Do nudges increase consumer search and switching? Evidence from financial markets

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As nudge interventions have become more popular, academic research is developing that aims to assess to what extent and under what circumstances these interventions are effective. My paper contributes to this stream of research in a specific context: collating and synthesising evidence on the effectiveness of nudge interventions that aim to increase consumer search and switching in retail financial markets. Following a systematic search strategy, I identified 33 papers containing relevant research, including qualitative studies, online laboratory experiments, field experiments and ex post data analyses, covering a wide range of retail financial products and a number of different types of nudges. The review of these papers results in two main contributions. First, it demonstrates that different study designs serve different purposes in evidence accumulation. In particular, qualitative studies and online lab experiments should not be used to ascertain the magnitude of the intervention's impact. Second, based on over 400 estimates from the quantitative analyses in these papers, it establishes that the currently available evidence shows that nudges increase consumer search and switching in retail financial markets by 2-3 percentage points on average. The most effective interventions appear to be the ones that make the consumer's life easier by taking some of the administrative burden over, and the ones that make a relatively major change in the structure of the decision-making environment. Disclosures, reminders, simplifications and informational nudges tend to have a smaller impact. In other words, nudges that change the choice architecture more profoundly have a higher impact on search and switching than nudges that only provide, simplify or highlight information. Overall, while nudge interventions may be efficient on a cost-benefit basis and can lead to large increase in relative terms (e.g. doubling switching rates from 1% to 2%), regulators cannot expect them to alter consumer behaviour to the extent that it would lead to a significant change in the competitive landscape.

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I. Introduction

It is a common finding in competition analyses, and in particular in market studies, that there are problems on the demand side: consumers do not shop around and do not switch between providers and hence do not put much pressure on firms to compete. For instance, low consumer engagement was identified as a feature in the markets for retirement income, cash savings and retail banking. Low levels of shopping around and switching are not in themselves enough to conclude that there is a problem in the market – it could very well be that firms compete vigorously and as a result, their offerings are similarly good value and consumers do not need to switch. However, in all of these cases other types of analyses showed that many consumers would benefit from shopping around and switching as they could get cheaper and/or better quality products than they currently purchase.

Behavioural economics provides us with explanations for why this might be happening. For instance, we as consumers have limited attention, make decisions based on rules of thumb, are often overconfident about our abilities or actions in the future and exhibit present bias. These 'biases' are particularly prevalent in retail financial markets because financial products are inherently complex, involve a trade-off between the present and the future, may require assessing risk and uncertainty and some of them (e.g. mortgages) do not permit learning from past mistakes (Erta et al, 2013).

Advocates of behavioural economics also offer a potential solution: nudging people towards more desirable behaviours. The nudge movement became widespread following Richard Thaler and Cass Sunstein's book "Nudge: Improving Decisions About Health, Wealth, and Happiness", published in 2008. Following this, authorities, and in particular the UK's Financial Conduct Authority that was in the forefront of applying behavioural research in practice, started trialling whether nudges could be used to increase consumer search and switching.

The goal of this paper is to ascertain what we can say about the effectiveness of these nudge interventions over ten years down the line. In addition, I wanted to find out whether there are any types of nudges that appear to work better (Q1), and whether there are any products (Q2) or groups of consumers (Q3) for which nudges seem to be more effective than for others.

In order to do this, I carried out a systematic search for relevant research using a set of pre-defined inclusion criteria. I found 33 relevant studies in total, providing both qualitative and quantitative evidence on the effectiveness of nudges in a wide range of retail financial markets in the UK, the US, in Mexico and within the European Union. Based on over 400 observations extracted from these papers I find that the currently most reliable evidence suggests that nudges increase consumer search and switching in retail financial markets by 2-3 percentage points on average. The most effective nudges appear to be the ones that make the consumer's life easier by taking some of the administrative burden over and the ones that make a relatively major change in the structure of the decision-making environment. Disclosures, reminders, simplifications and nudges that provide some extra information tend to have a smaller impact. In other words, nudges that change the choice architecture more profoundly have a higher impact on search and switching than nudges that only provide, simplify or highlight information. Default interventions, that achieved larger effects in other domains, have not been tested for financial products with the aim of inducing more consumer search and switching.[1] There is no clear evidence that nudge interventions would work better for certain products or for certain groups of consumers, but there is an indication that it is easier to nudge people to shop around than to switch.

I also found evidence on the different roles of different study designs in evidence accumulation. Qualitative research on which nudges may make consumers search and switch may be useful in identifying features that could increase their efficacy but provide limited information on the likely impact of these. Online laboratory experiments appear to significantly overestimate the impact of nudges but they are considered to be useful in providing evidence on the ranking on different interventions. Unfortunately, there are only a few *ex post* evaluations and even these suffer from methodological issues, such as not being able to establish causality. Currently field trials appear to be the most reliable source for calculating the average impact of nudge interventions.

To my knowledge, I am the first to carry out a comprehensive overview of the available evidence on the effectiveness of nudges in increasing consumer search and switching. While nudge interventions may still be efficient on a cost-benefit basis (see Benartzi et al, 2017) and potentially result in a large increase in relative terms (e.g. a 100% increase in switching rates from 1% to 2%), the review demonstrates that regulators cannot expect them to alter consumer behaviour to the extent that it would lead to a major change in the competitive landscape. I restricted the review to retail financial markets but the findings are likely to be highly relevant for policy-makers more broadly.[2]

The structure of the paper is as follows. Section II covers the related literature and section III describes the methodology I used for the literature search, data extraction and the analysis. Section IV presents the results and section V concludes.

II. Literature

As behavioural economics became popular, its ideas found their way into policy-making, nudge units were created and authorities started testing demand-side interventions that were based on behavioural principles. After a number of successful and less successful trials, the question naturally arose about how effective these nudges really are. This has led to the development of a new stream of academic literature: systematic reviews and meta-analyses compiling results of different studies and drawing conclusions on the average impact and its determinants. Most of the early reviews focus on the context of health (Hummel-Maedche, 2019) but I do not discuss those here. Instead, below I briefly summarise those reviews that also covered nudges in finance or consumer choice as my paper is more closely related to these.

DellaVigna and Linos (2020) carried out a meta-analysis of 126 trials by two nudge units and 26 trials published in academic journals, comparing the average impact in the two sets. Their main finding is that academic papers on average estimate an 8.7 percentage point impact of nudge interventions, compared to only 1.4 percentage points in the nudge unit studies – a difference which can be fully explained by publication bias, exacerbated by low statistical power.

Another meta-analysis is by Jachimowicz et al (2019), who investigated the effectiveness of default interventions. They collated 58 studies from various domains, including consumer choice, and found that while defaults have a considerable effect on average, there is also substantial variation in the results. This variation is partly explained by defaults being more effective in consumer policy than in environmental settings.

[1] With one exception of a qualitative study. See below in section IV, subsection b.
[2] See, for example, the UK's Competition & Markets Authority's recommendations following the 'loyalty penalty' super-complaint, that set out that regulators should capture and share best practice on nudge remedies that have been tested (CMA, 2018).

Hummel and Maedche (2019) performed a quantitative review on nudging based on 100 papers from different research areas, including finance. They provide a morphological box of nudge studies that gives an overview of the settings, types and outcomes of these papers. They find that the median effect size is 21%, ranging from 0 to 1681%, which varies by context and by nudge category.

Benartzi et al (2017) selected a small number of nudge studies in order to compare their effectiveness to those of traditional tools (e.g. tax incentives). Their main conclusion is that while nudges may have a small absolute impact, they are often relatively cheap and as a result, highly cost-effective. Referring to their work, David Laibson made the point in its presentation at the American Economic Association that governments should invest more in developing nudge interventions but also in other types of paternalistic interventions as nudges in themselves will not achieve enough.[3]

Finally, two papers by Cai (2019) and Szaszi et al (2018) provide an overview of the research on nudges and an analysis of the characteristics of the relevant studies, but without attempting to estimate an average impact.

Two of the above papers compare the relative impact of different types of nudges. Given that each paper (including mine) covers a different set of policy areas, it is not surprising that the nudge categories applied differ by study. There are, however, some results that appear consistent across these papers.

DellaVigna and Linos (2020) split nudges into the following categories: simplification, personal motivation, reminders and planning prompts, social cues, framing and formatting, and choice design. Choice design covers nudging people towards an active choice or making choices more salient (but not defaults which are excluded from the analysis). They find that changes in choice design (such as prompting recipients to enrol into retirement savings plans, sign up for flu vaccinations or blood donation) have the highest impact. In addition, in the academic sample they also find that simplifications work well, and the example they give is providing pre-filled fields in tax returns. In my categorisation this would fall into the “increases in ease and convenience” category, which is indeed one of the types that appear to have a larger impact.

Hummel and Maedche (2019) use the following nine groups: defaults, simplifications, social references, change effort, disclosures, warnings/graphics, precommitments, reminders, and implementation intentions, and find that defaults have larger median and average effect sizes than other categories. Consistently with my findings, they show that reminders and disclosures have small effects on average. However, contrary to my findings, their category of “change effort” which may correspond to the “increases in ease and convenience” category in my analysis only shows medium impact relative to other categories.

My paper contributes to the research stream on the effectiveness of nudges, focusing on a specific policy-relevant question. It is different from the papers above in the sense that it is restricted depending on the outcome measure (only papers measuring search and switching are included) and the domain (retail financial products only). It is, however, broader in the sense that I collected all available evidence irrespective of study design, and in addition to calculating average effects, I provide insights into qualitative findings and results from *ex post* evaluations.

III. Methodology

In this section, I first describe the strategy for identifying relevant research and list the studies included in the final sample. The next subsection briefly summarises what data I extracted from these studies

[3] The presentation is available at: https://www.aeaweb.org/webcasts/2020/aea-afa-joint-luncheon-nudges-are-not-enough.

and how I obtained a dataset of comparable observations. Finally, I set out the three different methodologies I applied to analyse this dataset.

a. Identifying relevant research

In order to identify a set of papers that can help answer my research question, I defined the following inclusion criteria.

Study design

I did not apply any restriction on the study design. That is, I included any study that met the other inclusion criteria, irrespective of whether it was a qualitative or a quantitative assessment and whether it analysed existing data or generated data specifically for the purposes of the research.

Type of intervention

I included all studies that analyse the impact of an intervention that uses nudges with the aim of increasing consumer search or switching. Here, I relied on the definition of nudge by Thaler and Sunstein (2008): a nudge "is any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid." Choice architecture refers to the way choices are presented to consumers and how these different presentations affect consumer decision-making, including the presence of defaults or the number of choices. Studies that refer to nudges but do not meet this definition were excluded and studies that do not use the term "nudge" but in fact apply an intervention that meets this definition are included.

Outcome measure

All the studies included have at least one outcome measure of search or switching. This includes soft measures, such as "intention to switch" but excludes other measures of consumer engagement, like "awareness" or "contact with firm". Studies that measure search for information on the same product (e.g. reading the terms and conditions) are excluded as these do not constitute shopping around.

Products

As mentioned above, I restricted the sample of studies to those on retail financial products.

Population

I applied no restriction on the population that was used in the study.

Language

Only research in English was included.

I have searched online on the following:

- Websites of financial regulators, competition authorities and nudge units in Australia, Canada, EU, Ireland, UK and the US;
- Websites of international organisations (OECD and World Bank);
- A number of databases (TEN, RePEc, NBER, Open Grey, Proquest and EthOS); and
- Search engines (Google, Google Scholar and Microsoft Academic).

In addition, I selected five journals for hand-searching and reviewed all their editions between 2015 and 2020. These journals were the Journal of Behavioral and Experimental Finance, the Journal of Behavioral Finance, the Journal of Behavioral Economics for Policy, Behavioural Public Policy and Behavioral Science & Policy.

Finally, I reviewed the bibliographies of all the selected papers.

Throughout the literature search, I used combinations of the following terms: nudge; search / shopping around / switching; credit cards / bank accounts / savings accounts / current accounts / loans / insurance / mortgages / pensions / investment / financial product; trial / experiment / evaluation / survey; disclosure; choice architecture; policy and intervention.

Table 1 shows the complete list of the 33 studies that met all the inclusion criteria.

b. Data extraction

I recorded data about the relevant studies at two levels.

