ERASMUS UNIVERSITY ROTTERDAM
Erasmus School of Economics

Master Thesis Behavioral Economics

Can Price Promotions Alter Preferences?

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Date final version: 15th January 2022

Acknowledgments

Throughout the writing of my thesis, I have received a great deal of support from several people. First, I would like to thank my supervisor Professor Georg Granic for his valuable guidance. Your advice and critical feedback greatly encouraged me to bring my thesis to a higher level. I am truly thankful that you have been patient with me and that you took the time to answer all my questions. Furthermore, I want to thank my family, friends, and colleagues who have supported and encouraged me during my academic journey and especially during the writing of my thesis.

Abstract

Psychologists find that we do not only choose based on our preferences, we also change our preferences based on what we choose. Thus far, the experimental evidence of such *choice-induced preference change* has not met the requirements of a valid economic experiment. A recent experiment by Alós-Ferrer & Granic (2018), which met those requirements, found no consistent *choice-induced preference change*. The lack of evidence in Alós-Ferrer & Granic's (2018) economic experiment can be attributed to two methodological shortcomings. I address these shortcomings in an alternative experiment that uses price promotions to direct choices. The results show that as participants choose the promoted products, their preference for those products increases. Although this suggests evidence of a *choice-induced preference change*, replication of my experiment with slight alterations is required to confirm my findings.

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Can Price Promotions Alter Preferences?

Imagine yourself in a cafe choosing whether to order a Tonic or a Coke. You are indifferent between the two. But since you have to choose, you pick the Tonic. Psychologists find that after this choice, you will subconsciously start favoring the Tonic more and the Coke less. In other words, your choices guide your preferences. This phenomenon is called a *choice-induced preference change* and has been observed by psychologists in a simple rating-choose-rerating experiment called the *free-choice paradigm* (Brehm, 1956). The observation is as follows. First, a participant rates several drinks and, for example, gives Tonic and Coke the same rating. Then, the experimenter offers the choice between the similarly rated Tonic and Coke, after which the participant chooses the Tonic. After this choice, the participant is asked to rerate all drinks. Psychologists find that participants give the chosen Tonic a higher rating than the rejected Coke. The occurrence of a *choice-induced preference change* challenges the assumption of stable preferences, an important assumption in economics. Yet, economists have, thus far, used three arguments to discredit the phenomenon. First, the *free-choice paradigm* contains a selection bias, because, briefly explained, the choice itself is determined by preference. Second, choices in psychological experiments were often presented as hypotheticals and therefore unincentivized, thus, lacking relevance to economics, a field in which choices are required to have real consequences. Third, a recent experiment by Alós-Ferrer & Granic (2018), which addresses the two aforementioned shortcomings, found no evidence of a *choice-induced preference change*.

Still, the researchers state that absence of evidence is not evidence of absence. The subject must be reexamined because Alós-Ferrer & Granic (2018) disregarded the theories that explain the occurrence of a *choice-induced preference change*. This accounts for the absence of a consistent

effect found by the researchers. Therefore, I reinvestigate the research question that was central in Alós-Ferrer & Granic (2018); *Can a choice-induced preference change be observed in economics?*

To answer this question while addressing my critique on Alós-Ferrer & Granic (2018), I present a novel experimental design. My design is economically relevant as it uses incentivized choices on real retail products, and it avoids selection bias by randomly assigning choices. Essentially, I present the equally preferred Tonic and coke in a choice, where one of the two is randomly given a price promotion. The expectation is that participants choose the promoted product, and this choice consequently induces an increase in preference for that promoted product. Additionally, I investigate if the effect differs between three promotion types; ~~*€1,99*~~ *€1,00*, ~~*€1,99*~~ *50% off,* and ~~*€1,99*~~ *€0,99*.

The findings are debatable. I find that promoted products increase in ranking and that this effect does not depend on the type of promotion used. Although this suggests evidence of a *choice-induced preference change*, a potential misconception among participants calls into question whether this observed effect can be attributed to the act of choice alone. Nonetheless, my findings should not be written off completely. Rather, the experiment should serve as an incentive for further replication, with slight adjustments, to confirm if a *choice-induced preference change* is present in economics.

My research proceeds as follows: the subsequent section contains a review of the existing theory and methods, followed by an introduction of my novel experimental design and corresponding hypotheses. Thereafter, I elaborate upon my methods and procedures and present my results. My research concludes with a discussion of my findings, which includes limitations and suggestions for further research.

Literature Review

Before I present my points of critique on the experimental design of Alós-Ferrer & Granic (2018) that motivated my research, I will first describe the theoretical background of *choice-induced preference change*. Namely, as I will subsequently argue, the methodology of Alós-Ferrer & Granic (2018) lacks crucial aspects for the theory to be applicable.

Theoretical Framework

Since Brehm (1956) initiated the research in *choice-induced preference change* using the *free-choice paradigm*, two theories have been put forward that explain the occurrence of the phenomenon. These are *cognitive dissonance theory* (Festinger, 1957) and *self-perception theory* (Bem, 1967).

Cognitive Dissonance Theory

Cognitive dissonance is a state of mental discomfort that occurs when our beliefs (what we think) conflict with our actions (what we do). We reduce this mental discomfort by adjusting our beliefs (Festinger, 1957). For example: “I despise animal cruelty in the farming industry” (belief), yet “I regularly eat meat” (action). Our minds feel a sense of discomfort due to this conflict. To ease our minds, we try not to think of animal cruelty when consuming meat. A similar mental discomfort emerges when we need to make a difficult choice between two things we like equally. Take the example of Tonic and Coke. You like the bitter taste of Tonic but you also like the sweet taste of Coke. Given that you have to choose, you decide to go with the Tonic. To ensure that you feel happy with your choice, you tell yourself that you didn’t like the sweet taste of Coke that much anyway and remind yourself how much you like Tonic’s bitterness. Whereas before the choice, you liked both options equally, after the choice, you subconsciously favor the chosen Tonic. In other words, we observe a *choice-induced preference change* because of a subconscious need to

realign our beliefs with our actions. For cognitive dissonance to emerge, the rejected alternative (Coke) must have certain desirable characteristics (sweetness) that conflict with the choice. Thus, it should be emphasized that cognitive dissonance, as an explanation of *choice-induced preference change*, requires a tradeoff between certain personally appealing (or unappealing) characteristics of each option at hand. In my critique of the methods of Alós-Ferrer & Granic (2018), I argue that their experimental method lacks such a tradeoff in choice.

Self-perception Theory

An alternative explanation of a *choice-induced preference change* is the *self-perception theory* (Bem, 1967). According to *self-perception theory*, we infer our preferences from observing our own behavior, similar to how we would infer the preferences of others from observing their behavior. For example, when a friend sees you choosing Tonic over Coke, he will infer that you like Tonic and dislike Coke. Similarly, when you observe your own behavior of choosing Tonic, you may – subconsciously – conclude that you must like Tonic and dislike Coke. Thus, you infer your preferences from observing your own choices. As opposed to *cognitive dissonance theory*, the theory of self-perception does not require a tradeoff in choice. Rather, it is important that the decision-maker observes and remembers what they chose and what they did not choose. As I will argue below, the methodology of Alós-Ferrer & Granic (2018) disregards that choices should be observed and remembered.

In the following section, I discuss the experimental designs that have been proposed thus far to give context to the motivations of Alós-Ferrer & Granic (2018) to apply an improved methodology in economics. Subsequently, I present my critique in response to Alós-Ferrer & Granic (2018), which need to be addressed in my alternative experimental design.

Methodological Framework

The experiments on *choice-induced preference change* can be categorized into three streams. First, Brehm's (1956) original experimental design called the *free-choice paradigm*, which was found to contain a selection bias. Second, the *blind-choice paradigm* (Sharot et al., 2010) and the *implicit-choice paradigm* (Alós-Ferrer et al., 2012), which use randomization to solve the selection bias of the *free-choice paradigm*. Last, the methodology of Alós-Ferrer & Granic (2018), which uses randomization to solve the selection bias and tests if a *choice-induced preference change* is upheld in a setting that is relevant to economics.

Free-choice Paradigm

The standard experimental approach was originally designed by Brehm (1956) and inspired many others to replicate the experiment in different variations. The *free-choice paradigm* has a simple structure that consists of three stages. In the first stage, subjects state their preferences on a set of alternatives. The experimenter uses either a method of ranking (e.g., Gerard and White, 1983; Lieberman et al., 2001) or rating (e.g., Brehm, 1956; Shultz et al., 1999) to elicit the subject's preferences. In my description of the original *free-choice paradigm*, I will focus on ratings. In the second stage, the participant is offered a choice between *A* and *B*. Here, *A* and *B* are two alternatives that received a similar rating in the first stage. In the third stage, subjects re-rate all alternatives. I will refer to the three respective stages as *pre-choice rating(/ranking) task (1)*, *choice task* (2), and *post-choice rating(/ranking) task (3)*. As shown in Figure 1, the expectation is that if the participant chooses *A* and rejects *B* in the choice task, *A* will increase in post-choice rating relative to *B*. Alternatively, if *B* is chosen, *B* is expected to increase in post-choice rating relative to *A*.

