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# The Elasticity of Preferences

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**Abstract**
We explore how preferences for attributes are constructed when people choose between multiattribute options. As found in prior research, we observed that while people make decisions, their preferences for the attributes in question shift to support the emerging choice, thus enabling confident decisions. The novelty of the studies reported here is that participants repeated the same task 6 to 8 weeks later. We found that between tasks, preferences returned to near their original levels, only to shift again to support the second choice, regardless of which choice participants made. Similar patterns were observed in a free-choice task (Study 1) and when the favorableness of options was manipulated (Study 2). It follows that preferences behave in an elastic manner: In the absence of situational pressures, they rest at baseline levels, but during the process of reaching a decision, they morph to support the chosen options. This elasticity appears to facilitate confident decision making in the face of decisional conflict.

**Keywords**
decision making, constructed preferences, elasticity, coherence-based reasoning, cognitive-consistency theories, coherence effect, open data, open materials

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One of the distinctive contributions of behavioral decision research has been to challenge the proposition that the act of choosing between options reveals the chooser's preferences in an unadulterated manner (Samuelson, 1938; Von Neumann & Morgenstern, 1944). A persistent body of research has shown that people's choices are readily affected by various features of the task, such as the elicitation method, the choice context, and the decision environment. This research suggests that preferences are often *constructed* ad hoc to fit the features of the particular task (Ariely & Norton, 2008; Bettman, Luce, & Payne, 1998; Lichtenstein & Slovic, 2006).

But decision theorists have noted that preferences are not born equal. Some preferences—such as those for novel nuisances (Ariely, Loewenstein, & Prelec, 2003) or values presented without reference points (Simonson, 2008b)—are readily constructed. Other preferences, such as brand loyalty and love of dark chocolate, can be relatively stable and insensitive to the features of the situation (Bettman, Luce, & Payne, 2008; Kivetz, Netzer, & Schrift, 2008; Simonson, 2008a, 2008b). The two studies reported here were designed to shed light on the susceptibility of preferences to construction and to critically explore the perceived distinction between preferences that are deemed to be stable and preferences that yield to construction.

We present a framework according to which a given preference can be relatively stable under some conditions but constructed under others. These characteristics evoke the core features of elastic matter: stability and pliability. Elastic matter, such as a rubber duck, typically rests at its natural shape, which is largely consistent over time. When the duck is squeezed, it will morph in accordance with the physical properties of that force, and when the force is removed, it will return to its original shape. If squeezed again in the same manner, the duck will likely morph to a similar shape, though if squeezed from another direction, it will assume a different shape. In either case, once the force is removed, the duck will again return to its original shape.

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We suggest that the elastic features of stability and pliability can hold true also for preferences for attributes in choices between multiattribute options. Absent situational pressures, preferences rest around a baseline level. These are the preferences that people will normally report when probed by marketing researchers for their attitudes toward the features of a product or a service. In the simplest of choice tasks, as when a decision maker's preferences for the attributes of one option dominate his or her preferences for the attributes of the other, that dominance will determine the choice straightforwardly. But when the task entails decisional conflict and requires engaged processing, the preferences will often yield to the situational features of the task and shift toward a different state, a phenomenon that has been characterized in the literature as preference construction. Once the situational demands of the task subside, the preferences will return to roughly their original baseline levels. When the decision maker faces a new task, the preferences will again transform to meet the demands of the new situation. Thus, when a person makes the same choice on two occasions, he or she will report similar constructed preferences, whereas making opposite choices will generate roughly opposite constructed preferences. This pattern should hold both when choices are made spontaneously and when they are swayed by an exogenous manipulation.

More generally, in this article, we look beneath the hood of the multiattribute decision-making process to explore the relationship between a global choice of a composite decision alternative and the chooser's preferences for its underlying attributes (Simonson, 2008b; Warren, McGraw, & Van Boven, 2011). Our hypothesis diverges from conventional decision-making models that posit a straightforward and unidirectional aggregation of the preferences for the underlying attributes (e.g., Edwards & Newman, 1982; Hammond, Keeney, & Raiffa, 1999). Such models are premised on purely stable preferences. In contrast, our proposal is based on parallel constraint-satisfaction processing (Holyoak & Thagard, 1989; Read, Vanman, & Miller, 1997), which is embedded in a connectionist cognitive architecture (McClelland, Rumelhart, & the PDP Research Group, 1986) and rooted in Gestalt psychology (Heider, 1960; Wertheimer, 1923/1967). The core insight of parallel constraint-satisfaction models is that the variables cross-activate in a manner that drives the representation toward a state of global coherence, which is characterized by similar activation among positively related variables and opposite activation of negatively related variables. The coherence-maximizing function operates by strengthening the variables that support the emerging conclusion and by weakening those that support the losing alternative (Thagard, 2002). This *coherence effect* has been observed across a range of tasks involving high-level reasoning (Holyoak & Simon, 1999), factual determinations (Glöckner & Engel, 2013; Simon, Snow, & Read, 2004), social judgment (Simon, Stenstrom, & Read, 2015), and probabilistic determinations (Glöckner, Betsch, & Schindler, 2010).

In the context of decision making, the coherence effect is manifested by changes in preferences that effectively spread apart the attractiveness of the options, which in turn enables choice and facilitates confident decisions (Simon & Holyoak, 2002). It follows that the coherence effect can be understood as a form of preference construction (Lichtenstein & Slovic, 2006; Simon, Krawczyk, Bleicher, & Holyoak, 2008; Simon, Krawczyk, & Holyoak, 2004). (For related findings, see Carpenter, Yates, Preston, & Chen, 2016; DeKay, Miller, Schley, & Erford, 2014; DeKay, Stone, & Sorenson, 2012; and Russo, Carlson, Meloy, & Yong, 2008.) Naturally, for the coherence effect to emerge, the preferences need to be malleable.

