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Revisiting the role of consumer search in competition policy

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This work is dedicated to my sister, Vera, and to my husband, Patrick,
the two devoted researchers in my life.

Abstract

This thesis contains three independent studies on different aspects of consumer search for complex products. First, I carry out a systematic review of existing evidence on the effectiveness of nudge interventions to increase consumer search and switching in retail financial products. I find that nudges have limited impact on average and that informational nudges are less effective than nudges that increase ease and convenience or implement a major structural change in the decision-making environment. Second, I build a theoretical industrial organization model to assess welfare in equilibrium in a market where prices are observable but consumers need to incur a search cost to understand horizontally differentiated product features. I find that consumers tend to be better off when they do not invest in learning about the products but choose simply based on price, as it incentivises firms to compete on price more vigorously. Finally, I carry out an empirical analysis using survey data to establish whether shopping around can increase consumers' awareness of product complexity. The main finding of this chapter is that some consumers who search before taking out a credit card are indeed more likely to agree that credit cards are complicated products but this relationship depends on what they use their credit cards for.

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Introduction

This thesis contains three independent studies on different aspects of **consumer search**, which, in this context, is the process of acquiring information on providers and their products and services prior to making a purchase as a consumer. In each case, the focus is on **complex products**.

Search is an important element of interaction between firms and consumers, and has been extensively studied in the theoretical and empirical industrial organization literature. The basics of this literature originate from a seminal paper by Stigler (1961) in which he noted that economists often assume away the lack of knowledge of market players, which is inappropriate when it comes to market prices.[1] Consumers do not and cannot know all the prices in the market and they need to do something to find them out.

Identifying sellers and finding out the prices and features of their products require some effort. This effort was introduced in the literature in the form of **search costs** that consumers incur to acquire information.

Subsequent research focused on the impact of search costs on market outcomes. Diamond (1971) described a theoretical model in which the introduction of search costs leads to the monopoly price in equilibrium, even though there are many firms offering the same product (this became known as the Diamond-paradox).[2] A few related papers contain models that obtain the Diamond result as an extreme case (e.g. Stahl, 1989, and Anderson and Renault, 1999).[3,4]

In line with these early results, the literature typically finds that **higher search costs lead to less competitive outcomes**. The intuition behind is simple: if it is

[1] Stigler, G.J., 1961, The Economics of Information, *Journal of Political Economy*, Vol. 69, No. 3, pp. 213-225.
[2] Diamond, P.A., 1971, A Model of Price Adjustment, *Journal of Economic Theory*, Vol. 3, No. 2, pp. 156-168.
[3] Stahl, D.O., 1989, Oligopolistic Pricing with Sequential Consumer Search, *American Economic Review*, Vol. 79, No. 4, pp. 700-712.
[4] Anderson, S.P. and Renault, R., 1999, Pricing, Product Diversity, and Search Costs: A Bertrand-Chamberlin-Diamond Model, *RAND Journal of Economics*, Vol. 30, No. 4, pp. 719-735.

costly for consumers to find the cheapest prices or their most preferred product, they will not acquire all information about all sellers, which softens competition.

It is now increasingly well understood in **policy-making** as well that consumer engagement, which includes search, is a driving force of competition (Fletcher, 2021).[5] Competition authorities and sectoral regulators thus consider the demand side along with the supply side when assessing the state of competition in a given product market. The UK's market investigation regime is an ideal tool for exploring demand side problems. See, for example, market studies and investigations by the Competition and Markets Authority or the Financial Conduct Authority.

However, there are still numerous aspects of consumer search that have **not been fully explored,** either in academic research or in policy. The three essays in this thesis contribute to our understanding of the role of consumer search in markets of complex products in three distinct ways. In particular, I assess (i) the extent to which certain interventions are successful in increasing consumer search (and switching), (ii) market configurations in which it may not always be beneficial for consumers to search, and (iii) whether search can bring some other benefits in addition to allowing consumers to find more suitable products and increasing the competitive pressure.

I investigate these questions using **three different research methods**, which is a distinctive feature of the dissertation. I answer the first question through a systematic review of existing research, build a theoretical industrial organization model to assess the second, and carry out empirical data analysis for the third.[6]

The **first chapter** looks at interventions that aim to increase consumer engagement. In particular, it assesses the **effectiveness of nudge interventions in increasing consumer search and switching in retail financial markets**.[7] Nudges are changes in any aspect of the decision-making

[5] Fletcher, A., 2021. Disclosure as a tool for enhancing consumer engagement and competition, *Behavioural Public Policy*, Vol. 5, No. 2, pp. 252-278.
[6] The choice of different research techniques is a reflection of my personal motivation for completing this programme – the aim was to deepen my knowledge of a broad set of academic methods.
[7] Switching is the second step after shopping around: once a consumer identified a better offer, she also needs to be able to switch providers in order to reward the ones who provide good deals and penalise the ones whose offers are inferior. Search and switching are often measured alongside each other, hence I decided to include both in the review.

environment which do not significantly alter the consumers' incentives but may still affect their behaviour. They have been shown to achieve great results in directing people to make better decisions in other areas, such as eating behaviour (Hanks et al, 2012) or contributions to savings (Karlan et al, 2016).[8,9]

The review encompasses 35 papers that cover academic and policy publications, qualitative and quantitative evidence, a wide range of retail financial products and a number of geographic areas. Its **key findings** are the following.

1. Nudge interventions increase consumer search and switching in retail financial markets by 2-3 percentage points on average. While this may be a large increase in relative terms and it could be a cost-efficient intervention, regulators cannot expect nudges to alter consumer behaviour to the extent that it would lead to a significant change in the competitive landscape.
2. Providing, simplifying or highlighting information has limited impact. The most effective nudges are the ones that do more than that by (i) removing some of the administrative burden or (ii) making a major change in the decision-making environment.
3. There is no clear evidence that nudge interventions work better for certain products or for certain groups of consumers. There is an indication that it is easier to nudge people to shop around than to switch.
4. Currently field trials appear to be the most reliable source for assessing the average impact of nudge interventions. Laboratory experiments seem to significantly overestimate the impact, while the existing *ex post* evaluations suffer from methodological issues. Qualitative research, such as interviews and focus groups, is not suitable for estimating the expected magnitude of the changes.

The **second chapter** assesses **situations in which consumers observe prices in the market but are unable to interpret product features without investing costly time and effort to understand them (i.e. without search)**. I build a theoretical model and derive the equilibrium outcomes to compare welfare under

[8] Hanks, A.S., Just, D.R., Smith, L.E. and Wansink, B., 2012, Healthy convenience: nudging students toward healthier choices in the lunchroom, *Journal of public health*, Vol. 34, No. 3, pp. 370-376.
[9] Karlan, D., McConnell, M., Mullainathan, S. and Zinman, J., 2016, Getting to the top of mind: How reminders increase saving, *Management Science*, Vol. 62, No. 12, pp. 3393-3411.

different assumptions. The model is motivated by consumer electronics such as computer screens, tablets and sound systems, which consumers use on a daily basis but buy infrequently. Prices are easily accessible and comparable but understanding product specifications requires knowledge of technical terms. Therefore, consumers can invest in learning about these terms so that they can establish which product they prefer, or instead choose based on price without fully understanding what they are buying and whether that is a good match for them. The **key findings** of this chapter are the following.

1. On a market that is characterised by observable prices and complicated product differentiation, higher search costs can lead to *more* competitive outcomes. This is because firms are only able to take advantage of the fact that some consumers prefer their product if consumers incur the search cost to understand firms' offerings and how they map to their own preferences. If search costs are high and consumers decide not to search but choose simply based on price, firms are forced to compete prices down.
2. As a result, firms have no incentive to make search more difficult. Instead, it would be in their best interest to help consumers learn about product features so that they can extract rent that consumers are willing to pay for their preferred product. Consumers tend to be better off when they do not search. While they derive some disutility from not getting the perfect match, this is offset by the low prices firms set in this scenario.
3. These findings are in contrast with the classic literature on search costs that establishes that higher search costs lead to less competitive markets, and with the obfuscation literature that shows that firms have incentives to make search more difficult. However, the findings are in line with more recent models that assume observable prices and costly search for other product features. A distinctive feature of my model, compared to these previous ones, is that I allow consumers to buy from a firm after observing its price but without evaluating its product. This idea has been used in only a few very recent publications.[10,11] Another important difference is that I interpret search as the effort to understand technical terms and product specifications. This

[10] Chen, Y., Li, S., Lin, K. and Yu, J., 2021, Consumer search with blind buying, *Games and Economic Behavior*, Vol. 126, pp. 402-427.
[11] Petrikaitė, V., 2022, Escaping search when buying, *International Journal of Industrial Organization*, Vol. 82, 102828.

implies that the consumer only needs to incur it once, after which she knows how much she likes any product. In contrast, existing literature tends to assume that search is the act of evaluating one product at a time and consumers need to incur the search cost at each product they consider ('sequential search').

The **third chapter** looks at another potential benefit of consumer search, beyond finding a good deal and putting pressure on firms to compete. In particular, it empirically investigates **whether shopping around is associated with increased awareness of product complexity**. The motivation for this research originates from the behavioural economics literature that establishes that consumers are often overconfident regarding their ability to assess products, and, as a result, make sub-optimal decisions when purchasing and using them. In this chapter, I return to retail financial markets and analyse whether consumers who search before taking a credit card out are more likely to agree that credit cards are complicated products. The motivating idea is that shopping around may help consumers discover complex prices and terms and conditions. Even if consumers do not understand these fully, just being aware may help them avoid mistakes when choosing and using the product. The **main findings** of this chapter are the following.

1. Some consumers who search before taking out a credit card are indeed more likely to say that credit cards are complicated than consumers who do not search, but this relationship depends on what consumers use their credit cards for. Specifically, there is a positive relationship between search and views on product complexity among consumers who do *not* use their credit cards for day-to-day purchases. In this group, the odds of finding credit cards complicated for those who search are 1.5-1.6 times the odds for those who do not search. However, the views of those consumers who use their credit card for day-to-day purchases do not differ depending on whether they search or not.
2. While I can show a positive relationship between search and awareness of product complexity, the dataset is such that I am unable to ascertain with certainty that the former causes the latter to increase (and it is not a third factor instead that positively affects both). Notwithstanding this limitation, given the methodologies I apply, I conclude that it is likely that search helps

certain groups of consumers to realise that credit cards are more complicated than they had thought.

Further research would be needed to test the hypothesis with data collected specifically for these purposes, and to test whether increased awareness of complexity indeed helps consumers avoid mistakes.

As is apparent from the brief summaries above, all three chapters contain **lessons for policymakers** who intend to intervene on the demand side of the market of complex products. Firstly, whether interventions that reduce consumers' search costs will be beneficial depends on market characteristics. It is possible that if prices are easily observable but product features are complicated, consumers on aggregate are better off when high search costs prevent them from searching. Secondly, in the context of retail financial products, shopping around may increase awareness of product complexity for some subset of consumers. This has the potential to mitigate the problem of overconfidence, which could provide regulators with a further reason to encourage search. Thirdly, though, increasing consumer engagement is difficult. In particular, nudge interventions have limited impact (even though they may still be a cost-effective way of achieving small improvements). Regulators considering nudge interventions need to do more than just improving the information the consumer receives – nudges that make the consumer's life easier are likely to be more effective.

The three chapters are interlinked by their focus on consumer search in markets of complex products such as retail financial products and consumer electronics. However, each looks at a distinct research question and **brings something novel** to the vast literature on search. The first chapter provides a comprehensive overview of the available evidence on the effectiveness of nudges in increasing consumer search and switching, which, to my knowledge, has not been done before. The second chapter introduces a model in which I relax the common and unrealistic assumption in the literature that consumers need to incur the search cost to buy from a firm. Instead, I allow consumers to buy from a firm after simply observing its price but without fully understanding the features of its product. Finally, the hypothesis tested in the third chapter (that shopping around may increase awareness of product complexity) is novel and has not been considered before.

Given that the three research questions relate to different areas of the literature, I provide separate literature reviews embedded in each chapter.

1. Do nudges increase consumer search and switching? Evidence from financial markets[12]

A shorter version of this paper is accepted and forthcoming in the journal of Behavioural Public Policy

1.1. Introduction

It is a common finding in competition analyses, and in particular in market studies by regulators and competition authorities, 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

[12] I would like to thank Paul Adams, Stefan Hunt, Sean Ennis, Stephen Davies and participants of the 2021 CLEEN conference for feedback and comments on this chapter.

that was in the forefront of applying behavioural research in practice, started trialling whether nudges could be used to increase consumer search and switching. Such trials can be hugely valuable as it may be hard to assess consumer reactions, sometimes even directionally (Fletcher, 2021).

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. The review is restricted to assess the impact on consumer search and switching, while these may not be the only (or even the main) measures of regulatory success. When a nudge intervention is implemented at scale, policymakers would have to consider second-round effects, such as suppliers' response in their pricing and in other dimensions of competition.

To answer the research question, I carried out a systematic search for relevant research using a set of pre-defined inclusion criteria. I found 35 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.[13] There is no clear evidence that nudge interventions would work better for certain products or for certain groups of

[13] With one exception of a qualitative study. See below in section 1.4.2.

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. 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.[14] I restricted the review to retail financial markets but the findings are likely to be highly relevant for policy-makers more broadly.[15]

The structure of the paper is as follows. Section 1.2 covers the related literature and section 1.3 describes the methodology I used for the literature search, data extraction and the analysis. Section 1.4 presents the results and section 1.5 concludes.

[14] As an example of the scale of potential effects, a counterfactual simulation on the Dutch retail deposit market shows that a 25% reduction in consumer inertia leads to a few percentage points increase in the combined market share of the small banks over four years (Deuflhard, 2018).
[15] 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).

1.2. Related 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 (2022) 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.

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, behavioural economist David Laibson made the point in his 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.[16]

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 (2022) 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. 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, pre-commitments, 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.

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

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.

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

1.3.1. Identifying relevant research

In order to identify a set of papers that can help answer my research question, I follow a set of pre-defined inclusion criteria. This is summarised in Table 1.1 below.

Table 1.1: Inclusion criteria

Criterion	Filter
Study design	Any
Type of intervention	Nudge
Outcome measure	Search or switching
Product	Retail financial products
Population	Any
Language	English

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.

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.

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.

The literature search took place in December 2020 and covered websites of financial regulators, competition authorities, nudge units and international organisations, a number of databases and search engines. In addition, I reviewed all editions of five relevant journals between 2015 and 2020. Finally, I reviewed the bibliographies of all the selected papers. Details of this search are listed in Appendix 1.1.

Table 1.2 shows the complete list of the 35 studies that met all the inclusion criteria.

Table 1.2: 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 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 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 analysis	UK
4	Charles et al (2019)	Evaluation Paper 19/1: An evaluation of our general insurance renewal transparency intervention	FCA	Ex post 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	Marzili 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

	Author	Title	Publisher	Study design	Country
18	TNS (2012)	Bank Fees Behaviour Study	EC	Lab experiment	EU
19	Duke et al (2014)	Study into the sales of Add-on General Insurance Products: Experimental consumer research	FCA	Lab experiment	UK
20	Oxera-CESS (2016)	Increasing consumer engagement in the annuities market: can prompts raise shopping around?	FCA	Lab experiment	UK
21	Suter et al (2017)	Study on consumers' decision making in insurance services, a behavioural economics perspective	EC	Lab experiment	EU
22	BIT (2018)	The impact of improved transparency of foreign money transfers for consumers and SMEs	BIT	Lab experiment	UK
23	Suter et al (2019)	Behavioural study on the digitalisation of the marketing and distance selling of retail financial services	EC	Lab experiment	EU
24	Burke et al (2020)	OP56 Fair exchange: presenting foreign exchange quotes to improve consumer choice	FCA	Lab experiment	UK
25	Timmons et al (2019)	Official advice improves mortgage-holders' perceptions of switching: experimental evidence	Behavioural Public Policy	Lab experiment	Ireland
26	Marandola et al (2020)	Applying behavioural insight to encourage consumer switching of financial products	EC	Lab experiment	EU
27	Archer et al (2014)	Research with payday lending customers	CMA	Qualitative	UK
28	Worton-Reynolds (2015)	Cash Savings Remedies	FCA	Qualitative	UK
29	B&A (2016)	Mortgage Holding & Switching, Market Research Findings	CCPC	Qualitative	Ireland
30	Optimisa Research (2016)	Informing the development of communication tools designed to increase consideration of switching among PCA and SME customers	CMA	Qualitative	UK
31	Worton et al (2016)	Cash Savings Switching Box	FCA	Qualitative	UK
32	Central Bank of Ireland (2017)	Mortgage Switching Research	CBI	Qualitative	Ireland
33	Collaborate Research (2017)	Future personal current account prompts and alerts	FCA	Qualitative	UK
34	Decision Technology (2018)	FCA Prompts and Alerts Design: Behavioural Evidence	FCA	Qualitative	UK
35	Savanta ComRes (2020)	Mortgage switching research	FCA	Qualitative	UK

Abbreviations: BCFP – Bureau of Consumer Financial Protection; BIT – Behavioural Insights Team; CBI – Central Bank of Ireland; CCPC – Competition and Consumer Protection Commission; CMA – Competition and Markets Authority; EC – European Commission; FCA – Financial Conduct Authority; OFT – Office of Fair Trading

1.3.2. 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 35 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 1.4.

Second, I recorded as a separate observation each estimated impact from the 26 quantitative studies for all interventions that met the definition of nudge.[17] 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 800 rows.

Note that the number of nudge interventions tested in these papers is much lower (102) 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 1.3 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:

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

- 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 476 comparable estimates that belong to 89 different nudges from 19 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 1.2.

Table 1.3: Number of nudges and estimates by paper

	Paper	Design	Number of nudges	Number of estimates	Number of pooled estimates	Number of estimates with interaction terms	Number of estimates not showing percentage point difference	Number of comparable estimates	Number of comparable nudges	Comparable paper
1	LECG (2008)	Ex post analysis	1	2	0	0	0	2	1	1
2	Bhattacharya et al (2012)	Ex post analysis	1	0	0	0	0	0	0	0
3	Hunt et al (2015)	Ex post analysis	3	35	0	1	0	34	3	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	Marzili 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	Farghly et al (2020)	Field experiment	2	4	0	0	0	4	2	1
17	Adams et al (2021)	Field experiment	12	50	19	21	0	23	12	1
18	TNS (2012)	Lab experiment	4	12	0	0	12	0	0	0
19	Duke et al (2014)	Lab experiment	3	6	0	0	3	3	3	1
20	Oxera-CESS (2016)	Lab experiment	5	125	0	60	0	65	5	1
21	Suter et al (2017)	Lab experiment	2	24	8	0	0	16	2	1
22	BIT (2018)	Lab experiment	3	3	0	0	3	0	0	0
23	Suter et al (2019)	Lab experiment	10	28	0	0	0	28	10	1
24	Burke et al (2020)	Lab experiment	2	2	0	0	2	0	0	0
25	Timmons et al (2019)	Lab experiment	1	0	0	0	0	0	0	0
26	Marandola et al (2020)	Lab experiment	6	15	0	0	0	15	6	1
	Total:		**102**	**797**	**99**	**242**	**23**	**476**	**89**	**19**

1.3.3. 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.[18] 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.[19] A few observations did not have a corresponding standard error, p-value or significance level – these are excluded from the analysis.

[18] For this analysis, I used the metan command in Stata 16.

[19] 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).

I calculated the average impact overall, by study design, by product, by type of nudge and outcome measure. As described below in section 1.4 in more detail, there is a considerable difference between the estimated impacts 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 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.[20]

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

[20] 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.

kept all of them if they were distinct categories but selected only one if they overlapped.[21]

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 158 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.[22]

[21] 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.

[22] For estimates where the standard error was missing, I used the minimum t-value (obtained from the significance level reported as explained in footnote 19) 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.

1.4. 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 section 1.4.2, I summarise the findings of the qualitative studies, drawing out common themes and lessons. Section 1.4.3 sets out the findings of the quantitative review.

1.4.1. Overview of studies and nudges covered

Study characteristics

Table 1.4 shows a morphological box of the 35 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 the majority are online; respondents did not have to show up in person in a laboratory. This 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).

Table 1.4: Morphological box of the studies included

<table>
<tr><th>Dimension</th><th colspan="6">Characteristic</th></tr>
<tr><td>Study design</td><td>Field experiment (13)</td><td>Lab experiment (9)</td><td>Qualitative (9)</td><td colspan="3">Ex post analysis (4)</td></tr>
<tr><td>Population</td><td>Users of product (26)</td><td>Users of product and students (1)</td><td>Nat. rep. sample (6)</td><td>Grown-up population (1)</td><td colspan="2">Pot. users of product (1)</td></tr>
<tr><td>Geographic area</td><td>UK (21)</td><td>US (5)</td><td>EU (4)</td><td>Ireland (3)</td><td>Germany (1)</td><td>Mexico (1)</td></tr>
<tr><td>Authority</td><td>FCA (15)</td><td>EC (4)</td><td>CMA (2)</td><td>Other (8)</td><td colspan="2">None (6)</td></tr>
<tr><td>Policy stage</td><td>Remedy testing (15)</td><td>Exploratory (12)</td><td>Evaluation (2)</td><td colspan="3">N/A (6)</td></tr>
<tr><td>Search or switching main outcome variable</td><td colspan="2">Yes (29)</td><td colspan="4">No (6)</td></tr>
</table>

Abbreviations: FCA – Financial Conduct Authority, EC – European Commission, CMA – Competition and Markets Authority

Table 1.5: Morphological box of nudges in the quantitative papers

<table>
<tr><th>Dimension</th><th colspan="9">Characteristic</th></tr>
<tr><td>Product</td><td>Current accounts (25)</td><td>Cash savings (18)</td><td>Insurance (18)</td><td>Pensions (15)</td><td>Credit cards (7)</td><td>Mortgages (7)</td><td>Personal loans (6)</td><td>Currency transfer (5)</td><td>Retail inv. (1)</td></tr>
<tr><td>Type of nudge</td><td>Informational (74)</td><td>Reminder (8)</td><td>Increases in ease and conv. (7)</td><td>Structural change (6)</td><td>Simplification (4)</td><td colspan="4">Disclosure (3)</td></tr>
<tr><td>Impact measured through search or switching</td><td colspan="3">Switching (55)</td><td colspan="3">Search (27)</td><td colspan="3">Both (20)</td></tr>
<tr><td>Significance (best)</td><td>Less than 1% (46)</td><td>Between 1 and 5% (11)</td><td>Between 5 and 10% (5)</td><td>Insignificant (more than 10%) (35)</td><td colspan="5">Missing (5)</td></tr>
</table>

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 and 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 but one 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), with some restricting their sample to certain groups of consumers (e.g. those nearing retirement for pensions or those close to automatic reenrolment for insurance). One paper run experiments with both users and students, and the rest relied on a nationally representative sample or grown-up population.

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 1.5 shows a morphological box of the characteristics of the 102 nudges covered in the 26 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 more than half of the nudges was measured on switching metrics, about a quarter on search metrics and less than quarter on both. Close to 60% of those nudges where significance is reported have at least one significant estimate.

Table 1.5 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 102 nudges covered in the papers, I classified 74 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 1.6 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.

Table 1.6: Features of informational nudges

Feature	Number of nudges with this feature	Proportion of informational nudges with this feature
Includes a call to action	39	53%
Text encouraging shopping around / switching	28	38%
Includes a question	23	31%
Information about availability of independent advice	19	26%
Estimate of potential savings / losses	19	26%
Disclosure (e.g. fee structure, rules)	18	24%
Information about the process / cost of search / switching	17	23%
General information about the market / product or warning	14	19%
Past fees / charges the consumer incurred	12	16%
Information about the benefits of search / switching	11	15%
Graphical illustration	10	14%
Personalised estimates	10	14%
Other offers from the same provider	9	12%
Offers from competitors	9	12%
Use of social norms / highlighting other people's mistakes	9	12%
Reminder	7	9%
Cost summary with repr. examples / based on expected usage	6	8%
Eliciting implementation intentions	4	5%
Reference to price comparison website	2	3%
Total	74	

1.4.2. 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.[23] 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. This lack of default interventions could be due to the fact that the outcome measure is

[23] Note, however, that according to the definition applied in DellaVigna and Linos (2020), even this intervention would not qualify as default as it does not change the outcome automatically if an individual remains passive. The definition in Jachimowicz et al (2019) appears broader, it encompasses all interventions that consist of pre-selecting one choice option to increase the likelihood of its uptake.

a deliberate act of the consumer (shopping around or switching), which she may or may not decide to do. This contrasts with a choice between different options (e.g. retirement savings plans) in which case defaults can be more easily applied while the consumer remains passive.

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.

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.[24]

Finally, while in almost all studies a large proportion (20-60%) of respondents indicate that a nudge would encourage them to search or switch,[25] 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). This is of concern as in the presence of price discrimination, increasing the engagement among already engaged consumers may not affect or even increase prices the less engaged consumers face (Fletcher, 2021).

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.

1.4.3. Results of the quantitative review

The findings below are based on the 19 papers with comparable quantitative estimates (as shown in Table 1.3) using the three different methods (calculation of averages, regression analysis and the best estimate analysis) described above in section 1.3. The detailed results are shown in appendices 1.3 to 1.5.

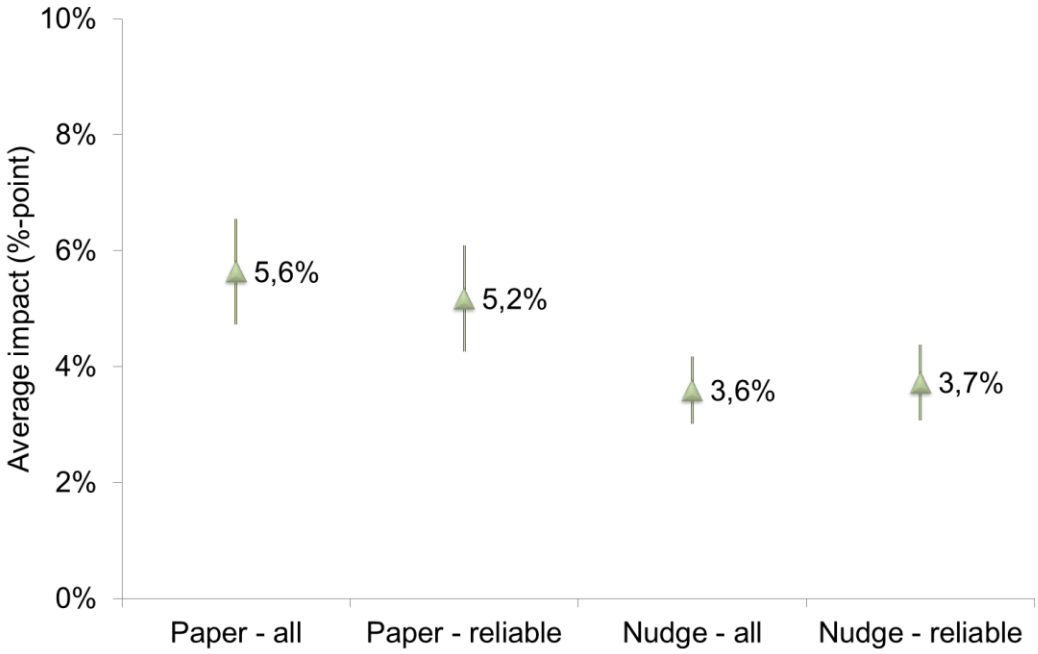
Result 1 – the overall average impact of nudge interventions is a 4-6 percentage point increase in search / switching

Using comparable estimates from 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 1.1 below, this varies slightly depending on whether the estimates are weighted using the inverse of the number of estimates by paper or by nudge (labelled as “Paper” and “Nudge”), and also whether less reliable

[24] In fact, theoretical modelling suggests that the imposition of measures encouraging internal switching could even be detrimental to consumers (Beckert and Siciliani, 2021).
[25] 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.

estimates (such as those that come from non-causal analyses or use self-reported outcome measures) are included or not (labelled as “all” and “reliable”).

