also has little predictive power. This result is important in our setting because it shows that a dimension that might—and does, as we show below—help explain the take-up of robo-advice does not simultaneously capture pre-existing differences in subjects' ability to solve loan repayment problems. Finally, we find that gender is unrelated to savings forgone, which suggests that when financial literacy and other demographic characteristics are held constant, men and women behave similarly. Systematic differences in the financial decisions of men and women have been documented for choices under risk (e.g., Barber and Odean (2001) and D'Acunto (2019)). In the context of loan repayment, however, subjects are being asked to make non-risky choices.

2.3. Robo-Advice and Borrower Repayment Performance: Treatment on the Treated

We estimate the average difference in percent savings forgone between subjects who received robo-advice and subjects who did not, the treatment on the treated (TOT). When robo-advice is provided for free, because the tool always recommends the loan repayment allocation that minimizes interest and fees, subjects who receive robo-advice can only forgo savings if they choose to override the recommended allocations. In contrast, the loan repayment allocations chosen by subjects who have no access to the tool should capture any mistakes that borrowers make and any biases that they exhibit when facing loan repayment problems.

We estimate a multivariate OLS specification of the following form, restricting the sample to trials 4 through 6 for subjects in the control group and both free robo-advice

[29]Estimated TOT and ITT are quantitatively similar when, in unreported regressions, we re-estimate Table 2 and Table 3 using only the 37.1% of subjects with experience with two or more forms of debt over the past 12 months.

treatment arms:

$$\% \ Savings \ Forgone_{i,p} = \alpha_p + \beta_1 RoboAdvice_{i,p} + \delta Education \ Treatment_p + X_i \gamma + \epsilon_{i,p}, \quad (3)$$

where $\% \ Savings \ Forgone_{i,p}$ reflects the performance on loan repayment problem p by subject i; $RoboAdvice_{i,p}$ is a dummy variable that equals one if subject i was offered and accepted free robo-advising (with or without education) when facing problem p, and zero otherwise; $Education \ Treatment_p$ is a dummy variable that equals one if subject i was assigned to the free robo-advice with education treatment arm; and the problem fixed effects and demographic controls $X_{i,p}$ are defined as in equation (2). Because we include problem fixed effects, TOT and other estimated coefficients are identified using variation across subjects within a given repayment problem and difficulty level (easy versus medium or hard).

We report the results in columns (3)-(4) of Table 2. The estimated TOT coefficient for free robo-advising is -19.6 percentage points (statistically significant at the 1-percent level). This effect is almost as large as the 22.1% average savings forgone by the treatment groups in the pre-intervention phase and just as large as the 19.6% average savings forgone by the control group in the intervention phase, indicating that the vast majority of subjects who are provided with robo-advising for free follow its recommendations.[30] Regarding the control variables, we continue to find that higher financial and numerical literacies are associated with better performance, but the estimates are attenuated, consistent with robo-advice differentially improving the quality of financial decision-making by less financially literate and numerate subjects. There are no differences in performance associated with random assignment to robo-advice or robo-advice with education. If we limit our sample to the 79.8% of subjects that report managing any debt over the past 12 months, the TOT estimate is -18.4 percentage points; if we limit it further to the 66.8% of subjects that report managing credit card debt over the past 12 months, the TOT estimate remains economically and statistically significant at -16.7 percentage points.

While the randomized assignment of subjects across treatment arms should eliminate concerns that treatment is correlated with observed or unobserved individual-level characteristics, subjects who seek robo-advice within a treatment arm may differ from

[30]While those with the lowest level of financial literacy are more likely to override free robo-advice recommendations than those with the highest level of financial literacy (7.4% versus 2.6%), deviations from optimal allocations by the least literate participants when overriding are smaller, resulting in average actual % *Savings Forgone* of 21.8% for the least literate versus 33.7% for the most literate. For participants with an intermediate level of financial literacy, the likelihood of overriding the robo-advice is 4.9%, and the average actual % *Savings Forgone* are 27.9%.

subjects who do not seek robo-advice along dimensions that are correlated with their performance on loan repayment problems. To address this concern, in Table 3 we report two-stage least-squares (2SLS) specifications in which $Robo\ Advice_{i,p}$ is instrumented with a dummy variable indicating whether subject i was assigned to a free robo-advice treatment arm. The 2SLS results are similar to the OLS results. The first-stage estimated coefficients imply that the instrument is relevant, and the high F-statistics rule out the possibility that the instrument is weak.

2.4. Robo-Advice and Borrower Repayment Performance: Intention to Treat

Next, we estimate Intention to Treat (ITT) effects, which compare the average savings of subjects offered free robo-advising or robo-advising with education, irrespective of whether they sought the advice, with the average savings of control subjects. ITT effects account for both TOT effects and the fraction of subjects who, despite being offered access to free robo-advice, prefer not to receive the recommended allocations. They are relevant not only to understand consumers' willingness to accept robo-advice but also for policy purposes. Indeed, if a policy maker wanted to assess the effects of offering robo-advice to the broader population, it should want to capture the extent to which consumers would seek and follow such advice.

We summarize demand for and utilization of robo-advice across the trials and treatment arms in Figure 3.[31] Pooling trials 4 through 6, we find that subjects seek access to free robo-advice in 74.7% of the problems. However, demand is consistently higher for free robo-advice without education (80.1%) than for free robo-advice with education (69.3%). We estimate a multivariate OLS regression similar to equation (3) but in which the $Robo\ Advice_{i,p}$ dummy variable equals one for all subjects who were assigned to an experimental arm with robo-advice. The ITT estimate in column (4) of Table 2 is -14.6 percentage points (statistically significant at the 1-percent level), which is approximately three-quarters of the TOT estimate in column (3). The positive estimated effect of the Education Treatment dummy variable is consistent with lower demand for free robo-advice with education among some subjects who would have benefited from relying upon it. Within the sample of subjects that managed any debt over the past 12 months, the ITT estimate is -14.2 percentage points, and within the sample of subjects that managed credit card debt, it is still -12.8 percentage points.

[31]See also Table A.3.

2.5. Robo-Advice and Borrower Repayment Performance: Heterogeneity

While the average effects of exposure to robo-advice are a helpful benchmark, we are more interested in potential heterogeneous effects by demographics. The question of who benefits more from robo-advice arises because the treatment effects are likely to vary with subjects' underlying characteristics, including financial literacy. On the one hand, because less financially sophisticated decision-makers are likely to perform worse on these tasks when unassisted, the TOT effects are likely to be more positive. On the other hand, less sophisticated subjects might be warier of technologies that they do not understand and with which they are unfamiliar, leading them not to adopt the robo-advice tool, driving down ITT effects. They might also be more likely to override its recommendations if the recommendations clash with popular heuristics, driving down TOT effects.

We consider the characteristics we elicited in the debriefing survey (and that we include as control variables in Table 2). In Figure 4, each panel refers to one such characteristic. Each bar represents the estimated TOT effect $\hat{\beta}_1$ for a version of equation 3 that interacts the dummy variable for being exposed to free robo-advising with dummy variables for each tercile of a given characteristic. For each characteristic, tercile 1 refers to lower values, and tercile 3 refers to higher values. The control variables otherwise mirror column (3) of Table 2.

We estimate monotonic (and statistically significant) heterogeneous treatment effects for three of the eight characteristics: financial literacy, self-assessed numeracy, and risk aversion. In each case, lower values are associated with larger TOT estimates, with the largest differences for financial literacy. The difference between estimates for the first and third tercile are statistically significant for all three of these characteristics. Interestingly, free robo-advising for loan repayment not only levels the playing field across subjects who have different levels of financial literacy but it makes low-literacy subjects better off in levels. A higher share of low-literacy subjects implements the robo-advice fully relative to high-literacy subjects. This result speaks to the trade-off between the potentially positive and negative relative and absolute effects of access to robo-advising for less sophisticated consumers. The results for patience are also worthy of mention. Even though we do not detect a monotonic heterogeneous pattern, we do detect an economically and statistically significant differential effect between terciles 1 and 2, which suggests that the most impatient subjects gain relatively more from robo-advice than others. For general knowledge, the estimated effects are monotonic but small. For the remaining characteristics—confidence in one's own ability to manage their finances, recent experience with debt, and trust in algorithms—we do not detect monotonic patterns.

3 Willingness to Pay for Robo-Advice

Next, we study subjects' WTP for robo-advice. Our incentive-compatible procedure for eliciting WTP allows us to measure variation that is unobserved in field settings, where any consumer whose WTP is higher than a pre-set fee chooses to pay for robo-advice. WTP is elicited as the maximum amount of potential savings in each trial that subjects are willing to pay to observe the robo-advisor's debt repayment allocations. The subject states an amount in pounds, which we divide by the problem-specific denominator in equation 1, to convert the answer into a percentage of savings forgone. For example, the maximum avoidable interest and fees in the medium and high difficulty versions of scenario C is £50. Assume that a subject confronted with this scenario expects to make mistakes that generate £10 in avoidable interest and fees (where £10 equals 20% of the maximum avoidable interest and fees). If the subject does not derive any additional value from relying on robo-advice, she should bid £10 for access to the robo-advised allocations.[32] To the extent that the subject derives utility from not having to solve the debt-repayment problems on their own, expects to learn optimal strategies from robo-advice that they can apply on her own in the future, underestimates her ability to solve debt-repayment problems, or is risk averse over the outcome of the debt-repayment problems, we would expect her to bid more than £10. In the context of our experiment, these additional considerations should give rise to subject WTP estimates that are systematically higher than the average percent savings forgone in the pre-intervention trials.

In Table A.4, we report the maximum avoidable interest and fees for 18 distinct scenarios, along with the number of subjects in the paid robo-advice treatment groups presented with each scenario. We combine the medium and high difficulty versions because they have the same distribution of potential interest and fees. We focus on trial 4 because it is the first time subjects in the paid robo-advice treatment arms are offered paid robo-advice. We partition subjects into three groups: those who do not seek paid robo-advice (16.3%), those who seek it but then set their WTP equal to £0 (13.7%), and those who seek paid robo-advice and report a positive WTP (70.1%). While this partitioning reveals

[32]If the randomly chosen cost of robo-advice is equal to £10, the subject expects to be indifferent between relying on robo-advice and solving the scenario on her own. If the cost is less than £10, the subject expects to benefit from relying on robo-advice. If the cost is more than £10, the subject expects to be better off financially when solving the debt repayment scenario on her own.

high average demand for paid robo-advice, it also reveals how demand for paid robo-advice varies across scenarios with different stakes. When WTP is measured in pounds, we expect it to be positively correlated with the level of avoidable interest and fees. Indeed, when the level of avoidable interest and fees is higher—and this information is provided to subjects before they decide whether to seek paid robo-advice—subjects are more likely to seek paid robo-advice and less likely to bid £0 conditional on seeking it. Similarly, the likelihood of reporting a positive WTP is increasing with the level of avoidable interest and fees (the correlation between these two measures is 0.57). In other words, subjects are more likely to report a positive WTP when the stakes are larger. Finally, the table quantifies how the level of WTP responds to the level of avoidable interest and fees. When we calculate the mean and median WTP within each scenario, we find that both statistics are strongly positively correlated with the level of avoidable interest and fees; the correlations exceed 0.90 in the sample of subjects with a positive WTP and also in the full sample (where we set WTP equal to £0 if the subject does not seek paid robo-advice).

There is some selection into paid robo-advice. Among the 219 subjects who do not seek paid robo-advice in trial 4, average $\%$ *Savings Forgone* during the pre-intervention phase is 19.0%, whereas among the 1,127 subjects who seek paid robo-advice, it is 23.0%. In Figure 5, we plot the distribution of WTP for the 1,127 subjects who seek paid robo-advice in trial 4 (including cases where the subject sets WTP equal to £0), against the distribution of the average percent savings forgone during the pre-intervention trials, when these subjects lacked access to robo-advice. To the extent that subjects successfully infer the likelihood and magnitude of debt repayment mistakes from the pre-intervention trials, and do not derive any additional benefits from relying on robo-advice, the two distributions should overlap. This is approximately the case in Figure 5, but with the caveat that the fraction of subjects willing to bid more than 60% of avoidable interest and fees to obtain paid robo-advice exceeds the fraction of subjects that incurred these levels of avoidable interest and fees during the pre-intervention phase. As a result, average WTP in trial 4 is 28.3% versus average $\%$ *Savings Forgone* in trials 1-3 of 23.0%.

Next, we ask how WTP responds to the individual subject's performance on debt repayment scenarios during the pre-intervention phase. Within the full sample, the correlation between a subject's WTP and their average percent savings forgone during the pre-intervention phase is only 0.18. The correlation is similar when we restrict the sample to subjects that seek paid robo advice. In Figure 6, we plot the distribution of the difference between WTP and average percent savings forgone for each of the 1,127 subjects that seeks paid robo-advice. Although the average value is 5.3%, and the median value is 0%, the distribution extends all the way from -100% (WTP equals 0% and the subject paid the maximum possible level of interest and fees in each of the three pre-intervention trials) to 100% (WTP equals 100% and the subject paid the minimum level of interest and fees in each of three pre-intervention trials).

