Testing the Effectiveness of Consumer Financial Disclosure: Experimental Evidence from Savings Accounts*

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Abstract

Consumer disclosure—the practice of providing information to support consumer decision making—has obvious potential benefits and has been widely mandated in public policy. While most evidence on the effectiveness of consumer financial disclosure design stems from lab experiments or post-implementation analysis, we provide direct field evidence from randomized-controlled trials with 130,000 savings account holders from five major UK providers. Groups of treated consumers received various salient information about an available better product. Motivated by theoretical research on market frictions and consumer mistakes, our experimental variation is designed to allow us to examine the relative importance of (i) simplifying search and comparison within and across providers, (ii) making the switching process easier, and (iii) increasing attention to the task. We find that explicit rules around disclosure design are a necessary element of effective consumer protection regulation. Despite the switching process taking 15 minutes on average and the moderate size of average potential gains (£123 in the first year), we find that overall attention to disclosure is low, significantly limiting its potential effectiveness and shedding light on the nature of the stickiness of deposits.

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1 Introduction

In practice, consumer choice is sticky. Across an array of decisions including insurance, telecommunications contracts, pension plans, mortgage choice, and others cited below, individuals act as if they are reluctant to optimize. The stickiness of deposits in particular has featured prominently in recent empirical banking models (e.g., Drechsler et al., 2017). Such inertia poses a challenge for consumer disclosure, the (often mandated) practice of providing information to consumers to support their decision making. Informational disclosures have been one of the most popular types of regulatory intervention in retail financial markets across the world. Yet, owing to a variety of practical and methodological challenges, little is known about how the delivery of information actually affects consumer behavior. In particular, attempts to assess disclosure effectiveness generally suffer from a joint-hypothesis problem, where the researcher must both model optimal choice and measure deviations therefrom.

In this paper, we use results of a large-scale field experiment to evaluate the effectiveness of various disclosure designs aimed at supporting consumer choice across savings products—a financial setting where normative statements about optimal choices are relatively straightforward to make. Testing how disclosure actually works in practice is best done in the field, where, among other important considerations, disclosure has many competitors for consumer attention.[1] A randomized-controlled trial (RCT) further permits controlling consumer selection across treatment and control and precisely manipulating disclosure in a way that permits robust comparison across design alternatives.

Cash savings accounts are the most popular formal household savings vehicle in the UK: 93% of consumers have a savings account amounting to total holdings of £700 billion (FCA, 2015) equivalent to 37% of UK GDP. Instant-access savings accounts, in particular, are among the simplest financial products—their key feature is the interest rate payable on the balance. Yet significant differences in interest rates persist on similar accounts both across providers and within providers across nearly identical products. Many providers offer higher interest rates on a visible set of accounts (known as the "front book") while reducing the rates on legacy accounts ("back book"). In principle, higher front-book rates may persuade consumers, especially those with back-book rates, to switch. Although there is significant heterogeneity across providers, we find that switching is not widespread—most consumers seldom switch their savings accounts and thereby forgo higher interest earnings.

To inform policy remedies aimed at increasing competition in the savings account market, the UK Financial Conduct Authority partnered with five retail financial institutions to con-

[1] See Harrison and List (2004) for a discussion and framework of the virtues of field versus laboratory experiments.

duct multiple RCTs testing the extent to which having readily accessible (and in some cases personalized) disclosures about market interest rates for comparable savings products would be useful to consumers. Over 134,000 savings account holders were randomly assigned into treatment and control groups. Consumers in the treatment groups received various salient information about a better available product—an equivalent savings account with a higher interest rate, a simplified way to switch to it by returning a pre-filled form, and reminders about imminent reductions to the interest rate payable on their account. We collect rich administrative data on account balances, demographics, and switching behavior from each of the five financial institutions. For a subgroup of consumers, we conduct follow-up surveys to supplement our analysis of consumer responses to trial interventions with direct explanations for consumer behavior.

In this simple disclosure setting with a very simple financial product, we test the importance of disclosure design in protecting consumers and improving competition. We test three types of regulatory interventions: the disclosure of clear information to aid search and comparison (a "switching box"), sending clear reminders to customers to bring attention to the task, and a mail-back switching form to help make the task easier to implement.

Our trials are motivated by theoretical research on market frictions and consumer mistakes when choosing the best price: experimental variation allows us to examine the importance of (i) simplifying search and comparison, (ii) making the switching process easier and (iii) increasing attention to the task. Because treatment groups are on average identical to control groups (by virtue of random assignment) except for the amount of information they receive about savings account market conditions, we can causally attribute any differential account switching to the information they receive. Moreover, while many studies of optimal consumer behavior face a joint-hypothesis problem—the challenge of simultaneously estimating optimal and actual behavior—our setting allows us to make strong statements about the optimality of changing behavior in response to the disclosure. For example, attempts to learn whether mortgage or credit-card disclosures are effective are confounded by the problem of first needing to determine what the "right" mortgage or credit-card choice is for a given consumer in order to be able to learn whether the disclosure has moved decisions closer to that optimum.

By contrast, we analyze the behavior of consumers that are presented with the opportunity to switch to an equivalent savings account from their current provider that differs only in the amount of interest it pays, subject to relatively low switching costs which are further reduced in some trials. If consumers provided with additional information are significantly more likely to switch accounts, this suggests that the reasons for low baseline switching are lack of information and the cost of attention. However, if salient information on opportu-

nities to increase interest income does not materially affect consumer behavior, this points towards demand-side frictions limiting the usefulness of disclosure.

Previewing our results, even among consumers that were provided with reliable information about more attractive interest rates (often at the same provider), there is still a low level of switching. The sensitivity of switching behavior with respect to the level of interest income is limited, although for half of the consumers in the sample the amount of annual foregone interest income from not switching is less than £32 ($50). One obvious explanation is that with such low amounts of money at stake (account balance × (potential interest rate – current interest rate)), it doesn't take a high opportunity cost of time to justify not switching. While we find some evidence for this explanation (e.g., switching is higher among retired depositors, who presumably have a lower opportunity cost of time), even among the highest balances in our sample, switching remains low.

Among the interventions, a simple pre-filled return switching form (Trial 3) and well-timed reminders (Trial 4) led to the highest absolute increases in switching of up to 9 percentage points. The return switching form increased switching from a baseline of 3% to 12%. Prominent disclosures—in particular front page information on better available products (Trial 1)—had marginal positive effects, raising switching from 3% to 6%, while non-front page disclosures showed no effect (Trial 2).

We also find evidence that the timing of a reminder relative to the interest rate decline matters. Reminders were more effective when sent shortly before the rate decrease, compared to other timing (Trial 4), consistent with previous research. The importance of information acquisition in close proximity to the rate decrease is consistent with results from lab and field experiments in Tu and Soman (2014), who find that consumers are more likely to take action (for example open a bank account or move funds) if it occurs before (rather than after) a salient event.

The rest of the paper proceeds as follows. Section 2 contextualizes our study in the relevant literatures on the efficacy of informational disclosure, consumer mistakes, and rational inattention. Section 3 provides background on our experimental design. We describe our data and conduct balance tests for each trial in Section 4. Sections 5 and 6 present our experimental and survey findings, respectively. We conclude in Section 7.

2 Context in Literature

A significant literature spanning industrial organization, behavioral economics, household finance, law, and marketing critically examines consumer disclosure regulations in many

consumer markets.[2] These papers provide mixed evidence for disclosure effectiveness, highlighting stories of both disclosure successes and failures. Much of the disclosure literature seeks to explain variation in disclosure effectiveness with ex-post arguments contrasting estimates across designs and domains. In contrast, we experimentally vary disclosure design for a single product with a relatively clear hierarchy of consumer benefits, allowing us to make causal assertions about the relative importance of disclosure content and design features in generating benefits for consumers.

To the extent that lacking information is a significant reason for sticky (and perhaps suboptimal) consumer choice, disclosures have potential to affect decisions. Even when reoptimizing based on the information content of disclosures has the potential to increase the flow utility of a given choice, there are several impediments to disclosures improving outcomes. Many economic decisions exhibit inertia in the sense that after initial product choices, demand across alternatives becomes less elastic. See evidence in domains such as health insurance plans, retirement investment plans, bank accounts, cell-phone plans, utility contracts, credit cards, and gym memberships.[3]

While it may be that in many settings the foregone utility from not reoptimizing is small (an Envelope Theorem argument), there are several prominent cost-side explanations for apparent inertia in consumer behavior, each of which poses an obstacle to disclosure effectiveness. A rich literature on rational inattention in macroeconomics (discussed below) offer a search-costs-based explanations for such behavior—see also DellaVigna (2009) and Gabaix (2017) for surveys of the limited attention literature in behavioral economics. To the extent that search costs are driven by the costliness of information acquisition, mandated disclosure has the potential to alleviate search frictions and increase consumer welfare.[4] For choices consisting of repeated interactions, switching costs, including convenience costs and the loss of value of complementary choices (such as network effects), inhibit changing providers ex-post (Farrell and Klemperer, 2007). Financial literacy is likely a necessary condition for effective financial disclosure (see Lusardi and Mitchell, 2004). Work in behavioral economics on commitment-problems provides evidence for the prevalence of procrastination and the

[2] A complete treatment of these literatures is outside the scope of this paper; see Dranove and Jin (2010) and Ben-Shahar (2010) for comprehensive surveys of disclosure-related research.