Free-choice paradigm

Pre-choice rating task (1)	**Choice task (2)**		**Post-choice rating task (3)**
Please rank these drinks on a scale from 1 "not at all desirable" to 10 "extremely desirable":	Choose one drink that you would like to receive:		Please rank again:
Tonic, Fanta, Sprite, Coke, Beer, Juice	Tonic (*A*)	**Tonic** →	**Tonic** ^ 8 (*A*) Fanta 5 Sprite 6 Coke v 6 (*B*) Beer 9 Juice 4
Tonic 7 (*A*) Fanta 5 Sprite 6 Coke 7 (*B*) Beer 9 Juice 4	vs Coke *(B)*	**Coke** →	Tonic v 6 (*A*) Fanta 5 Sprite 6 **Coke** ^ 8 (*B*) Beer 9 Juice 4

Figure 1. Schematic overview of the experimental design of Brehm's (1956) *free-choice paradigm.*

This relative change in rating is expressed through the spread (See Equation 1). A significant positive spread is interpreted as evidence of a *choice-induced preference change*.

$$Spread = rating\ change\ chosen\ item\ - rating\ change\ rejected\ item \quad (1)$$

Despite the popularity of the approach, the *free-choice paradigm* was suggested to suffer from a methodological flaw by Chen (2008) and Chen & Risen (2010). The researchers argued that the observation of a significant spread in the *free-choice paradigm* can be explained by selection bias. The selection bias is formed as follows. Ratings of participants often contain a certain degree of noise. That is, the ratings may not always perfectly reflect participants' actual preferences. Since the post-choice rating contains the same alternatives as the pre-choice rating, participants are essentially given the chance to rethink their preferences more accurately. As a result, post-choice ratings likely reflect participants' pre-existing preferences more accurately than

pre-choice ratings. This is problematic because the decision in the choice task is based on these same pre-existing preferences. Hence, selection bias emerges. The rationale of Chen and Risen (2010) was confirmed[1] by Izuma & Murayama (2013). The researchers ran a computer-simulated experiment that adopts the *free-choice paradigm*. In the simulation, artificial decision-makers are programmed to have consistent, yet noisy, preferences. Despite preferences being programmed to be consistent – thereby excluding the possibility of a *choice-induced preference change* –, a significantly positive spread was still found.

The selection bias of the *free-choice paradigm* called for new and improved methodologies that separate the choice from these pre-existing preferences. Experimenters should randomly determine what the participant chooses. If then the chosen alternative increases in post-choice preference as compared to the rejected alternative, there is evidence of *choice-induced preference change* free from selection bias. Below, I will discuss newly proposed alternatives to the *free-choice paradigm* that use the same three-stage structure as the *free-choice paradigm* while randomly assigning choices.

Choice Randomization Methods

The two methods I will describe are the *blind-choice paradigm* (Sharot et al., 2010) and the *implicit-choice paradigm* (Alós-Ferrer et al., 2012). Both paradigms use a different method to achieve the same purpose; randomly assigning choices to avoid selection bias.

Blind-choice Paradigm. Take again the similarly preferred alternatives *A* and *B*. In the *blind-choice paradigm* of Sharot et al (2010), the experimenters randomly determined one (say, *B*)

[1] Izuma & Murayama (2013) suggested that the mathematical evidence of Chen and Risen (2010), and the assumptions their computations were based on, were flawed. However, the experimenters used the reasoning of Chen and Risen (2010) to find computer-simulated evidence of selection bias.

to be chosen and one (say, *A*) to be rejected. In the choice task, participants chose between *A* and *B* without seeing the alternatives, after which they were made to believe[2] they chose option *B* over *A*. The participant was then asked to re-rate all alternatives, including *A* and *B*, in the post-choice rating task, convinced that they subconsciously chose *B*. Interestingly, Sharot et al. (2010) found that the 'chosen' *B* increased in rating relative to the 'rejected' *A*. The findings suggest that, interestingly, the mere belief of having chosen something results in a significant *choice-induced preference change*.

In response to the findings of Sharot et al. (2010), Alós-Ferrer et al. (2012) proposed an alternative method in which choices were randomly assigned. The researchers argued that the results of the *blind-choice paradigm* show no indication of ***choice**-induced preference change*, as no real choice was made.

Implicit-choice Paradigm. Alós-Ferrer et al. (2012) conducted two experiments, one ranking, and one rating experiment, using the *implicit-choice paradigm*. I focus on the ranking experiment for concision. The experimenters randomly assigned one (say, fourth-ranked *B*) to be chosen and one (say, third-ranked *A*) to be rejected. Rather than offering the participant the choice between *A* and *B* directly, the experimenters presented the subject with two choices, *A* vs *H* and *B* vs *L*. The experimenters purposely picked *L (Low ranked*[3]*)* and *H (High ranked)* such that *H* had a higher pre-choice ranking than *A* and *L* had a lower pre-choice ranking than *B*. Here, the

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[2] Participants were convinced of the concept of subliminal decision making, in which the experimenter can accurately observe subconscious choices. In the choice task, participants were shown nonsense scribbles for two milliseconds, after which they had to press one of two buttons. Regardless of what button was pressed, the experimenter told the participant that *B* was subconsciously chosen.

[3] It is important to note that in my research, I refer to a rank with low preference such as 5 as a low ranking and 1 as a high ranking. Hence, I refer to a change in ranking from rank 5 to rank 1 as an increase in ranking. This may be confusing to readers who saw that in Alós-Ferrer & Granic (2018) a change in ranking from 5 to 1 was called a rank decrease; because 5 is a higher number than 1.

expectation was that participants chose in line with their pre-choice ranking and thus chose *B* over *L* and *H* over *A*. Hence, making *B* chosen and *A* rejected. Although *A* and *B* were never directly presented as a choice pair, the two form what the researchers referred to as an implicit choice pair (Alós-Ferrer et al., 2012). As *B* was chosen and *A* rejected, the expectation was that *B* would increase in rank relative to *A*. Figure 2 shows a schematic overview of how this implicit choice pair was formed. In this figure, as well as in subsequent methods where choices are assigned by the experimenter, the alternative that is assigned to be chosen is marked **bold and underlined**.

Incongruent implicit choice pair

Pre-choice ranking task	**Choice task**	**Post-choice rating task**
Please rank these holiday destinations: 1. Sweden 2. Brazil *(H)* 3. Mexico *(A)* 4. Spain *(B)* 5. Greece *(L)* 6. Morocco	*Choice AH*: Mexico (*A*) vs **Brazil *(H)*** *Choice BL*: **Spain (*B*)** vs Greece (*L*) *Incongruent* implicit pair: *A* rejected, *B* chosen	Please re-rank these holiday destinations: 1. Sweden 2. Brazil *(H)* 3. ^ **Spain (*B*)** 4. v Mexico (*A*) 5. *Greece (L)* 6. Morocco

Figure 2. Schematic overview of an *incongruent* implicit choice pair (Alós-Ferrer et al., 2012).

If the participant chose as the experimenters intended, the expected ranking change of *A* and *B* in the post-choice ranking task was incongruent with the rankings of the pre-choice ranking task. Hence, I refer to such implicit choice pairs as *incongruent* implicit choice pairs. Of course, participants did not always choose as the experimenter intended. In some cases, the participant chose *A* over *H (High ranked)* or *L (Low ranked)* over *B*. Alós-Ferrer et al. (2012) referred to such choices as incompliant choices. If either *Choice AH* or *Choice BL* was incompliant, no *incongruent* implicit choice pair of *A* and *B* could be constructed. Additionally, the experimenters formed *congruent* implicit choice pairs. This was achieved by offering *choice AL* and *choice BH* as shown

in Figure 3. I refer to such pairs as *congruent* because the expected ranking change of *A* and *B* in the post-choice ranking task reinforces the ranking difference in the pre-choice ranking task.

Congruent implicit choice pair		
Pre-choice ranking task	**Choice task**	**Post-choice rating task**
Please rank these holiday destinations: 1. Sweden 2. Brazil *(H)* 3. Mexico *(A)* 4. Spain *(B)* 5. Greece *(L)* 6. Morocco	Choice *AL*: **Mexico (*A*)** *vs* Greece (*L*) Choice *BH*: Spain (*B*) vs **Brazil (*H*)** *Congruent* implicit pair: *A* chosen, *B* rejected	Please re-rank these holiday destinations: 1. Sweden 2. ∧ **Mexico (*A*)** 3. Brazil *(H)* 4. Greece *(L)* 5. ∨ Spain (*B*) 6. Morocco

Figure 3. Schematic overview of a *congruent* implicit choice pair (Alós-Ferrer et al., 2012).

If one of the two choices was incompliant – meaning the participant chose either *L (Low ranked)* over *A* or *B* over *H (High ranked)* – no *congruent* implicit choice pair of *A* and *B* could be constructed. In the experiments of Alós-Ferrer et al. (2012) 19.8% of choices were incompliant and 81.2%[4] compliant. The researchers referred to this latter percentage as the *compliance rate*. As the variable of interest for *choice-induced preference change*, Alós-Ferrer et al. (2012) computed two spreads. First, the regular spread of the *free-choice paradigm*; (*ranking change chosen – ranking change rejected*). For this spread, observations were only included if an implicit choice pair was successfully constructed, that is, both choices were compliant. Second, a robustness check. For this robustness check, all choices, including those where choices were incompliant, were used to calculate the spread. Here, the spread was calculated as (*ranking change assigned as chosen – ranking change assigned as rejected*). In this robustness check, the treatment

[4] Of the two experiments of Alós-Ferrer et al. (2012) the rating experiment had a compliance rate of 86% and the ranking experiment had a compliance rate of 77.5%

of assigning choice was analyzed, rather than the actual choice; thereby excluding potential selection bias. Alós-Ferrer et al. (2012) found both spreads to be significantly positive, thus, robust evidence was found of a *choice-induced preference change*.

Following their findings in a psychological experiment (Alós-Ferrer et al., 2012), the researchers questioned whether the observations were upheld in an economic experiment (Alós-Ferrer & Granic, 2018). Namely, if a *choice-induced preference change* holds in an economic experiment, one of economics' most important assumptions – the stability of preferences – could be at risk. The researchers argued that the psychological experiment in Alós-Ferrer et al. (2012) could not be seen as economically viable given that the participants' choices had no consequences. That is, Alós-Ferrer et al. (2012) asked "How happy would you be to spend your next year's holiday at this destination?" with options being e.g. "Australia, Brazil, Sweden". The question was hypothetical; hence, participants had no incentive to state preferences accurately. Furthermore, the alternatives presented were ill-defined, as apart from the country, no details of the trip – how will I go there, where will I stay – were specified.

