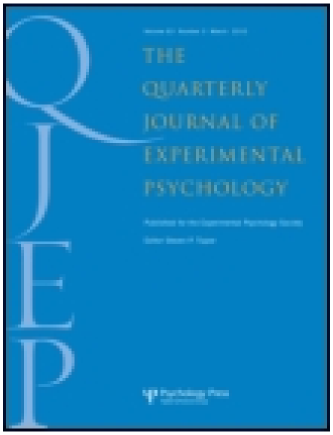

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Choice both affects and reflects preferences

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Choice both affects and reflects preferences

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The free-choice paradigm is a widely used paradigm in psychology. It has been used to show that after a choice between two similarly pleasant stimuli, the pleasantness of the chosen one tends to increase, whereas the pleasantness of the rejected one tends to decrease—a spreading of alternatives. However, the methodological validity of the free-choice paradigm to study choice-induced preference change has recently been seriously questioned [Chen, K. M., & Risen, J. L. (2010). How choice affects and reflects preferences: Revisiting the free-choice paradigm. *Journal of Personality and Social Psychology*, *99*, 573–594. doi:10.1037/a0020217]. According to this criticism, the classically reported spreading of alternatives between the first and second rating sessions cannot be unambiguously interpreted to reflect a true change in preferences and can be observed even for completely static preferences. Here, we used two measurement sequences, a classical Rating 1–choice–Rating 2 sequence and a control Rating 1–Rating 2–choice sequence, to disentangle the spreading of alternatives driven by the effect of choice from the artefactual effect highlighted by Chen and Risen. In two studies using different stimulus material (faces and odours), we find that choice has a robust modulatory impact on preferences for rejected odours, but not for chosen odours and not for faces.

Keywords: Preference; Choice; Decision making; Cognitive dissonance; Sequence of measurements; RCR; RRC.

The free-choice paradigm (Brehm, 1956) is a widely used paradigm in psychology. It is divided into three parts: (a) a first hedonic evaluation of stimuli presented individually from a stimuli set (i.e., a presentation, or an imagination, of a stimulus or a situation followed by a rating of how pleasant it is); (b) choices between pairs of stimuli, the choices being either difficult (i.e., choices between two stimuli similarly rated during the first hedonic evaluation), or easy (i.e., choices between two stimuli with very different ratings during the first hedonic evaluation); and (c) a second hedonic evaluation of the same stimuli as those presented during the first

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hedonic evaluation.[1] The classical finding in this paradigm is the following: After a difficult choice, the rating of the chosen alternative tends to increase, and the rating of the rejected alternative tends to decrease. Such a modulation of hedonic evaluations is called *spreading of alternatives* and has classically been interpreted as caused by the choice. Studies that use this paradigm aim to explore this phenomenon.

This spreading of alternatives in the framework of the free-choice paradigm has been an important topic in psychology, which has led to hundreds of studies and generated some of the most influential hypotheses about psychological functioning. In particular, Festinger's influential cognitive dissonance theory (Festinger, 1957) is based on results obtained using the free-choice paradigm, in addition to those obtained with the effort-justification paradigm or the induced-compliance paradigm. Cognitive dissonance has been defined as a psychologically uncomfortable state created by the existence of simultaneously held but conflicting ideas. In this framework, if such an uncomfortable state is present, people are motivated to reduce it. In the more specific context of the free-choice paradigm, the choice is thought to enter into conflict with the desirable aspects of the rejected alternative and the undesirable aspects of the chosen alternative, creating an uncomfortable state of so-called *cognitive dissonance*. This discomfort can be reduced by reevaluating the chosen alternative more positively and the rejected alternative less positively by the aforementioned spreading of alternatives (for a review of studies pertaining to this framework, see notably Harmon-Jones & Mills, 1999). More recently, the free-choice paradigm has been used to investigate the influence of choice on preferences, beyond the scope of the classical version of the cognitive dissonance theory. Some recent studies have argued in favour of a potential implicit form of cognitive dissonance (e.g., Coppin, Delplanque, Cayeux, Porcherot, & Sander, 2010; Lieberman, Ochsner, Gilbert, & Schacter, 2001). Coppin et al. (2010) have notably shown that olfactory pleasantness ratings can be modulated by choices, not only in the absence of any outcome of the choice, but even when the choices were forgotten. Other recent studies have focused on exploring the neural basis of preference modulation induced by choice (e.g., Sharot, De Martino, & Dolan, 2009; Van Veen, Krug, Schooler, & Carter, 2009) and have suggested that postchoice preference change has a unique brain signature.