We further assess subjects' WTP for robo-advice in Table A.5. In Panel A, we sort subjects into bins based on their WTP and then report average WTP and average *% Savings Forgone* during the pre-intervention phase. While average percent savings forgone tend to be higher when WTP is higher, average percent savings forgone increases much less dramatically than WTP. On average, subjects with WTP above 30% have WTP that are much higher than their actual losses during the pre-intervention phase. In Panel B, we find that lower levels of financial literacy are associated with both higher percent savings forgone during the pre-intervention phase and higher WTP. Interestingly, average WTP exceeds average percent savings forgone across all three financial literacy terciles. As mentioned above, there are several nonmutually exclusive explanations for why WTP might exceed the purely monetary benefits of relying on robo-advice. For example, subjects might underestimate their ability to solve the debt-repayment problems. In our experiment, we purposefully did not provide subjects with feedback on their performance after each trial, which would have been an unrealistic feature given that consumers in the field do not observe the differences between their loan repayment choices and the optimal choices. Alternatively, subjects might value the reduction in cognitive and psychological costs associated with not solving the problems themselves.

The possibility that subjects value robo-advising more than its monetary value because it allows them to avoid paying the cognitive costs required to solve quantitative problems finds support in the debriefing survey. Anyone who received robo-advice at least once during the intervention phase was asked what they liked about robo-advice tool; 72% of the subjects stated that they liked robo-advice because it was quick and easy to use, 31% stated that they liked it because it allowed them to avoid thinking about how to solve the debt repayment problems, and 21% cited both benefits. Around 30% liked the fact that there was no need to talk to anyone, with those in the lowest financial literacy tercile being the most likely to cite this benefit. The least enticing elements were "cheaper to do it myself" (chosen by 50% in the paid robo-advice treatments but also by 27% in the free robo-advice treatment) and "I do not understand how it made decisions" (33% for the lowest financial literacy tercile versus 21% in the highest tercile).

Those subjects who chose to seek robo-advice at least once during the intervention phase were also asked why they chose to seek robo-advice. Around 43% claimed they did so either because they could not beat a machine or because the scenarios were best suited for an algorithm to solve. Approximately 9%, concentrated among those in the lowest financial literacy tercile, stated that they like to delegate their financial choices. Finally, around 29% of robo-advice users chose “Other” and recorded their reason for using robo-advice as an open response. The likelihood of selecting “Other” was increasing in financial literacy (42% for those in the top tercile versus 22% for those in the bottom tercile), and the most common open response comment is that users were curious to compare their solutions to those proposed by the robo-advice tool, suggesting that some users expected either to learn optimal strategies from the robo-advice tool or confirm that they already know how to solve these problems.

The motivations of non-adopters are also worth highlighting. Among those who decided not to seek robo-advice at least once, 54% stated they wanted to make their own decisions. In addition, 21% claimed that they could make better decisions than the algorithm, which raises a red flag regarding consumers’ abilities to understand their own limitations when it comes to loan repayment because, by construction, subjects could, at best, match the performance of the robo-advice tool. Curiously, the conviction that one is able to beat the algorithm does not vary substantially with our measure of financial literacy: 22% of non-adopters in the bottom tercile of financial literacy claimed that they could make better choices than the tool versus 24% of the non-adopters in the top tercile. Among those in the paid robo-advice treatment, 36% stated that they were discouraged from seeking robo-advice because it was too expensive. (The corresponding number in the free robo-advice treatments was 3%.)

An indirect way to assess the extent to which users value robo-advice is to estimate equation (3) for subjects in the paid robo-treatment arms and control group, focusing on $\%\ Net\ Savings\ Forgone_{i,p}$. For the subset of subjects who receive paid robo-advice, this variable equals the subject i’s WTP in problem p, measured as a fraction of the potential savings forgone, plus $\%\ Savings\ Forgone_{i,p}$, which captures their actual performance on the problem. To the extent that the subject accepts the robo-advice, $\%\ Net\ Savings\ Forgone_{i,p}$ will equal their WTP. For everyone else, it equals $\%\ Savings\ Forgone_{i,p}$. The economically large and *positive* TOT and ITT estimates in columns (5) and (6) of Table 2 reflect an interaction between the incentive-compatible elicitation mechanism we used to generate the cost of robo-advice and the fact that subjects with the highest WTP exhibit the largest gaps between WTP and how they performed during the pre-intervention stage. When interpreting the TOT estimate, it is important to understand that only 22.4% of the subjects receive paid robo-advice recommendations, and, within that group, average WTP is 60.2%. As we just saw in Table A.5, average $\%\ Savings\ Forgone$ for subjects with WTP above 50% is around 29%.

Paying around 60% of maximum possible interest and fees to save around 29% implies a positive TOT estimate of around 31%. These estimates do not imply that offering paid robo-advice would harm consumers. First, there are real and perceived non-monetary benefits to relying on robo-advice. Second, the real-world price of robo-advice is likely to be significantly lower than the WTP of the adopters in our experiment.

The debriefing survey also asked three questions related to robo-advice pricing, but only of subjects that had received robo-advice at least once. Around 21% stated that they would trust the robo-advice tool more if they had to pay for it, 13% stated that they would trust it less, and the remaining 66% stated that they would trust it the same amount. The likelihood that pricing would influence trust was slightly higher for those in the paid robo-advice treatment and for those in the lowest tercile of financial literacy. Around 33% of subjects who received robo-advice at least once stated that they would be willing to pay more for access to a human advisor versus 12% who would be willing to pay less. Finally, around 19% stated they would be willing to pay more for "robo-advice from a brand you recognize" while 14% would be willing to pay less. The answers to the last two questions varied little across treatment groups or financial literacy terciles.

4 What Explains Take-up and Compliance with Robo-Advice?

To shed additional light on consumers' demand for and reliance upon robo-advice, in Table 4, we estimate several cross-sectional specifications of the form:

$$RoboChoice_i = \alpha + X_i\beta + \epsilon_i, \tag{4}$$

where $RoboChoice_i$ is the decision to seek free robo-advice in column (1), the decision to seek paid robo-advice in column (2), the WTP for robo-advice in column (3), the decision to override any robo-advice recommendation in column (4), and a dummy variable that indicates whether the subject is willing to pay more for a human advisor than for robo-advice in column (5). In each column, we focus on the decisions of subject i in trial 4. For the paid robo-advising treatments, this is the trial in which subjects report their willingness to pay before learning anything about the cost function.

Demand for free robo-advice is uncorrelated with measures of financial literacy. This is not surprising because free robo-advice allows more and less sophisticated consumers to costly minimize interest and fees without exerting any effort. In contrast, when robo-advice is offered for a fee, subjects with higher financial literacy are less likely to demand it than others. Low-financial-literacy subjects thus understand that their loan repayment skills are relatively low, which leads them to value robo-advising more than others. Consistently, in column (3), when we limit the sample to those who bid on paid robo-advice (including those who bid £0), we find that WTP is substantially lower—both economically and statistically—for subjects with higher financial literacy. We also find evidence that subjects with higher self-assessed levels of risk aversion have higher WTP (statistically significant at the 10-percent level).

Demand for robo-advice, whether free or paid, is higher for subjects who report greater trust in algorithms, which is measured at the end of the experiment. WTP is also higher for subjects who report greater trust in algorithms. Because WTP is measured before subjects have been exposed to the robo-advice tool, however, this difference is more likely to capture differences in pre-existing views about the trustworthiness of algorithms. While men are no more likely to demand paid robo-advising (column (2)), they have a systematically higher WTP for robo-advice conditional on bidding for it (column (3)). The estimated coefficients on the Education Treatment dummy variable are negative but statistically insignificant in columns (1) and (2). However, in Figure 3, we see that demand for robo-advice with education declines more across the three trials than robo-advice without education and that the decline is particularly large for free robo-advice. If we expand the regression samples in columns (1) and (2) to include trials 5 and 6, we find that demand for free robo-advice with education is 10.3 percentage points lower than demand for free robo-advice without education. The difference remains 3.0 percentage points when we focus on paid robo-advice but gains statistical significance within the larger sample.

Approximately 5% of the subjects who receive free or paid robo-advice choose to override the recommended allocations in trial 4. In column (4), we find that subjects are less likely to override robo-advice when the advice is free and trust in algorithms is higher. Interestingly, the decision to override robo-advice is uncorrelated with our measures of financial literacy, financial confidence, and numeracy. In the final column, we ask which of the subjects who received robo-advice in trial 4 state, in the debriefing survey, that they would prefer to pay more for access to a human advisor (which is true for 33.6% of the sample). Stated willingness to pay more for a human advisor is lower for subjects with high numeracy skills and trust in algorithms. Counter to our expectations, however, it is *increasing* in financial literacy. Although we lack direct evidence, one possible explanation is that financially literate subjects derive more value from interacting with an expert with whom they can talk through the pros and cons of their pre-existing debt repayment strategies. This explanation is broadly consistent with our finding above that more financially literate subjects were more likely to state that they wanted to compare their answers to those of the robo-advisor.

5 Learning About Loan Repayment? Learning-by-doing, Imitation, and Education

Whether consumers learn from relying on robo-advice is an open question. On the one hand, consumers making repayment decisions on their own may learn by doing as they confront the trade-offs involved in allocating payments across loans; by reducing such deliberation, robo-advice may limit this learning. On the other hand, exposure to optimal repayment allocations may foster learning-by-imitation, provided consumers understand the logic of recommendations, which might allow them to learn faster than through trial and error.

To test for learning-by-doing and learning-by-imitation, we use the post-intervention phase, in which subjects solve three new debt repayment problems without robo-advice. If subjects learn during earlier stages, both reliance on suboptimal heuristics and % *Savings Forgone* should decline relative to the pre-intervention phase. As described in Section *2.1.*, we classify each allocation into one of six repayment strategies—*Optimal*, *1/N*, *Balance Matching*, *High Balance*, *Low Balance*, and *Worst*—distinguishing between *Optimal (0%)* and *Optimal (>0%)* depending on whether the allocation attains the optimum or merely approximates it. We then estimate subject fixed-effects regressions on the pre- and post-intervention data, replacing the performance outcome in equation (2) with either % *Savings Forgone* or the repayment-strategy indicators.

The top panel of Table 5, tests for changes in % *Savings Forgone* and debt repayment strategies within the control group, free robo-advice treatment arms, and paid robo-advice treatment arms, respectively. The control group solves six debt repayment problems before reaching the post-intervention phase. During the pre-intervention phase, 33.3% of the control group's repayment strategies are classified as Optimal (0%) and another 18.2% as Optimal (>0%). The two most common heuristics are "1/N" (19.7%) and Balance Matching (15.7%), but all heuristics are represented. Average % *Savings Forgone* is 21.2%. During the post-intervention phase, we estimate a 4.0 percentage point increase in Optimal (0%) with small offsetting reductions in the heuristics; % *Savings Forgone* declines by approximately 2.5 percentage points. These patterns are consistent with some level of learning-by-doing. Figure A.2, which plots average % *Savings Forgone* for the control group separately for each of the nine trials, shows the same gradual improvement across trials.

As expected under randomization, pre-intervention % *Savings Forgone* and repayment strategies are similar across treatment and control groups. Subjects offered free robo-advice exhibit larger shifts toward optimal repayment strategies than the control group, although their reduction in % *Savings Forgone* is more modest (1.6 percentage points). This reflects changes in the average cost of each strategy between the pre- and post-intervention phases: while more subjects adopt *Optimal (0%)*, the average % *Savings Forgone* among *Optimal (>0%)* allocations rises from 19.1% to 26.7%, partially offsetting the gains. We do not observe comparable changes in the control group. Estimates for the paid robo-advice treatments are attenuated because many subjects in the paid robo-advice treatments never received it.

We partition the treatment arms into three groups: subjects who received robo-advice with education at least once, subjects who received robo-advice without education, and subjects who never received robo-advice, either because they did not seek it or because their WTP was too low. This comparison isolates whether bundling robo-advice with just-in-time financial education enhances learning-by-imitation. Subjects receiving robo-advice with education exhibit the largest shifts away from suboptimal heuristics and the greatest reduction in % *Savings Forgone*, although the latter is not statistically distinguishable from the other groups. Thus, while the evidence is consistent with learning-by-imitation, it is weak.

Table A.6 examines whether learning varies with financial literacy by partitioning subjects into low-, medium-, and high-literacy terciles. As expected, low-literacy subjects exhibit substantially higher pre-intervention % *Savings Forgone* and greater reliance on suboptimal heuristics than high-literacy subjects. For example, 22.7% of low-literacy treatment subjects use balance matching, compared with 7.6% of high-literacy subjects. Learning-by-imitation appears strongest among subjects with medium financial literacy: the share of *Optimal (0%)* allocations rises by 3.7, 9.3, and 6.7 percentage points across the low-, medium-, and high-literacy terciles, respectively. In contrast, the control group exhibits smaller differences across financial literacy levels.