First, I selected the characteristics that do not change within a paper and recorded these for all the 33 studies. These include study design, geographic area, population, involvement of an authority and if there was one, the policy stage at which they carried out the study. Finally, I noted down whether search or switching was one of the main outcome variables. This is relevant as in some cases the study was not designed to measure the impact on search or switching but nevertheless the authors report a variant of these measures. I believe that it is important to take this into account in the overall assessment as studies that are not designed specifically to measure search or switching may provide less accurate estimates. The summary of the paper-level characteristics is discussed in section IV.

Second, I recorded as a separate observation each estimated impact from the 24 quantitative studies for all interventions that met the definition of nudge.[4] Most papers report their result as a percentage point change and therefore I focused on these measures. If a paper included percentage point estimates and also other results, I only recorded the former. However, if a paper did not include an estimate of the percentage point impact, I recorded the estimated impact and added an explanation of what it measures. A few of the papers did not include a valid estimate (e.g. because the study was inconclusive). In these cases, I added one observation per paper but with a missing value for the estimate. The final dataset contains 785 observations.

Note that the number of nudge interventions tested in these papers is much lower (95) than the number of estimates recorded. This is primarily because many papers estimate the impact of the same nudge using different specifications (e.g. with or without control variables) and on different outcome measures (e.g. all switching and internal switching only).

Table 2 shows the number of nudges and estimates by paper as recorded in the dataset. However, some of these estimates are not comparable for the following reasons:

- They do not show a percentage point difference (e.g. instead, they show the change in the absolute number of products the consumer viewed);
- The specification includes interaction terms with the treatment; or
- They are already pooled results of other estimates.

Taking these out, I get a dataset of 461 comparable estimates that belong to 83 different nudges from 18 papers. Note, however, that over 40% of these observations come from three papers (Adams et al, 2015b; Charles et al, 2019 and Oxera-CESS, 2016).

For each estimate, I recorded 54 variables. These included details of the design (e.g. type of study, intervention and product), the outcome measures used (e.g. search or switching, self-reported or not) and the estimation (e.g. specification, sample size, standard error). The full list of variables can be found in Appendix I.

[4] Several papers tested other types interventions as well, these are not discussed in this review.

c. Analytical methods

I treated the qualitative and the quantitative studies separately throughout the analysis. I first reviewed the qualitative papers and drew out the most important / common themes. For the quantitative papers, I performed three types of analysis on the extracted dataset. I set out these three methodologies in more detail below. Finally, I reviewed the findings of the quantitative papers that did not include a comparable percentage point estimate and assessed whether they alter the conclusions drawn from the data analysis.

Calculation of averages

I calculated the average impact of interventions, its pooled standard errors and confidence intervals using the metan command in Stata 16. I used two different weighting methods: the inverse of the number of estimates (i) by paper and (ii) by nudge. This allows me to account for the fact that the number of nudges tested in a paper varies between one and twelve, and the number of estimates per paper goes from one to seventy-two. I performed this analysis for all observations and also excluding the less reliable estimates. These are estimates where the causal relationship between the intervention and the change in the proportion of those who search or switch was not established or estimates that rely on self-reported measures. An example is the *ex post* evaluation in LECG (2008) where they compared survey responses before and after the intervention without controlling for other changes.

There are nine studies that reported significance levels but not the standard error for at least some observations in the final dataset of comparable observations. In order to be able to obtain pooled standard errors I calculated the minimum or the maximum t-value from the significance level and used these to obtain the largest or the smallest possible standard error. For example, if a paper reported that an estimate was significant at 5% (but not the standard error), I took the corresponding t-value of 1.96 and divided the estimate with it to obtain the maximum value of the standard error. Similarly, if a paper reported that an estimate was not significant at 10%, I took the corresponding t-value of 1.645 and divided the estimate with it to obtain the minimum value of the standard error. In other words, I used the upper bound of the standard error where the observation was reported to be significant and the lower bound when it was insignificant (and no standard errors were provided). A few observations did not have a corresponding standard error, p-value or significance level – these are excluded from the analysis.

I calculated the average impact overall, by study design, by product, by type of nudge and outcome measure. As described below in section IV in more detail, there is a considerable difference between the estimated impact depending on the design of the study. To further explore this, I calculated averages by product, by type of nudge and outcome measure separately for different study designs.

Regression analysis

As a cross-check on the previous analysis, I run univariate and multivariate OLS regressions with dummies included for study design, product, type of nudge and outcome measure, using robust standard errors clustered by paper. Note, however, that variation across the different dimensions is often limited and as such, does not allow us to fully isolate the impact of these features. For example, interventions in cash savings have only been tested in field experiments, while only online lab experiments have looked at nudges to induce search or switching in personal loans.

Best estimate analysis

Given the large differences in the number of estimates per nudge and per paper, I run a further analysis that narrows down the set of observations to the “best” estimates. This does not mean the highest value, instead, it is the estimate that appears to be the most representative given the design and estimation techniques. This analysis is necessarily subjective but it provides a useful check on the results of the quantitative analysis that includes all observations.[5]

For this analysis, I selected one estimate for each nudge, product, country and outcome measure combination. That is, if a nudge was tested in more product or geographical markets, I kept one estimate for each. Similarly, if the study reports the impact on both search and switching outcome measures, I kept an observation for both. If there was more than one search or switching measure, I kept all of them if they were distinct categories but selected only one if they overlapped. For example, Adams et al (2021) reports both 'any switching' and 'internal switching' – in this case, I kept the estimate for any switching as it includes internal switching. On the other hand, Hunt et al (2015) reported the impact on full (external) switching, internal switching and inactivity, which are distinct categories so I kept an estimate for each.

If there were several estimates using different specifications for a unique combination of nudge, product, country and outcome measure, I selected the one I consider to be the most representative. For this judgement, I checked what the authors included as their main result, whether they used control variables and whether the estimate was comparable with those from other papers (i.e. whether it expressed the change in percentage points). When this guidance was insufficient to make a decision, I selected an estimate randomly and checked whether it was materially different from the estimates in other specifications. In all cases, the differences were immaterial.

This selection process narrowed down the dataset to 149 observations, for which I calculated averages by outcome measure, design, product and type of nudge. I also calculated confidence intervals for the individual estimates using the standard errors. For estimates where the standard error was missing, I used the minimum t-value (obtained from the significance level reported as explained above) to get the highest value of the standard error. Where that was not available, I used the maximum of the t-value to get the lowest value of the standard error. Standard errors were missing for 61 observations: for 25 of these (that were reported to be significant at 1 or 5%) I could calculate the upper bound and for 36 (that were reported to be insignificant at 5 or 10%) I could calculate the lower bound of the standard error and thus the confidence interval.

IV. Overview of the sample and results

In this section, I first set out the features of the studies and interventions covered in order to obtain an overview of what is included in the analysis. In subsection b, I summarise the findings of the qualitative studies, drawing out common themes and lessons. Subsection c sets out the findings of the quantitative review. Finally, I consider the issue of publication bias in this context in subsection d.

a. Overview of studies and nudges covered

Study characteristics

Table 3 shows a morphological box of the 33 studies included.

In terms of study design, the final set of papers contains qualitative analyses, laboratory and field experiments and *ex post* data analyses. Qualitative studies include focus groups, interviews and consumer surveys. Some of these surveys are carried out on a large sample of consumers but I nevertheless included them among the qualitative studies given that their other features are more similar to these than to those of other categories. Regarding the laboratory experiments, it is worth noting that they are all online; respondents did not have to show up in person in a laboratory. This

[5] I considered performing this analysis by selecting the highest estimate for each nudge. However, for completeness, some papers report results for specifications that they do not view realistic. Including these would bias the results but selecting the highest reasonable estimate on a case by case basis would no longer be objective, and therefore it would not be a superior methodology to the one I used.

method has become popular given that it allows the researcher to reach out to a large number of participants at lower costs. The field experiments are all randomised controlled trials (RCTs) but in some cases participation was voluntary and/or the outcomes were measured through a survey. Finally, the *ex post* analyses include two evaluations and two studies that took existing datasets and used them to analyse the impact of some change that happened (without specifically designing the intervention or the data generation for the purposes of the analysis).

The majority of the studies originate from the UK but there are a few from the US, the EU (studies that cover several countries with coordination at the EU level), Ireland, Germany and Mexico. The reason why the UK has the most studies is because its regulators and governing bodies have been at the forefront of applying behavioural research in competition analyses that often assess search and switching. The Financial Conduct Authority (FCA) issued 15 publications that passed all the inclusion criteria, but there are also studies from the Competition & Markets Authority (CMA), its predecessor, the Office of Fair Trading (OFT), from Pension Wise and The Money & Pensions Service. Other authorities that carried out relevant research are the European Commission (EC), the Bureau of Consumer Financial Protection (BCFP) in the US, the Central Bank of Ireland and the Competition, Consumer Protection Commission (CCPC) in Ireland and the Mexican Banking Commission (CNBV). The papers with no involvement of any authority are mostly academic papers from the US.

More than half of the papers where an authority was involved carried out the research with the purpose of testing possible remedies for already identified problems. All of these studies belong to the FCA or the CMA. Others used research to explore issues and solutions but without having done a full analysis of market failures. As already mentioned, only two papers evaluated the impact of an intervention that had been put in place.

Over two-thirds of the studies drew samples from users of the product in question (in one case, potential users), while others relied on a nationally representative sample or grown-up population. Some papers restricted their sample to certain groups of consumers (e.g. those nearing retirement for pensions, those close to automatic reenrolment for insurance) but no study used an artificially restricted set of subjects (such as students).

Six studies reported some form of search or switching measure but they were not specifically designed to assess the impact on these. For instance, all of the papers prepared for the EC looked at the proportion of consumers who choose the right product (and the impact of an intervention on this proportion) and measures of search (e.g. how many products the consumer looked at) are only described as secondary results. Again, it is worth bearing in mind these differences in design when assessing the overall impact.

Nudge characteristics

Table 4 shows a morphological box of the characteristics of the 95 nudges covered in the 24 quantitative papers included in the review. These nudges were implemented in a number of different retail financial markets, such as current accounts, cash savings, insurance, pensions, credit cards, personal loans, currency transfer services, mortgages and retail investments. Insurance includes add-on, car rental, home, contents, health, prescription drug, motor and pet insurance, and also extended warranties.

The impact of roughly half of the nudges was measured on switching metrics, about a quarter on search metrics and another quarter on both. About 60% of those nudges where significance is reported have at least one significant estimate.

Table 4 also shows the number of nudges per type, using the following categories.

- **Reminders**: simply remind the consumer of an upcoming or a recent event, e.g. rate decrease on cash savings, annual renewal of insurance policy, without any new information content.

- **Disclosures**: general (non-personalised) information about the product or its features, including fee structure but excluding specific fees applied or actual fees paid by the consumer.
- **Simplifications**: simplification of communication that may result in more succinct, shorter text or simpler language.
- **Increases in ease and convenience**: changes that make it easier for the consumer to switch or to search by removing some of the administrative burden of these.
- **Structural changes**: changes in the structure of the decision-making environment, e.g. in the order or prominence of options, but without providing new information.
- **Informational**: providing some information beyond the ones covered in previous categories. Informational nudges could also include elements of the others, e.g. providing extra information in a reminder.

The first four categories (reminders, disclosures, simplifications and increases in ease and convenience) are based on Sunstein (2014). However, his list of nudges is not exhaustive and the papers I reviewed included a number of interventions that were different in nature. I thus added two new categories: structural changes and informational nudges, as per the definitions above. Structural changes can be major such as introducing time limitation when the consumer makes a decision or introducing add-on products at different points during the sales process, or minor such as changing the colour of the paper on which information is shown or presenting annual prices instead of monthly. Informational nudges include all interventions where the consumer is presented with some extra piece of information. Out of the 95 nudges covered in the papers, I classified 67 as informational. Note that a reminder that includes, for example, extra information on the potential gains from switching is classified as an informational nudge. Similarly, disclosures that also include personalised price information are treated as informational nudges.

Given that there are a large number of informational nudges, it would have been useful to split them into further distinct categories. However, I was unable to do this as there are many different elements of informational nudges that are used and combined in various ways in the interventions. Table 5 lists these features and the number and proportion of informational nudges that apply them. The most common features are including a call to action, some text encouraging the consumer to shop around or to switch, including a question, information about the availability of independent advice and estimates of potential savings or losses.

b. Findings of the qualitative studies

All the nine qualitative studies are from the UK or Ireland and all of them are commissioned by regulators. They test interventions in cash savings, current accounts, mortgages and payday loans through interviews, focus groups and surveys. They mostly cover three types of interventions: informational nudges, simplifications and increases in ease and convenience. One exception is in Savanta ComRes (2020) which also explores consumers’ views on a default intervention: being automatically booked into an appointment about switching before the initial fixed rate expires on a mortgage. No other research (including the quantitative studies) tested any form of default intervention, which is somewhat surprising, given the popularity of defaults in other policy areas.

