Flexible Correction Processes in Social Judgment: Correcting for Context-Induced Contrast

RICHARD E. PETTY AND DUANE T. WEGENER

The Ohio State University

Received June 1, 1992

Previous studies of correction processes in social judgment have suggested that contextual contrast is a more cognitively effortful process than contextual assimilation because contrast results from efforts to correct for assimilation biases. These studies appear to assume that assimilation is the natural or default effect of a context and that instigation of correction processes leads to displacement of judgments away from the context. In Experiment 1, we showed that people believe that either contrast or assimilation can be the natural effect of a context depending upon the specific context and target items paired. In Experiment 2, we employed a context that subjects believed would naturally produce a contrast effect, and as expected, contrast was produced in the “no-correction” condition. When an explicit instruction to correct for the context was made, subjects adjusted their judgments toward rather than away from the contextual items. Experiment 3 showed that corrections observed with explicit instructions were not due to changes in response language. Finally, in Experiment 4, more subtle correction cues produced the same correction effects. We argue that it is premature to conclude that movement away from a context (i.e., judgmental contrast) is invariably the result of context correction processes. Rather, people are more flexible in their corrections. In situations where contrast is the expected effect of a context, correction processes result in movement toward rather than away from the context. © 1993 Academic Press, Inc.

Sometimes people attempt to correct their initial perceptions or judgments in light of potentially biasing factors. That is, when assessing the qualities of a target person or object, people may realize that there is some factor in the judgmental situation that is capable of having influenced

This research was supported by NSF Grant BNS 9021647 to the first author and an NIMH traineeship (T32 MH19728) to the second author. We are grateful for feedback provided by the 1991–1992 Ohio State Group for Attitudes and Persuasion, and for the insightful comments by Charles Judd, Leonard Martin, Thomas Ostrom, Norbert Schwarz, and Steven J. Sherman. Correspondence should be addressed to Richard E. Petty, Department of Psychology, Ohio State University, 1885 Neil Avenue Mall, Columbus, OH 43210. Electronic mail address: Petty.1 @ osu.edu.
their perceptions. If this influence is viewed as inappropriate or biasing, judges may attempt to take this factor into account and correct for it in making their assessments of the target. Consider, for example, a person who arrives home after a long day at work and is greeted by a spouse who provides a reminder about a domestic task that was uncompleted. The person may initially become angry at the spouse and view the complaint as mean-spirited. What if something reminds our wage-earner that frustration about work is at least partially responsible for the current state of anger, however? Because the bad mood engendered by the frustration with work should be irrelevant to perceptions of requests from one's spouse, the reaction to the spouse may be tempered or corrected for the perceived biasing factor. That is, the spouse may be seen as less mean-spirited than if no correction for the bad work day was made.

Recently, correction processes have received considerable attention in the area of context effects in social judgment. The context in which a target stimulus is embedded can often have significant effects on people's assessments of that target. For example, the presence of a positive context might make a target seem more positive than would normally be the case—an assimilation effect. At other times, however, the presence of a positive context might make the target seem less positive than would normally be the case—a contrast effect. One factor that is frequently considered in explanations of assimilation and contrast effects is the distribution of contextual stimuli (e.g., Helson, 1964; Parducci, 1965). For example, in ratings of ambiguous targets, a very extreme context is likely to lead to contrast, whereas a moderate context is more likely to lead to assimilation (Herr, Sherman, & Fazio, 1983; Sherif & Hovland, 1961).

Interestingly, recent investigations have found evidence of assimilation and contrast effects even when the distribution of contextual stimuli has been held constant (e.g., Martin, 1986; Martin, Seta, & Crelia, 1990; Newman & Uleman, 1990). For instance, Martin (1986, Experiment 1) had subjects categorize a series of phrases in terms of two traits. For some subjects, various phrases (e.g., “volunteered for espionage duty while in the Navy”) were judged in terms of two positive traits—adventurous and self-confident. For other subjects, different phrases (e.g., “smokes cigarettes near an open can of gasoline”) were judged in terms of two negative traits—reckless and egotistical. Although all subjects were stopped after categorizing eight phrases, some of the subjects were led to believe that they would have to categorize twelve phrases rather than eight. Thus, subjects who had been asked to categorize eight phrases believed that they were finished with the priming task, whereas subjects who had been asked to categorize twelve believed that they were not finished. Subjects were then asked to form an impression of a target person described in terms that were ambiguous regarding the traits adventurous and reckless. Subjects showed relative assimilation of their
impressions of the target to the primed concepts when they believed that the priming task had been interrupted (i.e., they rated the target more positively after the positive than the negative priming task), but they showed relative contrast when they believed that the priming task had been finished (i.e., they rated the target more positively after the negative than the positive priming task).

Martin (1986; Martin et al., 1990) has explained such results in terms of his set/reset model. According to this model, one’s representation of the target may include some positive and some negative elements. When a context primes a set of thoughts (elements), some of these primed reactions might overlap with the representation of the target, making the target seem more like the context (referred to as setting). When people realize that they are thinking particular thoughts (positive or negative) because they were primed by the contextual stimuli, however, they may avoid using these thoughts in forming their impression of the target in an attempt to be accurate. In doing so, people attempt to “partial out” (or subtract) the primed reactions (referred to as resetting). When this happens, people may subtract out some of the elements of their true reaction to the target. Because of this, contrast away from their contextual reactions can result. Although contrast is not the inevitable outcome of resetting, the correction is always in a direction away from the primed reaction to the contextual stimuli. For instance, whereas setting leads to assimilation to the primed reaction, resetting can lead to reduced assimilation, neither assimilation nor contrast, or to a contrast effect depending on the extent of correction (see Martin & Achee, 1992). Importantly, the corrections that lead to these various outcomes are all in the same direction—away from assimilation (i.e., away from the person’s reaction to the contextual stimuli).

Support for the set/reset model comes from a number of studies that have varied contextual stimuli along with the likelihood of resetting. For instance, Martin (1986, Experiment 3) found assimilation in task-interrupted conditions and found contrast in task-completed conditions when the priming task was the writing of self-referent statements indicative of positive or negative moods. People have been shown to be more likely to continue thinking about tasks they have not completed than about tasks they have completed (Marrow, 1938; Martin & Tesser, 1989; Zeigarnik, 1927/1938). Because of this, Martin (1986; Martin et al., 1990) reasoned that subjects in the conditions where the contextual task was interrupted would be more likely to perseverate about the primed information than those in the task-completed conditions. As a result, subjects in the task-interrupted conditions would find it difficult to avoid the primed thoughts in forming their impressions. Subjects in the task-completed conditions, on the other hand, would be able to “partial out” the primed thoughts from the now completed task.
Because resetting (correcting) constitutes an additional step beyond setting (i.e., resetting involves subtraction of elements primed by the context), Martin et al. (1990) reasoned that contrast effects would require more cognitive effort than assimilation effects. Using the same self-referent statement-priming task as Martin (1986; Experiment 3), in three separate studies Martin et al. (1990) found contrast when subjects were motivated and able to form thoughtful impressions, but assimilation when motivation or ability was low. That is, when subjects were unable, due to distraction (Petty, Wells, & Brock, 1976), or unmotivated, due to group responsibility (Petty, Harkins, & Williams, 1979) or low need for cognition (Cacioppo & Petty, 1982), to put forth effort in forming impressions of the target, assimilation resulted. When subjects were motivated and able to put forth cognitive effort, however, and presumably engage in correction processes, contrast resulted. Thus, contrast was found when subjects were not distracted, when their responses were individually identifiable, and when they were high in need for cognition.