However, the methodological validity of the free-choice paradigm has recently been seriously questioned (Chen, 2008; Chen & Risen, 2009, 2010; Risen & Chen, 2010). Chen and Risen (2010) presented a mathematical proof relying on a preference-driven model of choice (see the original paper for details), demonstrating that when using the free-choice paradigm, the chance of measuring a spreading of alternatives is high, even if preferences remain entirely stable. Their core argument is that the initial similarity of ratings between the chosen versus the rejected stimuli (presented in difficult pairs in the choice phase) may be due to errors in translating the underlying stable "true" preferences into rating measures. The choice may then simply reveal which of the two had already initially been preferred. Consequently, observing a spreading of alternatives does not necessarily need to be driven by a true preference change, rendering the interpretation inconclusive. Chen and Risen's (2010) methodological argument hence casts doubt on how to interpret the results of hundreds of studies that have used this paradigm. It is important to note that Chen and Risen's argument is methodological in nature and does not by itself undermine the rationale of cognitive dissonance theory. While it does render results based on the free-choice paradigm inconclusive, it does not affect the effort-justification paradigm or the induced-compliance paradigm. The question that results is to what extent psychological effects remain after controlling for choice information.

[1]According to the studies, the rated dimension can vary a bit; it can, for example, be the "desirability" of the alternatives, such as in Brehm's (1956) original study. Other variations exist in terms of the exact task required from participants: It can be a ranking rather than a rating of the stimuli, such as in Chen and Risen's (2010) study.

How have researchers reacted to the point made by Chen and Risen (Chen, 2008; Chen & Risen, 2010)? Sagarin and Skowronski (2009a, 2009b) have formally shown that the more random the choices, the less important Chen and Risen's (2010) argument. Two recent studies (Egan, Bloom, & Santos, 2010; Sharot, Velasquez, & Dolan, 2010) have empirically demonstrated a spreading of alternatives when choice is manipulated by the experimenter—that is, in circumstances in which Chen and Risen's argument does not apply. Their approach was to use a "blind" choice—a choice that participants were not actually making, even though they had the feeling that they did. In this approach, the fact that choices were imposed exogenously means that they are not a mere reflection of preference. When such a paradigm was applied to human adults in a study by Sharot, Velasquez, et al. (2010), there was still a spreading of alternatives for the chosen stimuli. Furthermore, in a recent study conducted by Izuma et al. (2010) using food pictures, postchoice preference modulation of both ratings and brain activity levels was demonstrated after the investigators controlled for the information revealed by choice. To do so, they adopted a strategy suggested by Chen and Risen (2010) to properly test the impact of choice on preferences in the free-choice paradigm. This strategy involves running two sequences of measurements: first, the classical Rating 1–choice–Rating 2 (RCR) sequence and, second, a control sequence of Rating 1–Rating 2–choice (RRC) in which choice can reveal preferences, but not influence the second rating session because the choice takes place after this session. The rating change observed in RCR was shown to be significantly larger than that in RRC for rejected stimuli. This study is particularly relevant regarding the two experiments conducted by Chen and Risen (2010) using the same methods. Indeed, in these two experiments, Chen and Risen failed to demonstrate a difference in the amount of rating change in the RCR condition compared with the RRC condition (no statistical effect in Study 1; a statistical trend in Study 2). A noteworthy difference between Chen and Risen's studies and that of Izuma and colleagues is that the latter investigators presented participants with the choice they made (e.g., "You rejected it") while reevaluating the stimuli during the second rating session in the RCR condition, which was not possible in the RRC condition, as participants had not yet made any choices before the second rating session. Such a procedure creates an asymmetry between the RCR and the RRC sequences, as experimental demand, possibly induced by the presentation of the previous choices during the second rating session, could not be formally excluded in the RCR sequence.

The present study aims to extend the results of Izuma et al. (2010) to two other experimental set-ups (one using faces and one using odours) by disentangling true preference modulation driven by the effect of choice from the artefact pointed out by Chen and Risen (2010). To do so, we conducted two experiments that both used the two sequences of measurements previously described: an RCR sequence and an RRC sequence in a within-subjects design. However, to further explore the robustness of Izuma et al.'s initial positive findings, we did not give participants feedback about the choice they made during the second rating session. Moreover, this study is also different from Chen and Risen's studies in that we used a rating rather than a ranking procedure. Consequently, we had 20 pairs of comparison in Experiment 1 and 16 in Experiment 2, rather than only one pair, as in Chen and Risen's studies. The experimental design used here consequently combines Chen and Risen's RCR and RRC conditions with Izuma et al.'s use of ratings, which are the measure that is classically used in the free-choice paradigm (Brehm, 1956).

EXPERIMENT 1

Method

Participants

One hundred female students (mean age = 24.03 ± 5.66 years) from the University of Geneva took part in this experiment. Before starting the experiment, each participant completed a consent form.

Pictures

In order to ensure interest in the task from our participants, we used infant pictures, which have been shown to be of particular relevance for adult humans (see, e.g., Brosch, Sander, Pourtois, & Scherer, 2008). Forty full-faced colour pictures of infant faces were used that were selected from a previous study that examined attractiveness of infant faces (see Van Duuren, Kendel-Scott, & Stark, 2003, for details). The pictures were framed so that only the face of the baby could be seen. The presentation order of the faces was randomized for each participant.