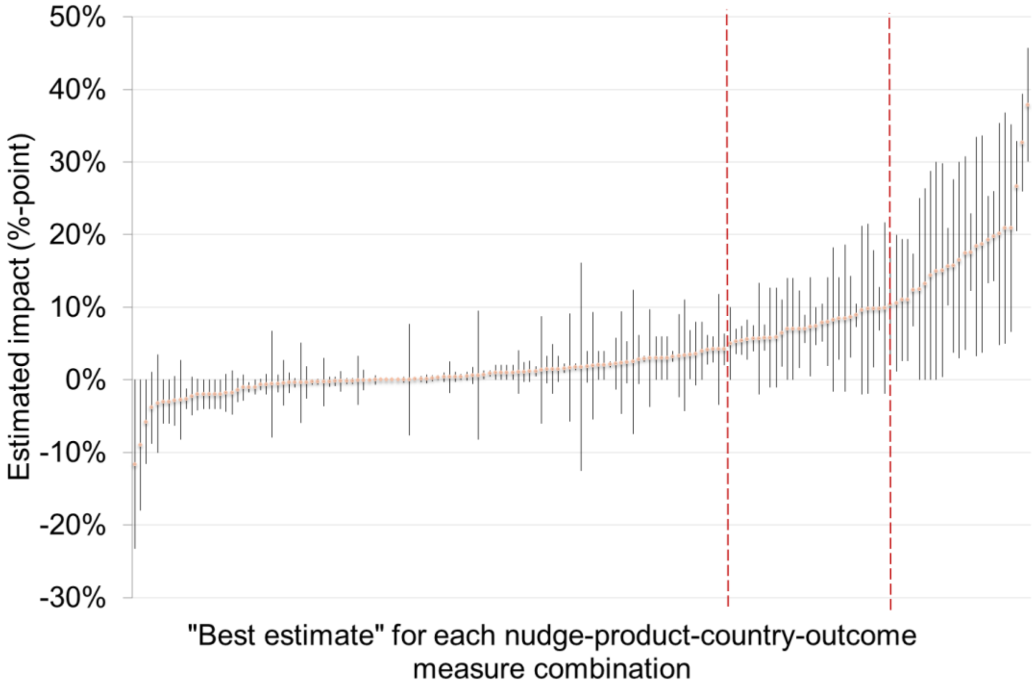
Figure 1.1: Overall average impact of nudge interventions (point estimates and confidence intervals)



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: 461, sample size reliable: 408, (iv) vertical lines show confidence intervals at 95% significance level

These results are confirmed in the best estimate analysis that finds a 4.6 percentage point average impact for all estimates (158 observations) and a 5.5 percentage point impact for reliable estimates only (117 observations). Only a third of this set of observations show an estimated impact above 5 percentage points and only about 16% generate one above 10 percentage points. This is shown on Figure 1.2 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 1.2: Distribution of best estimates (point estimates and confidence intervals)

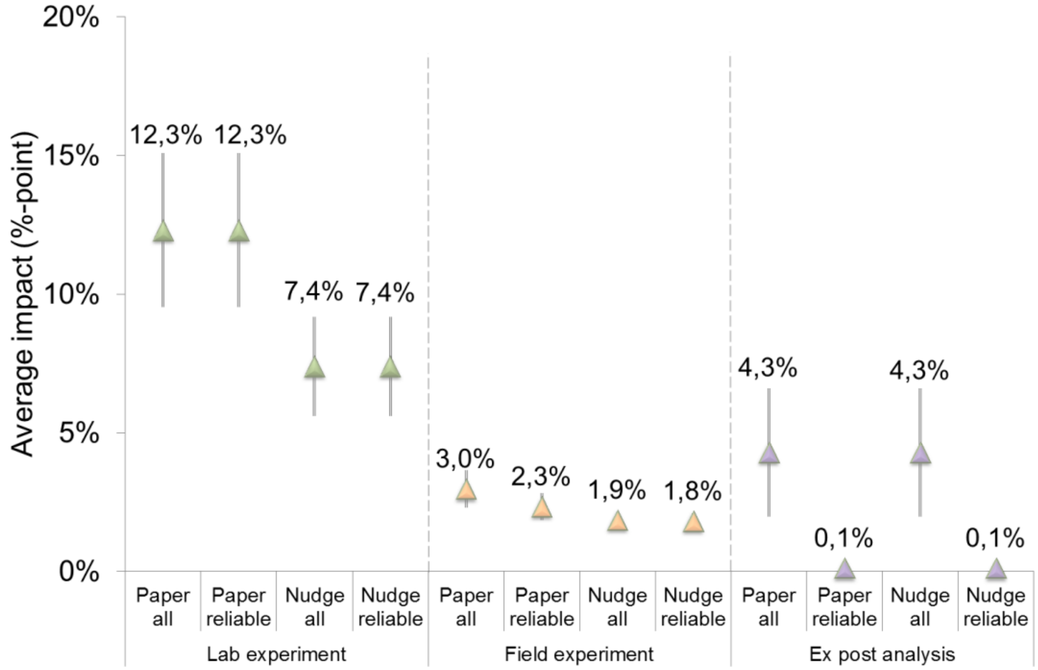


Notes: (i) sample size 157; (ii) one outlier is excluded; (iii) red vertical lines indicate 5 and 10 percentage point impact; (iv) vertical lines per point estimate show confidence intervals at 95% significance level

Result 2 – 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 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 7 and 12 percentage points in the lab, 2-3 percentage points in the field and basically zero in *ex post* analyses. This is shown on Figure 1.3 below.

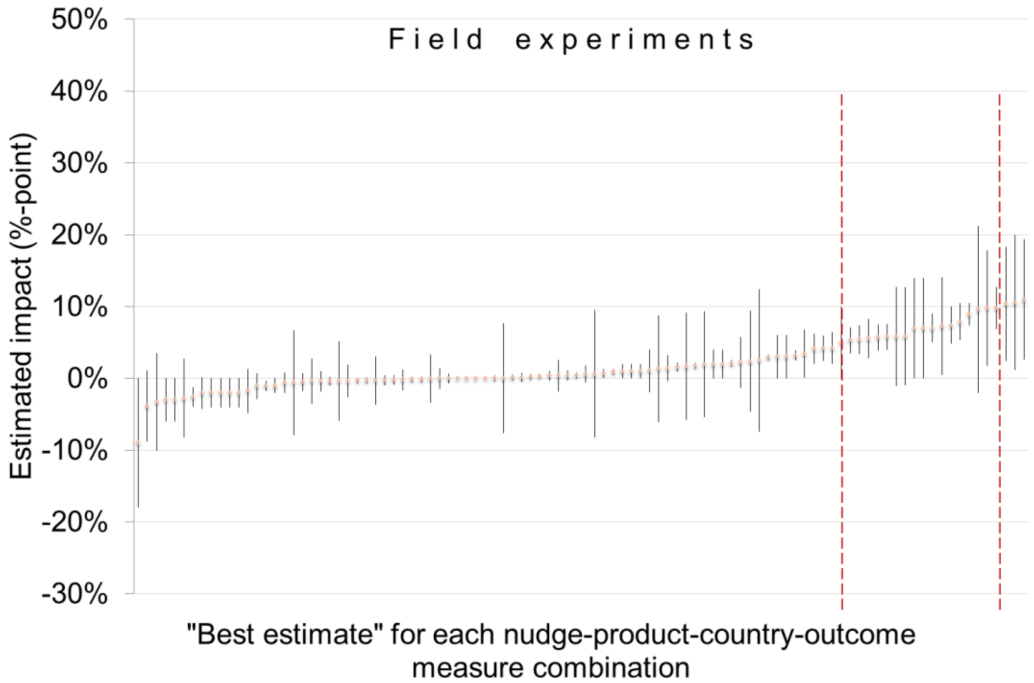
Figure 1.3: Average impact of nudge intervention by study design (point estimates and confidence intervals)



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 lab experiments: all 127, reliable 127; field experiments: all 241, reliable 192, *ex post* analysis: all 93, reliable 89, (iv) vertical lines show confidence intervals at 95% significance level

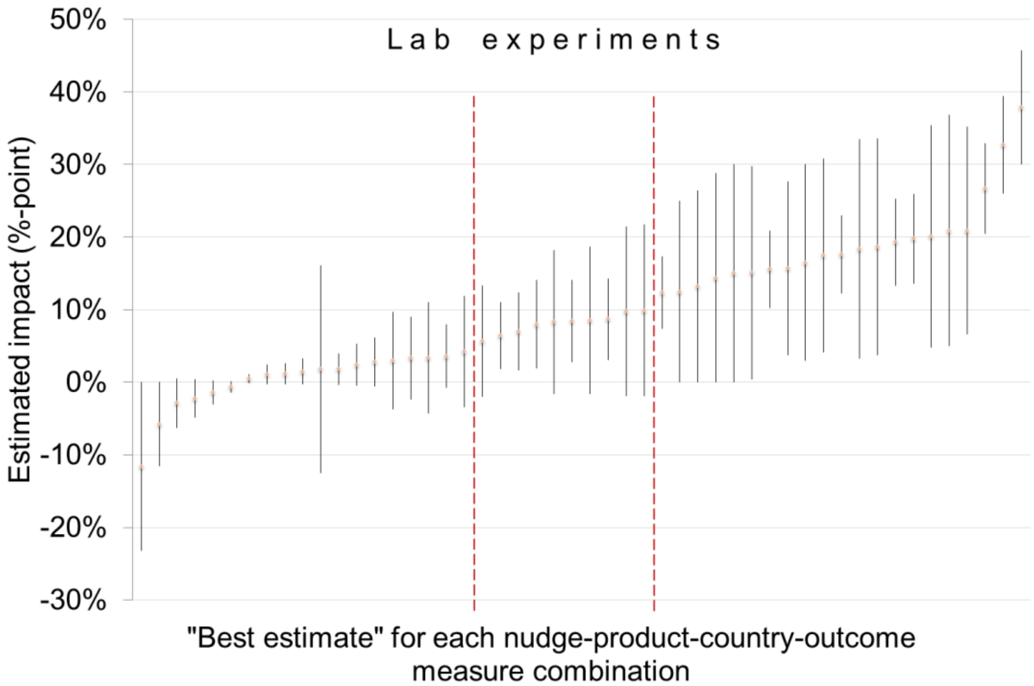
The above results are confirmed in the best estimate analysis which shows an 8 percentage point difference between the average impact found in lab experiments and in field experiments. When looking at the distribution of best estimates separately by study design, I find that only 3% of the estimates are above 10 percentage points in field experiments, compared to 43% 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 1.4 and Figure 1.5 below that split the data points from Figure 1.2 by study design (excluding *ex post* analyses). Figure 1.4 and Figure 1.5 also highlight that the confidence intervals tend to be larger for estimates in lab experiments.

Figure 1.4: Distribution of best estimates in field experiments (point estimates and confidence intervals)



Notes: (i) sample size: 98; (ii) red vertical lines indicate 5 and 10 percentage point impact; (iii) vertical lines per point estimate show confidence intervals at 95% significance level

Figure 1.5: Distribution of best estimates in lab experiments (point estimates and confidence intervals)



Notes: (i) sample size: 50; (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

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 lab experiment dummy is 10 percentage points with and without controlling for the type of nudge, the outcome measure and product (that is, lab experiments estimate a 10 percentage point higher impact than field experiments). These coefficients are statistically significant 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).[26] This is in line with how some of the lab experiments included in this review position their

[26] 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).

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).[27]

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.[28]

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 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.

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.

[27] 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.
[28] 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).

- 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.[29] Note also

[29] 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.

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.

Result 3 – certain types of nudges appear to work better than others

Figure 1.6 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 1.3.

Figure 1.6: 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	13% (-) 2 (0)			No impact 91 (89)		
Field experiment	No impact 6	2-3% 31 (28)	2-4% 19 (15)	1-2% 174 (132)	9% 6	No impact 5
Lab experiment				5-8% 108	8% 1	21% 18

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

As shown on Figure 1.6, 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 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). Ten studies estimated the impact of a number of different informational nudges in field experiments, and the average is a 1-2 percentage point increase in search and switching with very few observations over 10 percentage points. Lab experiments (Oxera-CESS, 2016 and Suter et al, 2019) show a somewhat higher impact on average (5-8 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 an online 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 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 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.[30] Adams et al (2021) investigated the impact of sending a letter to cash savings customers with a tear-

[30] 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).

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). Another 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 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 (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).[31]

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:

[31] 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).

- 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 79 nudges with comparable estimates that fall into the combined informational category.

The difference between the average impact of the two categories is 13-15 percentage points with informational nudges averaging around 2-4 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 1.6. 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.

Result 4 – 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. Field experiments do not show any impact on users of current accounts and credit cards. Interventions in insurance, mortgages and pensions have a higher impact when tested in lab experiments but limited 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,[32] a large part of this 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.

Result 5 – 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 1A.9 in Appendix 1.7.

However, field experiments measure the impact of nudges more often on switching (197 observations out of 241), whereas lab experiments tend to use outcome measures of search (97 out of 127 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, I assess the difference in the estimated average impact between search and switching outcome measures separately for different study designs (see Table 1A.10 in Appendix 1.7).

I find that the difference in average estimated impact on search and switching is not robust to different weighting regimes for 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

[32] In the regression analysis, only the coefficient of the cash savings dummy is significant (relative to current accounts) when further controls are included.

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 is only mildly significant 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 1A.5).

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.[33]

Result 6 – 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. Nine 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

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

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.

Result 7 – the review of quantitative studies with no comparable estimate does not change the above conclusions

Out of the 26 papers with quantitative analysis, seven 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.

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. Finally, Timmons et al (2019) estimated the impact of a detailed guidance on consumers' willingness to switch on a scale of one to seven, and concluded that after reading the guidance respondents' self-assessed confidence increased and participants who felt more competent were more willing to switch. However, this is a subjective outcome measure that describes future intentions, rather than past actions and as such, cannot be expected to reliably estimate the quantitative impact of an intervention. Taken all this together, I believe that the findings of these papers do not change the picture drawn from the quantitative analysis.

Result 8 – publication bias is unlikely to be a major concern

DellaVigna and Linos (2022) find that a 7.3 percentage point difference between the average impact of nudge interventions in academic publications and in a comprehensive set of 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. Given the findings in DellaVigna and Linos (2022), one could expect the presence of publication bias in these papers. However, 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 a six percentage point increase in shopping around but no impact on switching. Only the remaining one purely academic study finds a significant, almost ten percentage point impact on switching (Kling et al, 2012). It is, therefore, unlikely that the results of academic publications are heavily biased upwards.

Second, as far as policy research is concerned, about half of the quantitative studies 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.

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 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.

1.5. Summary

Following a systematic literature search, I identified 35 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. Lab experiments are considered to be useful in ranking different interventions but they are likely to overestimate the actual impact of these. There are only a few *ex post* evaluations and even these suffer from methodological issues (such as the lack of establishing causality). This is unfortunate not only because *ex post* evaluations are in principle the most reliable source for assessing the impact on search and switching but also because they can take into account supplier response (which is not possible to assess in experiments) and provide additional incentives to suppliers to act in a way that helps achieve the desired outcomes (Fletcher, 2021). 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.

APPENDICES FOR CHAPTER 1

Appendix 1.1 – Details of the literature search

The literature search covered the websites of the following institutions.

Financial regulators:

- Australian Securities & Investments Commission
- Financial Consumer Agency of Canada
- Central Bank of Ireland
- Financial Conduct Authority (UK)
- Consumer Financial Protection Bureau (US)

Competition authorities:

- Australian Competition & Consumer Commission
- Competition Bureau Canada
- European Commission
- Competition and Consumer Protection Commission (Ireland)
- Competition and Markets Authority (UK)
- Federal Trade Commission (US)
- Department of Justice (US)

Nudge units:

- The Behavioural Economics Team of the Australian Government
- Behavioural Economics in Action at Rotman (Canada)
- Competence Centre on Behavioural Insights (EU)
- Irish Government Economic and Evaluation Service
- Economic & Social Research Institute, Behavioural Research Unit (Ireland)
- Behavioural Insights Team (UK)
- Social and Behavioral Sciences Team (US)
- Ideas42 (US)

International organisations:

- OECD
- World Bank

Databases:

- TEN
- RePEc
- NBER
- Open Grey
- Proquest
- EthOS

In addition, I searched using the following search engines:

- Google
- Google Scholar
- Microsoft Academic

The five journals selected for hand-searching (covering editions between 2015 and 2020):

- Journal of Behavioral and Experimental Finance
- Journal of Behavioral Finance
- Journal of Behavioral Economics for Policy
- Behavioural Public Policy
- Behavioral Science & Policy

List of search terms used:

- 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
- intervention

The exact search term used depends on the domain (e.g. I searched for financial products on the website of competition authorities and for search and switching on the websites of financial regulators).

Appendix 1.2 – List of variables recorded for estimates in the quantitative papers

General information about the study

1. Study design: Type of the study, such as 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 doing it 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 1.4.1.
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 estimates, 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 the estimate can be found in the original paper (page number, table, etc.).

Appendix 1.3 – Detailed results, averages

Table 1A.1: 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	461	0,056	0,005	0,048	0,065
By search/switching					
Search	145	0,083	0,007	0,068	0,097
Switching	316	0,028	0,003	0,023	0,033
By design					
Field experiment	241	0,030	0,003	0,023	0,036
Lab experiment	127	0,123	0,014	0,096	0,150
Ex post analysis	93	0,043	0,012	0,020	0,065
By search / switching and design					
Search					
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					
Lab experiment	30	0,090	0,009	0,073	0,107
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	66	0,017	0,005	0,007	0,027
Insurance	161	0,090	0,012	0,067	0,113
Mortgages	4	0,023	0,011	0,001	0,045
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					
Lab experiment	16	0,032	0,010	0,012	0,052
Field experiment	22	0,004	0,004	-0,004	0,011
Ex post analysis	28	0,001	0,000	0,000	0,001
Insurance					
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
Mortgages					
Lab experiment	3	0,044	0,023	0,000	0,088
Field experiment	1	0,002	0,002	-0,002	0,006
Pensions					
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	373	0,033	0,003	0,028	0,038
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					
Lab experiment	1	0,080	0,031	0,019	0,141
Field experiment	6	0,089	0,010	0,070	0,107
Informational					
Lab experiment	108	0,085	0,006	0,072	0,097
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					
Lab experiment	18	0,213	0,048	0,118	0,308
Field experiment	5	-0,004	0,002	-0,007	-0,001

Table 1A.2: 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	408	0,052	0,005	0,043	0,061
By search/switching					
Search	116	0,093	0,009	0,076	0,110
Switching	292	0,023	0,002	0,019	0,026
By design					
Field experiment	192	0,023	0,002	0,019	0,028
Lab experiment	127	0,123	0,014	0,096	0,150
Ex post analysis	89	0,001	0,001	-0,001	0,003
By search / switching and design					
Search					
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					
Lab experiment	30	0,090	0,009	0,073	0,107
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	44	0,021	0,007	0,008	0,035
Insurance	142	0,078	0,013	0,052	0,105
Mortgages	4	0,023	0,011	0,001	0,045
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					
Lab experiment	16	0,032	0,010	0,012	0,052
Field experiment					
Ex post analysis	28	0,001	0,000	0,000	0,001
Insurance					
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
Mortgages					
Lab experiment	3	0,044	0,023	0,000	0,088
Field experiment	1	0,002	0,002	-0,002	0,006
Pensions					
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	329	0,029	0,002	0,026	0,032
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					
Lab experiment	1	0,080	0,031	0,019	0,141
Field experiment	6	0,089	0,010	0,070	0,107
Informational					
Lab experiment	108	0,085	0,006	0,072	0,097
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					
Lab experiment	18	0,213	0,048	0,118	0,308
Field experiment	5	-0,004	0,002	-0,007	-0,001

Table 1A.3: 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	461	0,036	0,003	0,030	0,041
By search/switching					
Search	145	0,063	0,006	0,051	0,075
Switching	316	0,024	0,002	0,020	0,027
By design					
Field experiment	241	0,019	0,001	0,016	0,021
Lab experiment	127	0,074	0,009	0,057	0,091
Ex post analysis	93	0,043	0,012	0,020	0,065
By search / switching and design					
Search					
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					
Lab experiment	30	0,080	0,009	0,063	0,098
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	66	0,010	0,004	0,002	0,018
Insurance	161	0,072	0,011	0,050	0,094
Mortgages	4	0,034	0,017	0,001	0,067
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					
Lab experiment	16	0,021	0,010	0,002	0,040
Field experiment	22	0,004	0,004	-0,004	0,011
Ex post analysis	28	0,001	0,000	0,000	0,001
Insurance					
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
Mortgages					
Lab experiment	3	0,044	0,023	0,000	0,088
Field experiment	1	0,002	0,002	-0,002	0,006
Pensions					
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	373	0,023	0,002	0,019	0,028
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					
Lab experiment	1	0,080	0,031	0,019	0,141
Field experiment	6	0,090	0,012	0,066	0,114
Informational					
Lab experiment	108	0,047	0,006	0,036	0,059
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					
Lab experiment	18	0,213	0,048	0,118	0,308
Field experiment	5	-0,012	0,004	-0,019	-0,005

Table 1A.4: 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	408	0,037	0,003	0,031	0,044
By search/switching					
Search	116	0,069	0,007	0,055	0,084
Switching	292	0,026	0,002	0,023	0,030
By design					
Field experiment	192	0,018	0,001	0,016	0,020
Lab experiment	127	0,074	0,009	0,057	0,091
Ex post analysis	89	0,001	0,001	-0,001	0,003
By search / switching and design					
Search					
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					
Lab experiment	30	0,080	0,009	0,063	0,098
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	44	0,018	0,008	0,002	0,035
Insurance	142	0,062	0,012	0,038	0,086
Mortgages	4	0,034	0,017	0,001	0,067
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					
Lab experiment	16	0,021	0,010	0,002	0,040
Field experiment					
Ex post analysis	28	0,001	0,000	0,000	0,001
Insurance					
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
Mortgages					
Lab experiment	3	0,044	0,023	0,000	0,088
Field experiment	1	0,002	0,002	-0,002	0,006
Pensions					
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	329	0,026	0,002	0,021	0,030
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					
Lab experiment	1	0,080	0,031	0,019	0,141
Field experiment	6	0,090	0,012	0,066	0,114
Informational					
Lab experiment	108	0,047	0,006	0,036	0,059
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					
Lab experiment	18	0,213	0,048	0,118	0,308
Field experiment	5	-0,012	0,004	-0,019	-0,005

Appendix 1.4 – Detailed results, regressions

Table 1A.5: Regression results

		All	Reliable	Field experiments	Field experiments, reliable
Search vs. switching	Switching	(dropped)	(dropped)	(dropped)	(dropped)
	Search	0.022*	0,019	0.031**	0.056***
		(0.012)	(0.016)	(0.010)	(0.005)
Study design	Field experiment	(dropped)	(dropped)		
	Ex post analysis	0,005	0.010		
		(0.009)	(0.010)		
	Lab experiment	0.097***	0.099***		
		(0.012)	(0.013)		
Type of nudge	Informational	(dropped)	(dropped)	(dropped)	(dropped)
	Disclosure	0.020	0.000	-0,007	-0,001
		(0.028)	(0.007)	(0.009)	(0.003)
	Increases in ease and convenience	0.049**	0.049**	0.067***	0.045***
		(0.021)	(0.020)	(0.011)	(0.007)
	Reminder	0,006	0,003	0,004	0,002
		(0.010)	(0.010)	(0.011)	(0.010)
	Simplification	0,003	0,006	0.000	0,002
		(0.008)	(0.011)	(0.009)	(0.004)
	Structural change	0,028	0,033	-0.021*	-0.052***
		(0.024)	(0.028)	(0.010)	(0.006)
Product	Current accounts	(dropped)	(dropped)	(dropped)	
	Cash savings	0.049**	0.062***	0.033*	(dropped)
		(0.017)	(0.021)	(0.015)	
	Credit cards	0.013	0.025	-0.003	-0.037**
		(0.012)	(0.017)	(.)	(0.015)
	Insurance	0.016	0.023	0.001	-0.039**
		(0.011)	(0.016)	(0.009)	(0.013)
	Mortgages	-0.026	-0.016	-0.002	-0.036**
		(0.019)	(0.021)	(.)	(0.015)
	Pensions	0.012	0.027	-0.017***	-0.046**
		(0.019)	(0.023)	(0.004)	(0.014)
	Personal loans	-0.026	-0.013		
		(0.022)	(0.027)		
	Constant	-0.013	-0.025	0.004	0.038**
		(0.012)	(0.017)	(.)	(0.015)
	R-squared	0.375	0.385	0.308	0.465
	N	461	408	241	192

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

Appendix 1.5 – Detailed results, averages using the best estimate analysis

Table 1A.6: Averages, best estimate analysis

	All estimates	Reliable estimates only
All	0,046	0,055
By search/switching		
Search	0,072	0,087
Switching	0,027	0,033
By design		
Field experiment	0,017	0,018
Lab experiment	0,099	0,099
Ex post analysis	0,022	0,003
By product		
Cash savings	0,036	0,036
Current accounts	0,028	0,068
Insurance	0,049	0,053
Mortgages	0,034	0,034
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	0,040	0,051
Reminder	0,041	0,028
Simplification	0,025	0,020
Structural change	0,130	0,130

Appendix 1.6 – Results using combined nudge categories

Table 1A.7: 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	444	0,036	0,003	0,030	0,042
Paper reliable					
Structural	17	0,171	0,019	0,133	0,209
Informational	391	0,027	0,002	0,023	0,030
Nudge all					
Structural	17	0,171	0,028	0,115	0,227
Informational	444	0,024	0,002	0,021	0,028
Nudge reliable					
Structural	17	0,171	0,028	0,115	0,227
Informational	391	0,023	0,002	0,020	0,027

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 1A.8: Regression analysis including combined nudge categories

		All	Reliable	Field exp.	Field exp., reliable
Nudge category	Informational	(dropped)	(dropped)	(dropped)	(dropped)
	Structural	0.133**	0.140**	0.068***	0.057***
		(0.047)	(0.051)	(0.011)	(0.008)
Search vs. switching	Switching	(dropped)	(dropped)	(dropped)	(dropped)
	Search	0.012	0.004	0.030***	0.043***
		(0.013)	(0.019)	(0.009)	(0.009)
Study design	Field experiment	(dropped)	(dropped)		
	Ex post analysis	0.005	0.011		
		(0.007)	(0.009)		
	Lab experiment	0.087***	0.092***		
		(0.015)	(0.013)		
Product	Current accounts	(dropped)	(dropped)	(dropped)	
	Cash savings	0.045**	0.057**	0.034**	(dropped)
		(0.017)	(0.020)	(0.013)	
	Credit cards	0.010	0.022	-0.003	-0.038**
		(0.010)	(0.014)	(.)	(0.013)
	Insurance	0.013	0.017	0.002	-0.037**
		(0.010)	(0.014)	(0.006)	(0.013)
	Mortgages	-0.022	-0.014	-0.002	-0.036**
		(0.017)	(0.019)	(.)	(0.013)
	Pensions	0.020	0.036*	-0.019***	-0.051***
		(0.019)	(0.020)	(0.004)	(0.014)
	Personal loans	-0.010	0.005		
		(0.023)	(0.025)		
	Constant	-0.010	-0.021	0.004***	0.038**
		(0.010)	(0.014)	(0.000)	(0.013)
	R-squared	0.467	0.489	0.311	0.435
	N	461	408	241	192

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

Appendix 1.7 – Average impact by outcome measure

Table 1A.9: Average impact of nudge interventions by search and switching

	Paper - all	Paper - reliable	Nudge - all	Nudge - reliable
Search	0.083	0.093	0.063	0.069
Switching	0.028	0.023	0.024	0.026
Difference	0.055	0.070	0.039	0.043

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 – search all 145, switching all 316, search reliable 116, switching reliable 292

Table 1A.10: Difference between average impact for outcome measures of search and switching by study design

	Paper - all	Paper - reliable	Nudge - all	Nudge - reliable
Lab experiment	0.047	0.047	0.000	0.000
Field experiment	0.021	0.039	0.028	0.031
Ex post analysis	0.079		0.079	

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 lab experiment all 127, reliable 127, 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

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2. Harmful search[34]

2.1. Introduction

Technological innovation has led to consumers buying a number of complicated products without fully understanding how the product actually works or what each product specification means. Consumer electronics such as computer screens, tablets and sound systems are all products that we use on a daily basis but buy relatively infrequently, and unless we have a specific interest in technology, it is unlikely that we can make perfect sense of the information made available to us when choosing a product.

Not fully understanding the features of the product we are looking for also means that it is impossible to determine our own preferences precisely. Another way of thinking of this is that while consumers know their own preferences, they do not understand the mapping between their preferences and the product features. For example, a review site lists the following key features of a computer screen: "superb sRGB colour and contrast; loads of adjustment options; good selection of ports; no real Adobe RGB ability; some uniformity issues; not bright enough for HDR".[35] Clearly, reading such information is not informative for a consumer without knowledge of these specifications (even if she has a rough idea about what she would like to get). Compared to the effort required to understand these aspects, price information tends to be simple and easily understandable.

Consumers facing such a purchase decision have two options: either they invest some time in understanding the meaning of technical specifications so that they can translate them to their own preferences, or simply pick a product based on the easily observable price and find out while using it whether it is a good fit or not. In this sense, consumers have a choice of treating these products as search or experience goods. Note, however, that given that these products are typically used for a number of years during which technological innovation can change the key product characteristics, the consumer does not benefit much from previous experience when facing a new purchase decision.

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[34] I would like to thank Andrew Rhodes, Massimo Motta, Chris Wilson and Stephen Davies for their comments on this chapter.

[35] See https://www.trustedreviews.com/reviews/asus-proart-pa279cv, accessed on 15 March 2022.

This paper investigates equilibrium outcomes when consumers observe prices but are unable to interpret product features without making an effort to learn about them (i.e. without search). While firms offer these kinds of products at a range of quality levels, in this paper I focus on horizontal differentiation. This could be interpreted as assuming that consumers know quality levels and only choose products from a particular category.[36]

I show that consumers will choose to invest in understanding product characteristics if the cost of this effort is low compared to the expected disutility from not getting the best match. For example, if a consumer expects to use a computer screen often or for a particular purpose (e.g. gaming), she may think it is worth working out which product meets her needs best. On the contrary, if she is not too choosy or the cost of understanding product characteristics is particularly high, she may decide to choose the cheapest product (available at a given quality level).

The main finding of this paper is that for complex products that require some mental effort and time to evaluate, the decision to incur these costs – which may be a perfectly rational decision *ex ante* – can actually make consumers worse off. The underlying mechanism is that firms are only able to take advantage of the fact that some consumers prefer their product to the competitors' (i.e. to benefit from product differentiation) if consumers make an effort to understand their own preferences and firms' offerings. Otherwise consumers choose simply on the basis of the easily observable price without knowing what exactly they are buying, which results in more aggressive price competition.[37]

Under these market conditions firms have no incentives to make search more difficult. Instead, it would be in their best interest to reduce search costs so that consumers get a better understanding of their own valuation of the product. However, given the inherent complexity of such products, it is limited how much firms can do to improve consumers' understanding without them incurring the cost of learning (so search costs are assumed to be exogenously determined in the model).

[36] While each of the characteristics may be vertical in the sense that the higher (or lower) some value is, the better the quality, considering all features as a package can be described as horizontal differentiation.
[37] This trade-off between match quality and prices has been recognised in the literature (see, for example, Armstrong and Zhou, 2022).

My findings are consistent with existing research that studies markets of horizontally differentiated products with observable prices and costly search for match values, that finds that market prices can indeed be higher when search costs are lower (see the next section on the related literature). However, a distinctive feature of my model is that it allows consumers to buy from firms after costlessly observing prices but without searching their offerings. I consider this assumption realistic as in many markets consumers are able to buy products without understanding what they are getting. Nonetheless, with a few recent exceptions (see next section), existing literature typically requires that consumers can only buy after searching the firm which results in becoming fully informed about the characteristics of its product. A few papers allow for partial product evaluation but they are different in some other key assumptions, such as using a monopoly setting, sequential search or assuming that consumers deduce rather than observe prices.

The paper is structured as follows. The related literature is summarised in the next section. Section 2.3 presents the base model and derives the conclusions. Sections 2.4 and 2.5 introduce extensions, section 2.6 summarises the derived equilibria and section 2.7 concludes.