Recently, Schwarz and Bless (1992a) have proposed an inclusion/exclusion model of assimilation and contrast effects that extends Martin’s set/reset analysis. The basic tenets of this model can be summarized in a few brief statements: (1) Contextual information that is clearly irrelevant to the judgment task is ignored and does not influence judgment (i.e., an “early exit” from the model occurs in which case no assimilation or correction for assimilation takes place). (2) The default is to include potentially relevant contextual information in the representation of the target which produces assimilation if no correction (exclusion) takes place. (3) Features of the judgment task or communicative setting may trigger exclusion of potentially relevant information. (4) Exclusion can result in a contrast effect for one of two reasons: (a) as in Martin’s reset hypothesis, exclusion of the contextual material (subtraction) can lead to overcorrection, and (b) the information excluded from the target can be included in the representation of the standard of comparison or the scale anchor (Schwarz & Bless, 1992b). These processes would lead to contrast to the extent that the excluded material is more extreme than the default standard of comparison, scale anchor, or target representation. (5) In agreement with Martin, the emergence of a contrast effect by either of the exclusion mechanisms above requires more processing steps and more cognitive effort than the default inclusion process that produces assimilation.

One interesting characteristic of all of the studies investigating reset and exclusion processes is that in every case correction processes moved judgments away from the context (i.e., away from assimilation and toward contrast). Importantly, all of these studies have used contextual stimuli that would normally be expected to lead to assimilation effects when no correction processes are activated. Perhaps because of this, the models
of assimilation and contrast used to interpret these results explicitly have
treated assimilation as the default outcome and have viewed contrast as
the outcome associated with more effortful correction processes (see Martin et al., 1990; Schwarz & Bless, 1992a).1

The current research examines the notions that contrast can sometimes
be the default outcome and that effortful correction processes can move
judgments closer to the context rather than further away from it. Spe-
cifically, if one could find contextual stimuli for which contrast is viewed
by people as the normal consequence of exposure, then correction pro-
cesses should adjust ratings of targets toward the context. Our goal in a
first study was to examine the idea that people are aware that contrast
as well as assimilation can be the natural judgmental effect of contextual
stimuli. If people believe (correctly or incorrectly) that contexts can pro-
duce assimilation or contrast, then factors in the situation that invoke
effortful correction processes should lead to correction for either assim-
ilation or contrast. To date, the extent to which people are aware of the
assimilation and contrast effects of contextual stimuli has not been ex-
plored in empirical research.

EXPERIMENT 1

Are people aware of contextual configurations that elicit contrast effects,
or do people tend only to hold naive theories of assimilation for the effects
of contextual stimuli? If we can identify contexts for which people believe
contrast effects are likely, we could then use such contexts to determine
the extent to which people are capable of correcting for the presence of
such contexts. In addition, by showing that the contexts used in past

1 Although we have focused on “priming” paradigms for evidence relevant to categori-
ization models of assimilation and contrast effects, additional contexts and instigations of
correction processes have shown similar results. For instance, Schwarz and Clore (1983)
found that happy subjects rated their overall life satisfaction higher than sad subjects when
no attention was drawn to the source of their mood state. When attention was directed
ward a potential cause of mood that was irrelevant to life satisfaction (e.g., the weather),
however, happy and sad subjects rated their satisfaction equally high. That is, when mood
was made salient, and was made to appear irrelevant to the question at hand, subjects
adjusted their ratings of life satisfaction away from the context (i.e., away from assimilation
to mood). Although Schwarz (1990) prefers to view mood as an answer to a “How do I
feel about it?” heuristic, the effects of the Schwarz and Clore (1983) studies could also be
explained through consideration of mood as a context for judgment. That is, in the Schwarz
and Clore studies, there were assimilative effects of mood when mood was a subtle contextual
cue. When mood was made salient (similar to the blatant primes used in the Martin studies),
however, correction processes took place that adjusted life satisfaction ratings away from
judges’ reactions attributed to the contextual mood state (see Eiser, 1990). Because mood
manipulations have sometimes elicited contrast effects (e.g., Dermer, Cohen, Jacobson, &
Anderson, 1979, Experiment 2) instead of the assimilative effects predicted by the “How
do I feel about it?” heuristic, it seems reasonable to view mood as a potential contextual
variable.
studies of context correction processes are seen by subjects as likely to lead to assimilation effects, it becomes possible that past results are a special case of a more flexible bias-correction process. In order to examine this, we described various contexts and judgments to subjects and asked them what the natural effect of these contexts would be on people's target ratings.

**Method**

**Subjects and Procedure**

Twenty-three undergraduate psychology students at Ohio State University participated in partial fulfillment of a class requirement. All subjects received a questionnaire describing five contexts and judgments to be made. Instructions explained that contexts can sometimes create biases in people's judgments and that we wanted to find out what people thought about how some specific contexts might affect their ratings. Subjects were asked to decide what effect they thought that each context would tend to have on people's perceptions—whether it would make people see the target as more or less favorable.

Two of the items described contexts that based on past research would be expected to bring about assimilation effects if no correction processes were activated. These contexts included the effects of the priming of a trait on judgments of a person engaging in ambiguous behavior (Martin, 1986, Martin et al., 1990) and the effects of mild positive mood on judgments of the pleasantness of everyday activities (Schwarz & Clore, 1983). Two other items described extreme contexts that past research suggested would bring about contrast effects if no correction processes were activated (Herr et al., 1983; Sherif & Hovland, 1961). These contexts included the effects of being in the location of one's dream vacation (Dermer, Cohen, Jacobsen, & Anderson, 1979) on judgments of the average location, and the effects of seeing a group of very attractive people on judgments of average-looking people (Kenrick & Gutierres, 1980). Whereas these four items were adapted from contexts that have produced assimilation or contrast in previous research, the remaining item was a filler item assessing the effects of a room with red paint on the walls on judgments of how considerate a person is.

Ratings of the likely effects of such contexts were tailored to the individual items. Specifically, subjects' perceptions of the judgmental effects of being in the location of one's dream vacation were measured by asking whether being in that place would make one's perception of an ordinary location seem less pleasant than when not in the fantastic place (-4 on a nine-point scale) as opposed to more pleasant than when not in the fantastic place (+4). Perceptions of effects of the blatant priming of hostility were assessed by asking whether an ambiguous target person
would then be viewed as more hostile (−4) versus less hostile (+4). The perceived effects of being in a red room were assessed by asking whether the room would make a person seem less considerate (−4) as opposed to more considerate (+4). The effects of being in a good mood were measured by asking if the mood would make a normal day's events seem less pleasant (−4) or more pleasant (+4). Finally, perceptions of the effects of seeing a group of incredibly attractive people were assessed by asking whether an average-looking target person would seem less attractive (−4) as opposed to more attractive (+4). For all subjects, the presentation of the contexts was in the order just described.