Procedure

The experiment was divided into four parts: a first rating session, a first choice session, a second rating session, and a second choice session. For half of the stimuli, participants had to make choices between the two rating sessions, and, for the other half, participants had to make choices after the two rating sessions. As participants were asked to rate and choose between infant faces, we asked them to rate the "cuteness" rather than the "pleasantness" of the pictures.[2] Participants were then first asked to rate the cuteness of 40 infants presented on a screen on a scale from 1 (*not cute at all*; left on the scale) to 10 (*very cute*; right on the scale = 10). Participants had to move a vertical marker with the mouse across a horizontal line and to click to indicate their rating. On the basis of these first ratings, a computer program created 20 pairs of pictures such that the difference of cuteness within each pair was minimized. Half of these pairs (10 pairs) were randomly selected and then presented for choice only in the first choice session, right after the first rating. In the choice session, participants were required, for each pair, to indicate the baby they considered cuter by clicking on the baby's picture. Note that there was no consequence of the choice for the participants. This procedure has been shown to be efficient at producing post-choice rating modulation (e.g., Coppin et al., 2010; Sharot et al., 2009; Sharot, Shiner, & Dolan, 2010; Shultz, Léveillé, & Lepper, 1999). Despite the absence of real-life consequences from the choice, it still involves a reevaluation of the options at stake. Next, participants were asked to rate the cuteness of all 40 babies again (second rating). Finally, participants were presented with the 10 remaining pairs in the second choice session (same task as that in the first choice session). The order in which the different pairs were presented during the experiment for a given participant, both inside one choice session and between the two choice sessions, was random.

Data analysis

In the free-choice paradigm, choice-induced changes are typically reported when the choice is difficult (Brehm, 1956)—that is, when the difference between the pleasantness ratings obtained for the two paired stimuli before the choice is small. In contrast, no modulation is thought to occur in easy pairs (Festinger, 1957). To test for this, we split the 20 pairs of comparisons available in this experiment into two groups—the 10 easiest pairs (mean rating differences = 4.17, $SD = 1.07$, on the 10-point subjective scale described above) and the 10 most difficult pairs (mean rating differences = 0.03, $SD = 0.04$, on the 10-point subjective scale described above)—on an individual participant basis. The difficult pairs were created so that the absolute difference between the two paired stimuli was minimized. We proceeded iteratively. The first step was to find the two most similarly rated stimuli, which then formed the first difficult pair. If there was more than one pair that had minimal distance, then one of these was randomly selected. We then iterated this procedure with the remaining stimuli (i.e., only those that were not yet part of any pair). The easy pairs were created so that the absolute difference between the two paired stimuli was maximized, which was operationalized

[2]Such a procedure allows the extension from traditional results found while measuring "pleasantness" or "desirability" to the dimension of "cuteness," which is equivalent to "attractiveness" in the context of infants' evaluation (e.g., Hildebrant, 1983).

analogously. We first found the two most differently rated stimuli and formed a pair of them. Again, ties were broken randomly. We then iterated this procedure with the remaining stimuli.

Moreover, in a nonstandard version of the free-choice paradigm, recent results have raised the possibility that differential psychological processes underlie postchoice hedonic modulation for chosen and rejected stimuli. Sharot, Velasquez, et al.'s (2010) blind-choice study showed divergent results according to choice: Although a significant modulation was observed for chosen stimuli, none occurred for rejected stimuli. In contrast, Izuma et al. (2010) showed a true preference modulation induced by choice (by comparing the RCR and the RRC conditions) for rejected stimuli but not for chosen stimuli.

We first conducted a four-way analysis of variance (ANOVA) on the ratings scores with choice (chosen, rejected), pairs (difficult, easy), procedure (RRC, RCR), and ratings (R1, R2) as within-subject factors. Although the four-way interaction failed to reach statistical significance, $F(1, 99) = 0.34$, $p = .559$, we conducted specific planned contrasts that were based on the theory and previous research as detailed above. Planned contrasts were run for easy and difficult pairs, but also for chosen and rejected pictures separately. To control for multiple comparisons (i.e., Type I error), we used the Bonferroni correction ($p = .05/8 = .00625$).

Results

Chosen stimuli

First, we checked whether an increased preference was observed between the two rating sessions in the classical RCR sequence of measurement (Brehm, 1956), as well as in the control RRC sequence. For difficult pairs, cuteness ratings were significantly increased in the second rating session in comparison to the first one, in both the RCR and the RRC conditions [planned contrasts, $Fs(1, 99) = 79.54, 76.17$, $ps < .001$] (see Figure 1). For easy pairs, cuteness ratings for chosen stimuli tended to be increased in the second rating session for the RCR condition [planned contrasts, $F(1, 99) = 3.57$, $p = .006$, $\eta^2 = .03$], but not in the RRC condition [planned contrasts, $F(1, 99) = 0.27$, $p = .607$].

