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Combining nudges and boosts to increase precautionary saving: A large-scale field experiment

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Abstract: Many households lack savings to cushion them from financial shocks. While behavioural economics offers insights into why some households who want to save may fail to do so, successful behavioural interventions to increase precautionary saving are elusive. We incorporated multiple evidence-based "nudges" and "boosts" into a savings account application form at a major commercial bank and designed an electronic marketing communication to explain cumulative risk based on a scalable boost. In a pre-registered randomised controlled trial, these interventions increased saving account uptake by 25-40%. Moreover, some effects were concentrated among low income households.

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Introduction

Many households in multiple countries save no money in a typical year (Demirgüç-Kunt, Klapper, Singer & Oudheuden, 2015). Without a savings buffer, temporary financial shocks may put these households at risk of borrowing at high interest rates (Stavins, 2021), potentially exposing them to problem debt and associated impacts for psychological health (Gathergood, 2012). Lower income households are most vulnerable. Interventions that help households to save, when they have the means, are hence likely to improve financial wellbeing. This paper reports a randomised controlled trial that tested behavioural interventions designed to increase precautionary saving.

Any such intervention assumes that its target households do not save in accordance with their own best financial interests, because behavioural factors result in under-saving. By contrast, the life-cycle hypothesis proposes that households optimally adjust their saving and borrowing to smooth consumption over the life-course, anticipating risks and balancing the associated need for borrowing against expected future income (Modigliani, 1966). Empirical evidence on how far households depart from this ideal remains controversial (Karlan, Ratan & Zinman, 2014), with much argument focused on the adequacy or otherwise of retirement savings (Beirne, Nolan & Roantree, 2020; Poterba, Venti, & Wise, 2011; Scholz, J. K., Seshadri, A., & Khitatrakun, 2006).

The present paper does not contribute directly to this debate, but is premised on the idea that a proportion of households save too little, because of imperfect self-control and underappreciation of the likelihood of a financial shock. The proposition that some households struggle to control expenditure is consistent with an accumulation of evidence on how people respond to transitory changes in income and credit availability. Hall and Mishkin (1982) were among the first to record a high marginal propensity to consume out of transitory income. Although such results appear counter to the life-cycle hypothesis, they can potentially be explained by models that preserve optimisation but incorporate liquidity constraints (Deaton, 1991; Carroll, 1997). However, recent work demonstrates that anticipated decreases in income also result in abrupt changes to consumption (Ganong & Noel, 2019; Gerard & Naritomi, 2021). Such findings are consistent with present-biased households (Laibson, 1997) who struggle to control expenditure when money is available, despite future needs. A notable contribution is Baugh et al. (2021), who analysed millions of transactions aggregated over multiple bank accounts. Households, including those with high

liquidity, responded asymmetrically to anticipated tax payments and refunds. The data were most consistent with a behavioural life-cycle model that incorporates "mental accounting" (Shefrin & Thaler, 1988), in which households struggle with self-control and attempt to exert it by treating different sources of money as non-fungible, with greater or lesser degrees of success.

Successful saving involves not only self-control but motivation. While households engage in precautionary saving driven by uncertainty about future income, there is considerable heterogeneity (Lugilde, Bande & Riveiro, 2019). Unanticipated expenses may also generate financial shocks. When considering a specific potential shock (e.g. property damage not covered by insurance), households may correctly perceive it as unlikely. However, there are many such possibilities and it is not trivial to assess the cumulative probability of experiencing *any one* of them. It is well established that people often underestimate such disjunctive probabilities (e.g. Brockner, Paruchuri, Idson & Higgins, 2002). Moreover, in decisions under uncertainty, individuals are prone to optimism bias, including financial decisions (e.g., Jacobsen et al., 2014; Solnik & Zuo, 2017). Reviewing studies undertaken in developing countries, Karlan, Ratan, & Zinman, (2014) highlight evidence that simple reminders can increase saving. Thus, in addition to present bias and optimism bias, under-saving may result from inattention, especially if low income households pay attention to immediate needs and give insufficient attention to the potential for future income and expenditure shocks.

Given the above, a proportion of households may generally underestimate the likelihood of experiencing a financial shock. Some empirical estimates suggest that as many as six in ten households can expect to face a financial shock each year, with 30% experiencing more than one (Pew Charitable Trusts, 2015). Yet over one third of households in the US and Europe report that they would be unable to handle an unexpected expense[1] without borrowing (Demertzis, Domínguez-Jiménez & Lusardi, 2020; Hasler, Lusardi & Oggero, 2018). Notwithstanding financial constraints to saving, this proportion appears high.

[1] The size of an unexpected expense posed in questions varies across countries and is generally set to 1/12th of the national at-risk-of-poverty threshold.

Overall, evidence points to behavioural factors likely to result in under-saving among a meaningful proportion of the population. Given such a diagnosis, it is natural to consider the possibility of designing behavioural interventions with the aim of increasing financial resilience. In recent years, "nudges" have developed as the predominant approach. Nudges aim to alter the "choice architecture" in ways that behavioural science suggests will steer people in a desired direction (e.g., to start a savings account), but without coercion through financial incentives and always while preserving the liberty to make a different choice (Thaler & Sunstein, 2008). From a cost-benefit perspective, nudges can compare favourably with traditional policy tools, such as tax incentives or other financial instruments (Benartzi et al., 2017). Nudges sometimes produce large impacts on financial decisions. A popular example is defaulting workers into pension plans, which leaves them free to opt-out but increases plan uptake (Madrian & Shea, 2001). However, more recent analysis of interventions designed by self-styled "Nudge Units" working on policy problems has concluded that most nudges they implement either do not work or produce small effect sizes (DellaVigna & Linos, 2022). It is perhaps more challenging to design effective nudges to address endemic policy problems than early proponents of the approach anticipated.

"Boosts" are an alternative to nudges. Boosts do not attempt to direct decisions explicitly, but instead to improve people's competence to make choices that align with their own goals (Hertwig & Grüne-Yanoff, 2017). Techniques include deploying behavioural science to design and test decision aids (calculators, decision trees, etc.) or enrichment of the decision environment (easily accessible advice, training in effective heuristics, etc.). Compared to nudges, boosts give greater priority to individual autonomy. Nevertheless, the outcome of a boost may be in a predictable direction. If people would like to save more but find it hard to devise and stick to a plan, a boost may increase saving. To date, in personal finance applications, there is little evidence by which to evaluate the success of boosts relative to nudges, although training people to use simple heuristics or rules-of-thumb when organising their finances outperforms standard accounting training (Drexler, Fischer, & Schoar, 2014).

A specific difficulty when designing both nudges and boosts is that many rely on pre-existing motivations.[2] For instance, altering the choice architecture of a savings account application

[2] An obvious exception is automatic enrolment, but this is not always feasible. For example, it would not be practical to propose a policy to automatically enrolling all consumers into an emergency savings plan (although this may be worth testing for those in regular employment, see Beshears et al., 2020).

form requires that consumers are motivated to access the form in the first place. We located few empirical tests of ways to motivate consumers to save for emergencies. One exception is Dur, Fleming, van Garderen and van Lent (2021) who tested the effect of (truthfully) informing relevant households that their savings buffer is lower than other households in their neighbourhood. This "social norm" treatment was informed by successful interventions in other domains and led to increased visits to savings account websites, but there was no effect on savings account uptake.

We contend that many households under-save and that there are behavioural reasons for this, but that it is not straightforward to design effective behavioural interventions to address the problem. One approach is to combine multiple interventions for which there is some evidential support into a larger treatment. This is the approach we take. Combining multiple behavioural interventions into a single treatment increases the likelihood of impact. It also has a cost, because it becomes impossible to infer which interventions are responsible for any effects observed. Yet it is a mistake to dismiss behavioural approaches to policy problems on account of small effect sizes if combining interventions into a behavioural "package" generates meaningful improvements. Furthermore, a null finding when testing a combination of evidence-based behavioural interventions arguably has stronger implications for the usefulness of the approach.