[3] See Samuelson and Zeckhauser (1988), Choi et al. (2011), Ericson (2014), and many others for inertia in health insurance and retirement plan decisions. Grubb and Osborne (2015) and Della Vigna and Malmendier (2006) document evidence of stickiness in cell-phone plan choice and gym memberships, respectively. Ater and Landsman (2013) document the slow learning of retail deposit account holders. Ponce et al. (2017) show existing credit-card borrowers have cross-price elasticities near zero. Hortaçsu et al. (2017) find consumers reluctant to switch to an otherwise identical lower-cost utility provider.

[4] See, for example, evidence from Kling et al. (2012), who find in the health care context that individualized information comparing providers' products can affect choice.

difficulty of completing intended tasks.[5] A parallel literature in psychology probes the cognitive cost of reoptimization and the extent to which abundant choice can be overwhelming and reinforce the status quo (e.g. Schwartz, 2004).

Closest to the mandated disclosure setting we study, two recent studies have experimentally tested disclosure effectiveness in the Mexican credit-card market. Ponce et al. (2017) find that credit-card borrowers in Mexico are insensitive to disclosures about the interest rates of available alternative credit cards. Directly testing Truth-in-Lending-Act-type disclosures, Seira et al. (2017) conduct an RCT with high-risk credit-card borrowers in Mexico and find very small effects that they attribute to consumer inattention. Our study adds to this burgeoning literature testing disclosure effectiveness using field experiments in several ways. By looking across several providers at depositors as opposed to borrowers, our setting allows clear normative predictions about dominating choices among plausibly financially savvy households in a developed economy. Whereas it may not be optimal for high-risk borrowers to reduce their debt levels, many savings depositors in our setting could obtain a higher interest rate worth hundreds of dollars in the first year alone by spending a few minutes asking their bank to costlessly relabel their savings account to qualify for a materially higher interest rate. Note, too, the results of Medina (2018), who shows that directing consumer attention toward paying down debt product may decrease welfare by leading to an increase in otherwise avoidable overdraft fees. Our savings setting helps here, too, as the the opportunity cost of reclassifying savings to a higher account type is zero.

Our work is also conceptually and methodologically related to papers that experimentally vary the salience of product attributes. Several of these papers study the effects of advertising design. For example, the field experiment of Bertrand et al. (2010) tests for the importance of various design features in stimulating loan demand. As they acknowledge, because of the difficulty of asserting optimal consumer behavior in their setting, they focus on evaluating advertising persuasiveness and consumer demand instead of whether advertising design features matter for consumer benefits. See, too, the public finance literature on salience (e.g., Finkelstein, 2009 and Chetty et al., 2009), which demonstrates the important role of price information delivery design in affecting consumer demand.

Grubb (2015) and Persson (2018) find endogenous complexification responses by firms to be an additional obstacle to first-best decision making. While disclosure may un-shroud important attributes (a la Gabaix and Laibson, 2006), firms endogenously obfuscating the disclosure or complexifying the information set may be able to blunt any benefits of dis-

[5] Again, see DellaVigna (2009) for a survey of the self-control literature. O'Donoghue and Rabin (1999) model self-control problems in the context of savings decisions where naïve procrastinate immediate-cost activities.

closure. Empirically, Ferman (2015) demonstrates using a field experiment that existing high-risk credit-card borrowers in Brazil have less elastic demand when interest rates are disclosed in fine print. Célérier and Vallée (2017) find an endogenous complexity response issuers of structured securities to retail investors.[6] Even in our setting, where products are largely one-dimensional and confusion is likely to be low, we find significant scope for firms to obfuscate mandated disclosures by manipulating disclosure design, e.g., by burying information on the back page of annual statements. In a classic illustration of the such responses, a UK judge specified the font size and prominent website placement for Apple's mandated apology to Samsung for patent infringement after Apple's initial apology was posted posting in small print, using unclear language, in a remote area of their website (Leach, 2012).

In search-theory models, agents do not update their choices because they are uninformed about the location of attractive alternatives and becoming informed requires costly effort. If the (often non-monetary) cost of acquiring information about competitive alternative choices to their current decision exceeds expected gains from such activities, then the inattention is said to be rational. By contrast, consumers with commitment problems may fail to switch even if they are costlessly informed of dominating alternatives because they procrastinate reoptimizing indefinitely. Moreover, consumers may choose to ignore even costless information about alternatives to avoid cognitive costs such as the disutility of feeling overwhelmed by the complexity of pricing (Grubb, 2005), the disutility of the bad news that an account pays under market rates (Karlsson et al., 2009; Olafsson and Pagel, 2017), inadequate financial literacy to weigh options (Calvet et al., 2009), or an abundance of choices (Schwartz, 2004). Burnham, Frels, and Mahajan (2003), decompose switching costs into procedural costs (time and effort), financial costs, and relational costs (psychological/behavioral). They find all three types of costs are more important than satisfaction in explaining why consumers stay with their current providers. An established literature documents the importance of default choices in retirement plans (e.g., Choi et al., 2011) because consumers in general seem reluctant to participate in opt-in programs. Calem, Gordy, and Mester (2006) provide evidence on the persistence of credit card interest rates at high levels, and the existence of informational barriers and switching costs, though they find there may have been some decline in informational barriers since Calem and Mester (1995). On deposit accounts in particular, Kiser (2002) examines the role of self-reported switching costs in predicting switching behavior, finding particularly high switching costs (and low levels of switching) among people on the ends of age, geographic mobility, and income spectrums.

[6]Recent work in accounting finds similar responses: firms adjusting information presentation when investors have limited attention (Hirshleifer and Teoh, 2003) and external attention from institutional investors driving the frequency but not quality of voluntary disclosures by target firms (Abramova et al., 2018).

2.1 Literature on consumer mistakes

This paper also contributes to a broad literature documenting consumer financial mistakes and the role of disclosures or reminders in preventing them. Credibly documenting consumer mistakes entails a high burden of proof given the challenge of modeling and estimating optimal behavior in complex real-world settings. For example, the failure to search documented across a variety of high-stakes retail financial markets could be rationalized with sufficiently high search costs; borrowing high-cost debt could be optimal at low levels of consumption (with high marginal utility) and acute liquidity needs. Reviewing the literature, Campbell et al. (2011) argue for robust consumer financial protection, using mortgage choice, payday lending, and retirement saving as case studies in consumer finance market failures.

Much of the literature on consumer financial mistakes focuses on credit cards.[7] Agarwal et al. (2015) analyze the results of a bank experiment that offered customers a choice between two credit card contracts. They find that, on average, consumers chose the contract that minimized their costs, but nearly 40% did not. The likelihood of making a suboptimal choice was declining in the estimated magnitude of the potential error (with those with larger potential errors more likely to subsequently switch into the optimal contract). Jorring (2018) finds widespread payment of otherwise avoidable credit card fees. Ausubel (1991) shows that credit-card borrowers are in large part sensitive to teaser rates because of underestimating their future borrowing. Keys and Wang (2016) show the anchoring effect of minimum payment levels on credit-card statements, and importantly for our results, the fade-out of disclosures over time.

Other papers that have focused on savings include Stango and Zinman (2009) document consumers systematically underestimating interest rates on short-term loans and underestimate the benefits of long-term saving. Karlan et al. (2018) use evidence from three field experiments to show the effectiveness of reminders in increasing saving commitment attainment. They provide a model where limited attention generates under-saving, which is mitigated by reminders.

[7]Notable recent example of consumer mistakes in non-financial product markets include Abaluck and Gruber (2011), who find significant evidence of choice inconsistency among Medicare Part D subscribers, and Hortaçsu et al. (2017), who show consumers choosing to forego substantial cost savings by not switching utility providers.

3 Background and Experimental Setting

3.1 Overview of trial design

Partnering with five UK depositories, we test multiple disclosure designs in a range of field trials. Each financial institution helped to complete one trial for a total of five trials. See Table 1 for a harmonized overview of the five trials and Figures 1-8 for redacted examples.

All trials were conducted with customers who held an easy-access savings account with one of the partnering UK financial institutions at the time of random assignment. Customers were experiencing a rate decrease in three trials (Trial 2, 4 and 5) and were already on a relatively low rate in two trials (Trial 1 and 3).

Customers in the reverse page switching box trial (2) and in the reminder trials (4 and 5) faced an interest rate decrease to a level that was significantly below the average of what new customers could obtain. In these trials, the firms sent letters to customers informing them of the old and new interest rates and some general contact details for further information no later than 60 days before the interest rate decrease, in accordance with EU regulatory requirements. In the reverse page switching box (2) and the SMS reminder trials (5) the rate decrease applied to all customers holding the particular type of the account and occurred on the same actual date. In the digital reminder trial (4) the rate decrease occurred a fixed period of time since the individual account opening date and was part of the account terms and conditions.

Customers in the front page switching box (1) and the switching form trials (3) faced no interest rate decrease but had already been receiving an interest rate that was significantly below market average of what new customers could obtain. These customers received no other specific communication in advance of the information sent out during the trial.

We oversampled consumers with large balances in all trials relative to the market to ensure sufficient power to detect switching behavior among consumers with ample financial motivation to do so. However, we included customers with lower balances in order to understand the applicability of our findings to those customers.

Trials 1, 2 and 3 all provided customers with forward looking information about interest rates currently available to them. The trials varied in terms of the situation in which the customer received the disclosures (whether at the point of an interest rate decrease) and the specific treatments tested within each trial.

