

NBER WORKING PAPER SERIES

TESTING THE EFFECTIVENESS OF CONSUMER FINANCIAL
DISCLOSURE: EXPERIMENTAL EVIDENCE FROM SAVINGS ACCOUNTS

Paul D. Adams
Stefan Hunt
Christopher Palmer
Redis Zaliauskas

Working Paper 25718
<http://www.nber.org/papers/w25718>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2019

For helpful comments, we thank our discussants Xiao Liu and Daniel Martin; participants at Advances with Field Experiments 2018 Conference, RAND Behavioral Finance Forum, CFPB Research Conference, Boulder Summer Conference on Consumer Financial Decision Making, NBER Summer Institute (Law & Economics); and Joseph Briggs, Michael Grubb, Arvind Krishnamurthy, Sheisha Kulkarni, David Laibson, Peter Lukacs, Brigitte Madrian, Adair Morse, Philipp Schnabl, and Jonathan Zinman. Matthew Ward and Tim Burrell provided invaluable help with execution. We thank Margarita Alvarez-Echandi, Sam Hughes, Tammy Lee, and Lei Ma for their research assistance. This research was conducted in conjunction with the Financial Conduct Authority, where Adams, Hunt, and Zaliauskas were employed during the trials. The views expressed in this paper are those of the authors and not the Financial Conduct Authority, which has reviewed the paper for human subjects compliance and the release of confidential information. All errors and omissions are the authors' own. This RCT was registered in the American Economic Association Registry for randomized control trials under trial number AEARCTR-0004053. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at <http://www.nber.org/papers/w25718.ack>

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2019 by Paul D. Adams, Stefan Hunt, Christopher Palmer, and Redis Zaliauskas. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

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

Paul D. Adams, Stefan Hunt, Christopher Palmer, and Redis Zaliauskas

NBER Working Paper No. 25718

March 2019, Revised May 2020

JEL No. D14,D83,E21,G28,M38

ABSTRACT

While popular with policymakers, most evidence on consumer financial disclosure's effectiveness studies borrowing decisions (where optimality is unclear) or lab experiments (where attention is not scarce). We provide field evidence from randomized-controlled trials with 124,000 savings-account holders at five UK depositories. Treated consumers were disclosed varying degrees of salient information about alternative products, including one with their current provider strictly dominating their current product. Despite switching taking roughly 15 minutes and the moderate average potential gains (\$190/year), switching is rare across disclosure designs and depositories. We find pessimistic beliefs drive disclosure inattention and limit disclosure's effectiveness, helping explain deposit stickiness.

Paul D. Adams
Autoriteit Financiële Markten
Netherlands
paulduncanadams@gmail.com

Stefan Hunt
Competition and Markets Authority
UK
stefanhunt@gmail.com

Christopher Palmer
MIT Sloan School of Management
100 Main Street, E62-639
Cambridge, MA 02142
and NBER
cjpalm@mit.edu

Redis Zaliauskas
Lloyds Banking Group
48 Chiswell Street
London
United Kingdom
redis.zaliauskas@gmail.com

1 Introduction

In practice, consumer choice is sticky. Across an array of decisions including insurance, utilities contracts, pension plans, mortgage choice, and others cited below, individuals act as if they are reluctant to reoptimize. The stickiness of bank deposits in particular has featured prominently in empirical banking models (e.g., Drechsler et al., 2018). 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, owing in part to their simplicity and relatively low cost.¹ Yet despite this policy reliance on disclosure, little is known about how the delivery of disclosures actually affects financial behavior because of a variety of practical and methodological challenges. Generally, attempts to assess disclosure effectiveness suffer from a joint-hypothesis problem, where the researcher must both model optimal choice and measure deviations therefrom. For example, estimates of whether mortgage or credit-card disclosures are effective are confounded by the problem of first needing to determine what the “right” debt choice is for a given consumer in order to be able to learn whether the disclosure has moved decisions closer to that optimum.²

In this paper, we are the first to use a large-scale field experiment with consumers in an advanced economy to evaluate the effectiveness of various disclosure designs aimed at supporting consumer choice across savings products. We partnered with five UK retail financial institutions to conduct a series of randomized-controlled trials (RCTs) testing the extent to which disclosures about comparable-product interest rates would be useful to consumers. By experimentally varying disclosures consumers received about their savings account, competing products, and the switching process, we provide evidence on the origins of both deposit stickiness and inattention to mandated disclosures. Over 124,000 savings account holders were randomly assigned into treatment and control groups, and we collect rich administrative data on account balances, demographics, and switching behavior from each of the five financial institutions. Consumers in the treatment groups received various forms of information about their account. Comparisons of outcomes for treatment and control groups within each trial provide internally valid estimates of the absolute importance of various frictions in the optimization process. Comparing effects across trials, each operating in a different context with a different customer mix, allows us to benchmark the external validity of our experimental results and study the importance of disclosure context. Finally, we conduct follow-up surveys with a subsample to supplement our analysis of behavioral responses with

¹See Loewenstein et al. (2014) for an overview of the economic motivation for disclosure.

²For an example of lower debt not necessarily being optimal, Medina (2018) finds that interventions that induce debt repayment can cause financial distress elsewhere on household balance sheets.

direct explanations for consumer behavior.

Importantly, this straightforward disclosure setting with a relatively simple financial product is free of many of the confounds present studying other markets. Compared to the borrowing context, savings-account decisions are a financial setting where normative statements about optimal choices are relatively straightforward to make. A further virtue of our design is that we analyze the behavior of consumers 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.³ Given this option, at a minimum, switching to a higher-paying equivalent product at the same institution theoretically dominates doing nothing apart from the time cost (15 minutes on average) and cognitive cost (reduced in some trials) spent to switch products. However, despite modest gains from switching (an average increase in first-year interest income of \$190), only 8.9% of consumers across all of our trials at baseline take any action to move their savings account balances.⁴ While we explore several possible rationalizations of this inertia, we emphasize that many explanations for infrequent account switching are also impediments to the effectiveness of consumer disclosure to incumbent account holders.⁵

To test among explanations for low baseline switching levels, our trials are intended to address various frictions consumers face when deciding whether to attend to a disclosure. Our experimental variation allows us to examine the importance of (i) increasing the return on attention by simplifying and improving the precision of comparison across products, (ii) lowering switching costs through process improvements, and (iii) increasing attention to the switching decision itself to promote active choice. If the non-monetary cost of information acquisition has led to rational inattention, then providing information through mandated disclosure has the potential to improve consumer outcomes.⁶ However, if easily acquired salient information on opportunities to increase interest income does not materially affect consumer behavior, this points towards behavioral frictions limiting the usefulness of disclosure. For example, if the source of inertia is the psychological cost of focusing on financial decisions, then mandated disclosure may even overwhelm consumers with an onslaught of

³As we detail below, this internal switching option allows consumer to keep the same ATM and branch network, account linkages, etc.

⁴Because introductory interest rates could decrease after one year, our gains from switching calculations focus on the first year of additional interest income as a lower bound.

⁵For example, consider the possibility that consumers do not switch their accounts because they skeptically view the provided information to be “too good to be true.” Such an explanation for low switching reinforces our thesis; while disclosure could be designed to partially mitigate these effects, the potential existence of such beliefs highlights challenges to consumer disclosure’s effectiveness.

⁶In a rational inattention framework, consumers may be inert when they believe that the net benefits of action to be small or that a signal of the magnitude of those net benefits will be imprecise or costly to obtain (for an overview, see Gabaix, 2019).

notices and fine print. Similarly, if cognitive processing costs are structured in a way that consumers choose to either pay attention to all disclosures or no disclosures, then even salient mandated disclosures may not be enough for consumers to pay the ongoing cost of vigilance.

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 (Financial Conduct Authority, 2015), equivalent to 37% of UK GDP.⁷ With an average account balance of £4,900, instant-access savings accounts, in particular, are among the simplest financial products—they are usually not used for routine transactions and their primary feature is the interest rate payable on the balance (although we discuss other relevant dimensions of product differentiation below). Robust deposit insurance also means that counterparty risk is a non-factor for the majority of consumers—UK depositors’ savings accounts are currently insured against bank failure up to £85,000. Yet even though most consumers report being most concerned with obtaining the highest possible interest rate (Financial Conduct Authority, 2015), 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 marketed 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, missing out on an average of around 120 bp higher rates in our data.

Estimates of how disclosure actually works in practice are most externally valid when tested in the field, where, among other important considerations, disclosure has many competitors for consumer attention.⁸ While the typical disclosure may apprise consumers of a particularly unlikely state of the world, we test one with quantifiable and certain impacts on interest income. Although an observational comparison of baseline switching rates across providers can be informative, an RCT further controls consumer selection by precisely manipulating disclosure across treatment and control in a way that permits robust comparison across design alternatives. Because treatment groups are on average identical to control groups by virtue of random assignment, we can causally attribute any differential account switching to the amount and form of information they receive to understand the relative importance of each component of inattention.

Previewing our results, even among consumers that were provided with reliable informa-

⁷According to SCF data, 51% of U.S. households had a savings account in 2016 with aggregate holdings around \$2 trillion.

⁸See Harrison and List (2004) for a discussion of the virtues of field versus laboratory experiments.

tion about more attractive interest rates (including at the same provider), there is still a low level of switching. While we find interesting heterogeneity in effectiveness across designs, the average disclosure increased switching behavior by 0.7 percentage points, from 8.7% in the control group to 9.5% in the treatment group. Survey results complement our precisely estimated, small effects of disclosure with evidence on consumer inattention, beliefs, and preferences over non-price features. In particular, consumers report overly pessimistic beliefs about whether they could earn significantly higher interest income by switching and how time consuming the switching process would be. Given these beliefs, many consumers report ignoring the disclosure, failing to update their priors on the net benefits of reoptimizing their savings account and highlighting how difficult it is for traditional informational disclosures to help consumers in real-world financial settings.

What are the strongest drivers of consumer inertia and sticky deposits? Our results are most consistent with models with a fixed-cost of attention rather than a search cost incurred for each price quote attained. Process improvements (Trial 3) led to modest increases in switching, as did well-timed reminders (Trial 4) even though the monetary gains to switching are nearly identical just before and after a rate decrease. A just-sign-here return switching form increased switching to 12% from a baseline of 3%, and timely emails and text messages increased switching by 4-5 percentage points. Particularly prominent disclosures—e.g., readable front-page information on better available products (Trial 1)—had marginal positive effects, raising switching from 3% to 6%, while disclosures not on the front page of a mailing had no effect (Trial 2).