Our contribution is to design and test two novel treatments to increase savings account uptake for use against financial shocks. The treatments were tested in a large, pre-registered randomised controlled trial (RCT). The first treatment was a combined nudge-and-boost consisting of multiple behavioural interventions. The second was a communication designed to engage precautionary motivation to save. The nudge-and-boost package featured multiple changes to a bank's online saving account application form, including changes to the order in which questions were asked and an interactive calculator to boost understanding of savings accumulation. The communication was an animated email that highlighted the cumulative risk of financial shocks. It illustrated that although the probability of a specific financial shock might be low, the disjunctive probability of any financial shock is much higher. Both treatments were informed by both previous behavioural trials on saving behaviour and a diagnostic analysis of saving behaviour among the target population based on pre-existing survey data.

The study was commissioned by the Competition and Consumer Protection Commission in Ireland and was facilitated by Bank of Ireland, a major commercial bank. It consisted of two six-month field trials run concurrently. The first was a large-scale RCT which began with emails sent to over 160,000 of Bank of Ireland's customers and tested both treatments. The second was a smaller-scale randomised experiment of the nudge-and-boost treatment among "organic" traffic to the bank's online saving account application form (i.e. customers who engaged online without having received any communications). The design, our directional hypotheses and outcomes of interest were pre-registered on the Open Science Framework (Munafó et al., 2017).

Results from the large-scale RCT show that both treatments led to significant increases in savings account uptake. Enhancing the application form resulted in a 27% increase in the number of customers who opened a savings account, with effects significant compared to a no-contact group who were otherwise eligible for the trial but were not enrolled and to an active control treatment. The cumulative risk email led to a 20% increase in account openings, compared to both control groups. In the smaller test among organic traffic, the effect of the enhanced application form was even larger: a 40% increase in the number of customers who completed the process to open an account. The enhanced application led to lower monthly deposits and totals saved. However, analysis of socio-demographic data showed that this difference was driven by differences in the incomes of those who had opened accounts; the nudge-and-boost treatment increased the number of savers in lower income bands.

Importantly from a welfare perspective, we find no evidence for negative spillover effects (see also Beshears, Choi, Laibson, Madrian & Skimmyhorn, 2022). [3] At the end of the trial period, customers who opened a savings account in the treatment conditions were no more likely to hold debt at the bank than customers in the control conditions, and no more likely to enter an overdraft on their current account. We also find no evidence for substitution effects, with no difference between treatments in current account balances nor in the likelihood of

[3] Ethical issues related to conducting a trial on real saving behaviour were given careful consideration from the outset of the study, including by ESRI's Research Ethics Committee. Customers who showed any sign of financial difficulty were automatically excluded from enrolment. Indicators of negative spillovers (e.g. increased borrowing or current account overdrafts) were assessed halfway through the trial, with agreement by all parties that the trial would cease should there be evidence of consumer detriment among treatment groups. All data were fully anonymised prior to transfer and analysis.

opening another savings account with the bank. Moreover, the target bank account had the highest interest rate in the market at the time of the study, meaning it is unlikely any customers would have opened a savings account elsewhere over the trial period. Together, these findings highlight the potential for behavioural treatments to improve consumer financial wellbeing.

The rest of this paper is structured as follows. Section 1 describes the diagnostic analysis of saving behaviour that informed the design both treatments. Section 2 gives details on the treatments. Section 3 describes the sample and the timing and context for the trial. Section 4 presents the results. Section 5 concludes.

1. Diagnostic Analysis of Saving Behaviour

We informed the design of the treatments in two ways. First, we conducted a narrative review of previous trials that have aimed to help consumers save. Second, we utilised survey data the funder had recently collected. The survey included measures of self-reported saving behaviour and responses to psychological characteristics, such as impulsivity.

1.1 Literature Review

Our review of previous research focused almost exclusively on experiments and studies that aimed to promote general saving rather than saving for retirement. We omit studies that tested financial incentives to save, such as lotteries or matched contributions. While there is good evidence that these treatments can be effective (e.g. Atalay et al., 2014; Cooper et al., 2018; Gertler et al., 2018; Schaner, 2016; Wang et al., 2018), our aim was to test treatments that could be enacted at scale by the regulator. We also omit tests of financial education. In addition to meta-analyses showing little effect on saving behaviour, particularly for lower income individuals (Kaiser & Menkho, 2016; Fernandes, Lynch & Netemeyer, 2014), large and expensive educational interventions were beyond our scope. The remaining treatments that have shown promise for increasing saving can be summarised under three headings: mental accounting, pre-commitments and inattention.

1.1.1 Mental accounting

Mental accounting describes how consumers psychologically organise, evaluate and keep track of their finances (Kahneman & Tversky, 1984; Thaler 1985, 1999). People deviate from treating money as fungible, instead creating boundaries by assigning different activities to specific categories, which in turn can constrain spending (e.g. money for entertainment, money for food, and so on). However, mental accounting can be a barrier to saving if consumers do not create a mental boundary to build and maintain savings. There is strong evidence that encouraging them to do so is effective at increasing savings rates. For example, encouraging consumers to set a savings goal has proven effective in field trials in multiple low and lower-middle income countries (Aggarwal et al., 2020; Aker et al., 2020; Soman & Cheema, 2011; Soman & Zhao, 2011). One trial in Kenya tested prompts to create goals for an emergency saving fund among a sample of 600 vulnerable women (Jones & Gong, 2021). Results showed increases in saving, with no negative spillovers on other finances. The women who began to save for emergencies subsequently demonstrated fewer risky coping behaviours in response to financial shocks, suggesting that the account was used as intended.

How savings goals within a mental account are framed also matters. A field trial in the US with almost 9,000 participants showed that presenting equivalent goals on a daily rather than monthly scale (e.g. save $5 per day vs. $150 per month) increases uptake of a fintech savings app (Hershfield, Shu & Bernartzi, 2020). The framing effect was particularly pronounced among lower income earners. These findings imply that consumers may not have a good intuitions about the relationship between regular savings and how they accumulate over time (see also McGowan, Denny & Lunn, 2022). Lab and online studies suggest complementary effects of goal scale: students report being more willing to forego spending in order to save when a larger goal is divided into subgoals, and saving is rated as more important when benefits are framed on larger scales (e.g. interest earned over 2 years rather than weekly or monthly earnings; Colby & Shapman, 2013; Common Cents Lab, 2018).

Other ways in which people engage in mental accounting and evaluate their earmarked savings can influence efficacy (Soman & Zhao, 2011). For example, specific goals are more effective when people think about their reasons for saving, rather than how they might save (Ülkümen & Cheema, 2011). Finally, if people judge an earmarked account to be for a particularly "responsible" use, such as for their child's education, they tend to preserve those savings in favour of borrowing at high interest rates to handle financial shocks (Amar et al., 2011; Sussman & O' Brien, 2015).

1.1.2 Pre-commitments

While individuals may be present biased (Laibson, 1997; Frederick, Loewenstein & O' Donoghue, 2002), some are aware of their own tendency and willing to sign up to mitigate it. People will commit to saving in advance of the date on which they start saving and to future increases in savings rates, particularly if automatic transfers are feasible, as in the highly-cited Save More Tomorrow programme (Thaler & Bernartzi, 2004; see also Rogers & Bazerman, 2008). Pre-commitment to specific conditions, such as withdrawal restrictions before a specified date or before a specific savings total has been reached, can increase totals saved (Ashraf, Karlan & Yin, 2010; Beshears et al., 2015; Brune et al., 2017). However, the evidence concerning willingness to engage with such restrictions is mixed (e.g. Afzal et al., 2019; Beshears et al., 2020). Some consumers may prefer early withdrawal penalties, all else equal, but lower-income consumers and those who more steeply discount the future may avoid programmes that impose such external restrictions (e.g. Karlan & Linden, 2014).

Instead “soft” commitments, such as pledges to save for specific reasons, can be effective for these groups (Burke, Luoto & Perez-Arce, 2018).

1.1.3 Inattention

A third psychological barrier is that consumers may simply not think about saving; they may be inattentive to their future finances and prone to inaction (Civelli, Deck & Tutino, 2019; Madrian & Shea, 2011). In the absence of automatic transfers or the possibility to default consumers into savings programmes, multiple studies show that issuing prompts to remind consumers of their savings goal can boost savings (e.g. Ashraf et al., 2010; Rodrígues & Saavedra, 2015). Such reminders are more effective when tied to a personalised goal or if future expenditure needs are made salient (Azevedo et al., 2019; Karlan, McConnell, Mullainathan & Zinman, 2016).