The primary purpose of these qualitative studies is to explore consumers’ reactions to a nudge and to identify features that are more likely to make them work. Overall, they suggest that communications need to be clear and standardised, include a graphical representation and personalised information on the (financial) benefits of search and switching, as well as information about the process itself. Consumers are in general of the view that there is little to gain by shopping around and switching for financial products and they consider the process to be cumbersome. As a result, nudges that highlight potential savings for that particular consumer (rather than in general) and help with the process receive the most favourable feedback in these studies.

The review of these papers also reveals a number of lessons for the practical implementation of nudges.

First, it is difficult to find a channel that can grab consumers' attention. Consumers view pop-ups as spam (Archer et al, 2014), question the authenticity of text messages (Collaborate Research, 2017), miss prompts that are embedded into annual statements (Optimisa Research, 2016) and rarely read standalone letters (Collaborate Research, 2017). Online and mobile app notifications were suggested in a couple of interviews (Optimisa Research, 2016 and Collaborate Research, 2017) but there is less past experience with these and it needs to be explored whether they would indeed work in practice, including once these channels become commonplace.

Second, consumers do not like the idea of introducing new tools, such as a standalone comparison tool on quality of banks (Optimisa Research, 2016) or separate rate cards in addition to summary boxes for cash savings accounts (Worton-Reynolds, 2015), and say that they would not want to use them. Given this, prompts that direct consumers to new tools are less likely to be effective.

Third, while most studies find that new communications work best when they arrive from the consumers' own provider, they also find that providers telling their customers to switch away causes confusion (Worton-Reynolds, 2015; Worton et al, 2016 and Collaborate Research, 2017). This suggests that nudging consumers to switch products within provider is more likely to work than nudging them to switch away to another provider. More internal switching could help the problem of price discrimination between engaged and disengaged consumers, but it is less effective in increasing the competitive pressure on firms.

Finally, while in almost all studies a large proportion (20-60%) of respondents indicate that a nudge would encourage them to search or switch,[6] it appears that any prompt is more likely to work for those who are already considering switching (CBI, 2017; Savanta ComRes, 2020) and will not change the behaviour of those who are otherwise reticent to switch (Collaborate Research, 2017).

In sum, qualitative research on which nudges may make consumers search and switch sets out features that could increase their efficacy but provides little information on the actual impact of these. There are indications that implementers will face a number of practical constraints.

c. Quantitative review

The findings below are based on the 18 papers with comparable quantitative estimates (as shown in Table 2) using the three different methods (calculation of averages, regression analysis and the best estimate analysis) described above in section III. The detailed results are shown in appendices II to IV.

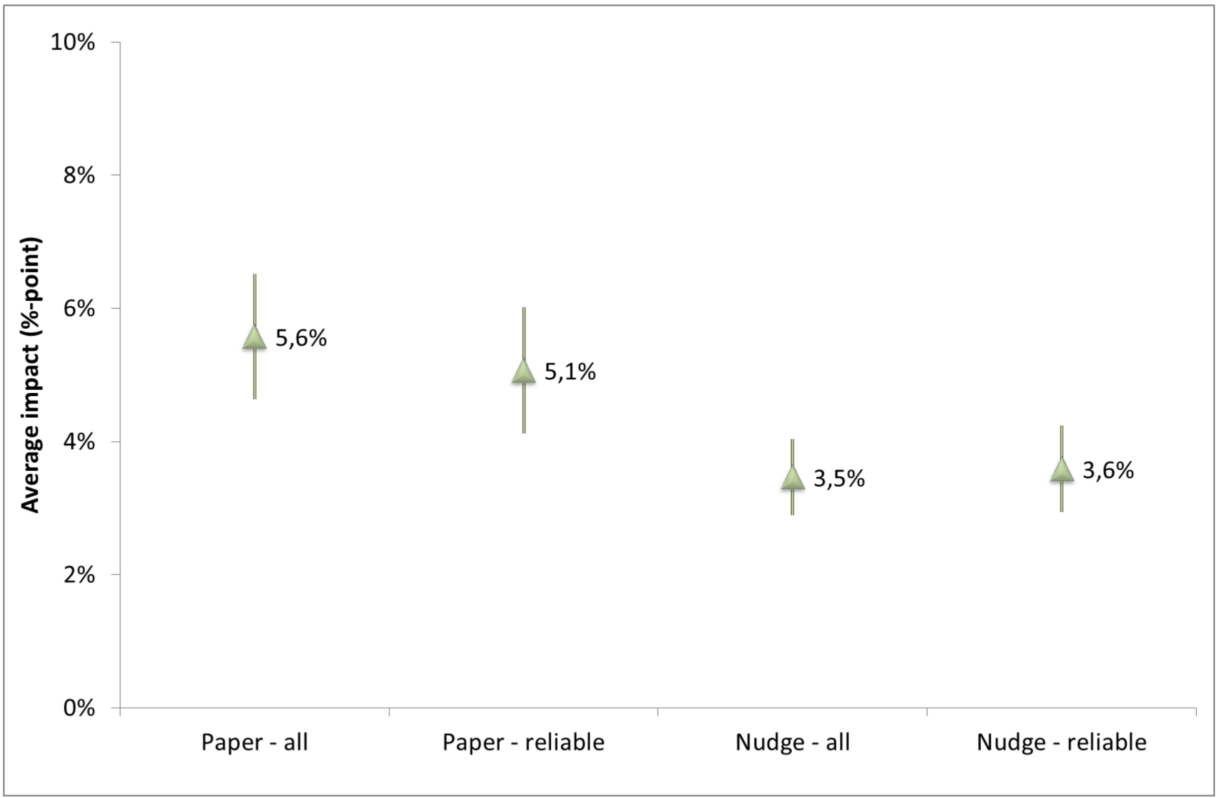
The overall average impact of nudge interventions is a 4-6 percentage point increase in search / switching

Using comparable estimates from online lab and field experiments and *ex post* analyses, I find that the average impact of nudge interventions that aim to increase consumer search and switching is between 4 and 6 percentage points. As shown on Figure 1 below, this varies slightly depending on whether the estimates are weighted using the inverse of the number of estimates by paper or by nudge, and also whether less reliable estimates (such as those that come from non-causal analyses or use self-reported outcome measures) are included or not.[7]

[6] An outlier is CBI (2017) in which only 1-2% of those who never switched say an intervention would encourage them to do so. It is not clear why their results are so significantly different from those in other studies.

[7] In the final dataset, there are only four papers that tested one nudge only. The remaining thirteen papers test between two and twelve nudges, with the number of estimates per nudge varying between one and twenty-eight. Nudges with more estimates get a higher weight when the inverse of the number of estimates per nudge is used, relative to when the inverse of the number of estimates per paper is used. The former results in a lower overall average than the latter, suggesting that nudges with more estimates have a smaller impact on average than the ones with fewer estimates.

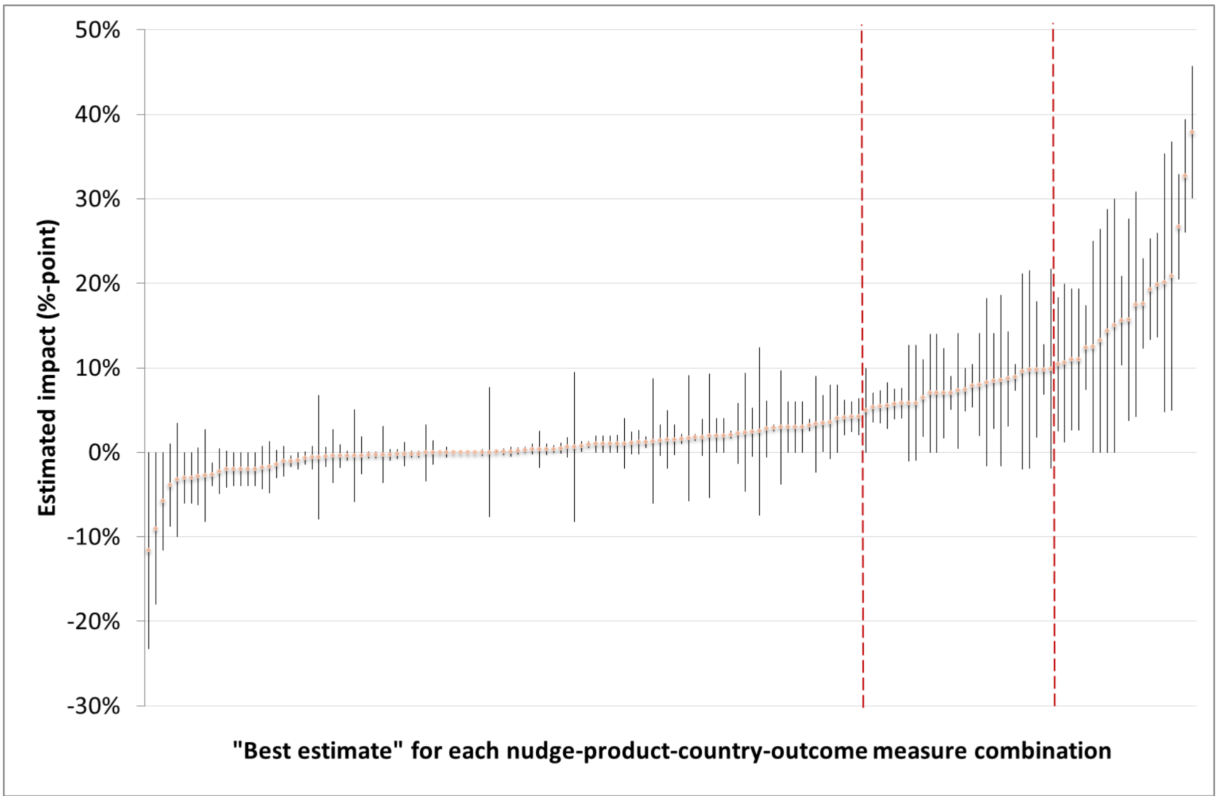
Figure 1: Overall average impact of nudge interventions



Notes: (i) paper and nudge indicate results weighted by the inverse of the number of estimates in the paper / for the nudge; (ii) all indicates that all estimates are included, reliable indicates that less reliable (non-causal and self-reported) estimates are excluded; (iii) sample size all: 446, sample size reliable: 393, (iv) vertical lines show confidence intervals at 95% significance level

These results are confirmed in the best estimate analysis that finds a 4.2 percentage point average impact for all estimates (149 observations) and a 5 percentage point impact for reliable estimates only (108 observations). Only a third of this set observations show an estimated impact above 5 percentage points and less than 15% generate one above 10 percentage points. This is shown on Figure 2 below. Observations with higher estimated impact often have large confidence intervals but this is partly a result of the methodology – where standard errors were missing, I calculated the upper bound of the confidence interval from the significance where it was possible.

