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Reports

Choices and preferences: Evidence from implicit choices and response times

Carlos Alós-Ferrer *, Đura-Georg Granić, Fei Shi, Alexander K. Wagner

University of Konstanz, Germany

HIGHLIGHTS

- ► Our design improves on the free-choice paradigm avoiding selection bias.
- ► Choices change preferences even if options are not directly compared to each other.
- ► Response times show that reappraisal processes are triggered in the choice phase.
- ► Response times are shorter for choice pairs leading to larger post-choice spreads.
- ► The larger the distance in pre-choice ratings, the faster the choice is made.

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ABSTRACT

We present a new experimental paradigm where choice-induced preference change is measured for alternatives which are never compared directly, but rather confronted with other alternatives in a way which keeps choices predictable without exogenously manipulating them. This implicit-choice design improves on the free-choice paradigm, avoiding the recently criticized selection bias. Rating and ranking spreads in two experiments show that preference-based choices feed back into and alter preferences even if choices are not directly among similarly evaluated alternatives. In agreement with recent brain-imaging evidence, response time measurements for direct choice pairs in our experiments indicate that reappraisal processes are already triggered during decision making, with larger post-choice spreads (sharper attitude change) being associated to quicker decisions.

Introduction

The phenomenon of choice-induced attitude change follows a long tradition in psychology (see Ariely & Norton, 2008). According to cognitive dissonance theory (Festinger, 1957), a decision between two similarly valued alternatives creates a psychological tension (dissonance) mediated by the desirable aspects of the unchosen alternative and the undesirable aspects of the chosen one. This tension is reduced by a post-choice re-evaluation of the alternatives, the chosen item being evaluated more positively and the rejected item more negatively.

Experimentally, the phenomenon that choices alter preferences was first demonstrated within the free-choice paradigm (FCP) by Brehm (1956). In this paradigm, participants rate a variety of objects like holiday destinations or household goods on desirability. The participants then make choices between pairs of items which they have previously rated as similarly desirable and finally re-rate all objects to assess the consequences of choices on preferences. A large number of studies has found positive spreading in this paradigm, where chosen alternatives are re-rated as more desirable and rejected ones as less desirable (Harmon-Jones & Mills, 1999).

We present an improved variant of the FCP and examine post-choice preference change both at the behavioral level (through choices and attitudes) and at the process level (through response times). Our work is motivated by two recent developments on cognitive dissonance and especially the FCP. The first development concerns the question of when processes of attitude reappraisal occur. Recent neuroscientific evidence implies that processes of attitude change might be already engaged during the act of choice (as opposed to a later act of re-evaluation). Specifically, Jarcho, Berkman, and Lieberman (2011) found activity in brain regions associated with reappraisal processes and conflict resolution during the decision phase of the FCP. In line with these results, we hypothesize that the onset of such processes in the decision phase can also be pinned down through the analysis of response times associated to choices. The second development concerns a critique on the FCP recently raised by Chen (2008) and Chen and Risen (2010). These authors suggested that studies of choice-induced preference change might suffer from a selection bias. Observed post-choice rating spreads are, according to their argument, nothing but a statistical bias that occurs merely because choices are derived from preferences. Although the mathematical argument provided by these authors has been

* Corresponding author at: Department of Economics and Center for Psychoeconomics, Box 150, University of Konstanz, 78457 Konstanz, Germany. Fax: +49 7531 88 4119.
E-mail address: carlos.alos-ferrer@uni-konstanz.de (C. Alós-Ferrer).

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challenged by Alós-Ferrer and Shi (2011),[1] the debate initiated by Chen (2008) has made clear the need for improved experimental designs to help clarify the impact of choices on preferences. For instance, Chen and Risen (2010) suggest comparing the results of a standard FCP with those of an additional treatment where choices are made after two rating phases. Izuma et al. (2010) follow this approach and still find significant effects.

Before we present our own design, it is important to observe that a remarkable variant of the FCP answering Chen and Risen's (2010) critique was developed by Sharot, Velasquez, and Dolan (2010). Ostensibly participating in a study on "subliminal decision making", the participants were made to believe to have made choices on the basis of subliminal information. In reality, presented stimuli were non-sensical strings and choices were exogenously determined by the experimenter. This blind-choice design escapes the above mentioned selection bias because choices are independent of underlying preferences.[2] Since positive spreading is still observed, the paradigm delivers strong evidence in favor of (perceived) choice-induced attitude change.

