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Studying choice-induced preference change within the implicit-choice paradigm: the role of real incentives and data collection methods.

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Abstract

Cognitive dissonance theory implies that individuals tend to change their preferences as a result of a decision. This implication is widely tested and confirmed within the free-choice paradigm, where a large amount of studies find significant spreading of rating or ranking alternatives subsequent to a choice task. However, recent criticism on the free-choice paradigm has questioned the methodological validity of this specific paradigm. The present study uses and slightly modifies the implicit-choice paradigm as first introduced as a solution for the methodological flaws by Alós-Ferrer et al. (2012). The biggest modification of the paradigm is the introduction of real incentives with incentive compatibility mechanisms. Instead of making hypothetical choices, as prevailing in the literature on this subject, the participants actually faced the consequences of the stated preferences and choices made during the experiment. Contrasting to the general finding in the literature, the present study does not find significant evidence for choice-induced preference change. The present study neither finds significant differences between the observed spread for data collected on pen and paper and that on laptop. In addition, two introduced treatments neither significantly influenced the observed spread. Gender is the only variable found to significantly influence the distribution of spread.

Keywords: choice-induced preference change, implicit-choice paradigm, real incentives, mode of data collection

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Introduction

Decision making is arguably one of the most crucial elements in the field of economics. The way that preferences are shaped and the decisions that follow from these preferences are intertwined with the concept of utility. The original utility theory posed by Bentham (1789), implied that decisions are based on the hedonic experience of an event, e.g. the expected sum of pain and pleasures of a decision. These expected positive or negative utilities may serve as the input for decision-making, shaping an individual's behavior (Samuelson, 1938; Stigler, 1950a, b; Ariely & Norton, 2008). This traditional view on utility has been challenged in the literature and replaced with perhaps a more practical view on this subject: utility can be derived from observed choices and can subsequently be used to account for these choices (Stigler 1950a, b; Kahneman et al., 1997; Kahneman & Snell, 1992). This method of inferring utility is not only a choice of methodology, but inherently touches upon a basic economic assumption: all economic agents are rational. In this sense, rationality is defined as such that economic agents always choose what is most likely to be the best choice for them.

However, some researchers argued that it is not always preferences that shapes choices, but choices can also shape preferences (Ariely & Norton, 2008). This phenomenon is referred to as choice-induced preference change and was first established by Brehm (1956) within the free-choice paradigm. In this paradigm, participants were asked to rate several alternatives, choose between two equally preferred alternatives and then to rate the alternatives again. A significant change in valuation of chosen and rejected items between the first and the second rating task has been considered evidence for a change in preference which originated merely due to the choice itself (see appendix A for an overview of the literature). An explanation for the change in preference that has been offered in the literature is related to the cognitive dissonance theory. Cognitive dissonance theory, as introduced by Festinger (1957), implies that whenever an individual faces a choice between two equally preferred alternatives, a psychological tension arises referred to as dissonance. This dissonance is subsequently reduced by a reevaluation of the alternatives in such a way that the chosen item is evaluated more positively and the rejected item more negatively, resulting in coherence between the decisions in the past and the current cognitions of the individual.

Several points of criticism have been brought forward in recent studies regarding the methodology used to research choice-induced preference change. This criticism originated in a paper by Chen (2008) and was formally addressed in subsequent papers by Chen and Risen (2010a, b). Even though several solutions were brought up to cope for the

methodological flaws (see Izuma & Murayama, 2013 for an overview), not all were appointed to fix the issues properly. One method that is considered to elicit choice-induced preference change more correctly is a variant of the implicit-choice paradigm, introduced by Alós-Ferrer et al. (2012). In the present study, an experiment that follows this methodology is therefore used to study choice-induced preference change. Even though not all points of criticism by Chen and Risen (2010 a, b) are fixed with this design, it is a step towards adequately researching choice-induced preference change.

One unresolved issue with the implicit-choice paradigm is that stated preferences may not completely reflect true preferences (Chen and Risen, 2010a). The majority of the experiments that study choice-induced preference change have been conducted with hypothetical questions, where participants received a fixed show-up fee to participate (see appendix A for an overview). Although in some studies the choice task has been incentivized, the rating or ranking task has never been. In other words, the participants have (almost) never faced the consequences of the stated preferences provided during the study. Lusk and Schroeder (2004) argued that this may induce inconsistent behavior, which is extremely harmful for establishing the phenomenon of choice-induced preference change. Inconsistent behavior may have been seen as evidence supporting preference change, even though it did not arise due to the choice situation. This is accounted for in the design of the current study, as the conducted experiment employs a fully incentive-compatible reward mechanism.

Besides providing the participants with real incentives, the present study is interested in the possible differences between two data collection methods: participants either fill out a survey on pen and paper or do so on a laptop. For objects that rely mostly on vision to be examined, a picture or a physical present item should be evaluated equally well (McCabe & Nowls, 2003). The products of interest in the current study are candy bars, products for which vision arguably is the most important sense. However, pictures of food products as opposed to physically present food products were found to induce different valuations; more specifically, the items that were physically present received a constant markup in rating (Bushong et al., 2010). The two different forms of data collection may thus produce different changes in valuation.

The design that is followed in the present study is proclaimed to provide evidence for choice-induced preference change (Alós-Ferrer et al., 2012; Izuma & Murayama, 2013). However, one may argue that the use of real incentives is such a crucial element that has not been

introduced in this field of research, which could induce different results. Therefore, the main research question that the present study aims to answer is the following:

Does the phenomenon of choice-induced preference change holds when real incentives are introduced?

The previous findings regarding this phenomenon may imply that previous choices tend to be of great influence on future decisions. This can be particularly helpful in the domain of fast moving consumer goods, since this may provide evidence in favor of promotional activities. For example, if a company is able to induce the first purchase when the customer is uncertain about which alternative to choose, this may provide great value for the company in the long run. As stated, the majority of previous studies have not incentivized the experiment accordingly, which may have resulted in inconsistent behavior and therefore unjustified evidence in favor of choice-induced preference change (Lusk & Schroeder, 2004). Finding choice-induced preference change in the present, incentivized study could therefore provide great value in this research domain and may translate this phenomenon into a real world situation.

The next section of the present study entails the theoretical framework regarding the related concepts that are introduced, ending with the research hypotheses that form the core of the present study. Subsequently, the methodology implemented to study these hypotheses will be elaborated on and the statistical hypotheses are presented, which are followed by the results. The results will then be discussed and additionally the limitations and implications of the present research are presented. Lastly, some recommendations for future research are provided. In contradiction to the existing literature on choice-induced preference change, the current study does not find significant evidence of this phenomenon being present, possibly due to the introduced real incentives. Furthermore, this study finds significant differences in gender, which is also not in line with the prevailing literature.

Literature review

Utility

As briefly touched upon in the introduction, it is widely accepted that decision making of an individual is related to the concept of utility. Decisions can be explained as the way for consumers to express their underlying needs and wants, since these underlying preferences are triggered when consumers are faced with decisions (Hoeffler & Ariely, 1999). From these decisions a utility function can be inferred, referred to as revealed preference theory (Samuelson, 1938, 1948, Arrow, 1959, Houthakker, 1950). This approach inclines that, when faced with two options (let's call them A and B) under a certain budget, choosing option A over option B indicates preference of A over B. The economic assumption of rationality serves as the basis of this utility-inferring method, where it is assumed that individuals choose what is in their best interest[1]. This seems as a pretty logical way to go about: people will choose what they prefer, right?

Unfortunately, decision making is not as straightforward as it seems, which may drive individuals away from making these defined 'rational' decisions. Individuals may want to be guided in their choices by values and principles rather than by the hedonic experience (Sen, 1987). A failure to assess future utility gains correctly or weighing correct beliefs inappropriately could also serve as the basis for decisions that do not maximize utility (Kreps, 1979; March, 1978; Varey & Kahneman, 1992). Research from the field of psychology confirms that individual decision making is not so rational and easy to grasp as believed.

In the psychological literature the hedonic experience of an event is referred to as attitudes, which have been considered imperfect predictors of behavior (Eagly & Chaiken, 1993; Fazio, 1990). Hence, the relationship between utilities and actions is less perfect than assumed. Using revealed preferences to infer utility therefore seems flawed to some extent, since behavior is not accurately predicted using this measure. This imperfect relationship could originate due to the fact that preferences are often not as clear cut or stable as economists have suggested (Kahneman & Snell, 1992; Payne et. al, 1993; Shaffir et. al 1993; Slovic, 1995).

Ambiguity and instability of preferences may have far-reaching consequences besides being harmful for academic research. Decisions of individuals were found to be highly sensitive to

[1] Rational preferences can additionally be defined as transitive and complete (Besanko & Breautigam, 2011).

situational factors, even when these factors had no influence on the utility of that course of action (Ariely & Norton, 2008). The decisions made by the individual are experienced as behavior that arises from stable underlying preferences. Instead, these actions could partially arise due to unrelated situational factors and are wrongfully perceived as good indicators of underlying preferences, which may influence future preferences and decisions (Bem, 1972). Using past experience as a guideline for future decisions is not a bad thing per se, as constructing preferences with each decision would take a lot of mental effort (Hoeffler & Ariely, 1999). It becomes problematic when these unrelated factors that influenced a decision in the past serve as the input for current and future decision making. One theory that has provided some useful insights why past decisions (erroneously shaped or not) play such a big role in decision making is the cognitive dissonance theory.

Cognitive dissonance

The cognitive dissonance theory implies that when an individual faces a choice situation that induces conflicting thoughts, a psychological tension arises referred to as dissonance (Festinger, 1957). The theory revolves around the post-decisional process subsequent to a choice and therefore choice tasks seem appropriate to induce and study dissonance (Brehm & Cohen, 1962, Beauvois & Joule, 1996, 1999; Festinger, 1964). Choice situations must result in an individual committing to one alternative, creating dissonance since there is information available contradictory to this commitment. Any positive thought on the rejected situation and any negative thought on the chosen situation are contradicting values to the decision, inducing dissonance (Brehm, 1956; Festinger 1957, 1964). A commonly used example is that of a smoker, who knows that smoking is bad for his/her health. Yet he/she commits to the action every time, inducing tension with every cigarette lit.

In any case where cognitive dissonance arises, Festinger (1957) argued that the magnitude of dissonance depends on the amount and weight of contradictory thoughts relative to the amount and weight of the consistent thoughts. The theory predicts that choosing between two equally preferred alternatives induces more dissonance than a situation where one alternative is heavily preferred over the other. In the former case, more conflicting thoughts arise since absolute preference difference between the alternatives is closer to zero. The origin of these conflicting thoughts lie beyond the scope of this research and are therefore only discussed briefly. The self-consistency perspective brought up by Aronson (1968, 1999) has received the most support in the literature (Harmon-Jones, 1999, 2000a, 2000b, 2001; Harmon-Jones & Harmon-Jones, 2002; Harmon-Jones et al., 1996; McGregor et al., 1999; Simon et al., 1995). This view proposes that dissonance originates from the need to reduce

the inconsistency between actual behavior (e.g. choices) and the self-concept of competence or rationality. A specific model, consistent with the self-consistency view of Aronson, proposed by Harmon-Jones (1999, 2000b) is based on action orientation of the individual. This action-oriented model embraces the propositions of Festinger (1957) that the extent to which dissonance is present depends on the relation between inconsistent and consistent thoughts about a decision.