Second, we checked whether preference modulations between Rating 1 and Rating 2 were increased in the RCR sequence of measurement when compared to the RRC sequence. To do so, we computed the individual signed differences between cuteness scores during rating 2, minus those during rating 1, for difficult choices.[3] A repeated measures ANOVA with the within-subject factor sequence of measurement (RCR, RRC) on these difference scores did not reach significance, $F(1, 99) = 1.76$, $p = .188$ (see Figure 1). In easy choices, there was not such a difference either, $F(1, 99) = 1.33$, $p = .251$.

Rejected stimuli

For difficult choices, cuteness ratings were not significantly decreased between the two ratings in either the RCR condition [planned contrast, $F(1, 99) = 1.50$, $p = .223$] or the RRC condition [planned contrast, $F(1, 99) = 4.36$, $p = .039$, $\eta^2 = .04$]. For easy choices, in the RCR condition cuteness ratings were increased in the second rating session [planned contrast, $F(1, 99) = 13.06$, $p < .001$, $\eta^2 = .12$]. This was not the case in the RRC condition [planned contrast, $F(1, 99) = 2.61$, $p = .109$].

Was this modulation increased in RCR condition as compared to RRC condition dependent on the sequence of measurement? For rejected pictures in difficult choices, the signed difference between the two cuteness scores was not significantly higher in the RCR condition than in the RRC condition [planned contrast, $F(1, 99) = 0.11$, $p = .738$]. The same was true for rejected

[3]The repeated measures ANOVA on the factors choice (chosen, rejected) and sequence (RCR, RRC) on these scores did not reach significance, $F(1, 99) = 0.819$, $p = .367$. However, as mentioned before, because of the possibility that differential psychological processes underlie postchoice hedonic modulation for chosen and rejected stimuli, we have conducted separate planned contrasts for chosen and rejected stimuli.

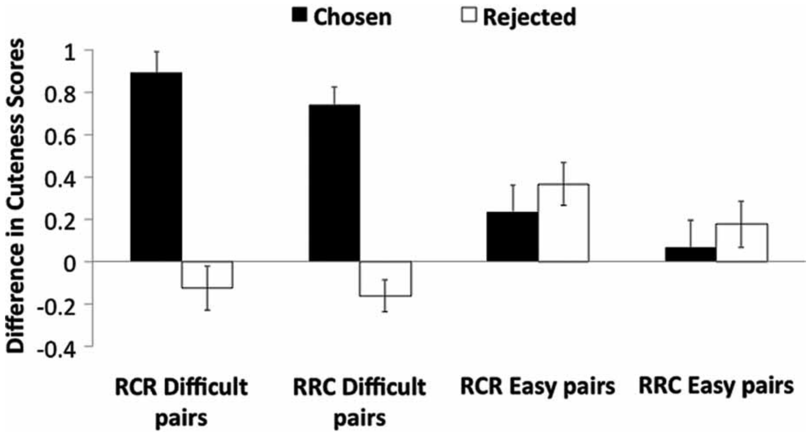

Figure 1. *Cuteness signed difference scores between Rating 1 and Rating 2 from Experiment 1 according to the sequence of measurements, Rating 1–choice–Rating 2 (RCR) and Rating 1–Rating 2–choice (RRC), and the difficulty (difficult, easy) of the choice. Error bars represent the standard error of the mean.*

pictures in easy choices [planned contrast, $F(1, 99) = 2.39$, $p = .126$].

Discussion

Our results showed that there is a significant rating modulation in the RRC condition for chosen items in difficult pairs. In this condition, choice cannot drive preference modulation, because it occurs after the two rating sessions. Such results confirm the validity of Chen and Risen's (2010) point that a rating modulation can be observed in the free-choice paradigm without it being driven by choice.

If there were no true preference modulation by choice, then one would not expect to observe any significant difference in the magnitude of rating modulation in the RCR and the RRC sequences. In difficult choices, the rating modulation was not significantly more pronounced for chosen pictures in the RCR than in the RRC condition. It is consequently not possible to conclude that there was truly a choice-induced preference modulation, rather than the rating modulation being a mere artefact of the free-choice paradigm. These results are not in line with those found by Sharot, Velasquez, et al. (2010) of an increased preference for "chosen" stimuli in a blind-choice paradigm (stimuli that participants had the impression of choosing but actually did not).

However, even if, for chosen pictures in difficult pairs, rating modulation would have been more pronounced in the RCR than in the RRC sequence, it would not have been possible to conclude that there was truly a choice-induced preference modulation. This interpretation would have been limited by two observations. First, our results do not enable us to rule out that a mere exposure effect (described in detail below) may be at work. This deserves attention because preference modulation, according to cognitive dissonance theory, is thought to occur only for difficult choices, whereas no modulation should occur when choice is easy (see Festinger, 1957). However, in our RCR condition, preference modulation for chosen stimuli was observed not only for difficult choices, but also for easy choices. A mere exposure effect, constituted by a preference increase for a given stimulus following repeated exposure (Zajonc, 1968), could account for this. Note that this mechanism is not restricted to either difficult or easy pairs, but may apply to both alike. During the second rating in RCR, participants had seen the stimuli one time more than they had in the second rating of RRC. Hence, in summary, the mere exposure hypothesis would predict increased ratings in RCR relative to RRC in both easy and difficult choices, which is what we observed. Therefore our data are consistent with both mere exposure effects and choice-induced preference modulation operating simultaneously for chosen stimuli in difficult pairs. In order to conclude that choice-induced preference modulation was unambiguously driving the effect, modulation would have had to be observed exclusively for difficult pairs.

