Experiments on consumer preferences and decision making

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The thesis is submitted for the Ph.D. degree
University of East Anglia
School of Economics
June 30th, 2014

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Acknowledgements

I would like to express my sincere gratitude to all those who made it possible for me to complete this thesis.

I am deeply indebted to my two supervisors, Professor Daniel John Zizzo and Professor Robert Sugden, for welcoming me to the UK, teaching me to use English, encouraging me to pursue a Ph.D. study at the University of East Anglia, recording video greetings for my wedding ceremony, for their invaluable support throughout the work, for their countless break of dawn supervision meetings and midnight working e-mails, for their suggestions, comments and inspiration over the past four years and, last but not least, for introducing me to the amazing world of experimental economics. I must say that being your student is one of the most unforgettable experiences in my life. I will forever be proud to be your student.

I would like to say thank you to Stefania, David, Emanuela, Kei, Fabio, Frederick, Zoe, Bing, Ailko, Meleney Anders, Abhijit, Subhasish, Joo young, Ted, Chris, Lingbo, Lu, Axel, Gerardo and Peter for their valuable help and suggestions on my experiments and data analysis.

I also wish to thank Yiding, Dalu, Meilan, Jack, Bahar, Abdul, Fuyu, Mengjie, Lian, Liang, Chris, Mike, Han, Rich, Yun, Xin, Saeed, Ann, Tim, Steffie and all people who share lives with me over the past few years. I wish you all the best.

I must say thank you to my parents and to Yu, the prettiest angel and my beloved wife. This thesis could not have been completed without your constant support, encouragement and caring. Thanks for loving me, believing in me and giving all what you have to help me pursue my dream. Love you forever.

Abstract

Consumers are not as rational as assumed by conventional economic theories. The present thesis reports three studies of consumers' bounded rationality. It has three chapters. In Chapter 1, I investigate the effects of a range of different types of anchor on WTP and WTA valuations of familiar consumer products, elicited through individuals' buying or selling decisions at given prices. I find anchoring effects only when the anchor value is framed as a plausible price for the good for which the individual is a potential buyer or seller. Anchoring effects are stronger for WTA than for WTP. I conclude that anchoring effects can affect market behaviour, but that not all anchors are equally effective. In Chapter 2, I demonstrate a set of three experiments and find that consumers are likely to stick to defaults and achieve suboptimal outcomes. I unpack two key psychological reasons why they do this - complexity (in terms of non-linearity, number and bundling of tariffs) and consumer inattention. The complexity induced by product bundling, non-linearity and number of tariffs has an important role, but this is overstated if the explanatory power of inattention is neglected. I show that a 'smart nudge' policy of automatically switching default tariffs can be used to exploit inattention-based consumer inertia to achieve better consumer outcomes. In Chapter 3, I report an experiment in which participants faced purchasing decisions involving complexity and common standards. The majority of participants employed the "dominance editing" (DE) heuristic. However, for cognitively constrained participants, the DE heuristic is less efficient than an alternative shortlisting heuristic - the "largest common standard" (LCS).

Introduction

Perfect rationality is one of the essential assumptions employed in conventional economic theories. In consumer theory, perfectly rational consumers are commonly endowed with features such as stable preferences and unlimited cognitive abilities. Perfectly rational consumers value product variety and can respond optimally and maximise their utilities even when facing complex decision problems. In a competitive market, rational consumers are able to reap benefits from competition and will never be lazy or misjudge prices. Firms can never harm outcomes for consumers by deliberately making product comparisons and price search harder.

However, the perfectly rational consumers described in economic theories are not always like the real consumers in our daily life. In the real world, we can observe that consumers queuing for Christmas discounts may buy products they will never use; consumers purchasing the "3 for 2" food deal may end up putting the redundant food into waste bins; consumers who care about changes in the price of potatoes may not try to save money by switching their suboptimal default energy tariffs.

There is now a substantial body of evidence showing that consumers are not as rational as assumed by conventional economic theories. Consumers' bounded rationality is widely discussed by a large number of psychologists and experimental/behavioural economists empirically and theoretically. There is evidence that in some environments individuals' preferences and economic behaviour systemically violate the assumption of rationality (Kahneman & Tversky, 1979; Tversky & Kahneman, 1974). For example, consumer preferences are unstable and context dependent (Tversky & Simonson, 1993; Koszegi & Rabin, 2006; Loomes et al., 2003; Tufano, 2010; Isoni et al., 2011; OFT, 2010) ; consumers' willingness to pay (WTP) and willingness to accept (WTA) may be affected by non-informative numerical cues (Tversky & Kahneman, 1974; Mussweiler, 2002; Ariely et al., 2003); individuals' willingness to participate in a market and satisfaction with the chosen options may decrease when the choice set is too large (Iyengar & Lepper 2000; Boatwright & Nunes 2001); some consumers are cognitively constrained and respond suboptimally when facing complex options (Jamasb & Pollitt, 2005; OFT, 2008; DG Sanco, 2010); and consumers use simplifying heuristics to deal with complex decision problems (Tversky & Kahneman, 1974; Hauser et al., 2010; Mazini & Mariotti, 2007; Gaudeul & Sugden, 2012).

These findings raise questions about the workings of retail markets that need to be considered both by researchers and by policy makers – for example, how one can identify retail markets in which consumers are subject to bounded rationality; whether boundedly rational consumers' behaviour can affect both themselves and rational consumers; and what can be done to help these consumers. Some orthodox economists believe that, excluding cases of negative externalities and market power, governmental intervention would only dull the efficiency of the market, because under the control of the "invisible hand", rational consumers could always respond optimally in terms of maximising their material payoffs. However, many behavioural and experimental economists hold a different opinion: in the presence of behaviourally biased consumers, markets cannot always be expected to self-correct (OFT, 2011). Regulators are beginning to take account of the existence of bounded rationality in policy making (OFT, 2010, Ofgem, 2011, 2012).

OFT (2011) classified consumers' bounded rationality into six categories: willingness to pay and reference point effects; willingness to pay and misperception of future demand quantities; search and inertia; search and misjudgement of prices; quality and misperception of desired product attributes and quality; and misjudgement of vertical quality. They state that although intensifying competition through increasing the number of firms may improve outcomes for some consumers, it is not a panacea. When cognitively constrained or lazy consumers misjudge the quality of goods, misjudge prices, or display inertia, increasing the number of firms may not help consumers; rather, in some situations it harms them (Carlin, 2009; Spiegler, 2006). At the same time, without governmental intervention, firms may have little incentive to educate boundedly rational consumers. On the contrary, profit-maximising firms may have an incentive to exploit consumers' bounded rationality (Gabaix & Laibson, 2006).

The present thesis contains three chapters and sheds light on the following questions:

1. How can consumers' bounded rationality affect their economic behaviour?
2. How far is the behaviour of boundedly rational consumers suboptimal, relative to that of perfectly rational consumers?
3. Which heuristics do consumers employ to make economic decisions in specific complex environments?
4. Are these heuristics optimal for consumers, given the consumers' cognitive constrains?

In answering these questions, I shall consider policy implications as appropriate.

The thesis employs an experimental methodology for two reasons: first, trading contexts can be controlled in well-designed lab experiments so, in comparison with field data, experimental data allow me to isolate the effects of particular variables; second, lab experiments are able to capture variables which are hard to observe in the field but which may be crucial for the study of consumers' bounded rationality.

The first chapter of the thesis is based on Sugden, Zheng and Zizzo (2013)[1]. It reports an incentivized experiment which investigates the effects of a variety of different anchors (i.e. non-informative numerical cues), on consumers' purchasing and selling decisions. Previous

[1]The chapter is not the same as the published paper. I have added more text, tables and figures both to the main text and to the appendices, to ensure integration with the hypotheses of this thesis. I have made significant contributions to the paper on which this chapter is based. The original research idea was mine. Under the guidance of my supervisors Robert Sugden and Daniel Zizzo, I was primarily responsible for the experimental design, experimental implementation and data analysis, and I wrote the first draft of the paper.

anchoring studies have found salient and robust anchoring effects in various domains, such as the numerical estimation of general knowledge (Blankenship et al., 2008; Epley & Gliovich, 2001,2005; McElroy & Dowd, 2007; Mussweiler & Englich, 2005; Mussweiler & Strack, 1999, 2001; Strack & Mussweiler, 1997), probability estimates (Plous, 1989; Tversky & Kahneman, 1974), legal judgment (Campman & Bornstein, 1996; Englich & Mussweiler, 2001; Englich & Soder, 2009; Englich et al., 2005, 2006), willingness to pay and willingness to accept (e.g. Ariely et al., 2003; Fudenberg et al., 2012) and forecasting (Critcher & Gilovich, 2008). If anchoring effects occur in markets, profit maximising firms may be able to exploit boundedly rational consumers by manipulating the presentation of information about prices. This kind of manipulation might attenuate the efficiency of retail markets and blunt the effectiveness of policies which rely on consumers' price sensitivity.

While it is useful to ask whether or not anchors have effects on consumers' valuations of goods, this question is too general. Anchors can be classified into different types and the relative strengths of anchoring effects for different types of anchor may be different between consumers' purchasing and selling decisions. Although some researchers (Jacowitz & Kahneman, 1995; Wilson et al., 1996) have explored a limited number of anchor types in ordinary social situations, it is important to investigate, in incentivized settings in which individuals buy or sell familiar consumer goods, the strength of the effects induced by different types of anchor, and whether the effects of given anchor types are equally strong for buying and selling decisions. The results of this investigation are relevant for the design of policies aimed at consumer protection or the maintenance of competition. After all it is possible that consumers may behave more rationally when facing familiar economic decision problems than when responding to the non-economic judgement tasks that psychologists have usually studied.

Anchoring effects are not the only effects which could lead to price insensitivity of consumers. Chapter 2[2] tries to give answers to three real problems that policy makers face today: First, do consumers behave suboptimally in the retail markets involving homogenous goods with complex tariffs used in UK electricity and gas retail markets? Second, if the answer to question one is positive, then why do consumers respond suboptimally? Is it because of complexity or inattention? Third, what can we do to help these consumers? According to conventional economic theories, when a homogeneous good is sold by competing firms and when there are no monetary switching costs, perfectly rational consumers will always purchase the good at the lowest price. However, researchers find that in some retail markets, for example the energy market, even if there is no monetary switching cost, many consumers do not switch service providers even though the tariffs on which they are buying are suboptimal (Jamasb & Pollitt, 2005; OFT, 2008; DG Sanco, 2010; Lunn, 2013). One prevalent explanation as to the suboptimal response is complexity exploitation. Since some consumers are cognitively constrained, spurious complexity may hamper consumers' utility maximising behaviour. Since cognitively constrained consumers cannot find out the optimal tariff in a complex environment, some of them would rather keep the defaults while some others switch to sub-optimal tariffs. Some policy makers believe that firms deliberately use complex tariffs to perplex boundedly rational consumers, and have proposed policies aimed at simplifying tariffs. These policies normally aim at two main dimensions - reducing the number of available tariffs and facilitating comparisons (BBC, 2012; Ofgem, 2011, 2012).

However, given the fact that price ranking services for homogeneous products (such as electricity and gas) are pervasively provided online, it is sensible for me to ask the

[2] Chapter 2 is based on the working paper Sitzia, Zheng and Zizzo (2012). I have made significant contributions to this working paper. These include having the original research idea and working on the experimental design, experimental implementation, data analysis and writing up. I have rewritten several sections of the working paper to insure integration with the hypotheses of this thesis.

following question: Is complexity exploitation the only reason why consumers respond suboptimally in retail markets involving spurious complexity and default tariffs? If tariffs can be simply ranked online according to one's consumption using a search engine, spurious complexity would not be a problem to those who are able to use the price ranking services. In this case, firms are not able to employ complex tariffs to exploit these consumers. In fact, there might be another potential reason why consumers keep suboptimal defaults - inattention. Inattention implies that the real world consumers may simply not pay attention to saving money from switching service supplier: their routine activities in their everyday life are more prominent. There is not a point in time in the day, the week, the month or even the year when, as a routine, subjects are required to pay attention to the task of choosing a service supplier, as there is anyway a default service supplier. Chapter 2 presents an experiment based on the UK energy market and experimentally tests these two potential explanations – complexity and inattention - on consumers' default keeping and suboptimal switching behaviour. This chapter also considers the effectiveness of two nudges – a warning nudge and a best default nudge – which may potentially make consumers' behaviour optimal. This is obviously relevant for policy.

Although many policy makers now treat consumers' bounded rationality as one important element when proposing policies, it is still unclear whether or not consumers can be fully protected by these policies and if they can, how and why these policies work and whether or not better policies can be proposed. To answer these questions, it is essential for me to know how consumers make decisions in an environment involving complexity that might be spurious. Previous studies show that people use heuristics to simplify complex decision problems (Tversky & Kahneman, 1974; Hauser et al., 2010). Gaudeul and Sugden (2012) introduced two psychologically grounded heuristics - "Largest common standard" and "Dominance editing" which may be potentially employed by consumers to tackle complex decision problems such as choosing the cheapest tariff among a large set of complex tariffs.[3] Consumers who use the largest common standard heuristic only look at and choose from among tariffs which are easily comparable, whereas consumers who use the dominance editing heuristic first find the cheapest tariff from among those that are easily comparable and then compare this with the other tariffs. If consumers employ the largest common standard heuristic, even if there is no regulatory intervention from the government, in the long run profit maximising retail firms will be unwilling to offer tariffs which are difficult to compare with those of their competitors. However, if consumers employ the dominance editing heuristic, firms have an incentive to offer tariffs which are not easily comparable with their competitors'. The two heuristics have opposite implications about firms' behaviour based on which, researchers would present different advice about consumer protection policy.

However, Gaudeul and Sugden (2012) did not test empirically which heuristic consumers would employ to deal with complex decision problems. Chapter 3 sheds light on this question. Moreover, it is important for me to know whether or not the heuristic employed by the majority of consumers is efficient; if not, are we able to nudge these consumers to employ other, more efficient heuristics? Knowing the answer to these questions may help policy makers propose better consumer protection policies and help with our understanding of consumers' bounded rationality.

[3] They also introduced a psychologically ungrounded heuristic - the signal first (SF) heuristic. More details can be found in Chapter 3.

Chapter 1

1.1 Introduction

There is now a substantial body of experimental evidence supporting the hypothesis that individuals' reported valuations of goods can be affected by *anchors* – that is, non-informative numerical cues (e.g., Ariely et al., 2003; Mazar et al., 2010; Tufano, 2010; Alevy et al., 2011; Fudenberg et al., 2012). In a typical experiment, each subject is first asked whether she would buy (or sell) a specific good at a stated price that is clearly arbitrary, and then is asked to state her maximum willingness-to-pay (WTP) or minimum willingness-to-accept (WTA) for that good; the usual finding is that valuations are positively correlated with the arbitrary 'anchor' price. Taken at face value, these findings may have important implications for the efficiency of retail markets, for two reasons. First, if individuals' purchasing decisions can be influenced by irrelevant anchors, firms may be able to use related mechanisms to manipulate those decisions to the detriment of consumers. Second, many policies aimed at ensuring the competitiveness of retail markets rely on consumers' ability to find the lowest prices; the existence of anchoring effects raises doubts about the effectiveness of this mechanism.

However, most of the evidence of anchoring effects on economic valuations has been derived from a narrow class of experimental designs which may not be representative of real-world interactions between firms and consumers. With a few exceptions, these experiments have investigated only one type of anchor, and this type may not be the best model of the opportunities for manipulation that are open to firms. Theory and evidence from psychology suggest that anchoring effects – and hence the scope for failures of price competition – might be much more general than those on which economists have focused. Furthermore, most experiments have used an ‘open-ended’ method of eliciting valuations which is not typical of retail markets and which may be particularly susceptible to anchoring effects.

In this chapter I report an experiment which investigates the effects of a range of different types of anchor on WTP and WTA valuations, elicited through individuals’ buying or selling decisions at given prices. A further feature of my experiment is that it allows me to compare the strength of anchoring effects on buyers and sellers. Since consumers act as buyers in most retail markets, differences between the susceptibility of buyers and sellers to anchoring effects are relevant in assessing the impact of these effects on competition and consumer welfare. To date, there have been few such direct comparisons, and these have generated conflicting results.

Section 2 reviews the existing evidence of anchoring effects on economic valuations, drawing attention to some of its limitations. Section 3 identifies four dimensions on which anchors can vary, and discusses theoretical reasons for expecting variation along these dimensions to affect the strength of anchoring effects. Section 4 describes the experimental design I use to investigate these forms of variation. My results are presented in Section 5. Their implications are discussed in Section 6.

1.2 Anchoring effects for valuations: existing evidence

The hypothesis that judgements can be subject to anchoring effects was proposed by Slovic and Lichtenstein (1968) as an explanation of ‘preference reversal’ between choices and relative valuations. It was later used by Tversky and Kahneman (1974) in a more general account of heuristics and biases in judgements under uncertainty. (Viewed in a psychological perspective, valuation is a special case of judgement.) The first direct experimental investigation of anchoring effects on valuations of commodities was by Johnson and Schkade (1989), who studied the effects of anchors on certainty-equivalent valuations of lotteries.

That experiment was not incentivized, but in other respects it pioneered what is now the most widely-used experimental design for investigating anchoring effects on valuations.

This *canonical design* has been used in relation to both WTP and WTA; for simplicity, I will describe the WTP version. Each subject first faces an *anchoring task* in which she is asked whether she would buy a specific commodity at a stated price. Usually, this price is fixed by some mechanism that is clearly arbitrary (for example, it is constructed from the digits of the subject's social security number, or set by a random device), but in some experiments the price is simply stated with no explanation of its origin. The subject then faces a *valuation task* which elicits the highest price at which she would buy the same commodity. Usually the elicitation mechanism is *open-ended* (i.e. the subject simply states her highest price), but sometimes it uses *multiple binary choice* (i.e. the subject states whether she would buy at each of a set of alternative prices, and her WTP valuation is inferred from those choices). The valuation task is incentivized, either by the Becker–DeGroot–Marschak (BDM) mechanism or by treating subjects' responses as bids in a Vickrey auction. Usually, but not always, the anchoring task is also incentivized. This design has been used with many different commodities, including standard consumer products, lotteries, sportscards, and unpleasant sounds and tastes (e.g. Ariely et al., 2003; Bateman et al., 2006; Bergman et al., 2010; Mazar et al., 2010; Tufano, 2010; Alevy et al., 2011; Fudenberg et al., 2012). Many but not all implementations of the canonical design have found significant positive relationships between reported valuations and anchor prices (the experiments of Bateman et al., Tufano, and Fudenberg et al. are exceptions).

In a variant design, the anchor is framed as a price expectation. In an experiment reported by Isoni, Brooks, Loomes, and Sugden (2011), the valuation task is incentivized by a median-price Vickrey auction; the anchoring task asks subjects to predict the price that will emerge in this auction, and different questionnaire designs are used to prompt high or low predictions. Mazar et al. (2010) report an experiment in which the anchoring manipulation is to tell subjects the distribution of prices that will be used in the BDM mechanism that incentivizes the valuation task; left-skewed and right-skewed distributions respectively generate low and high price expectations. Both experiments find significant anchoring effects. Anchoring effects induced by the manipulation of price expectations are closely related to *shaping effects* – the tendency for bids and asks in repeated incentive-compatible auctions to be positively correlated with previously-observed prices (Loomes et al., 2003; Tufano, 2010; Isoni et al., 2011).

A few studies have investigated factors which might influence the strength of anchoring effects. It has been found that anchoring effects are weaker for individuals with higher cognitive ability (Bergman et al., 2011) and for individuals with more experience of trading the relevant good (Alevy et al., 2011). Mazar et al. (2010) find that anchoring is stronger when the elicitation method is open-ended than when it uses multiple binary choice. However, there has been little systematic investigation of the relative strength of the effects of different types of anchors on incentivized valuations. Almost all of the existing evidence comes from experiments in which the anchor was a price (or an expectation of a price) for the same commodity that appears in the valuation task, though Ariely et al. (2003, Experiment 5) find that anchor tasks relating to one type of unpleasant noise influence subjects' WTA for experiencing other types. In contrast, psychologists investigating judgement tasks in general have considered many other types of anchor, at least some of which are potentially relevant in economic contexts.

Another under-investigated issue is whether the strength of anchoring effects differs according to whether valuations are elicited from buyers or sellers. Simonson and Drolet (2004, Study 1) report a non-incentivized experiment in which anchoring effects were stronger in buying tasks. Fudenberg et al. (2012) use a design that allows comparisons between buying and selling. In almost all the cases they investigate, anchoring effects are not significant, but the summary statistics suggest (contrary to Simonson and Drolet's findings) that if anchoring does occur, its effects are stronger in selling tasks.

1.3 Anchoring effects for valuations: issues to be investigated

My experiment was designed to investigate, in incentivized valuation tasks, the relative strengths of anchoring effects for different types of anchor, and between buying and selling tasks. I focus on four specific dimensions of anchoring: the *plausibility* of anchor values, the *relevance* of the anchor task to the valuation task, the subject's *engagement* in the anchoring task, and whether the valuation task was one of *buying or selling*. In this Section, I consider theoretical arguments from psychology and economics concerning the effects of variation along these dimensions. I must emphasize, however, that it is not the purpose of my experiment to *discriminate between* the theories I will discuss. With respect to the issues I am investigating, the predictions of those theories often overlap.

1.3.1 Plausibility

It is natural to ask whether anchors are more effective, the more plausible they are as answers to the corresponding judgement tasks. One reason for thinking that this might be the case is provided by the hypothesis that experimental subjects are influenced by the *conversational norms* that apply in ordinary social situations and by the inferences that those norms license (Grice, 1975; Schwarz, 1994). Consciously or unconsciously, subjects may assume that the experimenter would not have presented an anchor unless it was informative, and so treat it as such. Thus, for example, the anchor question 'Would you buy good *A* at price £*x*?' is interpreted as implying that £*x* is a normal or reasonable price for *A*. It seems unlikely that that inference would be made if the supposition of an £*x* price was wholly implausible.

A related mechanism is implied by the hypothesis of *bad-deal aversion* (Thaler, 1985; Isoni, 2011). An individual who is bad-deal averse uses prices as reference points, and derives disutility (respectively: utility) from trading at prices that are less (more) favourable to her than those reference points. Preferences of this kind induce anchoring effects if anchors are treated as reference prices for the good used in the valuation task. One might expect this mechanism to depend on the plausibility of the anchor price.

A different reason for expecting more plausible anchors to have stronger effects is offered by the psychological theory of *selective accessibility* (Mussweiler and Strack, 1999, 2001). This theory proposes that an anchor task activates items of knowledge that are relevant for that task; if immediately afterwards the subject faces a judgement task, those items are particularly accessible and so have a disproportionate effect on her response. A similar hypothesis was previously proposed by Jacowitz and Kahneman (1995) to explain the observation that responses to dichotomous judgement tasks (e.g. 'Is the height of Mount Everest more than 10,000 metres?') are biased by the anchors provided by those tasks (10,000 metres in the example). One apparent implication is that the more obvious the answer to the anchor task is, and so the less need there is to access knowledge in answering it, the less effect the anchor will have on the subsequent valuation task. Thus, implausibly high or implausibly low anchor prices should have relatively weak effects.

1.3.2 Relevance

In the canonical design, the anchoring task requires the subject to consider an arbitrary buying or selling price for the *same* commodity as is featured in the valuation task. An obvious question is whether this condition is necessary for anchoring effects to occur or, more generally, whether the strength of anchoring effects is affected by the relevance of the anchor task to the valuation task.

The explanations of anchoring considered in the previous subsection also provide reasons why anchoring effects should be stronger when anchors are more relevant. If the anchor question is about buying *good A* at a price of £*x* but the valuation task is about WTP for *good B*, conversational norms do not give obvious support to the inference that £*x* is a normal, reasonable or reference price for good *B*. If this inference is not made, bad-deal aversion also does not come into play. The selective accessibility hypothesis implies that the strength of anchoring effects will depend on the degree of overlap between the knowledge requirements of the anchor and valuation tasks. Thus, one might expect anchoring effects to be stronger, the greater the relevance of the anchoring task to the judgement task.

However, some striking evidence suggesting that wholly irrelevant cues can work as anchors comes from the *basic anchoring* effects reported by Wilson et al. (1996). In a typical design, the anchoring task requires student subjects to copy five pages of numbers (framed as a handwriting test); they are then asked to make judgements about the number of fellow-students who will get cancer in the near future. Subjects who have copied larger numbers tend to give larger numbers as judgements. Wilson et al. speculate that this effect may be due to *backward priming*: the need to give a numerical answer to a question triggers a search for possible answers, and numbers in short-term memory, even if unrelated to the task at hand, are then retrieved. A similar hypothesis was earlier proposed by Slovic and Lichtenstein (1968) as an explanation of anchoring effects in preference reversal tasks; Slovic and Lichtenstein's hypothesis is that the money value of the prize in a lottery acts as an anchor for a task which elicits a valuation of that lottery.

1.3.3 Engagement

The selective accessibility and backward priming hypotheses explain anchoring as a side-effect of psychological processes for storing and retrieving items in memory. In the performance of the anchor task, particular pieces of knowledge are accessed, or particular numbers are stored in short-term memory. These items are then selectively retrieved in the valuation task. So, if either of these hypotheses were correct, it would be natural to expect the strength of anchoring effects to depend on the extent of the subject's engagement with the anchor task (Wilson et al., 1996).

For example, if incentivization of a task increases subjects' engagement with it, memories associated with the anchor task might be more retrievable in designs in which that task is incentivized. Another possibility is that the memory of the anchor value might be more retrievable, the more the subject had been involved in the process (however arbitrary) in which that value was determined. For example, a subject has more mental engagement with an anchor value that she is required to construct from certain digits of her social security number than with one that is simply stated by the experimenter.

1.3.4 Buying or selling

The possibility of anchoring effects seems to depend on some degree of imprecision in subjects' 'true' valuations. One might therefore conjecture that anchoring effects would be weaker, the more experience subjects had had in making judgements similar to those elicited in the valuation task. Most people have much more experience of buying low-value consumer goods, such as those used in my experiment, than of selling them. Thus, a typical subject comes to the laboratory with a firmer sense of how to respond to given prices when acting as a buyer than when acting as a seller; as a result, WTP might be subject to less imprecision than is WTA, and less susceptible to the effects of arbitrary cues.

The results reported by Simonson and Drolet (2004, Study 1), referred to in Section 2 above, may seem to provide evidence against this conjecture. However, the framing of Simonson and Drolet's non-incentivized WTA task asked subjects 'to assume that they had received new products as gifts and had decided to sell them'. Since this frame suggests that the products are unwanted and that the subject has already decided to dispose of them, it seems unlikely to induce the sense of endowment typical of incentivized WTA tasks (which may explain the very unusual result that WTA was *less than* WTP). If the subject can assume that the products are unwanted, she has no need to consider what they are 'really' worth to her, and so the problem of preference imprecision does not arise.

1.4 Experimental Design

1.4.1 Overview

The experiment had separate buying and selling treatments, faced by different subjects. The buying treatment elicited WTP valuations for a range of consumption goods and lotteries (the *trading commodities*); the selling treatment elicited WTA valuations for the same commodities. In each treatment, each subject faced eleven tasks in random order, presented on a computer screen. Ten of these tasks had the two-part structure of the canonical design. The first part of such a task was a question that was framed to provide a potential anchor value. Different tasks used different types of anchor, differentiated in terms of plausibility, relevance and engagement. The eleventh task, used as a control, differed from the others in that its first part was a 'filler' question with no anchoring significance. The second part of each task elicited the relevant valuation.

The ten non-control tasks faced by any given subject can be grouped into five pairs. (This pairing was not described explicitly to subjects; because of randomization, the two tasks in a pair were usually not adjacent to one another.) In any given pair, the two tasks were identical except that one task provided a relatively low anchor value while the other provided a relatively high one. Thus, my design allows within-subject tests of the existence and size of anchoring effects, in both buying and selling, for each anchor type.

Because I wanted to investigate more than five anchor types but did not want to overload subjects or make the experiment last too long, the subjects in each treatment (i.e.

buying and selling) were randomly divided into two groups, A and B. These groups faced different (but overlapping) sets of tasks, involving nine anchor types in all (in addition to the control task). To minimize learning and across-tasks effects, and to ensure that my results were not dependent on the use of any specific commodity, I used six different trading commodities, each with a market value of approximately £5. Each subject's eleven tasks involved all six commodities (one for each pair of non-control tasks and one for the control task.) To ensure that effects due to differences between anchor types were not confounded with effects due to differences between commodities, anchor types and commodities were counterbalanced.

Because each subject faced eleven tasks rather than just one, I was able to collect a rich body of data and to use within-subject tests. The downside of this design strategy is that the anchor used in one task might influence the valuations reported by subjects in later tasks. If such contamination were to occur, it would add noise to the data. However, because the order of tasks was randomized, it would not impart systematic biases to my tests.

1.4.2 Anchor types

The first part of each non-control task was a *comparative question* relating to an *anchor commodity*. Depending on the anchor type, this might or might not be the same as the trading commodity. Using A to denote the anchor commodity and £x to denote an amount of money, the comparative question took the form 'If you had A, would you sell your A if we offered you £x?' (in the selling mode) or 'If you had £12, would you buy A if you had to pay £x?' (in the buying mode). Thus, the subject was prompted to focus on the *anchor value* x. In the control task, the comparative question 'Do you like dogs more than cats?' was used as a filler. I will say that the control task had the *no lab anchor* type. (I use this term to signal that the 'anchor value' provided by the experiment is not the only value that a subject might retrieve from memory when reporting her valuation of the trading commodity.)

Anchor type	Process to set anchor value	Relation of anchor commodity to trading commodity	Low anchor value	High anchor value	Incentives	Subject groups facing task
Baseline	None	Same	£1–£2	£10–£12	No	A and B
Implausible price	None	Same	£0.01	£1000	No	A
Similar good	None	Similar	£1–£2	£10–£12	No	A
Dissimilar good	None	Dissimilar	£1–£2	£10–£12	No	B
Incentivized	None	Same	£1–£2	£10–£12	Yes	B
Passive number search	Computer finds number	Same	£1–£2	£10–£12	No	A
Passive price search	Computer finds price	Same	£1–£2	£10–£12	No	B
Active number search	Subject finds number	Same	£1–£2	£10–£12	No	B
Active price search	Subject finds price	Same	£1–£2	£10–£12	No	A
No lab anchor	N/A	N/A	N/A	N/A	N/A	A and B

Table 1.1: Anchor types

The anchor types used in the experiment, and the subject groups to which they were assigned, are described in Table 1.1. The first row of this table describes the *baseline* anchor type, which is similar to that of the canonical design. When this anchor type was used, the subject's first exposure to the anchor value was when it appeared in the comparative question (indicated by 'none' in the 'process to set anchor value' column). The anchor commodity was the same as the trading commodity. The low anchor value was drawn at random from the interval from £1 to £2; the high anchor value was drawn at random from the interval from £10 to £12. High and low anchor values were both intended to be perceived as plausible prices or valuations. The baseline comparative question was not incentivized. This was for reasons of external validity. Outside the lab, an anchoring manipulation is typically a way of framing a given decision problem (as when a supermarket prices a product at £6.95, with the label 'Special offer! Normal price £9.95'); the frame does not have an incentive structure independent of that problem. The entry in the final column indicates that the baseline anchor type was faced by both subject groups. By making two anchor types common to both groups, I was able to check that the randomization was effective and that the particular assignment of anchor types to groups was not inducing systematic effects. In fact, there was no significant difference in WTA or WTP valuations for the common tasks between the two groups.

The other anchor types differed from the baseline in the following ways.

The *implausible price* anchor type was used to investigate the effect of variation along the dimension of plausibility. In this anchor type, the low anchor value was £0.01 and the high anchor value was £1000. I assumed that such extreme values would not be perceived as providing information about (or reference points for) responses to the trading questions in part 2.

The *similar good* and *dissimilar good* anchor types were used to investigate the effect of variation along the dimension of relevance. In these anchor types, the anchor commodity was not the same as the trading commodity, but was approximately equal in market value. In the *similar good* case, the two goods were chosen so that individuals' 'true' valuations of the commodities were likely to be positively correlated. In the *dissimilar good* case, the two commodities were unrelated to one another (see Section 4.4 below).

The *incentivized* anchor type was used to investigate the effect of one form of engagement. In this anchor type, the comparative question was incentivized in the same way as the trading questions (see Section 4.5 below).

The final four non-control anchor types were used to investigate a different form of engagement – involvement in the determination of the anchor value. In each of these anchor types, the comparative task was preceded by a *matrix problem*, whose solution determined the anchor value. In the two *price search* anchor types, the subject was shown an 8×8 matrix of monetary values, described as 'prices'. These values were determined randomly, subject to the constraint that the lowest value was in the range from £1 to £2 (for low anchor tasks) or from £10 to £12 (for high anchor tasks). In the two *number search* anchor types, the only difference was that the entries in the matrix were dimensionless numbers. (Thus, for example, the 'number' 1.45 was shown instead of the 'price' £1.45.) In the two *active* anchor types, the subject was asked to find the lowest price (or number) in the matrix and to type it into a blank space on the screen. (If the response was incorrect, the subject was prompted to try again.) This (or the corresponding) price then became the anchor value for the comparative question.

The final row of Table 1.1 describes the *no lab anchor* control, which was faced by both subject groups.

1.4.3. The elicitation of WTA and WTP

The second part of each task began with a screen telling the subject 'You are endowed with *T* and you have an opportunity to sell *T*' (in the selling mode) or 'You are endowed with

£12 and you have an opportunity to buy *T*' (in the buying context). Here *T* denotes the name of the trading commodity. The subject was then asked to answer 'yes' or 'no' to each of 25 *trading questions* of the form 'If I am offered £*y* for *T*, I will sell' (in the selling mode) or 'If the price of *T* is £*y*, I will buy' (in the buying mode). The trading questions used 25 different prices: *y* = 0.01, 0.50, 1.00, 1.50, ... 12.00. Thus, a subject's responses to these questions located her WTA or WTP within a £0.50 band (or revealed that valuation to be less than £0.01 or greater than £12.00).[4]

Notice that this design elicits valuations by multiple binary choices rather than by a single open-ended question. I used this elicitation method for three reasons. First, 'Would you pay £x for *T*?' is cognitively simpler than 'What is the highest price you would pay for *T*?' and so less likely to induce confusion. Second, the multiple binary choice format can be linked to the BDM mechanism by telling subjects that one binary choice will be selected at random to be 'real'. This presentation makes the incentive-compatibility of the mechanism more transparent than when valuations are open-ended. Third, most retail transactions take place at take-it-or-leave-it prices; cases (such as sealed-bid auctions) in which consumers record open-ended valuations or bids are much rarer. Thus, in the context of retail markets, the multiple binary choice format has greater external validity.

As I noted in Section 2, there is some evidence that anchoring effects are weaker when valuations are elicited by binary choices. One possible explanation is that the greater transparency of this method allows subjects to be more confident in their responses and so less susceptible to irrelevant cues. An alternative explanation is compatible with the hypothesis of backward priming. Binary choice questions require yes/no answers while

[4] The software was designed so that the subject did not need to click 'yes' or 'no' to every question. For example, in the selling mode, if a subject clicked 'yes' (respectively 'no') to an offer of *x*, 'yes' ('no') was automatically entered for every offer greater than (less than) *x*. Thus only two clicks were needed to answer all 25 questions. This procedure prevented subjects from making inconsistent responses.

open-ended valuation questions require numerical answers. Thus, numbers in memory are more likely to be accessed when subjects are dealing with open-ended questions.

1.4.4 Trading and anchor commodities

Six different trading commodities were used in the experiment: a lottery which all the prizes were positive, which I called a 'win-win gamble'; five National Lottery scratch cards; two bottles of Chinese sauce; a box of chocolates; a bath towel; and a luxury pen. The win-win gamble gave the prizes £15.53, £3.08 and £0.01 with probabilities 0.3, 0.5 and 0.2 respectively; its expected value was £6.20. The other commodities had market prices in the range from £4.50 to £5.10. (Multiple items, such as five scratch cards, are treated as a single commodity.) In the 'similar good' tasks, the corresponding anchor commodities were respectively: a different win-win gamble with approximately the same expected value; five National Lottery scratch cards of a different type; three bottles of Thai sauce; a box of a different type of chocolates produced by the same firm; five face cloths; and a different type of pen. In the 'dissimilar good' tasks, the anchor commodity was an iTune coupon.

1.4.5 Incentives

At the end of the experiment the computer picked one of the eleven tasks at random. If the anchor type of that task was not 'incentivized', the computer then picked one of the 25 trading questions for that task. What the subject took away from the experiment was determined by her response to that task. In a selling task, if the subject had declared her willingness to sell the trading commodity at the £*x* price of the relevant trading question, she received £*x*; otherwise, she received the commodity. In a buying task, if the subject had declared her willingness to buy at the £*x* price, she received the commodity and £(12 – *x*); otherwise, she received £12. If the subject received a win-win gamble, it was resolved by the computer, using a random-number generator. If the anchor type of the task picked was ‘incentivized’, the computer then randomly picked either the first or second part of that task. Depending on which part was picked, the subject’s earnings were determined either by her response to the comparative question or by her response to one of the 25 trading questions.

1.5 Results

1.5.1 Summary statistics and aggregated tests

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) Laboratory at the University of East Anglia in Spring 2011. Subjects were recruited using a campus-wide online system. There were 228 subjects, 108 in the selling treatment and 120 in the buying treatment. Most of the subjects were students, from a wide range of academic disciplines and with an age range from 19 to 47. The experiment lasted around 45 minutes with an average payment of £10.73 per person, in addition to commodities that subjects took away from the experiment.

	No lab anchor	Low anchor	High anchor
WTA	5.01 (0.31)	4.91 (0.23)	5.43 (0.25)
WTP	1.62 (0.18)	1.45 (0.11)	1.56 (0.12)

Table 1.2: WTA and WTP means and standard errors
Note: 108 observations for WTA, 120 observations for WTP.
Valuations are in £.
Numbers in brackets are standard errors.

Table 1.2 reports means and standard errors of WTA and WTP, averaging over all anchor types and all commodities, broken down according to whether there was no lab anchor, a low value anchor or a high value anchor.[5] In calculating standard errors, I treat

5 A subject’s WTA is recorded as the highest value of x at which she answered ‘Yes’ to the question asking if she would sell at the price £x. A subject’s WTP is recorded as the lowest value of x at which she answered ‘No’ to the question asking if she would buy at the price £x. If a subject was not willing to sell (was willing to buy)

subjects as the units of observation; for each subject, I observe the mean of WTA or WTP across the relevant tasks. Recall that each subject reported five 'low anchor' and five 'high anchor' valuations, but only one 'no lab anchor' valuation. Thus, there is more noise in the data for 'no lab anchor' data than in those for high or low anchors.

In both selling and buying contexts, high-anchor valuations are greater than low-anchor valuations. The relative (and still more, the absolute) magnitude of the anchoring effect is greater in the selling context, where high-anchor valuations are 11 per cent greater than low-anchor valuations, than in the buying context, where the corresponding measure is 7 per cent. To test for the significance of these differences, I use Wilcoxon signed-rank tests, applied to 'observations' as defined above. (Throughout the chapter, all within-subject tests are of this type. For between-subject comparisons I use Mann-Whitney tests.) In both cases, the anchoring effect is significant (z = –5.28, p < 0.001 for WTA, z = –2.02, p = 0.04 for WTP).

In the selling context, mean low-anchor and high-anchor WTA (£4.91 and £5.43 respectively) are well above the range of low anchor values (the highest of which was £2) and well below the range of high anchor values (the lowest of which was £10). The implication is that, in general, WTA was pulled up by high anchors and/or pulled down by low anchors. This is consistent with the observation that the mean of 'no lab anchor' WTA (£5.01) lies between the high- and low-anchor means. 'No lab anchor' WTA valuations are significantly less than high-anchor valuations (z = –2.13, p = 0.03), but not significantly different from low-anchor ones (z = –0.35, p = 0.73). It should be borne in mind that my Wilcoxon tests are less powerful when comparisons involve 'no lab anchor' valuations, because of the greater noise in those data. Nevertheless, my findings give some indication that the tendency for

at the highest price of £12, her WTA (WTP) is recorded as £12.50. In fact, there were only 7 (out of a possible 1188) observations of WTA greater than £12.00, and no observations of WTP greater than £12.00. If a subject was not willing to sell (was willing to buy) at the lowest price of £0.01, her WTA (WTP) is recorded as £0.00. Notice that, because of these conventions, our measures of WTA and WTP are not directly comparable. (For example, a subject whose minimum selling price is £9.75 is recorded as having a WTA of £10, while a subject whose maximum buying price is £9.75 is recorded as having a WTP of £9.50.) However, our focus is on the differential effects of low and high anchor values, holding the valuation mode constant.

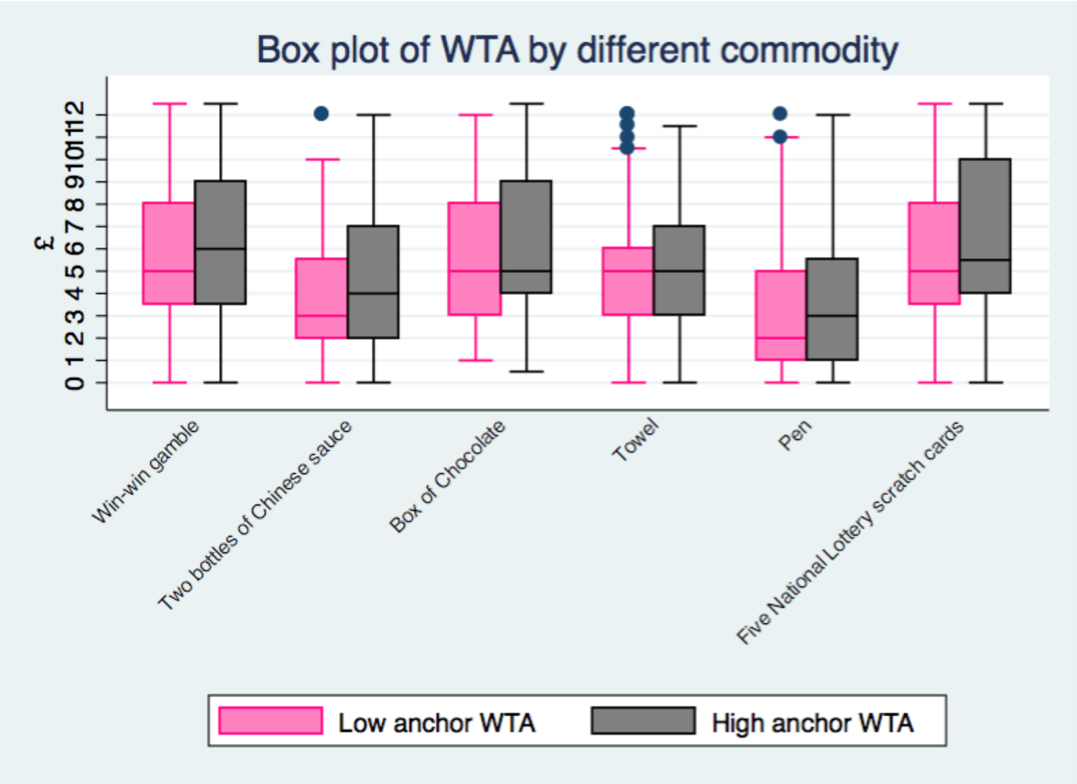
WTA valuations to be pulled up by high anchors may be stronger than the tendency for them to be pulled down by low ones – an asymmetry that has also been observed for shaping effects, and that is consistent with bad-deal aversion (Isoni et al., 2011).

In the buying context, mean low-anchor and high-anchor WTP (£1.45 and £1.56) both lie within the range of low anchor values, implying that there was little scope for WTP to be pulled down by low anchors, and hence that the observed anchoring effect was primarily due to the effect of high anchors. 'No lab anchor' WTP valuations are not significantly different either from high-anchor valuations (z = –0.48, p = 0.64) or from low-anchor ones (z = –0.007, p = 0.995) In the light of these high p-values, the apparently surprising observation that 'no lab anchor' valuations have a higher mean (£1.62) than high-anchor valuations may reasonably be attributed to sampling error.

Commodity	WTA		WTP	
	Low anchor (90 observations)	High anchor (90 observations)	Low anchor (100 observations)	High anchor (100 observations)
Win-win gamble	5.91 (0.31)	6.30 (0.34)	2.15 (0.22)	2.22 (0.21)
Two bottles of Chinese sauce	3.82 (0.30)	4.51 (0.34)	0.82 (0.11)	0.98 (0.12)
Box of chocolate	5.56 (0.31)	6.19 (0.32)	1.58 (0.14)	1.76 (0.16)
Towel	4.84 (0.30)	5.24 (0.31)	1.57 (0.19)	1.61 (0.20)
Pen	3.37 (0.31)	3.95 (0.35)	0.54 (0.08)	0.58 (0.10)
Five National Lottery scratch cards	5.96 (0.34)	6.41 (0.35)	2.04 (0.21)	2.19 (0.23)

Table 1.3: WTA and WTP means and standard errors by commodity

Note: Numbers in brackets are standard errors.



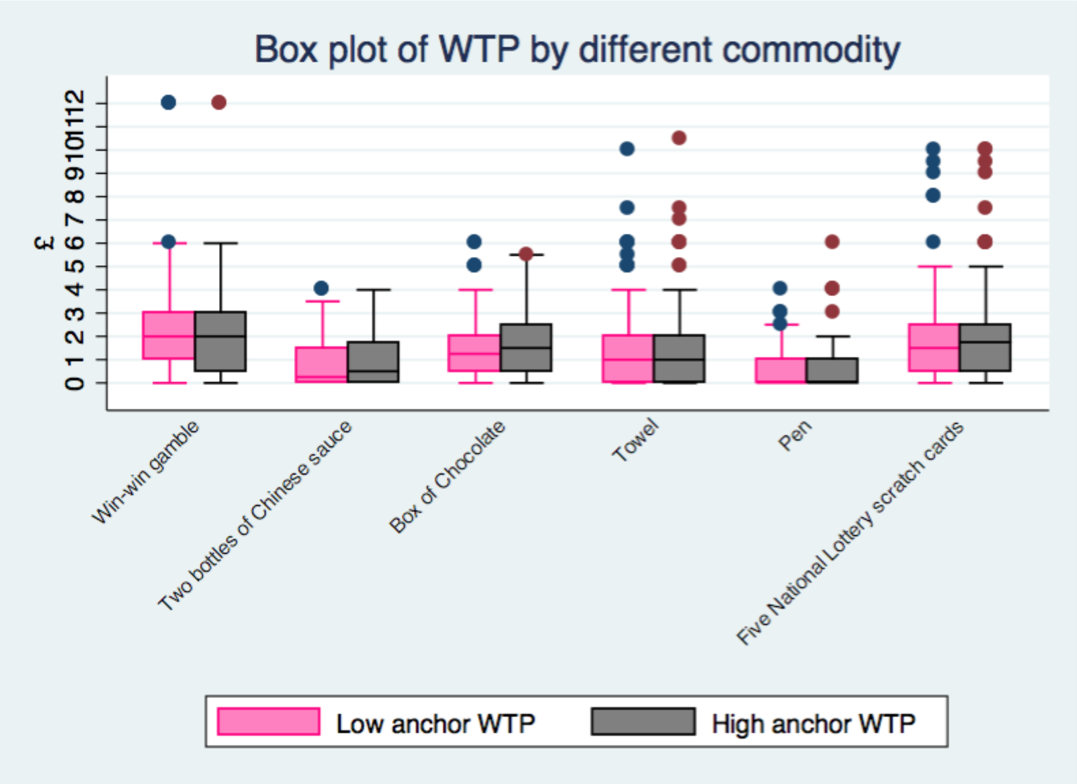


Figure 1.1 Willingness to accept (WTA) and to pay (WTP) for different commodity

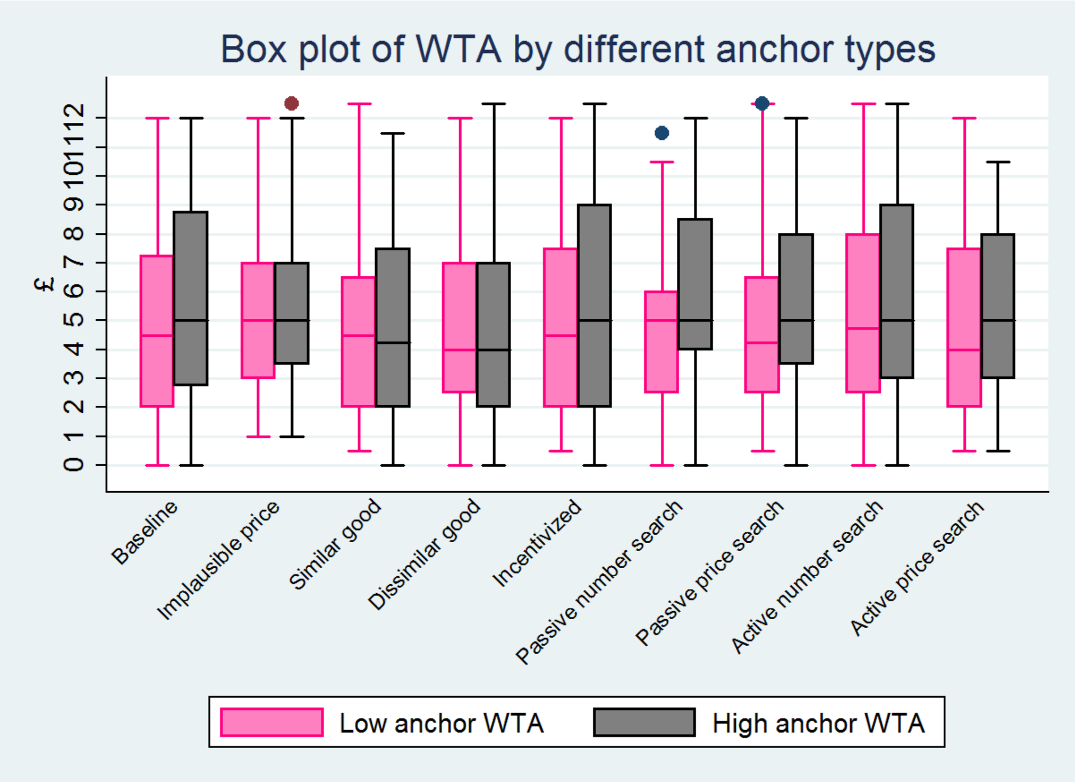
Table 1.3 reports mean WTA and WTP valuations for high and low anchors, broken down by commodity but aggregated across anchor types. (I do not report 'no lab anchor' valuations at this level of disaggregation because sample sizes are small. For each commodity, there are only 18 observations of 'no lab anchor' WTA and only 20 of 'no lab anchor' WTP.)

Standard errors are calculated using subjects as units of observation. For each commodity and for each valuation mode, the high-anchor mean is greater than the low-anchor mean, suggesting that anchoring effects are robust across different commodities.

	WTA				WTP			
Anchor type	Low anchor		High anchor		Low anchor		High anchor	
	Group A	Group B	Group A	Group B	Group A	Group B	Group A	Group B
Baseline (WTA 108 observations; WTP 120 observations)	4.61 (0.45)	4.98 (0.41)	5.42 (0.50)	5.58 (0.44)	1.64 (0.22)	1.34 (0.23)	1.68 (0.23)	1.54 (0.25)
Implausible price (WTA 54 observations; WTP 60 observations)	5.29 (0.40)		5.55 (0.43)		1.48 (0.28)		1.58 (0.29)	
Similar good (WTA 54 observations; WTP 60 observations)	4.74 (0.41)		4.94 (0.43)		1.50 (0.25)		1.56 (0.22)	
Dissimilar good (WTA 54 observations; WTP 60 observations)		4.81 (0.45)		4.76 (0.48)		1.14 (0.18)		1.27 (0.20)
Incentivized (WTA 54 observations; WTP 60 observations)		4.86 (0.46)		5.51 (0.49)		1.39 (0.17)		1.57 (0.20)
Passive number search (WTA 54 observations; WTP 60 observations)	4.78 (0.38)		5.67 (0.42)		1.43 (0.24)		1.54 (0.26)	
Passive price search (WTA 54 observations; WTP 60 observations)		4.94 (0.42)		5.48 (0.44)		1.21 (0.16)		1.44 (0.22)
Active number search (WTA 54 observations; WTP 60 observations)		5.26 (0.46)		5.93 (0.47)		1.90 (0.34)		1.81 (0.30)
Active price search (WTA 54 observations; WTP 60 observations)	4.83 (0.41)		5.48 (0.40)		1.47 (0.20)		1.57 (0.22)	

Table 1.4: WTA and WTP means and standard errors by anchor type

Note: Numbers in brackets are standard errors.



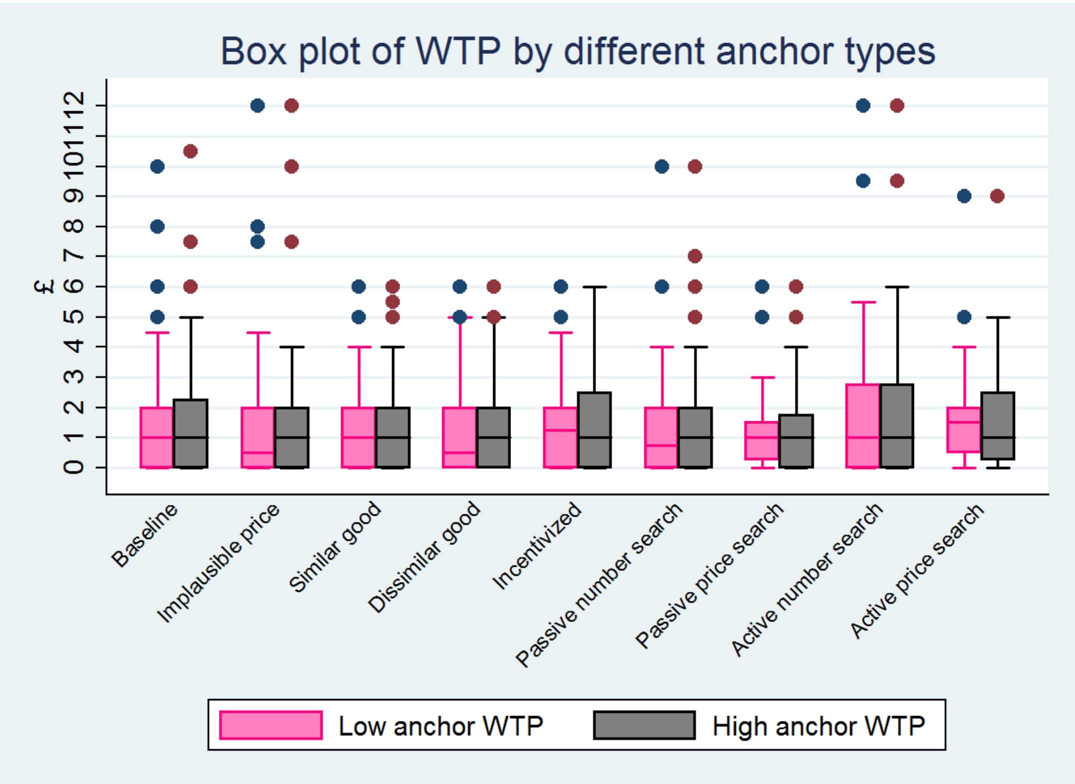


Figure 1.2 Willingness to accept (WTA) and to pay (WTP) for different anchor types

Table 1.4 reports mean WTA and WTP valuations for high and low anchors, broken down by anchor type but aggregated across commodities. Again, standard errors are calculated at the subject level. Figure 1.1 shows box plots of WTA and WTP, using the same disaggregations. In the selling context, mean high-anchor WTA is higher than mean low-anchor WTA for eight of the nine anchor types (the exception being 'dissimilar good'). In the buying context, mean high-anchor WTP is higher than mean low-anchor WTP for eight of the nine anchor types (the exception being 'active number search').

Unsurprisingly, WTA is consistently higher than WTP. Averaging over all commodities, all anchor types and all anchor values, WTA is £5.16 and WTP is £1.51. In interpreting these data, I must allow for the fact that £0.50 of the difference between my WTA and WTP measures is attributable to my elicitation procedure and accounting conventions (see footnote 2). However, after the measures have been made comparable by subtracting £0.25 from WTA and adding £0.25 to WTP, the ratio of WTA to WTP is 2.8, and the difference between the two is strongly significant (z = 10.30, p < 0.001). This finding agrees with many previous findings of WTA–WTP disparities (e.g. Coursey, Hovis, & Schulze, 1987; Horowitz and McConnell, 2002).

1.5.2 Regression analysis

Table 1.5 reports how valuations are affected by each commodity when regressions are run to verify their effects.[6] In each regression equation, the commodity is fixed and the dependent variable is either WTA or WTP. Each of the reported coefficients is the key coefficient for an 'anchor value' dummy variable (equal to 1 when the anchor value is high and 0 when it is low) in a regression that also contained a period variable (between 1 and 11) to pick up any trends in responses over the course of the experiment.

[6] The appendix C contains the full regression results.

Commodity	OLS with error clustering		Random effects	
	WTA	WTP	WTA	WTP
Win-win gamble	0.384	0.099	0.392	0.096
(WTA 180 observations; WTP 200 observations)	(.239)	(.117)	(.238)	(.121)
Two bottles of Chinese sauce	0.596***	0.163***	0.625***	0.161***
(WTA 180 observations; WTP 200 observations)	(.206)	(.055)	(.205)	(.054)
Box of chocolate	0.607***	0.157*	0.663***	0.141
(WTA 180 observations; WTP 200 observations)	(.190)	(.084)	(.171)	(.087)
Towel	0.402**	0.037	0.400**	0.044
(WTA 180 observations; WTP 200 observations)	(.161)	(.071)	(.159)	(.070)
Pen	0.494**	0.045	0.563***	0.044
(WTA 180 observations; WTP 200 observations)	(.216)	(.054)	(.196)	(.054)
Five National Lottery scratch cards	0.445*	0.170*	0.434*	0.158*
(WTA 180 observations; WTP 200 observations)	(.262)	(.092)	(.260)	(.083)

Table 1.5: Commodity Coefficients in Regressions on WTA and WTP

Note: Each coefficient is the 'anchor type' dummy coefficient in a regression controlling also for period. Each dummy is equal to 1 when there is a high anchor, 0 with a low anchor. Numbers in brackets are standard errors. Clusters or random effects control for non-independence of observations at the subject level. ***** = p<0.1, ** = p<0.05 and *** = p<0.01.

The results reported in the first two columns of Table 1.5 control for the potential non-independence of observations at the subject level by employing OLS regressions with error clustering. The results of using an alternative method of control, that of random effects regressions, are reported in the third and fourth columns.[7] The results of the two sets of regressions are very similar. In the rest of this section, I will refer to the OLS results.