2.2. Related literature

This paper adds to the search cost literature which explores market outcomes when consumers are not fully informed initially but can acquire information at cost. The first search cost models assume that products are homogeneous and consumers need to incur search costs to learn prices (e.g. Salop and Stiglitz, 1977; Stahl, 1989). Seminal papers that introduce horizontal product differentiation into search cost models are Wolinsky (1986) and Anderson and Renault (1999). In these papers consumers observe neither prices nor match values. More recent papers incorporating horizontal differentiation and search for price and match value (at the same time) include Armstrong et al (2009), Zhou (2011), Bar-Isaac et al (2012), Larson (2013), Zhou (2014) and Garcia-Shelegia (2018). All of these papers assume that consumers search firms sequentially and some focus on exploring the order of visiting firms. For example, Armstrong et al (2009) assume that consumers visit the most prominent firm first, Zhou (2011) introduces a predetermined search order and the consumers in Garcia-Shelegia (2018) decide which firm to sample first after observing previous consumers' decisions.

An early model that separates the cost of searching for price information and for information on product characteristics is developed in Bakos (1997) in the context of electronic marketplaces that reduce consumers' search costs. The paper discusses a scenario where the price information is available for free but consumers face some cost to search for product characteristics. This is conceptually the same as the starting point of my model and leads to a similar result (if the price is observable but product characteristics are not, firms will compete price down to marginal cost). However, Bakos (1997) uses a different framework (Salop's circular model), assumes a different search process (sequential search) and interprets the consumer's problem differently (consumers do not know the location of the firms).

As price information became widely and easily accessible on the Internet, the search cost literature developed further models with observable prices and costly search for product characteristics. Some papers investigate consumers' optimal search strategies and/or firms' willingness to provide information in monopoly settings (Branco et al, 2012; Branco et al, 2016; Liu et al, 2019). Oligopoly models with horizontal product differentiation, observable prices and costly search for match value include Ke et al (2016), Armstrong (2017), Choi et al (2018), Ding-Zhang (2018) and Haan et al (2018). Having conceptually the same starting point as my model, many of these papers reach the conclusion that equilibrium prices can be higher when search costs are lower. Note that all of these papers assume sequential search, whereby the consumer needs to incur the search cost every time she visits a new firm. This is a major difference to the assumption in the model presented here. I consider that once a consumer has incurred the cost of understanding product specifications, she can costlessly evaluate the products of each firm. Just like prices, most information about products is easily accessible on the Internet, so in my model the search cost is incurred once to learn how to evaluate them.

There exist a couple of papers that use the same modelling framework (Hotelling line) that I do, albeit still different in assuming sequential search. Shen (2015) separates match value into two parts: consumers know their location which represents their *ex ante* brand preference, but they still need to search to learn the additional match value they attach to each firm's product. Armstrong and Zhou (2011) include several models in their paper, one of them being a Hotelling duopoly with observable prices and costly search for match value. One key difference in these papers to my model (in addition to sequential rather than simultaneous search) is that consumers are only able to buy from a firm once they have incurred the search cost to learn its match value. In Armstrong and Zhou (2011) incurring the search cost once when sampling the first firm reveals the value of both products for the consumer (as she learns her location and so the distance from both firms) but they still require that the consumer incurs the search cost again if she wants to buy from the second firm. I relax these conditions – in my model consumers are able to purchase products after observing the price, with or without understanding how much they value the products.

Very recently, Chen et al (2021) and Petrikaité (2022) published research in which they build duopoly models where consumers are able to buy a product after observing the price but without learning its match value. There are, however, some key differences to my model. They do not use the Hotelling framework, and both assume sequential search. In addition, Chen et al (2021) introduce a prior value of the product that consumers costlessly observe, while Petrikaité (2022) assumes that the first search is costless. Earlier research that allows consumers to buy without learning the match value assumes that consumers *ex ante* are also uninformed about prices, and can buy the product without knowing the price (Gamp, 2015) or incur a separate cost to learn the price first (Fishman and Lubensky, 2018).

Liu and Dukes (2016) and Chen et al (2022) allow consumers to buy products after partial evaluation. Liu and Dukes (2016) assume simultaneous search where consumers decide about how many products to sample and the evaluation depth of the sampled products. In their paper, consumers deduce rather than observe firms' prices. Chen et al (2022) use sequential search where price is a characteristic that the consumer always learns if she decides to evaluate the product in some depth. In either paper, consumers are unable to buy from a firm they did not sample or visit.

The three key elements of my model are horizontal differentiation, observable prices and costly search for product characteristics. Papers that investigate a similar setting but differ in one of these key assumptions include (i) papers with horizontal differentiation, known match values but costly search for prices (Anderson and Renault, 2000; Kuksov, 2004), and (ii) papers with observable prices and costly search for product quality (i.e. vertical differentiation; Bester and Ritzberger, 2001; Boyacı and Akçay, 2018). There are similarities with the findings of some of these papers. For example, Anderson and Renault (2000) find that prices are lower the greater the proportion of uninformed consumers – a result I obtain in the extension with heterogeneous search costs. Kuksov (2004) concludes that lower search costs can lead to higher product differentiation, which then decreases price competition, which is again in line with my results.

Another stream of related literature is on advertising. Similarly to search cost models, these papers assume imperfect information but in this case consumers obtain information through firm advertising rather than search. Cabral (2000) describes a model of *informative* advertising with similar assumptions to the base model presented in this paper. The key lesson of that model is that absent advertising firms price at marginal cost but advertising transforms the Bertrand outcome to the Hotelling outcome – just as search does in my model. Meurer and Stahl (1994) discuss a model with horizontally differentiated products and two groups of consumers, informed and uninformed, where both groups observe prices but uninformed consumers do not know their best match. Firms can educate consumers by sending out adverts at cost and consumers receiving an advert become fully informed. Meurer and Stahl (1994) find that consumer surplus can fall with advertising as advertising increases product differentiation and thereby market power, which is similar to my finding that consumer surplus can fall if consumers search for product characteristics.

Further, Anderson and Renault (2009) investigate the effect of *comparative* advertising in a model where products are both vertically and horizontally differentiated. Consumers know qualities and can become informed about horizontally differentiated attributes *via* adverts. They find that comparative advertising helps consumers, improves the profitability of the small firm but reduces overall welfare because of the reduction in the large firm's profits. Marz (2019) presents a duopoly model with *persuasive* advertising, and shows that persuasive advertising has anti-competitive effects if consumers are aware of prices but uncertain about match values.

Whilst these findings point in the same direction (more information does not necessarily increase consumer welfare), there is a key difference in whether the market player who changes consumers' information set benefits from it or not. Firms are willing to invest in advertising because it softens price competition and leads to higher profits. Consumers search as it is rational for them to do so, not realising that it may lead to a worse outcome for them on aggregate.

A number of papers combine search costs with advertising, i.e. assume that consumers can obtain information through search or adverts (e.g. Robert and Stahl, 1993; Bagwell and Ramey, 1994; Bester and Petrakis, 1995; Janssen and Non, 2008; and McCarthy, 2016). I am not aware of any papers that use the same key assumptions to my model and also incorporate advertising.

My finding that firms prefer consumers to have low search costs is at odds with the findings of the obfuscation literature. This is because most of the papers discussing obfuscation strategies assume that consumers cannot observe or compare prices and higher search costs mean that consumers are more likely to stop once they have found an acceptable price or indeed to choose randomly (e.g. Wilson, 2010; Ellison and Wolitzky, 2012; Piccione and Spiegler, 2012). On the contrary, in my model prices are directly observable and comparable and search costs are incurred to find out more about product differentiation (which allows firms to extract more of the surplus from consumers).

Finally, it is worth noting that papers presenting firms' obfuscation strategies often assume that consumers can observe the price of the main product but not the price of the add-ons (e.g. Ellison, 2005; and Gabaix and Laibson, 2006). The main difference between these models and my model is that obtaining information about add-ons does not increase the firms' market power, on the contrary, it leads to more intense price competition, whereas understanding product characteristics allows firms to charge higher prices. As a result, the findings on welfare and firms' incentives are qualitatively different.

2.3. Base case – homogeneous search costs

Framework

The framework I use is the standard horizontal differentiation model of Hotelling with a line of unit length between 0 and 1. There are two firms, A and B, each producing one product at zero marginal cost. Firm A is located at 0 and firm B is located at 1.[38] Firms compete on price and are unable to price discriminate.

There are a large number of consumers (normalised to one), uniformly distributed along the line. Each consumer values the product at v and has unit demand. Consumers observe the location of firms as well as their prices but they do not

[38] I discuss the intuition of allowing for endogenous location choice at the end of this section.

know their *own* location. In other words, consumers know that they are somewhere between 0 and 1 along the line but do not know exactly where. This is the assumption that reflects the consumers' problem of initially not knowing how to map features of products and services into their own preferences.

A consumer can learn her own location at a fixed search cost of $s > 0$. This can be thought of as investing costly time and effort in understanding product features, which allows consumers to determine their own preferences. Once a consumer incurred the search cost, she understands how far she is from either firm. Consumers are able to purchase from a firm with or without incurring s.

Firms are unable to influence the size of consumers' search cost. This assumption reflects the fact that while firms may be able to inform consumers about the features of their product through advertising, it is limited how much they can do in terms of improving consumers' understanding of technical and more detailed terms.

A consumer located at x and buying from firm A located at 0 incurs a cost of tx^2, while the same consumer buying from firm B located at 1 incurs a cost of $t(1-x)^2$, where $t > 0$ is the unit transport cost. The total transport cost gives the cost of buying a product that does not perfectly match the consumer's needs.

The transport cost t and the search cost s are common knowledge.[39]

Timing

First, firms set prices p_A and p_B. Second, consumers observe these prices and decide whether to choose a product based on the observed prices or to search.

Consumers' decision

As consumers are assumed to be homogeneous, all will make the same decision whether to choose based on price or search. If consumers decide not to search, they buy from the firm that set a lower price or choose randomly between the two firms if they set the same price. If consumers search, they incur the search cost to learn their own location and then choose the product that gives a higher surplus taking into account both the price and the transport cost.

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[39] It is a common assumption in the search cost literature that consumers' search costs are known by firms. In fact, the optimal pricing strategy of firms is normally derived using the cost of information acquisition, e.g. where consumers search sequentially, firms set a price such that the expected benefit of continuing the search would be equal to the cost of an additional search.

No search

The expected surplus of consumers if they do not make an effort to find out their location but choose between the two firms based on price is:

$$E(CS_{ns}) = v - min\{p_A, p_B\} - tE(x^2) = v - min\{p_A, p_B\} - \frac{t}{3} \tag{1}$$

See Derivation 1 in Appendix 2.1. Note that while the consumer does not know how far the selected product is from her preferences (i.e. does not know x), she still expects to derive some disutility from not getting a product that perfectly matches her taste.

Note also that for consumers to be willing to enter the market without searching, it must be the case that:

$$E(CS_{ns}) = v - min\{p_A, p_B\} - \frac{t}{3} \geq 0 \leftrightarrow v - \frac{t}{3} \geq min\{p_A, p_B\} \tag{2}$$

Search

If consumers decide to search, they need to incur the search cost but can determine which firm they prefer to buy from so the consumer surplus is:

$$CS_s = v - min\{(p_A + tx^2), (p_B + t(1-x)^2)\} - s \tag{3}$$

Under perfect information, there is a consumer who is indifferent between buying from firm A and firm B, given prices p_A and p_B. Let us denote the location of this consumer by $\bar{x}$ and assume that the price difference is such that $0 < \bar{x} < 1$.[40] Knowing prices but not her location, a consumer expects to prefer firm A with probability $\bar{x}$, and firm B with probability $1 - \bar{x}$. Thus, the expected consumer surplus if consumers search is:

$$E(CS_s) = v - \left[\int_0^{\bar{x}} (p_A + tx^2)dx + \int_{\bar{x}}^1 (p_B + t(1-x)^2)dx\right] - s =$$

$$= v - \left[p_B + \frac{t}{3} - \frac{(p_B - p_A + t)^2}{4t}\right] - s \tag{4}$$

See Derivation 2 in Appendix 2.1. For consumers to be willing to enter the market and search, it must be the case that:

$$E(CS_s) = v - \left[p_B + \frac{t}{3} - \frac{(p_B - p_A + t)^2}{4t}\right] - s \geq 0 \leftrightarrow v - \frac{t}{3} - s \geq p_B - \frac{(p_B - p_A + t)^2}{4t} \tag{5}$$

[40] This condition holds in equilibrium.

Search vs. no search

Consumers will search if the expected consumer surplus is higher under searching than otherwise:

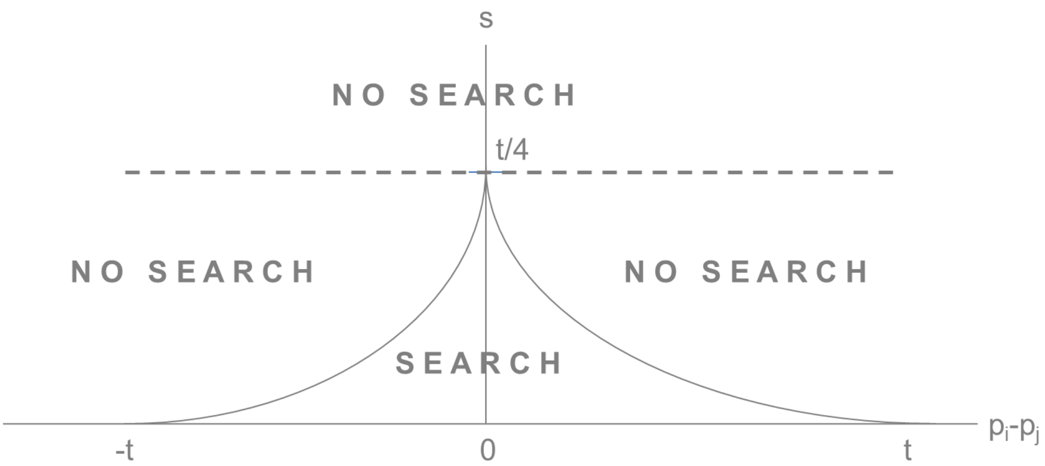
$$E(CS_{ns}) \leq E(CS_s) \quad (6)$$

This translates into the following condition:

$$s \leq \frac{(p_i - p_j + t)^2}{4t} \; if \; p_i \leq p_j, i, j = \{A, B\} \quad (7)$$

See Derivation 3 in Appendix 2.1. Notice that when $p_i = p_j$, inequation (7) simplifies to $s \leq \frac{t}{4}$. In addition, when $p_i < p_j$, it is true that $\frac{(p_i - p_j + t)^2}{4t} < \frac{t}{4}$.[41] This implies that if the search cost is sufficiently large ($s > \frac{t}{4}$), consumers will never decide to search irrespective of whether firms charge the same price or not. If the search cost is not that large relative to the transport cost, consumers' decision whether to search or not also depends on the observed price difference: the larger the price difference between firms, the less likely that the condition for search will hold, i.e. the more likely that consumers will just buy from the firm that sets a lower price. The consumers' decision is illustrated on Figure 1 below.

Figure 2.1: The consumers' decision



The trade-off consumers face is the following: if they choose based on price, they avoid the search cost but are unable to minimise the sum of the price and the

[41] For this to hold, it must also be true that $|p_i - p_j| < 2t$. As it will be shown, the highest price firms might charge in equilibrium is t, the lowest is their marginal cost, 0, therefore the above condition will be satisfied.

transport cost. On the contrary, if they search, they will incur the search cost but can take into account in their decision the actual rather than the expected travel costs. It is thus intuitive that the relative size of the search and transport costs determines the consumers' action, and that consumers will decide to search if the transport cost is relatively large compared to the search cost.

The intuition behind the result that consumers are less likely to search if they observe a large price difference is that the larger this difference, the less transport cost can matter for consumers in their decision.

Firms' decision and equilibrium outcomes

Recall that the transport cost and the search cost are common knowledge, which firms can take into account when setting prices.

If the relative size of transport and search costs is such that consumers would never decide to search ($s \geq \frac{t}{4}$) but would choose a product based on price, the standard homogeneous Bertrand result applies:

$$p_{ns}^{*} = 0 \tag{8}$$

In this case, both firms earn zero profit. The intuition behind this result is that without understanding their own preferences, consumers cannot decide which product they prefer, and so they view the two products as equally good (or bad) matches. This results in consumers behaving as if the products were homogeneous.

If the transport cost is relatively large compared to the search costs ($s < \frac{t}{4}$), consumers' decision whether to search will depend not only on the relative size of these costs but also on the difference between the prices charged.

If consumers search and become fully informed, firms can maximise profit by setting the Hotelling price:

$$p_{s}^{*} = t \tag{9}$$

See Derivation 4 in Appendix 2.1. In this case, firms split the market equally and earn a profit of $\frac{t}{2}$ each.

This equilibrium is locally stable. If one of the firms decided to decrease its price by a small amount, the resulting price difference would be insufficient to incentivise consumers to simply buy the cheaper product instead of searching. As a result, neither firm has an incentive to undercut its rival by a small amount.

The Hotelling price is a globally stable equilibrium if no firm would find it profitable to unilaterally decrease its price to the level where it can capture the whole market. To attract all consumers (and stop them searching), one firm would need to decrease its price to a level where inequation (7) does not hold. This is not profitable if the profit earned at this much lower price is lower than the Hotelling profit. As shown in Derivation 5 in Appendix 2.1, this is true (the Hotelling equilibrium is globally stable) if:

$$s \leq \frac{t}{16} \tag{10}$$

Thus, when the search cost is sufficiently small compared to the transport cost, it is not worth undercutting the Hotelling price. The reason is that with low search costs, it is hard to convince consumers to stop searching and choose the cheaper product. It would require such a low price (compared to the Hotelling price) that would result in a smaller profit than when both firms charge the Hotelling price.

In summary,

- if $s \geq \frac{t}{4}$, consumers choose based on price and firms set prices at marginal costs, and
- if $s \leq \frac{t}{16}$, consumers search and firms set the Hotelling price.

However, the model does not have a single price equilibrium for the middle range of parameters, that is, when $\frac{t}{16} < s < \frac{t}{4}$. For these parameters, if there is no or little difference between firm A's and firm B's price, consumers would search. If consumers search, firms have incentives to increase their prices to extract rent that consumers are willing to pay to obtain the preferred product. However, once one firm's price is high enough, the rival is able to undercut profitably – setting a low price that will prevent search and attract all consumers.

Given the homogeneity of consumers, there also cannot be a two-price equilibrium or a tractable price distribution equilibrium. The lack of tractable equilibrium is a common result in the search cost literature for models with observable prices and horizontal product differentiation (see, for example, Armstrong, 2017 and Ding-Zhang, 2018). Sections 2.4 and 2.5 introduce extensions that are able to partially overcome this problem. Before turning to these, I investigate consumer and producer welfare in the two existing equilibria.

Welfare and implications

The following table summarises consumer, producer and total surplus in the two equilibria described above (assuming that consumers' valuation is large enough to participate in the market).

Table 2.1: Welfare in the two equilibria in the base case

	Search cost small relative to transport cost ($s \leq t/16$)	**Search cost large relative to transport cost ($t/4 \leq s$)**
Consumer surplus	$v - t - t/12 - s$	$v - t/3$
Producer surplus	t	0
Total surplus	$v - t/12 - s$	$v - t/3$

Note: The transport cost of consumers for $s \leq t/16$ is derived in Appendix 2.1 in Derivation 6.

Firms are clearly better off in the search equilibrium. They make zero profit if the search cost is so high that consumers decide to remain uninformed whereas they can earn some rent if the search cost is sufficiently low so that consumers search. This is because once consumers understand their own preferences they are willing to pay a premium for getting the product that better matches their taste. Thus, firms benefit from lower search costs as that means consumers are more likely to make the effort to learn, which leads to softer price competition.

Consumers are better off in the equilibrium when they do not search. As explained above, this is due to the fact that search will make horizontal differentiation apparent which increases consumers' willingness to pay for the preferred product and allows firms to set a higher price. Thus, even though searching ensures that all consumers can choose the product that best meets their needs, this comes at a cost that makes it overall worse for consumers than if they had chosen based on price only. Consumers are essentially in a prisoner's dilemma type of situation when search costs are small: it may be individually rational for each of them to search but they would all be better off if they could agree and commit not to search.

The consumers' decision whether to search or not leads to an outcome that is optimal if we are considering social welfare, i.e. including both consumer and producer surplus.[42] That is, where the search cost is small compared to the transport cost, there is higher surplus generated if consumers search, and where the search cost is large compared to the transport cost, there is higher surplus generated if consumers do not search.

The results imply that consumers do not necessarily benefit from search. If search costs are high enough to stop consumers learning product characteristics, competition will push the easily observable prices down to the extent that consumers benefit from remaining uninformed. In addition, if firms are able to differentiate themselves, they do not have incentives to make search more difficult.[43] Instead, it is in their best interest to minimise search costs and maximise product differentiation. While the model does not appear to have a tractable equilibrium for the middle range of parameters, and therefore it does not offer general insights for policymakers, it does show that lower search costs may not always benefit consumers.

Endogenous location choice

The model assumes that firms' locations (at the two ends of the line) are exogenously given. Under perfect information and quadratic transportation cost, it can be shown that this is exactly how profit-maximising firms would choose their location (see Tirole, 1988). This is because the strategic effect (firms want to move away from the centre to increase product differentiation) dominates the market share effect (firms want to move towards the centre to increase market share).

This result is likely to hold in my model for parameters where consumers may decide to search. In addition to the strategic effect, here firms also have incentives to locate further away from each other to incentivise consumers to search. This is because it becomes more important to know to which firm the consumer is located closer when firms are far away from each other. If the two firms are close to each other, there is little to gain by knowing which is closer to the consumer.

When search costs are so high that consumers would never decide to search, the strategic effect disappears and firms are expected to locate in the centre (as

[42] $v-\frac{t}{3}\geq v-\frac{t}{12}-s$ if $\frac{t}{4}\leq s$ and $v-\frac{t}{3}\leq v-\frac{t}{12}-s$ if $\frac{t}{16}\geq s$.

[43] In this paper, search costs are exogenously given but it is easy to see that firms would have incentives to reduce consumers' search costs if they were able to do so.

then consumers would choose based on the observed price and the expected distance). In other words, if firms cannot extract any rent through product differentiation, they will offer homogeneous products.

Thus, depending on the relative size of the search and the transport cost, introducing endogenous location choice may result in firms locating at the centre or at the two ends of the line. Equilibrium prices are the same as with exogenous location: price equals marginal cost when consumers do not search and the Hotelling price when they do search. However, when firms are located in the centre, consumers travel a smaller distance overall, which results in higher consumer welfare than in the case with exogenous location choice.[44]

[44] The expected transport cost when both firms are located in the middle and consumers do not know their own location is: $tE(x^2) = t\int_0^{0.5} x^2 f(x)dx + t\int_{0.5}^{1}(1-x)^2 f(x)dx = \frac{t}{12}$, compared to the transport cost of $\frac{t}{3}$ when firms are located at the two ends.

2.4. Extension I – cost of foregone sales

As explained above, the problem with the base case model is that there does not appear to be a tractable equilibrium for the middle range of parameters. The argument is as follows. Whether consumers search or not depends on the observed price difference – if it is large, they simply decide to buy from the cheaper firm. Given this, a firm would have an incentive to set a price much lower than its rival's, stop consumers searching and sell to all of them. The rival would then also decrease its price to gain some sales, at which point consumers would again decide to search. But when consumers search and understand product features, firms have an incentive to increase their prices, up to the point when it is again profitable to undercut.

One potential solution to the problem of no equilibrium is if it becomes less attractive for firms to sell to consumers who do not search. Let us call these 'uninformed' purchases (consumers), while purchases that occur after searching are 'informed'. I investigate below what happens if firms incur an additional cost when they sell to uninformed consumers which does not arise when selling to informed consumers.

Consider a consumer who buys a product without understanding its features. She is more likely to be disappointed with how the product works than someone who bought it knowing what to expect. As a result, she is more likely to conclude that this firm does not offer products that match her preferences and so she will refrain from further purchases from the same firm. This could be rational if the firm's other products are similarly poorly matched to her preferences, or irrational if product profiles are independent of each other across markets and over time. In the remainder of this section, I refer to this as the 'cost of foregone sales'.[45,46]

I incorporate the cost of foregone sales into the model by introducing a marginal cost c. In the case of uninformed purchases, $c > 0$. While not all uninformed

[45] Another element of this cost could be the impact of negative reviews by disappointed consumers on future sales. However, such a setup would require a dynamic setting in which consumers can make decisions not only based on price and location but also past reviews, and goes beyond the scope of this paper.

[46] An alternative explanation for a cost that the firm incurs when selling to uninformed consumers could also relate to having to provide additional advice after sale or dealing with complaints. Consumers who do not understand the features of the product they have purchased may contact the customer services team for help or submit complaints that need to be dealt with.

consumers will be disappointed and refrain from future purchases from the same firm, firms can distribute their total cost of foregone sales across all uninformed purchases; hence it can be represented as a marginal cost. I assume that informed purchases do not lead to loss of future sales, hence the cost of foregone sales of serving informed consumers is zero.

I assume that consumers are myopic in the sense that they do not consider the impact of their current decision whether to search or not on future purchases. In other words, the consumers' decision is assumed to be unaffected by the introduction of the cost of foregone sales into the model. This implies that their decision rules are the same as in the base case model. Given this, when the search cost is so high relative to the transport cost that consumers would never search ($s \geq \frac{t}{4}$), firms again compete the price down to the marginal cost, which, in this case, is the cost of foregone sales as all consumers buy uninformed: $p_{ns}^* = c$.

When search costs are lower relative to the transport cost ($s < \frac{t}{4}$), consumers' decision whether to search also depends on the observed price difference. When consumers search, the profit-maximising price is the Hotelling price, as before. This remains the equilibrium outcome for small values of search cost, that is, when $s \leq \frac{t}{16}$. The question is under what circumstances the cost of foregone sales makes the Hotelling price a globally stable equilibrium outcome for $\frac{t}{16} < s < \frac{t}{4}$. We know from Derivation 5 in Appendix 2.1 that if one firm sets the Hotelling price t, the highest price that the rival can set to stop consumers searching and sell to all of them is $2\sqrt{ts}$. Hence the profit it can earn is $2\sqrt{ts} - c$, as it will sell to all consumers but all buy without becoming informed. The undercutting profit is lower than the Hotelling profit if $c > \frac{t}{2}$; as shown in Derivation 7 in Appendix 2.1. Thus, $p_s^* = t \; if \; c > \frac{t}{2}$.

Thus, the existence of equilibrium requires the assumption that the cost of foregone sales is at least half of the price the consumer pays for the product. Recall that this is a cost that the firm bears, not the consumer. Its magnitude depends on to what extent the negative experience of uninformed consumers affects firms' future sales. If firms are active on a number of product markets where the same consumers are present or if they sell updated versions of the same product over time, it could lead to a sharp decline in sales, in which case the assumption is plausible.

The table below shows the welfare implications in the two equilibria.

Table 2.2: Welfare in the two equilibria after including the cost of foregone sales

	Search cost smaller relative to transport cost $(s < t/4)$	**Search cost large relative to transport cost $(t/4 \leq s)$**
Consumer surplus	$v - t - t/12 - s$	$v - \mathrm{c} - t/3$
Producer surplus	t	0
Total surplus	$v - t/12 - s$	$v - \mathrm{c} - t/3$

As before, firms are clearly better off when consumers search. Whether consumer welfare is higher in the Hotelling or the Bertrand outcome depends on the relative size of consumers' search and transport cost, and the firms' foregone sales cost. If the cost of foregone sales is not too large $\left(c < \frac{3}{4}t + s\right)$, consumers are better off not searching and paying the price equal to this cost. When this cost is relatively large, consumer surplus is higher with search.

The consumers' decision whether to search or not leads to an outcome that is optimal if we are considering social welfare, i.e. including both consumer and producer surplus, whenever $s < \frac{t}{4}$.[47] On the contrary, when $\frac{\mathrm{t}}{4} \leq s$, the resulting Bertrand outcome is only socially optimal if the cost of foregone sales is relatively small $\left(c < s - \frac{t}{4}\right)$ and the search cost is relatively large $\left(s > \frac{3}{4}t\right)$.

In other words, if the search cost is small compared to the transport cost, there is higher surplus generated if consumers search, which is the equilibrium outcome. If the search cost is large compared to the transport cost, whether the equilibrium outcome of no search and marginal cost pricing is optimal also depends on the (relative) size of the foregone sales cost.

The policy implications are mostly in line with that of the base case model. Firms have no incentives to make search more difficult, and, in fact, with a potentially large impact on future sales, they actively want to stay away from selling to consumers who do not know what they are buying. Consumers, on the other hand, do not necessarily benefit from becoming informed. If the cost of foregone sales (that determines the price) is not too high, they may be better off not searching.

[47] $v - \mathrm{c} - \frac{t}{3} < v - \frac{t}{12} - s$ for $s < \frac{t}{4}$.

2.5. Extension II – heterogeneous search costs

The previous extension eliminates the problem of no equilibrium but only under the assumption that firms' foregone sales cost is at least half of the price consumers pay for the product. In this section I turn to a different solution: introducing heterogeneous search costs into the model (and dropping the cost of foregone sales). This is common in the search cost literature, many models assume that consumers differ with respect to how much information they initially hold and/or at what cost they can acquire further information (e.g. Salop and Stiglitz, 1977; Varian, 1980; Stahl, 1996; Armstrong and Zhou, 2011).

Here I assume that some consumers (proportion $0 < \alpha < 1$) have search cost $s > 0$, and the remaining consumers (proportion $1 - \alpha$) have zero search costs. The former is the group of (initially) uninformed consumers who need to search to find out their location and the latter is the group of informed consumers who do not need to search.

The informed consumers always choose the best offer taking into account location and prices. The uninformed consumers are in the exact same situation as all the consumers in the base model described in Section 2.3 and so decide whether to search or not depending on the relative size of the transport and search costs. I present the low and high search cost cases separately below.

