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Anchoring, Activation, and the Construction of Values

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Anchoring is a pervasive judgment bias in which decision makers are systematically influenced by random and uninformative starting points. While anchors have been shown to affect a broad range of judgments including answers to knowledge questions, monetary evaluations, and social judgments, the underlying causes of anchoring have been explored only recently. We suggest that anchors affect judgments by increasing the availability and construction of features that the anchor and target hold in common and reducing the availability of features of the target that differ from the anchor. We test this notion of anchoring as activation in five experiments that examine the effects of several experimental manipulations on judgments of value and belief as well as on measures of cognitive processes. Our results indicate that prompting subjects to consider features of the item that are different from the anchor reduces anchoring, while increasing consideration of similar features has no effect. The anchoring-as-activation approach provides a mechanism for debiasing anchoring and also points to a common mechanism underlying anchoring and a number of other judgment phenomena. © 1999 Academic Press

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INTRODUCTION

Think of the last three digits of your phone number. Now add 400 to that number, and think of the resulting number as a date. Consider whether Attila the Hun was defeated in Europe before or after that date. In what year would you guess Attila the Hun was actually defeated?

The correct answer is 451. When Russo and Shoemaker (1989, p. 90) asked college-educated respondents this question, their answers were strongly influenced by the dates computed from their telephone numbers. Figure 1 illustrates the relationship between the estimates and the initial anchors. These noninformative numbers are in no plausible way related to the year of Attila's defeat, but are nevertheless influential. Similarly, Northcraft and Neale (1987, Experiment 2) asked students and real estate agents to tour a house and appraise it. Appraisal values assigned by both experts (real estate appraisers) and amateurs (students) were positively related to the provided anchor, the listing price of the house, even though it should be irrelevant to the appraised value. This demonstration is particularly impressive, since all estimates were made after a tour of the house, in the presences of substantial sets of alternative information, and because anchors affected both experts and novices.

The effect of anchors is one indication that many statements of value and belief are not directly retrieved from memory but rather are constructed online in response to a query (Payne, Bettman, & Johnson, 1992; Sudman, Bradburn, & Schwarz, 1996). Anchoring clearly illustrates this construction, because judgments of value and belief are influenced by irrelevant or uninformative starting points present only at the time of questioning. In everyday judgments, the construction of values may involve an informative anchor, an uninformative

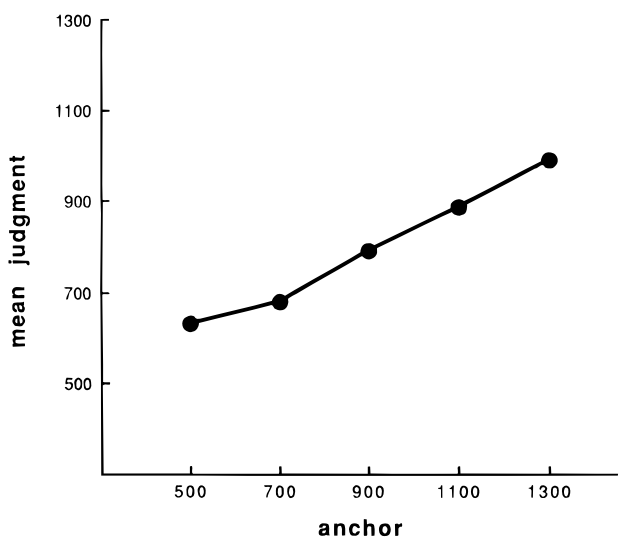

FIG. 1. Mean answers to the question "In what year was Attila the Hun defeated in Europe?" plotted as a function of anchor values derived from subjects' telephone numbers. (Data are from Russo & Shoemaker, 1989.)

anchor, or even a self-generated anchor. In contrast, demonstrations of anchoring (including the experimental tasks in the studies presented here) involve anchors which are explicitly specified as uninformative so that any influence of an anchor can be identified as a bias.

Perhaps the earliest mention of the anchoring bias comes from the psychophysical research where one extreme weight influenced judgments of other weights (e.g., Brown, 1953; see also Kahneman, 1992). Notions of anchoring were first introduced to decision making research in early descriptions of preference reversals (Lichtenstein & Slovic, 1971; Slovic, 1967; Slovic & Lichtenstein, 1968). Demonstration of the anchoring effect are plentiful in studies of judgment. For example, anchoring affects the pricing and rating of simple gambles (Carlson, 1990; Chapman & Johnson, 1994; Johnson & Schkade, 1989; Schkade & Johnson, 1989), the estimation of probabilities (Plous, 1989; Wright & Anderson, 1989), and answers to factual knowledge questions (Jacowitz & Kahneman, 1995; Kahneman & Tversky, 1974). Moreover, anchoring affects social judgments of the self and others, specifically, judgments of self efficacy (Cervone & Peake, 1986), judgments of spousal preferences such as false consensus effects (Davis, Hoch, & Ragsdale, 1986), and predictions of future performance (Switzer & Sniezek, 1991).

Not only does anchoring occur in many varied domains but it is also a key component of theories explaining other judgment phenomena. For example, anchoring and adjustment is a central theoretical part of explanations of the effect of ambiguity on probability judgments (Einhorn & Hogarth, 1985) and of belief updating (Hogarth & Einhorn, 1992), and it is a suggested mechanism in the expression of values (Busemeyer & Goldstein, 1992; Goldstein & Einhorn, 1987). Anchoring is also described as a cause of preference reversals (Lichtenstein & Slovic, 1971; Schkade & Johnson, 1989), and biases in utility assessment (Hershey & Schoemaker, 1985; Johnson & Schkade, 1989). Anchoring has been implicated in biases in the judgment of belief, leading to information framing effects (Levin, Schnittjer, & Thee, 1988), resulting in biased causal attribution (Quattrone, 1982), and influencing detection of deception (Zuckerman, Koestner, Colella, & Alton, 1984).

Despite its prevalence in demonstrations and explanations, anchoring itself has received much less theoretical attention. In contrast to its use as an explanation of other phenomena, there has been little discussion of the causes of anchoring itself until recently. An understanding of the mechanisms underlying anchoring is important because it can lead to debiasing manipulations. Early descriptions of anchoring described it as a process in which people anchor on a starting point and then adjust from that starting point to their final answer, albeit insufficiently (Kahneman & Tversky, 1974; Lichtenstein & Slovic, 1971). In recent years, however, a new account of anchoring has been offered (Chapman & Johnson, 1994; Jacowitz & Kahneman, 1995; Kahneman and Knetsch, 1993; Mussweiler & Strack, 1999, in press; Strack & Mussweiler, 1997). We describe what we see as the core idea of this theoretical account of anchoring, and we propose some new ways of testing this account. Five experiments examine the effect on outcome measures of anchors and other experimental

manipulations as well as measures of cognitive processes that assess the pattern of attention while judgments are made. We close by discussing the relation between the mechanism we implicate in anchoring with those involved in other judgment tasks.

Recent Anchoring Research

Anchoring effects have often been explained in conjunction with the idea of insufficient adjustment away from the anchor. The name "anchoring and adjustment" suggests a particular cognitive process whereby decision makers first focus on the anchor and then make a series of dynamic adjustments toward their final estimate. Because these adjustments are insufficient, the final answer is biased toward the anchor. Tversky and Kahneman (1974) described this adjustment process as occurring in their well-known anchoring demonstration. Recently, however, a new type of explanation for anchoring has been proposed by several authors.

Strack and Mussweiler (1997; Mussweiler & Strack, 1999, in press) proposed a Selective Accessibility Model in which anchoring is a special case of semantic priming. Specifically, information that is retrieved in order to compare the anchor to the target is consequently more available for use when estimating the target value. This account predicts that the primed information will influence the target judgment only if it is semantically relevant, and Strack and Mussweiler (1997) found just this result. In one study, subjects estimated the height or width of the Brandenburg gate after considering a numerical anchor described as the height or width of the gate. Anchoring occurred if both the anchor and the target judgment represented the height (or both the width); it did not occur if one was height and the other was width. In other words, for anchoring to occur, the anchor must be relevant to the judgment. (These results are consistent with those of Chapman & Johnson (1994) but contrast somewhat with those of Wilson, Houston, Etling, & Brekke (1996), who found that an anchor compared to the number of physicians in the phone book influenced a later judgment about the number of countries in the United Nations.) In a further experiment, Strack and Mussweiler (1997) found that extreme anchors resulted in shorter response times for the comparative judgment (deciding if the anchor was higher or lower than the target) but longer response time for the absolute judgment (target estimation). This result indicates that comparisons to implausibly extreme anchors do not require relevant target information to be retrieved, yielding a faster comparative judgment. Because this target information has not been primed, however, the absolute judgment takes longer. The implication is that for less extreme anchors, the information primed during the comparative judgment is used in the absolute judgment.

Jacowitz and Kahneman (1995) proposed that the anchor acts as a suggestion; that is, it is considered as a candidate response that subjects entertain, at least as a transient belief, and therefore influences the target value. This account follows directly from Gilbert's (1990, 1991; Gilbert, Tafarodi, & Malone,

1993) work showing that comprehension includes an initial belief in the assertion presented, followed only later by rejection of false information. Jacowitz and Kahneman (1995) do not provide direct evidence for this account. Instead they present an experiment that rules out a competing account of anchoring—that anchoring involves an adjustment process. In their experiment, trivia questions (e.g., the height of Mount Everest) were presented to a calibration group. The 15th and 85th percentiles of the estimates from this group were used as anchors for a second group of experimental subjects. These subjects were presented with a high or low anchor and asked whether it was higher or lower than the target value (e.g., the height of Mount Everest). These comparison judgments showed an anchoring effect: Although 15% of the calibration subjects had given target estimates lower than the low anchor (or higher than the high anchor), a much larger percentage of the experimental subjects said that the target value was lower (or higher) than the anchor. That is, the comparison question influenced judgments of the target value even before subjects were asked to give a point estimate of the target value. Green, Jacowitz, Kahneman, and McFadden (1998) found a similar result. This result is notable because it demonstrates an anchoring effect in a task where no adjustment is possible because subjects had not (yet) given a final estimate, but only judged whether the anchor was higher or lower than the target. Thus, anchoring occurred in a task where adjustment cannot occur, indicating that an adjustment process is certainly not necessary for an anchoring effect and suggesting that another type of mechanism, such as activation, may be involved.