Comparing control-group switching rates across trials, a few notable patterns emerge. In general, switching rates were higher for trials with younger consumers, although the effect of age on switching was not consistent across trials. Whether having a checking account with the same provider or having a large savings account balance increased or decreased switching also varied across trials. Switching in the control group was highest for Trial 4 that involved the impending expiration of a one-year introductory rate. Average account balances (and thus the gain from switching) were also largest for Trial 4, suggesting that the particular savings account studied in this trial attracted an attentive and responsive set of consumers compared with the other trials, where the average account had been open for 5-16 years.

However, despite significant heterogeneity in depositor characteristics across the trials, we find only weak evidence for individual-level heterogeneity in these effects. For example, the sensitivity of disclosure responsiveness with respect to the level of interest income is limited. One obvious explanation is that when a low amount of money is at stake (account balance \times potential interest-rate gain), even a modest opportunity cost of time can justify not ignoring disclosures, and those with the highest balances may face the highest opportunity cost of

time. We find at best mixed evidence for this explanation; switching is not consistently higher among retired depositors and even among the highest balances in our sample, the effect of disclosure improvements on switching remains low. Overall, our results on cross-sectional heterogeneity suggest that the low effectiveness of disclosure that we document is not specific to a particular type of depositor.

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 Related Literature

In this section, we briefly discuss the contribution of our findings to related work on disclosure effectiveness, sticky deposits, and consumer decision making.

A significant literature spanning industrial organization, behavioral economics, household finance, accounting, law, and marketing critically examines consumer disclosure regulations in many consumer markets.⁹ These papers provide mixed evidence for disclosure effectiveness, highlighting cases of both disclosure successes and failures. If lacking information is a significant reason for sticky (and perhaps suboptimal) consumer choice, disclosures have potential to affect decisions. However, 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 claims about the relative importance of disclosure content and design features in generating benefits for consumers.

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. See also Kulkarni et al. (2018), who use quasi-experimental variation to contrast the effects of contract stan-

⁹A complete treatment of these literatures is outside the scope of this paper; see Dranove and Jin (2010), Ben-Shahar and Schneider (2014), Loewenstein et al. (2014), Leuz and Wysocki (2016), and Roychowdhury et al. (2019) for recent surveys of disclosure-related research.

standardization and discourse in the Chilean personal loan market. Our study adds to these papers 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 obviously 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 risk free 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 may decrease welfare by leading to an increase in otherwise avoidable overdraft fees.¹⁰ Our savings setting helps here, as well, given that the externality on financial distress of reclassifying savings to a higher-paying savings account is plausibly zero.

A long banking literature documents the stickiness of both deposit pricing and deposit balances, and our experimental and survey results speak to many of the mechanisms at play. Flannery and James (1984) and Hutchison and Pennacchi (1996) find deposit flows to be under-sensitive to interest rates, and our results on inattention provide a key reason why in addition to the liquidity function of deposits (Nagel, 2016). Kahn et al. (1999) examine bank's price-posting strategies, modeling banks' slow adjustments to deposit pricing as optimal given the limited recall of retail depositors. We provide direct evidence for limited recall in our survey section, showing a general unawareness of own and market interest rates and that even when provided benevolently designed, salient information about interest rates, consumers struggle to recall receiving information about their account. 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. Our results complement this literature on consumer inertia and are most consistent with consumer optimization models wherein consumers must first pay a fixed cost to open the reoptimization decision before searching across alternative choices. Recent influential work by Drechler et al. (2017, 2018) studies the implications of sticky deposits for the pass-through of monetary policy and banks' exposure to interest rate risk. We provide microeconomic evidence on why deposits are sticky, tracing deposit stickiness to inattention arising from consumer beliefs and preferences over non-price features, even among consumers with substantial foregone interest income. Finally, Allen et al. (2019) find strong brand loyalty effects in mortgage lending in Canada. We confirm this finding in a different market and product, show the implications of such attachment for

¹⁰See also recent work by Hall and Madsen (2020) on the negative consequences of traffic safety disclosures.

disclosure effectiveness, and highlight the role of beliefs in inhibiting switching.

The potential importance of disclosure design also poses a risk to successful policy implementation by increasing the scope for non-robust regulations to be undone. Grubb (2015) and Persson (2018) find endogenous complexification responses by firms to be an additional obstacle to first-best decision making. While disclosure may unshroud 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 disclosure. C el erier and Vall e (2017) find evidence of issuers endogenously increasing the opacity of structured securities marketed to retail investors.¹¹ Johnson et al. (2018) show how consumer suspicions about the veracity of mortgage advertising can inhibit take-up of otherwise attractive offers. Other studies test for the importance of various advertising design features in stimulating loan demand (Bertrand et al., 2010; Ferman, 2015; Kulkarni et al., 2018). Acknowledging the difficulty of asserting optimal consumer behavior in their setting, Bertrand et al. (2010) focus on evaluating advertising persuasiveness and consumer demand instead of whether advertising design features matter for consumer benefits.¹² Even in our setting, where products are largely one-dimensional, confusion is likely to be low, and discloser credibility is high, we find significant scope for firms to (intentionally or not) obfuscate mandated disclosures through disclosure design, e.g., by burying information on the back page of annual statements. Such potential responses by firms have motivated disclosure policy to often specify detailed design elements such as font, font size, and placement.¹³ Prominent examples of formatting specifications in US lending policy include the Truth In Lending Act, the CARD Act, and the prescribed HUD settlement form for residential mortgages.

This paper also contributes to a broad literature documenting consumer financial mistakes and the potential role of disclosures or reminders in preventing them. Much of the literature on consumer financial mistakes focuses on credit cards, e.g., Ausubel (1991), Agarwal et al. (2015), Keys and Wang (2018), Jorring (2018), and Gathergood et al. (2019). However, 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 mortgage refinancing rule of Agarwal et al. (2013) requires several simplifying assumptions

¹¹Jin et al. (2018) find in a lab setting that disclosing parties choose complex disclosure designs more than half the time when forced to reveal harmful private information. Recent work in accounting finds similar responses: firms adjust information presentation when investors have limited attention (Hirshleifer and Teoh, 2003) and external attention from institutional investors drives the frequency but not quality of voluntary disclosures by target firms (Abramova et al., 2018).

¹²See also the public finance literature on tax salience (e.g., Finkelstein, 2009; Chetty et al., 2009), which demonstrates the important role of price information delivery design in affecting consumer demand.

¹³In a classic illustration of 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 in small print, using unclear language, in a remote area of their website (Leach, 2012).

about preferences, transaction costs, unobserved heterogeneity, and probability distributions to approximate an optimal option exercise rule. Borrowing high-cost debt could be optimal at low levels of consumption (with high marginal utility) and acute liquidity needs (Medina, 2018). In many settings, optimal choice is not obvious. Optimality is more straightforward in a savings setting where reoptimization transaction costs are close to zero (Stango and Zinman, 2009; Karlan et al., 2016). Even in our simplified setting, we cannot identify mistakes per se without knowing switching costs and beliefs, although the survey evidence in section 6 suggests the former are negligible when there is an option to switch to another savings product with the same provider. When consumers do not believe attention to be worthwhile, disclosure will fail to be impactful.

More broadly, many economic decisions exhibit inertia in the sense that after initial product choices, demand across alternatives becomes inelastic.¹⁴ A rich literature on rational inattention in macroeconomics (e.g., Sims, 2003) offers an explanation for inertial behavior, relevant to evidence here of consumers not making an active choice of savings product.¹⁵ However, while it may be that in many settings the foregone utility from not reoptimizing is uncertain and relatively small (Sallee, 2014), there are several impediments to effectual disclosures even when reoptimizing has the potential to increase the utility of a given choice. For choices consisting of repeated interactions, switching costs including convenience costs and the loss of value of complementary choices inhibit changing providers ex-post (Farrell and Klemperer, 2007), motivating our inclusion of an option to retain one’s current provider. Financial literacy (Calvet et al., 2009; Lusardi and Mitchell, 2014) is likely a necessary condition for effective financial disclosure, although our study focuses on savers in an advanced economy, and we do not find evidence that account holders with larger balances are more likely to respond to disclosures. Work in behavioral economics on commitment-problems provides evidence for the prevalence of procrastination and the difficulty of completing intended tasks, and we test treatments that provide salient follow-up digital reminders.¹⁶

¹⁴See Samuelson and Zeckhauser (1988), Abaluck and Gruber (2011), Choi et al. (2011), Ericson (2014), and many others for inertia in health insurance and retirement plan decisions. Andersen et al. (2017) and Keys et al. (2016) provide recent evidence on mortgage refinancing reticence, and 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. Calem and Mester (1995) and 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.

¹⁵See DellaVigna (2009) and Gabaix (2019) for surveys of the limited attention literature in behavioral economics.

¹⁶An established literature documents the importance of default choices in retirement plans (e.g., Choi et al., 2011) because consumers in general seem reluctant to make active choices. See DellaVigna (2009) for a survey of the self-control literature. Consumers may also choose to ignore even costless information about alternatives to avoid cognitive costs such as the disutility of feeling overwhelmed by the complexity of

3 Experimental Design

To test for the role of context and whether some disclosure designs are particularly effective for certain depositor types, we partnered with five UK depositories to 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 Appendix Figures 1-8 for redacted example disclosures. 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. Notably, the trials varied in terms of the situation in which the customer received the disclosures (for example, whether at the point of an interest rate decrease) and the specific disclosure designs tested within each trial. Customers in both the control and treatment groups were experiencing an already-scheduled rate decrease unrelated to our experiment in three trials (Trials 2, 4 and 5) but were already on a relatively low rate in two trials (Trials 1 and 3) with no scheduled rate change. Trials 1, 2 and 3 provided customers with forward looking information about market interest rates currently available to them.

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 date for everyone. In the digital reminder trial (4), the rate decrease occurred one year after 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 were already 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. In the remainder of this section, we provide additional details on the design of 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

pricing (Sallee, 2014; Grubb, 2015; Haushofer, 2015) or the disutility of the bad news that a current product generates under-market returns (Karlsson et al., 2009; Olafsson and Pagel, 2017; Andries and Haddad, 2017).

page. For treatment groups, different information was added to the front page of the annual statement depending on the treatment arm. This included a simple encouragement to shop around for another account (Appendix Figure A1); a comparison of the currently applicable rate with the highest rate available on a comparable account with the current provider (best internal rate); the best internal rate with the same provider and 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 (Appendix Figure A2). 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 £250 the illustration was for £100). We excluded customers with balances lower than £100.