The design of Sharot et al. (2010), however, is not appropriate for our main research questions. First, as no choices are actually made by the participants, it is impossible to obtain process data associated to the act of free choice. Second, the design does not test the effect of preference-based choices on preferences: since choices are externally determined by the experimenter, it is not clear whether choices which follow from the participant's own preferences also alter those preferences. The observed blind-choice-induced preference change might be due to an artificial dissonance where the participants rationalize their alleged choices, i.e., wrongly perceive the blind choice as informative about their underlying preferences. The experimental design we present below avoids the selection bias pointed out by Chen and Risen (2010) without manipulating choices exogenously. Because choices are derived from underlying preferences, we are able to measure process data during actual choices and can test whether preference-based choices alter participants' original preferences.

The present research

Imagine a decision maker who is asked to evaluate four meals: liver, Italian pasta, Chinese pasta, and chicken. She reports being almost indifferent between the two pasta dishes, loving the liver, and disliking the chicken. The FCP would then ask her to choose among the two pasta dishes and measure attitude change after the choice. A selection bias (Chen & Risen, 2010) arises because the classification into chosen and unchosen objects endogenously depends on the participant's decision. Sharot et al. (2010) avoid this caveat by taking the choice away from the participant. In their blind choice design, the experimenter randomly declares one of the pasta dishes (say, the Italian pasta) as the chosen one before informing the participant about her alleged subliminal choice of Italian over Chinese pasta. Hence there is no selection bias but also no actual choice.

In our paradigm, we aim to measure attitude change also by randomly precommitting to one of the pasta dishes (say, Italian) as chosen and the other one (say, Chinese) as unchosen. The crucial difference is that the two pasta dishes are not presented as a direct choice pair but as an implicit choice pair. We construct a choice pair with Italian pasta (which we want the participant to choose) and the chicken. The most likely result is that the Italian pasta is freely chosen from this pair. Similarly, we construct another choice pair consisting of Chinese pasta (which we do not want the participant to choose) and the liver she loves. Most likely, she does not choose the Chinese pasta. Although the two pasta dishes (which form an implicit choice pair) are never presented directly, we combine each of the two pasta dishes with another option (which form a direct choice pair) to ensure that, quite likely, one of the pasta dishes is freely chosen and the other not. In this way, choices in the direct pairs are predetermined and follow from underlying preferences. However, which of the items within an implicit pair is chosen or not (against alternatives outside the implicit pair) is randomly determined.

This paper reports on two experiments that share the implicit choice pair design outlined above. After the first rating task (Experiment 1) or ranking task (Experiment 2) we measure post-choice attitude change within a pair of objects. We randomly assign one of the alternatives in the pair to be chosen (called b) and the other (called a) not to be chosen. This random assignment avoids any possible selection bias. In contrast to the classical FCP, the participant is never presented with the choice (a,b) directly. She is instead presented with two different, independent choices: (a,h) and (b,ℓ). The options h and ℓ are selected in such a way that the participant rated h strictly higher than a and b, and ℓ strictly lower than a and b. Since choices reflect preferences, the most likely result is that b is indeed chosen and a is not. Whether they are chosen or not refers to two different decisions; but those decisions are still derived from the participant's preferences. Most of the time, the participant freely selects as chosen/unchosen the alternative randomly predetermined by the experimenter. The price we need to pay for allowing free, noisy decisions is that choices occasionally contradict the participant's preferences, hence violate expectations. The observation of systematic, positive spreading in implicit choice pairs (a,b) however delivers clear evidence for the impact of preference-generated choices on preferences while escaping the selection bias critique.

Data analysis proceeds in three steps. First, the design generates data on direct choices allowing a comparison with previous FCP studies. This data is susceptible to criticism along the lines of Chen and Risen (2010), nevertheless, it allows for a general validation of our implementation of the FCP. Second, the construction of implicit choice pairs enables us to establish post-choice attitude changes within the framework of preference-based choices while escaping the selection bias problem. Third, since choices are free, we analyze response times for the participant's (direct) choices to obtain evidence on reappraisal processes.