Kahneman and Knetsch (1993) also proposed an activation anchoring mechanism; specifically, that the target question acts as a memory probe that retrieves the anchor mentioned earlier. The anchor is then treated as a candidate response. This priming is in some ways shallow, however, because the anchor does not always influence the target judgment, even when it primes relevant information. For example, they asked Toronto residents whether they would pay $25 (low anchor) or $200 (high anchor) to clean up specified lakes so as to maintain fish populations. The anchor influenced estimates of the amount that the average Toronto resident would contribute but not estimates of the percentage of Toronto residents who would pay $100. The anchor influenced the first question but not the second presumable because the first shares more surface features with the anchor. Thus, anchor priming is shallow in that it is influenced by surface features rather than deep structure.

Anchoring as Activation

All of these recent anchoring accounts propose that the anchor makes information that is consistent with the anchor more available. That is, anchoring is the result of what Arkes (1991) calls an association-based error. The key feature of these activation accounts is that the anchor biases the information used in the target evaluation by selectively activating target information that is consistent with the anchor (see Mussweiler & Strack, in press, for an explicit model). “Anchoring as activation” describes the notion that anchors influence

the availability, construction, or retrieval of features of the object to be judged. Assume a decision-maker, when requested to make a judgment, can access or construct a variety of features about the target item, either from memory or the external environment. Some subset of this information is retrieved and used as the basis for the preference judgment. If a representative sample is retrieved, the judgment will be unbiased. However, factors that affect retrieval or construction can bias the judgment. Specifically, the presence of an anchor increases the activation of features that the anchor and target hold in common while reducing the availability of features of the target that differ from the anchor.

For example, suppose a decision maker asked to evaluate a bus trip was presented with a low anchor. The decision maker might contemplate a bumpy, noisy, crowded ride aboard a city-owned vehicle that spouts diesel fumes and has hard plastic seats and no air conditioning. Conversely, if a high anchor were presented, the decision maker might imagine a luxurious tour bus with reclining plush velour seats, sound system, and wet bar, perhaps used on other weekends by touring rock stars.

Memory-based judgments of confidence have been shown to demonstrate just such differential availability. Koriat, Lichtenstein, and Fischhoff (1980) asked subjects to answer trivia questions and then estimate the probability that each of their answers was correct. The observed overconfidence in their predictions was caused, Koriat et al. demonstrated, by the retrieval of reasons why their answer was right, but not those that would suggest that the answer was wrong. Overconfidence was reduced by prompting subjects for reasons they might be wrong, but was unaffected by prompts for reasons why subjects might be right.

Koriat et al.'s (1980) results indicate that decision makers naturally consider reasons they might be right (so prompting them to do so changes nothing) but usually fail to consider opposing reasons (so prompts for these reasons has an effect). Hoch (1984) found a similar result when subjects predicted their future behaviors. They could more easily report reasons supporting their prediction than those suggesting why they might be wrong. These initial biases can be particularly powerful for memory based search, since initial search for confirming information leads to a decreased availability of disconfirming information.

This bias toward attending to similarities is analogous to a number of phenomena often labeled collectively as confirmation bias (Klayman, 1995). In a variety of tasks, decision makers tend to seek external information that, if consistent with the current hypothesis, would yield positive feedback (e.g., Wason, 1960) or to interpret evidence as consistent with the hypothesis (e.g., Lord, Lepper, & Preston, 1984). Although this strategy is often effective (Klayman & Ha, 1987), it occurs even if the information sought is not diagnostic because it is consistent with many hypotheses. In contrast, hypothesis testers are unlikely to seek information expected to be inconsistent with the hypothesis, even if that information is quite diagnostic (e.g., Beattie & Baron, 1988; Skov & Sherman, 1986; Snyder & Swann, 1978). This bias is similar to the activation

account of anchoring in that decision makers examine evidence expected to confirm the hypothesis—evidence similar to the hypothesis—rather than evidence that could disconfirm the hypothesis.

The activation view of anchoring suggests that the effects of anchoring are related to judgments of similarity. In judging whether the value of a target object is above or below an anchor, decision makers actually consider how the anchor and target are similar (Tversky, 1977). As a result, anchors have their effect because decision makers consider reasons why their value for the target item is like the anchor, but show relative neglect for reasons why their value for the item is unlike the anchor.

Although accounts similar to Anchoring as Activation have been explored in several recent investigations, very few studies have explicitly tested whether the presence of the anchor biases the retrieved target information. Mussweiler and Strack (1999, in press), however, conducted two studies that support this proposition. In one study (described in Mussweiler & Strack, in press), subjects answered an anchoring question ("Is the annual mean temperature in Germany higher or lower than 5°C [or 20°C]?") and then participated in a lexical decision task. Those given the low anchor were faster at identifying words such as "cold" and "snow" while those given the high anchor were faster at identifying words such as "hot" and "sun." This result shows that the anchor primed consistent information in memory. In another study (Mussweiler & Strack, 1999, Experiment 4), some subjects answered an anchoring question (e.g., about the length of the river Elbe) and were instructed to list the features of the target that came to mind. Those given a high anchor tended to list thoughts that implied a high target value, while those given a low anchor tended to list thoughts that implied a low target value.

The purpose of the present experiments was to extend these previous results by providing a new type of test of the idea that the presence of an anchor biases the target information that is retrieved, collected, or constructed. These studies also evaluate a debiasing method that follows from the activation view.

Predictions

The activation view of anchoring makes several predictions about the anchoring phenomenon that have not previously been examined.

Attentional prompts. If anchoring occurs because decision makers attend to target features similar to the anchor, then anchoring effects can be reduced by increasing the salience of target features that are different from the anchor, for example, by prompting subjects to consider these features. The activation account predicts that subjects naturally attend to target features that are similar to the anchor. A prompt to consider different features does not prevent subjects from considering similar features, but it causes them to consider different features in addition to the similar features. By calling subjects' attention to the features that are different from the anchor, the influence of all features become comparable, neutralizing the anchoring effect. In contrast, prompting subjects for similar features should not increase the anchoring effect because

such a prompt induces subjects to consider only those features they would consider anyway. Thus, a similar prompt should have no effect relative to a control condition.

Random anchors. One might argue that decision makers consider target features that are similar to the anchor because they perceive a demand effect to attend to the anchor. That is, they may conclude that the very presentation of the anchor indicates its informativeness and that it is therefore rational to consider responses to the target prompt that are consistent with the anchor. The activation account predicts, however, that the anchor will facilitate retrieval of target features similar to the anchor even when the anchor is not perceived to be informative. That is, activation is not an intentional strategy but instead a more automatic cognitive process (Jacowitz & Kahneman, 1995). Consequently, the attentional prompt prediction should hold true even for anchors that subjects consider to be truly random and uninformative.

Decision process measures. Anchoring as activation predicts that decision makers will give more attention to target features that are like the anchor as compared to features that are different from the anchor. This prediction reflects the overweighting of target features similar to the anchor. Process measures that reflect attention, such as looking time, should reveal this differential weighting.

Elaboration. According to the activation account, anchoring occurs because the anchor facilitates retrieval of target features similar to the anchor. Consequently, anything that enhances this selective facilitation should strengthen the anchoring effect. If a large pool of target features exists, the anchor will have more opportunity to selectively enhance the similar features. Making all target features more accessible would essentially expand the pool of features and thereby enhance the anchor's ability to facilitate retrieval of similar features. One method of making features more accessible is through elaboration, that is, listing features or reasons associated with the target. We therefore predict that anchoring will be increased by previous elaboration of the target features.

We conducted five experiments to test the activation account of anchoring. Experiment 1 provided a test of the feature prompt prediction that anchoring of preference judgments can be reduced by prompting subjects to consider features of the target item that are different from the anchor. In this study we also looked for process measure evidence of activation. Experiment 2 replicated the key finding of Experiment 1 with belief judgments. Experiment 3 demonstrated the anchoring effect with completely random anchors, and Experiment 4 replicated the attentional prompt effect with completely random anchors. Experiment 5 demonstrated that anchoring is enhanced when target features are more accessible to priming because of elaboration.

EXPERIMENT 1

Experiment 1 provided a test of the attentional prompt and decision process predictions. The activation account asserts that anchoring is caused by an

overweighting of the similar features between the anchor and the target. In Experiment 1, we attempted to disrupt this process by interposing a comparison task prior to the pricing task.

The procedure used in this experiment differed from that used an many previous anchoring demonstrations (e.g., Tversky & Kahneman, 1974) in that the anchor was not merely a number, but instead had multiple features. Specifically, subjects judged the value of student apartments in the presence of another "anchor" apartment which had previously been evaluated as attractive or unattractive. This procedure provided both the target and the anchor with features that could be similar or dissimilar. The activation account suggests that subjects will normally attend to the features that the anchor and target have in common and produce prices (rents) that are closer to the anchor apartment than if no anchor had been present. To mitigate this effect, we asked some subjects to identify which feature of the target apartment was different from the anchor.

Subjects considered apartments described in terms of three features: distance to campus, appearance, and safety. Subjects were asked to compare a target apartment to an anchor apartment and find the attribute on which they were most similar, or most different, or the "middle" attribute—the one that was neither most similar nor most different. Thus, subjects made a similarity, dissimilarity, or neutral judgment before indicating the most they would pay for the target apartment. We expected that prompting subjects to identify the most similar feature should have no effect, compared to the neutral "middle" condition. In contrast, prompting subjects to identify the most different feature should reduce anchoring.

Experiment 1 collected measures of information search using a computer-based display of information employing the *Mouselab* software package[1] (Johnson, Payne, Schkade, & Bettman, 1989). The collection of information search measures allowed us to assess whether subjects spent the most time considering target features most similar to the anchor. *Mouselab* has been used extensively to replicate a number of decision phenomena, such as preference reversals, without significantly affecting the phenomena (Payne, Bettman, & Johnson, 1992).

Method

Subjects. Subjects were 24 college students from Philadelphia area colleges and universities. They were paid $6.00 per hour.

Materials. The items used were apartments described in terms of three attributes: distance to campus, appearance, and safety. Each attribute had four levels (four descriptions). Also, there were two versions (slightly different wordings) of each level, so that one version could be presented with a high anchor and the other with a low anchor. These descriptions are presented in Table 1.

The 12 target apartments were constructed from the three attributes. Table

[1] *Mouselab* is available at http://www-marketing.wharton.upenn.edu/~mouselab/.