6 Conclusions

We design and implement a pre-registered experiment on a representative sample of UK individuals to assess the extent to which robo-advice for debt repayment is adopted, valued, and used by consumers, many of whom may have difficulty minimizing interest payments and late fees in real-world situations. We find that access to free robo-advice significantly improves repayment decisions. The high levels of demand and low levels of overriding among the least sophisticated subjects, who benefit the most from using the tool, suggest that providing free robo-advice for loan repayment would decrease wealth inequalities arising from mistakes in this domain.

We also find that the average willingness to pay for robo-advice is higher than its monetary benefits (the expected interest payments and late fees that it allows borrowers to save), which suggests either that consumers are excessively pessimistic about their performance in loan repayment tasks when unassisted or that they value non-monetary benefits of using robo-advice, such as avoiding the cognitive and psychological costs of making choices that are consequential to their wealth. In the cross-section, we find that higher financial literacy decreases the willingness to pay for robo-advice. Non-adopters and overriders are both more likely to state that they do not trust algorithms at the end of the experiment. Finally, we find evidence of both learning-by-doing among the control group and learning-by-imitation among those exposed to robo-advice, especially among those exposed to robo-advice bundled with education.

How can we reconcile consumers' high valuation of the benefits of robo-advice for debt repayment and the simplicity of providing it with the lack of successful and widespread implementations in the field? First, lenders' profits are a non-monotonic function of borrowers' debt. While too much debt may result in default, which is costly for borrowers and lenders, too little debt reduces lenders' revenues. Because lenders have an incentive to let households accumulate debt and pay interest and fees so long as doing so does not trigger default, we should not expect lenders to proactively offer robo-advice for debt repayment.

Second, debt management has attracted the attention of several FinTech providers around the world. Tally claims to have "saved people millions of dollars on credit card interest and late fees since 2015." Similarly, SuperFi in the UK claims that it is "addressing increasing consumer debt in the UK by utilizing Open Banking to help borrowers locate and understand their different sources of debt - enabling them to optimize their repayments to achieve debt freedom faster." And, while neither Mint nor Personal

Capital currently possess the data on consumer debt that is required to offer repayment advice, it is easy to imagine these types of financial services firms expanding in this direction.

Third, the recent rapid evolution of generative AI algorithms, such as ChatGPT, will soon provide no- or low-cost access to the correct solutions to real-world debt repayment scenarios like the ones we ask our subjects to solve. When running a prompt based on one of our debt repayment scenarios using both ChatGPT 3.5 and ChatGPT 4 in the Fall of 2023, we did not obtain correct answers, even though ChatGPT 4 provided a plausible explanation about the principles that should be applied to solve the question.[33] In contrast, running the same prompt on ChatGPT 4 in January 2024 returned the optimal allocation and a clear description of how the allocation was obtained. The same was true of ChatGPT 5 in November 2025. Commercial applications, such as Cleo in the UK, claim that they are currently "working to implement and utilize GPT to continue developing our hyper-personalized AI assistant."

The growing availability of LLMs does not imply that the demand for debt-specific robo-advice will disappear. First, obtaining accurate repayment advice from a general-purpose LLM requires consumers to provide complete account information, reintroducing the information-entry and awareness frictions that a dedicated tool integrated with their accounts would eliminate. Second, LLM outputs can vary with prompts, model versions, and over time (Choukhmane et al. (2026)), while the consumers who benefit most from repayment advice are the least able to detect errors. These considerations motivate two questions for future research: whether LLM repayment advice accords with standard economic models and whether consumers follow it well enough to realize interest savings (Kuchler and Pagel (2021)). Finally, our results identify trust as a determinant of adoption: non-adopters and overriders are significantly more likely to distrust algorithms. Accordingly, our WTP estimates capture demand for trustworthy, debt-specific advice, whether delivered by dedicated robo-advisors, LLMs with verified computational back-ends, or regulated intermediaries.

Finally, while useful robo-advice on when it is optimal to borrow to finance consumption or investments requires complicated assessments based on consumer preferences

[33]The prompt we ran reads as follows: "This month, I have \$1000 that I would like to allocate across my three loans to minimize interest payments and prevent any late fees. There are two credit cards and one revolving loan. The first credit card has a balance of £1040.55, APR of 22.5%, minimum required payment of \$26.01, and late fee (when missing the minimum required payment) of \$12. The second credit card has a balance of \$466.74, an APR of 45.9%, and no minimum payment. The revolving loan has a balance of \$879.04, an APR of 49.5%, a minimum payment of \$19.78, and a late fee of \$48. How much of my \$1000 budget should I allocate to the first credit card, second credit card, and revolving loan? Please make sure that the three payments sum to \$1000. Also, please explain your proposed allocation."

and characteristics, expanding the scope of robo-advice for liability management is more straightforward. For example, robo-advisors could address questions such as choosing between different credit cards using consumers' transaction histories to value different reward schemes, choosing across mortgage options based on consumers' monthly budgets, or deciding whether and when to refinance an existing mortgage. The design and assessment of the adoption patterns and effects of such tools are interesting frontiers for future research.

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Figure 1: Experimental Design

Experimental Design

Phase	Trials	Group 1	Group 2	Group 3	Group 4	Group 5
Pre-intervention	1-3	← No robo-advice →				
Intervention	4-6	No robo-advice	Free robo-advice	Free robo-advice with education	Paid robo-advice	Paid robo-advice with education
Post-intervention	7-9	← No robo-advice →				

- Subjects are randomly assigned into the five experimental arms.
- Each subject solves nine debt-management games, in random order. Within each phase, each subject solves one easy, one medium, and one hard version, also in random order.
- Trials 1-3 allow us to determine baseline performance on debt-management games and correlation with characteristics.
- Trials 4-6 allow us to estimate treatment-on-treated and intention-to-treat effects separately for free and paid robo-advice tools. They also allow us to measure willingness to pay (WTP).
- Trials 7-9 allow us to test for learning effects.

Figure 1 describes the experimental design of the RCT discussed in the paper.

Figure 2: Sample Trial Across Difficulty Versions

Panel A. Trial E: Low Difficulty Version

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off...
Credit card	£1,040.55	22.5% APR	£0.00	£0	£0.00
Overdraft	£466.74	45.9% APR	£0.00	£0	£120.96
Revolving loan	£879.04	49.5% APR	£0.00	£0	£879.04

Panel B. Trial E: Medium Difficulty Version

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off...
Credit card	£1,040.55	22.5% APR	£26.01	£12	£26.01
Overdraft	£466.74	45.9% APR	£0.00	£0	£94.95
Revolving loan	£879.04	49.5% APR	£19.78	£48	£879.04

Panel C. Trial E: High Difficulty Version

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off...
Credit card	£1,040.55	22.5% APR	£26.01	£12	£26.01
Overdraft	£466.74	45.9% APR	£0.00	£0	£94.95
Revolving loan	£879.04	40.9% p.a. (49.5% APR)	£19.78	£48	£879.04

Figure 2 reproduces the low-, medium-, and high-difficulty versions of trial E. Each screen reports the loan type, balance, APR, minimum payment, and late fee for each debt account. The low-difficulty version (Panel A) omits minimum payments and late fees; the medium-difficulty version (Panel B) includes them; and the high-difficulty version (Panel C) additionally reports irrelevant information (e.g., the annual rate alongside the APR). Subjects entered repayment allocations in the final column, with running allocation totals updated automatically. For those receiving robo-advice, the fields were pre-filled with the recommended allocations, although subjects could modify them. The screenshots display the optimal allocations for illustration. Subjects were not shown the interest and fees associated with alternative allocations; treatment subjects were told only the maximum savings achievable in each trial.

Figure 3: Demand for and Utilization of Robo-Advice

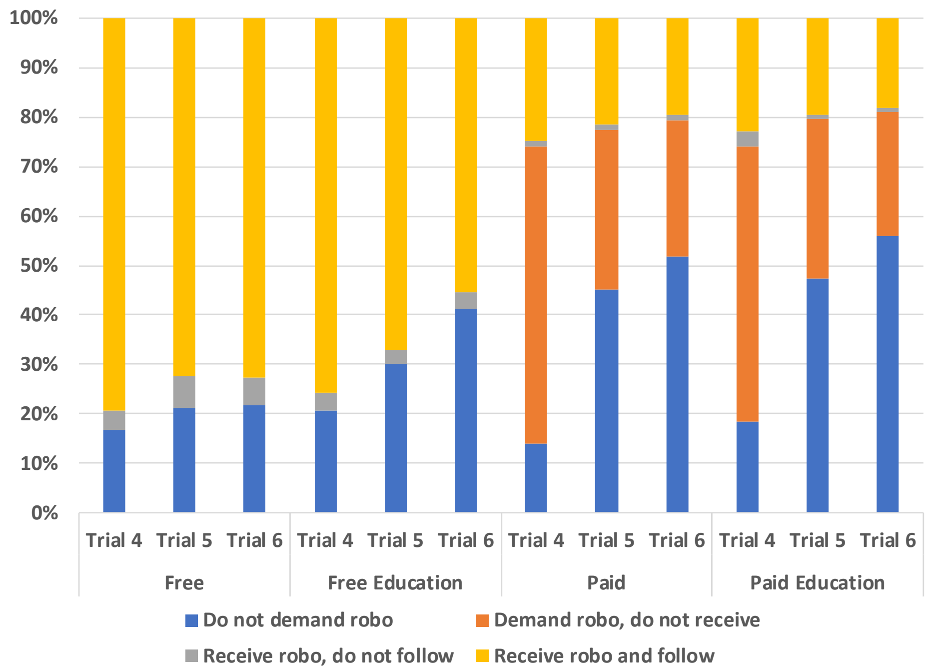


Figure 3 reports the fraction of subjects who: (a) receive and follow robo-advice (yellow bar), (b) receive but do not follow robo-advice (gray bar), (c) demand but do not receive robo-advice (orange bar), and (d) do not demand robo-advice (blue bar). The percentages in the four categories sum to 100%. We report the fractions separately for the four treatment arms, and separately for trials 4, 5, and 6. The fewer subjects whose WTP is above the randomly chosen cost for the paid robo-advice tool, the larger the size of the orange bar relative to the yellow plus gray bars. We observe decreased demand for paid robo-advice in trials 5 and 6, but also higher WTP conditional on positive demand.

Figure 4: Treatment on the Treated: Heterogeneous Effects

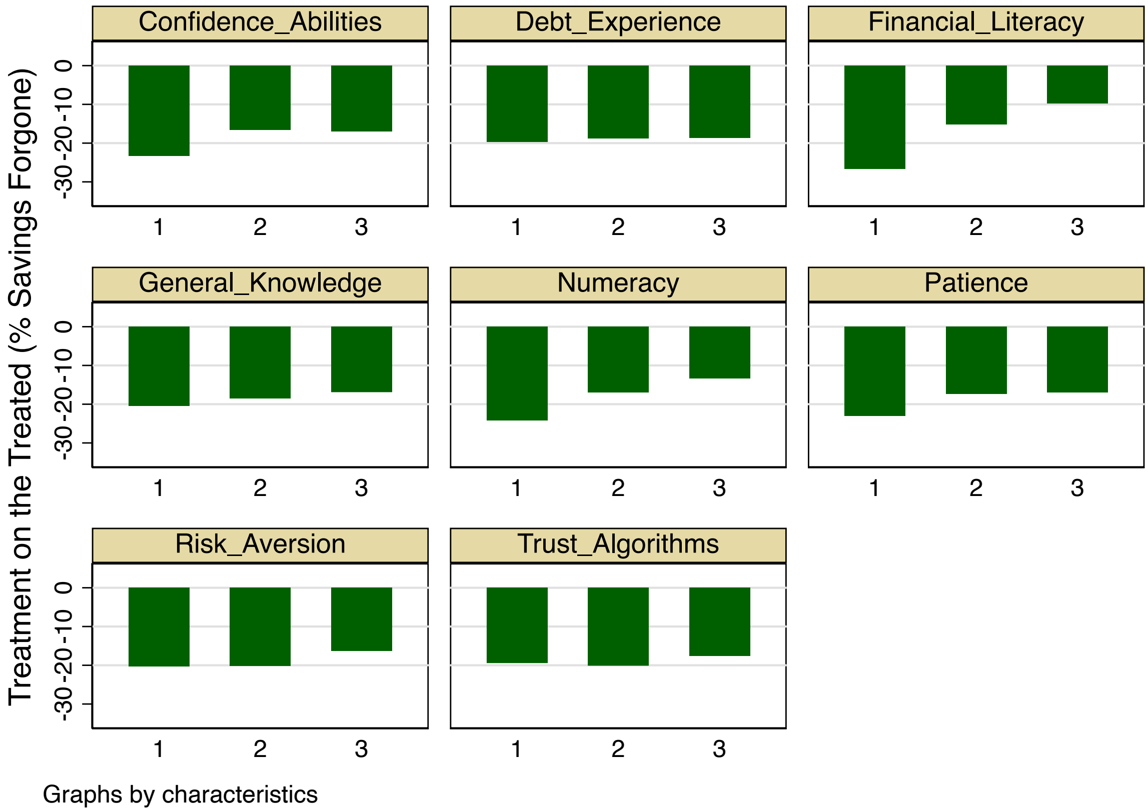


Figure 4 plots the estimated TOT effect of free robo-advice across terciles of subjects sorted based on a set of characteristics we elicit in the post-experiment debriefing survey. We describe the construction of the terciles in Table A.2. The treatment effect is measured in terms of the percentage point reduction of the share of potential savings subjects forgo when they do not make the optimal allocation decision in a loan repayment problem.