Thus, to answer the research question of whether a *choice-induced preference change* is relevant to economics, Alós-Ferrer & Granic (2018) presented a novel experimental design. This design, as well as my own subsequent design, are based on the methods of the implicit-choice paradigm of Alós-Ferrer et al. (2012).

Choice-induced Preference Change in Economics

Alós-Ferrer & Granic (2018) created an economic experiment by using incentivized standard lotteries as the objects of choice. An example of such a lottery is '*77% chance to receive €5.50, 23% chance to receive €2.00'*, or in short '*77% €5.5, 23% €2'*. The researchers adopted the

traditional three-staged format and used a method of ranking to elicit preferences in the pre-choice and post-choice stages.

The basic design is as follows. From the pre-choice ranking task, the experimenter randomly assigns one (say, fourth-ranked *Lottery B*) to be chosen and one (say, third-ranked *Lottery A*) to be rejected. Rather than offering the participant the choice between *A* and *B* directly, the experimenter presents the subject with two choices, *A* vs *C* and *B* vs *D*. Here, lotteries *C* and *D* are newly generated lotteries (that is, not part of the pre-choice ranking task) such that lottery *C* is slightly more appealing than lottery *A* while lottery *D* is slightly less appealing than lottery *B*. Here, the expectation is that participants choose *C* over *A* and *B* over *D*. The experimenters used first-order stochastic dominance to make the lottery that is assigned to be chosen more attractive. E.g. for lottery *A* (*77% €5.5, 23% €2*) new lottery *C* (*81% €5.5, 19% €2*) is generated such that *C* is chosen and *A* rejected. For lottery *B* (*14% €18, 86% €2*) new lottery *D* (*10% €18, 90% €2*) is generated such that *B* is chosen and *D* is rejected. The expectation is that whereas the third-ranked lottery *A* will be rejected and consequently decrease in post-choice ranking, the fourth-ranked lottery *B* is expected to be chosen and increase in post-choice ranking. Thus, *A* and *B* form an *incongruent* choice pair, as shown in Figure 4.

Incongruent implicit choice pair

Pre-choice ranking task	**Choice task**	**Post-choice rating task**
Please rank these lotteries:	*Choice AC:*	Please re-rank these lotteries:
	77% €5.5, 23% €2 (*A*)	
1. *95% €5, 5% €1*	*vs*	1. *95% €5, 5% €1*
2. *40% €8, 60% €2.5*	***81% €5.5, 19% €2 (C)***	2. *40% €8, 60% €2.5*
3. *77% €5.5, 23% €2* (*A*)		3. ^ ***14% €18, 86% €2 (B)***
4. *14% €18, 86% €2* (*B*)	*Choice BD:*	4. v *77% €5.5, 23% €2* (*A*)
5. *11% €34, 89% €1*	***14% €18, 86% €2 (B)***	5. *11% €34, 89% €1*
6. *32% €5.5, 86% €2.5*	*vs*	6. *32% €5.5, 86% €2.5*
	10% €18, 90% €2 (D)	

Figure 4. Schematic overview of an *incongruent* implicit choice pair in Alós-Ferrer & Granic (2018).

Alternatively, the experimenter could generate lotteries *C* and *D* such that *A* is chosen and *B* is rejected. Here, *A* and *B* form a *congruent* choice pair, as shown in Figure 5.

Congruent implicit choice pair		
Pre-choice ranking task	**Choice task**	**Post-choice rating task**
Please rank these lotteries:	*Choice AC:*	Please re-rank these lotteries:
	***77% €5.5, 23% €2* (A)**	
1. *95% €5, 5% €1*	*vs*	1. *95% €5, 5% €1*
2. *40% €8, 60% €2.5*	*73% €5.5, 27% €2 (C)*	2. ^***77% €5.5, 23% €2 (A)***
3. *77% €5.5, 23% €2* (*A*)		3. *40% €8, 60% €2.5*
4. *14% €18, 86% €2* (*B*)	*Choice BD:*	4. *11% €34, 89% €1*
5. *11% €34, 89% €1*	*14% €18, 86% €2* (*B*)	5. v*14% €18, 86% €2* (*B*)
32% €5.5, 86% €2.5	*vs*	6. *32% €5.5, 86% €2.5*
	18% €18, 82% €2 (D)	

Figure 5. Schematic overview of a *congruent* implicit choice pair in Alós-Ferrer & Granic (2018).

In their experiments, Alós-Ferrer & Granic (2018) found that incompliant choices were rare using their methodology. That is, an average compliance rate was reached of 97.7%. Thus, there was no need for an additional robustness check as was done in Alós-Ferrer et al. (2012). Even without a robustness check, the methodology avoided the selection bias effectively. The objects of choice were lotteries that have a clearly defined payoff with well-understood consequences, thus the experiment was relevant to economics. Although an unbiased and economically relevant experiment was conducted, the researchers found no consistent *choice-induced preference change*.

As I will argue below, the lack of clear evidence in Alós-Ferrer & Granic (2018) can be attributed to the absence of two qualities: a tradeoff in choice, whose importance is emphasized by *cognitive dissonance theory*, and the necessity of observed and remembered choices, as defined by *self-perception theory*.

Tradeoff in Choice

In the discussion of their results, Alós-Ferrer & Granic (2018) recognize that their methodology focuses on the isolated effect of choice on preference and disregards incorporating some form of a tradeoff in choice. As discussed in the theoretical framework, a precondition for cognitive dissonance is that the choice is effortful and the rejected alternative has positive qualities that conflict with the choice. The whole purpose of the design of Alós-Ferrer & Granic (2018) is that the first-order stochastically dominated – and rejected – lottery *A* (*77% €5.5, 23% €2*) contains no positive qualities that could potentially make it more attractive than the dominant – and chosen – lottery *C* (*81% €5.5, 19% €2*). Hence, even if Alós-Ferrer & Granic (2018) had found a significant *choice-induced preference change*, *cognitive dissonance theory* could not explain those results.

Memorability of Choices

As discussed in the theoretical framework, *self-perception theory* requires that participants observe and remember their choice to change preferences accordingly. However, in Alós-Ferrer & Granic (2018) the lotteries displayed in the choice had a very similar visual appearance (See Figure 6). Hence, when participants were asked to re-state their preferences (Figure 7), it is likely that they had already forgotten which lottery they had chosen in the choice task.

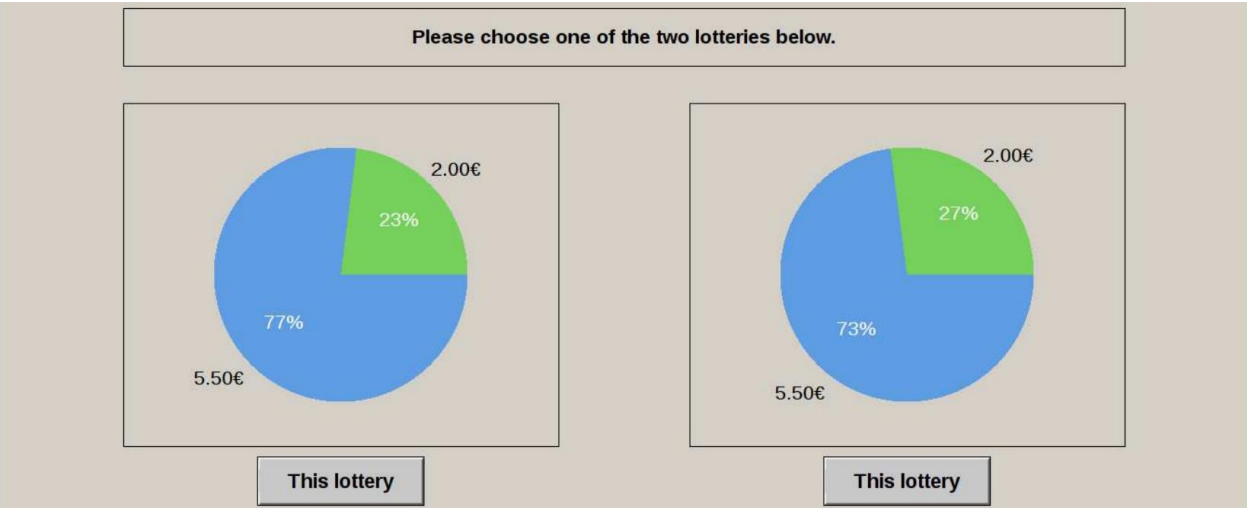


Figure 6. Choice task example from Alós-Ferrer & Granic (2018)

Each participant made 10 consecutive choices between two visually similar lotteries as displayed in Figure 7. Thus, it is not unlikely that the problem, which I describe above, may apply to multiple post-choice rankings.