Lower search costs ($s < \frac{t}{4}$)

In this scenario, uninformed consumers' search cost is relatively low. Whether they decide to search or choose based on price depends on the relative size of search and transport costs, as well as the observed price difference.

As before, when the search cost is very small, that is, when $s \leq \frac{t}{16}$, the Hotelling outcome prevails, irrespective of the proportion of uninformed consumers. However, for the middle range of parameters ($\frac{t}{16} < s < \frac{t}{4}$), the situation is now different. Before, the Hotelling price was worth undercutting because one firm individually deviating could increase the price difference by a large amount to a level where all consumers stopped searching and decided to buy from the cheaper firm. However, in the current version of the model there are $1 - \alpha$ consumers who always choose based on price and location and some of them would require a much larger decrease in price to buy from the deviating firm. The question I investigate below is whether the presence of informed consumers with zero search cost makes the Hotelling equilibrium stable, and if so, under what conditions.

Firm A's demand can take the following three forms depending on the price difference between the two firms:

$$Q_A = (1-\alpha)\bar{x} + \alpha\bar{x} = \bar{x} \; if \; s \leq \frac{(p_i - p_j + t)^2}{4t}; \; p_i \leq p_j, i, j = \{A, B\} \tag{11}$$

$$Q_A = (1-\alpha)\bar{x} + \alpha \; if \; s > \frac{(p_A - p_B + t)^2}{4t} \; and \; p_A < p_B \tag{12}$$

$$Q_A = (1-\alpha)\bar{x} \; if \; s > \frac{(p_B - p_A + t)^2}{4t} \; and \; p_A > p_B \tag{13}$$

The first element in all three equations shows the demand from informed consumers, $\bar{x}$ being the location of the consumer who is indifferent between buying from firm A and firm B. Equation (11) shows firm A's demand if the price difference between the two firms is small,[48] uninformed consumers search and choose based on price and location. Equation (12) shows firm A's demand when it charges a much lower price than firm B and all uninformed consumers buy its product without search. Equation (13) shows firm A's demand if it charges a much higher price than firm B and none of the uninformed consumers buys its product.

Let us assume that $p_B = t$ and investigate firm A's best response. If firm A decides to charge a price that is sufficiently close to firm B's price so that uninformed consumers search, the best it can do is to charge $p_A = t$ and earn $\pi_A = \frac{t}{2}$. See Derivation 8 in Appendix 2.1. If firm A decides to charge a price that is sufficiently lower than firm B's price to stop uninformed consumers searching, the best it can do is to charge $p_A = 2\sqrt{ts}$ and earn a profit of $\pi_A = 2\sqrt{ts} - 2(1-\alpha)s$. See Derivation 9 in Appendix 2.1. When firm B charges the Hotelling price, it is of course not a profitable strategy for firm A to increase its price. See Derivation 10 in Appendix 2.1.

From this, the Hotelling price is the equilibrium price if:

$$\frac{t}{2} > 2\sqrt{ts} - 2(1-\alpha)s$$

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[48] This is included in the condition $s \leq \frac{(p_i - p_j + t)^2}{4t}$.

$$1-\frac{\sqrt{ts}}{s}+\frac{t}{4s}>\alpha \qquad (14)$$

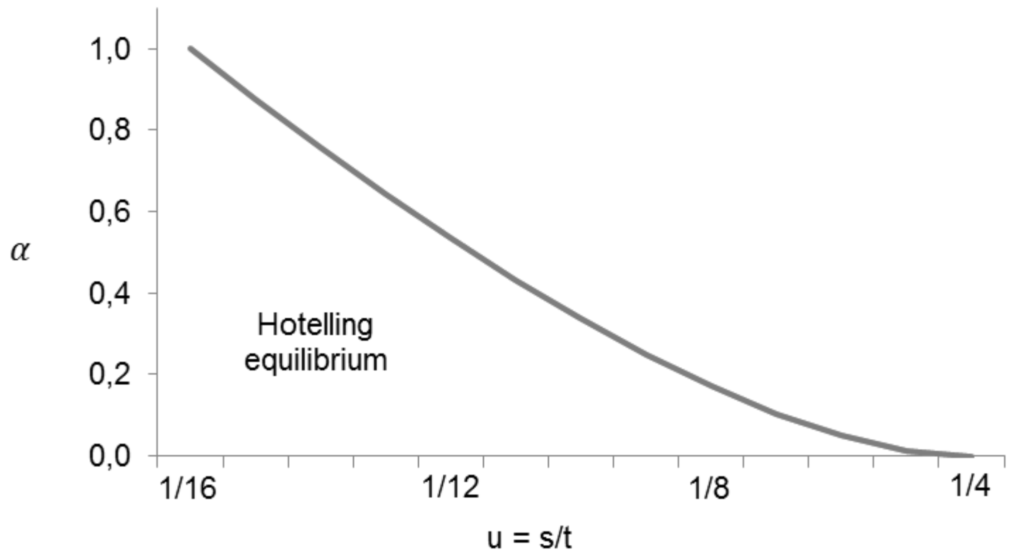
See Derivation 11 in Appendix 2.1. Note that the left hand side of inequation (14) is always positive for the relevant range of search and transport costs. As a result, for any pair of search and transport cost parameters, there is an α below which it is not worth undercutting the Hotelling price.

Introducing the notation $u=\frac{s}{t}$, where $\frac{1}{16}<u<\frac{1}{4}$, inequation (14) can be rewritten as:

$$1-\frac{\sqrt{u}}{u}+\frac{1}{4u}>\alpha \qquad (15)$$

Using inequation (15), the graph below depicts the range of αs for which the Hotelling price prevails in equilibrium for a given pair of search and transport costs.

Figure 2.2: The relationship between search cost, transport cost and the proportion of uninformed consumers



The graph above shows that as the search cost increases relative to the transport cost (that is, as u increases), charging the Hotelling price is an equilibrium strategy for a smaller and smaller proportion of uninformed consumers. At the extreme, where $u=\frac{1}{16}$, and so $s=\frac{t}{16}$, the Hotelling equilibrium is stable for any proportion of uninformed consumers (just like in the base model in the previous section where all consumers are initially uninformed). At the other extreme, where the search cost approaches its upper bound, $\frac{t}{4}$, even a small proportion of uninformed consumers can make the Hotelling equilibrium unstable as these uninformed consumers can be deterred from searching by a small decrease in price. However, unlike in the base model, pricing at marginal cost is not an equilibrium outcome here as firms face a strong incentive to extract rent from informed consumers who are willing to pay a higher price.

There is not a tractable equilibrium outcome for parameter values above the line on Figure 2; that is, where $\frac{t}{16} < s < \frac{t}{4}$ and inequality (15) does not hold.

High search costs ($s \geq \frac{t}{4}$)

In this scenario, uninformed consumers' search cost is so high that they never search, simply buy from the firm that charges a lower price without understanding what they are getting. This represents a downward pressure on firms' pricing. However, there is a proportion $1-\alpha$ consumers with zero search cost who always become informed and choose based on price *and* location. Firms thus have an incentive to charge higher prices and extract rent from these informed consumers.

The demand firm A faces depending on the relative prices is:

$$Q_A = (1-\alpha)\bar{x} + \alpha; if\ p_A < p_B \tag{16}$$

$$Q_A = (1-\alpha)\bar{x}; if\ p_A > p_B \tag{17}$$

$$Q_A = (1-\alpha)\bar{x} + \frac{\alpha}{2}; if\ p_A = p_B \tag{18}$$

The first element in all three equations is the demand from informed consumers. The second is the demand from uninformed consumers: all, if firm A charges a lower price, none, if it charges a higher price, and half, if it charges the same price as firm B.

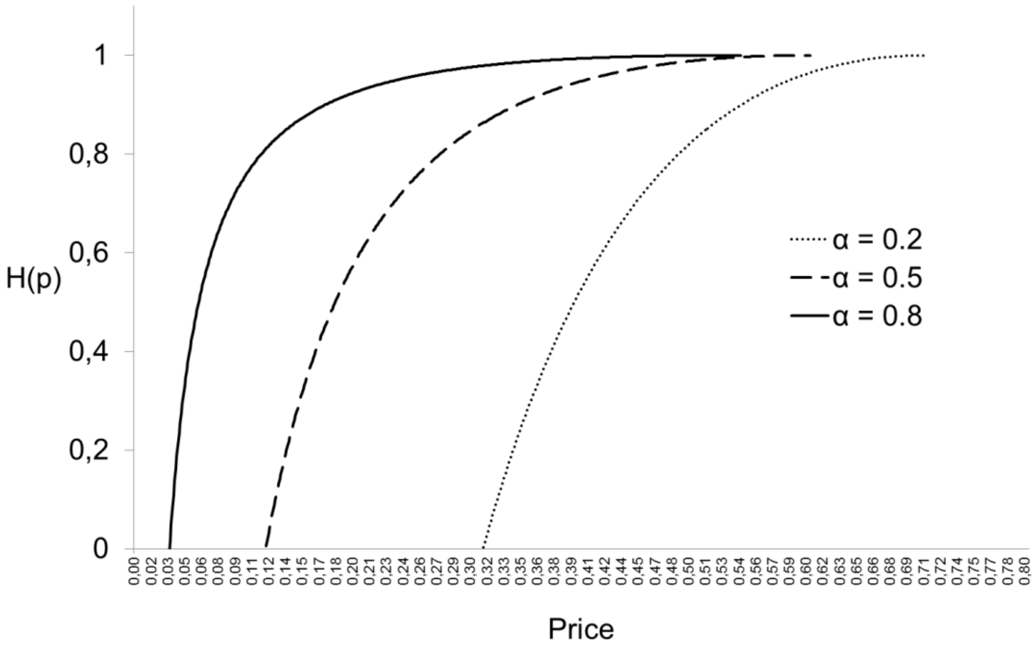
Given that a very small decrease in price can attract all uninformed consumers while still maintaining most sales to informed consumers, there cannot be a single price equilibrium. There is, however, a price distribution equilibrium whereby firms choose from a range of prices. This price distribution equilibrium is characterised by a cumulative distribution function $H(p)$ that satisfies the following equation:

$$\frac{1-\alpha}{2t}(\bar{p} + t - p) + \alpha\big(1 - H(p)\big) = \frac{\pi}{p} \tag{19}$$

where π is the firm's expected profit, $\bar{p} = \int_{p_{min}}^{p_{max}} p dH(p)$ is the firm's expected price in equilibrium and p_{min} and p_{max} are the lower and upper bounds of the price distribution. The derivation of this mixed-strategy equilibrium follows closely the steps in Armstrong-Zhou (2011) and is shown in Appendix 2.2.

The graph below depicts the cumulative distribution function of prices for small, medium and large proportions of uninformed consumers. The transport cost is assumed to be 1 for these illustrations.

Figure 2.3: Cumulative price distribution function for different proportions of uninformed consumers



Note: The transport cost is assumed to be 1.

As it is shown on Figure 2.3, both the minimum and the maximum price decreases as the proportion of uninformed consumers increases.[49] This implies that the presence of uninformed consumers benefits all consumers as they incentivise firms to offer lower prices. This result is confirmed in Table 2.3 that shows the expected price and profit for certain proportions of uninformed consumers and different values of transport cost.

[49] In fact, there is first-order stochastic dominance as we move from small proportions of uninformed consumers to larger proportions.

Table 2.3: Expected price and expected profit for selected values of transport cost and proportion of uninformed consumers

	Expected price			Expected profit		
	$t = 0.5$	$t = 1$	$t = 1.5$	$t = 0.5$	$t = 1$	$t = 1.5$
$\alpha = 0.2$	0.21	0.42	0.62	0.10	0.20	0.30
$\alpha = 0.5$	0.11	0.21	0.32	0.05	0.09	0.14
$\alpha = 0.8$	0.04	0.09	0.13	0.01	0.03	0.04

Table 2.3 shows that both the expected price and the firms' expected profit increases with the importance of product differentiation (that is, with the unit transport cost, t). On the contrary, the price and the profit decrease as the proportion of uninformed consumers rises.

Consumer surplus changes not only with the price but with the total transport cost consumers incur. Uninformed consumers do not know their location and are thus unable to minimise the sum of the price and the transport cost. Given this, a higher proportion of uninformed consumers means lower prices (as above) but higher overall transport cost. In order to see how consumer surplus changes with the proportion of uninformed consumers, I carried out numerical integration for the parameter values depicted on Figure 2.3. Numerical integration is used when it is not possible to find the antiderivative or when it is easier to compute a numerical approximation than to compute the value of the antiderivative.[50] It involves evaluating the integrand at a finite set of points, weighting these values and summing them up to obtain an approximate value of the integral. In my case, it involved calculating the total price paid and total transport cost incurred by both uninformed and informed consumers for a large number of combination of prices between the minimum and a maximum price. I then weighed these values with the probability of those prices occurring (given the probability distribution function) and summed it all up to obtain a value of the expected total cost of consumers for a given proportion of uninformed consumers. Appendix 2.3 includes further details and a stylised table as an example. The results of the numerical integration show that consumer surplus increases with the proportion of uninformed consumers.

[50] The antiderivative of a function f is a differentiable function F whose derivative is equal to the original function f.

Welfare and implications

In this section, I compare the equilibrium outcomes with heterogeneous search costs for the two scenarios when uninformed consumers may search ($s < \frac{t}{4}$) and when their search cost is prohibitive and so they never search ($s \geq \frac{t}{4}$). Note that I can only compare consumer welfare for parameter values for which there exists a tractable equilibrium (see Figure 2.2 above).

Using numerical integration to calculate welfare in the price distribution equilibrium, I again find that consumer welfare is higher when search costs are high. For example, assuming that the unit transport cost is one ($t = 1$) and 20% of consumers are initially uninformed ($\alpha = 0.2$), the total cost of consumers is about twice as much when uninformed consumers search and firms set the Hotelling price than when these consumers do not search and firms choose from a price distribution. The total cost of consumers includes the price they pay and the transport cost, that is, the disutility of buying a product that does not perfectly match their taste. The difference in consumer welfare increases with the proportion of uninformed consumers: when $\alpha = 0.5$, total consumer cost is about 2.8 times bigger with search than without and when $\alpha = 0.8$, the multiplier is about 3.3. Firms earn more in the Hotelling equilibrium than with price distribution.

Notwithstanding the fact that there is no tractable equilibrium for all parameter values, the results of this extension reinforce the conclusion from the base model with homogeneous search costs – in the presence of product differentiation, observable prices and initially unknown product features, consumers do not necessarily benefit from search or lower search costs. Firms, on the contrary, clearly benefit from consumers becoming more informed and choosing products which match their taste.

2.6. Equilibria

The table below summarises equilibria in the three cases discussed above.

Table 2.4: Equilibria in the three cases

	Base case	Extension I	Extension II
Small search cost ($s \leq t/16$)	Hotelling outcome	Hotelling outcome	Hotelling outcome
Medium search cost ($t/16 < s < t/4$)	No tractable equilibrium	Hotelling outcome if the cost of foregone sales is bigger than half of the transport cost, otherwise no tractable equilibrium	Hotelling outcome depending on the proportion of uninformed consumers, otherwise no tractable equilibrium
Large search cost ($t/4 \leq s$)	Bertrand outcome	Bertrand outcome with price equals to the cost of foregone sales	Price distribution equilibrium with prices lower than the Hotelling price

As can be seen in the table above, if understanding product features is relatively easy (the search cost is small), consumers will invest in doing so, learn their own preferences and pay a higher price. However, this scenario is less likely to occur in practice as it requires a relationship between the search cost and the transport cost in which the unit transport cost is at least 16 times bigger than the search cost. If products are differentiated to a large extent (the transport cost is large), it seems unlikely that the cost of understanding this differentiation is so small.

The middle range of parameters, where the transport cost is between 4 and 16 times larger than the search cost, is more likely to occur. Here the Hotelling equilibrium prevails under certain conditions (large foregone sales cost or relatively small proportion of uninformed consumers) but there is no tractable equilibrium for all parameter values. Future work could involve the introduction of a distribution of search costs across consumers. This more realistic assumption may lead to some consumers searching, others do not, and a more stable equilibrium outcome.

Finally, when the unit transport cost is smaller than four times the search cost, that is, when it is relatively costly to understand product features compared to the disutility of not getting their most preferred product, consumers do not search and firms compete on price more vigorously. This may be the case in many markets with products that have lots of different features (like high tech products), where it may not be too painful for a consumer not to get the perfect match but it is rather time consuming to learn and understand all product characteristics.

2.7. Summary

I presented three variations to the situation in which prices are easily observable, products are horizontally differentiated but consumers need to invest costly time and effort in understanding product features. The base case involves homogeneous consumers, to which the first extension adds the cost of foregone sales that the firm incurs when it sells to consumers who do not know what they are buying. The second extension instead adds heterogeneity by introducing two groups of consumers: one with positive search costs and another with zero search costs.

In all three cases there is some range of parameters for which a tractable equilibrium outcome does not exist. Notwithstanding this, the results are consistent in demonstrating the main message: on a market that is characterised by observable prices and complicated product differentiation, firms benefit from consumers learning about product features. Firms have no incentive to make search more difficult as they are able to extract rent when consumers are better informed about which products they prefer. This may explain why firms invest in showrooms that attract consumers and where they can demonstrate the features of their products. Making the search process less cumbersome by inviting consumers to experience the products themselves could be a way of reducing search costs and thereby allowing firms to set higher prices.

Consumers tend to be better off when they do not make the effort to understand product features, as choosing simply based on price incentivises firms to compete prices down. Consumers still incur some disutility from not getting the product that best fits their preferences but on aggregate this can be offset by the gain from paying lower prices. This is true even with consumer heterogeneity – the presence of consumers with prohibitive search costs leads to an equilibrium outcome that is overall better for consumers than if this group was able to learn about product characteristics.

This chapter provides a cautionary lesson for policy-makers: while search usually leads to more competitive outcomes, there may be market configurations in which the opposite is true. Future research could incorporate strategic motives of firms and investigate their impact on the outcome.

APPENDICES FOR CHAPTER 2

Appendix 2.1 – Derivations

Derivation 1

The expected consumer surplus if consumers choose based on price without searching is given by: $E(CS_{ns}) = v - min\{p_A, p_B\} - tE(x^2)$, where x is uniformly distributed on $(0,1)$. As $E(x^2) = \int_0^1 x^2 f(x)dx$ and $f(x) = 1$ under uniform distribution, the expected distance is $E(x^2) = \int_0^1 x^2 dx = \frac{x^3}{3}\Big|_0^1 = \frac{1}{3}$, which yields $E(CS_{ns}) = v - min\{p_A, p_B\} - \frac{t}{3}$.

Derivation 2

The expected consumer surplus if consumers search:

$$E(CS_s) = v - \left[\int_0^{\bar{x}} (p_A + tx^2)dx + \int_{\bar{x}}^1 (p_B + t(1-x)^2)dx\right] - s$$

$$= v - \left[p_A x + \frac{tx^3}{3}\Bigg|_0^{\bar{x}}\right] - \left[p_B x + tx - tx^2 + \frac{tx^3}{3}\Bigg|_{\bar{x}}^{1}\right] - s$$

$$= v - \left[p_A\bar{x} + \frac{t\bar{x}^3}{3} + p_B + t - t + \frac{t}{3} - p_B\bar{x} - t\bar{x} + t\bar{x}^2 - \frac{t\bar{x}^3}{3}\right] - s$$

$$= v - \left[(p_A - p_B - t)\bar{x} + t\bar{x}^2 + p_B + \frac{t}{3}\right] - s$$

Substituting $\bar{x} = \frac{p_B - p_A + t}{2t}$ (see Derivation 4) and assuming that the price difference is such that $0 < \bar{x} < 1$:

$$= v - \left[(p_A - p_B - t)\frac{p_B - p_A + t}{2t} + t\left(\frac{p_B - p_A + t}{2t}\right)^2 + p_B + \frac{t}{3}\right] - s$$

$$= v - \left[-\frac{(p_B - p_A + t)^2}{2t} + \frac{(p_B - p_A + t)^2}{4t} + p_B + \frac{t}{3}\right] - s$$

$$= v - \left[p_B + \frac{t}{3} - \frac{(p_B - p_A + t)^2}{4t}\right] - s$$

Derivation 3

Consumers will search if:

$$E(CS_p) \leq E(CS_s)$$

$$v - min\{p_A, p_B\} - \frac{t}{3} \leq v - \left[p_B + \frac{t}{3} - \frac{(p_B - p_A + t)^2}{4t}\right] - s$$

If $p_A < p_B$:

$$v - p_A - \frac{t}{3} \leq v - \left[-\frac{(p_B - p_A + t)^2}{4t} + p_B + \frac{t}{3} \right] - s$$

$$-p_A - \frac{t}{3} \leq \frac{(p_B - p_A + t)^2}{4t} - p_B - \frac{t}{3} - s$$

$$\mathrm{s} \leq \frac{(p_B - p_A + t)^2}{4t} + p_A - p_B$$

Defining $p_A - p_B \coloneqq M$, this can be rewritten as:

$$\mathrm{s} \leq \frac{(t - M)^2}{4t} + M$$

$$\mathrm{s} \leq \frac{t^2 - 2tM + M^2 + 4tM}{4t}$$

$$\mathrm{s} \leq \frac{t^2 + 2tM + M^2}{4t}$$

$$\mathrm{s} \leq \frac{(t + M)^2}{4t}$$

$$\mathrm{s} \leq \frac{(p_A - p_B + t)^2}{4t}$$

If $p_B < p_A$:

$$v - p_B - \frac{t}{3} \leq v - \left[-\frac{(p_B - p_A + t)^2}{4t} + p_B + \frac{t}{3} \right] - s$$

$$-p_B - \frac{t}{3} \leq \frac{(p_B - p_A + t)^2}{4t} - p_B - \frac{t}{3} - s$$

$$\mathrm{s} \leq \frac{(p_B - p_A + t)^2}{4t}$$

If $p_A = p_B$:

$$v - (0.5p_A + 0.5p_B) - \frac{t}{3} \leq v - \left[-\frac{(p_B - p_A + t)^2}{4t} + p_B + \frac{t}{3} \right] - s$$

$$\mathrm{s} \leq \frac{t^2}{4t}$$

$$\mathrm{s} \leq \frac{t}{4}$$

The three conditions combined:

If $p_i \leq p_j$:

$$s \leq \frac{\left(p_i - p_j + t\right)^2}{4t}$$

Derivation 4

The location of the consumer who is indifferent between buying from firm A and firm B (when A is located at 0 and B is located at 1) is given by: $p_A + tx^2 = p_B + t(1-x)^2$. From this, the firms' respective demand functions are: $\bar{x} = Q_A(p_A, p_B) = (p_B - p_A + t)/2t$ and $1 - \bar{x} = Q_B(p_A, p_B) = (p_A - p_B + t)/2t$. The profit functions are: $\pi_A = p_A(p_B - p_A + t)/2t$ and $\pi_B = p_B(p_A - p_B + t)/2t$, which gives the first-order conditions of: $p_B + t - 2p_A = 0$ and $p_A + t - 2p_B = 0$. Then, by symmetry, $p^* = t$.

Derivation 5

Consumers will not search if the reverse of equation (7) is satisfied:

$$s \geq \frac{(p_A - p_B + t)^2}{4t}$$

Keeping $p_B = t$:

$$s \geq \frac{(p_A - t + t)^2}{4t}$$

$$4ts \geq {p_A}^2$$

$$2\sqrt{ts} \geq p_A$$

The highest p_A that satisfies this condition is:

$$p_A = 2\sqrt{ts}$$

In this case firm A will capture the whole market so its profit is given by:

$$\pi_A = 2\sqrt{ts}$$

Thus, the Hotelling price equilibrium is stable if the undercutting profit is lower:

$$2\sqrt{ts} \leq \frac{t}{2}$$

$$4\sqrt{ts} \leq t$$

$$16ts \leq t^2$$

$$s \leq \frac{t}{16}$$

Derivation 6

When consumers search and firms charge the same price, the consumers between 0 and ½ will buy from firm A and the consumers between ½ and 1 will buy from firm B. Given uniform distribution and symmetry, the actual transport cost incurred by all consumers can be calculated as:

$$2t\int_0^{1/2} x^2 dx = 2t * \frac{x^3}{3}\bigg|_0^{1/2} = \frac{t}{12}$$

Derivation 7

The Hotelling price equilibrium is stable if the undercutting profit is lower:

$$2\sqrt{ts} - c < \frac{t}{2}$$

$$2\sqrt{ts} - \frac{t}{2} < c$$

Recall that this scenario involves $s < \frac{t}{4}$. As $s \to \frac{t}{4}$, $2\sqrt{ts} - \frac{t}{2} \to \frac{t}{2}$. Thus, the Hotelling equilibrium holds for any pair of t and s, as long as

$$\frac{t}{2} < c$$

Derivation 8

Substituting $\bar{x} = (p_B - p_A + t)/2t$ and $p_B = t$ into the profit function that corresponds to equation (11):

$$\pi_A = \bar{x}p_A = \frac{(t - p_A + t)}{2t} * p_A = p_A - \frac{{p_A}^2}{2t}$$

The FOC yields that:

$$\frac{\partial \pi_A}{\partial p_A} = 1 - \frac{p_A}{t} = 0 \leftrightarrow p_A = t$$

Resulting in profit of $\pi_A = \frac{t}{2}$.

Derivation 9

Substituting $\bar{x} = (p_B - p_A + t)/2t$ and $p_B = t$ into the profit function that corresponds to equation (12):

$$\pi_A = (1-\alpha)\bar{x}p_A + \alpha p_A = \frac{(1-\alpha)(t - p_A + t)p_A}{2t} + \alpha p_A = \frac{1-\alpha}{2t}(2tp_A - {p_A}^2) + \alpha p_A$$

The FOC yields that:

$$\frac{\partial \pi_A}{\partial p_A} = \frac{1-\alpha}{2t}2t - \frac{1-\alpha}{2t}2p_A + \alpha = 0$$

$$1 - \alpha + \alpha = \frac{1-\alpha}{t}p_A \leftrightarrow p_A = \frac{t}{1-\alpha}$$

The constraint of firm A's price is also specified in equation (11):

$$\mathrm{s} > \frac{(p_A - p_B + t)^2}{4t} \; where\; p_A < p_B$$

Substituting $p_B = t$:

$$\mathrm{s} > \frac{(p_A - t + t)^2}{4t} = \frac{{p_A}^2}{4t}$$

$$4ts > {p_A}^2$$

$$2\sqrt{ts} > p_A$$

The constraint is binding if:

$$2\sqrt{ts} < \frac{t}{1-\alpha}$$

$$1 - \alpha < \frac{t}{2\sqrt{ts}}$$

$$1 - \frac{t}{2\sqrt{ts}} < \alpha$$

As for $s < t/4$ (which is the case in this scenario) we obtain that $\frac{t}{2\sqrt{ts}} > 1$, the constraint is binding. Then $p_A = 2\sqrt{ts}(-\varepsilon)$, resulting in a profit of:

$$\pi_A = \frac{1-\alpha}{2t}(2tp_A - {p_A}^2) + \alpha p_A = (1-\alpha) * 2\sqrt{ts} - \frac{1-\alpha}{2t}\left(2\sqrt{ts}\right)^2 + \alpha * 2\sqrt{ts}$$

$$= 2\sqrt{ts} - \frac{1-\alpha}{2t} * 4ts = 2\sqrt{ts} - 2(1-\alpha)s$$

Derivation 10

Substituting $\bar{x} = (p_B - p_A + t)/2t$ and $p_B = t$ into the profit function that corresponds to equation (13):

$$\pi_A = \frac{(1-\alpha)(t - p_A + t)p_A}{2t} = \frac{1-\alpha}{2t}(2tp_A - {p_A}^2)$$

The FOC yields that:

$$\frac{\partial \pi_A}{\partial p_A} = \frac{1-\alpha}{2t}2t - \frac{1-\alpha}{2t}2p_A = 0$$

$$1 - \alpha - \frac{1-\alpha}{t}p_A = 0$$

$$1 - \frac{p_A}{t} = 0 \leftrightarrow p_A = t$$

However, this result does not satisfy the constraint that firm A's price must be higher than firm B's price, so it suggests that it is not a profitable strategy for firm A to increase its price above t.

Derivation 11

$$\frac{t}{2} > 2\sqrt{ts} - 2(1-\alpha)s$$

$$2(1-\alpha)s > 2\sqrt{ts} - \frac{t}{2}$$

$$2s - 2\alpha s > 2\sqrt{ts} - \frac{t}{2}$$

$$2s - 2\sqrt{ts} + \frac{t}{2} > 2\alpha s$$

$$1 - \frac{\sqrt{ts}}{s} + \frac{t}{4s} > \alpha$$

Checking whether the left-hand side is positive:

$$1 - \frac{\sqrt{ts}}{s} + \frac{t}{4s} > 0$$

Introducing the notation $u = s/t$, where $1/16 < u < 1/4$:

$$1 - \frac{\sqrt{t*u*t}}{u*t} + \frac{t}{4u*t} > 0$$

$$1 - \frac{t*\sqrt{u}}{t*u} + \frac{1}{4u} > 0$$

$$1 - \frac{\sqrt{u}}{u} + \frac{1}{4u} > 0$$

$$1 + \frac{1}{4u} > \frac{\sqrt{u}}{u}$$

$$\left(1 + \frac{1}{4u}\right)^2 > \frac{u}{u^2}$$

$$1 + \frac{1}{2u} + \frac{1}{16u^2} > \frac{1}{u}$$

$$1 - \frac{1}{2u} + \frac{1}{16u^2} > 0$$

$$\left(1 - \frac{1}{4u}\right)^2 > 0$$

which is satisified for $1/16 < u < 1/4$.