The model implies that the thoughts most likely to induce dissonance are those providing useful information for action (Harmon-Jones & Harmon-Jones, 2002). Hence, dissonance inducing thoughts caused by contradictory information can potentially interfere with effective and unconflicted action. In order to reduce the dissonance, individuals may increase the value of the chosen alternative and/or decrease the value of the rejected alternative (Festinger, 1957; Harmon-Jones & Harmon-Jones, 2002). Changing these valuations results in more efficient and unconflicting behavior.

Let's take the example of a choice between two equally preferred alternatives. Increasing (decreasing) the valuation of the chosen (rejected) alternative results in updating preferences in such a way that the chosen item is now preferred over the rejected item. Moreover, when the individual looks back on the decision it now matches the updated preferences, restoring the consistency between thoughts and actions. This shift in preference may have only occurred *because* the individual faced the choice, as the individual was indifferent prior to the choice. Throughout this paper, changing preferences due to a choice situation will be referred to as choice-induced preference change. This so-called choice-induced preference change has been tested empirically in the free-choice paradigm.

Free-choice paradigm

Brehm (1956) was the first to establish choice-induced preference change in the free-choice paradigm (FCP in short). This paradigm consists of three major tasks:
1) Subjects are asked to rate a variety of objects on desirability
2) Subjects are asked to choose between several pairs of two equally rated objects
3) The subjects are asked to rate the desirability of all the objects once more

The rating tasks in these experiments infer preferences, where a higher rating for a particular object should reflect higher preference for that object. The last rating task is included in order to assess the consequences of choices on preferences (Brehm, 1956). A survey of studies have found that the chosen objects in task 2 were re-evaluated as more desirable and

rejected objects were re-evaluated as less desirable in the third task (for an overview, see Harmon-Jones & Mills, 1999 and also see appendix A). The spread (e.g. changes in preference of the chosen and rejected items) of easy choices is often subtracted from the spreading of difficult choices. Observed significant positive spread is considered evidence supporting the concept of choice-induced preference change, coherent with the view on post decisional re-evaluation of alternatives following dissonance.

Studies in which subjects ranked objects instead of rating them have also produced significant spread (Gerard & White, 1983; Lieberman et. al, 2001; Alós-Ferrer et al., 2012; Chen and Risen, 2010a; Lee & Schwarz, 2010; Imada & Kitayama, 2010; Kimel et al., 2012; Hoshino-Browne et al., 2005; Kitayama et al., 2004; Qin et al., 2011). It is important to denote the difference between the two methods, since these two methods ask for a different approach. The rating of an alternative is a cardinal measure, where two alternatives can be assessed with an equal rating: these equally-ranked alternatives are subsequently used as the objects in the choice task (see Harmon-Jones & Mills, 1999). It is not possible for two alternatives to possess the same rank and construction of choice pairs in the ranking variant has varied among the studies.

Furthermore, a rating can be seen as a value from which the distance of preference between two alternatives can be inferred. Contrastingly, two alternatives with distance 1 in rank can be very close or very far in terms of preference intensity, which cannot be directly observed (Brown, 2011; Allen & Seaman, 2007). Moreover, ranking of alternatives is considered to be more cognitively demanding and more complex than ratings, especially when the list of alternatives is lengthy[2] (Rokeach, 1973, Feather, 1973). Research has suggested that complexity of a choice experiment influences the consistency of stated preferences (Swait & Adamowicz, 2000; DeShazo & Fermo, 2002). This may specifically be very harmful for studying choice-induced preference change, since inconsistent preferences resulting from complexity can be interpreted as preference change caused by choices. Providing the participants with less alternatives already seems sufficient to reduce the problem of complexity. Ranking tasks may also be preferred over rating tasks due to the ordinal approach to preferences, conventional for other decision sciences (Alós-Ferrer et al., 2012).

[2] Although these particular findings have been found in psychological value research, rating or ranking alternatives in this context still revolves around revealing one's preferences.

Criticism on the FCP

Recently a point of criticism has been brought up with respect to the experimental approach of the FCP. Chen and Risen (2010a) claimed that the spread found in studies using the FCP partially arose due to the presence of selection bias. Selection bias in this paradigm may be present because the classification of chosen and unchosen objects depends fully on the participant's decision. If an individual faces a choice between A and B and chooses A, this individual most likely possesses higher preference for A than individuals that chose B. It is unable to infer if this result is present because of the *choice situation* or because the *choice revealed extra information about preferences* (Chen and Risen, 2010b). In another paper, the authors argue that this information is often neglected in FCP studies (Chen and Risen 2010a). Although the choices in the FCP are used to calculate spread or determine who is included in the analysis[3], the authors argue that this introduces a measurement bias. Adding instead of neglecting the information about the choices enhances the entire picture of preferences.

In addition, if the preferences are imperfectly measured before the choice is made, choices provide even more additional information about the participants' preferences (Chen, 2008). If we assume that choices are at least partially explained by preferences and stated preferences are not accurately reflecting true preference, this will result in a spreading of alternatives even though preferences never changed (Chen and Risen, 2010a). A change in rank or rating may be the result of noise instead of a change in actual preferences. Hence, if stated preferences are equal to 'real' preferences (no measurement error), choices would reflect stated preferences.

Several researchers have offered methods to cope with the discussed problem of selection bias[4]. One of these solutions was the implicit-choice paradigm, which is employed in the present study (Alós-Ferrer et al., 2012). Instead of comparing two equally preferred items in

[3] The chosen or unchosen objects are the objects of interest, spread is therefore calculated based on the movement in rating or ranking of these particular objects. Furthermore, individuals that do not choose according to their previously stated preferences are often excluded from the analysis due to the inconsistency.

[4] Two other methods that are not discussed in the current study are the Blind Choice Paradigm by Sharot et al. (2010b) and changing the order of the tasks in the FCP, initiated by Chen and Risen (2010a, b). In the Blind Choice Paradigm, the experimenter determined choices without influence of the participant and participants were made to believe that presented non-sensical stimuli served as the basis for the choices made. This may produce artificial dissonance which could cause choice-induced preference change.
The second method entailed changing the order of the tasks from Rank – Choose – Rank to Rank – Rank – Chose. If spreading is observed in the RRC task, it is impossible to infer that the choice process influences the spreading. By comparing the spreading across the two different orders the spreading caused by choice can be estimated since the imperfect measuring of preferences could be present in both task orders.

a direct choice pair, the two items of interest are split up in two implicit choice pairs. These two items (*a* and *b*) are not compared directly to each other, but compared to alternatives that were rated higher (*h*) or lower (*ℓ*) instead. These alternatives *h* and *ℓ* have an equal prior specified distance D to *a* and *b*. This results in two 'easy' choice pairs: (*a, h*) and (*b, ℓ*), where item *a* is expected to be the unchosen item and *b* the chosen item, on the basis of the previously stated preferences. The classification in chosen and unchosen objects in this construction is randomly determined: both of the equally preferred objects can be matched with a higher or lower preferred alternative. The authors argue that this formation of choice pairs resolves the selection bias critique raised by Chen and Risen (2010), as classification of objects is not entirely determined by the participant.

Izuma and Murayama (2013) addressed the critique of Chen and Risen (2010) specifically on the ICP. They claim that the methodological issues are not fully resolved using this method[5], since the choices made by the participant are still used to classify the subjects among groups: participants that do not choose according to their stated preferences are excluded from the analysis. Although this particular design has not received much attention, Izuma & Murayama (2013) argue that the extended analysis (which I will explain below) performed by Alós-Ferrer et al. (2012) does provide evidence for choice-induced preference change.

In the main analysis, Alós-Ferrer et al. (2012) excluded the individuals that did not choose in according to their stated preferences (e.g., an object was rejected in a choice pair while it was rated higher). In the extended analysis of the paper, these individuals were included in the analysis, under the assumption that they chose as expected based on their stated preferences in the first task: e.g. 'forcing' compliance. By treating participants as if they choose the objects based on the expectation, the participants are not selected based on the choices made, fully accounting for the selection bias critique. By doing so, the lower bound of the effect of choices on preference can be estimated, since there is no preconditioning on choices with regard to *a* and *b*. More generally speaking, if the compliance rate is 100% in the extended implicit-choice framework (everyone chooses as expected based on provided preferences), this 'lower bound effect' is equal to the true effect. Thus, the more people deviate their choices from the stated ratings or rankings, the less meaningful the results of

[5] However, Izuma and Murayama (2013) fail to report detailed information about their simulation on the ICP. More specific, they neither mention different distances nor do they mention compliance rates. Through personal communications with one of the authors in Alós-Ferrer et al. (2012), I have been informed that the simulations by Izuma and Murayama (2013) were replicated by the authors themselves. The findings entailed that the spreads in ICP were always lower than those in FCP, where a positive spread was only observed with small distances if choices are noisy. The less noise in the choice phase and the higher the distance, the lower the spread (many of their parameter constellation did not show significant spreading with stable preferences).

the implicit-choice paradigm are. The previously explained forced compliance completely mitigates this issue. On the other hand, this entirely removes the information that is obtained by choices, indicating that this point of criticism of Chen and Risen (2010a, b) is not at all coped for. Especially if revealed preferences in the valuation tasks turn out to be imperfect measures of real preferences, neglecting information obtained by the choices may produce wrong conclusions.

Real incentives

One way to avoid the potential measurement bias pointed out by Chen and Risen (2010a) is by providing the participants' incentives to answer more truthfully. Participating in an experiment undoubtedly requires mental effort to be exerted by the subjects and compensating the subjects financially has been primarily used in the experimental economic setting to induce this cognitive effort and to answer more honestly[6] (Smith, 1976). Although an increase in monetary reward is found to increase effort (see Camerer & Hogarth, 1999 for an extended overview on financial incentives in experimental economics), this does not always lead to an increase in performance, especially if the participants lack the cognitive ability to complete the task (Smith & Walker, 1993; Camerer & Hogarth, 1999). More specific, within decision tasks financial incentives were found to improve but also hinder performance, remaining inconclusive on the effect in the decision context (Camerer & Hogarth, 1999).

The large majority of FCP studies have compensated the participants with a fixed show-up fee[7], leaving subjects only incentivized based on the height of the flat monetary incentive. The choices faced within a FCP experiment have therefore been hypothetical ones, where subjects had to imagine facing these choices in real life. Regardless of the answers provided on these hypothetical questions, all the participants received the fixed show-up fee. This indicates that any answer provided is as good as any other answer in terms of influence on the subject's utility level (Carson & Groves, 2007). Moreover, Lusk & Schroeder (2004) argue that subjects might display inconsistent behavior when choices do not face real consequences. This inconsistency in behavior could be particularly harmful in studying choice-induced preference change, since the dependent variable in these studies revolves around the rating or ranking of chosen and rejected items. If the participant is not incentivized

[6] Money has been the most used incentive since it is considered to be value neutral and lets itself for the economic assumption of monotonicity: a situation that results in a higher monetary reward should be preferred to a situation that results in an inferior monetary reward when effort is equal.