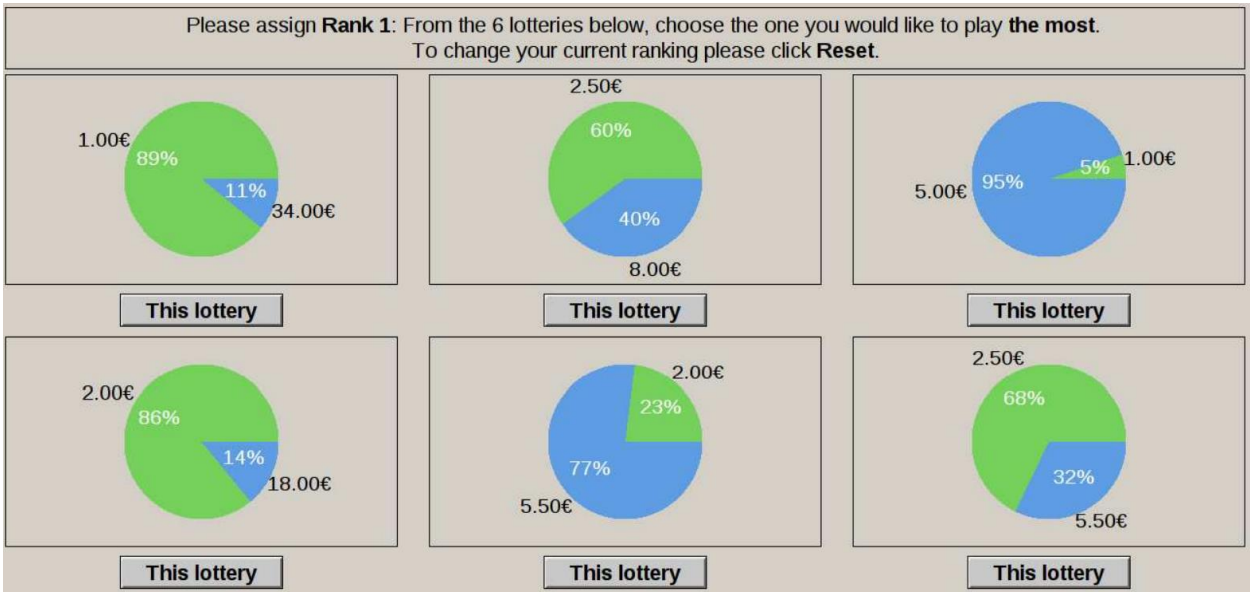


Figure 7. Ranking task example from Alós-Ferrer & Granic (2018)

Thus, even if Alós-Ferrer & Granic (2018) had found a significant *choice-induced preference change*, *self-perception theory* could not explain those results.

I see the findings of Alós-Ferrer & Granic (2018) as a motivation to create an alternative experimental design that addresses both aforementioned problems. This experimental design

should incorporate effortful choices that contain a potential tradeoff. To measure if choices are sufficiently effortful, I use the compliance rate as an indicator. Namely, if choices are more effortful, this will be reflected in a lower compliance rate. Comparing the compliance rate of 97.7% in Alós-Ferrer & Granic (2018) to the 81.2% in Alós-Ferrer et al. (2012) shows that for the latter, choices were substantially more effortful. Given that Alós-Ferrer et al. (2012) find a significant *choice-induced preference change*, I will, in my improved design, use the 81.2% compliance rate as a reference.

To ensure that participants remember choices, objects of choice should be used that differ substantially in visual appearance. Furthermore, the number of choices made per participant should be limited, to minimize the probability of participants forgetting one of the choices made.

In addition to addressing the critiques on Alós-Ferrer & Granic (2018), the improved experimental design should also avoid selection bias by effectively randomizing choices and ensure economic relevance by presenting well-defined alternatives with proper incentives.

Below, I present my improved experimental design addressing all critiques to answer the research question of my thesis: *can a choice-induced preference change be observed in economics?*

Guiding Choices With Price Promotions

To investigate the presence of a *choice-induced preference change* in economics, I apply a new experimental design in an economically well-defined retail setting. That is, participants, rank and choose between different meals. To be able to apply incentivized choices and ensure that all meals are clearly defined, I present all meals in the form of an existing retail product. More specifically, each meal is represented by a ‘meal base’; a small bag of flavored ingredients in a powdered form that serves as a base to create the corresponding meal (See examples in Figure 8).

Figure 8. Variants of meal bases.

The basic design is as follows. In the pre-choice ranking task, participants rank six meals in *Set 1* and six meals in *Set 2* from 1 (most desirable) to 6 (least desirable). In the choice task, the participant is offered two choices. Before each choice, the participant is given €2,00. In the first choice, €2,00 can be spent on either *A* (third-ranked in *Set 1*) or *C* (third-ranked in *Set 2*). I will refer to this choice as *Choice AC*. In the second choice, *Choice BD*, €2,00 can be spent on either *B* (fourth-ranked in *Set 1*) or *D* (fourth-ranked in *Set 2*). I randomize choices as follows. In each choice, I present the product which I intend to be chosen with a price promotion (~~1,99~~ €1,00) and the other, which I intend to be rejected, at the regular non-promoted price (€1,99). The intention is that participants choose the product with the price promotion. The exception is that under *choice-induced preference change*, this product, which was promoted in the choice-task, will increase in post-choice ranking.

In *Choice AC*, I assign *C* to be chosen by pricing *A* at €1,99 and promoting *C* with ~~1,99~~ €1,00. In Choice *BD*, I assign *B* to be chosen by promoting *B* with ~~1,99~~ €1,00 and pricing *D* at €1,99. See Figure 9 for a schematic overview of my experimental design.

Pre-choice ranking task	**Choice task**	**Post-choice rating task**
Set 1		*Set 1 (incongruent)*
1. Beef stew	*Choice AC*:	1. Beef stew
2. Mushrooms		2. Mushrooms
3. Capers (*A*)	Capers (*A*) €1,99	3. ^ **Noodles (*B*)**
4. Noodles (*B*)	vs	4. v Capers (*A*)
5. Fried Rice	**Curry (C)** ~~€1,99~~ €1,00	5. Fried Rice
6. Hawaiian		6. Hawaiian
Set 2	*Choice BD*:	*Set 2 (congruent)*
1. Macaroni	**Noodles (*B*)** ~~€1,99~~ €1,00	1. Macaroni
2. Lasagna	vs	2. ^ **Curry (*C*)**
3. Curry (*C*)	Chili (*D*) €1,99	3. Lasagna
4. Chili (*D*)		4. Napolitano
5. Napolitano		5. v Chili (*D*)
6. Bolognese		6. Bolognese

Figure 9. Schematic overview of my experimental design.

If both *Choice AC* and *Choice BD* are compliant, it is expected that under *choice-induced preference change* the rejected *A* decreases in ranking, and the chosen *B* increases in ranking in *Set 1*. Thus, *A* and *B* form an *incongruent* implicit choice pair in *Set 1*. Additionally, if both *Choice AC* and *Choice BD* are compliant, chosen *C* is expected to increase in ranking, and rejected *D* is expected to decrease in ranking in *Set 2*. Thus, *C* and *D* form a *congruent* implicit choice pair in *Set 2*. A more detailed description of the experimental design is given in the methodology section.

My experimental design addresses the critique on Alós-Ferrer & Granic (2018) as follows. First, the way choice pairs are constructed allows for effortful choices that include a tradeoff. Namely, participants should make a tradeoff between the desirable characteristics of the non-promoted alternative and the monetary benefit of choosing the promoted alternative.

Second, choices can be more easily remembered. Each meal is visually distinguishable (see Figure 8) and contains several memorable characteristics. Additionally, in my experimental design, the number of choices can be limited to increase memorability. That is, two compliant choices

create one *incongruent* and one *congruent* implicit choice pair as opposed to Alós-Ferrer et al. (2012) and Alós-Ferrer & Granic (2018) where two compliant choices create only one *(in)congruent* implicit choice pair.

The fact that I allow a tradeoff in choice suggests that incompliant choices will be more prevalent than in Alós-Ferrer & Granic (2018). Namely, if either *Choice AC* or *Choice BD* is incompliant – i.e., the non-promoted product is chosen – neither the *incongruent* implicit choice pair (*A* and *B*) nor the *congruent* implicit choice pair (*C* and *D*) can be constructed. Thus, the spread cannot be computed for these pairs. Hence, in addition to the regular spread (*rank change chosen – rank change rejected*) as my main variable of interest, I additionally conduct a robustness check similar to that of Alós-Ferrer et al. (2012). In this robustness check, I compare the ranking change of the products that I assign to be chosen (the promoted) to the ranking change of those that I assign to be rejected (the non-promoted). Thus, for this robustness check, the spread is (*rank change promoted – rank change non-promote)*.

In my research, I refer to the regular spread, which is based on choice, as the *Choice Spread*.

$$Choice\ Spread = rank\ change\ chosen - rank\ change\ rejected \quad (2)$$

Here, a significantly positive *Choice Spread* is interpreted as (non-robust) evidence of *choice-induced preference change*. Additionally, I refer to the spread of the robustness check, which is based on promotion, as the *Promotion Spread*.

$$Promotion\ Spread = rank\ change\ promoted - rank\ change\ non\text{-}promoted \quad (3)$$

A significantly positive *Promotion Spread* is interpreted as robust evidence of *choice-induced preference change*.

It should be considered that comparing the rank changes of the promoted products to that of the non-promoted products could have additional implications. For example, DelVecchio et al. (2006; 2007) found that large price promotions negatively affect post-promotion preference. That is, if a product is in a price promotion, consumers use that lower price as an anchor (Tversky and Kahneman, 1974) to estimate the value of the product. If then the promotion ends, the consumer will still perceive that product as being relatively low in value. Thus, in my experiment, participants may use the lower ~~1,99~~ €1,00 promotion price to estimate the value of that product and infer that this is a low-value product. Consequently, the participant may give that product a lower post-choice ranking. This opposes my expectation that promoted products increase in post-choice ranking as expressed through a significantly positive *Promotion Spread*.

It should be noted that, as opposed to my experimental design, in DelVecchio et al. (2006; 2007) participants did not make a choice during the promotion. Thus, DelVecchio et al. (2006; 2007) observed the isolated effect of promotion on post-promotion preference. Hence, this isolated negative effect of promotion on post-promotion preference, which opposes my expectations under *choice-induced preference change*, serves as a natural robustness check for the expected positive *Promotion Spread* in my research.

In addition to the effect of promotion on post-promotion preference, DelVecchio et al. (2007) studied if the way a price promotion is framed (Tversky & Kahneman, 1981) affects post-promotion preferences. Framing is presenting the same thing in a different way such that it appears less or more appealing, e.g., glass half full or glass half empty. The researchers found no evidence of an effect of promotion framing of post-choice preference. However, in an experiment such as mine, where the participant makes a choice during the promotion, such an effect could be observed. Hence, in addition to my main *choice-induced preference change* hypothesis, I investigate if the

framing of the price promotion affects post-choice preferences as measured by the *Promotion Spread*. To compare the framing effects, I assign participants to one of three treatment groups which each adopt a different promotion frame in the choice task: (*~~€1,99~~ €1,00)*, (*~~€1,99~~ 50% off)* and *(~~€1,99~~ €0,99)*.

