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Contrast Effects in Judgments of Health Hazards

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ABSTRACT. Researchers commonly use 2 models to explain contrast effects (CEs): the standard-of-comparison model and the set–reset model. The 2 models focus on the role of categorization to predict when a CE (instead of an assimilation effect) will happen, while minimizing the role of knowledge accessibility and relevance in determining whether any effect will occur. A 3rd model, the selective-accessibility model (F. Strack & T. Mussweiler, 1997), focuses on knowledge accessibility and relevance, but it is a model of assimilation effects in the anchoring bias. In the present study of CEs, the authors tested 3 predictions implied by the selective-accessibility model. The authors found a CE only when anchor- and target-rating dimensions matched and only in the 1st of multiple targets rated. The CE required a minimum amount of attention to the anchor. These results support the account that selective knowledge accessibility and relevance play an important role in CEs.

Key words: anchoring effect, contrast effect, knowledge accessibility, relevance

CONTRAST EFFECTS (CEs) have a long history in research on social judgment. For example, after being in a hot and stuffy bus, a person who walks outside into an otherwise warm day experiences the day as cool and refreshing, if only for a moment. The contextual cue of extreme magnitude (the hot bus) is often called the *anchor*, and its influence on judgment (causing a person to think a warm day is actually cool) is called the CE. The finding of CEs is related to the broader literature on judgmental heuristics and biases (Tversky & Kahneman, 1974, 1981, 1983).

CEs were first demonstrated in experiments on psychophysics (e.g., Hunt & Volkman, 1937). The first exploration of CEs in experiments on social judgment

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was by McGarvey in 1943. Marsh and Parducci (1978, Study 1) 35 years later replicated McGarvey's experiment by using more careful methodology.

In their experiment, Marsh and Parducci (1978) asked participants to rate the morality of various behaviors such as working in a summer camp for blind children or spreading rumors that an acquaintance is a sexual pervert. Participants used a rating scale that ranged from *very, very bad* to *very, very good* with *neutral* as the midpoint. All participants rated a common set of 27 target behaviors mixed with a group of 29 anchor items that varied by condition. The 27 target items varied from low to high in morality according to a pilot study. The anchor items varied in extremity by anchor condition: low, medium, or high. The mixing of multiple anchors with multiple targets of varying magnitudes assured that the anchors were not identifiable in any way. Marsh and Parducci found a CE in that the addition of low-morality anchor behaviors caused ratings of the target behaviors to be higher than did the addition of high-morality anchors. The CE was the same as that demonstrated by McGarvey and similar to those in numerous other social judgment experiments (e.g., Herr, 1986; for a review, see Ford & Thompson, 2000).

Two models are commonly used to explain CEs. The first is the standard-of-comparison model that views the CE as resulting from a heuristic (Herr, 1986). According to the model, people asked to make a judgment do not draw all information that they have available to them but rely on a subset of their total knowledge and cues from the immediate context. Extreme anchors will be noticeably different from moderate targets and thus become a standard for comparison. In noticing the difference between anchor and target, people's judgments of the target move away from the anchor.

The standard-of-comparison view can be further refined by noting that attitudes expressed when one is rating a target are constructed in relation to the norm suggested by the anchor well after the target was first experienced (Kahneman, 1992; Kahneman & Miller, 1986). When people map an anchor onto a response scale, the extreme anchor defines or "anchors" one end of the scale. Thus, when a moderate target is mapped onto the response scale, the moderate rating is pushed toward the end of the scale not occupied by the anchor, causing a CE. The latter explanation seems appropriate for Marsh and Parducci's (1978) study, in which extreme anchors redefined the meaning of the rating scale.

A second model, the set–reset model, suggests that people attempt to correct for the influence of the information primed by the anchor (Martin, 1986; Wilson & Brekke, 1994). In the case of CEs, people are aware of the anchor and attempt to correct for its effect. This seems an unlikely explanation of Marsh and Parducci's (1978) results because the mixing of anchors and targets would have prevented people from noticing the anchor that they needed to correct for. However, the two theories need not be mutually exclusive. A series of insightful experiments by Moskowitz and Skurnik (1999) showed that both models are needed to explain certain judgmental phenomena.

Selective-Accessibility Model

In the present article, we tested an alternative account of CEs. This account is borrowed from the literature on the anchoring effect, a bias affecting judgments under uncertainty (Chapman & Johnson, 1994, 1999; Tversky & Kahneman, 1974). Researchers typically study the anchoring bias by using a design with a single anchor and a single target (Brewer & Chapman, 2002; Wilson, Houston, Eitling, & Brekke, 1996). One recent study of assimilation effects in the anchoring bias is of particular interest.