In previous research, the coherence effect was demonstrated by comparing participants' preferences for relevant attributes that were presented in isolation (at baseline) with their preferences for the same attributes when choosing between alternatives that were constituted by those attributes. The novelty of the current studies is that we administered the same task to the same participants at two sessions, conducted between 6 and 8 weeks apart. Thus, we were able to track the participants' preferences over four points in time: at the baseline and at the point of making the choice in the first session, then again at the same points in the second session. This design enabled us to retest the prediction that preferences return to baseline levels following the completion of the decision, as observed by Simon et al. (2008). More important, this design enabled us to compare preference construction when the same participants made the same choice at different points in time and, intriguingly, when they made opposite choices. Our core prediction was that preferences would shift from baseline levels toward a state of coherence with the emerging choice at the first session and then recede to baseline levels prior to the start of the second session, only to shift again to cohere with the second choice. We expected that the constructed preferences would look similar when the participant made the same choice at the two sessions but would look largely opposite when he or she made opposite choices. Moreover, we expected that these patterns would hold both when participants chose freely (Study 1) and when they were induced to switch their choice between sessions (Study 2). In short, our studies were designed to test the proposition that elasticity is a key property of preference construction and to explore the potentially adaptive effect of preference elasticity in the face of decisional conflict.

## Study 1

### Method

***Participants.*** Our a priori data-collection plan was to solicit 260 participants and stop data collection if (a) at least 150 completed both sessions (to ensure a sample size nearly twice that of previous studies using these materials; Simon et al., 2008; Simon, Krawczyk, & Holyoak, 2004) and (b) at least 25% of participants switched their choices from Session 1 to Session 2. Two hundred twenty-nine undergraduate participants completed the first session of the study. Of those, 175 completed the second session, which took place some 7 to 8 weeks later.[1] The analyses reported here included data only from those participants who completed both sessions (though results for Session 1 did not change when we included participants who dropped out before Session 2). The study was conducted online, and participants could complete the tasks at a location and time of their choosing within a few days of when each survey was announced. Participants were given a total compensation of $8.

***Design.*** In Session 1, participants completed two questionnaires relating to a search for a postgraduation job (materials taken from Simon, Krawczyk, & Holyoak, 2004). First, they simply evaluated job-related attributes in isolation, absent any decision task. Later, they were given a second questionnaire concerning two job offers that comprised the same attributes. Participants were asked to choose between the jobs and to evaluate the attributes again. In Session 2, participants repeated the entire procedure with questionnaires that were nearly identical to those used in Session 1.

***Materials and procedure.*** In each session, participants completed the first questionnaire, a distractor task, and then the second questionnaire. The first questionnaire, "Waiting for a Job Offer," was intended to measure participants' preferences prior to the introduction of the choice task (baseline preferences). Participants were told to imagine that they were about to graduate from college, had interviewed for jobs in the field of marketing, and were waiting to receive an offer. They were asked to evaluate the desirability of 11 job-related features that "might be included in job offers." Three of these attributes served as fillers. Our focus was on the other 8, 1 relatively favorable and 1 relatively unfavorable on each of four dimensions: the length of the commute, the kind of office, the vacation package, and the salary. Participants were asked to rate the desirability of each item on a 10-point scale that ranged from −5 (*highly undesirable*) to +5 (*highly desirable*), with no midpoint (i.e., 0 was not on the scale). Following the desirability evaluations, participants were asked to rate the importance of each of the

**Table 1.** Job Attributes in Study 1

| Dimension | Job X attribute | Job Y attribute |
|---|---|---|
| Salary | $49,250 | $51,000 |
| Office provided | Private office | Noisy cubicle |
| Vacation package | 2 weeks | 2 weeks plus retreat |
| Commute | 18 min each way | 40 min each way |

four dimensions, assuming that they were included in a job offer. To assist the participants in this task, we provided relevant reference points, delimiting each dimension by values that were the high and low attributes on that dimension. The importance ratings were made on a 9-point scale ranging from 0 (*no weight*) to 8 (*maximum weight*).

Following a brief distraction task, participants were given a questionnaire labeled "Choosing Your Next Job." They were told to imagine that they had received job offers from two large retail-store chains. The company names were counterbalanced, and were different in Sessions 1 and 2. For ease of reporting here, we denote the companies as X and Y. They were described as being similar in size, reputation, stability, and opportunities for promotion. Participants were also informed that they had met with key personnel at the two companies and found them to be stimulating and pleasant.

The job offers differed on four dimensions, with each offer being stronger than the other on two dimensions and weaker on two dimensions. One of the jobs had a shorter commute and offered a better office space, but the other job paid a higher salary and offered a superior vacation package (see Table 1). The eight attributes mentioned in the offers were the same ones that had been tested in the baseline measure just minutes earlier.

Participants were asked to report which offer they would choose and how confident they were in that choice (on a scale from 1 to 11, with 11 representing very high confidence). To obtain a measure of preferences at the time of decision, we asked participants to evaluate the desirability of the eight attributes and the importance of the four dimensions (decision preferences). These questions were similar to those in the first questionnaire, except that they were worded in terms of the job offers.

At the end of the first session, participants responded to demographic questions (sex, age, education, and ethnicity). At the end of the second session, they provided information needed for them to receive payment.

In all, participants rated the same attributes twice in Session 1, first at baseline and then at the time of the decision, and twice in Session 2, at baseline and at the time of the decision. Ten days following the first session, participants completed a brief instrument containing two personality measures: a five-item regret scale (Schwartz et al., 2002) and the emotional and cognitive items from the Uncertainty Response scale (Greco & Roger, 2001). As neither scale moderated the key findings, we do not discuss these measures further.

## Results

Our primary dependent variable was the overall composite preference for Job X over Job Y. We scaled the desirability ratings of the eight attributes (e.g., 18-min commute, private office) from −1 to 1 (Job Y attributes were reverse-coded) and the importance weights of the four dimensions from 0 to 1. Then, for each attribute, we multiplied the two measures. We then summed the four scores for the Job X attributes and the four scores for the Job Y attributes to create an overall measure of preference for Job X. This measure had a potential range from −8 to 8 and an actual range from −3.1 to 3.5; positive values indicated support for Job X, and negative values indicated support for Job Y. (Tables S1 and S2 in the Supplemental Material available online provide the means and standard deviations for the desirability and importance ratings.)