First, I will compare the *Promotion Spread* of participants in the *~~€1,99~~ €1,00* group to those in the *~~€1,99~~ 50% off* group. Earlier literature (McKechnie et al, 2012; DelVecchio et al., 2007) found inconclusive results on whether relative price promotions, as compared to absolute price promotions, differently affect post-choice preference. Hence, a priori, I expect no difference between the two groups.

Second, I will compare the *Promotion Spread* of participants in the *~~€1,99~~ €1,00* group to those in the *~~€1,99~~ €0,99* group. Here, I do expect a difference between the groups. That is, the left-digit effect predicts that participants perceive €0,99 as disproportionately lower than €1,00 because more weight is put on the leftmost digit (Thomas & Morwitz, 2005; Manning & Sprott, 2009). This left digit being smaller for €0,99 than for €1,00, results in €0,99 appearing disproportionally smaller. Hence, the expectation is that if the *~~€1,99~~ €0,99* promotion is used, as opposed to the *~~€1,99~~ €1,00* promotion, the decision-maker will anchor down the value of the product more, resulting in a smaller *Promotion Spread*.

Additionally, participants in the *~~€1,99~~ €0,99* group may choose the promoted option more often than participants in the *~~€1,99~~ €1,00* group because the *~~€1,99~~ €0,99* promotion is more appealing. This would result in a higher compliance rate for participants in the *~~€1,99~~ €0,99* group. This is problematic when comparing the *Promotion Spreads* between the groups, given that choice is expected to affect post-choice preferences. Thus, to compare the *Promotion Spread* between the groups, it is a precondition that compliance rates are not significantly different.

Methodology

Experimental Design and Procedures

I applied my methodological design in a digital experiment (conducted on tablets) that was created using the software of Qualtrics. Although the experiment was digital, all participants were physically approached so that the experimenter could be present to answer potential questions and give payouts after the experiment. Responses were collected over six days in two separate locations; on-campus and onboard public transportation. In total, 134 observations were collected, seven of which were left out due to incompleteness. Of the remaining 127 participants, 73 were female and 54 were male. The mean age was 22.6, ranging between 16 and 37 years old.

The experiment started with a short explanation of the tasks followed by a set of demographic questions. Participants were given a practice ranking task on monetary values to familiarize themselves with the ranking method used. The products that were used in the experiment were clearly described to ensure participants made well-thought decisions.

To avoid accumulated earnings and subsequent wealth effects, the experiment used a random lottery incentive. That is, of all the tasks performed, one was randomly picked and paid out after the experiment (see Appendix A for the full payout scheme).

To double the number of spreads observed per participant, each participant took part in the experiment twice. Because there were only limited meal bases available, in the second experiment, all choices were made for soup bases (powdered soup ingredients to which boiling water can be added). See Appendix B for all meals and soups used in the experiments. For concreteness, I hereafter explain the stages for the meal experiment only.

Pre-choice Ranking Task

Each subject was presented with two sets – *Set 1* and *Set 2* – each containing six products. For each set, participants ranked the products according to desirability by dragging each item to a rank-box. To ensure that participants gave accurate preferences, each ranking was incentivized. The ordinal incentive system was made as transparent as possible. That is, each rank-box was marked by the actual probability of receiving it. That is, ranks 1, 2, 3, 4, 5, and 6 were respectively displayed with the percentual probabilities of 33%, 27%, 20%, 13%, 7%, and 0% of receiving that rank (see Figure 10).

Figure 10. Visual of the ranking task

The composition of the sets and the presentation order of the objects were fully randomized.

Choice Task

In the choice tasks, subjects were presented with *Choice AC* (regularly priced *A* vs promoted *C)* and *Choice BD* (promoted *B* vs regularly priced *D)*. Here, *A* and *C* were both third-ranked in their respective sets, and *B* and *D* were both fourth-ranked in their respective sets. The two middle ranks – i.e., third and fourth – were used deliberately to avoid floor and ceiling effects in post-choice preferences. For each choice, participants received €2,00 to spend on one of the two

alternatives presented. The choices were presented such that they resembled an online retail store. Figure 11 shows the choice tasks for each treatment group.

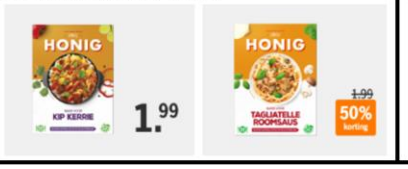
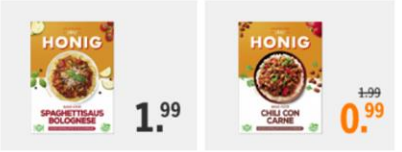
~~*€1,99*~~ *€1,00*	~~*€1,99*~~ *50% off*	~~*€1,99*~~ *€0,99*
You receive €2,00 to spend on one of these two:	You receive €2,00 to spend on one of these two:	You receive €2,00 to spend on one of these two:
		

Figure 11. Choice tasks for each treatment group

Of the total 127 participants, 38 were randomly placed in the ~~*€1,99*~~ *€1,00* group, 45 in the ~~*€1,99*~~ *50% off* group, and 44 in the ~~*€1,99*~~ *€0,99* group. To ensure that participants chose the promoted alternative, it was clarified that if the promoted option was chosen, the participant would keep the unspent money[5]. Furthermore, to ensure that products were not chosen based on underlying retail price, the variants did not vary substantially in real retail price (see Appendix C).

Post-choice Ranking Task

The post-choice ranking task was an exact replication of the pre-choice ranking task. The only difference was that the presentation order of sets and products was randomized[6]. Participants were told that the post-choice ranking task was not a memory task and to state their current preferences. In the post-choice ranking, the expectation was that the products that were promoted – and hence chosen – would increase in ranking. Figure 12 shows an overview of all rankings and choices for one participant in the ~~*€1,99*~~ *€1,00* group for the meal as well as the soup experiment.

[5] To avoid overcomplication, if the promoted option was chosen, €1.00 was paid out for each treatment group (rather than €1.01 for the ~~*€1,99*~~ *€0,99* group or €1.005 for the ~~*€1,99*~~ *50%* group) . For non-promoted choices, no remaining monetary amount was paid out (rather than €0.01).

[6] It should be noted that although the order in which sets were displayed was intended to be randomized, due to a programming error the order of sets was only partially randomized. This is a minor error and is not expected to impede with the validity of the experiment.

Note that this hypothetical participant makes only compliant choices, hence the *Choice Spread* can be computed for all implicit choice pairs. The ranking changes displayed in the post-choice ranking task are examples of expectations under *choice-induced preference change.*

Meals		Soups	
1) Pre-choice ranking task			
Set 1	*Set 2*	*Set 1*	*Set 2*
1. Beef stew	1. Macaroni	1. Vegetable soup	1. Curry soup
2. Mushrooms	2. Lasagna	2. Stock soup	2. Queen soup
3. Capers (*A*)	3. Curry (*C*)	3. Beef soup (*A*)	3. Cream soup (*C*)
4. Noodles (*B*)	4. Chili (*D*)	4. Ox soup (*B*)	4. Onion soup (*D*)
5. Fried Rice	5. Napolitano	5. Chicken soup	5. Asparagus soup
6. Hawaiian	6. Bolognese	6. Chinese soup	6. Tomato soup
2) Choice task (expected compliant choices)			
Choice AC:	Capers (*A*) €1,99 vs **Curry (C)** ~~€1,99~~ €1,00	*Choice AC:*	Beef soup (*A*) €1,99 vs **Cream soup (C)** ~~€1,99~~ €1,00
Choice BD:	**Noodles (*B*)** ~~€1,99~~ €1,00 vs Chili (*D*) €1,99	*Choice BD:*	**Ox soup (*B*)** ~~€1,99~~ €1,00 vs Onion soup (*D*) €1,99
3) Post-choice ranking task (example of expectation)			
Set 1 (incongr.)	*Set 2 (congr.)*	*Set 1 (incongr.)*	*Set 2 (congr.)*
1. Beef stew	1. Macaroni	1. Vegetable soup	1. ⩓ **Cream soup (*C*)**
2. Mushrooms	2. ∧ **Curry (*C*)**	2. Stock soup	2. Curry soup
3. ∧ **Noodles (*B*)**	3. Lasagna	3. Chicken soup	3. Queen soup
4. ∨ Capers (*A*)	4. Napolitano	4. **Ox soup (*B*)**	4. Asparagus soup
5. Fried Rice	5. ∨ Chili (*D*)	5. ⩔ Beef soup (*A*)	5. ∨ Onion soup (D)
6. Hawaiian	6. Bolognese	6. Chinese soup	6. Tomato soup
Rank changes: Chosen *B* = +1 Rejected *A* = -1	Rank changes: Chosen *C* = +1 Rejected *D* = -1	Rank changes: Chosen *B* = 0 Rejected *A* = -2	Rank changes: Chosen *C* = +2 Rejected *D* = -1
Choice Spread = 2	*Choice Spread* = 2	*Choice Spread* = 2	*Choice Spread* = 3

Figure 12. Full experimental design for a participant placed in the ~~*€1,99*~~ *€1,00* group

The experiment was concluded with a thank you note after which subjects were given their payoff. See Appendix D for the full experiment.

Results

Compliance Rates

Pooling the data, the 127 subjects made a total of 508 choices of which 414 were compliant and 94 incompliant. This amounts to a pooled compliance rate of 81.5%, which is almost identical to the compliance rate of 81.2% in the experiment of Alós-Ferrer et al. (2012) and visibly lower than the 97.7% of Alós-Ferrer & Granic (2018). Thus, it appears that on average, choices required an appropriate amount of effort, which is necessary for *cognitive dissonance theory* to be applicable. Table 1 shows the mean compliance rate within each group. In groups ~~*€1,99*~~ *€1,00*, ~~*€1,99*~~ *50% off*, and ~~*€1,99*~~ *€0,99*, respectively 84.2%, 75.0% and 85.8% of choices were compliant.

