On the Importance of Context in Sequential Decision-Making

Hsiao, Yu Chin and Kemp, Simon and Servátka, Maroš

University of Canterbury MGSM Experimental Economics Laboratory, Macquarie Graduate School of Management, University of Canterbury, MGSM Experimental Economics Laboratory, Macquarie Graduate School of Management University of Economics in Bratislava

3 May 2019

On the Importance of Context in Sequential Decision-Making

Yu Chin Hsiao, University of Canterbury & MGSM Experimental Economics Laboratory, Macquarie Graduate School of Management (hsiao.annie@gmail.com)

Simon Kemp, University of Canterbury (simon.kemp@canterbury.ac.nz)

Maroš Servátka, MGSM Experimental Economics Laboratory, Macquarie Graduate School of Management & University of Economics in Bratislava (maros.servatka@mgsm.edu.au)

May 3, 2019

Abstract: We experimentally investigate whether framing an individual-choice decision in a market setting results in a different outcome than when the decision is described in a context-free frame. We further explore whether the context effect is triggered by the frame itself or whether a richer descriptive content is required to establish familiarity with the decision-making environment. Understanding what constitutes context is central to formulating practical recommendations aiming to improve the quality of individual decisions. Our results show that framing a sequential search problem as selling houses leads to better decisions than a context-free frame. Manipulating whether or not the framed decision-making scenario includes a description of the house, which would be naturally available in a real estate market, does not impact the length of search or the total earnings.

Keywords: Schema Activation, Secretary Problem, Sequential Search, Context, Framing, Decision-Making

JEL Classification: C91, D83

Acknowledgements: This paper is based on Yu Chin Hsiao's dissertation chapter written jointly at the University of Canterbury and Macquarie Graduate School of Management (Hsiao, 2018). Financial support was provided by the University of Canterbury, College of Business and Economics and Macquarie Graduate School of Management. We thank Cary Deck, Matt Ward, Lyla Zhang, Peter Earl, Douglas Elliffe, and colleagues who provided insight and expertise that greatly assisted the research. Maroš Servátka thanks University of Alaska – Anchorage for their kind hospitality while working on this paper.

1 INTRODUCTION

Economic rationality can often be observed in social and economic institutions that provide interactive experience within a particular context in which the decisions are made (see Plott 1987; Smith, 1962 and 1991). However, a large fraction of individual decision-making experiments testing for rationality is context free and employs neutral framing. At the same time, empirical evidence points out that decisions, whether in an individual or strategic setting, are sensitive to framing (e.g., Tversky & Kahneman, 1981; McNeil, Pauker, Sox & Tversky, 1982; Meyerowitz & Chaiken, 1987) and that embedding a decision-making problem in a context can improve the quality of decisions (e.g., Eger & Dickhaut, 1982; Griggs & Cox, 1982).[1] The ability to make better decisions in context can be explained by the dual-processing theory. Dual-processing theory proposes that most daily decisions are made by associating a new situation with existing knowledge in similar experiences, rather than forming new knowledge and information for each new experience (Kahneman, 2003). People use existing schemas that contain effective strategies constructed from previous experiences to make decisions. A schema is a system of organizing and perceiving new information, which is then encoded as default assumptions about the world. Schemas form mental structures that describe how the world works, and how we interact with the world (see Bower & Cirilo, 1985; Dimaggio, 1997; Narvaez & Bock, 2002, for more details).[2] For instance, when someone holds a schema that maximizing profit is the best approach to make decisions, she will consistently re-apply this schema in various economic situations. Gilboa and Schmeidler (1995) and Jehiel (2005) propose arguments related to schema activation that effective contexts work through memory cues from past experience. People with existing experience in the presented context can evoke this past experience to guide their behavior in the current task.

Since in everyday life virtually all decisions are made within a context, it is crucial to understand the link between the amount of information needed in a context in order to activate a schema and change the behavior. We study whether framing an individual-choice decision in a market setting results in a different outcome than when the decision is described in a neutral (context-free) frame. We further explore whether schema activation is triggered by the frame itself or whether a richer descriptive content is required to establish familiarity with the decision-making environment in order to activate the schema. Understanding what constitutes context is central to formulating practical recommendations aiming to improve the quality of individual decisions. It is important to note that certain contexts cannot be created or easily replicated in the lab, however one can frame (label) the decision and

---

[1] See also Kay & Ross (2003), Rege & Telle (2004), Liberman, Samuels, & Ross (2004), Hennig-Schmidt, Sadrieh, & Rockenbach (2010), Dufwenberg, Gächter, & Hennig-Schmidt (2011), Ellingsen, Johannesson, Mollerstrom, & Munkhammar (2012) for more examples of decisions being sensitive to framing.

[2] Note that the literature implicitly assumes that the "correct" schema is activated, which then in turns improves the quality of decisions. In our study we will be able to verify this assumption by observing and evaluating the quality of decisions through the lens of a particular theory.

provide additional information about the environment to invoke a particular context and enhance the link between the laboratory and every day life decision-making.[3]

We explore our questions in a sequential search task, known as the secretary problem (Gardner, 1960), in which individuals decide whether to accept the presented offer or whether to keep searching for a better one. Within this setting we frame the decisions as selling houses and manipulate whether or not the decision-making scenario includes a description of the house, which would be naturally available in a real estate market. Note that, in reality, buying or selling a house do often follow the processes of secretary problem. The experimental design ensures that at no stage our participants receive information regarding the distribution of offers or the optimal (= highest) offer, which is crucial for a clean identification of the source of decision variation. Our findings show that decisions framed as selling houses, irrespectively of whether house descriptions are available or not, result in higher earnings and are closer to the optimal amount of search (approximated by numerical methods) than neutrally-framed (and thus context-free) decisions. Our experiment thus provides evidence that schema can be activated solely with framing and that no additional descriptive information is necessary.

The contribution of our study also has a methodological aspect. For certain research questions in social sciences and economics in particular, the lack of context in subject instructions is desirable as the sole focus on induced values leads to more control over the data generating process than simulating alleged circumstances would (Smith, 1976). Using loaded language and engaging participants in "roleplay," runs the risk that home-grown values and preconceived notions of how one "should" behave in a given emotionally-charged scenario will dominate the pecuniary incentives (see Cox & Oaxaca, 1989 and Friedman & Sunder, 1994 for a discussion; Hoffman, McCabe, Shachat & Smith, 1994 and Hoffman, McCabe & Smith, 1996 for early experimental studies framing ultimatum game and dictator game decisions as market interactions; and Alekseev, Charness & Gneezy, 2017 for a survey of using contextual instructions in economics experiments). At the same time, it is crucial to recognize that the lack of context itself might result in loss of control, for example when comparing the behavior of participants from different populations and attributing the observed difference to "culture" or "group preferences," without properly understanding the context that the subjects might self-impose to help them interpret the experimental scenario and incentives. Ultimately, whether context enhances or diminishes control depends on the research question and deciding whether or not to implement it or not is an important design issue. Our main methodological contribution to this debate stems from our observation that framing itself is sufficient to generate context in an individual

[3] There appears to be a lack of consensus in the literature regarding the difference between the context and framing effect, with some authors using the two terms interchangeably. In the current paper, we refer to decisions being made in a particular context and this context could be experienced in a natural setting or introduced by framing. We use the term "context effect" when behavior changes due to a change in context in which the decision is made. In our experiment, such a change is caused by framing.

decision-making environment and providing evidence that a house-selling frame results in longer search and higher payoffs. We investigate this hypothesis by employing a context that few of our participants had past experience with: selling houses. We argue that a context that has (likely) never been experienced can still improve decisions and facilitate learning by activating schema created from "similar" contexts experienced in our daily lives.

2 Relationship to the Literature

Many decision-making situations are sequential in life; such decisions often need to be made immediately and in certain instances cannot be revisited. This type of sequential decision-making situation displays the features of the secretary problem.[4] The classical secretary problem has been specified in the following way (Gardner & Mathematical Association of America, 2009). A known number of n candidates is presented randomly in a sequence. The decision-maker must either accept or reject the presented candidate immediately and the decision cannot be recalled.[5] A positive payoff is earned only if she chooses the best overall candidate. The optimal decision rule of the classical version of the secretary problem allows the decision-maker to maximize the probability of finding the best candidate. The decision rule states that the decision-maker should reject the first n/e (≈0.37 as n approaches infinity) of the candidates and then accept a candidate who is better than any of the previously rejected candidates (see Lindley, 1961; Gilbert & Mosteller, 1966 for a detailed proof). The chance of finding the best candidate increases to approximately 58% as n approaches infinity (Gilbert & Mosteller, 1966) when the distribution of the quality of candidates is available and known. However, often it is not and must be inferred during the process itself.

In Gilbert and Mosteller (1966), the decision rule is based on assuming that only the relative (ordinal) rank of each candidate is known, rather the objective (cardinal) value of each candidate. There exist also other variations of how payoffs in the secretary problem are calculated; e.g. in Seale & Rapoport (1997) only the best choice results in a positive payoff whereas in Bearden et al. (2006) payoffs are dependant on rank. Teodorescu, Sang, and Todd (2018) implement objective values in an experiment and point out their several advantages, namely the ecological validity and the ability of researchers to observe the learning effect as well as behavioral strategies for different values associated with candidates. Angelovski and Güth (2019) use dynamic modelling to calculate the optimal decision rule for objective values when the distribution is known. In a subsequent experiment they then find that people stop searching too early. Early stopping behavior is actually a frequent finding in experiments on the variation of the secretary problem (e.g. Zwick, Rapoport, Lo &

[4] The secretary problem was first published in February 1960 Scientific American of Martin Gardner column of mathematical games. According to Gardner, it was originally devised in 1958 by John Fox of the Minneapolis-Honeywell Regulator Company and Gerald Marnie of the Massachusetts Institute of Technology and called the game of googol. See Ferguson (1989) and Freeman (1983) for historical reviews.
[5] Recall in the current paper refers to the ability to withdraw any previously made decision.

Muthukrishnan, 2003). While many of these experiments frame the decisions in a particular context (e.g., interviewing candidates for a position or search for new apartments) and present a specific content (e.g., relative rank of the current candidate with respect to the already interviewed candidates or the relative rank of the current apartment and the probability of successfully recalling a previously rejected apartment), we are unaware of any studies explicitly exploring whether framing exacerbates or alleviates early stopping behavior.