Resolving the tension postulated by cognitive dissonance theory requires a detection of the conflict in the choice and then its resolution through ex-post shifts in alternative valuations. Independently of whether the underlying processes of detection and resolution take place unconsciously or deliberately, they should be more time-consuming than in the case where no underlying tension exists. Hence, a first natural hypothesis is that a larger distance in pre-choice ratings between elements of a choice pair generates less dissonance, leading to shorter response times. This hypothesis is in accordance with observations by Shultz, Léveillé, and Lepper (1999) and Sharot, De Martino, and Dolan (2009).

A second hypothesis is that response times are an indicator of the degree of tension being resolved. Jarcho et al. (2011) found a significant positive correlation between the degree of attitude change

[1] Chen and Risen (2010) state as a theorem that, in any formal model where both choices and ratings are noisy perturbations of underlying preferences, under a set of reasonable assumptions, positive spreading of alternatives occurs even without choice-induced preference change. Alós-Ferrer and Shi (2011) construct a model fulfilling all assumptions in Chen and Risen (2010) and show that post-choice spreading need not be positive. Hence, the mentioned theorem is false. The problem is that the proof of Chen and Risen (2010, p. 578) contains a mistake. The authors crucially use that if the difference in ratings between two alternatives in the first rating phase is D, the expected difference in ratings in the second choice phase is also D. This is incorrect, because it confounds one observation of a random variable (the realization in the first rating phase) with its expectation, and the latter depends on the underlying preferences.

[2] The use of blind choice was suggested by Sagarin and Skowronski (2009a,b). Egan et al. (2010) used blind choice in a related paradigm involving two consecutive choices.

(measured by the spread in the FCP) and activity in brain regions associated to conflict resolution (right inferior frontal gyrus and the ventral striatum) during the choice phase. The degree of brain activity as measured by the BOLD signal in fMRI studies, however, does not immediately deliver a clear prediction for response times. It is conceivable that, controlling for the distance in pre-choice ratings, stronger or more evident conflicts (resulting in larger attitude adjustments) are resolved more quickly and hence result in shorter response times. In any case, a significant effect of spreads on response times can be taken as additional evidence of the presence of reappraisal processes during decision making, confirming the interpretation of the results of Jarcho et al. (2011).

Experiment 1: ratings of holiday destinations

In Experiment 1, we implemented the implicit choice paradigm using rating tasks for attitude measurement. We predicted that post-choice rating changes would be observed, even though choices are both derived from preferences (as opposed to Sharot et al., 2010) and predictable (i.e., escape the selection bias critique of Chen & Risen, 2010).

Method

Participants

Forty university students (17 females, 23 males) from various disciplines, excluding majors in economics or psychology, participated in exchange for a payment of €10 (approximately $14 at the time of the experiment). Data from four participants (1 female, 3 males) was excluded from the analysis because their (heavily skewed) rating distributions did not allow for the construction of at least three quadruples (a,h,b,ℓ) for our main manipulation.

Procedure

The participants arrived at the laboratory and were greeted by the experimenter. The study was carried out in two group sessions (with 21 and 19 participants, respectively) with each participant seated in an individual, isolated workstation. The presented stimuli consisted of 80 different country names (e.g., Australia, Brazil, Sweden) framed as possible holiday destinations (following Sharot et al., 2010). The experiment was computer-implemented using *z-Tree* (Fischbacher, 2007) and consisted of a pre-choice rating task (R1), a choice task, and a post-choice rating task (R2). The rating task question was "How happy would you be to spend your next year's holiday at this destination?" In task R1, the participants rated alternatives on a unipolar 8-point rating scale (from 1 = *a bit happy* to 8 = *very happy*). Each destination was presented on the computer screen for 6 s (in random order). After this time, the rating scale was displayed below the destination and the participants had 20 s to submit their rating. We gave the participants more time for each trial than in previous studies to avoid time pressure effects. In the choice task, each participant was sequentially presented with 16 pairs of destinations on screen and asked to choose (hypothetically) among the two destinations in each pair. A participant had 20 s for each decision. The sequence of pairs and the on-screen position of the stimuli (left–right) were fully randomized and no alternative was used twice. Pairs for the choice task were determined by an algorithm that searched for pairs (a,b) of equally rated destinations (i.e., at rating distance $D=0$), preferably at central rating scores of 4 or 5. Four such pairs were used to construct direct choice pairs. For each of the remaining (a,b) pairs, the algorithm searched for a destination h rated higher and a destination ℓ rated lower than a and b. Destinations were chosen in a symmetric way, i.e., the distance between options in the constructed pair (a,h) was equal to the distance in the pair (b,ℓ), with distances varying from $D=1$ to $D=3$. The algorithm attempted to randomly construct six (a,h) and six (b,ℓ) pairs, hence a total of six implicit pairs. This was possible for 21 participants. For 10 participants, only five implicit pairs could be constructed; for five further participants, only three or four such pairs were found. In case less than six such pairs were found, the algorithm added further direct pairs (a,b) of equally-rated alternatives to ensure that each participant faced exactly 16 choices. Direct choice pairs were included as a control and correspond to the classical FCP. The post-choice rating task (R2) was identical to R1. The participants were again presented with all 80 stimuli in randomized order and re-rated them. It was explicitly stated that the task was not a memory task to avoid consistency effects.