Table 1
Compliance rates for each treatment group.

Group	Promoted alternative chosen	Non-promoted alternative chosen	Mean compliance rate
~~*€1,99*~~ *€1,00*	128	24	84.2%
~~*€1,99*~~ *50% off*	135	45	75.0%
~~*€1,99*~~ *€0,99*	151	25	85.8%
Pooled	414	94	81.5%

A Pearson's Chi-squared test rejects the null hypothesis of equal compliance rates between the three groups at a five percent significance level (P=0.019). More specifically, Fisher's exact tests reject the null hypothesis of equal compliance rates in the ~~*€1,99*~~ *50% off* group compared to the ~~*€1,99*~~ *€1,00* group (P=0.084)[7] and the ~~*€1,99*~~ *€0,99* group (P=0.044) respectively at a ten and five percent significance level. This suggests that subjects in the ~~*€1,99*~~ *50% off* group chose the non-promoted alternative significantly more frequently than subjects in the two other groups. This

[7] P-values of pairwise tests are corrected using the Holm-Bonferroni correction for pair-wise hypothesis testing.

is problematic when comparing the *Promotion Spread* between the treatment groups and will be considered in the analysis.

Choice Spread

Pooling the data, out of 508 potential pairs, 324 implicit choice pairs could be constructed where both choices were compliant. These implicit choice pairs each generate one *Choice Spread* (*ranking change chosen – ranking change rejected*). Figure 13 shows the distribution of *Choice Spreads* for the pooled sample.

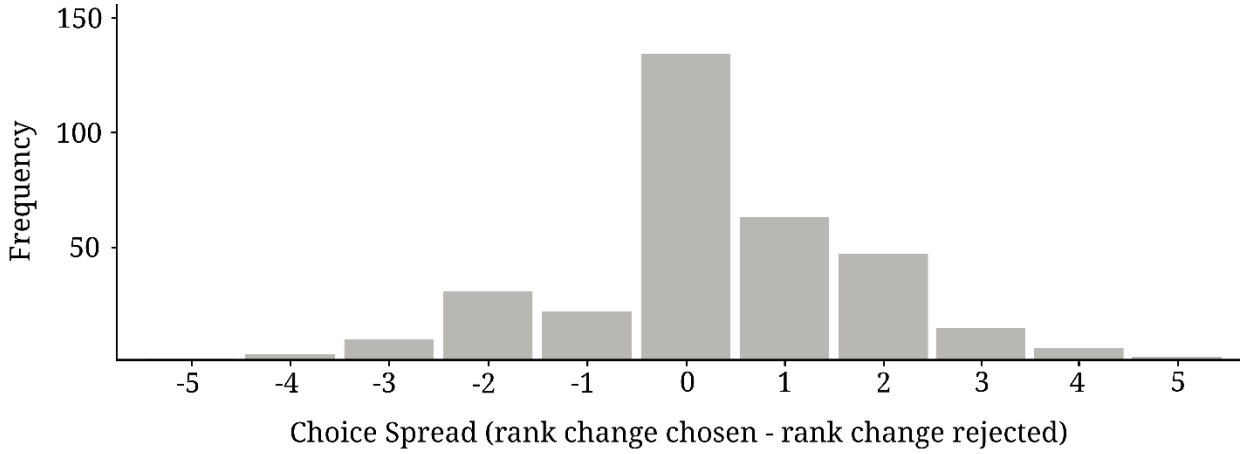


Figure 13. Frequencies of *Choice Spreads*.

Pooling the data, implicit choice pairs had an average *Choice Spread* of 0.315. In other words, if both *Choice AB* and *Choice CD* were compliant, the ranking change of chosen products was on average 0.315 ranks higher than that of rejected products. According to a two-sided Wilcoxon signed-rank test, the median *Choice Spread* is significantly different from zero (P=0.000) at a five percent significance level. The significant positive *Choice Spread* provides evidence of a (non-robust) *choice-induced preference change* in the pooled sample.

Promotion Spread

The *Promotion Spread* (*ranking change promoted – ranking change non-promoted*) provides a robustness check for the *Choice Spread*. Figure 14 shows the distribution of *Promotion Spreads* for the pooled sample.

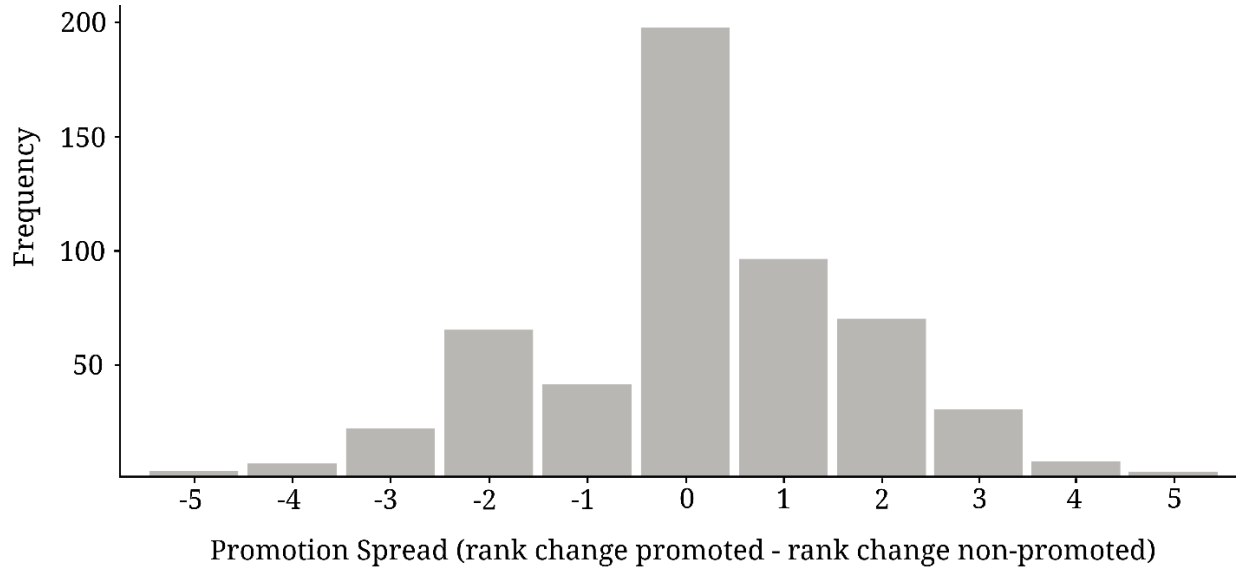


Figure 14. Frequencies of Promotion Spreads.

Pooling the data, implicit choice pairs, including unconstructed pairs, had an average *Promotion Spread* of 0.130. In other words, the ranking change of promoted products, chosen or not, was on average 0.130 ranks higher than for non-promoted products. A two-sided Wilcoxon signed-rank test shows the median *Promotion Spread* to be significantly different from zero (P=0.017) at a five percent significance level. The significantly positive *Promotion Spread* confirms the results of the *Choice Spread* and suggests that a robust *choice-induced preference change* is present.

Additionally, I compare the *Promotion Spread* of the ~~*€1,99*~~ *50% off* and the ~~*€1,99*~~ *€0,99* group to that of the ~~*€1,99*~~ *€1,00* group. Figure 15 shows the mean *Promotion Spread* across the treatment groups.

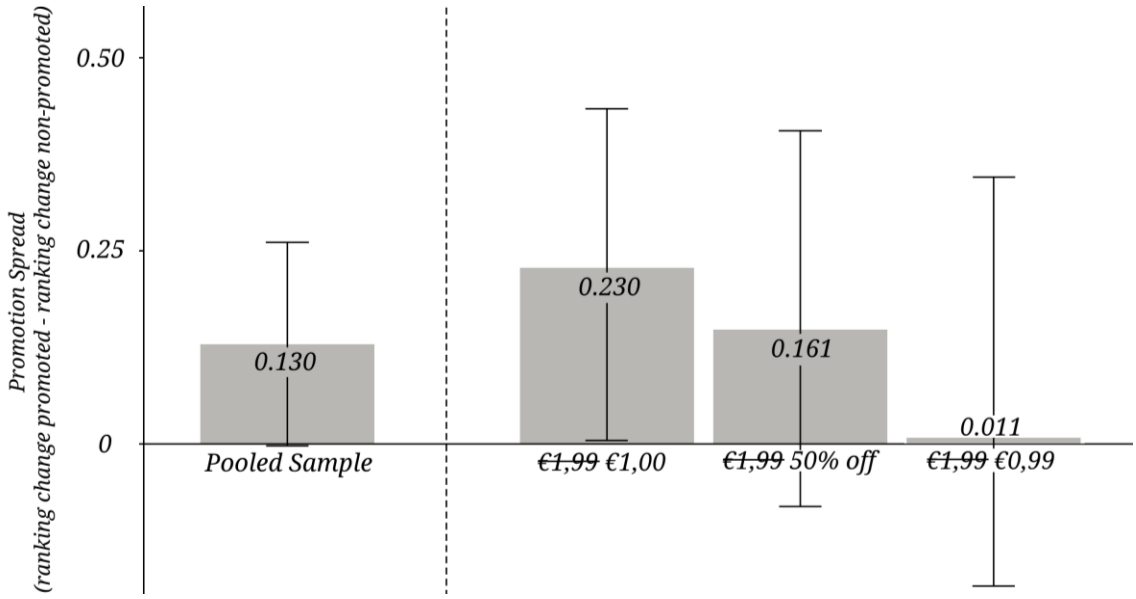


Figure 15. Promotion Spread for each treatment group and the pooled sample

A two-sided Wilcoxon rank-sum test fails to reject the null hypothesis of an equal median *Promotion Spread* in the ~~*€1,99*~~ *50% off* group as compared to the ~~*€1,99*~~ *€1,00* group (P=0.523). Here, it should be noted that the mean compliance rate is significantly lower in the ~~*€1,99*~~ *50% off* group. Hence, the comparison may be biased. Likewise, a two-sided Wilcoxon rank-sum test fails to reject the null hypothesis of an equal median *Promotion Spread* in the ~~*€1,99*~~ *€0,99* group compared to the ~~*€1,99*~~ *€1,00* group (P=0.418).