Figure 5: Willingness to Pay for Robo Advice versus % Savings Forgone during Pre-Intervention

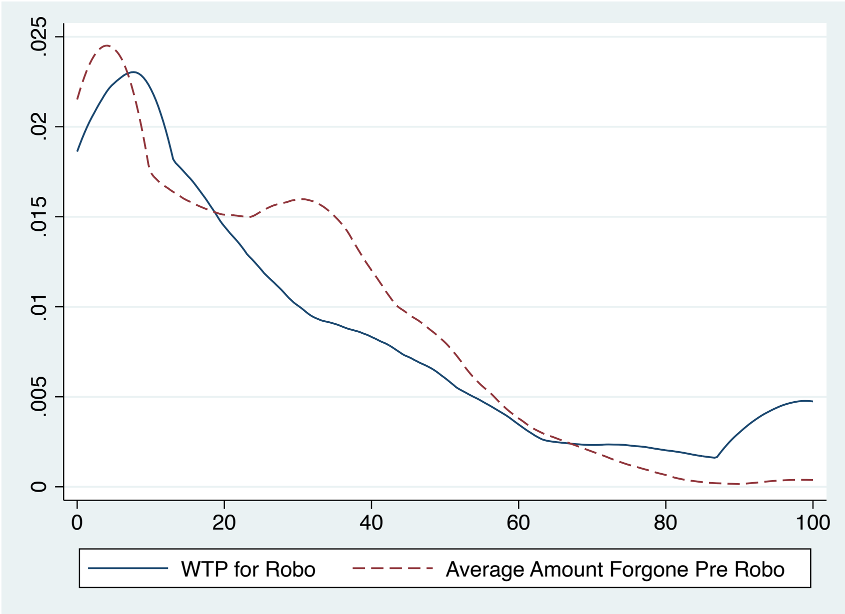


Figure 5 plots the distribution of the willingness to pay (WTP) elicited with an incentive-compatible mechanism in the first trial in which subjects in the paid robo-advice treatment choose to bid for robo-advice (trial 4) against the distribution of the savings forgone by the same subjects in the pre-intervention phase (trials 1-3), when they had to implement allocations without access to robo-advising.

Figure 6: Willingness to Pay for Robo Advice minus % Savings Forgone during Pre-Intervention

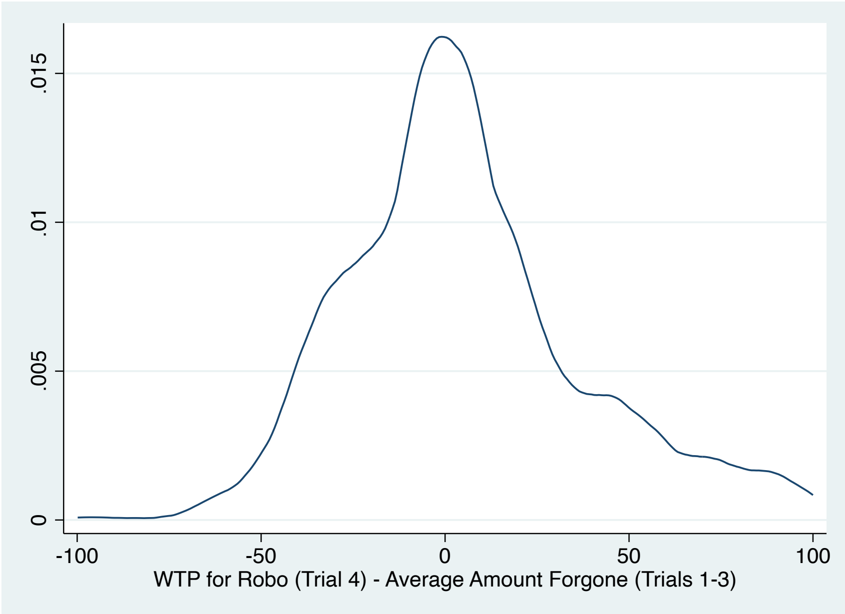


Figure 6 plots the difference between a subject's WTP in trial 4 and the subject's percent savings forgone in each of trials 1-3, for the subset of subjects who bid on paid robo-advice in trial 4.

Table 1. Pre-Intervention Summary Statistics

Sample	N	% Subjects with error	Average % savings forgone	Average pound savings forgone	Average potential spread in interest and fees in pounds	Average pound savings forgone scaled by total debt	% Subjects savings forgone greater than median of random allocations
All Trials	10,269	68.5%	21.9%	5.93	32.03	0.29%	20.6%
All Trials with Error	7,034	100.0%	32.0%	8.65	33.84	0.42%	30.0%
CC Debt	6,861	64.7%	19.9%	5.30	31.93	0.26%	18.7%
CC Debt with Error	4,438	100.0%	30.8%	8.19	33.86	0.40%	29.0%
Any Debt	8,190	67.0%	21.1%	5.68	32.01	0.28%	19.8%
Any Debt with Error	5,489	100.0%	31.5%	8.48	33.90	0.41%	29.6%
Low Difficulty	3,423	62.0%	24.5%	4.84	18.14	0.22%	16.6%
Medium Difficulty	3,423	68.1%	17.9%	5.77	38.53	0.28%	18.0%
High Difficulty	3,423	75.4%	23.3%	7.17	39.42	0.37%	27.1%
2 Loans	3,397	63.9%	26.3%	6.84	20.83	0.27%	25.7%
3 Loans	3,457	69.3%	19.0%	2.46	24.62	0.14%	20.4%
4 Loans	3,415	72.3%	20.5%	8.53	50.68	0.46%	15.6%
A Low	358	57.3%	26.3%	0.06	0.22	0.01%	12.6%
A Medium/High	771	62.4%	29.7%	0.06	0.22	0.01%	16.5%
B Low	366	68.3%	28.7%	0.25	0.88	0.01%	24.9%
B Medium/High	774	68.9%	2.6%	0.41	15.84	0.01%	24.9%
C Low	379	52.8%	34.4%	18.52	53.90	0.74%	31.9%
C Medium/High	749	66.8%	42.0%	21.01	50.05	0.84%	39.4%
D Low	391	45.8%	14.4%	0.76	5.28	0.09%	9.5%
D Medium/High	759	60.1%	9.0%	2.49	27.50	0.28%	26.4%
E Low	382	65.2%	14.9%	2.50	16.72	0.10%	9.2%
E Medium/High	757	81.6%	5.6%	3.62	64.35	0.15%	18.8%
F Low	401	70.8%	31.9%	1.95	6.05	0.05%	19.2%
F Medium/High	767	79.4%	39.5%	2.41	6.05	0.07%	28.0%
G Low	370	74.3%	21.5%	2.80	12.98	0.29%	9.5%
G Medium/High	762	86.2%	9.4%	7.03	74.80	0.72%	5.2%
H Low	349	68.5%	18.0%	5.38	29.81	0.14%	6.3%
H Medium/High	757	76.5%	11.1%	8.67	77.99	0.22%	11.0%
I Low	427	56.4%	29.7%	10.54	35.53	0.52%	24.4%
I Medium/High	750	63.9%	36.8%	13.07	35.53	0.65%	33.3%

Table 1 reports summary statistics for all loan repayment problems completed during the Pre-Intervention stage. N denotes subject-trials, with each subject contributing three observations. We report the fraction of suboptimal allocations ("error"), average % Savings Forgone, savings forgone in pounds, the difference between the best- and worst-case interest and fees, savings forgone scaled by total debt, and the fraction of trials performing worse than the median random allocation. The first row includes all trials; the second restricts the sample to suboptimal allocations. The third and fourth rows repeat these statistics for subjects who used a credit card in the previous 12 months. Remaining columns report results by loan repayment problem.

Table 2. Effects of Robo-Advising: Free and Paid

	Unassisted Choices (Control Group)		Free Robo-Advising		Paid Robo-Advising (WTP adjusted)	
Dependent variable:			TOT	ITT	TOT	ITT
% Savings Forgone	(1)	(2)	(3)	(4)	(5)	(6)
RoboAdvice			-19.55***	-14.60***	30.84***	16.53***
			(-28.71)	(-17.54)	(20.36)	(13.99)
Education Treatment			0.82	2.51***	4.79***	-0.58
			(1.29)	(3.74)	(4.43)	(-0.42)
Medium Financial Literacy	-9.51***	-7.53***	-4.66***	-4.22***	-7.13***	-8.21***
	(-8.30)	(-6.19)	(-6.60)	(-5.64)	(-6.08)	(-6.70)
High Financial Literacy	-14.19***	-11.51***	-7.32***	-6.93***	-11.39***	-13.71***
	(-11.33)	(-8.35)	(-8.23)	(-7.61)	(-7.25)	(-8.49)
Medium Numeracy		-0.39	-2.11***	-1.77**	-2.68**	-2.69*
		(-0.28)	(-2.62)	(-2.10)	(-1.98)	(-1.89)
High Numeracy		-4.26***	-2.87***	-2.20**	-4.87***	-5.49***
		(-3.09)	(-3.43)	(-2.56)	(-3.62)	(-3.78)
Medium Financial Confidence		-1.76	-0.54	-0.17	-3.23***	-3.66***
		(-1.43)	(-0.71)	(-0.21)	(-2.71)	(-2.90)
High Financial Confidence		-0.32	-1.25	-0.89	-1.11	-1.21
		(-0.20)	(-1.25)	(-0.86)	(-0.74)	(-0.73)
Medium Patience		-4.28***	-1.44**	-1.48**	-3.06**	-2.92**
		(-3.72)	(-2.03)	(-1.99)	(-2.71)	(-2.47)
High Patience		-3.15**	-0.51	-0.26	-1.45	-1.99
		(-2.04)	(-0.53)	(-0.26)	(-1.00)	(-1.31)
Medium Risk Aversion		1.73	1.13	0.80	1.74	2.57**
		(1.52)	(1.59)	(1.56)	(2.20)	(-3.36)
High Risk Aversion		0.26	-0.42	-0.37	0.24	0.98
		(0.20)	(-0.51)	(-0.43)	(0.18)	(0.70)
Medium General Knowledge		0.05	0.09	-0.828	0.08	0.02
		(0.04)	(0.12)	(-1.11)	(0.07)	(0.02)
High General Knowledge		-2.77**	-0.93	-0.75	-1.26	-1.21
		(-2.27)	(-1.04)	(-0.08)	(-0.86)	(-0.77)
Trust in Algorithms		-0.24	-0.72	-2.77***	0.87	1.76*
		(-0.23)	(-1.10)	(-4.09)	(0.98)	(1.69)
Male		0.49	0.76	0.91	0.84	2.28**
		(0.47)	(1.19)	(1.36)	(0.82)	(2.11)
Has Debt Outside RCT		-2.35	-0.74	-0.74	-3.99***	-4.34***
		(-1.64)	(-0.92)	(-0.86)	(-3.06)	(-3.16)
Problem FE	✓	✓	✓	✓	✓	✓
Education Category FE	✓	✓	✓	✓	✓	✓
Age Category FE	✓	✓	✓	✓	✓	✓
Adj. R^2	0.223	0.237	0.255	0.176	0.231	0.182
N	6,228	5,940	5,841	5,841	5,679	5,679

Table 2 reports the estimated coefficients in the following set of specifications:

$$\%\ Savings\ Forgone_{i,p} = \alpha_p + \beta_1 RoboAdvice_{i,p} + X_i\gamma + \epsilon_{i,p},$$

where $\%\ Savings\ Forgone_{i,p}$ is defined in Section *2.3.*, X_i includes the set of individual-level characteristics indicated in the table, α_p is a problem fixed effect (e.g., easy version of problem A, medium/hard version of problem A, easy version of problem B, ...), and $RoboAdvice_{i,p}$ defines the experimental condition subjects face. Columns (1)-(2) are estimated using trials 1-9 for the control group; columns (3)-(4) are estimated using data from trials 4-6 in the free robo-advising experiment arms; and columns (5)-(6) are estimated using data from trials 4-6 for subjects in the paid robo-advising experimental arms. In columns (3) and (5), $RoboAdvice_{i,p}$ is a dummy that equals one if subject i obtained robo-advising in trial p, and zero otherwise (TOT). In columns (4) and (6), it equals one if subject i was assigned to one of the robo-advice treatment arms, irrespective of receiving it, and zero otherwise (ITT). In columns (5)-(6), $\%\ Savings\ Forgone_{i,p}$ also includes subjects' elicited WTP for robo-advice (scaled by maximum potential savings). We report t-statistics below estimated coefficients. Standard errors are clustered at the subject level.