In Trial 2, customers at the time of disclosure had rates close to market rates and were being notified of an impending rate decrease.¹⁷ All letters were sent more than two months ahead of the rate decrease. Customers were randomly assigned to one of five equally sized groups: a control group and four treatment 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 (Appendix Figure A3). The control letter included no additional information about internal or external rates. The treatment groups received additional information on the back page of the letter, formatted into a call-out box (referred to as the “switching box”) with a graphical comparison of interest rates. Each treatment group received information about the best available interest rate with the current provider. The four treatment groups differed by whether the disclosure was personalized and whether it included information on the best competitor rates (the average of the three highest rates on comparable accounts), for a total of four possible combinations. The two non-personalized treatments (one displaying external rates and one not) had an illustration of monetary gains from switching based on an assumed balance of £5,000, and the personalized disclosures used each individual’s balance at the time of mailing to illustrate the gains (see Appendix Figure A4).

Trial 3 featured a disclosure bundled with a switching-process improvement designed to lower both the expected time cost of switching and its uncertainty. In addition to providing information as in Trials 1 and 2, 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 at the same provider. In August 2015, the provider sent a one-off mailing 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

¹⁷Customers who had opted out of marketing communications were excluded from this trial.

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 and a prepaid, addressed envelope (Appendix Figure A5).

In Trials 4 and 5, we test the effect of timely repetition of informational disclosures 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 2015.¹⁸ 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 in addition to receiving the same mailing as the control group. The email reminder was similar in its content to the letter sent to all groups (Appendix Figure A6). 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 (Appendix Figure A7). 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. 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.

In Trial 5, we test the effect of an SMS reminder around the time of a rate decrease in early summer 2015.¹⁹ 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, one week before, one week after, or on the day of the rate decrease (Appendix Figure A8). 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. 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.

¹⁸The trial sample consisted only of customers who all had an email address and a mobile phone number on record. Over 90% of customers in the sample had both email and phone number on record. The partnering institution reported that around 2% of email reminders and around 10% of SMS reminders could not be delivered due to invalid records.

¹⁹All customers in the trial had a mobile phone number on record. The partnering institution evaluated that around 8% of reminders were not delivered to customers in the treatment groups due to invalid phone number records.

4 Data and Balance Tests Across Treatment and Control

We collected detailed administrative data from each bank that includes account balances, account closures, and new account openings before, during and after the trial intervention. The dataset also provides us with age, gender, account age, mobile and online banking behavior, whether each consumer held a checking account at the same bank, and the number of other financial product holdings. Variation in depositor types within a trial allows us to test whether a given disclosure design is particularly effective for certain groups. Heterogeneity in customer mix and context across trials (e.g., whether consumers are facing an impending rate decrease) allows us to use the control-group to measure the importance of the decision setting in overall attentiveness and willingness to switch.

We report descriptive statistics for each trial in Table 2. The average consumer in our data is 54 years old, and the average savings account has been open for nine years. About half of our data’s savings account holders also have a checking account with the same depository, and the average consumer has roughly three other financial products at the same provider. Across all trials, the average savings account balance is £15,740 (\$24,396 in 2015). Within each trial, we oversampled consumers with large balances in all trials relative to the market to ensure sufficient power to detect any differential switching behavior among consumers with ample financial motivation to do so. However, we included customers with lower balances in order to contrast the applicability of our findings across customers with varying degrees of forgone interest income. We calculate the potential gain from switching as the additional interest income from moving each consumer’s entire balance from their current interest rate to the best available external rate. The average potential gain from switching is £123, meaning that the average depositor could earn an additional \$190 in interest income in the first year after switching their account.

The sample size of each trial varies depending on the number of treatments tested and the available customer base.²⁰ Average age, average savings balance, proportion of customers who have their current account with the same bank, and average account age all vary across trials, in line with our understanding of the different customer mixes across the five providers as summarized above. For example, only 9% of consumers in Trial 1 use online banking, whereas 90% of the consumers in Trial 5 use online banking, consistent with the depository in Trial 1 having older consumers and older accounts than Trial 5. Average account ages (the number of years the average account has been open) are smallest and average balances are largest in Trials 4 and 5, consistent with these providers attracting younger customers

²⁰The length of the observation period also varies due to practical data constraints, however our main results consider effects within four weeks and our results are robust to considering different observation horizons.

willing to move their money in search of better rates.

As shown in Table 3, the means of key demographic statistics are well balanced across treatment and control groups, as expected given random 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 given the large sample sizes, although the differences in means are not practically significant. For Trials 1-4, p -values for the test that all variable means are equal suggest that the key customer demographic variables are equally distributed across treatment and control groups within each trial. While the joint test rejects the equality of means across treatment and control for Trial 5, the magnitude of the significant differences are economically small (six-month and one-month differences in depositor age and account age, respectively).

The primary outcome we are interested in is switching. We define switching as when customers convert, close, or withdraw at least 95% of their savings account. We define *internal switching* as when customers convert their account to another instant-access savings product or fund a new instant-access savings account with the same firm and empty their old account. We define *external switching* as all remaining switching that does not fall into internal switching, including cases where, for example, the depositor converted their savings account to an investment account with the same firm. Internal and external switching are mutually exclusive and always sum up to *any switching*.²¹

External switching contains many alternatives including transferring the balance to an account outside the firm or a different type account with the incumbent firm (such as a certificate of deposit) or withdrawing the money and spending or investing it elsewhere. However, when an option for internal switching to a higher-paying rate exists, at a minimum, internal switching dominates not switching (subject to switching costs). Of course, an even better option may exist for the household than internal switching, motivating our focus on any switching. Given this, but for the disutility of the switching process (including processing and deliberation about the disclosure), any switching should be optimal for consumers that have an option to internally switch.

To understand consumer choices of accounts, it is useful to consider the relative attractiveness of internal and external 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 is no higher than 50 bp per year. Within each provider, the highest available internal rate ranges from 20 to 90 bp higher than the the applicable

²¹Where our measures overlap, we record the latest action as the final action. For example, if a customer first converts an account internally and then closes the newly converted account, we classify that as external switching.

rate on each consumer’s current account. The best competitor interest rates are comparable for all trials and range between 108-135 bp, for an incremental gain from external switching compared to internal switching ranging from 10 to 60 basis points.

However, the incremental cost of external switching may be substantially higher than the cost of internal switching. Switching to another provider could involve (1) search and evaluation of alternative brands and product features, (2) the operational cost of time spent opening the new account and transferring funds, and (3) an ongoing convenience cost of monitoring the account and/or having different level of service, such as the presence of a network of branches. In contrast, internal switching would involve a simplified version of (2) and almost none of (1) and (3) such that fully attentive and informed households may well prefer internal to external switching despite more attractive interest rates available to external switchers.

Our survey results (section 6) contain evidence that consumers report a greater willingness to reoptimize their savings product choice given the opportunity to increase interest income with the same provider. Internal switchers report spending less than 15 minutes on the switching process, whereas most external switchers report spending less than 30 minutes on the switching process and up to two hours on shopping for alternative products. In the next section, we will contrast effects on any switching and internal switching to provide revealed preference evidence on the relative attractiveness of internal switching, mindful that consumers may prefer internal switching despite lower available income both because of non-price product features and differential switching costs.

5 Empirical Strategy and Results

Given random assignment, trial-specific treatment effects are internally valid estimates of the causal effect of disclosure on depositor switching. However, comparing disclosure effects across trials raises selection concerns. For example, might a given disclosure have been particularly ineffective not because of its design but because of the mix of customers at that bank? After presenting estimates for each trial and discussing factors potentially explaining each result, we will compare effects across trials to motivate additional empirical specifications testing candidate explanations for differences in disclosure effectiveness across trials.

We begin by reporting treatment effects from a linear-probability model of a given switching outcome for individual i on a treatment indicator $Treatment_i$ that pools all treatment arms within a trial.

$$Switching_i = \beta \cdot Treatment_i + X_i' \gamma + \varepsilon_i \tag{1}$$

The omitted category is the control group such that the estimated coefficient $\hat{\beta}$ reports the treatment effect of the disclosure in percentage points. 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), potential gains squared, and an indicator for whether consumer i also holds a checking account at the same financial institution. Given the random assignment of treatment (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 treatment effects in Table 4 for any switching (top panel) and internal switching (bottom panel).²²

Column 1 of Table 4 reports the average effect of the disclosures tested in Trial 1, which featured an interest-rate disclosure on the front page of the annual statement. Baseline switching for this trial was small; 2.6% of consumers in the control group switched their accounts in the month following the disclosure. Panel I shows that the treatment increased any switching (internal or external) by 1.9 percentage points, and panel II shows that 1.7 percentage points of this was accounted for by internal switching. This pattern holds across specifications and trials—despite larger gains available at other banks, most switching we document is internal switching, consistent with consumers valuing non-monetary product attributes of savings accounts or a more behavioral explanation such as choice overload, as we discuss below when contrasting the effects of disclosure designs.

Trial 2, which placed disclosures on the reverse side of a regulation-mandated letter informing consumers about an upcoming rate decrease, was generally ineffective. Even though the marginal monetary returns to switching were similar for depositors at both institutions, baseline switching in Trial 2 was five percentage points higher than in Trial 1. While higher control-group switching could be due to the sense of immediacy provided by the looming rate decrease (or other compositional differences across institutions), it is still low in absolute terms at 7.7%, and the disclosure treatment in Trial 2 was even less effective than in Trial 1. Column 2 of Table 4 shows that we cannot reject no treatment effect and can reject disclosure effects on any switching of more than one percentage point and on internal switching of 0.3 percentage points.

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-

²²The results presented here measure switching behavior as of four weeks after disclosures were received. Our results over longer horizons are quite similar.

filled form and a prepaid envelope. Signing and mailing this form would automatically switch an individual’s account classification to an otherwise identical savings product with a higher interest rate. The pre-filled detachable form the treatment group received increases any switching by 9 percentage points (column 3 of panel I) and internal switching by 8 percentage points (column 3 of panel II).