To confirm if the findings of the non-parametric tests are robust to the inclusion of several control variables, I run additional OLS regressions with *Promotion Spread* as the dependent variable. Table 2 shows the corresponding results. Standard errors are clustered at the participant level. In all three regressions, ~~*€1,99*~~ *50% off* and ~~*€1,99*~~ *€0.99* present the dummy variables for each corresponding group as compared to the ~~*€1,99*~~ *€1.00* reference group. In the second regression, two binary variables are added; *Congruent* taking the value of 1 for *congruent* and 0 for *incongruent* pairs, and *Meal*, taking the value of 1 for meals and 0 for soups. In the third regression, several between-participant controls are added; *Demographics* (gender, age, highest completed education, and whether the subject was vegetarian) and *Setting* (day of participation, and location).

Table 2
OLS regressions with participant-clustered standard errors.

Dependent Variable		*Promotion Spread*	
~~€1,99~~ 50% off	-0.069	-0.069	-0.058
	(0.234)	(0.235)	(0.259)
~~€1,99~~ €0.99	-0.219	-0.219	-0.280
	(0.181)	(0.182)	(0.189)
Congruent		-0.512***	-0.512***
		(0.187)	(0.191)
Meal		0.063	0.034
		(0.130)	(0.136)
Constant	0.230	-0.120	0.537
	(0.103)	(0.212)	(0.670)
Demographics	No	No	Yes
Setting	No	No	Yes
Clustered SE	Yes	Yes	Yes
Participants	127	127	127
Spreads	508	508	508

Note. Clustered standard errors in parentheses. Significance at a *=10%, **=5% and ***=1% level. Variables: Congruent=0 for incongruent, 1 for congruent Type: 0 for Soup, 1 for Meal. Demographics: gender, age, highest completed education, and whether the subject was vegetarian. Day (and Location): day of participation, which includes location (either on campus on in public transport, depending on the day).

The results of the OLS regressions are no different from those of the non-parametric tests. Moreover, the findings are confirmed by several Tobit regressions (see Appendix E). Thus, neither participants in the *~~€1,99~~ 50% off* group nor those in the *~~€1,99~~ €0.99* group displayed significantly different *Promotion Spreads* as compared to the *~~€1,99~~ €1.00* group. Interestingly, there is a significantly negative effect of *Congruent* on *Promotion Spread*. This suggests that on average, for *congruent* implicit choice pairs, the *Promotion Spread* was 0.512 lower than for *incongruent* pairs. This observation can be explained by a regression to the mean. That is, statistically, a third-ranked alternative was more likely to decrease in ranking than to increase in ranking. Similarly, a fourth-ranked alternative was more likely to increase in ranking than to decrease in ranking. Hence, *incongruent* implicit choice pairs were more likely to display a significant positive *Promotion Spread* than *congruent* implicit choice pairs.

Discussion

In this thesis, I created an experimental method to test the presence of a *choice-induced preference change* in economics. My methodology was inspired by recent developments in the literature that used implicit choice pairs as a way to effectively randomize choices (Alós-Ferrer et al., 2012; Alós-Ferrer & Granic, 2018). In my experiment, all choices were well-defined and incentivized, thereby incorporating the critiques that were raised by Alós-Ferrer & Granic (2018) on the preceding psychological experiments in the *choice-induced preference change* literature. Furthermore, I improved upon two methodical aspects of Alós-Ferrer & Granic (2018) by ensuring choices required effort (*cognitive dissonance theory*) and were memorable (*self-perception theory*). Most importantly, the selection bias, whose discovery marked all research using the *free-choice paradigm* as methodologically flawed, was successfully avoided by randomizing choices and conducting an additional robustness check. Choices were randomly assigned by presenting one of the alternatives with a price promotion, with the intention that these promoted products were chosen and the expectation that these increased in post-choice ranking. I found significant positive spreading for the regular (*Choice Spread*) as well as the robust (*Promotion Spread*) analysis. The results are interpreted as evidence of a robust *choice-induced preference change*.

The observations can be explained by the theory. First, in line with *cognitive dissonance theory*, the positive qualities of the non-promoted – and rejected – alternative conflicted with the choice of the promoted alternative. Participants reduced the tension caused by this conflict by adjusting their preference in favor of the promoted item post-choice. Second, according to *self-perception theory*, participants observed their own choice of the promoted alternative and

inferred preference for that alternative accordingly. This was expressed through an adjustment in post-choice ranking.

Despite my observations being evidence of these theories, there is an alternative explanation for the observed effect that may have major consequences for the internal validity of the experiment. That is, multiple participants inquired whether the promotions of the choice task still applied in the post-choice ranking task. To these subjects, it was clarified that this was not the case. However, it is not unlikely that unquestioning participants ranked the products in the post-choice phase under the assumption that the previously presented promotions still applied. As a consequence, these participants may have given a higher post-choice ranking to products falsely assumed to be promoted. Then, the significantly positive *Promotion Spread* can be attributed to this misconception.

In addition to the main findings, I found no significant difference in *Promotion Spread* between the groups. Either, the theory proposed by DelVecchio et al. (2007) on post-promotion choice may not apply to a *choice-induced preference change* experiment, or the absence of an effect can be explained by the aforementioned misconception. Namely, if participants expressed post-choice preference under the false assumption that the products were still promoted, the post-choice ranking task cannot be seen as ‘post-promotion’. If this were the case, the theory of DelVecchio et al. (2007) on post-promotion value perception did not apply.

I see this shortcoming as a motivation for further research to replicate the experiment with an additional clarification that promotions in the choice task do not apply in the post-choice ranking task. Furthermore, it should be noted that I addressed the shortcomings of Alós-Ferrer & Granic (2018) by ensuring choices required effort and included a tradeoff (*cognitive dissonance theory*) and were memorable (*self-perception theory*). However, a *choice-induced preference*

change can potentially be observed in an experiment that relies solely on *self-perception theory* while disregarding the tradeoff required for *cognitive dissonance theory*. For example, future research could address only the memorability issues of Alós-Ferrer & Granic (2018). This can be achieved by duplicating the experiment of Alós-Ferrer & Granic (2018), reducing the number of choices, and assigning additional characteristics, such as colors, to the lotteries (See Appendix F).

All in all, the ideal method to unbiasedly research the presence of a *choice-induced preference change* in economics is yet to be found. Although my experiment successfully addresses the shortcomings of earlier methods, its validity can be questioned. A *choice-induced preference change* may exist, but until a more proper method is designed, the implications of the phenomenon, especially in the economic domain, remain unclear.

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Appendix A

Incentive Payout Probabilities

The probabilities of receiving each rank and choice as a payout are displayed in Figure A1 in the form of a probability tree.

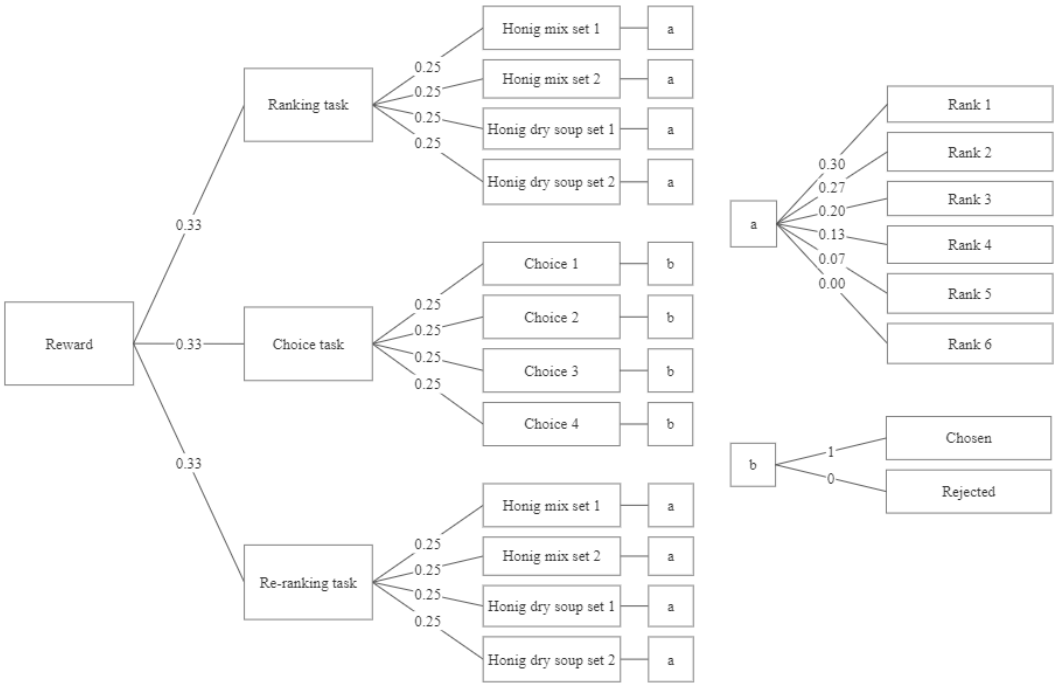


Figure A1. Probability tree of payouts

Appendix B

Variations of Meals and Soups

Below in Figure B1, all Honig Meal variations used in the experiment are displayed. Figure B2 shows the Honig dry soup variations used. Each meal or soup represents a different dish which is displayed on the packaging.