Cox and Oaxaca (1989) investigate a context-free job search problem that could be considered a variant of the secretary problem with multiple relaxed assumptions. In particular, in their experiment the participants are informed about the distribution of offers prior to making decisions, and use search with replacements, meaning that the same draw can re-appear in later search. While this reduces the complexity of the search task, Cox and Oaxaca still report early stopping behavior in their experiment. Although their paper discusses the importance of avoiding emotive terms in a sequential job search task (for their research question), the study does not include a context treatment that would permit a conclusion as to whether sequential search is influenced by context.

A type of context effect – the order in which the offers are presented – is experimentally investigated by Corbin, Olson and Abbondanza (1975) in the classical secretary problem. Corbin et al. (1975) show that the probability of finding the highest offer is influenced by how the offers are presented and also by the size of the offers experienced in the sequence. People are more likely to find the highest offer in an initial decrease followed by an increase pattern than in a monotonically increasing pattern. People are also more likely to find the highest offer when experiencing a large range of offers than small or medium range. This earlier research therefore suggests that the quality of sequential search decisions might be susceptible to how the offers are presented.

Finally, in a study related to ours, Palley and Kremer (2014) investigate the effect of rank feedback when the secretary problem is framed as real property agents searching for an apartment. Palley and Kremer find that when the actual value of each offer and the distribution of offers are presented, people search more than when only the relative rank is available. Therefore, the amount of information available may potentially influence the length of search.

DIFFERENT DECISIONS UNDER DIFFERENT CONTEXT FRAMES

The human brain is often thought to be the result of an evolutionary process to resolve problems and enhance survival (Cosmides & Tooby, 1992). The brain enables a collection of cognitive mechanisms that guide our behavior and decision-making. As suggested by schema activation, invoking a particular context while making a decision is possibly one of these mechanisms. In fact, in a recent study Thunström, Cherry, McEvoy and Shogren (2016) find that people actively seek out richer context to facilitate their decisions in the dictator game. Ample empirical evidence also shows that framing a decision-making problem in a particular context might result in different choices from a context-free (neutral) frame.

A long line of research has found that decisions change under different frames (e.g., Levin & Gaeth, 1988; Duchon, Dunegan & Barton, 1989; Gamliel & Peer, 2010). An early study of the issue is that of Kahneman and Tversky (1984), who explore how different phrasing with the same outcome affected people's preference in hypothetical life-and-death decision-making scenarios. The decision is presented to participants either with positive framing, for example, 2 of 3 people would die or with negative framing, 1 of 3 people would live. They find that although the outcome is the same in both scenarios, a decision made with the positively framed scenario often differed from that made with the negatively framed one.

Dual-processing theory has been proposed to explain why different decisions result from how the problem is framed (e.g., Kahneman, 2003; Evans, 2008), emphasizing that the decision-making process relies on both intuitive/heuristic and analytic/executive processes. System one involves implicit (subconscious) processing that uses intuitive and heuristic forms of reasoning that operate in most of our everyday reasoning and decision-making. It is a domain-specific and contextualised, fast, and automatic responding requires very little effort. System two involves explicit (conscious) processing; these analytic/executive operations tend to be slow, controlled, serial, and effortful (De Neys, 2006). The context effect can be caused by different decision-making schemas belonging to system one.

Some schemas are activated chronically due to the regular contact with environmental context (Freeman, 2007). For example, when an individual learns from repeatedly looking for a car-parking space that is closest to the destination, she learns that a certain way facilitates finding the best parking space, and other ways do not. These schemas are activated involuntarily. They are formed from previous experiences and are then used to organize or integrate new information (see Bower & Cirilo, 1985; Dimaggio, 1997; Narvaez & Bock, 2002, for a more detailed discussion on schemas). Once schemas are formed, they operate constantly in the brain and are activated by stimuli that resemble the stimuli that were present when the schema was first created (Higgins & Chaires, 1980; also see Narvaez, & Bock, 2002). To return to the previous search example if the experiences of finding car-parking spaces couple with experiences in searching for the best car to buy within a given price range, the brain may form a fuller mental model of how to make sequential search decisions generally. This may then be activated when a similar situation, for example, finding an apartment, arises. Heider and Simmel (1944) find evidence to support the hypothesis of re-applying existing schema to explain a new experience. In their experiment, the participants watch a short animated film. The film has a motionless large square with a door that opens and closes in one side. First, a big triangle appeared inside the square, then a small triangle and circle appeared. As the door flapped open and shut, the geometric figures slid around the screen. After ninety seconds or so, the small triangle and the small circle disappeared, and the big triangle breaks down the big square.[6]

After the participants watch the animated film, they are asked to describe what they saw. Only 3 of 114 participants report seeing geometric shapes moving around a screen. The majority report a narrative and attribute agency and intent to the shapes: for example, a romance story between the small triangle and the small circle, the big triangle is the angry parent who wants to separate them, and so on. This experiment demonstrates that people can explain a new situation using a similar, existing mental structure or schema. Although people may feel they are experiencing novelty every day, the novelty is perceived and interpreted by existing schemas without consciously being processed by the brain (Wegner & Wheathey, 1999).

Evidence of making better decisions with context

Just as a schema can be activated through the same or a similar stimulus encountered in previous experiences, making decisions in a context allows us to effectively resolve problems and make decisions without starting from scratch every time. For example, experimentation on the Wason selection task, testing deductive reasoning. demonstrates how context enhances people's ability to solve problems. In the Wason selection a set of four cards is placed on a table, where each card has a number on one side and a letter on the other side. The visible faces of the cards show A, D, 4 and 7. Which card(s) must be turned over in order to test whether the proposition that if a card has a vowel on one side, then it has an even number on the other side, is true?

Only 5 % of the participants are able to solve the context-free problem correctly (Johnson-Laird & Wason, 1970). A drastic increase in correct answers is reported in versions involving a social exchange to detect cheaters (e.g., Cosmides, 1989; Cosmides et al., 2010) and cross-cultural experiments with the SchiWiar of Ecuadorian Amazonia (Sugiyama et al., 2002). Griggs and Cox (1982) report that when people are asked to solve the Wason problem as a drinking-age problem, 73% of them are able to solve it correctly, suggesting that in some instances context aids understanding of the task and reduces confusion among partipants. In this form of the problem, the task framed the problem as a police officer who is ensuring drinking-age regulations are being followed in a bar. The participants are presented with 4 cards, each with information about one person in a bar. One side of a card tells a person's age and on the other side is what the person is drinking. The task is to identify the card(s) that violate this rule: If a person is drinking beer, then the person must be over 19 years of age.

[6] The complete film can be watched on YouTube https://www.youtube.com/watch?v=VTNmLt7QX8E .

Pollard and Evans (1987) argue that two different aspects that change between the context-free problem and the context-framed problem can potentially contribute to better performance. For example, the drinking-age problem provides content (drinks and age), as well as the context or scenario (policeman checking patrons' age in a bar), within which this content is relevant. Pollard and Evans demonstrate that both the relevant content and context are necessary to facilitate an increase in accurate responses. However, they suggest that context may be a stronger contributor to people's performance than content, a claim our experiment is able to verify by observing the marginal effect caused by providing additional informational content on the top of framing. There are two other major differences between our experiment and that of Pollard and Evans. First, Pollard and Evans employ a reasoning task while our experiment employs a sequential search task. Second, in Pollard and Evans the participants are not incentivized for their performance, whereas the decisions in our experiment have monetary consequences.[7] Economic experiments also demonstrate that context affects incentivized behavior. Alekseev et al. (2017) survey the literature and come to the conclusion that context often but not invariably improves performance with the improvement being more likely if the task requires sophisticated reasoning. (While the survey did not contain any secretary problem studies, the task seems to qualify as requiring sophisticated reasoning.)

Our contribution to this line of research stems from varying the amount of context provided which is critical to our understanding of schema activation. The aforementioned study by Thunström et al. (2016) suggests that people endogenously seek information which then in turn alters their behavior. A richer descriptive information may potentially be more effective in activating the appropriate schema and facilitating better quality decisions. Thus, apart from extending the analysis of context effects to the area of sequential search, our study addresses a previously unexplored link between the amount of information necessary to generate a context effect. In what follows, we experimentally identify whether framing itself is capable of improving search decisions or whether a richer description of the environment aids people to extend their search closer to the optimal level.[8]

3. Experimental Design and Testable Hypotheses

We design an experiment to analyze the effect of context on sequential search activity in the secretary problem in which the participants earn payoffs based on the offers

---

[7] It is important to note that not all contexts have a positive effect on performance and learning. For example, embedding the Wason selection task in the contexts of city transportation, and stamps on letters did not improve accuracy (Griggs & Cox, 1982). Cooper, Kagel, Lo and Gu (1999) suggest that for the context to facilitate understanding and learning, the context needs to be relevant to the task and familiar to the participants (e.g., college students may be more familiar with the drink and age context than the letter and stamp context).

[8] Naturally, we also verify whether the change in behavior, if any, is an improvement or deterioration of the quality of decisions.

they accept.[9] In particular, we identify whether framing of sequential search as selling houses results in different behavior than when the search is described in a context-free manner and whether a richer descriptive content is required to generate the context effect. An important feature of our experiment is employing a context that likely only few of our participants (if any, due to their age) had past experience with. The experiment therefore presents a conservative test of our hypotheses. If we observe that a context that has never been experienced can still improve decisions by activating schema created from similar market contexts of being a seller, employing a context that one has direct experience with is likely to yield even a stronger effect.

The experiment consists of three treatments implemented in an across-subject design: *No Frame, House Frame,* and *House Frame with Info*. There is no monetary search cost. The offers are presented in experimental currency units with the exchange rate of 1000 ECU = 1 NZD, announced at the beginning of the experiment. The experiment, programmed and conducted with zTree software (Fischbacher, 2007), consists of 2 practice rounds and 10 cumulatively paid rounds. In each round, there are 20 available offers, distribution of which is unknown to participants. The offers are identical across the three treatments (see Table 1 for details). Each sequence of offers, including those in the practice rounds, was generated in MS Excel by randomly sampling from an interval of the average house price (in thousands of NZD) in a different Christchurch suburb plus/minus the standard deviation for that suburb.[10] The house transactions took place in October 2014. The transaction information was obtained from the Quotable Value Ltd. database (qv.co.nz).