Results and Discussion

Ratings of the perceived judgmental effects of each context were analyzed by testing the difference between the average rating for each context against the zero-point of each scale (i.e., no perceived influence of the context) using the Student's t statistic. Although no effects were predicted for the filler item, subjects rated the likely effect of being in a bright red room as making people seem somewhat less considerate ($M = −.74; t = 2.04, p < .054$).

Of greater interest, the stimuli used in past studies of context–effect correction processes led to assimilation effects. Consistent with our expectation, the priming of hostility ($M = −1.22$) and good mood ($M = +2.43$) were both seen as encouraging ratings consistent with the respective contexts ($t = 2.95$ and 5.60, respectively; $ps < .007$ and .0001). More importantly, it appears that subjects do view at least some contexts as likely to bring about contrast rather than assimilation effects. That is, subjects believed that both being in the location of one's dream vacation ($M = −1.57; t = 2.98, p < .007$) and seeing a group of very attractive people ($M = −1.74; t = 3.83, p < .001$) would lead to significant contrast effects on judgments of average targets. That is, people rated the likely effect of these contexts as moving judgments away from the context.

Thus, it seems possible to construct situations such that people perceive contrast effects to be the natural consequence of a context. This implies that contrast might occur in the absence of correction processes, and more importantly that corrections brought to bear on these judgments might be in a direction opposed to contrast rather than toward it. Of course, simply showing that subjects believe that some contexts lead to contrast effects does not show that people know how to correct for such contextual effects.

Instead, it could be that all attempts to make corrections for contexts result in “subtracting out” or excluding primed elements of the context and one's reaction to it. If the reset or exclusion approaches provide the sole mechanisms for the corrections for contexts that people actually make, then presenting people with one of the “contrast” contexts from Study 1
and inducing them to correct for this context should lead them to adjust target ratings away from the context, thereby creating more pronounced contrast. It could be, however, that if subjects are aware of the effects of a contrast-inducing context, they would appropriately correct their judgments by moving them back toward the context. Such corrections would provide evidence for a more flexible adjustment model of context correction.\(^2\)

We expected that people induced to correct for a context they believed would create a contrast effect would correct for that context by adjusting their ratings of the target back toward the context. In order to show that people can correct toward the context rather than away from it, we explicitly asked subjects in some conditions of Experiment 2 to attempt to keep contextual stimuli from influencing their judgments. Whereas past studies investigating correction processes have used stimuli that would be expected to produce assimilation in the absence of correction processes, we used stimuli that would be expected to produce contrast in the absence of correction processes.

**EXPERIMENT 2**

In Experiment 2, the context consisted of either five very popular and exciting vacation locations (Extreme Positive context—an operationalization of the dream vacation context described in the preliminary study) or a set of neutral American cities (Neutral context). After rating the set of context locations, subjects either immediately rated two neutral target locations (No correction instruction or “default” condition) or were asked not to let their perceptions of the initial locations influence their perceptions of the targets (Correction instruction condition).

In accordance with Study 1 and with past research, it was expected that an extreme positive context with no instruction to correct would lead to less positive ratings of the target location than would the neutral context (i.e., a contrast effect). Because subjects acknowledged that such contexts would lead to contrast effects in Study 1, however, we expected that asking them to avoid being influenced by the context would lead to a decrease in the contrast effect when compared with the default no-instruction condition (i.e., subjects would correct their judgments by adjusting their ratings toward the context). If, however, corrections for contexts always occur through processes in which subjects subtract out or exclude their reactions to the context, then asking subjects who encounter

---

\(^2\) It is important to note that strictly speaking, set/reset correction involves subtraction of one’s reaction to the context rather than the context per se (see Martin & Achee, 1992). Our assumption in this research is that people’s reactions to the context are congruent with the valence of the presented context (i.e., considering a fantastic vacation location induces favorable thoughts and associations).
an extreme context to avoid being influenced by their perceptions of the initial locations should result in adjustments in judgments away from the context as compared with the no-instruction condition.

Method

Subjects and Design

Eighty-six undergraduate psychology students at Ohio State University participated in the study in partial fulfillment of a class requirement. Subjects were randomly assigned to the 2 (Context: Extremely Positive, Neutral) × 2 (Instruction: Correction, None) between-subjects design.

Procedure

Subjects participated in groups ranging from two to eight people. The first page of a packet of experimental materials distributed to subjects contained all the items for the present study. At the top of the page, a set of instructions informed subjects that the experimenter was interested in people’s perceptions of many locations. The instructions asked participants to imagine that they were given the opportunity to take a 2-week vacation during the next fall, and were asked to rate how much they would like to spend the 2 weeks in each of the locations listed. Ratings were made on an 11-point scale (1 = “like very much” to 11 = “dislike very much”). After completing the experimental materials, subjects were thanked, debriefed, and dismissed.

Independent Variables

Context manipulation and target locations. Subjects began by rating their liking for five locations that formed the context manipulation. These contexts were either very desirable and popular vacation spots (i.e., Hawaii, Paris, the Bahamas, San Francisco, and Jamaica—the Extremely Positive condition) or relatively neutral American cities (i.e., Minneapolis, Houston, St. Louis, Pittsburgh, and Atlanta—the Neutral condition). The two target locations that followed the contextual locations were also neutral American cities (i.e., Indianapolis and Kansas City).

Instruction manipulation. Subjects either rated the five context locations and then the two target locations, or they rated the five context locations and then were asked to “please make sure your ratings of the following locations are not influenced by your perceptions of the locations you just rated.” The two target items at the bottom of the page actually formed the end of the experimental materials for this study.

Results and Discussion

Subjects’ ratings of context and target locations were reverse-scored so that higher ratings corresponded to greater liking of the rated locations.
Fig. 1. Liking of target locations as a function of context (neutral or extremely positive) and correction instruction in Experiment 2.

Ratings of the five context locations were averaged to form a manipulation check on context. A 2 (Context) \( \times \) 2 (Instruction) ANOVA showed that context was manipulated successfully. The extreme positive context (\( M = 9.69 \)) was seen more positively than the neutral context (\( M = 6.69 \)), \( F(1, 82) = 87.3, p < .0001 \).

Ratings of the two target locations were averaged to form the primary dependent measure. On these ratings, there was only a significant Context X Instruction interaction, \( F(1, 82) = 4.55, p < .04 \) (see Fig. 1). When no instruction was given, the targets were seen more positively by subjects who received the neutral context (\( M = 6.10 \)) than by subjects who received the extreme positive context (\( M = 4.85 \)), \( F(1, 82) = 4.13, p < .05 \) (a contrast effect). When subjects were asked not to be influenced by perceptions of the contextual locations, however, there was no significant difference in ratings of the target between the neutral and positive context groups (\( Ms = 5.87 \) and 6.50, respectively). In fact, the difference in means was actually in the direction of an assimilation effect. Importantly, the presence of the correction instruction led subjects who had received an extreme positive context to rate the target locations significantly more positively compared to those who were not so instructed, \( F(1, 82) = 7.02, p < .01 \). That is, people corrected by rating the targets as more similar to the extreme positive context. Subjects who received the neutral context showed no correction (\( F < 1 \)).