Table 3. 2SLS Estimates: TOT Effects of Free Robo-Advising

	Second Stage		First Stage	
Dependent variable:	*% Savings Forgone*		*RoboAdvice?*	
Range:	[0.00%, 100.00%]		{0, 1}	
	(1)	(2)	(3)	(4)
RoboAdvice	-18.16***	-18.67***		
	(-18.24)	(-18.28)		
Education Treatment	0.49	0.57	-0.11***	-0.10***
	(0.87)	(0.98)	(-6.06)	(-5.91)
Exposed Treatment			0.80***	0.78***
			(65.03)	(59.10)
Problem FE	✓	✓	✓	✓
Education Category FE	✓	✓	✓	✓
Age Category FE	✓	✓	✓	✓
Controls from Table 1		✓		✓
F-statistic			4,228.3	3,492.2
N	6,231	5,841	6,231	5,841

Table 3 reports the estimated coefficients in the following 2SLS specification:

$$RoboAdvice_{i,p} = \alpha_p + \beta_1 Exposed\ Treatment_{i,p} + X_i\gamma + \epsilon_{i,p},$$

$$\%\ Savings\ Forgone_{i,p} = \alpha_p + \beta_2 \widehat{RoboAdvice_{i,p}} + X_i\gamma + \epsilon_{i,p},$$

where $\%\ Savings\ Forgone_{i,p}$ is defined in Section *2.3.*, X_i includes the set of individual-level characteristics indicated in Table 1 of the main paper in columns (2) and (4), α_p is a set of fixed effects for each problem by difficulty level (easy versus not easy). $RoboAdvice_{i,p}$ is a dummy that equals one if subject i obtained robo-advising in trial p, and zero otherwise (TOT). *Exposed* $Treatment_{i,p}$ is a dummy that equals 1 if subject i was randomly exposed to the possibility of obtaining robo-advising, and zero otherwise. *Education* $Treatment_{i,p}$ is a dummy that equals 1 if subject i was randomly exposed to the possibility of obtaining robo-advising with education, and zero otherwise. The sample is limited to observations from trials 4-6 where subjects are offered and choose to receive free robo-advising. Note that $\%\ Savings\ Forgone_{i,p}$ has a range from 0 to 100 while $RoboAdvice_{i,p}$ equals either zero or one; the different ranges explain the different magnitudes in the first and second stage regressions. We report t-statistics below estimated coefficients. Standard errors are clustered at the subject level.

Table 4. Robo-Advising Demand, Willingness to Pay, and Compliance

	Seeks Free Robo? (1)	Seeks Paid Robo? (2)	Willingness to Pay (3)	Override Robo? (4)	Prefers Human? (5)
Paid RoboAdvice				0.040**	0.003
				(2.40)	(0.09)
Education Treatment	-0.024	-0.030	0.883	0.012	-0.022
	(-1.17)	(-1.45)	(0.49)	(1.00)	(-0.86)
Medium Financial Literacy	-0.030	-0.067***	-7.953***	-0.009	0.068**
	(-1.25)	(-2.84)	(-3.70)	(-0.65)	(2.26)
High Financial Literacy	-0.012	-0.097**	-16.333***	-0.014	0.077*
	(-0.35)	(-2.62)	(-6.07)	(-0.86)	(1.81)
Medium Numeracy	0.003	-0.020	0.984	-0.025	0.027
	(0.11)	(-0.73)	(0.38)	(-1.64)	(0.76)
High Numeracy	-0.054*	-0.036	-2.797	-0.019	-0.071**
	(-1.79)	(-1.23)	(-1.07)	(-1.17)	(-2.00)
Medium Financial Confidence	-0.039	-0.032	-2.989	0.013	0.041
	(-1.62)	(-1.37)	(-1.34)	(0.91)	(1.33)
High Financial Confidence	-0.038	-0.119***	-0.208	0.009	-0.025
	(-1.05)	(-3.29)	(-0.07)	(0.51)	(-0.58)
Medium Patience	0.012	0.021	-0.005	-0.005	0.044
	(0.50)	(0.90)	(-0.00)	(-0.38)	(1.50)
High Patience	-0.015	0.038	-1.272	0.036*	-0.006
	(-0.44)	(1.27)	(-0.48)	(1.67)	(-0.16)
Medium Risk Aversion	0.031	-0.017	2.607	0.028**	0.031
	(1.31)	(-0.69)	(1.25)	(1.99)	(1.02)
High Risk Aversion	-0.002	-0.016	4.461*	-0.009	0.040
	(-0.07)	(-0.57)	(1.76)	(-0.63)	(1.16)
Medium General Knowledge	-0.025	0.028	-0.661	0.013	-0.040
	(-1.02)	(1.25)	(-0.32)	(0.94)	(-1.34)
High General Knowledge	-0.049	0.005	-0.932	-0.017	0.017
	(-0.51)	(0.16)	(-0.33)	(-1.02)	(0.45)
Trust in Algorithms	0.138***	0.064***	3.264*	-0.031**	-0.195***
	(6.20)	(2.99)	(1.79)	(-2.28)	(-7.15)
Male	-0.014	0.006	7.240***	-0.019	-0.020
	(-0.65)	(0.27)	(3.72)	(-1.61)	(-0.78)
Has Debt Outside RCT	0.049*	-0.028	-0.337	0.018	0.000
	(1.88)	(-1.13)	(-0.15)	(1.23)	(0.01)
Problem FE	✓	✓	✓	✓	✓
Education Category FE	✓	✓	✓	✓	✓
Age Category FE	✓	✓	✓	✓	✓
Adj. R^2	0.133	0.094	0.089	0.042	0.057
N	1,287	1,233	1,028	1,376	1,366
Mean Dependent Variable	0.815	0.834	29.118	0.052	0.336

Table 4 reports the estimated coefficients for the following specification:

$$RoboChoice_i = \alpha + X_i\beta + \epsilon_i,$$

where $RoboChoice_i$ is a variable that measures a different choice about robo-advising in each column. The dependent variable in column (1) is a dummy variable that equals one if subject i sought free robo-advice in trial 4 and zero otherwise (this variable is not defined for subjects who were not offered robo-advice). The dependent variable in column (2) is similar except the focus is on paid robo-advice. The dependent variable in column (3) is the subject's reported willingness to pay (WTP) for robo-advice in trial 4, and the sample is limited to subjects who seek paid robo-advice. WTP is measured as the percentage of maximum potential savings from choosing the optimal allocation that the subject is willing to pay to the robo-advisor; it equals zero for the 15.5% of subjects who demand paid robo-advice but then set WTP to zero. The samples in columns (4) and (5) are limited to subjects that receive free or paid robo-advice. The dependent variable in column (4) is a dummy variable that equals one if subject i received a proposed allocation from the robo-advisor but changed the allocation, and zero otherwise. The dependent variable in column (5) is a dummy that equals one if subject i declared in the debriefing survey that they are willing to pay more to interact with a human advisor (not all subjects answered this question). X_i includes the set of individual-level characteristics indicated in the table. We report t-statistics below estimated coefficients. Standard errors are clustered at the subject level (and hence equivalent to Huber-White standard errors since these specifications only include one observation per subject).

Table 5. Learning-by-Doing versus Learning-by-Imitation

	Control Group $N = 4{,}152$		Free Robo Treatments $N = 8{,}310$		Paid Robo Treatments $N = 8{,}076$	
	Pre	Post	Pre	Post	Pre	Post
% Savings Forgone	21.24***	-2.45***	21.48***	-1.60***	22.54***	-1.26**
	(59.15)	(-3.41)	(75.61)	(-2.82)	(80.19)	(-2.24)
Optimal (0%)?	33.28%***	4.00%***	32.31%***	7.22%***	29.74%***	5.30%***
	(58.35)	(3.51)	(77.45)	(8.65)	(74.40)	(6.63)
Optimal (>0%)?	18.17%***	0.07%	17.59%***	1.25%	19.44%***	-1.23%
	(33.20)	(0.06)	(40.20)	(1.43)	(46.40)	(-1.47)
1/N?	19.68%***	-1.59%	19.75%***	-3.88%***	20.77%***	-1.30%
	(36.52)	(-1.47)	(48.34)	(-4.75)	(51.67)	(-1.62)
Balance Matching?	15.65%***	-0.13%	16.53%***	-3.28%***	16.75%***	-0.60%
	(30.68)	(-0.13)	(46.84)	(-4.65)	(44.79)	(-0.81)
High Balance First?	5.15%***	-0.72%	4.20%***	0.55%	4.22%***	-0.51%
	(16.35)	(-1.14)	(18.60)	(1.22)	(20.05)	(-1.22)
Low Balance First?	7.40%***	-1.27%*	8.93%***	-1.59%***	8.18%***	-1.55%***
	(19.84)	(-1.70)	(30.32)	(-2.70)	(30.20)	(-2.85)
Worst?	0.67%***	-0.37%	0.69%***	-0.27%	0.91%***	-0.10%
	(5.87)	(-1.62)	(8.26)	(-1.60)	(8.60)	(-0.49)
	Partitioning Treatment Group by Treatment Received					
	Treated w/ Education $N = 5{,}550$		Treated w/o Education $N = 5{,}730$		Never Treated $N = 5{,}106$	
	Pre	Post	Pre	Post	Pre	Post
% Savings Forgone	23.51***	-1.85**	23.37***	-1.19*	18.77***	-1.16*
	(63.27)	(-2.49)	(71.62)	(-1.83)	(55.31)	(-1.71)
Optimal (0%)?	28.70%***	7.78%***	26.64%***	6.23%***	38.57%***	4.61%***
	(56.77)	(7.69)	(54.35)	(6.36)	(76.11)	(4.55)
Optimal (>0%)?	17.64%***	3.88%***	19.11%***	-1.99%*	18.73%***	-1.86%*
	(32.82)	(3.61)	(36.78)	(-1.91)	(37.07)	(-1.84)
1/N?	20.89%***	-3.63%***	22.77%***	-2.75%***	16.64%***	-1.16%
	(42.44)	(-3.68)	(43.92)	(-2.65)	(35.25)	(-1.22)
Balance Matching?	18.55%***	-5.24%***	17.93%***	-1.72%**	13.11%***	1.32%
	(41.43)	(-5.85)	(41.37)	(-1.99)	(29.30)	(1.48)
High Balance First?	3.84%***	0.21%	4.34%***	0.57%	4.45%***	-0.76%
	(14.51)	(0.41)	(15.88)	(1.03)	(16.84)	(-1.43)
Low Balance First?	9.24%***	-2.41%***	8.64%***	-0.50%	7.78%***	-1.97%***
	(26.84)	(-3.50)	(23.68)	(-0.68)	(23.96)	(-3.03)
Worst?	1.13%***	-0.60%**	0.57%***	0.16%	0.72%***	-0.19%
	(9.08)	(-2.41)	(5.19)	(0.73)	(6.43)	(-0.85)

In Table 5, coefficients and significance levels come from six sets of eight OLS regressions. The unit of observation is subject-trial. Each regression includes trial-by-difficulty-level fixed effects and participant fixed effects, along with a dummy variable that equals one in the post-intervention stage. Because the sample is limited to debt repayment problems solved during stages 1 and 3, the estimated coefficient on the post dummy variable measures the average change between pre-intervention and post-intervention. The dependent variables are % Savings Forgone and dummy variables for the different repayment strategies. Repayment strategies that are best classified as optimal are further divided into those with 0% savings forgone and those with positive savings forgone. The top panel reports results for the control group, and the free and paid treatment groups (regardless of whether the subject ever sought robo-advice). The bottom panel partitions the treatment group into those who received robo-advice with education at least once during stage 2, those who received robo-advice without education at least once during stage 2, and those in the treatment group who never received robo-advice during stage 2. We report t-statistics below estimated coefficients. Standard errors are clustered at the subject level.

Online Appendix

(Not for publication)

Survey Instrument

This section reproduces the instructions and the key screens of the experiment as they were displayed to participants on the Qualtrics platform. The complete survey instrument is available from the authors.

General Instructions

All subjects read the following instructions before starting the experiment:

> This study is being run by the Financial Conduct Authority. We're interested in understanding how people manage their debts.
>
> A debt is money that you have borrowed from a lender, for example, using a credit card, store card or an overdraft attached to your bank account. Each month that you have not paid off your debt in full, you will have to pay additional money (interest) to the lender. Some lenders also ask you to pay a minimum payment each month, though you are allowed to pay more than this. If there is a minimum payment and you do not pay it, you may have to pay an extra fee and your credit score (which enables you to borrow more money in the future) may be negatively affected.
>
> - We are about to show you 9 debt scenarios. Each scenario involves a different combination of debts with different interest rates.
> - In each scenario, we will ask you to imagine you have set aside a given amount of money this month to pay back part or all of some of these debts.
> - You need to decide how to split the money across the different debts. You should allocate the money to the debts to minimise the amount of interest you will need to pay this month. You should avoid taking action which will negatively affect your credit score. You pay interest on any debts you do not repay.
> - This is the kind of screen you will see in each of these scenarios:

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off...
Credit card	£329.06	25.6% APR	£7.40	£12	0.00
Overdraft	£184.22	38.5% APR	£0.00	£0	0.00
Credit card	£6.25	18.9% APR	£5.00	£12	0.00
Revolving loan	£452.50	5.0% per month (79.5% APR)	£75.00	£50	0.00
Amount still to allocate: £500					Total: £0.00

- You must allocate the whole amount to paying off debts (i.e. you cannot leave any money unallocated).
- There is a correct, or optimal, way to repay these debts. Over the course of 9 trials, if you make the wrong choices, the hypothetical amount of money you could lose in interest and fees is £160.
- You will get your normal payment for taking part. On top of this, you can win an additional bonus of up to £2 in the currency of your panel, which you will get if you make all the right choices in the experiment. For every hypothetical £8 you save in the task, you'll win 10p.
- In some scenarios, you will be offered some help from an automated assistant that will calculate how much to allocate to each debt to minimise the amount of interest and fees you have to pay. You can either accept or reject the use of this automated assistant.