In the selling context, participants were affected by anchoring effects in most commodities. There is no evidence showing that subjects' WTA can be affected by anchoring when the trading commodity is a Win-win gamble. However, in the buying context, the anchoring effects are relatively weaker. That said, I still have strong evidence that participants can be affected by anchoring effects in relation to two bottles of Chinese sauce,

[7] As a further robustness check, to allow for the truncation of reported valuations at £12, we also used Tobit regressions with cluster with the upper bound £12. Unsurprisingly, given the very small number of valuations above this bound (see footnote 2), the results of these regressions are in line with the others.

and mild evidence that they can be affected by anchoring effects in relation to five national lottery cards.

Result 1: *Anchors are less effective in the buying context than in the selling context. In the selling context, participants' WTA can be affected by anchoring effects in relation to most goods. In the buying context, anchors have different anchoring effects on participants WTP when trading commodities are different.*

Table 1.6 reports how valuations are affected by each anchor type when regressions are run to verify their effects.[8] In each regression equation, the commodity is fixed. The dependent variables and independent variables are the same as the regression equations used in Table 1.5, except that I also controlled for the commodity type. There was no significant trend in any of the WTA regressions. In a few of the WTP regressions, there was a significant downward trend in valuations. I tried adding a variable to pick up interaction between 'anchor value' and 'period', but this was not significant for either WTA or WTP.

[8] Appendix C contains the full regression results.

Anchor type	OLS with error clustering WTA	WTP	Random effects WTA	WTP
Baseline	0.764***	0.078	0.675***	0.093
(WTA 216 observations; WTP 240 observations)	(.180)	(.067)	(.176)	(.067)
Implausible price	0.284	0.073	0.292	0.090
(WTA 108 observations; WTP 120 observations)	(.283)	(.081)	(.275)	(.081)
Similar good	0.188	0.050	0.231	0.070
(WTA 108 observations; WTP 120 observations)	(.326)	(.103)	(.308)	(.092)
Dissimilar good	-0.048	0.129	-0.048	0.126*
(WTA 108 observations; WTP 120 observations)	(.290)	(.080)	(.283)	(.072)
Incentivized	0.685**	0.163	0.676**	0.188*
(WTA 108 observations; WTP 120 observations)	(.297)	(.113)	(.288)	(.107)
Passive number search	0.933***	0.058	0.916***	0.092
(WTA 108 observations; WTP 120 observations)	(.197)	(.095)	(.179)	(.085)
Passive price search	0.590*	0.253*	0.577*	0.257*
(WTA 108 observations; WTP 120 observations)	(.340)	(.142)	(.341)	(.135)
Active number search	0.711 ***	0.000	0.647***	0.007
(WTA 108 observations; WTP 120 observations)	(.251)	(.185)	(.242)	(.184)
Active price search	0.745**	0.187**	0.721***	0.150**
(WTA 108 observations; WTP 120 observations)	(.290)	(.086)	(.257)	(.075)

Table 1.6: Anchor Type Coefficients in Regressions on WTA and WTP

Note: Each coefficient is the 'anchor type' dummy coefficient in a regression controlling also for period and commodity type. Each dummy is equal to 1 when there is a high anchor, 0 with a low anchor. Numbers in brackets are standard errors. Clusters or random effects control for non-independence of observations at the subject level. ***** = p<0.1, ** = p<0.05 and *** = p<0.01.

In the selling context, there is clear evidence of an anchoring effect for the baseline anchor (t = 4.24, p < 0.001). Using the high anchor value rather than the low one increases WTA by £0.76 (approximately 15 per cent of the overall mean WTA). As explained in Section 4.2, five of the anchor types can be interpreted as adding some additional element of engagement to the baseline task. The anchoring effect is statistically significant at least at the 10 per cent level in all of these cases ('incentivized' t = 2.31, p = 0.03; 'passive number search' t = 4.74, p < 0.001; 'passive price search' t = 1.74, p = 0.09; 'active number search' t = 2.83, p = 0.01; 'active price search' t = 2.57, p = 0.01). However, there is no evidence that engagement systematically increases (or decreases) the size of the anchoring effect relative to the baseline case. There is no significant anchoring effect when anchor values are implausible, or when the anchor commodity is not the same as the trading commodity.

Result 2: In the selling context, anchors are effective in distorting behaviour if and only if the anchor takes the form of a plausible price for the trading commodity.

In the buying context, there is no significant anchoring effect in the baseline case. Given this result, it is unsurprising that there is also no significant effect when anchor values are implausible or when the anchor and trading commodities are different. The only anchor type that is significant at the 5 per cent level is 'active price search' (t = 2.18, p = 0.03). In this case, using the high anchor value rather than the low one increases WTP by £0.19 (approximately 12 per cent of the overall mean WTP). 'Passive price search' is significant at the 10 per cent level (t = 1.79, p = 0.08). One possible interpretation of these results is that there are anchoring effects for WTP when the degree of engagement with anchor values is sufficiently great. An alternative interpretation is that the crucial property of 'price search' is not so much engagement by the subject in the construction of the anchor, as that the anchor value is framed as the lowest of an array of prices. Since 'lowest' is a salient feature of price for buyers, this framing draws attention to the anchor value *as a price*, and so is particularly likely to prime conversational norms and/or bad-deal aversion. But however one interprets the significant coefficient for 'active price search', it is clear that anchoring effects are weaker for WTP than for WTA.

Result 3: *There is some evidence that, in the buying context, anchoring effects occur when subjects are engaged with the construction of the anchor value and when that value is framed as a price of the trading commodity*.

1.6 Discussion and Conclusion

I conclude that anchoring effects *can* affect individual behaviour in incentivized tasks involving familiar consumer goods, but that not all anchors are effective.

For some readers, the most striking feature of my results may be that the anchoring effects I find are relatively small. They are much smaller, for example, than those reported by Ariely et al. (2003); and, as a glance at Figure 1.1 shows, valuation disparities due to anchoring are far smaller than disparities between WTA and WTP. In this respect, however, my results are not outliers: as noted in Section 1, several comparable studies have found anchoring effects to be small or even non-existent. But it should be remembered that my experiment was designed to allow controlled investigations of the *relative* effectiveness of different types of anchor and of the *relative* effectiveness of anchors in selling and buying contexts – not of the absolute effectiveness of anchors in general. In my experiment, each subject faced eleven tasks, each with its own set of trading questions and its own anchor. It would not be surprising if this feature of my design had some tendency to dampen the specific effects of individual anchors. Further, as explained in Section 1.2, my method of eliciting valuations through binary choices is likely to induce weaker anchoring effects than the more usual (but, I have argued, less externally valid) method of open-ended questions. But these considerations do not affect the validity of comparisons across anchor types and across valuation modes.

The sharpest result of my experiment is that anchoring effects are stronger in the context of selling than in the context of buying. This result is consistent with the tentative finding of Fudenberg et al. (2011). One important implication of this result is that experiments that study anchoring effects in selling contexts are likely to overstate the significance of such effects in retail markets.

A second general finding is that anchoring effects are strongest when the anchor value is framed as a plausible price for the good for which the individual is a potential buyer or seller. In an incentivized design investigating trading decisions about familiar consumer products, I found no evidence of the 'basic' anchoring effects that have been observed for various kinds of non-economic judgements, and only weak evidence that the strength of anchoring effects is influenced by the extent of individuals' engagement with the process by which arbitrary anchor values are constructed. This suggests that, in economic contexts, anchoring effects work primarily by suggesting that the (plausible) prices that are presented as anchors are in some way reasonable or normal, despite their arbitrariness. The implication is that consumers' preferences are somewhat less labile than the psychological literature might suggest. Nevertheless, that still leaves plenty of scope for firms to try to manipulate consumers' perceptions of reasonable prices.

Chapter 2

2.1 Introduction

This Chapter presents a set of three experiments trying to identify *whether*, in markets for services, consumers are likely to stick to defaults and achieve suboptimal outcomes, *why* they do this and *what* can be done about it. In order for consumers to reap benefits from competition, they have to be actively engaged in spotting the best deal that is available to them. This is true both in the tautological sense that they are worse off if they go for a suboptimal choice and in the less obvious sense that firms may be under less competitive pressure if they do so (Giulietti et al., 2005). It is a stylized fact however that, in a number of services markets where choice is possible, consumers do not switch service providers even though the tariffs they are holding are suboptimal (Jamasb and Pollitt, 2005; OFT, 2008; DG Sanco, 2010; Lunn, 2011); furthermore, when choices are made, there is a question mark about whether they are necessarily optimal (Joskow, 2008; Wilson and Waddams Price, 2010). Relevant services markets include both ones that have always been in the hands of the private sector, such as bank account, mobile telephony and internet services, and ones that have been opened up to competition in many countries, such as consumer electricity and gas services, fixed telephony and multichannel TV services.[9] Market models that have introduced these and other deterrents to change supplier by postulating switching costs have shown the distortions that this produces for competition, for example in terms of market entry and prices (e.g., Klemperer, 1995; NERA, 2003).

Undoubtedly, financial switching costs can act as a partial deterrent to changing services supplier in some cases. Identifying the role of different kinds of switching costs can be hard with field data, though important attempts have been made with survey data (Wilson

[9] Vulnerable consumers (older, less educated and more disadvantaged consumers) are likely to be especially affected (see DG Sanco, 2009, for some across Europe evidence).

and Waddams Price, 2010) and very little switching, compared to the savings opportunities available, is observed even in markets, such as the U.K. retail electricity and gas markets, where financial switching costs are minimal. Attempts have been made to use survey data to infer non-financial reasons for not switching: the role of complexity in the tariffs employed and in the number of the tariffs employed has been highlighted (e.g., Lambrecht and Skiera, 2006; OFT, 2008; Garrod et al., 2009; DG Sanco, 2009; Lunn, 2011) and driven policy recommendations (e.g., Joskow, 2008; Xavier and Ypsilanti, 2008; Ofgem, 2009, 2011; Independent Commission on Banking, 2011). For example, it has been brought as a good reason for why the drive for liberalization of consumer energy markets has halted in USA (Joskow, 2008) and for envisaging requiring tariffs to be simpler in the UK (Ofgem, 2011). Carlin (2009), Gabaix and Laibson (2006), Spiegler (2006) and Ellison and Ellison (2009) provide models explaining how complexity and confusion inducing strategies may be desirable for firms.

The potential role of inattention in explaining suboptimal consumer outcomes has been mentioned, but is, in comparison, somewhat understated.[10] Yet, I suspect that, as with the inattentive agents of Sims (2003) and Reis (2006), although perhaps not necessarily due to a rational allocation of cognitive effort, real life time constrained consumers may simply not pay attention to tasks regarding the choices of services. Putting it simply, it may not be in their minds in the way in which saving 20 cents at a supermarket buying groceries is.[11] A key contribution of this chapter is to build on this intuition.

[10] Using survey data, Giuletti and Waddams Price (2005) claimed lack of awareness did not play a big role in lack of switching in U.K. energy markets, whereas, using more recent survey data, Wilson and Waddams Price (2010) could not reject that it did, though their evidence is not unequivocal on what did. Oftel (2000) noted inertia/lack of interest and lack of awareness of alternatives as two out of four reasons for not switching supplier in a survey on the telecoms market.

[11] As supermarket shopping becomes increasingly an online shopping experience with default consumer baskets from previous purchases, supermarket shopping might arguably itself become more sensitive to inattention problems. There are a number of other models of economic behaviour incorporating inattention, such as Hong and Stein (1999), Hirshleifer and Teoh (2003), Gabaix (2011) and Woodford (2012); Della Vigna (2009) contains a review of some of the implications. The usual interpretation of inattention is in terms of lack of consideration of some features of a product. Inattention could however be in relation to a whole task.

While survey data are insightful and important as they directly refer to real life choices, when it comes to understand the psychological motivations behind behavioural choices, they obviously suffer from potential limitations[12] such a difficulty to draw clear conclusions because of a range of alternative and undeclared factors,[13] forgetfulness and selectivity in recall,[14] the unconscious nature of many of the choices that people make,[15] and/or the need to self-justify past choices towards those conducting the survey or indeed to engage in self-deception to rationalize possibly suboptimal choices that one has done in the past.[16] A specific problem lies with the fact that, if a significant part of the suboptimality of consumer behaviour is because consumers do not pay attention, drawing attention of survey responders to issues they have not thought themselves of before may not be the best way to identify the extent to which inattention is a problem, as ex post rationalizations may then be unavoidable and survey responses may underestimate the role of inattention. A further problem is that it is difficult to see in most surveys whether, as suggested in the context of the number of binary lotteries and of available US 401(k) retirement plans by Iyengar and Kamenica (2010), and more broadly by Beshears et al. (2008), it is the case that complexity may interact with a status quo bias, in the sense that consumers may be less likely to want to take a decision if faced with a more complex decision problem. More seriously, unless

[12] Different studies are obviously affected by specific limitations to different degrees, depending on how the surveys are devised.

[13] For example, Coombs and Shaharudin (2011) criticize survey studies on the suboptimality of banking services supplier choices because of their inability to control for enough alternative explanations. A key reason is that surveys simply do not control enough for the possibility that, given their preferences, consumers may be getting a good deal. In a contingent valuation study with US survey data on electricity supply, Hartman et al. (1991) find a significant status quo bias in terms of stated valuations.

[14] Li (2013) reviews evidence on selectivity in memory recall and presents an experiment this taking place significantly within just six weeks, if in a different setup.

[15] That there is a split between conscious, explicit knowledge and subconscious, implicit knowledge is a well known stylized fact in psychology (e.g., Shanks and St. John, 1994). Sub-thalamic brain activity takes place when subjects stick with the default, whereas heightened pre-frontal activity takes place when such default is overridden (Fleming et al., 2010), suggesting that, whereas rejecting the default may require a conscious effort, sticking with the default does not. Zizzo (2003) show a dissociation between learning to provide optimal verbal responses and learning to make the optimal behavioural choices.

[16] Psychologists label the tendency for survey responders to provide the responses that they see as putting them in as good light as possible with the researchers the social desirability bias (e.g., Crowne and Marlowe, 1960; Stober, 2001), which can be connected to the desire to receive respect (e.g., Ellingsen and Johannesson, 2007) and both self-image and self-deception as two dimensions of it (Paulhus, 1984).

natural experiments are possible or ex ante and ex post surveys have been done when structural policy breaks have taken place, it is difficult to test the effects of policy changes with field data.

My chapter addresses these issues by using an experimental methodology. My first goal is to verify *whether*, in the absence of financial switching costs and using the stylized environment of the U.K. electricity and gas markets as a benchmark, I can identify a lack of switching and suboptimal outcomes when switching does take place.[17] The second goal is to get a better understanding of *why* suboptimal outcomes take place. I test the role of complexity, which I decompose as complexity in the relationship between prices and quantity (linear vs. non-linear tariffs), in the presence or absence of bundling (single product vs. dual products tariffs), and in the number of tariffs.[18] I also test the role of consumer inattention by suitably developing a methodology used by Lei et al. (2001) based on the presence of an alternative task, and I consider two possible alternative tasks across different treatments.

Learning why suboptimal choices take place helps me achieve the third goal, which is to test the effectiveness of policies designed to improve consumer outcomes. I am able to evaluate policies putting limits on the number and type of tariffs such as the regulatory constraints on complex tariffs recently proposed by the UK regulator Ofgem (2011). I also test two nudge manipulations that may help outcomes without reducing the consumer's freedom to choose. The first is a simple awareness raising manipulation by which subjects are advised of the existence of a better tariff when they have made a suboptimal choice. This

[17] Cason et al. (2003) describes a market experiment considering the implications of financial switching costs for market structure. Our experiment does not have financial switching costs but rather lets insufficient switching emerge endogenously from consumer decisions (or failures to decide).
[18] Kalayci and Potters (2011) have an interesting experiment where sellers choose product complexity, in terms of number of attributes of an abstract product, and find some evidence of consumer exploitability, though subject to consumers having to make decisions within 15 seconds; in an experiment again on product complexity (with products modeled as abstract lotteries) but no time constraints, Sitzia and Zizzo (2011) find some qualified (though only qualified) evidence of consumer exploitability. Unlike these experiments, we consider tariff complexity, number of tariffs and product bundling, and we employ tariffs mapped up from a real world markets. Also, in treatments with time constraints subjects do have anyway plenty of time to decide (2 minutes), as verified against a control treatment without such time constraint.

can be connected to Ofgem's (2012) consideration to trial out a 'market cheapest deal' warning scheme, by which companies would be required warn consumers of the existence of a better deal in the market.

The second turns the power of defaults on its head by making it work to achieve better rather than worse consumer outcomes: a 'smart nudge' is employed which automatically identifies the best tariff and uses this as the default choice. This may be connected to the U.K. Prime Minister David Cameron's recent suggestion of forcing energy companies to offer the cheapest of their tariffs (BBC, 2012a; Waddams, 2012), but, while these proposals are company specific, my nudge would work in terms of the best tariff *in the market* as opposed to *by a specific company*.[19]

My key finding is that tariff complexity and the number of tariffs matter, but that inattention matters as well. Regulatory measures to reduce complexity are likely, therefore, to be of only partial value. By using smart nudges and making the power of default work for instead against consumer welfare, I can obtain optimal outcomes around 85% of the times.[20]

The rest of the chapter is structured as follows. Section 2 provides some background on the specific markets that my experiments use as model and on the existing evidence on insufficient switching and suboptimal choices. Section 3 describes some common features of the experiment. Sections 4, 5 and 6 are on Experiment 1, 2 and 3 respectively. Section 7 provides a discussion and section 8 concludes.

2.2 The Institutional Background

The institutional setup on which I model my experiments primarily is the UK electricity and gas markets. These are mature markets which have been liberalized since 1996-1999 and which are comparatively simple in terms of the product they offer (energy).

[19] The relationship between our proposal and David Cameron's suggestion will be discussed further in Section 7.
[20] A growing literature is emerging testing and discussing nudging and optimal defaults in a number of other contexts: they include among others Choi et al. (2003), Thaler and Sunstein (2003, 2008), Carroll et al. (2009), O'Neill (2007), Kooreman and Prast (2010), Beshears et al. (2013) and Downs et al. (2009).

They are also comparatively transparent markets with a wide availability of online search and switching websites.[21] These websites enable both the identification of the best tariffs for any given level of consumption and easy switching of service provider at the click of a mouse. One element of complication is that tariffs can be either for electricity only, or for gas only, or they can be dual tariffs bundling together both electricity and gas; my experiment will focus on electricity only and dual tariffs.[22] As a further complication, the number of tariffs in the market is large: as an illustration, when I collected data for my experiment, I found as many as 72 electricity and 80 dual tariffs in the London, UK, energy market using an online website.[23] 70% of consumers found the number of available tariffs confusing in a UK Ofgem regulator survey (Ofgem, 2008).

Consumers tend to stick to their status quo in terms of energy supplier (NERA, 2003; Ofgem, 2009, 2011; Behavioural Insights Team, 2011): this acts as their default choice. For example, only 18% of all respondents to an Ofgem consumer survey switched electricity supplier in 2009, and only 17% switched gas supplier (Ofgem, 2010). While 2009 was a year where average prices fell slightly (by 6%), even in a year such as 2008 where average prices went up considerably (by 37%) and so one would have imagined a push to shop around for better deals, only 19% and 20% of gas and electricity customers respectively switched supplier (Ofgem, 2010). In a recent U.K. energy market survey, DECC (2012) reports that only 5% of consumers planned to switch in the next 12 months with just 26% of consumers treating switching as a possibility. While internet penetration and so the access to search and switching engines has increased with time, Jamasb and Pollitt (2005) reported fairly comparable switching figures of just 22% from domestic and small commercial figures in 2002/2003.

[21] Examples include http://www.which.co.uk/switch/, http://www.uswitch.com/, http://www.gocompare.com/gas-and-electricity/ and http://www.confused.com/gas-electricity.
[22] A positive correlation between switching electricity and gas has been found (Giuletti et al., 2005).
[23] Ofgem (2012) contains estimates, as do earlier Ofgem retail market reviews.

Furthermore, when switching takes place, the best (cheapest) tariff is often not chosen. Using data from 2005 and 2000 surveys, Wilson and Waddams Price (2010) estimated that only 8 to 20% of consumers opted for the best tariff given their annual consumption levels. Only around 2/3 of consumers stated that they felt they got a better deal by switching suppliers in 2008 and 2009 (Ofgem, 2010).

While I model key stylized features from the UK electricity and gas markets, I believe that the key issues that I identify and help address are more general. Joskow (2008) discusses the US experience with electricity market liberalization where again there is evidence of consumers failing to switch suppliers with the assumption of the consumers' ability "to shop intelligently" being called into question (pp. 34-35) and being one source of the apparent failure of US consumers to reap much benefits from liberalization. Jamasb and Pollitt (2005) report and discuss limited consumer switching in all EU countries where switching is possible, with the UK actually being the lead with its 22%, and Nunez (2011) notes the failure of ensuring effective competition due to, among other things, the lack of transparency and problems with price setting. An EU DG Sanco study found that electricity switching rates are above 10% in only seven EU countries, with just 32% of EU consumers having compared offers from different suppliers and with average savings to be obtained € 100 per year (DG Sanco, 2010).

Going beyond the energy sector, other services sectors have similar recognized stylized features: the existence of a default option, the potential complexity of the associated tariffs and costs, the limited switching and a significant likelihood of suboptimal switching. Ofcom (2009) reports switching rates roughly equivalent or lower than for electricity and gas in relation to bank account, internet, fixed and mobile telephony services, and multichannel TV services. In a study on bank users, OFT (2008) found that 47% of surveyed consumers had not even considered switching bank account. International evidence on limited switching in telecommunications services is discussed by Xavier and Ypsilanti (2008) and Lunn (2011). This is notwithstanding the fact that online search engines may also be available for such services.[24]

2.3 Experimental Design: Some Common Features

The experiments were run at the University of East Anglia in 2011 and 2012. Before the beginning of the experiment, subjects had to read instructions and complete a questionnaire with the purpose of checking they had understood what the tasks involved. If they had any doubts they could ask for clarification. Once questionnaires were collected and doubts clarified the experiment started. All experiments involved individual choices where subjects had repeated opportunities to choose among a set of tariffs. The experimental instructions and details on all tariff tasks are in the appendix D.

The tariffs. In February 2011, I collected all the electricity and gas tariffs available in the UK market as available to a London consumer using the "Which?" website. The tariffs ranged from simple ones with one tier (i.e., a single marginal price) to more complicated ones with two tiers and a ceiling (i.e. a marginal price and, once consumption exceeds a ceiling, a second and lower marginal prices) or a standing charge and one tier (i.e. a fixed price plus a single marginal price). The tariffs in my experiment were partly real tariffs collected in this way and partly derived by me using the same structure as the real ones (*derived* tariffs in what follows). The process of selection and derivation of all tariffs, as well as the full list, is described in detail in the appendix E. I employed 144 tariffs. Two thirds of the tariffs were real and one third was derived. The real tariffs were half for a single service (these are electricity tariffs) and the other half were dual tariffs (both gas and electricity). The derived

[24] For example, in relation to internet services, http://broadband-comparison.net/ is a price comparator for USA, http://www.comparebroadband.com.au/ is one for Australia and http://www.uswitch.com/broadband/ is one for the UK.

tariffs were all dual ones. Subjects were only told that the tariffs related either to one good or to two goods (labeled as good A and good B). Table 2.1 shows a sample of tariffs used.

	Good A				Good B			
Tariff	Standing charge	Tier 1	Ceiling	Tier 2	Standing charge	Tier 1	Ceiling	Tier 2
1	-	16.259	-	-	-	-	-	-
2	-	19.467	728	8.432	-	-	-	-
3	12.957	12.128	-	-	-	-	-	-
4	-	13.398	-	-		4.516		
5	-	19.992	900	12.19		5.878	670	5.521
6	13.23	12.338	-	-	9.569	7.269		

Table 2.1: Sample of Tariffs

The tariff tasks. The tasks were 36 overall. At the beginning of each task, in most treatments, subjects were assigned a default tariff, the details of which will be discussed later. They could either stick to the default or look at other available tariffs and then decide which one they wanted. After choosing the tariff, they selected how much they wanted to consume out of 5 possible consumption levels: 1000, 2000, 3000, 4000 and 5000.[25]

The consumption level determined the revenue while the cost depended on the tariff chosen and amount consumed. The revenue was maximized when the optimal consumption level was 4000.[26] After subjects chose their consumption, they were told their earnings, which was determined as revenue minus cost. At the end of the experiment, one of the 36 tasks was chosen randomly and subjects were paid according to the choice made in that task.[27] Average earnings were around 20 pounds.

[25] Actual consumers of course do not have pre-defined possible levels of consumption. By having only 5 levels, we wished to keep things as simple as possible in this part of the experiment, however, bearing in mind that actual consumers do have past consumption as a guide to future consumption, and so the level of consumption is not that much of an issue.
[26] The average yearly household electricity consumption in the UK is around 4000 kwh. The gas consumption is approximately 4 times this amount; in the experiment, we scaled this down by a factor of 4 for simplicity.
[27] In all treatments, subjects could use a calculator on the computer screen to help them with their choices of tariffs and consumption levels. The calculator had 4 boxes for inputting consumption levels and the values of tier 1, tier 2, ceiling and standing charge of the tariff they wanted to check the cost of.

The default tariff. The default tariff was always a derived tariff designed in such a way that it was never the best to maximize earnings (except in two treatments, discussed later). The difference between the default tariff and the best tariff was usually at least around 6 pounds.

Nature of the tariffs employed in each task. The order of the 36 tasks was randomized. Half of them involved 4 tariffs and the other half 24. For both sets of tasks, 1/3 of the tariffs were single tariffs (all real), 1/3 real dual ones and 1/3 derived dual ones. For each of this subset of tasks, 2 tasks involved a choice among all simple tariffs (only one tier), 2 tasks a choice among all complex tariffs (two tiers plus ceiling, or standing charge plus one tier), and 4 tasks a choice among a mix of tariffs, with half of the tariffs being simple and the other half complex.[28]

2.4 Experiment 1 – Product Complexity

Experiment 1 tests for the effect of complexity – in terms of tariff complexity, number of tariffs and bundled nature of products - on consumer outcomes. It also acts as a control for key features present in the other experiments: specifically, the effect of having a search engine, of having two minutes to make a decision and of having a default tariff as I have implemented it. It has 5 treatments (DE, mDE, D, DF and F). Figure 2.1 shows the relationships between these treatments and those in the other experiments.

[28] These four tasks differed depending on the combination of default tariff (simple or complex) and best tariff (again, simple or complex).

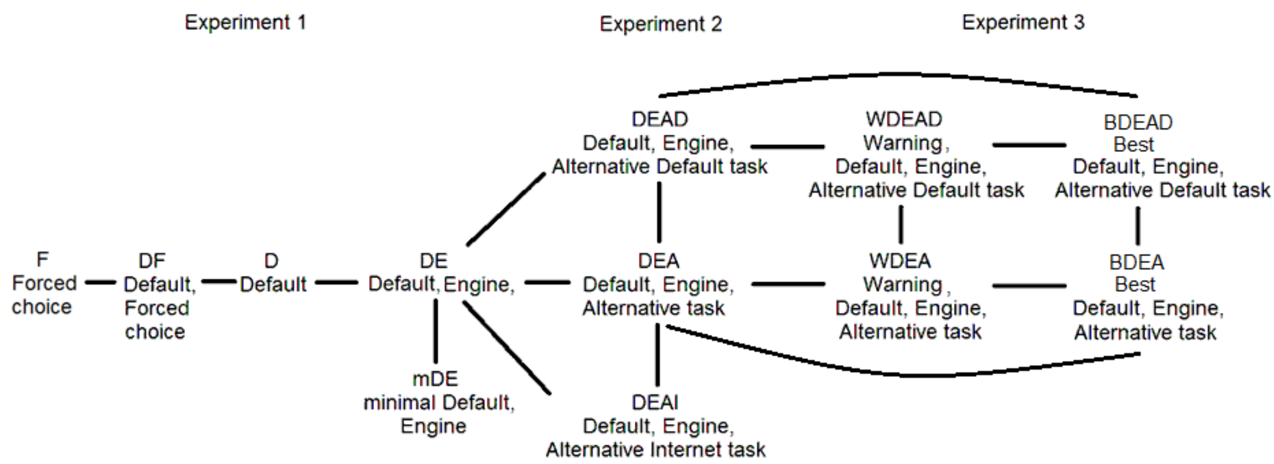


Figure 2.1: Map of Experimental Treatments

2.4.1 The Treatments

*Treatment DE (**D**efault tariff and search **E**ngine)*. This treatment had a default tariff and a search engine. In each task subjects were shown the default tariff in a first screen; from this screen subjects could either stick to the default tariff or go to a second screen where they could see all the tariffs involved with the default tariff highlighted (see Figure 2.2).

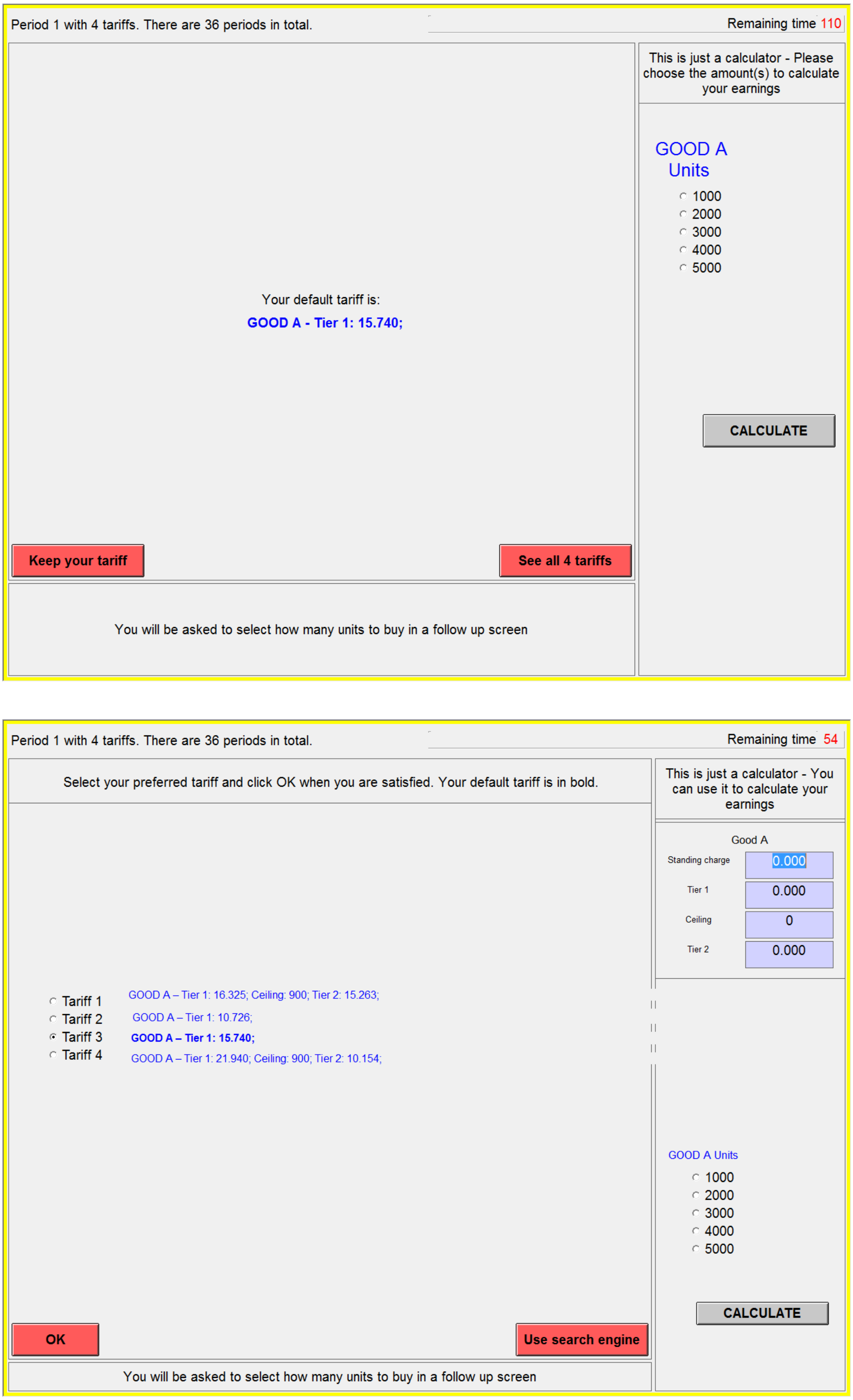


Figure 2.2: The Tariff Task in Treatment DE (screenshot)

For each task, if subjects did not make a choice within two minutes, they were assigned the default tariff.[29] When deciding the tariff to select, subjects could use a search engine, which was a stylized version of internet search engines: subjects provided the default tariff details and a consumption level; the search engine then gave the entire list of tariffs with the difference in earnings relative to the default tariff.

*Treatment mDE (**m**inimal **D**efault tariff and search **E**ngine).* This treatment also had the search engine, but the default tariff was implemented more mildly. Specifically, there was no first screen with just the default tariff; rather, in each task I simply showed subjects the screen with all tariffs, with the default one highlighted.

*Treatment D (**D**efault tariff).* This is the same as treatment DE but does not implement a search engine. It therefore allows me to control for the effect of having a search engine.

*Treatment DF (**D**efault tariff and **F**orced choice).* This treatment is the same as treatment D but removes another feature of the market services: namely, the idea that, if you do not make a decision, you are simply stuck with the default. In the previous treatments subjects that did not make a choice within 2 minutes would stick automatically to the default. Treatment DF does not allow for this: subjects do not go ahead until they choose a tariff.

*Treatment F (**F**orced choice).* This treatment makes a final step relative to the DF treatment: unlike the DF treatment, there is no default tariff.

2.4.2 Results

2.4.2.1 Overview

Subjects can end up in two ways with a less good a deal than if they had chosen the best tariff: either by sticking with the default tariff (in treatments other than F) or by

[29] Based on other treatments without this time cutoff, we knew that subjects take around 1 minute to make a choice. We then fixed the 2 minutes cutoff so that we are comfortable that any difference in behaviour cannot be attributed to subjects simply not having the time to take a decision.

switching to another suboptimal tariff. Define the *default rate* as the percentage of times subjects stick to the default tariff, and the *suboptimal switching rate* as the percentage of times subjects switch to a suboptimal tariff. In Experiment 1 (and 2, discussed later), the *suboptimal outcome rate* is then defined as the sum of the default rate and suboptimal switching rate.

Table 2.2 and Figure 2.3 present default rates, suboptimal switching rates and suboptimal choices for the 5 treatments. In treatment DE the default rate is around 14%. Have a milder default does not make a difference (Mann-Whitney $p = 0.89$).[30]

[30] All p values in this chapter are two tailed. For all bivariate tests, unless specified otherwise, tests are run at the subject level to control for any within-subject non independence of observations. In the DE and mDE treatments, the search engine was used 25% and 32% of the times respectively.

	Type of Market		Complexity of Task			Number of Tariffs per Task		Overall
Treatment	Single Market	Dual Market	All Simple	All Complex	Mixed	4 tariffs	24 Tariffs	
Panel a - Default Rates								
DE	0.114	0.160	0.117	0.178	0.143	0.146	0.143	0.144
mDE	0.128	0.154	0.144	0.161	0.142	0.180	0.111	0.145
D	0.347	0.422	0.400	0.423	0.389	0.386	0.408	0.397
DF	0.155	0.166	0.157	0.207	0.153	0.161	0.163	0.162
F	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Average	0.202	0.242	0.223	0.260	0.223	0.232	0.226	0.229
Panel b - Suboptimal Switching Rates								
DE	0.275	0.297	0.167	0.339	0.308	0.185	0.394	0.290
mDE	0.281	0.338	0.217	0.306	0.347	0.204	0.433	0.319
D	0.237	0.263	0.153	0.337	0.259	0.151	0.358	0.254
DF	0.253	0.289	0.153	0.360	0.288	0.158	0.397	0.277
F	0.368	0.432	0.253	0.517	0.423	0.274	0.547	0.411
Average	0.283	0.324	0.189	0.372	0.325	0.194	0.426	0.310
Panel c - Suboptimal Outcome Rates								
DE	0.389	0.457	0.283	0.517	0.451	0.331	0.537	0.434
mDE	0.408	0.492	0.361	0.467	0.489	0.383	0.544	0.464
D	0.583	0.685	0.553	0.760	0.648	0.537	0.766	0.651
DF	0.408	0.455	0.310	0.567	0.440	0.319	0.560	0.439
F	0.368	0.432	0.253	0.517	0.423	0.274	0.547	0.411
Average	0.438	0.510	0.412	0.632	0.548	0.426	0.652	0.539

Panel d - First half and Second half

	Default Choice Rates		Suboptimal Switching Rates		Suboptimal Choice Rates	
	First half	Second Half	First half	Second Half	First half	Second Half
DE	0.204	0.085	0.306	0.274	0.509	0.359
mDE	0.180	0.111	0.300	0.337	0.480	0.448
D	0.432	0.361	0.249	0.260	0.681	0.621
DF	0.193	0.131	0.293	0.261	0.487	0.392
F	0.036	0.028	0.398	0.360	0.433	0.388
Average	0.212	0.152	0.310	0.297	0.522	0.449

Table 2.2: Experiment 1 – Average Performance

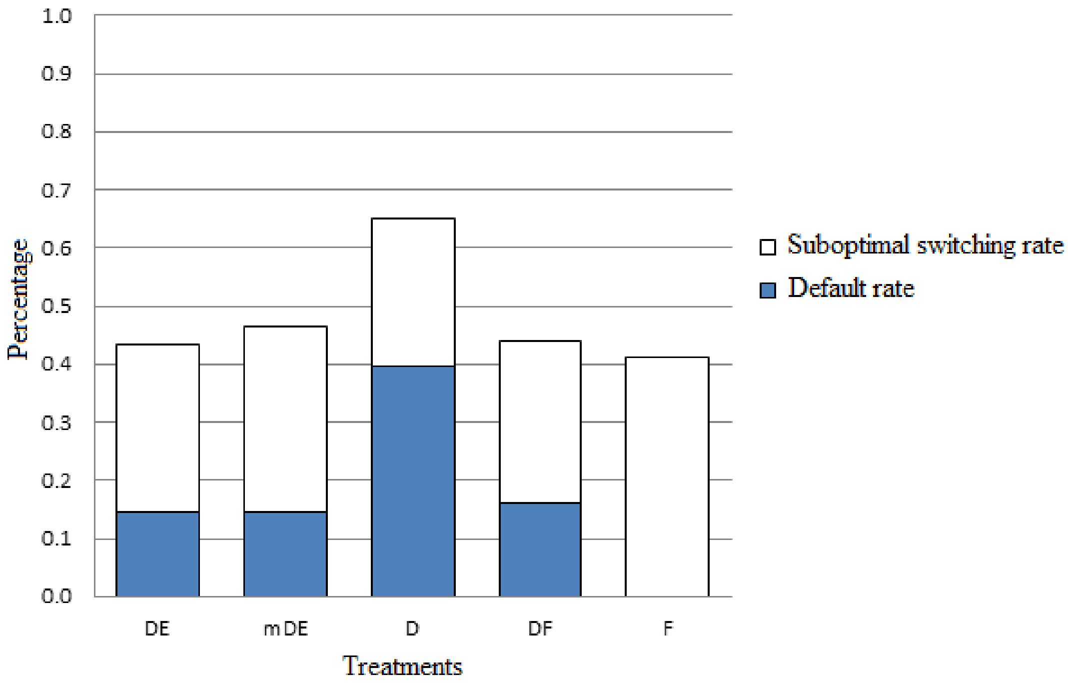


Figure 2.3: Suboptimal Outcome Rates in Experiment 1

Result 1: The search engine is effective in reducing consumer inertia and this translates into more optimal consumer outcomes.

Support: While making no difference in terms of suboptimal switching rate ($p = 0.28$), having a search engine (DE treatment) clearly reduces the default rate relative to the D treatment ($p = 0.004$). Correspondingly, the suboptimal outcome rate goes down ($p = 0.002$).

Result 2: When subjects are forced to take action, inertia reduces substantially and this implies more optimal outcomes. Removing a tariff marked as default does not further change the proportion of optimal outcomes.

Support: The default rate in the DF treatment is significantly lower than in the D treatment ($p < 0.001$). With an unchanged suboptimal switching rate ($p = 0.57$), this leads to a lower suboptimal outcome rate ($p = 0.002$). Conversely, Table 2.2 shows that this rate is basically identical between the DF and the F treatment (p = 0.68), though subjects who would have stuck with the default rate are replaced by subjects who simply make a wrong choice.[31]

2.4.2.2 The Role of Complexity

Table 2.2 also reports averages of my 3 key variables for different dimensions of complexity. [32]

Result 3: Product bundling has a statistically significant effect of about 8% on the suboptimal outcome rate, marginally affecting both default rate and suboptimal switching rate.

Support: In Wilcoxon tests, the suboptimal outcome rate is significantly higher for dual markets than for single product ones.[33] The effects on default and suboptimal switching rates are small enough not to be always significant by treatment, though they are so in the pooled data.[34]

Result 4: Having a mix of tariffs of different complexity is sufficient to increase suboptimal outcomes by about 14%, going up to 22% when all tariffs are complex. The result is driven by suboptimal switches rather than by default choices.

Support: Table 2.2 shows the average performance of subjects according to tariff complexity. Differences in suboptimal outcome rates are significant between the simple and mixed tariffs tasks on the one side, and the complex tariffs on the other side, in the pooled

[31] In the F treatment there is not, by definition, a default tariff as such. We can however check for the likelihood that the tariff that is the default tariff in the other treatments is chosen, so as to control for the idiosyncratic preference for such tariff, i.e. for the extent that, in the other treatments, the default tariff is chosen not because it is the default. This likelihood is listed in Table 2.2 as the 'default rate' for the F treatment; it turns out that it is only 3% on average. In Table 2.2 we classify any other suboptimal choice under the heading of 'suboptimal switching rate'; this is purely for comparability with the other treatments, as obviously there is no default tariff to switch from as such, and is higher than the suboptimal switching rate in the DF treatment (p = 0.016).

[32] For task simplicity, we constructed all tasks in such a way that the optimal tariff was such for whatever level of consumption. That said, we also checked the data on consumption level; on average subjects choose the optimal consumption level 75% of the times.

[33] Wilcoxon p = 0.004, 0.002, < 0.001, = 0.004, 0.01 and p<0.001 respectively for DE, mDE, D, DF, F and the pooled data.

[34] For the suboptimal switching rate, Wilcoxon p = 0.17, 0.008, 0.26, 0.03, 0.02 and < 0.001 for treatment DE, mDE, D, DF, F and pooled data respectively. For the default rate, Wilcoxon p = 0.01, 0.14, < 0.001, = 0.59, 0.06 and < 0.001 for treatment DE, mDE, D, DF, F and pooled data respectively.

data and for most treatments individually.[35] While not unequivocal, the result is also supported by the regression analysis in the appendix F. It is largely driven by more switches being suboptimal[36] rather than by changes in default rates, though the latter are significant when comparing simple and complex tariffs in the pooled data (Wilcoxon p = 0.01), though not when comparing simple and mixed tariffs (Wilcoxon p = 0.25).[37]

Result 5: Having a higher number of tariffs increases suboptimal switching and consequently suboptimal outcomes by around 23%. This is not purely a random choice effect.

Support: From a glance at Table 2.2, the default rate is virtually unchanged (23%) between 4 tariffs and 24 tariffs tasks, whereas the suboptimal switching rate jumps up and consequently the suboptimal outcome rate goes up by around 23% (Wilcoxon p < 0.001 for all treatments and the pooled data). This pattern cannot be explained by observing that, if subjects choose at random, they should go for the best tariffs 1/6 less under the 24 tariffs than they do under 4 tariffs (a random choice effect). If this were the case, I would observe the default rate too should go down when there are 24 tariffs, which I do not observe. More broadly, I do not observe a random choice of tariffs.[38]

2.4.2.3 Other Results

Earnings. Subjects sticking to the default tariff gained 56046 points on average. In contrast, on average switchers earned 78804 points (over 6 pounds more) if they got the best tariff and 65800 points (around 3 pounds more) if they did not. Switching was therefore,

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[35] In relation to mixed vs. simple tariffs, Wilcoxon p = < 0.001, 0.02, < 0.001, < 0.001, < 0.001 and < 0.001 for treatment DE, mDE, D, DF, F and pooled data respectively. In relation to complex vs. simple tariffs, Wilcoxon p = 0.001, 0.07, < 0.001, < 0.001, < 0.001and < 0.001for treatment DE, mDE, D, DF, F and pooled data respectively.

[36] p <0.001 in all treatments and in the pooled data, for complex (mixed) tariffs vs. simple tariffs, except for treatment mDE, where p = 0.15 (0.004, respectively) and DE for mixed vs simple (p = 0.002).

[37] In relation to mixed vs. simple tariffs, Wilcoxon p = 0.07, 0.38, 0.68, 0.33, 0.96 and 0.25 for treatment DE, mDE, D, DF, F and pooled data respectively. In relation to complex vs. simple tariffs, Wilcoxon p = 0.08, 0.41, 0.22, 0.09, 0.79 and 0.01 for treatment DE, mDE, D, DF, F and pooled data respectively.

[38] We can test this at the level of individual choices by noting that, if subjects simply randomized their choices across outcomes, each tariff should be chosen ¼ of the times in 4 tariffs tasks and 1/24 of the times in 24 tariffs tasks. Both hypotheses are rejected (p < 0.001 in a test of proportions).

broadly speaking, a winning strategy, even if the best tariff was not chosen; 'super suboptimal' choices, i.e. switching choices to tariffs worse than the default, were few.[39] The appendix H contains a table with earnings by treatment for all three experiments.

Complexity of default tariff and best tariff. By looking at mixed tariffs tasks, I can isolate whether having a complex default tariff or best tariff as complex makes a difference. The only consistent effect I find is one of complexity of the best tariff on the suboptimal switching rates (and, consequently, the suboptimal outcome rate): if the best tariff is complex, it is harder to spot it and as a result the suboptimal switching rate more than doubles from 19% to 45% on average (Wilcoxon $p < 0.001$). As a large increase applies also to treatments with a search engine, this suggests that the search engine is insufficiently used.[40]

Learning and use of search engine. Table 2.2, panel (d), compares performance in the first half and the 2nd half of the experiment.[41] Unsurprisingly given the picture on earnings, the default rate tends to fall in all treatments, but remains close to 36% in treatment D in the lack of a search engine, whereas for the DE treatment it goes below 10%. The fall in suboptimal switching rate is less pronounced, with a minimum of about 1 switch out of 4 remaining sub-optimal in all treatments. On average 25% and 32% of subjects used the search engine in treatment DE and mDE, respectively, with little variation between first half and second half of each experiment (see the appendix G for more details).

2.5 Experiment 2 – Inattention to the Task

Experiment 2 considers the impact of inattention to the task on consumer behaviour. This is very difficult in an experimental setting because there is a natural bias that subjects have in coming to the lab to *do something*; this is different from households not paying

[39] Only 3% of switches were 'super suboptimal' in 4 tariffs mixes, and only 4% in 24 tariffs mixes.
[40] In the DE (mDE) treatment, the suboptimal switching rates are 0.16 (0.27) and 0.46 (0.42) with simple and complex best tariffs, respectively (Wilcoxon $p < 0.001$ and $p = 0.03$ respectively).
[41] The regression analysis in the appendix F controls for experimental period. The appendix H also provides figures on how performance evolved on a period by period basis for all three experiments.

attention to specific tasks, such as choices of services, because it is not part of their weekly or monthly or yearly routines. All treatments are identical to the treatment DE from Experiment 1 except for the following. My first treatment (DEAI) adds an alternative task for subjects to engage in: specifically, a second computer screen where subjects can browse the internet, check email or Facebook, and the like. My second treatment (DEA) also has an alternative (unincentivized) task, but now an unpleasant one employed in the real effort experiment literature (e.g., Abeler et al., 2011) to measure psychologically costly real effort, counting 1s in a matrix of 0s and 1s. My third treatment (DEAD) primes subjects to pay attention to this alternative task, by having this alternative screen on the first screen of each task. Figure 2.1 shows the relationships between Experiment 2 and the others.

2.5.1 The Treatments

*DEAI (**D**efault with search **E**ngine and **A**lternative **I**nternet task) treatment*. In this control treatment, subjects had the choice either to freely surf the web or to pay attention to the tariff tasks on another screen. In one computer screen they could browse the web; in the other one they could perform the tariff tasks. If they did not make any active choice of tariff in any period within two minutes, as in the DE (or mDE) treatments the default tariff was selected for them; they were then still required to select their consumption level.

*DEA (**D**efault with search **E**ngine and **A**lternative task) treatment.* In this other control treatment, subjects again had two screens in front of them. In one they could perform the tariff tasks. In the other, they could perform a counting task consisting in counting the 1s in 0-1 matrices (see Figure 2.4).

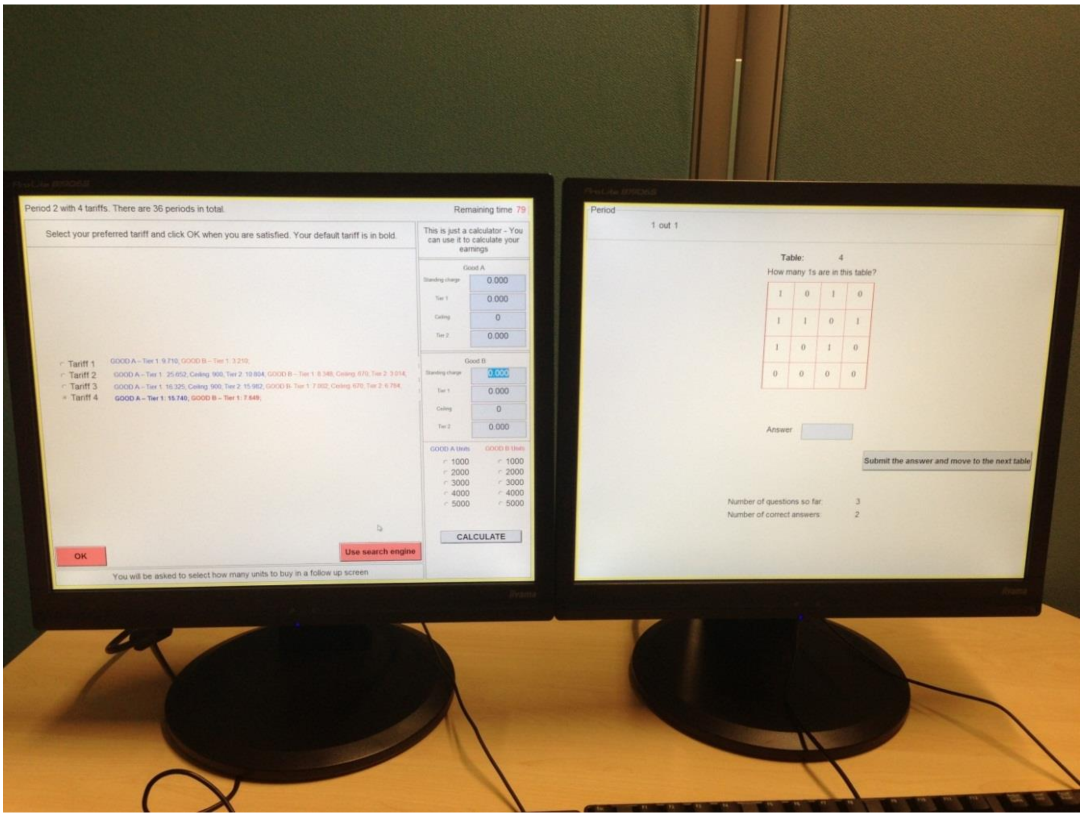

Figure 2.4: Alternative Counting Task in DEA, BDEA and WDEA Treatments

This is a task deemed unpleasant enough in the real effort literature (as in Abeler et al., 2011) as to be considered a good measure of real effort. In my experiment, and as made clear in the instructions, it was also entirely unincentivized, which means that subjects should have ignored the task and focused entirely on the tariff tasks, on which their earnings depended exclusively. By comparing performance in the DEAI and the DEA treatment, I can verify whether the nature of the alternative task – and the degree it may be pleasant – matters for my results.

*DEAD (**D**efault with search **E**ngine and **A**lternative **D**efault task) treatment*. This is the key treatment of experiment 2. I employed the same counting task as in DEA, but this was now placed on the first screen of each task (see Figure 2.5).

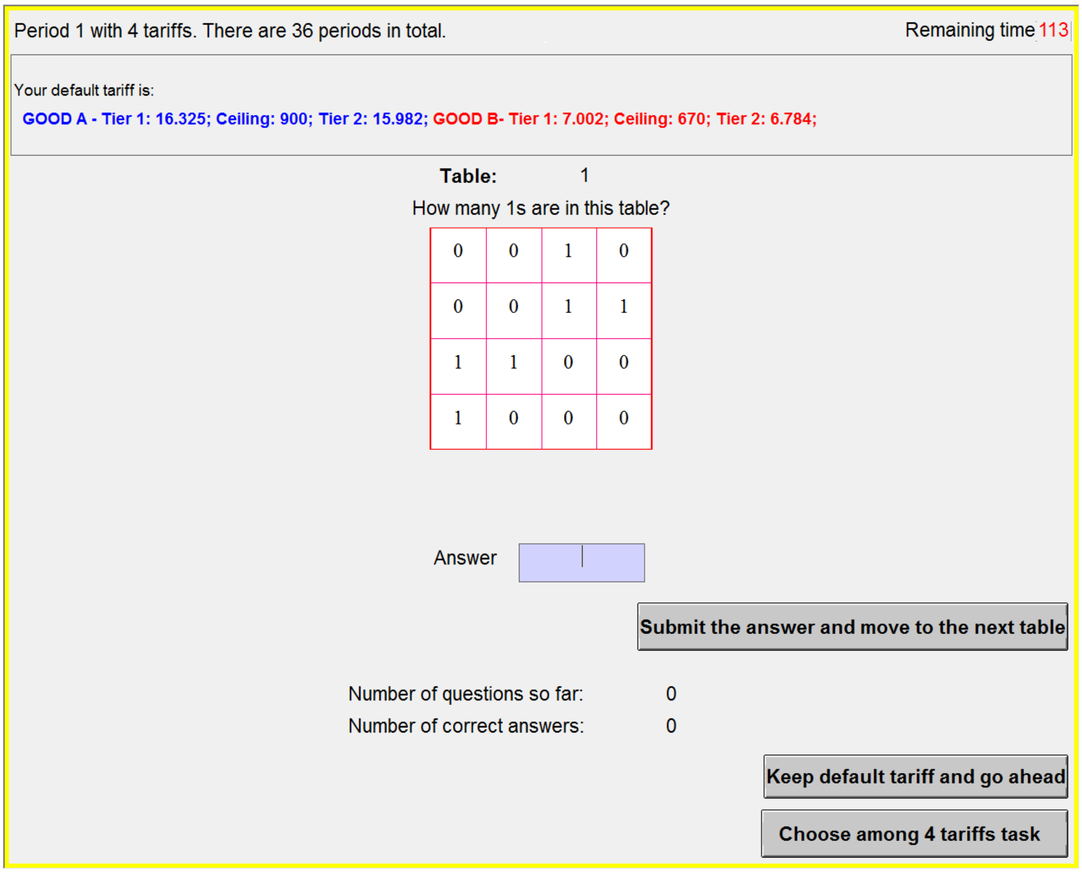

Figure 2.5: Counting Task Screen in DEAD, BDEAD and WDEAD Treatments

On the same screen subjects also saw the default tariff and so, if they wished, they could choose this tariff in this screen and move straight to the consumption page. Alternatively they could opt to see all the tariffs involved in the task and select the tariff of their choice as usual.

Using the language of Zizzo (2010), my experimental manipulation deliberately employs a *purely cognitive* experimenter demand effect as an experimental tool to make subjects pay attention as a default to the counting task.[42] I would argue that, even with this purely cognitive experimenter demand, the tariffs task is likely to be more salient in the

[42] Again in the language of that paper, this is akin to a legitimate *magnifying glass* use of experimenter demand effects: namely, one that employs demand effects as an artificial tool to replicate in the laboratory real world conditions that would otherwise not be paralleled. A purely cognitive experimenter demand effect relates to the cognitive process by which subjects make sense of an unfamiliar laboratory decision environment, and in this case can be seen to underpin treating the counting task as a default. It does *not* involve a desire to do what is perceived as what an experimenter wishes them to do, which we discuss next.

experiment than going to a switching website and changing energy tariffs can ever be for real world households. As a result, my inattention manipulation is likely to simply provide *lower bounds* on the kind of effects that inattention may produce in the real world. The comparison between performance in the DEA and the DEAD treatments will be especially useful in isolating this effect as the alternative task is the same in the two treatments. As a result, a preference for the alternative task would not be able to explain any differential performance between the two treatments.

In further treatments in Experiment 3, I address the potential criticism that subjects may not be inattentive as a result of a purely cognitive experimenter demand effect, but rather may simply want to do what they see that the experimenter wants them to do. This would be a form of *social* experimenter demand effect (Zizzo, 2010), that would be incompatible with my inattention interpretation. To anticipate my conclusion from section 6.2, Experiment 3 will allow me to reject this interpretation.[43]

2.5.2 Results

2.5.2.1 The Role of Inattention

Table 2.3 and Figure 2.6 present default rates, suboptimal switching rates and suboptimal choices for the 3 treatments of Experiment 2.

[43] Additional evidence against this interpretation is also available. During Experiment 2 (and in Experiment 3) we found it useful to add a social desirability questionnaire at the end of the experiment, which can be interpreted as a measure of sensitivity to social pressure (Stöber, 2001) and has been found as capable of predicting behaviour (Zizzo and Fleming, 2011). Regression analysis on the default rate described in the appendix F shows that all of the key effects described below survive controlling for social desirability.