Appendix 2.2 – Characterisation of the price distribution equilibrium

Step 1. Expected demand if the other firm chooses its price with a cumulative distribution function H and picks price $\tilde{p}$:

$$Q(p) = \int_{p_{min}}^{p} \left((1-\alpha)\frac{\tilde{p}-p+t}{2t}\right) dH(\tilde{p}) + \int_{p}^{p_{max}} \left((1-\alpha)\frac{\tilde{p}-p+t}{2t}+\alpha\right) dH(\tilde{p})$$

$$Q(p) = \frac{1-\alpha}{2t}\int_{p_{min}}^{p_{max}} \tilde{p}\, dH(\tilde{p}) + \left[\left((1-\alpha)\frac{t-p}{2t}\right)H(\tilde{p})\right]_{p_{min}}^{p}$$

$$+\left[\left((1-\alpha)\frac{t-p}{2t}+\alpha\right)H(\tilde{p})\right]_{p}^{p_{max}}$$

$$Q(p) = \frac{1-\alpha}{2t}\bar{p} + \left((1-\alpha)\frac{t-p}{2t}\right)H(p) + \left((1-\alpha)\frac{t-p}{2t}+\alpha\right)$$

$$-\left((1-\alpha)\frac{t-p}{2t}+\alpha\right)H(p)$$

$$Q(p) = \frac{1-\alpha}{2t}\bar{p} + (1-\alpha)\frac{t-p}{2t} + \alpha - \alpha H(p)$$

$$Q(p) = \frac{1-\alpha}{2t}(\bar{p}+t-p) + \alpha\big(1-H(p)\big) \qquad \text{(A.1)}$$

Step 2. Given that the firm's expected profit must be the same for all p:

$$\frac{1-\alpha}{2t}(\bar{p}+t-p) + \alpha\big(1-H(p)\big) = \frac{\pi}{p} \qquad \text{(A.2)}$$

Step 3. From (A.2), derive the probability density function $h(p)$:

$$H(p) = \frac{1}{\alpha}\left(\frac{1-\alpha}{2t}(\bar{p}+t-p) + \alpha - \frac{\pi}{p}\right)$$

$$\frac{\partial H(p)}{\partial p} = h(p) = \frac{1}{\alpha}\left(\frac{\pi}{p^2} - \frac{1-\alpha}{2t}\right) \qquad \text{(A.3)}$$

Step 4. Check (A.2) for p_{max} and p_{min}:

$$\frac{1-\alpha}{2t}(\bar{p}+t-p_{max}) + \alpha\big(1-H(p_{max})\big) = \frac{\pi}{p_{max}}$$

$$\frac{1-\alpha}{2t}(\bar{p}+t-p_{max}) = \frac{\pi}{p_{max}} \qquad \text{(A.4)}$$

$$\frac{1-\alpha}{2t}(\bar{p}+t-p_{min}) + \alpha\big(1-H(p_{min})\big) = \frac{\pi}{p_{min}}$$

$$\frac{1-\alpha}{2t}(\bar{p}+t-p_{min})+\alpha=\frac{\pi}{p_{min}} \tag{A.5}$$

Step 5. Derive an expression for the expected price substituting (A.3) into its definition:

$$\bar{p}=\int_{p_{min}}^{p_{max}} p dH(p)$$

$$\bar{p}=\int_{p_{min}}^{p_{max}} p h(p) dp$$

$$\bar{p}=\int_{p_{min}}^{p_{max}} p\frac{1}{\alpha}\left(\frac{\pi}{p^2}-\frac{1-\alpha}{2t}\right)dp$$

$$\bar{p}=\frac{1}{\alpha}\int_{p_{min}}^{p_{max}}\left(\frac{\pi}{p}-\frac{1-\alpha}{2t}p\right)dp$$

$$\bar{p}=\frac{1}{\alpha}\left[\pi\ln p-\frac{1-\alpha}{4t}p^2\right]_{p_{min}}^{p_{max}}$$

$$\bar{p}=\frac{1}{\alpha}\left(\pi\ln p_{max}-\frac{1-\alpha}{4t}p_{max}^2-\pi\ln p_{min}+\frac{1-\alpha}{4t}p_{min}^2\right)$$

$$\bar{p}=\frac{1}{\alpha}\left(\pi\ln\frac{p_{max}}{p_{min}}-\frac{1-\alpha}{4t}\left(p_{max}^2-p_{min}^2\right)\right) \tag{A.6}$$

Step 6. It must be that the profit is lower if the firm charges $p>p_{max}$. If $p>p_{max}$, the firm will not sell to any uninformed consumers so its profit is given by:

$$\pi(p)=(1-\alpha)\bar{x}p=(1-\alpha)\frac{\bar{p}-p+t}{2t}p$$

$\pi(p)$ is concave and it is decreasing in p if

$$\frac{\partial\pi(p)}{\partial p}=\frac{1-\alpha}{2t}(\bar{p}+t)-\frac{1-\alpha}{t}p<0$$

$$\frac{1-\alpha}{2t}(\bar{p}+t)<\frac{1-\alpha}{t}p$$

$$\frac{\bar{p}+t}{2}<p$$

which implies that it must be that

$$\frac{\bar{p}+t}{2}\leq p_{max} \tag{A.7}$$

Step 7. Use the fact that the expected profit must be constant at any price, including p_{max}, which implies that the derivative of the profit function equals zero at p_{max}:

$$\frac{\partial \pi(p)}{\partial p} = Q(p) + p\frac{\partial Q(p)}{\partial p} = 0$$

$$\frac{1-\alpha}{2t}(\bar{p} + t - p_{max}) - p_{max}\left(\frac{1-\alpha}{2t} + \alpha h(p_{max})\right) = 0$$

$$\frac{1-\alpha}{2t}(\bar{p} + t) - \frac{1-\alpha}{t}p_{max} - \alpha p_{max}h(p_{max}) = 0$$

$$\tfrac{1-\alpha}{2t}(\bar{p} + t) - \tfrac{1-\alpha}{t}p_{max} = \alpha p_{max}h(p_{max}) \tag{A.8}$$

Given (A.7), the left hand side of (A.8) is smaller or equal to zero, which implies that $h(p_{max}) = 0$ and

$$p_{max} = \tfrac{\bar{p}+t}{2} \tag{A.9}$$

Step 8. Obtain an expression for p_{max} using (A.4) and (A.9). From (A.4):

$$\bar{p} = \frac{2t}{1-\alpha}\frac{\pi}{p_{max}} - t + p_{max}$$

Substitute this expression into (A.9):

$$p_{max} = \frac{t}{1-\alpha}\frac{\pi}{p_{max}} - \frac{t}{2} + \frac{p_{max}}{2} + \frac{t}{2}$$

$$\frac{p_{max}}{2} = \frac{t}{1-\alpha}\frac{\pi}{p_{max}}$$

$$p_{max}^2 = \frac{2t\pi}{1-\alpha}$$

$$p_{max} = \sqrt{\tfrac{2t\pi}{1-\alpha}} \leftrightarrow \pi = \tfrac{1-\alpha}{2t}p_{max}^2 \tag{A.10}$$

Step 9. Express $\bar{p}$ from (A.9) and substitute into (A.5):

$$\frac{1-\alpha}{2t}(2p_{max} - t + t - p_{min}) + \alpha = \frac{\pi}{p_{min}}$$

$$\frac{1-\alpha}{2t}(2p_{max} - p_{min}) + \alpha = \frac{\pi}{p_{min}}$$

$$\frac{1-\alpha}{t}p_{max}+\alpha=\frac{\pi}{p_{min}}+\frac{1-\alpha}{2t}p_{min}$$

Using the expression for π in (A.10):

$$\frac{1-\alpha}{t}p_{max}+\alpha=\frac{1-\alpha}{2t}\frac{p_{max}^2}{p_{min}}+\frac{1-\alpha}{2t}p_{min}$$

$$\frac{1-\alpha}{t}+\frac{\alpha}{p_{max}}=\frac{1-\alpha}{2t}\frac{p_{max}}{p_{min}}+\frac{1-\alpha}{2t}\frac{p_{min}}{p_{max}}$$

Introducing $z=\frac{p_{min}}{p_{max}}$ and using the expression for p_{max} from (A.10):

$$\frac{1-\alpha}{t}+\frac{\alpha}{\sqrt{\frac{2t\pi}{1-\alpha}}}=\frac{1-\alpha}{2t}\frac{1}{z}+\frac{1-\alpha}{2t}z$$

$$\frac{\alpha}{\sqrt{\frac{2t\pi}{1-\alpha}}}=\frac{1-\alpha}{2t}\left(z+\frac{1}{z}-2\right)$$

$$\sqrt{\frac{2t\pi}{1-\alpha}}=\frac{2\alpha t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)}$$

$$\frac{2t\pi}{1-\alpha}=\frac{4\alpha^2t^2}{(1-\alpha)^2\left(z+\frac{1}{z}-2\right)^2}$$

$$\pi=\frac{2\alpha^2t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)^2} \qquad \text{(A.11)}$$

Step 10. Using the two expressions for p_{max} in (A.9) and (A.10) and also (A.11):

$$\frac{\bar{p}+t}{2}=\sqrt{\frac{2t\pi}{1-\alpha}}$$

$$\bar{p}=2\sqrt{\frac{2t\pi}{1-\alpha}}-t$$

$$\bar{p}=2\sqrt{\frac{2t}{1-\alpha}\frac{2\alpha^2t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)^2}}-t$$

$$\bar{p} = 2\sqrt{\frac{4\alpha^2 t^2}{(1-\alpha)^2\left(z+\frac{1}{z}-2\right)^2}} - t$$

$$\bar{p} = \frac{4\alpha t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)} - t \quad \text{(A.12)}$$

Step 11. Rewrite (A.6) using (A.10):

$$\bar{p} = \frac{1}{\alpha}\left(\frac{1-\alpha}{2t}p_{max}^2 \ln\frac{p_{max}}{p_{min}} - \frac{1-\alpha}{4t}\left(p_{max}^2 - p_{min}^2\right)\right)$$

$$\alpha\bar{p} = \frac{1-\alpha}{2t}p_{max}^2 \ln\frac{p_{max}}{p_{min}} - \frac{1-\alpha}{4t}\left(p_{max}^2 - p_{min}^2\right)$$

$$\frac{\alpha\bar{p}}{p_{max}^2} = \frac{1-\alpha}{2t}\ln\frac{p_{max}}{p_{min}} - \frac{1-\alpha}{4t}\left(1-\frac{p_{min}^2}{p_{max}^2}\right)$$

$$\frac{\alpha\bar{p}}{\frac{2t\pi}{1-\alpha}} = \frac{1-\alpha}{2t}\ln\frac{1}{z} - \frac{1-\alpha}{4t} + \frac{1-\alpha}{4t}z^2$$

$$\frac{\alpha\bar{p}}{\pi} = \ln\frac{1}{z} - \frac{1}{2} + \frac{1}{2}z^2$$

$$\frac{2\alpha\bar{p}}{\pi} = z^2 + 2\ln\frac{1}{z} - 1 \quad \text{(A.13)}$$

Using (A.11) and (A.12):

$$2\alpha\left(\frac{4\alpha t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)} - t\right)\frac{(1-\alpha)\left(z+\frac{1}{z}-2\right)^2}{2\alpha^2 t} = z^2 + 2\ln\frac{1}{z} - 1$$

$$\left(\frac{4\alpha t}{(1-\alpha)\left(z+\frac{1}{z}-2\right)} - t\right)\frac{(1-\alpha)\left(z+\frac{1}{z}-2\right)^2}{\alpha t} = z^2 + 2\ln\frac{1}{z} - 1$$

$$\frac{4\alpha t - t(1-\alpha)\left(z+\frac{1}{z}-2\right)}{(1-\alpha)\left(z+\frac{1}{z}-2\right)} * \frac{(1-\alpha)\left(z+\frac{1}{z}-2\right)^2}{\alpha t} = z^2 + 2\ln\frac{1}{z} - 1$$

$$\frac{\left(4\alpha - (1-\alpha)\left(z+\frac{1}{z}-2\right)\right)\left(z+\frac{1}{z}-2\right)}{\alpha} = z^2 + 2\ln\frac{1}{z} - 1$$

$$4\left(z+\frac{1}{z}-2\right)-\frac{1-\alpha}{\alpha}\left(z+\frac{1}{z}-2\right)^2=z^2+2\ln\frac{1}{z}-1$$

$$0=\frac{1-\alpha}{\alpha}\left(z+\frac{1}{z}-2\right)^2-4\left(z+\frac{1}{z}-2\right)+z^2+2\ln\frac{1}{z}-1 \qquad \text{(A.14)}$$

If (A.14) has a solution for z, this value can be used to calculate $\bar{p}$ using (A.12) and to calculate π using (A.11). Using the value of π in (A.10), we can obtain p_{max} and from $z=\frac{p_{min}}{p_{max}}$, also p_{min}. These altogether specify the price distribution equilibrium.

Next, I show that (A.14) has a (unique) solution. Given that $p_{min}<p_{max}$, $0<z<1$. As $z\to 0$, the right hand side of (A.14) goes to infinity, so it is positive. As $z\to 1$, the right hand side of (A.14) goes to zero, approaching from below zero. This can be shown by taking the first derivative of the right hand side of (A.14) and evaluating it at $z=1$:

$$\frac{2(1-\alpha)}{\alpha}\left(z+\frac{1}{z}-2\right)\left(1-\frac{1}{z^2}\right)-4\left(1-\frac{1}{z^2}\right)+2z+2z>0, if\ z=1$$

The right hand side of (A.14) is a continuous function, which is thus positive for small values of z and negative as $z\to 1$, hence it must cross the x-axis at least once. Instead of a formal proof of uniqueness, I show graphically below how the function behaves for small, medium and large values of α.

Figure 2A.1: Equation (A.14) for small, medium and large values of α

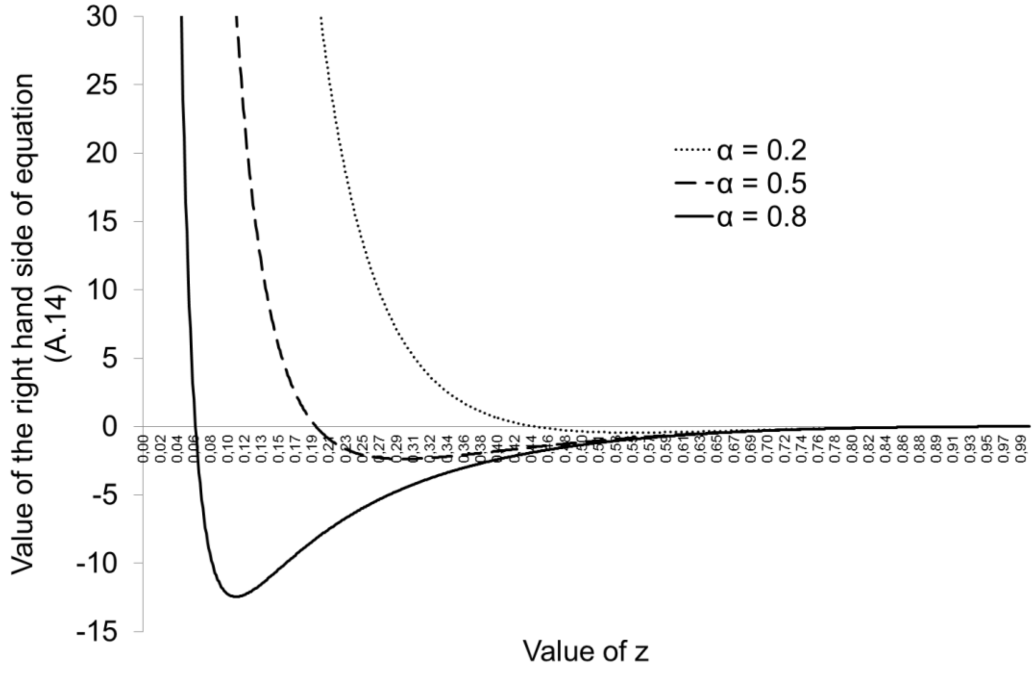


Figure Figure 2A.1 also shows that as the proportion of uninformed consumers (α) increases, the solution z^* decreases.

Appendix 2.3 – Numerical integration; stylised example

The table below shows the matrix for calculating the total cost incurred by all consumers in the price distribution equilibrium where the search cost is bigger than the fourth of the unit transport cost ($s \geq t/4$), the proportion of uninformed consumers is assumed to be 20% and the unit transport cost is 1.

Table 2A.1: Example of numerical integration

$\boldsymbol{\alpha = 0.2}$ $\boldsymbol{t = 1}$			**Firm B**				
		Prob	0.00822	0.00816	0.00810	…	0.00001
	Prob	**Price**	0.313	0.314	0.315	…	0.708
Firm A	0.00822	0.313					
	0.00816	0.314					
	0.00810	0.315					
	…	…					
	0.00001	0.708					

Each row is a price that firm A may charge in equilibrium, starting at the minimum price and going up to the maximum price with three digit precision. Columns are the same for firm B. Each price has a corresponding probability with which the firm picks this price. This is calculated using the probability density function: $Pr(p) = h(p)/\sum_{p_{min}}^{p_{max}} h(p)$. The product of the two probabilities corresponding to each cell gives the probability of that outcome occurring. For example, the probability of firm A charging the minimum price, 0.313, and firm B charging the maximum price, 0.708, is $0.00822 * 0.00001 \approx 0.0000001$.

The cost consumers incur for each pair of prices has the following elements:

(i) the price paid by uninformed consumers – this is the smaller price out of the two;

(ii) the transport cost incurred by uninformed consumers – this is $t/3$ (see Derivation 1 above);

(iii) the price paid by informed consumers – this is firm A's price for those who are situated to the left of the indifferent consumer on the line, and firm B's price for those who are situated to the right;

(iv) the transport cost incurred by informed consumers – which is $\int_0^{\bar{x}} tx^2dx + \int_{\bar{x}}^1 t(1-x)^2dx$.

The total cost of consumers for each pair of prices (that is, in each cell of the table) is the sum of (i) and (ii), multiplied by the proportion of uninformed consumers, plus the sum of (iii) and (iv), multiplied by the proportion of informed consumers. To obtain the expected total cost, I multiply the value in each cell with the probability of that outcome occurring and sum them all up.

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3. Search and awareness of product complexity Evidence from credit cards[51]

3.1. Introduction

Most retail financial products are complex and involve contingent charges that consumers only incur if they use them in a particular way. This applies to credit cards that offer a number of different features and can be used in many ways. For example, in the UK, credit is typically interest free for consumers who repay the total outstanding amount fully every month. Consumers who do not repay within the interest free period will incur interest.[52] It is also possible to use credit cards for cash withdrawal, which usually involves a fee, and interest is incurred from the first day until repayment (i.e. no interest free period on cash withdrawals). '0% deals' that offer zero interest on new purchases and/or a balance transferred from an existing credit card for a given period of time are common, and also come with a set of additional features such as the length of the offer, balance transfer fees, conditions required not to lose the offer and so on.

Despite this complexity, consumers are often confident in their own ability to assess financial products and to anticipate how they will use them in the future, which leads to sub-optimal decisions. For example, Ausubel (1999) finds that consumers systematically underestimate the extent of their future credit card borrowing. A consumer survey among credit card holders shows that only a small proportion think that credit cards are complicated products (FCA, 2015b).

In this paper, I investigate whether the act of shopping around has the potential to mitigate the problem of overconfidence in this context. My hypothesis is that when consumers search for retail financial products, they may inadvertently discover some of the complexities.

For example, consider a consumer who receives a '0% balance transfer' offer from a credit card company. This means that she can get a new credit card,

[51] I would like to thank Walter Beckert and colleagues during my time at the Financial Conduct Authority for feedback on an earlier version of this chapter.

[52] The calculation of interest charges in itself is complicated, e.g. because of compounding (that is, when unpaid interest charges are added to the outstanding balance and from then on, interest is charged on the increased balance).

transfer her existing credit card debt on to it and pay no interest for a period of time. If the consumer is interested, she can take it or decide to compare it with other offers. If she shops around, she may read about credit card deals online, visit a price comparison website, have discussions with different providers about their offers, etc. During this process, she is more likely to find out that she will lose her 0% deal if she does not repay a minimum amount every month or that a relatively high interest rate applies on any unpaid balance when the introductory deal expires (e.g. because the price comparison website highlights these clearly or the online reviews she reads mention them explicitly). By contrast, if she signs up for the first deal (most likely without reading the standard terms and conditions),[53] she is less likely to discover these complexities and potentially more likely to make some mistakes.

To test this hypothesis, I empirically examine whether consumers who search before taking out a new credit card are more likely to say that credit cards are complicated, suggesting that search may help raising awareness of product complexity. In addition, I explore whether there are any consumer characteristics that affect this potential relationship between search and views on product complexity. Note, however, that I do not test whether consumers' actual understanding of complicated features improves through search. I also do not test whether realising that a product is complicated indeed helps consumers avoid mistakes when using it.

I use survey data on consumers' use and perceptions of credit cards. The survey was commissioned by the Financial Conduct Authority (FCA) as part of its credit card market study in 2015.

I find that some consumers who search before taking out a credit card are indeed more likely to say that credit cards are complicated than consumers who do not search, but this relationship depends on what consumers use their credit cards for. In particular, the views of those consumers who use their credit card for day-to-day purchases do not differ depending on whether they search or not. However, there is a positive relationship between search and views on product complexity among consumers who do *not* use their credit cards for day-to-day

[53] According to a 2016 research by CreditCards.com, in the US 26 percent of cardholders say they read the terms and conditions of their credit card contracts (CreditCards.com, 2016). For software, Yannis et al (2014) show that only one or two of every 1,000 consumer who buy a software online access the license agreement, and even they read only a small portion of it.

purchases. In this group, the odds of finding credit cards complicated for those who search are 1.5-1.6 times the odds for those who do not search.

It is beyond the scope of this paper (and what the dataset allows) to test the reasons for the difference between consumers who use their credit cards for day-to-day purchases and those who do not but I provide some intuitive explanations in the discussion. Note also that the dataset is such that I cannot ascertain causality with certainty – this is discussed below in section 3.8.

Notwithstanding the limitations, I conclude that search is likely to help certain groups of consumers realise that a product is more complicated than they had thought. The main contribution of this paper is thus showing the existence of a potential additional benefit of search (beyond finding a good deal), which could provide regulators with another reason to encourage consumer engagement in the market. To my knowledge, no previous research has assessed whether consumer search can raise awareness of product complexity. Future work could focus on testing the hypothesis using data collected specifically for the purposes of such analysis, and on the impact of increased awareness of product complexity on consumers' knowledge and behaviour.

The paper is structured as follows. I summarise the related literature in section 3.2 and describe the data in section 3.3. Sections 3.4, 3.5 and 3.6 present the analysis. I discuss the findings in section 3.7 and the limitations in section 3.8. Section 3.9 concludes.

3.2. Related literature

This paper connects search and perceptions of product complexity in the domain of credit cards in a novel way. The most closely related paper, albeit still with a quite different focus, is by Allgood and Walstad (2016), who study *inter alia* the relationship between perceived financial literacy and search. They find that consumers with high perceived financial literacy are more likely to shop around for mortgages and car loans.

There are a few papers that link search and an objective measure of product complexity. For example, Swaminathan (2003) finds in a laboratory experiment that the effect of product complexity on search is insignificant (except in the presence of recommendation agents). Holland and Jacobs (2015) find that the use of price comparison engines is inversely related to product complexity, similarly to Muir et al (2013) who conclude that price complexity significantly increases search costs, limiting the size of consideration sets. This latter paper empirically supports the idea developed in the theoretical literature that firms may have incentives to increase consumers' search costs, e.g. by making their pricing more complicated or less comparable (see, for example, Gabaix and Laibson, 2006, and Ellison and Wolitzky, 2012).

It is indeed well established in the theoretical literature that the level of search costs influences how much consumers are willing to invest in looking for a better product and that with lower search costs the equilibrium outcome tends to be more competitive (see, for example, Wolinsky, 1986, and Stahl, 1989).[54] There are empirical papers that estimate the level of search costs and how it influences the amount of search consumers are willing to undertake (see, for example, Hong and Shum, 2006, and Santos et al, 2012), as well as research that empirically supports the notion that reducing search costs intensifies competition (see, for example, Lynch and Ariely, 2000, and Agarwal et al, 2015a). Specifically for credit cards, Stango and Zinman (2016) find that the dispersion of borrowing costs across the US is due to price variations across lenders and differences in shopping intensity. Kim et al (2005) and Kerr and Dunn (2008) show that the benefits of shopping around for credit cards can outweigh the costs.

A number of policy documents from the Federal Reserve, the US Government Accountability Office and the FCA conclude that credit cards are complex products (see Canner and Ellienhausen, 2013; GAO, 2006; FCA, 2015a). It is also well-established in the literature that consumers make mistakes when using credit cards. For instance, Stango and Zinman (2009) found that the median consumer could avoid 60 percent of all credit card interest charges and fees by using different cards and reallocating debt across cards. Similarly, Ponce et al (2017) show that consumers incur 31 percent higher costs than the minimum by allocating a large fraction of their debt on high-interest cards. Agarwal et al (2015b) found that about 40 percent of consumers choose the *ex post* suboptimal contract but that consumers with larger errors are more likely to subsequently switch to the optimal contract. Agarwal and Mazumder (2013) show that credit card consumers who make mistakes learn to avoid them in the future – they find that paying a fee last month reduces fee payment in the current month by 40% and monthly fee payments fall by 75% during the first four years of a card

[54] See, however, Chapter 2 on when this is not the case.

holder's account life. Finally, the FCA found in its market study that there may be a significant number of consumers who are able to repay their outstanding balance on 0% balance transfer cards at the end of the promotional period but only do so with a few months delay when they start incurring interest (FCA, 2015c). Interestingly, Allgood and Walstad (2016) find that both perceived and actual financial literacy has a statistically significant relationship with credit card behaviour: consumers with high perceived financial literacy are less likely to engage in suboptimal credit card usage such as making minimum repayments, incur late fees or exceed credit limit.

In sum, the existing literature establishes that (i) consumers benefit from shopping around both in general and specifically for credit cards, (ii) product complexity can hinder shopping around through increasing search costs, (iii) credit cards are complex products and (iv) consumers make mistakes when using credit cards. My paper is novel by adding a new consideration: it analyses whether the act of searching changes the perception of product complexity.

3.3. Data and definitions

As part of its credit card market study, the FCA commissioned a market research company, YouGov, to conduct a large scale online consumer survey. YouGov carried out the survey in April 2015 and provided the FCA with a dataset containing the responses. The analysis described in this paper relies on the dataset obtained from the FCA.

YouGov primarily used its own online panel of consumers. The survey was designed to obtain a nationally representative sample of the UK grown-up population, and the composition of the achieved sample was close to the target profiles in terms of age, gender, region and social grade. The final sample consists of close to 40,000 responses which include people who actively use credit cards, people who have credit card(s) but do not use it (them) and people who did not have a credit card at the time of responding to the questionnaire.

Further details on data collection, fieldwork procedures and the quality of the sample are available in YouGov's technical report (YouGov, 2015). In addition, the FCA published a note on the design which explains the approach to and the process of developing the questionnaire (including piloting) and contains the final version of the full questionnaire (Leston, 2015).

The aim of my research is to establish whether there is a relationship between shopping around (searching) for a product and what consumers think about the complexity of this product. The key questions of interest are thus the ones that investigate (i) whether consumers shopped around before taking out a credit card and (ii) their views about how complex credit cards are.

Definition of searchers

The basis of the delineation between searchers and non-searchers is question 11 of the survey:

“Q11 Have you done any of the following in the past 12 months?

> i. I considered two or more credit cards and took out a credit card as a result.
>
> ii. I considered two or more credit cards but did not take out a credit card as a result. *This includes any instances when you may have made an unsuccessful application.*
>
> iii. I took out a new credit card without considering other credit cards.”

Respondents could select ‘yes’, ‘no’ or ‘unsure’ for each of the three options. That is, someone who took out two credit cards in the 12 months before filling out the questionnaire, one with and one without considering other credit cards, could indicate both (i) and (iii).

The group of searchers is defined as those who took out a credit card after considering two or more credit cards (selected yes when responding to Q11i) but did not take out a credit card without considering other credit cards (selected no when responding to Q11iii). The group of consumers who do not search is a distinct subset, defined as those who took out a credit card without considering other credit cards (selected yes when responding to Q11iii) but did not consider two or more credit cards on another occasion, either with or without taking one out (selected no when responding to Q11i and Q11ii).

There were 6,309 respondents in total who indicated that they took out a credit card in the 12 months before filling out the questionnaire. Out of these respondents, the definitions above exclude everyone where there is ambiguity over which group they belong to (see details in Appendix 3.1). These exclusions result in a sample of 5,358 consumers, 57% of whom are in the search group and 43% of whom are in the no search group.

The questionnaire also asked respondents who considered more credit cards before taking one out about the information sources they used:

“Q42a How many of the following sources did you use when considering which credit card to choose?

- Price comparison website
- Company website
- Other online sources
- Family or friend recommendation
- Staff in store/branch (face-to-face or on the phone)
- Advertisement”

The response options in each case were ‘zero’, ‘one’, ‘two or more’ and ‘unsure’. This question helps refine the definitions of searchers and non-searchers. In particular, anyone who did not use a price comparison website, a company website, other online sources or staff members to obtain information about credit cards is unlikely to have shopped around properly, i.e. during which firstly hidden information may have been revealed. Therefore, I move all of those who indicated that they did not use any of these information sources (only family or friend recommendation or advertisements) from the search group to the no search group (248 respondents). This move results in the following split: 53% (2,819) searchers and 47% (2,539) non-searchers in the sample of 5,358 consumers.

Table 3.1 displays a comparison of characteristics of searchers and non-searchers for a selection of variables that show the highest differences. While there is a smaller difference, I also included the variable of whether the consumer uses her credit card for day-to-day purchases, given the importance of this in the results.

As the table shows, searchers are more likely to work full time, have a mortgage and some other debt compared to non-searchers, more of whom are retired, own their property outright and have no other outstanding debt. Searchers are more likely to have some outstanding balance and to pay interest on their credit cards. A higher proportion of them use balance transfers and take out a credit card with a provider with whom they did not have any relationship before. In contrast, non-searchers are more likely to take out a credit card with a provider who offered it to them and with whom they had a current account or some other financial relationship.

Searchers are more likely to take out a credit card because their personal or financial circumstances have changed, because the introductory deal on a previous credit card ended and to benefit from low fees and introductory offers, and a larger proportion of them say that they chose a particular credit card because it suited their needs the best or because a price comparison website ranked it highly.