The results of this study indicate that people do not always make corrections by moving their judgments away from the context or their re-
actions to it. Whereas past studies of context effects have shown corrections leading to judgments of targets as less similar to the context (e.g., Martin, 1986; Martin et al., 1990; Schwarz & Clore, 1983), the current study shows correction processes leading to judgments of targets as more similar to the context. Presumably, if subjects in the current study had believed that holding positive views of the extreme contextual locations would bias them toward too positive a view of the target locations, their corrections would have been in the direction of further contrast. Instead, asking subjects to correct for any influence of the contextual stimuli actually decreased (wiped out) the contrast effect observed in the no-instruction conditions.

One might argue, however, that the contrast and correction effects observed in Experiment 2 could be effects of response language rather than contrast and correction per se. Or, as postulated by the Schwarz and Bless (1992a) inclusion/exclusion model, subjects may have excluded the contextual items from the targets to be judged, but included them in their representation of the scale anchors (or comparison standard). That is, if subjects used the extremely positive contextual items (e.g., Hawaii) to define the positive content endpoint of the response scale (i.e., “like very much”), the meaning of this endpoint may have been more positive than in the neutral context conditions. If the scale was expanded in a positive direction when the context was very positive, then ratings of the target stimuli may be lower than in neutral conditions because of the expanded range of possible responses considered by subjects (e.g., see Ostrom & Upshaw, 1968). When correction instructions were given, however, subjects may have taken such instructions to mean that the contextual stimuli should not be used in defining the scale endpoints. Thus, if subjects ignored the contextual stimuli in the correction conditions, they may have reverted to their “default” notions of what the scales meant yielding the “corrected” ratings of the target cities. If this explanation were true, subjects would not have corrected their perceptions of the target cities for the perceived effect of the context, but would have simply adjusted the end anchors of the rating scale.

There are a number of reasons to believe that the results of Experiment 2 are not due to changes in scale definition, however. For instance, this explanation either assumes that subjects’ “default” conceptions of positive vacation locations does not include locations as desirable as Hawaii, San Francisco, or the Bahamas, or that the positive context conditions led subjects to consider locations even more positive than these. It seems unlikely that locations as desirable as Hawaii or San Francisco would not come to mind within the category of “desirable vacation spots” to anchor the scale even if these locations were not explicitly presented. Furthermore, given the high desirability of the positive locations employed, it seems unlikely that subjects would generate even more positive locations
to anchor the scale. In addition, it would be somewhat odd for subjects to redefine the rating scale at the time of the correction instructions because at this point they had no idea of the valence of the additional cities they would have to rate. Specifically, because they did not know if the cities on the next page of their booklet would be more, less, or equally positive, making the end anchor less positive might mean that the scale would not accommodate the cities that were to be rated next.

Despite its implausibility, the possibility exists that the effects observed in Experiment 2 are at least partially due to redefinitions of the positive response scale endpoint. We conducted a third experiment in an attempt to directly address the possibility that the corrections we observed were due to response scale redefinition. If the corrections observed in Experiment 2 were due to changes in the meaning of response scale endpoints, then asking people not to be influenced by perceptions of initial locations should produce little or no difference in target ratings if the response scale endpoints were not redefined. That is, if scale endpoints include a specific referent that does not change across conditions, then corrections due to changes in scale meaning should be minimized or eliminated. Past attempts at distinguishing between response language and actual perceptual changes have generally attempted to use scales that are less likely to be redefined because of the existence of an external reference point—such as measuring distance in inches (Krantz & Campbell, 1961) or weight in ounces (Harvey & Campbell, 1963) rather than using the more subjective terms of "short/long" or "light/heavy." Because liking has no "concrete" external counterpart to physical measures, we decided to use a different means to achieve a similar conceptual purpose. Specifically, in Experiment 3 we used an explicitly defined reference point that remained constant across conditions. That is, in some conditions of the following experiment, we labeled the response scale endpoints as representing a specific pair of extreme exemplars on the dimension of judgment and this remained constant in all of the experimental conditions.

EXPERIMENT 3

Proponents of a response language interpretation of Experiment 2 could contend that, in the extreme positive context conditions, the label "like very much" was redefined as less positive in the correction than in the no-correction conditions. If the scale endpoint is defined by direct reference to a specific location or pair of locations, however, and if those locations remain constant even when correction instructions are given, then little or no redefinition of the scale should take place. Therefore, in Experiment 3, we provided all subjects with the positive contextual items from Experiment 2, but the scale anchors were either the same anchors used in Experiment 2 (Abstract anchor condition) or included reference to a pair of specific locations (Specific anchor condition). Subjects either
rated the target locations immediately following the contextual locations or received the correction instructions from Experiment 2 before rating the targets. Thus, subjects were assigned to a 2 (Instruction: none, correction) × 2 (Scale anchor: abstract, specific) between-subjects design.

The response language (Scale anchor) hypothesis predicts that the correction observed in Experiment 2 would be replicated in the Abstract anchor condition of the present study. In the Specific anchor condition, however, either no effect or a reduced effect of the correction instruction would be expected. Thus, the response language alternative predicts an interaction of the Instruction and Scale anchor factors. Because we believed that the results of Experiment 2 were not due to scale redefinition, we predicted that the effect of the correction instructions observed in Experiment 2 would be replicated in both the Abstract and Specific conditions. Therefore, we predicted only a main effect of the Instruction factor.

Method

Subjects and Design

Eighty undergraduate psychology students at Ohio State University participated in the study in partial fulfillment of a class requirement. Subjects were randomly assigned to the 2 (Instruction: correction, none) × 2 (Scale anchor: abstract, specific) between-subjects design.

Procedure and Independent Variables

Subjects participated in groups ranging from two to five people. The procedure, correction instructions, and locations to be rated were identical to those used in Experiment 2. The only difference was in the labeling of the scale anchors of the response scale. In the Abstract condition, the scale anchors were identical to those in Experiment 2. In the Specific condition, however, subjects rated each location on a scale anchored at 1 = “like as much as staying in Hawaii or Paris for 2 weeks (like very much)” and 11 = “dislike as much as staying in Cambodia or Iran for 2 weeks (dislike very much).” This scale remained constant across the instruction conditions. Thus, although correction instructions asked subjects not to let the context items influence their ratings of the targets, the meaning of the positive endpoint of the scale still explicitly referred to the level of liking associated with the extremely positive contextual stimuli (e.g., Hawaii and Paris) and the negative endpoint was similarly anchored in specific locations. After completing the experimental materials, subjects were thanked, debriefed, and dismissed.

Results and Discussion

Subjects’ ratings of context and target locations were reverse scored so that higher ratings corresponded to greater liking of the rated locations.
Ratings of the five context locations were averaged to form a check on perceptions of the context. A 2 (Instruction) by 2 (Scale anchor) ANOVA showed that context was unaffected by these manipulations (all Fs < 1). The contextual locations were liked to an extent similar to that in Experiment 2 (M = 9.40).