Treatment	Type of Market: Single Market	Type of Market: Dual Market	Complexity of Task: All Simple	Complexity of Task: All Complex	Complexity of Task: Mixed	Number of Tariffs per task: 4 tariffs	Number of Tariffs per task: 24 Tariffs	Overall
Panel a - Default Rates								
DEAD	0.462	0.456	0.447	0.473	0.457	0.438	0.478	0.458
DEA	0.262	0.298	0.267	0.313	0.284	0.274	0.298	0.286
DEAI	0.211	0.286	0.261	0.272	0.258	0.274	0.248	0.261
Average	0.327	0.356	0.335	0.365	0.345	0.337	0.356	0.346
Panel b - Suboptimal Switching Rates								
DEAD	0.167	0.176	0.127	0.193	0.179	0.100	0.246	0.173
DEA	0.215	0.207	0.140	0.237	0.220	0.118	0.301	0.209
DEAI	0.236	0.239	0.133	0.272	0.256	0.154	0.322	0.238
Average	0.201	0.202	0.133	0.228	0.213	0.119	0.285	0.202
Panel c - Suboptimal Outcome Rates								
DEAD	0.628	0.632	0.573	0.667	0.636	0.538	0.723	0.631
DEA	0.477	0.505	0.407	0.550	0.504	0.392	0.599	0.496
DEAI	0.447	0.525	0.394	0.544	0.514	0.428	0.570	0.499
Average	0.528	0.558	0.468	0.594	0.557	0.456	0.640	0.548
Panel d - First half and Second half								
	Default Choice Rates			Suboptimal Switching Rates			Suboptimal Choice Rates	
	First half	Second Half		First half	Second Half		First half	Second Half
DEAD	0.533	0.382		0.170	0.176		0.703	0.558
DEA	0.340	0.232		0.226	0.193		0.566	0.426
DEAI	0.289	0.233		0.283	0.193		0.572	0.426
Average	0.403	0.290		0.218	0.186		0.620	0.476

Table 2.3: Experiment 2 – Average Performance

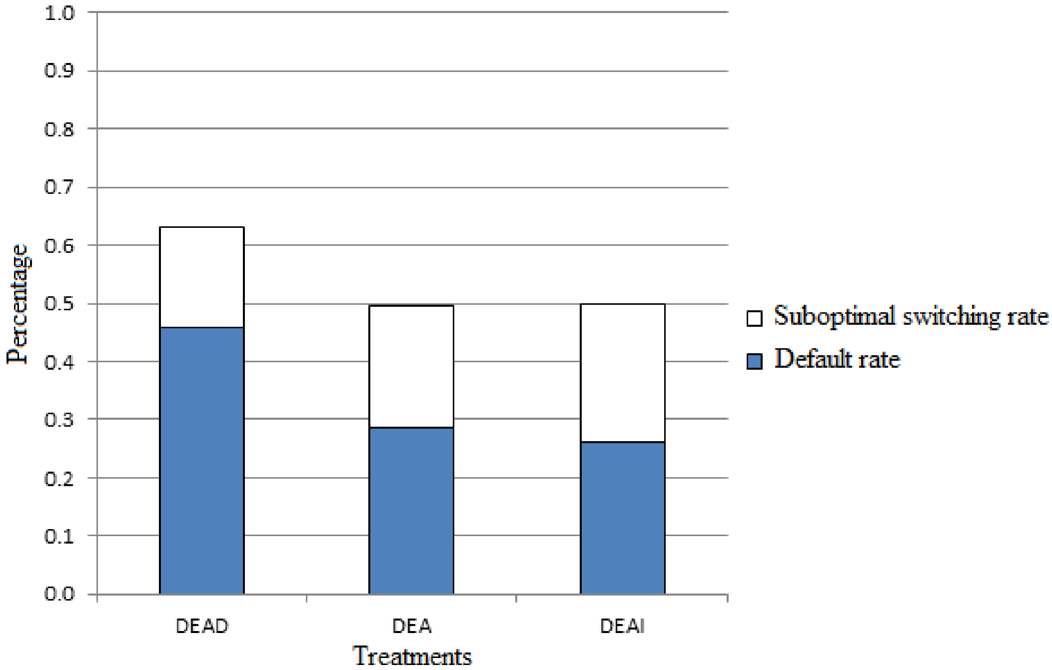

Figure 2.6: Suboptimal Outcome Rates in Experiment 2

Result 6: The default rate and suboptimal switching rates, and consequently the suboptimal outcome rate, is not different between the DEAI and the DEA treatments. There is no support for the nature of the alternative task being different.

Support: Table 2.3 shows that in both cases suboptimal outcome rates are about 50% (Mann-Whitney p = 0.99), roughly equally split between default choice rate and suboptimal switching rate (respectively Mann-Whitney p = 0.48 and 0.49).

Result 7: The default choice rate is significantly higher in the DEAD than in the DEA treatment, and three times as large as in DE. Overall suboptimal outcomes go up by 20% in the DEAD treatment relative to DE. Inattention matters.

Support: Tables 2.2 and 2.3 show that that the default choice rate jumps up to 46% in the DEAD treatment (Mann-Whitney p < 0.001 and 0.03 relative to DE and DEA). The suboptimal switching rate is value is lower in DEAD though the effect is marginal or insignificant (Mann-Whitney p = 0.09 and 0.002 respectively relative to DEA and DE). Overall, 63% of outcomes were suboptimal in DEAD, against 43% in DE and 50% in DEA

(Mann-Whitney $p = 0.05$ and 0.08 relative to DEA and DE). That the DEAD treatment has higher default rates and suboptimal outcome rates than DE is also confirmed by the regression analysis in the appendix F. The different default rate between DEAD and DEA can be interpreted in terms of inattention. Further support for the interpretation of the worse performance in DEAD in terms of inattention is provided by Result 8.

Result 8: In the DEAD treatment subjects pay less attention to the tariffs task than in the DEA treatment, and there is a strong correlation between engagement with the alternative task and higher default rate.

Support: In the DEA and DEAD treatments I have a measure of engagement with the alternative task, specifically the percentage of times each subject has played the counting task. Subjects do 164 counting tasks on average in the DEAD treatment, vs. 51 in the DEA treatment (Mann-Whitney $p < 0.001$).[44] In the DEAD treatment the two variables are strongly correlated, unlike in the DEA treatment (Spearman $\rho = 0.56$, $p < 0.001$, in DEAD, vs. 0.17, $p = 0.24$ in DEA).

2.5.2.2 The Role of Complexity

Result 9: Product bundling no longer matters when inattention to the task is a problem.

Support: As shown by Table 2.3, the 8% difference in suboptimal outcome rate with product bundling found in Experiment 1 (Result 3) is almost unchanged with DEAI (Wilcoxon $p = 0.10$) but reduces to just 3% in DEA (Wilcoxon $p = 0.001$) and disappears entirely in the DEAD treatment (Wilcoxon $p = 0.79$).

Result 10: There is again a tariff complexity effect driven mainly by suboptimal switches, but this appears smaller when inattention is a problem.

[44] The proportion of subjects that played the counting task at least once is also greater in the DEAD treatment (Mann-Whitney, $p < 0.001$).

Support: Table 2.3 shows that, under DEAD, the Result 4 effect sizes are roughly halved, with an increase in suboptimal outcomes of 7% with mixed tariffs and 10% with all complex tariffs, relative to having all simple tariffs (Wilcoxon $p = 0.09$ and 0.09, respectively). The effects in DEA and DEAI are of intermediate size.[45]

Result 11: There is again a tariffs number effect driven by suboptimal switching, and consequently the suboptimal outcome rates by around 18%.

Support: As per Table 2.3, Result 5 on the tariffs number effect is replicated quite robustly. Default rates are virtually unchanged with 24 tariffs, but suboptimal switching rates and consequently suboptimal outcome rates go up substantially in all treatments, including DEAD (Wilcoxon $p < 0.001$ in all cases).[46]

2.5.2.3 Other Results

The results on earnings and on the complexity of default and best tariffs from Experiment 1 are replicated in Experiment 2.[47] Table 2.3, panel (d), compares performance in the first half and the 2nd half of the experiment. As in Experiment 1, the fall in default rate is more pronounced than the fall in suboptimal switching rate, which remains virtually unchanged in the DEAD treatment. However, even in the 2nd half of the experiment some 35% of the subjects stick to the default in the DEAD treatment, with the inattention problem basically offsetting the beneficial effect of the search engine in moving from the treatment without (D) to the treatment with search engine (DE; see section 4.2.3). Subjects used the search engine in 16%, 28% and 24% of the times in the DEAD, DEAI and DEA treatment

[45] For the suboptimal switching rate, Wilcoxon $p = 0.05$ and 0.002 (all complex vs. all simple tariffs) and 0.005 and 0.001 (mixed vs. all simple tariffs) for treatments DEA and DEAI respectively; for the default rate, $p = 0.09$ and 0.38 (all complex vs. all simple tariffs) and 0.37 and 0.45 (mixed vs. all simple tariffs) for treatments DEA and DEAI respectively.

[46] An exercise to check the non-randomness of choices, as we did for Result 5, would yield the same outcome.

[47] Earnings were 55790, 66551 and 78838 points on average for subjects who stuck with the default tariff, switched suboptimally or switched optimally. Considering the mixed tariff tasks, the proportion of suboptimal switches was again roughly twice as large if the best tariff was complex: e.g. 0.238 vs. 0.120 in DEAD (Wilcoxon $p < 0.001$).

respectively. Worryingly, even in the 2nd half of the experiment only 18% of the subjects used the search engine in the DEAD treatment. If many subjects do not pay attention to the tariffs task in the first place, the scope for the search engine is obviously more limited.[48]

2.6 Experiment 3 – The Nudges

Experiment 3 has two objectives, using the DEA and the DEAD treatments from Experiment 2 as the benchmark. First, it aims to test the effect of two nudges versions of which could be implemented by policy makers to obtain more consumer optimal outcomes. Second, as commented in section 5.1, it aims to test the interpretation of the DEAD treatment results in terms of subjects simply wanting to do what they see that the experimenter wants them to do, a form of *social* experimenter demand effect.

A *warning nudge* is one where, at the end of each of the 36 tasks, subjects who achieved a suboptimal outcome were given a message that they could have earned more money had they chosen a different tariff. This nudge could potentially be useful for policy, since policy makers could place as a requirement for companies to specify in energy bills that better tariffs could be available, and indeed Ofgem (2012) has been considering trialing this out in the U.K. energy market. The nudge also helps me test for the social experimenter demand effect interpretation: if subjects felt that they were supposed not to focus on the tariffs task because this was not what I wanted of them, by telling them that they could have earned more money by choosing another tariff, and indeed telling them repeatedly and insistently that this was the case if they kept ignoring the tariffs task, I made clear that this was not what I wanted at all. In this case in the DEAD treatment I should expect the default tariff rate to be comparable to that of the DE treatment.

[48] The proportion of times subjects engage in the counting task did decrease as the experiment progressed, from 43% to 21% in the DEAD. For DEA and DEAI, we do not have that information because subjects played on separate screens so the counting tasks and tariff periods cannot be linked.

A *default nudge* is one where the best tariff is selected automatically as the best tariff. Subjects are not told that this is the case and can still switch to a different tariff if they so wish. The inattention problem is solved by not requiring subjects to be attentive for achieving better outcomes. The intuition is that, outside the laboratory, if the policy makers were to identify a likely best tariff based on either personal or aggregate information (e.g. individual past consumption), and review this at intervals, this would enable better outcomes while enabling consumers who have a different preference to choose which tariff they actually prefer. Inside the laboratory, the welfare analysis is obviously more straightforward as the best tariff is always such for all subjects.

Figure 2.1 shows the relationships between Experiment 3 and the others.

2.6.1 The Treatments

*WDEAD treatment (**W**arning Nudge with **D**efault, search **E**ngine and **A**lternative **D**efault task).* This treatment is identical to the DEAD treatment (and so with a prominent alternative task: Figure 2.5) with the only difference that I provide warning nudges whenever subjects make a suboptimal choice.

*WDEA treatment (**W**arning Nudge with **D**efault search **E**ngine and **A**lternative task).* This treatment is identical to the DEA treatment (and so with an alternative task on a different screen: Figure 2.4) with the only difference that I provide warning nudges whenever subjects make a suboptimal choice.

*BDEAD treatment (**B**est **D**efault with search **E**ngine and **A**lternative **D**efault task).* This treatment is identical to the DEAD treatment with the only difference that the default tariff is now the best one.

*BDEA treatment (**B**est **D**efault with search **E**ngine and **A**lternative task).* This treatment is identical to the DEA treatment with the only difference that the default tariff is now the best one.

2.6.2 Results

2.6.2.1 Overview

Table 2.4 and Figure 2.7 describe the consumer performance in Experiment 3. In BDEA and BDEAD, the suboptimal outcome rate now coincides with the suboptimal switching rate as sticking to the default tariff is optimal.

Treatment	Type of Market: Single Market	Type of Market: Dual Market	Complexity of Task: All Simple	Complexity of Task: All Complex	Complexity of Task: Mixed	Number of Tariffs per task: 4 tariffs	Number of Tariffs per task: 24 Tariffs	Overall
Panel a - Default Rates								
BDEAD	0.850	0.850	0.878	0.828	0.849	0.878	0.822	0.850
BDEA	0.861	0.888	0.900	0.872	0.875	0.880	0.878	0.879
WDEAD	0.367	0.414	0.433	0.439	0.379	0.400	0.396	0.398
WDEA	0.100	0.136	0.100	0.156	0.122	0.102	0.146	0.124
Average	0.544	0.572	0.578	0.574	0.556	0.565	0.561	0.563
Panel b - Suboptimal Switching Rates								
BDEAD	0.150	0.150	0.122	0.172	0.151	0.122	0.178	0.150
BDEA	0.139	0.113	0.100	0.128	0.125	0.120	0.122	0.121
WDEAD	0.214	0.218	0.144	0.233	0.231	0.143	0.291	0.217
WDEA	0.172	0.207	0.150	0.256	0.192	0.106	0.285	0.195
Average	0.169	0.172	0.129	0.197	0.175	0.123	0.219	0.171
Panel c - Suboptimal Outcome Rates								
BDEAD	0.150	0.150	0.122	0.172	0.151	0.122	0.178	0.150
BDEA	0.139	0.113	0.100	0.128	0.125	0.120	0.122	0.121
WDEAD	0.581	0.632	0.578	0.672	0.610	0.543	0.687	0.615
WDEA	0.272	0.343	0.250	0.411	0.314	0.207	0.431	0.319
Average	0.285	0.309	0.263	0.346	0.300	0.248	0.355	0.301

Panel d - First half and Second half

	Default Choice Rates: First half	Default Choice Rates: Second Half	Suboptimal Switching Rates: First half	Suboptimal Switching Rates: Second Half	Suboptimal Choice Rates: First half	Suboptimal Choice Rates: Second Half
BDEAD	0.833	0.867	0.167	0.133	0.167	0.133
BDEA	0.833	0.924	0.167	0.076	0.167	0.076
WDEAD	0.448	0.348	0.209	0.224	0.657	0.572
WDEA	0.180	0.069	0.226	0.165	0.406	0.233
Average	0.574	0.552	0.192	0.150	0.349	0.254

Table 2.4: Experiment 3 – Average Performance

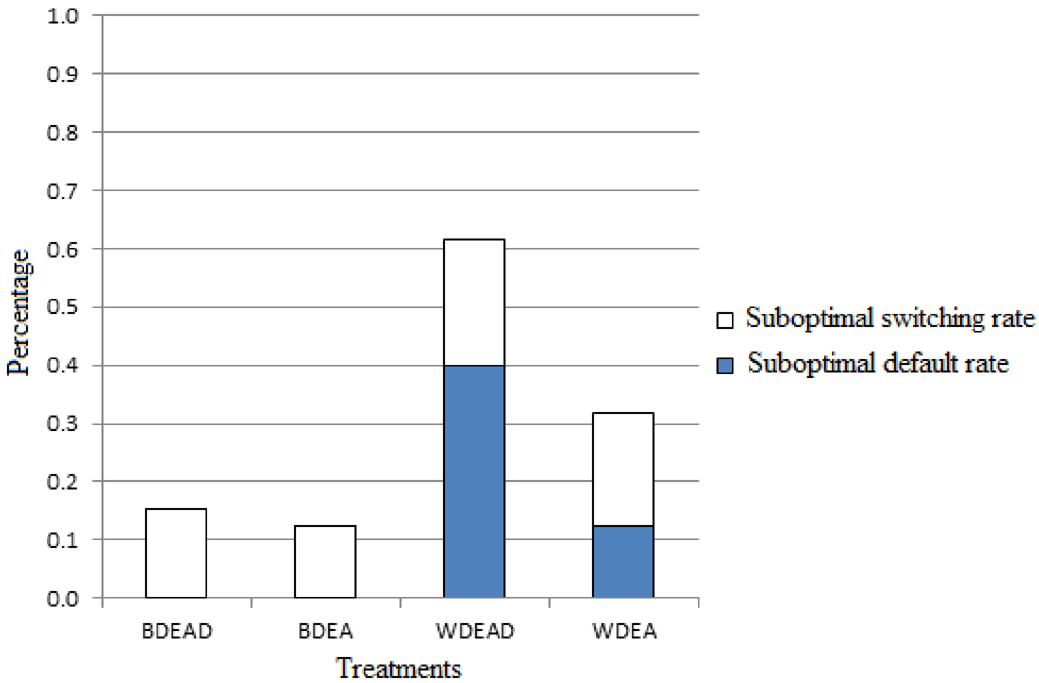

Figure 2.7: Suboptimal Outcome Rates in Experiment 3

Result 12: When inattention was a problem, providing a warning nudge did not help. There is no evidence for a social experimenter demand effect driving the worse performance by subjects in the DEAD treatment.

Support: Table 2.4 points to a fall in default rate (Mann-Whitney p = 0.063) and consequently suboptimal outcome rate in WDEA relative to DEA (Mann-Whitney p = 0.01).[49] WDEAD's default rate however is not statistically significantly different than DEAD's default rate (Mann-Whitney p = 0.48), and as the suboptimal switching rate is also unchanged (Mann-Whitney p = 0.42) this leads to six subjects out of ten still having suboptimal outcome rates in both treatments (Mann-Whitney p = 0.73). Default rate and suboptimal outcome rates are however clearly higher than in the DE treatment (Mann-Whitney p = 0.001 and 0.03, respectively), a fact confirmed by the regression analysis. Subjects, therefore, do not focus on the prominent counting task because they think they are

[49] The suboptimal switching rate is virtually unchanged (Mann-Whitney p = 0.64).

told not to look at the tariffs task, for even when it is highlighted to them again and again that they are making wrong choices, they do not change their behaviour.[50]

Result 13: A default nudge is effective in achieving better consumer outcomes.

Support: Table 2.4 shows that, with a default nudge, just around 15% of outcomes is suboptimal, with the large majority of subjects sticking to the default. The suboptimal outcome rate in BDEAD are significantly lower than in DEAD, WDEAD or DE (Mann-Whitney $p \leq 0.001$ in all cases), and those in BDEA are significantly lower than in DEA, WDEA or DE (Mann-Whitney $p < 0.001$ in all cases). This is confirmed by the regression analysis. No other treatment across all three experiments comes as close to an optimal outcome as these treatments.

2.6.2.2 The Role of Complexity

I first consider the effect of complexity in WDEAD and WDEA, and then move on to BDEAD and BDEA.

Result 14: In the presence of a nudge warning, product bundling matters only marginally when inattention to the task is a problem.

Support: In WDEA, there is a 7% increase in suboptimal outcome rate with product bundling roughly in line with DEA and Experiment 1 (Wilcoxon $p = 0.02$). The WDEAD there is 5% increase, a small effect just enough to achieve a marginal significance level (Wilcoxon $p = 0.06$).

Result 15: In the presence of a nudge warning, there is again a tariff complexity effect driven mainly by suboptimal switches, and again this is smaller when inattention is a problem.

[50] One could argue that, in a social experimenter demand effect interpretation of the prominent alternative task, the existence of repeated and insistent warnings should be especially effective in the 2nd half of the experiment, when subjects not attending to the tariffs task will have received a number of them. However, as discussed below in section 2.6.2.3, there is no evidence of this.

Support: This result simply replicates Result 10. In WDEAD, the increase in suboptimal outcomes is only 3% and 9% in tasks with mixed tariffs and in tasks with all complex tariffs respectively, in comparison to the case of all simple tariffs (Wilcoxon p = 0.42 and 0.06, respectively). The corresponding effects in WDEA are instead 6% and 16%.[51]

Result 16: In the presence of a nudge warning, there is again a tariffs number effect driven by suboptimal switching, and consequently an increase in the suboptimal outcome rate by 14% (in the WDEAD treatment) or 22% (WDEA treatment) as the number of tariffs goes from 4 to 24.

Support: As per Table 2.4, Results 5 and 11 on the tariffs number effect are again replicated fairly robustly. Default rates are similar with 24 tariffs, but suboptimal switching rates and consequently suboptimal outcome rates go up substantially in both WDEA and WDEAD (Wilcoxon $p < 0.001$ and $p = 0.003$ respectively).[52]

Result 17: With a nudge default, there is no effect of product bundling or of tariff complexity, and at most only a small effect of the number of tariffs.

Support: Table 2.4 shows that suboptimal switching/outcome rates are virtually identical with product bundling in both BDEAD and BDEA. There is a 3-5% effect in moving from all simple to all complex tariffs.[53] There is a statistically significant effect in BDEAD in moving from 4 to 24 tariffs (Wilcoxon p = 0.004), but a small one (6%) and not replicated in BDEA (2%, Wilcoxon p = 0.53).

[51] For the suboptimal switching rate, Wilcoxon p = 0.04 (all complex vs. all simple tariffs) and 0.02 (mixed vs. all simple tariffs) for WDEAD; p = 0.06 (all complex vs. all simple tariffs) and 0.12 (mixed vs. all simple tariffs) for WDEA. For the default rate Wilcoxon p = 0.58 (all complex vs. all simple tariffs) and 0.27 (mixed vs. all simple tariffs) for WDEAD; p = 0.24 (all complex vs. all simple tariffs) and 0.36 (mixed vs. all simple tariffs) for WDEA.

[52] An exercise to check the non-randomness of choices, as we did for Result 5, would yield the same outcome.

[53] For the suboptimal switching rate, Wilcoxon p = 0.04 and 0.29 (all complex vs. all simple tariffs) and 0.13 and 0.16 (mixed vs. all simple tariffs) for treatments BDEAD and BDEA respectively; for the default rate, p = 0.07 and 0.29 (all complex vs. all simple tariffs) and 0.09 and 0.16 (mixed vs. all simple tariffs) for treatments BDEAD and BDEA respectively.

2.6.2.3 Other Results

The results on earnings and on the irrelevance of the complexity of default tariffs from Experiments 1 and 2 are replicated in Experiment 3.[54] As in the earlier experiments, there is an effect by which, if the best tariff is simple in a mix of tariffs, this helped subjects, but, in the light of the power of the nudge default, in this case the suboptimal outcome rate goes down by just 6-8%.[55]

Table 2.4, panel (d), compares performance in the first half and the 2nd half of the experiment. In WDEAD, the suboptimal outcome rate in the 2nd half of the experiment (57%) remains stuck at around the level which I found in the DEAD treatment (or the D treatment from Experiment 1). This confirms the ineffectiveness of the warning nudge and, since subjects sticking to the default tariffs will have received a large number of such repeated and insistent warnings by then, provides further evidence for the implausibility of the claim that subjects simply felt that the experimenter wanted them to ignore the tariffs task. The use of the search engine was also virtually indistinguishable (20%) from that in the DEAD treatment.[56]

Since the default tariff is the best tariff in BDEAD and BDEA, it is not surprising but is nevertheless reassuring that in these two treatments subjects tend to stick to the default tariff more with time.[57]

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[54] Earnings were 55759, 65306 and 78613 points on average for subjects who stuck with a suboptimal default tariff, switched suboptimally or ended with the best tariff.

[55] Wilcoxon $p = 0.27$, 0.11, 0.05 and 0.04 for treatments WDEAD, WDEA, BDEAD and BDEA respectively.

[56] In the WDEA, BDEAD and BDEA treatments, the corresponding numbers were 39%, 12% and 18%. Note that subjects did an average of 120, 57, 159 and 41 counting tasks in the WDEAD, WDEA, BDEAD and BDEA treatments (Mann-Whitney $p = 0.04$ for WDEAD vs. WDEA and 0.03 for BDEAD vs. BDEA); the proportion of subjects that played the counting task at least once is greater in WDEAD than in WDEA ($p = 0.01$) and greater in BDEAD than in BDEA ($p < 0.001$). The divide between WDEAD and BDEAD on the one side and WDEA and BDEA on the other broadly replicates Result 8.

[57] Spearman ρ (period, default choice rate) = 0.32 ($p = 0.06$) and 0.54 ($p < 0.001$) for BDEAD and BDEA respectively.

2.7. Discussion

2.7.1 Institutional Setup

I began my introduction by noting that my goals were to identify (1) *whether*, in markets for services, consumers are likely to stick to defaults and make suboptimal choices, (2) *why* they do this and (3) *what* can be done about it. To explore these questions, I chose the U.K. electricity and gas market as the stylized setup for my experiments, as this is a mature market in which financial switching costs are already minimal and search engines enable finding best deals virtually at the click of a mouse. In this sense, it represents a good model to answer the first question in a controlled laboratory environment in which alternative explanations – such as financial switching costs or whether consumers actually made the choice that is best for them – can be ruled out.

I used stylized real world tariffs, or stylized tariffs derived from real world tariffs, and I varied the number of tariffs, their linear or non-linear structure, and whether they are bundled up or not, in ways that are realistic and which present issues for tariffs for other services and countries, as is apparent for example from Joskow's (2008) analysis of the US consumer energy markets, Lunn's (2011) review for telecoms markets, European Commission (2007) for European wide evidence on banking services, or DG Sanco's (2009) study across a range of services across European markets. Undoubtedly, further research based on other real world institutional setups would be useful.

2.7.2 Are Outcomes Suboptimal?

The answer to my first question, emerging already from Experiment 1, is that a significant fraction of consumers does tend to make suboptimal choices, either because of sticking to a default or because of switching to a suboptimal choice. In my paradigmatic DE treatment where there is a default and a search engine but only one activity available, even with just 4 tariffs about 1/3 of the choices are suboptimal, rising to over a half when there are 24 tariffs (Table 2.2). Note though that subjects stick to the default in only around 15% of cases, which does not seem to fit with real world stylized facts regarding the percentage of consumers not switching (e.g, DECC, 2012). One key reason of difference is that real world consumers may simply not pay attention to saving money from switching energy supplier: their routine activities in their everyday life are more prominent. There is not a point in time in the day, the week, the month or even the year where, as a routine, subjects are required to pay attention to the task of choosing energy supplier, as there is anyway a default energy supplier; there is no equivalent of, say, the weekly major supermarket shopping trip that a household may do every Saturday morning in order not to run out of food. Conversely, subjects come to the laboratory with an expectation that they need to pay attention and engage in a task (see Lei et al., 2001) and it is no surprise that, given the availability of a search engine, they use it to get to much better outcomes, as I would expect with real world consumers as well. The question then becomes why, in the real world, consumers do not use search engines in an equally effective way. My intuition is that, because consumers do not pay attention, they often do not get to the stage where they are faced with a search engine: the problem may be made simple but this is not enough if it is simply not in their minds.

Experiment 2 enabled me to capture more precisely inattention-to-the-task issues by adding either a not prominent task (DEAI, DEA) or a prominent (DEAD) alternative task for subjects to engage in *while retaining the search engine*. The prominence of the alternative task is used as a tool to potentially induce inattention, if much less than what can be expected in the real world.[58] I tested the alternative interpretation that the prominent alternative task may make subjects believe that experimenters want them not to engage in the tariffs task.

[58] Given the experimental constraints, the instructions need to be clear about the nature of the tariffs task and this remains one of two possible tasks available to subjects. This, together with the natural bias of experimental subjects to do things, implies that what we have is just a lower bound to the type of real world inattention problem that we are trying to model in the laboratory.

The WDEAD treatment provides direct evidence against this interpretation: this treatment differed from DEAD in that subjects sticking to the default task would be insistently and repeatedly told that they could have made more money by choosing an alternative task, and so, if anything, they should be led to believe that they should focus on the tariffs task. However, even in the second half of the experiment - after potentially 18+ such warnings -, there is no noticeable difference in consumer performance relative to the DEAD treatment (Result 12).[59]

In my DEAD treatment, as many as around 45% of choices stuck to the default, and even with just 4 tariffs over half of the choices are suboptimal. This is closely connected to the degree they pay attention to the alternative task (as per Result 8), which fits with the inattention interpretation of my manipulation. Given the presence of the search engine and the unincentivized nature of the alternative task, this is a surprising finding.

2.7.3 Why Are Outcomes Suboptimal?

I tackled the second question by aiming to identify the causal role of complexity and inattention, as well as their combination, in explaining suboptimal outcomes. I interpret inattention as inattention to a task, as opposed to inattention to some detail of a task. Of course, complexity and inattention – even interpreted as inattention to a task - may be connected concepts: an environment which is complex is one where subjects may be more put off from bothering with cognitive effort. I could control for alternative explanations of suboptimal outcomes – e.g., financial switching costs or uncertainty of prospects in switching – by removing them from the laboratory environment.

In line with research emphasizing the potential of consumer exploitation due to complexity (e.g., DG Sanco, 2009, or Carlin, 2009), I found complexity effects involving

[59] Additional evidence against this interpretation is referred to in a footnote in section 5.2.1, where there is also a further discussion of the relationship between our design and experimenter demand effects using the conceptual distinction between purely cognitive and social experimenter demand effects presented by Zizzo (2010).

product bundling, whether the tariff is linear or non-linear, and whether there are 4 or 24 tariffs. However, by neglecting the role of inattention, the role of complexity is overstated. The effect of product bundling virtually disappears once inattention is a problem (Results 9, 14). An effect of tariff complexity remains but is smaller when inattention is a problem (Results 10, 15): e.g., moving from a mixed tariffs decision environment like that currently present in UK energy markets to one with all simple tariffs – a greater simplification than may be possible or indeed desirable once one considers the potential costs that firms may incur by having less tariffs – would only reduce the suboptimal outcome rate by around 5% (DEAD and WDEAD in Tables 2.3 and 2.4). The number of tariffs effect is the most robust in affecting the choices of switchers, but might again require a complexity reduction (to 4 tariffs) beyond what may be realistic; furthermore, if a nudge default remedy is introduced to deal with the inattention problem, only a small effect remains (Result 17). Inattention is a problem because it implies that many subjects stick to the default, and this explains most of the suboptimal outcomes in treatments with an inattention problem such as DEAD and WDEAD, with default rates of the order of 40-45% and suboptimal switching rates of just around 15-20% (Tables 2.3 and 2.4). In essence, complexity matters, but economists and policy makers should pay more attention to the role of inattention[60] for tasks that do not fit in the usual household consumption routines.[61]

2.7.4 What Can be Done about Suboptimal Outcomes?

I can answer this question in terms of direct effects on consumer welfare. Obviously any policy measure would also need to be looked at in terms of its effects on competition. By

[60] Chetty et al. (2009) have noted the connection between salience and attention in terms of features of a task in an important paper on consumer responses to taxation.

[61] This last qualification is important. Our conclusions apply to the use of services with a default choice and which are not part of the regular routines of consumers in terms of what they pay attention to. Buying groceries at a (physical) supermarket would not qualify. Equally, buying a car would not qualify because, while not part of a usual routine, it does require an active choice (there is not a 'default car' which is bought automatically unless an action is taken). The more active choices are taken, the more the potential role of complexity in affecting consumer outcomes.

unpacking the psychological determinants of switching costs, however, the experimental methodology does allow to provide clear-cut messages on the potential effectiveness of measures either tackling them or not. Furthermore, the direction in which consumer welfare in my experiment is clear, unlike survey studies where, as per Coombs and Shaharudin's (2011) critique, it is not necessarily obvious whether consumers may be getting a good deal after all.

A radical and traditional paternalistic way of answering a large problem of consumer choice failure, with its direct and indirect welfare losses, is to remove the choice from the consumers by having a regulated monopolist. This is arguably one reason justifying the slowing down or halting of liberalization in consumer energy markets in USA (Joskow, 2008). This was not the approach considered in this chapter.

I am able to evaluate milder, if still traditional, paternalistic policy measures restricting the number of tariffs, the linear vs. non-linear nature of tariffs, and the bundling of tariffs. The bundling of two tariffs together does not have much effect. Restricting the number of tariffs or forcing them to be linear improves consumer outcomes. However, even restrictive regulatory measures forcing tariffs to be linear and only four – with the potentially distorting effects on competition that such restrictions may have – would still only help partially, as consumers would need to pay attention to the choice of tariffs and many of them simply would not. In this sense, the scope of Ofgem's (2012) proposal to limit the number of tariffs provided *by each firm* to 4 per fuel, meter and payment type, will be only of partial help, as the number of tariffs in *the market as a whole* is likely to remain above my experimental upper number of 24 tariffs. Equally, while online search engines are useful and their use should be encouraged by policy makers in markets where they are not so common, their usefulness is limited if consumers do not pay attention to the task and stick to the default as a result.

Experiment 3 tested two nudge remedies to try to tackle the inattention problem. The first was a warning that there exists a better tariff when a suboptimal tariff is chosen, of the kind that could be required of companies to put on the bills. However, I found no evidence of an improvement in consumer outcomes as a result of this nudge when inattention is a problem.[62] Undoubtedly the nature of the warning may matter, and future research needs to look at the effect of differently framed warnings, e.g. providing precise information on the amount of money that could have been earned if the best tariff was chosen.

The second is a 'smart nudge' which automatically identifies the best tariff and uses this as the default choice: rather than requiring consumers to be attentive, it provides them with an optimal default if they are inattentive, while leaving consumers with the freedom to choose an alternative tariff were they to so wish. This was sufficient to achieve optimal consumer outcomes 86% of the times.[63] The idea of a nudge default for consumer tariffs is in line with research on optimal defaults as developed in the context of retirement plans, such as Choi et al. (2003) and O'Neill (2007), and in my view can strike an appropriate compromise between limited regulator knowledge and need to achieve better consumer outcomes. Again, future research may be desirable, for example on the likely response of firms to the introduction of a policy based on this.

The results of my experiment can inform practical policy measures being proposed. For example, Ofgem's (2012) recent proposals require all tariffs to be of the form of a standing charge and unit price (both determined by firms), which would make them complex from my viewpoint though Ofgem thinks of them as a simplification. On the same line, the

[62] Cronqvist and Thaler (2004) discuss the Swedish experience of encouraging active decisions for retirement plans, a policy that backfired and was abandoned in the light of suboptimal choices being made as a result.

[63] While giving the freedom to make suboptimal choices in our environment may not seem to make sense, it could be justified in a number of ways: (a) it avoids a traditional paternalistic solution of forcing consumers to make a choice; (b) it enables competition among multiple firms rather than creating a regulated monopolist; (c) real world consumers are heterogeneous in preferences and may, e.g., care about non price dimension of real world tariffs (e.g., 'green' energy based), and, if they do, in a 'smart nudge' environment, they would still be able to act on this preference from switching from the automatically determined default choice.

U.K. Energy Secretary Ed Davey (BBC, 2012b) announced measures to reduce the number of tariffs, but, even with these measures, there would still be a large number of tariffs, including complex tariffs, that subjects would need to negotiate.[64] However, even if I believed that these measures could be successful in tackling complexity issues, I would still expect significant suboptimal consumer outcomes as inattentive consumers stick to suboptimal defaults.

A crucial issue from a policy viewpoint is therefore to consider how to raise consumer attention or otherwise deal with consumer inattention. David Cameron's suggestion of giving customers the cheapest tariff offered by each firm (BBC, 2012a) could be considered as a 'smart nudge' in the sense that it could be interpreted as each company being required to give their best tariff as default one to consumers. A key problem with it though is that it is not clear what incentive it would give to firms to provide lower tariffs, and in this sense it would risk soften the competition rather than making it tougher (Waddams, 2012). My 'smart nudge' proposal is more radical and would rely on the concept of market's cheapest deal that is being developed by Ofgem (2012). While obviously further research is needed to evaluate its competitive effect and company costs that may be passed on to consumers, it would create a significant incentive for firms to undercut each other insofar as this would enable them to take business from competitors.[65]

2.7.5 Methodological issues

It is worth concluding this section by taking stock of three methodological issues that my experiments raise. First, they presented different methods of identifying experimental complexity and I saw that their effect is different, with bundling of two tariffs presenting very

[64] They would imply a minimum of 4 tariffs per company and there are a number of companies in the market.
[65] Note that consumers would, of course, retain the freedom to switch to a different tariff. While this is unequivocally suboptimal in our BDEA and BDED treatments, it may not be so in the real world, where consumers have a preference for e.g. green tariffs.

few problems. In evaluating models of complexity or identifying policy recommendations, the devil may lie in the detail of *what* complexity is being affected.

Second, what I provide is a methodology to study inattention in an experimental setup, based on more or less prominent alternative tasks, which in my view may be usefully applied in entirely different settings where inattention is considered a significant issue. For example, one may argue that the first task that a charity needs to overcome in encouraging giving is to make people pay attention to what it is about; or that, in real effort experiments, one needs to take into account the distractions that alternative tasks (e.g., browsing the internet) may have (see Gómez-Miñambres et al., 2012 and Corgnet et al., 2014, for two recent other papers doing this).

Third, I do not focus in this chapter on whether inattention is rational in the sense that bounded-rational agents would optimally choose to allocate effort in that way (e.g., Sims, 2003; Gabaix, 2011; Woodford, 2012).[66] However, I note that subjects lost over 6 pounds on average (equivalent to over 9 U.S. dollars) by sticking to the default tariff, which is a significant amount by the standard of the university students that made my sample. Conversely, they knew they could not earn any money from the alternative task, and, while in the case of internet browsing (our control alternative task) it is possible to argue that subjects may do it as a pastime, this does not look equally plausible with the counting task, which has been used in experiments as a tool to measure psychologically costly real effort (e.g., Abeler et al., 2011); nor do I find any significant difference in performance depending on the nature of the alternative task (Result 6). It is hard to avoid the conclusion that subjects should have paid attention only to the tariffs task, given the sizeable incentives offered in that task and that task only. The fact that the alternative task still induced more suboptimal choices in the

[66] Cherumkin et al. (2011) try to fit Sims' (2003) rational inattention model to a dataset on risky choice. Goecke et al. (2013) find that, the more expensive information is, the less subjects collect it in a macroeconomic forecasting experiment.

way it did at least suggests that the allocation of attention might not *always* be optimal, which reinforces the policy message of the need to take inattention seriously. Obviously, further research is needed.

2.8 Conclusions

I found that, in markets for services and even in the presence of a search engine, consumers are likely to stick to defaults and achieve suboptimal outcomes. The experiment aimed to unpack two key psychological reasons why they do this – complexity (in terms of non-linearity, number and bundling of tariffs) and consumer inattention. By employing an experimental methodology, I am in a position not only to identify the causal role of different psychological dimensions, but I am also able to test the effectiveness of policies designed to improve consumer outcomes. My experiment, and tariffs, are inspired by stylized features of UK electricity and gas markets, but the lessons I draw are likely to be more general, as both underlying features (such as non-linear tariffs and the presence of defaults) and psychological mechanisms are obviously more general.

Task complexity matters. In the experiment, this is a function of product bundling, tariff complexity and of number of tariffs. However, in the presence of a default tariff and of consumer inattention, markets are affected by large amounts of consumer inertia. Similarly, providing a warning on the existence of a better tariff does not improve outcomes when inattention is a problem, though further research is needed to look at alternative warning messages that one can provide. The reason why reducing complexity of the task solves the consumer inertia problem only partially is because subjects do not pay enough attention to the task in the first place and as a result just stick to the default.

A ‘smart nudge’ policy of automatically switching default tariffs was a pragmatic and effective policy solution in our experiment to obtain better consumer outcomes. This policy solution is in the same spirit, but more radical than, the one suggested by David Cameron in the context of the U.K. energy market, and, while further research is needed, it is likely to create a greater competitive pressure than the latter. It automatically changes default tariffs to the optimal one in a given time period, and by doing so it exploits inattention-based consumer inertia to achieve better consumer outcomes while leaving consumers free to choose an alternative tariff if so they wish.

Chapter 3

3.1 Introduction

There is a substantial amount of evidence showing that in some situations cognitively constrained consumers respond suboptimally when they have to make decisions from an excessively large choice set or among complex available options. An individual's willingness to participate in a market and the satisfaction derived from chosen options decrease when the choice set is overlarge (Iyengar and Lepper, 2000; Boatwright and Nunes, 2001; Chernev, 2003; Huberman et al., 2007). In Chapter 2, I have also listed evidence indicating that consumers are reluctant to switch to a new service provider even though the tariffs they are holding are suboptimal (Jamasb and Pollitt, 2005; OFT, 2008; DG Sanco, 2010; Lunn, 2013) and even when switching occurs, consumers often fail to switch to the tariffs that are optimal for their consumption levels (Joskow, 2008; Wilson and Waddams Price, 2010; Chapter 2).

If consumers behave suboptimally when facing complex choice problems, firms in retail markets, especially those who sell homogenous goods, may have incentives to create unnecessary complexity so as to exploit consumers' cognitive limitations. The evidence from the literature described in the previous paragraph is often used by advocates of regulatory interventions to protect consumers from being exploited by retailers offering spuriously complex tariffs. To date, established policies of this kind appear to have two main dimensions - reducing the number of available options and facilitating comparisons. For example, in the energy market, as mentioned in Chapter 2, the U.K. Energy Secretary Ed Davey (BBC, 2012) announced measures to reduce the number of energy tariffs; Ofgem's (2012) proposal required firms to limit the number of energy tariffs to 4 per fuel, meter and payment type; Ofgem's (2011) proposals required all energy tariffs to have a common standard; and the energy company npower claimed that it would use "petrol style" energy tariffs if other energy companies did the same (Skynews, 2013).

So far, the efficiency of these policies is still ambiguous. Some firms argue that a large number of tariffs cater to variety in consumers' preferences. Some researchers have raised doubts about the existence of choice overload, pointing out that suboptimality in responses to choice overload may not be a robust phenomenon (Scheibehenne et al., 2010). If this is the case, resources used to develop and implement corresponding policies should be reallocated to other places.

Given the conflicting arguments, it is reasonable for us to ask whether consumers require and can be efficiently protected by these policies, and whether the established policies for dealing with choice overload problems can effectively promote competitiveness in retail markets. If the answers are positive, it is also important for one to know how and why these policies work, and whether or not more efficient policies can be proposed.

An essential element for giving answers to these questions is to understand how consumers make decisions when facing complexity that might be spurious. Knowing what heuristics the majority of consumers employ to cope with complex decision problems can potentially help policy makers propose more efficient policies to nudge cognitively constrained consumers to behave optimally. Although many scholars have explored consumers' decision-making processes in retail markets, few empirical studies focus on consumers' *shortlisting heuristics* in a specific environment, for example, an environment that involves complexity and common standards.

In the present chapter, we report an experiment which sheds light on the following question: In a retail market in which consumers face complex decision problems involving homogenous goods priced according to different standards, do consumers use shorlisting heuristics, and if so, what form do these heuristics take? Our experiment also allows us to compare the effectiveness of different heuristics for cognitively constrained consumers.

Section 2 reviews previous research on shortlisting heuristics, introducing the concept of common standards and mainly describing three different heuristics by which consumers might use common standards to simplify decisions problems. Section 3 and 4 describe the experimental design and results respectively. Section 5 provides the discussion and conclusion.

3.2 Literature review

3.2.1 Heuristics: previous studies

One prevalent view about how consumers make decisions in a complex environment is that they use simplifying heuristics (Kahneman and Tversky, 1974, 1979; Todd and Gigerenzer, 2000). There is research suggesting that, given a large choice set with complex options, people use shortlisting heuristics (also called consideration-set heuristics or two-stage choice procedures). A person who uses a shortlisting heuristic first constructs a subset (shortlist) of the whole choice set and then evaluates the options in this subset to make a final decision (Eliaz et al., 2011; Hauser, 2010; Eliaz and Spiegler, 2011; Hauser et al., 2010; Manzini and Mariotti, 2007). One interesting question to ask is what rules people use to construct the shortlist. Hauser (2010) introduced several potential rules including "conjunctive", "disjunctive", "subset conjunctive", "lexicographic", "elimination by aspects", "disjunctions of conjunctions" and "compensatory" for shortlisting products with several attribute dimensions. Eliaz, Richter, and Rubinstein (2011) analysed theoretically three possible rules – "the top two", "the two extremes" and "the top and the top" – for constructing a shortlist of two "finalists". However, neither research deals explicitly with the issue of how consumers use shortlisting heuristics to choose among a large set of complex prices/tariffs when common standards exist.

3.2.2 Common standards

Tariffs for products in retail markets, especially homogenous products, do not always have the same structure. In the energy market some energy tariffs are linear – for example, 10.53p/kWh. Some non-linear tariffs have two tiers with a celling – for example, Tier 1 has a unit price of 4.33p/kwh up to a ceiling of 720 kWh, and then Tier 2 has a unit price of 13.76p/kWh. Other non-linear tariffs have a daily standing charge – for example, 26.41p/day plus 6.43p/kWh. In the travel market, some airlines (e.g. KLM) quote an inclusive price for flights, while others (e.g. Ryanair and easyJet) quote an original price plus add-ons such as taxes, fees for extra luggage, seat choice and insurance. In the entertainment market, cinemas and opera houses usually give their consumers different basic prices with, sometimes more than one, different discount rates. Unsurprisingly, it is hard for cognitively constrained consumers to minimize their expenditure when facing many tariffs with different add-ons or discounts. However, consumers can easily compare tariffs with the same numerical values of add-ons or discounts even if they are complex, as consumers can cancel out the common add-ons and discounts and only rank the basic prices (Kahneman and Tversky, 1979, pp.274-275).

In the present paper we define firms as using *common standards* for their tariffs when those tariffs have a common structure and use the same numerical values of add-ons or discounts. For example, airline tickets prices “£710+ £60 luggage fee + £38 insurance” and “£680+ £60 luggage fee + £38 insurance” have a common standard because both prices have the same numerical values of luggage and insurance fees; but cinema ticket prices “£23 with 5% discount” (i.e. £23*0.95) and “£25 with 9% discount” (i.e. £25*0.91) have *individuated standards* because of the different numerical values of discounts.

When consumers have to choose from a large number of tariffs, some but not all of which use common standards, what heuristics do they use to simplify the decision problem? Gaudeul and Sugden (2012) defined three psychological rules – “largest common standard”, “dominance editing” and "signal first” which might potentially be used by consumers to tackle such problems.

3.2.3 Some possible shortlisting heuristics

A consumer who uses the *largest common standard* (LCS) rule ignores individuated standard tariffs and shortlists only common standard tariffs for further evaluation. When there is more than one common standard, only the tariffs with the common standard used by the largest number of firms are shortlisted. For example, suppose there are three tariffs from firms A, B and C. The tariffs of firms A and B have a common standard, while that of firm C has an individuated standard. The final prices of firms B and C are equal, and less than that of firm A. For cognitively constrained consumers, tariffs A and B are easy to compare, but C is hard to compare with either B or A. According to Gaudeul and Sugden (2012), when comparing offer prices, consumers normally employ two evaluation systems. The *ranking system* ranks offers that use a common standard without working out the absolute value of the offers' final prices. The *calculating system* generates absolute values, but if consumers are cognitively constrained, absolute values contain errors. The calculating system is modelled by treating the consumer’s perception of the final price as a noisy ‘signal’ of true value.

In this case, if a consumer uses the LCS rule, she will first eliminate tariff C and construct a shortlist containing tariffs A and B, so the probabilities of choosing tariffs A, B and C are 0, 1 and 0 respectively. If a consumer follows the LCS rule, she will only use the ranking system. Crosetto and Gaudeul (2012) report experimental evidence of the use of the LCS rule. In their experiment, subjects were allocated with a budget and asked to purchase grey paint to cover a fixed square area. The prices of alternative paint products were described in terms of the cost of covering areas of various shapes and sizes; in some choice problems, two or more products were priced in terms of areas with the same shape and size, thus creating a common standard. There was a time constraint in each task of their experiment. Crosetto and Gaudeul (2012) found that when other factors were held constant, subjects were more likely to choose common standard products.

A consumer who follows the *Dominance editing* (DE) rule constructs a shortlist by eliminating tariffs that can be revealed to be suboptimal by means of common standard comparisons. The final decision has to be made by comparing the shortlisted tariffs, all of which use different standards. In the preceding example, A (which is dominated by B) is eliminated at the first stage. Assuming that errors in the second stage are unbiased, B and C (which have the same final price) are equally likely to be chosen, and so the probabilities of choosing tariffs A, B and C are 0, 0.5 and 0.5 respectively. If a consumer follows the DE rule, the order for her using the two systems is firstly the ranking system and secondly the calculating system. The DE rule is experimentally identified by Kahneman and Tversky (1979) in the context of choice between lotteries.

A consumer who uses the *signal first* (SF) rule provisionally selects the tariff with the lowest "price signal", that is, the tariff she perceives to be optimal after using some potentially erroneous method to estimate the actual price implied by each tariff. She then shortlists tariffs that use the same standard as the provisionally selected tariff. Because comparisons within this shortlist are easy, the best tariff on the shortlist is selected. Applied to the preceding example, if errors in the first stage are unbiased and non-zero, the probabilities that tariffs A, B and C are provisionally perceived to be optimal are $1 – 2x$, x and x respectively, where $1/3 < x \leq 1/2$. Thus the final choice probabilities are 0, $1 – x$ and x. If a consumer follows the SF rule, the order for her using the two systems is: first the calculating system and second the ranking system.

Note that first, for a consumer who is not cognitively constrained (i.e. for whom signals are always equal to true values), the DE and SF rules produce the optimal choice, while the LCS heuristic is suboptimal; and second, the DE and SF rules differ by using the two systems in different orders. Gaudeul and Sugden (2012) argued that, if consumers use the LCS heuristic, complex tariffs with individuated standards would not exist in the long run, unless firms were able to collude. But if consumers use the DE or SF heuristic, they will, sooner or later, inspect the individuated standard tariffs, trying to calculate the absolute values of these tariffs. If an individuated standard tariff has a lower price signal than the optimal common standard tariff, then people use DE or SF heuristic might choose it. For cognitively unconstrained consumers, the price signals are accurate, and so firms which use individuated standard tariffs cannot exploit such consumers. But for cognitively constrained consumers, price signals have errors. Thus, profit-maximising firms may have incentives to set high prices and use individuated standards with the aim of selling to cognitively constrained consumers.

These three heuristics have contrasting implications for firms' behaviour and give different suggestions to policy makers. Gaudeul and Sugden (2012) provide a theoretical prediction about which heuristic consumers will use in the long run. They show that, given the assumption that some consumers employ the LCS heuristic in retail markets at the very beginning, a situation in which all firms use individuated standards is an unstable equilibrium. If consumers learn to use heuristics that give better results and firms follow the principle of profit maximisation, based on evolutionary game theory, consumers who do not employ the LCS heuristic at the beginning will employ the LCS heuristic in the long run.

It is clear that the conclusions are based on one key assumption, that is, "there always exist at least some consumers who employ the LCS heuristic in retail markets". According to Gaudeul and Sugden (2012), the first reason why their model has this assumption is because

LCS is one possible explanation of the asymmetric dominance effect. The second reason is because using LCS heuristic is psychologically plausible. For example, when considering an employer who wants to employ one employee from some candidates, if all candidates except one have at least adequate educational qualifications of some standard kind, the candidate who has nonstandard and not obviously superior qualifications will be more likely to be disadvantaged in the shortlisting process. The third reason is because common standard heuristics are the best rules of dealing with some classical decision problems, such as the "filling station problem"[67]. However, Gaudeul and Sugden do not give any empirical evidence or relevant references showing that, in the short run, there exist some consumers who employ the LCS heuristic in retail markets.

Although many researchers have tried to explore what heuristics consumers employ in retail markets, there is a limited amount of empirical research focusing on studying which shortlisting heuristic consumers will employ for minimising their expenditures when facing decision problems involving complexity and common standards, and whether or not the employed heuristic is efficient. This chapter tries to give answers to these questions.

3.3 Experimental design

Gaudeul and Sugden's (2012) model has five features: (1) consumers choose between offers; (2) the only dimension relevant for choice is price; (3) offers have a "standard" for expressing price; (4) if the offers are in the same standard, comparison is easy; (5) if the offers are in different standards, comparison is difficult.

For investigating whether or not consumers will employ a common standard heuristic and if so, which of the three preceding shortlisting heuristics consumers will employ, my experiment implements features 1 to 5 of Gaudeul and Sugden's (2012) model, by using

[67] More details of the filling station problem can be found at Gaudeul and Sugden (2012), p. 213.

complex pricing schemes with common elements. For example, offer prices can be formatted as the following:

price of offer A: £10.60 * 95%

price of offer B: £9.50 * 95%

price of offer C: £12.20 * 86%

where offers A and B have a common standard and offer C has an individuated standard. In this design, I am able to find out how participants react to offers with different standards.

3.3.1 Overview

The experiment had a within subject design. Each participant was required to complete 10 different tasks in a randomized order. In each task, participants were given an endowment and told that they had to buy one out of twenty-four offers; their earnings from the task would be equal to the endowment minus the price of the chosen offer. At the end of the experiment the computer would randomly pick one of the ten tasks for real. The participant's payment depended on the offer she selected in that task. All prices were lower than the endowment. Thus, participants had an incentive to choose the offer with the lowest price.

In each task, the details of the twenty-four offers were not immediately visible to participants, but were revealed in response to participants' mouse clicks. This mechanism will be explained later. Each task comprised two pages - the *marketing page* and the *shopping basket page*. Examples of these two pages are shown in appendix J. Initially, participants saw the marketing page.

In the marketing page there were twenty-four coloured boxes representing twenty-four offers with different offer prices, coded from OFFER A to OFFER X. Offers with the same price structure (explained below) had the same colour. No details of the offers apart from the colours and the letter codes were visible. The endowment and the price details of the twenty-four offers were described in "game points", with a randomly generated exchange rate between game points and UK pounds (more details of the exchange rate will be given in section 3.2). In every task, the endowment was £32 and the final money prices of the 24 offers, prior to rounding, ranged from £20.270 to £28.895, in increments of £0.375. However, participants were not told that the distribution of prices was the same in all tasks. Because the exchange rate was randomly generated, independently for each task, and because price structures and price details differed between tasks, this feature of the design was not obvious to participants. The allocation of offers to the boxes A, .., X was randomized. The price structures and price details will be explained in the "*Tasks*" section.

In the marketing page participants were able to do four actions: (1) They could single click on an offer and see the *price structure* of that offer immediately. For example, they would see "price structure 1" showing on the bottom of the screen after they clicked an offer box. (2) They could click more than once on an offer and see the *price detail* of that offer after a 3 seconds delay. For example, they would see "*Original price = 4567 points; Final price = Original price * 36%"* showing below "*price structure 1*". (Actions (1) and (2) could be done for only one offer at any time, i.e. if the participant clicked on a new offer, the price structure and/or price detail of the previous offer disappeared.) (3) They could move an offer into the shopping basket by clicking the "Move into the shopping basket" button. (4) They could go to the shopping basket page with 3 seconds delay by clicking the "View the shopping basket" button.

In the shopping basket page, participants could see the price detail(s) of all the offer(s) that had been moved into the basket, displayed in the middle of the screen. Thus, price comparisons (particularly for offers with complex price structures) could be made much more easily in the shopping basket page than in the marketing page, where the participant could view the price details of only one offer at a time. Because the purpose of the experiment was to investigate shortlisting, it was important that participants' shortlists could be observed. The properties of the shopping basket were designed to encourage participants to use it to store shortlisted offers.

In the shopping basket page, participants were able to do three actions: they could move an offer out of the shopping basket; they could go back to the marketing page to continue shopping by clicking the "continue shopping" button; or they could make a final decision, deciding which of the offers in the basket to buy. There was no time delay for these three actions. The capacity of the shopping basket was nine, and so participants could not put more than nine offers into the shopping basket at the same time. If the shopping basket was full but participants still wanted to put some new offers into it, they first had to move some old offers out of the shopping basket, and then go back to the marketing page and put some new offers in.

There are two reasons why a 3 seconds time delay was added both on clicking a box more than once for seeing price details in the marketing page and on clicking the "View the shopping basket" button for going to the shopping basket page. The first reason is that it replicates the fact that in the real world, there will always be a time delay while opening a new website for seeing price details of a specific product or going to a shopping basket page for checking out. The time delay is normally caused by the limited computer and internet speed. The second reason is that the time delay provides an additional incentive for participants to use the shopping basket when comparing prices. For example, comparing the price details of 5 offers would involve 5*3=15 seconds of delay if the offers were inspected on the marketing page, but only 3 seconds of delay if the offers were moved to the shopping basket and then inspected in the shopping basket page.

3.3.2 Tasks

The 10 tasks used in the experiment contain two main dimensions - the *offer-type dimension* and the *common standard dimension*.

The offer-type dimension

The offer-type dimension comprises four offer types: P1, P1*D1, P1*D1*D2 and P1*D1+P2.

In the **P1 task**, all offer price details are framed as a single price such as

Final price = x points

where x is a positive round number. In this task, all 24 offers have the same price structure.

In the **P1*D1 tasks**, the price details are described as an original price with a discount:

*Original price = x points; Final price = Original price * y%*[68]

where x is a positive round number and y is a round number in the interval $0<y<100$. In these tasks I say that offers with the same numeric value of y have the same price structure.

In the **P1*D1*D2 tasks**, the price details are described as an original price with two discounts:

*Original price = x points; Final price = Original price * y_1% * y_2%*

where x is a positive round number and y_1 and y_2 are round numbers in the intervals $0<y_1<100$ and $0<y_2<100$. In these tasks I say that offers with the same values of both y_1 and y_2 have the same price structure.

In the **P1*D1+P2 tasks**, the price details are described as an original price with a discount plus an add-on price:

[68] In the experiment we used X instead of * for the multiplication sign in all price details because some participants do not treat * as a multiplication sign.

*Original price = x points; Final price = Original price * y% + z points*

where x and z are positive round numbers and y is a round number in the interval $0<y<100$. In these tasks I say that offers with the same values of both y and z have the same price structure.

The values of x, y (in the P1*D1 and P1*D1*D2 tasks), z (in the P1*D1+P2 tasks) and y_1 and y_2 (in the P1*D1*D2 tasks) were set so that, apart from the effects of rounding, the 24 final prices, converted into UK pounds at the relevant exchange rate, were always £20.270, £20.645, …. , £28.895. Subject to this constraint, to the constraints $x, z > 0$ and $0 < y, y_1, y_2 < 100$, and to the constraints implied by the common standard dimension (see below), the values were randomized independently for each offer and for each participant.

These four offer types have different complexity levels: offer type P1 is the simplest and offer types P1*D1*D2 and P1*D1+P2 are the hardest.

The common standard dimension

If two or more offers in a given task have the same price structure, I call these offers *common standard offers*. If an offer has a different price structure from all the other offers, I call this offer an *individuated standard offer*. The common standard is the key dimension I vary in the experiment across different tasks. It comprises three common standard types. In the first type of task – *All Common Standard (AC)* – all of the offers have one common standard. In the second type – *Part Common Standard (PC)* – 8 of the 24 offers have a single common standard, and the other 16 have individuated standards. The final money prices of the common standard offers were randomized, independently for each participant, subject to the constraint that the 24 final prices were always £20.270, £20.645, …. , £28.895. Thus, whether or not an offer was a common standard offer provided no information about its final price. In the third type – *No Common Standard (NC)* – all offers had individuated standards.

The 10 tasks used in the experiment are the P1 task plus the 3 offer types crossed with the 3 common standard types. (Task P1 is also an AC task but this offer type cannot be extended to PC and NC tasks. Because offer prices in a P1 task do not contain any add-ons or discounts, there is no space for one to manipulate these offers into individuated standard offers.) Table 3.1 shows the key features of these 10 tasks.

Tasks	Number of offers	Offer types	Common standard types	Number of price structures
1	24	P	AC	1
2	24	P1*D1	AC	1
3	24	P1*D1	PC	17
4	24	P1*D1	NC	24
5	24	P1*D1*D2	AC	1
6	24	P1*D1*D2	PC	17
7	24	P1*D1*D2	NC	24
8	24	P1*D1+P2	AC	1
9	24	P1*D1+P2	PC	17
10	24	P1*D1+P2	NC	24

Table 3.1: Features of the tasks

An example of offers with the same price structure (a common standard) in P1*D1*P2 tasks is the following:

*Original price = 265 points; Final price = Original price * 9% +43 points*

*Original price = 168 points; Final price = Original price * 9% +43 points*

*Original price = 437 points; Final price = Original price * 9% +43 points*

......