However, these interventions rely on consumers already setting a savings goal. Few studies investigate ways to motivate consumers to save. One exception is emerging research from psychology which seeks to reduce the “psychological distance” between the consumer’s current self and their future self. In a lab study, participants who were shown images of themselves digitally rendered to look older reported feeling closer to their future self and were more willing to invest a hypothetical windfall than spend it (Hershfield et al., 2011). Field trials support this mechanism, showing that encouraging workers to consider their future-self can increase pension contribution rates (Bryan & Hershfield, 2013; Shah, Hershfield, Munguia Gomez & Fishbane, 2022).

1.1.4 Summary

Our narrative review identified multiple candidate treatments for encouraging precautionary saving. Consumers could be nudged to earmark money specifically to absorb financial shocks, offered opt-in “soft” savings rules (not hard withdrawal restrictions), encouraged to choose a saving start date in the future rather than the immediate term, or their intuitions about how savings accumulate over time could be boosted with interactive decision tools. Each of these treatments could be tested individually but estimates of effect sizes from individual treatments tend to be small, if effective. Instead, we opted to combine them into one larger intervention. In doing so, we provide a stronger test for whether existing behavioural science levers hold any promise for increasing precautionary savings rates in the

target population; if a combination of multiple interventions identified in the literature as promising fails to encourage saving, there is likely little hope for individual ones.

1.2 Analysis of Existing Survey Data

Most of the research cited above was conducted in the US or in low/lower-income countries. One possibility for a null effect from the combined behavioural package treatment is that the target population is simply very different to these populations. To better understand the target population, we analysed microdata from a previous survey commissioned by the regulator in Ireland. The Competition and Consumer Protection Commission (CCPC) Financial Capability and Wellbeing Study (2018) is a population-based survey that closely followed similar ones conducted in Norway, Australia, New Zealand and Canada (e.g. ANZ, 2018; Kempson & Poppe, 2018). A nationally-representative sample of 1,500 adults answered 65 questions designed to assess financial wellbeing (e.g. about meeting commitments, being financially comfortable and feeling resilient for the future), behaviour (e.g. borrowing, spending, saving), financial literacy and psychological factors (e.g. financial locus of control), in addition to socio-demographic questions.

We were interested in the psychological characteristics that differentiated savers and non-savers. Our target population was consumers with the financial capacity to save but who would likely rely on borrowing to face financial shocks. To identify these consumers, we first excluded those categorised in the survey as "Struggling" (who do not have the capacity to save) and "Secure" (who typically have high levels of wealth). We then excluded respondents who reported that they struggle to pay bills "every now and then," "often" or "very often" and "rarely" or "never" have money left over after necessary expenses. We categorised the remaining 476 respondents as "Non-Savers" if they reported rarely or never saving money and "Savers" otherwise.

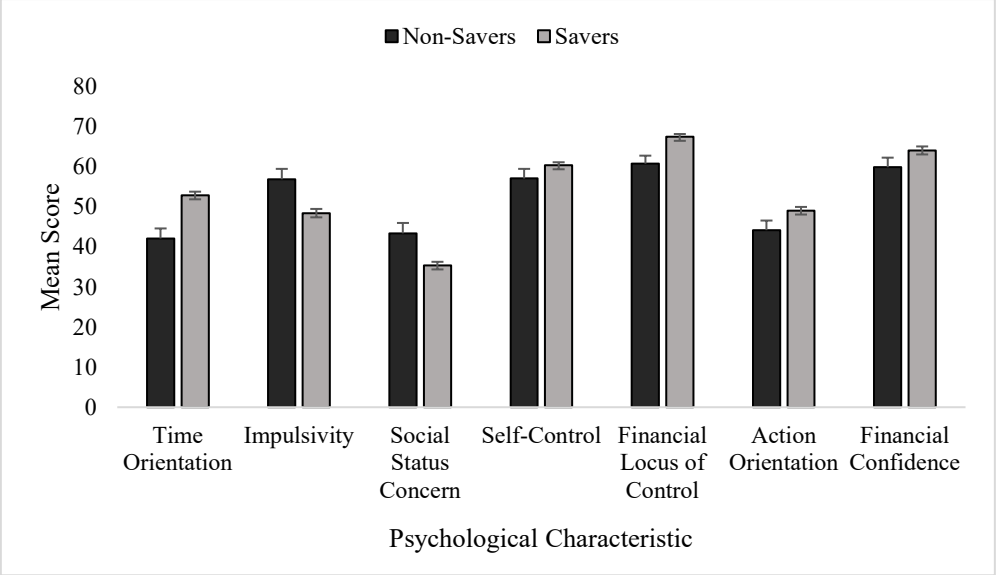


Figure 1. Average scores on psychological characteristics in the CCPC (2018) Financial Wellbeing and Capability Study. Error bars are the standard error of the mean.

Figure 1 shows the average scores for each of these groups on the seven psychological characteristics recorded in the survey: time orientation (i.e. extent to which they think about the future versus the present), impulsivity, concern about their social status, self-control, financial locus of control (i.e. extent to which they believe their financial outcomes are determined by their own actions), action orientation (i.e. extent to which they act quickly rather than procrastinate) and confidence in their financial capability. We ran multiple bivariate logistic regression models predicting status as a Saver, controlling for socio-demographic characteristics[4], from standardised scores on each of the measures (reported in the Appendix). Results show significant differences on all factors, except action orientation and financial confidence. A multivariate model (Model 1, Table 1) shows significant differences only on time orientation, impulsivity and social status concern. Hence, consumers in Ireland who think less about the future, act more on impulse and have greater concern about what others think of them are less likely to be High Savers.

[4] Being a man was the only socio-demographic predictor reliably associated with not saving when psychological factors were included in models. Note that the survey did not include measures of risk aversion.

Table 1. Logistic Regression Predicting Status as a Saver in CCPC (2018) Survey Data

	1
Time Orientation (TO)	-0.02**
	(0.01)
Impulsivity (Imp)	-0.02**
	(0.01)
Social Status Concern (SSC)	0.03***
	(0.01)
Self-Control (SC)	0.01
	(0.01)
Locus of Control (LOC)	-0.02
	(0.01)
Action Orientation (AO)	0.00
	(0.01)
Financial Confidence (FC)	0.00
	(0.01)
Socio-Demographic Controls	Yes
N	476

*$p < .1$; **$p < .05$; ***$p < .01$.

Source: Data collected in CCPC (2018)

1.2.1 Summary

One finding from this analysis worth noting is that self-control did not predict saving behaviour once other psychological factors were included in models. The difference between impulsivity and self-control is important. Impulsivity refers to a tendency to act without consideration of the consequences whereas a lack of self-control implies an inability to refrain from the behaviour despite being aware of the negative consequences. Table 1 suggests that it is not the case that Non-Savers fail to save despite being aware of the consequences of not having a financial buffer against shocks, but instead that they do not consider the consequences. Hence encouraging saving in the target population may rely on prompting consumers to think more about the consequences of not saving, such as not having the capacity to absorb a financial shock. Such a treatment would need to be administered *before* consumers access savings account application forms; if they're not thinking about the

consequences of being without a savings buffer, they're unlikely to access such forms in the first instance.

From our findings this section, we designed two treatments to encourage saving for financial shocks outlined in Section 2.

3. Treatments

Our first intervention was to apply multiple treatments to the application form used by the bank and was heavily informed by the review of previous trials in Section 1.1. The second was a communications treatment informed by the finding from Section 1.2 that non-savers and savers have different psychological profiles and aimed to make salient the risks of not holding precautionary savings. Our hypothesis, trial design and analysis plan was pre-registered (https://osf.io/bnwpg/).