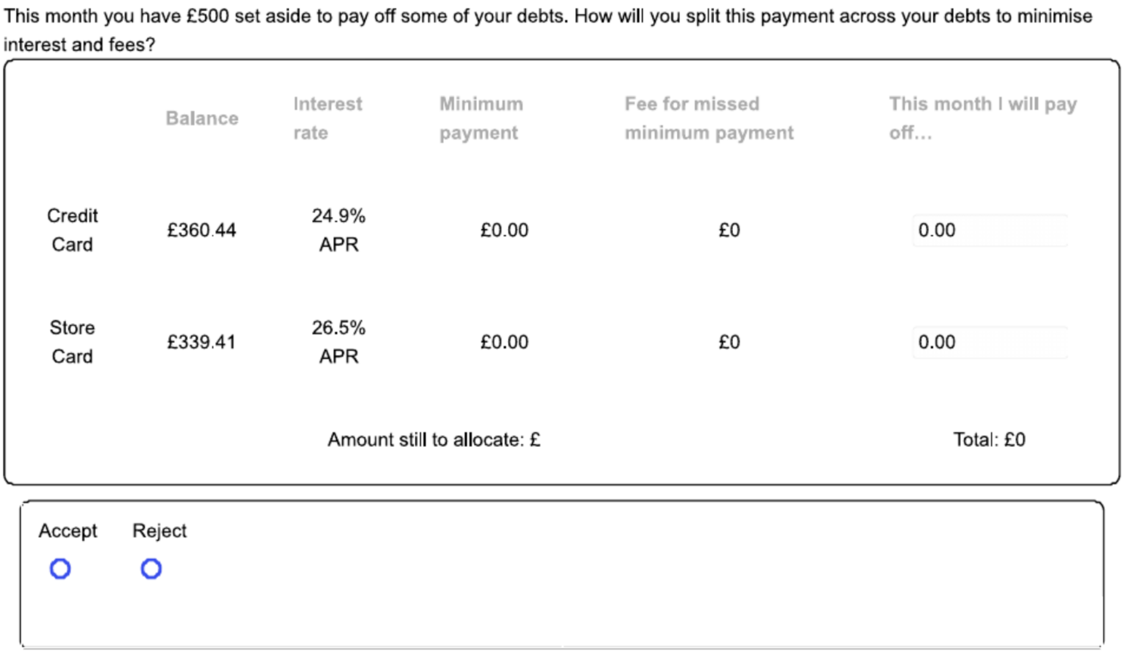
Click next to start the study.

Offer of Robo-Advice

In the trials in which robo-advice was available, subjects saw the following message, in which £XXX was replaced by the trial-specific maximum potential savings in interest and fees:

> Our automated assistant has calculated that you could save up to £XXX this month by optimally allocating repayments across your debts. Would you like to use the automated assistant?

Subjects then chose Accept or Reject below the debt repayment scenario, as shown in the following screen:


This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off...
Credit Card	£360.44	24.9% APR	£0.00	£0	0.00
Store Card	£339.41	26.5% APR	£0.00	£0	0.00

Amount still to allocate: £

Total: £0

Accept Reject

Willingness to Pay for Robo-Advice

In the paid robo-advice arms, subjects who sought robo-advice were asked to state their willingness to pay as follows, in which £XXX was replaced by the trial-specific maximum potential savings in interest and fees. Subjects answered using a slider ranging from £0 to the maximum potential savings:

> How much of the potential £XXX you could save this month would you be willing to pay for the automated assistant?
>
> You should respond truthfully—after you say how much you are willing to pay, the actual price of the assistant is picked randomly.
>
> If what you said you're willing to pay is higher than the random price, you'll buy the automated assistance at that price.

If what you said you're willing to pay is lower than the random price, you won't buy the automated assistance and will pay nothing.

How much of your potential savings are you willing to pay for the automated assistant?

Figure A.1. Explanation Steps in Robo-advising with Education

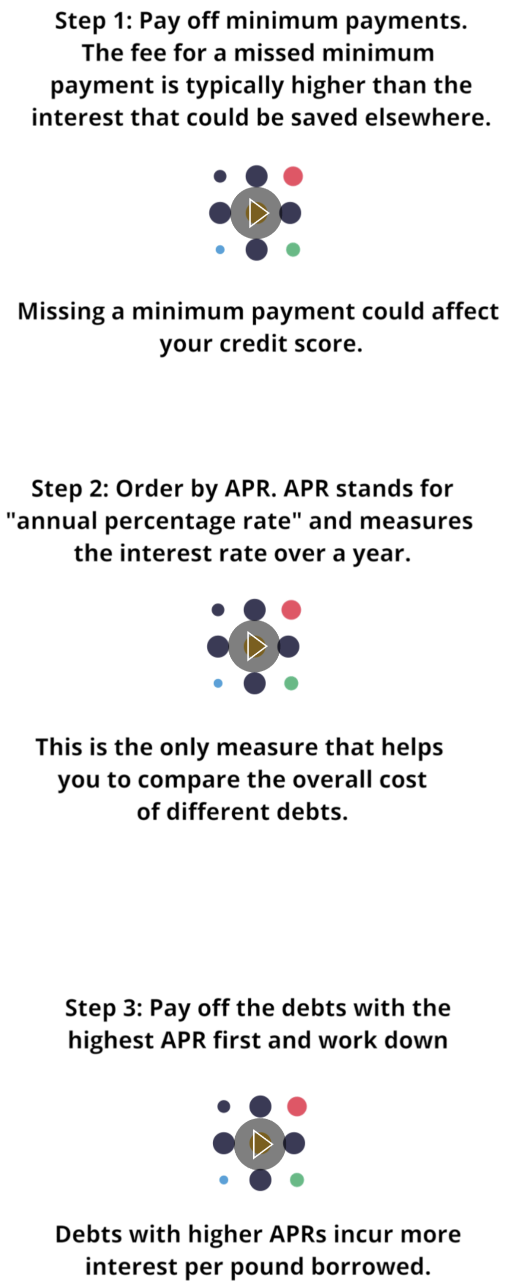
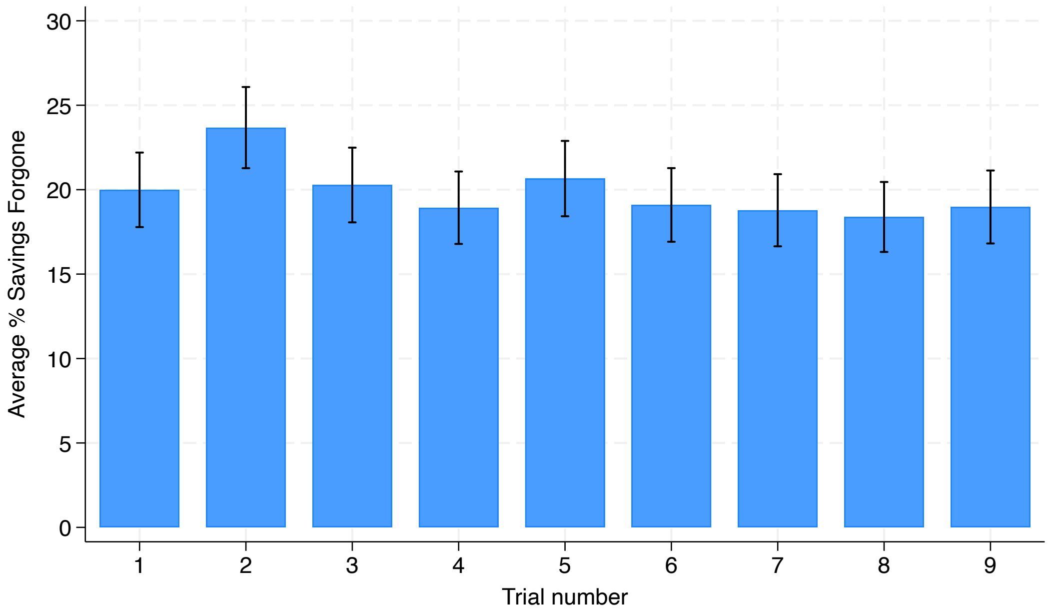


Figure A.1 reproduces the educational material shown to subjects in the robo-advice with education arms (groups 3 and 5) who received robo-advice. The three steps explain the principles behind the robo-advisor's recommended allocations—paying minimum payments first to avoid late fees, and then directing the remaining budget toward the debts with the highest interest rates—before the recommended allocation appeared on the subject's screen.

Figure A.2: Performance of Control Group across Trials



This figure reports the average % Savings Forgone for the control group within each of the nine trials. The point estimates and 95% confidence intervals are obtained by regressing the % Savings Forgone variable on a separate dummy variable for each trial number and omitting the constant term. The average percentage savings forgone across all subjects relative to optimal repayment is 19.86%. Standard errors are clustered at the subject level.

Table A.1. Time Spent on Experiment

Number of trials where % Savings Forgone is worse than median random allocation	Number of subjects	Average duration (minutes)	Average % Savings Forgone
0	179	25.62	5.77
1	187	26.14	14.03
2	156	24.85	23.86
3	91	24.82	32.21
4	44	19.17	39.25
5	18	18.19	45.10
6	14	21.15	48.75
7	2	9.51	61.24
8	1	11.55	92.22
9	0		
Total	692	24.72	19.86

For each subject in the control group, we count the number of times that their repayment allocation results in % Savings Forgone that is greater than the median outcome obtained when repayment allocations are assigned at random. The minimum number of times possible is zero and the maximum is nine. We report the number of subjects that fall into each of the ten bins, the average amount of time spent completing the experiment, and the average % Savings Forgone across the nine trials.

Table A.2. Variable Definitions

Variable	Definition
% Savings Forgone	Actual interest plus late fees minus minimum possible interest plus late fees scaled by the range in possible interest plus late fees; measured in percentage points; varies between 0 and 100.
WTP	Willingness to pay for access to the paid robo-advice tool; measured in percentage points (where WTP in pounds is scaled by the difference between the maximum and minimum possible interest plus late fees); varies between 0 and 100.
WTP of Treated	WTP when the subject received paid robo-advice, and zero otherwise; varies between 0 and 100.
% Net Savings Forgone	= % Savings Forgone + WTP of Treated; WTP of Treated captures cost of paid robo-advice for subset of subjects who seek and receive it; % Net Savings Forgone is equal to % Savings Forgone for all other subjects; because % Savings Forgone (%) and WTP each vary between 0 and 100, % Net Savings Forgone varies between 0 and 200.
Demand Robo-Advice	= 1 if subject seeks access to free or paid robo-advice tool in given trial (during intervention phase); true for 68.07% of subjects in free or paid robo-advice treatment arm during intervention phase.
Receive Robo-Advice	= 1 if subject seeks access to free robo-advice or seeks access to paid-robo advice and sets WTP above the randomly chosen cost; true for 48.94% of all subjects in free or paid robo-advice treatment arm and for 71.90% of subjects with Demand Robo-Advice equal to one.
Change Allocation	= 1 if subject receives recommended allocation from free or paid robo-advice tool but then changes the allocation; only defined for subjects for whom Receive Robo-Advice equals one; true for 5.69% of these subjects.
Prefer Human	= 1 if subject answers "Pay more" when asked "Would you be willing to pay more/less/same for person who is debt advisor?"; true for 32.85% of subjects who were asked the question; question limited to subjects for whom Receive Robo-Advice equals one at least once during intervention phase; = 0 if subject answers "Pay same" (55.34%) or "Pay less" (11.81%).
Trust in Algo	= 1 if subject answers "Automated service" or "Both" when asked "If you were facing problems with paying back multiple debts, would you prefer to work with Automated Service, Human advisor, or Both?"; true for 53.36% of subjects.
Trust in Algo2	= 1 if subject answers "Human advisor" (46.64%) when asked "If you were facing problems with paying back multiple debts, would you prefer to work with Automated service, Human advisor, or Both?"; = 2 if subject answers "Both" (33.24%); = 3 if subject answers "Automated service" (20.13%).
Low Financial Literacy	= 1 if subject answers between 0 and 3 of the 5 financial literacy questions correctly; true for 45.81% of the subjects.
Medium Financial Literacy	= 1 if subject answers 4 of the 5 financial literacy questions correctly; true for 36.90% of subjects.
High Financial Literacy	= 1 if subject answers all 5 financial literacy questions correctly; true for 17.29% of subjects.
Recent Debt	= 1 if subject claims to have used any source of debt in the past 12 months; true for 79.75% of subjects.
Low Debt Experience	= 1 if subject reports having between 0 and 1 sources of debt in the past 12 months; true for 62.90% of subjects.
Medium Debt Experience	= 1 if subject reports having 2 sources of debt in the past 12 months; true for 22.90% of subjects.