Strack and Mussweiler (1997, Study 1) used the single-anchor/target paradigm to explore the effect of having anchor and target rated on different dimensions. Participants answered two questions about the Brandenburg Gate. In the first stage of the experiment, half of the participants indicated whether the Brandenburg Gate was wider or narrower than a randomly assigned anchor width; the other half compared the gate's height with an anchor height. In the experiment's second stage, half of the participants estimated the Brandenburg Gate's width, and the other half estimated its height.

Thus, the experiment had four conditions, two in which the initial comparison and later estimate concerned the same dimension and two in which the dimensions did not match. The experiment yielded an assimilation effect. However, judgments assimilated more strongly toward the anchors in the matched than in the unmatched conditions. For example, participants' answers were closer to the anchor (and thus more biased) if they anchored on the gate's width before estimating it than if they first anchored on its height.

Strack and Mussweiler used the foregoing results to support their selective-accessibility model (Mussweiler & Strack, 1999; Strack & Mussweiler, 1997). The model explains that during the first stage of the experiment, people construct an internal representation, or mental model, of the target and its relation to the anchor. If the first judgment is about width, then information relevant to width is primed and becomes accessible. In the second stage of the experiment, the primed information exerts a biasing effect only if it is applicable to the judgment being made. The changing of rating dimensions is theorized to diminish the anchoring effect by minimizing the relevance of previously activated knowledge to the present target ratings. Thus, an initial judgment about width is inapplicable to a later judgment about height and is less potent as an anchor.

The selective-accessibility model also offers an explanation for the conditions in anchoring studies that yield contrast instead of assimilation—an explanation very similar to that offered by Herr (1986). Activated information can become a standard for comparison instead of acting as a basis for judgment (or having no effect at all). The conditions that determine whether an assimilation effect or a CE will result have been the subject of substantial debate, but in this study, we focused on CEs.

Strack and Mussweiler have used principally the selective-accessibility

model to explain assimilation effects found in the anchoring bias. To explain the few examples of CEs found in the anchoring bias (e.g., Strack & Mussweiler, 1997, Study 2), they borrowed the standard-of-comparison explanation. The selective-accessibility model does not, strictly speaking, have a prediction for the Marsh and Parducci (1978) study because the model requires that the anchor and the target be explicitly compared. During the comparison, the biasing effect of the anchor is exerted. Nonetheless, the notions that target-relevant information is activated when anchors are rated and that such information later exerts a biasing effect might be generalizable to CEs.

In the present study, we explored the role of knowledge accessibility in CEs through a novel approach suggested by the selective-accessibility model. With one variable, we manipulated whether anchors and targets were rated on the same scale, a direct application of Strack and Mussweiler's Brandenburg Gate study (1997, Study 1).

With a second variable, we manipulated the number of anchors to determine to what extent anchor information must be primed to create a CE. Previous research has shown that multiple anchors, when presented in a highly controlled experiment, can trigger CEs (Marsh & Parducci, 1978). Other experiments have shown that a single anchor can cause a CE, but typically researchers have embedded the anchor in a richly descriptive vignette (Markovsky, 1988). In the present study, we sought to examine whether as few as two anchors that were not embedded in a descriptive vignette would elicit a CE.

With a third variable, we explored whether the CE endured beyond the first target rating. If knowledge accessibility played an important role in CEs, then ratings of the extreme anchors would affect the first target. Ratings of the first target (of moderate likelihood) would interrupt the anchor information primed earlier in the rating of the anchors (of extreme likelihood), and no CEs would be found among other targets rated later.

We predicted that a CE would be found when there were many anchors, when the rating dimensions of anchor and target matched, and when the outcome measure was the first target rated. Conversely, we expected not to find a CE when there were few anchors, when the rating dimensions of anchor and target differed, and among ratings of additional targets.

Method

Overview

Participants completed a brief survey asking them to rate several health hazards. Participants rated two sets of health hazards, anchors and targets, in a three-way ($2 \times 2 \times 2$) between-subjects design. We varied the number of anchors presented, whether the anchor and target were rated on the same dimension, and whether the anchors were high or low on the dimensions on which they were

rated. Although the second variable technically represented two separate variables (anchor-rating dimension and target-rating dimension), the two were collapsed and coded for being matched (same) or unmatched (different). Participants rated multiple targets enabling a comparison of CEs in the first and multiple targets.