In Trial 1, consumers were already on a low rate and were due to receive their annual statement in autumn 2015. Customers were randomly selected into five equally sized groups. The control group received an annual statement with no additional information on the front page. For treatment groups, different information was added to the front page of the annual

statement. This included a simple encouragement to shop around for another account; a comparison of the currently applicable rate with the highest rate available on a comparable account with the current provider (best internal rate); best internal rate and in addition the average of three highest rates on comparable accounts with competitors (best competitor rates); and a final variant which added a graphical illustration of gains from switching. The monetary gains used in the illustration were based on an illustrative balance of £100, £1,000 or £10,000, depending on which was the next lowest to the customer's actual savings balance at the time (for example, for an actual balance of £650 the illustration was £100). We excluded customers with balances lower than £100.

In Trial 2 customers were on a good current rate and were being notified of an impending rate decrease. Customers were randomly selected into five equally sized groups. The control group received a letter which notified the customer of the rate decrease on all affected instant access accounts early summer of 2015. All letters were sent more than two months ahead of the rate decrease. The control letter included no additional information about internal or external rates. The treatments added information, formatted into box (known as the switching box] that included either the best internal rate only, or both best internal rate and best competitor rates (the average of the three highest rates on comparable accounts). Each of these two versions either had an illustration of monetary gains from switching based on an assumed balance of £5,000, or was personalized to the individuals balance at the time of mailing. All treatment versions included graphical comparisons of the rates in form of bar charts. Customers who had opted out of marketing communications were excluded from this trial.

Trial 3 goes beyond Trials 1 and 2. In addition to providing information, we test the effect of providing a form that can be completed and returned to the firm in order for the customer to be switched to an identical, "front book" product paying a better rate. In August 2015, the provider sent a one-off communication to encourage long-standing customers to switch to an equivalent internal account with a significantly higher rate. The customers were selected randomly into two equally sized groups. The control group received a letter with a switching box that included the best internal rate and the best competitor rate as noted above, as well as potential gains from switching based on a non-personalized balance example (£5,000). The treatment group received the same letter, but with a tear-off return switching form pre-filled for a switch to the best internal rate, along with a prepaid envelope.

In Trials 4 and 5, we test the effect of timely repetition of information through reminders sent via email or SMS. In Trial 4 we test email or SMS reminders sent to consumers who held accounts that experienced scheduled rate decreases during June-September 2015. Customers were randomly selected into three equally sized groups. The control group received only

an initial letter sent at least 60 days before the rate decrease, as mandated by current regulation. Two treatment groups were then issued either an email reminder or an SMS reminder. The email reminder was similar in its content to the letter sent to all groups. It included information about the previous and new interest rates, and in addition to the initial letter it included the best interest rate available on a comparable account with the firm. The SMS reminder was shorter and included no information on interest rates. Due to logistical constraints we sent the reminders on one actual date to all customers, as it was not possible to randomly allocate reminders to be sent at different points of time. Therefore the timing of reminders relative to the individual rate decrease dates was not assigned randomly. Each customer account had an interest rate decrease date which was within eight weeks before and seven weeks after the date of sending the reminders. The trial sample consisted only of customers who all had an email address and a mobile phone number on record[8]. Across the sample, 75% of the customers had balances of more than £4,000, and the remaining 25% had balances of between £1,000 and £4,000.

In Trial 5 we test the effect of an SMS reminder around the time of a rate decrease in early summer 2015. Customers were randomly selected into one of five groups. The control group received no further communication following the initial letter sent 60 days or more before the rate decrease. Customers in the four treatment groups received an SMS reminding them of the rate change, either one week before or one week after the rate decrease, or on the day of rate change. For those receiving the SMS on the day of the rate change, the SMS either encouraged switching or said that there was no higher rate on a comparable product available. Each treatment group included 16% of the trial sample and the control group included the remaining 35% of the sample. All customers in the trial had a mobile phone number on record[9]. In the sample, 60% of customers held balances of more than £5,000 and 40% held balances between £1 and £5,000. Customers who switched between assignment and the due date of the reminder still received the reminders and were retained in the sample to ensure that the comparison of effects of timing is consistent across all treatment groups.

[8]90% of customers in the sample had both email and phone number on record. The partnering institution evaluated that around 2% of email reminders and around 10% of SMS reminders could not be delivered due to invalid records. We do not adjust trial results for failed deliveries.

[9]The partnering institution evaluated that around 8% of reminders were not delivered to customers in the treatment groups due to invalid phone number records. We do not adjust trial results for failed deliveries.

4 Empirical Strategy

4.1 Data and Balance Tests Across Treatment/Control

We collect a rich administrative data set from each bank which includes account balances, account closures and new account openings before, during and after the trial intervention. In addition the dataset provides us with age, gender, account age, mobile and online banking behavior, whether they hold a checking account at the same bank and the number of other financial product holdings.

Descriptive statistics for each trial are shown in Table 2. The sample size of each trial varied depending on the number of treatments tested and the available customer base. The length of the observation period also varied due to practical constraints. We collected data on basic customer demographic characteristics and their financial product holdings with the partnering institution. We find that the variation of customer age and gender across the trials is relatively low, while average savings balance, proportion of customers who have their current account with the same bank, and average account age varied more widely, in line with our sample selection procedure and understanding of the different customer mixes across the five providers as summarized above.

As shown in Table 3, the means of key demographic statistics in control and treatment groups are well balanced and cause no concern about systematic selection bias in the assignment of customers into trial groups. In some instances, equality of means of age, account age and gender are rejected individually at 5% significance level, although the differences in means are not practically significant. The partnering institutions which conducted the trials stratified the trial groups across somewhat different subsets of key demographic and financial variables, explaining the differences in average values across groups observed in some variables. The joint test p-values suggest that the key customer demographic variables were equally distributed across all trial groups.[10]

The primary outcome we are interested in is switching. We define switching as when customers convert, close or empty their savings account. A conversion is when an account is changed from the existing, back-book interest rate to a new, front-book product available at the same provider. The old account is closed and the new account with new terms is created instantaneously, with the new account inheriting all of the product attributes of the former (bank-branch network, account number, ATM card, linked accounts, etc.) except for the savings product label and the associated interest rate. Individuals are also able to open

[10]The joint test rejects the equality of means across treatment and control in the SMS-only Trial 5, although the magnitude of the significant differences are economically small (six-months and one-month difference in depositor age and account age, respectively).

a new account with the same firm and start saving in that account or withdraw their money and use it for any other purpose. We consider an account emptied if 95% or more of the balance recorded at the start of the trial is withdrawn from the account. Where customers either converted their account, or opened a new comparable savings account with the same firm, moved some money into it and emptied their old account, we define this sub-set of switching as *Internal Switching*. We define all remaining switching that does not fall into Internal switching as *Other Switching*.[11]

The distinction between Internal, Other and Non-switchers is helpful, because we observe the interest rate only when customers switch internally (Internal switchers), or if they do not switch (Non-switchers). We do not observe the interest rate for customers who switch to a different type of account (such as tax-free savings) or who move to a different provider (Other switchers). Therefore, in our data, we could not separate the outcomes for Other switchers. Potential outcomes include transferring the balance to an account outside the firm or a different account with the firm (but not the specific comparable account), or withdrawing the money and spending it or investing elsewhere. We assume that these customers probably obtained a better interest rate or a more suitable product, because they typically would have had access to a wide range of options in the market. However, we do not know their exact outcome. When an option for internal switching to a higher-paying rate exists, at a minimum internal switching dominates not switching (subject to switching costs). An even better option may exist for the household than internal switching, motivating our focus on *All Switching*, defined as internal or other switching. Again, modulo the disutility of switching, not switching is a strictly dominated option for borrowers with an option to internally switch.

To understand consumer choices of accounts, it is useful to consider the relative attractiveness of Internal and Other switching given the rate differentials and the potential costs involved in switching. In all trials, the interest rate customers would receive if they took no action following the trial treatments was no higher than 0.5% per year. Within each provider, the difference between the applicable rate and the highest available internal rate ranged from 0.2% to 0.9%. The best competitor interest rates were comparable for all trials and ranged between 1.08-1.35% and the incremental gain from Other switching compared to Internal switching ranged from 0.1 to 0.6 percentage points. However, the incremental cost of Other switching could have been substantially higher than the cost of Internal switching. Switching to another provider would involve (1) search and evaluation of alternative brands and product features, (2) fixed cost of transition to a new provider, (3) ongoing convenience

[11]Where our measures overlap, we record the latest action as the final action. For example, customers who closed their account after converting it would be classified as Other switchers. Internal switching and Other switching are mutually exclusive and always sum up to All switching.

cost of monitoring the account and/or having different level of service, such as the presence of a network of branches, and (4) operational cost of time to open the account and transfer the funds. In contrast, Internal switching would involve (4) and almost none of (1)-(3). Contrasting pairs of columns in Table 4 provides evidence on this relative attractiveness of Internal switching. Nearly all of the All switching results are accounted for by Internal switching.

5 Results and Analysis

Table 4 reports our main treatment effects findings. We report results from an linear-probability model of a given switching outcome for individual i on a set of treatment dummies $Treatment_{ik}$ for each treatment k that was included in the trial $\mathcal{T}(i)$ in which i was enrolled.

$$Switching_i = \sum_{k \in \mathcal{T}(i)} \beta_k Treatment_{ik} + X_i'\gamma + \varepsilon_i \tag{1}$$

The omitted category is the control group such that each treatment coefficient β_k tells us about the percentage point increase in switching for a given treatment group k relative to the control group. Controls X_i consist of age (measured in tens of years), age squared, gender, potential gains from switching to the best available external rate (measured in hundreds of pounds), and potential gains squared. Given the random assignment (by construction and verified in Table 3), our results will be similar regardless of including controls. We include them, however, because they are of independent interest, enhance precision by reducing residual variation, and are useful context for studying treatment effect heterogeneity. We estimate equation (1) separately for each trial and report average treatment effects for various ways of measuring switching outcomes.