We interpret the relative success of Trial 3 as indicative that the disutility of the switching process itself is a key component of customer inertia and that simplifying the path to respond to a disclosure has potential to be effectual. Comparing Trials 1 and 3, which were similar in most respects, provides further evidence for the importance of the switching process improvement itself. Both Trials 1 and 3 had similar customer mixes and contexts (neither involved an impending rate change), and very similar control-group switching rates (2.6% and 3%, respectively). However, the treatment effect for Trial 3 was over four times larger than the treatment effect for Trial 1 that featured only information and did not include a pre-filled form making the switching process trivial. This interaction between disclosure and the ease of responding to the disclosure also highlights the potential for firms to undo any effect of consumer-facing disclosure by endogenously making taking a responsive action more cumbersome.

Trials 4 and 5 were digital in nature with treatment group account holders receiving informational disclosures by email or text message. In both trials, all customers received a legally mandated letter informing them of an impending decrease to their savings account rate sixty days before the decrease. Treatment group consumers then received additional reminders by email or text message. In Trial 4, the control-group mean switching is highest (40%), suggesting that these deposits are the least sticky of all the trials customer bases, perhaps due to selection into a high introductory rate and heightened attention around its expiry. However, these trials also had relatively high balances, young account ages, and high online banking shares. We explore the role of such heterogeneity across consumers within a trial in section 5.2 below.

The SMS disclosure in Trial 5 has a 1.6 percentage point effect on any switching (column 5 of Table 4). There was no internal switching option for consumers in Trial 5 because the provider only had one type of savings account paying the same rate for everyone in each account-opening cohort. Accordingly, we cannot assume that external switching (the only switching outcome for Trial 5) would dominate doing nothing even if there were no switching costs. It is conceivable that non-price considerations, such as switching account numbers, branch and ATM networks, etc. would cause a customer to incur sufficient disutility as to make not switching in response to a rate decrease to be optimal.

Comparing treatment effects and control-group means across trials supports several over-

all conclusions and highlights the benefits of studying multiple trials together. First, there are significant differences in baseline switching across the trials’ control groups, from 3% to 40%. The lack of substantial heterogeneity across depositor types within trials documented in section 5.2 points to a limited role for observable depositor heterogeneity to explain these differences across trials. However, we note that the trials with the highest fraction of control-group switching are also the trials where depositors faced an impending rate decrease (Trials 2, 4, and 5). Independent of any special disclosure, depositors facing a rate decrease seem to be much more likely to switch their savings account than those earning below-market but static rates. While this finding could be also attributable to selection into provider and product, in section 5.3, we test within trial whether the timing of disclosures relative to rate decreases matters to document the importance of context in disclosure attentiveness.

Second, even though only a small fraction of control-group switching is internal switching, most of the switching we observe in response to treatment-group disclosures is internal switching. This highlights that disclosure may be of limited value to consumers when not coupled with a straightforward alternative course of action. Finally, although few consumers responded to even the most successful disclosure, disclosure effectiveness varies meaningfully across trials. In the additional specifications below, we probe further whether this difference can be attributed to disclosure design, customer mix, or disclosure context.

5.1 Treatment Effect Heterogeneity by Disclosure Design

To study the various disclosure designs tested in each trial, we reestimate (1) using a full set of treatment dummies $Treatment_{ik}$ for each treatment k that was included in the trial $\mathcal{T}(i)$ in which i was enrolled.

$$Switching_i = \sum_{k \in \mathcal{T}(i)} \beta_k Treatment_{ik} + X_i' \gamma + \varepsilon_i \quad (2)$$

Here, the effect β_k of design k represents the percentage-point increase in switching for a given treatment group k relative to the control group. We estimate (2) for the three trials that had disclosure design variants (1, 2, 4) in Table 5, and alternating columns show effects on any switching and internal switching to check whether some designs were differentially effective at inducing internal switching as opposed to any switching.

For the first trial, there were four treatment arms, each a variant on the design of an informational disclosure situated on the front page of an annual statement. While we reject the null hypothesis that disclosure design doesn’t matter by testing whether the treatment effects are equal, even the best treatment effect still had only marginal absolute effects

on switching. The Call to Action treatment that did not provide any information about competitor rates, and increased any switching by 0.9 percentage points (column 1) and internal switching by 0.5 percentage points (column 2). While the option to switch internally should strictly dominate not switching (up to the cost of switching), it may be that an outside option dominates internal switching such that any switching is the optimal choice. Although 0.9 percentage points is high in relative terms given the low baseline level of switching for the population of consumers in the first trial (2.6%), it is low in absolute terms given the optimality of the choice and our conditioning on potential gains. In further results in section 5.2, we explore whether these small effects are driven by low average monetary gains from switching.

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 (e.g., Schwartz, 2004). Showing a graph to illustrate a consumer'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 quite similar when we restrict our attention on internal switching (column 2). 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 a similar set of treatment arms varying disclosure design as Trial 1 but were mailed to consumers 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 comparable information as the disclosures of Trial 1, with the added urgency of an impending rate decrease and with two treatment arms personalizing the amount of annual gains from switching, we cannot reject that the Trial 2 disclosures had zero effect on either type of switching, irrespective of design. Survey results discussed in section 6 suggest that part of this is low attention to the letter at all, handicapping well-designed disclosure from being effective.

For Trial 4, we find that the email reminder was slightly more successful than the text-message reminder at inducing any and internal switching (5.3 versus 4.2 percentage points, respectively, in column 5). 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 consumer 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.

5.2 Disclosure Effect Heterogeneity by Depositor Attributes

We explore several dimensions of treatment effect heterogeneity to test whether there are groups for which disclosure is particularly effective and whether observable differences in customer mixes can explain differences in disclosure effectiveness across trials. 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 plausibly 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 binary characteristics W . We estimate

$$Switching_i = \beta \cdot Treatment_i + Treatment_i \cdot W_i' \psi + W_i' \alpha + \varepsilon_i \quad (3)$$

where α is the coefficient on a set of indicator variable for each consumer characteristic including four age categories, four monetary gains categories, and an indicator for whether the depositor also had a checking account at the same institution. The coefficient vector ψ captures the degree to which the main treatment effect β is different for customers falling into each category described by W . 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.

We report the results of estimating equation (3) by trial in Table 6. Across all five trials, the control variables have little explanatory power on their own. The youngest and oldest consumers appear the most likely to switch, along with consumers with the highest gains, though the effect of both of these controls is small and of inconsistent sign across the trials. Intuitively, consumers less than 40 years of age (the omitted category) are more responsive to text message-disclosures (column 5) than any other age groups.

Looking at treatment effect heterogeneity across the trials, the preponderance of the evidence shows that disclosure is equally (in)effective for all age and balance levels. Interestingly, consumers who could be expected to have strong loyalty to their current depository because they have a checking account with the same provider are generally more likely to engage in some sort of switching, but are not necessarily more responsive to the treatment. Might low levels of disclosure responsiveness simply reflect a high opportunity cost of time relative to low returns on switching? In only one of the five trials (column 1) can we reject that there is heterogeneity in treatment effects by depositor characteristics. 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.

5.3 Treatment Effect Heterogeneity by Disclosure Timing

A unique feature of Trials 4 and 5 is that the timing of disclosures varied with respect to the date of an impending rate decrease. In Trial 4, 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-length introductory rate period. In Trial 5, treatment-group consumers received SMS reminders of the disclosure on the day of the rate decrease or one week before or after. We explore which groups had the strongest reaction to the disclosure and what timing was the most effective in Table 7. Treatment effects were strongest for emails received 0-2 weeks before the rate decrease in the digital reminder trial, and text-message effects were smallest for reminders received on the day of the decrease in the SMS-only trial.

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. 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 if consideration thereof occurs before (rather than after) a salient event. That said, even optimally timed reminders have economically small impacts on switching.

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 consumers facing a nearly costless task of switching to a nearly identical but strictly dominating internal account with pre-filled out paperwork worth

at least £100 in year one, the most optimally designed (or timed) disclosure only increases switching by nine percentage points relative to an already low baseline.

6 Survey Evidence

In this section we discuss the key findings of our follow-up surveys. We measure three categories of outcomes: 1) recall of information about the disclosure, 2) responsiveness to the disclosure, and 3) ex-post satisfaction with the individual decision taken. We conducted the surveys with 261 consumers from the Trial 1 sample (the front-page switching box trial) and with 500 consumers from the Trial 2 sample (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 accounts. However, given that participation in the follow-up survey was voluntary, respondents may be more likely to be drawn from the sub-set of customers who are relatively more engaged with their savings. Both surveys were conducted by a research company over the phone within three weeks after we stopped collecting data on trial outcomes. We imposed quotas on the survey sample along two dimensions: observed switching behavior and starting balance. Around one in ten contacted customers agreed to complete the phone interview which lasted up to 15 minutes, in line with typical response rates for such surveys.

To ensure a degree of response quality in the survey, we asked customers to indicate which providers they held their savings account with. As an initial quality screen, we terminated interviews with a small number of customers who failed to indicate they had or until recently had an account with the provider in question even after being prompted. 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, including under-reporting by those who switched their accounts and over-reporting by those who did not. We interpret this imperfect recall as further evidence that customers did not dedicate full attention to the task of switching, but lack of incentives to report accurately and possible drawbacks in questionnaire design could have also been at play. Given small sample sizes, possible selection into participation, and imperfect recall, we interpret survey evidence with caution and focus on key findings as reported below.

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 annual statement (Trial 1) or the rate-decrease notification letter (Trial 2). Of those respondents who remembered receiving a communication from their provider, 60% and 75% in the front-page and reverse-page trials, respectively, reported that they did not read beyond the first page or only skimmed the communication. Respondents who remembered receiving the letters found it difficult to recall the details without explicit prompting by the interviewer. When prompted, many control-group and treatment-group respondents were able to recall details of received disclosures. However, many respondents also reported information that they did not in fact receive. Customers in the age group of 60-80 years were most likely to correctly recall disclosure information (+25 p.p.) and have read the letter in detail (+15 p.p.), 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.