Results

Rating spreads were constructed as in previous studies (e.g. Sharot et al., 2010; Shultz et al., 1999). We standardized ratings of each participant (independently for R1 and R2) by replacing each rating x_i by the standardized value $(x_i-\mu_i)/\sigma_i$, where μ_i is participant i's average rating in the corresponding stage and σ_i is the standard deviation.[3] For each direct or implicit choice pair, we computed the rating spread as R2(chosen) − R2(unchosen) − (R1(chosen) − R1(unchosen)), with the chosen option determined in the choice task. For every statistical test below, a single observation is the average spread per participant for the appropriate type of choice pair.

Free choice

Each participant made at least four direct choices for pairs of the form (a,b) with $D=0$. With these pairs, we reproduced the classical effect in free-choice studies (Brehm, 1956). The rating spread was significantly larger than zero ($M=0.38$, $SD=0.30$; $t(35)=7.53$, $p<0.001$). We also considered every direct choice pair (of either $D=0$, 1, 2, or 3), that is, ignoring the implicit choice construction, and still obtained the same results ($M=0.26$, $SD=0.21$; $t(35)=7.44$, $p<0.001$). Similar results were obtained using only direct pairs with distance $D=1$ ($M=0.36$, $SD=0.52$; $t(35)=3.10$, $p<0.001$) or $D=2$ ($M=0.18$, $SD=0.39$; $t(35)=2.73$, $p<0.01$). However, the spread was not significantly different from zero for the extreme pairs with $D=3$ ($M=-0.01$, $SD=0.33$; $t(30)=-0.19$, $p=0.85$).[4] This is not surprising. Pairs with $D=3$ involve a clear choice, as people know exactly what they want, and hence are not expected to create dissonance in cognitive dissonance theory (Festinger, 1957). Further, with $D=3$ one of the destinations h or ℓ is always located at the boundary of the scale (1 or 8), and hence its rating either cannot be lowered or cannot be raised any further (boundary effect).

Implicit choice

We now turn to the spread analysis of implicit pairs (a,b) which were never presented to the participants in this form, but implicitly as part of pairs (a,h) and (b,ℓ). Our manipulation worked as intended: participants freely chose h in the pair (a,h) and b in the pair (b,ℓ) in 333 of 388 such pairs (86%). An implicit choice pair (a, b) was constructed from a quadruple (a,h,b,ℓ) whenever either a or b was chosen in its respective pair. This happened in 151 pairs (of which 145 were such that b was chosen and a was not, as intended). Other quadruples did not deliver implicit choice pairs (e.g. if the participant chose neither a nor b) and hence cannot be used. The analysis is conducted for the 151 pairs where one option was chosen and the other was not, including those where choices were not as expected. For each implicit choice pair (a,b), the distance between a and b was $D=0$. However, the two alternatives were compared to options

[3] Dividing by the standard deviation allows controlling for individual differences. None of the results depends on this particular transformation.

[4] The number of observations for each D was endogenous, that is constructed from a participant's R1 ratings. In some cases it was not possible to construct pairs with $D=3$.

h and ℓ at varying distances $D=1$, 2, and 3 from a,b. Results show a clear choice-induced preference change. Fig. 1 plots the spreads of all three types of implicit pairs. Pooling all data, the rating spread was significantly larger than zero ($M=0.25$, $SD=0.41$; $t(35)=3.75$, $p<0.001$). The result is confirmed by separate tests of implicit choice pairs with $D=1$ ($M=0.29$, $SD=0.64$; $t(28)=2.45$, $p=0.02$) or $D=2$ ($M=0.27$, $SD=0.58$; $t(33)=2.73$, $p=0.01$). As above, the effect was insignificant for implicit choice pairs with $D=3$ ($M=0.15$, $SD=0.61$; $t(30)=1.41$, $p=0.17$).