TABLE 1

Stimuli for Experiment 1

Distance attribute

Set A	Set B
1. 33 min to campus	1. 32 min to campus
2. 24 min to campus	2. 23 min to campus
3. 15 min to campus	3. 14 min to campus
4. 6 min to campus	4. 5 min to campus

Appearance attribute

Set A
1. Tiny apartment in poorly maintained building, no a/c but some conveniences
2. Small cozy apartment in adequately maintained building, equipped, ww/c, no a/c
3. Moderate-sized apartment in well maintained building, equipped, ww/c, a/c
4. Large, spacious, clean apartment in remarkably well maintained building, all conveniences, ww/c, a/c

Set B
1. Very small apartment in ill-maintained building, no a/c but some conveniences
2. Snug little apartment in moderately well maintained building, equipped, ww/c, no a/c
3. Medium-sized apartment in well maintained building, equipped, ww/c, a/c
4. Big, roomy, immaculate apartment in exceptionally well maintained building, all conveniences, ww/c, a/c

Safety attribute

Set A
1. POOR: Questionable neighborhood, recent muggings, no neighborhood watch
2. FAIR: Satisfactory location, reasonably safe if one takes proper precautions
3. GOOD: Secure area, responsible neighbors have organized a block watch that is somewhat effective
4. EXCELLENT: Terrific neighborhood is very safe; neighbors are alert and know one another, thus almost eliminating crime.

Set B
1. POOR: Questionable area, recurrent muggings, no block watch
2. FAIR: Reasonable neighborhood, fairly safe if one takes proper precautions
3. GOOD: Safe neighborhood, responsible neighbors have established a neighborhood watch that is partially effective
4. EXCELLENT: Great location is really safe; neighbors are watchful and know one another, thus nearly obliterating crime.

2 presents the levels of each attribute for each target apartment. Two versions of each of the 12 apartments were formed from the two different wordings of each attribute level. For each subject, one version of each apartment appeared with a low anchor, and the other version appeared with the high anchor. The high and low anchors consisted of the best and worst apartments, respectively. Six versions each of the best and worst anchors were formed using different combinations of the different wordings of each attribute description.

Procedure. The experiment was run using the *Mouselab* computer system. Subjects were assigned to one of three feature prompt conditions: similar, different, or neutral. In addition, the version of each item that appeared with the high and low anchors as well as trial order was counterbalanced across subjects.

TABLE 2

Target Apartments for Experiment 1

	Attribute level		
Apartment	Distance	Appearance	Safety
1	3	2	2
2	2	3	2
3	2	2	3
4	4	2	1
5	1	4	2
6	2	1	4
7	3	3	2
8	3	2	3
9	2	3	3
10	4	3	1
11	1	4	3
12	3	1	4

Note. Each attribute consisted of four descriptions, with 4 being best and 1 being worst.

In the first phase of the experiment, subjects priced 12 apartments (six versions each of the apartments later to be used as high and low anchors) by indicating the most they would pay in rent for each apartment. Information about the three attributes of an apartment was presented in three boxes. Subjects could open a box and read the contents by moving the mouse cursor into the box. To indicate a rent subjects had to move the cursor into a monetary response scale at the bottom of the screen and click the mouse. They could then adjust the cursor along the response scale if they desired; finally they had to click the mouse again to indicate their final answer and leave the response scale.

The anchoring and feature prompt manipulation occurred in the second phase of the experiment. Subjects were first given instructions about how to compare two apartments. All subjects were told that the similar attribute was the attribute on which they thought the two apartments were most similar, the different attribute was the one on which the two apartments were most different, and the "middle" attribute was the one that was neither similar nor different.

For each trial in this part of the experiment, subjects were presented with a screen containing a 2×3 matrix as shown in Fig. 2. Along the left side, the anchor apartment (labeled "A") was described in terms of the three attributes. The target apartment (labeled "B") was described in three boxes on the right side. The position of the attributes was varied across trials, as was the version of the anchor used. Subjects in the similar feature prompt condition were asked, "Apartments A and B are most similar on what attribute?" Subjects in the different condition were asked, "Apartments A and B are most different on what attribute?" Subjects in the neutral condition were asked, "What is the middle attribute for apartments A and B?" Subject in the neutral control condition were given the "middle" prompt, rather than no prompt at all, to make

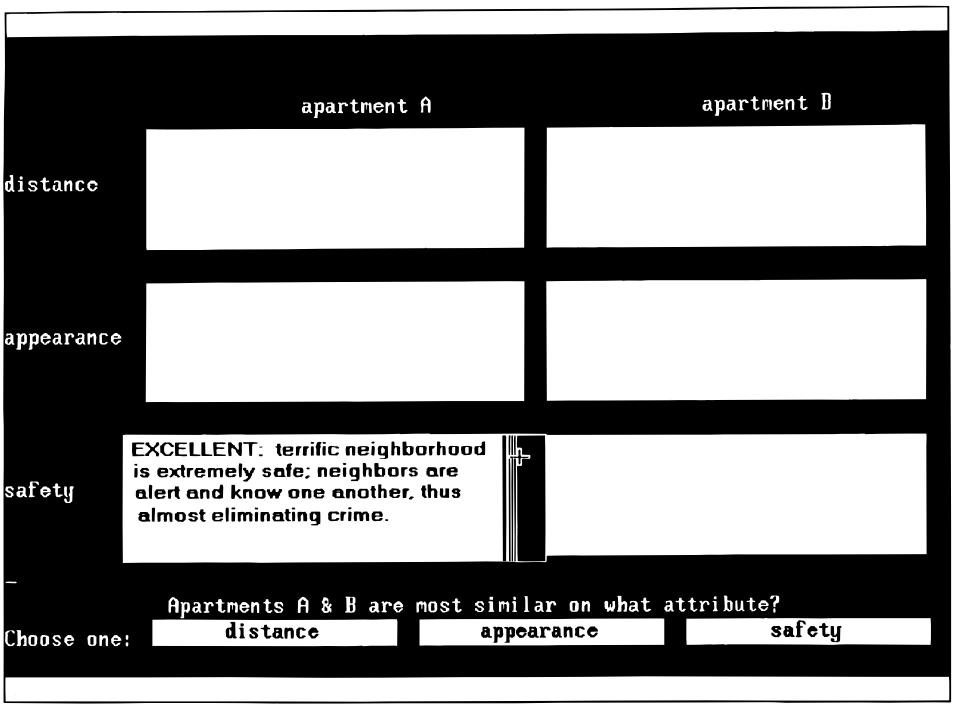

FIG. 2. A sample computer screen from Experiment 1. By moving the cursor into the lower left box, the subject has revealed the safety information for apartment A. The question on the bottom is the prompt for the similar condition.

the control condition similar to the experimental conditions to which it was compared. Subjects indicated a response by selecting one of three choice boxes.

On the next screen, all subjects were presented with a 2×4 matrix showing the same apartments A and B from the previous screen. Apartment A (the anchor) was described in terms of the three attributes and rent (the rent that the subject had indicated for that apartment in the first phase of the experiment). Apartment B was similarly described, but the rent box contained a question mark. Subject were asked, "What is the highest rent you would pay for apartment B?" They indicated their response on a scale. Subjects were instructed that their answers to the feature prompt question should not affect their answers to the rent question.

For the final part of the experiment, subjects rated the attractiveness of the attribute descriptions. For each of six sets (three attributes, two versions of each) subjects rated each of four attribute levels on a 0 to 100 scale. These ratings confirmed that subjects ranked the attribute levels as we intended. That is, the highest level of each attribute was rated significantly higher than the next lowest level, and so on. After finishing the experiment, subjects filled out a short questionnaire and were paid.

Preliminary analyses. Preliminary data analysis showed that prices given in this experiment tended to be positively skewed. This distribution was not due to the use of anchors, as pilot experiments that elicited unanchored preference

judgments showed the same distribution. Instead, the positive skew is likely due to the fact that prices have a lower bound of $0 but no upper bound. A square root transformation ($(x + .17)^{.5}$) was used to normalize the distributions. For ease of interpretation, data are converted from square root of dollars back to dollars for presentation in graphs. An examination of median responses using the nontransformed data demonstrated that the data transformation did not alter the pattern of results.

Results and Discussion

In the first phase of the experiment, subjects assigned prices to six different versions of both the high and low anchors. An ANOVA including the between-subjects factor of trial order and the within-subject factors of anchor (high or low) and anchor version revealed only a main effect of anchor ($F(1, 253) = 1059$, $p < .001$). The high anchor apartments (mean converted back to dollars: $433.52) were priced higher than the low anchor apartments ($126.33).

The second phase of the experiment was of most interest. Here subjects priced the target apartments after comparing them to the anchor apartments. Figure 3 presents the anchoring results. Responses (converted back to dollars) are plotted as a function of anchor for each of the three prompt conditions. It appears that anchoring occurred for the neutral control and similar prompt groups but not for the different prompt group.

This impression was confirmed by an ANOVA containing the between-subjects factors of prompt (similar, different, and neutral), trial order, and counterbalance condition, and the within-subject factors of anchor (high or low), the sum of the target attributes (the sum of the attribute levels shown in Table 2), and the interaction between prompt and anchor. Of primary interest, there

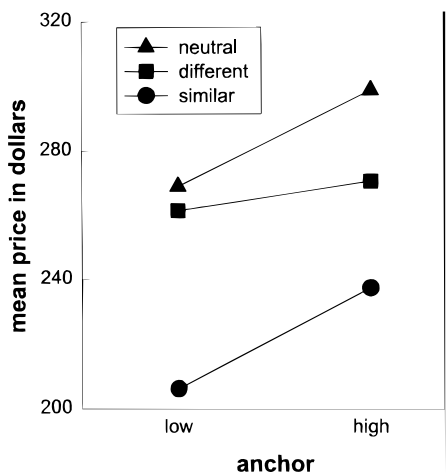

FIG. 3. Preference judgments (in dollars) given in Experiment 1 plotted as a function of anchor condition for each of three feature prompt conditions. Subjects in the similar condition were asked to identify the attribute of the target most similar to the anchor; those in the different condition identified the target attribute most different from the anchor. Subjects in the neutral condition identified the attribute that was neither most similar nor most different.

was a main effect of anchor, $F(1, 548) = 22.00$, $p < .001$, confirming the expected anchoring effect.

The anchoring effect seemed to depend upon the prompt. The feature prompt by anchor interaction was marginal ($F(1, 548) = 2.52$, $p < .09$). To explore this trend, we conducted separate ANOVAs for each prompt condition. The main effect of anchor was reliable in the neutral, $F(1, 182) = 20.02$, $p < .001$, and similar, $F(1, 182) = 7.44$, $p < .01$, groups, but not in the different group, $F(1, 182) = 1.10$, $p > .29$. The estimated slopes of the anchor effect for the neutral, similar, and different groups were 1.19, 1.05, and 0.28, respectively (each given as the change in square root of price from the low to high anchor). Post hoc tests indicated that the slope for the neutral group was not different from the similar group ($Z = .36$, $p > .35$), but was larger than the different group ($Z = 2.34$, $p < .01$, one-tailed). The slope for the similar group was also larger than that of the different group ($Z = 1.65$, $p < .05$, one-tailed).