[9] In contrast to the classical secretary problem (see Ferguson, 1989 for a discussion), which assumes people derive utility only from the optimal choice (i.e., the highest offer), our experiment allows the participants to earn money also from sub-optimal choices (see Bearden, Rapoport, & Murphy, 2006 for more details). That is, participants earn money in the experiment based on the actual value of the offer they accept, instead of zero payoffs when anything other than the highest offer is selected.

[10] Randbetween (lowerlimit, upperlimit).

Table 1: Price offer sequences implemented in the experiment.

Variable / Round	Sequence optimal		Predicted optimal		Min.	Average offer	Std. D.
	Position	Offer	Position	Offer			
1	8	848	8	848	276	509.6	165.4
2	10	875	8	818	2	469.15	284.4
3	10	708	10	708	207	437.6	147.2
4	20	733	20	733	267	518.5	145.5
5	13	578	10	484	186	331.15	114.4
6	10	1574	9	1400	89	714.25	447.4
7	19	581	19	581	197	369.2	128.1
8	3	966	20	541	250	636.4	234.4
9	14	1740	12	1264	105	756.4	396.2
10	4	625	20	553	250	440.4	101.3
Average	11.1	922.8	13.6	793.0	183	518.3	216.4

Notes: Sequence optimal position = the position with the highest price in the implemented sequence, see Appendix B for details; Sequence optimal offer = the highest offer value in each round, see Appendix B for details; Predicted optimal position = the stopping position predicted by the optimal decision rule (the decision rule which yields the highest earning), see Appendix C for details; Predicted optimal offer = the offer at the position predicted by the optimal decision rule, see Appendix C for details. Min. = the lowest offer in each round. Average offer = the average offer in the implemented sequence for each round. Std. D. = the standard deviation of 20 price offers in each round.

The participants in the *No Frame* treatment receive the following instructions about their task. Note that there is no mention of a house, its description, or any additional information. The full instructions are provided in Appendix A.1.

You will participate in 10 rounds. In each round, you will be asked to decide whether to accept or reject a number. The numbers are randomly generated by the computer and available one at a time. Once a number is presented, you can either accept or reject it. If you accept the number, you receive the amount represented by the number (in experimental currency units, as will be explained below). All decisions are final. If you reject the number, the number will disappear; you cannot go back to the previously rejected number. In total there are 20 numbers available; if you have not accepted a number prior to the 20th number, you will be forced to accept the 20th (i.e. the final) number.

In the *House Frame* treatment, the task is framed as selling houses, but no additional information about houses is provided.

You will participate in 10 scenarios, in which you will be selling houses. In each scenario, you will be asked to decide whether to accept or reject a price offer for a particular house. You will be given a brief description of the house that will be followed by a series of price offers. The price offers are randomly generated by the computer and available one at a time. Once a price offer is presented, you can either accept or reject it. If you accept the price offer, the house will be sold at the price you accepted. All sales are final. If you reject the price offer, the offer will disappear; you cannot go back to the previously rejected offer. In total there are 20 price offers available for each house; if you have not accepted an offer prior to the 20th offer, you will be forced to accept the 20th (i.e. the final) offer. Therefore, make your decisions carefully. There is no time limit on how long the price offers will be available for, so take as long as you need to evaluate each offer.

Finally, the *House Frame with Info* treatment employs identical instructions to the *House Frame* one. The only difference between these two treatments is that in the *House Frame with Info* treatment a description of a house, consisting of the floor area, the number of bedrooms, suburb and year the house was built in, is presented prior to the price offer. Each round features a different house description. The house descriptions were also obtained from the Quotable Value database.

We compare the participants' decisions based on the stopping position and their total earnings. We assume that the participants are risk-neutral decision-makers who aim to maximize their expected payoffs. The first hypothesis, formulated with reference to experiments on the Wason selection task, is that people make better decisions when presented with a context than without. Theoretically, this would be because having a context allows one to better assess the situation and access existing schema constructed from a similar experience, for example, selling an object in everyday life. According to previous psychology research, when no context is available, the person might experience difficulty in determining what schema to apply and the chance of applying an inappropriate schema is increased. Assuming we replicate early stopping behavior in our experimental set up, we expect the *House Frame* and *House Frame with Info* treatments to produce higher total earnings and longer search than the *No Frame* treatment.

Hypothesis 1: People search longer and obtain higher total earnings when decisions are framed as selling houses (both with and without additional information) than when they are framed neutrally.

It is possible that having a fairly complete description of the house – for example, floor area, number of bedrooms, the year the house was built – is also critical in activating a useful schema.

We therefore expect participants to search longer and their total earnings to be higher in the *House Frame with Info* treatment than in the *House Frame*.

Hypothesis 2: People search longer and obtain higher total earnings when decisions are framed as selling houses and more content-relevant information is available than when there is no such information.

At the same time, we recognize that having to process additional information in the *House Frame with Info* treatment could be distracting to participants and might result in more noise.

Procedures and Participants

The experiment was conducted at the University of Canterbury in Christchurch, New Zealand. The payoff protocol was single-blind, meaning that the experimenter was able to match participant decisions to their identity. The participants were recruited via ORSEE (Greiner, 2015). After arriving at the lab, the participants were randomly assigned to a cubicle and read the instructions (provided in Appendix A.1 – 3) at their own pace. Any questions were answered in private. A total of 137 students participated in the experiment: 46 in the *No Frame* treatment, 43 in *House Frame*, and 48 in *House Frame with Info*. A session lasted on average 45 minutes and the participants earned NZD 12.10 on average.

4. Simulation

What is the optimal decision rule for a payoff-maximizing risk-neutral decision-maker in our variation of the secretary problem? Since the distribution of offers in our experiment is unknown to participants, we conduct a simulation that allows to evaluate the performance of different decision rules. Each simulation compares the payoffs resulting from 20 different decision rules (as there was a maximum of 20 offers; each decision rule prescribes how many offers to reject in order to learn about the distribution, followed by accepting the next highest offer), which contain all possible stopping positions (i.e., an individual stops the search by accepting the n^{th} offer in a given sequence; where $1 \leq n \leq 20$). Each simulation iteration generates a set of 20 random offers in the same way as the actual offers used in the experiment were generated. Once a set of offers has been generated, the offers are (implicitly) ordered from the highest to lowest and assigned a rank within this particular order. These offer values and the rank for each offer are recorded to test the performance of each decision rule. The simulation runs separately for each round with 1.2 million iterations.

We compare the performance of all 20 possible decision rules using both the average payoffs (in ECUs) they yield and the frequency of each decision rule finding the optimal offer (in %). The average payoff statistic indicates which decision rule yields the highest payoff. The optimal offer frequency statistic shows which decision rule finds the optimal price offers most frequently.

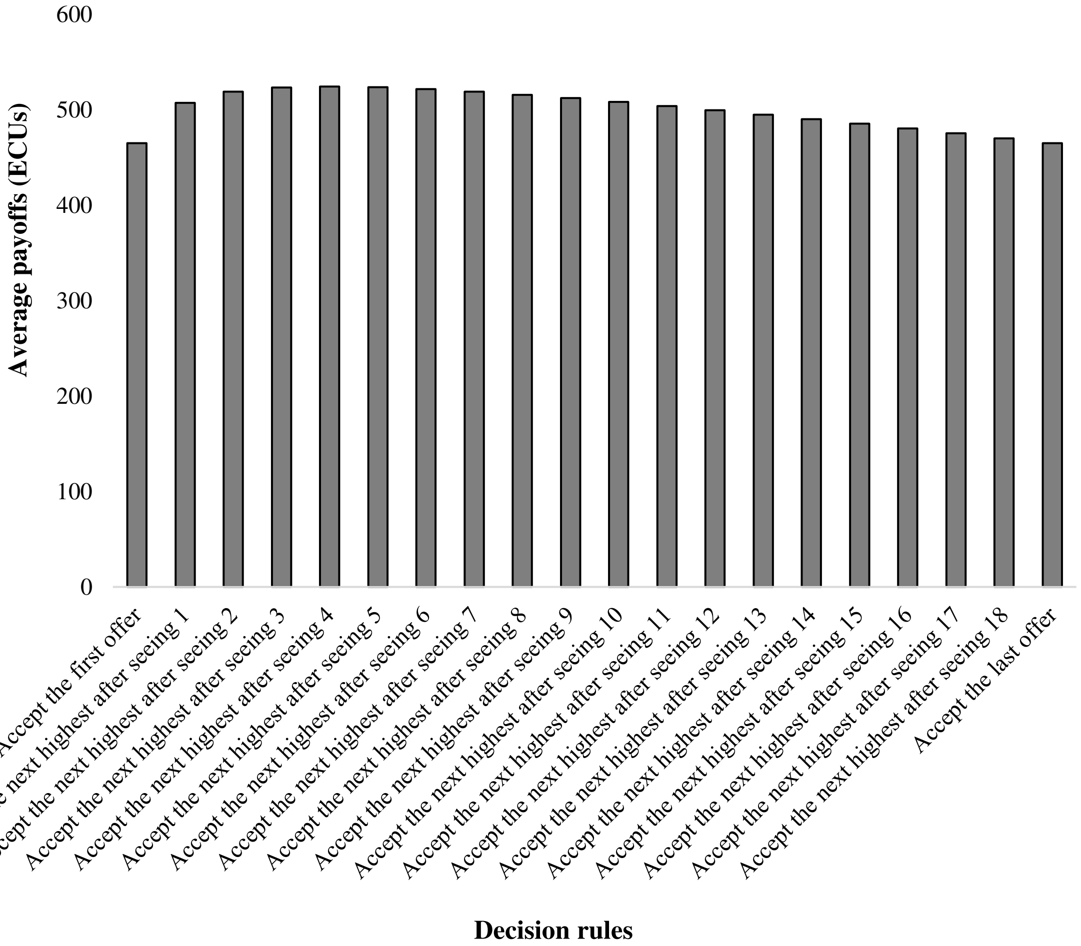

Figure 1. The average payoff for all decision rules.