Table 3.1: Comparison of characteristics of searchers and non-searchers

	No search (%)	Search (%)	Difference (%-point)
Demographics			
Works full time	50	68	-18
Retired	28	12	16
Has a mortgage	34	47	-13
Owns a property	34	20	14
No other debt	41	26	15
Use of credit cards			
Had a credit card when taking the last one out	68	79	-11
Has no outstanding balance	65	47	18
Never pays interest	65	53	12
Done balance transfer	15	42	-27
Uses credit card for day-to-day purchases	61	57	4
Relationship with credit card provider			
No previous relationship	46	56	-10
Current account	33	22	11
Other	29	18	11
Decided to take out a credit card because			
Personal or financial circumstances changed	30	44	-14
To benefit from low rates	13	25	-12
To benefit from an introductory offer	26	37	-11
On a previous card the introductory deal ended	5	22	-17
Decided to take out a particular credit card because			
It suited his/her needs the best	33	52	-19
A price comparison website ranked it highly	6	20	-14
The company offered it	21	9	12

Note: (i) the figures show the proportion of respondents who search / do not search and exhibit a particular characteristic, e.g. 68% of searchers work full time; (ii) the sample size varies due to a different number of observations missing for each variable; (iii) the results are similar in the sample used in the estimation

Consumers' view on product complexity

Question 50a of the survey asks respondents' opinion about credit card complexity: "Overall, as a product, how easy or difficult to understand do you think credit cards are?" The options respondents could choose were 'very difficult', 'quite difficult', 'neither difficult nor easy', 'quite easy', 'very easy' and 'unsure'. I drop those who selected 'unsure' (107 respondents), resulting in a sample of 5,251 responses. The table below shows how responses were split among the remaining options.

Table 3.2: Responses to Q50a (excluding 'unsure')

Credit cards are…	Number of responses	Proportion of responses
Very easy to understand	1,425	27%
Quite easy to understand	2,273	43%
Neither easy nor difficult to understand	924	18%
Quite difficult to understand	532	10%
Very difficult to understand	97	2%
Total	5,251	100%

As Table 3.2 shows, the majority of consumers are of the view that credit cards are not particularly complicated products. Only a minority (10%) think that credit cards are quite difficult to understand and only a few consumers (2%) agree that credit cards are very difficult to understand.

In order to avoid the problem of empty cells, that is, when there is no variation in the responses when split by another variable or variables, I aggregate responses into two groups: consumers who find credit cards difficult to understand and consumers who do not find credit cards difficult to understand. Note that this classification results in consumers who think that credit cards are neither easy nor difficult to understand falling into the 'not difficult' category.[55] The split of responses aggregated to two groups is shown in the table below.

Table 3.3: Responses to Q50a, aggregated (excluding 'unsure')

Credit cards are…	Number of responses	Proportion of responses
Not difficult to understand	4,622	88%
Difficult to understand	629	12%
Total	5,251	100%

[55] The reason why I define the dependent variable as difficult/not difficult instead of easy/not easy is because this definition serves better the purposes of the analysis (testing whether search helps consumers understand that credit cards are complicated products). Further research could focus on finding out whether search reduces consumers' confidence in to what extent they understand financial products, in which case a definition of easy/not easy may be more appropriate.

The breakdown shown in Table 3.3 above is what I use to define consumers' views on complexity, and thus is the main variable of interest (the outcome). Throughout this paper, I say that a consumer thinks credit cards are complex if she selected 'very difficult' or 'quite difficult' when responding to the question on how easy or difficult credit cards are to understand.

Other relevant questions/variables

The questionnaire contained a large number of questions which could potentially be used in the analysis. However, many of them were not asked from all respondents and thus cannot be compared across the sample. For example, respondents who took out a credit card without considering other credit cards were asked why they did not do so but this question would have not made sense to be asked from those who did look around. Other questions focused on specific features of credit cards and were only asked from respondents who indicated that they used that feature (e.g. transferred a balance from one credit card to another). Finally, many questions appeared less relevant for the purposes of the analysis.

The questions included in the initial set for consideration can be categorised in five groups:

1. **Questions exploring how consumers use their credit cards**, including on the number of credit cards held and used, total credit limit and outstanding debt on all of the consumer's credit cards, average monthly spending, frequency of paying interest, whether the consumer uses her credit cards for day-to-day purchases and/or to collect rewards, transfer balances, withdraw cash, etc., and past mistakes such as exceeding her credit limit and incurring unexpected charges.[56]
2. **Questions that describe consumers' reasons for taking out a credit card**, including a change in personal or financial circumstances, the intention to use it abroad or online safely, to build or improve credit history, as well as reasons linked to previous credit cards, such as that a

[56] Mistakes directly measure the consumer's past experience which is likely to influence her views on how complicated credit cards are. However, due to the structure and the wording of the questionnaire, I am unable to identify whether consumers made mistakes before or after taking out their latest credit card. Therefore these variables, although useful to build them in the model as controls, cannot be used to test whether consumers who search before taking out a credit card are more likely to avoid mistakes or not.

previous introductory offer expired or the terms and conditions were changed.

3. **Questions that describe consumers' reasons for choosing a particular credit card and provider**, including whether the consumer decided to take out a credit card with a particular provider because she had a credit card with them before, she shops with them (think of credit cards offered by retailers) and questions on whether the consumer chose a particular credit card because it suited her needs the best or because a company offered it to her.
4. **Questions on the relationship between the consumer and the provider before taking out a credit card with them in the previous 12 months**: the consumer had a credit card, a current account, some other financial or non-financial relationship with the company before, or no relationship at all.
5. **Questions on demographics**, including age, gender, employment status, education level, amount of savings, total monthly debt service (excluding credit card repayments).

As a result of the format of the survey (online questionnaire), most of these variables are categorical, often only taking two values (yes or no). However, this does not appear to be a problem during the estimation given the relatively large sample size.

The questionnaire offered respondents the option of choosing 'unsure' in almost all questions, and for most demographic variables respondents could also select 'prefer not to say'. I treat all unsure and prefer not to say responses as missing values throughout the analysis.

Finally, when considering these variables, I removed one outlier (claiming to hold 54 credit cards), resulting in a sample of 5,250 responses, with a split shown in the table below.

Table 3.4: Split of responses about product complexity, final sample

Credit cards are…	Number of responses	Proportion of responses
Not difficult to understand	4,621	88%
Difficult to understand	629	12%
Total	5,250	100%

For the full list of the 74 variables considered in the analysis as well as the proportion of responses in each category, see Appendix 3.2.

3.4. Contingency table

A simple way of investigating the research question using the data is a contingency table that shows the number and proportion of consumers in each group (search vs. no search) who say that credit cards are difficult / not difficult to understand. This is shown in the table below.

Table 3.5: Split of responses about product complexity by search

Credit cards are…	No search	Search	Total
Not difficult to understand	2,203 (90%)	2,418 (87%)	4,621 (88%)
Difficult to understand	254 (10%)	375 (13%)	629 (12%)
Total	2,457 (100%)	2,793 (100%)	5,250 (100%)

As Table 3.5 shows consumers who search before taking out a credit card are more likely to say that credit cards are complex: about 13% of them say that credit cards are difficult to understand, compared to 10% of those consumers who do not search (a statistically significant difference). While these proportions are relatively small, it may partly be due to the fact that many consumers use their credit cards in a simple way and thus are unlikely to think that credit cards are complicated products, irrespective of whether they search or not. The results can be translated into odds ratios: the odds of finding credit cards difficult to understand for consumers who search are about 1.3 times larger than the odds for consumers who do not search.[57]

If consumers were randomly assigned to the group of searchers and non-searchers (as they would be in a randomised controlled trial), these descriptive statistics would be sufficient to conclude that the act of searching for a credit card has a significant relationship with what consumers think about complexity of credit cards. However, in reality, those consumers who search and those who do not differ not only in whether they are willing to search but in some other respects (see Table 3.1 above). These other differences could explain why their views about product complexity differ and the descriptive statistics would misleadingly

[57] Calculated as the odds of finding credit cards difficult to understand for those who search (13%/87%), divided by the odds of finding credit cards difficult to understand for those who do not search (10%/90%).

suggest a relationship with search that does not exist in reality. To control for these other differences, I use regression analysis described in the next sections.

3.5. Estimation using the full sample

When the outcome is defined as binary (in this case, difficult vs. not difficult), one of the most frequently used regression models is the logistic regression model.[58] The logistic model assumes a particular relationship between the outcome and the explanatory variables and estimates the impact of each explanatory variable on the logit of the outcome.[59] More formally, I estimate the following equation:

$$\ln\left(\frac{p_i}{1-p_i}\right) = \beta_0 + \beta_1 s_i + \sum_k \beta_k x_{ik} + \varepsilon_i$$

where p_i is the probability that an individual finds credit cards complicated, s_i is a dummy that shows whether the individual searched or not, x_{ik} denotes the other control variables and ε_i is the error term.

While it is not possible to interpret the estimated coefficients directly, they can be used to obtain odds ratios – the same statistic as above but in this case after controlling for additional factors.

There are a number of methods empirical researchers use to build a logistic regression model. I follow the method of ‘purposeful selection of covariates’, described in the textbook of Hosmer et al (2013). I use Stata to carry out the analysis. Before building the model I investigate whether the fact that some variables are missing for certain observations could lead to biased estimates.

3.5.1. Potential selection bias due to missing values

As mentioned above, most questions allowed the respondent not to give an answer by offering an option to select ‘unsure’ or ‘prefer not to say’. If a respondent selected either of these two options that shows up in the dataset as a

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[58] Another functional form that is often used when the outcome is binary is probit. The logit and probit functions assume different forms of relationship between the outcome and the explanatory variables. However, logit and probit estimates differ noticeably only under special circumstances and there is little guidance on which one is better to use (see Aldrich and Nelson, 1985). As a cross-check, I run the final model using probit instead of logit specification and obtain similar results (see section 3.5.2 below).

[59] The logit is the natural logarithm of the odds of the outcome.

missing value for that variable. When a variable is included in the regression model, observations for which its value is missing cannot be used.

As long as each variable is missing for a random set of observations, this is not a cause for concern (unless it reduces the sample size significantly). However, it may well be that a particular group of consumers decide not to answer a question, which will lead to this group being underrepresented in the sample, resulting in biased estimates. If including a variable in the model leads to selection bias, it may be better to exclude it and keep all observations in the sample for which it is missing. However, this can lead to omitted variable bias, which is the problem that if an important variable is not included in the model, it can lead to biased estimates of the impact of the variables included.

I test both the omitted variable and the selection bias for all variables that are missing for more than 3.5% of the observations in the sample, with two exceptions. There are a number of dummies in the dataset explaining the reasons why consumers decided to take out a credit card and a number of dummies showing why they chose a particular credit card or company. The first set of dummies is missing for 7% of the observations and the second set is missing for 6% of the observations. To be able to use the observations for which these dummies are missing, one would have to disregard all of the dummies in the same set. As some of these dummies are key control variables, I decided to keep them despite the potential for selection bias. However, it appears that excluding these controls and including observations for which these are missing would increase the effect of search on the outcome so in that sense my choice of dealing with these variables is conservative (i.e. it leads to underestimating the association between search and views on complexity). In addition, I did not check the selection bias for the variable education because it is not related to the outcome even when considered in itself.

I compare the potential omitted variable bias and selection bias by estimating models including and excluding a particular variable, and including and excluding observations for which it is missing. By looking at the change in the estimated coefficients and in their significance, I can establish which bias is more likely to be of concern. I identify seven variables where the selection bias appears to be large (and, in particular, larger than the omitted variable bias) and so I exclude them from the next step of building the model. These variables are:

- Savings;

- Household income;
- Monthly debt repayments (excluding credit card debt);
- Profession;
- Total credit limit on all credit cards held;
- Paid interest on main credit card but did not expect to do so when taking it out;
- Had a credit card when taking out the new.

Details of this analysis can be found in Appendix 3.3.

3.5.2. Model building

As mentioned above, I use the method of 'purposeful selection of covariates' by Hosmer et al (2013) to decide which variables to include in the model. This method consists of seven steps:

1. Univariate analysis of each explanatory variable;
2. Fitting the logistic regression model including all variables that were selected in Step 1;
3. Fitting the logistic regression model including only those variables that were significant in the model of Step 2;
4. Including variables that were not selected in Step 1, one at a time, to assess whether they become significant in the presence of other controls;
5. Assessing whether the linearity assumption of the logit model is satisfied;
6. Adding necessary interaction terms; and
7. Assessing the adequacy of the model and checking its fit.

The advantage of this process is that, if properly implemented, it should result in a model that contains all the relevant variables that affect the outcome itself and/or the estimate of the relationship between other variables and the outcome. It also helps to minimise the number of variables included in the model, and therefore to avoid overfitting.[60]

Below I briefly summarise the key findings in each step. Appendix 3.4 contains further details of the analysis.

[60] See Hosmer et al (2013), page 90. Overfitting occurs when a model works very well (even perfectly) when used on the dataset on which it was developed but performs poorly in estimating the impact of explanatory variables on the outcome when run on another dataset.

Step 1 – univariate analysis

This step is used to check whether there is a significant impact of each variable on the outcome when used in itself. I use a low threshold: only variables with an estimated coefficient that is insignificant at 25% are excluded at this stage. This results in dropping 14 variables from the consideration set.

Steps 2 and 3 – fitting the model and removing insignificant controls

Step 2 of the model building process starts with running a logistic regression including all variables that were selected in Step 1. I then assess the significance of the estimated coefficients, and keep all that are significant at least at 10%. Step 3 starts with running a logistic regression using only this subset of variables. Next, I assess to what extent the estimated coefficients change between the long and the short model. As some estimates change considerably, I start again from the long regression, remove only a few variables at a time and always keep those that appear to affect any other (significant) estimates. This results in excluding 29 variables.

Step 4 – checking the variables excluded in Step 1

In Step 4, I check whether any of the variables not selected in Step 1 becomes significant when added to the model developed in Step 3. I conclude that none of these variables is associated with the outcome or materially affect other estimated coefficients, and thus can be ignored.

Step 5 – assessing the linearity assumption

Step 5 involves assessing whether the linearity assumption of the logit model is satisfied, that is, whether the logit of the outcome increases or decreases linearly with any continuous variable included in the model. The test applied shows that the assumption is satisfied for both variables that I treat as continuous (number of credit cards held and the age of the consumer).

Step 6 – adding interaction terms

In Step 6, I test whether interactions between pairs of explanatory variables should be included in the model. An interaction term is significant and thus should be included in the model if the impact of one variable on the outcome is not constant at different levels of another variable. I test all potential twoway interactions of the variables that are included in the model, use the partial likelihood test to establish whether they are significant, and include in the model all interaction terms that are highly significant (at 3%, both when included in themselves and together with the other interaction terms).

The interaction terms I add to the model are:

- Interaction between whether the consumer searched before taking out her credit card and whether she uses her credit card for day-to-day purchases;
- Interaction between whether the consumer decided to take out a credit card to use it for online purchases safely and whether she incurred unexpected charges in the last 12 months; and
- Interaction between the consumer's age and whether she spent more on her credit card than she budgeted for in the last 12 months.

As a result of including the interaction between search and day-to-day purchases, the estimated coefficient of search increases and remains significant, while the estimated coefficient of day-to-day purchases reduces to essentially zero and becomes insignificant.

Including the second interaction term leads to the coefficients of both individual components becoming smaller and insignificant. This suggests that whether a consumer took out her credit card to use it for online purchases safely and whether she incurred unexpected charges have no or limited relationship with her view on product complexity, unless both of these two things apply.

Finally, including the third interaction term results in the coefficient of spending more than budgeted for becoming insignificant while the coefficient of age remaining significant. In addition, the coefficient of age is the same magnitude as the coefficient of the interaction term but has the opposite sign (-0.02 and 0.02, respectively). This suggests that while the age of the consumer affects what she thinks about credit card complexity, for people who spent more than they had budgeted for age does not matter.

At the end of Step 6, two variables (decided to take out a credit card from company X because it offered text/email updates and alerts and average monthly spending) that were slightly significant become insignificant. I then drop these variables as that has minimal impact on the estimated coefficients of the other controls but allows me to expand the sample to include observations for which these are missing. The final logistic model and the results are shown in the table below (the discussion of the results is set out in section 3.7).[61]

[61] Note that the final logit model does not include all variables along which searchers and non-searchers differ materially, as shown in Table 3.1. Two variables were excluded due to selection bias. Others were excluded in Step 1 and checked in Step 4 – in all cases the inclusion of the variable would lead to a less than 5% change in the coefficient of search. Finally, a number of variables were excluded in Step 3 as they were insignificant and their exclusion did not change the coefficients of the remaining variables significantly.

Table 3.6: Logistic regression estimates, full sample

	Coef.	Std. error	95% confidence interval	
Searched before taking out a credit card	0.44**	0.15	0.14	0.74
Ways of using credit cards				
Number of credit cards held	-0.17**	0.05	-0.27	-0.07
Any of the consumer's credit cards offers rewards	-0.29**	0.18	-0.39	0.30
Uses the main credit card for day-to-day purchases	-0.05	0.38	-1.27	0.20
Spent more on a credit card than budgeted for in the past 12 months	-0.53	0.22	-0.34	0.53
Incurred unexpected charges in the past 12 months	0.10	0.24	0.00	0.96
Incurred higher than expected charges in the past 12 months	0.48*	0.14	0.33	0.89
Found that paying back a balance takes longer than expected in the past 12 months	0.61**	0.05	-0.27	-0.07
Total amount outstanding last month after repayment (£)	insig			
Frequency of paying interest	sig 5%			
Frequency of using the main credit card	insig			
Reasons for taking out a credit card				
Decided to take out a credit card to use it for online purchases safely	0.21*	0.13	-0.04	0.46
Decided to take out a credit card to build/improve credit history	0.29**	0.13	0.04	0.55
Decided to take out a credit card because on a previous card the terms and conditions were changed	0.45*	0.24	-0.02	0.92
Reasons for choosing a particular credit card or provider				
Decided to take out a particular credit card because it suited his/her needs the best	-0.40**	0.11	-0.61	-0.19
Relationship with the chosen provider				
Had a credit card with the chosen company before	-0.52**	0.22	-0.95	-0.09
Had some other relationship with the chosen company	-0.37**	0.17	-0.71	-0.04
Demographics				
Age	-0.02**	0.01	-0.04	-0.01
Property ownership	sig 5%			
Marital status	insig			
Employment status	insig			
Interaction terms				
Searched before taking out a credit card and uses her credit card for day-to-day purchases	-0.42**	0.21	-0.82	-0.01
Decided to take out a credit card to use it for online purchases safely and incurred unexpected charges in the past 12 months	1.08**	0.38	0.33	1.83
Spent more on a credit card than budgeted for in the past 12 months and age	0.02**	0.01	0.00	0.04
Constant	-0.46	0.42	-1.28	0.37

Notes: (i) ** indicates significance at 5%; (ii) * indicates significance at 10%; (iii) coefficients of variables with more categories are not included but the joint significance of their coefficients is mentioned where the variable is in the model (sig 5% - significant at 5%, sig 10% - significant at 10%, insig – insignificant); (iv) sample size: 4,406; (v) log-likelihood: -1,375; (vi) pseudo-R2: 0.10

Step 7 – adequacy and fit

The last step of the model building exercise is to assess the fit of the model. A model fits well if the probabilities it estimates (in this case, the probability of an individual saying that credit cards are difficult to understand) accurately reflect the actual outcome (in this case what the individual actually responded to the question).[62] This can be assessed through summary measures of goodness of fit that describe the difference between estimated and actual outcomes for all observations and through logistic regression diagnostics.

I carry out three statistical tests to assess goodness of fit and each confirms that the model fits well. Details of these tests can be found in Appendix 3.4.

Another method commonly used to assess the performance of a logistic model is a classification table. A classification table shows the proportion of observations for which the model correctly predicts the outcome. This involves specifying a cut point, below and above which the estimated probabilities are assumed to predict different outcomes. However, classification is sensitive to the relative sizes of the groups with differing outcomes and thus may not be appropriate to use as a measure of goodness of fit.[63] As in this case the proportion of consumers who think that credit cards are difficult to understand is much smaller than those who do not (12% vs. 88%, see Table 3.4 above), I do not rely on classification to assess the goodness of fit of the model.

Logistic regression diagnostics are useful to identify any covariate patterns[64] that do not support the conclusion that the model fits the data well. These can be assessed by estimating measures of the effect of each covariate pattern on the fit of the model and on the estimated parameters, and checking whether removing these covariate patterns from the sample has a significant impact on the estimates.[65] Based on visual inspection of graphs that plot these measures against the estimated probability, I identified 9 covariate patterns that poorly fit the model and 18 covariate patterns that are influential (that is, may change the estimated coefficients). Excluding these observations would eliminate variation in the outcome for one category of marital status (all remaining widowed consumers

[62] Hosmer et al (2013), page 153.
[63] See Hosmer et al (2013), page 171.
[64] A covariate pattern is a particular configuration of covariates (variables) in a model.
[65] See Hosmer et al (2013), section 5.3.

think that credit cards are not difficult to understand) but would have limited impact on the estimates otherwise.

Finally, as mentioned in footnote 58, another commonly used functional form for estimations when the outcome variable is binary is probit but there is little guidance on which is the right functional form to use. I run the final model using probit instead of logit specification and find that all significant coefficients change by a similar magnitude (the coefficients estimated in logit are 1.7-2.0 times larger than the coefficients estimated in probit, which is simply a result of the different specifications) but the results are otherwise unaffected.[66]

[66] Stata also offers the ‘link test’ to test for model specification, that is, whether logit is the appropriate functional form to use. The results of the test support the use of the logit form for the data.

3.6. Estimation using a balanced sample

In this section I extend the base methodology to include an additional step of balancing the covariate distribution across the two groups (I refer to this as the balanced sample) before estimating the relationship between search and consumers' views of complexity. Balanced covariates mean that the mean and distribution of certain variables (that is, of observed characteristics) are similar in the search and no search groups. In other words, the balanced sample is a subset of the full sample, selected in a way that makes sure that searchers and non-searchers remaining in the sample are similar in observable characteristics other than whether they search or not. Making the two groups more alike implies that differences in their views on complexity can be more certainly attributed to the difference in search behaviour.

I use propensity score matching to balance the covariates across the two groups. The propensity score is the propensity of an individual to receive a 'treatment' – in this case to search before taking a credit card out. The estimation of propensity scores typically involves building a logistic regression model – similar to that described in the previous section but in this case with the treatment (search vs. no search) as the dependent variable. Once the propensity scores are estimated, they can be used to find pairs of treated and untreated individuals who have the same propensity score (suggesting that they are similar but one of them searched before taking her credit card out and the other did not). This matched sample can then be used to estimate the impact of treatment on the outcome.

In the remainder of this section, I set out how I select the variables to estimate the propensity score, the method to create the balanced sample (matching) and the estimation of the impact of search on perceived product complexity using this matched sample.

3.6.1. Estimating the propensity score

There is no consensus in the theoretical and applied literature on which variables to use for estimating the propensity score (see, for example, Austin, 2011). Options include: (i) variables that are associated with the treatment, (ii) variables that are associated with the outcome, (iii) variables that are associated with the treatment and the outcome and (iv) all variables. Given the definition of propensity score (i.e. the propensity to receive the treatment, in this case, the probability that someone will search before taking out a credit card), a good starting point may be to use all variables that affect whether someone receives the treatment or not. However, another argument is that the aim of estimating propensity scores is to balance the sample with respect to the variables that have an impact on the outcome and therefore these are the variables that should be included in the model. In addition, there is some evidence that using variables that affect the outcome leads to more precise estimates for the treatment effect (Austin, 2011), whereas the opposite is true for variables that affect the treatment but not the outcome (Brookhart et al, 2006).

When considering which variables to use to estimate the propensity score, I take advantage of the fact that I already built a model that, if correctly specified, contains all the variables that affect the outcome either directly or through modifying the impact of another control variable. As the aim is to achieve a balanced sample rather than to obtain the propensity to search precisely, I proceed with selecting the variables that affect the outcome.

There is consensus in the literature that variables that might have been affected by the treatment should be excluded from the estimation of propensity scores (see, for example, Imbens, 2004). In this case, this implies excluding all variables that describe (i) how consumers use their credit cards and (ii) consumers' reasons for choosing a particular credit card or provider, and keeping everything else in. The table below lists all the variables included in the final logit model built on the full sample, indicating which ones I incorporate in the propensity score model.

Table 3.7: Variable selection for estimating propensity scores

	Included in the model to estimate propensity scores
Ways of using credit cards	
Number of credit cards held	No
Any of the consumer's credit cards offers rewards	No
Uses the main credit card for day-to-day purchases	No
Spent more on a credit card than budgeted for in the past 12 months	No
Incurred unexpected charges in the past 12 months	No
Incurred higher than expected charges in the past 12 months	No
Found that paying back a balance takes longer than expected in the past 12 months	No
Total amount outstanding last month after repayment (£)	No
Frequency of paying interest	No
Frequency of using the main credit card	No
Reasons for taking out a credit card	
Decided to take out a credit card to use it for online purchases safely	Yes
Decided to take out a credit card to build/improve credit history	Yes
Decided to take out a credit card because on a previous card the terms and conditions were changed	Yes
Reasons for choosing a particular credit card or provider	
Decided to take out a particular credit card because it suited his/her needs the best	No
Relationship with the chosen provider	
Had a credit card with the chosen company before	Yes
Had some other relationship with the chosen company	Yes
Demographics	
Age	Yes
Property ownership	Yes
Marital status	Yes
Employment status	Yes

As indicated in Table 3.7, how consumers use their credit cards and whether they make mistakes may have been influenced by whether they searched, and thus I exclude them from the model estimating the propensity scores. For example, a consumer who searched and chose a 0% purchase card (on which interest is not charged for a period of time on new purchases) may pay interest less frequently than someone who did not search and just accepted an offer without a 0% deal. Similarly, the level of outstanding debt on a consumer's credit card is bounded by her credit limit, which may be different depending on whether she searched for a credit card or not.

On the other hand, reasons for taking out a credit card are determined before making a decision whether to search or not, so cannot be affected by search. For example, if someone decides to take out a credit card because the terms and conditions were changed on one of her previous credit cards and this might affect whether she decides to shop around, but whether she searches or not does not affect this motivation. Similarly, it may be that having a credit card with a company and being happy with it provides incentives not to shop around, but search cannot change whether one had a credit card with a company before or not. Demographic characteristics may change over time but are unaffected by whether someone searches for a credit card or not.

The results of the model estimating the propensity to search are shown in the table below. A few things to note:

- I use the same sample as for the final logit model (4,406 observations).
- As the relationship between the logit of search and age appears to be nonlinear I include the square of age.
- The model does not include any interaction terms as these are mostly used to improve the balancing performance of the propensity score and that was not necessary in this case (see below).
- Summary measures of goodness of fit and logistic regression diagnostics do not reveal any major problems.

Table 3.8: Propensity score, logistic regression estimates

	Coef.	Std. error	95% conf. interval	
Reasons for taking out a credit card				
Decided to take out a credit card to use it for online purchases safely	0.14*	0.08	-0.02	0.30
Decided to take out a credit card to build/improve credit history	-0.04	0.09	-0.22	0.15
Decided to take out a credit card because on a previous card the terms and conditions were changed	0.92**	0.18	0.57	1.27
Relationship with the chosen provider				
Had a credit card with the chosen company before	0.07	0.11	-0.15	0.29
Had some other relationship with the chosen company	-0.22**	0.09	-0.40	-0.04
Demographics				
Age	-0.0002	0.0168	-0.0330	0.0327
Age squared	-0.0002	0.0002	-0.0006	0.0001
Property ownership	sig 5%			
Marital status	insig			
Employment status	sig 5%			
Constant	0.69*	0.39	-0.07	1.46

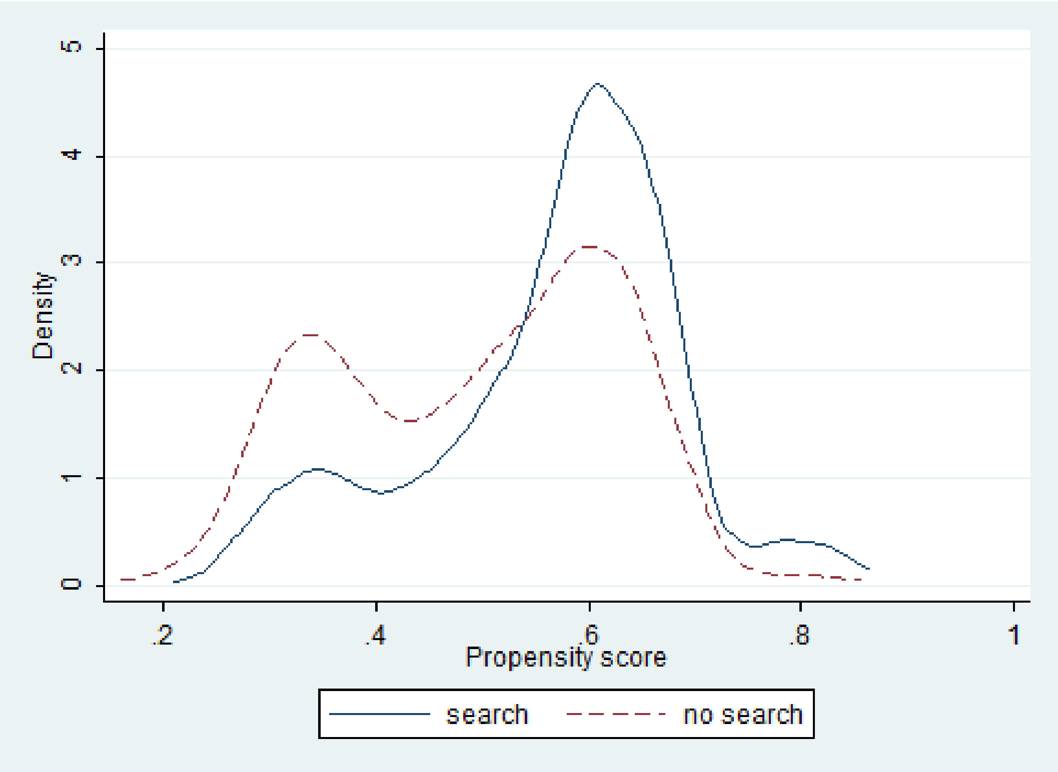
Notes: (i) ** indicates significance at 5%; (ii) * indicates significance at 10%; (iii) coefficients of variables with more categories are not included but the joint significance of their coefficients is mentioned where the variable is in the model (sig 5% - significant at 5%, sig 10% - significant at 10%, insig – insignificant); (iv) sample size: 4,406; (vi) log-likelihood: -2,887; (vii) pseudo-R2: 0.05

3.6.2. Creating the balanced sample (matching)

I use the model described above to estimate the propensity score for each individual in the sample. However, before the propensity scores can be used to create the balanced sample, it is necessary to check that there is sufficient overlap between the two groups. For example, if those who search before taking out a credit card all have high propensity scores and those who do not search all have low propensity scores, it may be that there is not sufficient overlap between the two groups to find many pairs of searchers and non-searchers with similar propensity scores.