Thus, the similar prompt did not affect anchoring relative to the neutral prompt. Both of these groups showed an anchoring effect. In contrast, the different prompt significantly reduced anchoring. In fact, although there is some suggestion of anchoring in the different group, the effect was not reliable. It is interesting to note that the similar and different prompts did not have analogous effects. That is, although the different prompt decreased the anchoring effect relative to the neutral condition, the similar prompt did not increase the anchoring effect relative to the control. These results indicate that decision makers normally focus on the features that the anchor and target hold in common; consequently, drawing attention to these features with the similar prompt has no effect. In contrast, drawing attention to the features of the target that differ from the anchor essentially eliminates the anchoring effect, indicating that decision makers do not normally focus on these features.

Process measures. The activation view also makes predictions about how anchors change the search for information. When presented with a high anchor, subjects should spend a long time looking at the most attractive attribute of the target but not much time looking at the least attractive attribute. Similarly, for low anchors, subjects should spend a long time looking at the least attractive attribute of the target but not much time looking at the most attractive attribute. In addition, this pattern should be reduced or reversed by the different prompt manipulation.

In order to test these hypotheses, we identified the best and worst attribute of each target apartment.[2] If two attributes had the same ranking level, both were identified as best. The worst attribute was similarly defined. On the first screen of each trial, subjects were asked to compare the target apartment to the anchor apartment and identify the similar, different, or middle attribute.[3]

[2] Each attribute had four levels, ranked 1 to 4, with 4 being the most attractive. Whichever attribute had the highest ranking level for a particular target, apartment was labeled the best attribute.

[3] We also examined looking times on the second screen, where subjects assigned a price to the target apartment. This second screen did not show differences in looking times, possibly because

They could use the mouse cursor to open any of the three boxes describing the target and the three boxes describing the anchor. One process measure that we examined was the amount of time that the mouse cursor was in one of the target boxes containing the best (or worst) attribute. A second measure was the number of times the cursor entered such a box (the number of acquisitions). A final measure was the number of transitions between a target box and an anchor box on the best (or worst) attribute. If there was more than one best (or worst) attribute, the appropriate measures from the two attributes were averaged. We examined these process measure with an analysis conducted on the means for each trial for each of the three prompt conditions (24 trials for each of three prompt conditions, for a total of 72 observations). This aggregation reduced noise in the data; analysis of the unaggregated data revealed similar results.

The top panel of Table 3 presents the geometric means for looking time as a function of anchor and attentional prompt. The left two columns show the geometric mean time spent looking at the target boxes on the best attributes. The right two columns show looking time for the worst attribute. Mean time is given separately for high and low anchors for the three prompt conditions. This table indicates that subjects in the similar and neutral groups looked at the best attribute more when the anchor was high than when it was low. They looked more at the worst attribute more when the anchor was low than when it was high. In other words, they looked most at the attribute that was similar to the anchor. Subjects in the different group, however, showed the opposite pattern. These subjects looked more at the best attribute when the anchor was low than when it was high. They looked more at the worst attribute when the anchor was high than when it was low. In other words, they looked most at the attribute that was different from the anchor.

This pattern of results was confirmed by the ANOVA, which revealed a three-way interaction between attribute rank (best or worst), anchor (high or low), and prompt condition, $F(2, 65) = 7.53$, $p < .01$. The middle and bottom panels of Table 4 show analogous results for the acquisition and transition data. These measures also showed three-way interactions between attribute rank, anchor, and prompt condition, Fs$(2, 65) > 14$, ps $< .001$.

The same data are presented in a more condensed fashion in Fig. 4. The left hand panel presents a difference score created by calculating the time spent looking at the best attribute minus the time spent looking at the worst attribute.[4] This measure is plotted as a function of anchor and attentional prompt. For subjects in the similar and middle prompt groups, this measure is higher for high anchors and lower for low anchors, indicating that subjects spent more time looking at the attribute that was similar to the anchor. The opposite pattern is indicated for subjects in the different prompt group. The middle and

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subjects had already gathered as much information as they wanted about both apartments on the first screen.

[4] The actual difference used was .483*best–.194*worst. These coefficients were calculated to correlate best with the rent for the target apartment.

TABLE 3

Process Measures from Experiment 1

Geometric mean time

Attribute:	Best		Worst	
Anchor:	Low	High	Low	High
Condition				
Neutral	1.32	1.61	1.48	1.29
Similar	1.42	1.94	1.95	1.50
Different	2.05	1.32	1.39	1.82

Geometric mean number of acquisitions

Attribute:	Best		Worst	
Anchor:	Low	High	Low	High
Condition				
Neutral	1.71	2.14	2.07	1.97
Similar	1.68	2.04	2.21	1.76
Different	2.47	1.92	1.90	2.23

Geometric mean number of transitions

Attribute:	Best		Worst	
Anchor:	Low	High	Low	High
Condition				
Neutral	1.52	1.93	1.92	1.58
Similar	1.34	1.79	2.07	1.20
Different	2.30	1.57	1.61	2.00

Note. Time refers to the total amount of time (in seconds) a target attribute box was open on one trial. Acquisitions refers to the number of times a subject opened a target box on a particular attribute. Transitions refer to the number of times a subject opened a target attribute box directly before or after opening the anchor box on the same attribute.

right hand panels present analogous results for the acquisition and transition data.[5]

Each of these three difference measures was used as the dependent measure in an ANOVA containing the factors of prompt condition, anchor, attribute sum, and the prompt by anchor interaction. These measures each showed an interaction between prompt condition and anchor, Fs(2, 65) $\geq$ 7, ps $<$ .01. These interactions were the result of the fact that for the similar and neutral groups the difference measures were positively related to anchor value, but for the different group the relation was negative. In an additional analysis including only the neutral and similar groups, none of the process difference measures showed a prompt condition by anchor interaction (Fs $\leq$ 1). Thus, the

[5] The difference measure used for acquisitions was 1.212*best–.278*worst; for transitions it was .930*best–.334*worst.

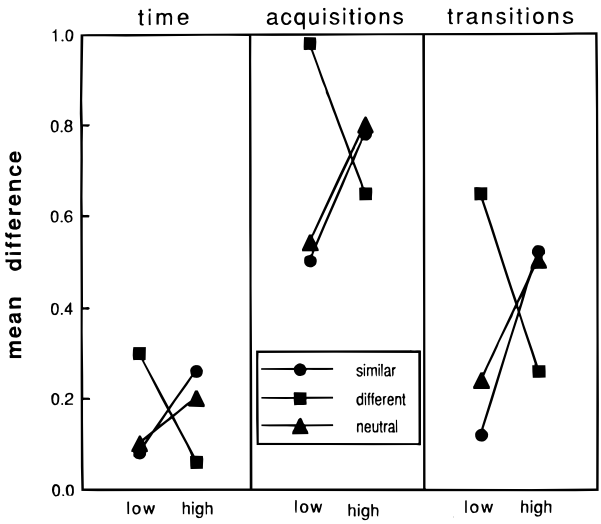

FIG. 4. Process measure differences between best and worst attributes for Experiment 1 plotted as a function of anchor condition for each of three feature prompt conditions. The process measures include geometric mean time spent (in seconds) looking at each target attribute (left panel), geometric mean number of times subjects opened the box containing each attribute (middle panel), and the geometric mean number of transitions between same-attribute target and anchor boxes (right panel).

neutral and similar groups showed generally the same relationship between the process measures and anchor level. In contrast, the different group demonstrated the opposite pattern.

These analyses indicate that the feature prompt instructions affected the pattern of processing, although they did not affect the amount of information processed. The similar prompt did not change the manner in which subjects processed information, relative to the neutral control group. Both of these groups devoted most of their attention to the feature of the target apartment that was most similar to the anchor apartment. In contrast, the different prompt caused subjects to spend more time looking at the feature of the target apartment that was most different from the anchor apartment. Thus, the pattern of looking times closely matches the anchoring pattern. Those groups (neutral and similar) that spent more time looking at similar features also showed an anchoring effect. Subjects in the different prompt group spent more time looking at different features and did not show an anchoring effect.

If the looking time patterns represent the underlying mechanism for the anchoring effect, we would also expect looking time to be directly related to the price that subjects gave for the target apartment. The more time spent looking at the best attribute of the target apartment, the higher the price. The acquisition and transition difference measures were correlated with subjects' rent responses (Pearson $rs = .28$, $p < .02$); the correlation between the time difference measure and rent was in the same direction but did not reach significance ($r = .17$, $p > .14$).

In addition, if looking time mediates the effect of feature prompt on anchoring, we would expect that including looking time as a covariate in an ANCOVA would decrease the effect of anchor on preference judgments. The three difference measures were included in an ANOVA using preference judgments (rent) as the dependent variable and anchor as the independent variable. As with the other process measure analyses, this ANOVA was based on the 72 observations that resulted from item by feature prompt means. Inclusion of the three process measures increased the R^2 of the model (from .12 to .18.) and decreased the variance explained by the effect of anchor. Because of the limited power, this difference was not reliable, $F(1, 67) = 2.04$, $p > .1$, but provided only directional support of mediation.

To summarize our results, we find a pattern of evidence quite consistent with the idea that activation of target features consistent with the anchor produces the anchoring effect. Our experimental task produced anchoring, and our prompt manipulation debiased the effect. Prompting subjects to identify the feature of the target that is most different from the anchor increases attention to different attributes which in turn eliminates the anchoring effect. In contrast, prompting subjects to identify the feature of the target that is most similar to the anchor has no effect on attention to the best and worst attributes and has no effect on the anchoring effect, relative to the neutral control group. In addition, the pattern of looking times across the three prompt groups parallels the anchoring effects for these three groups. This pattern appears clearly for the acquisition and transition measures, and is directional for the time measure. In addition, differences in acquisitions and transitions are themselves related to the prices subjects indicated, and including these covariates in the ANOVA slightly reduced the anchoring effect. Together, these results indicate that anchors encourage consideration of the attributes that the anchor and target hold in common and reveal a method, consistent with the activation account, which can debias the anchoring effect.

EXPERIMENT 2

Experiment 1 used a somewhat unconventional anchoring task: The anchors were multi-attribute apartments rather than simply numbers, and the control group was asked to identify the “middle” attribute, a task not included in other anchoring studies. The feature prompt results found in Experiment 1 might be limited to this type of task. It is also possible that these results are limited to preference judgements. To explore the generality of the results, Experiment 2 tested the attentional prompt prediction using a more conventional anchoring procedure and a different judgment domain. In this study we explored the influence of anchors on belief judgments rather than preference judgments. Like more traditional demonstrations of anchoring, Experiment 2 used conventional numerical anchors, rather than multi-dimensional anchors. Thus, Experiment 2 examined whether the attentional prompt prediction would hold true in a setting where the anchor and target do not have explicit features.