According to the simulation, the decision rule to "Accept the next highest offer after seeing 4 offers" yields the highest average payoff of 524.3 ECUs (see Figure 1). The decision rule "Accept the next highest offer after seeing 7 offers" finds the most optimal offers as presented in Figure 2, just as the optimal decision rule of the classical version of the secretary problem (20/*e* = 7.4 offers). However, the decision rule "Accept the next highest offer after seeing 7 offers" yields only 518.9 ECUs on average. The simulation thus indicates that, when any accepted offer generates a positive payoff (as opposed to only the best one as in the classical secretary problem) and the goal is to maximize the payoff, it might be better to stop the search sooner (i.e., accept an earlier offer) than prescribed by the solution to the classical secretary problem.[11]

[11] If one were to implement only one decision rule to sell all 10 houses, the decision rule "Accept the next

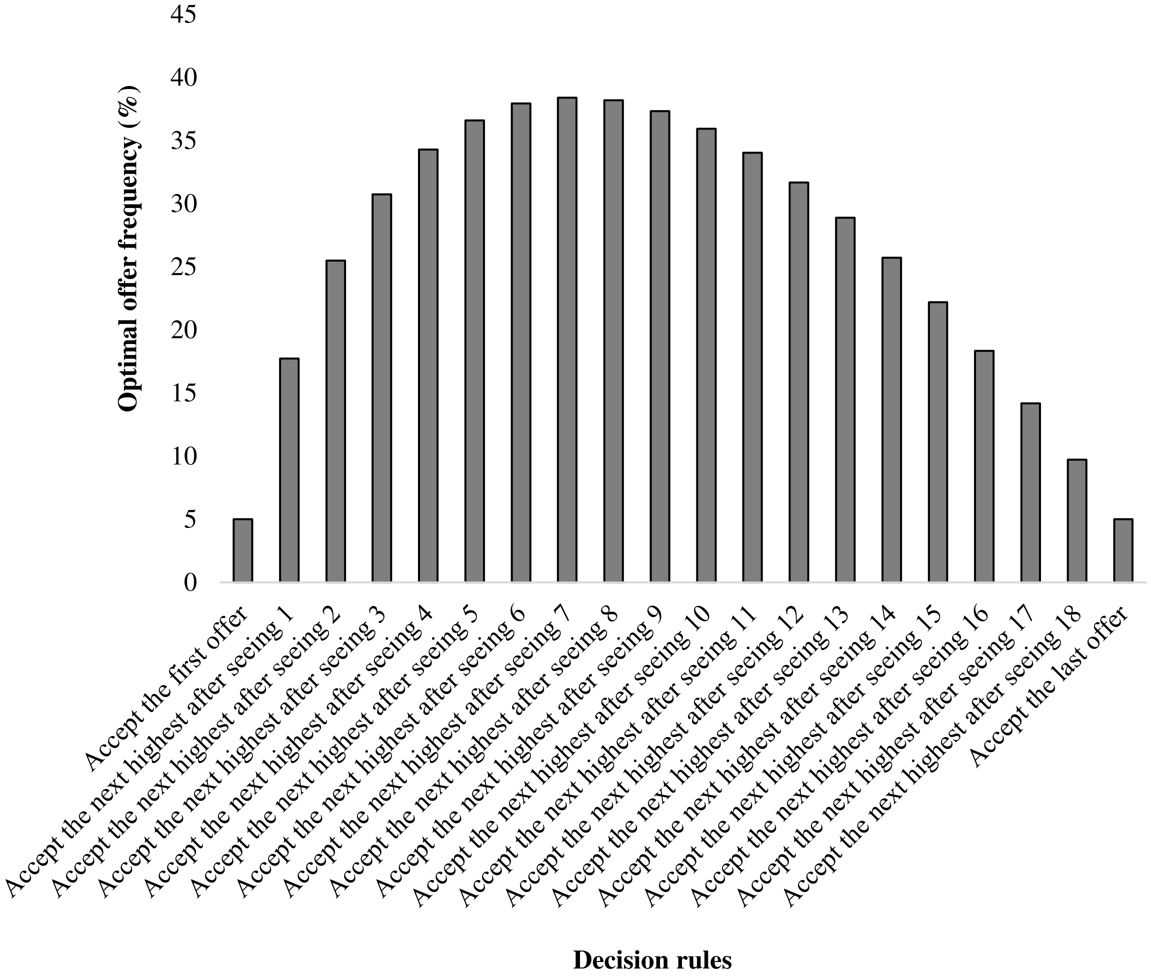

Figure 2. The frequency of finding the optimal offer for all decision rules.

5. RESULTS

First, we describe the summary statistics and test our hypotheses. Then, we examine whether there is a repetition effect found in any of the treatments, namely whether participants make better decisions in the latter rounds than in the earlier rounds. Finally, we present correlation results between the amount of search and total earnings.

There are two dependent variables: the amount of search exerted, i.e. the position in the sequence where the participant stops searching and accepts the offer (henceforth *stopping position*), and the amount of money the participant earned, i.e. the cumulative sum of 10 accepted offers in ECUs (henceforth *total earnings*). The summary statistics relating to these two dependent variables are presented in Table 2.

highest offer after seeing 7 offers" has the highest total earnings and "Accept the next highest offer after seeing 9 offers" finds the most optimal offers in the sequences used in our experiment (see Appendix C).

Table 2: Summary statistics and between treatments statistical tests.

Panel A. Stopping position

	Average	SD	Tukey HSD Post Hoc test***		Analysis of Variance			
			p		*F* (*df*)	MS error	*p*	Partial η^2
No Frame$^{(1)}$	9.8	3.6	0.07$^{(2)}$	0.015$^{(3)}$	4.3 (2, 134)	9.4	0.015	0.061
House Frame$^{(2)}$	11.2	2.8	0.07$^{(1)}$	0.86$^{(3)}$				
House Frame with Info$^{(3)}$	11.6	2.8	0.015$^{(1)}$	0.86$^{(2)}$				
Sequence optimal*			11.1					
Predicted optimal**			13.6					

Panel B. Total earnings (ECU)

	Average	SD	Tukey HSD Post Hoc test***		Analysis of Variance			
			p		*F* (*df*)	MS error	*p*	Partial η^2
No Frame$^{(1)}$	6880.3	750.0	0.005$^{(2)}$	< 0.001$^{(3)}$	8.1 (2, 134)	0.00003	< 0.001	0.11
House Frame$^{(2)}$	7269.5	361.4	0.005$^{(1)}$	0.84$^{(3)}$				
House Frame with Info$^{(3)}$	7338.9	580.7	< 0.001$^{(1)}$	0.84$^{(2)}$				
Sequence optimal*			9228.0					
Predicted optimal**			7930.0					

*Sequence optimal refers to the actual optimal position/offer from the sequences implemented in the experiment.

** The predicted result from applying the optimal decision rule to the sequences implemented in the experiment (see Appendix C for more detail). This serves as a benchmark only, not for a direct comparison to participants' decisions.

*** The result of Tukey HSD post hoc pairwise comparisons between two treatments; the small numbers in parentheses indicate the compared treatment. Notes: SD presents the standard deviation, *df* refers to the degree of freedom.

STOPPING POSITION

Participants in *No Frame* stopped at a significantly earlier average position (*M* = 9.8) than in *House Frame* (*M* = 11.2) and *House Frame with Info* (*M* = 11.6). Tukey HSD post hoc tests show that the participants in the *House Frame with Info* treatment stop their search at a significantly later position than in the *No Frame* treatment ($p = 0.015$, hypothesis 1). The participants in the *House Frame* treatment stop their search at a later position than in the *No Frame* treatment, however the difference is only weakly statistically significant ($p = 0.07$, hypothesis 1). There is no significant difference between *House Frame* and *House Frame with Info* ($p = 0.86$, hypothesis 2). The results are robust to using the non-parametric Mann-Whitney U test; see Appendix D for details.

The participants therefore searched less in the *No Frame* treatment and on average stopped their search 1.3 positions prior to the sequence optimal position (*M* = 11.1); the sequence optimal position refers to the position with the highest price offer in the implemented sequence. The stopping positions in the *House Frame* and *House Frame with Info* treatments are on average 0.3 positions higher than the sequence optimal position. (Table 2 panel A).

Result 1: The house-selling frame (with or without information) leads to more search than no frame. The house-description information has no effect on the amount of search when the task is framed as selling houses.

TOTAL EARNINGS

Participants in the *House Frame with Info* treatment had higher total earnings on average (*M* = 7338.9 ECUs) than participants in the *House Frame* treatment (*M* = 7269.5 ECUs) and the *No Frame* treatment (*M* = 6880.3 ECUs). On average, participants in the *House Frame with Info* and *House Frame* treatments earned 79.5% and 78.8% of the maximum total earnings (which is the sum of the highest offers in each of the 10 rounds) respectively. Participants in the *No Frame* treatment earned on average 75.0% of the maximum total earnings.

The analysis of variance show a significant main effect of treatment on the average total earnings, as presented in Table 2 Panel B. Tukey HSD post hoc tests confirm that the participants in the *House Frame* treatment received significantly higher total earnings than the participants in the *No Frame* treatment ($p = 0.005$, hypothesis 1). The participants in the *House Frame with Info* treatment also received higher total earnings than the participants in the *No Frame* treatment ($p < 0.001$, hypothesis 1). There is no significant difference in total earnings between the *House Frame* and *House Frame with Info* treatments ($p = 0.84$, hypothesis 2). All results are robust to using a non-parametric Mann-Whitney U test; see Appendix D for details.

Result 2: The house-selling frame (with or without information) leads to higher total earnings than no frame. The house-description information has no effect on the total earnings when the task is framed as selling houses.

REPETITION EFFECT

STOPPING POSITION

To examine whether the performance of participants improves with experience, we contrast participants' stopping positions in the first half of the session (rounds 1 – 5) with stopping positions in the second half of the session (rounds 6 – 10). We conduct a repeated-measures analysis of variance (ANOVA) with session halves (first vs. second) as a within-subject factor and treatments (*No Frame, House Frame,* and *House Frame with Info*) as a between-subject factor. The analysis of variance results, averaged across treatments and presented in Table 3, show a significant main effect of session halves. Participants stopped significantly earlier in the second half (M = 9.8) than in the first half (M = 11.9) of the session. There is also a statistically significant (p = 0.007) interactive effect of the treatments and session halves.