The graph below shows the distribution of propensity scores in the search and no search groups in the full sample.

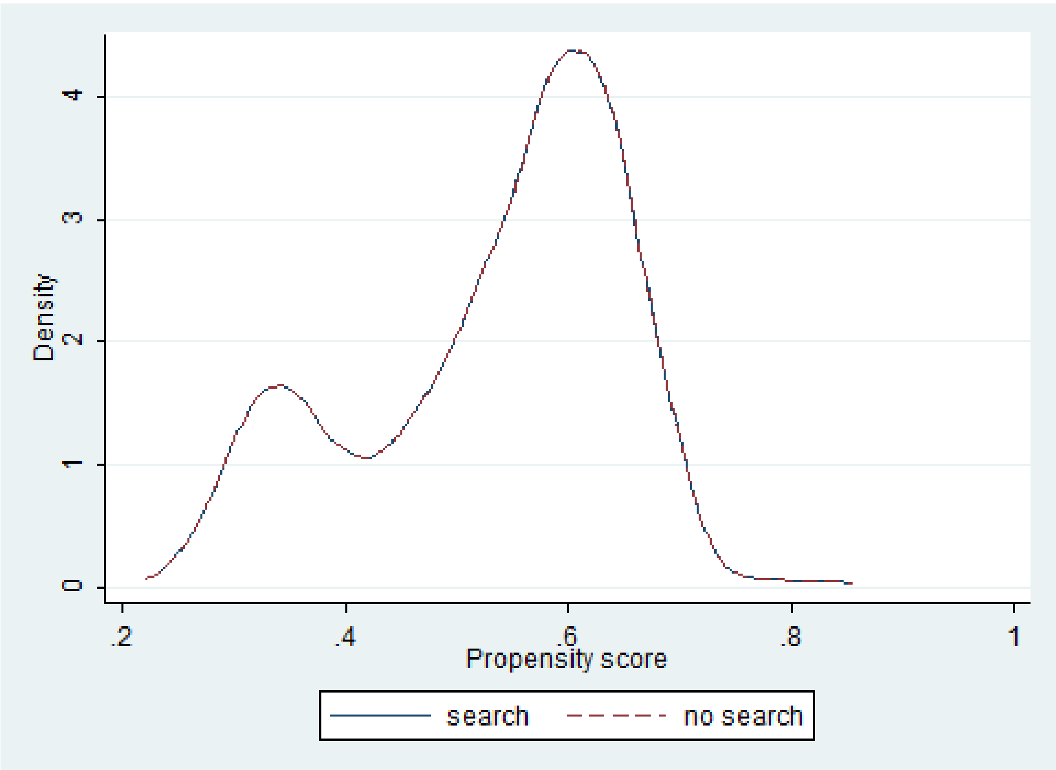
Figure 3.1: Distribution of propensity scores, full sample



As shown in Figure 3.1 above, there is a large overlap between the estimated propensity scores of searchers and non-searchers, although more searchers have higher propensity scores than non-searchers (as expected). Following the guidance in Hosmer et al (2013), I drop all observations that are outside of the common support, i.e. (i) consumers who did not search and have a lower propensity score than the lowest propensity score among searchers and (ii) consumers who searched and have a higher propensity score than the highest propensity score among non-searchers. This results in dropping 14 observations. I then refit the model and estimate propensity scores again – this time all observations are in the common support (minimum propensity score is 0.22 and the maximum is 0.86).

To match treated and untreated observations, that is, consumers who search and consumers who do not search and have similar propensity scores, I use one to one (nearest neighbour) matching without replacement. This means that each treated observation is matched with one untreated observation, and once an untreated observation is matched, it cannot be used again. I specify the maximum distance in propensity scores of matched pairs to be 0.0003. While this is significantly lower than the recommendations in the literature (0.2 times the standard deviation of the propensity score (see, for example, Austin, 2011), which, in this case would be 0.0258), it allows me to obtain a balanced sample without the need of adding any interaction terms, and still keeping the sample size sufficiently large. In the balanced sample there are 2,624 observations, 1,312 treated and 1,312 untreated. The graph below shows the distribution of propensity scores in the matched sample.

Figure 3.2: Distribution of propensity scores, matched sample



As Figure 3.2 shows, the distribution of propensity scores in the search and no search groups is almost identical. This is a result of the narrow maximum distance I specify when matching treated and untreated observations.

I check the balance between the treated and untreated subsamples and find no significant imbalance. The details of this analysis are shown in Appendix 3.5.

3.6.3. Estimating the relationship between search and product complexity

Analyses that rely on propensity scores assume that the model estimating the propensity score controls for all important variables, that is, the variables included capture all relevant pre-treatment differences between the treated and untreated groups. If this assumption (called unconfoundedness) applies, the relationship between the treatment and the outcome can directly be estimated by comparing the outcomes in the treated and untreated groups in the matched sample.

As noted above, some of the variables cannot be used in the estimation of propensity scores because there is uncertainty around timing – they may have been set before taking out a credit card but could also have been influenced by search. For example, it could be that the consumer incurred unexpected charges on her credit card and so decided to shop around and took out a new one. Equally, it may be that she incurred unexpected charges after taking out a new credit card, and she could have avoided these charges if she had shopped around. Given this, I cannot ascertain that the estimation of propensity scores controls for all relevant variables. To mitigate this concern, instead of simply comparing the outcomes in the treated and untreated groups, I estimate the relationship between search and product complexity on the balanced sample including additional controls, following the recommendation by Stuart (2010): if “it is deemed to be critical to control for a variable potentially affected by the treatment assignment, it is better to exclude that variable in the matching procedure and include it in the analysis model for the outcome”. Thus, I estimate the logistic model developed on the full sample on the matched sample.

Hosmer et al (2013) recommend three ways to take into account the correlation created in the matched sample when estimating the impact of treatment on the outcome. These three methods are: standard logistic regression with robust standard errors, conditional logistic model and population average model. The second method has a disadvantage of dropping all pairs where both observations of a pair have the same outcome. In this case, it resulted in dropping 2,132 observations, leaving only 492 to do the analysis on. I therefore decided to disregard this method. The first and the third methods give similar results so I report only the first of them. The results are shown in the table below and discussed in the next section.

Table 3.9: Logistic regression estimates, balanced sample

	Coef.	Std. error	95% confidence interval	
Searched before taking out a credit card	0.43**	0.20	0.04	0.82
Ways of using credit cards				
Number of credit cards held	-0.09	0.06	-0.21	0.04
Any of the consumer's credit cards offers rewards	-0.24	0.16	-0.55	0.07
Uses the main credit card for day-to-day purchases	-0.01	0.22	-0.45	0.42
Spent more on a credit card than budgeted for in the past 12 months	-0.29	0.49	-1.25	0.67
Incurred unexpected charges in the past 12 months	0.17	0.28	-0.37	0.71
Incurred higher than expected charges in the past 12 months	0.66**	0.31	0.06	1.27
Found that paying back a balance takes longer than expected in the past 12 months	0.78**	0.19	0.40	1.16
Total amount outstanding last month after repayment (£)	insig			
Frequency of paying interest	sig 5%			
Frequency of using the main credit card	insig			
Reasons for taking out a credit card				
Decided to take out a credit card to use it for online purchases safely	0.35**	0.17	0.01	0.70
Decided to take out a credit card to build/improve credit history	0.11	0.17	-0.23	0.44
Decided to take out a credit card because on a previous card the terms and conditions were changed	1.35**	0.39	0.58	2.12
Reasons for choosing a particular credit card or provider				
Decided to take out a particular credit card because it suited his/her needs the best	-0.58**	0.15	-0.88	-0.29
Relationship with the chosen provider				
Had a credit card with the chosen company before	-0.47*	0.28	-1.03	0.09
Had some other relationship with the chosen company	-0.40*	0.23	-0.84	0.04
Demographics				
Age	-0.03**	0.01	-0.05	-0.01
Property ownership	sig 5%			
Marital status	insig			
Employment status	insig			
Interaction terms				
Searched before taking out a credit card and uses her credit card for day-to-day purchases	-0.47*	0.27	-1.00	0.07
Decided to take out a credit card to use it for online purchases safely and incurred unexpected charges in the past 12 months	0.62	0.52	-0.40	1.64
Spent more on a credit card than budgeted for in the past 12 months and age	0.02	0.01	-0.01	0.04
Constant	-0.48	0.55	-1.56	0.59

Notes: (i) ** indicates significance at 5%; (ii) * indicates significance at 10%; (iii) coefficients of variables with more categories are not included but the joint significance of their coefficients is mentioned where the variable is in the model (sig 5% - significant at 5%, sig 10% - significant at 10%, insig – insignificant); (iv) sample size: 2,624; (v) log-likelihood: -788; (vi) pseudo-R2: 0.13; (vii) the estimates using the balanced sample depend on the order of the observations and thus can change slightly if the dataset is reordered

3.7. Discussion of results

The results of the logistic regression estimates using the full sample and the balanced sample are shown in Table 3.6 and Table 3.9, respectively. These tables report the estimated coefficients, their standard errors and 95% confidence interval. Below I repeat the results but this time transformed to odds ratios that are easier to interpret. In addition, I change the order of the variables to show the key results first.

Table 3.10: Odds ratios, full and balanced sample

	Full sample	**Balanced sample**
Key results – search and interaction with search		
Searched before taking out a credit card	1.56**	1.54**
Uses the main credit card for day-to-day purchases	0.96	0.99
Searched before taking out a credit card and uses her credit card for day-to-day purchases	0.66**	0.63*
Ways of using credit cards		
Number of credit cards held	0.85**	0.92
Any of the consumer's credit cards offers rewards	0.75**	0.79
Spent more on a credit card than budgeted for in the past 12 months	0.59	0.75
Incurred unexpected charges in the past 12 months	1.10	1.19
Incurred higher than expected charges in the past 12 months	1.61*	1.94**
Found that paying back a balance takes longer than expected in the past 12 months	1.84**	2.19**
Total amount outstanding last month after repayment (£)	insig	insig
Frequency of paying interest	sig 5%	sig 5%
Frequency of using the main credit card	insig	insig
Reasons for taking out a credit card		
Decided to take out a credit card to use it for online purchases safely	1.24*	1.43**
Decided to take out a credit card to build/improve credit history	1.34**	1.11
Decided to take out a credit card because on a previous card the terms and conditions were changed	1.57*	3.86**
Reasons for choosing a particular credit card or provider		
Decided to take out a particular credit card because it suited his/her needs the best	0.67**	0.56**
Relationship with the chosen provider		
Had a credit card with the chosen company before	0.59**	0.62*
Had some other relationship with the chosen company	0.69**	0.67*
Demographics		
Age	0.98**	0.97**
Property ownership	sig 5%	sig 5%
Marital status	insig	insig
Employment status	insig	insig
Interaction terms		
Decided to take out a credit card to use it for online purchases safely and incurred unexpected charges in the past 12 months	2.94**	1.86
Spent more on a credit card than budgeted for in the past 12 months and age	1.02**	1.02

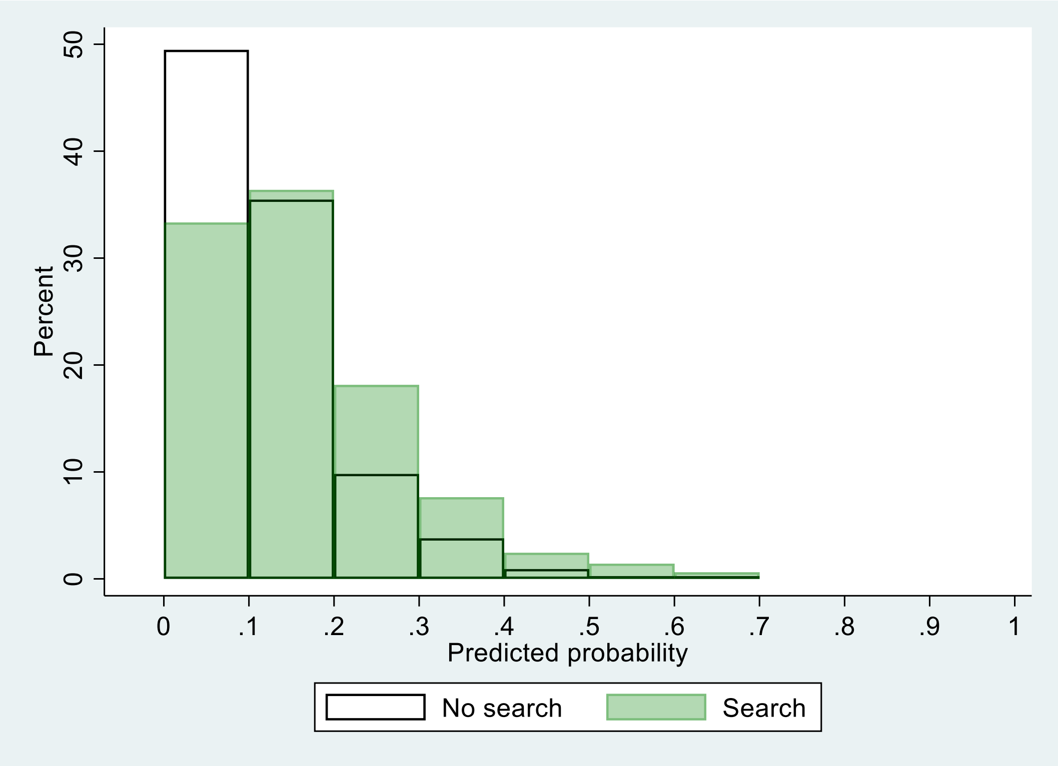
Notes: (i) ** indicates significance at 5%; (ii) * indicates significance at 10%; (iii) odds ratios for variables with more categories are not included but the joint significance of their coefficients is mentioned where the variable is in the model (sig 5% - significant at 5%, sig 10% - significant at 10%, insig – insignificant); (iv) full sample size: 4,406; (v) balanced sample size: 2,626; (vi) full sample log-likelihood: 1,375; (vii) balanced sample log-likelihood: -788; (viii) full sample pseudo-R2: 0.10; (ix) balanced sample pseudo-R2: 0.13; (x) the estimates using the balanced sample depend on the order of the observations and thus can change slightly if the dataset is reordered

As shown in Table 3.10, the estimated odds ratios are similar using the full and the balanced sample for most variables but their significance differs in some cases. For the main variables of interest, though, the two sets of results are consistent. In particular, the odds of finding credit cards difficult to understand for consumers who search are 1.54-1.56 times larger than the odds for consumers who do not search. However, this result only holds for consumers who do *not* use their credit cards for day-to-day purchases. For consumers who use their credit cards for day-to-day purchases, the odds of finding credit cards difficult to understand are about the same among searchers and non-searchers (this can be calculated using the estimated coefficients as shown in Table 3.6 and Table 3.9).[67]

Figure 3.3 below demonstrates the difference between those searchers and non-searchers who do not use their credit cards for day-to-day purchases. It shows a histogram of the probability that a consumer will find credit cards difficult to understand, as predicted by the model. For instance, the probability of finding credit cards difficult to understand is less than 0.1 for almost half of non-searchers but only for about a third of searchers. The proportion of searchers and non-searchers who are predicted to find credit cards difficult to understand with a probability between 0.1 and 0.2 is similar. However, a larger proportion of searchers falls into every ‘bucket’ of higher predicted probabilities, i.e. the model predicts a probability between 0.2 and 0.3 for about 10 percent of non-searchers and close to 20 percent of searchers, between 0.3 and 0.4 for about 4 percent of non-searchers and 8 percent of searchers, and so on.

[67] If the consumer uses the credit card for day-to-day purchases ($d = 1$), the effect of search on the logit of the outcome is given by the sum of the coefficients of search and the interaction term of search and day-to-day purchases, which is close to zero ($0.44 - 0.42 = 0.02\ in\ the\ full\ sample, 0.43 - 0.47 = -0.04\ in\ the\ balanced\ sample$), resulting in an odds ratio close to one, indicating no difference between searchers and non-searchers.

Figure 3.3: Histogram of the predicted probability of finding credit cards difficult to understand for consumers who do not use their credit cards for day-to-day purchases



In contrast, a similar graph for consumers who use their credit cards for day-to-day purchases does not reveal a consistent difference in the distribution of predicted probabilities for searchers and non-searchers.

Having found this result using the logistic regression model, I checked whether it also appears using descriptive statistics, i.e. without controlling for additional factors. I found that it does: among those consumers who do not use their credit cards for day-to-day purchases 17% of searchers say that credit cards are difficult to understand compared to 13% of those who do not search (the difference is statistically significant and gives an odds ratio of 1.4), whereas there is no difference between searchers and non-searchers among consumers who use their credit cards for day-to-day purchases (9% and 8%, respectively, resulting in an odds ratio of 1.0).

It is beyond the scope of this paper and what the dataset allows to test empirically the reasons why the impact of search on perceived product complexity varies by what consumers use their credit cards for. One potential explanation is that whether consumers use their credit cards for day-to-day purchases is indicative of the amount of experience in using credit cards before (and after) taking out the new credit card. It may be that search does not reveal much new information on complexity for consumers who use their credit cards for most purchases. In addition, it is also possible that consumers who use their credit cards for day-to-day purchases are generally more comfortable with using credit and have a better understanding of its complexities, in which case search may not make a difference.

A further possible explanation – which I was able to investigate to some extent with the available dataset – is that whether consumers view credit cards complex depends on how they use them: if they do not use complicated features and repay their balance every month, they will consider credit cards to be simple to use, because for them they are. A larger proportion of consumers who use their credit cards for day-to-day purchases never pay interest (68%) and a smaller proportion of them use balance transfers, cash withdrawals and money transfers (26%), compared to the group of consumers who do not use their credit cards for day-to-day purchases (46% and 43%, respectively), which could explain why search has a different impact on their views on product complexity. In order to test this hypothesis, I created a complexity dummy that equals one if the consumer pays interest, uses complicated features, incurred some unexpected charges or found that it takes longer to pay a balance back than expected, and zero otherwise. I then checked using descriptive statistics whether the effect of search on perceived complexity differs depending on whether the consumer has indeed faced complexity but found no significant relationship.[68] This suggests that the argument that the significance of the search and day-to-day interaction term is driven by day-to-day use acting as a proxy for whether consumers use their credit cards in a complex way is unlikely to be valid.

In addition to the main variable of interest, there are a number of factors that appear to have a significant relationship with consumers' views on product complexity (at least at 10% significance level using either sample). In line with expectations, the odds of finding credit cards difficult to understand are generally larger if the consumer has faced (or realised that she was facing) some of the complexities, such as incurring higher than expected charges, taking longer than expected to pay back a balance, paying interest frequently or having experienced

[68] 14% of those consumers who have faced complexity in some way and searched say that credit cards are difficult to understand. This compares with 13% of those who faced complexity and did not search. The difference is not statistically significant.

a change in the terms and conditions. Slightly more difficult to interpret but consumers are also more likely to consider credit cards complicated if they decided to take out a credit card to use it for online purchases safely and if they rent, rather than own a property. The odds of finding credit cards difficult to understand are smaller if the consumer decided to take out a particular credit card because it suited her needs the best and if the consumer had a previous credit card or some other (non-financial) relationship with the chosen company. The odds of finding credit cards difficult to understand decreases slightly with the age of the consumer, but this effect disappears if the consumer spent more on the credit card than she had budgeted for (as the coefficients of age and of the interaction term are the same magnitude but have the opposite sign).

For a few variables, e.g. the number of credit cards held or whether any of the consumer's credit cards offers rewards, the estimates are inconsistent showing a significant relationship with the outcome when using the full sample but not when using the balanced sample. These may be because the balanced sample consists of fewer observations (2,626 vs. 4,406) and thus results in less precise estimates or because the full sample reveals some 'false' relationships that disappear in the balanced sample.

3.8. Limitations

One methodological challenge in trying to measure the impact of search on consumers' views on product complexity is that there may be a causal effect in both directions: search can influence what consumers think about the level of complexity of the product but consumers' views on product complexity may also influence whether they search or not. Given this, ideally one would have data on consumers' views on product complexity before and after taking out a new credit card (with or without search) and measure the impact of search on the *change* in their view. Unfortunately the survey dataset only contains information on consumers' views at the point of filling out the questionnaire, which was after they took out a credit card (with or without search). This means that I am unable to ascertain the direction of the impact with certainty.

I address this concern by controlling for factors that do not directly measure consumers' views on complexity before taking out a credit card but may be good proxies for them. The purposeful method of selecting the important controls ensures that all variables that have a meaningful connection to the outcome are included in the model. Variables that could proxy consumers' views on product complexity before searching / not searching include reasons for taking out a credit card and the consumer's pre-existing relationship with the provider from whom she obtained the credit card. To the extent that these are sufficiently good proxies, the findings are more likely to describe the impact of search on the view on product complexity than the other way around.

In addition, one key assumption of the model is that the explanatory variables, including ways of using credit cards are exogenously determined, i.e. not affected by the consumers' views on complexity. For example, if a consumer needs credit, she will pay interest frequently; if she travels a lot, she will collect air miles (rewards); if she wants to build a good credit history, she will use her credit card frequently, etc.; and these choices do not depend on whether she thinks credit cards are difficult to understand. While this may not apply to all consumers (e.g. it could be that some consumers who think credit cards are complex decide to use them less frequently), it is not possible to incorporate that in the model. However, if we do not control for ways in which people use their credit cards, we leave crucial variables out that definitely influence consumers' views on complexity. The main reason why credit cards are complicated is exactly that they can be used in many ways and these different ways imply different levels of complexity.

3.9. Summary

This paper's main contribution is testing a potential benefit of consumer search that, to my knowledge, has not been considered before. I use data from a large scale consumer survey and apply a systematic model building process. As an extension, I repeat the analysis on a matched sample created using propensity score matching. In both cases, I find that consumers who do not use their credit cards for day-to-day purchases are more likely to say that credit cards are complex products if they shopped around than if they did not. Notwithstanding the limitations resulting from the structure of the dataset, I conclude that this is likely to suggest that search can raise awareness of product complexity. Even if consumers do not fully understand complicated features, being aware of them may help them make the right choice and avoid mistakes when using the product. Future research could cover testing the same hypothesis using data collected specifically for this purpose (to minimise identification concerns).

APPENDICES FOR CHAPTER 3

Appendix 3.1 – Excluding respondents with ambiguity on search behaviour

When defining the groups of searchers and non-searchers, I excluded the following respondents:

- 591 respondents (9.4%) who said that they took out a credit card both with and without considering other credit cards (yes to Q11i and Q11iii);
- 144 respondents (2.3%) who said they took out a credit card after considering two or more credit cards but were unsure whether they took out a credit card without considering others (yes to Q11i and unsure to Q11iii);
- 100 respondents (1.6%) who said they took out a credit card without considering other cards but considered two or more credit cards on another occasion without taking one out (yes to Q11iii and Q11ii);
- 85 respondents (1.3%) who said they took out a credit card without considering other cards but were unsure whether they took out a credit card after considering others (yes to Q11iii and unsure to Q11i);
- 31 respondents (0.5%) who said they took out a credit card without considering other cards but were unsure whether they considered two or more credit cards on another occasion without taking one out (yes to Q11iii and unsure to Q11ii).

Appendix 3.2 – Full list of variables considered in the analysis

Table 3A.1: Full list of variables considered in the analysis

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
Thinks that credit cards are difficult to understand	Q50a	Dummy	Not difficult (0)	4,621 (88%)	3,917 (89%)
			Difficult (1)	629 (12%)	489 (11%)
			Missing	-	-
Searched before taking out a credit card	Q11, Q42a	Dummy	No (0)	2,457 (47%)	2,058 (47%)
			Yes (1)	2,793 (53%)	2,348 (53%)
			Missing	-	-
Number of credit cards held	Q1	Treated as continuous	1 to 15	2.3 (average)	2.4 (average)
			Missing	-	-
Number of credit cards used	Q3	Treated as continuous	1 to 12	2.0 (average)	2.1 (average)
			Missing	-	-
Paid interest on main credit card but did not expect to do so when taking it out	Q4cii, Q6cii, Q16	Dummy	No (0)	4,507 (86%)	3,902 (89%)
			Yes (1)	388 (7%)	335 (8%)
			Missing	355 (7%)	169 (4%)
Any of the consumer’s credit cards offers rewards	Q8a, Q9a	Dummy	No (0)	1,694 (32%)	1,291 (29%)
			Yes (1)	3,556 (68%)	3,115 (71%)
			Missing	-	-
Made a balance transfer in the last 12 months	Q8c, Q8d, Q9e	Dummy	No (0)	3,693 (70%)	3,116 (71%)
			Yes (1)	1,557 (30%)	1,290 (29%)
			Missing	-	-
Any of the consumer’s credit cards is a low and grow card	Q8e, Q9i	Dummy	No (0)	4,442 (85%)	3,767 (85%)
			Yes (1)	808 (15%)	639 (15%)
			Missing	-	-
Uses the main credit card for day-to-day purchases	Q19	Dummy	No (0)	2,141 (41%)	1,745 (40%)
			Yes (1)	3,024 (57%)	2,661 (60%)
			Missing	85 (2%)	-
Uses the main credit card for cash advances	Q19	Dummy	No (0)	4,876 (93%)	4,185 (95%)
			Yes (1)	289 (5%)	221 (5%)
			Missing	85 (2%)	-