For the first trial, there were four treatment arms, each variants on the design of an informational disclosure situated on the front page of an annual statement. The Call to Action treatment that did not provide any information about competitor rates, and increased internal switching by 0.5 percentage points (column 2) and all switching by 0.9 percentage points (column 1). While the option to switch internally should strictly dominate not switching (up to a non-monetary switching cost), it may be that an outside option dominates internal switching such that all switching is the weakly optimal choice. Although 0.9 percentage points is high in relative terms given the low baseline level of switching for the population of borrowers in the first trial, it is low in absolute terms given the optimality of the choice and our conditioning on potential gains.

The other treatment arms of the first trial were more successful, although still ineffective

in absolute terms. Interestingly, showing the best internal rate led to more switching (2.9 percentage points higher in column 1) than showing the best internal rate available *and* the best competitor interest rate on savings accounts (1.8 percentage point increase in column 1). While this difference is small, it is statistically significant and consistent with choice overload models as in Schwartz (2009). Showing a graph to illustrate a borrower's rate in the context of available market rates increase switching slightly but is still dominated by simply showing the best internal rate. The relative success of various disclosure designs is also robust to focusing entirely on internal switching. Within the treatments of Trial 1, information is valuable and design matters, but even the most effective disclosure design that provides information still moves switching by less than three percentage points.

The second trial had the same set of treatment arms varying disclosure design as Trial 1 but were mailed to borrowers in a different context. The disclosures for the second trial were on the back page of mailing informing them that their savings account interest rate was about to decrease. Despite providing the same information as the disclosures of Trial 1, we cannot reject that the Trial 2 disclosures had zero effect on either type of switching, irrespective of design.

The most effective treatment we tested was Trial 3, in which both treatment and control groups received an informational disclosure about their rates and better available rates in a letter. Treatment-group account holders' letters were accompanied by a detachable pre-filled form and a prepaid envelope. Signing and mailing this form in a prepaid envelope would automatically switch an individual's account classification to an otherwise identical savings product with a higher interest rate. Although the control group received an informational treatment similar to the front-page switching box in Trial 1, their internal and all switching rates were 0.5% and 3%, respectively. By contrast, the pre-filled detachable form the treatment group received increased internal switching by 8 percentage points (column 6) and all switching by 9 percentage points (column 5). We interpret the relative success of Trial 3 at inducing switching as indicative that the cognitive costs or disutility of paperwork is a key component of customer inertia and that process improvements have the potential to be effectual.

Trials 4 and 5 were digital in nature with treatment group account holders receiving informational disclosures by email or text message. In both trials, customers received a legally mandated letter informing them of an impending decrease to their savings account rate sixty days before the decrease. Treatment group borrowers then received reminders by email or text message. In Trial 4, the control group mean switching is highest, suggesting that these deposits are the least sticky of all the trials customer bases, most likely due to their relatively high balances, young account ages, online banking share, and the sample being selected on

having an email address. The email reminder was more successful than the text-message reminder at inducing switching (5.3 versus 4.1 percentage points, respectively). While this could be plausibly attributed to the additional, personalized information content of the email relative to the text message or the relative ease with which a borrower could accomplish account switching once already on a computer instead of a cell phone, the difference between the two coefficients is small and not statistically significant.

A unique feature of Trial 4 is that although all customers received disclosures on the same day, cross-sectional heterogeneity in when this day fell relative to impending rate decreases varied because of heterogeneity in account opening dates and a fixed introductory rate period. We explore which groups had the strongest reaction to the disclosure and what timing was the most effective in Table 5. Treatment effects were strongest for emails received 0-2 weeks before the rate decrease, and text-message effects were smallest for reminders received on the day of the decrease. The presence of any sort of timing effects is curious. Even for customers with large balances, the difference in switching a few weeks early or late is small in monetary terms, and yet customers are twice as responsive to disclosures received immediately before an impending rate decrease as otherwise. We interpret this as evidence of the increased salience of reminders that appear to have a natural deadline attached and may be easier to dedicate the necessary attention. The low effect of text message disclosures on the day of switching may be driven by the likelihood that text messages are read at a time that is inopportune for consumers to take action or resolve to take action in the future and as a medium less conducive to converting into a memorable task.

Returning to Table 4, Trial 5 varied the design of the disclosure text message in a similar way to Trials 1 and 2. All forms of the disclosure were similarly small in magnitude (1-2 percentage point increase in switching), although the simple call to action treatment and the treatment that illustrated the difference between the customer's current rate and the best external rate were statistically significantly more effective. While it is interesting that the information-free call to action treatment was more effective in Trial 5 than Trials 1 and 2, we are limited in what we can conclude from Trial 5. Small effect sizes aside, there was no internal switching option for Trial 5 (the provider only had one type of savings account paying the same rate for everyone in each account-opening cohort). This means that it is not guaranteed that external switching (the only switching outcome we examine for Trial 5) would be a dominating option. It is conceivable that switching account numbers, branch and ATM networks, etc. would cause a customer to incur sufficient disutility as to make not switching through the rate decrease to be optimal.

Across all five trials, the control variables have little explanatory power. The youngest and oldest borrowers appear the most likely to switch, along with borrowers with the highest

gains, though the effect of both of these controls is small and of inconsistent sign across the trials. Comparing all switching and internal switching effects, we find that most switching was internal switching. In Trial 3, for example, 89% of the extra switching in the treatment group relative to the control group was internal switching.

In Table 6, we explore two dimensions of treatment effect heterogeneity to test whether there are groups for which disclosure is particularly effective. In particular, we are interested if knowing whether the lack of disclosure responsiveness is driven by customers with high opportunity cost of time or low balances, for whom switching costs may be large or switching gains may be small, respectively. Looking at age groups most likely to be retired and balance categories with substantial interest gains allows us to check whether our disclosure's relative ineffectiveness is likely due to purely rational factors such as time costs or low benefits. We pool all treatments within a trial into one treatment indicator and ask whether treatment effects are stronger for several discrete groups denoted j. We estimate

$$Switching_i = \beta \cdot Treatment_i + \sum_j (\psi_j \cdot Treatment_i + \alpha_j) + \varepsilon_i \tag{2}$$

where α_j is the coefficient on an indicator variable for each group j including four age categories and three monetary gains categories and ψ_j captures the degree to which the main treatment effect β is different for customers falling into each category j. The omitted age and gains categories are under 40 years old and under £50 in annual gains from switching to the best available interest rate. Looking across the trials, the preponderance of the evidence shows that disclosure is equally (in)effective for all age and balance levels. Put another way, even among customers for who the gains of switching are particularly high or who are likely to be retired, the overall effects of our tested disclosures are low.

Taking stock, we find that best-performing intervention increased switching to a higher-rate paying account by nine percentage points, while the worst-performing intervention had a precisely estimated zero effect on switching. While there is scope to improve disclosure effectiveness through optimal design, we note the converse is also true. Unless every aspect of disclosure design is precisely specified, firms may always have an incentive and scope to tweak design to render disclosure ineffectual, consistent with Grubb (2015) and Persson (2018). Moreover, even among borrowers facing a nearly costless task of switching to a strictly dominating internal account worth at least $100 in year one, the most optimally designed (or timed) disclosure only encourages an additional nine percentage points of switching off of a very low baseline.

6 Survey Evidence

In this section we describe the key findings of the follow-up surveys. We conducted the surveys with 261 consumers from the sample of the Front page switching box trial and with 500 consumers from the sample of the Reverse page switching box trial. Both survey samples were largely composed of long-standing customers with low overall propensity to engage with their savings. However, we note that participation in the follow-up survey was voluntary and the sub-set of customers who engage with their savings more were more likely to agree to answer the survey. Both surveys were conducted over the phone within three weeks after we stopped collecting data on trial outcomes by a research company. We imposed quotas on the survey sample along two dimensions: observed switching behavior and starting balance. As anticipated, around one in ten contacted customers agreed to complete the phone interview which lasted up to 15 minutes.

In the survey we measured:

- recall (interest rate, interest rate change, and communications received)
- intermediate actions of customers (shopping around, and the time it took)
- subjective evaluations (satisfaction with the individual decision taken)

To ensure a degree of response quality in the survey, we asked customers to indicate which providers they held their savings account with. We terminated interviews with a small number of customers who failed to indicate they had or until recently had had an account with the provider in question even after being prompted. Further, we asked customers who were eligible to continue the survey to recall details about the account in question, including basic properties such as interest rate payable and the actions they took. We matched survey responses to the administrative data provided by the institutions.

For a substantial share of customers actions recorded in the administrative data and actions reported in the survey did not match. There was a bias in under-reporting of action by those who took action, compared to over-reporting of action by those who did not take action. We attribute this mismatch to a combination of imperfect recall, lack of incentives to report truthfully, and possibly drawbacks in questionnaire design. We interpret the responses related to reported and intended action and recall of information with caution and focus on key findings as reported below.

6.1 Attention

The survey shows that customer awareness of their interest rate and recent changes to the rate was not widespread. Most respondents were not aware of the interest rate they were receiving on their savings account and the vast majority of those who estimated the rate upon prompting thought the rate was higher than the actual rate.

In both surveys the majority of respondents indicated that by far the most important single factor to make them consider switching their savings would be an equivalent product with a higher interest rate available with their current provider - 48% in the front page switching box trial and 36% in the reverse page switching box trial. For comparison, 4% of customers in the front page switching box trial and 8% in the reverse page switching box trial indicated a better rate available with another provider as the most important factor to switch – consistent with most consumers switching internally. However, a substantial proportion of respondents reported that they were not aware that their provider offered a higher rate on an equivalent account (56% in the front page trial survey and 58% in the reverse page trial survey).