Even though participants were given the same task at both sessions, we expected some participants to make different choices in the two sessions. Indeed, of the 125 participants who chose Job X in Session 1, 102 chose Job X and 23 chose Job Y in Session 2. Of the 50 participants who chose Job Y in Session 1, 25 chose Job Y and 25 chose Job X in Session 2. We refer to the 27% who chose different jobs in the two sessions as *switchers* and the 73% who chose the same jobs in the two sessions as *nonswitchers*. This switching rate is consistent with extant research findings that between one quarter and one third of participants switch their choices on repeated tasks, even within the same session (Starmer, 2000; see also Holyoak & Simon, 1999).

Figure 1 tracks the composite preference scores at the four measurement points, separately for switchers and nonswitchers. For both groups in both sessions, baseline preferences clearly shifted toward greater support of the chosen option, but receded to baseline levels between the sessions. The shifts in Session 1 and Session 2 were generally the same for the nonswitchers but reversed for the switchers.

Logistic regressions revealed that baseline preferences significantly predicted choice overall across participants, both in Session 1 ($z$ = 3.25, $p$ = .001) and in Session 2 ($z$ = 4.40, $p$ < .001). This relationship held true for nonswitchers, who constituted the majority of participants (Session 1: $z$ = 2.62, $p$ = .009; Session 2: $z$ = 4.29, $p$ < .001), but not for switchers (Session 1: $z$ = 0.86, $p$ > .250; Session 2: $z$ = −0.41, $p$ > .250). The interaction between baseline preferences and switching behavior (switch vs. no switch) was nonsignificant in Session 1 ($z$ = 0.87, $p$ > .250), but significant in Session 2 ($z$ = 3.14, $p$ = .002).

We next discuss three indicators of the elastic properties of preferences: shifts in preferences from baseline to the point of decision at each session, the tendency of preferences to return to baseline after shifting, and the relationship between preference shifts when the same decision task is presented on different occasions.

***Preference shifts.*** As Figure 1 shows, when participants made their choices, their preferences shifted from relatively neutral baseline levels to more polarized levels that cohered with their decisions. The difference between preference shifts (i.e., decision preferences minus baseline preferences) among participants who chose Job X and those who chose Job Y was statistically significant at both Session 1, $t(173)$ = 8.25, $p$ < .001, $d$ = 1.38, and Session 2, $t(173)$ = 8.44, $p$ < .001, $d$ = 1.43. Among participants who chose Job X, preferences shifted toward Job X—Session 1: $t(173)$ = 11.10, $p$ < .001, $d$ = 0.96; Session 2: $t(173)$ = 9.54, $p$ < .001, $d$ = 0.83. Among those who chose Job Y, preferences shifted toward Job Y—Session 1: $t(173)$ = −2.74, $p$ = .007, $d$ = 0.42; Session 2: $t(173)$ = −4.04, $p$ < .001, $d$ = 0.63. During Session 1, 81% of participants showed a preference shift in the direction of choice. During Session 2, 79% showed a preference shift in the direction of choice. Participants who showed the expected preference shift in Session 1 and participants who did not were each equally likely to show the expected preference shift in Session 2 ($p$ > .250).

These shifts were due to differences in attribute evaluations and to differences in attribute weights, both of which shifted toward greater support of the chosen option ($p$s < .001; for details, see Tables S5 and S6 in the Supplemental Material). Furthermore, the composite preference score of the chosen option increased and the composite preference score of the rejected option decreased ($p$s < .001), which is consistent with recent findings on pro-leader and antitrailer information distortion (Blanchard, Carlson, & Meloy, 2014; DeKay et al., 2014).

If preference shifts are driven by a coherence-maximizing function, not only should the average preferences change while the decision is being made, but also the preferences should exhibit stronger interconnectivity at the point of decision than at baseline (Carlson & Pearo, 2004; DeKay et al., 2014; DeKay, Stone, & Miller, 2011; Holyoak & Simon, 1999; Simon et al., 2015). For each dimension (office, commute, salary, and vacation), we calculated a component preference score by summing the weighted desirability score for that attribute for Job X and the reverse-coded weighted desirability score for that attribute for Job Y. We then calculated the six correlations among these summed scores and combined them using