Like Experiment 1, this experiment evaluated the possibility that the presence of an anchor increases consideration of reasons why the target is similar to the anchor, but not reasons why the target is different from the anchor. In order to test this prediction, we asked subjects first to consider a numerical anchor and then to estimate the probability that U.S. troops would be sent to the former Yugoslavia for military action within the next year. (This experiment was conducted during the Spring of 1993, before any U.S. troops were deployed.) We prompted subjects either to consider reasons consistent with the anchor or reasons inconsistent with the anchor. According to the activation view of anchoring, the presence of an anchor causes decision makers to focus on the reasons that the target and anchor are similar. Consequently, prompting subjects to think of similar reasons should have no effect compared to a control no-reason condition. In contrast, prompting subjects to think of different reasons should reduce anchoring.

Methods

Subjects. The subjects were 172 students at the University of Illinois at Chicago who participated for class credit.

Procedure. Subjects were given one of six versions of a paper and pencil questionnaire. These six versions were the result of crossing a reasons factor (similar, different, or control condition) and an anchor factor (high or low anchor condition). The questionnaire asked subjects to consider how likely it was that U.S. troops would be sent to the former Yugoslavia for military action within the next year. Subjects first considered a numerical anchor of either 30 or 70% and indicated whether they thought the actual likelihood was more, less, or the same as the anchor.

Subjects in the similar and different conditions were then asked to write down a reason why U.S. troops would or would not be deployed for military action in Yugoslavia. Subjects in the similar condition were asked to write "pro" reasons if the anchor was high (70%) and "con" reasons if the anchor was low (30%). Subjects in the different condition were asked the opposite. Subjects in the control condition were not asked for reasons. Finally, subjects indicated their best estimate.

Thirteen of the subjects in the similar and different conditions did not write any reasons. Data from these subjects were removed from the analysis.

Results and Discussion

Figure 5 shows the mean likelihood estimates for subjects in each of the six conditions. This figure indicates that likelihood estimates were affected by anchor condition (47.2 for high anchor versus 31.0 for low anchor) and that this effect appears to be stronger for the control and similar conditions.

An ANOVA containing the between-subjects factors of reason and anchor indicated a main effect of anchor ($F(1, 153) = 20.68$, $p < .0001$), but no effect of reason condition ($F < 1$). The reason by anchor interaction was only marginal

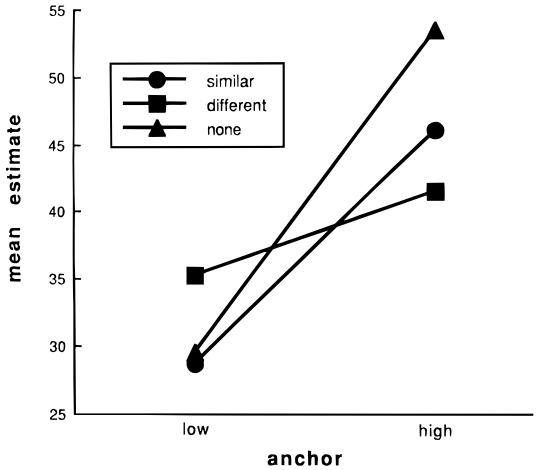

FIG. 5. Estimates given in Experiment 2 plotted as a function of anchor condition for each of three reasons conditions. Subjects in the similar condition wrote reasons consistent with the anchor; those in the different condition wrote reasons counter to the anchor. Those in the "none" condition wrote no reasons.

($F(2, 153) = 2.21$, $p < .12$). However, a contrast testing the predicted interaction pattern was significant. This contrast specified that anchoring was equally strong in the similar and control conditions but weaker in the different condition ($F(1, 153) = 11.31$, $p < .001$). In comparison, a contrast testing the alternative prediction that the size of the anchoring effect for the control condition was midway between that of the similar and different conditions was not significant ($F(1, 153) = 1.63$, $p > .2$).

The effect of anchoring was computed separately for each reason condition. In the control condition, there was a significant anchoring effect (53.5 vs 29.5, $F(1, 153) = 16.81$, $p < .0001$). Anchoring was also reliable in the similar condition (46.0 vs. 28.8, $F(1, 153) = 8.28$, $p < .005$). In contrast, anchoring was not significant in the different condition (41.5 vs 35.3, $F(1, 153) = .99$, $p > .3$). The anchoring effect for the control condition (slope = 23.98, given as the difference in probability judgments for low and high anchors) was larger than that for the different condition (slope = 6.18, $Z = 2.09$, $p < .05$), but not different from the similar condition (slope = 17.29, $Z = .80$, $p > .4$). The anchoring effect for the similar condition was marginally larger than that for the different condition using a one-tailed test ($Z = 1.29$, $p < .1$, one-tailed). Thus, it appears that the prompt for similar reasons did not alter the anchoring effect relative to the control condition. In contrast, the prompt for different reasons significantly reduced the anchoring effect.

Experiment 2 indicates that numerical anchors influence the reasons considered when selecting a numerical response, consistent with the anchoring as activation view. Although not tested in Experiment 2, this view would also predict that higher numerical anchors (e.g., 90% instead of 70%) would lead to the retrieval of more or more extreme reasons why it is likely that the United States will send troops. Similarly, lower anchors (e.g., 10% instead of 30%)

would lead to the retrieval of more reasons why it is unlikely that the United States will send troops.

The reason prompt effect illustrated in Experiment 2 is similar to the feature prompt result shown in Experiment 1. In Experiment 2, however, the anchor and target were numbers (probabilities) with no explicit features. Nevertheless, the anchors influenced the reasons considered when selecting a target judgment, as evidenced by the fact that a prompt to consider different reasons eliminated the anchoring effect. This experiment indicates that the activation notion is applicable to tasks using numerical anchors.

Experiment 2 did not include the process measures used in Experiment 1; thus it was not possible to test whether the feature prompt manipulation influenced attention to the target features. Since the target used in Experiment 2 did not have explicit features, process tracing measures could not be used. In fact, one important purpose of Experiment 2 was to confirm that the feature prompt manipulation had an effect even when the target did not have explicit features. A study by Mussweiler and Strack (1999, Experiment 4) summarized earlier, however, did measure the effect of anchors on processing of the target. When asked to list the features of a target (the length of the river Elbe), subjects given a high anchor listed thoughts that implied a high target value, while those given a low anchor listed thoughts consistent with a low target value. Mussweiler and Strack, however, did not test whether a feature prompt would influence the thoughts listed and thereby impact the anchoring effect.

EXPERIMENT 3

Participants in Experiments 1 and 2 may have viewed the anchors as informative or interpreted them as a hint as to an appropriate answer. They may have reasoned according to conversational norms that the experimenter would not have presented the anchors if they were not informative (Grice, 1975; Schwarz, 1994), and it was therefore reasonable to concentrate on target features or reasons that were similar to the anchor. According to this argument, anchoring may not occur if subjects view the anchors as truly uninformative, or, if anchoring does occur with uninformative anchors, it will not be the result of an activation process.

Although it is possible that such a conversational account could explain the basic phenomenon of anchoring, such an account cannot explain the feature prompt effects found in Experiments 1 and 2. According to such an account, the dissimilar prompt might provide a hint to subjects as to the target's actual value. For example, the con prompt in the high anchor condition in Experiment 2 would provide a hint that the actual target value was lower than the anchor and thus reduce anchoring. This line of argument, however, would also predict that the similar prompt would accentuate anchoring. For example, the pro prompt in the high anchor condition would provide a hint that the actual target value was higher than the anchor. If this prediction held true, we would see stronger anchoring in the similar prompt condition than in the control condition. But such a pattern did not appear in Experiments 1 and 2; instead, the

similar prompt and control conditions looked very similar. Consequently, the feature prompt effect supports an activation view of anchoring rather than a conversational account.

To address further the possibility that anchoring is based on conversation norms or perceived informativeness, Experiment 3 explored whether anchoring would occur with uninformative anchors, and Experiment 4 examined the feature prompt condition using uninformative anchors. The activation account predicts that both the anchoring bias and the feature prompt effect will occur even when the anchors are perceived to be uninformative.

The use of uninformative anchors dates back to Tversky and Kahneman's (1974) classic anchoring demonstration in which subjects were told that the anchors were selected randomly by spinning a wheel of fortune. Anchor selection in that study was not actually random, however, as only two anchor values (10 and 65) were used, and if subjects suspected as much, they may have viewed the anchors as informative. Another example of uninformative anchors is the telephone numbers used by Russo and Shoemaker (1989) as anchors for the question of the date of Attila the Hun's defeat. A phone number could not reasonably be viewed as informative of the correct answer because, whereas different people have different phone numbers, there is only one correct answer to the question. Nevertheless, subjects' answers were correlated with the telephone number anchors. Russo and Shoemaker, however, did not question subjects to confirm that they viewed the anchors an uninformative.

Like Russo and Shoemaker (1989), Experiment 3 used anchors which were not plausibly informative. Subjects indicated their minimum selling price for a lottery after considering an anchor derived from their social security number. In addition, we asked subjects whether they viewed the anchor as informative or relevant and examined whether anchoring varied with perceived informativeness.

Methods

Subjects. Subjects were 50 undergraduates at the University of Illinois at Chicago who participated for course credit.

Procedure. Each subject was asked to fill out a paper and pencil questionnaire. First, they were asked to list the last four digits of their social security number and add 2000 to that number. Then they were asked to place a decimal point before the last two digits and a dollar sign in front, to form a monetary amount. For our subjects, these anchor values ranged from $20.53 to $118.82. The data from three subjects were eliminated because they did not form the anchor correctly.

Subjects were told they had a ticket entitling them to play a lottery that gave a 17% chance to win $287.10 and an 83% chance to win $18.51. They were asked whether they would sell the lottery ticket for the anchor amount and chose one of three options: "I'd accept less than that amount," "I'd sell the lottery for exactly that amount," "I'd demand more than that amount." Then they were asked for the smallest amount for which they would sell the lottery.

Finally, subjects were asked several questions about the perceived informativeness of the anchor.

Results and Discussion

Selling prices were correlated with anchor values ($r = .45$, slope = .77, $n = 47$, $p < .002$), indicating an anchoring effect. Thirty-four percent of the subject said that the anchor provided information about their preference for the lottery and that they thought the experimenters wanted their selling price to be influenced by the anchor. Thus, a surprising number of subjects thought that the anchor was informative despite its construction from their social security numbers. The reason for such a high percentage is not clear, although it is possible that merely asking whether the anchor was informative suggested that it was.