[7] There are however some studies that deceive their subjects into thinking that the choice they make will have consequences, e.g. they would receive one of the chosen objects. In the end, they however still received a fixed show-up fee. Unfortunately, no specific research has been done regarding incentivizing the rating or ranking steps of an FCP experiment.

properly to assess these items and decisions carefully, the results may introduce a measurement bias pointed out by Chen and Risen (2010a).

Two concepts are explored to establish a better suiting incentive system for researching choice-induced preference change. Smith (1982) formulated several conditions for an economic experiment that, when met, allows the experimenter to achieve control and may better reflect a real life situation. The most important condition related to the FCP is the salience precept. This precept entails the concept that the rewards of participants should be related to the answers provided within an experiment (Smith, 1982). In addition to the salience condition, incentive compatibility can help to extract true preferences more accurately. Incentive compatibility means that each participant is able to receive the best outcome in his/her view by acting according to his/her true preferences (Nisan et al., 2007. p239). This complements and enhances the salience precept as defined by Smith (1982). Paying the subjects based on their answers and ensuring that reflection of true preference within these answers provides participants with the best outcome possible should incentivize the participants to reflect their preferences more accurate as opposed to a fixed show-up fee.

Type of data collection

Continuing on the debate between real and hypothetical choices, there could be another factor besides payment that relates to this discussion. Physical presence of an item with regard to choice situations can be seen as replicating the in-store environment, whereas inspecting an item based on a picture of the product can be seen as a remote environment (Wood, 2001). This difference may induce different levels of dissonance. For objects that rely mostly on information readily retrievable from memory and/or geometric information, e.g. shape and size, vision is the most important sense that is used to examine the product (Klatzky et al., 1993). More specifically, research has indicated that food selection is mainly driven by the visual system (Linné et al., 2002). For products that rely mostly on vision, an individual is argued to be able to examine products both in the in-store environment and the remote environment equally accurate (McCabe and Nowls, 2003).

Contrastingly, physically displayed snack items were found to significantly increase the valuation as compared to text or computer images of these items (Bushong et al., 2010). Compared to pictures of food products, the physically present food items received a constant

markup in terms of rating[8]. The authors claim that a reason for this behavior could be that the presence of appetitive food-related products induces behavior that will lead to contact with such appetitive items (Balleine, 2005; Balleine et al., 2008). Hence, physical presence of food items could lead to more induced dissonance as opposed to pictures.

[8] To note is that prior to this particular study the subjects were instructed not to eat for three hours, which may have induced confounding effects between hunger and the physical presence of the snack items compared to the pictures, also stated by the authors.

Main hypothesis development

In the following section, the main hypotheses are formulated which form the core of the present study. As previously discussed, the existing literature has provided quite some evidence in favor of choice-induced preference change within the FCP (see appendix A). The extended analysis by Alós-Ferrer et al. (2012) following the implicit-choice paradigm was appointed as a method that resolves the selection bias critique raised by Chen and Risen (2010a, b) on the FCP (Izuma & Murayama, 2013). Following this methodology is therefore deemed appropriate as a baseline for the present research. The first hypothesis will test whether choice-induced preference change holds after introducing more appropriate incentives. Based on previous findings in the literature, the first hypothesis that will be formulated and tested in this study is the following:

H1: The spreading of alternatives is different from zero

Other than attempting to confirm the findings of prior research with the introduction of real incentives, the present study introduces two different modes of data collection and tests for a possible difference between them. Data was collected either via a laptop survey or a pen and paper survey, where the main point of difference entailed whether the alternatives were physically present during evaluations or the subjects were presented with a picture of the alternatives. No specific research has been done on the effects of data collection with respect to choice-induced preference change. However, there is some evidence in the literature that finds differences in the valuation of food related alternatives when presented with either the physical items or pictures of those items (Bushong et al., 2010). Therefore, physical presence of an item is argued to induce higher dissonance. The following hypothesis is formulated to study the possible effect of the different modes of data collection:

H2: There is a difference in observed spread between the different modes of data collection

Additional hypothesis development

Following the main hypotheses, one additional hypothesis is developed that is not part of the core research. The results corresponding with this hypothesis are therefore presented as complementary findings. The additional hypothesis focuses around the so-called ‘lunch-dip effect’, which can be boiled down to a negative effect of consuming lunch on several cognitive tasks. This negative effect is induced by a drop in attention and arousal after eating a meal (Monk & Leng, 1986; Craig & Condon, 1984; Smith & Miles, 1986), which can be

seen more as of time-of-day effects, since the consumption of lunch is not necessary for this dip to occur (Monk et al., 1996). Tasks that require sustained attention are the tasks that are most affected by this phenomenon (for more information on meals affecting performance, see Smith & Kendrick, 1992). The task at hand in this study is perceived as a cognitive task that requires sustained attention, therefore found eligible for such an effect to take place. However, these time-of-day effects are found to be related to individual specific characteristics, especially to the circadian cycle of an individual[9] (Carrier & Monk, 2000). Nonetheless, one can imagine that an individual might have different attitudes towards candy bars before or after lunch, simply because candy bars are a snack item. Before lunch, one might have conflicting thoughts towards consuming candy bars at all, which may influence the magnitude of the dissonance being present. This assumption coupled with the possibility of a lunch-dip effect may induce different results for observations in the morning and in the afternoon. Hence, the following hypothesis is formulated to test for this effect:

H3: There is a difference in observed spread between observations in the morning and in the afternoon

[9] The circadian cycle is defined as the daily activity cycle based on 24-hour intervals. Can be seen as the body clock, which indicates when to sleep, eat, etcetera, which is different for all individuals.

Data & Methodology

Experiment

An experiment was conducted in order to formally analyze the possible impact of a choice task on a change in preferences. In the following section, the implemented experiment is discussed and elaborated on, followed by the statistical methods employed to analyze the obtained data. The most prevailing way to conduct this type of experiment has been asking subjects to engage in three tasks, as previously described in the literature review. Despite the critique on the methodology raised by Chen and Risen (2010), subsequent behavioral and neuroimaging studies on choice-induced preference change have not addressed this issue (Sharot et al., 2009, 2010a; Coppin et al., 2010, 2012; Imada and Kitayama, 2010; Lee and Schwarz, 2010; West et al., 2010; Harmon-Jones et al., 2011; Jarcho et al., 2011; Qin et al., 2011; Kimel et al., 2012; Kitayama et al., 2013). This can be seen as a flaw of these studies, since the critique could possibly undermine all the findings in studies that have used the classic form of the paradigm. As previously pointed out, the present study follows the methodology implemented in the extended analysis by Alós-Ferrer et al. (2012), since it was appointed as a way to provide evidence for choice-induced preference change (Izuma & Murayama, 2013). It however does not address the critique that choices may provide extra information on preferences (Chen and Risen, 2010a, b). This point of criticism is coped for by introducing incentivized tasks and incentive compatibility mechanisms, so that preferences are better reflected in the first place, without the choice providing extra information.

Experimental setup

The first section of the experimental setup focuses on the choices regarding the tasks at hand and the objects chosen to conduct the experiment with. Although both rating and ranking tasks have been used to analyze choice-induced preference change, ranking tasks maintain the ordinal approach to preferences which is common for other decision sciences (Alós-Ferrer et al., 2012). In the present study, a ranking variant of the ICP with six alternatives is chosen as the basis of the experiment, since lengthy rankings were found to induce complexity (Rokeach, 1973; Feather, 1973), which is argued to subsequently affect the consistency of preferences (Swait & Adamowicz, 2000; DeShazo & Fermo, 2002). It is therefore implicitly assumed that ranking six alternatives does not introduce complexity issues.

Participants of the experiment were asked to rank, choose and re-rank six alternative candy bars. Mars, Bounty, Kinder Bueno, Twix, Snickers and Kit Kat were chosen as the six candy bars to conduct the experiment with. Three of these candy bars were chosen as they were found to be well-selling and thus perceived to be popular candy bars. Snickers was prognosed to surpass M&M as the bestselling worldwide confectionary product in 2012 (Schultz, 2012). Additionally, Kit Kat positioned itself at rank 9 of the global candy sales market, whereas Twix was placed 6th in the US-only sales market. Although these are facts that may not represent the popularity among students of the Erasmus University Rotterdam, one may argue that these candy bars are well known in general. The other three candy bars were chosen out of personal interest of the researcher.

Two different reasons preceded the choice of candy bars as the items to conduct the experiment with. First, these items are considered to be fast moving consumer goods, for which the choice in a supermarket situation or an online purchase situation can be replicated with the current study. Second, one may argue that students have sufficient experience and knowledge about these products. Thus, it is assumed that every student at the Erasmus University has at least some affection regarding these products. Ranking and choosing among these six candy bars is considered a task that every subject should be able to execute properly[10]. The tasks at hand and the objects used in the experiment are sufficiently discussed, but we need to take a closer look at the formation of the choice pairs regarding the choice task. This is of great importance for understanding the mechanisms of the ICP framework.

The direct choice pairs constructed in the choice task were constructed following the design of Alós-Ferrer et al. (2012). In the implicit-choice paradigm, the choice pairs are constructed in such a way that instead of comparing two equally preferred items (*a* and *b*), these items are split up in two different choice pairs: (*a*,*h*) and (*b*,*ℓ*). It is important to note that in the formation of these choice pairs, the distance D from *a* to *h* and *b* to *ℓ* is a prior set distance, equal for both choice pairs. In these formed choice pairs, *a* is paired with alternative *h* which is ranked higher than both *a* and *b*. In such a particular choice pair (*a*,*h*) it is therefore assumed that *a* would be the unchosen candy bar. In the other choice pair, *b* is paired with

[10] To note is that the majority of the studies researching choice-induced preference change uses choice situations in which the participants are not (or at least) less familiar with the alternatives. As past choices have been argued to influence the construction of preferences, one may argue that choices within an experiment with respect to unknown alternatives may be more influential on the construction of (subsequent) preferences of the participant compared to choices regarding well-known alternatives.

alternative *ℓ* which is ranked lower than both *a* and *b*. In the choice pair (*b*,*ℓ*) it is therefore assumed that alternative *b* would be the chosen candy bar[11].

Important to note is that the present study differs from prior studies in the following way. Two different treatments were introduced, which determined the classification of alternatives into chosen and unchosen objects. Objects ranked 3rd and 4th were always present in the two choice pairs, but the treatment determined which of the two objects is expected to be chosen and which one is expected to be rejected. To note is that a distance D=2 was chosen, since otherwise formation of the choice pairs had not been possible when ranking six items in both treatments. A participant either participated in the so-called congruent treatment or in the incongruent treatment. The treatment a participant participated in was determined either by a coin flip (pen and paper treatment) or by a randomization feature in the Qualtrics software in the laptop version, both at the beginning of the questionnaire (Qualtrics, Provo, UT).

The incongruent treatment implied two choices that are, so to speak, incongruent with the previously stated rankings with respect to candy bars ranked third and fourth. This method of choice pair construction had been similar to that which is described on the previous page. Hence, in the incongruent treatment, individuals are asked to choose between rank 1 and 3 (*a*, *h*) and between rank 4 and 6 (b, *ℓ*). Rank 1 is strictly higher than rank 3 and rank 4 strictly higher than rank 6[12]. Thus, in this treatment it was expected that rank 4 is chosen (*b*) and rank 3 was rejected (*a*). This is incongruent in the sense that rank 4 is lower than rank 3, but is expected to be the chosen object in this particular treatment, whereas rank 3 is expected to be the unchosen object.