Figure B1. All twelve meals

Figure B2. All twelve soups

Appendix C

Price Spreads of Meals and Soups

The meals do not vary substantially in price as there is a 10 cent difference between the cheapest and the most expensive meal. The soups all have an equal price. See Table C1 for the prices of Albert Heijn, the largest retailer in the Netherlands.

Table C1

Prices of all variations including difference spread per product type

Meal name	Price	Soup name	Price
Bami Speciaal	1.39	Boeren Groentesoep	1.39
Champignonsaus	1.39	Bouillon Soep	1.39
Chile Con Carne	1.39	Chinese Groentesoep	1.39
Hachee	1.39	Chinese Kippensoep	1.39
Kapucijners	1.49	Deense Rundvleessoep	1.39
Kip Hawaii	1.49	Engelse Ossenstaartsoep	1.39
Kip Kerrie	1.39	Indiase Kerriesoep	1.39
Lasagnesaus	1.49	Koninginnensoep	1.39
Macaroni Stroganoff	1.39	Limburgse Aspergesoep	1.39
Napolitaanse Lasagnesaus	1.49	Romige Lente-uisoep	1.39
Fried Rice Speciaal	1.39	Tomaten-basilicumsoep	1.39
Spaghettisaus Bolognese	1.49	Tomaten-cremesoep	1.39
Min	1.39	Min	1.39
Min	1.49	Max	1.39
Difference	0.10	Difference	0.00

Note: Retrieved from http://www.ah.nl on 6-28-2021

Appendix D

Detailed Description of the Experiment

Introduction
Welcome to this experiment conducted as part of a master's thesis in behavioral economics related to consumer choices in retail.
In this experiment, you will make choices between different products that can be found in the supermarket. The experiment consists of three stages in which you either rank or choose between different Honig dry soups and Honig meals.
The experiment works with real rewards, that is, at the end of the experiment, all participants receive a reward from one of the three stages. What reward you get today depends on the choices you make in the experiment and partly on chance. Important: By giving your true opinion, you are more likely to receive a product that you prefer. More details will follow.
Your answers will be recorded anonymously and treated completely confidentially. By clicking "I agree", you agree to the use of your answers as data for my thesis.

Demographics
Before starting the experiment, please answer the following questions:

- What is your gender?
- What is your age?
- What is your highest completed education?

Test questions:
Test: Here is a test question to get acquainted with the ranking method used. Order the following amounts according to how much you want to receive them. If you would like to receive an amount, drag it to a box with a high chance of receiving the amount. If you don't like to receive it, assign it to a subject with a low probability. Assign one value to each box. Drag the amount to the desired box and shuffle them around where necessary. Please note: this question is a test question, so the amounts are not paid out. If you have any questions, let me know.

€ 55,- € 12,- € 356,- € 4,- € 56,- € 116,-

1. The chance that you will receive this amount is 33%	2. The chance that you will receive this amount is 27%	3. The chance that you will receive this amount is 20%
4. The chance that you will receive this amount is 13%	5. The chance that you will receive this amount is 7%	6. The chance that you will receive this amount is 0%

Test: Here is a test question to get acquainted with the choice method used. Choose the amount you prefer to receive. Please note: this question is a test question, so no amounts will be paid. If you have any questions, let me know.

☐ 34,- ☐ 15,-

Product information:

The products that are the subject of this experiment are Honig meals and Honig dry soups. Honig meals are bags with herbs or spices in powder form that forms the basis for a meal. Honig dry soups are bags of powder that can be used to make a soup by adding water.

Stage 1:

In the first stage of this experiment, you are asked to rank different Honig meals and Honig dry soups according to your personal preferences. If you would like to receive a product, drag it to a box with a high chance of receiving the product. If you don't like to receive it, assign it to a subject with a low probability. Assign one value to each box. Drag the product to the desired compartment and toss it around where necessary. Note: Your ranking will affect the product you receive at the end of the experiment, so rank wisely!

Stage 1: Arrange the following Honig dry soups according to which you prefer to receive. Attention: these decisions matter! *This was repeated 4 times*

Soup or Meal	Soup or Meal	Soup or Meal	Soup or Meal	Soup or Meal	Soup or Meal

1. The chance that you will receive this amount is 33%	2. The chance that you will receive this amount is 27%	3. The chance that you will receive this amount is 20%
4. The chance that you will receive this amount is 13%	5. The chance that you will receive this amount is 7%	6. The chance that you will receive this amount is 0%

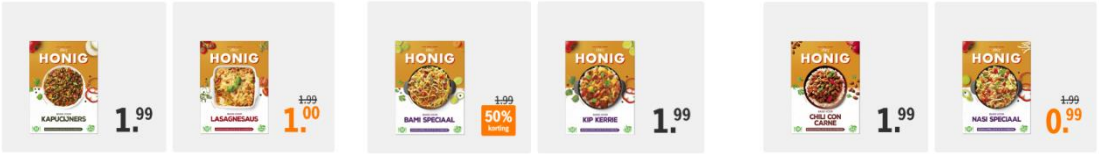
Stage 2:

In the second stage of this experiment, you will be faced with four scenarios. In each scenario, you will receive € 2,- to spend on one of the products. After each choice, you will receive the product you chose plus the remaining amount of money. That is, if you spend €1.50 on a product,

you will receive that product at the end of the experiment plus the remaining €0.50. Again: Your choices affect what you receive at the end of the experiment, so choose wisely!
Stage 2: You receive € 2,-. Spend this amount on one of the following two products. Note: these choices matter! *This was repeated 4 times*

Choice example:

Group 1 Group 2 Group 3



You have chosen:
chosen alternative

Stage 3:
In the third and final stage of this experiment, just like in stage 1, you will be asked to rank products according to your personal preferences. The products, as well as the method, are identical to those of stage 1. But beware: this is not a memory task! Rank the products according to your current preferences. Again: Your ranking will affect which product you receive at the end of the experiment, so rank wisely!
repeats stage 1

End:
Thank you for participating in this experiment! You may return the tablet to me and receive the following reward:
reward is shown

Appendix E

Statistical Methods

The findings of the OLS regressions are confirmed by an additional Tobit regression. The Tobit regressions are done because *Promotion Spread* could be censored from below and above. More specifically, *incongruent* pairs could be censored from below at -4 and from above at 6. *Congruent* pairs could be censored from below at -6 and from above at 4. In the Tobit regressions below, the above-mentioned limits are applied. As can be seen in Table E1, only two observations were censored from above and none were censored from below. Hence, there is no concern that the coefficients of the OLS in the main text are biased. Furthermore, the Tobit regressions confirm the findings of the OLS in the main text.

Table E1

Tobit regressions with limits depending on congruence of the implicit choice pair

Dependent Variable	*Promotion Spread*		
~~€1,99~~ 50% off	-0.076	-0.076	-0.065
	(0.175)	(0.174)	(0.175)
~~€1,99~~ €0.99	-0.226	-0.225	-0.289
	(0.176)	(0.173)	(0.177)
Congruent		-0.508***	-0.508***
		(0.139)	(0.135)
Meal		0.067	0.067
		(0.139)	(0.135)
Constant	0.237	-0.118	0.262
	(0.129)	(0.254)	(0.669)
Demographics	No	No	Yes
Setting	No	No	Yes
Clustered SE	Yes	Yes	Yes
Participants	127	127	127
Spreads	508	508	508
Uncensored	495	495	495
Censored below	0	0	0
Censored above	2	2	2

Note. Standard errors in parentheses. Significance at a *=10%, **=5% and ***=1% level. Variables: Congruent=0 for incongruent, 1 for congruent Type: 0 for Soup, 1 for Meal. Demographics: gender, age, highest completed education, and whether the subject was vegetarian. Day (and Location): day of participation, which includes location (either on campus on in public transport, depending on the day).

Appendix F

Suggestion for Future Research

Figure F1 displays a suggested improvement of the ranking task of Alós-Ferrer & Granic (2018). The use of colors predominantly helps to distinguish the two lotteries in the choice task displayed in Figure F2. The left (green) lottery first-order stochastically dominates the right (cyan) lottery and is consequently chosen. In the post-choice ranking task, the same colors as in the pre-choice ranking task are used. In this post-choice ranking task, the subject will remember choosing the green lottery and revalue this lottery upward. The proposed improvement keeps the structure of the original experiment of Alós-Ferrer & Granic (2018) while ensuring the chosen lottery is remembered as being chosen.

Figure F1. Left: the ranking task of Alós-Ferrer & Granic (2018). Right: suggestion with colors.

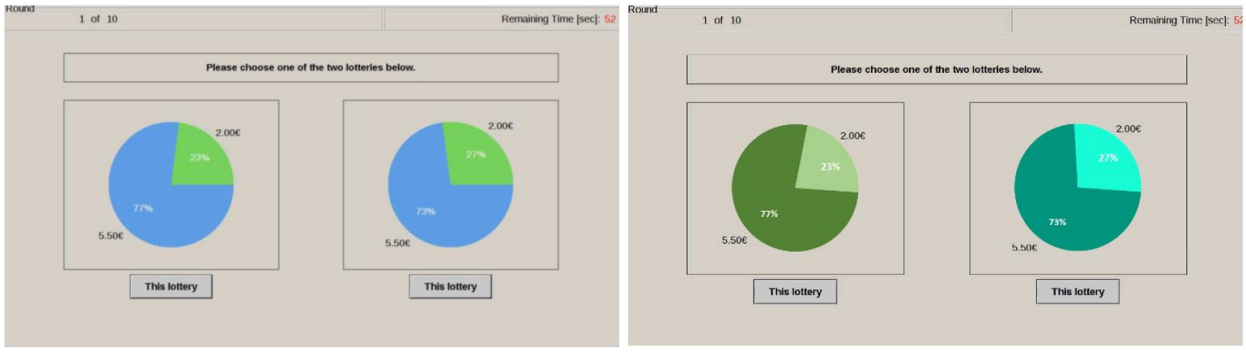


Figure F2. Left: the choice task of Alós-Ferrer & Granic (2018). Right: suggestion with colors.