We next examined whether perceived informativeness was responsible for the anchoring effect. We conducted an ANCOVA using selling price as the dependent variable, anchor as a continuous independent variables, and answers to the informativeness questions as a dichotomous independent variable. The interaction between anchor and perceived informativeness approached significance ($F(1, 43) = 3.05$, $p < .09$). To interpret this marginal interaction, we examined the anchoring effect separately for subjects who perceived the anchor to be informative and those who did not. Subjects who answered not informative showed a significant anchoring effect ($r = .54$, slope = 1.07, $n = 31$, $p < .002$), while subjects who answered informative did not ($r = .21$, slope = 0.25, $n = 16$, $p > .4$), a marginally significant difference ($p < .09$).

Thus, it appears that perceived informativeness of the anchor does not increase the anchoring effect; in fact, in this study, perceived informativeness seemed to reduce anchoring. One might speculate that subjects who perceived a demand to incorporate the anchor into their judgments tried to counter the demand effect by giving an answer different from the anchor. That is, subjects may try to correct what they perceive to be a mental contamination effect of the anchor (Wilson & Brekke, 1994). Because direct instruction to avoid the anchoring effect is unsuccessful, however (Wilson et al., 1996), this account seems unlikely. More plausibly, the marginal group difference is spurious and perceived informativeness is in fact unrelated to anchoring.

Strack and Mussweiler (1997) found that irrelevant anchors did not produce an anchoring effect. Their results might appear to contradict those of Experiment 3; however, it is important to distinguish between what Strack and Mussweiler call relevance and what we call perceived informativeness. In Strack and Mussweiler's study, irrelevant anchors were those in a semantic category different from the target judgment (e.g., width vs height of the Brandenburg Gate). In the current Experiment 3, all anchors were in the same semantic category (they all referred to a potential selling price). These anchors were uninformative, however, (and perceived so by most of the subjects) because they were derived from social security numbers. Thus, it appears that switching semantic categories (relevance) affects anchoring while randomly selecting anchors (uninformativeness) does not.

EXPERIMENT 4

Experiment 3 demonstrated that uninformative anchors can produce an anchoring effect, but it did not address whether the uninformative anchoring effect is the result of the anchor's activation of consistent information. It is possible that the activation mechanism operates when decision makers view the anchors as informative, but that an alternate process occurs when the anchors are uninformative. The purpose of Experiment 4 was to test the attentional prompt prediction using truly uninformative anchors. A result similar to that seen in Experiments 1 and 2 would indicate that the activation process does operate with uninformative anchors.

Methods

Subjects. Subjects were 234 undergraduates at the University of Illinois at Chicago who participated for course credit in Spring, 1996, during the presidential primaries.

Procedure. Subjects received a questionnaire and were asked to write down the last two digits of their social security number and treat it as a probability. They were then asked to think about "how likely it is that a Republican candidate will win the 1996 U.S. presidential election" and to compare their answer to the anchor.

Subjects were randomly assigned to one of three reasons conditions. Those in the "pro" condition then wrote down one reason why a Republican would win. Those in the "con" condition wrote down one reason why a Republican would not win. Those in the "neutral" condition were not asked for a reason. All subjects then gave their best estimate of the probability that a Republican would win. Finally, subjects indicated whether they though the anchor provided any information about the likelihood that a Republican would win (perceived informativeness of the anchor) and whether they thought the experimenters wanted the anchor to influence their judgment (perceived demand effect).

Results and Discussion

Anchor values ranged from 0 to 99% with a mean of 48% and a median of 51%. Subjects who were asked for a pro reason and had an anchor above 50% or who gave a con reason and had an anchor below 50% were categorized in the similar prompt condition; these subjects were asked for a reason consistent with the anchor. Those who gave a pro reason and had an anchor below 50% or who gave a con reason and had an anchor above 50% were categorized in the dissimilar prompt condition; they were asked for a reason that contradicted the anchor. Subjects who were not prompted for a reason were in the no prompt condition.

Twenty-eight subjects were removed from the analysis because they did not follow instructions. When prompted for a reason, they did not list one or wrote nonreasons (e.g., "I don't know about politics"), or they used an anchor different

from their social security number. An additional 16 subjects did not give a final estimate; they were also removed from analysis. Thus, 190 subjects (81% of the total) were included in analyses.

Final estimates were used as the dependent measure in an ANOVA. The between-subjects variables were prompt condition (similar, dissimilar, none), anchor value (a continuous variable), perceived informativeness of the anchor, perceived demand effect, and interactions. Anchors were correlated with subjects' responses, $F(1, 176) = 26.75$, $p < .0001$, slope = 0.37, $r = .34$. In addition, the anchoring effect by prompt interaction was significant, $F(2, 176) = 3.14$, $p < .05$. To interpret this interaction, we computed the correlation between anchor and response separately for each of the three prompt conditions. As predicted, the anchoring effect was significant only for the similar ($t(61) = 3.05$, $p < .005$, slope = 0.27, $r = .36$) and neutral conditions ($t(70) = 4.21$, $p < .0001$, slope = 0.31, $r = .45$). Anchoring was not significant for the dissimilar prompt condition ($t(53) = 1.47$, $p > .14$, slope = 0.11, $r = .20$).

Fifty-eight percent of subjects indicated that they thought the experimenter wanted the anchor to influence their judgment. Although this is a rather high rate of perceived demand effect, there was no interaction between anchoring and perceived demand, $F(1, 176) = 2.57$, $p > .11$. Those who perceived a demand effect ($N = 110$, slope = 0.27) did not show more of an anchoring effect than those who did not ($N = 80$, slope = 0.15).

Fifteen percent of subjects thought that the anchor was informative. The ANOVA revealed an interaction between anchor and perceived informativeness, $F(1, 176) = 8.31$, $p < .01$. Anchoring was stronger for subjects who perceived the anchor to be informative ($N = 28$, slope = 0.54) than for those who did not think the anchor was informative ($N = 162$, slope = 0.18). However, anchoring was significant even for subjects who did not think the anchor was informative, $F(1, 160) = 14.10$, $p < .0005$). This effect contrasts with the results of Experiment 3, where anchoring was smaller for subjects who perceived the anchor to be informative. It is possible that the reason prompts used in the current study caused subjects to view the anchor differently. Taken together, these two experiments suggest no overall relation between the anchoring effect and perceived informativeness of the anchor.

Of primary importance, the anchor by prompt interaction was not moderated by either perceived informativeness or perceived demand effect, Fs$(2, 176) < 1.9$, ps $> .15$). That is, the feature prompt effect is not limited to subjects who perceived a demand effect or thought the anchor was informative. The fact that perceived informativeness enhanced anchoring in Experiment 4 (although not in Experiment 3) provides some support for an alternative conversational account of anchoring: that subjects incorporate the anchor into their judgments thinking that the experimenter intended the anchor to be informative (Grice, 1975; Schwarz, 1994). This account cannot provide a complete explanation, however, because both the anchoring effect and the interaction with feature prompt occurred even when the anchor was viewed as uninformative. That is, Experiment 4 indicated that the feature prompt manipulation influences the

anchoring effect even when the anchors bear no relation to the target question and are perceived as uninformative by the subjects.

EXPERIMENT 5

Experiments 1, 2, and 4 supported the activation account of anchoring by demonstrating an attentional prompt effect. Prompting subjects to consider features or reasons that were inconsistent with the anchor reduced the anchoring effect, while prompts to consider features or reasons consistent with the anchor had no influence on the anchoring effect. These results indicate that decision makers naturally consider consistent features but do not consider inconsistent features unless expressly instructed to do so. Although Experiments 1, 2, and 4 all support the activation view of anchoring, they test only this one prediction (in a number of different tasks.)

Experiment 5 provided a somewhat different test of the activation account. We propose that anchoring occurs because of differential facilitation of target features or reasons. This facilitation cannot operate unless features or reasons exist. In other words, the larger the pool of features or reasons associated with a particular target, the more opportunity there is for the anchor to differentially facilitate the use of consistent features. Thus, anchors should have more of an effect if there is a larger pool of reasons or features available for priming.

In this experiment, we attempted to manipulate the size of the pool of reasons available for anchor facilitation. This manipulation involved asking subjects to elaborate on the target prior to the anchoring manipulation. Thus, one group of subjects elaborated on the topic of crime by listing things they did that made them more or less vulnerable to crime, while another group of subjects elaborated on an unrelated topic. Both groups them answered a crime question after first considering an anchor. Because the elaboration group has more crime-related reasons available in memory, the anchor should have a better chance of facilitating those reasons that are consistent with the anchor. Thus, we expect a larger anchoring effect for subjects who have elaborated on the target.

Methods

Subjects. Subjects were the same 234 undergraduates who participated in Experiment 4.

Procedure. Subjects filled out a three-part questionnaire. Part 1 contained the elaboration manipulation in which all subjects were asked to list some actions. Half the subjects were asked to list things that they do that improve or threaten their health. The remaining subjects were asked to list things they do that make them vulnerable or help them avoid being a victim of property crimes. Thus, some subjects elaborated about health, while others elaborated about crime. Different versions of the questionnaire counterbalanced whether subjects listed the positive actions (things that improve health or avoid crime)

before or after the negative actions (things that threaten health or make one vulnerable to crime). Subjects were asked to list 5 to 10 positive actions and 5 to 10 negative actions.

Part 2 contained some unrelated filler questions used to separate Parts 1 and 3. Part 3 contained the anchoring manipulation. Subjects answered two questions: (1) "How many people in the USA do you think will die of heart disease or cancer in the next 10 years?" (2) "How many people in Chicago do you think will be a victim of a property crime in the next year?" The order of these two questions was counterbalanced. Each subject saw one of three versions of Part 3. Subjects in the no-anchor version ($n = 39$) answered these questions without considering any anchor. Subjects in one anchoring condition ($n = 96$) were presented with a high anchor for the health question (100 million people) and a low anchor for the crime question (200 people). Subjects in the other anchor condition ($n = 99$) were presented with a low anchor for the health question (1000 people) and a high anchor for the crime question (500,000 people). Subjects in the anchoring conditions were presented with the question and an anchor and asked whether their answer to the question was higher, lower, or the same as the anchor. They then provided their best estimate of the answer and continued on to the next question.

Subjects were randomly assigned to one of six conditions that resulted from crossing the two elaboration conditions (elaborate about health or crime) with the three anchoring conditions (no anchors, high health and low crime anchors, or low health and high crime anchors.) We predicted a larger anchoring effect on the health question for those subjects who had elaborated about health. Similarly, the anchoring effect for the crime question should be enhanced for subjects who elaborated about crime.