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
Uses the main credit card for money transfer	Q19	Dummy	No (0)	5,059 (96%)	4,325 (98%)
			Yes (1)	106 (2%)	81 (2%)
			Missing	85 (2%)	-
Usually unable to repay the balance fully on the main credit card	Q23	Dummy	No (0)	4,736 (90%)	4,012 (91%)
			Yes (1)	514 (10%)	394 (9%)
			Missing	-	-
Uses direct debit, mobile or online banking to repay	Q24	Dummy	No (0)	535 (10%)	399 (9%)
			Yes (1)	4,715 (90%)	4,007 (91%)
			Missing	-	-
Spent more on a credit card than budgeted for in the past 12 months	Q52	Dummy	No (0)	4,287 (82%)	3,614 (82%)
			Yes (1)	963 (18%)	792 (18%)
			Missing	-	-
Exceeded credit limit in the past 12 months	Q52	Dummy	No (0)	4,987 (95%)	4,205 (95%)
			Yes (1)	263 (5%)	201 (5%)
			Missing	-	-
Incurred unexpected charges in the past 12 months	Q52	Dummy	No (0)	4,912 (94%)	4,145 (94%)
			Yes (1)	338 (6%)	261 (6%)
			Missing	-	-
Incurred higher than expected charges in the past 12 months	Q52	Dummy	No (0)	5,095 (97%)	4,284 (97%)
			Yes (1)	155 (3%)	122 (3%)
			Missing	-	-
Found that paying back a balance takes longer than expected in the past 12 months	Q52	Dummy	No (0)	4,649 (89%)	3,903 (89%)
			Yes (1)	601 (11%)	503 (11%)
			Missing	-	-
Had a credit card when taking out the new	Q36b, Q47c	Dummy	No (0)	1,262 (24%)	1,076 (24%)
			Yes (1)	3,552 (68%)	3,220 (73%)
			Missing	436 (8%)	110 (3%)
Total credit limit on all credit cards held (£)	Q4a	Categorical	0-250 (1)	152 (3%)	114 (3%)
			250-500 (2)	233 (4%)	183 (4%)
			500-750 (3)	141 (3%)	107 (2%)
			750-1,000 (4)	249 (5%)	194 (4%)
			1,000-2,500 (5)	674 (13%)	549 (12%)
			2,500-5,000 (6)	939 (18%)	769 (17%)
			5,000-7,500 (7)	605 (12%)	517 (12%)
			7,500-10,000 (8)	531 (10%)	461 (10%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
			10,000-15,000 (9)	648 (12%)	564 (13%)
			15,000-20,000 (10)	389 (7%)	356 (8%)
			over 20,000 (11)	504 (10%)	465 (11%)
			Missing	185 (4%)	127 (3%)
Total amount outstanding last month after repayment (£)	Q4bii	Categorical	0 (1)	2,875 (55%)	2,482 (56%)
			0-250 (2)	311 (6%)	244 (6%)
			250-500 (3)	216 (4%)	177 (4%)
			500-750 (4)	161 (3%)	131 (3%)
			750-1,000 (5)	216 (4%)	178 (4%)
			1,000-2,500 (6)	478 (9%)	400 (9%)
			2,500-5,000 (7)	459 (9%)	392 (9%)
			5,000-7,500 (8)	201 (4%)	167 (4%)
			7,500-10,000 (9)	115 (2%)	99 (2%)
			10,000-15,000 (10)	79 (2%)	69 (2%)
			over 15,000 (11)	79 (2%)	67 (2%)
			Missing	60 (1%)	(0%)
Average monthly spending (£)	Q4c	Categorical	0 (1)	186 (4%)	135 (3%)
			0-50 (2)	640 (12%)	503 (11%)
			50-100 (3)	635 (12%)	509 (12%)
			100-150 (4)	467 (9%)	391 (9%)
			150-250 (5)	548 (10%)	456 (10%)
			250-500 (6)	900 (17%)	773 (18%)
			500-1,000 (7)	891 (17%)	803 (18%)
			1,000-2,000 (8)	548 (10%)	493 (11%)
			over 2,000 (9)	287 (5%)	258 (6%)
			Missing	148 (3%)	85 (2%)
Frequency of paying interest	Q4cii	Categorical	Never (0)	3,039 (58%)	2,690 (61%)
			Rarely (1)	567 (11%)	478 (11%)
			Occasionally (2)	679 (13%)	549 (12%)
			Frequently (3)	905 (17%)	689 (16%)
			Missing	60 (1%)	-
Frequency of using the main credit card	Q18	Categorical	Less than once a year (0)	300 (6%)	236 (5%)
			Once or twice a year (1)	252 (5%)	203 (5%)
			Once a quarter (2)	398 (8%)	328 (7%)
			Once a month (3)	588 (11%)	486 (11%)
			2-3 times a month (4)	885 (17%)	736 (17%)
			Once a week (5)	886 (17%)	781 (18%)
			Several times a week (6)	1,807 (34%)	1,636 (37%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
			Missing	134 (3%)	-
Decided to take out a credit card because of a change in personal circumstances	Q32a, Q40a	Dummy	No (0)	3,904 (74%)	3,525 (80%)
			Yes (1)	986 (19%)	881 (20%)
			Missing	360 (7%)	-
Decided to take out a credit card because of a change in financial circumstances	Q32a, Q40a	Dummy	No (0)	3,871 (74%)	3,493 (79%)
			Yes (1)	1,019 (19%)	913 (21%)
			Missing	360 (7%)	-
Decided to take out a credit card to use it for online purchases safely	Q32a, Q40a	Dummy	No (0)	3,926 (75%)	3,515 (80%)
			Yes (1)	964 (18%)	891 (20%)
			Missing	360 (7%)	-
Decided to take out a credit card to use it abroad safely	Q32a, Q40a	Dummy	No (0)	4,101 (78%)	3,667 (83%)
			Yes (1)	789 (15%)	739 (17%)
			Missing	360 (7%)	-
Decided to take out a credit card to use it where debit cards are not accepted	Q32a, Q40a	Dummy	No (0)	4,562 (87%)	4,108 (93%)
			Yes (1)	328 (6%)	298 (7%)
			Missing	360 (7%)	-
Decided to take out a credit card to build/improve credit history	Q32a, Q40a	Dummy	No (0)	4,124 (78%)	3,716 (84%)
			Yes (1)	766 (15%)	690 (16%)
			Missing	360 (7%)	-
Decided to take out a credit card to benefit from an introductory offer	Q32a, Q40a	Dummy	No (0)	3,335 (63%)	2,992 (68%)
			Yes (1)	1,555 (30%)	1,414 (32%)
			Missing	360 (7%)	-
Decided to take out a credit card to benefit from rewards	Q32a, Q40a	Dummy	No (0)	3,271 (62%)	2,882 (65%)
			Yes (1)	1,619 (31%)	1,524 (35%)
			Missing	360 (7%)	-
Decided to take out a credit card to benefit from low APR	Q32a, Q40a	Dummy	No (0)	4,565 (87%)	4,114 (93%)
			Yes (1)	325 (6%)	292 (7%)
			Missing	360 (7%)	-
Decided to take out a credit card to benefit from low interest rate	Q32a, Q40a	Dummy	No (0)	4,325 (82%)	3,891 (88%)
			Yes (1)	565 (11%)	515 (12%)
			Missing	360 (7%)	-
Decided to take out a credit card to	Q32a, Q40a	Dummy	No (0)	4,558 (87%)	4,112 (93%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
benefit from low fees			Yes (1)	332 (6%)	294 (7%)
			Missing	360 (7%)	-
Decided to take out a credit card because on a previous card the introductory deal ended	Q32a, Q40a	Dummy	No (0)	4,213 (80%)	3,794 (86%)
			Yes (1)	677 (13%)	612 (14%)
			Missing	360 (7%)	-
Decided to take out a credit card because on a previous card the terms and conditions were changed	Q32a, Q40a	Dummy	No (0)	4,703 (90%)	4,228 (96%)
			Yes (1)	187 (3%)	178 (4%)
			Missing	360 (7%)	-
Decided to take out a credit card because incurred unexpected fees or interest on a previous card	Q32a, Q40a	Dummy	No (0)	4,742 (90%)	4,271 (97%)
			Yes (1)	148 (3%)	135 (3%)
			Missing	360 (7%)	-
Decided to take out a credit card because on a previous card the credit limit was too low	Q32a, Q40a	Dummy	No (0)	4,746 (90%)	4,277 (97%)
			Yes (1)	144 (3%)	129 (3%)
			Missing	360 (7%)	-
Decided to take out a credit card because on a previous card the customer service was bad	Q32a, Q40a	Dummy	No (0)	4,778 (91%)	4,302 (98%)
			Yes (1)	112 (2%)	104 (2%)
			Missing	360 (7%)	-
Decided to take out a credit card from company X because had a credit card with them before	Q32c, Q47a	Dummy	No (0)	4,447 (85%)	4,010 (91%)
			Yes (1)	437 (8%)	396 (9%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because had another financial product with them before	Q32c, Q47a	Dummy	No (0)	3,743 (71%)	3,355 (76%)
			Yes (1)	1,141 (22%)	1,051 (24%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because shops with them	Q32c, Q47a	Dummy	No (0)	4,194 (80%)	3,771 (86%)
			Yes (1)	693 (13%)	635 (14%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because likes the brand	Q32c, Q47a	Dummy	No (0)	3,878 (74%)	3,504 (80%)
			Yes (1)	1,006 (19%)	902 (20%)
			Missing	366 (7%)	-

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
Decided to take out a credit card from company X because it is linked to a sports club or charity	Q32c, Q47a	Dummy	No (0)	4,866 (93%)	4,393 (100%)
			Yes (1)	18 (0%)	13 (0%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because it offered a good/personalised credit card design	Q32c, Q47a	Dummy	No (0)	4,721 (90%)	4,261 (97%)
			Yes (1)	163 (3%)	145 (3%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because it offered good customer service	Q32c, Q47a	Dummy	No (0)	3,999 (76%)	3,605 (82%)
			Yes (1)	885 (17%)	801 (18%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because it offered a UK call centre	Q32c, Q47a	Dummy	No (0)	4,478 (85%)	4,037 (92%)
			Yes (1)	406 (8%)	369 (8%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because it offered text/email updates and alerts	Q32c, Q47a	Dummy	No (0)	4,728 (90%)	4,259 (97%)
			Yes (1)	156 (3%)	147 (3%)
			Missing	366 (7%)	-
Decided to take out a credit card from company X because it offered an easy to use online system	Q32c, Q47a	Dummy	No (0)	3,930 (75%)	3,541 (80%)
			Yes (1)	954 (18%)	865 (20%)
			Missing	366 (7%)	-
Decided to take out that credit card because it suited his/her needs the best	Q32c, Q47a	Dummy	No (0)	2,767 (53%)	2,465 (54%)
			Yes (1)	2,117 (40%)	1,941 (44%)
			Missing	366 (7%)	-
Decided to take out that credit card because the company offered it	Q32c, Q47a	Dummy	No (0)	4,173 (79%)	3,751 (85%)
			Yes (1)	711 (14%)	655 (15%)
			Missing	366 (7%)	-
Decided to take out that credit card because it was easy to get it	Q32c, Q47a	Dummy	No (0)	4,108 (78%)	3,695 (84%)
			Yes (1)	776 (15%)	711 (16%)
			Missing	366 (7%)	-
Decided to take out that credit card because saw an advert/offer	Q32c, Q47a	Dummy	No (0)	4,515 (86%)	4,067 (92%)
			Yes (1)	369 (7%)	339 (8%)
			Missing	366 (7%)	-
Decided to take out that card because a family	Q32c, Q47a	Dummy	No (0)	4,589 (87%)	4,130 (94%)
			Yes (1)	295 (6%)	276 (6%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
member/friend recommended it			Missing	366 (7%)	-
Decided to take out that credit card because a price comparison website ranked it highly	Q32c, Q47a	Dummy	No (0)	4,257 (81%)	3,819 (87%)
			Yes (1)	627 (12%)	587 (13%)
			Missing	366 (7%)	-
Decided to take out that credit card because it was the only credit card he/she was accepted for	Q32c, Q47a	Dummy	No (0)	4,762 (91%)	4,303 (98%)
			Yes (1)	122 (2%)	103 (2%)
			Missing	366 (7%)	-
Had a credit card with the chosen company before	Q35, Q45	Dummy	No (0)	4,496 (86%)	4,026 (91%)
			Yes (1)	430 (8%)	380 (9%)
			Missing	324 (6%)	-
Had a current account with the chosen company	Q35, Q45	Dummy	No (0)	3,592 (69%)	3,209 (73%)
			Yes (1)	1,334 (25%)	1,197 (27%)
			Missing	324 (6%)	-
Had some other financial relationship with the chosen company	Q35, Q45	Dummy	No (0)	4,586 (87%)	4,090 (93%)
			Yes (1)	340 (7%)	316 (7%)
			Missing	324 (6%)	-
Had some other relationship with the chosen company	Q35, Q45	Dummy	No (0)	4,281 (82%)	3,813 (87%)
			Yes (1)	645 (12%)	593 (13%)
			Missing	324 (6%)	-
Did not have any relationship with the chosen company	Q35, Q45	Dummy	No (0)	2,392 (46%)	2,153 (49%)
			Yes (1)	2,534 (48%)	2,253 (51%)
			Missing	324 (6%)	-
Age		Treated as continuous	18 to 90	45 (average)	46 (average)
			Missing	-	-
Gender		Dummy	Male (0)	2,694 (51%)	2,281 (52%)
			Female (1)	2,556 (49%)	2,125 (48%)
			Missing	-	
Region		Categorical	North East (1)	210 (4%)	180 (4%)
			North West (2)	581 (11%)	479 (11%)
			Yorkshire (3)	460 (9%)	385 (9%)
			East Midlands (4)	373 (7%)	311 (7%)
			West Midlands (5)	418 (8%)	354 (8%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
			East of England (6)	458 (9%)	370 (8%)
			London (7)	730 (14%)	616 (14%)
			South East (8)	762 (15%)	645 (15%)
			South West (9)	442 (8%)	369 (8%)
			Wales (10)	250 (5%)	215 (5%)
			Scotland (11)	431 (8%)	366 (8%)
			Northern Ireland (12)	135 (3%)	116 (3%)
			Missing	-	-
Profession		Categorical	Professional or higher technical work (1)	1,520 (29%)	1,334 (30%)
			Manager or senior administrator (2)	1,163 (22%)	997 (23%)
			Clerical (3)	804 (15%)	670 (15%)
			Sales or services (4)	380 (7%)	307 (7%)
			Foreman or supervisor of other workers (5)	134 (3%)	115 (3%)
			Skilled manual work (6)	334 (6%)	281 (6%)
			Semi-skilled or unskilled manual work (7)	389 (7%)	303 (7%)
			Missing	526 (10%)	399 (9%)
Property ownership		Categorical	Own outright (1)	1,395 (27%)	1,245 (28%)
			Own with mortgage (2)	2,132 (41%)	1,819 (41%)
			Own with shared ownership scheme (3)	52 (1%)	41 (1%)
			Rent from private landlord (4)	921 (18%)	761 (17%)
			Rent from local authority (5)	179 (3%)	132 (3%)
			Rent from housing association (6)	204 (4%)	144 (3%)
			Lives with family/friends and pays some rent (7)	197 (4%)	171 (4%)
			Lives with family/friends and does not pay rent (8)	121 (2%)	93 (2%)
			Missing	49 (1%)	-
Marital status		Categorical	Married (1)	2,590 (49%)	2,199 (50%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
			Living with a partner (2)	862 (16%)	708 (16%)
			In a relationship (3)	290 (6%)	249 (6%)
			Separated/divorced (4)	449 (9%)	373 (8%)
			Single (5)	952 (18%)	786 (18%)
			Widowed (6)	107 (2%)	91 (2%)
			Missing	-	-
Employment status		Categorical	Full time (1)	3,096 (59%)	2,638 (60%)
			Part time (2)	623 (12%)	514 (12%)
			Part time (less than 8 hours a week) (3)	70 (1%)	60 (1%)
			Student (4)	116 (2%)	89 (2%)
			Retired (5)	987 (19%)	882 (20%)
			Unemployed (6)	85 (2%)	64 (1%)
			Not working (7)	207 (4%)	159 (4%)
			Missing	66 (1%)	-
Household size		Categorical	1 (1)	945 (18%)	799 (18%)
			2 (2)	2,126 (41%)	1,832 (42%)
			3 (3)	961 (18%)	802 (18%)
			4 (4)	850 (16%)	705 (16%)
			5 (5)	233 (4%)	177 (4%)
			6 or more (6)	94 (2%)	64 (1%)
			Missing	41 (1%)	27 (1%)
Education level		Categorical	No formal education (1)	162 (3%)	132 (3%)
			High school (2)	838 (16%)	687 (16%)
			Post-high school (3)	955 (18%)	787 (18%)
			Vocational (4)	434 (8%)	355 (8%)
			University (5)	1,451 (28%)	1,261 (29%)
			Post-grad (6)	628 (12%)	545 (12%)
			Missing	782 (15%)	639 (15%)
Household income (£)		Categorical	Below 5k (1)	29 (1%)	19 (0%)
			5-10k (2)	144 (3%)	114 (3%)
			10-15k (3)	299 (6%)	241 (5%)
			15-20k (4)	362 (7%)	295 (7%)
			20-25k (5)	401 (8%)	305 (7%)
			25-30k (6)	479 (9%)	420 (10%)
			30-35k (7)	407 (8%)	346 (8%)
			35-40k (8)	380 (7%)	330 (7%)
			40-45k (9)	364 (7%)	325 (7%)

	Question number in the survey	Type	Categories	Responses taking into account missing values	Responses in the full sample used in the analysis
			45-50k (10)	312 (6%)	266 (6%)
			50-60k (11)	411 (8%)	365 (8%)
			60-70k (12)	319 (6%)	274 (6%)
			70-100k (13)	452 (9%)	408 (9%)
			100-150k (14)	175 (3%)	160 (4%)
			Over 150k (15)	56 (1%)	53 (1%)
			Missing	660 (13%)	485 (11%)
Savings amount (£)		Categorical	Less than 100 (1)	705 (13%)	554 (13%)
			100-250 (2)	177 (3%)	141 (3%)
			250-500 (3)	165 (3%)	140 (3%)
			500-1,000 (4)	232 (4%)	183 (4%)
			1,000-2,000 (5)	301 (6%)	251 (6%)
			2,000-3,000 (6)	224 (4%)	190 (4%)
			3,000-4,000 (7)	162 (3%)	140 (3%)
			4,000-5,000 (8)	156 (3%)	137 (3%)
			5,000-10,000 (9)	435 (8%)	389 (9%)
			10,000-20,000 (10)	413 (8%)	384 (9%)
			20,000-30,000 (11)	287 (5%)	251 (6%)
			30,000-40,000 (12)	168 (3%)	158 (4%)
			40,000-50,000 (13)	125 (2%)	104 (2%)
			50,000-75,000 (14)	192 (4%)	176 (4%)
			75,000-100,000 (15)	131 (3%)	122 (3%)
			Over 100,000 (16)	376 (7%)	341 (8%)
			Missing	1,001 (19%)	745 (17%)
Monthly debt repayments (£, excluding credit card debt)	Q113	Categorical	0 (1)	1,622 (31%)	1,408 (32%)
			0-50 (2)	325 (6%)	257 (6%)
			50-100 (3)	330 (6%)	285 (6%)
			100-250 (4)	531 (10%)	448 (10%)
			250-500 (5)	690 (13%)	588 (13%)
			500-750 (6)	533 (10%)	452 (10%)
			750-1,000 (7)	424 (8%)	368 (8%)
			1,000-1,500 (8)	289 (6%)	264 (6%)
			Over 1,500 (9)	166 (3%)	146 (3%)
			Missing	340 (6%)	190 (4%)

Appendix 3.3 – Excluding variables due to selection bias

To test and compare the potential omitted variable and selection biases for any variable Z, I estimate three models:

1. A model including variable Z and all other controls that have not been discarded previously;[69]
2. The same as 1 but excluding variable Z (keeping the sample the same, i.e. not including observations for which Z is missing);
3. The same as 2 but including observations for which Z is missing.

I compare models 1 and 2 to assess the effect of omitting variable Z, and compare models 2 and 3 to assess the effect of any selection bias. I look at the change in the estimated coefficients of those variables that are significant as well as any change in significance levels. In addition to this, I look at the distribution of demographic variables for the set of observations for which a variable is missing and for the set of observations for which the same variable is not missing to understand the source of selection bias. If the selection bias appears to be large (and, in particular, larger than the omitted variable bias), I exclude the variable from the next step of building the model. The variables excluded at this stage are listed in the table below together with a description of the group of respondents for whom this information is missing.

[69] Except for savings and household income, where, when including savings, I do not include income, and the same the other way around. This is because there is a large overlap between observations for which savings and household income are missing: 75% of those observations for which household income is missing are also missing for savings, and 49% of those observations for which savings is missing are also missing for household income.

Table 3A.2: Variables excluded because of selection bias

	Proportion of observations for which it is missing	Characteristics of respondents with missing values (compared to those with valid values)
Savings amount (£)	19%	Older, married, retired, own a property outright – fewer of them search
Household income (£)	13%	Retired, own a property outright – fewer of them search
Monthly debt repayments (£, excluding credit card debt)	6%	Single, not working (but not retired) – fewer of them search
Profession	10%	Student – fewer of them search
Total credit limit on all credit cards held (£)	4%	Older, married, retired, own a property outright – fewer of them search
Paid interest on main credit card but did not expect to do so when taking it out	7%	Younger, not married, not working (but not retired), rent or live with family/friends – fewer of them search
Had a credit card when taking out the new	8%	Younger, working, has a mortgage or rent – more of them search

As Table 3A.2 shows that, with one exception, a smaller proportion of consumers search before taking out a credit card in the group for whom some information is missing than in the group for whom the same information is not missing. Thus, including these variables in the model (and therefore excluding the observations for which they are missing) would change the composition of the no search group to a larger extent than the composition of the search group.

Information such as savings, household income and credit limit is more likely to be missing for a wealthier and older group of consumers. Debt is missing for those who do not work and the profession is missing for students. Data on whether the respondent had a credit card when taking the new one out is missing for a younger but relatively wealthier group, and data on whether the respondent paid interest despite not planning to do so is missing for a younger but relatively less wealthy group.

In all cases, omitting these variables (while keeping the sample size constant) had a smaller impact on the estimates than when the observations for which they are missing were included in the sample.

Appendix 3.4 – Details of the model building process

Step 1 – univariate analysis

To test the relationship between each explanatory variable and the outcome, I use the Pearson chi-square test for categorical variables and the Wald statistic for variables that are treated as continuous. All variables with a p-value below 0.25 are included in Step 2. The following variables with p-value over 0.25 are *excluded* at this stage:

- Whether the consumer decided to take out a credit card (i) to use it abroad; (ii) to benefit from low interest rates; (iii) because on a previous card the introductory deal ended; (iv) because on a previous card the customer service was bad;
- Whether the consumer decided to take out a credit card from a company X because (i) she liked the brand; (ii) it is linked to a sports club or charity she likes; (iii) it offered good customer service; (iv) it offered an easy to use online system;
- Whether the consumer decided to take out that credit card because (i) she saw an advert/offer she liked; (ii) a price comparison website ranked it highly;
- Whether the consumer had some financial relationship (other than a credit card or a current account) with the chosen company before taking this credit card out;
- Whether the consumer had any relationship with the chosen company before taking this credit card out;
- Region; and
- Education level.

Steps 2 and 3 – fitting the model and removing insignificant controls

Step 2 of the model building process starts with running a logistic regression including all variables that were selected in Step 1. I then assess the significance of the estimated coefficients, and keep all that are significant at least at 10%. Step 3 starts with running a logistic regression using only this subset of variables. Next, I assess to what extent the estimated coefficients change between the long and the short model. As some estimates change considerably,[70] I start again from the long regression, remove only a few variables at a time and always keep those that appear to affect any other (significant) estimates. The estimation results are shown in the table below.

Table 3A.3: Logistic regression estimates for models in Step 2 and Step 3

	Long model (Step 2)	Keeping only significant variables (start of Step 3)		After gradual removal of insignificant variables (end of Step 3)	
	Coef.	**Coef.**	**Diff.**	**Coef.**	**Diff.**
Searched before taking out a credit card	0.25**	0.23**	-8%	0.24**	-2%
Ways of using credit cards					
Number of credit cards held	-0.21**	-0.16**	-26%	-0.15**	-31%
Number of credit cards used	0.10				
Any of the consumer's credit cards offers rewards	-0.29**	-0.35**	20%	-0.32**	12%
Made a balance transfer in the last 12 months	0.06				
Any of the consumer's credit cards is a low and grow card	0.01				
Uses the main credit card for day-to-day purchases	-0.27**	-0.26**	-7%	-0.28**	0%
Uses the main credit card for cash advances	-0.01				
Uses the main credit card for money transfer	0.01				
Usually unable to repay the balance fully on the main credit card	0.09				
Uses direct debit, mobile or online banking to repay	-0.18				
Spent more on a credit card than budgeted for in the past 12 months	0.25*	0.29**	17%	0.27**	8%
Exceeded credit limit in the past 12 months	0.29				
Incurred unexpected charges in the past 12 months	0.28			0.37**	
Incurred higher than expected charges in the past 12 months	0.37			0.45*	
Found that paying back a balance takes longer than expected in the past 12 months	0.62**	0.65**	4%	0.66**	5%
Total amount outstanding last month after repayment (£)	insig			insig	
Average monthly spending (£)	sig 10%	insig		sig 10%	
Frequency of paying interest	insig			sig 5%	
Frequency of using the main credit card	sig 10%	sig 10%		sig 10%	

[70] According to Hosmer et al (2013), a more than 20% change in the estimated coefficient is sufficient to warrant further investigation (see on page 92). The formula for calculating the change is $\Delta\hat{\beta}\% = 100 * (\hat{\theta} - \hat{\beta})/\hat{\beta}$, where $\hat{\theta}$ is the estimated coefficient of a variable in the short regression and $\hat{\beta}$ is the estimated coefficient of the same variable in the long regression.

	Long model (Step 2)	Keeping only significant variables (start of Step 3)		After gradual removal of insignificant variables (end of Step 3)	
Reasons for taking out a credit card					
Decided to take out a credit card because of a change in personal circumstances	0.01				
Decided to take out a credit card because of a change in financial circumstances	0.02				
Decided to take out a credit card to use it for online purchases safely	0.26**	0.30**	15%	0.29**	11%
Decided to take out a credit card to use it where debit cards are not accepted	-0.12				
Decided to take out a credit card to build/improve credit history	0.20			0.25*	
Decided to take out a credit card to benefit from an introductory offer	-0.11				
Decided to take out a credit card to benefit from rewards	-0.05				
Decided to take out a credit card to benefit from low APR	-0.17				
Decided to take out a credit card to benefit from low fees	0.26				
Decided to take out a credit card because on a previous card the terms and conditions were changed	0.43*	0.48**	10%	0.40*	-8%
Decided to take out a credit card because incurred unexpected fees or interest on a previous card	0.16				
Decided to take out a credit card because on a previous card the credit limit was too low	-0.08				
Reasons for choosing a particular credit card or provider					
Decided to take out a credit card from company X because had a credit card with them before	-0.24				
Decided to take out a credit card from company X because had another financial product with them before	0.19				
Decided to take out a credit card from company X because shops with them	0.05				
Decided to take out a credit card from company X because it offered a good/personalised credit card design	0.14				
Decided to take out a credit card from company X because it offered a UK call centre	0.20				
Decided to take out a credit card from company X because it offered text/email updates and alerts	0.28			0.42*	
Decided to take out that credit card because it suited his/her needs the best	-0.38**	-0.46**	23%	-0.40**	7%
Decided to take out that credit card because the company offered it	0.07				
Decided to take out that credit card because it was easy to get it	0.15				
Decided to take out that credit card because a family member/friend recommended it	0.06				

	Long model (Step 2)	Keeping only significant variables (start of Step 3)		After gradual removal of insignificant variables (end of Step 3)	
Decided to take out that credit card because it was the only credit card he/she was accepted for	0.37				
Relationship with the chosen provider					
Had a credit card with the chosen company before	-0.50*	-0.44**	-11%	-0.57**	14%
Had a current account with the chosen company	-0.16				
Had some other relationship with the chosen company	-0.37**	-0.32*	-15%	-0.35**	-5%
Demographics					
Age	-0.02**	-0.02**	1%	-0.02**	6%
Gender	0.11				
Property ownership	sig 5%	sig 5%		sig 5%	
Marital status	insig			insig	
Employment status	insig			insig	
Household size	insig				
Constant	-0.40	-0.46		-0.37	
Pearson chi square value	341.27	269.75		320.27	

Notes: (i) ** indicates significance at 5%; (ii) * indicates significance at 10%; (iii) coefficients of variables with more categories are not included but the joint significance of their coefficients is mentioned where the variable is in the model (sig 5% - significant at 5%, sig 10% - significant at 10%, insig – insignificant); (iv) sample size: 4,295

As Table 3A.3 shows, the estimated coefficient of a number of variables changes considerably when all variables insignificant in the long model are excluded. Given this, I proceed with removing only a few variables at a time. In each step, I remove variables whose relationship with the outcome is estimated to be the least significant (p-value over 0.9, over 0.8, over 0.7 and so on). When removing two or more variables at a time, I check whether these are correlated with each other. This is to avoid removing variables that are insignificant when included together but would be significant if one or more of them were omitted.

As shown in Table 3A.3, the difference between the estimated coefficients of the long regression and the regression following the step by step removal are generally smaller than when I remove all significant variables, and are at most 14% (in absolute terms). One exception is the number of credit cards held, of which the coefficient changes over 30% when the variable number of credit cards used is removed from the model. The two variables are highly correlated, that is why I keep only one of them in the model. Some variables that were insignificant in the long regression become significant.

When comparing the above models, I keep the sample size fixed. At this point I expand the sample to include all observations for which any of the dropped variables had a missing value. This increases the sample size from 4,295 to 4,321. The change in the sample size results in slight changes in the significance of certain estimates.

Step 5 – assessing the linearity assumption

There are two variables in the model that I treat as continuous (even though they only take discrete values): the number of credit cards held and the age of the consumer. To assess whether the logit of the outcome is linear in these variables, I use Stata's 'boxtid' command. This command estimates a Box-Tidwell regression model, tests whether the assumption of linearity can be rejected and gives the power at which a continuous variable best satisfies the linearity assumption. The test showed that it is not necessary to include non-linear transformations of these variables in the model.

Step 7 – adequacy and fit

A simple summary measure of goodness of fit (automatically reported by Stata) is the statistic showing whether the model performs better than a model that includes only a constant. This test shows that the null hypothesis that the model does not do better than the constant only model can be rejected (with a p-value of 0.0000).[71] In addition, I use the Pearson and Hosmer-Lemeshow tests for assessing the fit of the model. The table below shows the Stata outputs of the respective commands ('estat gof' and 'lfit').

Table 3A.4: Tests of goodness of fit

Pearson test		**Hosmer-Lemeshow test**	
Number of observations	4,406	Number of observations	4,406
Number of covariate patterns	4,362	Number of groups	10
Pearson chi2(4,312)	4254.32	Hosmer-Lemeshow chi2(8)	9.97
Prob > chi2	0.7095	Prob > chi2	0.2669

Both of these tests calculate a summary measure of the level of differences between the probabilities the model estimates and the observed values. The main difference between the two tests is that the Hosmer-Lemeshow test groups

[71] Stata also reports the log-likelihood statistic and the pseudo-R2 statistic which show the level of unexplained variation in the data. However, these are useful to compare two model specifications rather than to assess the goodness of fit of a particular model.

the observations depending on the estimated probabilities before calculating the summary statistic. The high p-values (0.7095 and 0.2669) indicate that the model fits well.

Appendix 3.5 – Balance between treated and untreated groups

The table below shows the statistics describing the balance between treated and untreated observations in this matched sample.[72]

Table 3A.5: Balance between treated and untreated groups

	Unmatched sample					Matched sample				
	Mean search	Mean no search	% bias	t	p-value	Mean search	Mean no search	% bias	t	p-value
Reasons for taking out a credit card										
Decided to take out a credit card to use it for online purchases safely	0.22	0.19	7.3	2.42	0.02	0.19	0.19	0.9	0.23	0.82
Decided to take out a credit card to build/improve credit history	0.16	0.15	4.2	1.39	0.16	0.16	0.15	4.2	1.04	0.29
Decided to take out a credit card because on a previous card T&Cs were changed	0.05	0.02	15.8	5.15	0.00	0.02	0.02	0.1	-0.01	0.99
Relationship with the chosen provider										
Had a credit card with the chosen company before	0.09	0.08	3.0	1.00	0.32	0.08	0.08	1.1	0.27	0.79
Had some other relationship with the chosen company	0.12	0.15	11.1	-3.67	0.00	0.13	0.11	4.3	1.06	0.29
Demographics										
Age	42.8	48.8	39.6	-13.2	0.00	45.3	44.8	3.2	0.79	0.43
Age squared	2035	2644	41.6	-13.8	0.00	2272	2228	3.1	0.76	0.45

As shown in Table 3A.5, the means of the variables in the two groups are reasonably close to each other. The third column shows the absolute bias between the two groups, which takes into account not only the mean but also the variance of variables (see Rosenbaum and Rubin, 1985). If the absolute bias is below 5%-25%, the variable is considered to be sufficiently balanced. For example, Caliendo and Kopeinig (2005) suggest using 5%, Austin (2011) 10% and Stuart (2010) 25%. The last two columns show the t-test of the means of the variables being the same in the two groups – for none of the variables this null hypothesis can be rejected at 5% significance level.

For categorical variables with more than two categories (property ownership, marital and employment status) I use the Pearson chi square test to check whether they are associated with search in the matched sample. The null hypothesis that the distribution of these variables is the same in the search and

[72] Note that the variables included in Table 3A.5 are different from the variables listed in Table 3.1 as I use the variables that affect the outcome, rather than the treatment when estimating the propensity scores (see above in section 3.6.1).

no search groups cannot be rejected for any of them. Finally, I check the distribution of age in the two groups by looking at descriptive statistics and find no imbalance.

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