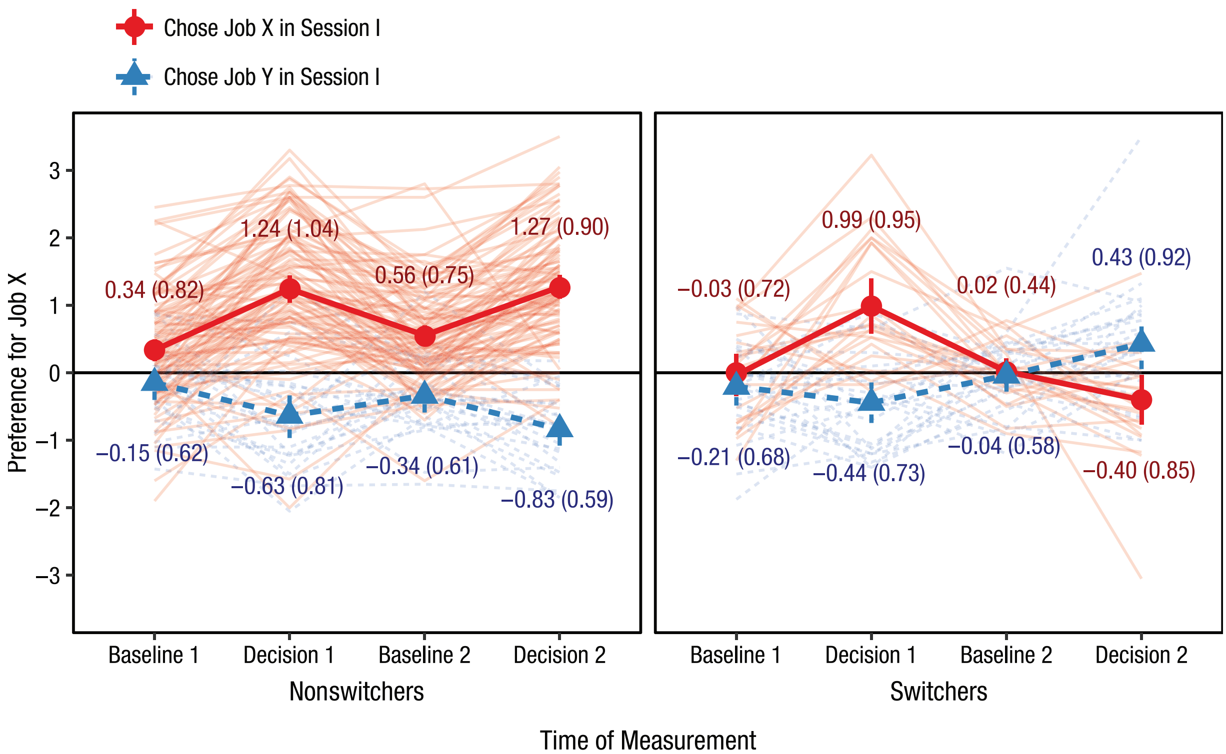


**Fig. 1.** Individual (thin lines) and mean (thick lines) preference scores among the nonswitchers (left) and switchers (right) at the Session 1 baseline and decision point and the Session 2 baseline and decision point in Study 1. Positive scores indicate support for Job X, and negative scores indicate support for Job Y. In each graph, results are presented separately for participants who chose Job X in Session 1 and those who chose Job Y in Session 1. Each mean is labeled, with the standard deviation given inside parentheses. Error bars (some too small to be seen here) represent between-subjects 95% confidence intervals.

Fisher's transformation. The average correlation among the component preferences was stronger for decision preferences (Session 1: .145; Session 2: .134) than for baseline preferences (Session 1: −.046; Session 2: −.087), the 99.9% bootstrapped confidence intervals (CIs) of the difference from baseline to decision both excluded 0.

***Return to baseline.*** As shown in Figure 1, in the absence of a choice task, participants' preferences rested at roughly the same state in Sessions 1 and 2. The shifts from Session 1 decision to Session 2 baseline depended on which job was chosen during Session 1, $t(173) = -7.10$, $p < .001$, $d = 1.19$. Preferences shifted away from Job X among participants who chose Job X, $t(173) = -9.01$, $p < .001$, $d = 0.78$, and away from Job Y among participants who chose Job Y, $t(173) = 2.70$, $p = .008$, $d = 0.42$ (see Simon et al., 2008). Of those who showed the expected preference shift in Session 1, 82% showed a shift toward their Session 1 baseline at the Session 2 baseline; in 81% of these cases, preferences reverted back by more than half of the shift in Session 1. There was weak evidence of persistence; the shift from Session 1 baseline to Session 2 baseline varied marginally with Session 1 choice, $t(173) = 1.68$, $p = .095$, $d = 0.28$. The average shift from Session 1 baseline to Session 2 baseline was 16% of the average shift from Session 1 baseline to Session 1 decision. In addition, there was no evidence of interconnectedness of component preference scores at baseline in Session 2. The mean correlation among these scores decreased from .145 at Session 1 decision to −.087; the 99.9% bootstrapped CI for the difference between these correlations excluded 0.

***Similarity and dissimilarity of preference shifts when the task is repeated.*** Asking participants to repeat the decision task in a second session enabled us to examine how preferences varied depending on whether participants made the same choice at both sessions or made different choices. We expected preference shifts to be similar at the two sessions for nonswitchers, but starkly different for switchers. This pattern is evident in Figure 1: The preference shifts of nonswitchers are quite similar in Session 1 and Session 2, whereas switchers' preference shifts in Session 2 are the reverse of their shifts in Session 1. The difference in preference shifts between participants who chose Job X in Session 1 and those who chose Job Y in Session 1 reversed across sessions for switchers, $t(171) = -6.51$, $p < .001$, $d = 1.56$, but was constant across sessions for nonswitchers, $t(171) = -0.70$, $p > .250$, $d = 0.17$; the interaction of switching behavior with Session 1 job choice was significant, $t(171) = -4.72$, $p < .001$.

To assess the correspondence between Session 1 and Session 2 measures, we regressed Session 2 baseline preferences, decision preferences, and preference shifts separately on the corresponding measures from Session 1, switching behavior, and their two-way interaction. We found that Session 2 baseline preferences were strongly positively predicted by Session 1 baseline preferences for both nonswitchers, $b = 0.56$, $SE = 0.07$, $t(171) = 8.21$, $p < .001$, and switchers, $b = 0.39$, $SE = 0.13$, $t(171) = 3.04$, $p = .003$; the interaction between Session 1 baseline preferences and switching behavior was not significant, $t(171) = -1.14$, $p > .250$. In contrast, Session 2 decision preferences were positively predicted by Session 1 decision preferences among nonswitchers, $b = 0.71$, $SE = 0.07$, $t(171) = 11.65$, $p < .001$, and negatively predicted by Session 1 decision preferences among switchers, $b = -0.28$, $SE = 0.11$, $t(171) = -2.53$, $p = .012$; the interaction of Session 1 decision preferences and switching behavior was significant, $t(171) = -7.78$, $p < .001$. As a result, Session 2 preference shift was positively predicted by Session 1 preference shift among nonswitchers, $b = 0.38$, $SE = 0.07$, $t(171) = 5.33$, $p < .001$, but negatively predicted by Session 1 preference shift among switchers, $b = -0.25$, $SE = 0.10$, $t(171) = -2.45$, $p = .015$; the interaction of Session 1 preference shift and switching behavior was significant, $t(171) = -5.03$, $p < .001$.

***Confidence.*** To assess the relationship between confidence and preferences, we regressed confidence reported at each session on job choice, baseline preferences, decision preferences, and the interactions of baseline and decision preferences with job choice. These interactions indicate how confidence varied with strength of preference in favor of the chosen option. Stronger decision preferences in favor of the chosen option (Job Choice × Decision Preferences interaction) were positively associated with confidence in both Session 1, $b = 0.87$, $SE = 0.33$, $t(169) = 2.63$, $p = .009$, and Session 2, $b = 1.42$, $SE = 0.37$, $t(169) = 3.84$, $p < .001$. Stronger baseline preferences in favor of the chosen option were not—Session 1: $b = 0.50$, $SE = 0.40$, $t(169) = 1.24$, $p = .216$; Session 2: $b = -0.25$, $SE = 0.49$, $t(169) = -0.51$, $p > .250$. In both sessions, nonswitchers (Session 1: $M = 8.26$, $SD = 1.59$; Session 2: $M = 8.00$, $SD = 1.69$) were more confident about their choices than were switchers (Session 1: $M = 7.63$, $SD = 1.48$; Session 2: $M = 7.08$, $SD = 1.81$)—Session 1: $t(173) = 2.39$, $p = .018$; Session 2: $t(173) = 3.15$, $p = .002$.