Table A.2. Variable Definitions

High Debt Experience	= 1 if subject reports having between 3 and 7 sources of debt in the past 12 months; true for 14.20% of subjects.
Low Numeracy	= 1 if subject answers between 1 and 7 when asked "How confident do you feel working with numbers when you need to in everyday life? Answer on a scale of 1 to 10"; true for 49.14% of subjects.
Medium Numeracy	= 1 if subject answers 8 when asked "How confident do you feel working with numbers when you need to in everyday life? Answer on a scale of 1 to 10"; true for 20.68% of subjects.
High Numeracy	= 1 if subject answers 9 or 10 when asked "How confident do you feel working with numbers when you need to in everyday life? Answer on a scale of 1 to 10"; true for 30.18% of subjects.
Low Financial Confidence	= 1 if subject answers between 1 and 7 when asked "How confident do you feel managing your money? Answer on a scale of 1 to 10"; true for 41.60% of subjects.
Med. Financial Confidence	= 1 if subject answers 8 or 9 when asked "How confident do you feel managing your money? Answer on a scale of 1 to 10"; true for 41.46% of subjects.
High Financial Confidence	= 1 if subject answers 10 when asked "How confident do you feel managing your money? Answer on a scale of 1 to 10"; true for 16.94% of subjects.
Low General Knowledge	= 1 if subject answers between 0 and 3 of the 5 general knowledge questions correctly; true for 57.72% of subjects.
Medium General Knowledge	= 1 if subject answers 4 of the 5 general knowledge questions correctly; true for 27.29% of subjects.
High General Knowledge	= 1 if subject answers all 5 of the general knowledge questions correctly; true for 15.19% of subjects.
Low Patience	= 1 if subject answers between 1 and 6 when asked "Are you willing to give something up today to benefit later? Answer on a scale of 1 to 10"; true for 37.07% of subjects.
Medium Patience	= 1 if subject answers 7 or 8 when asked "Are you willing to give something up today to benefit later? Answer on a scale of 1 to 10"; true for 44.35% of subjects.
High Patience	= 1 if subject answers 9 or 10 when asked "Are you willing to give something up today to benefit later? Answer on a scale of 1 to 10"; true for 18.58% of subjects.
Low Risk Aversion	= 1 if subject answers between 1 and 3 when asked "Are you a person that takes risks with finances? Answer on a scale of 1 to 10"; true for 42.30% of subjects.
Medium Risk Aversion	= 1 if subject answers between 4 and 6 when asked "Are you a person that takes risks with finances? Answer on a scale of 1 to 10"; true for 35.12% of subjects.
High Risk Aversion	= 1 if subject answers between 7 and 10 when asked "Are you a person that takes risks with finances? Answer on a scale of 1 to 10"; true for 22.58% of subjects.
Male	= 1 if subject identifies as Male; true for 48.9% of subjects.
Education Categories	"General Certificate of Secondary Education (GCSE)" (12.88%), "A level" (17.67%) , "Higher Education" (10.96%), "Degree" (36.49%), "Higher degree" (21.41%), and "Other including Overseas" (0.58%).
Age Categories	18-24 (11.36%), 25-34 (19.19%), 35-44 (17.85%), 45-54 (17.24%), 55-64 (22.99%), 65-74 (10.17%), and 75+ (1.20%).

Table A.2. Variable Definitions

Ethnicity Categories	"Asian" (4.38%), "Black" (2.22%), "Mixed" (1.52%), "Other" (0.79%), "Prefer not to say" (0.26%), "White" (50.95%), and not answered (39.88%); missing values arise because ethnicity question was not included in Wave 2.
Language Categories	Response to question about whether English is first language: "English" (72.86%), "Not English" (8.50%), and not answered (18.64%).
Paid Treatment	= 1 if subject is assigned to either paid robo-advice treatment; true for 39.32% of subjects.
Education Treatment	= 1 if subject is assigned to free or paid robo-advice treatment that includes education; true for 39.59% of subjects.
Experimental Wave	= 1 for 40.17% of subjects; 2 for 39.88% of subjects; 3 for 19.95% of subjects.

Table A.3. Demand for and Utilization of Robo-Advice

	Free Robo				Free Robo with Education				Both Free Treatments			
	Trial 4	Trial 5	Trial 6	Trials 4-6	Trial 4	Trial 5	Trial 6	Trials 4-6	Trial 4	Trial 5	Trial 6	Trials 4-6
Receive robo and follow	79.4%	72.4%	72.7%	74.8%	75.8%	67.2%	55.4%	66.1%	77.6%	79.8%	64.0%	70.5%
Receive robo, do not follow	3.9%	6.3%	5.6%	5.3%	3.5%	2.6%	3.5%	3.2%	3.7%	4.5%	4.5%	4.2%
Demand robo, do not receive	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
Do not demand robo	16.7%	21.3%	21.7%	19.9%	20.7%	30.1%	41.2%	30.7%	18.7%	25.7%	31.4%	25.3%
	Paid Robo				Paid Robo with Education				Both Paid Treatments			
	Trial 4	Trial 5	Trial 6	Trials 4-6	Trial 4	Trial 5	Trial 6	Trials 4-6	Trial 4	Trial 5	Trial 6	Trials 4-6
Receive robo and follow	24.8%	21.4%	19.7%	22.0%	23.0%	19.5%	18.2%	20.3%	23.9%	20.5%	18.9%	21.1%
Receive robo, do not follow	1.2%	1.0%	0.9%	1.0%	2.9%	0.9%	0.9%	1.6%	2.0%	1.0%	0.9%	1.3%
Demand robo, do not receive	59.9%	32.5%	27.6%	40.0%	55.6%	32.2%	25.0%	37.6%	57.8%	32.3%	26.3%	38.8%
Do not demand robo	14.1%	45.1%	51.8%	37.0%	18.5%	47.4%	55.9%	40.6%	16.3%	46.2%	53.9%	38.8%

This table reports the fraction of subjects who: (a) receive and follow robo-advice, (b) receive but do not follow robo-advice, (c) demand but do not receive robo-advice, and (d) do not demand robo-advice. The percentages in the four categories sum to 100%. We report the fractions separately for the four treatment arms, and separately for trials 4, 5, and 6. We also report statistics that combine the treatments with and without education. We plot the numbers in this table in Figure 3.

Table A.4. Demand for Paid Robo-Advice and WTP in Trial 4 across Debt-Repayment Scenarios

Scenario	Maximum Avoidable Int. + Fees	N	Do Not Demand Paid Robo	Demand but Set WTP = 0	Demand and Set WTP > 0	All Subjects Mean WTP	All Subjects Median WTP	$WTP > 0$ Mean WTP	$WTP > 0$ Median WTP
A Low	0.21	50	20.0%	32.0%	48.0%	0.06	0.00	0.12	0.11
A Medium/High	0.21	105	44.8%	15.2%	40.0%	0.05	0.00	0.13	0.11
B Low	0.86	60	16.7%	21.7%	61.7%	0.32	0.15	0.53	0.48
B Medium/High	15.86	102	17.6%	14.7%	67.6%	3.70	2.00	5.47	5.00
C Low	53.88	43	18.6%	16.3%	65.1%	11.45	5.01	17.59	12.51
C Medium/High	50.00	104	16.3%	7.7%	76.0%	11.09	5.09	14.61	10.01
D Low	5.30	56	12.5%	19.6%	67.9%	1.63	1.00	2.40	1.99
D Medium/High	27.52	107	7.5%	18.7%	73.8%	6.85	4.99	9.27	5.03
E Low	16.76	45	15.6%	8.9%	75.6%	4.26	2.64	5.64	4.99
E Medium/High	64.38	97	9.3%	9.3%	81.4%	14.18	6.46	17.42	10.07
F Low	6.10	41	9.8%	9.8%	80.5%	2.16	1.65	2.69	2.01
F Medium/High	6.10	114	11.4%	20.2%	68.4%	1.61	1.00	2.35	1.77
G Low	12.98	62	12.9%	9.7%	77.4%	3.38	2.01	4.37	3.26
G Medium/High	74.84	96	9.4%	13.5%	77.1%	15.34	9.82	19.90	14.62
H Low	29.85	44	22.7%	2.3%	75.0%	7.67	3.04	10.23	5.06
H Medium/High	78.01	106	12.3%	4.7%	83.0%	10.74	5.08	12.94	9.26
I Low	35.52	32	15.6%	15.6%	68.8%	7.96	5.08	11.57	9.02
I Medium/High	35.52	82	19.5%	9.8%	70.7%	9.32	5.79	13.17	10.01
All	31.22	1,346	16.3%	13.7%	70.1%	6.61	2.00	9.43	5.01
Corr. with Maximum Avoidable Int. + Fees			-0.329	-0.504	0.565	0.960	0.902	0.939	0.920

This table focuses on choices during trial 4 for the 1,346 subjects assigned to the paid robo-advice treatment arms. The first column reports the maximum avoidable interest and fees for the debt-repayment scenario (calculated as the difference between the "Best outcome" and "Worst outcome" as displayed on in the slides for "Trial A: two debts, low difficulty" through "Trial I: four debts, high difficulty"). Next, we report the number of subjects exposed to each debt-repayment scenario, the fraction who choose not to seek paid robo-advice, the fraction who choose to seek paid robo-advice but then set willingness to pay (WTP) equal to 0 pounds, and the fraction who choose to seek paid robo-advice and set a positive WTP. The final four columns report the mean and median WTP for the full sample of subjects and the subsample of subjects who report a positive WTP. The last row reports the correlation between scenario-level statistics related to demand for paid robo-advice and WTP and maximum avoidable interest and fees.

Table A.5. WTP in Trial 4 versus Pre-Intervention Performance

Panel A.

WTP Range	N	Average WTP	Average Savings Forgone Pre-Intervention
0	184	0.00	15.01
(0,5]	85	2.50	22.00
(5,10]	126	7.42	22.46
(10,15]	92	11.96	22.23
(15,20]	128	17.45	21.31
(20,30]	103	25.00	24.32
(30,40]	114	34.82	25.40
(40,50]	86	45.81	26.76
(50,65]	57	56.25	29.13
(65,100]	152	90.53	28.60
All	1,127	28.33	23.02

Panel B.

Financial Literacy	N	Average WTP	Average Savings Forgone Pre-Intervention
Lowest Tercile	575	33.06	27.96
Middle Tercile	387	25.92	19.57
Highest Tercile	165	17.54	13.90
All	1,127	28.33	23.02

The sample is limited to subjects who seek paid robo-advice in trial 4, including those who seek paid robo-advice but then set WTP equal to 0 pounds. Panel A reports average WTP and the average percent savings forgone during trials 1-3 for different ranges of WTP. The correlation between WTP and percent savings forgone in trials 1-3 is positive (0.181), but the spread in WTP greatly exceeds the spread in average past performance. Panel B reports average WTP and the average percent savings forgone during trials 1-3 for each of the financial literacy terciles. The spread in WTP is 15.52 percentage points and the spread in average percent savings forgone is 14.06, and WTP is always above average percent savings forgone.