Second, the results for rejected stimuli would have given further reason to consider the impact of the mere exposure effect. As in Sharot, Velasquez, et al.'s (2010) study, we did not find a preference decrease between Ratings 1 and 2 for rejected stimuli in difficult choices in the RCR condition. Moreover, in the RCR condition, there was an increase in cuteness ratings for rejected stimuli in easy choices. This may be because the stimuli in this experiment were baby pictures. On a speculatory note, it might be evolutionarily counterproductive to devalue baby cuteness, as cuteness elicits caretaking in adults (Glocker et al., 2009; Parsons, Young, Kumari, Stein, & Kringelbach, 2011) and could consequently be an important factor in babies' survival. Conceivably, a mere exposure effect may explain the observation that, for rejected pictures, in the RCR condition, there was no significant decrease in difficult choices but there was a preference increase in easy choices.

To further investigate the extent to which choices could truly impact preferences by ruling out this alternative and counteracting the putative specific rating bias when babies' cuteness is evaluated, we conducted a second experiment using stimuli for which previous research has demonstrated devaluations after rejections. Following Coppin et al. (2010), who obtained this result in a classical free-choice paradigm, we used olfactory stimuli. If there is a true preference modulation by choice, we should observe (a) a rating modulation for chosen smells in difficult choices—that is, an increased pleasantness from the first to the second rating; and (b) a rating modulation for rejected smells in difficult choices—that is, a decreased pleasantness from the first to the second rating session. Both this increase for chosen smells and this decrease for rejected smells should be more pronounced in the RCR than in the RRC condition.

EXPERIMENT 2

Method

Participants

Sixty University of Geneva students (53 females, 7 males; mean age $= 22.53 \pm 4.52$ years) took part in this experiment. Before starting the experiment, participants completed a consent form. All participants reported a normal sense of smell. They were individually tested and were asked not to wear any fragrance during the days of testing.

Stimuli

Thirty-two odours were used in this experiment. They were divided into two lists of 16 odours each. Their mean ratings are provided in Table 1.

Procedure

Experiment 2 was divided into two sessions, separated by 1 week. The time of day of the experimental session was unchanged for a given participant. Spreading the experiment across two sessions allowed us to include a larger number of odorants and to achieve enough trials for adequate comparison in each sequence of measurements. The two sessions were identical, except that they were conducted with two different lists of smells. We used the same two lists across all participants, but randomized the sequence of odours within each of the two lists, as well as the order of the two lists across sessions, between participants.

Similarly to Experiment 1, each session was divided into four parts: a first rating session, a first choice session, a second rating session, and a second choice session. For half of the odours presented in a session, participants had to make choices *between* the two rating sessions. For the other half, the choices were made *after* the two rating sessions. Participants were first asked to rate the pleasantness of the 16 odours presented in the session. On the basis of the participant's pleasantness ratings, eight pairs were created for

Table 1. *Names of the 32 Odors Used in Experiment 2 and Their Mean Ratings and Standard Deviations*

Odor	*Mean pleasantness before choice*	*Mean pleasantness after choice*
List 1		
Orange	6.64 (± 2.07)	7.00 (± 2.09)
Lavender	4.99 (± 2.49)	5.04 (± 2.43)
Basil	5.16 (± 2.63)	5.12 (± 2.58)
Thyme	4.61 (± 1.71)	5.38 (± 1.54)
Fig flower	5.08 (± 2.22)	4.67 (± 2.38)
Cake	5.35 (± 2.94)	4.90 (± 3.06)
Peach	7.00 (± 1.72)	6.78 (± 1.76)
Strawberry	6.66 (± 2.17)	7.03 (± 1.73)
Shampoo fragrance	7.87 (± 1.79)	7.95 (± 1.85)
Incense	4.05 (± 1.94)	4.13 (± 1.88)
Lichen	4.54 (± 2.12)	4.32 (± 2.16)
Orange blossom	5.01 (± 2.26)	5.02 (± 2.28)
Pineapple	6.94 (± 1.98)	7.00 (± 2.03)
Chocolate	6.03 (± 2.45)	6.03 (± 2.08)
Lime	6.04 (± 1.85)	5.88 (± 2.07)
Tobacco	6.87 (± 2.06)	6.67 (± 1.84)
Magnolia	5.48 (± 2.05)	5.56 (± 1.94)
Leather	2.67 (± 1.93)	2.70 (± 1.85)
List 2		
Stone pine	5.53 (± 2.00)	5.72 (± 2.10)
Aladinate (floral note)	3.65 (± 2.35)	3.55 (± 2.29)
Violet flower	6.18 (± 1.90)	6.16 (± 1.67)
Lilac flower	6.60 (± 2.10)	6.82 (± 2.11)
Melon	3.06 (± 1.96)	2.82 (± 1.69)
Mint	6.62 (± 2.12)	6.77 (± 2.27)
Sandalwood	4.16 (± 1.98)	4.04 (± 1.75)
Tutti fruiti	6.60 (± 2.76)	6.89 (± 2.29)
Vetyver (woody note)	2.49 (± 1.86)	2.81 (± 1.91)
Mango	5.71 (± 2.41)	5.88 (± 2.20)
Honey	4.96 (± 2.18)	5.37 (± 1.86)
Beer	3.00 (± 1.98)	3.13 (± 2.09)
Rosemary	5.95 (± 2.17)	6.01 (± 2.07)
Standard soap fragrance	6.29 (± 2.38)	6.80 (± 2.12)