2.1 Behaviourally-Informed Application Form

Our first treatment was to enhance the bank's existing "GoalSaver" account application form. The existing application form (hereafter, "control application") was already somewhat "behaviourally-aligned" (Lourenço, Ciriolo, Almeida & Dessart, 2016): it encouraged consumers to think about the specific goal they are saving for, including an option to save for a "rainy day" fund. The account offered instant access to savings and required customers to set up an automatic, monthly payment (i.e. direct debit) from their current account to the savings account (between €20 and €2,000). The amount was fixed each month but could be altered on request.

Our treated application form (hereafter, "behavioural application") featured a combination of nudges and boosts (Hertwig & Grüne-Yanoff, 2017). Both forms asked customers what goal they were saving for and offered a list of options. Since the framing of savings goals and the reasons for saving are important, we reframed the "rainy day" goal to "unexpected need" – a "rainy day" may have fuzzier boundaries (Burke et al., 2018; Ülkümen & Cheema, 2011). Both forms then asked customers to input their overall savings target. Customers completing the behavioural form who selected the unexpected need goal were shown a tip featuring standard financial advice that their unexpected need fund should cover 3-6 months of expenses. Customers who did not select "unexpected need" for their savings goal (e.g. selected a car or holiday instead) were asked to consider adding 10% to their savings goal to help with any unexpected needs that crop up, without these getting in the way of their savings target (informed by Sussman & O' Brien, 2015).

In the control application, customers are asked when they would like to start saving on the final page of the application. In the behavioural application, this question was moved to the first page, together with an additional tip to choose a date soon after a regular payday. Our

rationale for this change was that encouraging customers to think about their savings start date sooner in the process would act as a form of psychological commitment to saving (e.g. Rogers & Bazerman, 2008; see also Beshears, Dai, Milkman & Benartzi, 2021).

We also developed two additional features for the form. The first was an interactive calculator, to boost understanding of the relationship between amount saved per month, time and total saved. Having already decided a target savings total, consumers were asked to input either a target date for reaching their goal, or an amount they were planning to save per month. The calculator then generated the missing value, factoring in the low level of annual interest at 0.25% (e.g. the amount they would need to save per month in order to reach their target total by their desired date). Customers could use the calculator as many times as they liked. Similar calculators can improve consumers' estimates of pension contributions (McGowan, Lunn & Robertson, 2019).

The second additional feature was a "pledge tool" for customers saving for an unexpected need. Applicants were encouraged to think about reasons they judged acceptable for withdrawing from their savings and could select from a list of options (e.g. a household repair). They were also encouraged to think about reasons they might be tempted by but later regret a withdrawal, again with a list of options (e.g. concert tickets). Customers were informed that use of this tool would not affect access to their funds and that it functioned solely to help them think about these reasons ahead of time. Hence the tool was a "soft" commitment device (Burke et al., 2018). Both the calculator and the pledge tool were optional; when asked if they would like to use either, both response options were defaulted to "no."

Other changes to the behavioural application included its descriptions on the landing page of the bank's website, to highlight that the account uses evidence from psychology and behavioural economics and that customers can "get help deciding how much to save and flexibility over when [they] start and when [they] can withdraw." Table 2 summarises the differences between the control and behavioural form.[5] Note that table shows the multitude

[5] We also sought to test an automatic escalation feature, whereby customers could pre-commit to their direct debit increasing by 10% after saving for three months. However this was not possible to implement in the desired form – customers were required to visit a bank branch or phone the bank's helpline to change their direct debit.

of treatments that could have been tested in individual trials. Individual tests of treatments allows for mechanisms to be more cleanly isolated, but may also lead to small effect sizes (DellaVigna & Linos, 2022). Our aim is to test whether combining promising treatments can generate larger effects.

Table 2. *Summary of the Application Form Treatment*

	Control	Behavioural	Example Supporting References
Account Description	Advertises general guides with hints and tips	Highlights use of psychology and behavioural economics Offers help deciding how much to save Emphasises flexibility of start dates and withdrawals	Afzal et al. (2019) Karlan & Linden (2014)
Goal Framing	Rainy Day	Unexpected Need (with tip for how much to save)	Ülkümen & Cheema (2011)
Savings Target	n/a	Financial advice on how much to save	Partner bank website
Mental Accounting	n/a	Encouraged to save 10% financial shock buffer if saving for other goal	Sussman & O' Brien (2015)
Start Date	Decided at end of form	Decided at beginning of form (with advice on when to start)	Rogers & Bazerman (2008) Thaler & Bernartzi (2004)
Interactive Calculator	n/a	Optional addition to help estimate how much to save per month	Hershfield et al. (2020) McGowan et al. (2019)
Pledge Tool	n/a	Optional "soft" commitment to reasons for withdrawal	Burke et al. (2018)

2.2 Communication Treatment

The application form treatment was designed to encourage consumers who investigated the application process to complete it. Thus, this could only be effective for consumers already partly motivated. However, our analysis of the CCPC's survey data (Section 1.2) suggested that non-savers have a lower propensity than savers to think about the future and the consequences of actions they take, implying that inattention to the need for emergency savings may be a barrier among the target population. Hence we developed a communication treatment that aimed to encourage consumers to appreciate the benefit of saving for financial shocks.

As outlined in Section 1.1, few studies have aimed to motivate saving. Two exceptions are recent tests of (1) reducing the psychological distance between the consumer's current and future self on saving for retirement (Shah et al., 2022) and (2) a social norm nudge to encourage saving (Dur et al., 2021). We did not seek to test these treatments. From a theoretical perspective, the psychological distance intervention is less suited to financial shocks, since the "future" self is less determined; retirement typically occurs at a fixed and expected age, whereas financial shocks are, by definition, unexpected. This intervention also posed practical difficulties. It would not have been feasible to digitally render images of all the bank's customers (Hershfield et al., 2011), or to have all customers complete stories about their envisioned future (Shah et al., 2022). The social norm treatment had a precisely-estimated null effect (Dur et al., 2021) and, moreover, research on peer information and saving for retirements suggests possible backfire effects (Beshears, Choi, Laibson, Madrian & Milkman, 2015).

Instead, we developed a novel intervention. People often underestimate the likelihood of bad outcomes and overestimate their ability to deal with them should they occur (e.g. Shepperd, Pogge & Howell, 2017; Jefferson, Bortolotti & Kuzmanovic, 2017). This combination of optimism and overconfidence can be especially problematic for financial behaviour, because when people are optimistic and confident about the future, they are more willing to spend and borrow and less likely to save (e.g. Brown & Taylor, 2006). This optimism bias for financial decisions can be further exacerbated by uncertainty, which is inherent in financial shocks (e.g. Solnik & Zuo, 2017).

Hence, the extent to which people believe that they will be affected by an unexpected expense is likely to be distorted by cognitive biases. For example, whether people can easily bring examples to mind influences their judgement of the likelihood of an event. This "availability heuristic" influences insurance uptake and investment decisions (Kliger & Kudryavtsev 2010; Simonsohn, Karlsson, Loewenstein & Ariely, 2008). Since unexpected expenses are unknown in advance, it may be difficult for consumers to readily call to mind examples of what they might need to save for.

Moreover, if consumers do consider the possibility of a financial shock, myopia (or "focalism") may lead them to assess only the likelihood of one specific expense – perhaps their car breaking down – and judge its probability to be very low (Wilson, Wheatley,

Meyers, Gilbert & Axsom, 2000; Ungemach, Chater & Stewart, 2009). This perceptions may be accurate, but it is the accumulation of low risks that matters; the *disjunctive* probability of experiencing at least one emergency is much higher. Disjunctive probabilities tend to be underestimated (Brockner et al., 2002) and estimates of cumulative risk are inaccurate and often biased (Kahneman, Slovic & Tversky, 1982; Knauper, Kornik, Atkinson & Guberman, 2005; McCloy, Byrne & Johnson-Laird, 2008). For example, many people fail to realise that although the probability of pregnancy when using contraceptives is low, it accumulates over time and hence consistently increases (Doyle, 1997). People often assume that the risk remains constant, declines or varies non-monotonically. Even those who correctly reason that the probability increases tend to underestimate the increase. When it comes to finances, it doesn't matter which emergency the consumer faces; any financial shock will require a savings buffer.

Figure 2. Elements of animated infographic issued as part of the risk accumulation treatment.