6.2 Search and comparison

The recall of any recent communication from their provider related to better available interest rates was modest – approximately 40% of customers in both surveys did not recall the letter or the annual statement. Of those respondents who remembered receiving a communication from their provider, 75% and 60% in the digital and SMS reminders respectively, reported that they did not read beyond the first page or only skim-read the communication. Customers in the age group of 60-80 years were most likely to recall and have read the letter in detail – by 25 and 15 percentage points more, respectively, relative to customers aged 40 years or younger. Gains from switching or the age of account did not predict whether customers recalled or read the communication in detail.

Both surveys showed that respondents who remembered receiving the letters found it difficult to recall the details without explicit prompting by the interviewer. When prompted explicitly, respondents reported recalling details of both the control letters and the treatments. However, many respondents, especially in the front page switching box trial recalled information that they did not receive, which points to a difficulty to recover factual information using survey methods.

6.3 Decision to switch and implementation

Gains from switching One third of respondents in both surveys said they would require a gain of £100 or more per year to switch, while two thirds would switch for £100 or less per year. In our sample, where the potential gains were significantly higher than the market average, 26% of customers had £100 or more to gain. The high proportion of customers for whom the potential gains were lower than their reported cost of switching could explain why many customers did not switch. However, we note that if required gains from switching remained similar and the difference in interest rates were to increase modestly in absolute terms, the net gains from switching would rise substantially in relative terms. In turn, this could make information about potential gains from switching relevant to more consumers and therefore increase its effectiveness.

Effort and time to search and switch In both surveys customers found searching as easy as expected, or easier than expected. In the front page switching box trial, 60% of respondents who reported having searched for alternative accounts found the process of searching a little or much easier than expected. In the reverse page switching box trial, 42% of customers who switched rated the process of searching 'easier than expected', while this accounted for only 14% of the customers who did not switch. Eighty percent of customers in the front page switching box trial and two thirds of customers in the reverse page switching box trial said they spent no longer than 2 hours searching for an alternative account. Regardless of final action, the majority of surveyed customers in both groups said searching took them as long as expected.

We estimate the time it took to switch internally to be around 15 minutes on average in the switching box trials and somewhat longer for External switching. We find that 83% of internal switchers in the reverse page switching box trial said it took them no longer than 30min to open a new account (54% of other switchers said that). 71% of internal switchers in the front page switching box trial said it took them no longer than 30min to open a new account (62% of other switchers said that).

Satisfaction with decision When evaluating the decision they made, respondents who switched their account reported to be more satisfied than respondents who did not switch their account.

6.4 Effects of treatments on survey outcome measures

In the front page switching box trial, treatments had a positive significant effect on the awareness of a better internal rate, but not on other survey outcome measures, including the likelihood of thinking about switching, searching for better accounts and the number of accounts compared. Treatments in the reverse page switching box trial had no significant effect on survey outcome measures, including consideration to switch, awareness of their interest rate, shopping around and recall of the communication received.

In summary, survey findings suggest that a large proportion of consumers do not actively engage with choosing optimal savings accounts and do not follow communications from their providers. Many of those who do engage review the communications quickly and remember only the key aspects.

7 Conclusion

The role of disclosure in retail financial markets has to date been assessed mostly in relation to consumer protection. For example, Campbell, Jackson, Madrian, and Tufano (2011) advocate robust consumer financial protection and conclude that while disclosure is a worthwhile measure, it may not be sufficient to ensure an appropriate degree of consumer protection in all circumstances.

In this paper, we examined the role of disclosure in stimulating switching to more competitive products. We conducted an experiment on savings accounts, a retail financial market with relatively homogeneous product features and high prevailing price dispersion, an equilibrium likely supported by consumer inertia. We tested the importance of disclosure design features and examined explanations for their role in affecting consumer financial decisions. We provided direct evidence using randomized controlled trials conducted to inform policy development by the UK regulator—the Financial Conduct Authority.

Good timing, salience of better alternatives, and a straightforward procedural way to take an informed action helped to mitigate the obstacles consumers face, and to some extent increased switching to better alternative products. However, disclosure interventions stimulated switching only within the current providers and had no effect on switching to better-paying products offered by other providers. Our interventions, especially the return switching form, had at best modest positive effects in increasing switching by long-standing customers on a low interest rate. Our findings reveal that while in some segments of the market there is a notable level of switching when interest rates decrease, limited consumer attention, coupled with the inconvenience of switching, prevents widespread benefits from

disclosure.

When firms have several dimensions of disclosure design left to their discretion, there are simple ways to present required information content that limits its salience to consumers, with the classical example being fine print. While the details of design, timing and delivery of our disclosure can alter its effectiveness without materially affecting costs, the overall effects of our most effectively designed disclosure on account switching remain modest. It is possible that the effects of disclosure would become more pronounced once customers become more habituated to receiving this type of information from their providers, or when returns on savings rise. Our survey and qualitative research findings, however, indicated that attention to disclosure is low and that consumers reacted cautiously to disclosure from their providers about better alternative accounts. This suggests that beyond testing and optimizing disclosure the regulators need to consider a wider set of interventions that are better targeted at achieving more substantial improvements in the market outcomes for many customers of retail banking services.

Finally, there may also be unintended consequences of mandatory disclosure of interest rates. Duarte and Hastings (2013) show that consumers may overly fixate on disclosed dimensions at the expense of other product characteristics and Medina (2018) documents that there could be negative externalities on the rest of a household's financial portfolio. While this is perhaps less of a concern in our setting because of our focus on saving instead of borrowing, UK deposit insurance, electronic access, and the fact that savings accounts are relatively homogenous products, firms interested in obfuscating lower prices (higher interest rates) still have many tools at their disposal.

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Figure 1: Example Call to Action Treatment Letter