Customers' awareness was not widespread both of their account's current interest rate and recent changes to the rate. Most respondents were not aware of the interest rate they were receiving on their savings account and the majority of those who estimated the rate upon prompting were overoptimistic, thinking their rate was higher than their actual rate. The most popular motivation for hypothetically considering switching accounts was if there were to be an equivalent product with a higher interest rate available with their current provider. 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) despite our disclosures being designed to convey this information. Only 4% and 8% of customers in the front-page and reverse-page switching box trials, respectively, indicated their primary consideration in deciding whether to switch their accounts was the availability of a better rate with another provider, consistent with the literature on deposit stickiness (e.g., Drechsler et al., 2018) and our empirical results that most switching was internal switching.

Did the disclosure treatment mitigate this unawareness of opportunities to earn a higher rate with the same provider? In the front-page switching box trial, treatments had a positive and 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 did not have significant effects on any surveyed outcome, including consideration of switching, awareness of their interest rate, shopping around, and recall of the communication received.

The survey responses also provide a window into how consumers trade off product preferences, the disutility of the switching process, and foregone interest income. Two thirds of respondents said they would require a minimum gain of less than £100 per year to switch

their account. In our survey sample, where the potential gains were significantly higher than average, 26% of customers had £100 or more to gain. While having potential gains lower than the reported cost of switching could explain why many customers did not switch, even among consumers with potential first-year gains well in excess of £100, switching is much lower than 26% (see section 5).

Beliefs about the onerousness of shopping and switching seem to matter. Most customers who switched found searching for alternative products as easier than expected. Those who switched reported an average time spent switching internally of 15 minutes and less than 30 minutes for those who switched to an external provider. Among those that shopped for an alternative savings product, 80% of customers in the front-page switching box trial and two thirds of customers in the reverse-page switching box trial said they spent less than two hours searching for an alternative account. However, when evaluating the decision they made, respondents who switched their account reported to be more satisfied than respondents who did not switch their account. Over 80% of switchers reported being fairly satisfied or very satisfied with their decision to switch, while less than half of non-switchers reported satisfaction with their decision to not switch. This disconnect between pessimistic beliefs about the cost and net benefits of switching versus higher ex-post satisfaction among switchers emphasizes the role of current consumer expectations around the usefulness of disclosure in driving inattention.

In summary, survey findings suggest that a large proportion of consumers do not actively engage with choosing optimal savings accounts and do not attend communications from their providers, significantly constraining the usefulness of even optimally designed disclosure. While the disclosure treatment significantly improved awareness of interest rates, many of those who do engage review the communications quickly and remember only the key aspects. Overall, pessimistic beliefs and the resulting inattention seem to harm consumers in this setting. Many consumers overestimate the time cost of reoptimization, forego a magnitude of interest income that they report would be sufficient for them to desire to switch, and end up less satisfied with their decision.

7 Conclusion

In this paper, we provide direct evidence on the effectiveness of consumer financial disclosure from a set of randomized-controlled trials. We study savings accounts, an important and ubiquitous retail financial product with relatively homogeneous features and significant price dispersion, test the importance of disclosure design features, and examine explanations for their role in affecting consumer financial decisions. A key virtue of our experimental design

relative to previous work is the ability to address joint-hypothesis concerns by focusing on the savings setting and leveraging the availability of an otherwise identical alternative product with each consumer’s current provider that has strictly better pricing. We test three main disclosure features: information to aid search and comparison across products, sending reminders to customers to call attention to the disclosure, and a pre-filled, prepaid-postage, mail-back switching form to help make acting on the disclosure easier. Although we find statistically significant effects for many of our disclosures, our interventions had at best modest effects on switching, even among those with substantial financial incentives to switch, the option to retain all non-price features of their current savings product, and who received our most effectively designed disclosure.

Why are deposits so sticky? The persistent locality of banking seems puzzling given the national, integrated, online nature of modern banking. Our findings suggest that non-price preferences over providers and pessimistic beliefs about both the returns to shopping around and the inconvenience of the switching process are key reasons for the price-insensitivity of individual deposits. In particular, our experimental and survey results point to limited consumer attention to information about competitive alternative deposit products as inhibiting more switching, consistent with the predictions of Loewenstein et al. (2014). Providing a procedurally straightforward way to take an informed action, designing disclosure to make alternative choices more salient, and complementary disclosure timing each help mitigate the obstacles consumers face and to some extent can each increase switching to better priced but otherwise identical products.

Of course, we have not tested every possible design and it is conceivable that there exist significantly more impactful ways to design disclosures. However, when firms have other dimensions of disclosure design left to their discretion, there are simple ways to present mandated content that limits its usefulness to consumers, with the classical example being fine print. Moreover, disclosure could be even less effective when the optimal contract is much less obvious than in our setting. Although 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, a variety of delivery media (routine statements, special mailings, email, and SMS) failed to have large impacts. Similarly, while in principle a rising interest-rate environment could stimulate active savings product choice by increasing the return on attention, we do not find compelling evidence that consumers with large potential gains from switching to respond differentially to disclosure.

The sufficiency of disclosure remains a subject of debate. In advocating for robust consumer financial protection, Campbell et al. (2011) conclude that disclosure is beneficial but may not provide enough support in all circumstances. Our qualitative research further in-

icates that while optimizing disclosure design can improve its effectiveness on the margin, inattention is a fundamental barrier to disclosure usefulness. Accordingly, when regulators independently consider whether to mandate each disclosure without accounting for externalities diluting the salience of other disclosures, the low expected return on attention to any given disclosure can reinforce beliefs about the low value of reading the fine print. Moreover, any reform to disclosure design is likely to improve outcomes only slowly as consumers adjust their expectations on the optimal level of attention. This suggests that beyond testing and optimizing disclosure, regulators could consider a wider set of interventions that are targeted at achieving more substantial improvements in market outcomes. Ultimately, disclosure's diluted efficacy has to be weighed against any distortionary effect of more active policy and the corresponding equilibrium responses by firms.

There may also be unintended distortions from mandatory consumer financial disclosures. Duarte and Hastings (2013) show that consumers may overly fixate on disclosed dimensions at the expense of other product characteristics, and Medina (2018) documents negative externalities of disclosure about one product 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, deposit insurance, and the homogeneity of savings products, firms interested in keeping their deposits sticky still have many tools at their disposal. Future research could seek to understand the efficiency of such product differentiation, the reasons behind consumers' (monetarily costly) brand loyalty, and the heterogeneous incidence of pricing models seemingly predicated on consumer inertia. Similarly, theoretical and empirical work could study the fixed (psychological) costs consumers incur when reoptimizing and the general disutility of financial decision-making along with a deeper empirical look at the determinants of consumer inertia.

References

- Abaluck, Jason and Jonathan Gruber**, “Choice inconsistencies among the elderly: evidence from plan choice in the Medicare Part D program,” *American Economic Review*, 2011, *101* (4), 1180–1210.
- Abramova, Inna, John Core, and Andrew Sutherland**, “Institutional Investor Attention and Demand for Inconsequential Disclosures,” 2018. SSRN Working Paper No. 3066136.
- Agarwal, Sumit, John C Driscoll, and David I Laibson**, “Optimal Mortgage Refinancing: A Closed-Form Solution,” *Journal of Money, Credit and Banking*, 2013, *45* (4), 591–622.
- , **Souphala Chomsisengphet, Chunlin Liu, and Nicholas S Souleles**, “Do consumers choose the right credit contracts?,” *The Review of Corporate Finance Studies*, 2015, *4* (2), 239–257.
- Allen, Jason, Robert Clark, and Jean-François Houde**, “Search frictions and market power in negotiated-price markets,” *Journal of Political Economy*, 2019, *127* (4), 1550–1598.
- Andersen, Steffen, John Y Campbell, Kasper Meisner Nielsen, and Tarun Ramadorai**, “Inattention and Inertia in Household Finance: Evidence from the Danish Mortgage Market,” 2017. NBER Working Paper No. 21386.
- Andries, Marianne and Valentin Haddad**, “Information Aversion,” October 2017. NBER Working Paper No. 23958.
- Ater, Itai and Vardit Landsman**, “Do customers learn from experience? Evidence from retail banking,” *Management Science*, 2013, *59* (9), 2019–2035.
- Ausubel, Lawrence M**, “The failure of competition in the credit card market,” *American Economic Review*, 1991, pp. 50–81.
- Ben-Shahar, Omri and Carl E Schneider**, *More than you wanted to know: The Failure of Mandated Disclosure*, Princeton University Press, 2014.
- Bertrand, Marianne, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman**, “What’s advertising content worth? Evidence from a consumer credit marketing field experiment,” *The Quarterly Journal of Economics*, 2010, *125* (1), 263–306.
- Calem, Paul S and Loretta J Mester**, “Consumer behavior and the stickiness of credit-card interest rates,” *American Economic Review*, 1995, *85* (5), 1327–1336.
- Calvet, Laurent E, John Y Campbell, and Paolo Sodini**, “Measuring the financial sophistication of households,” *American Economic Review*, 2009, *99* (2), 393–98.
- Campbell, John Y, Howell E Jackson, Brigitte C Madrian, and Peter Tufano**, “Consumer financial protection,” *Journal of Economic Perspectives*, 2011, *25* (1), 91–114.

- Célérier, Claire and Boris Vallée**, “Catering to investors through security design: Headline rate and complexity,” *The Quarterly Journal of Economics*, 2017, 132 (3), 1469–1508.
- Chetty, Raj, Adam Looney, and Kory Kroft**, “Salience and taxation: Theory and evidence,” *American Economic Review*, 2009, 99 (4), 1145–77.
- Choi, James J, David Laibson, and Brigitte C Madrian**, “\$100 bills on the sidewalk: Suboptimal investment in 401 (k) plans,” *Review of Economics and Statistics*, 2011, 93 (3), 748–763.
- DellaVigna, Stefano**, “Psychology and economics: Evidence from the field,” *Journal of Economic Literature*, 2009, 47 (2), 315–72.
- **and Ulrike Malmendier**, “Paying not to go to the gym,” *American Economic Review*, 2006, 96 (3), 694–719.
- Dranove, David and Ginger Zhe Jin**, “Quality disclosure and certification: Theory and practice,” *Journal of Economic Literature*, 2010, 48 (4), 935–63.
- Drechsler, Itamar, Alexi Savov, and Philipp Schnabl**, “The Deposits Channel of Monetary Policy,” *The Quarterly Journal of Economics*, 05 2017, 132 (4), 1819–1876.
- , – , **and** – , “Banking on Deposits: Maturity Transformation without Interest Rate Risk,” May 2018. NBER Working Paper No. 24582.
- Duarte, Fabian and Justine S Hastings**, “Fettered consumers and sophisticated firms: evidence from Mexico’s privatized social security market,” 2013. NBER Working Paper No. 18582.
- Ericson, Keith M Marzilli**, “Consumer inertia and firm pricing in the Medicare Part D prescription drug insurance exchange,” *American Economic Journal: Economic Policy*, 2014, 6 (1), 38–64.
- Farrell, Joseph and Paul Klemperer**, “Coordination and lock-in: Competition with switching costs and network effects,” *Handbook of Industrial Organization*, 2007, 3, 1967–2072.
- Ferman, Bruno**, “Reading the Fine Print: Information Disclosure in the Brazilian Credit Card Market,” *Management Science*, 2015, 62 (12), 3534–3548.
- Financial Conduct Authority**, “Cash savings market study report,” January 2015. Market Study MS14/2.3.
- Finkelstein, Amy**, “E-ztax: Tax salience and tax rates,” *The Quarterly Journal of Economics*, 2009, 124 (3), 969–1010.
- Gabaix, Xavier**, “Chapter 4 - Behavioral inattention,” in B. Douglas Bernheim, Stefano DellaVigna, and David Laibson, eds., *Handbook of Behavioral Economics - Foundations and Applications 2*, Vol. 2 of *Handbook of Behavioral Economics: Applications and Foundations 1*, North-Holland, 2019, pp. 261 – 343.