Ratings of the two target locations were averaged to form the primary dependent measure. On these ratings, there was only a significant main effect of Instruction, F(1, 76) = 9.22, p < .003. That is, regardless of whether the scale endpoints included reference to particular locations, the target items were rated more positively after correction instructions (M = 6.09) than when no instruction was given (M = 4.81). There was no Instruction X Scale anchor interaction, F < 1. This replicates the correction effect of Experiment 2 and shows that the same correction effect is obtained even if scale anchors are used that are difficult to redefine. Thus, it appears that the correction effects in Experiment 2 were not solely due to redefinitions of the response scale.  

Although response language effects do not appear to account for the corrections observed in Experiments 2 and 3, asking subjects not to be influenced by their perceptions of the initial locations basically told them that the locations were capable of affecting their later ratings. It is possible that such explicit instructions could create some of the differences between the outcomes of the present studies and those of past context–effect correction research. Perhaps our blatant correction instructions activate processes that are not usually active or that are unlikely to occur with more natural instigations of correction processes. For instance, without the explicit instructions, subjects may not consider that perceptions of the initial locations could affect later ratings. If this is the case, subjects encountering the same judgment situation without such an explicit instruction to correct might not adjust their ratings at all, or may adjust their ratings in the direction found by past context–effect research—in the direction of further contrast.  

3 In order to make sure that the main effect of the Correction factor was not driven primarily by ratings in the Abstract condition, we analyzed ratings in the Specific condition separately. When the scale anchor was specific, the target items were rated more positively after correction instructions (M = 6.50) than when no instruction was given (M = 5.05), F(1, 38) = 5.46, p < .026. Thus, correction took place even when the referent locations in the scale anchors were held constant across the correction conditions.

4 The current set of blatant instructions might also be viewed as doing things other than simply asking people to avoid the bias inherent in the presented context. One way that the current instructions might have brought about corrections is through telling the subject that his or her initial reactions to the targets were somehow inappropriate. Because inappropriateness is exactly the judgment thought to bring about corrections in the set/reset framework (Martin et al., 1990), however, such information should have brought about reset-like corrections within that framework. Alternatively, the instructions could be viewed as telling subjects that they would have to somehow defend their ratings, making them more
Past research has typically used more subtle means of cuing correction processes than simply asking people to correct. For example, Schwarz and Clore (1983) merely asked subjects about the weather without telling them that the weather could have influenced judgments of life satisfaction in any way. In the Martin (1986) studies, simply making subjects believe that they were finished with the contextual rating task brought about corrections. Similarly, asking target questions in a way that makes them appear to be asking for different information (Strack, Martin, & Schwarz, 1988) can bring about corrections in judgment. In Experiment 4, we used a variant of these procedures to explore the ability of subtle manipulations to elicit correction for contrast. That is, in the following experiment, we either provided no labeling of the target ratings (no correction condition) or labeled the targets as the “next group” of ratings (without revealing any possible influence of the contextual ratings on the targets). Subjects were simply informed that the first set of context ratings was finished and that the “second” set was now to be completed. If more subtle procedures are capable of bringing about corrections for contrast, then cues as subtle as those used in past research should instigate corrections in the same direction as the explicit correction instructions used in Experiments 2 and 3.

**EXPERIMENT 4**

The experimental materials and study design were identical to those used in Experiment 2 with a few notable exceptions. First, because we were investigating the ability of very subtle cues to induce correction processes in this study, we attempted to increase the salience of the initial contextual locations. We did this by having subjects write the first characteristic of each location that came to mind on a line below the rating scale for that location. Second, in addition to the default condition where no cue or instruction was provided, there were two correction cue conditions where some form of subtle or more blatant differentiation between target items and the contextual items was possible. However, unlike Experiments 2 and 3, subjects were never instructed to make corrections.

likely to move to more moderate (and presumably defensible) judgments (Cialdini, Levy, Herman, Kozlowski, & Petty, 1976). This interpretation also seems unlikely given the wording of the instructions (i.e., “Please try to make sure that your ratings of the next two places are not influenced by your perceptions of the locations you just rated.”). Nevertheless, to the extent that more subtle means of bringing about corrections (used in Experiment 4) are successful in producing results similar to those found in Experiment 2 (i.e., procedures that are unlikely to convey information about a need to defend one’s judgments), this interpretation of the present instructions becomes less plausible. Regardless of the interpretation one prefers for the instructions used, the results of these studies provide evidence of correction processes leading to judgments in a direction opposite to that observed in prior assimilation/contrast research.
In fact, neither correction cue condition mentioned any potential influence of the contextual locations on perceptions of the target locations. Rather, some very subtle or more blatant distinction between the contextual items and target items was made. In previous research (e.g., Martin, 1986), distinctions between context and target items have led subjects to correct for the context by displacing judgments away from the evaluative implications of the context. As noted above, however, previous studies have used contextual stimuli that ordinarily induce assimilation effects on judgments. The primary goal of the current experiment was to examine the nature of the correction induced when contextual stimuli that ordinarily induce contrast are used along with subtle correction cues. Based on the results of our previous experiments and on our flexible correction model, it was expected that the correction cues would again lead to corrections in the direction toward rather than away from the context.

**Method**

**Subjects and Design**

One hundred and seventy-two undergraduate psychology students at Ohio State University participated in the study in partial fulfillment of a class requirement. Subjects were randomly assigned to a 2 (Context: extremely positive, neutral) × 3 (Correction cue: no cue, subtle cue, blatant cue) between-subjects design.

**Procedure**

Subjects participated in groups ranging from 2 to 12 people. The same contextual locations, initial instructions, and rating scale (1 = “like very much” to 11 = “dislike very much”) were used as in Experiment 2. Experimental materials were presented on two pages, however. The contextual items were presented on the first page and the target items were presented on the next page. Upon completion of the experimental booklet, the subjects were debriefed, thanked, and dismissed.

**Independent Variables**

*Context manipulation and target locations.* As in Experiment 2, in the extreme positive context conditions, subjects rated five very desirable vacation destinations (e.g., Hawaii). In addition, they listed the first characteristic that came to mind about that location. In the neutral context conditions, they rated five neutral American cities (e.g., Minneapolis) and listed the first characteristic that came to mind.

In addition to the two targets used in Study 2 (i.e., Indianapolis and Kansas City) the midwestern cities of Green Bay and Des Moines were included as targets. It was thought that using a larger number of target
items would facilitate the perception that the second page contained a new and independent task in the correction conditions.\(^5\)

*Correction cue manipulation.* In the no-correction cue condition, the contextual ratings were followed immediately by four target locations on the following page. In this condition, it appeared that there was only one rating task consisting of nine items. In the subtle correction cue condition, the top of the second page of ratings simply stated that for the “next group” of ratings we had “more vacation spots to consider.” This vacuous instruction formed a sort of minimum baseline for correction in that the target locations were set aside as a “next group” but were a part of the same questionnaire from the same experimenter and were not labeled as different from the contextual locations. This was a very subtle way of “ending” the first task—a procedure found in past research to induce correction processes (see Martin, 1986). In the blatant correction cue condition, the target locations again formed the “next group” of ratings but also were labeled as “a group of midwestern cities whose characteristics are quite different from the vacation spots just rated.”