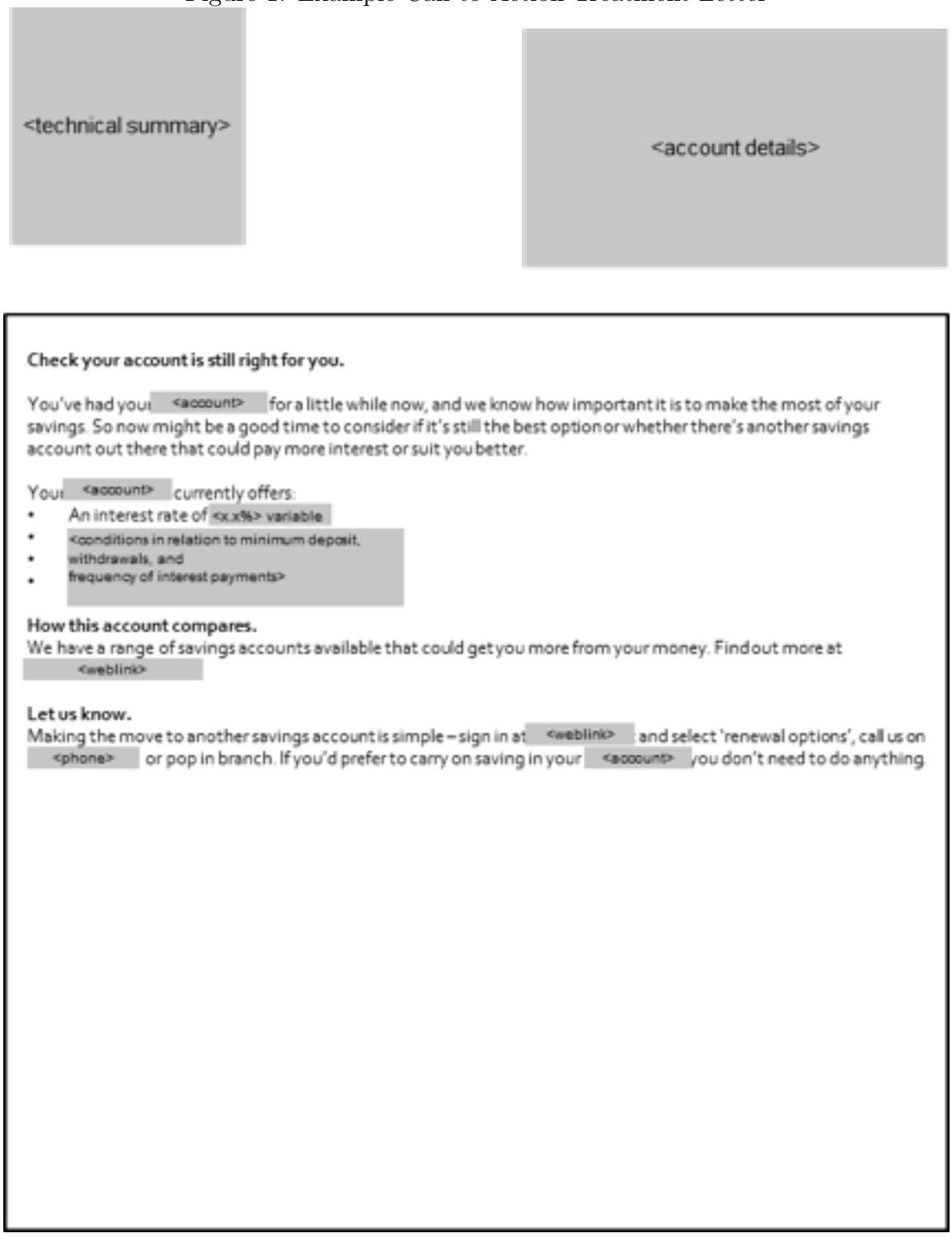


Figure 2: Example Treatment with Best Internal and Market Rates with Graph

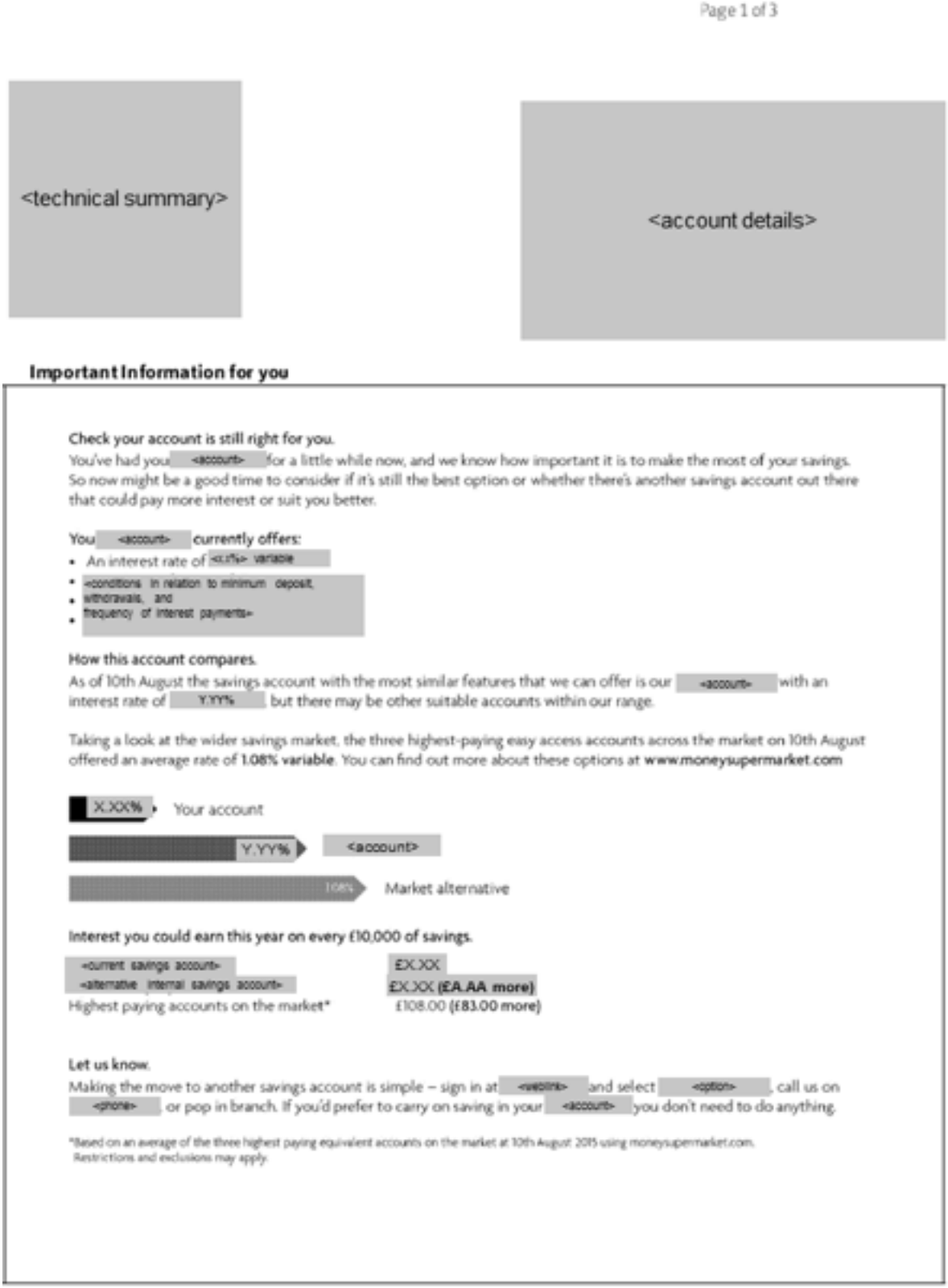
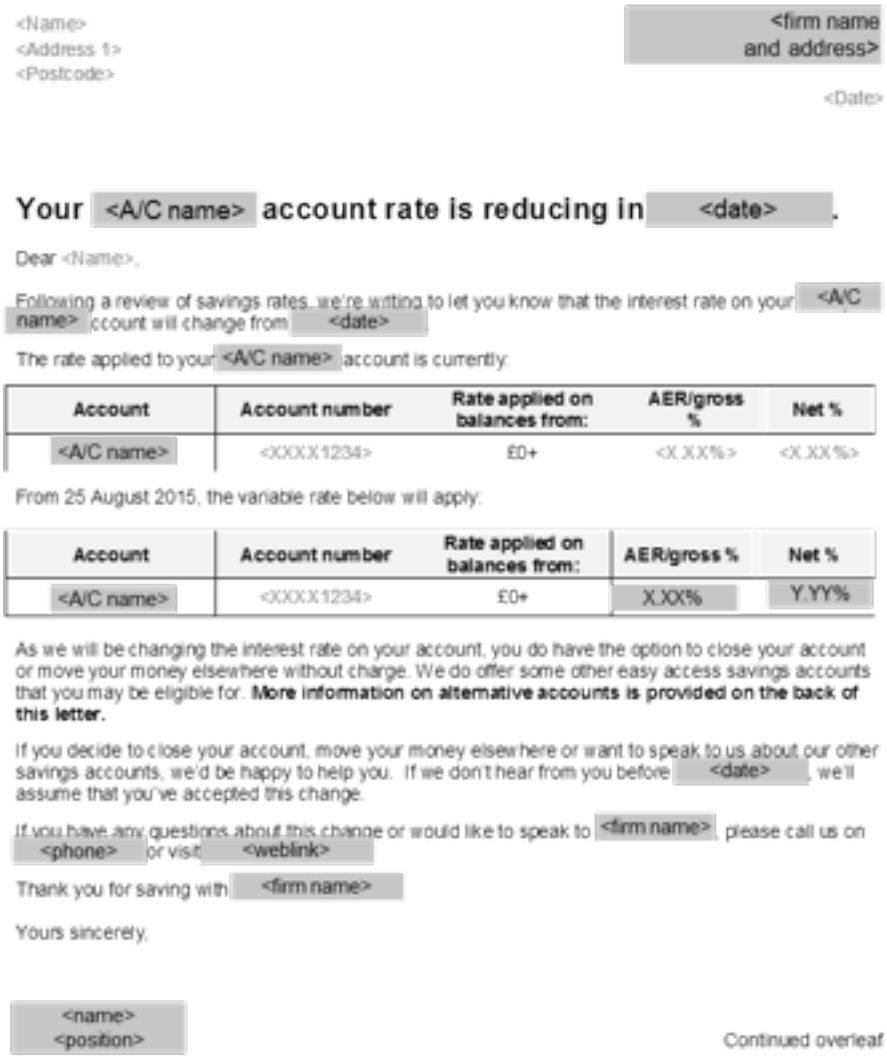


Figure 3: Example Rate Change Letter



<Name>
<Address 1>
<Postcode>

<firm name and address>

<Date>

Your <A/C name> account rate is reducing in <date>.

Dear <Name>,

Following a review of savings rates, we're writing to let you know that the interest rate on your <A/C name> account will change from <date>.

The rate applied to your <A/C name> account is currently:

Account	Account number	Rate applied on balances from:	AER/gross %	Net %
<A/C name>	<XXXX1234>	£0+	<X.XX%>	<X.XX%>

From 25 August 2015, the variable rate below will apply:

Account	Account number	Rate applied on balances from:	AER/gross %	Net %
<A/C name>	<XXXX1234>	£0+	X.XX%	Y.YY%

As we will be changing the interest rate on your account, you do have the option to close your account or move your money elsewhere without charge. We do offer some other easy access savings accounts that you may be eligible for. **More information on alternative accounts is provided on the back of this letter.**

If you decide to close your account, move your money elsewhere or want to speak to us about our other savings accounts, we'd be happy to help you. If we don't hear from you before <date>, we'll assume that you've accepted this change.

If you have any questions about this change or would like to speak to <firm name>, please call us on <phone> or visit <weblink>.

Thank you for saving with <firm name>.

Yours sincerely,

<name>
<position>

Continued overleaf

Figure 4: Example Reverse-Page Comparison Box



COULD YOU GET A BETTER RETURN ON YOUR <firm name> SAVINGS?

Your account: <A/C name>

Your balance: <£5,432> **as at 30 April 2015**

Your new interest rate: X.XX% **AER/gross**

Account type: <A/C name> – you can withdraw money without charge

Your rate: X.XX% AER/gross

Our best comparable rate: <A/C name> B.BB% AER/gross (inc. bonus)

Average of 3 of the highest paying accounts: 1.32% AER/gross (may inc. bonus)

How does my savings account compare?

As at 26 May 2015, the highest interest rate available from <firm name> account is B.BB% AER/gross on our <A/C name> (inclusive of <length> introductory bonus of C.CC% AER/gross). The <A/C name> is an easy access account that can only be managed online using Internet Banking – you cannot access the account in branch or over the phone.

Three of the highest paying easy access account offered by other banks and building societies offer an average rate of A.AA% AER/gross. Price comparison websites can provide information on rates offered by other providers.

How much more could I earn in interest?

A balance of £5,432 in a <firm and A/C name> would earn £X.XX this year.

Best comparable <alternative with firm> £Y.YY **in total** (or £A.AA more) a year.

Average of three of the highest paying accounts on the market: <£xx.xx> **in total** (or <£x.xx> more) a year.

Moving your money is easy.