Robustness check

In 43 of the original 194 quadruples either a and b were chosen or none of them was, producing unusable quadruples. These quadruples do not affect the logic of our design, because, for each implicit pair obtained from a usable quadruple, which option is chosen and which is not is randomized, i.e., there is no selection bias. However, it is still reasonable to ask whether the analysis is robust to the inclusion of data from unusable quadruples, even though they cannot be analyzed as above. A natural robustness test is as follows. Rather than using the actual choices of participants, we simply use the randomized assignments, i.e., treat option b always as chosen and option a always as not chosen. In this way, all 194 quadruples deliver implicit choice pairs. Since for some of those pairs actual choices were not as exogenously predetermined, this imputation works against our design. The rating spread was also significantly larger than zero for this robustness check ($M=0.18$, $SD=0.35$; $t(35)=3.13$, $p<0.01$).

Response times

Fig. 2 depicts average response times for different distances. We observed a clear pattern of declining response times with larger distance. In order to quantify this observation while controlling for individual differences, we conducted a generalized least squares regression with random effects at the individual level. Each choice is an individual observation. Following Wilcox (1993) and Moffatt (2005), the dependent variable is the log of response time, since response times are in principle censored variables (the results, however, remain unchanged using untransformed response times). Table 1 (left-hand side) summarizes the regression results. Analogously to Jarcho et al. (2011), the rating spread was included as a regressor and had a significantly negative effect. The distance in pre-choice ratings (D) was found to have a significant negative impact on response times. We also included the square of the distance as a regressor but found no evidence of a non-linear relationship. We hypothesized that choices made against the initial rating should take significantly longer. Hence a dummy variable (Choice-Against) was included which takes the value 1 if the choice was inconsistent with the ratings expressed in R1, i.e., the chosen option was rated strictly below the unchosen option. This variable had a highly significant positive effect on response times as expected. We included the round in which the decision was taken (Period) as a control but found no significant effect, that is, choices were not made faster as participants gained familiarity with the task.

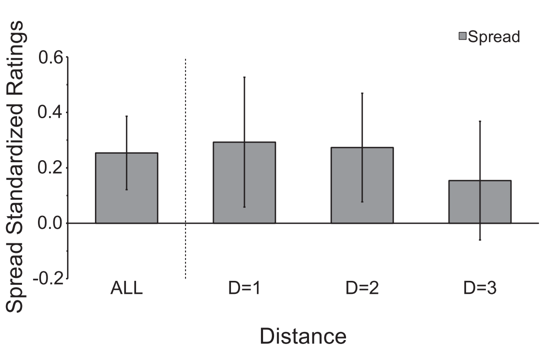

Fig. 1. Standardized rating spreads in Experiment 1 for implicit pairs (a,b), pooled and conditional on the distance to the alternatives h, ℓ.

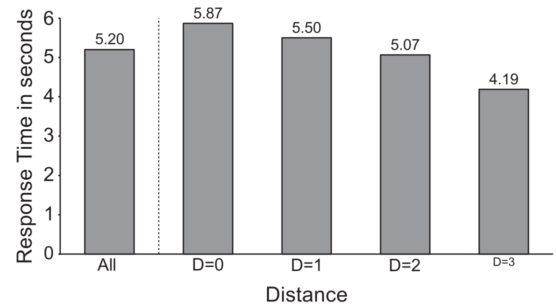

Fig. 2. Average response times in Experiment 1 for all direct pairs, pooled and conditional on the distance within the pair.

Discussion

The analysis of direct choices established the comparability of our data with the existing literature (which reports a systematic positive rating spread). Importantly, the results from our implicit choice approach showed that positive spreading of post-choice ratings is obtained even though the analysis is constrained to predetermined implicit pairs. This validates our paradigm as an improved version of the FCP, establishing post-choice attitude change in the absence of selection bias.