use in the choice sessions. Half of these pairs (four pairs) were randomly selected and then presented for choice in the first choice session, which occurred right after the first rating session. In the choice session, participants were required, for each pair, to indicate which smell they preferred.

Next, participants were asked to rate the 16 smells again in terms of pleasantness (second rating). Finally, participants were presented with the four remaining pairs in the second choice session (similar to the first choice session).

Data analysis

The statistical analyses conducted for this experiment were similar in every respect to those conducted in Experiment 1. We split the eight pairs of comparisons available in this experiment into two groups, following the same procedure as that in Experiment 1—the four easiest pairs (mean rating differences = 2.91, $SD = 0.86$, on the 10-point subjective scale described above) and the four most difficult pairs (mean rating differences = 0.14, $SD = 0.16$, on the 10-point subjective scale described above)—on an individual participant basis. Again, the four-way interaction [choice (chosen, rejected) by pairs (difficult, easy) by procedure (RRC, RCR) by ratings (R1, R2)] failed to reach statistical significance, $F(1, 59) = 0.27$; $p = .602$. As for Experiment 1, planned contrasts were run for easy and difficult pairs, but also for chosen and rejected pictures separately. To control for multiple comparisons (i.e., Type I error), we used the Bonferroni correction ($p = .05/8 = .00625$).

Results

Chosen stimuli

First, we checked whether an increased preference was observed between Rating 1 and Rating 2. To do so, we performed repeated measures ANOVAs on pleasantness ratings with rating (Rating 1, Rating 2) and sequence of measurement (RCR, RRC) as within-subject factors. As expected for difficult pairs, pleasantness ratings were significantly increased in the second rating session, in both the RCR and the RRC conditions [planned contrasts, $Fs(1, 59) = 18.43, 16.84$, $ps < .001$, $\eta^2s = .24, .23$] (see Figure 2). In easy choices, pleasantness ratings were not significantly different in the second rating session either for the RCR or for the RRC condition [planned contrasts, $Fs(1, 59) = 0.94, 1.85$, $ps = .338, .179$].

Second, we checked whether preference modulation between Rating 1 and Rating 2 was increased

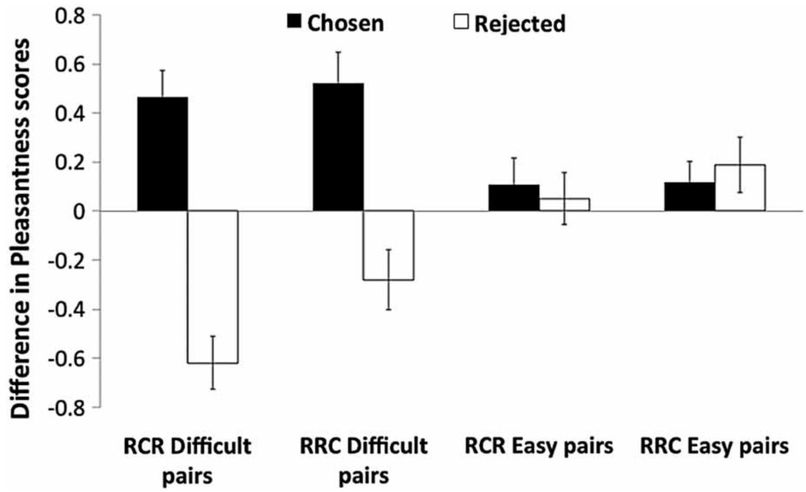

Figure 2. *Pleasantness signed difference scores between Rating 1 and Rating 2 from Experiment 2 according to the sequence of measurements, Rating 1–choice–Rating 2 (RCR) and Rating 1–Rating 2–choice (RRC), and the difficulty of the choice (difficult, easy). Error bars represent the standard error of the mean.*

on the RCR sequence of measurement when compared to the RRC sequence. Planned contrasts were performed on the signed difference between the pleasantness score during Rating 2 minus the pleasantness score during Rating 1.[4] There was no significant difference between the RCR and the RRC conditions for both difficult and easy choices: $Fs(1, 59) = 0.15, 0.01$, $ps = .704, .943$, respectively.