Hence our aim was to use principles from the risk communication literature to convey the cumulative risk of a financial shock to the bank's customers (e.g. Spiegelhalter et al., 2011). One effective way to improve peoples' reasoning about risk is to use numeric frequencies rather than probabilities, for example that 1 in 3 adults face a household repair bill each year (Gigerenzer & Hoffrage, 1995; Visschers, Meertens, Passchier & De Vries, 2009). Literature

on communicating medical risk shows that combining numeric frequencies with visual aids is effective for encouraging risk mitigation (Ancker, Senathirajah, Kukafka & Starren, 2006; Lipkus & Hollands, 1999).

We developed an infographic (Figure 2) to communicate to customers the risk of facing a financial shock in a given year. To inform its design and ensure accuracy of information, in December 2020 we surveyed a nationally representative sample of 1,000 adults and asked them to select from a list of options any financial shocks they had faced in the previous 12 months (adapted from a Pew Charitable Trusts, 2015). Results (see Appendix) showed that while each individual shock was experienced by a minority (e.g. 18.5% experienced car breakdown), a majority (59%[6]) faced at least one unexpected expense in 2020. For comparison, 60% of Americans are estimated to face an unexpected expense in any year (PEW, 2015). Moreover, 30% faced more than one expense, exactly in line with US estimates.

The infographic was distributed via email to a sample of the bank's customers and featured three GIFs (Graphic Interchange Formats) that allow basic animation. The first GIF communicated the natural frequency of facing a financial shock (6 in 10). The second showed a grid of people experiencing different types of financial shocks via icons (e.g. a car icon or medical sign) to illustrate that only a minority did not experience any shock. The third showed 3 in 10 facing more than one financial shock. This treatment also included a "Savings Need Quiz" that categorised customers based on their saving behaviour and responsibilities (e.g. if they would be responsible for paying for a household appliance repair themselves). Cookies were stored for customers who opened the email which allowed ad retargeting. These digital ads were displayed to customers on other websites and communicated similar ideas (see Appendix).

The comparison for our communication treatment (hereafter "risk accumulation communication") was an email that encouraged customers to save to improve their financial wellbeing. It also included GIFs (e.g. of money being added to a cartoon 'piggy' bank) and a

[6] The figure rises to 69% if COVID-related income shocks are included.

matched “Financial Wellbeing Healthcheck” quiz. This email aligned with the bank’s existing financial wellbeing campaign.

4. *Field Experiments*

This study received approval from the ESRI Research Ethics Committee on 8th February 2021. We ran two field experiments concurrently. The first tested both treatments in a 2 x 2 between-groups design, resulting in four groups of customers who were issued email communications (Figure 3). We refer to this as the "2x2 RCT", although the trial also included a fifth group of customers who were otherwise eligible but formed a "no-contact" control group (see below). This no-contact control groups provides a baseline of saving behaviour among consumers not prompted to save by direct bank communications, whereas the group who received control communications from the bank act as a "placebo" condition. The second tested only the behavioural application form among customers who accessed the application form for the target savings account without having been issued an email to prompt saving, i.e. "organic" online traffic to the application page. These customers were randomised to see either the control or behavioural form. We refer to this trial as the "Organic A/B Test".

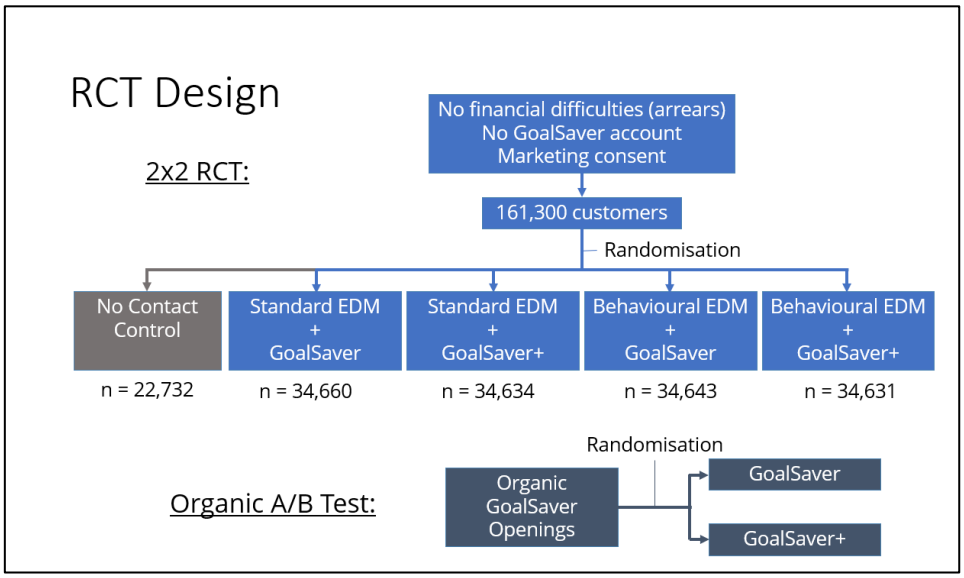


Figure 3. Overview of trials.

3.1 Sample

Customers were eligible for the 2x2 RCT if they (i) had a current account with the bank, (ii) showed no indication of financial difficulty (e.g. arrears), (iii) did not have an existing GoalSaver account and (iv) had consented to receive marketing communications. A total of 161,300 customers met these inclusion criteria. Of these, 22,732 were randomly selected to form the no-contact control group, who were otherwise eligible to be enrolled in the trial but

were not issued any communications and, if they sought to open a savings account, could only access the control application form. The remaining customers were randomised into one of four treatments, according to the trial's 2 (communication: financial wellbeing vs. risk accumulation) x 2 (application form: control vs. behaviourally-informed) design. There were between 34,631 and 34,650 customers in each of the treatment groups. In the Organic A/B test, 993 accounts were opened, although unbalanced randomisation meant most of these (n = 718, 72.3%) accessed the control account. Basic socio-demographic characteristics were available for subsets of customers and are summarised in Table 3 (noting that age and sex data were not available for any customers in the Organic A/B test, and salary information was available for a minority (38.8%) of those in the 2x2 RCT).

Table 3. Socio-Demographic Characteristics of the Sample

		2x2 RCT		Organic A/B Test	
		n	% (of total sample)	*n*	% (of total sample)
Sex	Male	71,167	44.1	-	-
	Female	59,750	37.0	-	-
	Missing	30,383	18.9	993	100.0
Age	18-24 years	27,331	16.9	-	-
	25-29 years	18,968	11.8	-	-
	30-34 years	16,360	10.1	-	-
	35-39 years	16,562	10.3	-	-
	40-44 years	15,740	9.8	-	-
	45-49 years	13,016	8.1	-	-
	50-54 years	10,129	6.3	-	-
	55-59 years	7,481	4.6	-	-
	60+ years	6,238	3.9	-	-
	Missing	29,475	18.2	993	100.0
Salary (estimate)	<€20,000	22,374	13.9	166	21.6
	€20,000-€29,999	19,262	11.9	208	27.0
	€30,000-€39,999	11,438	7.1	199	25.8