***Summary and statistical artifacts.*** In all, the results of Study 1 provide initial support for our elasticity hypothesis. Participants' baseline preferences shifted toward coherence with the chosen option, then receded to a state that was close to the original baseline, and shifted again to cohere with the second choice. Chen and Risen (2010) have argued convincingly that the results of many studies purportedly showing that choices affect preferences may merely show that choices reveal preferences. In the Supplemental Material, we explain in depth why neither this account nor a related account of regression to the mean is likely to explain our results. We further ruled out these alternate accounts and extended our results by manipulating choice in Study 2.

## Study 2

In Study 2, we sought to extend our findings in two ways. To test their robustness, we introduced a manipulation designed to sway participants' choices, and to maximize the effect of this experimental treatment, we induced choice of opposing alternatives in the two sessions. We also wanted to probe more deeply into the nature of the constructed preferences. In particular, we were interested in learning whether after having made their final choice, participants would be able to recall their baseline preferences. If, as we predicted, participants' preferences would actually be altered by the coherence-maximizing pressures, the polarized evaluations at the point of decision might well distort participants' recall of their baseline preferences (see Goethals & Reckman, 1973). If so, the recalled preferences would be skewed toward the decision preferences, as observed in a similar, nonincentivized task reported by Holyoak and Simon (1999, Studies 2 and 3).

### *Method*

***Participants.*** Given the size of the effects and sample size in Study 1, we sought to recruit 300 participants for Session 1; 293 undergraduate participants completed that session. Of those, 254 completed the second session approximately 6 weeks later.[2] As in Study 1, our analyses included data only from those participants who completed both sessions, though including data from the participants who dropped out between sessions did not change any of the results for Session 1. This study was conducted online, and participants could complete the sessions at a location and time of their choosing, within a few days of when each survey was announced. They were given compensation of $7 for completing both sessions, plus an additional (initially unannounced) payment of up to $3 based on performance.

***Design and procedure.*** The design and procedure of Study 2 closely mirrored those of Study 1, with four exceptions. First, we used a new set of stimuli relating to choice between apartments (loosely following DeKay

**Table 2.** Critical Apartment Attributes in Study 2

| Dimension | Apartment on Elm Street | Apartment on Cedar Street |
|---|---|---|
| Size | 50 square feet larger than most two-bedroom apartments | 40 square feet smaller than most two-bedroom apartments |
| Utilities | Monthly utilities $50 more than average | Monthly utilities $35 less than average |
| Landlords | Attentive landlords who quickly address any problem | Inattentive landlords who are slow to address problems |
| Lighting | Poor lighting | A lot of natural light |
| Parking manipulation (counterbalanced across apartments) | On-site parking at no additional cost (favorable value) | Street parking only (unfavorable value) |

et al., 2014). The apartment choice was structurally similar to the job choice in Study 1, as the two apartments were roughly balanced on four attributes; each apartment was described as superior on two attributes and inferior on the other two attributes. Specifically, one apartment was slightly larger and had attentive landlords, whereas the other apartment had better lighting and lower utility costs (see Table 2). These apartment attributes (size, cost of utilities, landlords' attentiveness, and lighting) were selected on the basis of pilot data to be moderately important and to ensure that neither apartment dominated the other. Second, for our experimental manipulation, we added a fifth attribute to induce choice of one option or the other. Third, after reporting their decision preferences in Session 2, participants completed an incentive-compatible task assessing their memory for their baseline preferences. Fourth, no personality measures were included.

At both sessions, we assessed baseline preferences for the 8 critical attributes and the additional 2 attributes (related to parking availability) that were designed to manipulate choice (all 10 are shown in Table 2). The baseline questionnaire also included 6 filler attributes that were not included in the subsequent apartment descriptions.

Following a distractor task, participants were presented with two apartments, one on Elm Street and one on Cedar Street. Participants were randomly assigned to one of two conditions. In one condition, the Elm Street apartment had better parking than the Cedar Street apartment. In the other condition, the Cedar Street apartment had better parking than the Elm Street apartment. To maximize switching between the two sessions, we reversed the parking manipulation in Session 2. If the Elm Street apartment had better parking in Session 1, the Cedar Street apartment had better parking in Session 2, and vice versa. The manipulation was designed to be strong enough to induce choice of a particular apartment, but not so strong as to overwhelm consideration of the other attributes. At both sessions, following the choice of the apartment, participants were asked again to report their preferences for the attributes.

At the end of Session 2, after assessing decision preferences, we tested participants' memory for their baseline preferences. We informed them that their preference ratings averaged across the two baseline measures represented "our best understanding of your underlying preferences." We then presented participants with the attributes and dimensions, and incentivized them with an additional payment of up to $3 to report their average baseline evaluations and weights as accurately as possible. Participants who were interested in the precise method for calculating the bonus were invited to open a window that detailed the compensation mechanism.

## Results

The manipulation was effective at inducing choice of the apartment with better parking (see Table S11 in the Supplemental Material for counts by condition, session, and choice). Across conditions and sessions, 72% to 80% of participants chose that apartment. Each of these percentages differed from chance ($p$s $< .001$). However, the manipulation was not deterministic, as approximately 20% to 30% of participants across sessions and conditions chose the apartment with worse parking. As a result, only 143 (56.3%) switched their choice from Session 1 to Session 2.

Our primary dependent variable was the overall composite preference for the Elm Street apartment over the Cedar Street apartment. Again, we calculated an overall measure of preference by summing across the four critical weighted attributes. This preference measure excluded evaluation of the parking attribute, as we were interested in the extent to which that attribute induced shifts in the preferences for the other attributes. The composite preference score could potentially range from −8 to 8 and actually ranged from −3.6 to 2.3; positive values indicated support for the Elm Street apartment, and negative values indicated support for the Cedar Street apartment. (Tables S3 and S4 in the Supplemental Material provide the means and standard deviations for the desirability and importance ratings.)