Rejected stimuli

Pleasantness ratings were significantly decreased in the second rating session in comparison to the first rating session in difficult choices in the RCR but not in the RRC condition [planned contrasts, $Fs(1, 59) = 33.42, 5.17$, $ps < .001, .027$, $\eta^2 s = .36, .08$]. For easy choices, pleasantness ratings in the RCR condition were not significantly different in the first and second rating sessions [planned contrasts, $F(1, 59) = 0.23$, $p = .630$], and the same was true in the RRC condition [planned contrasts, $F(1, 59) = 2.79$, $p = .01$, $\eta^2 = .05$].

As in Experiment 1, we checked whether preference modulations between Rating 1 and Rating 2 were larger in the RCR sequence of measurement than in the RRC sequence. To this end, we first computed the individual signed differences between pleasantness scores during Rating 2 minus those during Rating 1. A repeated measures ANOVA with the within-subject factor sequence of measurement (RCR, RRC) on these difference scores revealed that they were indeed significantly higher in the RCR condition than in the RRC condition, $F(1, 59) = 5.58$, $p = .021$, $\eta^2 = .09$ (see Figure 2). For easy choices, there was no such difference [planned contrast, $F(1, 59) = 0.97$, $p = .328$].

Magnitude of the rating modulation induced by choice

Finally, to test the global impact of choice on pleasantness ratings, we checked whether the absolute difference between Rating 2 and Rating 1 for chosen stimuli and between Rating 2 and Rating

[4]The repeated measures ANOVA on the factors choice (chosen, rejected) and sequence (RCR, RRC) on these scores did not reach significance, $F(1,59) = 2.357$, $p = .013$. However, as mentioned before, because of the possibility that differential psychological processes underlie postchoice hedonic modulation for chosen and rejected stimuli, we have conducted separate planned contrasts for chosen and rejected stimuli.

1 for rejected stimuli was higher in the RCR than in the RRC condition. There was no significant difference between the RCR and the RRC conditions for difficult choices, $F(1, 59) = 0.20$, $p = .659$.

Discussion

The data of this second experiment show a preference modulation for both chosen and rejected smells in difficult choices in the RCR condition. We also find a preference modulation for chosen smells in difficult choices in the RRC condition. These results replicate those found in a previous study (Coppin et al., 2010) that measured preference modulation for smells in a classical version of the free-choice paradigm—that is, using the RCR condition exclusively. Additionally, as in Experiment 1, the fact that there was a preference modulation for chosen smells in the RRC condition shows that rating modulations can be observed in the free-choice paradigm even when this cannot possibly be driven by choice, by construction. Moreover, for chosen smells in difficult pairs, there was no difference between RCR and RRC. A specific choice-induced preference modulation could consequently not be demonstrated for chosen stimuli. Combined with previous results (Chen & Risen, 2010; Izuma et al., 2010), such findings underscore the relevance of Chen and Risen's (2010) point.

Cognitive dissonance theory does not predict preference modulation in easy pairs in the free-choice paradigm (Festinger, 1957). Preference was not significantly increased for chosen stimuli in easy choices within both the RCR and the RRC conditions. Contrary to Experiment 1, it is unlikely that a mere exposure effect can account for the results that we found for chosen stimuli in difficult pairs.

Finally, for rejected stimuli in difficult pairs, the pleasantness decrease between the two ratings was higher in the RCR than in the RRC condition. A mere exposure effect cannot account for this, because the additional exposure in RCR would imply a pleasantness increase, rather than the decrease that we observed. Hence, this result suggests that there was truly a choice-induced preference modulation. These empirical results are consistent with, and extend, those obtained by Izuma et al. (2010), who showed a significantly larger decrease for rejected food items in the RCR sequence than in the RRC sequence.

However, the absolute *combined* difference between Rating 2 and Rating 1 for chosen stimuli and between Rating 2 and Rating 1 for rejected stimuli did not depend on the sequence of measurement. This result limits the broadness of the conclusion drawn from Experiment 2 that choice can impact preferences—it restricts this influence to rejected smells.

GENERAL DISCUSSION

In these two experiments, the classically reported increased hedonic rating modulation (measured via cuteness ratings in Experiment 1 and via pleasantness ratings in Experiment 2) observed between the first and the second rating sessions for chosen stimuli in difficult choices was observed in both the RCR and the RRC conditions. The classic decrease in hedonic ratings was observed for rejected stimuli for the RCR condition, but only in Experiment 2. Preference modulation for difficult choices in the RRC sequence, where, by construction, choice cannot impact the ratings, replicates Chen and Risen's (2010) and Izuma et al.'s (2010) experimental results. Taken together, these results support Chen and Risen's (2010) and Risen and Chen's (2010) argument that classically reported rating modulation in the free-choice paradigm might be observed without any genuine change in preference.