The congruent treatment implied two choices that are congruent with the previously stated rankings. The choice pairs were formed somewhat different in this particular treatment. Instead of choosing between (*a*, *h*) and (*b*, *ℓ*), individuals were asked to choose between rank 2 and 4 (*h, b*) and between rank 3 and 5 (*a*, *ℓ*). Hence, since rank 2 and 3 are strictly higher than rank 4 and 5[13], the participant was expected to reject rank 4 (*b*) and to choose rank 3 (*a*) based on the preferences provided. This is congruent in the sense that rank 3 is higher than rank 4, and that rank 3 is expected to be the chosen object whereas rank 4 is expected as the unchosen object. Since rejecting the fourth ranked object and choosing the third ranked object is more in line with the cognitions of the individual with regard to these objects, it was

[11] These assumptions however would not hold if a participants is indifferent between the candy bars *a, h, b* and *ℓ*.
[12] Except for the extreme case in which a participant is indifferent between the six candy bars
[13] Except for the extreme case in which a participant is indifferent between the six candy bars

expected that this treatment produces less cognitive dissonance than the incongruent treatment. Hence, in the incongruent treatment, one was expected to reject the object ranked third and choose the object ranked fourth, which is contradictory to the revealed preferences, even though the objects were never in a direct choice pair. Since the cognitive dissonance theory arguably revolves around internal consistency, it was expected that individuals within the incongruent treatment produced higher spreading of alternatives than within the congruent treatment.

Since individuals were randomly assigned to both treatments, the candy bar ranked 3 or 4 was either a chosen or an unchosen candy bar, resolving the selection bias critique raised by Chen and Risen (2010). Hence, the classification in chosen and unchosen candy bars partially relied on the randomized treatment a subject participated in instead of solely relying on the participant's decisions. The resolution of the selection bias issues may also justify the calculation of spreading of alternatives based on the candy bars ranked 3 and 4, since they can be classified as chosen or unchosen depending on the randomized treatment. Spread in the present study was calculated using the following formula: (Rank2Chosen - Rank1Chosen) + (Rank1Unchosen - Rank2Unchosen). Hence, based on the posed theories and evidence from studying the FCP, the expectation was that the chosen object moves up in rank, producing a positive value for R2Chosen-R1Chosen. In addition, the expectation was that the unchosen object moves down in rank, also producing a positive value for R1Unchosen-R2Unchosen. Adding both these components implies that a positive spread score possibly indicates that the participant is subject to the expectations of the cognitive dissonance theory, which may provide evidence for choice-induced preference change.

As already mentioned, in most of the existing literature on the FCP the rank and choice tasks have been related to hypothetical tasks, for which the participants often received a fixed show-up fee. In the present study, participants actually received one of the six candy bars at the end of the experiment. The candy bar that a participant received depended on the answers that were provided in one of the three tasks. If participants reflected their true preferences during the experiment, they were ensured to receive the best possible outcome as a reward. This was clearly stated to the participants at the start of the experiment and should have incentivized them accordingly to state their preferences as accurate as possible. If subjects decided to misstate their preferences, this only resulted in them receiving an inferior reward, following incentive compatibility mechanisms (Nisan et al., 2007; p239). In previous research the choice tasks have sometimes been incentivized but mostly participants were deceived into thinking the task was incentivized (appendix A). The rating or ranking step has not been incentivized in previous studies.

Walkthrough

The walkthrough entails a detailed description of every step of the experiment, including the payment procedure. If the experiment is already clarified sufficiently to you or if you are simply not interested in the exact procedure, you may want to skip this section. Before the experiment started, the subjects were presented with an instruction form that can be found in appendix B1. By reading the instruction form, the participants were informed about the task they were asked to complete and the entire procedure of the experiment was elaborated on. It was stated clearly that this experiment was not a memory task, since this could have influenced the answers of the participants. The instructions were short and clear to make sure that the participant remained concentrated to complete the tasks of interest. The main three tasks of the experiment in this particular study were the following:

Ranking task one. The subject ranked six candy bars from 1-6 based on preference, where rank 1 was the most preferred candy bar and rank 6 was the least preferred candy bar. In the pen and paper version these candy bars were displayed physically and were attempted to be displayed randomly to all participants. For the laptop version, the candy bars were displayed using images of the candy bars, for which the displaying order was randomized using the Qualtrics software (Qualtrics, Provo, UT).

Choice task. The subject made two choices among two direct choice pairs constructed as previously discussed. The order of choice tasks in the pen/paper task was always equal: the participants first chose from the 'best' choice pair (e.g., rank 1/3 for the incongruent treatment and rank 2/4 for the congruent treatment). In the laptop treatment the order was sometimes reversed, a flaw accountable to the experimenter.

Ranking task two. The subject re-ranked the six candy bars based on preference. The same procedure as in the first ranking task applied.

To note is that between the first and second task, a filler task was inserted to ensure the experimenter on having enough time to construct the choice pairs in the pen and paper condition. This task was a summation of three 3-digit numbers, adding up to over a 1000. Besides winning time, the participants were perhaps distracted by this task which made it harder for them to base their second ranking on memory.

The demographic questions were placed after the second ranking task to ensure that the three main tasks were focused on. These questions included gender, age and whether the

participant was part of the Erasmus School of Economics (ESE). The payment procedure started once these three demographic questions were completed. The procedure was explained to the participant once this stage was reached, based on a schematic, displayed in appendix B2. The experimenter and the participant followed this schematic step by step and based on the outcome, the participant received one of the candy bars. The payout structure was formed in such a way that randomization of the task on which the payment was based was ensured. In other words, every stage of the experiment could have been used as a basis for the payout, inferring that the best outcome possible was received by revealing true preferences in each of the three stages.

Data collection

Following the experimental walkthrough, the way the data was collected is briefly discussed. Every observation was collected at the Erasmus University Rotterdam, where students were approached randomly, a typical group of interest for this field of research (appendix A). All subjects participated in one experiment only, leading to a comparison of spread on a between-subject basis. Subjects either filled out a questionnaire on the laptop or filled out the questionnaire on paper. Both surveys are found in appendix B3, which were designed with the intention to be as similar as possible. The choice pairs in the pen and paper version had to be formed by hand instead of automatic formation by the Qualtrics software[14]. Editing previous questions was not possible for both data collection methods and next questions could not be accessed without answering previous questions[15].

The data has been collected on two following Tuesdays, Wednesdays and Thursdays to control for possible day of the week effects that might be present with respect to the weekend. Although there has been little support for effect of Mondays (often referred to as 'Blue Monday') on mood ratings of students, other weekdays had significantly lower mood ratings than Fridays, Saturdays and Sundays (McFarlane et. al, 1988). Although these findings are coherent with other conducted studies (see Ryan et al., 2010 for an overview), Monday had still been excluded to avoid possible effects from being present. Since mood had been argued to influence food choice (Gibson, 2006), the days that possibly have a relation to better or worse moods had been deliberately not chosen as days to collect the data. Data was collected before 12.00 a.m. and after 1.00 p.m. to test for the lunch dip effect,

[14] One of the 90 participants was however excluded due to wrong formation of the choice pairs.

[15] To note is that the demographic questions placed at the end of the survey could be left empty, something that was not accounted for. This however only resulted in one participant leaving a question empty. This resulted in excluding that participant from the sample.

where it was implicitly assumed that individuals before 12.00 a.m. had not consumed lunch and individuals after 1.00 p.m. had consumed lunch.

Data

The data collection process led to a total of 180 observations, evenly split between the two data collection methods. A schedule with the exact days, day sections, treatments and collection methods is provided in appendix B4. Two observations were excluded from the data set as one of the laptop participants did not provide age and for one subject in the pen and paper version the choice pairs were constructed wrongly. Twelve individuals violated their first ranking with the choices that were made in the choice task, but were included in the analysis as done in the robustness check by Alós-Ferrer et al. (2012). 178 data points were therefore used in the main analysis. To note is that the compliance rate in the current study is higher than in the study by Alós-Ferrer et al. (2012).

As displayed in the experimental schedule in appendix B4, the distribution in treatments was somewhat skewed: a total of 109 individuals participated in the congruent treatment, whereas only 69 subjects participated in the incongruent treatment. This may be considered as a flaw of the randomization procedure for both the laptop and paper version, which was based on a coin flip or randomization feature of the Qualtrics software. Hence, it would have been better if randomization procedures were chosen that ensured a more equal distribution of treatments. The population of the Erasmus University was accurately reflected in terms of gender, with 91 males and 87 females that participated in the experiment[16] (Erasmus University Rotterdam, 2015). On the other hand, the share of students part of the ESE that participated was however larger than to be expected, with 81 ESE students and 97 Non-ESE students. The average age of the subjects was 21.6. A quick glance at the data reveals that the mean spread was only 0.011 (SD=1.008), which is much lower than in previous studies (see appendix A for an overview of the studies). This low spread was most likely caused by the fact that 120 out of the 178 participants did not produce any spread at all, following from the formula used to calculate the spread. This also resulted in interquartile ranges of 0, which implies that boxplots are not very useful to explore the data. Excluding the 12 participants that violated their own rankings with the choices made did result in a higher mean spread of 0.054, but is still very small.

[16] These figures are derived from the year 2015. It is assumed that the composition of males and females has not changed drastically over the past two years.

Methodology

In the following section, the statistical methods that were employed to analyze the data are discussed which is followed by the statistical hypotheses that were tested using these methods. At last, some additional statistical methods are discussed that were employed to explore all the dimensions of the collected data. Generally speaking, parametric tests are preferred over non-parametric tests since they have greater statistical power. However, these tests require a lot of assumptions that need to be met in order for them to produce unbiased results. One of the assumptions is that the data approximates a normal distribution (Anderson, 1961). The assumption of a normal distribution is not met for any of the tested groups, since the null hypothesis of such a distribution is rejected at the 1% significance level (p=0.000 for all groups, see appendix C for an overview). This leads to nonparametric tests being the main method of analysis for the present study. Nonetheless, parametric tests were performed and presented as complementary evidence. To note is that none of the conclusions drawn based on nonparametric tests were contradicted by the results of their parametric counterparts.

The first research hypothesis that was formulated entailed whether the spreading of alternatives among students significantly differed from zero. This hypothesis was formally tested using the Wilcoxon Signed Rank (WSR) test, complemented by a one sample t-test. Although the WSR test is mostly used to compare two sets of scores, it can also be used compare one set of scores to a particular value. The test compares the median of the distribution of positive and negative rankings of spread with respect to that particular value, which in our case would be zero. The one sample t-test was additionally used to compare the mean spread to zero. The statistical hypothesis that was tested using this method can be found below.

Research hypothesis 1: *The spreading of alternatives is not different from zero*
Null hypothesis: The median of the distribution of the negative and positive ranks of spread is equal to zero

Whereas the first hypothesis revolved around comparing the spread to zero, the second and additional hypothesis aim to compare the spread of two groups. The nonparametric test that was used to study these two hypotheses is the Mann-Whitney U test, complemented by the parametric one-way ANOVA. The Mann-Whitney U test is used to compare the distributions of spread for the two groups. More formally, it checks if the two independent samples come from the same underlying distribution.