Figure 2: Distribution of best estimates

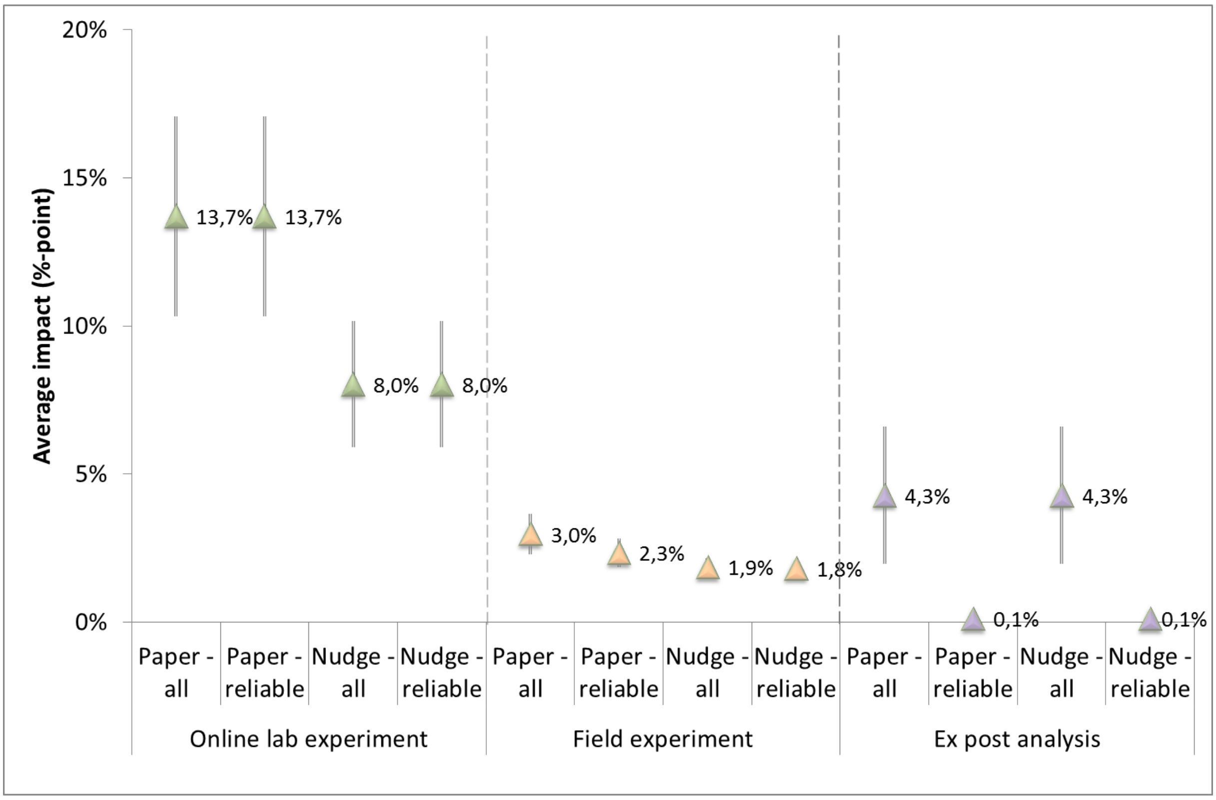


Notes: (i) sample size 148; (ii) one outlier is excluded; (iii) red vertical lines indicate 5 and 10 percentage point impact; (iv) vertical lines per point estimate show confidence intervals at 95% significance level

Online lab experiments show much higher impact than field experiments and ex post analyses

However, the overall average is likely to overestimate the real impact of nudges on search and switching. When looking at the results by study design, I find that online lab experiments show a four times higher impact than field experiments, which are in turn higher than the results of *ex post* analyses once less reliable estimates are excluded. In particular, the estimated average increase in search and switching is between 8 and 14 percentage points in the lab, 2-3 percentage points in the field and basically zero in *ex post* analyses. This is shown on Figure 3 below.

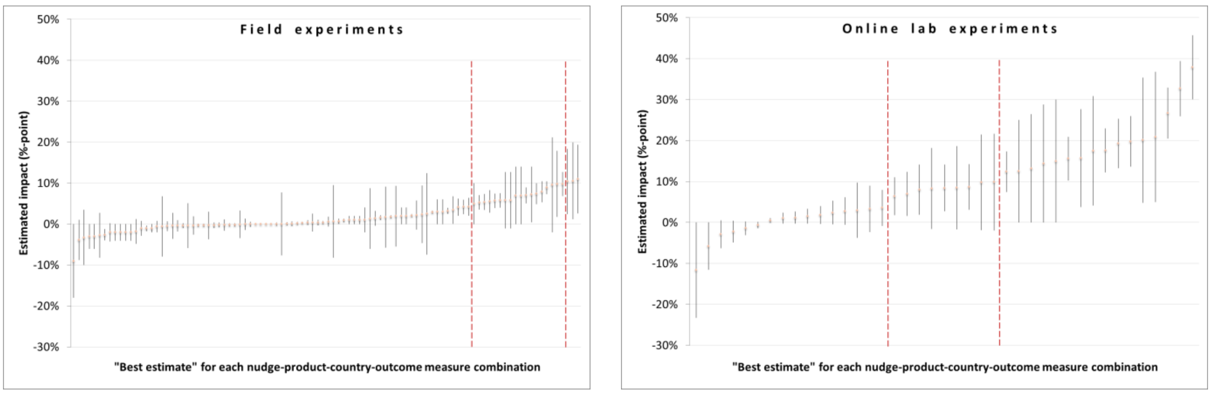
Figure 3: Average impact of nudge intervention by study design



Notes: (i) paper and nudge indicate results weighted by the inverse of the number of estimates in the paper / for the nudge; (ii) all indicates that all estimates are included, reliable indicates that less reliable (non-causal and self-reported) estimates are excluded; (iii) sample size online lab experiments: all 112, reliable 112; field experiments: all 241, reliable 192, *ex post* analysis: all 93, reliable 89, (iv) vertical lines show confidence intervals at 95% significance level

The above results are confirmed in the best estimate analysis which shows an 8 percentage point difference between the average impact found in online lab experiments and in field experiments. When looking at the distribution of best estimates separately by study design, I find that only 3% of the estimates are above 10 percentage points in field experiments, compared to 40% in lab experiments. Similarly, about one fifth of the estimates is above 5 percentage points in field experiments, whereas over 60% of lab estimates are higher than 5 percentage points. These are shown on Figure 4 below that splits the data points from Figure 2 by study design. Figure 4 also highlights that the confidence intervals tend to be larger for estimates in lab experiments.

Figure 4: Distribution of best estimates separately for field and online lab experiments



Notes: (i) sample size 98 for field experiments and 41 for online lab experiments; (ii) one outlier is excluded (online lab experiment); (iii) red vertical lines indicate 5 and 10 percentage point impact; (iv) vertical lines per point estimate show confidence intervals at 95% significance level

I tested the significance of the difference between lab and field experiments in the regression analysis by introducing dummies for each study design and using field experiments as the base category. This analysis also found that the coefficient of the online lab experiment dummy is 10 percentage points without and with controlling for the type of nudge, the outcome measure and product (that is, online lab experiments estimate a 10 percentage point higher impact than field experiments). These coefficients are significant at 0.1% and robust to excluding less reliable estimates.

What explains this difference? There is a criticism in the literature that the laboratory setup is unrealistic. For example, real economic decisions take longer than the time available during a lab experiment (Reiley, 2015), participants receive a complete description of the rules in lab experiments whereas social interactions can lead to very different patterns of behaviour (Erev-Greiner, 2015), and the stakes of the game and the cost of the effort may not reflect those of real economic decisions and as such alter how participants behave (Levitt-List, 2007a). In addition, there is a concern around selection effect: those who volunteer to take part are likely to be different from those who do not and non-random selection of participants can bias results (Levitt-List, 2007b). These criticisms led to a view that questions the external validity of lab experiments, that is, whether their results apply in real world situations.

These concerns are relevant for the experiments at hand. Any metric of shopping around or switching inevitably requires less time and effort in a laboratory environment than what it takes to actually shop around for financial products or to switch between providers. Some of the elements of consumer decision making (such as brand loyalty), cannot necessarily be replicated in the lab. The sample is usually drawn from large panels of market research companies and those who subscribe to these may have more time than those who do not, the latter group being less likely to have time to search for the best deals in reality. It is thus likely that what we observe here is simply a demonstration of the above mentioned criticisms.

However, the criticism only concerns the quantitative results – the external validity of qualitative findings of lab experiments is generally not in doubt (Iscenko et al, 2014; Charness, 2015; Gneezy-ReyBiel, 2015; Levitt-List, 2007a).[8] This is in line with how some of the lab experiments included in this review position their results: their main findings relate to which intervention had the largest impact (e.g. Burke et al, 2020) and some specifically argue that the key outcome is the *ranking* of the different treatments, not necessarily the magnitudes of differences between them (Oxera-CESS, 2016).[9]

Note also that the cause of low levels of consumer engagement is often inertia, which is hard to capture in the lab (Iscenko et al, 2014). Participants of a lab experiment are there to make a decision and as such, important barriers to acting on information are less pronounced than in the real world. If these barriers explain low levels of search and switching and they are not replicated in the lab, one would expect that nudges that are designed to trigger action would show higher impact in the lab than in the field.[10]

Given all of the above, I believe that the results of field experiments provide us with a more precise estimate of the impact of nudge interventions on search and switching than those of the online lab experiments. There remains, however, a difference between the average impacts found in field experiments and in *ex post* analyses that needs to be explained.

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[8] Kessler and Vesterlund argue that for most laboratory studies it is only relevant to ask whether the qualitative results are externally valid and for this, it is sufficient if the observed relationship is monotonic and does not change direction (Kessler-Vesterlund, 2015). Levitt and List considers that at least we need intuition whether an empirical estimate from the lab is biased upwards or downwards (Levitt-List, 2007a).

[9] Unfortunately, these interventions have not been tested in the field and as a result, it is not possible to say whether the ranking obtained from lab experiments is confirmed in the field.

[10] However, null results could be relied on: if an intervention does not have an impact in the lab among an incentivised sample of consumers who are able to pay full attention, it is unlikely that the intervention will be effective in the field (Lunn-Choisdealbha, 2018).

The four papers that look at the impact of an intervention *ex post* can be summarised as follows.

- LECG (2008) carried out an *ex post* evaluation for the OFT about the impact of its intervention on extended warranties. They found that the proportion of consumers who considered alternatives increased from 4% to 15% and the proportion who got extended warranties from the point of sale provider decreased from 82% to 68%. However, these are comparisons of consumer survey responses from before and after the intervention without controlling for further changes in the environment. In fact, the research states that about two-thirds of the change in the proportion of those who obtained extended warranty from the point of sale provider was because many customers got it from manufacturers for free.
- Bhattacharya et al (2012) investigated the impact of a provider offering automated free investment advice to its customers and found that less than 5% of those who received the offer accepted it (search) but even they hardly followed it afterwards (switching). This paper does not include a comparable percentage point estimate of the impact of offering the advice and it is thus not part of the quantitative review.
- Hunt et al (2015) looked at the impact of the introduction of annual summaries, mobile banking apps and text alerts for current accounts. They found that annual summaries had no impact on switching (causal analysis). Signing up to mobile banking apps and text alerts was positively correlated with inactivity (2.6% and 2.4%) and with internal switching (2.7% and no effect) but *negatively* correlated with full switching (-0.9% and -0.2%). Note, however, that these are not causal estimates and the paper did not report the corresponding significance levels or p-values so they are not included in the quantitative review.
- Charles et al (2019) evaluated the FCA's intervention in insurance that required that insurers show last year's premium on renewal notices and include some text encouraging consumers to shop around. They found that self-reported shopping around was 3-4 percentage points larger after the intervention (non-causal analysis) and switching and negotiating increased by 1.2 percentage points in pet insurance, by 1.3-1.7 percentage points in motor insurance but *decreased* by 0.8-3.0 percentage points in home insurance (causal analysis).

The main lesson emerging from this brief summary is that even the few available *ex post* analyses suffer from methodological issues, such as not being able to establish causality between the intervention and the observed changes. Secondly, the observed impact varies by outcome measure and/or products and due to the adverse impact in some cases, the average impact is close to zero. This does not necessarily mean that nudge interventions should be abandoned as *ex post* evaluations show that they have no impact – instead, it indicates that there is more variation to be explored in how they affect outcomes.[11] Note also that there may be compliance concerns. For example, the estimates in Charles et al (2019) are higher assuming full, rather than actual compliance level.

Taken all this together, I consider that the average impact obtained in field experiments (2-3 percentage points) is likely to be the currently most reliable estimate of the impact of nudge interventions on search and switching.

Certain types of nudges appear to work better than others

Figure 5 below shows the number of observations and the average estimated impact by type of nudge and study design. It also shows the number of observations once less reliable (i.e. non-causal and self-reported) estimates are excluded. Detailed results including pooled standard errors and confidence intervals are shown in Appendix II.

[11] In addition, the introduction of nudges may also impact the suppliers' response. For example, Charles et al (2019) found that despite the varying effect on switching the FCA's intervention was still beneficial, largely because insurers did not increase their premiums by as much as they would have without having to show last year's premium in the renewal letter.

Figure 5: Number of observations and average estimated impact (percentage points) by type of nudge and study design

	Disclosure	Reminder	Simplification	Informational	Inc. in ease and conv.	Structural change
Ex post analysis	2 (0) 13% (-)			91 (89) No impact		
Field experiment	6 No impact	31 (28) 2-3%	19 (15) 2-4%	174 (132) 1-2%	6 9%	5 No impact
Online lab experiment				93 5-9%	1 8%	18 21%

Notes: (i) first number indicates the number of estimates in the category; (ii) number in parentheses indicates the number of estimates when less reliable ones are excluded; (iii) second row shows the average estimated impact in percentage points

As shown on Figure 5, the average estimated impact varies somewhat by type of nudge.