In Experiment 1, the hedonic modulation was not significantly higher in the RCR condition than in the RRC condition for chosen stimuli in difficult pairs. Even if this difference had been statistically significant, this result could not have been unambiguously interpreted in terms of preference modulation driven by choice classically associated with the free-choice paradigm (Brehm, 1956). This is because in the RCR condition, participants have been exposed to the pictures one more time than they have in the RRC condition when they rate the pictures for the second time. Consequently, it would not have been possible to know whether the difference in magnitude of the rating modulation between RCR and RRC conditions for chosen stimuli was driven by a mere exposure effect or by a true modulation of preference by choice.

Experiment 2 partially disambiguates this question: Its results show a higher devaluation of rejected stimuli in the RCR condition than in the RRC after difficult choices. This devaluation of rejected items cannot be explained by the mere exposure effect, because the additional exposure in RCR would result in a preference increase rather than in the observed decrease. Hence, Experiment 2 constitutes evidence in support of the idea that choice can indeed modulate preferences. However, as the absolute difference between Rating 2 and Rating 1 for chosen stimuli and between Rating 2 and Rating 1 for rejected stimuli did not depend on the sequence of measurement, this evidence is only partial and is limited to rejected smells.

Whereas we used baby pictures in Experiment 1, we used olfactory stimuli in Experiment 2. In difficult pairs in Experiment 1, we failed to observe a higher preference modulation in the RCR than in the RRC sequence for both chosen and rejected stimuli. In contrast, in Experiment 2, this modulation was observed for rejected stimuli. Why might this be? Could choice-induced preference modulation differ as a function of stimulus types or sensory modalities? And if so, how could the adaptive purposes of each class of stimuli drive this differentiation?

In Experiment 1, the absence of cuteness devaluation for rejected stimuli in difficult pairs could be due to the use of baby pictures—stimuli of particular relevance to human adults (e.g., Brosch et al., 2008). As cuteness elicits caretaking behaviours (e.g., Glocker et al., 2009; Parsons et al., 2011), devaluating a baby's cuteness might be more harmful, and consequently less easily done, than for other types of stimuli. This explanation does not, however, account for the absence of significant results for chosen stimuli.

In Experiment 2, the higher hedonic rating modulation in the RCR sequence than in the RRC sequence in difficult pairs was specific to rejected stimuli. The hedonic tone of chemosensory stimuli is highly flexible (Coppin & Sander, 2011). A recent review of the functions of olfaction notably argues that "odors are especially adept at eliciting negative emotions in humans" (Stevenson, 2010, p. 14). Because of the high adaptive relevance of unpleasant odours, and in order to prevent potential harmful features, it could be difficult to evaluate a malodour more positively, even a familiar one (Delplanque et al., 2008), but easy to devalue it. The flexible character of the pleasantness of smells, in particular in terms of pleasantness devaluation, could explain the higher impact of choice on rejected rather than on chosen smells. These results extend Izuma et al.'s (2010) findings of a higher decrease in pleasantness ratings of visually presented food items in the RCR condition than in the RRC one. However, in contrast to the participants in Izuma et al.'s study, our participants were not presented with their previous choice while reevaluating the stimuli during the second rating session in the RCR condition.

In Chen and Risen's (2010) article, the question of whether "including a parameter for dissonance reduction, learning, or drift in preferences" (p. 585) in the model will better describe the data remained open. Our results seem to indicate that the answer is "yes." Results suggest that the free-choice paradigm can be used to measure a true preference modulation induced by choice if the appropriate controls are added. This modulation, however, seems to be much less strong than what was believed before Chen and Risen made their methodological argument (see Holden, 2013), underscoring the importance of their contribution.

However, the RRC sequence, suggested by Chen and Risen (2010) as a control for the classical free-choice RCR sequence, might not be the ultimate control for this paradigm. As elaborated earlier, the RRC control might induce potential confusion between *true modulation of preferences by choice* and *mere exposure effect*, which may limit the interpretability of studies using this method.

The mere exposure effect therefore appears as an important third factor to consider while trying to disentangle rating modulation that is driven by choice (classical free-choice paradigm interpretation) from rating modulation that is driven by errors in translating underlying stable “true” preferences into rating measures (Chen & Risen’s, 2010, argument). Moreover, the RCR and RRC conditions have other intrinsically different components that might impact ratings differently. In the RRC condition, the second rating might influence the choice. Participants could base their choice on the second rating they have made, trying, for example, to be consistent in choosing the smell they remembered to have rated the highest. This cannot be the case in the RCR condition, as choice is made before the second rating. The RCR measurement does not consequently constitute the ultimate control for the classical RCR sequence. For the future, it seems crucial to adopt a reliable and nonambiguous strategy to “fix the free-choice paradigm” (Risen & Chen, 2010).

Conclusion

In summary, the reported empirical evidence is consistent with Chen and Risen’s (2010) point that part of the effect measured in classical free-choice paradigm studies, which was previously exclusively interpreted as postchoice preference modulation, may in fact be an artefact of the measurement method. However, results also show that the modulation of preferences by choice can still be partially demonstrated empirically, when using the free-choice paradigm with the addition of a suitable control condition.

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