The one-way ANOVA is used to compare the means of two independent groups. A less strong variant of the one-way ANOVA, called Welch's ANOVA, can be used even when the assumption of homogeneity of variances is violated. Hence, the appropriate form of the ANOVA was used for every single test. The statistical hypotheses that were formulated to employ the Mann-Whitney U test and ANOVA can be found below.

Research hypothesis 2: *There is no difference in spread between the different modes of data collection*
Null hypothesis: The distribution of spread is equal among the different modes of data collection

Research hypothesis 3: *There is no difference in spread between observations in the morning and in the afternoon*
Null hypothesis: The distribution of spread is equal among the two day sections

Additional statistical methods

Some additional methods that were employed are discussed now, which are not very relevant for the main analysis. To fully explore the obtained data, an Ordinary Least Squares (OLS) regression was estimated to look for possible relations between the collected variables and the observed spread. There is no need for robust standard errors, since the null hypothesis of the Breusch-Pagan test for heteroskedasticity is not rejected at the 5% level (p=0.283, see appendix C2). Additionally, there could be order effects present for the laptop participants due to the way the order of choice pairs were presented to them. The null hypothesis of a Mann-Whitney U test which compared the distribution of spread between participants with an equal order and those with a different order was rejected at the 5% significance level (p=0.686). It is therefore assumed that no order effects are present (appendix C3).

Independence of the variables is also tested in the current study. The Chi-Square test is employed for categorical variables such as gender, whereas the Kruskal-Wallis test is performed for the only collected continuous variable in this study, age. The Fisher's Exact Test is preferred over the Chi-Square test if the variables are in a 2×2 table, since this test is more conservative and precise. If the null hypotheses of the three discussed statistical tests are rejected, there is significant evidence that these variables are dependent of one another. The results are not discussed in the main text, but can be found in appendix G.

Results

Main results

The main results of the current study are now elaborated on. First of all, it was tested if the results found in previous studies held when the participants were provided with real incentives. Second, the influence of data collection methods was researched. Before the main results are discussed, it is important to once more note that the prevailing tests performed in the analysis are the nonparametric tests, since neither the entire group or separate groups of the collected data was subject to a normal distribution (see appendix C1). The parametric tests therefore solely serve as complementary evidence to the nonparametric tests, which can be found in the appendices referred to. If you are solely interested in the performed tests regarding the first hypothesis, see appendix D1.

Research hypothesis 1*: The spread of alternatives is not different from zero*

To study the formulated hypothesis, the spread of the entire sample was analyzed to see if the observed spread was different from zero. The present study did not find a spread that is significantly different from zero if the 5% significance level is used (n=178, M=0.01, SD=1,008. p=0.956). Contrasting to other findings in the literature, there was no significant evidence that the observed spread is significantly different from zero when the entire sample was analyzed.

Following this finding it may be interesting to use the same methodology for specific groups, instead of analyzing the entire sample. The results of the analysis can be found in table 1 on the following page. All of the performed tests pointed to one direction, indicating that the null hypothesis regarding the first research hypothesis was not rejected. In other words, there was no evidence supporting the observed spread being significantly different from zero for the entire sample as well as the specific groups that were analyzed. The parametric tests performed as complementary evidence indicated the same conclusions (see appendix D1).

Group	n	p-value WSR test	Mean spread	Standard deviation
Entire sample	178	0.956	0.01	1.008
Laptop	89	0.610	0.04	1.033
Pen/paper	89	0.641	-0.02	0.988
Congruent	109	0.540	-0.03	0.907
Incongruent	69	0.354	0.07	1.155
Laptop congruent	55	0.700	-0.05	0.951
Laptop incongruent	34	0.156	0.21	1.149
Pen/paper congruent	54	0.709	0.00	0.869
Pen/paper incongruent	35	0.775	-0.06	1.162

Table 1. An overview of the results for the groups compared to zero.

The second part of the main analysis was aimed towards studying possible differences in spread between the two modes of data collection instead of comparing the spreading of alternatives of these groups to zero. Several comparisons of this kind were performed using the Mann-Whitney U test. The results of these tests are discussed to formulate an answer on the second hypothesis. The corresponding p-values of the performed tests can be found in appendix D2.

Research hypothesis 2*: There is no difference in spread between the different modes of data collection*

In order to solely test for the effect of the different modes of data collection, the first test that was performed compared the spread between the group that participated on laptop (n=89, M=0.04, SD=1.033) and the group that participated on pen and paper (n=89, M=-0.02, SD=0.988). The Mann-Whitney U test resulted in a p-value of 0.434, indicating that the null hypothesis was not rejected at the 5% significance level.

Following these findings, one may argue that the introduced treatments in combination with mode of collection may induce different results. The spread of laptop and pen and paper participants is compared for the two introduced treatments separately. Comparing the different modes of collection for participants in the congruent treatment produced a p-value of 0.914 (n=55, M=-0.05, SD=0.951 for laptop and n=54, M=0.00, SD=0.869 for pen and

paper). For the incongruent group a p-value of 0.290 was found (n=34, M=0.21, SD=1.149 for laptop and n=35, M=-0.06, SD=1.162 for pen and paper), indicating that the null hypotheses of an equal distribution of spread for both subjects in the congruent and incongruent treatment were both not rejected at the 5% significance level.

These findings indicate that there was no significant difference in spread between modes of data collection for separate treatments. Comparing the spreads of the entire sample of laptop and pen and paper participants neither showed significant differences between the two in terms of distribution. These findings were confirmed by the parametric ANOVA tests applied and by the estimated regression (see appendix D2 for the ANOVA's and appendix D4 for the estimated regression). We can therefore conclude that there is no significant evidence that the mode of data collection had an influence on the observed spread.

Additional analysis

One additional hypothesis was formed to test whether the day section, split up in morning and afternoon, influenced the observed spread. A Mann-Whitney U test was performed to compare the observed spread between individuals that participated in the morning to those who participated in the afternoon. An overview of the tests performed related to this hypothesis can be found in appendix E1.

Research hypothesis 3: *There is no difference in spread between observations in the morning and in the afternoon*

Before the spread of the two groups is compared, it might be interesting to test if the spread of the separate groups significantly differed from zero. The WSR tests conducted indicated that the observed spread of both these groups did not significantly differ from zero at the 5% significance level (n=89, M=0.00, SD=0.839 and p=0.986 for the morning group; n=89, M=0.02, SD=1.158 and p=0.932 for the afternoon group). The Mann-Whitney U test was subsequently performed to compare the distribution of spread across participants in the morning and afternoon. A p-value of 0.786 following this test indicated that the null hypothesis of equal distribution among groups was not rejected. Hence, following from these results, we can conclude that the third hypothesis was not rejected, indicating that there was no significant difference in spread between the subjects participating in the morning or in the afternoon. The variable for day section neither showed any significant effect in the performed

regression analysis at the 5% significance level (p=1.000, see appendix D4), which confirmed the results of the Mann-Whitney U test.

Treatment

But what about the introduced treatments? Although no hypothesis is formed regarding the possible influence of the congruent and incongruent treatments, it has been analyzed whether this is the case. The results may not surprise: no significant effect of treatment was found using the 5% significance level. The specific tests performed are elaborated on briefly.

The spread between all participants in the congruent and in the incongruent treatment was compared at first using a Mann-Whitney U test. This resulted in a p-value of 0.379, indicating that the null hypothesis was not rejected at the 5% level (n=109, M=-0.02, SD=0.988 for the congruent group, n=69, M=0.07, SD=1.155 for the incongruent group). It could however be that treatment had an effect if we look at the two data collection methods separately. The first test that was performed compared the spread of the congruent and the incongruent treatment for subjects that participated on pen and paper. The Mann-Whitney U test indicated a p-value of 0.166 (n=54, M=0.000, SD=0.869 for the congruent group and n=35, M=-0.06, SD=1.162 for the congruent group), which resulted in the fact that the null hypothesis is not rejected at the 5% level. Following this test, the spread of the congruent and incongruent treatment was compared for subjects participating on laptop (n=55, M=-0.05, SD=0.951 for congruent group and n=34, M=0.21, SD=1.149 for the incongruent group). The null hypothesis was not rejected at the 5% level with a p-value of 0.859 for the Mann-Whitney U test. Resulting from the findings, we can conclude that there was no significant difference between treatments when looking at the two different modes of collection. Furthermore, comparison of the entire congruent group with the incongruent group neither showed significant differences.

Gender

Although a small portion of the studies on the free-choice paradigm has found significant differences in gender (Heine & Lehmann, 1997; Kitayama et al., 2004), the majority of prior research with respect to cognitive dissonance and choice-induced preference change have not revealed significant differences between males and females (Soutar & Sweeny, 2003; Elliot & Devine, 1994; Imada & Kitayama, 2010; Kimel et al., 2012). Most of the time, gender was not addressed at all (see appendix A for an overview). The present study found a near

significant effect of gender in the performed regression (p=0.062, see appendix D4), which could embark some curiosity regarding the role of gender. Additional tests were performed to further address the possible effect of gender. The same methods were employed as for hypothesis two and three. An overview of the results can be found in appendix E2, including the parametric tests used to analyze the effects of gender.

The spread of the separate groups of males and females were compared to zero at first and subsequently compared to each other. The WSR tests result in p-values of 0.142 for the spread of males (n=91, M=-0.14, SD=0.839) and 0.124 for the spread of females (n=87, M=0.17, SD=1.158). These results indicated that the spread of both genders does not significantly differ from zero at the 5% significance level. However, if we compare both groups, a glance at the mean spread might already imply a difference, where the mean spread of females was 0.17 and the mean spread for males was -0.14 respectively. The Mann-Whitney U test comparing the distribution of spread among genders produced a p-value of 0.030, indicating that the null hypothesis assuming equal distribution was rejected at the 5% significance level. These results implied that there was a significant difference in the spreading of alternatives between genders, contradicting the findings in the majority of the literature. Based on these findings and the estimated regression, it could also be interesting to separately study the entire group of females. The results of this analysis are separately presented in appendix F.

In addition to the gender effects, the estimated regression implies that the variable indicating whether a participant is part of the ESE is not found to have a significant effect on the spreading of alternatives (p=0.842, see appendix D4). This finding was supported by a Mann-Whitney U test comparing distribution of spread across categories of students. The test produced a p-value of 0.290, indicating that the null hypothesis of an equal distribution of spread across ESE and non-ESE students was not rejected at the 5% significance level (see appendix E3 for all the performed tests specifically performed for analyzing the variable ESE).

Discussion, implications and limitations

Following the theory of Festinger (1957), choosing between two alternatives induces conflicting thoughts on these objects which causes a psychological tension called dissonance. In order to reduce this dissonance, it is argued that the objects are re-evaluated in order to restore the consistency between actions and thoughts, inducing a change in preference. This phenomenon has been established in the free-choice paradigm (see appendix A for an overview of studies), but argued to inherently possess some methodological flaws (Chen, 2008, Chen and Risen, 2010a, b, Izuma & Murayama, 2013). The design by Alós-Ferrer et al. (2012), called the implicit-choice paradigm, partially copes for the brought up criticism, since the design fixed the selection bias issues present in the classic free-choice paradigm. The authors still found significant spreading of alternatives when using the implicit-choice paradigm. Especially the robustness check performed by these researchers is appointed as a method to adequately study choice-induced preference change (Izuma & Murayama, 2013). This design was therefore used in the current study, but somewhat adapted to cope for other points of criticism, such as the reflection of real preferences by using stated preferences. The current study attempted to mitigate this specific point of criticism by making the experiment fully incentive compatible.