Pure **disclosures** such as sending a glossary of key terms to consumers have no impact (Adams et al, 2015b). Displaying leaflets, the price and the duration of extended warranties next to the price of the primary product (LECG, 2008) may have an impact but causality was not established in the analysis.

Reminders and simplifications were only tested in field experiments and they show a small (but statistically significant) average impact of 2-4 percentage points (ranging from 0 to 10 percentage points). These include reminders about rates decreasing on cash savings accounts (Adams et al, 2015a and Adams et al, 2021) and about the renewal of insurance policies (Adams et al, 2015b), simplifying insurance renewal letters by using bullet points or simpler language (Adams et al, 2015b), and replacing retirement “wake-up” packs with a one pager containing key information about next steps (Glazebrook et al, 2017).

Informational nudges, which account for the vast majority of the tested interventions, show on average no impact in *ex post* analyses due to adverse effects in some cases (see above). Nine studies looked at a number of different informational nudges in field experiments, and the average estimated impact is a 1-2 percentage point increase in search and switching with very few observations over 10 percentage points. Online lab experiments (Oxera-CESS, 2016 and Suter et al, 2019) show a somewhat higher impact on average (5-9 percentage points) but as discussed above, this is likely to be inflated due to the specific design elements of these. For instance, search is measured through clicks in the online laboratory environment, which requires less effort than shopping around for a financial product in reality. Note also that one of these experiments (Suter et al, 2019) was not designed to measure search specifically.

Looking at the features of informational nudges that led to a relatively larger (higher than 5 percentage point) increase in search and switching in field experiments, I find that they contain some kind of number that makes it clear to the consumer what is at stake. Examples are potential gains / losses from switching / not switching (Adams et al, 2015a and Marzilli Ericson et al, 2017), indicating how much the consumer paid last year (Adams et al, 2015b and Accent Research, 2018) or specifying the lowest cost alternative (Kling et al, 2012). The majority of these contain personalised price information. Similar findings emerge from the online lab experiments: nudges with graphical illustrations of personalised estimates (Oxera-CESS, 2016) and cost summaries with representative examples or based on expected usage are the ones that result in the highest impact (Suter et al, 2019). Note, however, that these are qualitative observations – the regression analysis does not show a statistically significant impact of building in a (personalised) number in the nudge.

The estimated impact of nudges that fall into the **increase in ease and convenience** category was reported in three papers.[12] Adams et al (2021) investigated the impact of sending a letter to cash savings customers with a tear-off return switching form pre-filled for a switch to the best internal rate and a prepaid, addressed envelope and found a 9 percentage point increase in switching. Note, however, that most of it is internal, i.e. switching to another product of the same provider. Farghly et al (2020) tested an intervention whereby when customers call their pension provider, the call handler provided information about Pension Wise (an independent advice service) and offered to book an appointment with them, or transferred the line to Pension Wise to book the appointment. They found that 13-14% booked and 11% attended an appointment compared to 3% in the control group. Finally, Duke et al (2014) tested the impact of making it easier to compare information about add-on insurance offers in a lab experiment (whereby in one treatment all the viewed offers were displayed on the screen and in another respondents had to switch between pages to see the standalone offers) and found that this led to a 8 percentage point decrease in the proportion of those who bought the first offer seen. While these interventions are very different in nature, the common feature is that they offer something that makes the consumer's journey easier by reducing some of the administrative burden. And although the sample is small, the results appear consistent in their magnitude, even across different study designs.

Finally, examples of **structural changes** show very different impacts but this is not only due to study design. The field experiment in Glazebrook et al (2017) tested a minor structural change of trying to draw attention to Pension Wise in the retirement wake-up pack by placing it in the front or printing it on orange paper but found no effect. Similarly, Suter et al (2017) changed the relative prominence of the first offer and the option to compare further products and found no statistically significant effects (apart from in one subgroup). A similar minor change of presenting insurance prices on an annual basis, rather than in monthly instalments led to a 7 percentage point decrease in the proportion of those who bought the first offer (Duke et al, 2014). However, there are also two major changes that were tested in online lab experiments and these had a significantly higher effect. Duke et al (2014) designed an experiment that allowed them to compare the impact of introducing add-on insurance upfront vs. only at the point-of-sale of the primary product. They found that over 70% of participants only viewed one insurance when it is introduced at the later stage, compared to less than 20% when it is introduced upfront; and that 65% purchased the first insurance viewed compared to 17% - a difference of around 50 percentage points. Another major change is to introduce time limitation on reviewing information and choosing an insurance product, as in Suter et al (2017), which led to a 33 percentage point decrease in the proportion of respondents who looked at alternatives. While the above mentioned caveats on lab experiments apply here as well, and thus it is likely that these numbers are somewhat inflated, it still seems safe to conclude that major changes in the structure of the choice architecture can have a relatively large impact on consumer search.

In sum, the above analysis shows that disclosures are unsuccessful in increasing search and switching, informational nudges have a 1-2 percentage point impact, reminders and simplifications have a 2-4 percentage point impact, and increases in ease and convenience and major structural changes are the most effective in altering consumer behaviour. These results are broadly confirmed in the best estimate and in the regression analysis.

The above analysis revealed that nudges that rely on changing the information provided appear to have a smaller impact than nudges that change the choice architecture more profoundly. To test this idea further, I introduced a new delineation: all nudges that provide, simplify or highlight information

[12] Two further papers reported on the impact of nudges that fell into the increases in ease and convenience category. However, these either did not provide any information on the significance of the estimate (Hunt et al, 2015) or did not use a comparable outcome measure showing the percentage point impact (Burke et al, 2020).

(including reminders) are classified as *informational,* and the remaining are *structural.* Disclosures, reminders, simplifications and informational nudges from the original categorisation are now all classified as informational, and increases in ease and convenience and structural changes are now classified as structural. The only exceptions are nudges that change the prominence of information – previously these were in the structural changes category (as they did not provide any new information) but they fall into the informational group under the new classification (as they highlight information).[13]

Under this new classification ten nudges are considered to be structural, seven of which have estimates that are comparable with the rest and are provided with information on their significance (and as such can be included in the analysis). These are the following:

- Sending a letter with a tear-off return switching form pre-filled for a switch to the best internal rate and a prepaid, addressed envelope (field experiment, Adams et al, 2021);
- When customers call their pension provider, the call handler provides information about Pension Wise and offers to book an appointment (field experiment, Farghly et al, 2020);
- When customers call their pension provider, the call handler provides information about Pension Wise and offers to transfer the customer to Pension Wise where they can book an appointment (field experiment, Farghly et al, 2020);
- Adding time limitation on reviewing information and choosing (online lab experiment, Suter et al, 2017);
- Introducing add-on insurance upfront vs. only at the point-of-sale (online lab experiment, Duke et al, 2014);
- Making it easier to find information about standalone insurance products by allowing to see them on the same screen (online lab experiment, Duke et al, 2014);
- Showing yearly prices instead of monthly (online lab experiment, Duke et al, 2014);.

In contrast, there are 74 nudges that fall into the combined informational category.

The difference between the average impact of the two categories is 14-15 percentage points with informational nudges averaging around 2-3 percentage points and structural nudges having an average impact of 17 percentage points. This is confirmed in the regression analysis that controls for study design, product and outcome measure (search vs. switching). The detailed results are shown in Appendix V. Note, however, that four out of seven nudges in the structural category were tested in a laboratory environment so we need to treat the quantitative results with caution. On a reduced sample of only field experiments, the difference between the impact on search and switching of structural and informational nudges is lower (6-7 percentage points) but remains highly significant.

Overall, we can conclude that nudges that change the choice architecture more profoundly have a higher impact on search and switching than nudges that only provide, simplify or highlight information.

No clear evidence that nudge interventions work significantly better for certain products than for others

There is no clear evidence that nudge interventions aiming to increase consumer search or switching would work significantly better for certain products than for others. On average, there does not appear to be any impact on users of current accounts, credit cards or mortgages (although the latter is only one observation). Interventions in insurance and pensions have a higher impact when tested in online lab experiments but lower (2-4 percentage points) in field experiments. Nudging people to shop around for personal loans was only tested in a lab experiment but in several European member states and the interventions had high impact in some but no impact in others. Finally, while interventions in cash savings appear to have a robust impact of 3-4 percentage points on average, a large part of this

[13] This change in the categorisations affected three nudges that were previously under the structural heading but are now informational: (i) making the option of comparison visually less prominent than the first offer (Suter et al, 2017), (ii) placing the information about Pension Wise on the top of the wake-up pack (Glazebrook et al, 2017), and (iii) printing the information about Pension Wise on orange paper (Glazebrook et al, 2017).

is internal switching, i.e. when the consumer moves to a different product with the same provider. Internal switching does not bring the same benefits for competition as when consumers switch between different providers. This is confirmed in the regression analysis: only the coefficient of the cash savings dummy is significant (relative to current accounts) when further controls are included.

There is an indication that it is easier to nudge people to shop around than to switch

In terms of outcome measures, there is an indication that it is easier to nudge people to shop around than to switch. Simple means of estimated impacts are 4-7 percentage points higher for outcome measures of search than for outcome measures of switching. This is shown in Table 6.

However, field experiments measure the impact of nudges more often on switching (197 observations out of 241), whereas online lab experiments tend to use outcome measures of search (97 out of 112 observations) and it is possible that the observed difference is due to differences in study design, rather than in outcome measure. To further investigate this, Table 7 sets out the difference in the estimated average impact between search and switching outcome measures separately for different study designs.

Table 7 shows that the difference in average estimated impact on search and switching is not robust to different weighting regimes for online lab experiments. It is more consistent in field experiments, where I estimate a 2-4 percentage point difference. The results of the *ex post* analyses should be handled with caution as they only contain four observations that measure the impact on search and all of these are non-causal estimates.

These results are confirmed in the regression analysis: the difference between the average impact on search and switching measures becomes insignificant if I control for products and study design. When looking at field experiments only, however, I estimate that there is a significant 3-6 percentage point higher impact on search than on switching (see Table 12).

The result that nudges are more effective in inducing search than switching is in line with expectations for two reasons. First, shopping around generally requires less effort from the consumer than switching as it can usually be done online from home and it typically does not involve filling in forms or contacting providers. Second, it is relatively hard to quantify search precisely. Some measures are objective, like the proportion of people visiting a website, but these do not necessarily provide much information about the extent of the search the consumer carried out. Other measures may try to capture the level of shopping around but these tend to be self-reported and as such, less reliable. Note that the two reasons are different in nature: the first suggests a real difference, whereas the second is due to differences in measurement.

If valid, this result is arguably also good news from a competition perspective as search behaviour, while harder to measure, is a better indicator of competitive constraints imposed by consumers than switching. This is because switching without shopping around will not incentivise firms to offer better deals and because effective search followed by a decision *not* to switch can still be pro-competitive.[14]

Weak evidence that the impact of interventions varies by consumer groups

There is only weak evidence that the impact of interventions varies by consumer groups. Seven studies investigated heterogeneity in the results, including splitting consumers by age, gender, education level, income or by how much they could gain by switching. One clear finding by Adams et al (2015b) is that including last year's premium next to the new premium offered in insurance renewal letters is more effective when consumers face a larger price increase relative to a previous price they paid. The rest of the significant results do not appear to be robust or consistent across studies, and may indeed just be random findings.

[14] I am grateful to Amelia Fletcher for drawing my attention to this point.

The review of quantitative studies with no comparable estimate does not change the above conclusions

Out of the 24 papers with quantitative analysis, six did not include a comparable percentage point estimate and therefore was not part of the calculation of average impacts above. Furthermore, Seira et al (2017) included a description of an additional *ex post* analysis in their appendix, which again did not contain a percentage point estimate.

Four of these analyses found no real impact of the tested interventions. The *ex post* analysis of Bhattacharya et al (2012), described above, did not contain a comparison to a control group, instead, it was a diff-in-diff analysis. However, the overall conclusion is that the intervention (offering unbiased automated advice) had minimal impact on search and switching. Similarly, the field experiment in Keys et al (2016) was inconclusive – as only a very small number of households switched mortgages, they could not establish whether there was any meaningful difference in the treatments. However, it does allow us to draw the conclusion that letters encouraging refinancing were ineffective. Seira et al (2017) found that an intervention of showing competitor prices in the annual statements of credit cards did not lead to any economically or statistically meaningful reduction in credit balances (used as a proxy for switching). Finally, the lab experiment by TNS (2012) found no impact of glossaries and standardised offers, and only a small positive impact of cost summaries with representative examples in current accounts.