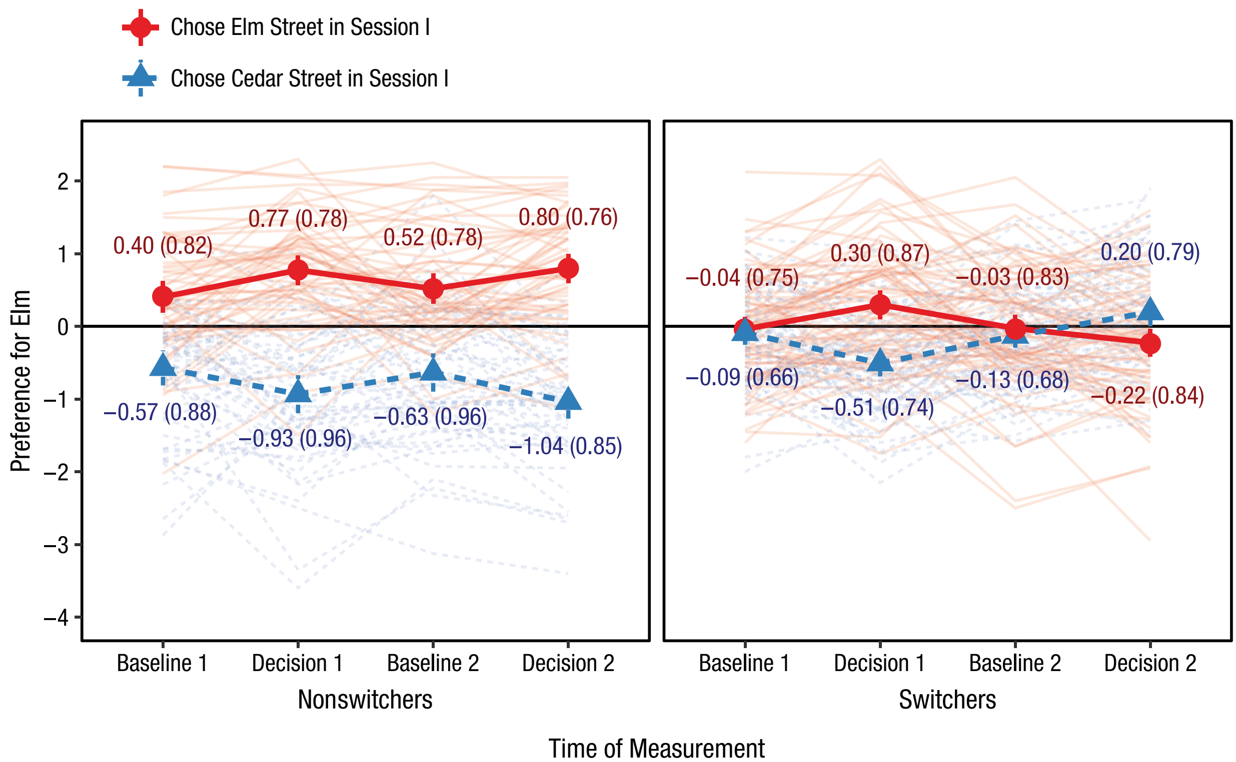


**Fig. 2.** Individual (thin lines) and mean (thick lines) preference scores among the nonswitchers (left) and switchers (right) at the Session 1 baseline and decision point and the Session 2 baseline and decision point in Study 2. Positive scores indicate support for the Elm Street apartment, and negative scores indicate support for the Cedar Street apartment. In each graph, results are presented separately for participants who chose the Elm Street apartment in Session 1 and those who chose the Cedar Street apartment in Session 1. Each mean is labeled, with the standard deviation given inside parentheses. Error bars (some too small to be seen here) represent between-subjects 95% confidence intervals. Note that the preference scores excluded participants' evaluations of the parking attributes.

The data are summarized in Figure 2, which shows the same patterns as in Figure 1. Controlling for condition, and as in Study 1, baseline preferences significantly predicted choice overall across participants in both Session 1 ($z$ = 5.65, $p$ < .001) and Session 2 ($z$ = 4.75, $p$ < .001). This relationship held true for nonswitchers (Session 1: $z$ = 6.01, $p$ < .001; Session 2: $z$ = 5.88, $p$ < .001), but not for switchers (Session 1: $z$ = 0.44, $p$ > .250; Session 2: $z$ = −0.90, $p$ > .250). The interaction between switching behavior and baseline preferences was significant in both Session 1 ($z$ = 4.57, $p$ < .001) and Session 2 ($z$ = 5.21, $p$ < .001).

Again, to explore the elastic properties of preferences, we first focus on preference shifts, then the return to baseline, and then the nature of repeated preference shifts. Figure S1 in the Supplemental Material presents a version of Figure 2 in which the data are separated by condition rather than by choice.

***Preference shifts.*** Preferences shifted toward greater coherence with emerging decisions. The shift in preferences depended on the chosen option in both Session 1, $t$(252) = 7.60, $p$ < .001, $d$ = 0.95, and Session 2, $t$(252) = 6.68, $p$ < .001, $d$ = 0.84. Among participants who chose the Elm Street apartment, preferences shifted toward that apartment—Session 1: $t$(252) = 5.22, $p$ < .001, $d$ = 0.48; Session 2: $t$(252) = 4.82, $p$ < .001, $d$ = 0.45. Among those who chose the Cedar Street apartment, preferences shifted toward that apartment—Session 1: $t$(252) = −5.52, $p$ < .001, $d$ = 0.48; Session 2: $t$(252) = −4.63, $p$ < .001, $d$ = 0.39. At both sessions, most participants showed a preference shift in the direction of the chosen option (Session 1: 66.9%; Session 2: 63.4%). Participants who showed the expected preference shift in Session 1 and participants who did not were each equally likely to show the expected preference shift in Session 2 ($p$ > .250). As in Study 1, the shifts were due to significant changes in both attribute evaluations and attribute weights ($p$s < .001; for details, see Tables S7 and S8 in the Supplemental Material), and the composite preference score of the chosen option increased, whereas the composite preference score of the unchosen option decreased ($p$s < .001).