Results and Discussion

Seventeen subjects did not provide answers to both questions in Part 3, leaving 217 subjects in the analysis. Figure 6 presents the results for the anchoring portion of the experiment (Part 3). The left panel shows the mean responses (after a *ln* transform) for the crime question, and the right panel shows the health question. Results from subjects in the low anchor, no anchor, and high anchor conditions are shown for each of the two elaboration conditions. We predicted that for the crime question, the anchoring effect would be stronger when subjects had elaborated on crime. Likewise, for the health question, the anchoring effect would be stronger when subjects had elaborated on health. Consistent with that prediction, on the left panel the anchoring function appears slightly steeper for subjects who elaborated about crime, while on the right panel the function is steeper for those who elaborated about health.

Statistics were conducted using only the 187 subjects in the anchoring conditions. Data from the no anchor condition are presented in Fig. 6 only to illustrate that responses in this condition fell between responses from the low and high anchor conditions. To capture the within-subject design of this experiment, we conducted an ANOVA involving both questions. The final estimate answers to

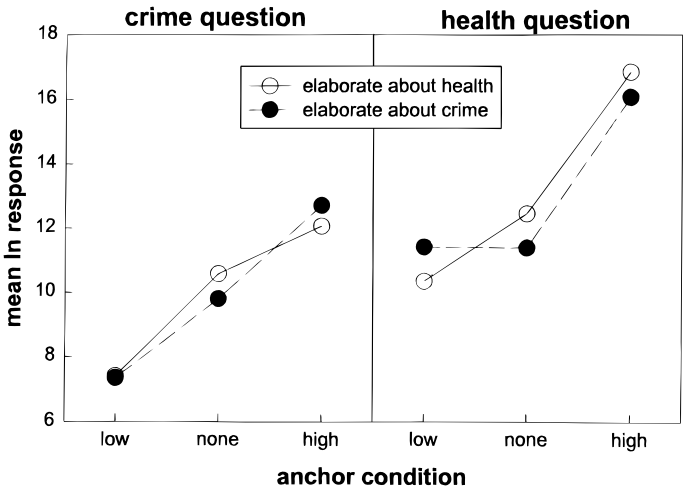

FIG. 6. Mean ln responses given to the crime and health questions in Experiment 5. Results are shown for the three anchor conditions and the two elaboration conditions.

the two questions were used as the dependent variable (after a *ln* transform). The independent variables were anchor (within-subject), question (within-subject), and elaboration condition (between-subjects). The interaction between anchor and question was between-subjects (even though each factor was within-subjects) because subjects who saw the low anchor with the crime question also saw the high anchor with the health question, and vice versa. We computed an a priori contrast on the three-way interaction among anchor, question, and elaboration condition. The contrast specified that the anchoring effect for the crime question was larger for the crime elaboration condition, but that the anchoring effect for the health question was larger for the health elaboration condition. In other words, the difference between the anchoring effects for the crime and health elaboration conditions on the crime question was algebraically larger than the difference between the anchoring effects for the crime and health elaboration conditions on the health question. This contrast was significant, $F(1, 183) = 5.58$, $p < .02$, consistent with our prediction.[6]

We also conducted two individual ANOVAs for the crime and health questions separately. The between-subjects independent variables were anchor condition (high or low), elaboration condition (elaborate on crime or health), and a counterbalance condition indicating the order in which the positive and negative

[6] This contrast is identical to the two-way interaction between two between-subjects factors: elaboration condition (crime or health) and the counterbalance condition that indicated whether a subject was in the group with a high crime anchor and a low health anchor or vice versa. This interaction indicates that subjects in the crime-elaboration condition who had a high crime and low health anchors gave high answers to both questions—a high crime answer because the crime-elaboration helps the high crime anchor to pull the estimate up, and a high health answer because the lack of health elaboration inhibits the low health anchor from pulling the estimate down. Subjects in the crime-elaboration condition who had a low crime and low health anchor gave low answers to both questions for an analogous reason. Those in the health-elaboration condition showed the opposite pattern.

actions were listed in Part 1. Our prediction should result in an anchor by elaboration interaction for each of the questions. For the health question, the anchoring effect was significant, $F(1, 179) = 148$, $p < .0001$, and the main effect of elaboration condition was not, $F < 1$. Of most interest, the interaction between anchor and elaboration condition was significant, $F(1, 179) = 4.29$, $p < .05$. For the crime question, there was a main effect of anchor, $F(1, 179) = 285$, $p < .0001$, and no main effect of elaboration condition, $F(1, 179) = 1.07$. $p > .3$. The interaction was not significant, $F(1, 179) = 1.52$, $p > .2$ but the trend was in the predicted direction. Thus, the elaboration manipulation produced a larger effect on anchoring for the health question than for the crime question.

The anchoring effect was stronger when more target features were available because of the elaboration manipulation. According to the activation proposal, the anchor increases the salience and use of target features or reasons that are consistent with the anchor. This process can operate only if there are target features present in memory (or the environment) that can be made more or less salient. The elaboration manipulation used in this study served to make a larger pool of reasons available in memory. The anchor then facilitated the use of the subset of that pool that was consistent with the anchor. This result is interesting, since it suggests than anchoring effects might interact with available information about a domain. That is, those who have more information or reasons at their disposal, but who must construct (and not retrieve) an answer, may be more prone to the effects of irrelevant anchors, because they have a larger set of reasons which can be affected by biased search.

This result might appear to conflict with earlier findings showing that anchoring is weaker for those who have more knowledge in a judgment domain. Wilson et al. (1996), for example, found stronger anchoring in participants who had little knowledge in a judgment domain. Similarly, Jacowitz and Kahneman (1995) showed that anchoring is stronger for subjects who are most uncertain. Northcraft and Neale (1987) did not find that experts showed less anchoring, but at least found no increase in anchoring with expertise. In contrast, in the current study the subjects who elaborated on a judgment domain (and thus had more information accessible) show a stronger anchoring effect.

To resolve this apparent discrepancy, it is important to emphasize the distinction between elaboration (the manipulation used in the current study) and knowing more or being more certain about a judgment domain. We propose that elaboration causes one to have more (potentially contradictory) retrievable evidence that can bear on the task. It increases the amount and distribution of accessible knowledge pertinent to the judgment task without increasing the resolution or accuracy of that knowledge. An analogy can be drawn between the current elaboration study and a study by Shafir (1993) in which "enriched" options (those that have a broad distribution of features) are more susceptible to the biasing effects of different question prompts. Shafir found that when asked to accept one of two options, decision makers focus on the positive features of the options. In contrast, when asked to reject one of two options, decision makers focus on the negative features of the options. Consequently, an enriched

option with many positive and many negative features can be both accepted and rejected over a second option with only average features. These results are similar to those of Experiment 5 because in both cases, the activation process has a stronger effect when a larger pool of features is available.

GENERAL DISCUSSION

The five experiments present several facts about the anchoring bias. First, Experiments 1, 2, and 4 demonstrated that a prompt to consider reasons or features different from the anchor reduced or eliminated the anchoring effect. In contrast, prompts to consider similar reasons or features had no effect relative to a control condition. Second, process measures collected in Experiment 1 showed that subjects spent more time considering target features that were similar to the anchor except when they were explicitly instructed to identify different features. These process measures pointed to strong similarities between the similar and control conditions. Third, Experiments 3 and 4 indicated that anchoring occurs even when the anchor is obviously random and irrelevant, suggesting that activation of similar features does not occur merely because subjects think the anchor is informative. Furthermore, the feature prompt manipulation is effective even with random anchors. Finally, Experiment 5 indicated that the anchor is more effective in facilitating consideration of similar features if there is a larger pool of features from which to draw (because the entire set of features has been made more available through elaboration). This set of results supports the view of anchoring as activation, which argues that anchoring occurs because the presence of the anchor promotes consideration of target features or reasons that are consistent with the anchor. Although several other investigators have proposed an activation account of anchoring (Jacowitz & Kahneman, 1995; Kahneman & Knetsch, 1993; Mussweiler & Strack, 1999, in press; Strack & Mussweiler, 1997), this account has not previously been tested with feature prompt or elaboration manipulations.

The present experiments demonstrate a method to reduce or eliminate the anchoring bias. Prompting subjects to identify target features different from the anchor (Experiment 1) or to consider reasons at odds with anchor (Experiment 2 and 4) greatly reduced the anchoring bias. This debiasing result is important because previous efforts to debias anchoring have often met with little success. Wilson et al. (1996, Experiment 5), for example, found no effect of giving subjects instructions to avoid the anchoring bias. Even when subjects were told the direction in which the anchor would affect their responses, they were unable to avoid the bias.

One previous study was successful in reducing the anchoring bias. Quattrone, Lawrence, Finkel, and Andrus (1981) provided some subjects with a hint not to let the anchor influence their judgments. Subjects in the hint condition received a practice question with a known answer: “Does the month of January have more or less than 83 days?” Subjects in the no hint condition were told that they would be answering questions like “Does Saturn have more or less

than X moons?" Both groups of subjects were then given the same set of eight questions with anchors (potential answers) and asked to estimate the answers. Subjects in the hint condition subsequently demonstrated a somewhat smaller anchoring effect for extreme anchor values.

Activation in Anchoring and Other Phenomena

Our characterization of activation as a significant contributor to anchoring suggests that anchoring may be related to other judgment biases. Moreover, activation processes may play a unifying role as an explanation for a number of other judgment phenomena. Indeed, mechanisms similar to the one we describe here have been demonstrated in numerous judgment tasks.

One primary example is overconfidence. As mentioned in the Introduction, Koriat et al. (1980) showed that overconfidence is due to a failure to consider why the selected answer might be wrong and demonstrated that a prompt to list counter-reasons, similar to our attentional prompts, was effective in debiasing overconfidence. Using a similar manipulation, Koehler (1994) found that subjects who generated a hypothesis were less overconfident than those who merely evaluated the hypothesis, presumably because generation involves considering alternative hypotheses. This finding suggests that self-generated anchors will lead to less bias than experimenter-generated anchors. Block and Harper (1991) found just this result. Subjects gave more accurate confidence intervals if they generated their own anchor (a point estimate) than if they were given another subject's point estimate.

Another related phenomena may be the hindsight bias, the tendency for decision makers with outcome knowledge to believe falsely that they would have predicted the outcome (Fischhoff, 1975). Anchoring has been suggested as a possible explanation of this bias (Hawkins & Hastie, 1990); specifically, knowledge of the outcome acts as an anchor to influence judgments of the predictability of the outcome. An activation account of the hindsight bias has also been suggested (Hawkins & Hastie, 1990). In hindsight bias experiments, evidence consistent with the outcome is more easily recalled than facts that contradicts the outcome (Dellarosa & Bourne, 1984). Thus, the outcome knowledge draws attention to reasons why that outcome was predictable, but not reasons why alternative outcomes were predictable. Much like our attentional prompt manipulation, hindsight bias is reduced by asking subjects how they would explain alternate outcomes if they had occurred (Arkes, Faust, Guilmette, & Hart, 1988; Slovic & Fischhoff, 1977). These findings suggest that hindsight and anchoring share an activation mechanism.