The remaining three papers used an absolute number as their outcome measure, such as the number of mortgage lenders contacted (BCFP, 2018) and the number of quotes looked at in currency transfer services (BIT, 2018 and Burke et al, 2020). The estimated change in relative terms varies between 5 and 28%, but the absolute changes are small in all three cases: an increase from 1.6 to 2.0, from 1.8 to 2.1 and from 2.8 to 2.9. As a result, I believe that these do not change the picture drawn from the quantitative analysis.

Summary of quantitative review

My overall conclusion from the quantitative review is that nudge interventions on average increase consumer search and switching by 2-3 percentage points in retail financial markets. Certain types of nudges appear to be more effective (Q1) but there is no clear evidence that they would work better for some products (Q2) or for any consumer groups (Q3). The review also revealed that different study designs lead to significantly different estimates and that online lab experiments are likely to overestimate the real impact of interventions. *Ex post* evaluations and specifically designing interventions so that their causal impact can be measured could help further evidence accumulation.

d. Publication bias

Della-Vigna and Linos (2020) find that a 7 percentage point difference between the average impact of nudge interventions in academic publications and in studies by nudge units suggests the presence of publication bias in academia. Publication bias arises if researchers are less likely to write up and submit for publication analyses with statistically insignificant results, and journals are less likely to accept these papers if they receive them. As a result, a meta-analysis attempting to estimate the average impact will be biased upwards.

While this may be an issue in general, I believe that publication bias for this review is less of a concern, for the following reasons.

First, there are five papers in the dataset that are "purely" academic, that is, were only published in scientific journals without any involvement of authorities. Three of these (Bhattacharya et al, 2012; Keys et al, 2016 and Johnson et al, 2019) find no impact of the interventions tested and one of them (Marzilli Ericson et al, 2017) finds an increase in shopping around but not in switching. The work of Seira et al (2017) was supported by the Mexican Banking Commission, subsequently published in the

American Economic Journal with the main finding of no effect. It is, therefore, unlikely that the results of academic publications are heavily biased upwards.

Second, many of the papers with involvement of an authority are prepared for or by the FCA and the FCA claims to publish the results of all experimental trials it carries out (Smart, 2016). Again, while it is possible that some relevant studies could not be included in the review as they were not published, the indication is that the impact of that is limited.

Finally, even if there is undetected publication bias, it would only strengthen the conclusion that nudge interventions have a limited impact on the proportion of consumers who shop around or switch between products.

V. Summary

Following a systematic literature search, I identified 33 papers that assess the impact of nudges on consumer search and switching in retail financial markets. This set of papers consists of qualitative analyses, lab experiments, field trials and *ex post* data analyses and covers a wide range of retail financial markets in the UK, the US, Mexico and within the European Union. The majority of the papers were prepared by or for a regulator to assess policy options, but there are also some "purely" academic publications.

The review of these papers yields the following main contributions.

First, it demonstrates that specific study designs serve different purposes and contribute to evidence gathering in different ways. Qualitative studies provide us with a list of features that are likely to make nudges more effective and yield a number of practical lessons for the implementation. Online lab experiments are considered to be useful in ranking different interventions but they are likely to overestimate the actual impact of these. While *ex post* evaluations are in principle the most reliable source for assessing effectiveness, unfortunately there are only a few and even these suffer from methodological issues (such as the lack of establishing causality). Currently field experiments appear to be the most reliable source for ascertaining the likely impact of nudge interventions.

Secondly, based on over 400 estimates extracted from the quantitative analyses I estimate that nudge interventions increase consumer search and switching by 2-3 percentage points on average. The most effective nudges appear to be the ones that make the consumer's life easier by taking some of the administrative burden over and the ones that make a major change in the structure of the decision-making environment. Disclosures, reminders, simplifications and nudges that provide some extra information have a smaller impact. In other words, nudges that change the choice architecture more profoundly have a higher impact on search and switching than nudges that only provide, simplify or highlight information. Default interventions, that achieved larger effects in other domains, have not been properly tested for financial products with the aim of inducing more consumer search and switching. There is no clear evidence that nudge interventions would work better for certain products or for certain groups of consumers, but there is an indication that it is easier to nudge people to shop around than to switch.

These results can be used by policy-makers when considering developing and testing nudge interventions to increase consumer search and switching. While nudges may be cost-effective because their implementation is cheap, and they may result in a large change in relative terms (e.g. increasing switching rates by 100% from 1% to 2%), regulators cannot expect them to achieve a major improvement in the level of consumer engagement. Future research will have to focus on what worked on other markets and what other, potentially more paternalistic interventions could policy-makers consider.

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TABLES

Table 1: List of studies included

	Author	Title	Publisher	Study design	Country
1	LECG (2008)	Evaluating the impact of the Supply of Extended Warranties on Domestic Electrical Goods Order 2005	OFT	Ex post data analysis	UK
2	Bhattacharya et al (2012)	Is Unbiased Financial Advice to Retail Investors Sufficient? Answers from a Large Field Study	The Review of Financial Studies	Ex post data analysis	Germany
3	Hunt et al (2015)	OP10 Message received? The impact of annual summaries, text alerts and mobile apps on consumer banking behaviour	FCA	Ex post data analysis	UK
4	Charles et al (2019)	Evaluation Paper 19/1: An evaluation of our general insurance renewal transparency intervention	FCA	Ex post data analysis	UK
5	Kling et al (2012)	Comparison Friction: Experimental Evidence from Medicare Drug Plans	Quarterly Journal of Economics	Field experiment	US
6	Adams et al (2015a)	OP7 Stimulating Interest: Reminding savers to act when rates decrease	FCA	Field experiment	UK
7	Adams et al (2015b)	OP12 Encouraging consumers to act at renewal: Evidence from field trials in the home and motor insurance markets	FCA	Field experiment	UK
8	Keys et al (2016)	Failure to refinance	Journal of Financial Economics	Field experiment	US
9	Glazebrook et al (2017)	Improving engagement with pension decisions: The results from three randomised controlled trials	BIT	Field experiment	UK
10	Marzilli Ericson et al (2017)	Nudging Leads Consumers In Colorado To Shop But Not Switch ACA Marketplace Plans	Health Affairs	Field experiment	US
11	Seira et al (2017)	Are information disclosures effective? Evidence from the credit card market	American Economic Journal	Field experiment	Mexico
12	Accent Research (2018)	Personal and business current account prompt pilot findings	FCA	Field experiment	UK
13	Adams-Ernstsone (2018)	OP38 Testing retirement communications: Waking up to get wise	FCA	Field experiment	UK
14	BCFP (2018)	Know Before You Owe: Mortgage shopping study	BCFP	Field experiment	US
15	Johnson et al (2019)	What's the Catch, Suspicion of Bank Motives and Sluggish Refinancing	The Review of Financial Studies	Field experiment	US
16	Farghly et al (2020)	The Stronger Nudge	BIT	Field experiment	UK
17	Adams et al (2021)	Testing the Effectiveness of Consumer Financial Disclosure, Experimental Evidence from Savings Accounts	Journal of Financial Economics	Field experiment	UK
18	TNS (2012)	Bank Fees Behaviour Study	EC	Online lab experiment	EU
19	Duke et al (2014)	Study into the sales of Add-on General Insurance Products: Experimental consumer research	FCA	Online lab experiment	UK
20	Oxera-CESS (2016)	Increasing consumer engagement in the annuities market: can prompts raise shopping around?	FCA	Online lab experiment	UK
21	Suter et al (2017)	Study on consumers' decision making in insurance services, a behavioural economics perspective	EC	Online lab experiment	EU
22	BIT (2018)	The impact of improved transparency of foreign money transfers for consumers and SMEs	BIT	Online lab experiment	UK
23	Suter et al (2019)	Behavioural study on the digitalisation of the marketing and distance selling of retail financial services	EC	Online lab experiment	EU
24	Burke et al (2020)	OP56 Fair exchange: presenting foreign exchange quotes to improve consumer choice	FCA	Online lab experiment	UK
25	Archer et al (2014)	Research with payday lending customers	CMA	Qualitative	UK
26	Worton-Reynolds (2015)	Cash Savings Remedies	FCA	Qualitative	UK
27	B&A (2016)	Mortgage Holding & Switching, Market Research Findings	CCPC	Qualitative	Ireland
28	Optimisa Research (2016)	Informing the development of communication tools designed to increase consideration of switching among PCA and SME customers	CMA	Qualitative	UK
29	Worton et al (2016)	Cash Savings Switching Box	FCA	Qualitative	UK
30	Central Bank of Ireland (2017)	Mortgage Switching Research	CBI	Qualitative	Ireland
31	Collaborate Research (2017)	Future personal current account prompts and alerts	FCA	Qualitative	UK
32	Decision Technology (2018)	FCA Prompts and Alerts Design: Behavioural Evidence	FCA	Qualitative	UK
33	Savanta ComRes (2020)	Mortgage switching research	FCA	Qualitative	UK

Table 2: Number of nudges and estimates by paper

Paper ID	Paper	Design	Number of nudges	Number of estimates	Number of pooled estimates	Number of estimates with interaction terms	Number of estimates not showing %- point difference	Number of comparable estimates	Number of comparable nudges	Comparable paper
1	LECG (2008)	Ex post analysis	1	0	0	0	0	0	0	0
2	Bhattacharya et al (2012)	Ex post analysis	3	35	0	1	0	34	3	1
3	Hunt et al (2015)	Ex post analysis	1	2	0	0	0	2	1	1
4	Charles et al (2019)	Ex post analysis	1	72	0	0	0	72	1	1
5	Kling et al (2012)	Field experiment	1	2	0	0	0	2	1	1
6	Adams et al (2015a)	Field experiment	6	126	36	54	0	36	6	1
7	Adams et al (2015b)	Field experiment	8	79	0	16	0	63	8	1
8	Keys et al (2016)	Field experiment	1	0	0	0	0	0	0	0
9	Glazebrook et al (2017)	Field experiment	3	7	0	0	0	7	3	1
10	Marzilli Ericson et al (2017)	Field experiment	2	108	36	90	0	12	2	1
11	Seira et al (2017)	Field experiment	7	42	0	0	0	42	7	1
12	Accent Research (2018)	Field experiment	11	22	0	0	0	22	11	1
13	Adams-Ernstsone (2018)	Field experiment	5	29	0	0	0	29	5	1
14	BCFP (2018)	Field experiment	1	3	0	0	3	0	0	0
15	Johnson et al (2019)	Field experiment	1	1	0	0	0	1	1	1
16	Adams et al (2021)	Field experiment	12	50	19	21	0	23	12	1
17	Farghly et al (2020)	Field experiment	2	4	0	0	0	4	2	1
18	TNS (2012)	Online lab experiment	4	12	0	0	12	0	0	0
19	Duke et al (2014)	Online lab experiment	3	6	0	0	3	3	3	1
20	Oxera-CESS (2016)	Online lab experiment	5	125	0	60	0	65	5	1
21	Suter et al (2017)	Online lab experiment	2	24	8	0	0	16	2	1
22	BIT (2018)	Online lab experiment	3	3	0	0	3	0	0	0
23	Suter et al (2019)	Online lab experiment	10	28	0	0	0	28	10	1
24	Burke et al (2020)	Online lab experiment	2	2	0	0	2	0	0	0
		Total:	95	782	99	242	23	461	83	18

Table 3: Morphological box of the studies included

Dimension	Characteristic					
Study design	Field experiment (13)	Qualitative (9)	Online lab experiment (7)	Ex post analysis (4)		
Population	Users of product (25)	Nat. rep. sample (6)	Grown-up population (1)	Pot. users of product (1)		
Geographic area	UK (21)	US (5)	EU (3)	Ireland (2)	Germany (1)	Mexico (1)
Authority	FCA (15)	EC (3)	CMA (2)	Other (7)	None (6)	
Policy stage	Remedy testing (14)	Exploratory (11)	Evaluation (2)	N/A (6)		
Search or switching main outcome variable	Yes (27)	No (6)				