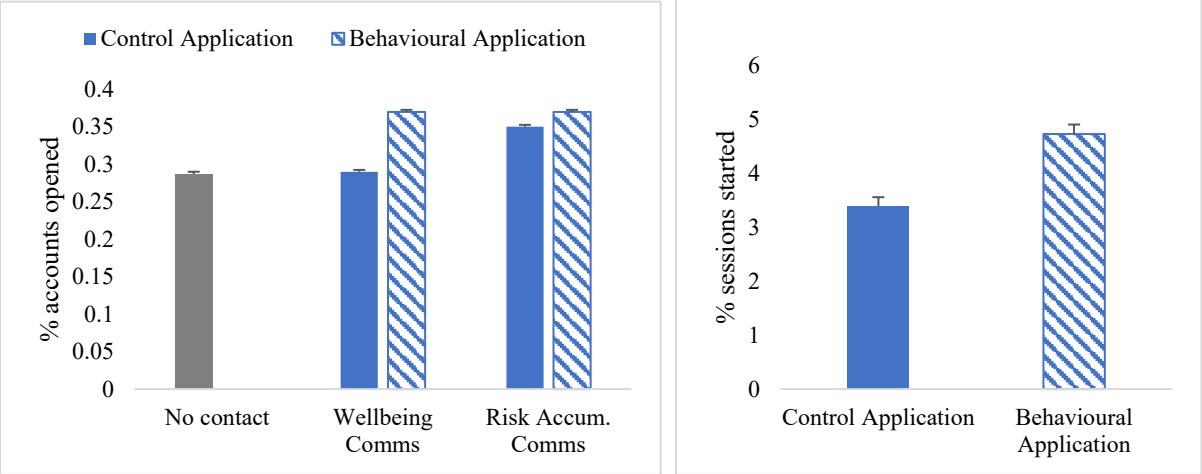
	€40,000+	9,466	5.9	197	25.6
	Missing	98,760	61.2	-	-
Region	Dublin	45,117	28.0	317	31.9
	Rest of Leinster	37,790	23.4	255	25.7
	Munster	32,470	20.1	270	27.2
	Connacht-Ulster	23,191	14.4	151	15.2
	Missing	22,732	14.1	-	-

3.2 Timing and Context

The trial ran for six months starting May 17th, 2021. The trial period was one of relatively low savings account uptake, resulting from the on-going pandemic. Consumers in Ireland had, on average, accumulated more savings than usual in 2020 and the first quarter of 2021, as opportunities for consumption were limited by economic restrictions (Central Statistics Office, 2021; Lydon & McIndoe-Calder, 2021). This context meant that our trial had fewer accounts opened than in a typical six-month period. Low uptake has implications for statistical power when comparing savings behaviour within accounts opened, but less so for our primary outcome of number of accounts opened. However, we account for the low numbers of accounts opened by treating opening an account as a rare event and we model it with a penalized likelihood logistic regression model using the Firth method (Firth, 1993; Williams, 2019). Closely similar results are observed using rare events logits and standard logistic regression models (e.g. Toms, King & Zeng, 2021). We have no reason to believe that this context affected the relative difference between treatments.

5. Results

We received anonymised data at the customer level on their treatment, whether they opened the target saving's account, the balance each week, their current account balance, their combined loan and credit card debt (excluding any debt with other providers) and whether any other savings account was opened over the trial period with the bank. We also had access to the bank's Google Analytics[7] data on use of account features (e.g. the Calculator tool) and engagement with the communication treatments (e.g. "click-through rates"). For the Organic A/B test, we do not have data on customers who did not take out the savings account. Instead, we rely on Google Analytics data which was available for a subset of customers who started the application form (n = 6,645). In this section, we present analyses on accounts opened, amount saved, specific treatment interactions and additional checks on the customer's wider financial picture.



Figures 4a and b. Percentage of customers who opened the target savings account in each 2x2 RCT treatment (left) and conversion rate in the Organic A/B Test (right). Error bars are the standard error of the proportion.

4.1 Accounts Opened

Figures 4a shows the percentage of customers in each RCT condition who opened the target savings account. Model 1 in Table 4 compares each RCT treatment and the no-contact control group against the group who received financial wellbeing communications and

[7] Google Analytics tracks website traffic and engagement with website features. Data were available at aggregate (i.e. group) levels.

accessed the control application form (i.e. the "business as usual" group). It shows no difference in the likelihood of opening an account between the no-contact group and "business as usual" group, but significantly greater likelihood in each of the treatment groups. Model 2 shows these treatment effects are significant at the 1% level when available socio-demographic characteristics are included as controls, as in the pre-registration. Note that adjusting treatment effects for covariates in RCTs leads to gains in statistical power (see Hauck, Anderson & Marcus, 1998; Lingsma, Roozenbeek & Steyerberg, 2010; Ciolino, Palac, Yang, Vaca & Belli, 2019). Tests of coefficients between treatments revealed no significant differences, nor were there significant interactions between the treatments and socio-demographic characteristics. Compared to the business as usual treatment, the other treatments had an average effect size of a 25%[8] increase in accounts opened.

Figures 4b shows the conversion rate (i.e., the number of completed applications as a percentage of application forms started) for both treatments. A test of proportions shows that a significantly higher proportion of customers who started the behavioural application form completed it compared to the control application form ($Z = 2.89$, $p = .004$). The effect size is a 39.5% increase in savings accounts opened when the behavioural form was encountered, compared to the control application form.

Table 4. Penalised Likelihood Models Predicting Account Opening

	1	2
Treatment		
(*Ref: Wellbeing + Control App.*)		
No Contact	0.00	0.22
	(0.16)	(0.21)
Wellbeing + Behavioural App.	0.25**	0.48***
	(0.15)	(0.18)
Risk Accum. + Control App.	0.20*	0.43***
	(0.15)	(0.18)

[8] A 27% increase for each of the treatments that features the behavioural application form and a 20% increase for the risk accumulation communication only.

Risk Accum. + Behavioural App.	0.25**	0.54***
	(0.15)	(0.18)
Socio-Demographic Controls	No	Yes
N	161,300	71,671

*$p < .1$; **$p < .05$; ***$p < .01$; note p-values are one-tailed in line pre-registered directional hypotheses

4.2 Amount Saved

Figure 5a shows the median monthly deposit into opened savings accounts. Our hypothesis for amount saved was non-directional. OLS models predicting $\log_{10}$-transformed monthly deposits show no differences between the treatments in the 2x2 RCT, with or without socio-demographic controls. The same is true for totals saved at the end of the trial period[9] (Table 5). In the Organic A/B test, customers who completed the behavioural application form selected significantly lower monthly deposits and saved less by the end of the trial (Figure 5b; Table 6). However, this effect becomes non-significant when controls for income are added. Analysis of customer characteristics shows that the behavioural application form led to a higher proportion of accounts being opened by lower-income earners. Since there was imbalanced randomisation for the A/B test, Figure 6 presents a "hypothetical world" for the six-month trial period. It illustrates how many accounts would have been opened by each income bracket if all customers saw the control application form versus if all customers saw the behavioural application form. The behavioural application more than doubled the likelihood of account uptake by lower income earners, without detriment to accounts opened by higher earners.

[9] The sample size for total saved are slightly smaller due to a small number of empty accounts at the end of the trial period (n = 31). It's important to note that an empty account may simply signal that the customer faced a financial shock and used their savings to absorb it. Another Firth logistic model showed no differences between treatments groups in the RCT or A/B test (see Appendix).

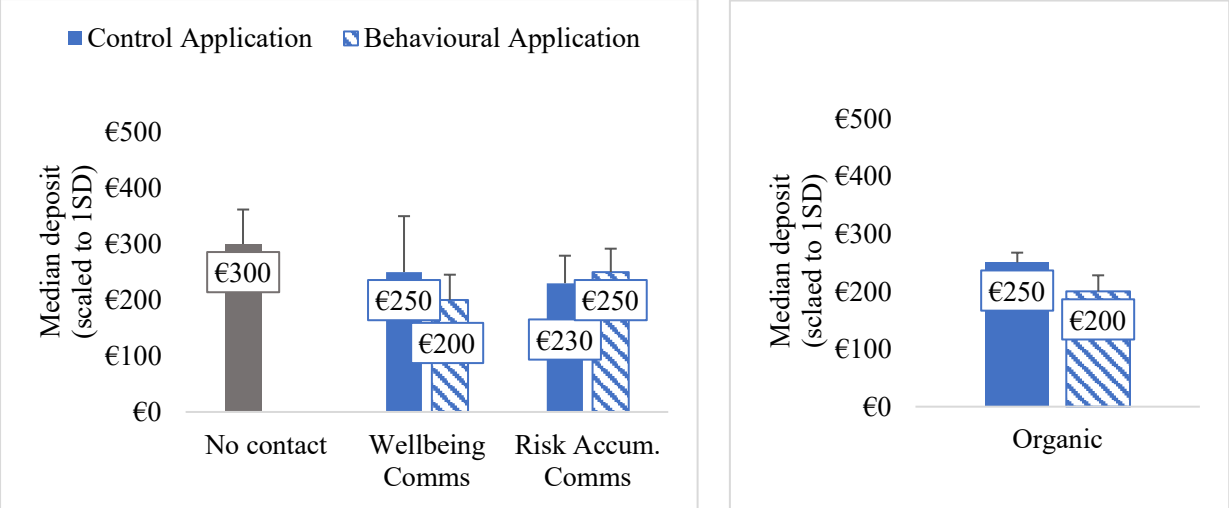


Figure 5a and b. Median monthly deposits in the 2x2 RCT treatments (left) and Organic A/B Test (right). Error bars are the standard error of the mean.