Discussion and conclusion

Before answering the main research question, the empirical findings of the current study are briefly discussed to formulate a complete answer. The most surprising result arguably arose from the results regarding the first hypothesis. The first hypothesis entailed the establishment of choice-induced preference change as a whole, expected to be present based on the prevailing literature. However, no evidence was found for choice-induced preference change; not when looking at the entire sample nor when looking at any of the specific groups. This finding may provide some food for discussion for further research.

One may argue that no choice-induced preference change was established since the experiment was incentive compatible. Thus, participants faced the consequences of their choices and rankings in contrast to previous research, which lead to better thought through decisions. However, the simplicity of the task could have also played a role. Even though the participants were informed that the task at hand was not a memory task, it could be that the second ranking was still based solely on memory. Additionally, it could also very well be that

preferences for candy bars are more stable in contrast to the products often used in the free-choice paradigm, such as CDs.

The second research hypothesis which looked for differences in spread for two data collection methods was neither rejected. The biggest difference between these two data methods was whether the candy bars were physically present or that the subjects were presented with pictures of the candy bars. Although Bushong et al. (2010) found a significant difference in valuation between pictures and physical products, this is found to be a constant markup of rating, which is impossible for ranking alternatives. It was however argued that physical presence might induce more cognitive dissonance as opposed to pictures of products, but no significant effects were established.

The additional hypothesis revolved around the so-called 'lunch-dip' effect: after lunch, a drop in attention and arousal has been found to have a negative influence on cognitive tasks (Monk & Leng, 1986; Craig & Condon, 1984; Smith & Miles, 1986). Even though these effects were argued to be individual specific (Carrier & Monk, 2000), one could imagine having different attitudes towards candy bars before and after lunch. However, the results indicate that there is no significant difference in spreading of alternatives between individuals approached before and after lunch.

Lastly, some complementary analysis is performed regarding the two introduced treatments and gender. In one of the treatments the individual was expected to make decisions coherent with one's beliefs, whereas in the other treatment the expectation was that the choice contradicted the individual's beliefs. Since the cognitive dissonance theory arguably revolves around the consistency of choices, it was argued that the incongruent treatment may have produced higher spreading of alternatives as opposed to the congruent treatment. The results show that this line of argumentation was not supported, as no significant differences in spreading of alternatives between the two treatments were found.

In most of the studies, gender has not been addressed at all (see appendix A), and only two of the studies on choice-induced preference change found gender effects (Kitayama et al., 2004; Heine & Lehmann, 1997). More specifically, Heine & Lehmann (1997) found that Japanese women negatively correlated with spread, which was not really applicable to the current study. Kitayama et al. (2004) find that males produce a significant higher spread than females, but do not address this result further since it was not found in other studies using the free-choice paradigm. Although the current study did not find a significant effect of any of

the two genders per se, it did find that the distribution of spread across genders significantly differs. By looking at the mean spread of both groups, 0.17 for females and -0.14 for males, one could already see that the spread for females was higher compared to males. Interesting to see was that the spread of males is negative, arguably having an opposite expected cognitive dissonance effect. This is speculation however, since the mean spread of males was not significantly different from zero. Furthermore, since gender was either ignored or if effects were found these are not addressed, these findings provide very little implications for this field of study. This is especially the case since the studies that do find significant differences indicate that females negatively correlate with spread (Heineman & Lehmann, 1997) or that males produce significant higher spread than females (Kitayama et al., 2004), contradicting the findings of the present study.

One major question is left open for debate since the implications based on the hypotheses and extra analysis have been addressed: the research question. For clarification purposes it is repeated:

Does the phenomenon of choice-induced preference change holds when real incentives are introduced?

Strictly speaking, if one looked at the results of the present study, formulating an answer on the research question seems quite obvious. Choice-induced preference change was not established in the current study. Thus, a bold conclusion would be that the phenomenon therefore was not found to hold when real incentives were introduced. Although the implicit-choice paradigm was appointed as a method to study choice-induced preference change adequately (Izuma & Murayama, 2013), this paradigm has not received much attention in the literature. The main evidence is from the paper by Alós-Ferrer et al. (2012), in which this method was introduced. Although there is plenty of evidence on choice-induced preference change from the free-choice-paradigm, the methodology was found to be flawed. In other words, if evidence of the implicit-choice paradigm to the classic paradigm is compared, we cannot draw any conclusions with regard to effects of real incentives in this field of research. Therefore, the only comparison to make is that with the results of the founders of the implicit-choice paradigm.

But what if we do make this direct comparison to the study of Alós-Ferrer et al. (2012)? The compliance rate in the current study was higher, as only 12 out of the 178 individuals violated their stated preferences with observed choices. This indicates that in the current study, the

implicit-choice paradigm without forced compliance would approximate the paradigm with forced compliance, an effect that could be allocated to the use of real incentives and incentive compatibility mechanisms. Furthermore, the observed spread was not significantly different from zero, contradicting the findings of Alós-Ferret et al. (2012). One could argue that the finding of the present study is that real incentives diminish choice-induced preference change in the implicit-choice paradigm. It is unable to draw conclusions on real incentives with regard to the free-choice paradigm, since this was not tested in the present study. Lastly, it is to note that the conclusion drawn only holds for the ranking variant of the paradigm, since ratings were not addressed.

Limitations

The limitations of the present research were especially related to the data collection procedure. One of the aspects that has been studied previously in the field of choice-induced preference change is the effect of culture, which was found to have a certain effect on the spreading of alternatives (Hoshino-Browne et al., 2005; Kitayama et al., 2004; Heine & Lehmann, 1997; Imada & Kitayama, 2010). The current study however did not address nationality in the demographic questions, which could have provided extra information. In addition, with regard to the possibility of international students participating in the experiment, a pretest indicating the familiarity of the candy bars would have been useful. Instead of relying on the assumption that the candy bars are well known, it could have been empirically validated. Hence, it may have been that several participants were unfamiliar with one or two candy bars, which is especially harmful since there were only six candy bars used.

Another limitation regards the possibility of order effects that were introduced due to the order of the choice pairs for laptop participants. Even though no order effects were present, this is something that could have easily been accounted for by the experimenter in the first place. Also with regard to data collection, the randomization procedures used (e.g. coin flip for the pen and paper version and the randomization feature in Qualtrics for the laptop version) produced a skewed distribution among treatments. Unfortunately, the procedures used led to 109 participants in the congruent treatment and 69 participants in the incongruent treatment. Even though no significant effects of both treatments were found, an even distribution among treatments was preferred.

The last limitation that is discussed is related to the use of real incentives. The current study was fully incentivized, e.g. every single participant actually received one of the candy bars

based on the choices made in the experiment. However, since the implicit-choice paradigm is not as well established as the free-choice paradigm, there is not a lot of literature on this particular subject. In other words, the absence of choice-induced preference change could be related to other factors present in the current study besides providing the participants with real incentives. This problem could have been mitigated by performing the exact same experiment without the use of real incentives. A comparison of the incentivized experiment and non-incentivized experiment could have been very useful in terms of investigating the effects of real incentives. As mentioned, it was very hard to draw solid conclusions with regard to the use of real incentives in this particular paradigm since there is very little to no research to compare it with.

Recommendations for future research

As far as the findings of the present research are concerned, the most important recommendation for future research relates very much to the last limitation of the present research. In order to fully grasp and adequately establish the effect of real incentives in the implicit-choice paradigm, it would be helpful to perform two experiments that are exactly alike, fully incentivizing one experiment but keeping the other hypothetical. The fact that no significant preference change was observed in the current study might be a strong implication for the possible effects of real incentives, but also adds speculation to the debate on choice-induced preference change. Moreover, gender effects found in the current study are the exact opposite of previous gender effects found, if gender effects were found or addressed at all. Does this relate to real incentives? Obviously, this question cannot be answered by solely looking at the present research, but is something that can be elaborated on in experiments as described in the previous paragraph.

Even though cognitive dissonance is a very popular research topic in social psychology, it appears that the debate on how to adequately research this phenomenon is not yet resolved. Especially in relation to choice-induced preference change, several methods have been offered in the literature, but there seems to be no final solution on which of these methods is the most appropriate to use. Although the method used in the current study is appointed as one that can truly elicit choice-induced preference change (Izuma & Murayama, 2013), it feels as if the debate on methodology has not reached a conclusion yet. The most important step to make for future research regarding choice-induced preference change is to reach a consensus among the majority of the researchers on this topic, which would help to adequately study the phenomenon.

Appendix

Appendix A: Overview of studies

Study	Gender addressed?	Fixed show-up fee?	Classical FCP established	Ranking task	Participants
Chen and Risen (2010a)	No	Yes + Incentivized choice	Yes, although argued that the methodology is flawed	Yes	Students
Heine & Lehmann (1997)	Yes. Japanese females negatively correlated with spread	Deceived that choice was incentivized but received fixed fee at the end	Yes, but not for all studied cultural groups	Yes, but this was only used to determine the choice pairs	Individuals with age ranging from 18-30
Gerard & White, 1983	No	Deceived that choice was incentivized but received fixed fee at the end	Yes	Yes, this task was used to determine the spread	Students
Imada & Kitayama, 2010	Yes but no gender effect found	Yes	Yes, but not for all studied cultural groups	Yes, this task was used to determine the spread	Students
Kitayama et al., 2013	No	Deceived that choice was incentivized but received fixed fee at the end	Yes	No	Students
Alós-Ferrer et al., 2012	No	No	Yes, also the implicit-choice paradigm established	Yes	Students
Sharot et al., 2009	No	No payment	Yes	No	Unspecified
Sharot et al., 2010a	No	Yes	Yes	No	Individuals with age ranging from 18-40

Sharot et al., 2010b	No	Yes	Not researched, effects of blind-choice on preferences established	No	Individuals with age ranging from 18-31
Izuma et al., 2010	No	Not specified	Yes	No	Individuals with age ranging from 18-24
Brehm (1956)	No	Deceived that choice was incentivized. Not specified if participants received any fee	Yes	No	Students
Vroom (1966)	No	Not specified	Yes	Yes, but rating was used to detect spread	Students
Lieberman et al., 2001	No	Study 1: No Study 2: Fixed fee	Yes	Yes	Study 1: Amnesiacs ages 45-75 Study 2: students
Lee & Schwarz, 2010	No	Deceived that choice was incentivized. Not specified if participants received any fee	Yes	Yes	Students
Kimel et al., 2012	Yes, but no effects found	Deceived that choice was incentivized but received fixed fee at the end	Yes	Yes	Students
Hoshino-Browne et al., 2005	No	Course credit or a fixed-fee	Yes, but different effects found for different cultural groups studied	Yes	Students

Kitayama et al., 2004	Yes, males in study 2 showed a significant greater spread of alternatives than females	Fixed fee	Yes, but different effects found for different cultural groups studied	Yes, the experiments were also conducted with rating data but showed weaker results	Students
Jarcho et al., 2011	No	Fixed fee	Yes	No	Mean age 22 with a standard deviation of 3.42
Qin et al., 2011	No	Fixed fee	Yes	Yes	Students
Coppin et al., 2010	No	Fixed fee	Yes	No	Students

Appendix B: Experiment-related documents

Appendix B1: Instruction forms

Instruction form presented to the subjects that participated on pen and paper at the start of the experiment.