Pilot Study

We first conducted a pilot study to select rating dimensions and to select anchor and target hazards. The design of the main study called for two rating dimensions that had only a minimal correlation with one another. Highly correlated dimensions would make it very difficult to identify anchor hazards that were high on one dimension and low on the other. In the pilot and main studies, participants completed the surveys anonymously and were debriefed afterwards about their purpose.

Procedure. The pilot study had a between-subjects design in which each participant used one of five dimensions, but all participants rated the same 120 hazards. In exchange for extra credit, 138 undergraduate college students participated. Instructions on the first page of the survey asked participants to give all answers in percentages, from 0% to 100%, and to use lower numbers to indicate lower levels of the dimension being rated and higher numbers to indicate higher ratings on that dimension. Participants were instructed to write down their best estimate for each hazard, even if they were unsure of their answer.

Rating dimensions. Participants rated the hazards on one of five dimensions: likelihood, behavior, avoidance, control, and controllability. The question assessing likelihood was "What is the chance that this event will happen to you in the next year?" The question assessing behavior was "How much of the time is this event influenced by the average person's behavior?" The question assessing avoidance was "What portion of a year's tuition would you be willing to pay to avoid experiencing this event for a year?" The question assessing control was "How much of the time can you control whether this event happens to you?" A modified version of the control dimension that we called controllability was tested after the wording of the original was deemed to be unclear. The revised question assessing controllability was "How much can you control whether this event happens to you?"

Results. As explained earlier, it was desirable to have two rating dimensions that had a low correlation with one another. The dimensions of likelihood and controllability were selected from among the five dimensions tested in the pilot study because of their low correlation with one another ($r = .23$). We selected 8 hazards (get the flu, sprain an ankle, have cold ears in the winter, have a root canal, need to go to the dentist, get indigestion, get a cold sore, develop a fear of heights)

for use as targets because the pilot study showed them to be rated close to 50% on both the likelihood and controllability dimensions. We chose 16 anchor hazards (e.g., get a wart, have back pain) that were extreme on both of the rating dimensions (see Table 1).

Participants

For the main study, 295 college students were recruited from two undergraduate psychology courses. Data for 4 participants who returned incomplete surveys were dropped from the analysis.

Rating Dimensions

Participants rated hazards on the two dimensions, likelihood and controllability, by writing down a percentage between 0% and 100%. Thus, a rating of an anchor on one dimension could potentially have been used as a target rating on the other dimension. This arrangement was necessary so that we could reasonably expect the anchor ratings on one dimension to influence target ratings on another dimension.

Match

Participants first rated anchor hazards on one dimension and then rated target hazards on a second dimension. In the *same* (matched) condition, the anchor and target were rated on the same dimension: either both on likelihood or both

TABLE 1. Anchor Hazards Presented in the Four Anchor Conditions

Likelihood	Controllability	
	Low	High
Low	<i>Get laryngitis</i> Get a wart Have frequent constipation Develop a mild stuttering problem	<i>Have a hangover</i> Get a cavity Develop a drinking problem Get frostbite
High	<i>Get chapped lips</i> Bite tongue Sneeze twice in one day Get a small paper cut	<i>Use a Band-Aid</i> Get an upset stomach Have back pain Eat too much in a meal

Note. All hazards were used in the eight-anchor condition. Only the italicized hazards were used in the two-anchor condition.

on controllability. In the *different* (unmatched) condition, the anchor and target were rated on different dimensions: either one on likelihood and then the other on controllability or vice versa.

Anchor Level

To create the low- and high-extremity anchor conditions, we selected hazards that were uniformly extreme on one rating dimension but balanced on the other. In the first step, hazards that were extreme on both the likelihood and controllability dimensions were selected on the basis of ratings in the pilot study. Specifically, hazards were selected for each of four conditions: low likelihood–low controllability, low likelihood–high controllability, high likelihood–low controllability, and high likelihood–high controllability (see Table 1).

In the second step, we created subsets of the four conditions that held one of the dimensions constant. For example, in the low-controllability condition, we selected hazards so that all hazards were low in controllability but had a mixture of high and low likelihood (see the low-controllability column in Table 1). Mixing the anchors in this way produced four groups of anchors—2 (anchor extremity) \times 2 (rating dimension): low likelihood, low controllability, high likelihood, and high controllability.