To move your money to <alternative with firm> simply call us on <phone>, visit <weblink> or visit us in branch to find out more. To move your money to an account offered by an alternative provider, open a new account with them and transfer your funds.

Average of 3 of the highest paying accounts currently on offer on the market at 26 May 2015 using moneyfacts.co.uk. Some restrictions may apply.

Calculations based on interest rates at 26 May 2015 and show interest earned prior to appropriate tax deductions dependent on your individual circumstances and your current tax status. Rates are variable and subject to change. To open an <A/C name> account you must be 16 or over, and have a current account with us.

If you'd like this in another format such as large print, Braille or audio please ask in branch.

<directions to customers with hearing or speech impairment>

Figure 5: Example Rate Change Letter with Detachable Switching Form

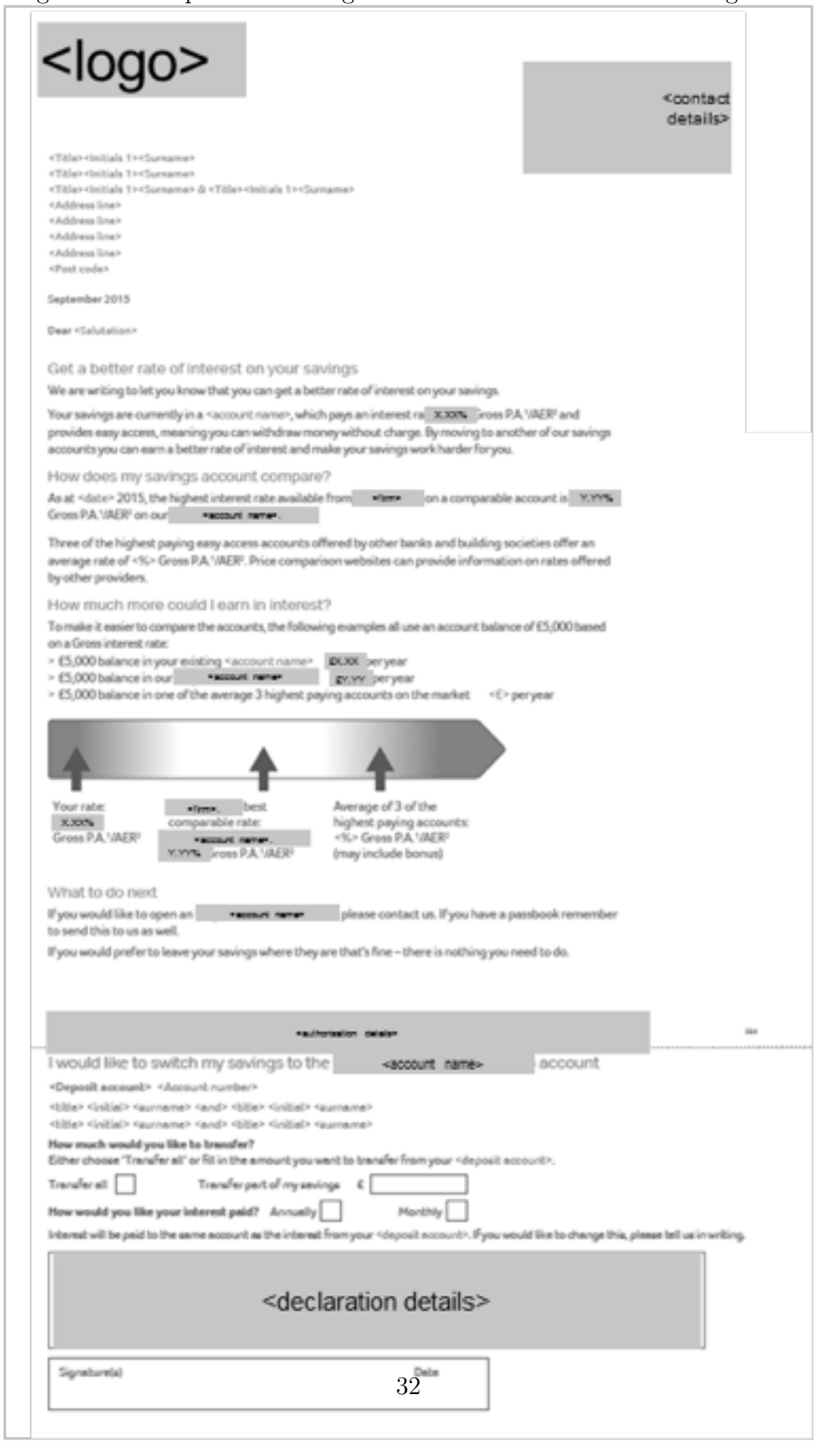


Figure 6: Example Email Disclosure

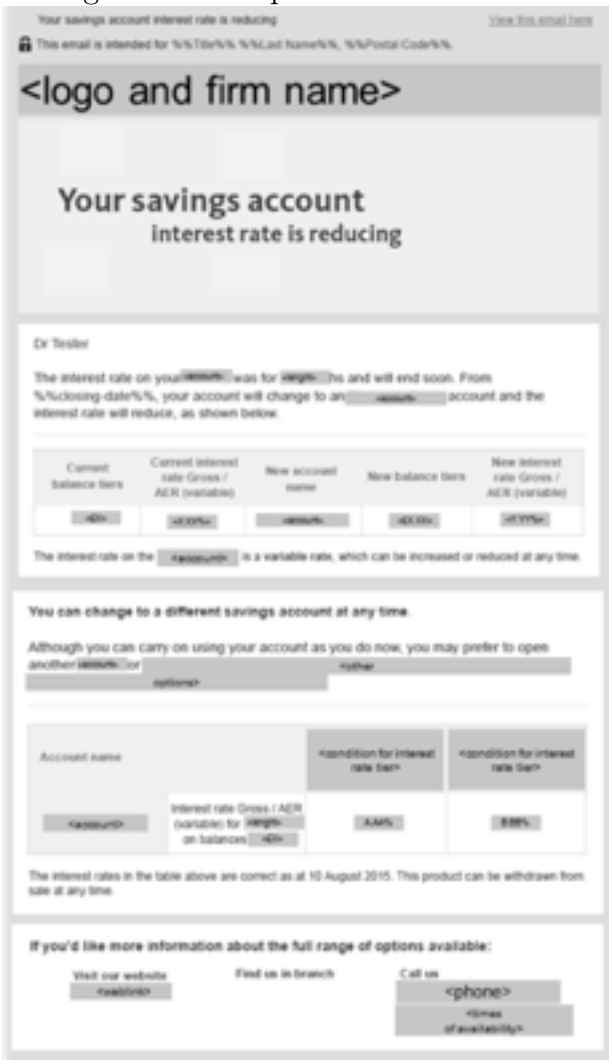


Figure 7: Example Text Message Disclosure

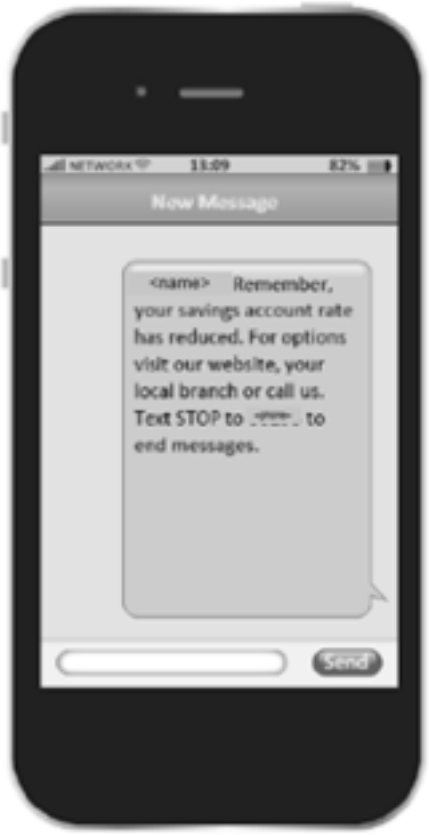


Figure 8: Example Test Message Disclosure with Varied Timing

One week before rate change:	"Reminder: the interest rate on your <firm and> account> will reduce from <date>. Log on to online banking to assess your savings options."
Day of rate change, Version 1:	"Reminder: the interest rate on your <firm and> account> will reduce today. Log on to online banking to assess your savings options."
Day of rate change, Version 2:	"Reminder: the interest rate on your <firm and> account> will reduce today. This is the highest instant access rate on a non-ISA savings account"
One week after rate change:	"Reminder: the interest rate on your <firm and> account> reduced on <date> Log on to online banking to assess your savings options."

Table 1: Trials Overview

	Trial	Treatment details	Rate change	Customer tenure
1	Front-page switching box	Comparison with market rates on front page of annual statement	None	Long
2	Reverse-page switching box	Comparison with market rates on back of rate-change notification letter	Yes, 60 days after treatment to all customers	Mixed
3	Return switching form	Tear-off form pre-filled to switch to higher rate-paying account with same provider	None	Long
4	Digital reminder	Rate decrease reminder via email or SMS	Yes, end of individual bonus period seven weeks before to eight weeks after treatment	Short
5	SMS reminder	Rate decrease reminder via SMS	Yes, one week before to one week after treatment to all customers	Mixed

Note: Table overviews five trials, describing the treatment, whether it was accompanied by a change in interest rates, and the typical customer tenure for the purpose of this research, we define customer relationship as "long" if it is longer than 10 years and as "short" if it is shorter than 2 years. The indications in the table are only approximate and reflect the distribution of account age in each trial sample.