Instruction form pen and paper version

Dear participant,

Thank you for participating in this experiment. Your task today is to evaluate the desirability of several candy bars. This is not a memory task. There are no right or wrong answers for these particular tasks. When asked to evaluate the candy bars, simply think about how much you like them at that moment. It is in your own interest to answer honestly as you will receive one of the candy bars today. Which bar you receive will partially depend on your answers. The more honest your answers, the higher the chances that you get what you prefer.

During this experiment, you will be asked to:

1. Flip a coin

2. Rank six candy bars from most liked to least liked

3. Provide an answer to a mathematical question

4. Choose among two pairs of candy bars

5. Once more rank the six candy bars from most to least liked

After these tasks have been completed, your reward will be determined. If you do not understand the procedure, please let me know. The data that is collected in this experiment is for scientific purposes only and your privacy is guaranteed. If you have finished reading, please hand back the instruction form.

Instruction form laptop version

Dear participant,

Thank you for participating in this experiment. Your task today is to evaluate the desirability of several candy bars. This is not a memory task. There are no right or wrong answers for these particular tasks. When asked to evaluate the candy bars, simply think about how much you like them at that moment. It is in your own interest to answer honestly as you will receive one of the candy bars today. Which bar you receive will partially depend on your answers. The more honest your answers, the higher the chances that you get what you prefer.

During this experiment, you will be asked to:

1. Rank six candy bars from most liked to least liked

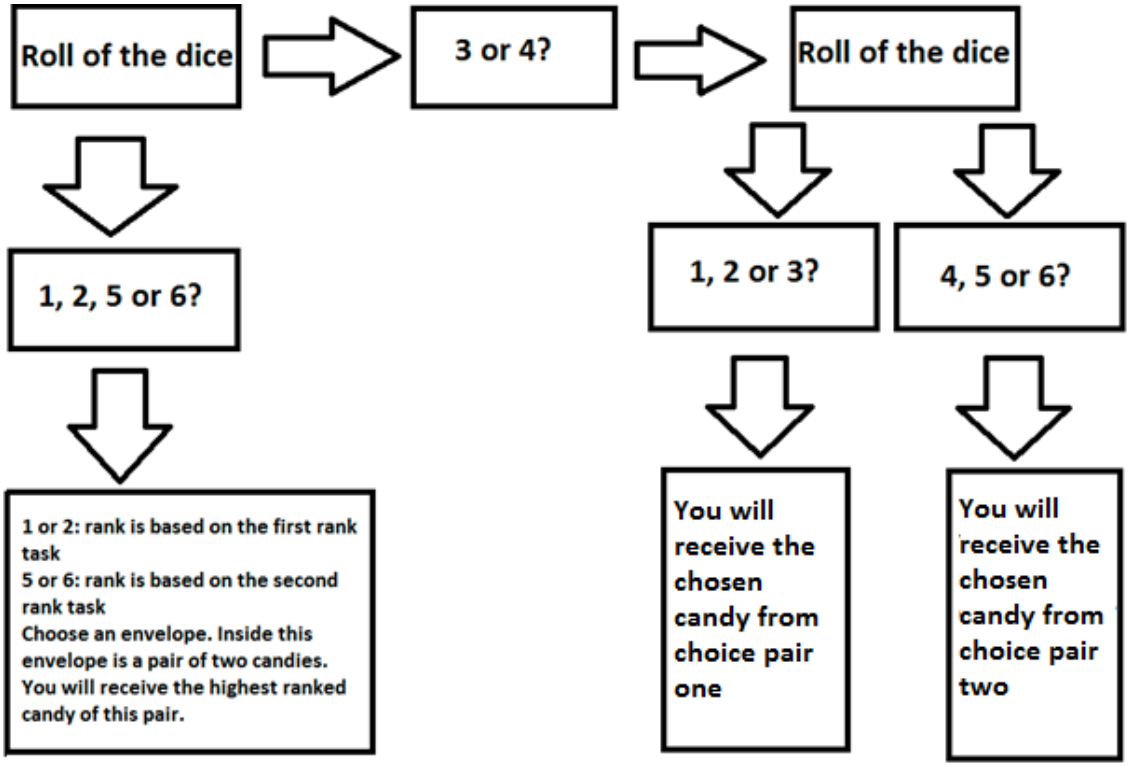
2. Provide an answer to a mathematical question

3. Choose among two pairs of candy bars

4. Once more rank the six candy bars from most to least liked

After these tasks have been completed, your reward will be determined. If you do not understand the procedure, please let me know. The data that is collected in this experiment is for scientific purposes only and your privacy is guaranteed. If you have finished reading, please hand back the instruction form.

Appendix B2: Payment schedule



To note is that for outcomes 1, 2, 5 and 6 there had been some envelopes prepared. Every possible pair of two candy bars had been printed on one page and were put into separate envelopes.

Appendix B3: Surveys

Survey pen/paper format

Each of the questions, except for question one and two, were presented on a separate sheet of paper.

1) **Please flip a coin and write down the outcome: _____**

2) **Please rank the six candy bars from most liked (rank 1) to least liked (rank 6) and write them down below.**

Rank 1:

Rank 2:

Rank 3:

Rank 4:

Rank 5:

Rank 6:

Please hand in this form to the instructor.

3) **Please provide an answer to the mathematical question**

342 + 519 + 182 = _____

Please hand in this form to the instructor.

4) **Imagine that you are faced with a choice between these two candy bars. Please indicate which candy bar you would choose and the one that you would not choose.**

Choice pair one:

Chosen candy bar: _____ Unchosen candy bar: _____

Choice pair two:

Chosen candy bar: _____ Unchosen candy bar: _____

Please hand in this form to the instructor.

5) **Please re-rank the six candy bars from most liked (rank 1) to least liked (rank 6). Remember, this is not a memory task. Simply think about how much you like each candy bar right now. There are no right or wrong answers here.**

Rank 1:

Rank 2:

Rank 3:

Rank 4:

Rank 5:

Rank 6:

Please hand in this form to the instructor.

What is your age?

_____

What is your gender?

- **Male**
- **Female**

Is your study faculty the Erasmus School of Economics (ESE)?

- **Yes**
- **No**

Please hand in this form to the instructor. Thank you for participating in this experiment, your payout will now be determined.

Survey laptop format

Each of the questions were presented on a separate page. The question descriptions (e.g. “First ranking task” and “Mathematical question”) were not included in the questionnaire.

1) First ranking task

Please rank the six candy bars from most liked (rank 1) to least liked (rank 6)

>>

2) Mathematical question

Please provide an answer to the following mathematical question:

342 + 519 + 182 =

>>

3) First choice pair. Participant could drag the candy bars to the chosen and unchosen box.

Imagine that you are faced with a choice between these two candy bars. Please indicate which candy bar you would choose and the one that you would not choose.

	I choose this candy bar	I do not choose this candy bar
		
		

>>

3) Second choice pair. In this picture, the candy bars have already been dragged into the chosen/unchosen box.

Imagine that you are faced with a choice between these two candy bars. Please indicate which candy bar you would choose and the one that you would not choose.

I choose this candy bar	I do not choose this candy bar
	

>>

4) Second ranking task

Please re-rank the six candy bars from most liked (rank 1) to least liked (rank 6). Remember, this is not a memory task. Simply think about how much you like each candy bar right now. There are no right or wrong answers.

5) Demographic questions

What is your age?

What is your gender?

Male

Female

Is your study faculty the Erasmus School of Economics (ESE)?

Yes

No

>>

Appendix B4: Experimental schedule

Day	Total amount of participants	Laptop	Pen/paper
Tuesday 1	30	18 (morning)	12 (afternoon)
Wednesday 1	46	23 (afternoon)	23 (morning)
Thursday 1	38	16 (morning)	22 (afternoon)
Tuesday 2	41	25 (afternoon)	16 (morning)
Wednesday 2	23	7 (afternoon)	16 (morning)

Appendix C: Statistical assumptions

Appendix C1: Assumptions for the applied statistical tests

As mentioned in the main text, several assumptions need to be met to produce unbiased results with the parametric tests that could be used to analyze the data in the present study. These are the following six assumptions, equal for both the t-test and the (Welch's) ANOVA. For nonparametric tests, complying to the first three assumptions is sufficient to produce unbiased results.

1. The independent variable is measured on a continuous scale, meaning that it is measured at the interval or ratio level
2. The independent variable consists of two categorical, independent groups
3. All of the data points are subject to independence of observations. This means that each data point is independent of other data points, e.g. there is no relationship within or between the groups that are analyzed.
4. The dependent variable should approximate a normal distribution for every group that is compared.
5. No significant outliers should be present
6. The tests require homogeneity of variances. This assumption is not necessary for the Welch's ANOVA and is therefore preferred over the regular ANOVA if this assumption is not met for a particular sample.

Assumptions 1, 2 and 3 can be checked without the use of any statistical tests. The dependent variable in this study, spread of alternatives, is a continuous variable measured at the ratio level since a value of zero indicates that there is no spread present. This indicates that the first assumption is met. The second assumption is also met, since in the present study spread is compared between two independent groups (e.g. spread between participants in the morning and in the afternoon). Lastly, the third assumption requiring independence of observations is also accounted for, since every subject participated individually in the experiment without having contact with other participants. The assumption of normality will be tested formally.

The first assumption that is formally tested is the fourth one, which requires an approximation of a normal distribution for every group that is compared. A Shapiro-Wilk test for normality is performed for every group that is tested in the present study. The results can be found in the below, indicating that for every group we reject the null hypothesis of a normal distribution at the 1% significance level (p=0.000). This already implies that the data is not suited to be analyzed with parametric tests, thus nonparametric tests will serve as the main statistical methods employed.