Anchor Number

Participants received either two or eight anchors. These levels for the number of anchors were chosen as follows. Two anchors were the minimum number that this design would allow. This minimum was because the total set of anchors had to be high or low on one dimension but balanced on the other. Having only one anchor would have meant that it would have been extreme on both scales (e.g., either high on both likelihood and controllability or high on likelihood and low on controllability). The choice of eight as the quantity of high-extremity anchors was somewhat arbitrary. A post hoc analysis of the first eight targets rated in a pilot study (data not shown) suggested that eight hazards are the minimum required to produce a CE in this design.

Table 1 shows the hazards used in the two- and eight-anchor conditions. In the eight-anchor condition, all of the hazards were used, but in the two-anchor condition, only the italicized hazards were used. The anchor hazards chosen for the two-anchor condition were the most extreme on the two rating dimensions.

Targets

The eight hazards noted earlier served as targets. As described in the results of the pilot study, these targets were rated at approximately 50% on both the likelihood and controllability dimensions. Two ratings of the target hazards were the

dependent variables. One was the rating of the first target, and the other was the mean rating of the second through the eighth targets. We counterbalanced the presentation order of the first two targets to control for order effects and to be sure that the effects were not unique to one particular hazard. The last six targets were presented in the same order to all participants.

Surveys

The study had a 2 (match: whether anchor and target were rated on same dimension) \times 2 (anchor extremity: high vs. low) \times 2 (number of anchors: 2 vs. 8) design. Target counterbalance added another variable (\times 2) to the design that was later controlled for with a z-score transformation. The order of anchors was also counterbalanced, but this counterbalancing was done within the existing variables of the experiments by use of a Latin square design. Last, the match condition comprised two variables, 2 (anchor-rating dimension) \times 2 (target-rating dimension) that were subsequently collapsed in the analyses. All considered, these produced a 2^5 design, requiring 32 forms of the survey.

Procedure

We invited each participant to complete the questionnaire during a regular class period and gave him or her a candy bar as payment for participation. Each participant was randomly assigned to 1 of 32 experimental conditions. Other than the verbal invitation, all instructions were on the first page of the survey. Participants were instructed to give all answers in percentages, from 0% to 100%. Participants were instructed to use lower numbers to indicate lower levels of likelihood or controllability and higher numbers to indicate higher ratings on those dimensions. The two dimensions were introduced, and participants were told that they might be asked to rate hazards on one or both of the two dimensions. Finally, participants were instructed to write down their best estimate for each hazard, even if they were unsure of their answer.

Results

Manipulation Check

A manipulation check confirmed that the anchors were low and high in extremity relative to one another as expected. However, the low-extremity anchors were significantly lower in the eight-anchor condition than in the two-anchor condition. We performed a three-way analysis of variance (ANOVA)—Anchor Extremity \times Number of Anchors \times Anchor-Rating Dimension—and used the mean rating of all anchors as the dependent variable. The main effects of anchor extremity, number of anchors, and their interaction were significant,

$F_s(1, 287) = 218.95, 21.96, \text{ and } 7.55$, respectively; $ps < .01$. No effects of anchor-rating dimension on ratings of the anchors were found.

To inspect the interaction, we performed two Bonferroni-adjusted t tests. Significant differences between high and low anchor extremities in the two-anchor condition, $t(148) = -8.00, p = .0001$, and the eight-anchor condition, $t(143) = -14.91, p = .0001$, demonstrated that the anchor-extremity manipulation was quite successful. In both cases, ratings of the high-extremity anchors were higher than those of the low-extremity anchors.

z-Score Transformation

Target hazard ratings were transformed to z scores to allow us to put ratings of different hazards that were on different dimensions into the same ANOVA. Some participants had rated hazards on likelihood, and some had rated the target hazards on controllability; the two rating dimensions yielded somewhat different means and variances. Combining participant responses on the two dimensions required transforming the responses into comparable units. Similarly, the target hazard that participants first rated varied because of counterbalancing. The two hazards had different means and variances that also required that responses be transformed into comparable units. The means and variances for the four cells—2 (target-rating dimension) \times 2 (target counterbalance)—were calculated. Target hazard ratings for participants in each of the cells were then transformed into z scores by using the mean and standard deviation for ratings in that cell.

The z -score transformation did not eliminate the variance that was due to the experimental variables of interest. Within each of the four cells, the variability that was due to the 2 (match) \times 2 (number of anchors) \times 2 (anchor extremity) factorial design remained unaffected. For example, controlling for target-rating dimension did not affect the relative differences caused by high- and low-extremity anchors or by a change in rating dimensions between anchor and target.