Table 5. OLS Models Predicting Amount Saved (Log_{10}-tranfsformed) in the 2x2 RCT

	Initial Deposit		Total Saved	
	1	2	3	4
Treatment				
(*Ref: Wellbeing + Control App.*)				
No Contact	0.11	0.14	0.11	0.02
	(0.21)	(0.27)	(0.24)	(0.30)
Wellbeing + Behavioural App.	-0.08	-0.29	-0.02	-0.04
	(0.18)	(0.23)	(0.20)	(0.26)
Risk Accum. + Control App.	0.13	-0.23	0.15	-0.26
	(0.18)	(0.23)	(0.20)	(0.26)
Risk Accum. + Behavioural App.	0.06	-0.03	0.05	-0.03
	(0.18)	(0.24)	(0.20)	(0.26)
Socio-Demographic Controls	No	Yes	No	Yes
N	540	271	509	249

*$p < .1$; **$p <.05$; ***$p < .01$; note p-values are two-tailed in line pre-registered non-directional hypotheses.

Table 6. OLS Models Predicting Amount Saved (Log10-tranfsformed) in the A/B Test

	Initial Deposit	Total Saved

	1	2	3	4
Behavioural App.	-0.24**	-0.02	-0.23*	0.02
(Ref: Control App.)	(0.09)	(0.10)	(0.10)	(0.11)
Income Controls	No	Yes	No	Yes
N	993	748	962	720

*$p < .1$; **$p < .05$; ***$p < .01$; note p-values are two-tailed in line pre-registered non-directional hypotheses.

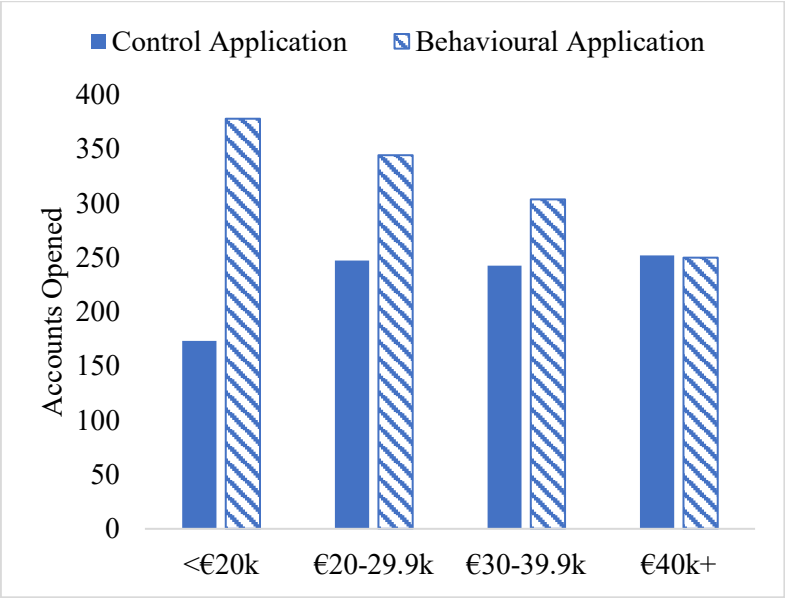


Figure 6. Number of accounts that would have been opened in the six-month trial period if all organic traffic saw each treatment, based on the A/B test of organic traffic.

4.3 Specific Treatment Analytics

We also have limited Google Analytics data (n = 6,645) on use of the Calculator and Pledge tools added to the behavioural application form (Figure 7). Tests of proportions showed that use of both Calculator ($Z = 3.53$, $p < .001$) and Pledge ($Z = 18.90$, $p < .001$) was significantly higher among those who ultimately opened a savings account compared to those who started the form but did not complete it.

Turning to analytics on the communications treatments, the website advertisements combined with the emails issued to consumers resulted in over 190,500 impressions (i.e. views) per treatment. The 'click-through rate,' measured as the number of customers who clicked on a link in the email or the retargeting advertisement as a percentage of all customers who saw the email or advertisement, was 0.058% for the wellbeing communication and 0.063% for the

risk accumulation treatment. Hence, the risk accumulation treatment had a 9.4% higher click-through rate, which is statistically significant ($Z = 7.81$, $p < .001$).

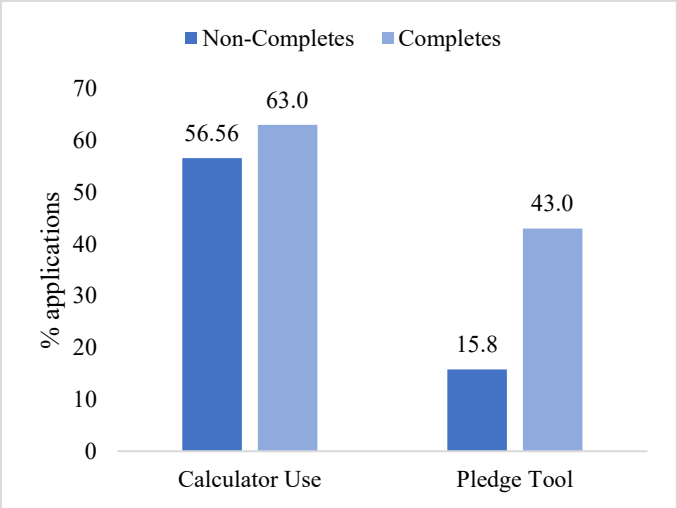


Figure 7. Use of new features among complete and non-complete applications in the A/B Test.

4.4 Negative Spillovers and Substitution

We checked available data for signs of negative spillovers among those who opened a savings account (see Beshears et al., 2022). We estimate spillovers in two ways: by whether the customer (1) went into overdraft on their current account ($n = 77$) and (2) had any debt at the end of the study period ($n = 251$).[10] Table 7. Penalized Likelihood Models Predicting shows models of overdrafts and debt for the RCT, again treating observations as rare events. There is no indication that those who opened savings accounts in any treatment were at more risk of taking an overdraft or having debt at the end of the trial compared to the "business as usual" treatment. In fact, customers who saw the wellbeing communication and accessed the behavioural application form were marginally less likely to have any debt at the end of the study. The same check on the Organic A/B test similarly shows no evidence of detriment from the behavioural application form on overdrafts ($\beta = -0.04$, $SE = 0.29$, $p = .879$) or debt ($\beta = -0.01$, $SE = 0.19$, $p = .943$).

[10] Due to low numbers of observations, we report here only our analysis of whether the account holder had any debt at the end of the study. Analysis of debt magnitude also show null effects.

Table 7. Penalized Likelihood Models Predicting Spillovers

	Overdraft	Any Debt	Other Saving Account
	1	2	3
Treatment (*Ref: Wellbeing + Control App.*)			
No Contact	0.70	-0.03	2.06
	(0.73)	(0.38)	(1.56)
Wellbeing + Behavioural App.	0.00	-0.68*	0.85
	(0.73)	(0.36)	(1.64)
Risk Accum. + Control App.	0.44	-0.20	0.91
	(0.68)	(0.33)	(1.64)
Risk Accum. + Behavioural App.	-2.22	-0.31	1.72
	(1.52)	(0.33)	(1.52)
N	540	540	540

*$p < .1$; **$p < .05$; ***$p < .01$; note p-values are two-tailed.