**Results and Discussion**

Subjects’ ratings of the contextual and target locations were reverse scored so that higher ratings corresponded to greater liking of the rated locations. Ratings of the five context locations were averaged to form a manipulation check on context. A 2 (Context) × 3 (Correction cue) ANOVA showed that context was manipulated successfully. The extreme positive context \((M = 9.52)\) was seen more positively than the neutral context \((M = 6.36)\), \(F(1, 166) = 198.0, p < .0001\). These means were very similar to those obtained in Experiment 2. There was also a significant Context × Correction cue interaction, \(F(1, 166) = 4.10, p < .02\). It appears that random assignment of subjects to correction cue conditions was not successful in equating subjects’ perceptions of the contextual locations. In order to statistically control for this, the data set was split into two groups (neutral and extreme positive context) and adjusted values of the target ratings were obtained from separate analyses of covariance (ANCOVAs) using ratings of the contextual locations as a covariate. These adjusted values were then submitted to the 2 (Context) × 3 (Correction cue) ANOVA reported below.\(^6\)

Ratings of the four target locations were averaged to form an overall index of target liking. On this index, there was a main effect of Context,

\(^5\) The results are the same, however, if only the first two target cities are analyzed.

\(^6\) Analysis of target ratings using unadjusted means does not significantly affect the conclusions reached. That is, the same Context × Correction cue interaction reported below is significant and in the expected direction using either set of analyses (though using the adjusted means seems to be the more conceptually justifiable method).
such that the target was seen more positively when preceded by a neutral context ($M = 5.51$) than by an extreme positive context ($M = 4.39$), $F(1, 166) = 122, p < .0001$—a contrast effect in judgment; see Fig. 2). In addition, there was a main effect of Correction cue such that targets were viewed more positively in the subtle cue condition ($M = 5.18$) than in either the blatant cue ($M = 4.91; p < .03$) or no cue ($M = 4.77; p < .001$) conditions. More importantly, the overall contrast effect was qualified by the expected Context × Correction cue interaction, $F(2, 166) = 21.5, p < .0001$.

We conducted separate $2 \times 2$ analyses to determine whether the subtle and blatant cues were each effective in bringing about corrections (as compared with the no correction cue conditions). When a subtle cue was given, a significant Context × Correction cue interaction was obtained, $F(1, 111) = 13.2, p < .0004$. Whereas the subtle correction cue produced no changes in the ratings of targets following a neutral context ($p > .8$), the subtle cue produced a significant increase in the positivity of target ratings following the extreme positive context ($p < .0001$). That is, following the subtle cue, judgments of targets within an extreme context were adjusted significantly toward the context. This provides a conceptual replication of the results of Experiments 2 and 3 with a quite subtle correction cue.

When a blatant cue was given, the results are quite similar. Once again, a significant Context × Correction cue interaction was observed, $F(1, 109) = 42.1, p < .0001$. The blatant cue produced a significant increase
in the positivity of target ratings following the extreme context \( p < .0001 \). That is, judgments of targets within an extreme positive context were significantly adjusted \textit{toward} the context, again replicating Experiments 2 and 3. In addition, the blatant cue produced a significant decrease in the positivity of target ratings following the neutral context \( p < .0002 \). This may have occurred because of the statement that the target locations were "quite different" from the contextual locations that had just been rated. Ratings of the target locations were slightly less positive than those of the neutral contextual locations in the no cue conditions. It may be that the wording of the blatant cue served to accentuate the initial differences between the target and neutral context locations.

In sum, this study showed that a significant correction \textit{toward} the context is obtained even when cues to elicit such corrections are quite subtle. Because of this, it seems unlikely that the explicit nature of the requests for correction in Experiments 2 and 3 were activating processes that were unnatural or somehow unlike corrections instigated by the more subtle procedures used in prior studies. Rather, significant correction for contextual contrast effects seems to be triggered just as easily—even by subtle cues—as is the correction for assimilation effects observed in prior research (e.g., Martin, 1986; Martin \textit{et al.}, 1990).

**GENERAL DISCUSSION AND CONCLUSIONS**

The current research suggests that corrections for context effects may be quite flexible. That is, perceptions of a target or group of targets may be adjusted to be either more or less like the context depending on whether the context is viewed as making assimilation or contrast more likely. Studies that have used contexts likely to create assimilation effects in "uncorrected" perceptions (e.g., Martin, 1986; Martin \textit{et al.}, 1990; Schwarz & Clore, 1983) have found correction processes to produce target ratings displaced away from the context (i.e., away from assimilation and toward contrast). In Experiment 1, we validated the notion that people are aware that some pairings of contexts and targets naturally lead to assimilation bias, but other pairings naturally lead to contrast bias. Of course, investigation of the factors leading people to expect assimilation or contrast is an interesting topic, but for our current purposes it was sufficient to identify (in Study 1) a context that people expected would lead to contrast. In Experiments 2, 3, and 4 we used such a context and found that a contrast effect was produced when subjects' judgments were "uncorrected." Importantly, we found that both explicit instructions to correct for the context and more subtle correction cues led subjects to adjust their ratings back toward the context (i.e., away from contrast and toward assimilation).
Implications for Current Models of Context Correction

Perhaps because many of the studies investigating corrections for context effects have used contexts that lead to uncorrected assimilation, current models have tended to equate correction processes with contrast (see Martin & Achec, 1992; Martin et al., 1990; Schwarz & Bless, 1992a). In doing so, these models treat contexts as though they lead to assimilation in “uncorrected” settings and treat contrast effects as the result of contextual correction (i.e., resetting or exclusion). This assumption does not match up with the expectations of the subjects in our first study, however, who believed that either assimilation or contrast could be the default impact of a context.

Without knowing the results of our first study, one might conclude that the conditions in Experiments 2 and 4 closely mirror those of past work on context effects. For instance, Martin (1986; Martin et al., 1990) created positive or negative contexts (through blatant priming), manipulated the likelihood of correction processes taking place (e.g., by allowing or disrupting psychological completion of the priming task), and measured perceptions of a neutral target (an ambiguous person). The present studies created a positive context (“dream” vacation locations), manipulated the likelihood of correction processes taking place (by explicitly asking people to correct or not or by giving a cue that the initial contextual target rating task was complete or not), and measured perceptions of neutral targets (midwestern American cities). In the Martin studies, subjects corrected by making target judgments less similar to the context. In our studies, subjects corrected by making judgments more similar to the context. Given the apparent similarities in procedures, but the seemingly opposite results, it seems useful to analyze the present studies from the perspective of current process frameworks such as the set/reset model (Martin, 1986; Martin & Achec, 1992) and the inclusion/exclusion model (Schwarz & Bless, 1992a).

The set/reset model. According to Martin et al. (1990), one’s reaction to a target stimulus (especially a neutral or ambiguous one) may initially contain both positive and negative features. When a positive context is presented, for instance, one’s representation of the context may partially overlap with one’s representation of the target. Because people may not be able to perfectly discriminate between reactions to the context and to the target, when they attempt to “partial out” their contextually induced reaction, they may inadvertently take out aspects of their true reaction to the target. In this case, because partialing out the positive context may result in taking out positive aspects of one’s true reaction to the target, the target may be seen as less positive than was initially the case (contrast).