Table 2: Descriptive statistics of consumers

	Front page switching box trial	Reverse page switching box trial	Return switching form trial	Digital (email and SMS) reminder trial	SMS reminder trial
	(1)	(2)	(3)	(4)	(5)
Age	59.2	53.2	64.4	52.9	42.4
	(16.58)	(17.23)	(15.92)	(16.15)	(13.92)
Gender	0.42	0.41	0.45	0.48	0.52
	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)
Checking Account Indicator	0.25	0.80	0.06	0.77	0.98
	(0.43)	(0.40)	(0.24)	(0.42)	(0.16)
Account balance (£)	8,436	7,407	6,812	37,939	24,162
	(20787.50)	(22861.81)	(18156.38)	(88632.98)	(78574.19)
Potential gains (£)	70.02	82.96	76.29	230.56	198.13
	(172.54)	(256.05)	(203.35)	(538.50)	(644.31)
Account age (years)	13.7	6.7	16.1	1.0	4.7
	(10.86)	(1.25)	(3.99)	(0.09)	(2.45)
Number of products with provider	1.6	4.6	1.6	4.6	5.4
	(0.88)	(1.88)	(1.28)	(2.55)	(2.86)
Share used online banking	0.09	0.58	-	0.84	0.90
	(0.28)	(0.49)	-	(0.37)	(0.29)
Share used mobile banking	0.09	0.29	-	0.22	0.30
	(0.29)	(0.45)	-	(0.42)	(0.46)
Observations	61,879	13,261	4,003	15,487	30,202

Standard deviations in parentheses.

Table 3: Means of demographic variables and tests of equality of means

	Number of Observations	Age	Balance £	Gender (% male)	Checking account holding (%)	Account age (years)	Joint test P-value
			Panel I. Front page switching box				
Control	12,723	59.33	8,685	0.42	0.25	13.76	
Treatment	49,156	59.20	8,371	0.42	0.24	13.71	
Difference P-value		*0.45*	*0.13*	*0.89*	*0.12*	*0.66*	*0.20*
Total Observations				61,879			
			Panel II. Reverse page switching box				
Control	2,659	53.93	7,359	0.41	0.80	6.74	
Treatment	10,602	53.01	7,419	0.41	0.80	6.71	
Difference P-value		*0.01*	*0.90*	*0.94*	*0.99*	*0.31*	*0.11*
Total Observations				13,261			
			Panel III. Switching form				
Control	1,999	64.65	6,749	0.44	0.06	16.00	
Treatment	2,004	64.22	6,874	0.46	0.06	16.12	
Difference P-value		*0.40*	*0.83*	*0.22*	*0.80*	*0.35*	*0.72*
Total Observations				4,003			
			Panel IV. Digital reminder				
Control	5,180	51.86	37,957	0.48	0.79	0.96	
Treatment	10,307	52.02	36,801	0.48	0.78	0.96	
Difference P-value		*0.57*	*0.43*	*0.56*	*0.51*	*0.31*	*0.66*
Total Observations				15,487			
			Panel V. SMS reminder				
Control	10,200	42.69	25,046	0.53	0.97	4.62	
Treatment	20,002	42.22	23,711	0.51	0.98	4.70	
Difference P-value		*0.01*	*0.16*	*0.00*	*0.70*	*0.01*	*0.00*
Total Observations				30,202			

Table 4: Treatment effects on All switching and Internal switching

	1. Front-page switching box annual statement		2. Reverse-page switching box		3. Switching form one-off letter		4. Digital reminder rate decrease		5. SMS reminder
	All	Internal	All	Internal	All	Internal	All	Internal	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Call to Action Treatment	0.009***	0.005***	-0.0002	0.002					0.019***
	(0.002)	(0.001)	(0.007)	(0.001)					(0.005)
Best Internal Rate Treatment	0.029***	0.025***	-0.006	0.001					0.013***
Best Internal and Competitor Rates	(0.002)	(0.002)	(0.007)	(0.001)					(0.004)
Best Internal and Competitor Rates	0.018***	0.016***	-0.004	0.000					0.012***
Treatment	(0.002)	(0.002)	(0.007)	(0.001)					(0.004)
Best Internal and Competitor Rates	0.021***	0.020***	-0.002	0.001					0.021***
+ Graph Treatment	(0.002)	(0.002)	(0.007)	(0.001)					(0.005)
Return Switching Form Treatment					0.089***	0.079***			
					(0.008)	(0.006)			
Email Treatment							0.053***	0.051***	
							(0.009)	(0.009)	
SMS Treatment							0.041***	0.037***	
							(0.009)	(0.008)	
Age (10s)	-0.024***	-0.001	-0.031***	0.002*	0.050***	0.043***	0.135***	0.124***	-0.065***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age^2 (10s)	0.000***	0.000***	0.0002***	-0.000	-0.000***	-0.000***	-0.001***	-0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Potential Gain (100s £)	-0.000	0.003***	-0.007***	0.000	0.006	0.006**	-0.012***	0.019***	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(Potential Gain)^2 (100s £)	0.000***	0.000	0.000***	-0.000	-0.000**	-0.000**	-0.000***	-0.000***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male	0.001	0.000	-0.000	0.000	-0.003	0.002	0.016**	0.009	0.009***
	(0.002)	(0.001)	(0.005)	(0.001)	(0.008)	(0.006)	(0.008)	(0.007)	(0.003)
Control-Group Mean	0.026	0.009	0.077	0.026	0.030	0.005	0.400	0.267	0.062
Observations	61,879	61,879	13,261	13,261	4,003	4,003	15,487	15,487	30,202
R-squared	0.005	0.012	0.008	0.001	0.034	0.043	0.067	0.099	0.010

Note: Table reports heterogeneity in disclosure treatment effects at four-week horizon by disclosure design. Internal switching is switching to an different instant-access savings product at the same bank. All switching is an indicator for whether the depositor switched to a different product with the same provider or withdrew their entire balance. For the SMS reminder trial, there was no internal alternative such that all switching is other switching. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 5. Treatment Effect Heterogeneity by Reminder Timing

	Digital	SMS
	(1)	(2)
Treatment *		
4+ weeks before	0.018*	
	(0.011)	
2-4 weeks before	0.046***	
	(0.012)	
0-2 weeks before	0.102***	0.019***
	(0.014)	(0.005)
Day of switch		0.012***
		(0.004)
0-2 weeks after	0.053***	0.021***
	(0.012)	(0.005)
2-4 weeks after	0.053***	
	(0.016)	
Age	0.013***	-0.006***
	(0.001)	(0.001)
Age^2	-0.000**	0.000***
	(0.000)	(0.000)
Potential Gain (£)	0.000***	-0.000**
	(0.000)	(0.000)
$(Potential\ Gain)^2$ (£)	-0.000***	0.000**
	(0.000)	(0.000)
Male	0.016**	0.009***
	-0.008	(0.003)
Constant	-0.119***	0.231***
	-0.026	(0.016)
Observations	15,487	30,202
R-squared	0.068	0.010

Table reports disclosure treatment effect heteroeneity by the size of reminders relative to interest-rate decrease for the digital (email and SMS) and SMS trials. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 6: Switching treatment effect heterogeneity by trial

	Front page switching box	Reverse switching box	Switching form	Digital reminder	SMS reminder
	(1)	(2)	(3)	(4)	(5)
Treatment Indicator	0.013**	-0.005	0.065**	0.021	0.027***
	(0.006)	(0.014)	(0.025)	(0.014)	(0.006)
Treatment *					
Age 40-60 yrs	-0.003	0.002	-0.009	0.018	-0.006
	(0.006)	(0.016)	(0.028)	(0.018)	(0.007)
Age 60-80 yrs	0.008	-0.002	0.035	0.028	-0.016*
	(0.006)	(0.017)	(0.029)	(0.019)	(0.008)
Age >80 yrs	0.027***	-0.026	0.019	0.025	0.001
	(0.008)	(0.024)	(0.032)	(0.050)	(0.027)
Gains £50-100	0.005	0.020	0.021	0.015	-0.005
	(0.005)	(0.015)	(0.035)	(0.021)	(0.009)
Gains £100-500	0.004	0.014	0.037	-0.003	-0.020***
	(0.005)	(0.012)	(0.023)	(0.017)	(0.007)
Gains >£500	0.009	-0.006	0.004	-0.011	-0.003
	(0.012)	(0.024)	(0.054)	(0.025)	(0.011)
Age 40-60 yrs	-0.017***	-0.032**	0.008	0.138***	-0.028***
	(0.005)	(0.014)	(0.012)	(0.014)	(0.005)
Age 60-80 yrs	-0.013**	-0.036**	0.023*	0.256***	-0.023***
	(0.005)	(0.015)	(0.013)	(0.015)	(0.007)
Age >80 yrs	-0.012*	-0.010	0.002	0.174***	-0.045***
	(0.006)	(0.022)	(0.014)	(0.041)	(0.016)
Gains £50-100	-0.008**	-0.055***	0.052***	0.042**	-0.032***
	(0.004)	(0.013)	(0.019)	(0.018)	(0.007)
Gains £100-500	-0.002	-0.064***	-0.007	0.065***	-0.036***
	(0.004)	(0.011)	(0.009)	(0.014)	(0.005)
Gains >£500	0.001	-0.047**	0.030	0.170***	-0.032***
	(0.010)	(0.022)	(0.031)	(0.020)	(0.008)
Constant	0.041***	0.118***	0.012	0.214***	0.093***
	(0.005)	(0.012)	(0.011)	(0.011)	(0.005)
Observations	63,321	13,293	4,108	21,180	30,204
R-squared	0.004	0.012	0.04	0.062	0.014

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1