- **and David Laibson**, “Shrouded attributes, consumer myopia, and information suppression in competitive markets,” *The Quarterly Journal of Economics*, 2006, *121* (2), 505–540.
- Gathergood, John, Neale Mahoney, Neil Stewart, and Jörg Weber**, “How Do Individuals Repay Their Debt? The Balance-Matching Heuristic,” *American Economic Review*, March 2019, *109* (3), 844–75.
- Grubb, Michael D**, “Failing to choose the best price: Theory, evidence, and policy,” *Review of Industrial Organization*, 2015, *47* (3), 303–340.
- **and Matthew Osborne**, “Cellular service demand: Biased beliefs, learning, and bill shock,” *American Economic Review*, 2015, *105* (1), 234–71.
- Hall, Jonathan D. and Joshua Madsen**, “Can behavioral interventions be too salient? Evidence from traffic safety messages,” May 2020. Working Paper.
- Harrison, Glenn W. and John A. List**, “Field Experiments,” *Journal of Economic Literature*, December 2004, *42* (4), 1009–1055.
- Haushofer, Johannes**, “The cost of keeping track,” 2015. Working Paper.
- Hirshleifer, David and Siew Hong Teoh**, “Limited attention, information disclosure, and financial reporting,” *Journal of Accounting and Economics*, 2003, *36* (1), 337 – 386. Conference Issue on.
- Hortaçsu, Ali, Seyed Ali Madanizadeh, and Steven L Puller**, “Power to choose? An analysis of consumer inertia in the residential electricity market,” *American Economic Journal: Economic Policy*, 2017, *9* (4), 192–226.
- Jin, Ginger Zhe, Michael Luca, and Daniel J Martin**, “Complex Disclosure,” June 2018. NBER Working Paper No. 24675.
- Johnson, Eric J, Olivier Toubia, and Stephan Meier**, “What’s the Catch? Suspicion of Bank Motives and Sluggish Refinancing,” *The Review of Financial Studies*, 05 2018, *32* (2), 467–495.
- Jorring, Adam**, “The Cost of Financial Mistakes: Evidence from U.S. Consumers,” 2018. Working Paper.
- Kahn, Charles, George Pennacchi, and Ben Sopranzetti**, “Bank deposit rate clustering: Theory and empirical evidence,” *The Journal of Finance*, 1999, *54* (6), 2185–2214.
- Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman**, “Getting to the top of mind: How reminders increase saving,” *Management Science*, 2016, *62* (12), 3393–3411.
- Karlsson, Niklas, George Loewenstein, and Duane Seppi**, “The ostrich effect: Selective attention to information,” *Journal of Risk and Uncertainty*, 2009, *38* (2), 95–115.

- Keys, Benjamin J and Jialan Wang**, “Minimum payments and debt paydown in consumer credit cards,” *Journal of Financial Economics*, 2019, 131 (3), 528–548.
- , **Devin G Pope, and Jaren C Pope**, “Failure to refinance,” *Journal of Financial Economics*, 2016, 122 (3), 482–499.
- Kiser, Elizabeth K**, “Predicting household switching behavior and switching costs at depository institutions,” *Review of Industrial Organization*, 2002, 20 (4), 349–365.
- Klemperer, Paul**, “Markets with consumer switching costs,” *The Quarterly Journal of Economics*, 1987, 102 (2), 375–394.
- Kulkarni, Sheisha, Santiago Truffa, and Gonzalo Iberti**, “Removing the Fine Print: Standardization, Disclosure, and Consumer Loan Outcomes,” 2018. Working Paper.
- Leach, Anna**, “British judge: Say you’re sorry Apple... this time like you MEAN it,” *The Register*, November 2012.
- Leuz, Christian and Peter D. Wysocki**, “The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research,” *Journal of Accounting Research*, 2016, 54 (2), 525–622.
- Loewenstein, George, Cass R. Sunstein, and Russell Golman**, “Disclosure: Psychology Changes Everything,” *Annual Review of Economics*, 2014, 6 (1), 391–419.
- Lusardi, Annamaria and Olivia S Mitchell**, “The economic importance of financial literacy: Theory and evidence,” *Journal of Economic Literature*, 2014, 52 (1), 5–44.
- Medina, Paolina C**, “Selective attention in consumer finance: Evidence from a randomized intervention in the credit card market,” 2018. Working Paper.
- Nagel, Stefan**, “The liquidity premium of near-money assets,” *The Quarterly Journal of Economics*, 2016, 131 (4), 1927–1971.
- Olafsson, Arna and Michaela Pagel**, “The Ostrich in Us: Selective Attention to Financial Accounts, Income, Spending, and Liquidity,” October 2017. NBER Working Paper No. 23945.
- Persson, Petra**, “Attention manipulation and information overload,” *Behavioural Public Policy*, 2018, 2 (1), 78–106.
- Ponce, Alejandro, Enrique Seira, and Guillermo Zamarripa**, “Borrowing on the Wrong Credit Card? Evidence from Mexico,” *American Economic Review*, April 2017, 107 (4), 1335–61.
- Roychowdhury, Sugata, Nemit Shroff, and Rodrigo S Verdi**, “The effects of financial reporting and disclosure on corporate investment: A review,” *Journal of Accounting and Economics*, 2019, 68 (2-3), 101246.

- Sallee, James M.**, “Rational Inattention and Energy Efficiency,” *The Journal of Law and Economics*, 2014, 57 (3), 781–820.
- Samuelson, William and Richard Zeckhauser**, “Status quo bias in decision making,” *Journal of Risk and Uncertainty*, 1988, 1 (1), 7–59.
- Schwartz, Barry**, *The Paradox of Choice*, New York: HarperCollins, 2004.
- Seira, Enrique, Alan Elizondo, and Eduardo Laguna-Müggenburg**, “Are information disclosures effective? evidence from the credit card market,” *American Economic Journal: Economic Policy*, 2017, 9 (1), 277–307.
- Sims, Christopher A.**, “Implications of Rational Inattention,” *Journal of Monetary Economics*, 2003, 50 (3), 665–690.
- Stango, Victor and Jonathan Zinman**, “Exponential growth bias and household finance,” *The Journal of Finance*, 2009, 64 (6), 2807–2849.
- Tu, Yanping and Dilip Soman**, “The categorization of time and its impact on task initiation,” *Journal of Consumer Research*, 2014, 41 (3), 810–822.

Table 1: Overview of Trials

Trial	Treatment details	Rate change	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	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	End of individual bonus period seven weeks before to eight weeks after treatment	Short
5 SMS reminder	Rate decrease reminder via SMS	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 approximate customer tenure. “Long” refers to average customer relationships longer than 10 years, and “short” if the average tenure is less than 2 years. See Table 2 for further details in composition across trials.

Table 2: Descriptive statistics of consumers

Trial	Front page	Reverse		Digital	
	switching box (1)	page switching box (2)	Return switching form (3)	(email and SMS) reminder (4)	SMS reminder (5)
Age (years)	59.2 (16.58)	53.2 (17.23)	64.4 (15.92)	52.9 (16.15)	42.4 (13.92)
Male Indicator	0.42 (0.49)	0.41 (0.49)	0.45 (0.50)	0.48 (0.50)	0.52 (0.50)
Checking Account Indicator	0.25 (0.43)	0.80 (0.40)	0.06 (0.24)	0.77 (0.42)	0.98 (0.16)
Account Balance (£)	8,436 (20,788)	7,407 (22,862)	6,812 (18,156)	37,939 (88,633)	24,162 (78,574)
Potential Gains (£)	70.02 (172.54)	82.96 (256.05)	76.29 (203.35)	230.56 (538.50)	198.13 (644.31)
Account Age (years)	13.7 (10.86)	6.7 (1.25)	16.1 (3.99)	1.0 (0.09)	4.7 (2.45)
Number of products with	1.6 (0.88)	4.6 (1.88)	1.6 (1.28)	4.6 (2.55)	5.4 (2.86)
Online Banking Indicator	0.09 (0.28)	0.58 (0.49)	- (-)	0.84 (0.37)	0.90 (0.29)
Mobile Banking Indicator	0.09 (0.29)	0.29 (0.45)	- (-)	0.22 (0.42)	0.30 (0.46)
Observations	61,879	13,261	4,003	15,487	30,202

Notes: Table reports means and standard deviations in parentheses of savings account holder characteristics across each trial. Checking Account Indicator is equal to one if a customer has a checking account at the same institution. Potential gains is equal to depositor's account balance multiplied by the difference between her account's interest rate and the best available external rate. Account age is the number of years each account has been open. Number of products with providers is the number of products including the savings account that a customer has with her institution. Online banking and mobile banking indicators equal one if the customer uses that account feature.