Most of these assumptions seem reasonable for the stimuli used in the present studies. Representations of the neutral targets (e.g., Indianapolis)
probably included positive and negative elements. A positive context was introduced (according to manipulation checks), and corrections took place. The direction of correction was not in the direction of contrast, however, but in the direction opposite to that of contrast. We have argued that this took place because subjects believed that the natural effect of the context we used was to produce contrast, and thus correction led to movement in the direction away from rather than toward contrast.

How might the set/reset model attempt to account for these results? There are a few possibilities. First, from the perspective of this model, it could be argued that the initial (default) contrast effects were actually the result of correction (partialing) processes. Although no cues in the design or instructions of the questionnaire were present to instigate correction (reset) processes in these circumstances, it could be that the sheer discrepancy between the context and target prompted effortful correction processes. If corrections were taking place in our “no-correction” conditions, however, it is not clear why including additional correction instructions or subtle or blatant correction cues would create further corrections (especially in the opposite direction). That is, the set/reset model currently provides no reason for subjects to correct if they have already corrected. According to the set/reset model, correction (reset) processes are performed with the goal of arriving at an “appropriate” (context-independent) response. Once an “appropriate” response is reached through correction processes, there should presumably be little reason to correct further. Thus, the set/reset model provides an incomplete account of our results.

Alternatively, it could be that the extreme context presented in our studies was so extreme that no overlap existed between representations of the target and the context. In such a case, however, the set/reset framework would seem to predict no change in target ratings. Within this framework, changes in target judgments are the result of imperfect discrimination between overlapping elements, and of a partialing process that may subtract out genuine reactions that are confused with contextually primed reactions. When there is no overlap (thus no confusion) of context with target, then no correction should take place. Yet, in our studies, we found that a correction did take place.

As noted earlier, in the set/reset model, people are postulated to correct for their reactions to the context rather than the context per se (see footnote 2). Usually these are the same in that a positive context produces a positive reaction. What if the positive context activates thoughts that are primarily of the opposite valence to that of the contextual stimuli, however? That is, what if our midwestern subjects generated negative thoughts because they were not in the fantastic vacation locations? According to the set/reset model, if someone feels negatively, even if the negative outcome stems from something good (e.g., a headache that comes
from "too much of a good time"), the person would partial out negative rather than positive reactions. By assuming that our positive context induced a negative reaction, the correction process would be in the direction of contrast rather than the direction of assimilation (with respect to the reaction). This set/reset explanation is interesting in that our corrections for contextual contrast are reinterpreted as corrections for assimilation to some unmeasured reaction that is incongruent with the actual valence of the contextual elements.

Although this subtlety of the set/reset model may be theoretically important in some situations, this interpretation seems poorly suited for the present results. That is, this approach deals with the contrast effect demonstrated in the present experiments as indicative of an assimilation to unmeasured negative reactions or feelings in response to the contextual stimuli. To the extent that imagination of vacations has been used in past research as an induction of positive rather than negative mood (e.g., Clore, Schwarz, & Kirsch, 1983; Gleicher, Baker, & Petty, 1989; Wegener & Petty, 1992) this set/reset interpretation appears implausible. In fact, in Experiment 4 we had subjects explicitly write their reactions to the contextual cities and in the positive context condition these reactions were almost unanimously positive!

Finally, it may be that negative thoughts were not elicited by the context per se, but through the subjects comparing the targets to the contextual stimuli. If people then corrected by removing some of those negative comparative thoughts, perceptions of the targets would become more positive (as observed). Of course, such an explanation would remove the set/reset model from the traditional priming processes on which it was developed. People would not be correcting for the idiosyncratic cognitive responses elicited by the context. Rather, people would correct for idiosyncratic thoughts that come about through comparison of target and contextual stimuli but are attributed to the context. Of course, this is a different mechanism than specified by previous models of context effects such as the set/reset formulation.

The inclusion/exclusion model. Similar to the set/reset model, it would appear that the Schwarz and Bless inclusion/exclusion model has difficulty accounting for the present results. First, in accounting for the contrast effect, it is not clear what feature in our judgment task would trigger exclusion of reactions to the contextual stimuli when judging the target locations. All locations in the no-correction conditions were labeled as vacation locations. Perhaps it could be argued that the target locations activated a different category than the context locations (or a subcategory that was more narrowly defined), but it is unclear when this would be expected to occur. In addition, if the contrast effect in the present studies is due to exclusion processes, then it is unclear from this model why subjects would exclude (correct) a second time. Because exclusion pro-
cesses are instigated in order to rid one's reaction to the target of any inappropriate information activated by biasing factors, further correction should be unnecessary.

Alternatively, one might reinterpret our correction conditions as explained by the "early exit" feature of the model. That is, subjects could have viewed our correction instructions (Experiment 2) and cues (Experiment 4) as indicating that the context was irrelevant to the target judgments. If subjects viewed the contextual locations as truly irrelevant in the correction conditions, no assimilation or contrast would be expected because neither inclusion nor exclusion processes would be operating. However, because ratings of stimuli on the same judgment dimension are relevant to target ratings according to the inclusion/exclusion model, it would seem that asking people not to let earlier ratings affect them should instigate exclusion processes. This did not occur, however.

More persuasively, perhaps, the "ignoring the context" explanation seems inconsistent with the results of Experiment 3 where correction processes were still observed even when we explicitly included the contextual items in the end anchor of the scale. When the contextual items are included in the scale anchor, they should have been difficult (if not impossible) to ignore. Because Schwarz and Bless (1992a) state that placing extreme information in the scale anchors (or standard of comparison) produces contrast effects, their model should predict contrast in all conditions where specific anchors were used in Study 3. This did not occur.

Finally, it is important to note that even if one accepted the (implausible) view that ignoring the contextual information was responsible for all of the corrections we observed, this interpretation of the effect would differ from ours in a number of essential respects. For instance, if subjects simply ignore the contextual items in the correction conditions, then the outcome of this correction could be no effect of the context, but the outcome could not be assimilation. That is, if a person ignores the context before any inclusion or exclusion occurs, no effect of the context is expected. According to our flexible correction model, however, the person may perceive a contrast bias to be greater than its actual magnitude. If corrections for this perceived bias occur, then the correction process may "overshoot" and produce assimilation. Such a prediction, it appears, cannot be derived by the "early exit" portion of the Schwarz and Bless model.

Even for the current data, however, the flexible correction and inclusion/exclusion frameworks differ in their predictions. Specifically, even if the outcome of the correction process is no effect of the context, the flexible correction model assumes that the correction process was more effortful than the no-correction default outcome. The "ignore" option of the inclusion/exclusion model would seem to be less effortful than exclusion. That is, because the "ignore" option represents an early exit, it represents fewer steps than exclusion. Thus, the inclusion/exclusion model
would predict that the correction conditions in the present studies were actually less effortful than the contrast effects observed in the no-correction conditions. The flexible correction model, on the other hand, expects that the corrections in the present studies were more effortful than the contrast effects observed in the no-correction conditions. These predictions and the differences between these models should be addressed in future research.