We also checked whether customers who opened the target savings account were less likely to open an alternative savings account with the bank, in which case our treatment effects would simply indicate substitution and not a real increase in the proportion of customers saving. Model 3 in Table 7 shows no differences across treatment groups in the RCT. Again, absolute numbers are small. Similarly, there are no differences in the Organic A/B Test (β = 0.96, SE = 2.00, p = .632). (Note that while we do not have data on whether customers opened accounts with other banks, the target savings account had the highest interest rate in the market at the time of the study.[11])

Our final check for substitution was on current account balances, to answer the counterfactual: would customers who opened an account have accumulated savings in their current account anyway? (see Bachas, Gertler, Higgins & Seira, 2021). Using an OLS model, we regressed the customer's current account balance onto their treatment with random effects

[11] One other account that had no restrictions on opening had a higher interest rate during the study period, however the bank was due to leave the Irish market in 2022.

at the customer and week level, with weeks nested within customers. Table 8 shows no differences across treatment groups in the 2x2 RCT and there were no differences in the Organic A/B Test (β = -0.01, *SE* = 0.05, *p* = .785; β = -0.05, *SE* = 0.06, *p* = .442 with income controls).

Table 8. OLS Models Predicting Weekly Current Account Balance (Log10-tranformed)

	1	2
Treatment		
(*Ref: Wellbeing + Control App.*)		
No Contact	-0.03	0.07
	(0.12)	(0.14)
Wellbeing + Behavioural App.	-0.03	0.08
	(0.10)	(0.13)
Risk Accum. + Control App.	0.03	0.10
	(0.10)	(0.13)
Risk Accum. + Behavioural App.	0.05	0.13
	(0.10)	(0.12)
Socio-demographic Controls	No	Yes
N	540	271
Obs.	7,702	5,756

*$p < .1$; **$p < .05$; ***$p < .01$; note p-values are two-tailed.

Note. A constant was added to balances before transformation to ensure all values were greater than 0.

5 Conclusion

Some households appear to under-save, generating exposure to financial risk. Evidence links this phenomenon to behavioural factors that influence the control of expenditure, financial planning and, perhaps, motivation. However, evidence that interventions based on behavioural research can increase saving is presently less convincing: we failed to locate any interventions that successfully increased precautionary saving rates outside of low/lower-income countries. We therefore combined multiple behavioural interventions into a treatment that included both nudges and boosts. We tested it alongside a novel motivational intervention in a large-scale field trial, undertaken in collaboration with a major retail bank.

Arguably, failure to increase saving with this strategy would have had negative implications for efforts to deploy behavioural approaches to address real-world financial problems. Yet we recorded substantial impacts. A savings account application form with a combination of evidence-based nudges and boosts – such as changing the order of questions and adding a calculator tool – led to an increase of 27-40% in account openings among customers, relative to a form already in the market (and that was in many ways "behaviourally-aligned"). The variation in effectiveness relates to the consumer's motivation to save; the increase was larger among consumers who were themselves motivated to save without being part of an email campaign. The motivational intervention was also effective. Moreover, the behaviourally-informed changes had stronger effects on low-income customers likely to be most vulnerable to high-cost borrowing if faced with a financial shock.

This effect is far larger than typically observed in other domains. Effect sizes for successful behavioural interventions are often estimated by relative increases in the desired outcome and, when low statistical power and publication bias in academic journals is accounted for, such differences tend to be small (~8%; DellaVigna & Linos, 2022). Moreover, the effect size from the 2x2 RCT (25%) is likely underestimated, as not all of those who received communications formed the target population of consumers who want to save and are not currently doing so; the effect size among consumers who were motivated to access the savings account form themselves was larger still, at 40%. While the trial was run during a period of low absolute uptake, this effect would be anticipated to accumulate over time as the flow into saving account applications increases.

The email campaign to help consumers understand the cumulative risk of the many possible financial shocks led to a 20% increase in savings accounts, relative to the bank's marketing campaign centred on financial wellbeing. There is good evidence that people underestimate both cumulative risks and disjunctive probabilities, particularly in relation to health risks (e.g. McCloy et al., 2008). Our trial provides suggestive evidence that this is also a problem for household finances. A simple infographic using principles from risk communication literature can motivate consumers to try to mitigate the risk. The idea is a novel one, not previously tested in relation to saving. One avenue for future research would be to test ways to improve the method of communication. The findings provide the first evidence for a communications intervention to encourage a precautionary motive for saving in a large-scale RCT with real bank customers.

While the mechanism underlying the success of the risk accumulation treatment is straightforward to identify – consumers began to consider the risk of financial shock – the mechanism underlying the enhanced application form is unclear. Nudge interventions often test just one treatment in isolation (e.g. Dur et al., 2021). This approach is reasonable if the aim is to isolate mechanisms cleanly and to advance theory. However individual nudges often fail and can have very small effect sizes (e.g. DellaVigna & Linos, 2021; Milkman et al., 2022). Since our aim was more practical – to see whether behavioural interventions could increase savings account uptake – there was a clear benefit to including multiple nudges and boosts in a 'behavioural package' intervention. The limitation is that we cannot identify whether the effect was driven by one or two of the manipulated features, or if the whole range of interventions was necessary. For financial institutions and policymakers wanting to increase savings buffers among consumers, however, this distinction is less important, as it is for behavioural economists seeking to test whether combining findings from their literature can generate a substantive effect size.

Crucially, we found no evidence of negative spillovers among consumers who started to save in our treatment groups compared to any of the control groups. There has been growing concern about unintended consequences of nudge interventions, for example that workers automatically enrolled into pension plans may persist at lower contribution rates than if they were to make active choices (Choi, Laibson & Madrian, 2004). We recorded no evidence for spillovers on indicators of debt, nor evidence that customers would have otherwise taken out another savings account with the bank (Beshears et al., 2022). It is also unlikely they would

have opened a savings account with another provider, as the target account had the highest interest rate in the market. Moreover, consumers rarely search and switch savings accounts, even if higher interests rates are available (Adams, Hunt, Palmer & Zaliauskas, 2021). Hence, given the randomised controlled design of the study, we are confident of the causal effects of our treatments on account uptake and reasonably certain in the overall welfare benefit to consumers.

Some households may not save due to expectations for future income, but others would benefit from protection against financial shocks. Interest rates on savings accounts are low, but at least 30% of households would need to take on debt that is likely to come with much higher interest to cover an unexpected expense (e.g. Demertzis et al., 2020). There is likely a net benefit to consumers having a small financial buffer. Our results show that prompting consumers to consider the cumulative risk of financial shocks or applying a suit of nudges and boosts to application forms can increase savings account uptake by 25-40%. More broadly the findings further demonstrate the benefit of using randomised controlled trials for tackling policy problems, such as improving the financial wellbeing of households.

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Appendix

Survey Data

Bivariate logistic regression models predicting saving status among the target population are shown in Table A1.

Table A1.

Logistic Regression Models Predicting Saving Status in CCPC (2018) Survey Data

	TO	Imp	SSC	SC	LOC	AO	FC
Time Orientation (TO)	-0.03*** (0.01)						
Impulsivity (Imp)		-0.02*** (0.01)					
Social Status Concern (SSC)			0.03*** (0.01)				
Self-Control (SC)				-0.02* (0.01)			
Locus of Control (LOC)					-0.03*** (0.01)		
Action Orientation (AO)						-0.01 (0.01)	
Financial Confidence (FC)							-0.01 (0.01)
Socio-Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	476	476	476	476	476	476	476

*$p < .10$; **$p < .05$; ***$p < .01$

Empty Account Balances

Few customers in either trial had an empty savings account at the end of the period. Table A2 shows a penalised logistic regression model for the RCT and shows no effect of treatment. There is similarly no effect in the A/B test (Table A3).

Table A2.

Penalized logistic regression model predicting empty account at end of trial

	A1	A2
Treatment (*Ref: Wellbeing + Control App.*)		
No Contact	0.43	1.33
	(0.77)	(1.27)
Wellbeing + Behavioural App.	0.67	1.61
	(0.65)	(1.00)
Risk Accum. + Control App.	0.85	1.69
	(0.64)	(1.11)
Risk Accum. + Behavioural App.	0.67	1.36
	(0.65)	(1.14)
Socio-Demographic Controls	No	Yes
N	540	271

Table A3.

Penalized logistic regression model predicting empty account at end of trial

	A3	A4
Behavioural App.	0.18	0.16
(Ref: Control App.)	(0.34)	(0.40)
Income Controls	No	Yes
N	993	770