Tukey HSD post hoc tests show there is no significant difference between the *No Frame* and *House Frame (p* = 0.53*)*, *No Frame* and *House Frame with Info* (p = 0.87), or *House Frame* and *House with Info* (p = 1.0) treatments in the first half of the session. There is also no significant difference between *No Frame* and *House Frame* (p = 0.23) or *House Frame* and *House Frame with Info* (p = 0.63) in the second half of the session. However, the participants in the *House Frame with Info* treatment stopped at a significantly later position than the participants in the *No Frame* treatment in the second half (p = 0.002) of the session. We also conducted the Mann-Whitney U tests to check for robustness; see Appendix D for details.[12] Note that while the test does not detect a statistically shorter search in the *No Frame* treatment than in the *House Frame* treatment in either half, the overall effect of participants searching less in *No Frame* than in *House Frame* is weakly statistically significant (p = 0.07), as reported in the previous section.

The comparison between the session halves may also be influenced by the actual sequence optimal position (there are five sequence optimal positions in each session half). The results show that participants in the *No Frame* treatment searched less (on average by

[12] The Mann-Whitney U tests show similar results for all comparison in the first half session, but, find both *House Frame* and *House Frame with Info* stopped at a significantly later position than *No Frame* in the second half of session (*House Frame* and *No Frame; p* = 0.002, *House Frame with Info* and *No Frame; p* = < 0.001 respectively). The *House Frame with Info* also stopped significantly later than the *House Frame* in the second half of the session (p = 0.03); see Appendix D for details.

1.0 position) than the sequence optimal position in the first half and were even further away (on average by 1.6 positions less) from the sequence optimal position in the second half of the session.[13] The participants in *House Frame* and *House Frame with Info* treatments stopped search closer to the sequence optimal position than in the *No Frame* treatment in both the first and second halves of the session, as shown in Figure 3, providing further evidence that framing improves the quality of search decisions.

Result 3: The house-selling frame with information leads to more search than no frame in the latter rounds. The house selling frame without information and no frame yield similar amount of search throughout the entire session. Similarly, the house-description information has no effect on the amount of search throughout the entire session when the task is framed as selling-houses.

Table 3: Summary of statistical findings for the session halves and treatments.

		Analysis of Variance				
		F	*df*	MS error	*p*	Partial η^2
Stopping position	Session halves	61.0	1, 134	4.84	< 0.001	0.31
	Interaction effect with treatments	5.1	2, 134		0.007	0.07
Total earnings	Session halves	472.4	1, 134	53690	< 0.001	0.78
	Interaction effect with treatments	1.6	2, 134		0.21	0.02

Note: ***df*** refers to the degree of freedom

[13] The sequence optimal position in the first half was 12.2 and in the second half 10.0.

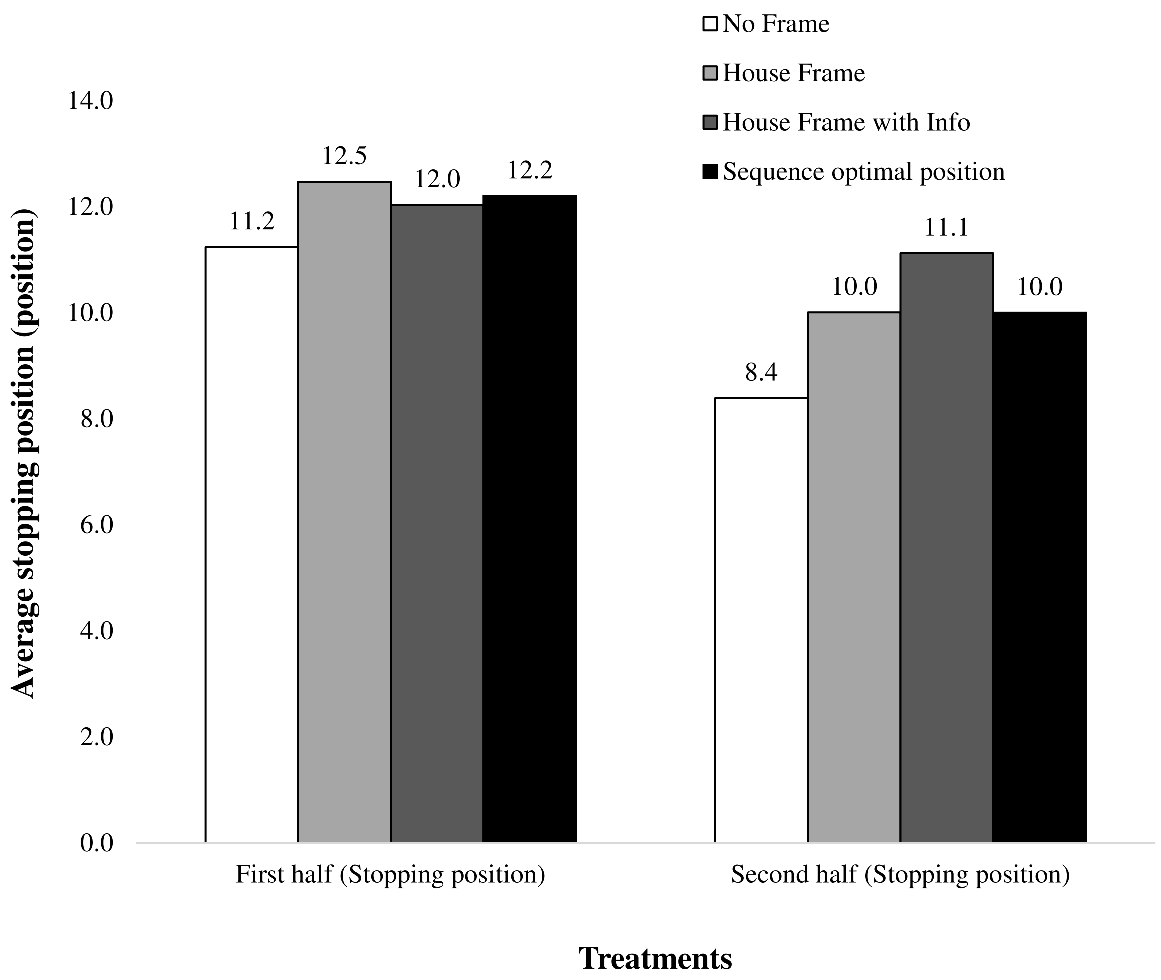

Figure 3. The average stopping position (averaged across participants) in the first half of the session (rounds 1 – 5) and the second half of the session (rounds 6 – 10) in the *No Frame, House Frame,* and *House Frame with Info* treatments compared to the *sequence optimal position.*

TOTAL EARNINGS

We next examine whether experience leads to higher total earnings. We compute the total earnings for each participant across the first half (rounds 1 – 5) and second half (rounds 6 – 10) of the session and conduct a repeated-measures ANOVA. The analysis detects a significant main effect of session halves in total earnings with participants decisions leading to higher total earnings in the second half of the session, but the interaction effect of the session halves and treatments is not significant ($p = 0.21$). The detailed results are presented in Table 3.

Participants in the *House Frame* (first half: $M = 3317.8$; second half: $M = 3951.7$). and *House with Info* (first half: $M = 3342.7$; second half: $M = 3996.2$) treatments received higher earnings in both the first and second half of the session than *No Frame* (first half: *M*

= 3170.1; second half: $M = 3710.2$). Tukey HSD post hoc tests show a significant difference between the *No Frame* and *House Frame (p* = 0.001*)*, *No Frame* and *House Frame with Info (p* = 0.006*)* in the second half, but not in the first half (*No Frame* and *House Frame; p* = 0.31, *No Frame* and *House Frame with Info; p* = 0.13, respectively) of the session. There is also no significant difference found between the *House Frame* and *House with info* treatments in either the first ($p = 1.0$) or the second half ($p = 1.0$) of the session. The Mann-Whitney U tests are also conducted to check for robustness; refer to Appendix D for details.[14] The results are graphically presented in Figure 4.

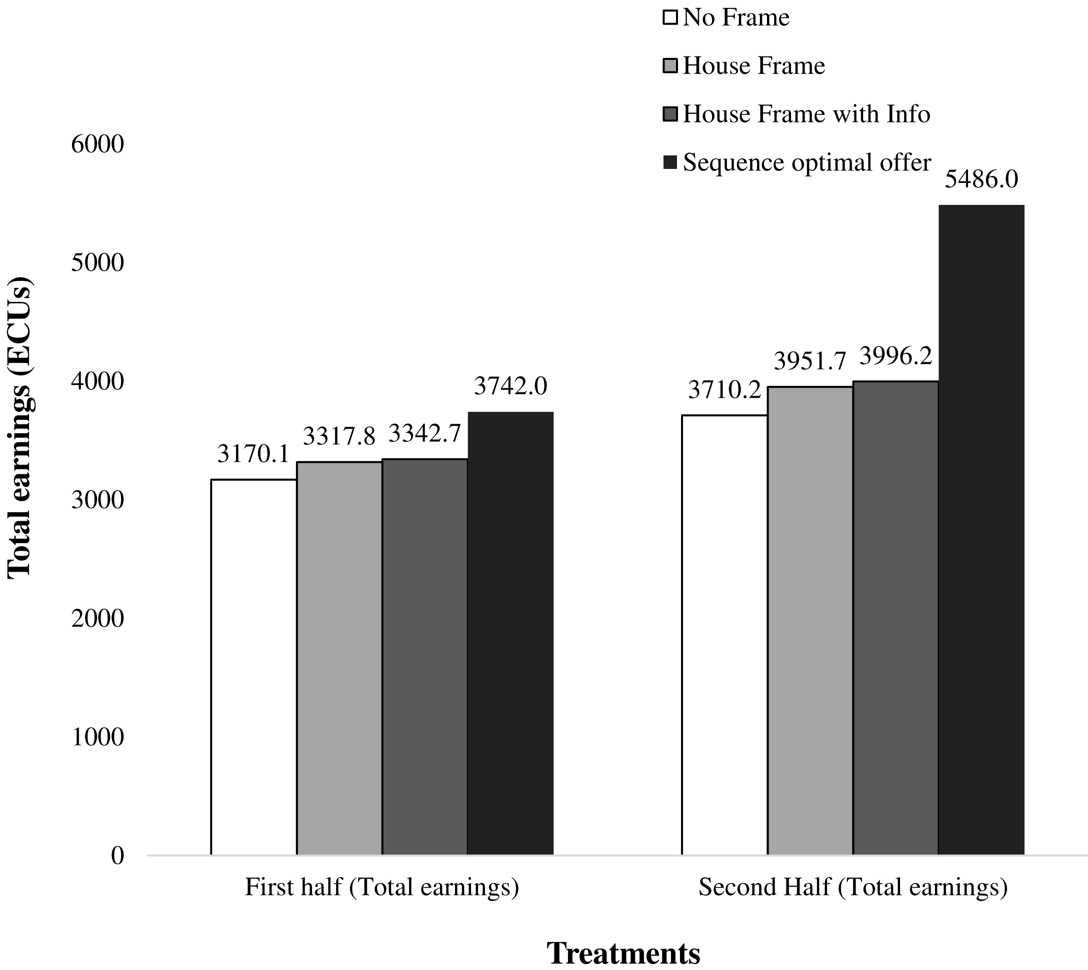

Figure 4. The average total earnings (averaged across participants) in the first half (rounds 1 – 5) and second half (rounds 6 – 10) of the session in the *No Frame, House Frame,* and *House Frame with Info* treatments, compared to the sequence optimal offer.