The flexible correction model of bias. Our alternative way of conceptualizing the outcomes from the current studies is through a flexible set of correction processes. That is, to the extent that social perceivers view any particular effect of a context as likely, factors that instigate correction processes (e.g., requests for accuracy, cues to context/target differentiation, etc.) should bring about adjustments in judgments of targets opposite in direction to the initial expected context effect. That is, people are hypothesized as correcting for their naive theories of bias. Whereas past models of assimilation and contrast have focused on perceptions of targets (e.g., Martin, 1986) or on the endpoints of the response scale (e.g., Upshaw & Ostrom, 1984), the current perspective emphasizes a different factor—perception of bias associated with a context. That is, social perceivers may have an initial reaction to a target stimulus and also have a perception of the possible or likely bias associated with the context surrounding that target. When a perceiver is motivated and able to take the perceived contextual effect into account (as documented in Martin et al., 1990), his or her final judgment of the target is the result of a correction process whereby the initial reaction is adjusted in light of the perceived contextual bias. Unlike current models of corrections for context, this flexible correction process would occur regardless of whether the bias is expected by the judge to be assimilation or contrast, and whether the perceived bias actually occurred or not. Whereas current models assume that corrections are in the direction of contrast (e.g., people subtract out their contextual reactions so that corrected judgments are displaced away from these reactions), the flexible correction model assumes that either assimilation or contrast can be the expected effect of a context and that either bias can be corrected by judges. That is, people can either correct in a direction toward or away from the context depending on whether their naive theories of bias are that assimilation or contrast is likely to occur as a result of exposure to the context. When the perceiver is unmotivated or unable to take the perceived contextual effect into account, his or her final judgment is more likely to be close to his or her initial reaction to the target (which may have been influenced by the context or other forces).

An important implication of this view is that there are multiple ways to arrive at relative assimilation and contrast outcomes. Relative contrast effects can be the result of correction processes (Martin et al., 1990) or
the result of a lack of correction processes (e.g., the default conditions of the present studies). Similarly, relative assimilation effects can be the result of correction or of lack of correction. Thus, assimilation and contrast effects may be either relatively effortful or non-effortful. To the extent that correction processes require effort (Gilbert, McNulty, Giuliano, & Benson, 1992; Martin et al., 1990; Schwarz & Bless, 1992a), correction-based contrast will require more effort than default (no-correction) assimilation; and correction-based assimilation will require more effort than default (no-correction) contrast. The latter notion should be addressed in future research.

The Generality of Flexible Correction Processes

Correction-like processes have long been a part of observation and theorizing in other areas of social psychology such as the area of causal attribution. For example, adjustments in people's perceptions as a result of salient situational factors have been shown for judgments ranging from the causes of a person's behavior (e.g., Himmelfarb & Anderson, 1975; see also Kelley, 1973) to assessments of a person's internal reaction to various events (e.g., Fazio, Zanna, & Cooper, 1977; Olson, 1992; Zillmann, 1983). Although some perspectives on attribution have dealt with nearly the whole of attribution as an effortful process (e.g., Kelley, 1967), more recent accounts assign greater cognitive effort to attributional correction processes—especially corrections of trait inferences for the presence of situational constraints (e.g., Gilbert, Pelham, & Krull, 1988; Quattrone, 1982; see also Trope, 1986). For example, Gilbert et al. treat the attribution process as consisting of three steps (1) categorizing the behavior (i.e., what is the actor doing?), (2) characterizing the actor (i.e., what trait does the action imply?), and (3) correcting the attribution (i.e., what situational constraints might have caused the action?). Importantly, Gilbert et al. (1988) note that "attributions are a product of dispositional inferences that are followed by situational adjustments" (p. 738).

There are a number of similarities between these assumptions and those of models of correction processes in context effects. For example, correction is assumed to be more effortful than non-correction. Thus, just as people high in need for cognition (Cacioppo & Petty, 1982) have been shown to be more likely to engage in context correction processes than low need for cognition individuals (Martin et al., 1990), so too have high need for cognition persons been shown to be more likely to engage in attributional correction processes (D'Agostino & Fincher-Kiefer, 1992). In addition, assuming that people always begin with trait inferences (i.e., that trait inferences are the default judgment) is analogous to assuming that contexts always lead to assimilation bias. Assuming that the impact of information about situational forces is due to a correction process is analogous to assuming that contrast is due to a correction process. Con-
sistent with the present flexible correction model of context effects, how-
ever, such a conclusion may be due at least in part to the types of target
stimuli used in empirical tests. Just as tests of context correction effects
have used contexts for which assimilation is the natural effect, tests of
attributional correction may have used scenarios for which trait inferences
were more natural than situational considerations. This does not preclude,
however, that stimuli exist for which a situational attribution would come
to mind before any consideration of trait qualities (see Anderson, 1983
for a discussion of differing situations activating differing causal alter-
natives).

Take, for instance, a target person that is wearing fatigue and a helmet
as he runs across a battlefield firing a machine gun. Although it is possible
that social perceivers would first infer trait hostility before taking into
account the situational constraints of being in a war, it seems more likely
that situational explanations might be the first causal alternatives to come
to mind, and that these alternatives could then be adjusted in light of
subsequent dispositional factors that are considered. To the extent that
such attribution contexts can be identified, it would seem that current
models of attributional inference will need to be expanded and/or revised.

In concluding, we note that corrections for bias have been studied in
a wide variety of domains. For instance, juries have been instructed to
disregard (or correct for) information brought forth in trials (e.g., Carretta
& Moreland, 1983; Sue, Smith, & Caldwell, 1973; Thompson, Fong, &
Rosenhan, 1981). Similarly, people have been asked to disregard pieces
of information in person-perception tasks (e.g., Wyer & Unverzagt, 1985;
Golding, Fowler, Long, & Latta, 1990). People have also been told in
debriefing (e.g., Anderson, Lepper, & Ross, 1980; Anderson, New, &
Speer, 1985) and persuasion (e.g., Gruder, Cook, Hennigan, Flay, Alessi,
& Halamaj, 1978) paradigms that information they used to form a belief
was false. According to the flexible correction model, for corrections to
occur, people must: (a) have a naive theory about the direction of the
bias induced by some information (e.g., “the prior convictions made me
think the criminal was guilty”; “the unattractiveness of the source might
have caused me to discount the merit of the position”), (b) the bias must
be made salient by some blatant or subtle means, and (c) the person must
be both motivated and able to make the correction. For example, legally
inadmissible evidence may often have its impact on jurors’ decisions be-
cause of jurors’ unwillingness to disregard evidence perceived as relevant
to accurate judgments of guilt (see Thompson et al., 1981). In at least
some cases, however, integrative processing of the information (Schul &
Burnstein, 1985; Wyer & Budesheim, 1987) may make it very difficult if
not impossible for people to correct for all the effects of the to-be-dis-
regarded information even if they were highly motivated to do so. Our
flexible correction model offers a potentially fruitful framework for organizing the phenomena investigated in these diverse areas.

REFERENCES


Schwarz, N. (1990). Feelings as information: Informational and motivational functions of