Main Analysis

A simple way to think about this experiment is to view it as a 2×2 design corresponding to the Match \times Number of Anchors interaction. Within each of the four cells in the design, there was an effect of anchor extremity. Thus, one could inspect each of the four cells to see whether a CE occurred. Note that when the effect of anchor extremity is made explicit, the design contains eight cells.

We performed multiple planned contrasts to test the first two experimental hypotheses about the number of anchors and the changing of rating dimensions. We predicted that eight anchors would be required to obtain a CE. We also predicted that only when anchor- and target-rating dimensions matched would a CE occur. Thus, no CE should have occurred when there were too few anchors or when the anchor and target ratings did not match. This means that, of the four

cells in the Match \times Number of Anchors interaction, a CE should occur only in the eight-anchor, same-dimension cell.

We tested the experimental hypotheses by using planned contrasts because of the very specific predictions that we had made about the effects of match and number of anchors in mitigating the CE. Factorial ANOVA would compare the means of the two cells on one diagonal with the means of the remaining two cells on the other diagonal. This test would obscure the predicted pattern of results by averaging the cell with a predicted CE with a cell with no predicted effect. A stronger test was the use of a planned contrast to compare the one cell where an effect of anchor extremity had been predicted to the mean of the other three cells where no effect had been predicted.

We used an F test to test this planned contrast with ratings of the first target as the dependent variable. By a second F test, we inspected whether there was a difference among the three cells where no CE had been predicted. To test whether other CEs existed that were obscured by those two analyses, we used additional post hoc Bonferroni-adjusted F tests to inspect the simple main effects of anchor extremity in each of the four cells individually. We tested the third experimental hypothesis, that a CE would occur over single but not multiple targets, by performing the aforementioned series of tests with the mean of the other seven targets as the dependent variable.

Match and number of anchors. As predicted, concordance between rating dimensions and more anchors was required to obtain a CE (means are shown in Table 2). A significantly larger CE occurred in the eight-anchor, same-rating dimension condition than in the mean of the other three conditions, $F(1, 287) = 5.86, p < .02$, with targets in the high-extremity anchor condition rated lower than those in the low-extremity anchor condition. The mean difference between high- and low-extremity anchor conditions in the eight-anchor, matched (rated on same dimension) cell was .69, and the other cells averaged .12. The other three cells were not significantly different from one another, $F(2, 287) = 1.36, ns$.

Additional analyses showed that the combination of more anchors and matched rating dimensions was the only manipulation that caused any CE. By four contrasts, we inspected the effect of anchor for each cell of the Match \times Number of Anchors interaction. Only the eight-anchor, matched hazard-rating-dimension cell showed a CE, $F(1, 287) = 9.08, p < .01$. This was a small effect (semipartial correlation, $r = .18$.) The planned contrasts for the other three cells were not significant.

Multiple targets. As predicted, the CE did not endure over multiple rating tasks (means are shown in Table 2). The CE was no stronger in the eight-anchor, matched rating-dimension condition than in the mean of the other three conditions, $F(1, 287) = 1.86, ns$. None of the other contrasts yielded significant results. We confirmed the absence of a CE by repeating the analyses as univariate tests

TABLE 2. Target Ratings Expressed as z Scores

Match	z Score		<i>r</i>
	<i>M</i>	<i>SD</i>	
<i>Target 1 ratings</i>			
Different (for anchor and target)			
2			
Low-extremity anchors	0.08	1.00	
High-extremity anchors	-0.01	1.12	.03
8			
Low-extremity anchors	-0.03	0.90	
High-extremity anchors	-0.19	0.90	.03
Same (for anchor and target)			
2			
Low-extremity anchors	0.10	1.10	
High-extremity anchors	0.21	0.88	.04
8			
Low-extremity anchors	0.27	1.03	
High-extremity anchors	-0.42	0.92	.18*
<i>Targets 2-8 ratings</i>			
Different			
2			
Low-extremity anchors	-0.07	0.53	
High-extremity anchors	-0.05	0.55	.01
8			
Low-extremity anchors	0.05	0.45	
High-extremity anchors	0.01	0.61	.09
Same			
2			
Low-extremity anchors	0.16	0.55	
High-extremity anchors	-0.04	0.61	.02
8			
Low-extremity anchors	0.09	0.52	
High-extremity anchors	-0.16	0.61	.12

Note. 2 = two-anchor condition; 8 = eight-anchor condition. The z-score transformation of the dependent variable controlled for the target rated and rating dimension. Higher z scores indicate a higher rating (i.e., on perceived risk or controllability). A drop in scores between low- and high-extremity anchor conditions indicates a contrast effect; an increase in scores indicates an assimilation effect. The last column contains semipartial correlations describing the magnitude of the anchoring effect. *N* = 291.