Table 4: Morphological box of nudges in the quantitative papers

Dimension	Characteristic								
Product	Current accounts (22)	Cash savings (18)	Insurance (18)	Pensions (15)	Credit cards (7)	Personal loans (6)	Currency transfer (5)	Mortgages (3)	Retail investments (1)
Type of nudge	Informational (67)	Reminder (8)	Increases in ease and conv. (7)	Structural change (6)	Simplification (4)	Disclosure (3)			
Impact measured through search or	Switching (48)	Search (27)	Both (20)						
Significance (best)	Less than 1% (44)	Between 1 and 5% (11)	Between 5 and 10% (3)	Insignificant (more than 10%) (33)	Missing (4)				

Table 5: Features of informational nudges

Feature	Number of nudges with this feature	Proportion of informational nudges with this feature
Includes a call to action	37	55%
Text encouraging shopping around / switching	23	34%
Includes a question	20	30%
Information about availability of independent advice	18	27%
Estimate of potential savings / losses	17	25%
Disclosure (e.g. fee structure, rules)	15	22%
Information about the process / cost of search / switching	12	18%
General information about the market / product or warning	11	16%
Past fees / charges the consumer incurred	11	16%
Graphical illustration	10	15%
Other offers from the same provider	9	13%
Information about the benefits of search / switching	8	12%
Offers from competitors	8	12%
Personalised estimates	8	12%
Reminder	7	10%
Use of social norms / highlighting other people's mistakes	7	10%
Cost summary with representative examples or based on expected usage	6	9%
Eliciting implementation intentions	4	6%
Reference to price comparison website	2	3%
Total	67	

Table 6: Average impact of nudge interventions by search and switching (percentage point)

	Paper - all	Paper - reliable	Nudge - all	Nudge - reliable
Search	8.3%	9.3%	6.3%	6.9%
Switching	2.4%	1.8%	2.1%	2.3%
Difference	5.8%	7.5%	4.2%	4.6%

Notes: (i) paper and nudge indicate results weighted by the inverse of the number of estimates in the paper / for the nudge; (ii) all indicates that all estimates are included, reliable indicates that less reliable (non-causal and self-reported) estimates are excluded; (iii) sample size search – all 145, switching all 259, search reliable 116, switching reliable 235

Table 7: Difference between average impact for outcome measures of search and switching by study design (percentage point)

	Paper - all	Paper - reliable	Nudge - all	Nudge - reliable
Online lab experiment	2.4%	2.4%	-3.3%	-3.3%
Field experiment	2.1%	3.9%	2.8%	3.1%
Ex post analysis	7.9%		7.9%	

Notes: (i) paper and nudge indicate results weighted by the inverse of the number of estimates in the paper / for the nudge; (ii) all indicates that all estimates are included, reliable indicates that less reliable (non-causal and self-reported) estimates are excluded; (iii) sample size online lab experiment all 112, reliable 112, field experiment all 241, reliable 192, *ex post* analysis all 93; (iv) the comparison is not possible excluding less reliable estimates for *ex post analyses* as there are no observations that measure the impact on search

APPENDICES

Appendix I – List of variables recorded for estimates in the quantitative papers

General information about the study

1. Study design: Type of the study, such as online laboratory experiment, field experiment, evaluation or analysis of existing data. The last two categories were combined into *ex post* analysis.
2. Study design detail: Contains details of the study design, e.g. for experiments whether treatment is randomised, participation is voluntary and outcomes are measured through a survey.
3. Country / area: Indicates the geographic area where the study was carried out.
4. Population: The population from which the sample was drawn – grown up population, nationally representative samples, users or potential users of the product.
5. Selection restriction: Any restrictions applied when selecting participants for the study, e.g. pension holders approaching retirement or cash savings holders facing a rate decrease.
6. Regulator / authority: Public body that was involved in study. Their role could be commissioning the study and/or carrying out the implementation themselves.
7. Policy stage: For studies with public body involvement, indicates the stage of policy development at which it was carried out – exploratory research, remedy testing or evaluation.
8. Role in policy: Further details on the role of the study in policy development, e.g. the aim of the report and whether the findings were incorporated into policy changes.
9. Is search or switching one of the main outcome variables: 0 if the study was designed to measure something else (e.g. optimal choice by consumers) and the impact on search and switching was reported only as a secondary outcome measure, 1 otherwise.

Product and nudge

10. Product: Type of retail financial product on which the intervention was tested.
11. Type of insurance: If the product is insurance, it indicates the type of insurance such as home, motor, health, etc.
12. Channel: The communication channel through which the nudge was delivered to consumers, e.g. post, phone, email.
13. Nudge / intervention: Short description of the tested intervention.
14. Type of nudge: Type of nudge following the classification described in section IV, subsection a.
15. Main finding: The main result of the paper relating to the nudge.

Features of informational nudges: 1 if the nudge has this particular feature, 0 otherwise.

16. Text encouraging shopping around
17. Information about the process / cost of search / switching
18. Information about the benefits of search / switching
19. Past fees / charges the consumer incurred
20. Offers from competitors
21. Other offers from the same provider
22. Estimate of potential savings / losses
23. Graphical illustration
24. Personalised estimates
25. General information about the market / product or warning
26. Reminder
27. Use of social norms / highlighting other people's mistakes
28. Eliciting implementation intentions
29. Disclosure
30. Includes a question
31. Includes a call to action
32. Cost summary with representative examples or based on expected usage
33. Information about availability of independent advice
34. Reference to price comparison website
35. Total number of informational features

Outcome measure

36. Outcome measure: Description of the outcome measure used to judge the impact of the intervention, e.g. clicked to shop around, switched internally to another product of the same provider, considered changing provider.
37. Search or switching: Indicates whether the outcome measure is a measure of search or switching activity.
38. Self-reported: 1 if the outcome measure is reported by the consumer, 0 otherwise.
39. Past behaviour or future intention: 1 if the outcome measure shows some action in the past, 0 if it is the consumer's intention to do something.

40. Causal relationship between nudge and estimate: 1 if the methodology is such that it can be accepted that the nudge caused the measured change (e.g. randomised controlled trials), 0 otherwise (e.g. comparisons of means obtained from survey responses).

Estimation

41. Specification: Description of specification as in the original paper.
42. Value of estimate: The estimated impact of the intervention, expressed as a difference compared to a baseline number.
43. Standard error: The standard error of the estimated impact, if reported.
44. Significance: The significance level of the estimated impact, if reported.
45. Sample size: The size of the sample on which the intervention was tested.
46. Constant / mean in control group: The baseline number against which the estimated impact is measured; e.g. value of the constant if regression analysis is used.
47. Significant controls: List of control variables that had a significant impact in that specification.
48. Pooled estimate: 1 if the recorded estimate is a pooled estimate of two or more nudges, 0 otherwise.
49. Includes interaction terms with treatment: 1 if the specification includes interaction terms with the treatment, 0 otherwise.
50. Shows percentage point change: 1 if the outcome measure shows percentage point change, 0 otherwise.
51. Controls included: 1 if the specification includes control variables, 0 otherwise.
52. Type of estimate: As much detail about the type of analysis as available, e.g. comparison of means, logistic regression, binary regression.
53. Notes on estimates: Descriptive notes on the estimate and the specification that help understand what they show.
54. Source: Indicates where can the estimate be found in the original paper (page number, table, etc.).

Appendix II – Detailed results, averages

Table 8: Averages, weighted by the inverse of the number of estimates per paper

	Number of estimates	Average effect size	Pooled st. error	Conf. int. lower limit	Conf. int. upper limit
All	446	0.056	0.005	0.047	0.065
By search/switching					
Search	145	0.083	0.007	0.068	0.097
Switching	301	0.024	0.003	0.019	0.030
By design					
Field experiment	241	0.030	0.003	0.023	0.036
Online lab experiment	112	0.137	0.017	0.104	0.170
Ex post analysis	93	0.043	0.012	0.020	0.065
By search / switching and design					
Search					
Online lab experiment	97	0.137	0.017	0.104	0.171
Field experiment	44	0.040	0.005	0.031	0.050
Ex post analysis	4	0.080	0.018	0.044	0.116
Switching					
Online lab experiment	15	0.113	0.006	0.101	0.126
Field experiment	197	0.020	0.003	0.013	0.026
Ex post analysis	89	0.001	0.001	-0.001	0.003
By product					
Cash savings	59	0.037	0.001	0.034	0.039
Credit cards	42	0.000	0.002	-0.004	0.005
Current accounts	54	-0.002	0.002	-0.007	0.003
Insurance	161	0.090	0.012	0.067	0.113
Mortgages	1	0.002	0.002	-0.002	0.006
Pensions	105	0.056	0.005	0.047	0.066
Personal loans	24	0.080	0.011	0.059	0.101
By product and design					
Current accounts					
Online lab experiment	4	-0.010	0.006	-0.022	0.002
Field experiment	22	0.004	0.004	-0.004	0.011
Ex post analysis	28	0.001	0.000	0.000	0.001
Insurance					
Online lab experiment	19	0.181	0.034	0.115	0.247
Field experiment	77	0.047	0.010	0.028	0.066
Ex post analysis	65	0.064	0.017	0.030	0.098
Pensions					
Online lab experiment	65	0.120	0.003	0.113	0.126
Field experiment	40	0.035	0.006	0.023	0.048
By type of nudge					
Disclosure	8	0.061	0.017	0.027	0.095
Increases in ease and convenience	7	0.086	0.012	0.062	0.109
Informational	358	0.031	0.002	0.026	0.035
Reminder	31	0.032	0.003	0.026	0.038
Simplification	19	0.036	0.006	0.024	0.048
Structural change	23	0.141	0.032	0.078	0.204
By type of nudge and design					
Disclosure					
Field experiment	6	-0.002	0.001	-0.004	0.000
Ex post analysis	2	0.125	0.035	0.057	0.193
Increases in ease and convenience					
Online lab experiment	1	0.080	0.031	0.019	0.141
Field experiment	6	0.089	0.010	0.070	0.107
Informational					
Online lab experiment	93	0.093	0.005	0.084	0.103
Field experiment	174	0.023	0.003	0.016	0.030
Ex post analysis	91	0.002	0.001	0.000	0.004
Structural change					
Online lab experiment	18	0.213	0.048	0.118	0.308
Field experiment	5	-0.004	0.002	-0.007	-0.001

Table 9: Averages, weighted by the inverse of the number of estimates per paper, excluding less reliable estimates

	Number of estimates	Average effect size	Pooled st. error	Conf. int. lower limit	Conf. int. upper limit
All	393	0.051	0.005	0.041	0.060
By search/switching					
Search	116	0.093	0.009	0.076	0.110
Switching	277	0.018	0.001	0.016	0.020
By design					
Field experiment	192	0.023	0.002	0.019	0.028
Online lab experiment	112	0.137	0.017	0.104	0.170
Ex post analysis	89	0.001	0.001	-0.001	0.003
By search / switching and design					
Search					
Online lab experiment	97	0.137	0.017	0.104	0.171
Field experiment	19	0.049	0.005	0.039	0.059
Ex post analysis					
Switching					
Online lab experiment	15	0.113	0.006	0.101	0.126
Field experiment	173	0.010	0.001	0.008	0.012
Ex post analysis	89	0.001	0.001	-0.001	0.003
By product					
Cash savings	59	0.037	0.001	0.034	0.039
Credit cards	42	0.000	0.002	-0.004	0.005
Current accounts	32	-0.005	0.003	-0.011	0.002
Insurance	142	0.078	0.013	0.052	0.105
Mortgages	1	0.002	0.002	-0.002	0.006
Pensions	93	0.057	0.005	0.047	0.066
Personal loans	24	0.080	0.011	0.059	0.101
By product and design					
Current accounts					
Online lab experiment	4	-0.010	0.006	-0.022	0.002
Field experiment					
Ex post analysis	28	0.001	0.000	0.000	0.001
Insurance					
Online lab experiment	19	0.181	0.034	0.115	0.247
Field experiment	62	0.014	0.001	0.012	0.017
Ex post analysis	61	0.002	0.002	-0.002	0.006
Pensions					
Online lab experiment	65	0.120	0.003	0.113	0.126
Field experiment	28	0.035	0.006	0.023	0.048
By type of nudge					
Disclosure	6	-0.002	0.001	-0.004	0.000
Increases in ease and convenience	7	0.086	0.012	0.062	0.109
Informational	314	0.026	0.001	0.024	0.028
Reminder	28	0.027	0.002	0.022	0.031
Simplification	15	0.032	0.006	0.021	0.044
Structural change	23	0.141	0.032	0.078	0.204
By type of nudge and design					
Disclosure					
Field experiment	6	-0.002	0.001	-0.004	0.000
Ex post analysis					
Increases in ease and convenience					
Online lab experiment	1	0.080	0.031	0.019	0.141
Field experiment	6	0.089	0.010	0.070	0.107
Informational					
Online lab experiment	93	0.093	0.005	0.084	0.103
Field experiment	132	0.013	0.001	0.011	0.015
Ex post analysis	89	0.001	0.001	-0.001	0.003
Structural change					
Online lab experiment	18	0.213	0.048	0.118	0.308
Field experiment	5	-0.004	0.002	-0.007	-0.001