Table A.6. Changes in % Savings Forgone and Debt Repayment Strategies by Financial Literacy

	Partitioning of Control Group by Financial Literacy					
	Low Fin. Lit. $N = 1{,}830$		Medium Fin. Lit. $N = 1{,}572$		High Fin. Lit. $N = 750$	
	Pre	Post	Pre	Post	Pre	Post
% Savings Forgone	28.00***	-3.00***	18.12***	-2.69**	12.02***	-2.10
	(50.30)	(-2.70)	(31.25)	(-2.32)	(16.35)	(-1.43)
Optimal (0%)?	16.63%***	3.23%**	40.26%***	5.87%***	59.03%***	2.48%
	(20.78)	(2.02)	(42.57)	(3.11)	(38.68)	(0.81)
Optimal (>0%)?	19.33%***	0.89%	17.58%***	-0.55%	16.50%***	-0.47%
	(23.13)	(0.53)	(19.77)	(-0.31)	(12.83)	(-0.18)
1/N?	28.13%***	-2.38%	15.39%***	-1.77%	7.91%***	0.99%
	(30.26)	(-1.28)	(19.04)	(-1.10)	(8.56)	(0.54)
Balance Matching?	22.65%***	-0.81%	12.04%***	0.86%	6.57%***	-1.40%
	(26.28)	(-0.47)	(15.82)	(0.57)	(7.08)	(-0.76)
High Balance First?	4.12%***	-0.16%	7.30%***	-2.38%**	3.50%***	0.73%
	(9.42)	(-0.18)	(13.06)	(-2.13)	(4.78)	(0.50)
Low Balance First?	8.07%***	-0.30%	7.06%***	-1.78%	6.24%***	-2.07%
	(13.56)	(-0.25)	(11.53)	(-1.45)	(8.06)	(-1.34)
Worst?	1.06%***	-0.48%	0.38%***	-0.25%	0.26%**	-0.26%
	(4.75)	(-1.07)	(2.97)	(-0.98)	(2.02)	(-0.99)
	Partitioning of Treatment Group by Financial Literacy					
	Low Fin. Lit. $N = 7{,}578$		Medium Fin. Lit. $N = 6{,}006$		High Fin. Lit. $N = 2{,}802$	
	Pre	Post	Pre	Post	Pre	Post
% Savings Forgone	28.68***	-0.94	18.35***	-2.37***	12.02***	-1.32
	(96.83)	(-1.59)	(56.54)	(-3.65)	(25.70)	(-1.41)
Optimal (0%)?	17.14%***	3.67%***	36.13%***	9.32%***	57.78%***	6.71%***
	(44.88)	(4.80)	(72.24)	(9.31)	(73.30)	(4.26)
Optimal (>0%)?	17.92%***	2.50%***	20.65%***	-2.18%**	15.33%***	-1.67%
	(38.48)	(2.68)	(42.39)	(-2.23)	(23.12)	(-1.26)
1/N?	27.83%***	-2.01%**	16.11%***	-3.07%***	8.44%***	-2.83%***
	(57.41)	(-2.08)	(37.43)	(-3.57)	(17.66)	(-2.95)
Balance Matching?	22.73%***	-3.11%***	13.27%***	-0.72%	7.60%***	-1.99%
	(52.76)	(-3.61)	(34.79)	(-0.95)	(16.32)	(-2.14)
High Balance First?	3.67%***	0.18%	4.64%***	-0.39%	4.73%***	0.53%
	(17.47)	(0.43)	(17.46)	(-0.73)	(11.67)	(0.65)
Low Balance First?	9.54%***	-1.00%	8.68%***	-2.64%***	5.68%***	-0.87%
	(30.45)	(-1.60)	(26.74)	(-4.06)	(14.04)	(-1.07)
Worst?	1.15%***	-0.22%	0.53%***	-0.32%**	0.44%***	0.12%
	(9.82)	(-0.95)	(6.48)	(-1.96)	(3.12)	(0.42)

In Table A.6, we estimate the same sets of regressions as in Table 5, except that we partition the control group and treatment groups into three groups based on the number of correct answers they gave on the financial literacy questions. We report t-statistics below estimated coefficients. Standard errors are clustered at the subject level.

Debt Repayment Scenarios

The following pages reproduce the 27 debt repayment scenarios used in the experiment: nine scenarios (A through I), each in a low, medium, and high difficulty version. For each scenario, the pages report the debt accounts shown to subjects—the type of loan, its balance, its interest rate, and any minimum payment and fee for a missed minimum payment—along with the budget to be allocated. The "Best outcome (interest)" and "Worst outcome (interest and fees)" values reported at the bottom of each scenario are the totals payable over the next month under the optimal and the interest-and-fee-maximizing allocation, respectively; these two values were not displayed to subjects.

FCA Official

Trial A: two debts, low difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£360.44	24.9% APR	£0.00	£0	£160.59
Store card	£339.41	26.5% APR	£0.00	£0	£339.41

Best outcome (interest):	£3.74	Worst outcome (interest and fees):	£3.95

1

FCA Official

Trial A: two debts, medium difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£360.44	24.9% APR	£10.81	£12	£160.59
Store card	£339.41	26.5% APR	£16.97	£15	£339.41

Best outcome (interest):	£3.74	Worst outcome (interest and fees):	£3.95

2

FCA Official

Trial A: two debts, high difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£360.44	£6.54 interest if minimum payment made (24.9% APR)	£10.81	£12	£160.59
Store card	£339.41	£6.38 interest if minimum payment made (26.5% APR)	£16.97	£15	£339.41

Best outcome (interest):	£3.74	Worst outcome (interest and fees):	£3.95

3

FCA Official

Trial B: two debts, low difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Revolving loan	£2,406.85	39.9% APR	£0.00	£0	£1,000.00
Overdraft	£1,379.64	38.5% APR	£0.00	£0	£0.00

Best outcome (interest):	£77.88	Worst outcome (interest and fees):	£78.74

4

FCA Official

Trial B: two debts, medium difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Revolving loan	£2,406.85	39.9% APR	£214.12	£15	£1,000.00
Overdraft	£1,379.64	38.5% APR	£0.00	£0	£0.00

Best outcome (interest):	£77.88	Worst outcome (interest and fees):	£93.74

5

FCA Official

Trial B: two debts, high difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Revolving loan	£2,406.85	34.0% p.a. (39.9% APR)	£214.12	£15	£1,000.00
Overdraft	£1,379.64	38.5% APR	£0.00	£0	£0.00

Best outcome (interest):	£77.88	Worst outcome (interest and fees):	£93.74

6

FCA Official

Trial C: two debts, low difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Payday loan	£1,800.00	292% APR	£0.00	£0	£1,500.00
Revolving loan	£698.22	66.5% APR	£0.00	£0	£0.00

Best outcome (interest):	£66.48	Worst outcome (interest and fees):	£120.36

FCA Official

Trial C: two debts, medium difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Payday loan	£1,800.00	292% APR	£738.00	£0	£1,394.31
Revolving loan	£698.22	66.5% APR	£105.69	£50	£105.69

Best outcome (interest):	£74.63	Worst outcome (interest and fees):	£124.63

FCA Official

Trial C: two debts, high difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Payday loan	£1,800.00	0.8% per day (292% APR)	£738.00	£0	£1,394.31
Revolving loan	£698.22	66.5% APR	£105.69	£50	£105.69

Best outcome (interest):	£74.63	Worst outcome (interest and fees):	£124.63

FCA Official

Trial D: three debts, low difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£260.00	39.9% APR	£0.00	£0	£260.00
Store card	£146.84	0% APR	£0.00	£0	£0.00
Credit card	£472.26	24.45% APR	£0.00	£0	£240.00

Best outcome (interest):	£4.27	Worst outcome (interest and fees):	£9.57

FCA Official

Trial D: three debts, medium difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£260.00	39.9% APR	£6.50	£12	£260.00
Store card	£146.84	0% APR	£20.00	£25	£20.00
Credit card	£472.26	24.45% APR	£10.63	£12	£220.00

Best outcome (interest):	£4.64	Worst outcome (interest and fees):	£32.16

FCA Official

Trial D: three debts, high difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£260.00	£7.19 interest if minimum payment made (39.9% APR)	£6.50	£12	£260.00
Store card	£146.84	0% APR	£20.00	£25	£20.00
Credit card	£472.26	£8.49 interest if minimum payment made (24.45% APR)	£10.63	£12	£220.00

Best outcome (interest):	£4.64	Worst outcome (interest and fees):	£32.16

Trial E: three debts, low difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£1,040.55	22.5% APR	£0.00	£0	£0.00
Overdraft	£466.74	45.9% APR	£0.00	£0	£120.96
Revolving loan	£879.04	49.5% APR	£0.00	£0	£879.04

Best outcome (interest):	£28.81	Worst outcome (interest and fees):	£45.57

Trial E: three debts, medium difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£1,040.55	22.5% APR	£26.01	£12	£26.01
Overdraft	£466.74	45.9% APR	£0.00	£0	£94.95
Revolving loan	£879.04	49.5% APR	£19.78	£48	£879.04

Best outcome (interest):	£29.19	Worst outcome (interest and fees):	£93.57

Trial E: three debts, high difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£1,040.55	22.5% APR	£26.01	£12	£26.01
Overdraft	£466.74	45.9% APR	£0.00	£0	£94.95
Revolving loan	£879.04	40.9% p.a. (49.5% APR)	£19.78	£48	£879.04

Best outcome (interest):	£29.19	Worst outcome (interest and fees):	£93.57

Trial F: three debts, low difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£586.35	44.0% APR	£0.00	£0	£586.35
Store card	£604.33	27.9% APR	£0.00	£0	£0.00
Revolving loan	£2,510.00	41.9% APR	£0.00	£0	£913.65

Best outcome (interest):	£59.76	Worst outcome (interest and fees):	£65.86

Trial F: three debts, medium difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£586.35	44.0% APR	£0.00	£0	£586.35
Store card	£604.33	27.9% APR	£0.00	£0	£0.00
Revolving loan	£2,510.00	41.9% APR	£296.81	£15	£913.65

Best outcome (interest):	£59.76	Worst outcome (interest and fees):	£65.86

Trial F: three debts, high difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£586.35	1p per £10 borrowed per day (44.0% APR)	£0.00	£0	£586.35
Store card	£604.33	27.9% APR	£0.00	£0	£0.00
Revolving loan	£2,510.00	3.0% per month (41.9% APR)	£296.81	£15	£913.65

Best outcome (interest):	£59.76	Worst outcome (interest and fees):	£65.86

Trial G: four debts, low difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£329.06	25.6% APR	£0.00	£0	£0.00
Overdraft	£184.22	38.5% APR	£0.00	£0	£47.50
Credit card	£6.25	18.9% APR	£0.00	£0	£0.00
Revolving loan	£452.50	79.5% APR	£0.00	£0	£452.50

Best outcome (interest):	£10.16	Worst outcome (interest and fees):	£23.14

Trial G: four debts, medium difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£329.06	25.6% APR	£7.40	£12	£7.40
Overdraft	£184.22	38.5% APR	£0.00	£0	£35.10
Credit card	£6.25	18.9% APR	£5.00	£12	£5.00
Revolving loan	£452.50	79.5% APR	£75.00	£50	£452.50

Best outcome (interest):	£10.29	Worst outcome (interest and fees):	£85.13

Trial G: four debts, high difficulty

This month you have £500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Credit card	£329.06	25.6% APR	£7.40	£12	£7.40
Overdraft	£184.22	38.5% APR	£0.00	£0	£35.10
Credit card	£6.25	18.9% APR	£5.00	£12	£5.00
Revolving loan	£452.50	5.0% per month (79.5% APR)	£75.00	£50	£452.50

Best outcome (interest):	£10.29	Worst outcome (interest and fees):	£85.13

Trial H: four debts, low difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Debt 1: store card	£380.41	25.9% APR	£0.00	£0	£0.00
Debt 2: store card	£412.98	0.0% APR	£0.00	£0	£0.00
Debt 3: flexible loan	£3,000.00	49.0% APR	£0.00	£0	£880.00
Debt 4: payday loan	£120.00	292% APR	£0.00	£0	£120.00

Best outcome (interest):	£79.01	Worst outcome (interest and fees):	£108.86

Trial H: four debts, medium difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Debt 1: store card	£380.41	25.9% APR	£8.56	£25	£8.56
Debt 2: store card	£412.98	0.0% APR	£20.65	£8	£20.65
Debt 3: flexible loan	£3,000.00	49.0% APR	£194.95	£50	£850.79
Debt 4: payday loan	£120.00	292% APR	£20.00	£0	£120.00

Best outcome (interest):	£79.83	Worst outcome (interest and fees):	£157.84

Trial H: four debts, high difficulty

This month you have £1,000 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Debt 1: store card	£380.41	25.9% APR	£8.56	£25	£8.56
Debt 2: store card	£412.98	0.0% APR	£20.65	£8	£20.65
Debt 3: flexible loan	£3,000.00	49.0% APR	£194.95	£50	£850.79
Debt 4: payday loan	£120.00	0.8% per day (292% APR)	£20.00	£0	£120.00

Best outcome (interest):	£79.83	Worst outcome (interest and fees):	£157.84

Trial I: four debts, low difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£183.89	48.9% APR	£0.00	£0	£0.00
Overdraft	£102.78	34.9% APR	£0.00	£0	£0.00
Payday loan	£720.00	148% APR	£0.00	£0	£487.50
Payday loan	£1,012.50	292% APR	£0.00	£0	£1,012.50

Best outcome (interest):	£27.08	Worst outcome (interest and fees):	£62.60

Trial I: four debts, medium difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£183.89	48.9% APR	£0.00	£0	£0.00
Overdraft	£102.78	34.9% APR	£0.00	£0	£0.00
Payday loan	£720.00	148% APR	£88.80	£50	£487.50
Payday loan	£1,012.50	292% APR	£328.50	£15	£1,012.50

Best outcome (interest):	£27.08	Worst outcome (interest and fees):	£62.60

Trial I: four debts, high difficulty

This month you have £1,500 set aside to pay off some of your debts. How will you split this payment across your debts to minimise interest and fees?

	Balance	Interest rate	Minimum payment	Fee for missed minimum payment	This month I will pay off…
Overdraft	£183.89	48.9% APR	£0.00	£0	£0.00
Overdraft	£102.78	34.9% APR	£0.00	£0	£0.00
Payday loan	£720.00	94.4% p.a. (148% APR)	£88.80	£50	£487.50
Payday loan	£1,012.50	0.8% per day (292% APR)	£328.50	£15	£1,012.50

Best outcome (interest):	£27.08	Worst outcome (interest and fees):	£62.60