The analysis of response times confirmed our hypothesis that reappraisal processes occur already during the choice phase. More specifically, rating spread had a significant, negative effect: conflicts resulting in larger attitude adjustments were resolved faster. Additionally, the larger the distance in pre-choice ratings, the faster the choice was made. This is compatible with a cognitive dissonance interpretation and is also in accordance with the view that decisions between alternatives where the underlying preferences are clearer (larger distance) are taken more quickly.

The fact that choices take significantly longer when they conflict with initial ratings is consistent with the interpretation that ratings and choices reflect the same underlying preferences, but are both noisy. For instance, the previous rating might have been the result of an error which is corrected in the subsequent choice stage. In doing so, the participant experiences an additional conflict which increases response time.

Table 1
Random-effects GLS regression on response times for Experiment 1 and Experiment 2.

Parameter	Experiment 1 (ratings)			Experiment 2 (rankings)		
	b	$SE(b)$	p	b	$SE(b)$	p
Spread	−0.05	0.01	<0.001	−0.03	0.01	<0.001
D	−0.15	0.05	<0.01	−0.26	0.11	0.01
D-squared	0.01	0.02	0.40	0.05	0.03	0.06
Choice-Against	0.38	0.06	<0.001	0.21	0.03	<0.001
Period	0.00	0.00	0.94	0.00	0.00	0.54
Intercept	1.69	0.05	<0.001	1.55	0.10	<0.001
Number of obs.	576 (36 participants)			640 (40 participants)		
Wald-Chi^2	105.87			83.03		
Degrees of freedom	*df* (5)			*df* (5)		
p-Value (Wald-Chi^2)	<0.001			<0.001		
R^2 between	0.24			0.01		
R^2 within	0.16			0.12		
R^2 overall	0.16			0.08		

Note. Dependent variable: log(response time).

Experiment 2: rankings of holiday destinations

In many implementations of the free-choice paradigm, the participants are asked to rank alternatives relative to each other, rather than rating them on a scale (e.g. Chen & Risen, 2010; Gerard & White, 1983; Lieberman, Ochsner, Gilbert, & Schacter, 2001). This is a conceptually important difference since ratings are a cardinal measure and rankings are a purely ordinal measure. Ranking experiments have the advantage of preserving the ordinal approach to preferences which is typical of other decision sciences. However, the participants are not allowed to express indifference between objects in a ranking. As a consequence, the construction of choice pairs with equally-ranked alternatives is not feasible. The FCP is instead implemented with alternatives ranked as close to each other as possible.

The aim of Experiment 2 was to establish that the experimental phenomenon described here is not limited to a purely cardinal approach. Hence, we adapted our implicit choice paradigm to the case of rankings. We predicted that the results of Experiment 1 regarding ranking spreads and response times would hold for rankings as well.

Method

Participants

Forty university students (24 females, 16 males) from various disciplines, excluding majors in economics or psychology, participated in exchange for payment as in Experiment 1. No participant was excluded from the analysis.

Procedure

The experiment used the identical stimuli and general procedure as in Experiment 1. For ranking tasks R1 and R2, the 80 holiday destinations were divided randomly (for each participant) into 10 different groups containing 8 destinations each. The participants were presented with each group sequentially and ranked destinations from most preferred to least preferred using a computer interface. There was no time pressure. The composition of the 10 groups of destinations remained fixed across ranking tasks R1 and R2 (but they were presented in random order). For each group, we used the participant's R1 rankings to build the choice pairs. Alternatives *a* and *b* in a choice pair were always the 4th- and 5th-ranked alternatives within a group. For implicit-choice pairs, the associated alternatives *h* and ℓ were chosen as those located at distance *D* above *a* and below *b* (symmetrically), for $D=1$, 2, and 3. For instance, for $D=2$, *h* was the 2nd-ranked and *l* the 7th-ranked alternative. For every participant, we constructed two quadruples with $D=1$, two with $D=2$, and two with $D=3$, using a total of six groups of destinations. From the remaining four groups, we created four direct choice pairs (*a*,*b*) using the 4th and 5th-ranked alternatives. Except for the construction of the pairs, the choice task was identical to that of Experiment 1.

Results

As in Experiment 1, average spreads per participant are treated as individual observations for the study of preference change. Ranking spread was constructed as the rating spread with the exception that no standardization was necessary as the available rankings (1 to 8) were fixed.