*Original price = 563 points; Final price = Original price * 9% +43 points*

An example of offers with different price structures (individuated standards) in P1*D1+P2 tasks is the following

*Original price = 263 points; Final price = Original price * 17% +45 points*

*Original price = 435 points; Final price = Original price * 68% +170 points*

*Original price = 699 points; Final price = Original price * 6% +98 points*

......

*Original price = 158 points; Final price = Original price * 93% +16 points*

3.3.3 Implementation of the experiment

When coming into the lab, participants were asked to put their belongings on a desk in order to prevent them from taking any pens, mobile phones or other electronic devices with calculating applications. In each task there was a randomly generated exchange rate: x points=£1 (where x is a round number in the interval $10 \leq x \leq 100$). The exchange rates were randomly generated because, for reasons of experimental control, the 24 offers' final prices (in £) in all tasks were always the same. If the exchange rate in each task were also the same, participants might ignore the complexity, fast learning the value of the cheapest offer in game points, and then finding the cheapest offer in each task without the need to compare all the offers. A randomly generated exchange rate can reduce the effect of this problem by allocating different game points to offers with the same final price. Each participant was endowed with some game points in each task, which at the exchange rate would always be worth £32.

Before starting the formal experiment, the experimenter read out the experimental instructions and activated all participants' computer screens to let them do a practice task. The practice task was similar to the PC tasks but with a different offer type[69]. Participants

[69] The offer type used in the practice task is Original price = x points; Final price = Original price * y% with y>100

had to follow the experimenter's instructions, doing the practice task step by step. After finishing the practice task, participants needed to answer questionnaires to make sure that they understood how to do the experiment. If the questions were answered correctly, the formal experiment would start.

3.4 Predictions

In the present experimental design, if an individual does not make mistakes and follows the LCS or DE heuristic, in AC tasks, she will only use the ranking system, cancelling the same add-ons or discounts in the price details and ranking offers by their original prices (compare the editing operation described by Kahneman and Tversky, 1979 pp.274-275). In the case of employing the SF heuristic, she will first use the calculating system, trying to work out the absolute values of the offers, and then use the ranking system to find out the best offer. Also, the individual will inspect all twenty-four offers. In this chapter, the term "inspect" is used when participants try to see an offer's price details either by clicking the offer box more than once, or by moving the offer into the shopping basket so as to see the offer's price details there. In AC tasks, if an individual employs one of the three heuristics above, once she puts the optimal common standard offer into the shopping basket, it will stay in the shopping basket for comparing with other suboptimal common standard offers. The optimal common standard offer will finally be chosen.

In PC tasks, if an individual employs the LCS heuristic, she will use the ranking system and inspect only the eight common standard offers. The sixteen individuated standard offers will not be inspected at all, as the participant eliminates them in the first place. However, if the individual employs the DE heuristic, she will first use the ranking system and then use the calculating system. Accordingly, her inspections will also follow an evaluation order, which is, she first inspects the eight common standard offers and eliminates suboptimal ones, then (subjectively) chooses the optimal offer from among the seventeen non-eliminated offers. The individual will always inspect all twenty-four offers. Once the cheapest common standard offer is put into the shopping basket, it will stay in the shopping basket and be chosen at the end only if the participant does not find lower price signals from among individuated standard offers. If they find one, the offer with the lowest price signal will be chosen and it is possible that the cheapest common standard offer may be moved out of the shopping basket before the participant makes the final decision. If an individual employs the "SF" heuristic, she will first use the calculating system and then use the ranking system. Accordingly she will inspect all 24 offers in a inspecting sequence that first inspects offers without considering standards, provisionally selecting the tariff with the lowest price signal, and then shortlists tariffs which use the same standard as the provisionally selected tariff. In NC tasks, although Gaudeul and Sugden (2012) do not explicitly discuss how an individual will behave if she employs the LCS heuristic in an environment where all offers have individuated standards, in the present chapter, I assume that the individual will inspect all twenty-four offers.[70] If an individual employs the DE or SF heuristic, she will also inspect all twenty-four offers. In NC tasks, the individual will only use the calculating system.

Gaudeul and Sugden's model represents a world where consumers do not have search costs but many of them are cognitively constrained, which means that the price signals they work out have errors. However, some classic search and choice models (Diamond, 1971; Salop and Stiglitz, 1977) have different assumptions. In these models, consumers have search costs but do not have cognitive limitations. A rational consumer can precisely work out an optimal *reservation price* that depends on her search cost. A consumer's reservation price is positively related to her search cost. She will stop searching once she finds an offer with a price lower than or equal to her reservation price.

[70] We shall see in the result section that, a different definition will not affect our conclusions.

Each type of model has a realistic element in terms of consumers' behaviour in the real world. In the present chapter, I assume that some consumers are cognitively constrained and that all consumers have search costs. In the current experiment, search costs can be defined as the effort and time used by a participant to rank offers and work out the absolute values of offer prices. When ranking common standard offers, a participant's search cost is low, so her reservation price will be low as well. Thus, the average number of offers inspected will be higher in AC tasks than in NC tasks.

Apart from the heuristics discussed above, a participant might employ some cruder heuristic such as a *random selection* heuristic, that is, a participant inspects a few (or only one) offers and randomly chooses one of these offers without trying to work out its final price or comparing it to other offers.

From the above discussions, we can see that different types of heuristics can be clearly differentiated in PC tasks. Table 3.2 shows the differences.

Heuristics	Number of offers inspected	Inspecting sequence	The status of the offers
Largest common standard (LCS)	8	Only inspect common standard offers.	1. Sub-optimal common standard offers will be moved out of the shopping basket very quickly. 2. Once the optimal common standard offer is put into the shopping basket, it stays until the end of the task. 3. The optimal common standard offer will finally be chosen.
Dominance editing (DE)	24	First inspect common standard offers and then inspect individuated standard offers.	1. Sub-optimal common standard offers will be moved out of the shopping basket very quickly. 2. Once the best common standard offer is put into the shopping basket it will stay until a participant finds (if any) a cheaper individuated standard offer (there may be errors if the participant is cognitively constrained)
Signal First (SF)	24	First inspect all offers without considering their standards. If the provisionally chosen offer is a common standard offer, then second, inspect other common standard offers, comparing them with the one provisionally chosen. If the provisionally chosen offer is not a common standard offer, stop inspecting.	In the first stage, provisionally inspect all offers and pick the one that is relatively good (not necessarily be the best because of errors). If the second stage occurs (i.e. the picked offer is an common standard offer), when inspecting common standard offers, 1. Sub-optimal common standard offers will be moved out of the shopping basket very quickly. 2. Once the best common standard offer is put into the shopping basket, it stays until the end of the task. 3. The optimal common standard offer will finally be chosen.
Classic search and choose models	Less than 24	Inspect offers without considering their standards.	Participants will stop searching once they find an offer's price that is lower than their reservation price. At the end of the task, they will tend to choose the best offer in the shopping basket.
Random selection heuristics	A few	Inspect offers without considering their standards.	Participants might inspect any number of offers, and randomly choose one of them in the shopping basket.

Table 3.2: Predictions of participants' behaviour in PC tasks

By using a range of different complexity levels for price details (i.e. P1, P1*D1, P1*D1*D2 and P1*D1+P2), we make it more likely that each participant will face some tasks in which working out the absolute values of offer prices is perceived as easy and some in which it is perceived as difficult. This design feature also allows us to investigate whether the heuristics that participants use are affected by the complexity of the offer type.

There are two reasons why AC and NC tasks are also included in the present experiment. The first reason is that, although it is plausible to assume that it is easy for consumers to make comparisons between common standard offers and hard for them to compare individuated standard offers, there is no clear empirical evidence showing this. Having AC and NC tasks can help us check the plausibility of this assumption by seeing whether or not participants spend less time and earn more in AC tasks than in NC tasks. If they do, then this implies that common standard offers are easier for participants to compare than individuated standard offers. The second reason is that Gaudeul and Sugden (2012) predict that, when comparing to the market in which all offers are individuated standard offers, consumers can reap more benefits from the market in which some offers have a common standard. Comparing time used and final earnings in PC and NC tasks helps us test this prediction.

3.5 Results

The experiment was conducted at the Centre for Behavioural and Experimental Social Science (CBESS) Laboratory at the University of East Anglia in the summer of 2013. Participants were recruited using a campus-wide online system. There were 171 participants attending the experiment. Most of the participants were students from a wide range of academic disciplines, ages ranging from 18 to 65. The experiment lasted approximately 65 minutes with an average payment of £10.76 per person. The maximum payment was £11.73 and the minimum payment was £3.10.

3.5.1 Final earnings and time used in each task

Table 3.3 reports means and standard deviations of the time used and final earnings in each task, averaged across participants.

Tasks	Time used (Sec.)	Earnings
AC tasks		
P1	157.9 [124.7] (103.9)	11.40 [11.73] (1.36)
P1*D1	177.3 [133.1] (145.4)	11.26 [11.73] (1.64)
P1*D1*D2	183.1 [138.3] (121.1)	11.05 [11.73] (2.06)
P1*D1+P2	187.2 [142.8] (131.5)	11.23 [11.73] (1.71)
Average	176.4 [133.9] (126.6)	11.23 [11.73] (1.71)
PC tasks		
P1*D1	341.9 [289.4] (255.8)	10.41 [11.355] (1.94)
P1*D1*D2	376.1 [363.5] (242.5)	9.43 [10.23] (2.44)
P1*D1+P2	353.6 [303.8] (244.8)	10.04 [10.98] (2.04)
Average	357.2 [312.2] (247.7)	9.96 [10.98] (2.19)
NC tasks		
P1*D1	343.6 [304.5] (226.5)	10.11 [10.98] (2.00)
P1*D1*D2	398.2 [355.4] (296.1)	9.26 [10.23] (2.52)
P1*D1+P2	362.9 [327.5] (225.2)	10.11 [10.98] (2.07)
Average	368.2 [321.9] (252.0)	9.83 [10.605] (2.24)

Table 3.3: Means medians and standard deviations of the time used and final earnings in each task

Note: The first number is the mean; the second number in the square brace is the median; the number under the mean in the parenthesis is the standard deviation. There are 171 observations in each task.

The average time used in AC tasks (176.4 seconds) is approximately 50% shorter than the average time used in NC (368.2 seconds) tasks (z = -9.96, p < 0.001)[71]. The average earnings in AC tasks (£11.23) are significantly higher than the average earnings in NC (£9.83) tasks (z = 9.85, p < 0.001). These results indicate that common standards make decision-making easier and better.

However, although the mean value of time used as well as final earnings in PC tasks (time used: 357.2 seconds; final earnings: £9.96) are slightly lower than those in NC tasks (time used: 368.2 seconds; final earnings: £9.83), the differences are not significantly different (z = 1.15, p = 0.25 for time used; z = -0.32, p = 0.75 for final earnings).

Let us now consider how these results shed light on what heuristics participants employed.

First, let us see how the employment of each heuristic would impact on the time spent on PC and NC tasks.

If participants employed the LCS heuristic, we would observe that the average time used in PC tasks was much lower than that in NC tasks. This is because, when employing the LCS heuristic, in PC tasks, participants only need to compare 8 common standard offers but in NC tasks, participants would compare all 24 offers.

If participants employed the DE heuristic, we would observe that the average time used in PC tasks was slightly lower than that in NC tasks. This is because, although both PC and NC tasks have 16 individuated standard offers, for a participant, the time spent on ranking 8 common standard offers in PC tasks should be shorter than time spent on working out the absolute values and then ranking 8 individuated offers in NC tasks.

[71] Throughout this chapter, unless otherwise stated, all within-subject tests are Wilcoxon signed rank tests, while for between-subjects comparisons we use Mann-Whitney tests. All p-values are 2-sided.

If participants employed the SF heuristic, we would observe that the average time used in PC tasks was slightly higher than that in NC tasks. This is because in PC tasks, after working out the absolute values (price signals) of all 24 offers, if the offer with the lowest price signal is a common standard offer, the participant still needs to compare the offer with other common standard offers. In NC tasks, as there is no common standard offer, the participant does not engage in the common standard offers comparison.

If participants employed a common standard unrelated heuristic[72], we would observe that the average time used in PC tasks was the same as in NC tasks.

Since we do not observe the very sharp difference between PC and NC tasks that would be generated if the LCS heuristic was widely used, it is clear that the results on time used in each task provide preliminary evidence against the possibility that participants employed this heuristic. However, because the DE and SF heuristics have weaker implications for time used, it is too early to eliminate the possibility that participants may have employed these heuristics.

Next, let us see how the employment of each heuristic has different impacts on the average final earnings.

As we do not know how cognitively constrained the participants were, it is unclear whether, if participants employed the LCS or DE heuristic, final earnings would be higher or lower in PC tasks than in NC tasks. However, if participants who were cognitively constrained used the SF heuristic, final earnings would be higher in PC tasks than in NC tasks. If participants employed a common standard unrelated heuristic, we would observe that the average final earnings in PC tasks were the same as that in NC tasks. The results of the final

[72] A heuristic is a common-standard-related heuristic if it involves consideration of offers' standards. In the present chapter, it includes the LCS, DE and SF heuristics. A heuristic is a common standard unrelated heuristic if it does not involve any consideration of offers' standards.

earnings in each task do not give us a clear answer to the question about which heuristic participants employed.

Result 1: Relative to individuated standard offers, if all offers have a single common standard, decision making is made easier and better, but if some but not all offers have a common standard, this effect is weak or non-existent. The data on time used in each task provide preliminary evidence against the possibility that participants employed the LCS heuristic.

3.5.2 Total number of offers participants inspected in each task

Although providing preliminary evidence against the usage of LCS heuristic, the preceding results do not give a clear answer to our key questions about whether or not the majority of participants employed a common-standard-related heuristic and if they did, which of the three common standard heuristics mentioned in the previous section they employed. For answering these questions, it is useful to consider the total number of offers participants inspected in each task.

Remember that there were two approaches for participants to inspect offers' price details. Participants could inspect an offer's price details in the marketing page by clicking the box more than once and waiting for three seconds. I call this way of inspection *approach 1*. They could also inspect price details in the shopping basket page by putting the offer into the shopping basket in the marketing page. I call this way of inspection *approach 2*. ClickTwice denotes the number of non-repeated offers' price details inspected by approach 1 and PutInBasket denotes the number of non-repeated offers' price details inspected by approach 2. Notice that, although ClickTwice and PutInBasket account only for non-repeated offers' price details inspected by different approaches, the sum of ClickTwice and PutInBasket may be still higher than the total number of offers (twenty-four). This is because one participant may inspect an offer's price detail by using both approaches.

Table 3.4 reports the means and standard deviations of ClickTwice and PutInBasket, aggregated across participants, and broken down according to offers' standards.

Tasks	ClickTwice of common standard offers	PutInBasket of common standard offers	TOTAL common standard offers	ClickTwice of individuated standard offers	PutInBasket of individuated standard offers	TOTAL individuated standard offers
AC tasks						
P1	11.39 [7] (11.09)	13.39 [9] (9.60)	24.78 [25] (5.29)			
P1*D1	10.95 [4] (11.17)	13.90 [10] (9.48)	24.86 [25] (5.90)			
P1*D1*D2	10.92 [5] (11.02)	13.82 [9] (9.60)	24.74 [25] (5.22)			
P1*D1+P2	11.02 [6] (11.07)	13.90 [11] (9.36)	24.92 [25] (5.88)			
Average	11.07 [6] (11.07)	13.76 [10] (9.49)	24.83 [25] (5.57)			
PC tasks						
P1*D1	3.34 [1] (3.73)	4.74 [5] (3.28)	8.09 [8] (2.24)	6.09 [1] (7.16)	9.49 [9] (5.55)	15.58 [16] (5.21)
P1*D1*D2	3.33 [1] (3.60)	4.48 [4] (3.36)	7.82 [8] (2.45)	5.90 [2] (6.76)	8.67 [8] (5.47)	14.58 [16] (6.03)
P1*D1+P2	3.44 [1] (3.68)	4.46 [4] (3.37)	7.89 [8] (2.57)	6.03 [2] (6.90)	9.05 [8] (5.64)	15.08 [16] (5.64)
Average	3.37 [1] (3.66)	4.56 [4] (3.33)	7.93 [8] (2.42)	6.01 [1] (6.93)	9.07 [8] (5.55)	15.08 [16] (5.64)
NC tasks						
P1*D1				9.25 [2] (10.54)	14.03 [11] (8.25)	23.29 [24] (8.17)
P1*D1*D2				8.73 [2] (10.12)	12.68 [10] (7.68)	21.41 [24] (8.36)
P1*D1+P2				9.39 [2] (10.56)	13.58 [10] (8.40)	22.97 [24] (8.19)
Average				9.12 [2] (10.39)	13.43 [10] (8.12)	22.56 [24] (8.27)

Table 3.4: Means medians and standard deviations of ClickTwice and PutInBasket of offers having a common standard or individuated standards

Note: The first number is the mean; the second number in the square brace is the median; the number under the mean in the parenthesis is the standard deviation. There are 171 observations in each task.

From the table, it can be seen that some participants inspected nearly half of the offers' price details by using approach 1, although this approach is comparatively time consuming. The results also imply that some participants inspected some offers' price details by using both inspecting approaches. This is because in the majority of the tasks, the sum of ClickTwice and PutInBasket is higher than twenty-four.

Table 3.5 reports the number of participants who inspected *x* offers (*x* ranges from 0 to 24) and Table 3.6 reports the means, medians, percentages and standard deviations of number of non-repeated offers inspected by participants (InspectionNum) across tasks[73].

[73] Notice that there is a difference between InspectionNum and the sum of ClickTwice and PutInBasket. The difference is that in a situation where a participant inspected the same offer's price details by using both approaches, InspectionNum will increase by 1 but the sum of ClickTwice and PutInBasket will increase by 2 (1 for each approach). A participant's InspectionNum will never be higher than twenty-four, but the sum of ClickTwice and PutInBasket might be.

Number of participants inspecting offers\tasks	AC (P1)	AC (P1*D1)	PC (P1*D1)	NC (P1*D1)	AC (P1*D1*D2)	PC (P1*D1*D2)	NC (P1*D1*D2)	AC (P1*D1+P2)	PC (P1*D1+P2)	NC (P1*D1+P2)
Number of participants inspecting 1 offer	1	0	0	1	1	3	2	1	2	2
Number of participants inspecting 2 offers	1	2	1	0	0	0	0	2	0	1
Number of participants inspecting 3 offers	1	0	0	2	0	0	1	0	1	1
Number of participants inspecting 4 offers	0	1	2	0	0	0	1	0	3	0
Number of participants inspecting 5 offers	1	1	0	2	1	0	0	0	0	1
Number of participants inspecting 6 offers	0	3	1	2	3	3	7	2	2	3
Number of participants inspecting 7 offers	2	0	2	2	1	1	2	1	1	1
Number of participants inspecting 8 offers	2	1	3	2	1	4	3	1	1	1
Number of participants inspecting 9 offers	2	4	3	8	3	7	13	5	2	10
Number of participants inspecting 10 offers	1	1	2	8	1	5	5	0	4	6
Number of participants inspecting 11 offers	0	0	3	3	0	2	4	0	3	5
Number of participants inspecting 12 offers	0	0	1	3	0	3	4	2	3	1
Number of participants inspecting 13 offers	1	0	2	3	0	3	2	0	4	2
Number of participants inspecting 14 offers	0	3	5	3	2	2	4	0	4	0
Number of participants inspecting 15 offers	0	0	1	2	1	4	3	3	3	2
Number of participants inspecting 16 offers	0	2	5	4	1	15	5	0	6	5
Number of participants inspecting 17 offers	1	0	6	3	4	9	8	0	10	7
Number of participants inspecting 18 offers	1	1	6	4	2	4	10	2	3	8
Number of participants inspecting 19 offers	1	0	4	3	2	3	3	1	5	1
Number of participants inspecting 20 offers	0	0	2	1	1	3	4	1	1	2
Number of participants inspecting 21 offers	1	1	6	0	0	5	4	2	7	6
Number of participants inspecting 22 offers	2	1	5	2	0	7	4	2	10	1
Number of participants inspecting 23 offers	4	3	13	4	3	13	5	5	9	7
Number of participants inspecting 24 offers	149	147	98	109	144	75	77	141	87	98
Total number of participants	171	171	171	171	171	171	171	171	171	171

Table 3.5: Number of participants who inspected *x* (from 0 to 24) offers

Tasks	InspectionNum of common standard offers	InspectionNum of individuated standard offer	Total InspectionNum
AC tasks			
P1	22.63 [24] {94.3%} (4.52)		22.63 [24] {94.3%} (4.52)
P1*D1	22.32 [24] {93.0%} (4.88)		22.32 [24] {93.0%} (4.88)
P1*D1*D2	22.35 [24] {93.1%} (4.53)		22.35 [24] {93.1%} (4.53)
P1*D1+P2	22.26 [24] {92.8%} (4.83)		22.26 [24] {92.8%} (4.83)
Average	22.39 [24] {93.3%} (4.69)		22.39 [24] {93.3%} (4.69)
PC tasks			
P1*D1	7.30 [8] {91.3%} (1.78)	13.46 [16] {84.1%} (4.03)	20.76 [24] {86.5%} (5.29)
P1*D1*D2	7.00 [8] {87.5%} (2.12)	12.29 [15] {76.8%} (4.62)	19.29 [23] {80.4%} (5.96)
P1*D1+P2	7.06 [8] {88.3%} (2.12)	12.94 [16] {80.9%} (4.46)	20.01 [24] {83.4%} (5.79)
Average	7.12 [8] {89.0%} (2.01)	12.90 [16] {80.6%} (4.39)	20.02 [24] {83.4%} (5.71)
NC tasks			
P1*D1		19.92 [24] {83.0%} (6.33)	19.92 [24] {83.0%} (6.33)
P1*D1*D2		18.38 [22] {76.6%} (6.61)	18.38 [22] {76.6%} (6.61)
P1*D1+P2		19.77 [24] {82.4%} (6.28)	19.77 [24] {82.4%} (6.28)
Average		19.36 [24] {82.7%} (6.43)	19.36 [24] {82.7%} (6.43)

Table 3.6: Means, medians, percentages and standard deviations of number of non-repeated offers inspected by participants (InspectionNum)

Note: The first number is the mean; the second number in the square braces is the median; the third number in the curly braces is the percentage; the number under the mean in the parenthesis is the standard deviation. There are 171 observations in each task.

Note: There are no individuated offers in AC tasks and no common standard offers in NC tasks.

It is clear from these tables that, in all tasks, most participants inspected most (and often all) the offers).

In AC tasks, 84.9% participants inspected all twenty-four offers (87.1%, 86.0%, 84.2% and 82.5% in P1, P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively). This value drops to 50.7% in PC tasks (57.3%, 43.9% and 50.9% in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) and to 55.36% in NC tasks (63.7%, 45.0% and 57.3% in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively).

The average numbers of offers inspected in AC, PC and NC tasks are 22.39 offers (93.3%), 20.02 offers (83.4%) and 19.36 offers (80.7%), respectively. The average number of offers inspected (InspectionNum) drops when tasks contain fewer common standard offers (AC vs PC: $z = 9.17$, $p < 0.001$; AC vs NC: $z = 9.07$, $p < 0.001$; PC vs NC: $z = 2.41$, $p = 0.02$).

Figures from 3.1 to 3.6 show, for each PC task, the number of participants who inspected x common standard or individuated standard offers (x ranges from 0 to 8 for common standard offers and from 0 to 16 for individuated standard offers). Corresponding tables K2 to K7 can be found in Appendix K.

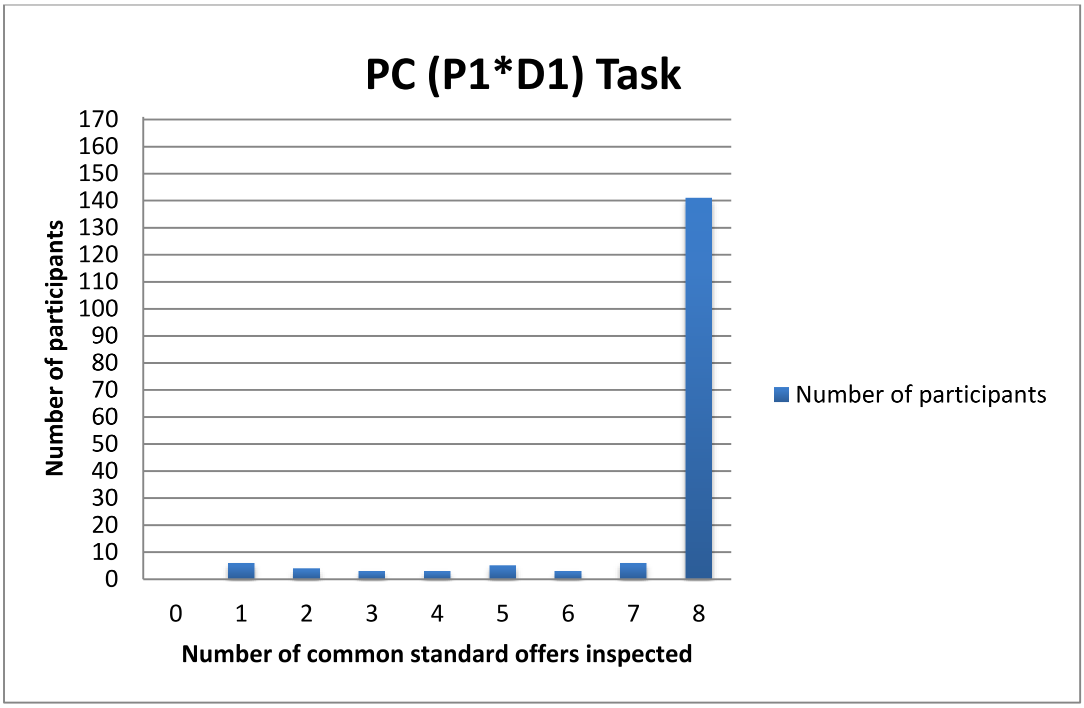

Figure 3.1: Numbers of participants who inspected different numbers of common standard offers: PC (P1*D1) task

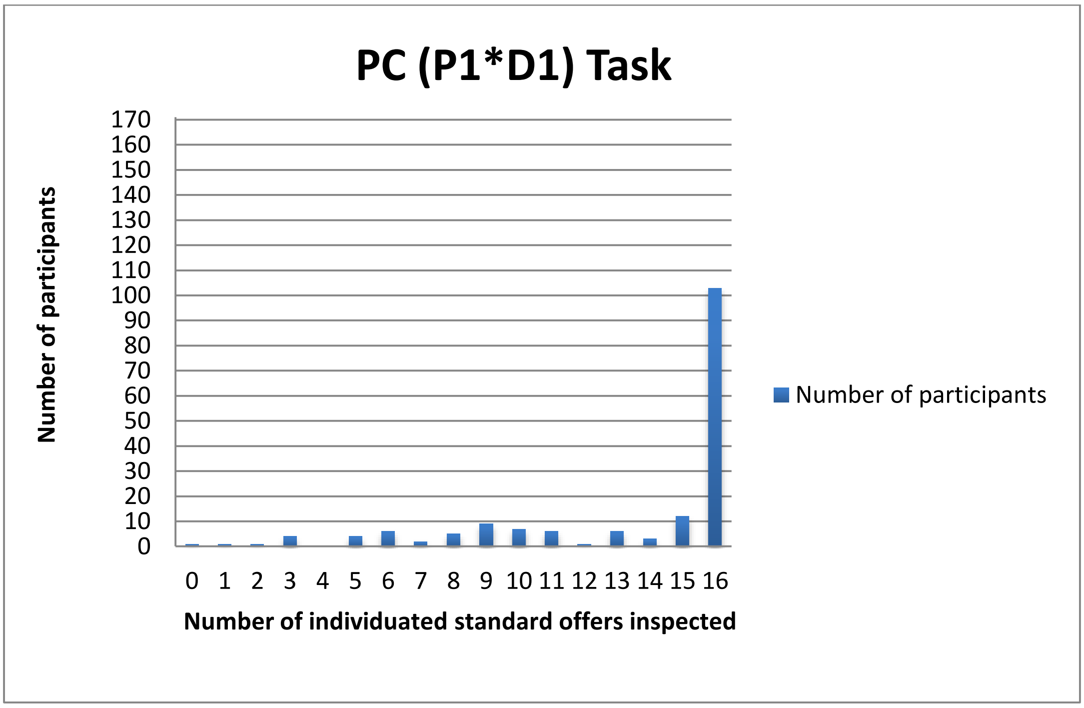

Figure 3.2: Numbers of participants who inspected different numbers of individuated standard offers: PC (P1*D1) task

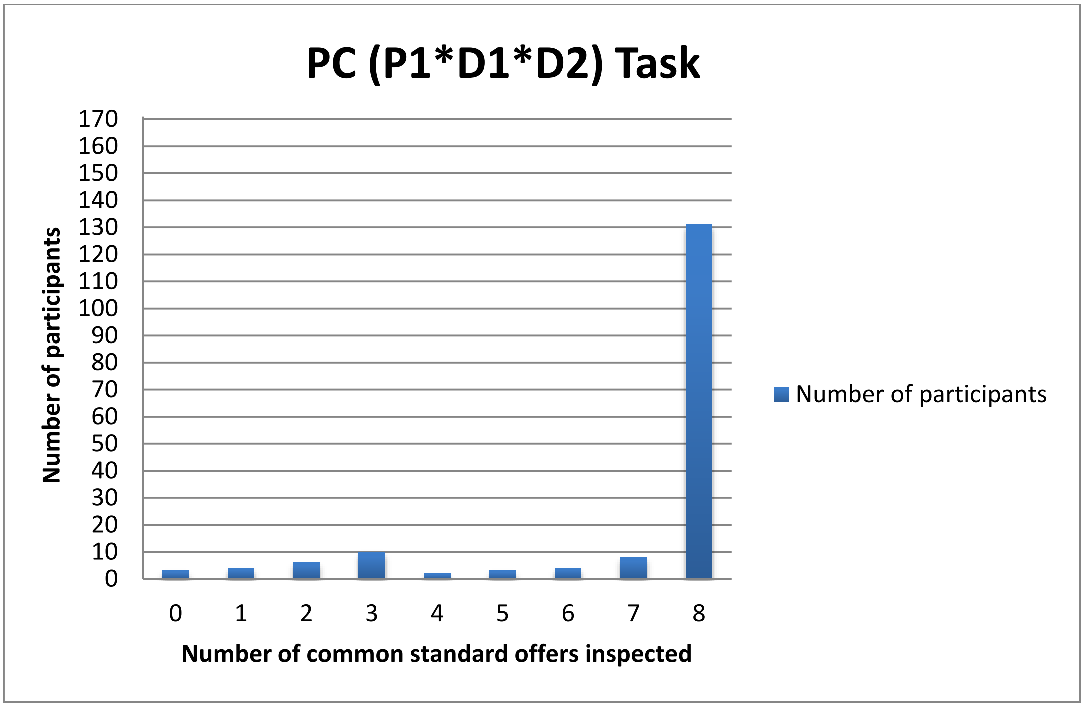

Figure 3.3: Numbers of participants who inspected different numbers of common standard offers: PC (P1*D1*D2) task

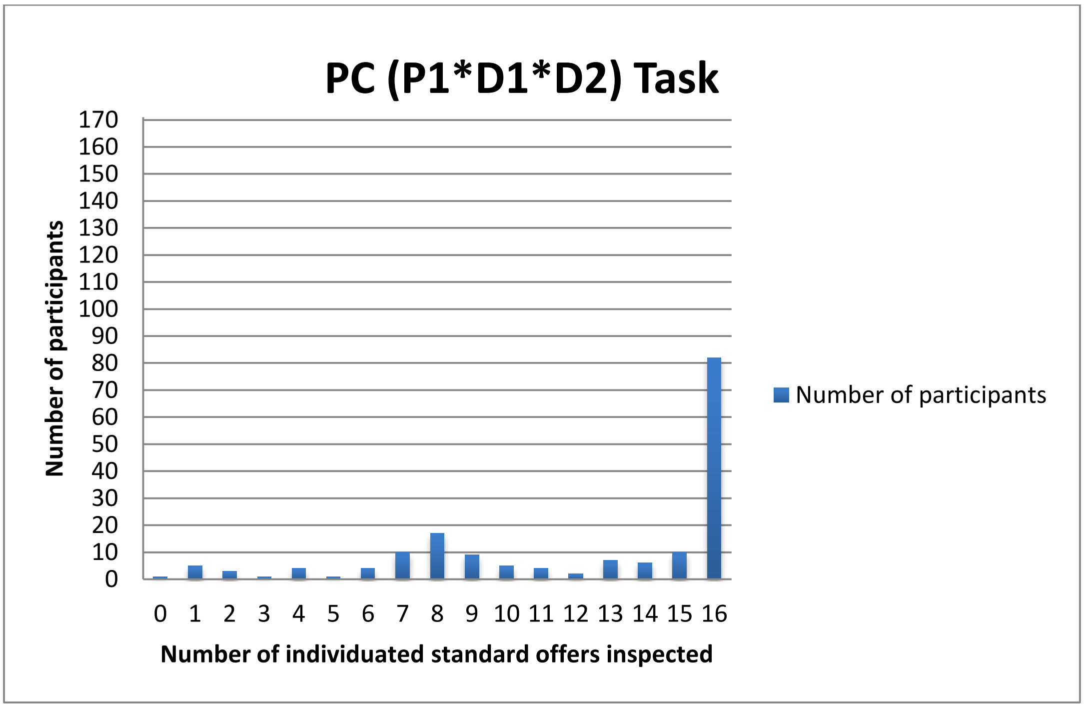

Figure 3.4: Numbers of participants who inspected different numbers of individuated standard offers: PC (P1*D1*D2) task

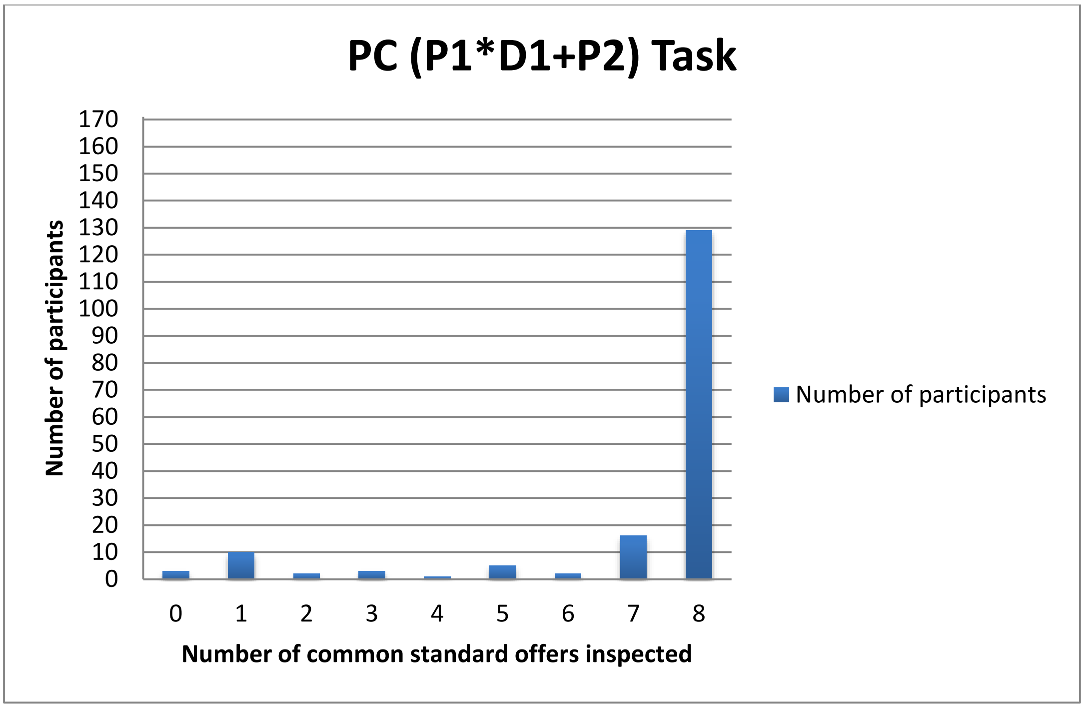

Figure 3.5: Numbers of participants who inspected different numbers of common standard offers: PC (P1*D1+P2) task

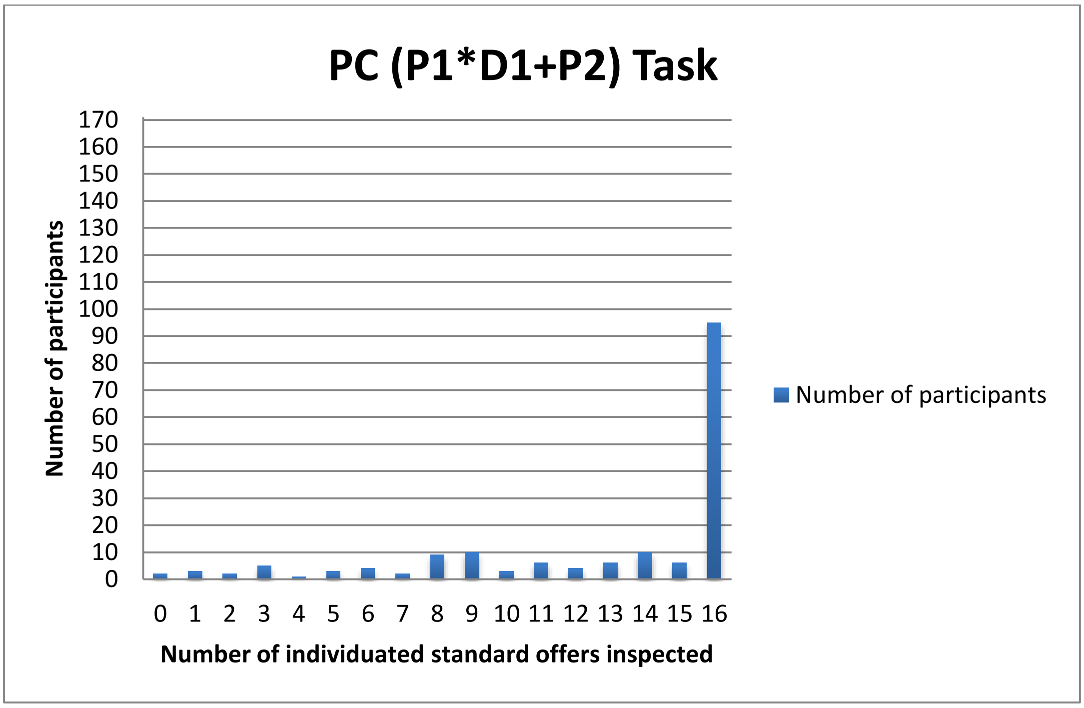

Figure 3.6: Numbers of participants who inspected different numbers of individuated standard offers: PC (P1*D1+P2) task

From the figures, we can see that in PC tasks, there are many participants inspecting all 8 common standard offers, (82.5%, 76.6% and 75.4% in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) and all 16 individuated offers. (60.2%, 48.0% and 55.6% in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) However, there are very few participants who did not inspect any individuated standard offers (0.6%, 0.6% and 1.2% in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively).

What do these results tell us about which heuristic was employed?

Given that the majority of participants inspected most of the offers, first, some common standard unrelated heuristics such as the random selection heuristic can be ruled out. Second, comparing PC and NC tasks, if participants employed the LCS heuristic, the number of offers inspected in PC tasks would be much lower than in NC tasks and individuated standard offers would not be inspected in PC tasks. Nevertheless, the results show the opposite. It is clear that in PC tasks, participants inspected both common standard offers (average 7.12 offers) and individuated standard offers (average 12.90 offers). The possibility that participants employed the LCS heuristic can be ruled out as well.

If all participants employed the DE or SF heuristics, there would be no systematic difference between the numbers of offers inspected in PC and NC tasks. However, although the observed difference is small, it is statistically significant. This is consistent with the prediction in section 4 of classic “search and choose” models, which is that the number of offers inspected falls as search costs increase.

Table 3.7 reports the means, medians and standard deviations of the number of clicks that participants made in each task.

Tasks	Number of clicks
AC tasks	
P1	66.32 [71] (15.56)
P1*D1	67.60 [71] (19.25)
P1*D1*D2	67.02 [71] (16.83)
P1*D1+P2	68.63 [71] (17.64)
Average	67.39 [71] (17.35)
PC tasks	
P1*D1	67.32 [72] (21.80)
P1*D1*D2	64.36 [69] (24.39)
P1*D1+P2	65.44 [69] (22.86)
Average	65.70 [70] (23.02)
NC tasks	
P1*D1	64.45 [71] (24.51)
P1*D1*D2	57.58 [61] (23.83)
P1*D1+P2	63.37 [68] (23.63)
Average	61.80 [66] (24.13)

Table 3.7: means, medians and standard deviations of the number of clicks that participants made in each task

Note: The first number is mean, the second number in the square brace is median and the number under the mean in the parenthesis is standard deviation. There are 171 observations in each task.

Notice that the minimum number of clicks required to inspect all 24 offers will be 52 if a participant only used approach 1 and 77 if she only used approach 2. From the table, we can see that the means and medians of number of clicks that participants made in AC, PC and

NC tasks are roughly the same, with means ranging from 61 to 69 and medians ranging from 61 to 71. This result is in line with the previous findings, that is, a majority of participants inspected all or almost all the offers.

Result 2: In all tasks, a majority of participants inspected all or almost all the offers. In PC tasks many participants inspected all individuated standard offers. The average number of offers inspected was lower when tasks contained fewer common standard offers. The evidence does not support the hypothesis that participants employed the LCS or random selection heuristics.

3.5.3 The offer inspecting sequence in PC tasks

Although LCS and some common standard unrelated heuristics are eliminated, it is still ambiguous which heuristic most participants employed.

Recall that the key difference between DE, SF and common standard unrelated heuristics is the sequence of inspecting common standard offers. If participants employed the DE heuristic, in PC tasks, one would observe a clear offer inspection sequence showing that participants first inspected common standard offers and then inspected individuated standard offers. If participants employed the SF heuristic, the offer inspection sequence would be different, that is, first all offers would be inspected without any distinction between common standard and individuated standard offers, and then only common standard offers would be inspected. But, if participants employed common standard unrelated heuristics, the inspection sequence would not discriminate between the two types of offer.

Now let us look at participants' offer inspection sequence in PC tasks. To simplify the analysis, some additional variables will now be introduced.

For each participant, the total number of clicks used during the course of a given task could be different. To normalise the spectrum of each task, each click of a participant is converted into a corresponding percentage of the whole spectrum of total clicks ranging from

0% to 100% calculated as the quotient of 100% divided by total number of clicks made by a participant. I use this normalisation to create a new variable called *clicking time*. Clicking time represents how much of a task has been completed according to the number of clicks used in the course of the task. The unit of clicking time is not "second" but "percentage". For example, if in the course of a task, a participant has clicked a total of 200 times, then each click is converted to 0.5% of the whole spectrum (100%/200 clicks), and so each click increases clicking time by 0.5 percentage points. But if a participant made a total of only 50 clicks in the course of a task, then her each click is converted to 2% of the whole spectrum (100%/50 clicks), for this participant, each click increases clicking time by 2 percentage points.

In the following figures and tables, the clicking time of a task is divided into 10 equal clicking time intervals. Remember that the number of clicks used in a clicking time interval may be different for different participants. The data in table 3.7 implies that on average each clicking time interval contains about 6 to 7 clicks.

Summing over all 171 participants, for each PC task and each clicking time interval, I count (a) the total number of offers inspected and (b) the total number of common standard offers inspected. The ratio of (b) to (a) is the *proportion of common standard inspections*. Figures 3.7, 3.8 and 3.9 plot the trends of these proportions for the three PC tasks. Corresponding tables K8, K9 and K10 can be found in Appendix K.

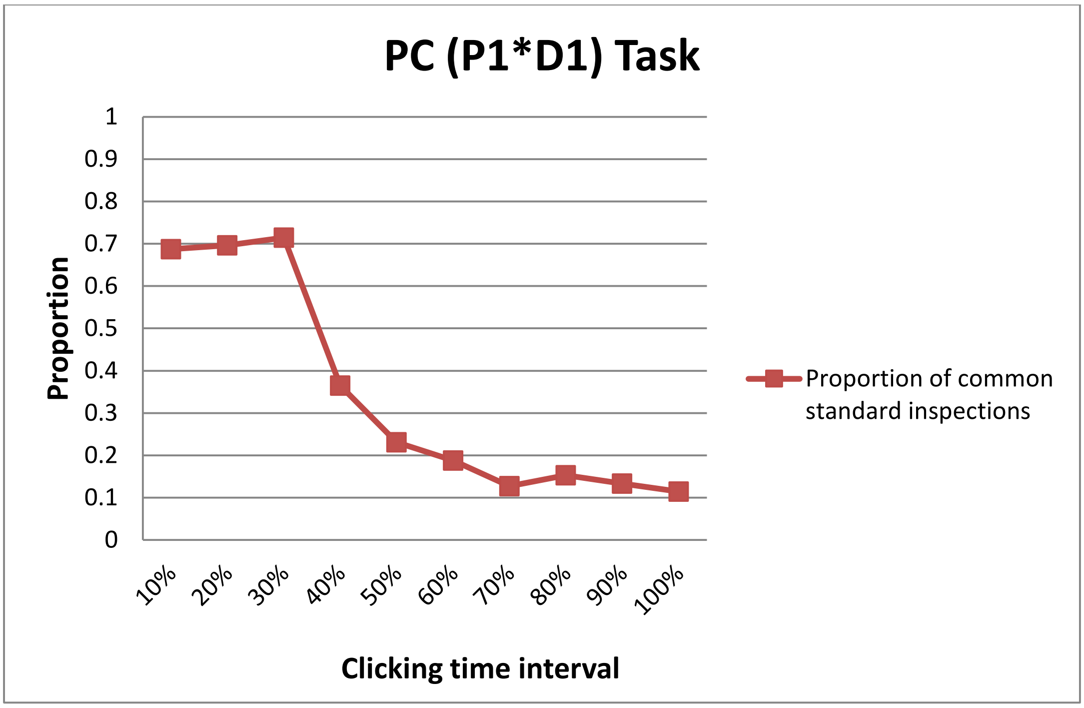

Figure 3.7: Proportion of common standard inspections over course of task: PC (P1*D1) task

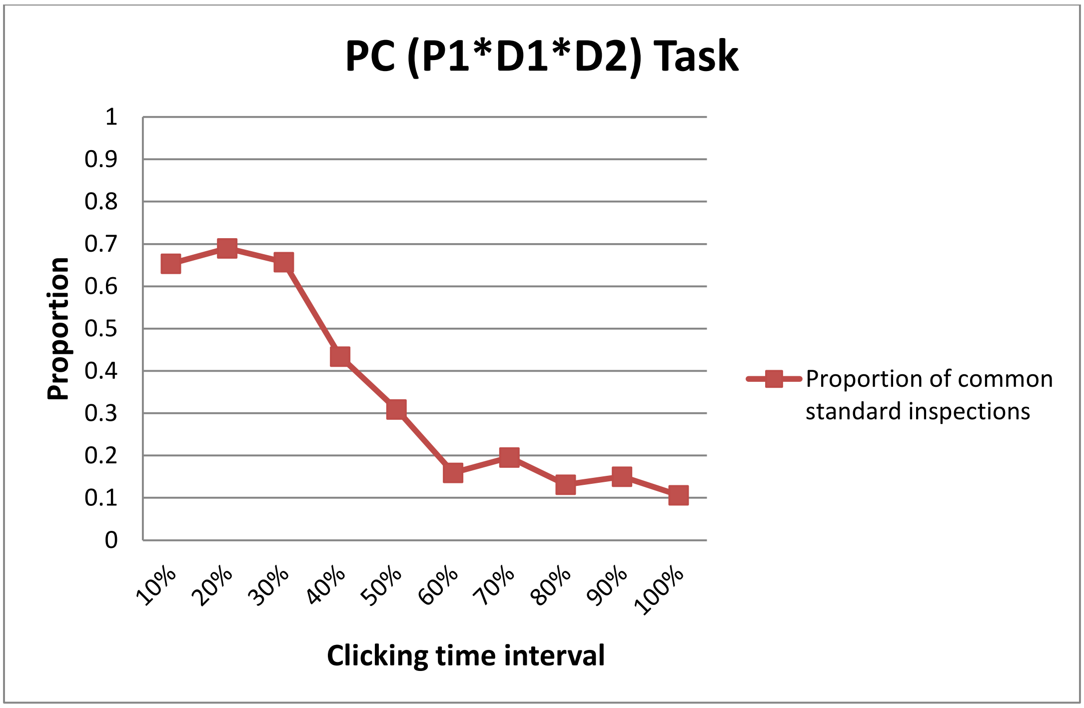

Figure 3.8: Proportion of common standard inspections over course of task: PC (P1*D1*D2) task

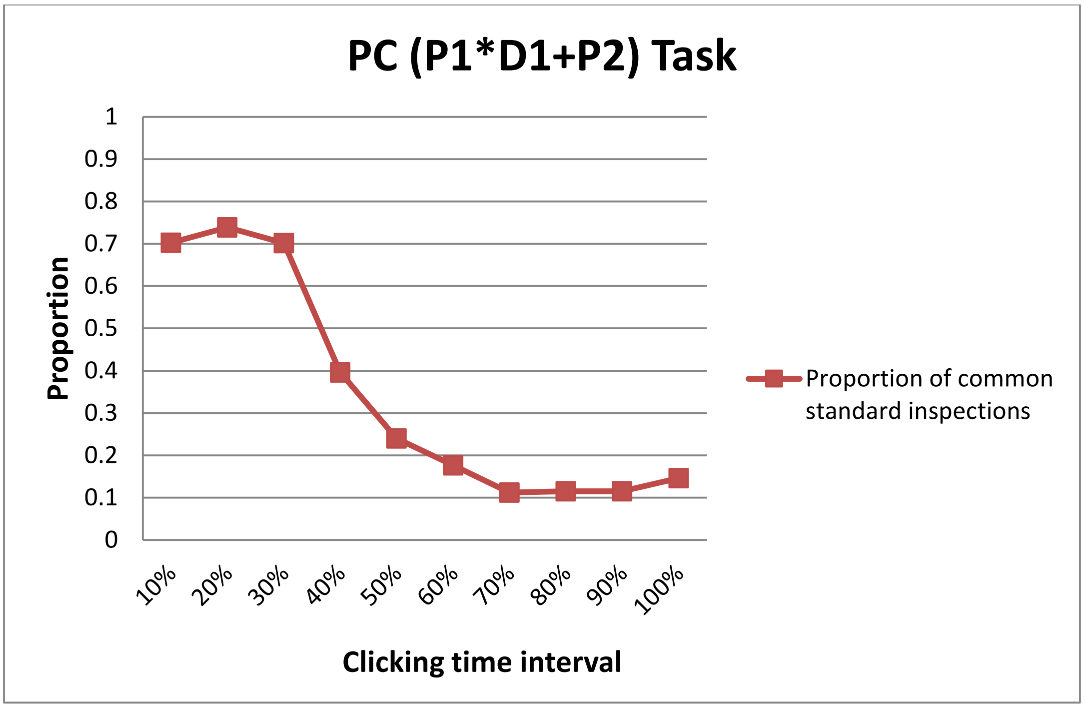

Figure 3.9: Proportion of common standard inspections over course of task: PC (P1*D1+P2) task

Since in each PC task eight out of the twenty-four offers are common standard offers, if participants inspected offers in a random sequence the proportion of common standard inspections would be around 0.33 throughout the task. However, in all PC tasks we observe that the proportion of common standard inspections is approximately 0.7 in the first clicking interval (0.687, 0.654 and 0.703 in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) and this value decreases over the course of the experiment to approximately 0.1 in the last clicking time interval (0.114, 0.107 and 0.146 in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively). This pattern indicates that many participants inspected the offer in a common-standard-related offer inspecting sequence, that is, they first inspected the common standard offers and then inspected the individuated standard offers. This offer inspecting sequence eliminates the possibility that participants employed the SF heuristic. It is consistent with the possibility that many participants employed the DE heuristic.

It is also useful to look at how the contents of the shopping basket changed over the course of a task. For each PC task and for each clicking time interval, aggregating over all participants, I calculate (a) the average number of offers in the shopping basket and (b) the average number of common standard offers in the shopping basket. The ratio of (b) to (a) is the *proportion of common standard offers in the shopping basket*. Figures 3.10, 3.11 and 3.12 plot the trends of these proportions for the three PC tasks. Corresponding tables K11, K12 and K13 can be found in Appendix K.

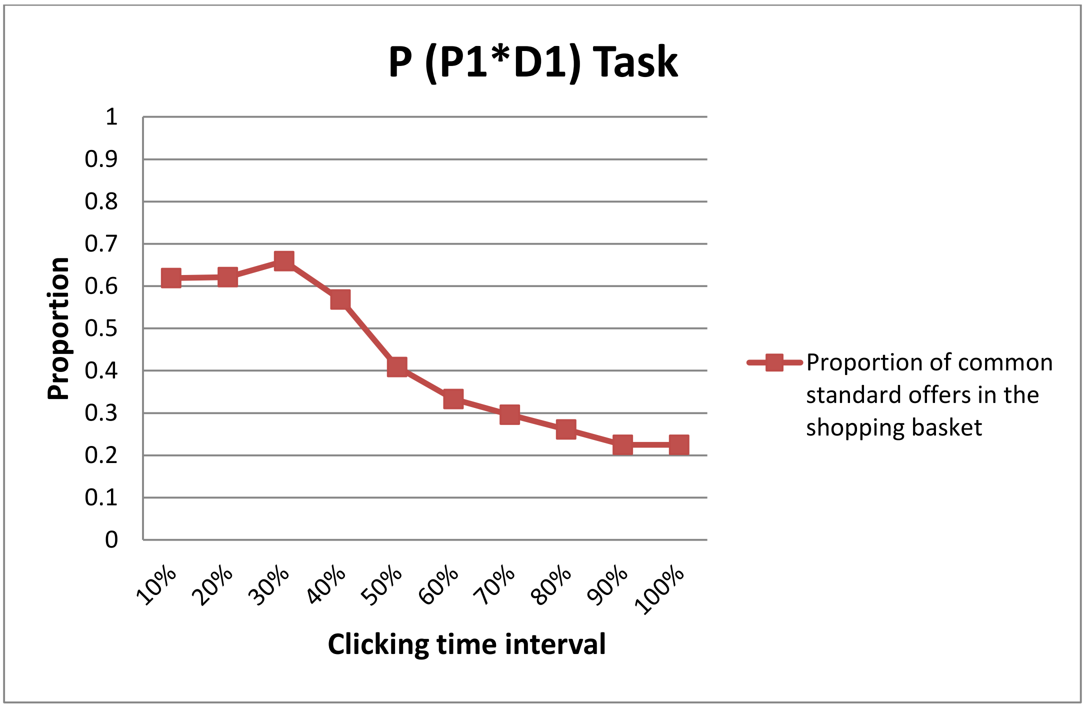

Figure 3.10: Proportion of common standard offers in the shopping basket: PC (P1*D1) task

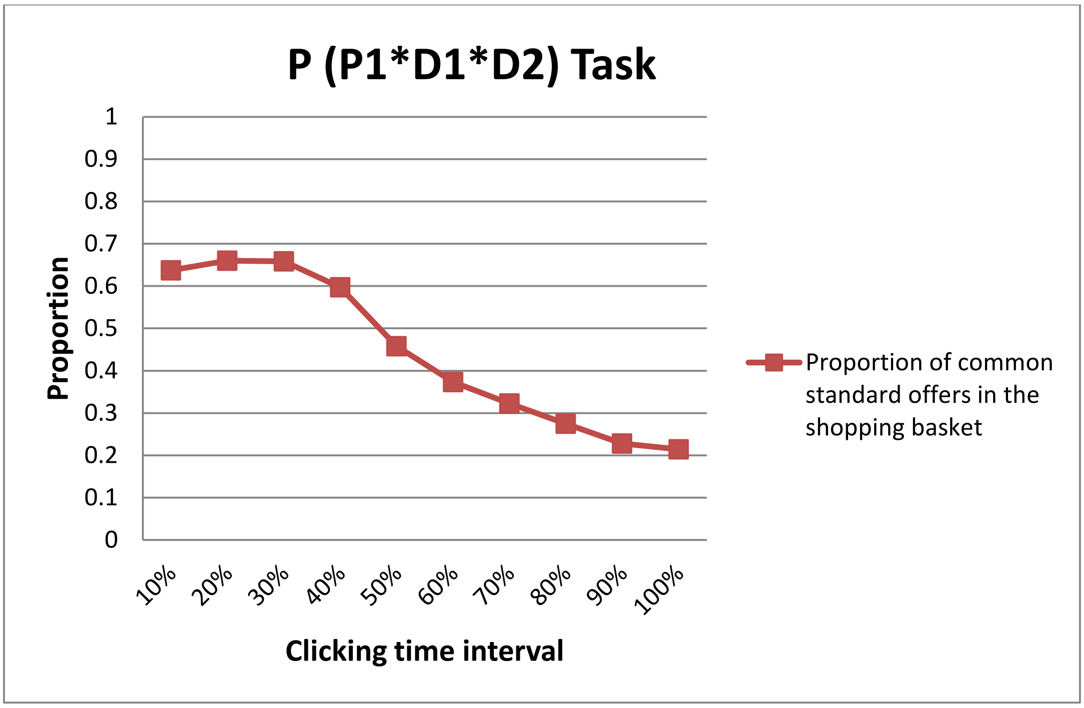

Figure 3.11: Proportion of common standard offers in the shopping basket: PC (P1*D1*D2) task

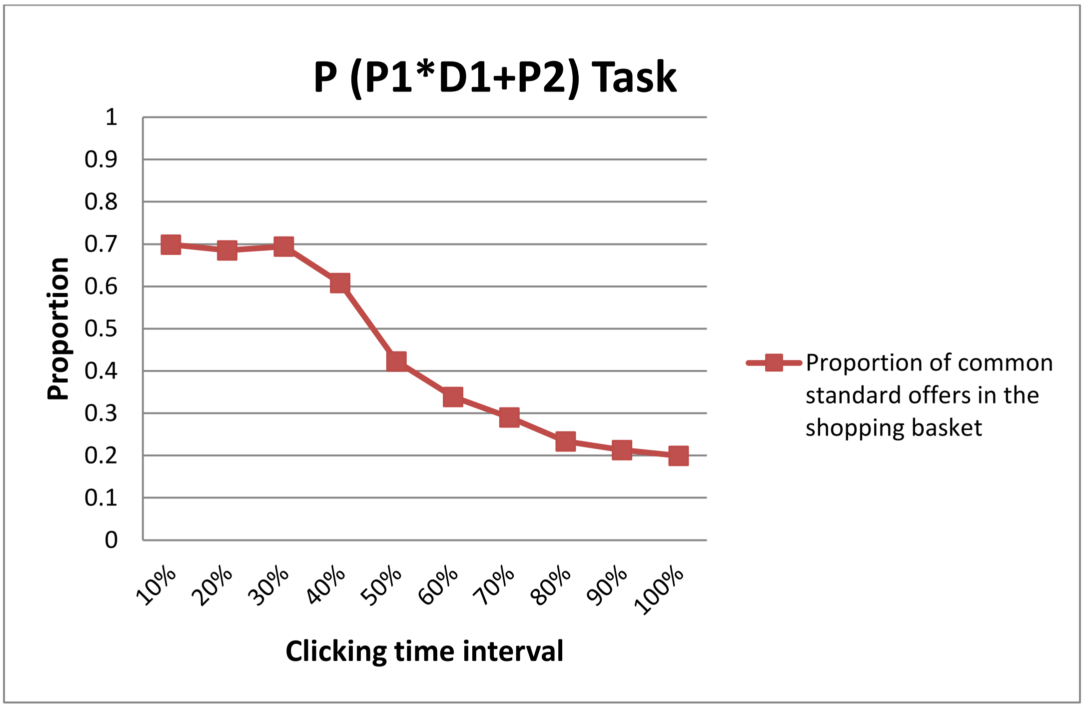

Figure 3.12: Proportion of common standard offers in the shopping basket: PC (P1*D1+P2) task

In all PC tasks, the proportion of common standard offers in the shopping basket was between 0.6 and 0.7 in the first clicking time interval (0.619, 0.637 and 0.699 in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively), decreasing over the course of the task to approximately 0.2 in the last clicking time interval (0.225, 0.214 and 0.199 in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively). This pattern is again consistent with the possibility that many participants employed the DE heuristic.