Group	Test statistic	p-value
Entire sample	0.779	0.000
Congruent	0.681	0.000
Incongruent	0.811	0.000
Laptop	0.764	0.000
Pen/paper	0.766	0.000

Congruent laptop	0.629	0.000
Congruent pen/paper	0.717	0.000
Incongruent laptop	0.812	0.000
Incongruent pen/paper	0.766	0.000
Male	0.747	0.000
Female	0.779	0.000
Morning	0.677	0.000
Afternoon	0.849	0.000

Appendix C2: Test for heteroskedasticity

Test	Null hypothesis	P-value
Breusch-Pagan test for heteroskedasticity	There is constant variance	0.2832

Appendix C3: Testing for order effects

Group	n	p-value MWU test	Mean spreads*	Standard deviations
Equal choice task order vs unequal order	89	0.686	0.067 0.022	0.939 1.131

*the first named group in the 'group' tab resembles the first mean spread and standard deviation

Appendix D: Corresponding values for the main hypotheses

Appendix D1: Corresponding values for the first hypothesis

Group	n	p-value WSR test	Mean spread	Standard deviation	p-value t-test
Entire sample	178	0.956	0.01	1.008	0.882
Laptop	89	0.610	0.04	1.033	0.682
Pen/paper	89	0.641	-0.02	0.988	0.831
Congruent	109	0.540	-0.03	0.907	0.752
Incongruent	69	0.354	0.07	1.155	0.604
Laptop congruent	55	0.700	-0.05	0.951	0.672
Laptop incongruent	34	0.156	0.21	1.149	0.304
Pen/paper congruent	54	0.709	0.00	0.869	1.00
Pen/paper incongruent	35	0.775	-0.06	1.162	0.773

Appendix D2: Corresponding values for the second hypothesis

Hypothesis 2					
Group	**n**	**p-value MWU test**	**Mean spreads***	**Standard deviations**	**ANOVA**
Laptop vs pen/paper	178	0.434	0.04 -0.02	1.033 0.988	0.657
Congruent, laptop vs pen/paper	109	0.914	-0.05 0.00	0.951 0.869	0.755
Incongruent, laptop vs pen/paper	69	0.290	0.21 -0.06	1,149 1.1262	0.348

*the first named group in the 'group' tab resembles the first mean spread and standard deviation

Appendix D3: Corresponding values for the third hypothesis

Hypothesis 3					
Group	**n**	**p-value MWU test**	**Mean spreads***	**Standard deviation**	**(Welch's) ANOVA**
Congruent vs incongruent	178	0.379	-0.03 0.07	0.907 1.008	0.543**
Laptop, congruent vs incongruent	89	0.859	-0.05 0.21	0.951 1.149	0.272**
Pen/paper, congruent vs incongruent	89	0.166	0.00 -0.06	0.869 1.162	0.792

*the first named group in the 'group' tab resembles the first mean spread and standard deviation

**groups for which the Welch's ANOVA is used instead of the normal ANOVA are indicated with a **

Appendix D4: Estimated regression

The results of the estimated regression. Treatments and other variables are specified as follows:

1 = laptop incongruent
2 = laptop congruent
3 = pen and paper incongruent
4 = pen and paper congruent
Gender = 0 if male, 1 if female
ESE = 0 if non-ESE, 1 if ESE
Daysection = 0 if morning, 1 if afternoon

Variable	**Coefficient**	**Standard Error**	**p-value**
Daysection	0.00003	0.161	1.000
Age	0.016	0.024	0.513
Gender	0.301	0.160	0.062
ESE	-0.033	0.166	0.842
Treatment			
2	-0.247	0.224	0.271
3	-0.212	0.252	0.402
4	-0.202	0.230	0.381
Constant	-0.283	0.562	0.614

Appendix D5: Power calculations

To note is that for all calculations, the conventional medium effect size of 0.5 and an alpha of 0.05 has been applied. In addition, only the statistical power of the nonparametric tests have been calculated since these form the core of the present study. All of the performed power tests have been done using the G*Power software version 3.1.9.2 (Faul et al., 2007).

Hypothesis 1:

Group	**n**	Statistical power
Entire sample	178	0.999
Laptop	89	0.995
Pen/paper	89	0.995
Congruent	109	0.999
Incongruent	69	0.979
Laptop congruent	55	0.945
Laptop incongruent	34	0.788
Pen/paper congruent	54	0.940
Pen/paper incongruent	35	0.801

Hypothesis 2 and 3:

Groups	**n first group**	**n second group**	**Statistical power**
Laptop vs pen/paper	89	89	0.900
Congruent, laptop vs pen/paper	55	54	0.714
Incongruent, laptop vs pen/paper	34	35	0.515
Congruent vs incongruent	109	69	0.884
Laptop, congruent vs incongruent	55	34	0.600
Pen/paper, congruent vs incongruent	54	35	0.605
Morning vs afternoon	89	89	0.900

Gender and ESE tests.

First the statistical power of the WSR tests will be provided, followed by the statistical power of the Mann-Whitney U tests.

Group	n	Statistical power
Male	91	0.996
Female	87	0.995
ESE	81	0.991
Non-ESE	97	0.997

Groups	n first group	n second group	Statistical power
Male vs female	91	87	0.900
ESE vs Non-ESE	81	97	0.898

Appendix E: Additional results

Appendix E1: Results for hypothesis 3

Group	n	p-value WSR test	Mean spread	Standard deviation	p-value t-test
Morning	89	0.986	0.00	0.839	1.000
Afternoon	89	0.932	0.02	1.158	0.855

Group	n	p-value MWU test	Mean spreads*	Standard deviation	Welch's ANOVA
Morning vs afternoon	178	0.786	0.00 0.02	0.839 1.158	0.882

*the first named group in the 'group' tab resembles the first mean spread and standard deviation

Appendix E2: Results for gender analysis

Group	n	p-value WSR test	Mean spread	Standard deviation	p-value t-test
Males	**91**	0.142	-0.14	0.839	0.193
Females	**87**	0.124	0.17	1.158	0.096

Hypothesis 5					
Group	**n**	**p-value MWU test**	**Mean spreads***	**Standard deviation**	**ANOVA**
Males vs females	178	0.030	-0.14 0.17	1.039 0.955	0.037

*the first named group in the 'group' tab resembles the first mean spread and standard deviation

Regression including an interaction variable between gender and ESE. Treatments and other variables are specified as follows:

1 = laptop incongruent
2 = laptop congruent
3 = pen and paper incongruent
4 = pen and paper congruent
Gender = 0 if male, 1 if female
ESE = 0 if non-ESE, 1 if ESE
Day section = 0 if morning, 1 if afternoon

Variable	**Coefficient**	**Standard Error**	**p-value**
Day section	-0.002	0.161	0.989
Age	0.015	0.024	0.533
Gender	0.352	0.214	0.102
ESE	0.021	0.224	0.925
Treatment			
2	-0.249	0.225	0.269
3	-0.216	0.253	0.395
4	-0.198	0.231	0.393
Gender*ESE			
Gender = 1, ESE = 1	-0.118	0.325	0.718
Constant	-0.300	0.565	0.596

Appendix E3: ESE tests

Group	n	p-value WSR test	Mean spread	Standard deviation	p-value t-test
ESE students	81	0.428	-0.07	1.070	0.535
Non-ESE students	97	0.408	0.08	0.954	0.397

Group	n	p-value MWU test	Mean spreads*	Standard deviation	ANOVA
ESE vs Non-ESE	178	0.290	-0.07 0.08	1.070 0.954	0.304

Appendix F: Separate female analysis

Since the female group of 87 subjects is furthermore split up into separate groups, the assumptions that need to be met for parametric tests are very unlikely to be met. Formal Shapiro-Wilk tests for normality indicate that for all the groups, the null hypothesis of a normal distribution is rejected at the 5% significance level (highest p-value 0.011). Therefore, the female group has been analyzed solely with the use of nonparametric tests. First, it is once more tested whether any order effects were present following from the order of choice pairs in the laptop task as opposed to the pen/paper task.

Test	Null hypothesis	p-value
Mann-Whitney U Test	The distribution of spread is equal across individuals with an equal order and those with an unequal order	0.445

We do not reject the null hypothesis that the distribution of spread is equal among participants having an equal order and those who did not at the 5% significance level. Thus, it is concluded that there are no significant order effects present.

The main text already entailed the comparison of the female group to zero, where the WSR test produced a p-value of 0.124, indicating that the spread of the entire group of females did not significantly differ from zero at the 5% significance level. In addition, the same methods and tests are applied as those in the main analysis to answer the last three hypotheses for the separate female group. The results are presented in the tables below, where the first table compared the spread of the tested group to zero using the Wilcoxon Signed Rank test. The tables on the next page are used to formulate an answer on the second, third and fourth hypotheses using Mann-Whitney U tests to do so.

Statistical hypothesis 1: The median of the distribution of the negative and positive ranks of spread is equal to zero
Tests in the table below are Wilcoxon Signed Rank tests. The mean spread and standard deviation for the groups are also provided.

Group	n	p-value WSR test	Mean spread	SD
Entire sample	87	0.124	0.17	0.955
Laptop	45	0.055	0.29	0.944
Pen/paper	42	0.821	0.05	0.962
Congruent	56	0.394	0.13	0.810
Incongruent	31	0.123	0.26	1.182
Laptop congruent	27	0.187	0.19	0.681
Laptop incongruent	18	0.092	0.44	1.247
Pen/paper congruent	29	0.855	0.07	0.923

Pen/paper incongruent	13	0.890	0.00	1.080
Morning	44	0.183	0.18	0.870
Afternoon	43	0.366	0.16	1.045

The p-values in the table above indicate that for none of the tested groups of females the spread is significantly different from zero at the 5% significance level. Hence, the first hypothesis when only the female participants are analyzed is rejected.

Statistical hypothesis 2: The distribution of spread is equal among the different modes of data collection. Tests in the table below are Mann-Whitney U tests. The mean spread and standard deviation for both compared groups are also provided.

Group tested	**P-value**	**Mean spread laptop participants**	**SD laptop participants**	**Mean spread pen/paper participants**	**SD pen/paper participants**
The entire group of females	0.274	0.29	0.944	0.05	0.962
Females that participated in the incongruent treatment	0.373	0.44	1.247	0.00	1.080
Females that participated in the congruent treatment	0.635	0.19	0.681	0.07	0.923

Statistical hypothesis 3: The distribution of spread is equal among the two treatments. Tests in the table below are Mann-Whitney U tests. The mean spread and standard deviation for both compared groups are also provided.

Group tested	**P-value**	**Mean spread congruent participants**	**SD congruent participants**	**Mean spread incongruent participants**	**SD incongruent participants**
The entire group of females	0.967	0.13	0.810	0.26	1.182
Females that participated on laptop	0.635	0.19	0.681	0.44	1.247
Females that participated on pen and paper	0.495	0.07	0.923	0.000	1.080

Statistical hypothesis 4: The distribution of spread is equal among the two day sections. Tests in the table below are Mann-Whitney U tests. The mean spread and standard deviation for both compared groups are also provided.

Group tested	P-value	Mean spread morning participants	SD morning participants	Mean spread afternoon participants	SD afternoon participants
The entire group of females	0.960	0.181	0.870	0.163	1.045

The results from the conducted Mann-Whitney U tests conclude that for the tested groups, the distribution of spread did not significantly differ between the indicated categories at the 5% significance level. Hence, for the separate female analysis the same conclusions hold as in the main text with regard to hypothesis one, two, three and four.

The tests for independence conducted on the separate female participants produced the following p-values:

Variables tested for independence	p-value Chi-Square test
Four introduced treatments and ESE	0.146

*p-values with an asterisk indicate p-values obtained from a Fischer's Exact test

These results imply that the null hypothesis assuming independence is not rejected at the 5% significance level.

Variables tested for independence	p-value Kruskal-Wallis test
Age and ESE	0.146
Age and the four introduced treatments	0.540

These results indicate that the null hypothesis of independence is not rejected at the 5% significance level for the tested variables.

Appendix G: Chi-Square and Kruskal-Wallis tests for independence

Chi-Square or Fischer's Exact tests

Variables tested for independence	p-value Chi-Square test
Four introduced treatments and gender	0.447
Four introduced treatments and ESE	0.123
Gender and ESE	0.000*

*p-values with an asterisk indicate p-values obtained from a Fischer's Exact test

Kruskal-Wallis tests

Variables tested for independence	p-value Kruskal-Wallis test
Age and gender	0.918
Age and ESE	0.230
Age and the four specified treatments	0.694

Amount of observations ESE and gender:

	Female	Male
ESE	26	55
Non-ESE	61	36

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