The shift in preferences across the nonparking attributes depended on which option was randomly assigned to have better parking—Session 1: $t(252) = 4.04$, $p < .001$, $d = 0.51$; Session 2: $t(252) = 4.10$, $p < .001$, $d = 0.51$. Among participants for whom the Elm Street apartment had better parking, preferences for nonparking attributes shifted toward that apartment in both Session 1, $b = 0.21$, $SE = 0.07$, $t(252) = 2.86$, $p = .005$, $d = 0.26$, and Session 2, $b = 0.19$, $SE = 0.06$, $t(252) = 2.92$, $p = .004$, $d = 0.25$. Among those for whom the Cedar Street apartment had better parking, preferences for nonparking attributes shifted toward that apartment in both Session 1, $b = -0.21$, $SE = 0.07$, $t(252) = -2.86$, $p = .005$, $d = 0.25$, and Session 2, $b = -0.19$, $SE = 0.06$, $t(252) = -2.88$, $p = .004$, $d = 0.26$. (These effects are shown in Fig. S1 in the Supplemental Material.) During both sessions, most participants' preferences for the nonparking attributes shifted in the direction of the option with better parking (Session 1: 60.6%; Session 2: 56.7%).

As in Study 1, we calculated the six correlations among the four component preference scores for the critical dimensions and combined these correlations using Fisher's transformation. The average correlation among the component preferences was stronger (in this case, less negative) for decision preferences (Session 1: −.040; Session 2: −.044) than for baseline preferences (Session 1: −.147; Session 2: −.105); the 99% bootstrapped CIs of the differences from baseline to decision excluded 0.

***Return to baseline.*** Following their distortion in Session 1, preferences returned to baseline at the beginning of Session 2, so that preferences at Session 1 baseline and Session 2 baseline were similar. The nature of this reversion depended on the choice in Session 1, $t(252) = -6.16$, $p < .001$, $d = 0.77$. Participants who chose the Elm Street apartment during Session 1 exhibited a negative shift from their Session 1 decision preferences to their Session 2 baseline preferences, $t(252) = -4.15$, $p < .001$, $d = 0.33$, whereas those who chose the Cedar Street apartment during Session 1 exhibited a positive shift from their Session 1 decision preferences to their Session 2 baseline preferences, $t(252) = 4.55$, $p < .001$, $d = 0.46$. Of those who showed the expected preference shift in Session 1, 76% showed a shift toward their Session 1 baseline at the Session 2 baseline; in 68% of these cases, preferences reverted back by more than half of the shift in Session 1. The average shift from Session 1 baseline to Session 2 baseline was only 14% of the average shift from Session 1 baseline to Session 1 decision.

Results were similar when we examined the effect of the manipulation, though somewhat weaker given the incomplete induction of choice of the apartment with better parking. The manipulation in Session 1 affected the change from Session 1 decision to Session 2 baseline, $t(252) = -1.99$, $p = .048$, $d = 0.25$, though neither simple effect reached significance on its own. Participants for whom the Elm Street apartment had better parking during Session 1 exhibited a nonsignificant negative shift from Session 1 decision preferences to Session 2 baseline preferences, $t(252) = -1.33$, $p = .186$, $d = 0.11$, whereas those for whom the Elm Street apartment had worse parking during Session 1 exhibited a nonsignificant positive shift from Session 1 decision preferences to Session 2 baseline preferences, $t(252) = 1.48$, $p = .140$, $d = 0.14$.

Across all participants, the average correlation among component preference scores decreased from Session 1 decision (−.040) to Session 2 baseline (−.105); the 95% bootstrapped CI of the difference between these correlations excluded 0.

***Similarity and dissimilarity of preference shifts when the task is repeated.*** Figure 2 shows that as in Study 1, preference shifts for nonswitchers were similar in Sessions 1 and 2, whereas switchers' shifts in Session 1 were reversed in Session 2. The difference in preference shifts between participants who chose the Elm Street apartment and those who chose the Cedar Street apartment in Session 1 reversed across sessions for switchers, $t(250) = -7.45$, $p < .001$, $d = 1.27$, but did not vary across sessions for nonswitchers, $t(250) = -0.25$, $p > .250$, $d = 0.05$; the interaction of switching behavior with Session 1 choice was significant, $t(250) = -4.74$, $p < .001$.

To assess the correspondence between Session 1 and Session 2 measures, we regressed Session 2 baseline preferences, decision preferences, and preference shifts separately on the corresponding measures in Session 1, switching behavior, and their two-way interaction. Baseline preferences in Session 1 positively predicted baseline preferences in Session 2 for both switchers, $b = 0.39$, $SE = 0.09$, $t(250) = 4.48$, $p < .001$, and nonswitchers, $b = 0.72$, $SE = 0.07$, $t(250) = 9.89$, $p < .001$, although the magnitude of this association was significantly smaller for switchers, $t(250) = -2.84$, $p = .005$. In addition, decision preferences in Session 2 were positively predicted by decision preferences in Session 1 for both switchers, $b = 0.29$, $SE = 0.07$, $t(250) = 3.93$, $p < .001$, and nonswitchers, $b = 0.78$, $SE = 0.06$, $t(250) = 12.66$, $p < .001$, but the magnitude of this association was significantly smaller for switchers, $t(250) = -5.18$, $p < .001$. As in Study 1, the interaction between Session 1 preference shift and switching behavior was significant, $t(250) = -3.21$, $p = .002$, such that Session 1 and Session 2 preference shifts were positively associated among nonswitchers, $b = 0.21$, $SE = 0.08$, $t(250) = 2.75$, $p = .006$, but negatively associated among switchers, $b = -0.14$, $SE = 0.08$, $t(250) = -1.78$, $p = .077$.

***Memory for baseline preferences.*** We tested participants' memory for their baseline preferences (averaged across Session 1 and Session 2) both at the level of overall preference and at the level of individual items (which was compatible with the incentivization method), and observed similar effects at these levels. For consistency with the analyses reported for other variables, we report analyses of composite preferences.