3.5.4 Sensitivity to price information

Although a common-standard-related offer inspecting sequence has been observed, it is still possible that a participant may not be sensitive to the final prices of offers. For example, she might choose a common standard offer from among all common standard offers at random, or according to its position on the screen, and then compare this offer with individuated standard offers in order to make a final decision. If this was the case, even though the participant would be using a common-standard-related offer inspecting sequence, she would not be using the DE heuristic.

Because an offer could be bought only when it was in the shopping basket, and because the shopping basket had a limited capacity, participants who are sensitive to prices need to keep offers with low final prices in the shopping basket and remove offers with high final prices. If the majority of the participants employed the DE heuristic and were cognitively unconstrained, one would observe that in AC and PC tasks, once the cheapest common standard offer has been put into the shopping basket, it will stay there until all more expensive common standard offers have been eliminated.

For each participant, the course of each task can be divided into a sequence of *moments* 1, …, N, each corresponding with one click. For each moment i, for any given offer, I define the offer's *basket status* B_i as 1 if the offer is in the shopping basket at that moment and 0 if it is not. The ten clicking time intervals can be defined by the sets of moments $T_1 = \{1, \ldots, N/10\}, \ldots, T_{10} = \{(9N/10)+1, \ldots, N\}$. I define the probability that the offer is in the shopping basket in interval T_t as:

$$ProbabilityInBasket = \sum_{i \in T_t} B_i \, / \, (\frac{N}{10})$$

In each task, each offer can be assigned one of the *ranks* 1, …, 24, according to its final price. The offer with the lowest final price, which we will call the *optimal offer*, is given the rank 1; the offer with the highest final price is given the rank 24.

First, I consider the AC tasks. In these tasks, all 24 offers are common standard offers. Consider how the basket status of offers with different ranks will change over the course of a task if all participants use the DE heuristic, without making any errors. Since putting offers in the basket uses clicking time, ProbabiltyInBasket will be low for all offers in the first clicking time interval. Because all visible non-price attributes of offers (e.g. position on screen) were randomized, the order of initial inspection of offers (identified by final price) must be random. Different participants will discover the optimal offer in different clicking time intervals. However, once a participant has discovered the optimal offer, it will not be removed from the basket. Thus, for the optimal offer, ProbabilityInBasket will increase over the course of the task until it reaches its maximum value of 1. Non-optimal offers, on the other hand, will tend to be moved out of the shopping basket when cheaper offers are discovered. The lower the final price of an offer, the higher the probability that it will stay in the basket until the end of the task. Figures 3.13 to 3.16 plot average ProbabilityInBasket over the course of each AC task for different sets of offers, classified by rank. To keep the diagram readable, we use only four sets of ranks, but because we are particularly interested in the optimal offer, we give this a set of its own. (The complete data for figures 3.13 to 3.25 are provided in tables K14 to K26 in Appendix K.)

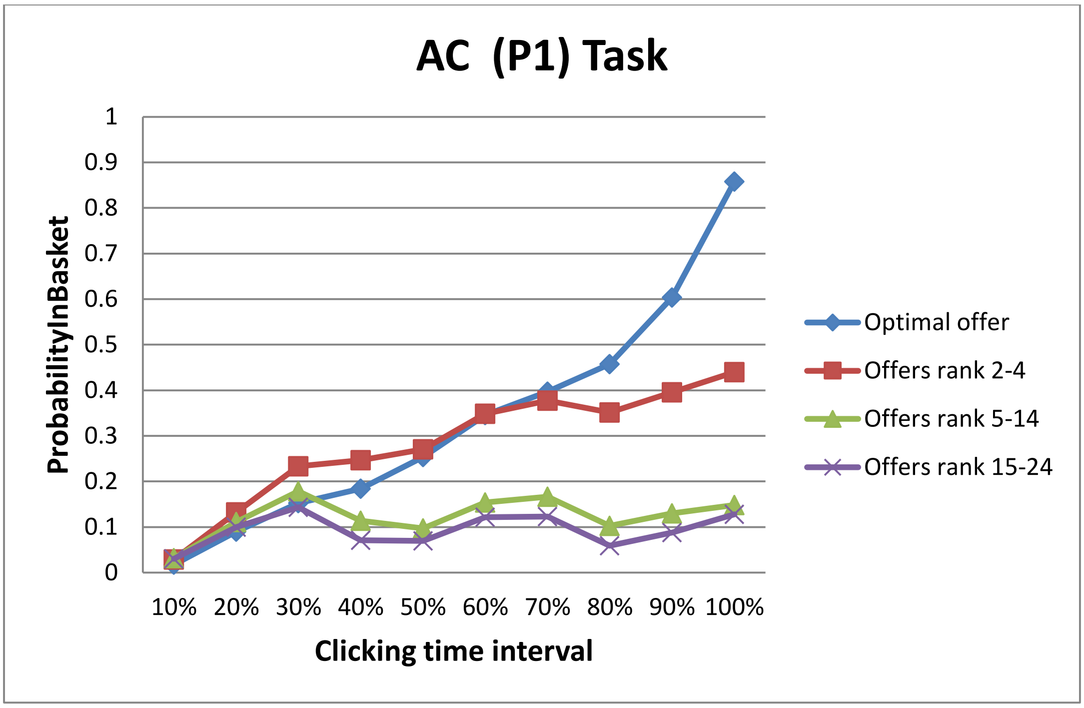

Figure 3.13: ProbabilityInBasket of common standard offers with different ranks: AC (P1) task

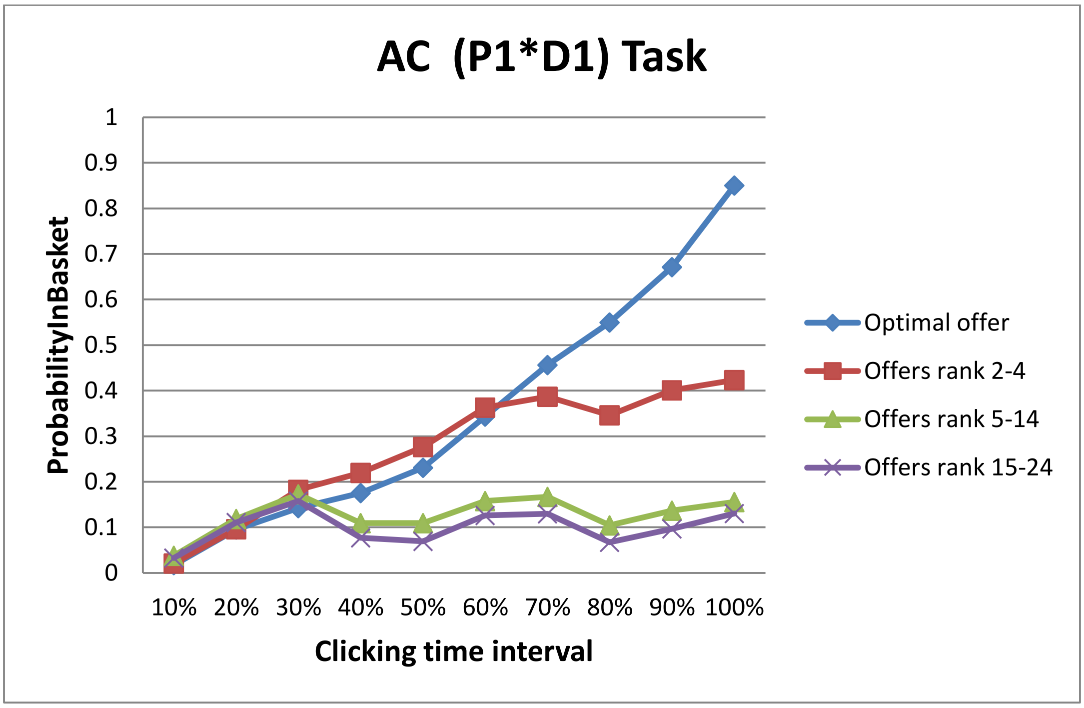

Figure 3.14: ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1) task

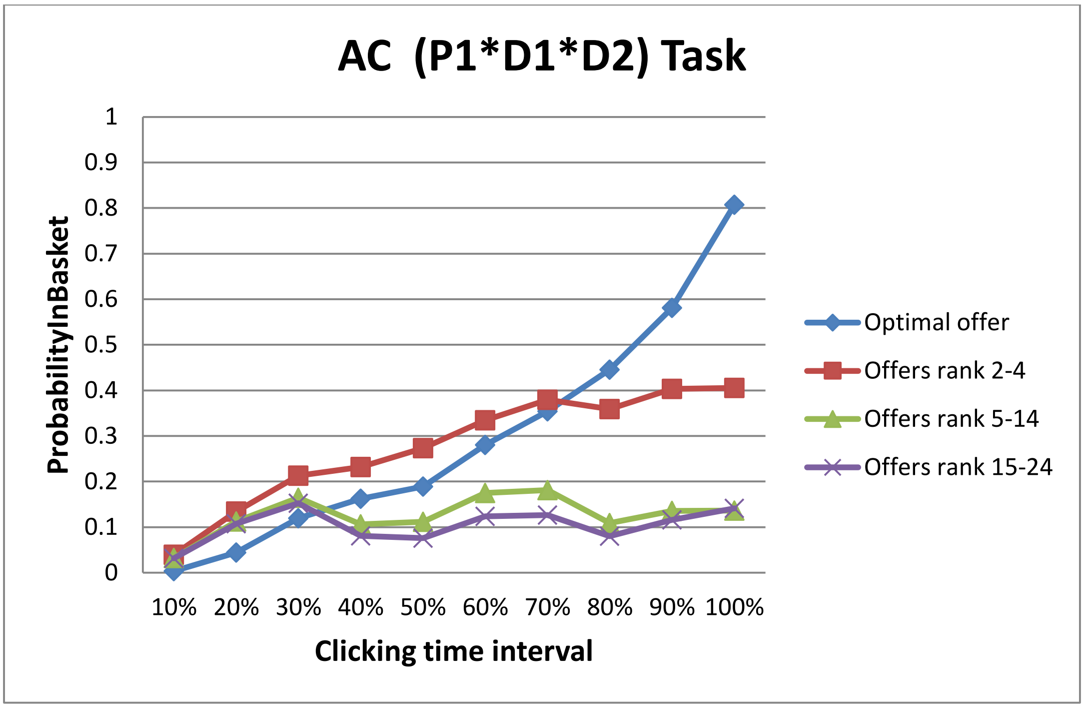

Figure 3.15: ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1*D2) task

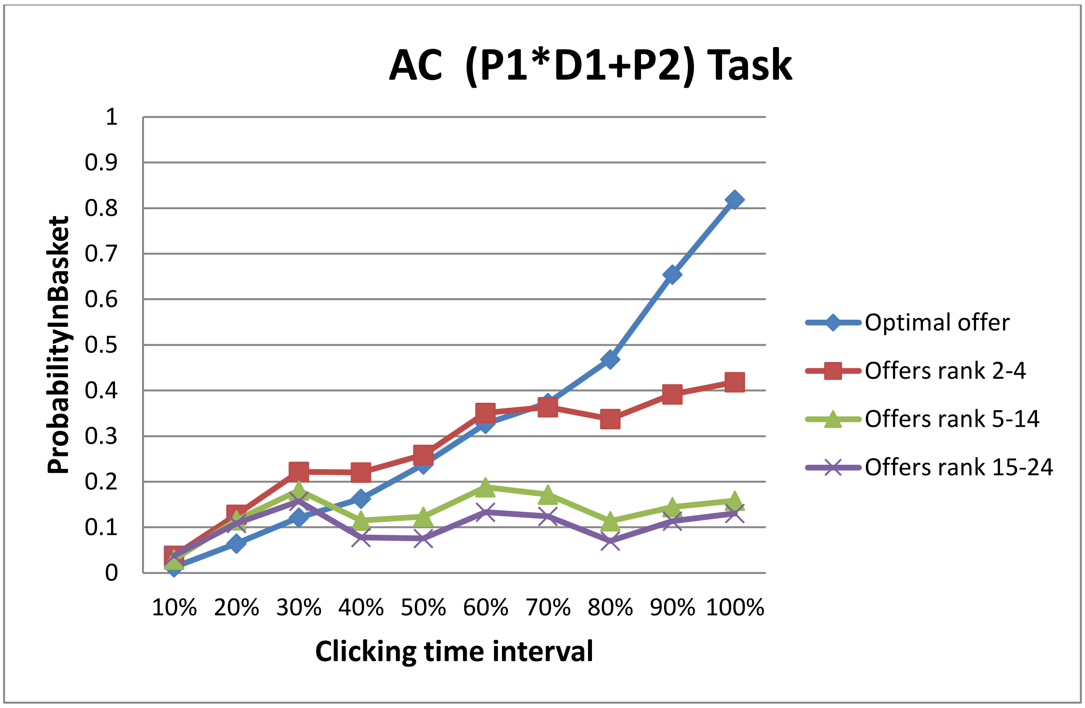

Figure 3.16: ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1+P2) task

In all AC tasks, ProbabilityInBasket for the optimal offer increases from under 0.02 in the first clicking time interval (0.017, 0.018, 0.003 and 0.012 in P, P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) to over 0.8 in the last interval (0.858, 0.851, 0.807 and 0.019 in P, P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively). Other high-rank offers (those with ranks 2, 3 and 4) have a higher probability of staying in the shopping basket until the end of the task than offers with lower ranks. These results support the hypothesis that participants discriminate between common standard offers according to their final prices.

We now consider the NC tasks. In these tasks, there is no common standard, and so the dominance editing operation of the DE heuristic cannot be used. Figures 3.17, 3.18 and 3.19 plot average ProbabilityInBasket over the course of each NC task for different sets of offers, classified by rank in the same way as before.

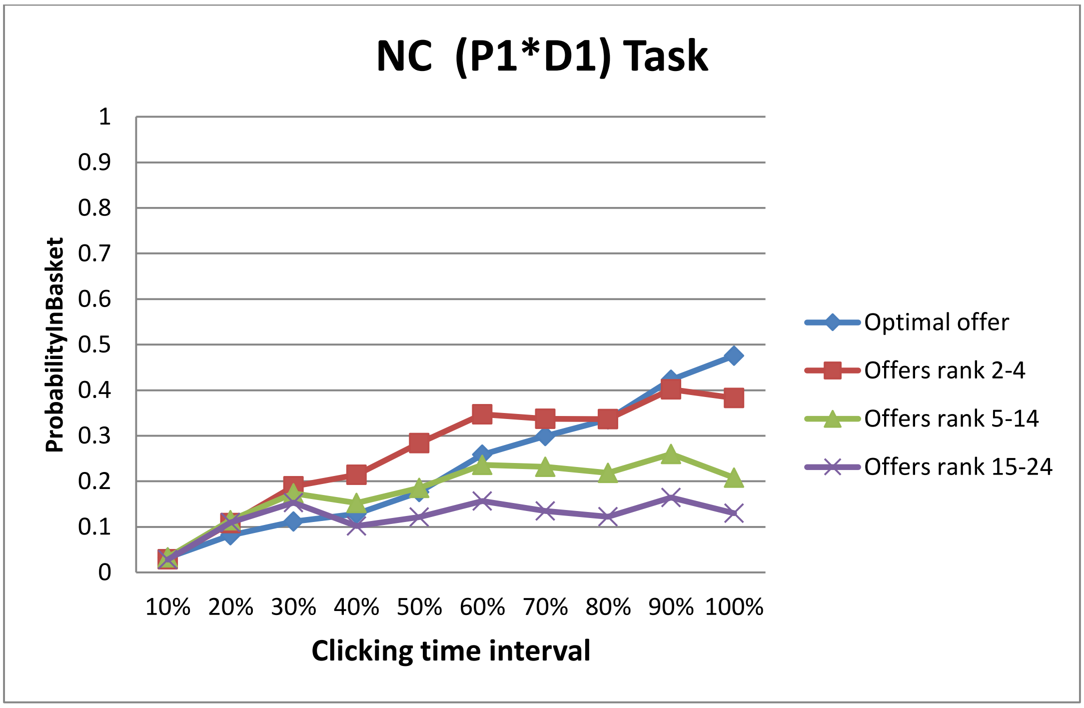

Figure 3.17: ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1) task

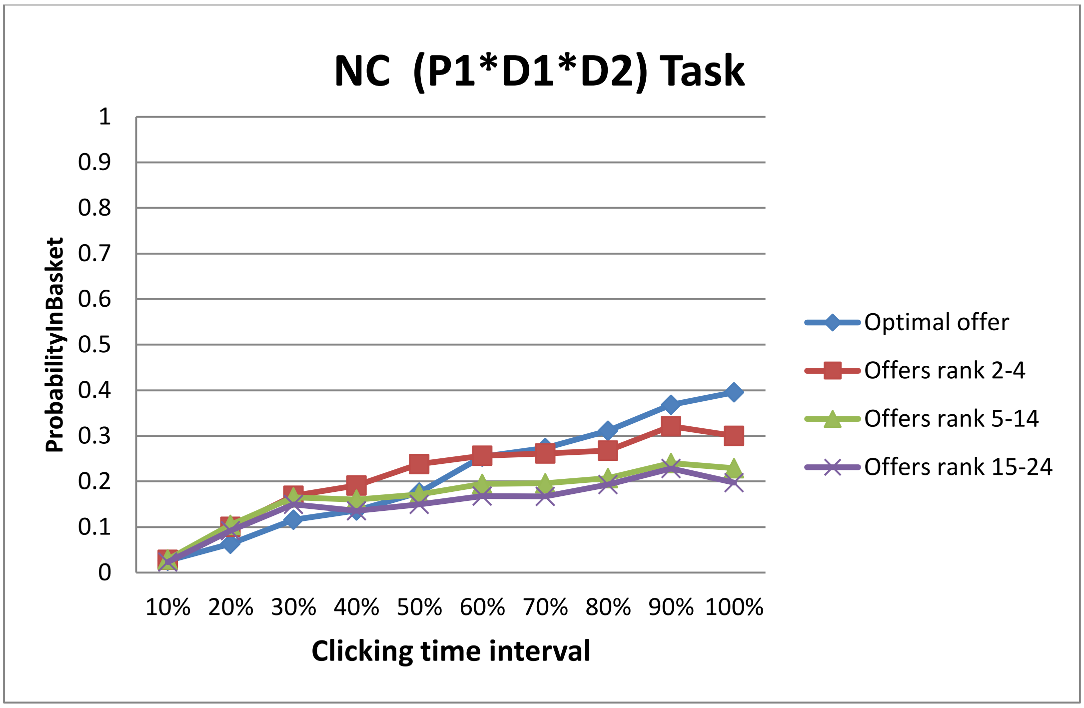

Figure 3.18: ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1*D2) task

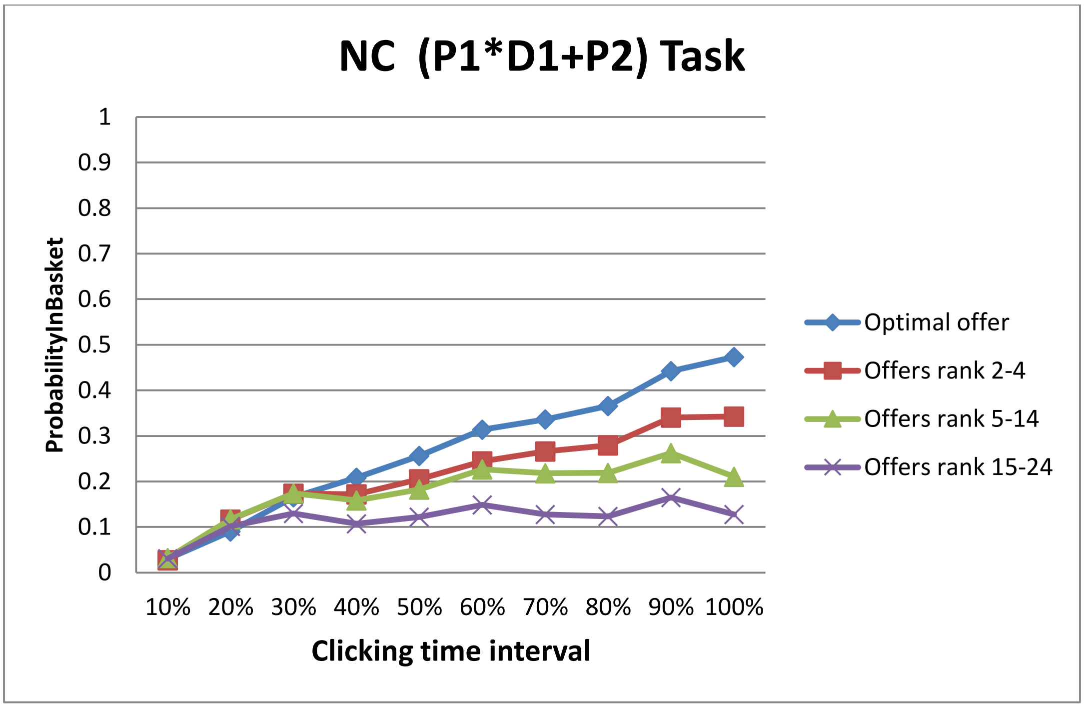

Figure 3.19: ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1+P2) task

In all three NC tasks, ProbabilityInBasket for the optimal offer increases slowly over the course of the task and in the final clicking time interval is higher for this offer than for lower-ranked offers. But the final value of this probability (0.476, 0.396, and 0.474 in P1*D1, P1*D1*D2 and P1*D1+P2 tasks, respectively) is much lower, and much closer to the probabilities for lower-ranked offers, than in the corresponding AC tasks. This may be because it is hard for cognitively constrained participants to compare individuated standard offers. Although participants are not completely insensitive to price information, many participants fail to discover and choose the optimal individuated standard offer due to its complexity and so they end up choosing a suboptimal offer.

Finally, I consider the PC tasks. In reporting ProbabilityInBasket data for these tasks, I classify offers not only by their ranks (defined as before) in the set of 24 offers, but also by whether they were common standard or individuated standard offers. I also give special attention to the *cheapest common standard offer* – that is, the offer with the lowest final price in the subset of eight offers that have a common standard. Because the 24 final prices were randomized between common and individuated standards, the cheapest common standard offer need not be the optimal offer (i.e. the offer with rank 1). The cheapest common standard offer has special significance in relation to the DE heuristic, because this heuristic begins by eliminating all common standard offers other than the cheapest.

Figures 3.20, 3.21 and 3.22 plot average ProbabilityInBasket *for common standard offers* over the course of the three PC tasks. In each figure, one graph plots ProbabilityInBasket for the cheapest common standard offer. The other three graphs plot ProbabiltyInBasket for the seven remaining common standard offers, grouped by their ranks *in the set of 24 offers*. (The highest possible rank is 2, because a common standard offer with rank 1 would necessarily be the cheapest common standard offer.)

Figures 3.23, 3.24 and 3.25 plot ProbabilityInBasket *for individuated standard offers* over the course of the three PC tasks. To allow comparisons with the graphs for common standard offers, offers are classified in a similar way – that is, one graph plots ProbabilityinBasket for the cheapest individuated standard offer, and the other three graphs plot ProbabiltyInBasket for the fifteen remaining individuated standard offers, grouped by their ranks in the set of 24 offers.

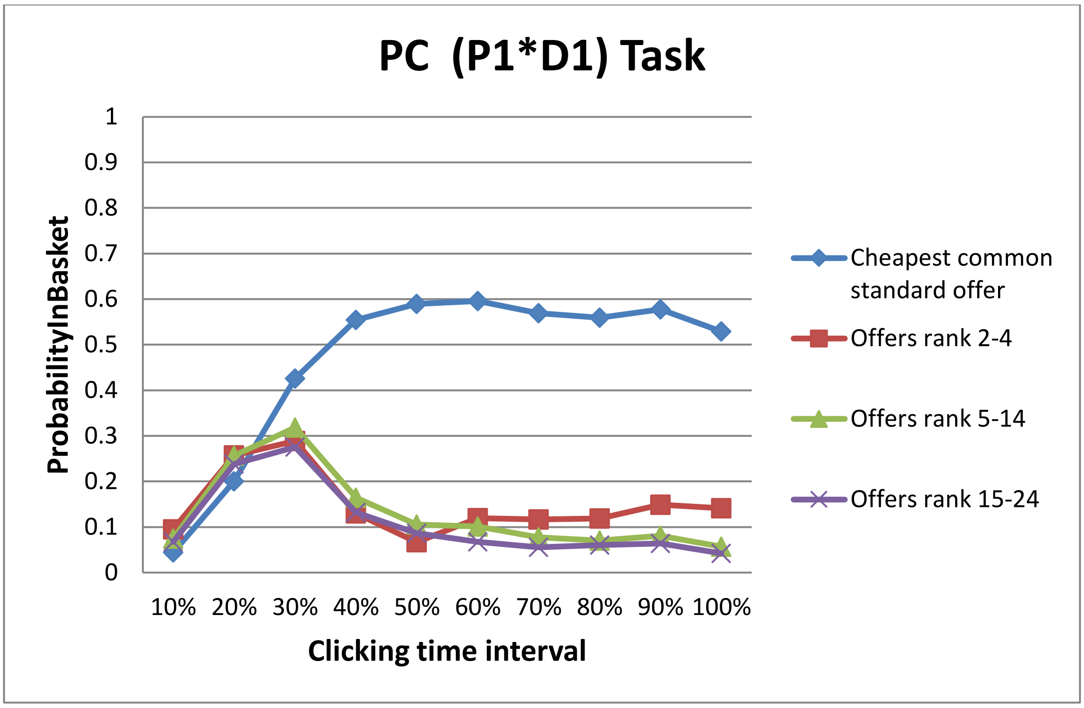

Figure 3.20: ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1) task

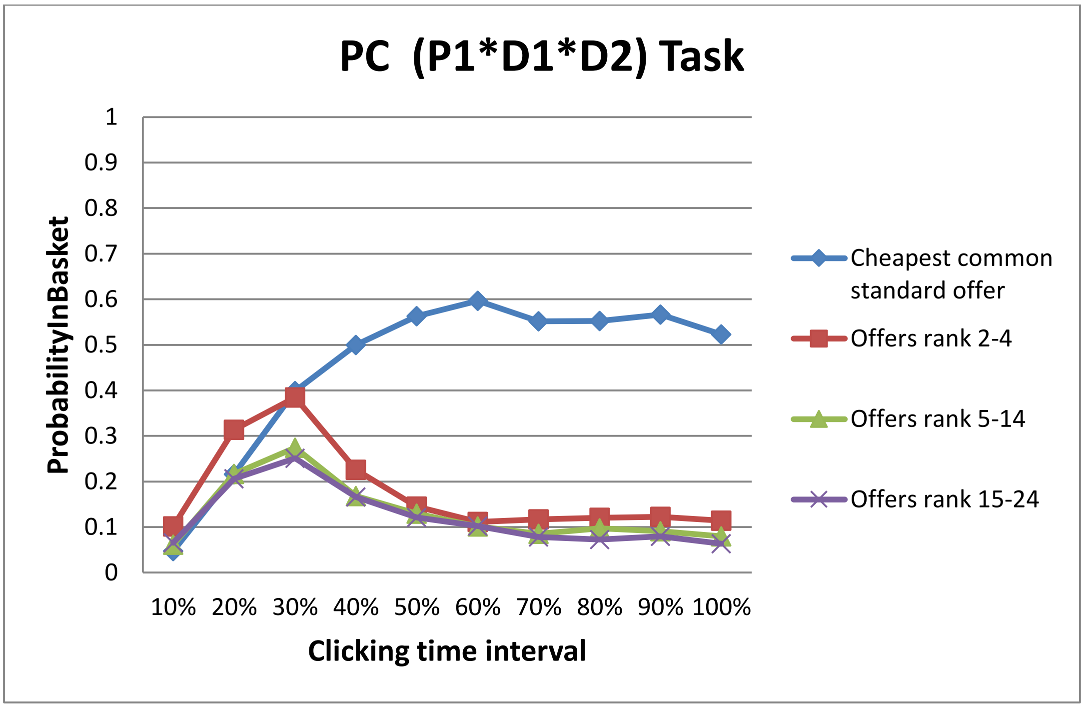

Figure 3.21: ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1*D2) task

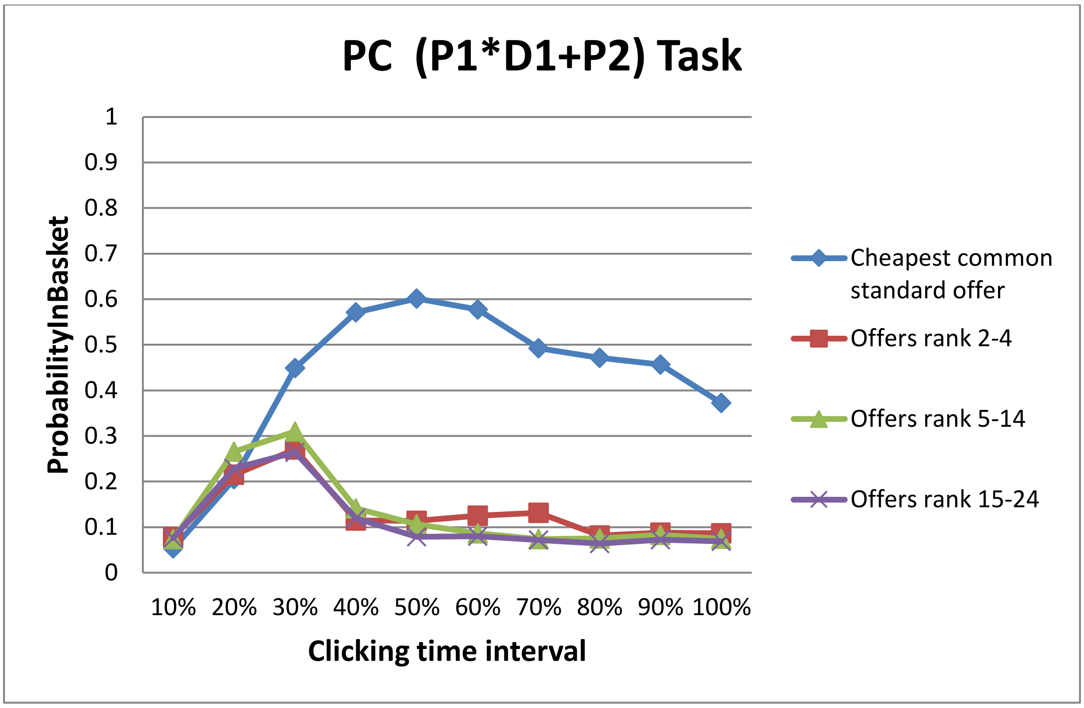

Figure 3.22: ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1+P2) task

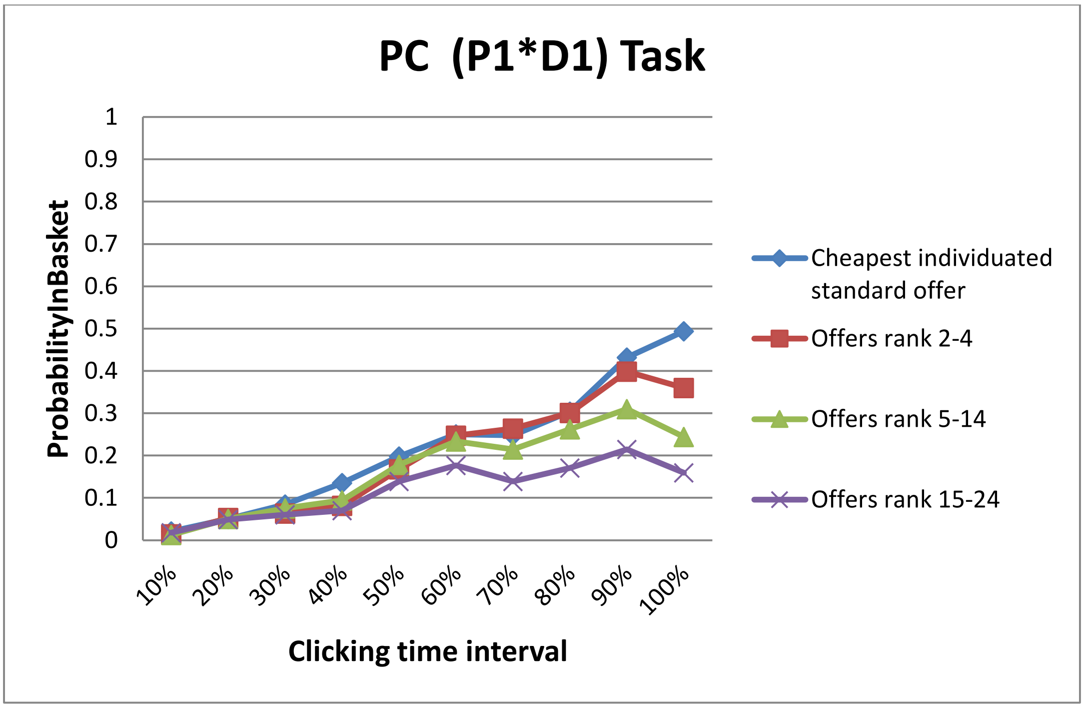

Figure 3.23: ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1) task

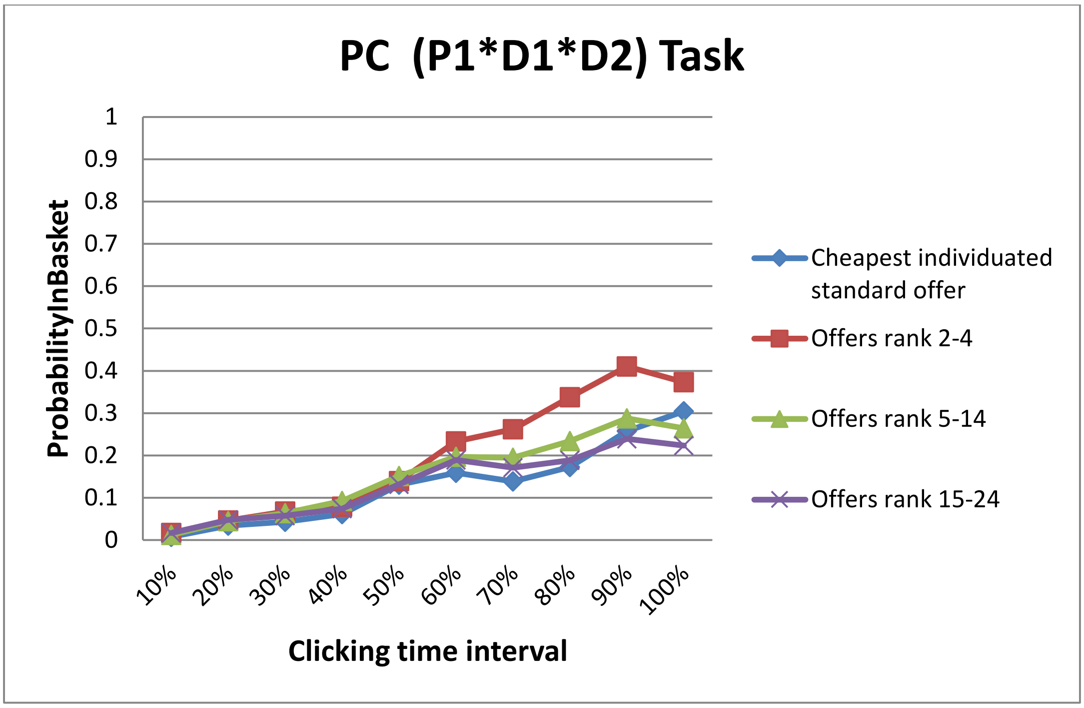

Figure 3.24: ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1*D2) task

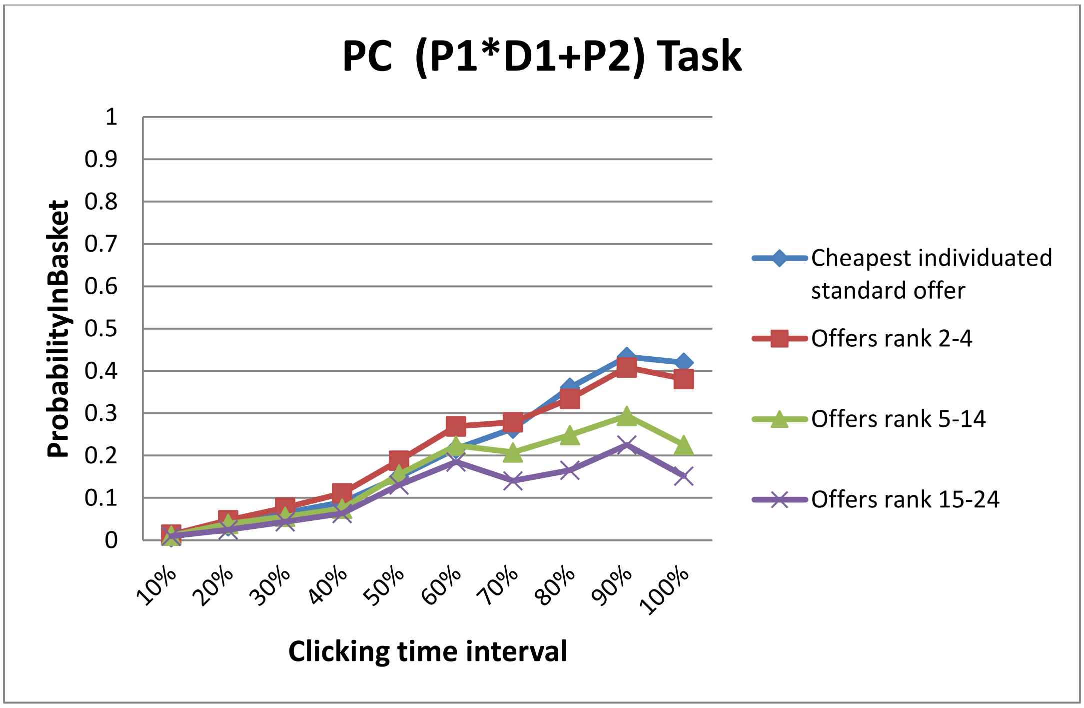

Figure 3.25: ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1+P2) task

In all three PC tasks, ProbabilityInBasket for the cheapest common standard offer increases steadily over the first half of the task, rising to approximately 0.6. In the PC (P1*D1) and PC (P1*D1*D2) tasks it remains almost constant, but with a slight downward trend, from then on; in the PC (P1*D1+P2) task it falls gradually over the second half of the task to approximately 0.4 in the final interval. On the other hand, ProbabilityInBasket for the other seven common standard offers rises over the first three clicking time intervals but drops sharply afterwards. This pattern implies that first, many participants compared the common standard offers at the beginning of a PC task and second, common standard offers other than the cheapest were moved from the shopping basket at an early stage in the task. This suggests that, in the later part of the task, most participants were comparing the cheapest common standard offer with individuated standard offers. Because of randomization, the cheapest common standard offer was optimal in only one third of cases. Thus, even if participants were not cognitively constrained, we would expect these comparisons to lead to some removals of the cheapest common standard offer from the shopping basket.

In all three PC tasks, ProbabilityInBasket for individuated standard offers of all ranks increases slowly but continuously over the course of the task (except for some slight declines in the final clicking time interval). The rate of increase for the cheapest individuated standard offer is not much greater than that for lower-ranked individuated standard offers. The patterns in these figures are similar to those observed in the NC tasks.

These results for PC tasks are consistent with the hypothesis that many participants used the DE heuristic. That is, participants first compared common standard offers, finding the cheapest of these, and then compared the cheapest common standard offer with individuated standard offers.

Result 3: In tasks in which some but not all offers have a common standard, many participants begin by comparing the common standard offers, eliminating all of them except *the cheapest. They then try to choose the lowest-priced offer from among non-eliminated offers.*

3.5.5 Cognitively constrained participants

Recall that, if a perfectly rational participant employs the DE heuristic, after all common standard offers other than the cheapest have been moved from the shopping basket, the cheapest common standard offer will remain in the shopping basket unless the participant finds an individuated standard offer with a lower final price. If such an offer is found before all offers have been inspected, the cheapest common standard offer may then be moved from the shopping basket to create space for further comparisons. If the cheapest common standard offer is the optimal offer, it will stay in the shopping basket until the end of the task and finally be chosen. However, for cognitively constrained consumers, an offer's price signal contains errors, so in a PC task, even if the cheapest common standard offer is the optimal offer, it is still possible that a cognitively constrained consumer would mistakenly move it out of the shopping basket and end up choosing a suboptimal individuated standard offer.

This raises the question: How many participants were constrained by complexity? It is clear that in AC tasks, the majority of participants successfully chose the optimal offer. This implies that very few participants were constrained by the complexity of comparisons between common standard offers. In NC tasks, the majority of participants failed to choose the optimal offer, which implies that many participants were constrained by the complexity of comparisons between offers with different standards. However, it is hard to answer the question for PC tasks using only the data in Figures 3.20 to 3.25. This is because in these tasks the cheapest common standard offer is not necessary the optimal offer, and so moving that offer out of the shopping basket may be rational behaviour for some participants. A similar argument applies to the cheapest individuated standard offer.

To investigate whether or not participants were constrained by the complexity of price comparisons in tasks with both common standard and individuated standard offers, I use Figures 3.26, 3.27 and 3.28. For each PC task, these figures plot ProbabilityInBasket for *the optimal offer* under two alternative conditions – first, that the optimal offer is a common standard offer, and second, that it is an individuated standard offer. Corresponding tables K27, K28 and K29 can be found in Appendix K.

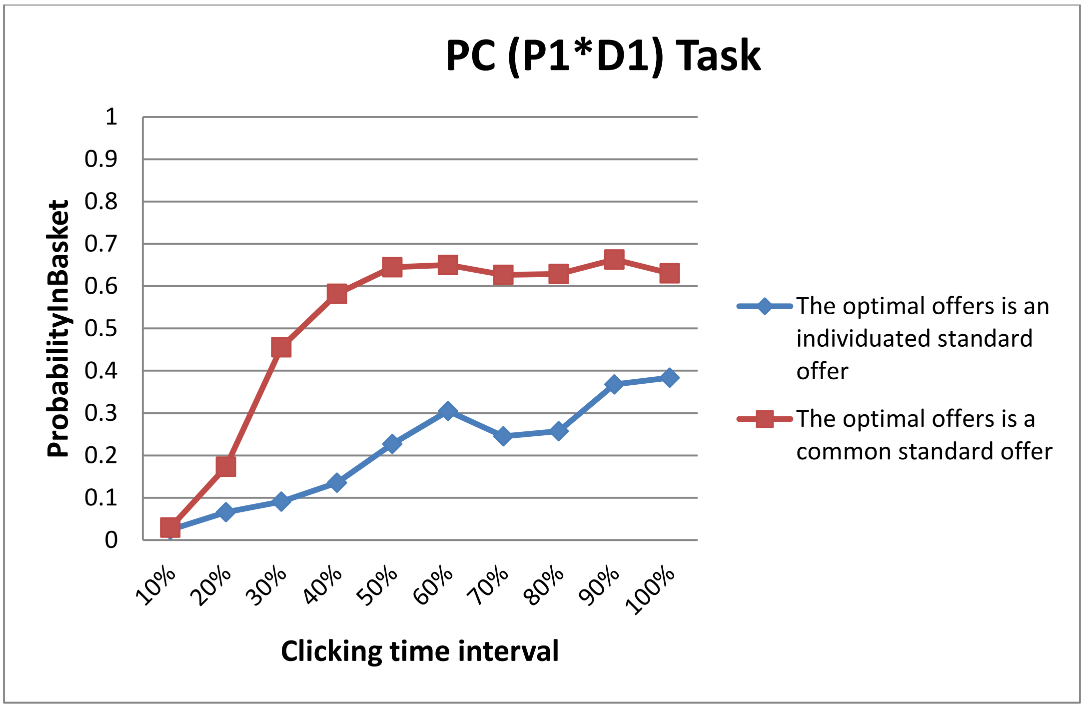

Figure 3.26: ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1) task

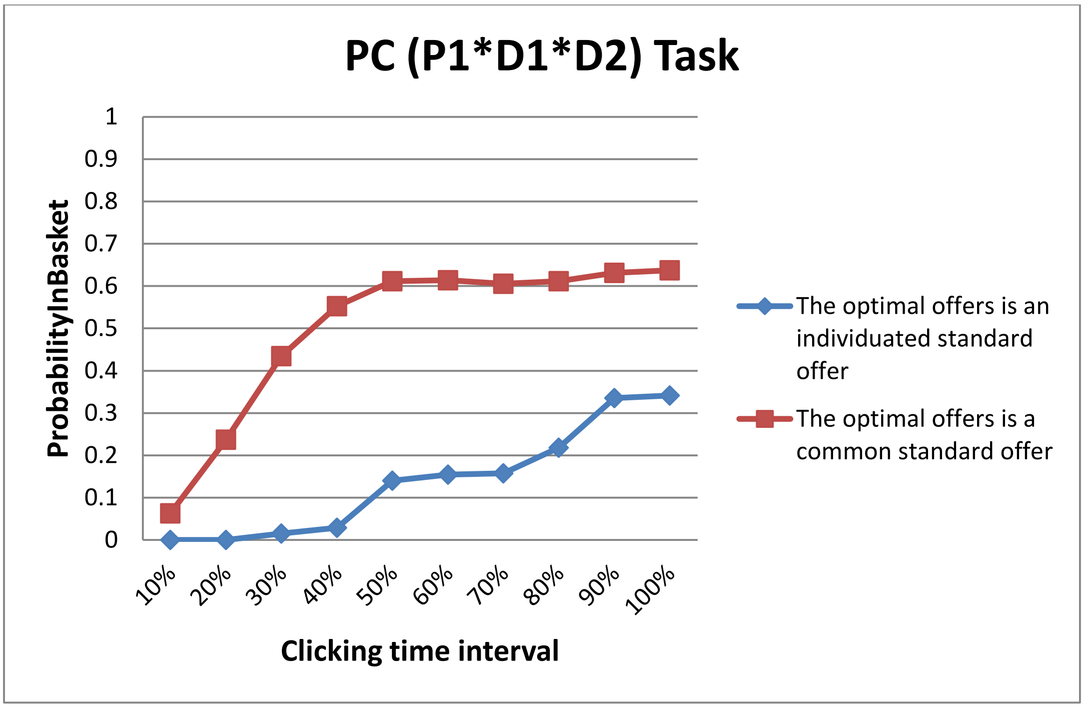

Figure 3.27: ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1*D2) task

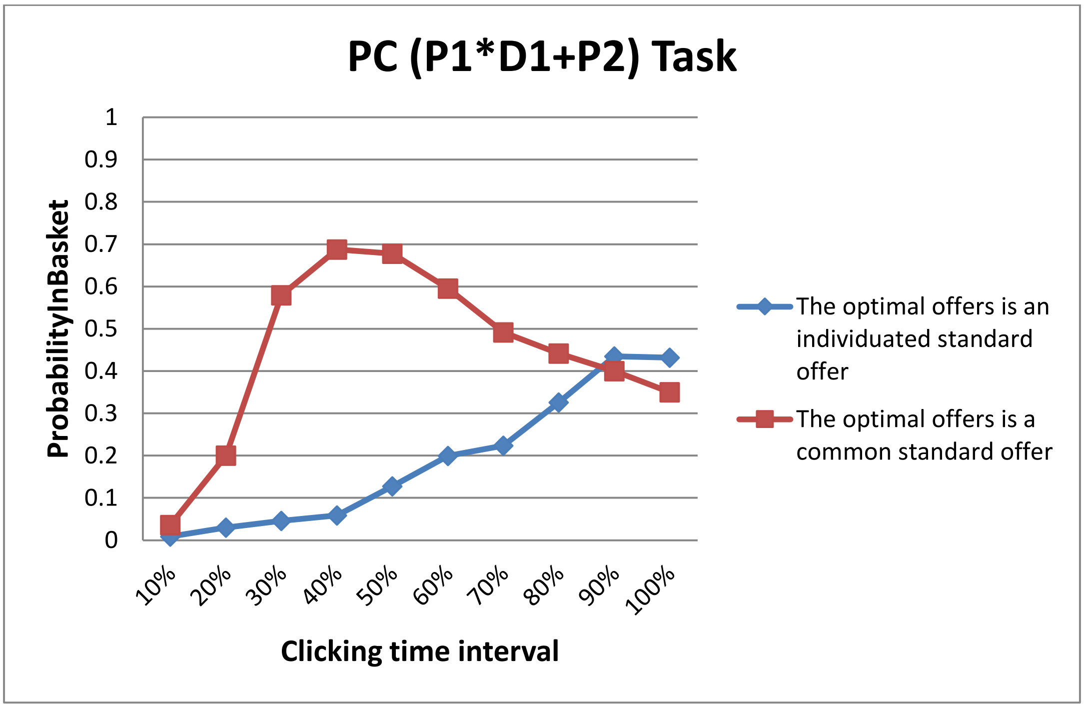

Figure 3.28: ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1+P2) task

In all three PC tasks, when the optimal offer is a common standard offer, its ProbabilityInBasket increases sharply during the first four clicking time intervals, reaching a value of around 0.6 or 0.7. In the P1*D1 and P1*D1*D2 tasks, this probability remains stable for the rest of the task; in the P1*D1+P2 task, it falls steadily to a final value of about 0.35. When the optimal offer is an individuated standard offer, its Probability-In-Basket increases throughout the task but, except in the final two intervals of the P1*P2+D1 task, is always below the corresponding probability for common standard offers. The trend of ProbabilityInBasket for the optimal offer clearly differs according to whether the optimal offer is a common or individuated standard offer. This again implies that many participants used a heuristic related to the common standard.

Recall that participants could only buy an offer if it was in the shopping basket, so if all participants were cognitively unlimited, ProbabilityInBasket for the optimal offer in the last clicking time interval would be very close to one. However, when the optimal offer is a common standard offer, this probability is 0.631, 0.637 and 0.350 in the P1*D1, P1*D1*D2 and P1*D1+P2 tasks respectively; when the optimal offer is an individuated standard offer, it is 0.384, 0.342 and 0.432 in the P1*D1, P1*D1*D2 and P1*D1+P2 tasks respectively. The fact that none of these values is close to 1 implies that many participants were constrained by the complexity of the PC tasks, and did not end up choosing the optimal offer (both when this was a common standard offer and when it was an individuated standard offer).

Consider the case in which the optimal offer is a common standard offer. If all participants employ the DE heuristic and some of them are cognitively constrained, ProbabilityInBasket for the optimal offer will increase sharply in the early part of the task (when participants are comparing common standard offers and eliminating those that are not cheapest), but is likely to decrease later in the task (when participants are comparing the optimal offer with sub-optimal individuated standard offers) since some participants may mistakenly move the optimal offer out of the shopping basket.

This pattern is observed in the P1*D1+P2 task, but not in the P1*D1 and P1*D1*D2 tasks. To try to understand why this is the case, it is useful to look in more detail at movements of the optimal offer into and out of the shopping basket. Table 3.8 shows, for every task in the experiment, the number of participants who moved the optimal offer out of the shopping basket during the course of a task. This table shows that in AC tasks, once the optimal offer had been put into the shopping basket, it was very unlikely to be moved out. The number of participants who moved the optimal offer out of the shopping basket was much higher in NC and PC tasks. Notice that in all PC tasks, optimal offers that were also common standard offers were sometimes moved out of the shopping basket, but this occurred most frequently in the PC (P1*D1+P2) task.

Tasks	Number of participants who moved the optimal offer, which is also a common standard offer, out of the shopping basket	Number of participants who moved the optimal offer, which is also an individuated standard offer, out of the shopping basket	Total
AC			
P1	2		2
P1*D1	0		0
P1*D1*D2	1		1
P1*D1+P2	4		4
PC			
P1*D1	15	14	29
P1*D1*D2	9	24	33
P1*D1+P2	28	24	52
NC			
P1*D1		36	36
P1*D1*D2		31	31
P1*D1+P2		41	41

Table 3.8: The number of participants who moved the optimal offer out of the shopping basket during the course of a task

Note: There are 171 observations in each task

Tables K30 to K42 in Appendix K provide further details about the movements of the optimal offer into and out of the shopping basket over the course of each task. For each clicking time interval, these tables show how many participants had the optimal offer in the shopping basket at the beginning of that interval, how many of them kept that optimal offer in the shopping basket and how many of them moved it out of the shopping basket during the clicking time interval.[74] Tables C30 to C36 show that participants were more likely to make mistakes in NC tasks than in AC tasks. Tables C37 and C39 show that, in the later intervals of PC tasks in which the optimal offer was a common standard offer, some participants were moving the optimal offer into the shopping basket while others were moving it out. In the PC (P1*D1) and PC (P1*D1*D2) tasks, these movements approximately offset one another, producing the aggregate stability of ProbabilityInBasket over these intervals that was

[74] According to the experimental design, no offer can be put into the shopping basket at the first click. So the number of participants for whom the optimal offer is in the shopping basket is at the beginning of the 0%-10% clicking time interval is always 0. For this reason, the clicking time intervals in these tables start from the 10%-20% clicking time interval.

observed in Figures 3.26 and 3.27. A possible explanation is that a significant minority of participants did not use the DE heuristic, and that these participants were responsible for the movement of optimal offers into the basket in the later intervals. (This explanation is also supported by the evidence, shown in Figures 3.7 to 3.9 above, that in the later intervals of PC tasks, between 10% and 20% of inspections were of common standard offers.)

Result 4: Many participants are cognitively constrained. The majority of the participants are able to find the optimal offer if all offers have a common standard. However, participants are more likely to make mistakes when comparing individuated standard offers, and as a result, many participants choose suboptimal offers when not all offers have a common standard.

There is clear evidence that many participants employ the DE heuristic when they face complex decision problems involving a mix of common and individuated standards. Nevertheless, as shown above, if participants are cognitively constrained, they may not be able to maximise their monetary payoffs in problems of this kind. In this case, the DE heuristic is not necessarily the optimal heuristic in terms of earnings.

Table 3.9 shows the average final earnings for participants and the hypothetical earnings if every participant always used the LCS heuristic and did not make errors. In AC tasks, the LCS heuristic always chooses the optimal offer. Actual earnings in these tasks are quite close to the hypothetical values. This indicates that most participants were able to find the cheapest of 24 common standard offers. It is therefore reasonable to assume that most participants had the cognitive ability to use the LCS heuristic accurately in PC tasks (where that heuristic would require them only to find the cheapest of eight common standard offers).

Tasks	Actual earnings	Hypothetical earnings
AC		
P1	11.40	11.73
	(1.36)	
P1*D1	11.26	11.73
	(1.64)	
P1*D1*D2	11.05	11.73
	(2.06)	
P1*D1+P2	11.23	11.73
	(1.71)	
Average	11.23	11.73
	(1.71)	
PC		
P1*D1	10.41	11.09
	(1.94)	(0.72)
P1*D1*D2	9.43	11.04
	(2.44)	(0.78)
P1*D1+P2	10.04	11.12
	(2.04)	(0.66)
Average	9.96	11.08
	(2.19)	(0.72)
NC		
P1*D1	10.11	N/A
	(2.00)	
P1*D1*D2	9.26	N/A
	(2.52)	
P1*D1+P2	10.11	N/A
	(2.07)	
Average	9.83	N/A
	(2.24)	

Table 3.9: Actual final earnings for participants and the hypothetical earnings if every participant always used the LCS heuristic

Note: Numbers in the parentheses are standard deviations. There are 171 observations in each task.

However, in PC tasks, actual average earnings (£9.96) are significantly lower than hypothetical earnings (£11.08) (sign test, pooled data, $z = 7.48$, $p<0.001$). The difference between actual and hypothetical earnings in the P1*D1 task is not significant (sign test, $z = 1.52$, $p=0.13$), but it is significant in the P1*D1*D2 and P1*D1+P2 tasks (sign test, $z = 6.53$, $p<0.001$; $z = 4.80$, $p<0.001$, respectively). Since the P1*D1 task has the least complex price structure, these results suggest that when the price structure is relatively complex, the DE heuristic is less efficient than the LCS heuristic in terms of final earnings.

Result 5: For cognitively constrained participants facing complex decision problems involving a mix of common and individuated standards, the LCS heuristic is more efficient than the DE heuristic in terms of maximising monetary payoffs.

3.6 Discussion and conclusion

The experiment reported above has investigated the heuristics consumers employ in specific environments. There are two main findings. First, many consumers are likely to employ the DE heuristic when they face complex decision problems involving homogenous goods priced according to different standards. Second, common standards help consumers to make price comparisons, but if most offers have individuated standards, the presence of a subset of common standard offers does little to improve decision-making. This is because many consumers are cognitively constrained.

The finding that very few consumers employ the LCS heuristic is inconsistent with the assumption made by Gaudeul and Sugden (2012). They assume that there exist some consumers who employ the LCS heuristic in retail markets. Apparently, at least in the short run, this assumption was a bit too optimistic. My results indicate that, at least prior to any long-run learning, consumers are likely to employ the DE heuristic rather than the LCS, SF or common standard unrelated ones. Notice that time constraints were not employed in the present experiment, because in most of the purchasing cases, consumers do not have to make decisions within an explicit time limitation. Possibly, in studies that involve time constraints (e.g. Crosetto and Gaudeul, 2012), some participants may switch from one shortlisting heuristic to another. But this is not the research question that I am trying to explore in the present chapter.