People often maintain beliefs that are based upon information that is eventually shown to be false. For example, Anderson, Lepper, and Ross (1980) presented evidence that risk taking (or caution) is associated with success as a fire fighter. The evidence was then discredited by telling subjects that it was fabricated; however, the initial manipulation continued to influence beliefs. An activation mechanism would suggest that the original information caused subjects to think of consistent evidence (e.g., reasons why risk taking would

lead to successful fire fighting) but did not prompt them to think of evidence inconsistent with the original information. When the original information was discredited, the additional evidence retrieved from memory continued to influence subjects' beliefs. This belief persistence bias could be reduced by asking subjects to consider whether they could argue for the other side (Anderson, 1982) or to "consider the opposite" (Lord, Lepper, & Preston, 1984). Similarly, presenting subjects with an alternative hypothesis (Lord et al., 1984) reduced belief perseverance. As mentioned earlier, Gilbert (1990, 1991; Gilbert et al., 1993) identified a similar phenomenon. He found that comprehension includes an initial belief in the assertion presented, followed only later by rejection of false information. Thus, the default evaluation is confirmation of the information; rejection occurs only with additional effort. Other research (e.g., Arkes, Hackett, & Boehm, 1989) has shown that merely repeating a statement increases its perceived validity. In addition, Tesser (1978) found that thinking about a topic recruits thoughts consistent with it. All of these lines of research suggest that accepting an assertion is the default evaluation.

As discussed in the Introduction, a number of results termed 'confirmation bias' (Klayman, 1995) are consistent with the activation view of anchoring. A related bias, pseudodiagnosticity (Doherty, Mynatt, Tweney, & Schiavo, 1979), is the tendency for decision makers to evaluate a hypothesis by considering the likelihood of the evidence given that hypothesis, but neglecting the likelihood of the evidence given an alternative hypothesis. Thus, the hypothesis under consideration acts like an anchor, cuing decisions makers to consider why the evidence is consistent with the hypothesis, but not why the evidence is consistent with another hypothesis.

As reviewed above, Shafir (1993) presents another judgment phenomenon that may be related to anchoring. Decision makers focus on the positive features when asked to accept one of two options, but focus on the negative features when asked to reject one of two options. Consequently, an option with many positive and many negative features can be both accepted and rejected over a second option with only average features. These results are consistent with the interpretation that the "accept" or "reject" instruction acts as an anchor by increasing the availability of features consistent with the instruction.

The numerous phenomena consistent with a type of activation mechanism suggest that it may be a common mechanism underlying a plethora of judgment biases. Baron (1994) described the tendency to search for evidence that favors a possibility that is already strong as one of the major biases leading to poor decisions, while Arkes (1991) described association-based errors as one of three main causes of judgment biases (along with strategy-based errors and psychophysically based errors.) Association-based errors result from considering evidence that is primed by the decision task. Arkes concludes that such errors cannot be corrected by increasing incentives but can be reduced by instructions or cues to perform a debiasing behavior, such as considering opposing evidence. Our results place anchoring in this class of phenomena and support Arkes' assertions about debiasing techniques.

One final area of research related to the activation view of anchoring is the

social judgment work on assimilation and contrast (Sherif & Hovland, 1961; Sherif, Sherif, & Neberfall, 1965). An assimilation effect refers to a positive relation between a piece of information (like an anchor) and a judgment; a contrast effect refers to a negative relation. Thus, the anchoring effects presented here could be viewed as assimilation effects. Interestingly, one finding from the assimilation literature is somewhat analogous to the results presented here. Schwarz and Bless (1992) found assimilation effects when the question to subjects encouraged inclusion of the information in the category to be judged and contrast effects when the question encouraged exclusion, a result somewhat analogous to the feature prompt results reported here.

There are important differences between the two areas of inquiry as well. One important difference is that our experiments did not reveal any contrast effects; "exclusion" prompts merely resulted in a smaller anchoring effect, not a reverse anchoring effect. Contrast effects are quite rare in decision making research on anchoring, although Stack and Mussweiler (1997) found reverse anchoring when the anchor and target were in opposite semantic categories. Second, the term "anchor" is used very differently in the two fields. In decision-making research an anchor is a piece of irrelevant information that results in an assimilation effect. Conversely, in social judgment research, a piece of information that is excluded from the category to be judged may act as a standard of comparison or scale anchor, resulting in a contrast effect.

Response Mode Effects on Preference Judgments

Anchoring and adjustment has been used as an explanation for a variety of other judgment phenomena, most notably preference reversals (Lichtenstein & Slovic, 1971; Schkade & Johnson, 1989; Slovic & Lichtenstein, 1983). For example, Busemeyer and Goldstein's (1992) dynamic model of matching formalizes the anchoring and adjustment explanation of preference reversals. As noted in the Introduction, many of these accounts have used anchoring as an account of other phenomena with little attention to the causes of anchoring itself. Could the activation mechanism of anchoring, described here, account for other phenomena that anchoring is purported to underlie? Specifically, can it explain preference reversals and related phenomena?

One possible mechanism proposed to explain preference reversals is compatibility. According to this account (Tversky, Sattath, & Slovic, 1988), the weight given to each attribute of a target item depends on the response mode. Specifically, those attributes that are compatible with the response scale are given more weight. Thus, in pricing lotteries, the dollar outcomes of the lotteries receive relatively more weight. Conversely, in rating lotteries on a 0 to 100 scale (Schkade & Johnson, 1989), the probabilities receive relatively more weight. This explanation of preference reversals is analogous to an activation mechanism. Specifically, the decision maker attends to the features of the target that are similar to some other salient characteristic of the task (the anchor or the response scale). Thus, the response scale may act as an anchor, increasing the weight of target attributes similar to the response scale. The

activation account of anchoring may be the mechanism underlying compatibility effects; thus, preference reversals and the anchoring bias may be related via this common mechanism.

The activation account goes beyond the compatibility explanation of preference reversals by suggesting a method for reducing preference reversals. If preference reversals result from activation of features similar to the response scale then they should be affected by the sort of debiasing manipulation used in Experiments 1, 2, and 4. Prompting subjects to attend to the probability of winning when pricing a gamble, or to attend to the payoff when rating a gamble, should decrease the influence of the response scale and consequently reduce reversals. In contrast, prompts to attend to the payoff when pricing a gamble, or to attend to the probability when rating a gamble, should have no effect on preference reversals. Schkade and Johnson (1989) demonstrated that appropriate use of explicit anchors also acts as a debiasing manipulation and markedly decreases the frequency of preference reversals. Thus, the activation view of anchoring provides an account of preference reversals, is similar to the compatibility explanation previously offered, and suggests a way to debias preference reversals.

As mentioned in the Introduction, anchoring has been offered as a explanation for a number of additional phenomena, including biases in utility assessment (Hershey & Schoemaker, 1985; Johnson & Schkade, 1989), information framing effects (Levin, Schnittjer, & Thee, 1988), causal attribution (Quattrone, 1982), and detection of deception (Zuckerman, Koestner, Colella, & Alton, 1984). These phenomena are also consistent with the activation view of anchoring.

Mechanisms of Anchoring

Recently, a number of authors have proposed an activation type of mechanism for anchoring (Chapman & Johnson, 1994; Jacowitz & Kahneman, 1995; Kahneman & Knetsch, 1993; Mussweiler & Strack, 1999, in press; Strack & Mussweiler, 1997). This class of explanations for judgment biases posits that decision makers focus on evidence that is consistent with the answer under consideration and neglect disconfirming evidence. An activation mechanism may not be the only mechanism involved in anchoring, however. Jacowitz and Kahneman (1995) point to three types of mechanisms that have been proposed for anchoring (see also Mussweiler & Strack, in press). As discussed earlier, they suggest that the anchor acts as a suggestion or prime; that is, the anchor is treated as a candidate answer in an automatic process. In addition to this activation account, a second possible mechanism of anchoring is the proposal that anchors provide a conversational hint. The mere fact that the anchor is mentioned may make it appear plausible. This account was addressed in Experiments 3 and 4 which found anchoring even when the anchors were obviously uninformative. Thus, anchors do not have their effect merely because they appear plausible. A third account is that an anchor is a starting point for adjustment. That is, decision makers make adjustments along some scale from the anchor to the final answer, but adjustment is insufficient. Jacowitz and Kahneman (1995),

however, found that anchoring can occur without an adjustment process. In their study, subjects' comparisons of the anchor to the target (without giving a final estimate of the target) demonstrated an anchoring effect.

Although the activation account appears to have the most support, it is possible that a conversational hint or starting point process is sometimes involved in anchoring. For example, Busemeyer and Goldstein (1992) formalized a dynamic model of anchoring that involves adjustment. In addition, the activation view of anchoring does not answer all questions about the anchoring bias. For example, it does not address the issue of when anchors are attended to or the conditions under which an initial comparison is made between the anchor and the target. These issues have been explored by Wilson et al. (1996). An important topic for future research is the relative contributions to the anchoring bias of different psychological processes.

Conclusions

One implication of the relation between the activation account of anchoring and a number of other judgment phenomena is that anchoring and anchoring-related biases are extremely pervasive. Anchors can be self-generated by decision makers (Johnson & Schkade, 1989) or may be presented by the nature of the decision task. For example, this year's budget figures and forecasts may be based on last year's figures. Although, unlike our experimental anchors, these realistic anchors are not random or uninformative, our results suggest that they are nonetheless overly influential. Anchors can also be explicitly manipulated, for example, in negotiation. A real estate agent might set a listing price that is extraordinarily high in an attempt to influence prospective buyers (Northcraft & Neale, 1987) or quite low in an attempt to receive a low tax appraisal. An injured plaintiff may ask for a huge compensatory award in a negligence case in order to influence the jury (Chapman & Bornstein, 1996). A late-night television commercial may ask viewers to compare a fabulous Ginsu knife set to some large dollar amount, say $100.

Our analysis also has implications for debiasing anchoring. The results of Experiments 3 and 4 imply that emphasizing the uninformative nature of anchors will not eliminate the bias. In contrast, Experiments 1, 2, and 4 do suggest a way to avoid anchoring biases. Our late night television viewer need only ask himself or herself what the Ginsu knife set has in common with a $10 paring knife.

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