Free choice

Restricting ourselves to direct pairs with $D=1$, we again reproduced the FCP effect. The ranking spread was significantly larger than zero ($M=0.97$, $SD=0.44$; $t(39)=13.95$, $p<0.001$). The same effect was obtained using all observations, i.e., all choices with $D>0$ ($M=0.86$, $SD=0.51$; $t(39)=10.61$, $p<0.001$). Similar results were obtained using only pairs with $D=2$ ($M=0.95$, $SD=1.06$; $t(39)=5.66$, $p<0.001$). For $D=3$ ($M=0.54$, $SD=0.96$; $t(39)=3.59$, $p<0.001$), the conclusion differs from that in Experiment 1 (where results for $D=3$ were not significant).

Implicit choice

We now turn to the analysis of implicit pairs (*a*,*b*) which form the core of our paradigm. In 372 of the 480 pairs of the form (*a*,*h*) or (*b*,ℓ) decisions were as intended (77.5%), i.e., participants freely chose *h* or *b*, respectively. This compliance rate is lower than in Experiment 1, possibly because participants were unable to declare indifference between two alternatives by assigning the same numerical ranking to them. We obtained 163 implicit choice pairs, i.e., quadruples (*a*,*h*,*b*,ℓ) where exactly one of the alternatives *a*, *b* had been chosen; for 146 of those, both decisions were as intended. The analysis uses all 163 implicit choice pairs, i.e., including those where choices were not as expected. For each implicit choice pair (*a*,*b*), the distance between *a* and *b* was $D=1$. However, they were compared to options *h* and ℓ at varying distances ($D=1$, 2, and 3) from *a*,*b*. The results again exhibit a clear choice-induced preference change. Fig. 3 depicts the spreads of all three types of implicit pairs. Pooling all data, the ranking spread was significantly larger than zero ($M=1.18$, $SD=1.53$; $t(39)=4.86$, $p<0.001$). This was also confirmed by looking at implicit choice pairs with $D=1$ ($M=1.24$, $SD=2.32$; $t(28)=2.88$, $p=0.01$), $D=2$ ($M=1.58$, $SD=2.00$; $t(35)=4.75$, $p<0.001$), and $D=3$ ($M=0.61$, $SD=1.69$; $t(37)=2.20$, $p=0.03$). Last, an ANOVA analysis using ranking spread as dependent variable and distance as independent variable did not detect a significant spreading difference between different distances ($F(2,93)=2.30$, $p=0.11$).

Robustness check

As in Experiment 1, we conducted a robustness test by reanalyzing the data assuming that all exogenously predetermined choices (choose *b*, do not choose *a*) were as intended, independently of actual choices. Hence, we obtain 240 pairs. The ranking spread was again significantly larger than zero ($M=0.47$, $SD=1.14$; $t(39)=2.60$, $p=0.01$).

Response times

Fig. 4 depicts the average response times for different within-pair distances for Experiment 2. Response times were quicker for larger distances ($D=2$, 3) than for $D=1$, but no clear monotonic relation at this level of aggregation was evident. Therefore, we turned to a generalized least squares regression with random effects at the individual level as in Experiment 1. It must be remarked that we necessarily treated rankings as a cardinal variable (because ranking spreads are computed as a numerical difference). Hence we did not expect a better fit than in Experiment 1. Table 1 (right-hand side) summarizes the results. As in Experiment 1, both the spreading of alternatives and the within-pair distance had a significant negative effect on response times. Unlike in Experiment 1, the squared distance had a positive, marginally significant effect. The

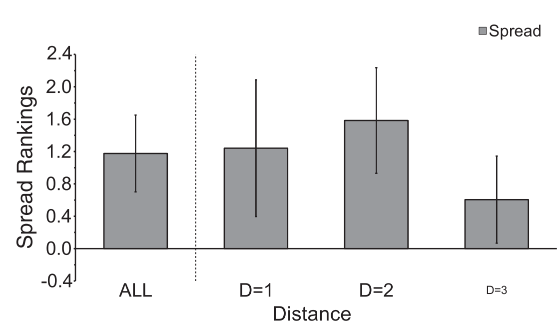

Fig. 3. Ranking spreads in Experiment 2 for implicit pairs (*a*,*b*), pooled and conditional on the distance to the alternatives *h*, ℓ.