The main reason why many individuals employed the DE heuristic rather than the LCS heuristic may be because of overconfidence (Adams & Adams, 1961; Oskamp, 1965; Camerer & Lovallo, 1999; Malmendier & Tate, 2006). There is evidence indicating that people tend to be more overconfident when facing complex tasks. This is the so called hard-easy effect (Lichtenstein & Fischhoff, 1977). After having found the cheapest common standard offer, consumers who are sufficiently confident about their cognitive abilities will continue inspecting offers in the belief that, if the optimal offer is an individuated standard offer, they will be able to find it. However, further comparisons may reduce their final earnings, because when trying to work out the absolute values for offer prices, the price signals for individuated standard offers contain errors. This is in fact what I found in the experiment. Many participants made mistakes when they compared the cheapest common standard offer to individuated standard offers. The present chapter does not provide direct evidence showing that overconfidence must be the only reason why many participants employed the DE heuristic and this will be interesting for future studies to explore. Research into overconfidence and underconfidence is relevant to domains such as finance (Thomson et al., 2003), eyewitness testimony (Wells et al., 1981; Loftus et al., 1989), autobiographical memory (Barclay & Wellman, 1986), meteorology (Murphy and Winkler, 1984) etc. Many previous studies attempt to express confidence in relation to probability judgments, but there are very few empirical studies exploring people's levels of confidence in their own cognitive abilities. This could be a productive research direction.

In conclusion, my findings suggest that consumers can make purchasing decisions more efficiently when the prices of all goods are expressed in a common standard, but that common standards will benefit consumers only if almost all firms use them. In contrast to what would happen if consumers used the LCS heuristic, the shortlisting heuristic that most consumers are inclined to use – the DE heuristic – gives firms incentives to deviate from common standards. If consumers use the DE heuristic, a pricing regime in which all firms used a common standard would not be self-sustaining because individual firms could benefit by switching to individuated standard offers as a way of exploiting cognitively constrained consumers. These findings challenge conventional assumptions about market efficiency and have implications for the regulation of retail markets. They suggest that, if policy makers want firms to use common standards, they may have to impose them by law. Alternatively, in order to help cognitively constrained consumers, government might promote third party institutions which convert individuated standard prices into a common standard. For example, government could promote or subsidize online price comparison search engines such as Which?, so as to protect cognitively constrained consumers from being exploited by firms that provide suboptimal individuated standard offers.

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Appendices

Chapter 1

A. Experimental Instructions
B. An example of experimental task's screen shots
C. Regression Analysis

Chapter 2

D. Experimental Instructions
E. Tariffs and Tariff Tasks
F. Regression Analysis
G. Time Trend of Consumer Performance
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Chapter 3

I. Experimental Instructions
J. An example of experimental task's screen shots
K. Tables

Appendix A

Experimental instructions (WTA)

Welcome to the experiment and thank you for coming. In this experiment you will be making a series of choices involving money and various goods. Everyone who takes part will earn some money or get some goods at the end of the experiment. We have cash and goods with us, so you will be paid before you leave today.

We shall say more about what will be involved in the experiment soon. Before we do this, I would like to set some ground rules, which you must all observe. There must be no talking during the experiment unless you want to ask me a question – in which case, simply raise your hand and one of the experimenters will come to you. You must not attempt to look at what other people are doing.

Also, please follow all instructions on your computer screen. There will be times during the experiment when you will be required to wait for others. Please wait patiently and please do not attempt to open any other application on your computer.

Please keep to these simple rules, because anyone breaking them may be asked to leave the experiment without any reward.

We are now ready to describe the nature of the tasks within the experiment.

Tasks

There are 11 periods in the experiment and at the end of the experiment your payoff will be determined by your decisions in one of these periods which the computer will pick randomly.

In each period you are *endowed* with one good. The good may be a food item, some other consumer product, some National Lotteries, or a 'win-win gamble'.

If this period is picked by the computer to determine your payoff and you choose to keep the good this good will be yours.

A win-win gamble has three different amounts of money, one of which you will win; the outcome of the gamble is determined by the computer.

Depending on what you have said you prefer to do, you will either keep the good or sell it for money.

There are two tasks in each period. In the first task, you will be asked to answer either one or two questions.

In the second task, you will be asked 25 questions. Each of these asks you how you would respond to an opportunity to sell the good you have been endowed with. You are asked to consider an offer of a stated amount of money, and you have to say whether or not you would accept this offer.

All 25 questions are shown on the same screen, with the lowest offer at the top and the highest offer at the bottom. To simplify this task, if in any question you say "Yes" to an offer of "x" for your good, the computer will assume that you accept all offers greater than "x" and so it will automatically answer "Yes" for you to these questions. And vice versa, if in any question you say "No" to an offer of "y" for your good, the computer will assume that you reject all offers less than "y" and so it will automatically answer "No" for you to these questions.

Whenever you are asked to answer a question, the screen will have either a RED or BLUE background. If a question has a RED background, there is a chance that the question is for real. We will explain what this means later.

After you have completed the tasks in all 11 periods, you can click OK and you will be asked to wait until everyone in this room has completed their tasks.

For each question, we want you to give an honest and considered response. For example, in a task in which you have been endowed with a win-win gamble, you should accept any offer which, FROM YOUR PERSONAL PERSPECTIVE, exceeds the value TO YOU of playing out the lottery. Similarly, you should reject any offer which YOU think is worth less to YOU than playing out the win-win gamble. The same applies when you have been endowed with other goods.

You may find that some of the tasks are quite similar to one another, but it is important for the experiment that you complete all of them.

So let me stress that there are no tricks involved in our tasks, neither are there right or wrong answers. We simply want you to give honest and considered responses; and it is in your interests to do so.

One of the red background tasks you face is for real. At the end of the experiment the computer will randomly pick one of the red background tasks and within that period, the computer will randomly pick one of the questions. That question will have asked you to consider an offer of a specific amount of money in return for the good you were endowed with in that period. If you chose to accept that offer, you will receive that amount of money instead of the good. We will pay you immediately in cash. If you chose to reject the offer, you will keep the good.

If the good is a food item, a consumer product or some National Lotteries, you will be given the good to take away. If it is a win-win gamble, the computer will run it and tell you the result; you will then be given the amount of money you won.

Before we move to the first task, please raise your hand if you have any questions. Before starting to take decisions, we ask you to answer the questionnaire in next several screens, with the only purpose of checking whether you have understood these instructions.

Experimental instructions (WTP)

Welcome to the experiment and thank you for coming. In this experiment you will be making a series of choices involving money and various goods. Everyone who takes part will earn some money or get some goods at the end of the experiment. We have cash and goods with me and so you will be paid before you leave today.

We shall say more about what will be involved in the experiment soon. Before we do this, we would like to set some ground rules, which you must all observe. There must be no talking during the experiment unless you want to ask me a question – in which case, simply raise your hand and one of the experimenters will come to you. You must not attempt to look at what other people are doing.

Also, please follow all instructions on your computer screen. There will be times during the experiment when you will be required to wait for others. Please wait patiently and please do not attempt to open any other application on your computer.

Please keep to these simple rules, because anyone breaking them may be asked to leave the experiment without any reward.

We are now ready to describe the nature of the tasks within the experiment.

Tasks

There are 11 periods in the experiment and at the end of the experiment your payoff will be determined by your decisions in one of these periods which the computer will pick randomly.

In each period you are endowed with £12 and you have an opportunity to buy a good or some goods. The good may be a food item, some other consumer product, some National

Lotteries, or a "win-win gamble". If this period is picked by the computer to determine your payoff and you choose to buy the good, this good will be yours.

A win-win gamble has three different amounts of money, one of which you will win; the outcome of the gamble is determined by the computer.

Depending on what you have said you prefer to do, you will either keep all of the £12 or you will spend some or all of that money to buy a good.

There are two tasks in each period. In the first task, you will be asked to answer either one or two questions.

In the second task, you will be asked 25 questions. Each of these asks you how you would respond to an opportunity to buy the good by using some or all of the money you have been endowed with. You are asked to consider a stated money price, and you have to say whether or not you would pay this price.

All 25 questions are shown on the same screen, with the lowest price at the top and the highest price at the bottom. To simplify this task, if in any question you say "Yes" to a price of "x" for your good, the computer will assume that you would pay all prices lower than "x" and so it will automatically answer "Yes" for you to these questions. And vice versa, if in any question you say "No" to a price of "y" for your good, the computer will assume that you reject all prices higher than "y" and so it will automatically answer "No" for you to these questions.

Whenever you are asked to answer a question, the screen will have either a RED or BLUE background. If a question has a RED background, there is a chance that the question is for real. We will explain what this means later.

After you have completed the tasks in all 11 periods, you can click OK and you will be asked to wait until everyone in this room has completed their tasks.

For each question, we want you to give an honest and considered response. For example, in a task in which you have the opportunity to buy a win-win gamble, you should pay any price which, FROM YOUR PERSONAL PERSPECTIVE, is less than the value TO YOU of playing out the win-win gamble. Similarly, you should reject any price which YOU think is worth more to YOU than playing out the win-win gamble. The same applies when you have opportunities to buy other goods.

It is important for the experiment that you complete all of them.

So let me stress that there are no tricks involved in our tasks, neither are there right or wrong answers. We simply want you to give honest and considered responses; and it is in your interests to do so.

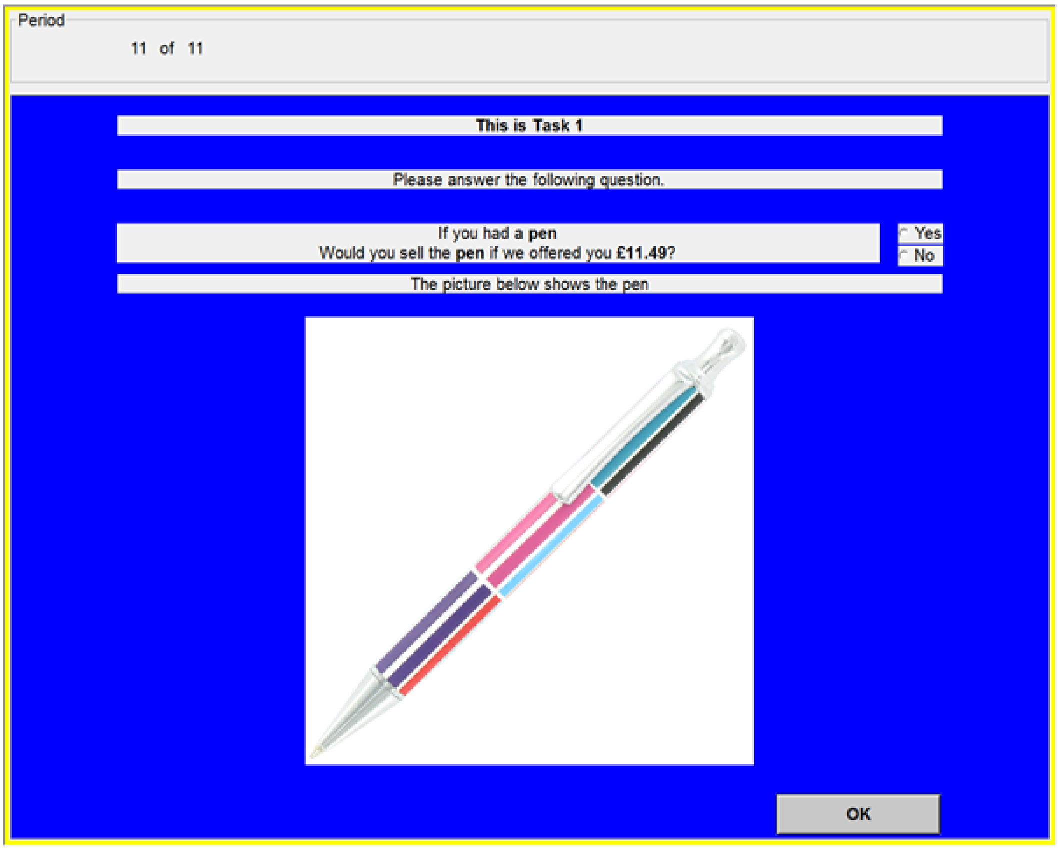
One of the red background tasks you face is for real. At the end of the experiment the computer will randomly pick one of the red background tasks and within that task, the computer will randomly pick one of the questions. That question will have asked you to consider a specific price for buying the good that was being offered in that period. If you chose not to pay that price, you will keep all of your £12 endowment. We will pay you immediately in cash. If you chose to pay that price, you will receive the good and the price will be deducted from your £12 endowment.

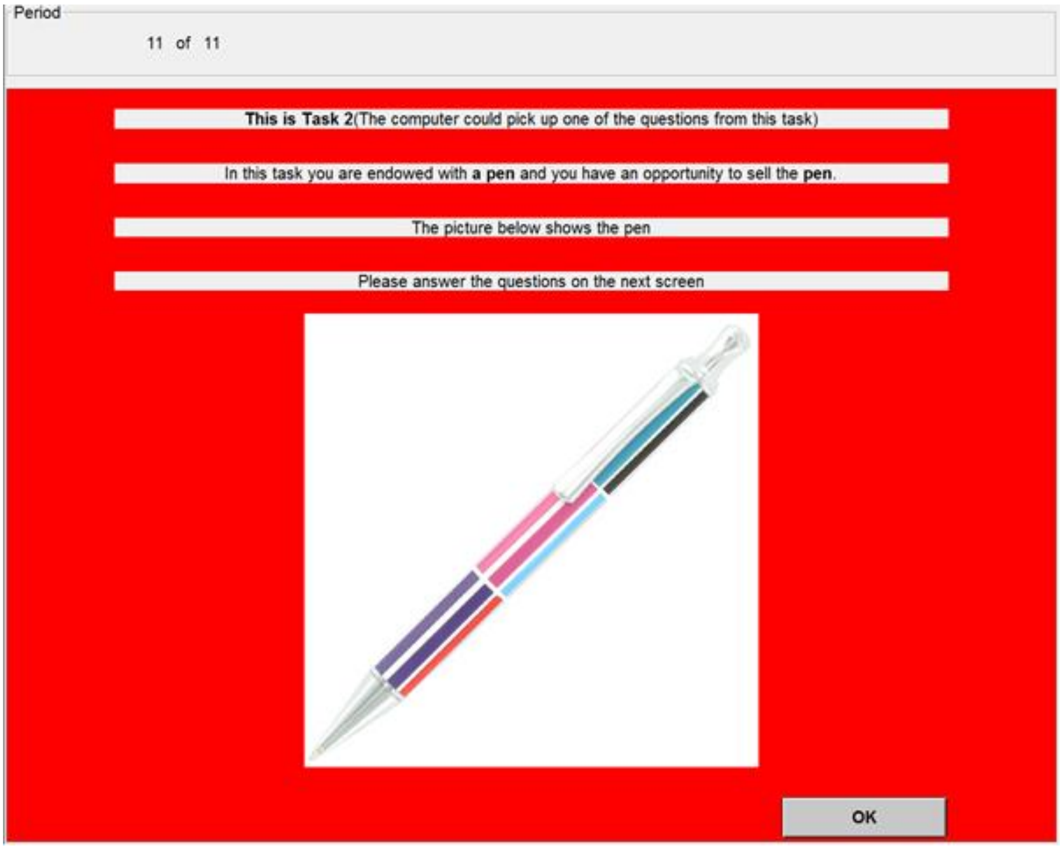
If the good is a food item, a consumer product or some National lotteries, you can take the good away. If it is a win-win gamble, the computer will run it and tell you the result; you will then be given the amount of money you won.

Before we move to the first task, please raise your hand if you have any questions. Before starting to take decisions, we ask you to answer the questionnaire in next several screens, with the only purpose of checking whether you have understood these instructions.

Appendix B

An example of experimental task's screen shots





Period

11 of 11

If I am offered **£0.01** for the **pen** , I will sell.	Yes	No
If I am offered **£0.50** for the **pen** , I will sell.	Yes	No
If I am offered **£1.00** for the **pen** , I will sell.	Yes	No
If I am offered **£1.50** for the **pen** , I will sell.	Yes	No
If I am offered **£2.00** for the **pen** , I will sell.	Yes	No
If I am offered **£2.50** for the **pen** , I will sell.	Yes	No
If I am offered **£3.00** for the **pen** , I will sell.	Yes	No
If I am offered **£3.50** for the **pen** , I will sell.	Yes	No
If I am offered **£4.00** for the **pen** , I will sell.	Yes	No
If I am offered **£4.50** for the **pen** , I will sell.	Yes	No
If I am offered **£5.00** for the **pen** , I will sell.	Yes	No
If I am offered **£5.50** for the **pen** , I will sell.	Yes	No
If I am offered **£6.00** for the **pen** , I will sell.	Yes	No
If I am offered **£6.50** for the **pen** , I will sell.	Yes	No
If I am offered **£7.00** for the **pen** , I will sell.	Yes	No
If I am offered **£7.50** for the **pen** , I will sell.	Yes	No
If I am offered **£8.00** for the **pen** , I will sell.	Yes	No
If I am offered **£8.50** for the **pen** , I will sell.	Yes	No
If I am offered **£9.00** for the **pen** , I will sell.	Yes	No
If I am offered **£9.50** for the **pen** , I will sell.	Yes	No
If I am offered **£10.00** for the **pen** , I will sell.	Yes	No
If I am offered **£10.50** for the **pen** , I will sell.	Yes	No
If I am offered **£11.00** for the **pen** , I will sell.	Yes	No
If I am offered **£11.50** for the **pen** , I will sell.	Yes	No
If I am offered **£12.00** for the **pen** , I will sell.	Yes	No

OK

Appendix C

Regression results for different commodity

WTA OLS cluster 6 regressions

Commodity	**Win-win gamble** (180 obs.)	**Two bottles of Chinese sauce** (180 obs.)	**Box of chocolate** (180 obs.)	**Towel** (180 obs.)	**Pen** (180 obs.)	**Five National Lottery scratch cards** (108 obs.)
Period	-0.035	0.182***	-0.032	0.038	-0.129*	0.020
	(0.08)	(0.06)	(0.07)	(0.40)	(0.08)	(0.44)
High anchor	0.384	0.596***	0.607***	0.402**	0.494**	0.445*
	(0.24)	(0.21)	(0.19)	(0.16)	(0.22)	(0.26)
Constant	6.134***	2.797***	5.769***	4.613***	4.196***	5.848***
	(0.57)	(0.40)	(0.48)	(0.48)	(0.63)	(0.53)

Observations: 108 participants over 11 periods

Estimation methods: OLS with clustering the variable “subjects”.

Dependent Variable: Participants’ WTA

Independent Variables: Period, High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** $p<0.01$, **$p<0.05$, *$p<0.10$) of 9 regressions using different anchor types.

WTP OLS cluster 6 regressions

Commodity	**Win-win gamble** (200 obs.)	**Two bottles of Chinese sauce** (200 obs.)	**Box of chocolate** (200 obs.)	**Towel** (200 obs.)	**Pen** (200 obs.)	**Five National Lottery scratch cards** (200 obs.)
Period	-0.089* (0.05)	-0.031 (0.02)	-0.031 (0.04)	-0.005 (0.31)	-0.019 (0.02)	-0.108** (0.05)
High anchor	0.099 (0.12)	0.163*** (0.06)	0.157* (0.08)	0.037 (0.07)	0.045 (0.05)	0.170* (0.09)
Constant	2.650*** (0.45)	1.015*** (0.19)	1.786*** (0.28)	1.599*** (0.26)	0.652*** (0.15)	2.674*** (0.43)

Observations: 120 participants over 11 periods

Estimation methods: OLS with clustering the variable “subjects”.

Dependent Variable: Participants’ WTP

Independent Variables: Period, High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

WTA random effect 6 regressions

Commodity	**Win-win gamble** (180 obs.)	**Two bottles of Chinese sauce** (180 obs.)	**Box of chocolate** (180 obs.)	**Towel** (180 obs.)	**Pen** (180 obs.)	**Five National Lottery scratch cards** (108 obs.)
Period	0.020	0.126***	0.056*	0.005	-0.016	0.060
	(0.05)	(0.04)	(0.03)	(0.03)	(0.04)	(0.05)
High anchor	0.392	0.625***	0.663***	0.400**	0.563***	0.434*
	(0.24)	(0.21)	(0.17)	(0.16)	(0.20)	(0.26)
Constant	5.787***	3.117***	5.201***	4.807***	3.473***	5.622***
	(0.46)	(0.38)	(0.38)	(0.36)	(0.43)	(0.46)

Observations: 108 participants over 11 periods

Estimation methods: Random effect regression.

Dependent Variable: Participants' WTA

Independent Variables: Period, High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

WTP random effect 6 regressions

Commodity	**Win-win gamble** (200 obs.)	**Two bottles of Chinese sauce** (200 obs.)	**Box of chocolate** (200 obs.)	**Towel** (200 obs.)	**Pen** (200 obs.)	**Five National Lottery scratch cards** (200 obs.)
Period	-0.078** (0.03)	-0.022* (0.01)	-0.052** (0.04)	-0.027* (0.01)	-0.016 (0.01)	-0.043** (0.02)
High anchor	0.096 (0.12)	0.161*** (0.05)	0.141 (0.09)	0.044 (0.07)	0.044 (0.05)	0.158* (0.08)
Constant	2.587*** (0.26)	0.957*** (0.14)	1.923*** (0.19)	1.720*** (0.21)	0.637*** (0.11)	2.295*** (0.24)

Observations: 120 participants over 11 periods

Estimation methods: Random effect regression.

Dependent Variable: Participants' WTA

Independent Variables: Period, High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

Regression results for different types of anchors

WTA OLS cluster 9 regressions

Anchor types	**Baseline** (216 obs.)	**Implausible price** (108 obs.)	**Similar good** (108 obs.)	**Dissimilar good** (108 obs.)	**Incentivized** (108 obs.)	**Passive number search** (108 obs.)	**Passive price search** (108 obs.)	**Active number search** (108 obs.)	**Active price search** (108 obs.)
Period	-0.060	0.039	-0.041	0.018	0.071	0.072	0.076	-0.068	0.088
	(0.08)	(0.08)	(0.09)	(0.10)	(0.08)	(0.10)	(0.09)	(0.10)	(0.06)
Chinese sauce	-0.866	-1.985*	-4.541***	-1.558	-2.481**	-2.025	-1.037	-3.438**	-0.693
	(1.08)	(1.17)	(1.07)	(1.46)	(1.23)	(1.41)	(1.41)	(1.42)	(1.03)
Chocolate	-0.129	0.178	-3.144***	0.738	3.166**	-2.119*	-0.907	-1.769	1.724
	(1.04)	(1.37)	(0.84)	(1.47)	(1.37)	(1.11)	(1.46)	(1.57)	(0.95)
Towel	-0.981	-0.951	-2.672*	-0.760	0.162	-1.544	-1.618	-0.418	-0.917
	(0.98)	(1.06)	(1.43)	(1.42)	(1.45)	(1.29)	(1.37)	(1.47)	(0.83)
Pen	-1.377	-3.431***	-4.313***	-3.249***	-1.139	-2.424	-1.179	-2.739*	-3.011***
	(1.15)	(1.05)	(0.82)	(1.21)	(1.49)	(1.59)	(1.46)	(1.55)	(0.83)
National Lottery scratch cards	0.053	2.062*	-2.482**	1.525	1.392	-1.571	-0.336	0.068	0.101
	(1.14)	(1.12)	(0.95)	(1.59)	(1.31)	(1.21)	(1.36)	(1.55)	(1.35)
High anchor	0.764***	0.284	0.188	-0.048	0.685**	0.933***	0.590*	0.711***	0.745**
	(0.18)	(0.28)	(0.33)	(0.29)	(0.30)	(0.20)	(0.34)	(0.25)	(0.29)
Constant	5.698***	5.734***	7.849***	5.257***	4.241***	5.952***	5.315***	7.026***	4.696***
	(0.99)	(1.00)	(0.82)	(1.18)	(1.07)	(1.10)	(1.07)	(1.20)	(0.65)

Observations: 108 participants over 11 periods

Estimation methods: OLS with clustering the variable "subjects".

Dependent Variable: Participants' WTA

Independent Variables: Period, Good dummies (Chinese sauce, Chocolate, Towel, Pen, National Lottery scratch cards; Win-win gamble is the baseline dummy), High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

WTA random effects 9 regressions

Anchor types	Baseline (216 obs.)	Implausible price (108 obs.)	Similar good (108 obs.)	Dissimilar good (108 obs.)	Incentivized (108 obs.)	Passive number search (108 obs.)	Passive price search (108 obs.)	Active number search (108 obs.)	Active price search (108 obs.)
Period	0.029	0.053	0.069	0.013	0.054	0.044	0.058	0.031	0.067
	(0.04)	(0.06)	(0.06)	(0.06)	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)
Chinese sauce	-0.797	-1.980*	-4.505***	-1.558	-2.479*	-2.036	-1.048	-3.367**	-0.700
	(1.07)	(1.15)	(1.22)	(1.40)	(1.39)	(1.37)	(1.40)	(1.49)	(1.21)
Chocolate	-0.020	0.181	-3.205***	0.734	3.153**	-2.127	-0.870	-1.862	1.717
	(1.07)	(1.15)	(1.22)	(1.40)	(1.40)	(1.37)	(1.41)	(1.49)	(1.21)
Towel	-0.906	-0.953	-2.733**	-0.757	0.136	-1.527	-1.636	-0.336	-0.917
	(1.07)	(1.15)	(1.22)	(1.40)	(1.40)	(1.37)	(1.40)	(1.49)	(1.21)
Pen	-1.353	-3.427***	-4.442***	-3.250**	-1.166	-2.453*	-1.163	-2.794*	-3.022**
	(1.07)	(1.15)	(1.22)	(1.40)	(1.40)	(1.37)	(1.40)	(1.49)	(1.21)
National Lottery scratch cards	0.182	2.064*	-2.604**	1.525	1.378	-1.597	-0.354	0.090	0.104
	(1.07)	(1.15)	(1.22)	(1.40)	(1.40)	(1.37)	(1.40)	(1.49)	(1.21)
High anchor	0.675***	0.292	0.231	-0.048	0.676**	0.916***	0.577*	0.647***	0.721***
	(0.18)	(0.28)	(0.31)	(0.28)	(0.29)	(0.18)	(0.34)	(0.24)	(0.26)
Constant	5.112***	5.650***	7.235***	5.281***	4.359***	6.131***	5.426***	6.465***	4.847***
	(0.80)	(0.89)	(0.95)	(1.05)	(1.06)	(1.00)	(1.09)	(1.09)	(0.93)

Observations: 108 participants over 11 periods

Estimation methods: Random effect regression.

Dependent Variable: Participants' WTA

Independent Variables: Period, Good dummies (Chinese sauce, Chocolate, Towel, Pen, National Lottery scratch cards; Win-win gamble is the baseline dummy), High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

WTP OLS with cluster 9 regressions

Anchor types	**Baseline** (240 obs.)	**Implausible price** (120 obs.)	**Similar good** (120 obs.)	**Dissimilar good** (120 obs.)	**Incentivized** (120 obs.)	**Passive number search** (120 obs.)	**Passive price search** (120 obs.)	**Active number search** (120 obs.)	**Active price search** (120 obs.)
Period	-0.052	-0.057	0.015	-0.064**	0.027	-0.104**	-0.033	-0.080	-0.100*
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)	(0.08)	(0.05)
Chinese sauce	-0.788*	-1.998*	-1.529**	-0.573	-0.607	-1.632**	-1.448**	-2.421**	-0.631
	(0.40)	(1.11)	(0.67)	(0.41)	(0.61)	(0.62)	(0.67)	(1.15)	(0.45)
Chocolate	-0.806**	-1.548	-0.359	1.457**	0.035	-0.550	-0.992	-2.095*	0.704
	(0.36)	(1.12)	(0.82)	(0.61)	(0.68)	(0.66)	(0.69)	(1.07)	(0.55)
Towel	0.137	-0.732	-1.678***	0.434	-0.932	-0.849	-1.493**	-1.259	0.469
	(0.67)	(1.29)	(0.61)	(0.63)	(0.55)	(0.77)	(0.62)	(1.22)	(0.63)
Pen	-1.340***	-2.538**	-1.943***	-0.607*	-1.495***	-2.101***	-0.876	-3.014***	-0.779*
	(0.37)	(1.06)	(0.70)	(0.35)	(0.49)	(0.53)	(0.69)	(1.09)	(0.45)
National Lottery scratch cards	0.256	-0.556	-0.741	0.130	0.346	0.107	-0.452	-0.319	0.585
	(0.54)	(1.33)	(0.73)	(0.46)	(0.64)	(1.11)	(0.80)	(1.42)	(0.90)
High anchor	0.078	0.073	0.050	0.129	0.163	0.058	0.253*	0.000	0.187**
	(0.07)	(0.08)	(0.10)	(0.08)	(0.11)	(0.09)	(0.14)	(0.19)	(0.09)
Constant	2.261***	3.058***	2.455***	1.392***	1.672***	2.929***	2.262***	3.856***	1.927***
	(0.37)	(1.11)	(0.59)	(0.33)	(0.50)	(0.60)	(0.66)	(1.37)	(0.40)

Observations: 120 participants over 11 periods

Estimation methods: OLS with clustering the variable "subjects".

Dependent Variable: Participants' WTP

Independent Variables: Period, Good dummies (Chinese sauce, Chocolate, Towel, Pen, National Lottery scratch cards; Win-win gamble is the baseline dummy), High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types.

WTP random effects 9 regressions

Anchor types	Baseline (240 obs.)	Implausible price (120 obs.)	Similar good (120 obs.)	Dissimilar good (120 obs.)	Incentivized (120 obs.)	Passive number search (120 obs.)	Passive price search (120 obs.)	Active number search (120 obs.)	Active price search (120 obs.)
Period	-0.034**	-0.021	-0.023	-0.020	-0.031	-0.034*	-0.038	-0.086**	-0.057***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)
Chinese sauce	-0.805	-2.044**	-1.515**	-0.575	-0.642	-1.625**	-1.448**	-2.411**	-0.692
	(0.53)	(0.94)	(0.68)	(0.58)	(0.57)	(0.81)	(0.60)	(0.95)	(0.69)
Chocolate	-0.800	-1.611*	-0.333	1.486**	0.119	-0.550	-0.991*	-2.081**	0.670
	(0.53)	(0.94)	(0.68)	(0.58)	(0.57)	(0.81)	(0.60)	(0.96)	(0.69)
Towel	0.141	-0.743	-1.732**	0.412	-0.969*	-0.782	-1.501**	-1.250	0.397
	(0.53)	(0.94)	(0.68)	(0.58)	(0.57)	(0.81)	(0.60)	(0.95)	(0.69)
Pen	-1.330**	-2.560***	-1.956***	-0.651	-1.501***	-2.097***	-0.872	-3.004***	-0.777
	(0.53)	(0.94)	(0.68)	(0.58)	(0.57)	(0.81)	(0.60)	(0.95)	(0.69)
National Lottery scratch cards	0.254	-0.583	-0.763	0.126	0.300	0.103	-0.452	-0.315	0.517
	(0.53)	(0.94)	(0.68)	(0.58)	(0.57)	(0.81)	(0.60)	(0.95)	(0.69)
High Anchor	0.093	0.090	0.070	0.126*	0.188*	0.092	0.257*	0.007	0.150**
	(0.07)	(0.08)	(0.09)	(0.07)	(0.11)	(0.08)	(0.14)	(0.18)	(0.07)
constant	2.141***	2.865***	2.688***	1.130***	2.024***	2.468***	2.293***	3.882***	1.745***
	(0.39)	(0.67)	(0.50)	(0.42)	(0.43)	(0.59)	(0.45)	(0.70)	(0.49)

Observations: 120 participants over 11 periods

Estimation methods: Random effect regression.

Dependent Variable: Participants' WTP

Independent Variables: Period, Good dummies (Chinese sauce, Chocolate, Towel, Pen, National Lottery scratch cards; Win-win gamble is the baseline dummy), High anchor (High anchor=1 if the anchor is high; 0 else)

This table shows the coefficients, standard errors (in brackets) and significant levels (*** p<0.01, **p<0.05, *p<0.10) of 9 regressions using different anchor types

Appendix D

Experimental Instructions

The experimental instructions were common across treatments except where specified otherwise below in square brackets.

This is an experiment on the choices of services such as electricity and gas. You will be asked to choose one service (Good A) or two services (Good A and Good B) for which a number of tariffs are available. You will then be asked to choose how much of each service you wish to buy. The experiment is divided into 36 tasks, in each of which you can earn experimental points.

The Tasks

In each task you will be given a choice of 4 or 24 tariffs. Tariffs can be either for one good (Good A) or two goods (Good A and Good B) depending on the task. In each task you will be told whether the tariffs are for one good or two goods. You will pay for what you buy based on the tariff that you are on.

[*DE mDE DEAI DEA DEAD WDEAD WDEA BDEAD and BDEA treatments*]

There is a default tariff that you are automatically assigned to and it is up to you whether to keep it or switch from it. If you decide to switch, you will be given a certain time to decide which tariff you wish to switch to. There is a search engine available to compare tariffs; were you to wish to use it, you will be required to input some information. If the time expires before you choose, you will automatically keep the default tariff.]

[*D treatment*

There is a default tariff that you are automatically assigned to and it is up to you whether to keep it or switch from it. If you decide to switch, you will be given a certain time to decide which tariff you wish to switch to. If the time expires before you choose, you will automatically keep the default tariff.]

[*DF treatment*

There is a default tariff that you are automatically assigned to and it is up to you whether to keep it or switch from it.]

You are asked to choose separately how much of each good you wish to buy. You will be given 5 buying options for each good: 1,000, 2,000, 3,000, 4,000 or 5,000 units. You can buy different amounts of each good.

[*DEA WDEA and BDEA treatments*

The Counting Exercise

At the beginning of each task you will have the chance to do a counting exercise. You can do this exercise for as little or as long as you like until time elapses. If you do it for the entire duration of the task, you will automatically keep the default tariff.]

[*DEAD WDEAD and BDEAD treatments*

The Counting Exercise

You will have the chance to use another computer to do a counting exercise. You can do this for as little or as long as you like until time elapses. If you do it for the entire duration of the task, you will automatically keep the default tariff; you will still need to use the experimental computer, at the end of each task, to choose how much of each good you wish to buy.]

[*DEAI treatment*

The Counting Exercise

You will have the chance to use another computer to browse websites. You can do this for as little or as long as you like until time elapses. If you do it for the entire duration of the task, you will automatically keep the default tariff; you will still need to use the experimental computer, at the end of each task, to choose how much of each good you wish to buy.]

The Tariffs

During the experiment you will be presented with tariffs having different structures. Tariffs can have a Tier 1 price, a Tier 2 price, a Ceiling and a Standing Charge.

Standing Charge. The standing charge is a fixed amount that you pay if you are on a given tariff, independently of how much you consume.

Tier 1 Price. The Tier 1 price is the price that you pay on each unit bought up to a certain number of units, which we label Ceiling. If the ceiling is not specified, then the Tier 1 price applies for any number of units bought.

Tier 2 Price. The Tier 2 price is the price that you pay for each unit bought above the Ceiling.

Two Goods Tariffs

The following table contains illustrative examples of two goods tariffs. While they are not tariffs actually used in the tasks you will face, they will give you an idea of the kind of tariffs that you may encounter.

Example Tariff 1: GOOD A – Tier 1: 34.52; GOOD B – Tier 1: 16.18;
Example Tariff 2: GOOD A – Tier 1: 20.89; Ceiling: 900; Tier 2: 16.52; GOOD B – Tier 1: 15.89; Ceiling: 1100; Tier 2: 12.59;
Example Tariff 3: GOOD A – Standing Charge: 10,875; Tier 1: 30.59; GOOD B – Standing Charge: 6,345; Tier 1: 10.26;

Table 1: Example Tariffs

Example tariff 1. Good A has a Tier 1 price of 34.52. This means that you pay 34.52 points for every unit of Good A you buy. As an illustration, were you to buy 1,000 units, you would pay 34,520 points; and, were you to buy 5,000 units, you would pay five times as much, and so 5 *34,520 = 172,600 points. Similar computations apply to Good B, except that the Tier 1 price is 16.18 for this good.

Example tariff 2. Good A has a Tier 1 price of 20.89, a Ceiling of 900 units and a Tier 2 price of 16.52. This means that you pay 20.89 points per unit for every unit of Good A you buy up to 900 units, and you pay 16.52 for any extra units you buy. As an illustration, were you to buy 1,000 units, you would pay 900 *20.89 = 18,801 and 100 *16.52 = 1,652 points, for a total of 18,801 + 1,652 = 20,453 points. Were you to buy 5,000 units you would pay 900 *20.89 = 18,801 and 4,100 *16.52 = 67,732 points, for a total of 18,801 + 67,732 points = 85,533 points. Similar computations apply to Good B, except that the Tier 1 and Tier 2 prices, and the Ceiling, are different.

Example tariff 3. Good A has a Standing Charge of 10,875 and a Tier 1 price of 30.59, with no Ceiling specified. This means that you always pay 10,875 points and, on top of that, you pay 30.59 points for every unit of Good A you buy. As an illustration, were you to buy 1,000 units, you would pay 1,000 *30.59 = 30,590 on top of the Standing Charge of 10,875, for a total of 10,875 + 30,590 = 41,465 points. Were you to buy 5,000 units, you would pay 5,000 *30.59 = 152,950 points on top of the Standing Charge of 10,875, for a total of 10,875 + 152,950 = 163,825 points. Similar computations apply to Good B, except that the Standing Charge and Tier 1 prices are different.

Returns with Two Goods Tariffs

For every tariff you choose you will get a return that depends on how much you buy. This return is specified in the table below.

Amount Bought	GOOD A Returns	GOOD B Returns
1,000	45,000	35,000
2,000	80,000	61,000
3,000	106,000	78,000
4,000	122,000	88,000
5,000	128,000	89,000

Table 2: Returns on Good A and Good B

As you can see from the table, returns depend on how much you buy. As an illustration, if you buy 1,000 units of Good A, your return is 45,000 points, while if you buy 3,000 units of Good A, your return is 106,000 points; if you buy 1,000 units of Good B, your return is 35,000 points, while if you buy 5,000 units of Good B, your return is 89,000 points.

Earnings with Two Goods Tariffs

Your net earnings are the difference between your returns on the services you buy and how much you spend to buy them. Specifically, for each tariff, they can be computed as follows:

Good A returns MINUS Good A buying cost PLUS Good B returns MINUS Good B buying cost.

Example. Suppose that you choose Example Tariff 1 in Table 1 and you buy 3,000 units of Good A and 4,000 units of Good B. From Table 2, you can find that your return from 3,000 units of Good A is 106,000 points. From Table 1, and using the calculator on the computer display, you can find that you pay 3,000*34.52 = 103,560 points for buying 3,000 units of Good A. Your net earnings from Good A are then 106,000 – 103,560 = 2,440 points. This can also be computed for you by the computer display calculator.

Similarly, from Table 2, you can find that your return from 4,000 units of Good B is 88,000 points. You can also find that you pay 4,000 *16.18 = 64,720 points for buying 4,000 units of Good B. Your net earnings from Good B are then 88,000 – 64,720 = 23,280 points.

Your overall net earnings are equal to (2,440 + 23,280) = 25,720 points. Note that this amount could have been higher or lower had you been on a different tariff and/or bought different amounts of Good A and/or B.

One Good Tariffs

One good tariffs have the same structure as above, but this time the tariff is only defined in relation to Good A rather than both Good A and Good B.

Returns with One Good Tariffs

Again, for every tariff you choose you will get a return that depends on how much you buy. This return will be as specified for Good A in Table 1 above.

Earnings with One Good Tariffs

Your net earnings are again the difference between your returns on the services you buy and how much you spend to buy them. Specifically, for each tariff, they can be computed as follows:

Good A returns MINUS Good A buying cost

Example. Suppose that you choose Example Tariff 1 in Table 1 and you buy 3,000 units of Good A. From Table 2, you can find that your return from 3,000 units of Good A is 106,000 points. From Table 1, and using the calculator on the computer display, you can find that you pay 3,000 *34.52 = 103,560 points for buying 3,000 units of Good A. Your net earnings from Good A are then 106,000 – 103,560 = 2,440 points. This can also be computed for you by the computer display calculator. Note that this amount could have been higher or lower had you been on a different tariff and/or bought different amounts of Good A.

Final Payment

At the end of the experiment one of the 36 tasks will be randomly selected by the computer.

If it is a task with one good tariffs, you will be paid for your net earnings at the rate of 1 penny for every 35 points earned in that task (or, equivalently, 1 pound for each 3,500 points earned).

If it is a task with two goods tariffs, you will be paid for your net earnings at the rate of 1 penny for every 70 points earned in that task (or, equivalently, 1 pound for each 7,000 points earned).

Questionnaire

1. Based on Table 2, what is your return on Good A if you buy 2,000 units?
    a. 50,000
    b. 80,000
    c. 90,000
    d. 120,000
    e. It depends

2. Assume that, in relation to a good, the Standing Charge is 10,000 points, that the Tier 1 price is 10 and that there is no Ceiling. If you buy 3,000 units of the good, how much do you need to pay?
    a. 10,010
    b. 13,000
    c. 30,000
    d. 40,000
    e. It depends

3. Assume that, in relation to a good, the Tier 1 price is 10 and there is no Ceiling or Standing Charge. If you buy 4,000 units of the good, how much do you need to pay?
    a. 4,000
    b. 4,010
    c. 30,000
    d. 40,000
    e. It depends

4. Assume that, in relation to a good, there is a Tier 1 price of 20, a Tier 2 price of 10, a Ceiling of 1,000 units, and no Standing Charge. If you buy 2,000 units of the good, how much do you need to pay?
    a. 10,000
    b. 20,000

c. 30,000
d. 40,000
e. It depends

5. What is the following statement is false?
    a. In the experiment you get money from buying goods at a price lower than the return on them.
    b. A default tariff is a tariff that is chosen for you as a default, unless you wish to make another choice.
    c. A calculator is available on the computer display in case you feel you need it.
    d. Net earnings may differ as a result of how much you buy and the tariff you choose.
    e. If there is a Ceiling of 3,000, this means that, if you buy 1,000 units, only the Tier 2 price matters in determining how much you pay.

[*Correct answers: 1. b; 2. d; 3. d; 4. c; 5. e*]

Appendix E

Tariffs and Tariff Tasks

In our three experiments we use 144 tariffs used that are differentiated according to four features.

The first feature is tariff complexity. The tariffs we employ are simple tariffs (tariffs 1-24, 49-72 and 97-120) and complex tariff (tariffs 25-48; 73-96 and 121-144). Simple tariffs only have one tier: that is, they have a single marginal cost regardless of the amount consumed, and no fixed costs. Complex tariffs are of two types. One type has two tiers and ceiling. One tier determines the cost if the amount consumed is below the ceiling, the other one applies to units consumed above the ceiling. The other type has one tier and a standing charge. The standing charge is a fixed cost independent of the amount consumed. The tier determines the marginal cost of each unit.

The second feature that differentiates our tariffs is whether they are for a single good (i.e. good A, modeled on energy – tariffs 1-48) or two goods (i.e. goods A and B, modeled on energy and gas – tariffs 49-144).

Third, tariffs can be differentiated into default tariffs, best tariffs and non-default suboptimal tariffs. The earnings of the default tariffs with the optimal consumption level are around 16 pounds. Apart from the best default treatments, in 24 tariffs tasks, the earnings of the default tariffs are neither the highest nor the lowest; in the 4 tariffs tasks, the earnings of the defaults tariffs can be the lowest. The earnings of the best tariffs with the optimal consumption level are around 22 pounds, which is over 6 pounds higher than the default tariffs and 3 pounds higher than the second-best tariffs.[75] If a tariff is the best tariff then it is always the best tariff in all consumption levels.

Finally, tariffs differ depending on whether they are collected from the real world or derived. All real tariffs used in our experiment were collected from the energy search engine website “Which?” (http://www.which.co.uk/energy/) in May 2011. Real tariffs come from the following energy companies: “e-on”, “npower”, “scottishpower”, “British Gas”, “EDF energy”, “npower juice”, “OVO new energy”, ”Southern Electric” and “The utility warehouse”. In order to use the search engine "Which?", some basic information needed to be provided, such as post-code, current energy supplier, and so on. Below is the list of items for which we had to provide such information and the information we provided.

Post code: SE1 0AA

Who is your supplier: E-on

[75] In a task, the second best tariff is the best tariff among the non-default suboptimal tariffs.

Do you have a gas mains supply to your property? No

Are you an IGT gas customer? No

Have you got a bill in front of you? Yes

Electricity I used Kwh 4000 in the last year

How do you pay for your bills? Monthly Direct Debit

Please tell us your tariff: Standard.

Some points of clarification:

SE10AA is a London post code. We have chosen the energy tariffs of the central London area for two reasons.

1. Firstly, London is a large market and so it is an obvious place where to use as a benchmark. some energy companies cannot offer as many of their tariffs as they can offer in London. London is one of the cities where has the most number of energy tariffs in UK.

2. In London, the household energy consumption per year from 2005 to 2009 is comparatively more stable than other cities. It is easy for us to use these stable yearly household energy consumptions in our experiment.

2. The sub-national authority electricity consumption statistics (2005, 2006, 2007, 2008 and 2009) (Department of Energy & Climate Change, 2010)[76] shows that the average domestic electricity consumption per household in "the London city" is around 4000kwh/year. This is the reason why we have inputted "I used Kwh 4000 in the last year" and the best consumption level in our experiment is also 4000 units.[77]

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[76] This workbook was produced in December 2010 – Publication URN: 10D/999 – For up to date statistics please visit the website:
http://www.decc.gov.uk/en/content/cms/statistics/energy_stats/regional/electricity/electricity.aspx

[77] The average consumption of gas is around 16000 units; we normalized the gas consumption to 4000 for comparability with the average consumption of electricity.

The derived tariffs are modeled after the collected real tariffs. Derived tariffs are created for comparative reasons. Despite the same final earnings, as the electricity tariff prices in the nature dual energy market are different from the electricity tariff prices in the nature single market, participants' different behaviour in the dual and single tariff tasks may not only arise (if it does) because of the different number of goods, but also because the electricity tariffs in the dual and single market tasks are different. To control for this, the derived dual tariffs have the same electricity tariffs as the single real electricity tariffs so as to ensure comparability.[78] Furthermore, we wanted our default, best and some sub-optimal tariffs (the second best tariff) used in the experiment to satisfy certain properties. We wanted the default tariff to be roughly comparable in every task in terms of incentives at the margin, with, for a consumption level of 4000, a 3 pounds difference in average earnings between first and second best tariff and around a difference of a further 3 pounds from the default tariff. Where necessary, we scaled real tariffs accordingly.

The list of tariff tasks is provided in Table E1, and the list of tariffs in Table E2. 144 tariffs are used in our experiment and they can be divided into 6 sets: simple single real tariffs, complex single real tariffs, simple dual real tariffs, complex dual real tariffs, simple dual derived tariffs and complex dual derived tariffs.[79]. Each set has 24 tariffs which includes 1 best tariff and 2 default tariffs. The reason we needed 2 default tariffs is because we had tasks that employed a mix of complex and simple tariffs (e.g. 12 simple tariffs and 12 complex tariffs). These tasks however needed to have both a simple default tariff and a complex default tariff to check whether the complexity of the default made a difference. The 12 simple tariffs and the 12 complex tariffs were selected from a bigger set of 24 tariffs (simple and complex respectively). Let as call SA a subset of 12 tariffs that contains the best

[78] In our experiment, we do not find a statistically significant difference in behaviour between dual natural tariffs tasks and dual derived tariffs tasks.

[79] For the reasons we mentioned above, some default tariffs in the "nature tariff sections" are constructed. To simplify these sections' names, here we still call them "nature tariff sections".

simple tariff and SB the remaining 12 tariff subset that contains the remaining (all sub-optimal) 12 tariffs. Let us call CA the subset of 12 complex tariff containing the best complex tariff and CB the other subset that contains the remaining (all sub-optimal) 12 tariffs. In order to create mixed tariffs tasks, we mixed the following sets: 1) SA *with* CB or 2) SB *with* CA. In the mixed set 1) the best tariff was the simple one while in the mixed set 2) the best tariff was the complex one. The mixed set 1) contained one of two default tariffs depending on the task: a simple default tariff in SA or a complex default tariff in set CB.[80] The mixed set 2) contained one of two default tariffs depending on the task, a simple default tariff in SB or a complex tariff in CA. Having two possible default tariffs (a single and a complex) allowed us to check, for every mixed tariffs task, how the complexity of the default tariff affects subjects' outcomes. This also explains why we needed two possible default tariffs for both simple and complex tariffs. A similar procedure applied for 4 tariffs tasks.

Reference Not in Paper

Department of Energy & Climate Change, 2010. The sub-national authority electricity consumption statistics (2005, 2006, 2007, 2008 and 2009). Publication URN: 10D/999 - http://www.decc.gov.uk/en/content/cms/statistics/energy_stats/regional/electricity/electricity.aspx

[80] Whether either tariff actually acted as a default tariff in the experiment depended of course on the experimental treatment.

Table E1. Tariff Tasks

Task No.	Simple/mixed/ complex	Single/Dual	4/24	Real/derived	Best simple/complex	Default simple/complex
1	Simple	Single	24	Real	Simple	Simple
2	Complex	Single	24	Real	Complex	Complex
3	Mixed	Single	24	Real	Complex	Simple
4	Mixed	Single	24	Real	Complex	Complex
5	Mixed	Single	24	Real	Simple	Simple
6	Mixed	Single	24	Real	Simple	Complex
7	Simple	Single	4	Real	Simple	Simple
8	Complex	Single	4	Real	Complex	Complex
9	Mixed	Single	4	Real	Complex	Simple
10	Mixed	Single	4	Real	Complex	Complex
11	Mixed	Single	4	Real	Simple	Simple
12	Mixed	Single	4	Real	Simple	Complex
13	Simple	Dual	24	Real	Simple	Simple
14	Complex	Dual	24	Real	Complex	Complex
15	Mixed	Dual	24	Real	Complex	Simple
16	Mixed	Dual	24	Real	Complex	Complex
17	Mixed	Dual	24	Real	Simple	Simple
18	Mixed	Dual	24	Real	Simple	Complex
19	Simple	Dual	4	Real	Simple	Simple
20	Complex	Dual	4	Real	Complex	Complex
21	Mixed	Dual	4	Real	Complex	Simple
22	Mixed	Dual	4	Real	Complex	Complex
23	Mixed	Dual	4	Real	Simple	Simple
24	Mixed	Dual	4	Real	Simple	Complex
25	Simple	Dual	24	Derived	Simple	Simple
26	Complex	Dual	24	Derived	Complex	Complex
27	Mixed	Dual	24	Derived	Complex	Simple
28	Mixed	Dual	24	Derived	Complex	Complex
29	Mixed	Dual	24	Derived	Simple	Simple
30	Mixed	Dual	24	Derived	Simple	Complex
31	Simple	Dual	4	Derived	Simple	Simple
32	Complex	Dual	4	Derived	Complex	Complex
33	Mixed	Dual	4	Derived	Complex	Simple
34	Mixed	Dual	4	Derived	Complex	Complex
35	Mixed	Dual	4	Derived	Simple	Simple
36	Mixed	Dual	4	Derived	Simple	Complex

Table E2. Tariffs

	Good 1				Good 2			
Tariff	Standing charge	Tier 1	Ceiling	Tier 2	Standing charge	Tier 1	Ceiling	Tier 2
1	-	16.325	-	-	-	-	-	-
2	-	10.726	-	-	-	-	-	-
3	-	13.398	-	-	-	-	-	-
4	-	13.577	-	-	-	-	-	-
5	-	14.499	-	-	-	-	-	-
6	-	15.74	-	-	-	-	-	-
7	-	16.259	-	-	-	-	-	-
8	-	17.378	-	-	-	-	-	-
9	-	19.992	-	-	-	-	-	-
10	-	20.663	-	-	-	-	-	-
11	-	22.322	-	-	-	-	-	-
12	-	23.252	-	-	-	-	-	-
13	-	13.514	-	-	-	-	-	-
14	-	17.537	-	-	-	-	-	-
15	-	18.061	-	-	-	-	-	-
16	-	19.548	-	-	-	-	-	-
17	-	16.46	-	-	-	-	-	-
18	-	16.926	-	-	-	-	-	-
19	-	17.115	-	-	-	-	-	-
20	-	17.699	-	-	-	-	-	-
21	-	18.753	-	-	-	-	-	-
22	-	18.921	-	-	-	-	-	-
23	-	21.94	-	-	-	-	-	-
24	-	21.857	-	-	-	-	-	-
25	-	16.325	900	15.263	-	-	-	-
26	-	19.467	728	8.432	-	-	-	-
27	-	21.94	900	10.154	-	-	-	-
28	3887.1	12.128	-	-	-	-	-	-
29	3969	12.338	-	-	-	-	-	-
30	-	19.278	900	11.96	-	-	-	-
31	4142.4	12.81	-	-	-	-	-	-
32	-	19.992	900	12.19	-	-	-	-
33	4041.6	13.514	-	-	-	-	-	-
34	-	18.921	900	13.324	-	-	-	-
35	-	20.663	900	13.221	-	-	-	-
36	-	15.74	900	15.598	-	-	-	-
37	-	23.252	720	10.886	-	-	-	-
38	-	17.378	900	12.128	-	-	-	-
39	-	27.447	900	11.561	-	-	-	-
40	-	20.412	900	14.374	-	-	-	-
41	-	18.753	900	11.435	-	-	-	-
42	-	15.74	728	12.506	-	-	-	-
43	-	16.926	720	13.451	-	-	-	-
44	-	18.291	900	12.768	-	-	-	-
45	7767.9	11.235	-	-	-	-	-	-
46	4038.3	12.484	-	-	-	-	-	-
47	3969	13.577	-	-	-	-	-	-
48	3887.1	11.938	-	-	-	-	-	-
49	-	16.325	-	-	-	7.523	-	-

50	-	10.726	-	-	-	2.009	-	-
51	-	13.398	-	-	-	4.516	-	-
52	-	13.577	-	-	-	5.815	-	-
53	-	14.499	-	-	-	4.53	-	-
54	-	16.259	-	-	-	5.269	-	-
55	-	15.74	-	-	-	7.659	-	-
56	-	17.378	-	-	-	9.651	-	-
57	-	19.992	-	-	-	8.56	-	-
58	-	20.663	-	-	-	9.12	-	-
59	-	22.322	-	-	-	8.45	-	-
60	-	23.252	-	-	-	9.21	-	-
61	-	13.514	-	-	-	4.692	-	-
62	-	17.537	-	-	-	1.012	-	-
63	-	18.061	-	-	-	1.036	-	-
64	-	19.548	-	-	-	1.965	-	-
65	-	16.46	-	-	-	2.365	-	-
66	-	16.926	-	-	-	2.101	-	-
67	-	17.115	-	-	-	3.251	-	-
68	-	17.699	-	-	-	2.695	-	-
69	-	18.753	-	-	-	1.543	-	-
70	-	18.921	-	-	-	3.957	-	-
71	-	21.94	-	-	-	1.693	-	-
72	-	21.857	-	-	-	3.625	-	-
73	-	16.325	900	15.982	-	7.002	670	6.784
74	-	19.467	728	8.432	-	7.032	670	1.301
75	-	21.94	900	10.154	-	5.126	670	4.876
76	3887.1	12.128	-	-	3785.1	4.268	-	-
77	-	23.252	720	10.886	-	6.562	625	5.269
78	-	17.378	900	12.128	-	6.263	1143	5.496
79	-	19.992	900	12.19	-	5.878	670	5.521
80	-	18.921	900	13.324	-	7.263	670	5.369
81	3969	12.338	-	-	2870.7	7.269	-	-
82	7767.9	11.235	-	-	3110.7	8.76	-	-
83	-	15.74	900	15.598	-	8.632	670	6.962
84	4038.3	12.484	-	-	4776	8.83	-	-
85	-	23.252	720	10.886	-	15.36	670	2.698
86	-	17.378	900	12.128	-	18.369	670	2.036
87	-	27.447	900	11.561	-	11.365	625	1.965
88	-	20.412	900	14.374	-	4.698	1144	1.036
89	-	18.753	900	11.435	-	5.964	670	4.457
90	-	15.74	728	12.506	-	4.911	670	4.663
91	-	16.926	720	13.451	-	4.532	625	3.858
92	-	18.291	900	12.768	-	11.033	1144	1.001
93	7767.9	11.235	-	-	1608.6	4.223	-	-
94	4038.3	12.484	-	-	5180.7	2.977	-	-
95	3969	13.577	-	-	8596.2	1.008	-	-
96	3887.1	11.938	-	-	3610.5	3.981	-	-
97	-	16.325	-	-	-	7.523	-	-
98	-	9.71	-	-	-	3.21	-	-
99	-	13.514	-	-	-	4.28	-	-
100	-	14.499	-	-	-	4.4	-	-
101	-	15.74	-	-	-	7.323	-	-
102	-	17.537	-	-	-	5.707	-	-
103	-	15.74	-	-	-	7.659	-	-

104	-	17.115	-	-	-	7.476	-	-
105	-	18.921	-	-	-	7.102	-	-
106	-	19.992	-	-	-	7.666	-	-
107	-	21.834	-	-	-	7.903	-	-
108	-	23.252	-	-	-	7.488	-	-
109	-	17.378	-	-	-	7.592	-	-
110	-	13.398	-	-	-	3.675	-	-
111	-	16.259	-	-	-	7.102	-	-
112	-	17.699	-	-	-	6.504	-	-
113	-	18.061	-	-	-	6.637	-	-
114	-	16.926	-	-	-	7.874	-	-
115	-	17.313	-	-	-	7.999	-	-
116	-	19.458	-	-	-	6.971	-	-
117	-	20.283	-	-	-	6.637	-	-
118	-	21.914	-	-	-	5.958	-	-
119	-	20.663	-	-	-	7.607	-	-
120	-	21.857	-	-	-	7.038	-	-
121	-	16.325	900	15.982	-	7.002	670	6.784
122	-	21.467	728	6.126	-	8.851	1,143	2.036
123	-	25.652	900	10.804	-	8.348	670	3.014
124	3887.1	11.938	-	-	8810.4	3.255	-	-
125	3887.1	12.128	-	-	8810.7	3.308	-	-
126	-	17.262	728	13.724	-	8.031	1,143	2.84
127	-	25.203	720	12.109	-	7.875	670	3.682
128	4142.4	12.81	-	-	6211.8	3.521	-	-
129	-	27.447	900	11.561	-	7.768	670	3.38
130	4041.6	13.514	-	-	3298.5	4.28	-	-
131	-	20.663	900	13.221	-	7.607	670	4.455
132	-	15.74	900	15.598	-	8.632	670	6.962
133	-	22.926	900	11.636	-	8.298	670	3.298
134	-	26.935	720	11.344	-	7.623	670	3.316
135	-	21.696	900	13.882	-	7.987	670	4.678
136	-	17.772	900	14.124	-	8.268	670	2.924
137	-	22.988	728	12.480	-	8.974	1,143	3.109
138	-	24.415	720	11.430	-	7.862	1,143	3.666
139	-	18.247	900	12.734	-	7.972	670	3.473
140	-	20.992	900	12.800	-	8.049	670	3.513
141	6141.87	11.290	-	-	8268.75	3.266	-	-
142	5421,15	11.636	-	-	8086.68	3.298	-	-
143	8156	11.797	-	-	4713.03	3.699	-	-
144	4081.455	11.907	-	-	9250.92	3.247	-	-

Appendix F

Regression Analysis

In this appendix we reproduce the results of our regression analysis (marginal effects are reported)[81]. This is listed in six tables:

- Table F1: probit regression on the suboptimal outcome rate, with error clustering to control for subject level non independence of observations. All experimental treatments are included.
- Table F2: random effects probit regression on the suboptimal outcome rate, controlling for subject level non independence of observations. All experimental treatments are included.
- Table F3: multinomial probit regression simultaneously estimating (a) the (suboptimal) default rate and (b) the suboptimal switching rate, with error clustering to control for subject level non independence of observations. Treatments BDEAD and BDEA cannot be included in these regressions since only two outcomes are possible in these treatments: to stick to the optimal tariff or to switch suboptimally.
- Table F4: probit regression on the suboptimal outcome rate / suboptimal switching rate, controlling for subject level non independence of observations and including only treatments BDEAD and BDEA.
- Table F5: random effects probit regression on the suboptimal outcome rate / suboptimal switching rate, controlling for subject level non independence of observations and including only treatments BDEAD and BDEA.
- Table F6: probit regression with error clustering on default rate for all subjects for which we have social desirability questionnaire data. The treatments for which we

have the social desirability answers are: DEAD (20 subjects out of 50), DEA, DEAI, BDEAD, BDEA, WDEAD and WDEA. Treatments BDEAD and BDEA are not included in the regression because the default tariff was also the best tariff, and so there is no clear interpretation of sticking to the default tariff as being driven by experimenter demand effects.