---

[14] The Mann-Whitney U tests show that participants in *House Frame* and *House Frame with Info* received significantly higher earnings than participants in *No Frame* in the first half (*House Frame* and *No Frame; p* = 0.03, *House Frame with Info* and *No Frame; p* = 0.001, respectively) as well as in the second half of the session ($p = 0.003$, $p < 0.001$, respectively). The earnings in *House Frame* and *House Frame with Info* in both the first ($p = 0.14$) and second half ($p = 0.29$) of the session are not statistically significantly different. See Appendix D for details.

Result 4: The house-selling frame (with or without information) leads to higher total earnings than no frame in the latter rounds. The house-description information has no effect on total earnings in the entire session when the task is framed as selling houses.

PEARSON CORRELATION ANALYSIS

We use Pearson correlation analysis to examine the relationship between the average stopping positions, the total earnings, and the optimal offer frequency. The average stopping position is obtained by averaging the 10 actual stopping position for each participant. The total earnings are calculated for each participant by adding up the 10 offers she accepted. The optimal offer frequency shows the percentage of accepting the optimal offer (out of all accepted offers) by a participant.

Unsurprisingly, we find a positive significant correlation between the frequency in which the optimal offer is accepted and the total earnings for all three treatments ($r = 0.57$, $p < 0.001$). This is expected because the optimal offer is the highest offer in each round. Accepting the optimal offer in more rounds will therefore result in higher total earnings. There is a large positive and significant correlation between the length of search and the size of the accepted offer ($r = 0.61$, $p < 0.001$), as well as the frequency of accepted optimal offers ($r = 0.24$, $p = 0.005$). The longer the participants search, the more often they accept the optimal offers.Also, the longer the participants search, the higher are their total earnings. However, an individual correlation analysis for each treatment shows that the correlation between the search length and the size of the accepted offer is statistically significant only for the *No Frame* and *House Frame with Info* treatments (*No Frame*, $r = 0.77$, $p < 0.001$; *House Frame with Info,* $r = 0.56$, $p < 0.001$, respectively), while for the *House Frame* treatment the correlation is not statistically significant ($r = 0.12$, $p = 0.40$). Only in the *No Frame* treatment there is a significant correlation between the length of search and the frequency of accepted optimal offers ($r = 0.54$, $p < 0.001$); the same correlations in the *House Frame* ($r = .- 0.02$, $p = 0.88$) and *House with Info* ($r = 0.02$, $p = 0.90$) treatments are not statistically significant.

6. DISCUSSION

This study extends the empirical analysis of context effects to the domain of sequential search where the implemented task was framed as selling houses. We further contribute to the literature by exploring the link between the amount of information necessary to generate the context effect. Using a conservative experimental design, we show that even a context which only few of our participants were likely to have had past experience with can result in improved decisions.

The experiment confirms the hypothesis that the participants search longer and accept higher offers when the house selling frame is introduced as an experimental manipulation (hypothesis 1). This result is consistent with the conjecture that having a context can activate existing schemas that enhance decision-making ability, as previously found in reasoning task experiments. At the same time, we do not find an effect of providing additional context-specific information on participants decisions; framing itself appears to be sufficient to activate the relevant schema (hypothesis 2).

Previous research finds that people search more when less information is available (see e.g. Palley & Kremer, 2014 for evidence when the distribution of offers is known). However, this is not what we find in our data. There are at least two potential underlying causes for observing less search in a context-free setting. First, without a context, system one is unable to effectively associate a new experience with existing knowledge and strategies that the decision-maker has obtained from past experiences in a similar situation. This means that our participants might be forming their search strategy through trial and error, which is supported by higher standard deviations in the *No Frame* treatment. Second, it is possible that, without context, people are applying an inappropriate schema, an explanation potentially relevant from the methodological perspective as a large fraction of economics experiments is conducted without framing and in a context-free setting. It is not entirely clear whether participants always apply their own framing in such situations (Thunström et al., 2016 provide evidence for the dictator game), which could potentially lead to a loss of control over the data generating process. Whether people indeed apply their preferred framing to context-free tasks and whether the frequency of own-framing adoption interacts with certain design features, such as the complexity of the task, clearly deserves further investigation as it has fundamental methodological implications.

It is important to keep in mind the limitations of our study. Our experiment adopts a house-selling context to explore schema activation. As there is no general theory explaining the effect of context on decision-making, it is unknown what type of framing one should use to achieve a particular outcome, for example, to improve people's decisions to obtain higher earnings. From that perspective, the results of a house-selling frame may not generalize to other contexts, such as to the context of buying houses, or selling or buying cars. While the issues of whether context and framing effects carry over from one environment to another or whether they are task specific are outside the scope of our research question, we view them as promising future areas of research.

To conclude, our research adds to the existing literature on the importance of context in decision-making (e.g. Alekseev et al. 2017). Our results extend the range of tasks requiring sophisticated reasoning, performance in which is improved by adding context, to include the secretary problem. Our results also show that once context is established, providing additional information in order to strengthen it does not appear to be critical.

REFERENCES

Alekseev, A., Charness, G., & Gneezy, U. (2017). Experimental methods: When and why contextual instructions are important. *Journal of Economic Behavior and Organization, 134*, 48-59. 10.1016/j.jebo.2016.12.005

Angelovski, A., & Güth, W. (2019). When to stop - A cardinal secretary search experiment. LUISS Working paper.

Bearden, J. N., Rapoport, A., & Murphy, R. O. (2006). Sequential observation and selection with rank–dependent payoffs: An experimental study. *Management Science, 52*(9), 1437–1449. 10.1287/mnsc.1060.0535

Bower, G. H., & Cirilo, R. K. (1985). Cognitive psychology and text processing. *Handbook of Discourse Analysis,* (pp. I: 71–I:105). Retrieved from https://stanford.edu/~gbower/1985/Cog_Psy_&_Text_Proc–Cirilo.pdf

Cooper, D. J., Kagel, J. H., Lo, W., & Gu, Q. L. (1999). Gaming against managers in incentive systems: Experimental results with Chinese students and Chinese managers. *The American Economic Review, 89*(4), 781-804. 10.1257/aer.89.4.781

Corbin, R. M., Olson, C. L., & Abbondanza, M. (1975). Context effects in optional stopping decisions. *Organizational Behavior and Human Performance, 14*(2), 207-216. 10.1016/0030-5073(75)90024-0

Cosmides, L. (1989). The logic of social exchange: Has natural selection shaped how humans reason? Studies with the Wason selection task. *Cognition, 31*, 187–276. 10.1016/0010–0277(89)90023–1

Cosmides, L., Barrett, H. C., & Tooby, J. (2010). Adaptive specializations, social exchange, and the evolution of human intelligence. *Proceedings of the National Academy of Sciences, 107,* 9007–9014. 10.1073/pnas.0914623107

Cosmides, L., & Tooby, J. (1992). Cognitive adaptations for social exchange. In J. H. Barkow, L. Cosmides, & J. Tooby (Eds.). *The adapted mind: Evolutionary psychology and the generation of culture* (pp. 163–228). New York, NY, US: Oxford University Press. From http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.140.7186&rep=rep1&type=pdf

Cox, J. C., & Oaxaca, R. L. J. (1989). Laboratory experiments with a finite-horizon job-search model. *Journal of Risk and Uncertainty, 2*(3), 301-329. 10.1007/BF00209391

De Neys, W. (2006). Dual processing in reasoning two systems but one reasoner. *Psychological Science*. *17*. 428–433. 10.1111/j.1467–9280.2006.01723.x

DiMaggio, P. (1997). Culture and cognition. *Annual Review of Sociology*. 23, 263–287. 10.1146/annurev.soc.23.1.263.

Duchon, D., Dunegan, K. J., & Barton, S. L. (1989). Framing the problem and making decisions: the facts are not enough. *IEEE Transactions on Engineering Management*, *36*, 1, 25–7. 10.1109/17.19979

Dufwenberg, M., Gächter, S., & Hennig–Schmidt, H. (2011). The framing of games and the psychology of play. *Games and Economic Behaviour, 73*(2), 459–478. 10.1016/j.geb.2011.02.003 10.1016/j.geb.2011.02.003

Eger, C., & Dickhaut, J. (1982). An examination of the conservative information processing bias in an Accounting framework. *Journal of Accounting Research*, *20*, 711–723. 10.2307/2490894

Evans, J. S. B. T. (2008). Dual-processing accounts of reasoning, judgement, and social cognition. *Annual Review of Psychology*, *59*, 255–278. 10.1146/annurev.psych.59.103006.093629

Ellingsen, T., Johannesson, M., Mollerstrom, J., & Munkhammar, S. (2012). Social framing effects: Preferences or beliefs? *Games and Economic Behavior, 76*(1), 117-130. 10.1016/j.geb.2012.05.007

Ferguson, T. S. (1989). Who solved the secretary problem? *Statistical Science, 4*(3), 282–296. 10.1214/ss/1177012493.

Fischbacher, U. (2007). z–Tree: Zurich toolbox for ready–made economic experiments. *Experimental Economics*, 10(2), 171–178. 10.1007/s10683–006–9159–4

Freeman, A. (2007). *Cognitive behavior therapy in clinical social work practice.* United States: Springer Pub.