We found that the difference between overall preference as remembered and as measured (averaged across baselines) was influenced by choice in Session 2, $b$ = 0.35, $SE$ = 0.08, $t(252)$ = 4.15, $p$ < .001, $d$ = 0.52. This difference was 57% of the aggregate preference shift. Thus, our results are consistent with Holyoak and Simon's (1999) findings that participants' memory for their original preferences was clouded by their altered preferences, as constructed by the decision-making process. Analysis of absolute differences revealed that remembered preferences deviated less from measured Session 2 decision preferences ($M$ = 0.46, $SD$ = 0.46) than from the average across measured baseline preferences ($M$ = 0.53, $SD$ = 0.45), $t(253)$ = −2.13, $p$ = .035, $d$ = 0.13, or from measured Session 1 decision preferences ($M$ = 0.69, $SD$ = 0.53), $t(253)$ = −5.69, $p$ < .001, $d$ = 0.36. Effects on both signed and absolute deviations held when we analyzed the deviation from Session 2 baseline preferences rather than the deviation from the average of Session 1 and Session 2 baseline preferences. The effect of condition on memory error was not significant, $b$ = 0.10, $SE$ = 0.09, $t(252)$ = 1.16, $p$ = .248, $d$ = 0.15, because the manipulation did not induce choice of the apartment with better parking in every participant and participants who chose the other apartment exhibited an effect in the opposite direction. Taken together, the findings from the memory task suggest that preference shifts do indeed alter people's preferences when they make difficult decisions, and that these changes transpire largely beneath the level of conscious awareness.

***Confidence.*** As in Study 1, to assess the relationship between confidence and preferences, we regressed confidence on choice, baseline preferences, decision preferences, and the interactions between choice and the preference measures. Again, stronger decision preferences supporting the chosen option (Apartment Choice × Decision Preferences interaction) positively predicted confidence in both Session 1, $b$ = 1.03, $SE$ = 0.31, $t(248)$ = 3.31, $p$ = .001, and Session 2, $b$ = 1.25, $SE$ = 0.35, $t(248)$ = 3.54, $p$ < .001. Stronger baseline preferences supporting the chosen option (Apartment Choice × Baseline Preferences interaction) did not—Session 1: $b$ = −0.31, $SE$ = 0.33, $t(248)$ = −0.92, $p$ > .250; Session 2: $b$ = 0.10, $SE$ = 0.36, $t(248)$ = 0.27, $p$ > .250. Unlike in Study 1, nonswitchers were not significantly more confident about their choices than switchers during either Session 1, $t(252)$ = −1.61, $p$ = .110, or Session 2, $t(252)$ = 0.44, $p$ > .250.

## General Discussion

### *The coherence effect*

In these experiments, participants were presented with a choice between closely balanced options that were composed of attributes that were relatively familiar, externally valid, and germane to their lives, and that were presented with relevant reference points (see Simonson, 2008b). As in prior research (Carpenter et al., 2016; DeKay et al., 2014; Russo et al., 2008; Simon et al., 2008; Simon, Krawczyk, & Holyoak, 2004), participants resolved the decisional conflict by altering their preferences to cohere with their choices: by increasing their preferences for the attributes that supported their emerging choices and decreasing their preferences for those that supported the other alternative (all comparisons within subjects). The constructed state of coherence had the effect of spreading the options apart, which in turn yielded confident decisions. The memory task in Study 2 showed that participants' memories of their baseline preferences, measured in an incentive-compatible format, were swayed by the most recently constructed decision preferences. This finding, which is consistent with prior research (Holyoak & Simon, 1999), suggests that the coherence effect actually alters participants' preferences, and that the changes occur mostly without participants' awareness (see also Goethals & Reckman, 1973).

The coherence effect is best understood as a result of constraint-satisfaction processing that is involved in choice under decisional conflict. The coherence effect is endogenous to the decision-making process itself, and it occurs spontaneously in the absence of experimental interventions (cf. Bettman et al., 1998; Lichtenstein & Slovic, 2006; Simonson, 2008a, 2008b). The endogenous, spontaneous, and nonconscious nature of the coherence effect makes for a ubiquitous and formidable form of preference construction.

### *The elasticity of preferences*

The primary purpose of this article is to propose the idea that preferences have elastic properties and to provide empirical support for that proposition. We found that preferences are pliable in that they shifted reliably and systematically toward a state of coherence with whichever choice emerged. When participants reached the same choice in both sessions, they displayed similar sets of constructed preferences in the two sessions. But when they switched their choice between sessions, they constructed their preferences in opposite directions. We also found evidence supporting the property of stability: In the absence of situational pressure, preferences rested at similar baseline levels at the two sessions conducted 6 to

8 weeks apart. Like a rubber duck, preferences morph to correspond with environmental pressures (in this case, pressures borne by coherence-maximization processes), and revert to baseline in their absence.

These studies present a challenge to the categorical distinction between stable and constructed preferences. The findings suggest that a given preference will be stable under some circumstances but can be constructed—even in opposite directions—under different sets of circumstances.

The elasticity of preferences may serve an important adaptive function. Constructed preferences spread the options apart and thus enable people to make confident choices. Following choice, the reversion of preferences to baseline levels disencumbers decision makers from changes driven by past decisions, and frees them to reconstruct their preferences to facilitate confident choice on subsequent decisional tasks. Thanks to this elasticity, this facilitation will be available regardless of whether the decision makers arrive at the same choice on different occasions or reach opposite decisions, and regardless of whether each attribute is bundled with the same set of attributes on the different occasions (as in Study 1) or with a different set of attributes (as in Study 2).

We do not claim that these findings of elasticity apply universally. There is reason to assume that the elasticity of a given preference will be influenced by a variety of factors, including the internal properties of the attribute, situational pressures, personality traits, and personal experience with the attribute (see Hoeffler & Ariely, 1999). It is also likely that the return to baseline is affected by whether the particular decision and the foregone alternatives continue to be salient. Testing these assumptions is left for future research.

## Action Editor

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## Author Contributions

The authors contributed equally to this work.

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The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

## Supplemental Material

Additional supporting information can be found at http://pss.sagepub.com/content/by/supplemental-data

## Open Practices

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## Notes

1. These counts do not include 2 participants who managed to enroll before the study was posted; we excluded their responses from all analyses before looking at their data. In addition, eight records from Session 1 and two records from Session 2 were excluded from all analyses because they were incomplete or provided by people who had already participated.
2. In addition, 24 records from Session 1 (5 of which included data recorded after assignment to a condition) and 15 records from Session 2 (8 of which had recorded responses) were excluded from all analyses because they were incomplete or provided by people who had already participated.

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