- Table F7: random effects probit-regression on default rate for all subjects for which we have social desirability questionnaire data.

The variables used in the regression analysis are as follows:

- Dual is a dummy variable for the type of market that takes value 1 when the market is for dual tariffs and value 0 otherwise;
- 24 is a dummy whose value is 1 when the number of tariffs employed is 24 and 0 otherwise;
- The dummy variables Complex and Mix represent the complexity of the tariffs employed. Complex takes value 1 when all tariffs are complex and 0 otherwise and Mix takes value 1 when all tariffs are mixed and 0 otherwise;
- Complex24, Mix24, ComplexDual, MixDual, Dual24 are all interaction variables between variables as listed above, e.g. Complex24 is Complex × 24;
- DefaultCompMix takes value 1 when the default tariff employed in the mix tariffs tasks is complex and 0 otherwise. This helps us to identify the effect of the complexity of the default tariff on suboptimal choices;
- BestCompMix has value 1 when the best tariff is complex and the tasks involve only mixed tariffs and 0 otherwise. This helps us to identify the effect of the complexity of the best tariff on suboptimal choices;
- The variable represents the period of play and takes value from 1 to 36;
- Dummy variables are added for each treatment as appropriate to the dataset used;

- Demographic variables are sometimes used: UK (=1 when subjects are from the UK), Gender (= 1 when subjects are males) and Age.

Social Desirability: first, assign a value of 1 for each socially desirable response to the 16 questions of Stöber's (2001) social desirability scale questionnaire; this provides an integer between 0 and 16 depending on the answers subjects gave; then subtract the resulting integer from the average social desirability score to get a zero-centered scale across subjects. Higher values pointing to higher sensitivity to social pressure.

Table F1. Probit Regressions on Suboptimal Outcome Rate with Error Clustering [82]

	Dep. Var. Suboptimal Choices			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	0.045***	0.040***	0.074***	0.060***
	(0.007)	*(0.009)*	*(0.014)*	*(0.020)*
24	0.180***	0.168***	0.145***	0.122***
	(0.008)	*(0.011)*	*(0.016)*	*(0.021)*
Complex	0.157***	0.147***	0.158***	0.135***
	(0.012)	*(0.015)*	*(0.020)*	*(0.024)*
Mix	0.013	0.019	0.040**	0.048**
	(0.009)	*(0.012)*	*(0.015)*	*(0.021)*
Complex24			0.069***	0.076***
			(0.020)	*(0.025)*
Mix24			0.01	0.009
			(0.016)	*(0.020)*
ComplexDual			-0.053***	-0.041*
			(0.017)	*(0.024)*
MixDual			-0.048***	-0.051**
			(0.014)	*(0.020)*
Dual24			0.025**	0.041**
			(0.012)	*(0.016)*
DefaultCompMix	-0.002	-0.012	-0.002	-0.012
	(0.006)	*(0.008)*	*(0.006)*	*(0.008)*
BestCompMix	0.170***	0.168***	0.169***	0.167***
	(0.013)	*(0.017)*	*(0.013)*	*(0.017)*
Period	-0.006***	-0.006***	-0.006***	-0.006***
	(0.000)	*(0.001)*	*(0.000)*	*(0.001)*
mDE	0.029	0.041	0.029	0.041
	(0.066)	*(0.077)*	*(0.066)*	*(0.077)*
D	0.102***	0.063	0.102***	0.063
	(0.029)	*(0.039)*	*(0.029)*	*(0.039)*
DF	0.006	-0.025	0.006	-0.025
	(0.056)	*(0.071)*	*(0.056)*	*(0.071)*
F	-0.022	-0.099	-0.022	-0.098
	(0.054)	*(0.062)*	*(0.054)*	*(0.062)*
DEAD	0.061***	0.076***	0.061***	0.076***
	(0.021)	*(0.021)*	*(0.021)*	*(0.021)*
DEA	0.057	-0.009	0.057	-0.009
	(0.058)	*(0.060)*	*(0.058)*	*(0.060)*
DEAI	0.061	-0.006	0.061	-0.006
	(0.066)	*(0.072)*	*(0.066)*	*(0.072)*
BDEAD	-0.311***	-0.378***	-0.311***	-0.378***
	(0.065)	*(0.067)*	*(0.065)*	*(0.067)*
BDEA	-0.357***	-0.396***	-0.358***	-0.396***
	(0.066)	*(0.064)*	*(0.066)*	*(0.064)*
WDEAD	0.166**	-0.013	0.166**	-0.013
	(0.072)	*(0.093)*	*(0.072)*	*(0.093)*
WDEA	-0.111*	-0.076	-0.111*	-0.076
	(0.062)	*(0.070)*	*(0.062)*	*(0.070)*
nationality		-0.064		-0.064
		(0.044)		*(0.044)*
Gender		-0.068**		-0.068**
		(0.033)		*(0.033)*
age		-0.003		-0.003
		(0.004)		*(0.004)*

[82] The Log pseudo-likelihood for the models is -9777, -9578, -9766, -9568. *n* = 16560 in model 1 and 3. *n* = 16344 in model 2 and 4 due to some missing values of the variable Age.

Table F2. Random Effect Probit Regressions on Suboptimal Outcome Rate[83]

	Dep. Var. Suboptimal Choices			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	0.059***	0.049***	0.107***	0.081***
	(0.008)	*(0.010)*	*(0.022)*	*(0.028)*
24	0.239***	0.218***	0.208***	0.169***
	(0.008)	*(0.011)*	*(0.021)*	*(0.028)*
Complex	0.204***	0.184***	0.224***	0.186***
	(0.013)	*(0.017)*	*(0.026)*	*(0.034)*
Mix	0.019	0.023	0.070***	0.075**
	(0.012)	*(0.016)*	*(0.022)*	*(0.029)*
Complex24			0.067***	0.074**
			(0.025)	*(0.033)*
Mix24			-0.001	-0.001
			(0.020)	*(0.027)*
ComplexDual			-0.079***	-0.060*
			(0.027)	*(0.035)*
MixDual			-0.074***	-0.075***
			(0.022)	*(0.029)*
Dual24			0.030*	0.055***
			(0.015)	*(0.020)*
DefaultCompMix	-0.003	-0.014	-0.003	-0.014
	(0.009)	*(0.011)*	*(0.009)*	*(0.011)*
BestCompMix	0.215***	0.207***	0.215***	0.206***
	(0.010)	*(0.012)*	*(0.010)*	*(0.012)*
Period	-0.008***	-0.008***	-0.008***	-0.008***
	(0.000)	*(0.000)*	*(0.000)*	*(0.000)*
mDE	0.027	0.048	0.027	0.048
	(0.094)	*(0.108)*	*(0.094)*	*(0.108)*
D	0.151***	0.102*	0.151***	0.102*
	(0.041)	*(0.057)*	*(0.041)*	*(0.057)*
DF	0.025	-0.001	0.025	-0.001
	(0.084)	*(0.106)*	*(0.084)*	*(0.105)*
F	-0.032	-0.110	-0.032	-0.110
	(0.084)	*(0.105)*	*(0.084)*	*(0.105)*
DEAD	0.093***	0.112***	0.093***	0.112***
	(0.028)	*(0.031)*	*(0.028)*	*(0.031)*
DEA	0.108	0.009	0.108	0.009
	(0.084)	*(0.097)*	*(0.084)*	*(0.096)*
DEAI	0.122	0.048	0.122	0.048
	(0.094)	*(0.108)*	*(0.094)*	*(0.108)*
BDEAD	-0.389***	-0.451***	-0.389***	-0.451***
	(0.095)	*(0.116)*	*(0.095)*	*(0.116)*
BDEA	-0.465***	-0.473***	-0.466***	-0.474***
	(0.096)	*(0.106)*	*(0.096)*	*(0.106)*
WDEAD	0.241**	0.029	0.241**	0.028
	(0.093)	*(0.120)*	*(0.093)*	*(0.120)*
WDEA	-0.110	-0.058	-0.110	-0.057
	(0.094)	*(0.113)*	*(0.094)*	*(0.113)*
nationality		-0.074		-0.074
		(0.052)		*(0.052)*
Gender		-0.082*		-0.082*
		(0.042)		*(0.042)*
age		-0.002		-0.002
		(0.005)		*(0.005)*

[83] The Log likelihood for the models is -6990, -6882, -6975 and -6867. *n* =16560 in model 1 and 3. *n* = 16344 in model 2 and 4.

Table F3. Multinomial Probit Regressions on Suboptimal Default Rate and on Suboptimal Switching Rate.[84]

	Default Rate				Sub. Switches				Best Tariff Rate			
Variables	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Dual	0.033***	0.026***	-0.02	-0.041**	0.022***	0.029***	0.116***	0.131***	-0.055***	-0.054***	-0.096***	-0.090***
	(0.006)	*(0.008)*	*(0.014)*	*(0.019)*	*(0.007)*	*(0.009)*	*(0.021)*	*(0.029)*	*(0.007)*	*(0.010)*	*(0.017)*	*(0.025)*
24	-0.003	-0.001	-0.081***	-0.067***	0.203***	0.194***	0.274***	0.234***	-0.200***	-0.193***	-0.194***	-0.168***
	(0.006)	*(0.009)*	*(0.016)*	*(0.022)*	*(0.009)*	*(0.011)*	*(0.023)*	*(0.031)*	*(0.009)*	*(0.012)*	*(0.020)*	*(0.026)*
Complex	0.026***	0.034***	-0.053***	-0.047**	0.151***	0.133***	0.272***	0.238***	-0.177***	-0.167***	-0.219***	-0.191***
	(0.009)	*(0.011)*	*(0.018)*	*(0.022)*	*(0.013)*	*(0.018)*	*(0.029)*	*(0.038)*	*(0.013)*	*(0.018)*	*(0.025)*	*(0.032)*
Mix	-0.005	0.004	-0.089***	-0.074***	0.022*	0.018	0.173***	0.162***	-0.017	-0.022	-0.083***	-0.089***
	(0.008)	*(0.011)*	*(0.016)*	*(0.019)*	*(0.012)*	*(0.016)*	*(0.027)*	*(0.035)*	*(0.010)*	*(0.013)*	*(0.020)*	*(0.028)*
Complex24			0.078***	0.068***			-0.045*	-0.021			-0.033	-0.047
			(0.017)	*(0.020)*			*(0.026)*	*(0.034)*			*(0.023)*	*(0.030)*
Mix24			0.080***	0.056***			-0.104***	-0.074***			0.024	0.018
			(0.014)	*(0.017)*			*(0.021)*	*(0.026)*			*(0.019)*	*(0.025)*
ComplexDual			0.052***	0.064***			-0.121***	-0.120***			0.069***	0.057**
			(0.017)	*(0.023)*			*(0.023)*	*(0.032)*			*(0.020)*	*(0.028)*
MixDual			0.053***	0.063***			-0.114***	-0.133***			0.061***	0.069***
			(0.014)	*(0.020)*			*(0.020)*	*(0.028)*			*(0.016)*	*(0.024)*
Dual24			0.015	0.024*			0.014	0.022			-0.029**	-0.046***
			(0.009)	*(0.013)*			*(0.013)*	*(0.019)*			*(0.013)*	*(0.018)*
DefaultCompMix	0.008	0.001	0.008	0.001	-0.009	-0.011	-0.008	-0.01	0.001	0.009	0	0.009
	(0.006)	*(0.008)*	*(0.006)*	*(0.008)*	*(0.007)*	*(0.009)*	*(0.007)*	*(0.009)*	*(0.006)*	*(0.009)*	*(0.006)*	*(0.009)*
BestCompMix	-0.003	-0.004	-0.003	-0.004	0.186***	0.187***	0.184***	0.185***	-0.182***	-0.182***	-0.181***	-0.180***
	(0.005)	*(0.007)*	*(0.005)*	*(0.007)*	*(0.014)*	*(0.018)*	*(0.014)*	*(0.018)*	*(0.014)*	*(0.018)*	*(0.014)*	*(0.018)*
Period	-0.005***	-0.006***	-0.005***	-0.006***	-0.001***	-0.001**	-0.001***	-0.001**	0.006***	0.007***	0.006***	0.007***
	(0.000)	*(0.001)*	*(0.000)*	*(0.001)*	*(0.000)*	*(0.001)*	*(0.000)*	*(0.001)*	*(0.000)*	*(0.001)*	*(0.000)*	*(0.001)*
mDE	0.001	0.064	0.001	0.063	0.027	-0.005	0.027	-0.005	-0.028	-0.058	-0.028	-0.058
	(0.063)	*(0.063)*	*(0.063)*	*(0.063)*	*(0.042)*	*(0.049)*	*(0.042)*	*(0.049)*	*(0.073)*	*(0.085)*	*(0.074)*	*(0.085)*
D	0.115***	0.123***	0.115***	0.122***	-0.01	-0.034	-0.01	-0.033	-0.105***	-0.089**	-0.105***	-0.089**
	(0.031)	*(0.045)*	*(0.031)*	*(0.045)*	*(0.018)*	*(0.024)*	*(0.018)*	*(0.024)*	*(0.030)*	*(0.042)*	*(0.030)*	*(0.042)*
DF	0.025	0.117	0.025	0.116	-0.015	-0.106***	-0.015	-0.105***	-0.01	-0.011	-0.009	-0.011
	(0.066)	*(0.076)*	*(0.066)*	*(0.076)*	*(0.035)*	*(0.038)*	*(0.035)*	*(0.038)*	*(0.063)*	*(0.078)*	*(0.063)*	*(0.078)*
F	-0.218***	-0.220***	-0.218***	-0.221***	0.119***	0.049	0.119***	0.049	0.099	0.171**	0.099	0.171**
	(0.059)	*(0.077)*	*(0.059)*	*(0.077)*	*(0.034)*	*(0.042)*	*(0.034)*	*(0.042)*	*(0.065)*	*(0.084)*	*(0.065)*	*(0.084)*
DEAD	0.090***	0.110***	0.090***	0.110***	-0.034***	-0.031**	-0.034***	-0.031**	-0.056***	-0.080***	-0.056***	-0.079***
	(0.020)	*(0.019)*	*(0.020)*	*(0.019)*	*(0.013)*	*(0.013)*	*(0.013)*	*(0.013)*	*(0.021)*	*(0.021)*	*(0.021)*	*(0.021)*
DEA	0.141**	0.119**	0.141**	0.119**	-0.080**	-0.105***	-0.080**	-0.106***	-0.061	-0.013	-0.061	-0.013
	(0.061)	*(0.059)*	*(0.061)*	*(0.059)*	*(0.035)*	*(0.036)*	*(0.035)*	*(0.036)*	*(0.062)*	*(0.067)*	*(0.062)*	*(0.067)*
DEAI	0.120*	0.1	0.120*	0.1	-0.053	-0.091**	-0.053	-0.092**	-0.067	-0.009	-0.067	-0.008
	(0.072)	*(0.082)*	*(0.072)*	*(0.082)*	*(0.041)*	*(0.046)*	*(0.041)*	*(0.046)*	*(0.070)*	*(0.078)*	*(0.070)*	*(0.078)*
BDEAD												
BDEA												
WDEAD	0.226***	0.190**	0.225***	0.189**	-0.059	-0.201***	-0.059	-0.200***	-0.167**	0.011	-0.167**	0.011
	(0.066)	*(0.079)*	*(0.066)*	*(0.079)*	*(0.049)*	*(0.066)*	*(0.049)*	*(0.066)*	*(0.075)*	*(0.098)*	*(0.075)*	*(0.098)*
WDEA	-0.024	0.049	-0.024	0.049	-0.088**	-0.111**	-0.088**	-0.111**	0.111	0.062	0.112	0.062
	(0.062)	*(0.066)*	*(0.062)*	*(0.066)*	*(0.041)*	*(0.045)*	*(0.041)*	*(0.045)*	*(0.068)*	*(0.077)*	*(0.068)*	*(0.077)*
nationality		-0.118**		-0.118**		0.036		0.036		0.082		0.08182
		(0.050)		*(0.050)*		*(0.032)*		*(0.032)*		*(0.051)*		*(0.051)*
Gender		-0.057		-0.057		-0.015		-0.016		0.072*		0.072*
		(0.039)		*(0.039)*		*(0.230)*		*(0.023)*		*(0.039)*		*(0.039)*
age		-0.016**		-0.016**		0.004		0.004		0.013**		0.013**
		(0.007)		*(0.007)*		*(0.003)*		*(0.003)*		*(0.006)*		*(0.006)*

[84] The pseudo log likelihood for the models is -13037, -12772, -13002 and -12739. *n* =14400 in model 1 and 3. *n* = 4184 in model 2 and 4 again this is due to some missing values of the variable Age.

Table F4. Probit Regressions on Suboptimal Outcome Rate with Error Clustering in BDEAD and BDEA[85]

	Dep. Var. Suboptimal Choices			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	-0.014	-0.033*	0.032	0.002
	(0.017)	*(0.019)*	*(0.035)*	*(0.039)*
24	0.028**	0.018	0.037	0.025
	(0.014)	*(0.015)*	*(0.036)*	*(0.036)*
Complex	0.040***	0.045**	0.049	0.05
	(0.015)	*(0.019)*	*(0.036)*	*(0.035)*
Mix	0.001	0.011	0.038	0.047
	(0.022)	*(0.031)*	*(0.035)*	*(0.043)*
Complex24			0.033	0.028
			(0.049)	*(0.048)*
Mix24			-0.010	-0.016
			(0.041)	*(0.035)*
ComplexDual			-0.040	-0.030
			(0.046)	*(0.048)*
MixDual			-0.048	-0.041
			(0.034)	*(0.037)*
Dual24			-0.012	-0.003
			(0.027)	*(0.032)*
DefaultCompMix	-0.016	-0.030	-0.016	-0.030
	(0.015)	*(0.021)*	*(0.015)*	*(0.021)*
BestCompMix	0.065***	0.054**	0.065***	0.054**
	(0.023)	*(0.024)*	*(0.023)*	*(0.024)*
Period	-0.003***	-0.004***	-0.003***	-0.004***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*
mDE				
D				
DF				
F				
DEAD				
DEA				
DEAI				
BDEAD	0.030	-0.009	0.030	-0.009
	(0.044)	*(0.039)*	*(0.044)*	*(0.039)*
BDEA				
WDEAD				
WDEA				
nationality		-0.046		-0.047
		(0.052)		*(0.052)*
Gender		0.005		0.005
		(0.040)		*(0.040)*
age		0.003***		0.003**
		(0.001)		*(0.001)*

[85] The pseudo log likelihood for the models is -835, -815, -833 and -814. N= 2160 in all models.

Table F5. Random Effects Regressions on Suboptimal Outcome Rate in BDEAD and BDEA [86]

	Dep. Var. Suboptimal Choices			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	-0.010	-0.026*	0.038	0.009
	(0.012)	*(0.014)*	*(0.035)*	*(0.040)*
24	0.026	0.015	0.035	0.019
	(0.012)	*(0.013)*	*(0.034)*	*(0.039)*
Complex	0.036	0.039	0.051	0.047
	(0.020)	*(0.023)*	*(0.042)*	*(0.048)*
Mix	0.003	0.011	0.042	0.045
	(0.019)	*(0.022)*	*(0.036)*	*(0.042)*
Complex24			0.028	0.026
			(0.039)	*(0.045)*
Mix24			-0.011	-0.013
			(0.032)	*(0.037)*
ComplexDual			-0.045	-0.033
			(0.043)	*(0.048)*
MixDual			-0.049	-0.040
			(0.036)	*(0.041)*
Dual24			-0.010	-0.001
			(0.023)	*(0.026)*
DefaultCompMix	-0.011	-0.023	-0.011	-0.023
	(0.013)	*(0.016)*	*(0.013)*	*(0.016)*
BestCompMix	0.055***	0.045**	0.055***	0.044**
	(0.016)	*(0.018)*	*(0.016)*	*(0.017)*
Period	-0.003***	-0.003***	-0.003***	-0.003***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*
mDE				
D				
DF				
F				
DEAD				
DEA				
DEAI				
BDEAD	0.030	-0.006	0.030	-0.006
	(0.035)	*(0.034)*	*(0.035)*	*(0.034)*
BDEA				
WDEAD				
WDEA				
nationality		-0.031		-0.031
		(0.042)		*(0.042)*
Gender		0.014		0.014
		(0.033)		*(0.033)*
age		0.003		0.003
		(0.002)		*(0.002)*

[86] The pseudo log likelihood for the models is -687, -685, -885 and -683. n = 2160 in all models.

Table F6. Probit regressions on Default Rate with Social Desirability as Independent Variable and Error Clustering[87]

	Dep. Var. Default Rate			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	0.039***	0.039***	-0.008	-0.007
	(0.010)	*(0.010)*	*(0.020)*	*(0.020)*
24	*0.007*	0.006	-0.011	-0.014
	(0.008)	*(0.009)*	*(0.022)*	*(0.023)*
Complex	0.034***	0.036***	-0.016	-0.014
	(0.013)	*(0.013)*	*(0.022)*	*(0.023)*
Mix	*0.002*	0.003	-0.039*	-0.036
	(0.012)	*(0.012)*	*(0.022)*	*(0.022)*
Complex24			0.009	0.010
			(0.021)	*(0.022)*
Mix24			0.016	0.018
			(0.020)	*(0.021)*
ComplexDual			0.068***	0.065**
			(0.026)	*(0.026)*
MixDual			0.048**	0.045**
			(0.021)	*(0.021)*
Dual24			0.008	0.010
			(0.014)	*(0.014)*
DefaultCompMix	0.018**	0.018*	0.018**	0.018*
	(0.009)	*(0.009)*	*(0.009)*	*(0.009)*
BestCompMix	*-0.013*	-0.013	-0.013	-0.013
	(0.009)	*(0.009)*	***(0.009)***	*(0.009)*
Period	-0.007***	-0.007***	-0.007***	-0.007***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*
mDE				
D				
DF				
F				
DEAD	0.102***	0.098***	0.102***	0.098***
	(0.028)	*(0.027)*	*(0.028)*	*(0.027)*
DEA	0.150**	0.157**	0.150**	0.157**
	(0.068)	*(0.064)*	*(0.068)*	*(0.064)*
DEAI	0.129	0.116	0.129	0.116
	(0.081)	*(0.079)*	*(0.081)*	*(0.079)*
BDEAD				
BDEA				
WDEAD	0.248***	0.227***	0.248***	0.227***
	(0.073)	*(0.071)*	*(0.073)*	*(0.071)*
WDEA	-0.042	-0.048	-0.043	-0.048
	(0.068)	*(0.065)*	*(0.068)*	*(0.065)*
UK		-0.072		-0.072
		(0.047)		*(0.047)*
Gender		-0.118***		-0.118***
		(0.043)		*(0.043)*
Age		0.000		0.000
		(0.006)		*(0.006)*
Social Desirability	0.013*	0.015**	0.013*	0.015*
	(0.007)	*(0.007)*	*(0.007)*	*(0.007)*

[87] The pseudo log likelihood for the models is -3639, -3489, -3637 and -3487. N= 6840 in models 1 and 3. N= 6768 in models 2 and 4.

Table F7. Random Effects regressions on Default Rate with Social Desirability as an Independent Variable[88]

	Dep. Var. Default Rate			
Variables	Model 1	Model 2	Model 3	Model 4
Dual	0.064***	0.063***	-0.003	0.001
	(0.013)	*(0.012)*	*(0.031)*	*(0.031)*
24	0.014	0.013	-0.014	-0.018
	(0.011)	*(0.011)*	*(0.031)*	*(0.031)*
Complex	0.049**	0.050***	-0.024	-0.020
	(0.019)	*(0.019)*	*(0.038)*	*(0.038)*
Mix	-0.001	0.000	-0.065**	-0.061
	(0.018)	*(0.018)*	*(0.032)*	*(0.032)*
Complex24			0.002	0.005
			(0.037)	*(0.037)*
Mix24			0.035	0.037
			(0.030)	*(0.030)*
ComplexDual			0.104**	0.098**
			(0.041)	*(0.040)*
MixDual			0.068**	0.063*
			(0.032)	*(0.032)*
Dual24			0.006	0.008
			(0.024)	*(0.023)*
DefaultCompMix	0.031**	0.031**	0.031**	0.031**
	(0.014)	*(0.013)*	*(0.014)*	*(0.013)*
BestCompMix	-0.022	-0.021	-0.022	-0.021
	(0.014)	*(0.013)*	*(0.013)*	*(0.013)*
Period	-0.010***	-0.010***	-0.010***	-0.010***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*
mDE				
D				
DF				
F				
DEAD	0.143***	0.135***	0.143***	0.135***
	(0.032)	*(0.031)*	*(0.032)*	*(0.031)*
DEA	0.179**	0.181**	0.179**	0.181**
	(0.083)	*(0.078)*	*(0.083)*	*(0.078)*
DEAI	0.154*	0.128	0.154*	0.128
	(0.093)	*(0.088)*	*(0.093)*	*(0.087)*
BDEAD				
BDEA				
WDEAD	0.303***	0.262***	0.303***	0.262***
	(0.091)	*(0.088)*	*(0.091)*	*(0.088)*
WDEA	0.005	-0.006	0.005	-0.006
	(0.092)	*(0.087)*	*(0.092)*	*(0.087)*
UK		-0.087		-0.087
		(0.053)		*(0.053)*
Gender		-0.133***		-0.133***
		(0.050)		*(0.050)*
Age		0.000		0.000
		(0.007)		*(0.007)*
Social Desirability	0.015*	0.016**	0.015*	0.016**
		(0.008)		*(0.008)*

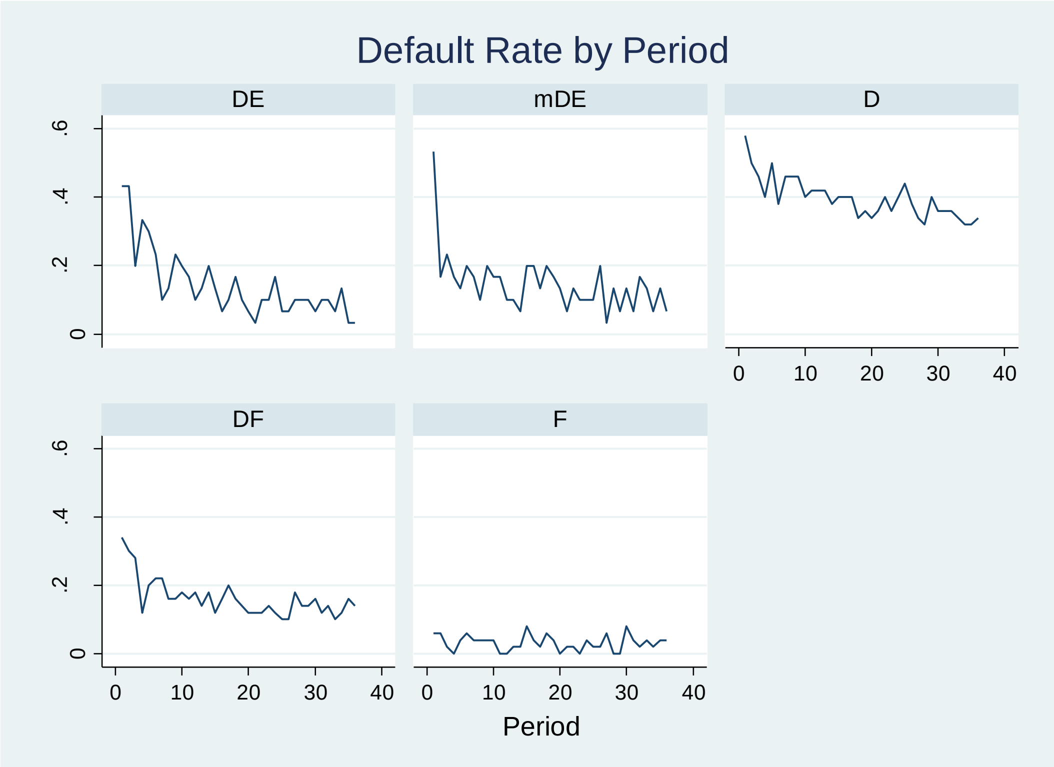
[88] The pseudo log likelihood for the models is -2161, -2147, -2157 and -2143. n = 6840 in models 1 and 3. n = 6768 in models 2 and 4.

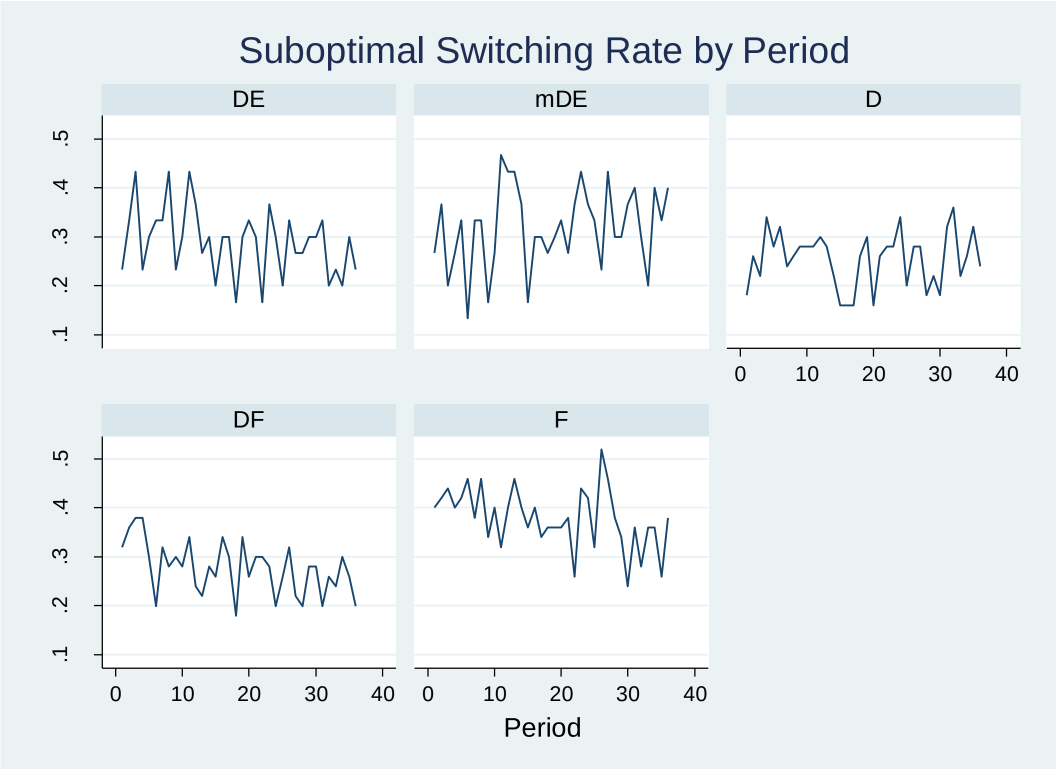
Appendix G

Time Trend of Consumer Performance

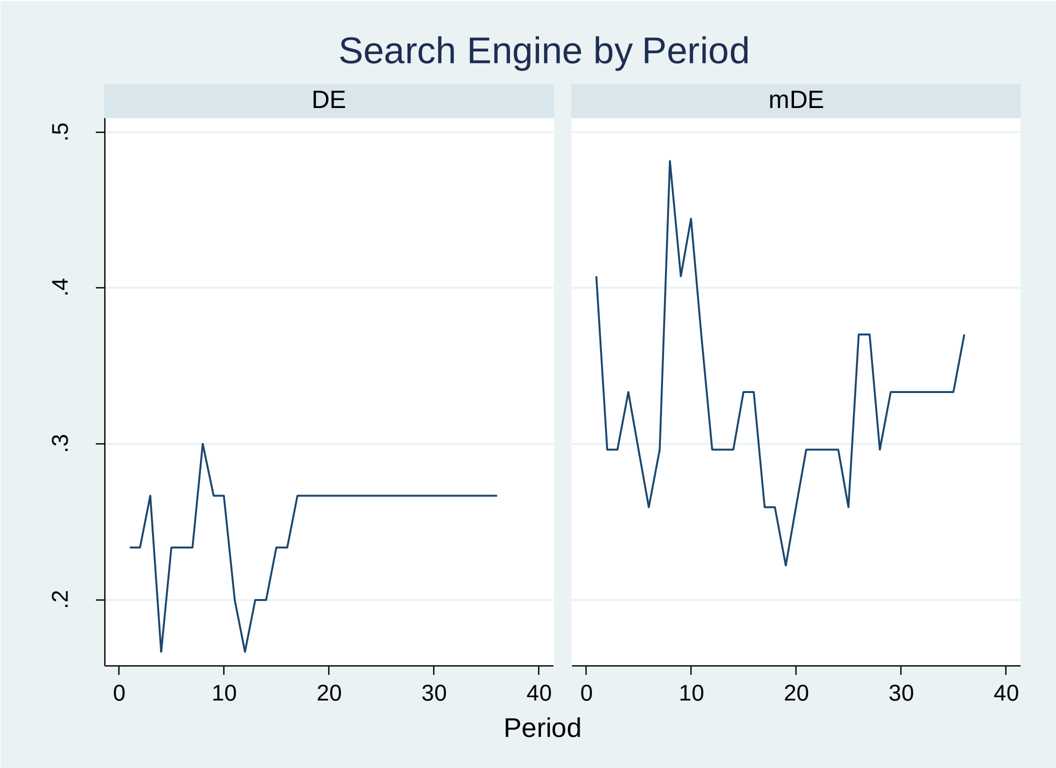
Below we list frequencies by period of key variables.

G1. Experiment 1



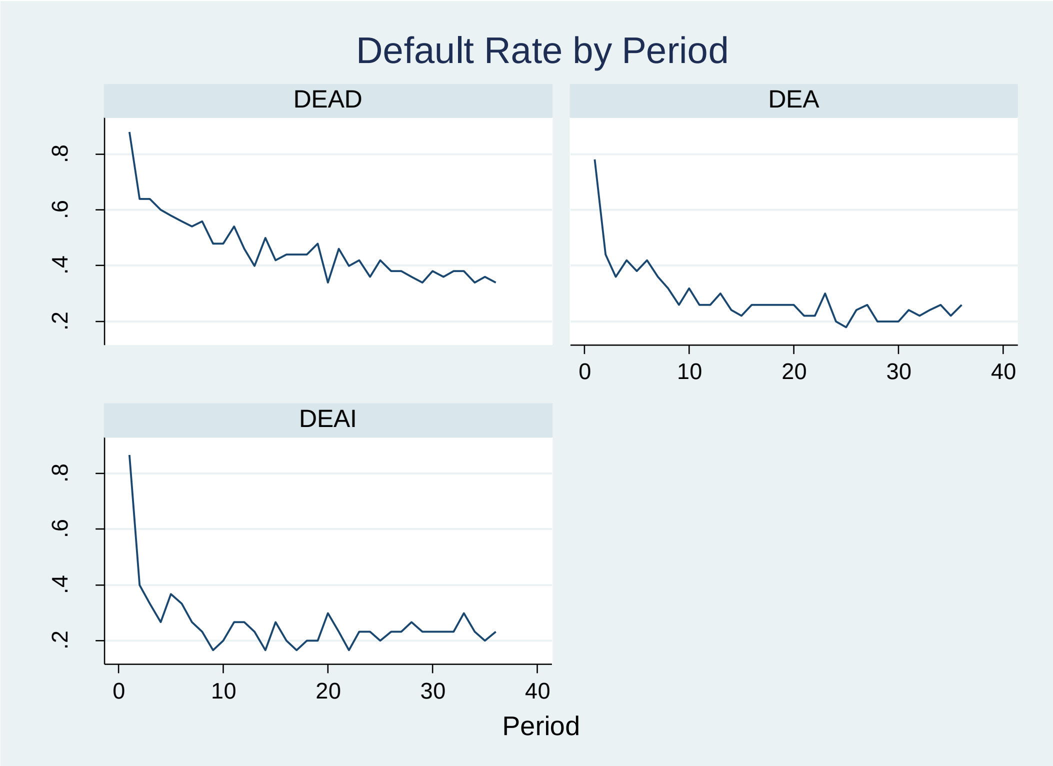


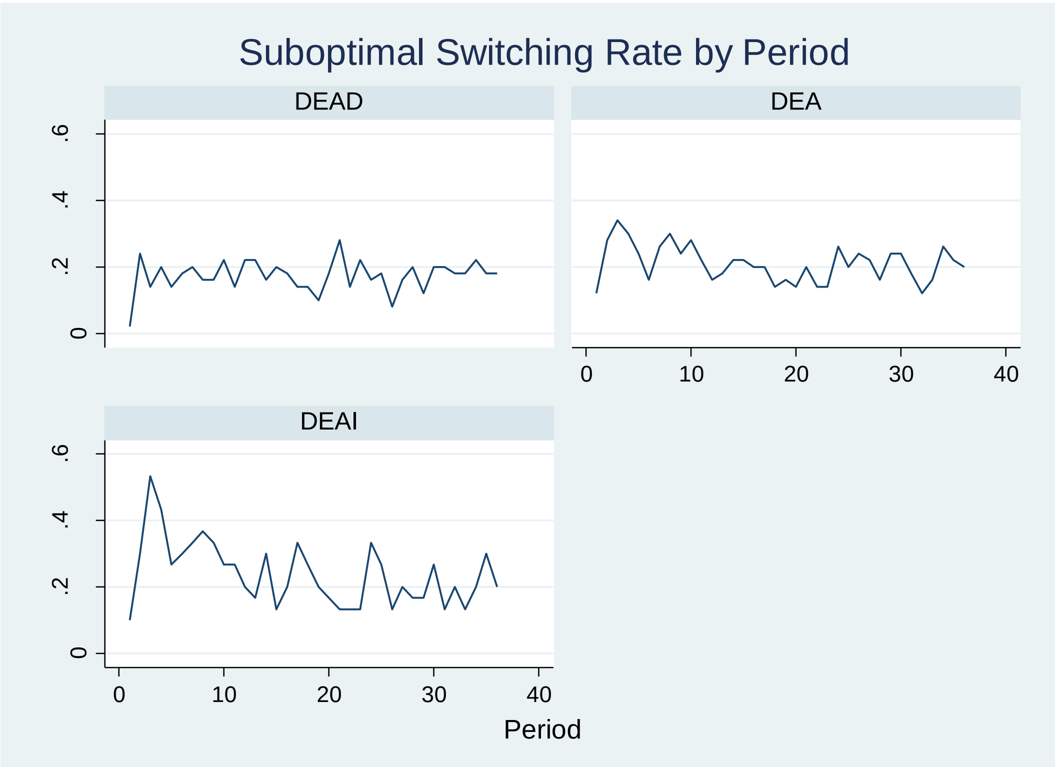
Suboptimal Switching Rate by Period
DE
mDE
D
DF
F
.1
.2
.3
.4
.5
0
10
20
30
40
Period

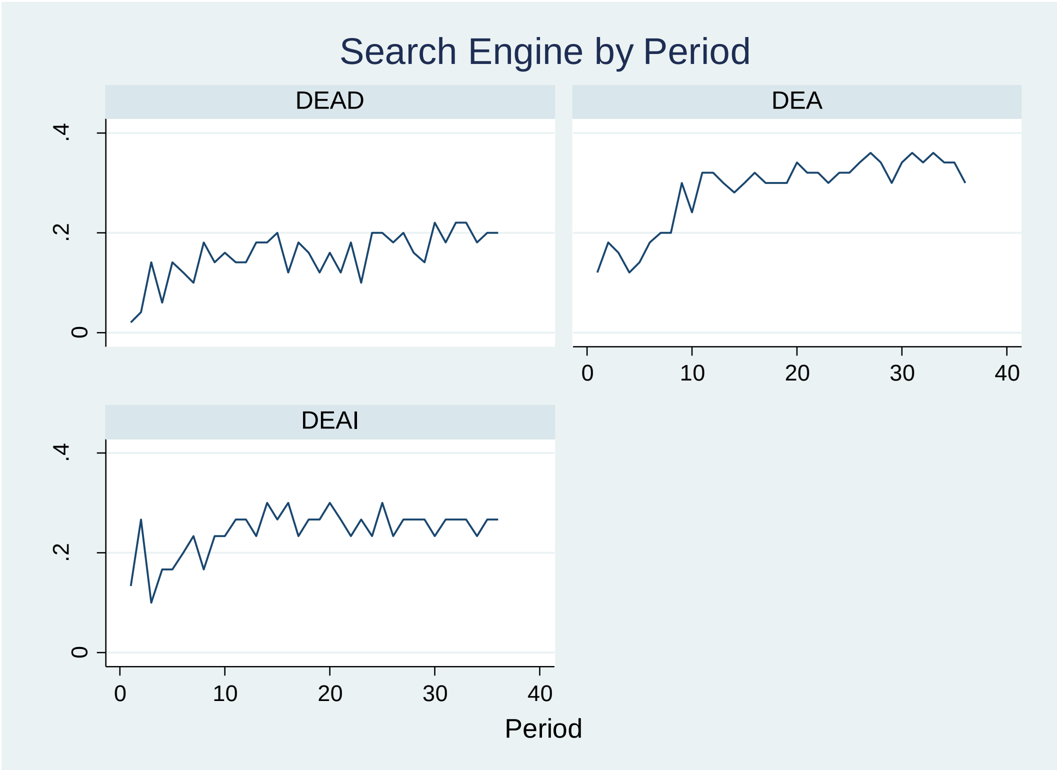


Search Engine by Period
DE
mDE
.2
.3
.4
.5
0
10
20
30
40
Period

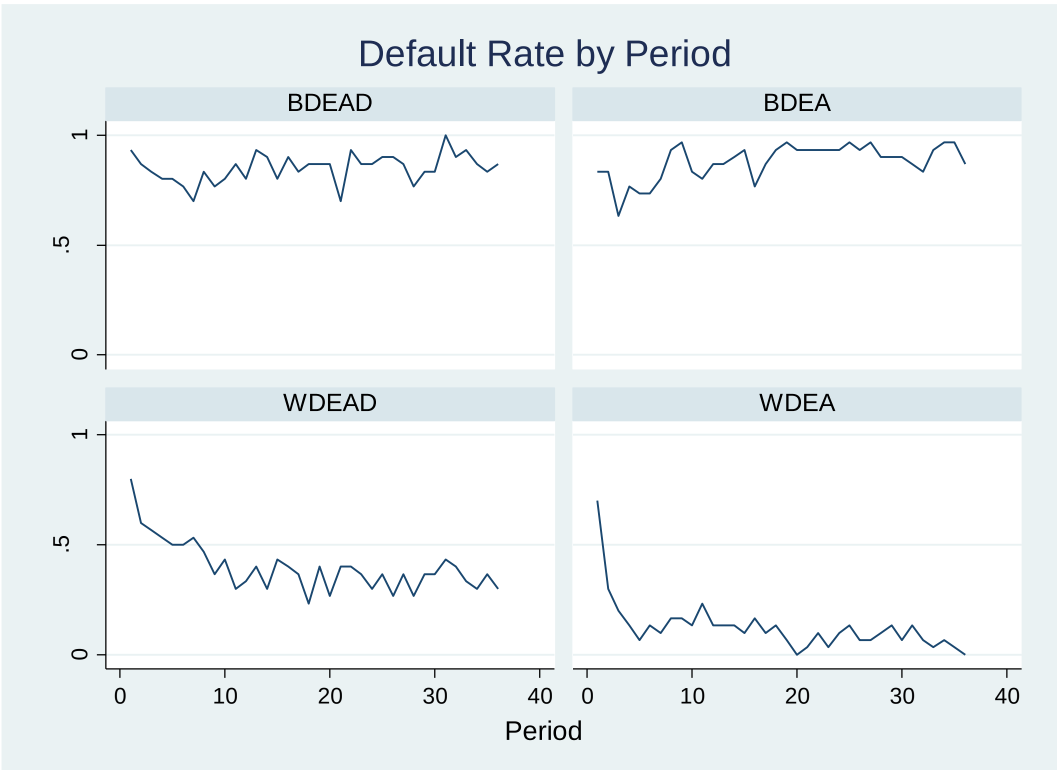
G2. Experiment 2

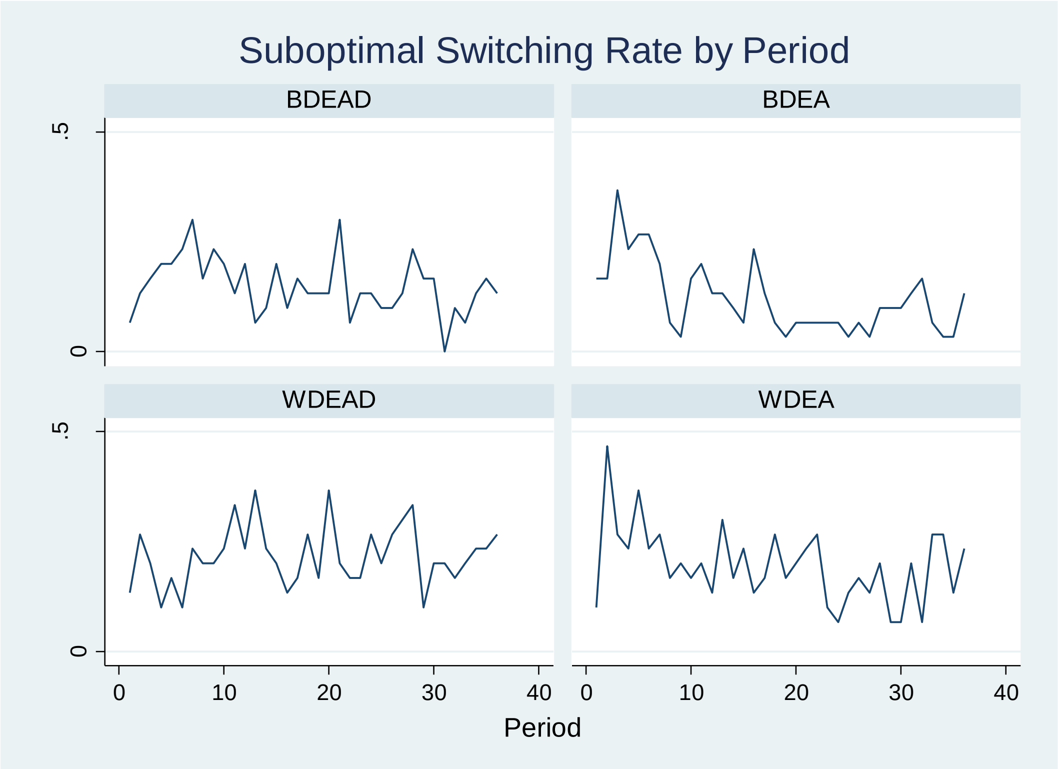




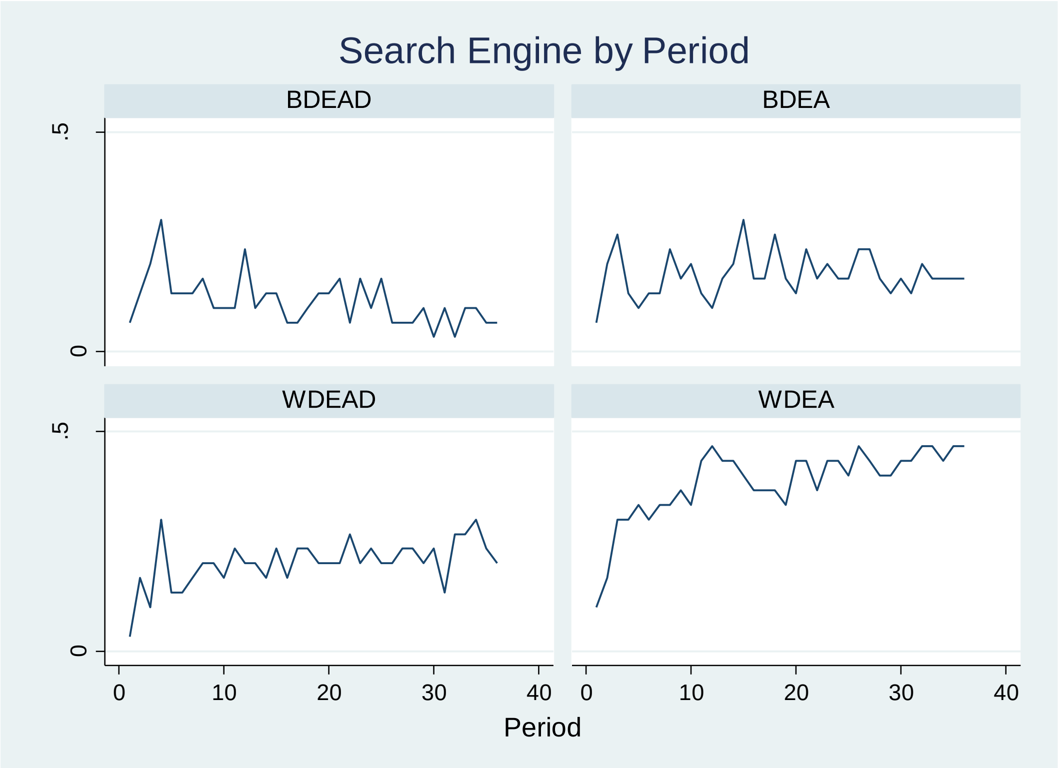


G3. Experiment 3





Suboptimal Switching Rate by Period
BDEAD
BDEA
WDEAD
WDEA
.5
0
0
10
20
30
40
Period



Search Engine by Period
BDEAD
BDEA
WDEAD
WDEA
.5
0
0
10
20
30
40
Period

Appendix H

Experimental Earnings (note that in treatments BDEAD and BDEA default tariff earnings and best tariff earnings are the same)

Treatment	Default Tariff	Suboptimal Tariff	Best Tariff
Panel a - Experiment 1			
DE	53951.49	66549.31	78923.59
mDE	55536.61	63210.58	78614.40
D	56836.60	67268.79	79074.65
DF	55935.77	66986.23	78735.26
F	53841.09	64907.16	78748.95
Average	56045.93	65799.78	78805.41
Panel b - Experiment 2			
DEAD	54619.51	63786.94	78336.90
DEA	56754.80	66215.39	79022.52
DEAI	57450.90	66710.52	79143.56
Average	55790.47	65550.84	78837.82
Panel c - Experiment 3			
BDEAD	77217.12	62763.29	77217.12
BDEA	78840.71	62703.03	78840.71
WDEAD	55519.44	63629.13	78580.61
WDEA	53207.95	64274.23	78423.91
Average	72689.58	63459.12	78209.50

Appendix I

Experimental instructions

Welcome to today's experiment on decision making. Today's session will begin shortly. Before we start, I have a few friendly reminders. First, to help us keep the lab neat and tidy, we ask that you do not eat or drink in the lab. Also, we ask that you turn off completely your mobile phones and other devices, as they may not be used during today's session. Please refrain from talking to other participants during the experiment. Finally, you are not allowed to use calculators, take notes or use the computer privately. Do not open any webpage or any other applications on this computer. Calculators, mobile phones, paper and pens are not allowed to be used in this experiment.

A copy of the instructions is on your desk. Please follow along as I read through the instructions. If you have a question, please raise your hand and I will come and answer it privately.

(On the desk in front of you, you will see a receipt form. DO NOT sign the receipt. You will sign the receipt form only once you have been paid.)

We are now ready to describe the nature of the tasks within the experiment.

Tasks

In the experiment, you will make decisions that involve buying goods from firms.

There are 10 tasks in this experiment. I will now describe a typical task for you.

At the beginning of each task, you are given an endowment of *game points*. Using this endowment, you must buy one unit of a good. Exactly the same good is being sold by 24 firms. Each firm offers you a price at which you can buy the good. We will call this the firm's *offer*. Each firm's offer is also in game points. Your job is to choose which firm to buy the good from. The price you pay the firm is deducted from your endowment to give your *payment points* for the round. Different firms may offer different prices, so how many payment points you earn will depend on which firm you buy the good from. However, all the prices will be less than your endowment. So, whichever firm you buy from, the number of payment points you earn will be positive.

It is in your interest to try to get as many payment points as possible in each task. At the end of the experiment, one of the 10 tasks will be selected randomly. We will find out how many payment points you earned in that task and convert them into UK pounds, using an *exchange rate*. We will pay you that amount of money, immediately after the experiment. The exchange rate between points and UK pounds may be different for different tasks. It will be shown on the screen for each task. In each task, the value of your endowment at the exchange rate will be £32.

Now I will explain more about the task.

[Activating the computer screen]

This is an example and your payment will NOT relate to this example. Please do not click anything except when I tell you to.

Each task has two pages: a "market page" and a "shopping basket page".

Market page

Now what you see is the market page.

Notice that your endowment and the exchange rate are shown on the top of the screen. It says: *"Your endowment is 64 points."* and *"The exchange rate is 2 points = £1."*

On the left hand side of the screen you can see 24 colored boxes, "OFFER A", OFFER B"……., "OFFER X". These are the offers of a good from the 24 firms (details of the offers are not yet visible). Different colors represent different "price structures" which I will explain later.

On the right hand side of the screen you can see a "shopping basket".

On the bottom of the screen you can see two buttons: The "View the shopping basket" button on the left and the "Move into the shopping basket" button on the right.

In the market page, you can do the following four actions:

1. Discover the *price structure* used by a firm to present its offer
2. Discover the *price details* of a firm's offer
3. Move an offer into the shopping basket
4. View the shopping basket

I will now explain these actions.

Please click ONCE on offer A. Notice that this is a red box. You can see that the offer now has a black frame round it. This tells you that you have chosen to look at this offer. The price structure of offer A is shown on the screen, below the 24 offers: it says "price structure 1". Now, click offer A again and wait. There is a delay of 3 seconds before anything happens. Now you can see the price details of offer A, below the price structure information: "*Original price = 15 points; Final price = Original price* x *110%*". This means that the final price is 15 points multiplied by 110%, which comes to 16.5 points. This is what you would have to pay if you bought the good from this firm. Your endowment in this task is 64 points. If you bought offer A, your payment points for this task would be 64 points - 16.5 points = 47.5 points. Please raise your hand if you have any questions.

If this was a real task, you would now be able to choose whether or not to put this offer into the shopping basket. For the purposes of this practice, let us now put this offer into the shopping basket. Click the "Move into the shopping basket" button at the bottom right of the screen. Now the offer A is moved into the shopping basket and you can see it in the shopping basket on the right side of the screen.

Now we'll look at another offer with the same price structure. Click once on offer D. Notice that this is also a red box. You can see the black frame has moved from offer A to offer D. Offer A's price structure and price details have both disappeared and offer D's price structure is now shown on the screen. Below the 24 offers, it says "price structure 1". It has the same price structure as offer A. Offers with the same color always have the same price structure.

Click offer D again and wait for 3 seconds. Now you can see the price details of offer D: "*Original price = 20 points; Final price = Original price* x *110%*". This means that the final price is 20 points multiplied by 110%, which comes to 22 points. Your endowment in this task is 64 points. If you bought offer D, your payment points for this task would be 64 points - 22 points = 42 points. Please raise your hand if you have any questions.

Remember that offer A was Original price x 110%. Offer A and offer D have different original prices (15 points or 20 points), but in both cases the original price is multiplied by the same percentage. This is because these two offers have the same price structure. In this example, all offers which use price structure 1 consist of an original price multiplied by 110%.

Put offer D into the shopping basket by clicking the "Move into the shopping basket" button.

Now we'll look at another offer with a different price structure. Click once on offer E. Notice that this is a yellow box. Below the 24 offers, it says: "price structure 2". Offers with different colors always have different price structures.

Click offer E again and wait for 3 seconds. Now you can see the price details of offer E showing below the price structure information: "*Original price = 18 points; Final price = Original price* x *120%*" This means that the final price is 18 points multiplied by 120%, which comes to 21.6 points. Your endowment in this task is 64 points. If you bought offer E, your payment points for this task would be 64 points - 21.6 points = 42.4 points. Please raise your hand if you have any questions.

Remember that offer A and offer D both had original prices multiplied by 110%. Offer E has an original price multiplied by 120%. This is because E has a different price structure than A and D. In this example, all offers which use price structure 2 consist of an original price multiplied by 120%.

Put offer E into the shopping basket by clicking the “Move into the shopping basket” button.

Now let us quickly put some more offers in the shopping basket. Do this for offer F, by clicking the offer once and then clicking the “Move into shopping basket” button.

And do the same for offer G.

And do the same for offer M.

And finally, do the same for offer W.

Now we will go to the shopping basket. Click “View the shopping basket” button.

Shopping basket page

After 3 seconds delay, you can see the shopping basket page.

Your endowment and the exchange rate are still shown at the top of the screen. These values stay the same throughout the task.

In the middle of the screen you can see the price details of the seven offers that you have put into the shopping basket. Notice that the shopping basket allows you to see the price details of several offers at the same time. This makes it easier to compare different offers.

On the left side of the screen, you can see a “Move offer out” button for each offer.

On the right side of the screen, you can see a “Buy the good from firm” button for each offer.

On the bottom of the screen, you can see a “Continue shopping” button.

In the shopping basket page, you can do the following three actions.

1. Move an offer out of the shopping basket
2. Continue shopping
3. Buy the good from one of the firms whose offers are in the shopping basket.

Let us move offer E out of the shopping basket. Click the “Move offer E out” button. After clicking, you can see the information of offer E, the “Move offer E out” button and the “Buy the good from firm E” button all disappear. This means that offer E has been moved out of the shopping basket.

Notice, the capacity of the shopping basket is nine. This means you cannot put more than nine offers into the shopping basket at the same time. If the shopping basket is full but you want to put some new offers into it, you must first move some old offers out of the shopping basket, and then go back to the market page.

Let us go back to the market page. Click the “Continue shopping” button.

Now you are in the market page again.

Put offer U into the shopping basket and click “View the shopping basket button”. Wait for 3 seconds.

Now you are back at the shopping basket page. You can see the price details of the offers in the basket, listed in the middle of the screen. Notice that this list includes offer U, which you just added to the basket.

If this was a real task, you would now be able to decide whether to buy the good from any one of the firms whose offers are in the shopping basket, or whether to continue shopping.