**p* < .01.

for each of the seven targets. None of these tests yielded significant results. Of most interest, the second target showed no contrast. The analysis for the second target addressed a potential concern that using different hazards in the remaining targets was responsible for the absence of a CE because the hazards used for the first and second targets were counterbalanced.

Discussion

Models of the processes that lead to CEs typically focus on the role of inclusion or exclusion of new information in existing categories. These models, such as the standard-of-comparison model (Herr, 1986) and the set–reset model (Martin, 1986), minimize or ignore the role of knowledge accessibility. The selective-accessibility model, with its focus on the role of knowledge accessibility and relevance in judgment formation, guided the present research in examining three variables that potentially affect CEs.

Our first prediction, that a change in anchor- and target-rating dimensions would eliminate CEs, was confirmed. The present study extended findings by Strack and Mussweiler (1997, Study 1), who found that changing rating dimensions reduced assimilation effects in the anchoring bias. Those authors explained this result by using their selective-accessibility model. According to the model, anchor information must be accessible and relevant when the target judgment is being made in order to exert a biasing effect. Anchoring and contrast both appear to be disrupted by targets' and anchors' being rated on different scales, a manipulation that presumably makes the anchor ratings less relevant when the target is rated.

In both the present and the Strack and Mussweiler (1997) studies, an anchoring effect or CE occurred when the anchor and target were rated on the same dimension. However, when anchor and target were rated on different dimensions, the assimilation effect was reduced in the Strack and Mussweiler (1997) study, and the CE was eliminated altogether in our study. It is possible that the minor difference in the findings—that one effect was reduced and the other was completely eliminated—was due to a difference in the designs used. Strack and Mussweiler used an anchoring paradigm that requires an explicit comparison of anchor and target, whereas in the present study, we did not. Mussweiler and Strack (1999) argued that their paradigm may simply be a more powerful way to elicit anchoring effects. It is also possible that our study did not have adequate power to detect a small CE in the mismatched (different) condition.

Our second prediction, that a minimum number of anchors is required to obtain a CE, was confirmed. As few as eight anchors yielded a CE, but two were not sufficient. The finding suggests that CEs require some minimal level of processing to activate anchor information before such information can bias the evaluation of targets. When insufficient anchor processing occurs, the target evaluation is unaffected. This finding also suggests that the number and potency of anchors limit the likelihood of inadvertently obtaining CEs in common settings such as survey design.

Our third prediction, that a CE would occur for the first target but not for multiple targets, was confirmed. CEs have been shown with both single targets (e.g., Strack & Mussweiler, 1997) and multiple targets (e.g., Marsh & Parducci, 1978; Parducci, 1968). However, the Parducci studies mixed multiple anchors with multiple targets, a difference that makes it difficult to interpret the reported CE. The CE found in the Marsh and Parducci study may have been due to the repeated presentation of anchors. In essence, the experiment was a continuous series of one-anchor–one-target experiments, not a demonstration of an effect that endured over multiple target ratings.

The failure of the CE to endure over multiple targets in the present study is consonant with a knowledge-accessibility account of CEs. Increased attention to previous anchor hazards increased their potency as anchors, but more recently presented target hazards were more potent than less recent anchors. Presumably the act of rating the first target health hazard established a new “anchor” in the minds of participants, thus making the earlier anchor information less accessible or at least diluting its impact.

In summary, applying the insights of the selective-accessibility model (Mussweiler & Strack, 1999; Strack & Mussweiler, 1997) to CEs yielded a number of useful insights into the conditions that limit the generality of CEs in both experimental and natural settings. Perhaps the most intriguing implication of the present research is that the processes that lead to assimilation effects and CEs react similarly to changes in rating dimensions, suggesting that both are sensitive to selective-knowledge-accessibility effects. An alternative conclusion is that selective-knowledge-accessibility effects (a) are a common phenomenon found in numerous judgmental biases and (b) are not limited to the anchoring bias. The findings of the present study may be useful in future modeling of the processes that give rise to CEs.

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