Table 10: Averages, weighted by the inverse of the number of estimates per nudge

	Number of estimates	Average effect size	Pooled st. error	Conf. int. lower limit	Conf. int. upper limit
All	446	0.035	0.003	0.029	0.040
By search/switching					
Search	145	0.063	0.006	0.051	0.075
Switching	301	0.021	0.001	0.018	0.023
By design					
Field experiment	241	0.019	0.001	0.016	0.021
Online lab experiment	112	0.080	0.011	0.059	0.101
Ex post analysis	93	0.043	0.012	0.020	0.065
By search / switching and design					
Search					
Online lab experiment	97	0.081	0.011	0.060	0.102
Field experiment	44	0.041	0.006	0.028	0.053
Ex post analysis	4	0.080	0.018	0.044	0.116
Switching					
Online lab experiment	15	0.113	0.006	0.101	0.126
Field experiment	197	0.013	0.001	0.010	0.015
Ex post analysis	89	0.001	0.001	-0.001	0.003
By product					
Cash savings	59	0.032	0.001	0.030	0.033
Credit cards	42	0.000	0.002	-0.004	0.005
Current accounts	54	0.000	0.003	-0.006	0.006
Insurance	161	0.072	0.011	0.050	0.094
Mortgages	1	0.002	0.002	-0.002	0.006
Pensions	105	0.055	0.003	0.049	0.061
Personal loans	24	0.019	0.013	-0.006	0.043
By product and design					
Current accounts					
Online lab experiment	4	-0.010	0.006	-0.022	0.002
Field experiment	22	0.004	0.004	-0.004	0.011
Ex post analysis	28	0.001	0.000	0.000	0.001
Insurance					
Online lab experiment	19	0.186	0.039	0.110	0.263
Field experiment	77	0.022	0.004	0.015	0.029
Ex post analysis	65	0.064	0.017	0.030	0.098
Pensions					
Online lab experiment	65	0.120	0.003	0.113	0.126
Field experiment	40	0.022	0.004	0.014	0.031
By type of nudge					
Disclosure	8	0.061	0.017	0.027	0.095
Increases in ease and convenience	7	0.088	0.012	0.064	0.111
Informational	358	0.020	0.002	0.017	0.024
Reminder	31	0.030	0.003	0.024	0.036
Simplification	19	0.028	0.005	0.018	0.038
Structural change	23	0.138	0.032	0.075	0.201
By type of nudge and design					
Disclosure					
Field experiment	6	-0.002	0.001	-0.004	0.000
Ex post analysis	2	0.125	0.035	0.057	0.193
Increases in ease and convenience					
Online lab experiment	1	0.080	0.031	0.019	0.141
Field experiment	6	0.090	0.012	0.066	0.114
Informational					
Online lab experiment	93	0.045	0.005	0.034	0.055
Field experiment	174	0.012	0.001	0.010	0.015
Ex post analysis	91	0.002	0.001	0.000	0.004
Structural change					
Online lab experiment	18	0.213	0.048	0.118	0.308
Field experiment	5	-0.012	0.004	-0.019	-0.005

Table 11: Averages, weighted by the inverse of the number of estimates per nudge, excluding less reliable estimates

	Number of estimates	Average effect size	Pooled st. error	Conf. int. lower limit	Conf. int. upper limit
All	393	0.036	0.003	0.030	0.042
By search/switching					
Search	116	0.069	0.007	0.055	0.084
Switching	277	0.023	0.001	0.021	0.025
By design					
Field experiment	192	0.018	0.001	0.016	0.020
Online lab experiment	112	0.080	0.011	0.059	0.101
Ex post analysis	89	0.001	0.001	-0.001	0.003
By search / switching and design					
Search					
Online lab experiment	97	0.081	0.011	0.060	0.102
Field experiment	19	0.044	0.005	0.035	0.053
Ex post analysis					
Switching					
Online lab experiment	15	0.113	0.006	0.101	0.126
Field experiment	173	0.013	0.001	0.011	0.015
Ex post analysis	89	0.001	0.001	-0.001	0.003
By product					
Cash savings	59	0.032	0.001	0.030	0.033
Credit cards	42	0.000	0.002	-0.004	0.005
Current accounts	32	-0.008	0.005	-0.017	0.002
Insurance	142	0.062	0.012	0.038	0.086
Mortgages	1	0.002	0.002	-0.002	0.006
Pensions	93	0.054	0.003	0.047	0.060
Personal loans	24	0.019	0.013	-0.006	0.043
By product and design					
Current accounts					
Online lab experiment	4	-0.010	0.006	-0.022	0.002
Field experiment					
Ex post analysis	28	0.001	0.000	0.000	0.001
Insurance					
Online lab experiment	19	0.186	0.039	0.110	0.263
Field experiment	62	0.006	0.002	0.003	0.009
Ex post analysis	61	0.002	0.002	-0.002	0.006
Pensions					
Online lab experiment	65	0.120	0.003	0.113	0.126
Field experiment	28	0.021	0.005	0.012	0.030
By type of nudge					
Disclosure	6	-0.002	0.001	-0.004	0.000
Increases in ease and convenience	7	0.088	0.012	0.064	0.111
Informational	314	0.022	0.002	0.018	0.026
Reminder	28	0.024	0.002	0.020	0.028
Simplification	15	0.021	0.004	0.013	0.029
Structural change	23	0.138	0.032	0.075	0.201
By type of nudge and design					
Disclosure					
Field experiment	6	-0.002	0.001	-0.004	0.000
Ex post analysis					
Increases in ease and convenience					
Online lab experiment	1	0.080	0.031	0.019	0.141
Field experiment	6	0.090	0.012	0.066	0.114
Informational					
Online lab experiment	93	0.045	0.005	0.034	0.055
Field experiment	132	0.012	0.001	0.009	0.014
Ex post analysis	89	0.001	0.001	-0.001	0.003
Structural change					
Online lab experiment	18	0.213	0.048	0.118	0.308
Field experiment	5	-0.012	0.004	-0.019	-0.005

Appendix III – Detailed results, regressions

Table 12: Regression results

		All	Reliable	Field experiments	Field experiments, reliable
Search vs. switching	Switching	(dropped)	(dropped)	(dropped)	(dropped)
	Search	0.020	0.018	0.031**	0.056***
		(0.012)	(0.014)	(0.010)	(0.005)
Study design	Field experiment	(dropped)	(dropped)		
	Ex post analysis	0.004	0.010		
		(0.009)	(0.013)		
	Online lab experiment	0.102***	0.101***		
		(0.015)	(0.015)		
Type of nudge	Informational	(dropped)	(dropped)	(dropped)	(dropped)
	Disclosure	0.020	-0.000	-0.007	-0.001
		(0.029)	(0.008)	(0.009)	(0.003)
	Increases in ease and convenience	0.051**	0.050**	0.067***	0.045***
		(0.022)	(0.021)	(0.011)	(0.007)
	Reminder	0.006	0.003	0.004	0.002
		(0.010)	(0.011)	(0.011)	(0.010)
	Simplification	0.003	0.007	0.000	0.002
		(0.008)	(0.012)	(0.009)	(0.004)
	Structural change	0.026	0.033	-0.021*	-0.052***
		(0.024)	(0.028)	(0.010)	(0.006)
Product	Current accounts	(dropped)	(dropped)	(dropped)	
	Cash savings	0.046**	0.062**	0.033*	(dropped)
		(0.019)	(0.028)	(0.015)	
	Credit cards	0.010	0.025	-0.003	-0.037**
		(0.013)	(0.025)	(.)	(0.015)
	Insurance	0.014	0.023	0.001	-0.039**
		(0.013)	(0.022)	(0.009)	(0.013)
	Mortgages	0.012	0.027	-0.002	-0.036**
		(0.013)	(0.025)	(.)	(0.015)
	Pensions	0.007	0.026	-0.017***	-0.046**
		(0.023)	(0.033)	(0.004)	(0.014)
	Personal loans	-0.032	-0.014		
		(0.029)	(0.037)		
	Constant	-0.010	-0.025	0.004	0.038**
		(0.013)	(0.025)	(.)	(0.015)
	R-squared	0.394	0.406	0.308	0.465
	N	446	393	241	192

Notes: (i) clustered standard errors in parentheses; (ii) *** indicates significant at 1%, ** indicates significant at 5%, * indicates significant at 10%

Appendix IV – Detailed results, averages using the best estimate analysis

Table 13: Averages, best estimate analysis

	All estimates	Reliable estimates only
All	0.042	0.050
By search/switching		
Search	0.072	0.087
Switching	0.017	0.020
By design		
Field experiment	0.017	0.018
Online lab experiment	0.103	0.103
Ex post analysis	0.022	0.003
By product		
Cash savings	0.036	0.036
Current accounts	0.002	-0.004
Insurance	0.049	0.053
Mortgages	0.002	0.002
Pensions	0.063	0.069
Personal loans	0.075	0.075
By type of nudge		
Disclosure	0.034	-
Increases in ease and convenience	0.096	0.096
Informational	0.034	0.044
Reminder	0.041	0.028
Simplification	0.025	0.020
Structural change	0.130	0.130

Appendix V – results using combined nudge categories

Table 14: Averages by combined nudge categories

	Number of estimates	Average effect size	Pooled st. error	Conf. int. lower limit	Conf. int. upper limit
Paper all					
Structural	17	0.171	0.019	0.133	0.209
Informational	429	0.034	0.003	0.028	0.040
Paper reliable					
Structural	17	0.171	0.019	0.133	0.209
Informational	376	0.023	0.002	0.020	0.027
Nudge all					
Structural	17	0.171	0.028	0.115	0.227
Informational	429	0.022	0.002	0.019	0.025
Nudge reliable					
Structural	17	0.171	0.028	0.115	0.227
Informational	376	0.020	0.001	0.017	0.023

Notes: (i) paper and nudge indicate results weighted by the inverse of the number of estimates in the paper / for the nudge; (ii) all indicates that all estimates are included, reliable indicates that less reliable (non-causal and self-reported) estimates are excluded

Table 15: Regression analysis including combined nudge categories

		All	Reliable	Field exp.	Field exp., reliable
Nudge category	Informational	(dropped)	(dropped)	(dropped)	(dropped)
	Structural	0.132**	0.140**	0.068***	0.057***
		(0.047)	(0.051)	(0.011)	(0.008)
Search vs. switching	Switching	(dropped)	(dropped)	(dropped)	(dropped)
	Search	0.011	0.004	0.030***	0.043***
		(0.013)	(0.018)	(0.009)	(0.009)
Study design	Field experiment	(dropped)	(dropped)		
	Ex post analysis	0.005	0.012		
		(0.008)	(0.010)		
	Online lab experiment	0.090***	0.093***		
		(0.019)	(0.016)		
Product	Current accounts	(dropped)	(dropped)	(dropped)	
	Cash savings	0.044**	0.058**	0.034**	(dropped)
		(0.018)	(0.024)	(0.013)	
	Credit cards	0.009	0.023	-0.003	-0.038**
		(0.011)	(0.020)	(.)	(0.013)
	Insurance	0.012	0.018	0.002	-0.037**
		(0.012)	(0.018)	(0.006)	(0.013)
	Mortgages	0.011	0.025	-0.002	-0.036**
		(0.011)	(0.020)	(.)	(0.013)
	Pensions	0.018	0.038	-0.019***	-0.051***
		(0.022)	(0.027)	(0.004)	(0.014)
	Personal loans	-0.012	0.006		
		(0.029)	(0.032)		
	Constant	-0.009	-0.023	0.004	0.038**
		(0.011)	(0.020)	(.)	(0.013)
	R-squared	0.491	0.516	0.311	0.435
	N	446	393	241	192

Notes: (i) clustered standard errors in parentheses; (ii) *** indicates significant at 1%, ** indicates significant at 5%, * indicates significant at 10%