Freeman, P. R. (1983). The secretary problem and its extensions: A review. *International Statistical Review*, *51*, 189–206.

Friedman, D., & Sunder, S. (1994). *Experimental methods: A primer for economists*. Cambridge [England];New York;: Cambridge University Press.

Gardner, M., & Mathematical Association of America. (2009). *Sphere packing, lewis carroll, and reversi: Martin Gardner's new mathematical diversions*. Cambridge;New York;: Cambridge University Press.

Gamliel, E., & Peer, E. (2010). Attribute framing affects the perceived fairness of health care. *Judgment and Decision Making, 5*(1), 11–20. Retrieved from https://doaj.org/article/934a99ad75bf4281be37bb9310886ee0

Gilbert, J. P., & Mosteller, F. (1966). Recognizing the maximum of a sequence. *Journal of the American Statistical Association, 61*(313), 35–73. 10.1080/01621459.1966.10502008

Gilboa, I., & Schmeidler, D. (1995). Case-based decision theory. *The Quarterly Journal of Economics, 110*(3), 605-639. 10.2307/2946694

Griggs, R. A., & Cox, J. R. (1982). The elusive thematic–materials effect in Wason's selection task. *British Journal of Psychology, 73,* 407–420. 10.1111/j.2044–8295.1982.tb01823.x

Heider, F., & Simmel, M. (1944). An experimental study of apparent behaviour. *The American Journal of Psychology, 57*, 243–259. 10.2307/1416950

Hennig-Schmidt, H., Sadrieh, A., & Rockenbach, B. (2010). In search of workers' real effort reciprocity— A field and a laboratory experiment. *Journal of the European Economic Association, 8*(4), 817-837. 10.1111/j.1542-4774.2010.tb00541.x

Higgins, E. T., & Chaires, W. M. (1980). Accessibility of interrelational constructs: Implications for stimulus encoding and creativity. *Journal of Experimental Social Psychology*, *16*(4), 348–361. 10.1016/0022–1031(80)90027–X

Hoffman, E., McCabe, K., Shachat, K., & Smith, V. (1994). Preference, property rights and anonymity in bargaining games. *Games and Economics Behaviour, 7*, 346–380. 10.1006/game.1994.1056

Jehiel, P. (2005). Analogy-based expectation equilibrium. *Journal of Economic Theory, 123*(2), 81-104. 10.1016/j.jet.2003.12.003

Hsiao, Y. C. (2018). *An experimental investigation of the secretary problem: Factors affecting sequential search behaviour* (Doctoral dissertation). Available at https://ir.canterbury.ac.nz/handle/10092/16330

Johnson–Laird, P. N., & Wason, P. C. (1970). A theoretical analysis of insight into a reasoning task. *Cognitive Psychology*, *1*, 134–148. 10.1016/0010–0285(70)90009–5

Kahneman, D. (2003). A perspective on judgment and choice: Mapping bounded rationality. *American Psychologist, 58,* 697–720. 10.1037/0003–066X.58.9.697

Kahneman, D. & Tversky, A. (1984). Choices, values, and frames. *American Psychologist*. 39 (4), 341–350. 10.1037/0003–066X.39.4.341

Kay, A. C., & Ross, L. (2003). The perceptual push: The interplay of implicit cues and explicit situational construals on behavioral intentions in the prisoner's dilemma.

Journal of Experimental Social Psychology, 39(6), 634-643. 10.1016/S0022-1031(03)00057-X

Levin, I. P., & Gaeth, G. J. (1988). How consumers are affected by the framing of attribute information before and after consuming the product. *Journal of Consumer Research*, *15*(3), 374–378. Retrieved from http://www.jstor.org/stable/2489471

Liberman, V., Samuels, S. M., & Ross, L. (2004). The name of the game: Predictive power of reputations versus situational labels in determining prisoner's dilemma game moves. *Personality and Social Psychology Bulletin, 30*(9), 1175-1185. 10.1177/0146167204264004

Lindley, D. V. (1961). Dynamic programming and decision theory. *Journal of the Royal Statistical Society. Series C (Applied Statistics), 10*(1), 39–51.

Meyerowitz, B. E., & Chaiken, S. (1987). The effect of message framing on breast self–examination attitudes, intentions, and behaviour. *Journal of Personality and Social Psychology, 52*(3), 500–510. 10.1037/0022–3514.52.3.500

Narvaez, D., & Bock, T. (2002). Moral schemas and tacit judgement or how the defining issues test is supported by cognitive science. *Journal of Moral Education*, *31*(3), 297–314. 10.1080/0305724022000008124

McNeil, B. J., Pauker, S. G., Sox, H. C., & Tversky, A. (1982). On the elicitation of preferences for alternative therapies. *The New England Journal of Medicine, 306*(21), 1259–1262. 10.1056/NEJM198205273062103

Palley, A. B., & Kremer, M. (2014). Sequential search and learning from rank feedback: Theory and experimental evidence. *Management Science*, *60*(10), 2525–2542.

Pollard, P., & Evans, J. S. B. T. (1987). Content and context effects in reasoning. *The American Journal of Psychology, 100*(1), 41–60. 10.2307/1422641

Plott, C. R. (1987). *Dimensions of Parallelism: Some Policy Applications of Experimental Methods.* In: Laboratory experimentation in economics: six points of view. Cambridge University Press , Cambridge, 193-219. ISBN 9780521333924. Retrived from https://authors.library.caltech.edu/44501/1/Plott_1987p193.pdf

Rege, M., & Telle, K. (2004). The impact of social approval and framing on cooperation in public good situations. *Journal of Public Economics, 88*(7), 1625-1644. 10.1016/S0047-2727(03)00021-5

Seale, D., & Rapoport, A. (1997). Sequential decision making with relative ranks: An experimental investigation of the "secretary problem". *Organizational Behavior and Human Decision Processes, 69*(3), 221–236. 10.1006/obhd.1997.2683

Sugiyama, L., & Tooby, J., & Cosmides, L. (2002). Cross–cultural evidence of cognitive adaptations for social exchange among the Shiwiar of Ecuadorian Amazonia. *Proceedings of the National Academy of Sciences of the United States of America,* 99. 11537–42. 10.1073/pnas.122352999.

Teodorescu, K., Sang, K., & Todd, P. (2018). Post-decision search in repeated and variable environments. *Judgment and Decision Making*, 13(5), 484-500.

Thunström, L., Cherry, T. L., McEvoy, D. M., & Shogren, J. F. (2016). Endogenous context in a dictator game. *Journal of Behavioral and Experimental Economics, 65*, 117-120. 10.1016/j.socec.2016.08.001

Tversky, A., & Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science, 211*(4481), 453–458. 10.1126/science.7455683

Smith, V. L. (1962). An experimental study of competitive market behavior. *Journal of Political Economy, 70*(2), 111-137. Retrieved from http://www.jstor.org/stable/1861810

Smith, V. L. (1976). Experimental economics: Induced value theory. *The American Economic Review, 66*(2), 274-279. Retrieved from http://www.jstor.org.ezproxy.canterbury.ac.nz/stable/1817233

Smith, V. L. (1991). Rational Choice: The contrast between economics and psychology. *Journal of Political Economy, 99*(4), 877-897. Retrieved from https://digitalcommons.chapman.edu/cgi/viewcontent.cgi?article=1012&context=economics_articles

Wegner, D. M., & Wheathey, I. (1999). Apparent mental causation: sources of the experience of will. *American Psychologist*, *54*, 480–492. 10.1037/0003–066X.54.7.480

Zwick, R., Rapoport, A., Lo, A. K. C., & Muthukrishnan, A. V. (2003). Consumer sequential search: Not enough or too much? *Marketing Science, 22*(4), 503–519. Retrieved from http://www.jstor.org/stable/4129735

APPENDIX A

A.1 INSTRUCTIONS FOR THE NO FRAME TREATMENT

GENERAL INSTRUCTIONS

Overview

You are about to participate in a decision-making experiment. If you follow these instructions carefully you may earn a considerable amount of money which will be paid to you in cash at the end of the experiment. If you have a question at any time, please raise your hand and the experimenter will approach you and answer your question in private. We ask that you not to talk otherwise during the experiment. Also, please turn off your cell-phone and do not use the computer for any other purpose than your participation in the experiment requires. If you break these rules, we will have to exclude you from the experiment and from all payments.

INSTRUCTIONS

You will participate in 10 rounds. In each round, you will be asked to decide whether to accept or reject a number. The numbers are randomly generated by the computer and available one at a time. Once a number is presented, you can either accept or reject it. If you accept the number, you receive the amount represented by the number (in experimental currency units, as will be explained below). All decisions are final. If you reject the number, the number will disappear; you cannot go back to the previously rejected number. In total there are 20 numbers available; if you have not accepted a number prior to the 20^{th} number, you will be *forced to accept* the 20^{th} (i.e. the final) number. *Therefore, make your decisions carefully.*

There is no time limit on how long the numbers will be available for, so take as long as you need to evaluate each number.

Practice rounds

There will be two practice rounds. These practice rounds are there to help you become familiar with the software. You will not be paid for the decisions you make in these two practice rounds.

How payoffs are determined

The payoffs will be denoted in experimental currency units (ECUs).

1000 ECUs = 1 NZD

Your ECUs will be converted into NZD at this rate, and you will be paid in NZD when you leave the lab. The more ECUs you earn, the more NZD you earn.

Your payoffs are determined as follows:

<u>Total ECUs you earn</u>

=

Accepted number for Round 1 + Accepted number for Round 2 + ….+ Accepted number for Round 10

Example: Suppose you accepted the number 450 for Round 1, 260 for Round 2, 380 for Round 3….., 658 for Round 10. The total amount of ECUs you earn is 450+260+380+…. +658.

Do you have any questions?

You are now ready to begin the experiment. First, we will conduct two practice rounds, with no money payoffs. Then, you will make decisions in 10 rounds with money payoffs.

A.2 Instructions for the house frame treatment

GENERAL INSTRUCTIONS

Overview

You are about to participate in a decision-making experiment. If you follow these instructions carefully you may earn a considerable amount of money which will be paid to you in cash at the end of the experiment. If you have a question at any time, please raise your hand and the experimenter will approach you and answer your question in private. We ask that you not to talk otherwise during the experiment. Also, please turn off your cellphone and do not use the computer for any other purpose than your participation in the experiment requires. If you break these rules, we will have to exclude you from the experiment and from all payments.