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

	Number of Observations	Age (years)	Account Balance (£)	Gender (% male)	Checking account colocated (%)	Account age (years)	Joint test <i>p</i> -value
<u><i>Panel I. Front-Page Switching Box</i></u>							
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	
<i>Equality p-value</i>		<i>0.45</i>	<i>0.13</i>	<i>0.89</i>	<i>0.12</i>	<i>0.66</i>	<i>0.20</i>
Total Observations				61,879			
<u><i>Panel II. Reverse-Page Switching Box</i></u>							
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	
<i>Equality p-value</i>		<i>0.01</i>	<i>0.90</i>	<i>0.94</i>	<i>0.99</i>	<i>0.31</i>	<i>0.11</i>
Total Observations				13,261			
<u><i>Panel III. Switching Form</i></u>							
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	
<i>Equality p-value</i>		<i>0.40</i>	<i>0.83</i>	<i>0.22</i>	<i>0.80</i>	<i>0.35</i>	<i>0.72</i>
Total Observations				4,003			
<u><i>Panel IV. Digital Reminder</i></u>							
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	
<i>Equality p-value</i>		<i>0.57</i>	<i>0.43</i>	<i>0.56</i>	<i>0.51</i>	<i>0.31</i>	<i>0.66</i>
Total Observations				15,487			
<u><i>Panel V. SMS Reminder</i></u>							
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	
<i>Equality p-value</i>		<i>0.01</i>	<i>0.16</i>	<i>0.00</i>	<i>0.70</i>	<i>0.01</i>	<i>0.00</i>
Total Observations				30,202			

Notes: Table reports means of consumer attributes for both treatment and control groups for each trial. Equality *p*-values in italics test for the equality of means across treatment and control. The *p*-value in the final column tests for the joint equality of all variable means within a trial.

Table 4: Disclosure Treatment Effect on Account Switching

Trial	Front page switching box (1)	Reverse switching box (2)	Switching form (3)	Digital reminder (4)	SMS reminder (5)
<i>I. Any Switching</i>					
Disclosure Treatment	0.019*** (0.002)	-0.003 (0.006)	0.089*** (0.008)	0.047*** (0.008)	0.016*** (0.003)
Controls	yes	yes	yes	yes	yes
Control-Group Mean	0.026	0.077	0.030	0.400	0.062
R-squared	0.005	0.008	0.034	0.071	0.010
Observations	61,879	13,261	4,003	15,487	30,202
<i>II. Internal Switching</i>					
Disclosure Treatment	0.017*** (0.001)	0.001 (0.001)	0.078*** (0.006)	0.044*** (0.007)	
Controls	yes	yes	yes	yes	
Control-Group Mean	0.009	0.026	0.005	0.267	
R-squared	0.011	0.001	0.043	0.107	
Observations	61,879	13,261	4,003	15,487	

Notes: Table reports heterogeneity in disclosure treatment effects on switching at a four-week horizon by disclosure design. The dependent variable in panel I is any switching, an indicator for whether the depositor switched to a different product with the same provider or withdrew their entire balance. The dependent variable in panel II is internal switching, defined as switching to an different instant-access savings product at the same depository. For the SMS reminder trial, there was no internal alternative such that all switching is other switching. Controls include a quadratic in age and annual potential monetary gains and indicators for gender and having a checking account with the same institution. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Treatment Effect Heterogeneity by Disclosure Design

Trial	Front-page switching box annual statement		Reverse-page switching box		Digital reminder rate decrease	
	<u>Any</u> (1)	<u>Internal</u> (2)	<u>Any</u> (3)	<u>Internal</u> (4)	<u>Any</u> (5)	<u>Internal</u> (6)
Call to Action	0.009*** (0.002)	0.005*** (0.001)				
Best Internal Rate	0.029*** (0.002)	0.025*** (0.002)	-0.0002 (0.007)	0.002 (0.001)		
Best Internal and Competitor Rates	0.018*** (0.002)	0.017*** (0.002)	-0.004 (0.007)	0.0005 (0.001)		
Best Internal and Competitor Rates + Graph	0.021*** (0.002)	0.020*** (0.002)				
Best Internal Rate, Personalized			-0.006 (0.007)	0.001 (0.001)		
Best Internal and Competitor Rates, Personalized			-0.002 (0.007)	0.001 (0.001)		
Email					0.053*** (0.009)	0.051*** (0.009)
SMS					0.042*** (0.009)	0.037*** (0.008)
Controls	yes	yes	yes	yes	yes	yes
Control-Group Mean	0.026	0.009	0.077	0.026	0.400	0.267
Treatment Effect Equality <i>p</i> -value	0.000	0.000	0.873	0.722	0.228	0.114
Observations	61,879	61,879	13,261	13,261	15,487	15,487
R-squared	0.005	0.012	0.008	0.001	0.067	0.099

Notes: Table reports disclosure treatment effects at a four-week horizon by disclosure design. Each row corresponds to a treatment arm with the indicated disclosure design. Any switching (odd columns) is an indicator for whether the depositor switched to a different product with the same provider or withdrew their entire balance. Internal switching (even columns) is switching to an different instant-access savings product at the same bank. Controls include a quadratic in age and annual potential monetary gains and indicators for gender and having a checking account with the same institution. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Disclosure Treatment Effect Heterogeneity by Individual Characteristics

	Front page switching box (1)	Reverse switching box (2)	Switching form (3)	Digital reminder (4)	SMS reminder (5)
Treatment Indicator	0.01*	-0.028	0.065**	0.026	0.016
	(0.006)	(0.018)	(0.026)	(0.023)	(0.022)
Treatment *					
Age 40-60 yrs	-0.0003	0.004	-0.006	0.046**	-0.004
	(0.006)	(0.016)	(0.028)	(0.021)	(0.010)
Age 60-80 yrs	0.009	0.002	0.036	0.055**	-0.000
	(0.006)	(0.017)	(0.030)	(0.022)	(0.013)
Age >80 yrs	0.029***	-0.022	0.017	0.031	0.007
	(0.008)	(0.024)	(0.032)	(0.058)	(0.053)
Gains £50-100	0.006	0.020	0.022	0.026	-0.007
	(0.005)	(0.015)	(0.036)	(0.025)	(0.013)
Gains £100-500	0.003	0.015	0.032	0.001	-0.029***
	(0.005)	(0.012)	(0.023)	(0.020)	(0.010)
Gains >£500	0.007	-0.007	0.005	-0.029	-0.006
	(0.013)	(0.024)	(0.056)	(0.029)	(0.016)
Checking Account	0.005	0.026*	0.011	-0.02	0.015
	(0.004)	(0.014)	(0.036)	(0.020)	(0.022)
Age 40-60 yrs	-0.017***	-0.034**	0.007	0.131***	-0.028***
	(0.005)	(0.014)	(0.012)	(0.017)	(0.005)
Age 60-80 yrs	-0.011**	-0.039***	0.025*	0.245***	-0.022***
	(0.005)	(0.015)	(0.013)	(0.018)	(0.007)
Age >80 yrs	-0.011	-0.012	0.003	0.150***	-0.045***
	(0.007)	(0.022)	(0.014)	(0.048)	(0.016)
Gains £50-100	-0.008**	-0.055***	0.053***	0.034*	-0.032***
	(0.004)	(0.013)	(0.019)	(0.021)	(0.007)
Gains £100-500	-0.003	-0.064***	-0.007	0.063***	-0.036***
	(0.004)	(0.011)	(0.009)	(0.016)	(0.005)
Gains >£500	0.001	-0.046**	0.032	0.180***	-0.032***
	(0.010)	(0.022)	(0.032)	(0.023)	(0.008)
Checking Account	0.011***	-0.015	-0.012	0.093***	0.031***
	(0.004)	(0.013)	(0.015)	(0.020)	(0.011)
Constant	0.037***	0.132***	0.012	0.144***	0.062***
	(0.005)	(0.017)	(0.011)	(0.019)	(0.011)
Treatment Effect Equality p -value	0.0002	0.44	0.27	0.10	0.21
Observations	61,879	13,261	4,003	15,487	30,202
R-squared	0.005	0.013	0.041	0.068	0.014

Notes: Table reports treatment effects, control variable main effects, and treatment-control interactions by trial. Dependent variable is any switching. All controls are indicator variables for the indicated category. Checking account indicates whether the depositor had a checking account at the same depository. P-values test whether all treatment-control interactions are jointly zero. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7. Disclosure Treatment Effect Heterogeneity by Reminder Timing

	Digital (1)	Digital (2)	SMS (3)	SMS (4)
Treatment *				
4+ weeks before	0.018* (0.011)	0.054*** (0.014)		
2-4 weeks before	0.046*** (0.012)	0.025 (0.017)		
0-2 weeks before	0.102*** (0.014)	0.083*** (0.023)	0.019*** (0.005)	0.020*** (0.005)
Day of switch			0.012*** (0.004)	0.013*** (0.004)
0-2 weeks after	0.055*** (0.012)	0.037** (0.017)	0.021*** (0.005)	0.021*** (0.005)
2-4 weeks after	0.054*** (0.016)	0.045* (0.026)		
Cohort Fixed Effects		yes		yes
Controls	yes	yes	yes	yes
Observations	15,487	15,487	30,202	30,202
R-squared	0.073	0.074	0.010	0.010

Notes: Table reports disclosure treatment effect heterogeneity on any account switching by the timing of reminders relative to an interest-rate decrease for the digital trial (email and SMS, columns 1-2) and SMS trial (columns 3-4). Cohort fixed effects consist of fixed effects for each account opening half month (column 2) and date (column 4). Controls include a quadratic in age and annual potential monetary gains and indicators for gender and having a checking account with the same institution. Robust standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix

Figure A1: Example Call to Action Treatment Letter



Check your account is still right for you.

You've had your <account> for a little while now, and we know how important it is to make the most of your savings. So now might be a good time to consider if it's still the best option, or whether there's another savings account out there that could pay more interest, or suit you better.

Your <account> currently offers

- An interest rate of <x.x%> variable
- <conditions in relation to minimum deposit,
- withdrawals, and frequency of interest
- payments>

How this account compares,

We have a range of savings accounts available that could get you more from your money. Find out more at <weblink>

Let us know,

Making the move to another savings account is simple – sign in at <weblink> and select 'renewal options', call us on <phone> or pop in branch. If you'd prefer to carry on saving in your <account> you don't need to do anything.

Figure A2: Example Treatment with Best internal and Market Rates with Graph



Important Information for you

Check your account is still right for you.

You've had your <account> for a little while now, and we know how important it is to make the most of your savings. So now might be a good time to consider if it's still the best option, or whether there's another savings account out there that could pay more interest, or suit you better.