Now, look at offer A:

"*Original price = 15 points; Final price = Original price* x *110%*". This means that the final price is 15 points multiplied by 110%, which comes to 16.5 points. This is what you would have to pay if you bought the good from this firm. Your endowment in this round is 64 points. If you bought offer A, your payment points would be 64 points - 16.5 points = 47.5 points.

Now, let us assume that this is a real task and at the end of the experiment this round is randomly picked by the computer. So you would have 47.5 payment points, which would covert to UK pounds.

We know that the exchange rate in this round is 2 points = £1, so we would pay you 47.5 points / 2 = £23.75. This £23.75 is called your *final payment.* Please raise your hand if you have any questions.

Now, look at offer D:

"*Original price = 20 points; Final price = Original price* x *110%*". This means that the final price is 20 points multiplied by 110%, which comes to 22 points. Your endowment in this round is 64 points. If you bought offer A, your payment points would be 64 points - 22 points = 42 points.

Now, let us assume that this is a real task and at the end of the experiment this round is randomly picked by the computer. You would have 42 points, which would covert to UK pounds.

We know that the exchange rate in this round is 2 points = £1, so we would pay you 42 points / 2 = £21.00 and your final payment is £21.00 Please raise your hand if you have any questions.

Now, look at offer E

"*Original price = 18 points; Final price = Original* x *120%*" This means that the final price is 18 points multiplied by 120%, which comes to 21.6 points. Your endowment in this round is 64 points. If you bought offer E, your payment points would be 64 points - 21.6 points = 42.4 points.

Now, let us assume that this is a real task and at the end of the experiment this round is randomly picked by the computer. You would have 42.4 points, which would covert to UK pounds.

We know that the exchange rate in this round is 2 points = £1, so we would pay you 42.4 points / 2 = £21.20. Please raise your hand if you have any questions.

For the purposes of this practice, let us now buy the good from any firm you want. Click a "Buy the good from firm" button at the right hand side of the screen.

If this was a real task, your payment points for this task would be your endowment minus the final price of the offer you have bought.

The practice is over.

Please answer the following questions. After finishing these questions, please raise your hand and I will come to you to check your answers… If all your answers correct, you will be able to start the experiment.

Questionnaire

Questions 1 to 6: please answer each question ticking ONE box.

1. The capacity of the shopping basket is:

A. 8 offers

B. 10 offers

C. 9 offers

D No limitation

2. At the beginning of each task, you are endowed with:

A. Payment points that convert to £32

B. Payment points that convert to £64

C. 32 payment points

D. Nothing

3. If you want to move an offer out of the shopping basket, you can do it in the

A. Market page

B. Shopping basket page

C. Both pages

D. You cannot move an offer out of the shopping basket

4. The exchange rate between payment points and UK pounds in different tasks:

A. Is always the same

B. May be different

5. Offers with the same color:

A. Have the same price structure

B. Have the same final price

C. May have different price structures and different final prices

6. You payment is equal to:

A. The final price of the good

B. Your endowment

C. Your endowment minus the final price of the good

Questions 7 to 9: Please answer each question by filling in the blanks.

7. If your endowment is 200 points and the final price of the offer you select is:

Original price = 100 points; Final price = Original price + 20 points

then your payment points are _______.

If this is the selected round and the exchange rate is *20 points = 1 pound*, your final payment is ____ pounds

8. If your endowment is 200 points and the final price of the offer you select is:

Original price = 200 points; Final price = Original price x *60%*

then your payment points are _______.

If this is the selected round and the exchange rate is *10 points = 1 pound*, your final payment is ____ pounds.

9. If your endowment is 200 points and the final price of a selected offer is:

Original price = 100 points; Final price = Original price x *60% + 40 points*

then your payment points are _______.

If this is the selected round and the exchange rate is *50 points = 1 pound*, your final payment is ____ pounds.

Answers:

1.C 2.A 3.B 4.B 5.A 6.C

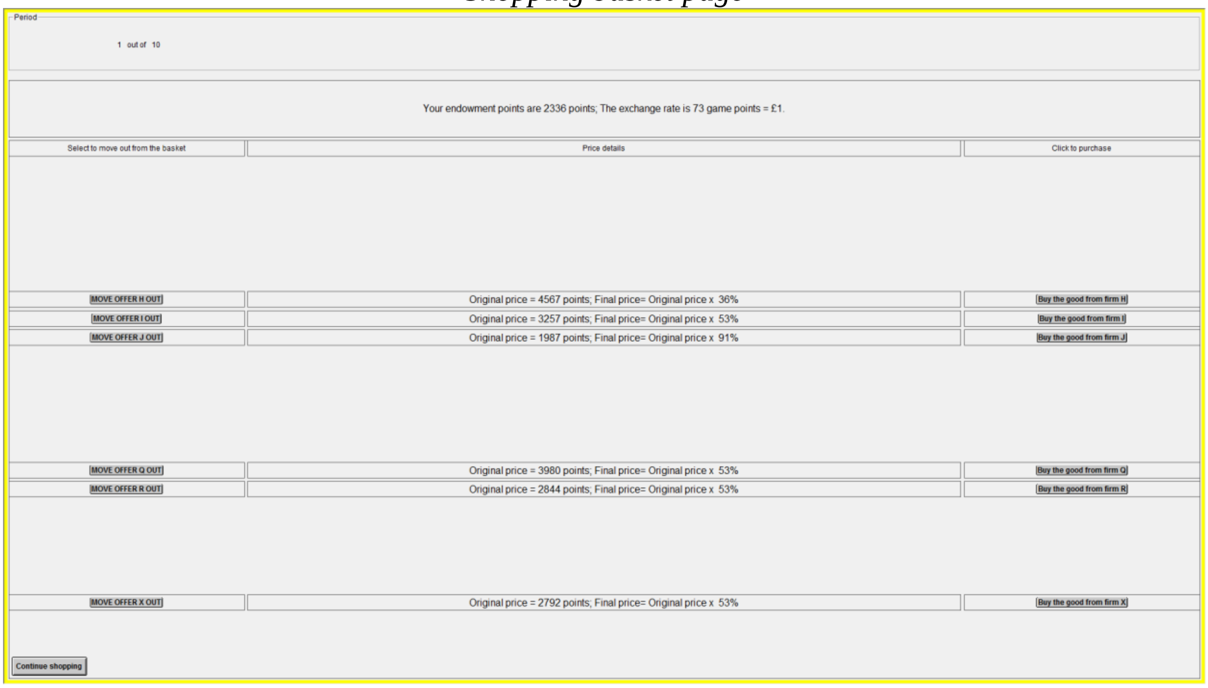
7. 80; 4 8. 80; 8 9.100; 2

Appendix J

An example of experimental tasks screen shots

Marketing page

Shopping basket page



Appendix K

Table K1
Twenty-four offers' final money prices from low to high and corresponding earnings if a participant chooses this offer

Offers	Final money prices (£)	Earnings (Endowment minus Final money price) (£)
1	20.27	11.73
2	20.645	11.355
3	21.02	10.98
4	21.395	10.605
5	21.77	10.23
6	22.145	9.855
7	22.52	9.48
8	22.895	9.105
9	23.27	8.73
10	23.645	8.355
11	24.02	7.98
12	24.395	7.605
13	24.77	7.23
14	25.145	6.855
15	25.52	6.48
16	25.895	6.105
17	26.27	5.73
18	26.645	5.355
19	27.02	4.98
20	27.395	4.605
21	27.77	4.23
22	28.145	3.855
23	28.52	3.48
24	28.895	3.105

Table K2
Number of participants who inspected different number of common standard offers: PC (P1*D1) task

Number of common standard offers inspected	Number of participants	Percent (%)
0	0	0
1	6	3.51
2	4	2.34
3	3	1.75
4	3	1.75
5	5	2.92
6	3	1.75
7	6	3.51
8	141	82.46

* 171 observations

Table K3
Number of participants who inspected different number of individuated standard offers: PC (P1*D1) task

Number of individuated standard offers inspected	Number of participants	Percent (%)
0	1	0.58
1	1	0.58
2	1	0.58
3	4	2.34
4	0	0
5	4	2.34
6	6	3.51
7	2	1.17
8	5	2.92
9	9	5.26
10	7	4.09
11	6	3.51
12	1	0.58
13	6	3.51
14	3	1.75
15	12	7.02
16	103	60.23

* 171 observations

Table K4
Number of participants who inspected different number of common standard offers: PC (P1*D1*D2) task

Number of common standard offers inspected	Number of participants	Percent (%)
0	3	1.75
1	4	2.34
2	6	3.51
3	10	5.85
4	2	1.17
5	3	1.75
6	4	2.34
7	8	4.68
8	131	76.61

* 171 observations

Table K5
Number of participants who inspected different number of individuated standard offers: PC (P1*D1*D2) task

Number of individuated standard offers inspected	Number of participants	Percent (%)
0	1	0.58
1	5	2.92
2	3	1.75
3	1	0.58
4	4	2.34
5	1	0.58
6	4	2.34
7	10	5.85
8	17	9.94
9	9	5.26
10	5	2.92
11	4	2.34
12	2	1.17
13	7	4.09
14	6	3.51
15	10	5.85
16	82	47.95

* 171 observations

Table K6

Number of participants who inspected different number of common standard offers PC (P1*D1+P2) task

Number of common standard offers inspected	Number of participants	Percent (%)
0	3	1.75
1	10	5.85
2	2	1.17
3	3	1.75
4	1	0.58
5	5	2.92
6	2	1.17
7	16	9.36
8	129	75.44

* 171 observations

Table K7

Number of participants who inspected different number of individuated standard offers PC (P1*D1+P2) task

Number of individuated standard offers inspected	Number of participants	Percent (%)
0	2	1.17
1	3	1.75
2	2	1.17
3	5	2.92
4	1	0.58
5	3	1.75
6	4	2.34
7	2	1.17
8	9	5.26
9	10	5.85
10	3	1.75
11	6	3.51
12	4	2.34
13	6	3.51
14	10	5.85
15	6	3.51
16	95	55.56

* 171 observations

Table K8
Proportion of common standard and individual standard inspections over course of task: PC (P1*D1) task

Clicking time interval	Proportion of common standard inspections	Proportion of individuated standard inspections
10%	0.687	0.313
20%	0.696	0.304
30%	0.714	0.286
40%	0.365	0.635
50%	0.231	0.769
60%	0.188	0.812
70%	0.127	0.873
80%	0.153	0.847
90%	0.134	0.866
100%	0.114	0.886

Table K9
Proportion of common standard and individual standard inspections over course of task: PC (P1*D1*D2) task

Clicking time interval	Proportion of common standard inspections	Proportion of individuated standard inspections
10%	0.654	0.346
20%	0.689	0.311
30%	0.658	0.342
40%	0.434	0.566
50%	0.309	0.691
60%	0.160	0.840
70%	0.196	0.804
80%	0.131	0.869
90%	0.150	0.850
100%	0.107	0.893

Table K10
Proportion of common standard and individual standard inspections over course of task: PC (P1*D1+P2) task

Clicking time interval	Proportion of common standard inspections	Proportion of individuated standard inspections
10%	0.703	0.297
20%	0.739	0.261
30%	0.702	0.298
40%	0.396	0.604
50%	0.240	0.760
60%	0.176	0.824
70%	0.113	0.888
80%	0.115	0.885
90%	0.116	0.884
100%	0.146	0.854

Table K11
Proportion of common standard and individuated standard offers in the shopping basket: PC (P1*D1) task

Clicking Time interval	Proportion of common standard offers in the shopping basket	Proportion of individuated standard offers in the shopping basket
10%	0.619	0.381
20%	0.621	0.379
30%	0.659	0.341
40%	0.568	0.432
50%	0.409	0.591
60%	0.333	0.667
70%	0.296	0.704
80%	0.261	0.739
90%	0.224	0.776
100%	0.225	0.775

Table K12
Proportion of the common standard and individuated standard offers in the shopping basket: PC (P1*D1*D2) task

Clicking Time interval	Proportion of common standard offers in the shopping basket	Proportion of individuated standard offers in the shopping basket
10%	0.637	0.363
20%	0.660	0.340
30%	0.659	0.341
40%	0.597	0.403
50%	0.458	0.542
60%	0.373	0.627
70%	0.322	0.678
80%	0.275	0.725
90%	0.228	0.772
100%	0.214	0.786

Table K13
Proportion of the common standard and individuated standard offers in the shopping basket: PC (P1*D1+P2) task

Clicking Time interval	Proportion of common standard offers in the shopping basket	Proportion of individuated standard offers in the shopping basket
10%	0.699	0.301
20%	0.685	0.315
30%	0.695	0.305
40%	0.608	0.392
50%	0.423	0.577
60%	0.339	0.661
70%	0.290	0.710
80%	0.233	0.767
90%	0.213	0.787
100%	0.199	0.801

Table K14
ProbabilityInBasket of common standard offers with different ranks: AC (P1) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.017	0.027	0.033	0.026	0.023	0.036	0.025	0.039	0.019	0.047	0.028	0.038
20%	0.090	0.122	0.147	0.126	0.105	0.095	0.116	0.107	0.067	0.128	0.133	0.117
30%	0.152	0.241	0.241	0.218	0.188	0.184	0.186	0.167	0.137	0.176	0.192	0.181
40%	0.184	0.287	0.233	0.220	0.158	0.147	0.088	0.123	0.099	0.097	0.093	0.118
50%	0.254	0.326	0.254	0.231	0.179	0.135	0.082	0.109	0.093	0.090	0.058	0.088
60%	0.346	0.429	0.330	0.287	0.224	0.198	0.163	0.167	0.155	0.141	0.088	0.142
70%	0.397	0.474	0.355	0.303	0.226	0.223	0.185	0.196	0.158	0.154	0.117	0.127
80%	0.457	0.470	0.349	0.236	0.166	0.174	0.113	0.111	0.092	0.074	0.067	0.076
90%	0.603	0.539	0.366	0.282	0.178	0.208	0.139	0.142	0.125	0.093	0.091	0.100
100%	0.858	0.584	0.398	0.338	0.227	0.224	0.168	0.137	0.126	0.123	0.126	0.121

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.042	0.022	0.024	0.029	0.046	0.022	0.026	0.031	0.020	0.048	0.044	0.015
20%	0.148	0.093	0.110	0.112	0.099	0.106	0.085	0.091	0.098	0.102	0.149	0.048
30%	0.221	0.152	0.150	0.155	0.145	0.151	0.143	0.117	0.144	0.171	0.172	0.084
40%	0.121	0.094	0.096	0.062	0.085	0.062	0.086	0.045	0.068	0.102	0.062	0.045
50%	0.074	0.065	0.083	0.077	0.060	0.066	0.086	0.064	0.054	0.086	0.080	0.041
60%	0.129	0.133	0.123	0.151	0.119	0.121	0.108	0.119	0.103	0.147	0.146	0.082
70%	0.117	0.159	0.119	0.143	0.129	0.122	0.116	0.124	0.099	0.148	0.132	0.098
80%	0.082	0.074	0.047	0.055	0.072	0.058	0.068	0.060	0.051	0.060	0.067	0.051
90%	0.135	0.094	0.097	0.071	0.102	0.082	0.101	0.081	0.073	0.059	0.095	0.116
100%	0.146	0.085	0.126	0.111	0.135	0.149	0.137	0.093	0.106	0.093	0.139	0.192

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K15

ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.018	0.033	0.012	0.017	0.034	0.040	0.039	0.048	0.021	0.041	0.045	0.038
20%	0.096	0.106	0.063	0.119	0.123	0.121	0.110	0.118	0.127	0.110	0.132	0.134
30%	0.143	0.177	0.152	0.218	0.183	0.188	0.166	0.176	0.178	0.180	0.151	0.187
40%	0.175	0.246	0.176	0.236	0.147	0.157	0.129	0.099	0.104	0.120	0.093	0.110
50%	0.231	0.315	0.237	0.277	0.177	0.159	0.153	0.082	0.098	0.085	0.109	0.107
60%	0.343	0.407	0.363	0.320	0.250	0.221	0.195	0.134	0.163	0.129	0.138	0.169
70%	0.457	0.434	0.402	0.324	0.250	0.239	0.204	0.165	0.160	0.155	0.148	0.164
80%	0.550	0.431	0.365	0.242	0.181	0.186	0.129	0.096	0.099	0.090	0.083	0.089
90%	0.671	0.507	0.405	0.291	0.226	0.206	0.169	0.122	0.124	0.118	0.114	0.137
100%	0.851	0.558	0.414	0.299	0.226	0.209	0.181	0.152	0.133	0.148	0.130	0.150

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.029	0.039	0.037	0.047	0.023	0.042	0.032	0.022	0.043	0.037	0.025	0.019
20%	0.118	0.087	0.100	0.133	0.123	0.131	0.105	0.093	0.141	0.131	0.089	0.059
30%	0.193	0.122	0.138	0.169	0.183	0.170	0.162	0.142	0.196	0.173	0.136	0.110
40%	0.075	0.062	0.071	0.066	0.091	0.086	0.059	0.062	0.094	0.084	0.097	0.058
50%	0.052	0.068	0.077	0.063	0.083	0.051	0.027	0.070	0.075	0.080	0.090	0.078
60%	0.089	0.092	0.144	0.133	0.141	0.124	0.072	0.099	0.117	0.170	0.139	0.125
70%	0.086	0.098	0.161	0.122	0.160	0.124	0.112	0.103	0.111	0.161	0.132	0.110
80%	0.034	0.060	0.058	0.063	0.075	0.056	0.068	0.068	0.048	0.096	0.071	0.067
90%	0.052	0.100	0.066	0.056	0.081	0.089	0.091	0.143	0.070	0.110	0.125	0.138
100%	0.093	0.132	0.110	0.109	0.107	0.103	0.112	0.166	0.102	0.142	0.173	0.179

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K16
ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1*D2) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.003	0.026	0.051	0.041	0.032	0.019	0.057	0.032	0.047	0.027	0.029	0.025
20%	0.045	0.113	0.137	0.153	0.092	0.087	0.113	0.110	0.142	0.128	0.118	0.111
30%	0.120	0.180	0.222	0.237	0.149	0.159	0.171	0.159	0.184	0.185	0.157	0.147
40%	0.162	0.229	0.253	0.213	0.153	0.130	0.126	0.087	0.113	0.089	0.073	0.093
50%	0.189	0.292	0.278	0.250	0.161	0.134	0.159	0.117	0.116	0.094	0.086	0.108
60%	0.280	0.373	0.317	0.314	0.221	0.245	0.207	0.177	0.201	0.168	0.140	0.171
70%	0.354	0.437	0.359	0.341	0.231	0.258	0.186	0.207	0.214	0.154	0.130	0.161
80%	0.445	0.441	0.354	0.283	0.148	0.159	0.117	0.141	0.125	0.086	0.055	0.104
90%	0.581	0.481	0.407	0.324	0.168	0.182	0.146	0.181	0.128	0.105	0.108	0.116
100%	0.807	0.532	0.393	0.292	0.171	0.179	0.143	0.188	0.142	0.079	0.126	0.118

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.034	0.021	0.023	0.011	0.038	0.030	0.038	0.041	0.023	0.047	0.055	0.011
20%	0.126	0.096	0.093	0.077	0.111	0.101	0.123	0.110	0.105	0.162	0.142	0.053
30%	0.191	0.140	0.138	0.170	0.162	0.135	0.144	0.134	0.165	0.219	0.174	0.085
40%	0.105	0.093	0.077	0.087	0.100	0.053	0.078	0.073	0.076	0.101	0.104	0.059
50%	0.064	0.076	0.069	0.074	0.074	0.064	0.121	0.052	0.063	0.078	0.099	0.069
60%	0.102	0.118	0.139	0.103	0.136	0.136	0.169	0.084	0.114	0.128	0.146	0.081
70%	0.112	0.157	0.152	0.113	0.127	0.141	0.156	0.120	0.105	0.125	0.142	0.085
80%	0.079	0.078	0.066	0.057	0.107	0.070	0.104	0.076	0.072	0.091	0.088	0.070
90%	0.113	0.111	0.094	0.095	0.161	0.087	0.121	0.096	0.115	0.128	0.114	0.149
100%	0.097	0.122	0.105	0.133	0.152	0.101	0.142	0.140	0.143	0.137	0.142	0.218

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K17

ProbabilityInBasket of common standard offers with different ranks: AC (P1*D1+P2) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.012	0.049	0.034	0.029	0.025	0.020	0.027	0.032	0.025	0.027	0.049	0.034
20%	0.065	0.144	0.108	0.130	0.113	0.060	0.128	0.131	0.128	0.132	0.136	0.106
30%	0.121	0.265	0.182	0.218	0.222	0.153	0.217	0.205	0.179	0.188	0.171	0.150
40%	0.162	0.321	0.177	0.164	0.201	0.136	0.151	0.111	0.106	0.098	0.104	0.086
50%	0.238	0.378	0.235	0.163	0.211	0.135	0.128	0.127	0.120	0.143	0.117	0.078
60%	0.327	0.457	0.330	0.267	0.256	0.231	0.211	0.208	0.139	0.235	0.160	0.150
70%	0.373	0.496	0.345	0.250	0.257	0.204	0.189	0.187	0.143	0.178	0.150	0.134
80%	0.468	0.502	0.295	0.216	0.202	0.147	0.122	0.130	0.101	0.095	0.091	0.084
90%	0.654	0.569	0.348	0.258	0.249	0.166	0.145	0.160	0.146	0.124	0.141	0.076
100%	0.819	0.585	0.383	0.288	0.250	0.216	0.180	0.176	0.148	0.125	0.141	0.094

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.026	0.024	0.040	0.049	0.031	0.059	0.032	0.041	0.026	0.038	0.038	0.019
20%	0.118	0.102	0.118	0.139	0.100	0.147	0.082	0.108	0.090	0.108	0.109	0.087
30%	0.181	0.142	0.160	0.207	0.167	0.183	0.115	0.151	0.152	0.145	0.156	0.135
40%	0.098	0.062	0.090	0.087	0.091	0.101	0.064	0.070	0.080	0.058	0.076	0.061
50%	0.104	0.071	0.094	0.073	0.105	0.097	0.051	0.053	0.090	0.051	0.086	0.056
60%	0.162	0.128	0.142	0.130	0.155	0.155	0.115	0.133	0.150	0.104	0.166	0.083
70%	0.164	0.113	0.108	0.122	0.149	0.130	0.127	0.140	0.135	0.102	0.150	0.078
80%	0.072	0.092	0.057	0.107	0.067	0.062	0.049	0.055	0.069	0.060	0.113	0.063
90%	0.108	0.131	0.091	0.131	0.095	0.106	0.102	0.114	0.116	0.136	0.131	0.115
100%	0.105	0.152	0.141	0.116	0.115	0.119	0.129	0.119	0.118	0.149	0.151	0.149

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K18

ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.033	0.028	0.027	0.031	0.019	0.053	0.022	0.029	0.041	0.022	0.043	0.029
20%	0.081	0.112	0.094	0.118	0.095	0.151	0.119	0.110	0.147	0.093	0.113	0.115
30%	0.112	0.187	0.171	0.208	0.152	0.207	0.215	0.179	0.197	0.185	0.130	0.169
40%	0.128	0.219	0.180	0.243	0.144	0.201	0.169	0.179	0.178	0.204	0.083	0.124
50%	0.177	0.304	0.252	0.295	0.166	0.232	0.205	0.195	0.236	0.250	0.139	0.121
60%	0.259	0.385	0.309	0.347	0.263	0.311	0.243	0.217	0.302	0.272	0.223	0.180
70%	0.299	0.379	0.298	0.333	0.296	0.289	0.244	0.224	0.269	0.192	0.227	0.224
80%	0.336	0.398	0.302	0.310	0.287	0.295	0.237	0.241	0.252	0.184	0.186	0.207
90%	0.423	0.483	0.380	0.342	0.351	0.327	0.262	0.286	0.306	0.229	0.215	0.270
100%	0.476	0.460	0.355	0.333	0.303	0.269	0.214	0.229	0.243	0.199	0.150	0.199

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.040	0.031	0.029	0.034	0.023	0.041	0.022	0.020	0.028	0.027	0.026	0.029
20%	0.119	0.084	0.130	0.121	0.099	0.128	0.105	0.114	0.121	0.104	0.088	0.083
30%	0.151	0.151	0.172	0.179	0.144	0.160	0.178	0.169	0.165	0.149	0.101	0.110
40%	0.109	0.134	0.113	0.111	0.111	0.073	0.144	0.108	0.109	0.114	0.068	0.067
50%	0.170	0.140	0.138	0.125	0.128	0.078	0.169	0.143	0.119	0.112	0.114	0.092
60%	0.195	0.157	0.160	0.161	0.124	0.136	0.241	0.183	0.138	0.146	0.142	0.136
70%	0.184	0.167	0.148	0.136	0.120	0.130	0.228	0.153	0.107	0.118	0.093	0.115
80%	0.159	0.140	0.124	0.116	0.142	0.107	0.204	0.144	0.090	0.093	0.111	0.088
90%	0.177	0.176	0.171	0.154	0.173	0.161	0.221	0.178	0.114	0.137	0.177	0.153
100%	0.146	0.130	0.155	0.124	0.108	0.131	0.165	0.150	0.091	0.091	0.146	0.139

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K19

ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1*D2) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.026	0.026	0.040	0.020	0.025	0.036	0.031	0.024	0.044	0.022	0.029	0.020
20%	0.063	0.086	0.131	0.084	0.082	0.132	0.126	0.100	0.138	0.082	0.097	0.085
30%	0.116	0.163	0.188	0.156	0.154	0.200	0.198	0.165	0.183	0.137	0.162	0.126
40%	0.136	0.178	0.198	0.199	0.152	0.178	0.217	0.180	0.167	0.144	0.148	0.114
50%	0.176	0.230	0.232	0.253	0.167	0.196	0.214	0.201	0.143	0.199	0.158	0.124
60%	0.255	0.237	0.260	0.273	0.238	0.205	0.229	0.227	0.164	0.214	0.159	0.171
70%	0.273	0.254	0.262	0.269	0.239	0.236	0.213	0.207	0.187	0.204	0.179	0.142
80%	0.311	0.270	0.251	0.283	0.254	0.211	0.235	0.206	0.219	0.208	0.209	0.168
90%	0.368	0.319	0.289	0.355	0.278	0.237	0.286	0.250	0.272	0.225	0.240	0.202
100%	0.396	0.313	0.260	0.327	0.276	0.223	0.279	0.229	0.241	0.223	0.261	0.194

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.042	0.019	0.018	0.021	0.019	0.042	0.036	0.015	0.028	0.021	0.015	0.016
20%	0.132	0.073	0.081	0.121	0.088	0.136	0.095	0.068	0.102	0.082	0.077	0.072
30%	0.187	0.136	0.139	0.169	0.154	0.203	0.140	0.142	0.165	0.146	0.134	0.109
40%	0.168	0.135	0.148	0.171	0.134	0.175	0.147	0.123	0.122	0.126	0.120	0.092
50%	0.201	0.111	0.182	0.175	0.139	0.178	0.172	0.164	0.114	0.191	0.100	0.079
60%	0.204	0.137	0.211	0.194	0.179	0.175	0.200	0.165	0.131	0.212	0.116	0.098
70%	0.203	0.153	0.206	0.167	0.174	0.153	0.188	0.167	0.166	0.213	0.126	0.113
80%	0.228	0.138	0.222	0.190	0.205	0.192	0.204	0.201	0.209	0.238	0.135	0.139
90%	0.239	0.176	0.266	0.232	0.242	0.220	0.235	0.232	0.251	0.225	0.185	0.195
100%	0.225	0.143	0.216	0.181	0.185	0.176	0.214	0.193	0.229	0.220	0.177	0.181

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K20

ProbabilityInBasket of individuated standard offers with different ranks: NC (P1*D1+P2) task

Clicking time interval	Optimal offer (171)	Offer rank 2 (171)	Offer rank 3 (171)	Offer rank 4 (171)	Offer rank 5 (171)	Offer rank 6 (171)	Offer rank 7 (171)	Offer rank 8 (171)	Offer rank 9 (171)	Offer rank 10 (171)	Offer rank 11 (171)	Offer rank 12 (171)
10%	0.030	0.026	0.025	0.030	0.042	0.043	0.016	0.022	0.031	0.047	0.035	0.033
20%	0.090	0.108	0.113	0.127	0.093	0.147	0.104	0.101	0.134	0.154	0.103	0.131
30%	0.166	0.162	0.164	0.195	0.154	0.187	0.187	0.154	0.213	0.207	0.167	0.180
40%	0.208	0.177	0.143	0.195	0.139	0.162	0.201	0.152	0.175	0.165	0.155	0.155
50%	0.256	0.205	0.179	0.230	0.188	0.173	0.227	0.172	0.212	0.163	0.180	0.194
60%	0.314	0.242	0.222	0.268	0.256	0.241	0.275	0.216	0.265	0.216	0.195	0.221
70%	0.336	0.278	0.245	0.275	0.264	0.227	0.269	0.218	0.238	0.219	0.197	0.201
80%	0.366	0.295	0.247	0.296	0.250	0.224	0.226	0.224	0.234	0.226	0.234	0.220
90%	0.442	0.349	0.303	0.369	0.301	0.285	0.251	0.268	0.278	0.277	0.281	0.269
100%	0.474	0.371	0.293	0.363	0.269	0.247	0.204	0.225	0.218	0.228	0.205	0.184

Clicking time interval	Offer rank 13 (171)	Offer rank 14 (171)	Offer rank 15 (171)	Offer rank 16 (171)	Offer rank 17 (171)	Offer rank 18 (171)	Offer rank 19 (171)	Offer rank 20 (171)	Offer rank 21 (171)	Offer rank 22 (171)	Offer rank 23 (171)	Offer rank 24 (171)
10%	0.020	0.030	0.031	0.026	0.014	0.036	0.044	0.028	0.053	0.017	0.018	0.039
20%	0.110	0.094	0.113	0.115	0.075	0.112	0.140	0.105	0.128	0.072	0.075	0.084
30%	0.141	0.150	0.142	0.155	0.102	0.131	0.157	0.111	0.174	0.123	0.118	0.087
40%	0.127	0.152	0.139	0.121	0.077	0.091	0.100	0.107	0.137	0.126	0.100	0.075
50%	0.136	0.182	0.165	0.139	0.099	0.145	0.108	0.092	0.155	0.127	0.104	0.089
60%	0.167	0.209	0.213	0.154	0.121	0.193	0.151	0.119	0.157	0.157	0.110	0.109
70%	0.176	0.174	0.178	0.114	0.120	0.166	0.149	0.087	0.124	0.137	0.107	0.096
80%	0.170	0.182	0.170	0.092	0.104	0.147	0.168	0.099	0.114	0.117	0.094	0.127
90%	0.213	0.204	0.200	0.155	0.169	0.178	0.183	0.150	0.141	0.129	0.146	0.195
100%	0.167	0.156	0.169	0.126	0.098	0.140	0.140	0.109	0.099	0.110	0.121	0.166

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K21
ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1) task

Clicking time interval	Cheapest common standard offer (171)	Offer rank 2 (19)	Offer rank 3 (34)	Offer rank 4 (33)	Offer rank 5 (47)	Offer rank 6 (43)	Offer rank 7 (44)	Offer rank 8 (57)	Offer rank 9 (52)	Offer rank 10 (56)	Offer rank 11 (47)	Offer rank 12 (58)
10%	0.044	0.194	0.055	0.036	0.047	0.139	0.031	0.083	0.043	0.051	0.060	0.091
20%	0.200	0.386	0.191	0.195	0.236	0.388	0.181	0.221	0.163	0.194	0.253	0.326
30%	0.426	0.502	0.169	0.195	0.357	0.455	0.324	0.235	0.322	0.268	0.325	0.356
40%	0.554	0.274	0.088	0.029	0.247	0.222	0.121	0.135	0.244	0.147	0.114	0.178
50%	0.590	0.123	0.067	0.010	0.204	0.192	0.051	0.081	0.116	0.086	0.070	0.093
60%	0.596	0.132	0.088	0.139	0.165	0.147	0.075	0.059	0.155	0.125	0.085	0.065
70%	0.569	0.148	0.069	0.133	0.135	0.114	0.068	0.048	0.113	0.095	0.052	0.039
80%	0.560	0.105	0.089	0.163	0.157	0.070	0.071	0.043	0.068	0.106	0.043	0.052
90%	0.577	0.111	0.140	0.195	0.167	0.113	0.055	0.076	0.077	0.093	0.043	0.057
100%	0.529	0.105	0.125	0.194	0.163	0.116	0.058	0.045	0.027	0.036	0.043	0.046

Clicking time interval	Offer rank 13 (70)	Offer rank 14 (56)	Offer rank 15 (58)	Offer rank 16 (66)	Offer rank 17 (68)	Offer rank 18 (65)	Offer rank 19 (51)	Offer rank 20 (54)	Offer rank 21 (59)	Offer rank 22 (48)	Offer rank 23 (57)	Offer rank 24 (55)
10%	0.099	0.099	0.090	0.062	0.062	0.098	0.050	0.095	0.044	0.073	0.064	0.039
20%	0.270	0.336	0.218	0.270	0.261	0.262	0.232	0.290	0.248	0.233	0.219	0.149
30%	0.270	0.274	0.312	0.293	0.346	0.301	0.270	0.280	0.291	0.221	0.250	0.189
40%	0.132	0.104	0.140	0.104	0.191	0.135	0.146	0.122	0.108	0.091	0.140	0.146
50%	0.080	0.080	0.100	0.055	0.123	0.108	0.080	0.040	0.092	0.041	0.087	0.140
60%	0.088	0.046	0.075	0.053	0.107	0.077	0.051	0.037	0.079	0.030	0.050	0.113
70%	0.083	0.030	0.041	0.058	0.097	0.052	0.072	0.037	0.036	0.013	0.044	0.105
80%	0.066	0.029	0.034	0.069	0.089	0.065	0.098	0.090	0.034	0.000	0.033	0.093
90%	0.076	0.052	0.025	0.078	0.064	0.091	0.082	0.081	0.034	0.016	0.062	0.107
100%	0.029	0.006	0.019	0.043	0.042	0.053	0.059	0.060	0.039	0.008	0.033	0.064

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K22

ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1*D2) task

Clicking time interval	Cheapest common standard offer (171)	Offer rank 2 (17)	Offer rank 3 (36)	Offer rank 4 (37)	Offer rank 5 (38)	Offer rank 6 (45)	Offer rank 7 (51)	Offer rank 8 (47)	Offer rank 9 (65)	Offer rank 10 (56)	Offer rank 11 (60)	Offer rank 12 (55)
10%	0.047	0.137	0.063	0.104	0.123	0.042	0.024	0.110	0.054	0.055	0.041	0.060
20%	0.216	0.317	0.319	0.303	0.323	0.196	0.161	0.259	0.235	0.211	0.143	0.222
30%	0.399	0.408	0.357	0.387	0.323	0.273	0.253	0.319	0.269	0.285	0.236	0.300
40%	0.499	0.127	0.286	0.263	0.169	0.144	0.212	0.227	0.166	0.133	0.171	0.198
50%	0.563	0.069	0.217	0.149	0.156	0.143	0.175	0.160	0.147	0.096	0.109	0.128
60%	0.596	0.059	0.149	0.125	0.100	0.135	0.113	0.116	0.140	0.062	0.114	0.092
70%	0.552	0.102	0.139	0.110	0.061	0.123	0.092	0.111	0.096	0.055	0.108	0.085
80%	0.552	0.076	0.144	0.140	0.054	0.189	0.128	0.126	0.083	0.055	0.106	0.047
90%	0.567	0.081	0.123	0.162	0.070	0.190	0.115	0.113	0.088	0.040	0.089	0.031
100%	0.523	0.118	0.090	0.133	0.076	0.171	0.081	0.106	0.053	0.050	0.075	0.025

Clicking time interval	Offer rank 13 (60)	Offer rank 14 (55)	Offer rank 15 (54)	Offer rank 16 (59)	Offer rank 17 (58)	Offer rank 18 (69)	Offer rank 19 (51)	Offer rank 20 (51)	Offer rank 21 (60)	Offer rank 22 (62)	Offer rank 23 (60)	Offer rank 24 (51)
10%	0.046	0.057	0.035	0.073	0.090	0.069	0.044	0.039	0.108	0.078	0.105	0.010
20%	0.188	0.229	0.167	0.201	0.236	0.230	0.158	0.178	0.284	0.255	0.298	0.056
30%	0.218	0.265	0.233	0.245	0.242	0.223	0.204	0.265	0.343	0.254	0.312	0.186
40%	0.092	0.166	0.082	0.198	0.170	0.130	0.125	0.171	0.195	0.206	0.190	0.189
50%	0.077	0.105	0.072	0.120	0.120	0.117	0.131	0.094	0.106	0.139	0.121	0.188
60%	0.079	0.060	0.073	0.079	0.100	0.082	0.128	0.075	0.107	0.157	0.082	0.134
70%	0.059	0.064	0.044	0.063	0.067	0.066	0.114	0.022	0.092	0.151	0.047	0.116
80%	0.115	0.073	0.034	0.078	0.068	0.082	0.111	0.021	0.085	0.105	0.034	0.108
90%	0.109	0.061	0.053	0.112	0.065	0.076	0.096	0.027	0.092	0.109	0.028	0.137
100%	0.093	0.064	0.046	0.075	0.041	0.059	0.078	0.020	0.083	0.099	0.028	0.105

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K23

ProbabilityInBasket of common standard offers with different ranks: PC (P1*D1+P2) task

Clicking time interval	Cheapest common standard offer (171)	Offer rank 2 (16)	Offer rank 3 (34)	Offer rank 4 (34)	Offer rank 5 (39)	Offer rank 6 (52)	Offer rank 7 (57)	Offer rank 8 (43)	Offer rank 9 (64)	Offer rank 10 (63)	Offer rank 11 (59)	Offer rank 12 (50)
10%	0.053	0.122	0.084	0.029	0.044	0.038	0.100	0.122	0.123	0.055	0.030	0.087
20%	0.205	0.250	0.257	0.139	0.334	0.196	0.331	0.249	0.321	0.275	0.187	0.292
30%	0.449	0.227	0.352	0.234	0.360	0.233	0.347	0.300	0.315	0.356	0.233	0.318
40%	0.571	0.088	0.153	0.104	0.231	0.092	0.131	0.157	0.106	0.168	0.086	0.161
50%	0.601	0.164	0.129	0.048	0.233	0.099	0.084	0.117	0.061	0.151	0.032	0.091
60%	0.577	0.219	0.088	0.069	0.231	0.085	0.071	0.060	0.038	0.127	0.024	0.106
70%	0.492	0.225	0.081	0.089	0.162	0.092	0.092	0.026	0.063	0.087	0.027	0.073
80%	0.471	0.125	0.059	0.061	0.167	0.096	0.086	0.023	0.058	0.081	0.036	0.087
90%	0.457	0.125	0.040	0.099	0.176	0.096	0.088	0.034	0.065	0.088	0.068	0.103
100%	0.372	0.125	0.029	0.105	0.144	0.073	0.078	0.034	0.094	0.104	0.041	0.090

Clicking time interval	Offer rank 13 (63)	Offer rank 14 (65)	Offer rank 15 (57)	Offer rank 16 (54)	Offer rank 17 (57)	Offer rank 18 (60)	Offer rank 19 (59)	Offer rank 20 (57)	Offer rank 21 (49)	Offer rank 22 (57)	Offer rank 23 (56)	Offer rank 24 (52)
10%	0.097	0.044	0.091	0.101	0.048	0.058	0.053	0.100	0.069	0.118	0.071	0.051
20%	0.270	0.204	0.262	0.276	0.165	0.210	0.234	0.232	0.253	0.276	0.215	0.168
30%	0.336	0.303	0.345	0.271	0.264	0.214	0.301	0.305	0.243	0.240	0.235	0.224
40%	0.150	0.127	0.164	0.088	0.118	0.127	0.142	0.130	0.133	0.089	0.105	0.101
50%	0.115	0.075	0.090	0.064	0.060	0.110	0.065	0.089	0.115	0.034	0.072	0.088
60%	0.078	0.042	0.055	0.074	0.035	0.101	0.121	0.071	0.143	0.036	0.090	0.077
70%	0.055	0.062	0.053	0.048	0.053	0.100	0.123	0.062	0.133	0.035	0.067	0.043
80%	0.033	0.083	0.055	0.033	0.061	0.088	0.090	0.086	0.109	0.024	0.063	0.033
90%	0.045	0.065	0.053	0.025	0.103	0.073	0.096	0.092	0.122	0.033	0.071	0.058
100%	0.036	0.048	0.039	0.046	0.103	0.083	0.093	0.070	0.122	0.018	0.058	0.058

*Numbers in the parentheses are the total numbers of optimal or suboptimal common standard offers at that rank.

Table K24

ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1) task

Clicking time interval	Cheapest individuated standard offer (171)	Offer rank 2 (73)	Offer rank 3 (93)	Offer rank 4 (117)	Offer rank 5 (112)	Offer rank 6 (123)	Offer rank 7 (121)	Offer rank 8 (113)	Offer rank 9 (117)	Offer rank 10 (115)	Offer rank 11 (124)	Offer rank 12 (112)
10%	0.021	0.017	0.000	0.026	0.026	0.014	0.001	0.011	0.007	0.008	0.016	0.018
20%	0.050	0.058	0.013	0.086	0.072	0.032	0.042	0.059	0.060	0.045	0.050	0.052
30%	0.085	0.055	0.027	0.107	0.085	0.051	0.087	0.078	0.124	0.057	0.091	0.069
40%	0.135	0.070	0.053	0.120	0.097	0.091	0.148	0.086	0.155	0.041	0.105	0.060
50%	0.198	0.134	0.149	0.221	0.161	0.211	0.242	0.161	0.258	0.123	0.167	0.126
60%	0.250	0.184	0.239	0.318	0.242	0.251	0.291	0.240	0.264	0.188	0.223	0.179
70%	0.248	0.234	0.240	0.317	0.274	0.221	0.256	0.212	0.263	0.165	0.205	0.172
80%	0.304	0.304	0.237	0.361	0.360	0.300	0.268	0.222	0.315	0.233	0.254	0.184
90%	0.432	0.407	0.353	0.436	0.420	0.350	0.314	0.330	0.336	0.292	0.237	0.220
100%	0.494	0.361	0.319	0.401	0.341	0.296	0.274	0.286	0.270	0.208	0.205	0.137

Clicking time interval	Offer rank 13 (101)	Offer rank 14 (115)	Offer rank 15 (113)	Offer rank 16 (105)	Offer rank 17 (103)	Offer rank 18 (106)	Offer rank 19 (120)	Offer rank 20 (117)	Offer rank 21 (112)	Offer rank 22 (123)	Offer rank 23 (114)	Offer rank 24 (116)
10%	0.010	0.016	0.018	0.005	0.023	0.009	0.014	0.033	0.009	0.032	0.024	0.012
20%	0.038	0.055	0.065	0.016	0.055	0.039	0.051	0.066	0.045	0.061	0.069	0.026
30%	0.036	0.077	0.087	0.057	0.052	0.057	0.056	0.073	0.057	0.068	0.075	0.024
40%	0.073	0.089	0.104	0.081	0.053	0.075	0.059	0.071	0.061	0.071	0.086	0.043
50%	0.179	0.153	0.161	0.139	0.156	0.131	0.171	0.130	0.132	0.139	0.157	0.073
60%	0.249	0.204	0.205	0.189	0.202	0.165	0.212	0.182	0.134	0.173	0.202	0.107
70%	0.231	0.146	0.148	0.128	0.155	0.156	0.149	0.148	0.121	0.134	0.161	0.088
80%	0.209	0.277	0.169	0.171	0.200	0.186	0.184	0.193	0.176	0.164	0.138	0.128
90%	0.256	0.344	0.222	0.229	0.222	0.201	0.189	0.219	0.273	0.165	0.183	0.239
100%	0.203	0.224	0.208	0.184	0.127	0.143	0.151	0.148	0.187	0.117	0.153	0.176

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K25
ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1*D2) task

Clicking time interval	Cheapest individuated standard offer (171)	Offer rank 2 (74)	Offer rank 3 (99)	Offer rank 4 (113)	Offer rank 5 (121)	Offer rank 6 (113)	Offer rank 7 (116)	Offer rank 8 (121)	Offer rank 9 (106)	Offer rank 10 (115)	Offer rank 11 (111)	Offer rank 12 (114)
10%	0.008	0.013	0.027	0.010	0.026	0.020	0.015	0.005	0.016	0.015	0.001	0.009
20%	0.034	0.042	0.059	0.038	0.078	0.052	0.045	0.018	0.052	0.053	0.017	0.052
30%	0.043	0.049	0.067	0.087	0.079	0.075	0.067	0.048	0.075	0.055	0.032	0.071
40%	0.062	0.054	0.083	0.099	0.079	0.129	0.100	0.089	0.096	0.092	0.051	0.093
50%	0.131	0.107	0.153	0.159	0.128	0.237	0.178	0.117	0.149	0.123	0.099	0.144
60%	0.159	0.210	0.245	0.245	0.155	0.311	0.228	0.154	0.203	0.199	0.130	0.165
70%	0.139	0.268	0.244	0.272	0.179	0.333	0.214	0.191	0.176	0.215	0.126	0.155
80%	0.173	0.352	0.329	0.334	0.215	0.367	0.303	0.206	0.243	0.260	0.149	0.222
90	0.257	0.423	0.406	0.402	0.294	0.418	0.347	0.264	0.304	0.311	0.215	0.286
100%	0.304	0.400	0.349	0.372	0.290	0.392	0.325	0.250	0.224	0.308	0.195	0.262

Clicking time interval	Offer rank 13 (111)	Offer rank 14 (116)	Offer rank 15 (117)	Offer rank 16 (112)	Offer rank 17 (113)	Offer rank 18 (102)	Offer rank 19 (120)	Offer rank 20 (120)	Offer rank 21 (111)	Offer rank 22 (109)	Offer rank 23 (111)	Offer rank 24 (120)
10%	0.012	0.001	0.011	0.026	0.009	0.014	0.008	0.014	0.031	0.024	0.013	0.018
20%	0.042	0.037	0.038	0.077	0.044	0.039	0.049	0.056	0.048	0.045	0.040	0.043
30%	0.062	0.073	0.042	0.059	0.075	0.048	0.072	0.089	0.049	0.039	0.066	0.041
40%	0.127	0.072	0.077	0.079	0.120	0.040	0.071	0.111	0.064	0.081	0.079	0.020
50%	0.180	0.155	0.156	0.159	0.182	0.085	0.136	0.168	0.106	0.134	0.127	0.064
60%	0.226	0.197	0.175	0.198	0.248	0.184	0.208	0.196	0.194	0.150	0.203	0.141
70%	0.212	0.146	0.142	0.181	0.211	0.180	0.179	0.172	0.165	0.148	0.159	0.172
80%	0.220	0.154	0.175	0.240	0.261	0.145	0.223	0.180	0.124	0.179	0.152	0.206
90%	0.238	0.196	0.261	0.280	0.335	0.220	0.268	0.221	0.152	0.221	0.210	0.222
100%	0.222	0.175	0.246	0.223	0.319	0.197	0.239	0.196	0.194	0.184	0.224	0.210

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K26

ProbabilityInBasket of individuated standard offers with different ranks: PC (P1*D1+P2) task

Clicking time interval	Cheapest individuated standard offer (171)	Offer rank 2 (72)	Offer rank 3 (100)	Offer rank 4 (113)	Offer rank 5 (117)	Offer rank 6 (113)	Offer rank 7 (111)	Offer rank 8 (127)	Offer rank 9 (105)	Offer rank 10 (108)	Offer rank 11 (112)	Offer rank 12 (121)
10%	0.007	0.007	0.015	0.018	0.006	0.021	0.006	0.008	0.019	0.006	0.006	0.010
20%	0.033	0.052	0.038	0.052	0.036	0.038	0.034	0.034	0.048	0.079	0.047	0.024
30%	0.065	0.104	0.056	0.071	0.064	0.030	0.055	0.046	0.060	0.092	0.075	0.037
40%	0.090	0.161	0.090	0.083	0.070	0.060	0.103	0.062	0.091	0.097	0.099	0.040
50%	0.150	0.220	0.178	0.168	0.145	0.156	0.166	0.145	0.161	0.138	0.173	0.151
60%	0.216	0.305	0.269	0.233	0.231	0.247	0.237	0.212	0.218	0.200	0.239	0.220
70%	0.264	0.331	0.271	0.236	0.261	0.242	0.247	0.218	0.214	0.211	0.217	0.159
80%	0.361	0.368	0.345	0.289	0.284	0.265	0.290	0.287	0.276	0.259	0.275	0.185
90%	0.434	0.443	0.451	0.331	0.351	0.344	0.306	0.342	0.306	0.330	0.288	0.230
100%	0.420	0.457	0.448	0.238	0.284	0.291	0.211	0.263	0.239	0.270	0.177	0.183

Clicking time interval	Offer rank 13 (108)	Offer rank 14 (106)	Offer rank 15 (114)	Offer rank 16 (117)	Offer rank 17 (114)	Offer rank 18 (111)	Offer rank 19 (112)	Offer rank 20 (114)	Offer rank 21 (122)	Offer rank 22 (114)	Offer rank 23 (115)	Offer rank 24 (119)
10%	0.020	0.005	0.001	0.010	0.008	0.014	0.008	0.015	0.016	0.004	0.010	0.007
20%	0.037	0.017	0.016	0.022	0.018	0.038	0.021	0.043	0.052	0.010	0.015	0.015
30%	0.047	0.051	0.039	0.034	0.055	0.078	0.033	0.045	0.063	0.033	0.036	0.018
40%	0.064	0.060	0.069	0.048	0.070	0.102	0.042	0.088	0.057	0.062	0.053	0.041
50%	0.176	0.145	0.138	0.096	0.110	0.169	0.128	0.178	0.100	0.169	0.134	0.088
60%	0.224	0.212	0.190	0.183	0.190	0.195	0.208	0.208	0.150	0.232	0.206	0.089
70%	0.172	0.136	0.202	0.161	0.125	0.162	0.157	0.104	0.119	0.166	0.157	0.055
80%	0.150	0.218	0.213	0.205	0.162	0.204	0.197	0.116	0.161	0.153	0.156	0.083
90%	0.179	0.264	0.264	0.246	0.211	0.297	0.230	0.160	0.229	0.214	0.203	0.196
100%	0.148	0.187	0.181	0.182	0.151	0.203	0.162	0.097	0.147	0.134	0.099	0.160

*Numbers in the parentheses are the total numbers of optimal or suboptimal individuated standard offers at that rank.

Table K27
ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1)

Clicking time interval	The optimal offer is a common standard offer (55)	The optimal offer is a individuated standard offer (116)
10%	0.029	0.024
20%	0.174	0.066
30%	0.455	0.091
40%	0.582	0.135
50%	0.645	0.227
60%	0.650	0.305
70%	0.627	0.246
80%	0.629	0.258
90%	0.663	0.368
100%	0.631	0.384

*Numbers in the parentheses are the number of observations

Table K28

ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1*D2)

Clicking time interval	The optimal offer is a common standard offer (56)	The optimal offer is a individuated standard offer (115)
10%	0.063	0.000
20%	0.237	0.000
30%	0.435	0.015
40%	0.553	0.029
50%	0.612	0.140
60%	0.614	0.154
70%	0.606	0.157
80%	0.612	0.218
90%	0.631	0.335
100%	0.637	0.342

*Numbers in the parentheses are the number of observations

Table K29

ProbabilityInBasket for the optimal offer, conditional on its standard: PC (P1*D1+P2)

Clicking time interval	The optimal offer is a common standard offer (60)	The optimal offer is a individuated standard offer (111)
10%	0.036	0.008
20%	0.200	0.030
30%	0.579	0.046
40%	0.687	0.059
50%	0.678	0.128
60%	0.596	0.199
70%	0.492	0.224
80%	0.442	0.326
90%	0.400	0.435
100%	0.350	0.432

*Numbers in the parentheses are the number of observations

Table K30

The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: AC (P1) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	9	9	0	100%
30%	24	24	0	100%
40%	28	28	0	100%
50%	37	37	0	100%
60%	51	51	0	100%
70%	65	65	0	100%
80%	71	70	1	98.6%
90%	90	90	0	100%
100%	124	124	0	100%
Last click[89]	157	N/A	N/A	N/A

*171 observations

*153 (89.47%) participants ended up choosing the optimal offer

[89] In the present table as well as the following 12 tables, the row "Last click" shows the number of participants who had the optimal offer in the shopping basket while clicking the "Buy offer xx" button (xx can be a letter from A to X, depending which offer a participant finally decided to buy). This number usually is higher than the number of participants who had the optimal offer in the shopping basket at the beginning of 90%-100% time interval, because some participants may put the optimal offer in the shopping basket "within", but not "at the beginning of", the last time interval.

Table K31

The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: AC (P1*D1) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	10	8	2	80%
30%	21	21	0	100%
40%	27	27	0	100%
50%	33	33	0	100%
60%	49	48	1	98.00%
70%	70	70	0	100%
80%	87	87	0	100%
90%	102	102	0	100%
100%	128	127	1	99.22%
Last click	154	N/A	N/A	N/A

*171 observations

*147 (85.96%) participants ended up choosing the optimal offer

Table K32
The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: AC (P1*D1*D2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	3	3	0	100%
30%	16	16	0	100%
40%	24	24	0	100%
50%	30	30	0	100%
60%	39	39	0	100%
70%	55	55	0	100%
80%	68	68	0	100%
90%	86	85	1	98.84%
100%	120	119	1	99.16%
Last click	148	N/A	N/A	N/A

*171 observations
*140 (81.87%) participants ended up choosing the optimal offer

Table K33

The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: AC (P1*D1+P2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	5	5	0	100.00%
30%	15	15	0	100.00%
40%	25	25	0	100.00%
50%	31	31	0	100.00%
60%	51	50	1	98.04%
70%	57	56	1	98.25%
80%	68	67	1	98.53%
90%	93	92	1	98.92%
100%	128	126	2	98.44%
Last click	149	N/A	N/A	N/A

*171 observations

*145 (84.80%) participants ended up choosing the optimal offer

Table K34
The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: NC (P1*D1) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	12	12	0	100.00%
30%	18	14	4	77.78%
40%	21	17	4	80.95%
50%	23	22	1	95.65%
60%	36	34	2	94.44%
70%	50	42	8	84.00%
80%	53	48	5	90.57%
90%	65	65	0	100.00%
100%	82	73	9	89.02%
Last click	79	N/A	N/A	N/A

*171 observations
*45 (26.32%) participants ended up choosing the optimal offer

Table K35
The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: NC (P1*D1*D2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	8	8	0	100.00%
30%	17	16	1	94.12%
40%	22	18	4	81.82%
50%	25	23	2	92.00%
60%	35	34	1	97.14%
70%	45	41	4	91.11%
80%	49	47	2	95.92%
90%	57	54	3	94.74%
100%	71	60	11	84.51%
Last click	64	N/A	N/A	N/A

*171 observations
*35 (20.47%) participants ended up choosing the optimal offer

Table K36

The number of participants, who had the optimal offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: NC (P1*D1+P2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	10	10	0	100.00%
30%	25	23	2	92.00%
40%	32	26	6	81.25%
50%	41	38	3	92.68%
60%	50	49	1	98.00%
70%	56	52	4	92.86%
80%	60	55	5	91.67%
90%	67	65	2	97.01%
100%	85	68	17	80.00%
Last click	73	N/A	N/A	N/A

*171 observations

*47 (27.49%) participants ended up choosing the optimal offer

Table K37

The number of participants, who had the optimal offer which is a common standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: PC (P1*D1) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	5	5	0	100.00%
30%	20	19	1	95.00%
40%	29	28	1	96.55%
50%	33	33	0	100.00%
60%	37	35	2	94.59%
70%	34	32	2	94.12%
80%	35	33	2	94.29%
90%	36	35	1	97.22%
100%	37	32	5	86.49%
Last click	33	N/A	N/A	N/A

*55 observations

*23 (41.82%) participants ended up choosing the optimal offer when it was a common standard offer

Table K38
The number of participants, who had the optimal offer which is an individuated standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: PC (P1*D1) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	3	3	0	100.00%
30%	8	7	1	87.50%
40%	12	11	1	91.67%
50%	19	18	1	94.74%
60%	23	21	2	91.30%
70%	26	25	1	96.15%
80%	32	30	2	93.75%
90%	44	44	0	100.00%
100%	61	55	6	90.16%
Last click	64	N/A	N/A	N/A

*116 observations
*40 (34.48%) participants ended up choosing the optimal offer when it was an individuated standard offer

Table K39
The number of participants, who had the optimal offer which is a common standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: PC (P1*D1*D2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	8	8	0	100.00%
30%	20	19	1	95.00%
40%	27	25	2	92.59%
50%	33	32	1	96.97%
60%	34	33	1	97.06%
70%	35	33	2	94.29%
80%	35	34	1	97.14%
90%	34	33	1	97.06%
100%	36	35	1	97.22%
Last click	35	N/A	N/A	N/A

*56 observations
*20 (35.71%) participants ended up choosing the optimal offer when it was a common standard offer

Table K40
The number of participants, who had the optimal offer which is an individuated standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: PC (P1*D1*D2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	3	3	0	100.00%
30%	6	5	1	83.33%
40%	7	6	1	85.71%
50%	11	10	1	90.91%
60%	18	13	5	72.22%
70%	16	11	5	68.75%
80%	14	12	2	85.71%
90%	19	18	1	94.74%
100%	33	25	8	75.76%
Last click	33	N/A	N/A	N/A

*115 observations
*13 (11.30%) participants ended up choosing the optimal offer when it was an individuated standard offer

Table K41

The number of participants, who had the optimal offer which is a common standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during clicking time interval: PC (P1*D1+P2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	4	4	0	100.00%
30%	27	27	0	100.00%
40%	40	38	2	95.00%
50%	42	38	4	90.48%
60%	39	32	7	82.05%
70%	32	25	7	78.13%
80%	26	25	1	96.15%
90%	26	22	4	84.62%
100%	22	20	2	90.91%
Last click	20	N/A	N/A	N/A

*60 observations

*10 (16.67%) participants ended up choosing the optimal offer when it was a common standard offer

Table K42
The number of participants, who had the optimal offer which is an individuated standard offer in the shopping basket at the beginning of a time interval, kept the optimal offer in the shopping basket, and moved the optimal offer out of the shopping basket during a clicking time interval: PC (P1*D1+P2) task

Clicking time interval	Number of participants who had the optimal offer in the shopping basket at the beginning of the time interval	Number of participants who kept the optimal offer in the shopping basket during the time interval	Number of participants who moved the optimal offer out of the shopping basket during the time interval	Percentage of participants who did not move the optimal offer out of the shopping basket
20%	2	2	0	100.00%
30%	7	7	0	100.00%
40%	10	9	1	90.00%
50%	13	13	0	100.00%
60%	22	20	2	90.91%
70%	26	24	2	92.31%
80%	36	35	1	97.22%
90%	45	40	5	88.89%
100%	48	38	10	79.17%
Last click	43	N/A	N/A	N/A

*111 observations
*27 (24.32%) participants ended up choosing the optimal offer when it was an individuated standard offer