INSTRUCTIONS

You will participate in 10 scenarios, in which you will be selling houses. In each scenario, you will be asked to decide whether to accept or reject a price offer for a particular house. You will be given a series of price offers for each scenario.The price offers are randomly generated by the computer and available one at a time. Once a price offer is presented, you can either accept or reject it. If you accept the price offer, the house will be sold at the price you accepted. All sales are final. If you reject the price offer, the offer will disappear; you cannot go back to the previously rejected offer. In total there are 20 price offers available for each house; if you have not accepted an offer prior to the 20^{th} offer, you will be *forced to accept* the 20^{th} (i.e. the final) offer. *Therefore, make your decisions carefully.*

There is no time limit on how long the price offers will be available for, so take as long as you need to evaluate each offer.

Practice scenarios

There will be two practice scenarios. These practice scenarios are there to help you become familiar with the software. You will not be paid for the decisions you make in these two practice scenarios.

How payoffs are determined

The payoffs will be denoted in experimental currency units (ECUs).

1000 ECUs = 1 NZD

Your ECUs will be converted into NZD at this rate, and you will be paid in NZD when you leave the lab. The more ECUs you earn, the more NZD you earn.

Your payoffs are determined as follows:

<u>Total ECUs you earn</u>

=

<u>Accepted price offer for House 1 + Accepted price offer for House 2 + ….+ Accepted price offer for House 10</u>

Example: Suppose you accepted the price offer 450 for House 1, 260 for House 2, 380 for House 3,…, 658 for House 10. The total amount of ECUs you earn is 450+260+380+…. +658.

Do you have any questions?

You are now ready to begin the experiment. First, we will conduct two practice scenarios, with no money payoffs. Then, you will make decisions in 10 scenarios with money payoffs.

A.3 Instructions for the House Frame with Info treatment

GENERAL INSTRUCTIONS

Overview

You are about to participate in a decision-making experiment. If you follow these instructions carefully you may earn a considerable amount of money which will be paid to you in cash at the end of the experiment. If you have a question at any time, please raise your hand and the experimenter will approach you and answer your question in private. We ask that you not to talk otherwise during the experiment. Also, please turn off your cellphone and do not use the computer for any other purpose than your participation in the experiment requires. If you break these rules, we will have to exclude you from the experiment and from all payments.

INSTRUCTIONS

You will participate in 10 scenarios, in which you will be selling houses. In each scenario, you will be asked to decide whether to accept or reject a price offer for a particular house. You will be given a brief description of the house that will be followed by a series of price offers. The price offers are randomly generated by the computer and available one at a time. Once a price offer is presented, you can either accept or reject it. If you accept the price offer, the house will be sold at the price you accepted. All sales are final. If you reject the price offer, the offer will disappear; you cannot go back to the previously rejected offer. In total there are 20 price offers available for each house; if you have not accepted an offer prior to the 20^{th} offer, you will be *forced to accept* the 20^{th} (i.e. the final) offer. *Therefore, make your decisions carefully.*

There is no time limit on how long the price offers will be available for, so take as long as you need to evaluate each offer.

Practice scenarios

There will be two practice scenarios. These practice scenarios are there to help you become familiar with the software. You will not be paid for the decisions you make in these two practice scenarios.

How payoffs are determined

The payoffs will be denoted in experimental currency units (ECUs).

1000 ECUs = 1 NZD

Your ECUs will be converted into NZD at this rate, and you will be paid in NZD when you leave the lab. The more ECUs you earn, the more NZD you earn.

Your payoffs are determined as follows:

Total ECUs you earn

=

Accepted price offer for House 1 + Accepted price offer for House 2 + ….+ Accepted price offer for House 10

Example: Suppose you accepted the price offer 450 for House 1, 260 for House 2, 380 for House 3,…, 658 for House 10. The total amount of ECUs you earn is 450+260+380+…. +658.

Do you have any questions?

You are now ready to begin the experiment. First, we will conduct two practice scenarios, with no money payoffs. Then, you will make decisions in 10 scenarios with money payoffs.

APPENDIX B.

Table 4. The actual price offers sequences used in the experiment

Offer \ Round	1	2	3	4	5	6	7	8	9	10
1	388	739	310	420	292	494	522	252	789	341
2	488	803	290	637	264	225	252	709	829	459
3	683	221	637	727	344	272	562	966	996	453
4	321	729	372	561	266	994	255	885	241	625
5	625	159	619	643	396	602	370	737	799	504
6	744	150	207	663	445	987	292	449	722	387
7	279	299	455	568	266	523	533	910	1088	250
8	848	818	400	636	241	683	237	250	876	308
9	276	585	251	422	370	1400	262	933	503	492
10	678	875	708	336	484	1574	343	491	650	455
11	408	130	452	414	264	1413	220	450	890	353
12	435	795	516	479	186	184	460	394	1264	588
13	679	481	420	332	578	1081	294	899	645	438
14	465	2	607	494	244	558	535	372	1740	408
15	393	525	410	546	189	273	297	505	1179	481
16	397	429	324	724	565	1182	452	608	250	467
17	588	62	214	411	271	305	284	827	840	418
18	358	459	480	267	235	661	436	712	272	273
19	644	748	463	357	350	785	581	838	449	554
20	495	374	617	733	373	89	197	541	105	553

APPENDIX C.

Table 5. Summary of predicted results for three dependent variables after applying different decision rules to the sequences used in the experiment.

Decision rule \ Variable	Total earnings (ECUs)	Optimal offer frequency (%)	Average stopping position
Accept the first offer	4547	0	1
Accept the next highest after seeing 1	6629	0	2.6
Accept the next highest after seeing 2	7352	20	3.7
Accept the next highest after seeing 3	7228	40	10.3
Accept the next highest after seeing 4	7562	30	12.4
Accept the next highest after seeing 5	7611	30	12.5
Accept the next highest after seeing 6	7754	40	13.1
Accept the next highest after seeing 7	7930	40	13.6
Accept the next highest after seeing 8	7634	40	15
Accept the next highest after seeing 9	7808	50	15.1
Accept the next highest after seeing 10	5825	30	18.4
Accept the next highest after seeing 11	5825	30	18.4
Accept the next highest after seeing 12	6301	40	18.6
Accept the next highest after seeing 13	6096	30	19.3
Accept the next highest after seeing 14	4461	20	19.9
Accept the next highest after seeing 15	4461	20	19.9
Accept the next highest after seeing 16	4461	20	19.9
Accept the next highest after seeing 17	4461	20	19.9
Accept the next highest after seeing 18	4461	20	19.9
Accept the last offer	4077	10	20

Notes: The total earnings (in ECUs) is the sum of the 10 price offers accepted by applying each decision rule. The optimal offer frequency is obtained by adding the number of rounds in which each decision rule finds the optimal offer (out of 10 rounds). The average stopping position is obtained by averaging the final stopping position (i.e. the offer accepted) across 10 rounds.

APPENDIX *D.*

Table 6: Summary of statistical tests for the comparisons between the *No Frame* and *House Frame*, *No Frame* and *House Frame with Info*, and *House Frame* and *House Frame with Info* treatments.

Panel A. No Frame and House Frame

Variables	Treatments	Average (Standard Deviation)	Mann-Whitney U	*p*
Average stopping position (round 1 – 10)	*No Frame*	9.8 (3.56)	793.5	0.11
	House Frame	11.2 (2.8)		
Total earnings (round 1 – 10)	*No Frame*	6880.3 (750.0)	649.5	0.005
	House Frame	7269.5 (361.4)		
First half average stopping position (round 1 – 5)	*No Frame*	11.2 (5.0)	888.5	0.41
	House Frame	12.5 (3.6)		
Second half average stopping position (round 6 – 10)	*No Frame*	8.4 (2.6)	618.0	0.002
	House Frame	10.0 (2.7)		
First half total earnings (round 1 – 5)	*No Frame*	3170.1 (355.0)	720.0	0.03
	House Frame	3317.8 (154.3)		
Second half total earnings (round 6 – 10)	*No Frame*	3710.2 (429.2)	627.5	0.003
	House Frame	3951.7 (314.4)		

Panel B. No Frame and House Frame with Info

Variables	Treatments	Average (Standard Deviation)	Mann-Whitney U	*P*
Average stopping position (round 1-10)	*No Frame*	9.8 (3.6)	780.5	0.014
	House Frame with Info	11.6 (2.8)		
Total earnings (round 1-10)	*No Frame*	6880.3 (750.0)	602.5	< 0.001
	House Frame with Info	7338.9 (580.7)		
First half average stopping position (round 1 – 5)	*No Frame*	11.2 (5.0)	1078.0	0.85
	House Frame with Info	12.0 (3.0)		
Second half average stopping position (round 6 – 10)	*No Frame*	8.4 (2.6)	487.5	< 0.001
	House Frame with Info	11.1 (3.3)		
First half total earnings (round 1 – 5)	*No Frame*	3170.1 (355.0)	680.0	0.001
	House Frame with Info	3342.7 (238.3)		
Second half total earnings (round 6 – 10)	*No Frame*	3710.2 (429.2)	627.5	< 0.001
	House Frame with Info	3996.2 (431.2)		

Panel C. House Frame and House Frame with Info

Variables	**Treatments**	**Average** (Standard Deviation)	**Mann-whitney** U	*p*
Average stopping position (round 1-10)	*House Frame*	11.2 (2.8)	930.0	0.42
	House Frame with Info	11.6 (2.8)		
Total earnings (round 1-10)	*House Frame*	7269.5 (361.4)	868.0	0.19
	House Frame with Info	7338.9 (580.7)		
First half average stopping position (round 1 – 5)	*House Frame*	12.5 (3.6)	939.1	0.74
	House Frame with Info	12.0 (3.0)		
Second half average stopping position (round 6 – 10)	*House Frame*	10.0 (2.7)	735.5	0.03
	House Frame with Info	11.12 (3.3)		
First half total earnings (round 1 – 5)	*House Frame*	3317.8 (154.3)	848.0	0.14
	House Frame with Info	3342.7 (238.3)		
Second half total earnings (round 6 – 10)	*House Frame*	3951.7 (314.7)	899.0	0.29
	House Frame with Info	3996.17 (431.2)		