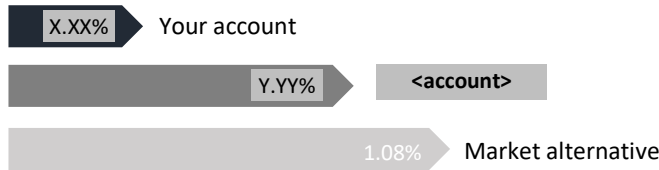
Your <account> **currently offers**

- An interest rate of <x.x%> variable
- <conditions in relation to minimum deposit,
- withdrawals, and frequency of interest payments>
-

How this account compares,

As of 10th August the savings account with the most similar features that we can offer is our <account> with an interest rate of < Y.YY% >, but there may be other suitable accounts within our range.

Taking a look at the wider savings market, the three highest-paying easy access accounts across the market on 10th August offered an average rate of **1.08% variable**. You can find out more about these options at www.moneysupermarket.com



Interest you could earn this year on every £10,000 of savings

<current savings account>	£X.XX
<alternative internal savings account>	£X.XX(£A.AA more)
Highest paying accounts on the market*	£108.00(£83.00more)

Let us know,

Making the move to another savings account is simple – sign in at <weblink> and select 'renewal options', call us on <phone> or pop in branch. If you'd prefer to carry on saving in your <account> you don't need to do anything.

*Based on an average of the three highest paying equivalent accounts on the market at 10th August, 2015 using moneysupermarket.com. Restrictions and exclusions may apply.

Figure A3: 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 A4: 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



<p>Your rate: X.XX% AER/gross</p>	<p>Our best Comparable rate: <A/C name> B.BB% AER/gross (inc. bonus)</p>	<p>Average of 3 of the highest paying account: 1.32% AER/gross (may inc. bonus)</p>
---	--	---

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 your <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 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 moneycats.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 A5: Example Rate Change Letter with Detachable Switching Form

<logo>

<contact details>

<Title><Initials 1><Surname>
 <Title><Initials 1><Surname>
 <Title><Initials 1><Surname>&<Title><Initials 1><Surname>
 <Address line>
 <Address line>
 <Address line>
 <Address line>
 <Post code>

September 2015

Dear <Salutations>

Get a better rate of interest on your savings

We are writing to let you know that you can get a better rate of interest on your savings. Your savings are currently in a <account name>, which pays an interest rate of <x.xx%> Gross PA/AER and provides easy access, meaning you can withdraw money without charge. By moving to another of our savings accounts you can earn a better rate of interest and make your savings work harder for you.

How does my savings account compare?

As at <date> 2015, the highest interest rate available from <firm name> on a comparable account is <y.yy%> Gross PA/AER on our <account name>

Three of the highest paying easy access accounts offered by other banks and building societies offer an average rate of <%> Gross PA/AER. Price comparison websites can provide information on rates offered by other providers.

How much more could I earn in interest?

To make it easier to compare the accounts, the following examples all use an account balance of £5,000 based on a Gross interest rate.

> £5,000 balance in your existing <account name> <£x.xx> per year
 > £5,000 balance in our <account name> <£y.yy> per year
 > £5,000 balance in one of the average 3 highest paying accounts on the market <£> per year

Your rate: <x.xx%> Gross PA/AER	<firm name> best Comparable rate: <account name> <y.yy%> Gross PA/AER	Average of 3 of the highest paying accounts: <%> Gross PA/ AER (may include bonus)
---------------------------------------	--	---

What to do next

If you would like to open an <account name> please contact us. If you have a passbook remember to send to us as well.

If you would prefer to leave your savings where they are that's fine – there is nothing you need to do.

<authorisation details>

I would like to switch my savings to the <account name> account

<Deposit account> <Account number>
 <title><initial><surname>&<title><initial><surname>
 <title><initial><surname>&<title><initial><surname>

How much would you like to transfer?

Either choose "Transfer all" or fill in the amount you want to transfer from your <deposit account>.

Transfer all Transfer part of my savings £

How would you like your interest paid? Annually Monthly

Interest will be paid to the same account as the interest from your <deposit account>. If you would like to change this, please tell us in writing.

<declaration details>

Signature(s)	Date
--------------	------

Figure A6: Example Email Disclosure

Your savings account interest rate is reducing [View this email here](#)
 This email is intended for %%Title%% %%Last Name%%, %%Postal Code%%.

<logo and firm name>

Your savings account interest rate is reducing

Dr Tester

The interest rate on your <account> was for <length> and will end soon. From %%closing-date%%, your account will change to an <account> account and the interest rate will reduce, as shown below

Current balance tiers	Current interest rate Gross / AER (variable)	New account name	New balance tiers	New interest rate Gross/AER(variable)
<£X>	<X.XX%>	<account>	<£X.XX>	<Y.YY%>

The interest rate on the <account> is a variable rate, which can be increased or reduced at any time

You can change to a different savings account at any time.

Although you can carry on using your account as you do now, you may prefer to open another <account> or <other options>

Account Name		<condition for interest rate tier>	<condition for interest rate tier>
<account>	Interest rate Gross/AER (variable) for <length> on balances <£X>	A.AA%	B.BB%

The interest rates in the table above are correct as at 10 August 2015. This product can be withdrawn from sale at any time.

If you'd like more information about the full range of options available:

Visit our website

<weblink>

Find us in branches

Call us

<phone>

<times of availability>

Figure A7: Example Text Message Disclosure

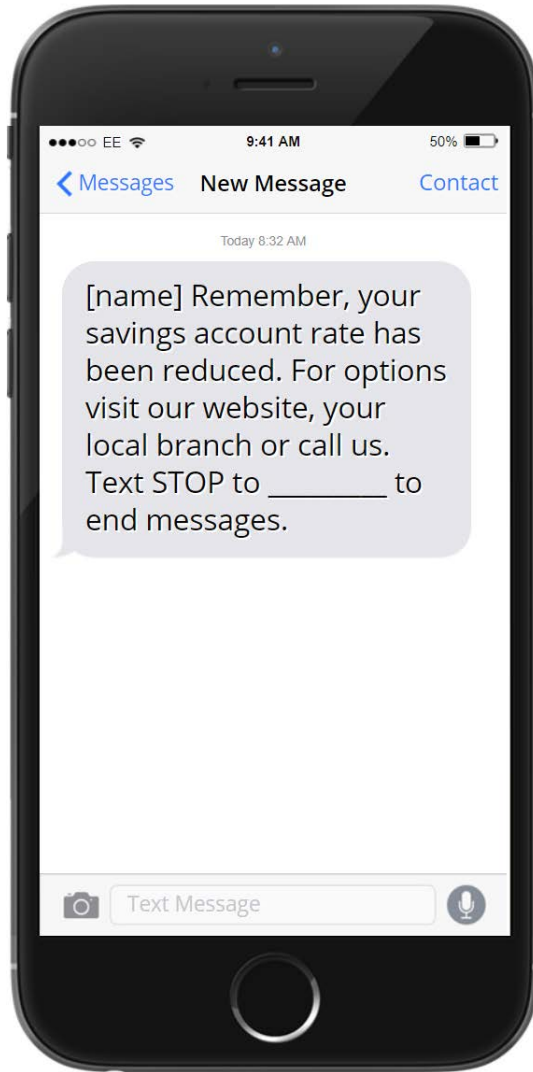
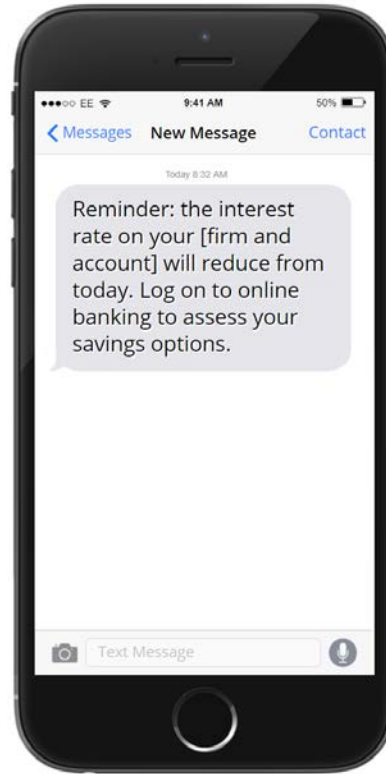
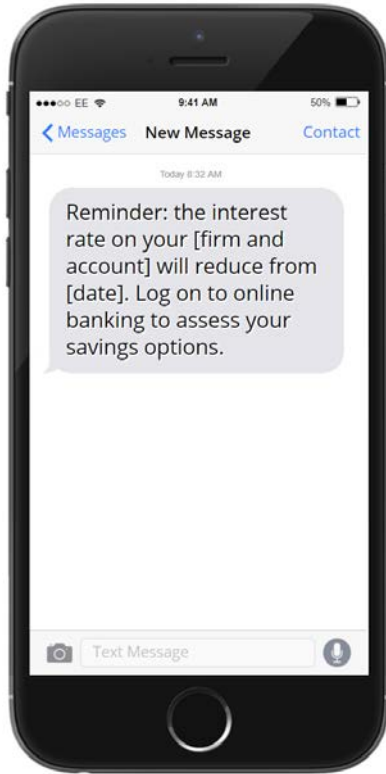
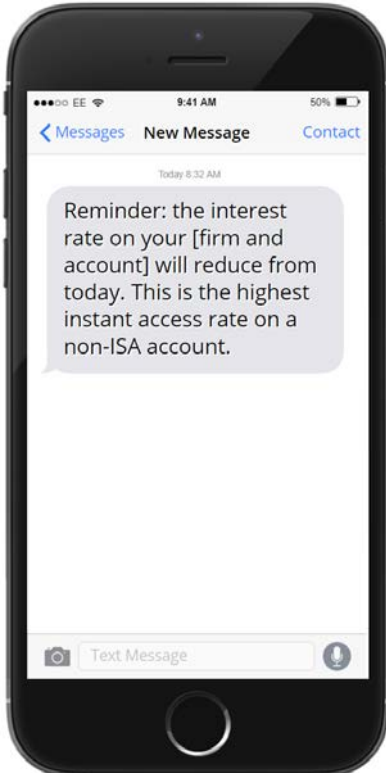


Figure A8: Example Test Message Disclosure with Varied Timing
One week before rate change Day of rate change, version 1



Day of rate change, version 2



One week after rate change