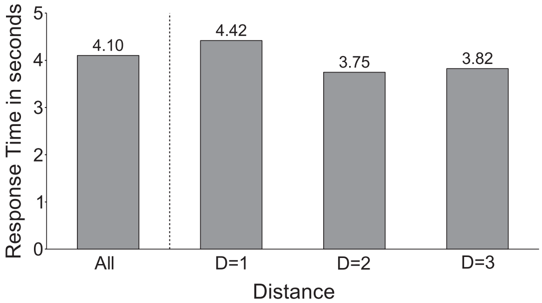

Fig. 4. Average response times in Experiment 2 for all direct pairs, pooled and conditional on the distance within the pair.

Choice-Against dummy had again a highly significant positive effect on response times, that is, decisions against the initial ranking took longer.

Discussion

Experiment 2 confirmed the results of Experiment 1. As expected, post-choice attitude change was established both for direct and implicit choices. The significant effect of ranking spread on response times is consistent with the interpretation that the onset of reappraisal processes occurs already during the choice phase. As in Experiment 1, we found that a larger distance in pre-choice rankings leads to a faster choice. We also found some evidence toward a non-linear relation which is in accordance with previous observations in economics (Moffatt, 2005).

Positive ranking spread was also observed for the case of extreme distances, $D=3$, while this effect was absent in the case of ratings (Experiment 1). However, since rankings are an ordinal measure, a participant might be close to indifference between two options and still rank them far apart, and vice versa. While ratings force participants to provide a cardinal measure anchored on a specific scale, rankings are merely ordinal comparisons, and hence $D=3$ cannot be interpreted as an extreme distance as in the case of ratings.

General discussion and conclusion

We have presented an experimental paradigm improving on the classical free-choice paradigm and implemented it for ratings (Experiment 1) and rankings (Experiment 2). Results are robust to the method of attitude measurement. In this paradigm, choices are based on preferences, enabling us to show that preference-based choices feed back into preferences and alter them. At the same time, selection bias is avoided because studied choices, in spite of being free, are randomized within each pair. In addition to blind choice studies (Egan, Bloom, & Santos, 2010; Sharot et al., 2010) and brain imaging studies (Izuma et al., 2010; Jarcho et al., 2011; Sharot et al., 2009), our results corroborate that the data on which cognitive dissonance theory was initially based do reflect a very real phenomenon, even if the original paradigm could be improved upon.

The study of response times for actual choices allowed us to confirm that the onset of reappraisal processes occurs during the act of choice, as previously indicated by the brain-imaging study of Jarcho et al. (2011). We find two qualitatively different effects. The first effect is that choosing among alternatives which have been previously evaluated more closely (i.e., where the decision maker is closer to being indifferent) results in longer response times. This is consistent with observations from both psychology (Izuma et al., 2010; Sharot et al., 2009; Shultz et al., 1999) and economics (Moffatt, 2005; Wilcox, 1993), which point out that response times reflect preferences. This is consistent with a dissonance interpretation (since closer alternatives presumably create larger dissonance when a choice is called for), but it might also simply indicate that closer options require a more complex decision-making process.

The second effect is that a larger rating/ranking spread results in shorter response times. This novel observation is consistent with an interpretation of alternative spreading as the intensity with which the conflict arising from dissonance has been resolved. Under this interpretation, a larger attitude spread would be associated with a sharper correction, which in turn occurs when the conflict is easier to resolve (hence resolved in shorter time).

In our framework, implicit choices were not actual, direct choices, in the sense that each of the two alternatives within the pair was actually part of a different pair presented to the participant. According to cognitive dissonance theory (Festinger, 1957), dissonance is created within the actual direct choices. The simplest interpretation of our results is that the positive spreading of alternatives within an implicit choice pair results from an independent reappraisal of each of the two alternatives, which arises because each of them is part of a different direct choice pair.

A possible, alternative interpretation of the results is as follows. Since reappraisal processes seem to start during choice, it is conceivable that the mere act of choosing one option and not choosing another, similarly ranked/rated option creates a dissonance, even if no direct choice is involved between the two. This interpretation would also be compatible with the results of Sharot et al. (2010). In that study, the participants made no actual choices, hence spreading resulted from the belief that a choice was made, but could not result from the deliberation processes leading to that choice. Under this interpretation, the actual mediator of preference change would be the act of choice itself, and not exclusively the direct choice between two similarly-ranked options.

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