

# Behavioral Adaptation, Confidence, and Heuristic-Based Explanations of the Probing Effect

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*Researchers have found that asking probing questions of message sources does not enhance deception detection accuracy. Probing does, however, increase recipient and observer perceptions of source honesty, a finding we label the probing effect. This project examined 3 potential explanations for the probing effect: behavioral adaptation, confidence bias, and a probing heuristic. In Study 1, respondents (N = 337) viewed videotaped interviews in which probes were present or not present, and in which message source behaviors were controlled. Inconsistent with the behavioral adaptation explanation, respondents perceived probed sources as more honest than nonprobed sources, despite the fact that source behaviors were constant across conditions. The data also were inconsistent with the confidence bias explanation. Studies 2 and 3 investigated the probing heuristic explanation. The data from Study 2 (N = 136) were ambiguous, but the results of third study (N = 143) were consistent with the heuristic processing explanation of the probing effect.*

Common sense suggests that interrogative probing should be an effective strategy for detecting deception. By asking message sources probing questions, message recipients gain additional information, and consequently should render more accurate judgments regarding the veracity of sources' messages. Police and military interrogations, cross examinations in courtrooms, and interviews by investigative reporters are just a few examples of how probing is used as a strategy for uncovering truths and lies.

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Contrary to intuition, interrogative probing has not been found to significantly enhance deception detection accuracy (Buller, Comstock, Aune, & Strzyzewski, 1989; Buller, Strzyzewski, & Comstock, 1991; Stiff & Miller, 1986). Even more surprising, researchers repeatedly have found that probing causes judgments of message source honesty to increase (Buller et al., 1989; Buller, Strzyzewski, & Comstock, 1991). Regardless of the actual truth or falsity of the messages presented (Buller et al., 1989; Buller, Strzyzewski, & Comstock, 1991), the interactants' relationship history (Buller, Strzyzewski, & Comstock, 1991), message recipients' suspicion (Buller, Strzyzewski, & Comstock, 1991) or whether the individuals making truth/deception judgments are interaction participants or observers (Buller, Strzyzewski, & Hunsaker, 1991), the simple act of probing, or witnessing a source being probed, enhances source believability. We label this consistent, robust, and counterintuitive finding *the probing effect*.

The behavioral adaptation explanation (BAE) is regarded by many as the most plausible account for why the probing effect occurs (e.g., Buller et al., 1989; Buller, Strzyzewski, & Comstock, 1991; Buller, Stiff, & Burgoon, 1996; Burgoon & Buller, 1996; Burgoon, Buller, Ebesu, & Rockwell, 1994; Burgoon & Floyd, 2000; Miller & Stiff, 1993; Stiff & Miller, 1986). The BAE proposes that the reason probed sources are judged as more truthful than nonprobed sources is that probed sources adapt their postprobe behaviors to appear "honest." Specifically, the BAE posits a three-link causal chain: (a) the probing of a source by a message recipient causes the source to believe that the recipient may be suspicious, (b) the source's recognition of recipient suspicion causes the source to alter his or her behaviors to appear more truthful, consequently (c) the source's truthful-appearing, adapted postprobe behaviors cause the message recipient to judge the source as honest (relative to nonprobed sources). Hence, probing (albeit indirectly) leads message recipients to attribute truthfulness to message sources.

Although several studies have examined probing (e.g., Buller, Comstock, Aune, & Strzyzewski, 1989; Buller, Strzyzewski, & Comstock, 1991; Stiff & Miller, 1986), the BAE has garnered little direct empirical support. The source behavioral changes that occur subsequent to probing typically are small in effect size, infrequent in occurrence, inconsistent across studies, and most often not in the directions predicted by the BAE (Levine & McCornack, 1996a, 1996b). Consequently, the probing effect remains a robust finding that must be judged to be without a credible explanation.

The goal of the current project is to test three different explanations for the probing effect. Our first study tests both the BAE and a confidence explanation derived from McCornack and Parks's (1986) research on relational deception. Our second and third studies test a third possibility, the existence of a probing heuristic.

## Probing Research

Probing has been defined and operationalized as the direct questioning of a message source regarding the veracity of information presented, or requesting that additional information be presented. Weiler and Weinstein (1972) examined the effects of supportive and suspicious probes on the type and frequency of credibility-enhancing statements used during a simulated job interview. The use of credibility-enhancing statements did not differ significantly between probing conditions.

Stiff and Miller (1986) probed subjects either in a fashion that supported belief in their veracity, or a fashion that questioned their veracity. Subjects' responses were videotaped and coded for 10 nonverbal behaviors. Positive and negative probes were edited from the audio-track of the videotapes and replaced with neutral probes. A different sample of subjects subsequently viewed the videotaped interrogations and were asked to rate the honesty of the videotaped, interrogated subjects' responses. Although probing did not enhance detection accuracy, sources who originally had been subjected to negative probes were judged as slightly more truthful than those who had been interrogated with positive probes ( $r = .14$ ).

Stiff and Miller (1986) then correlated each of 10 coded source nonverbal behaviors with the type of probe that had been used, and with veracity judgments. Four of the 10 source behaviors correlated significantly with honesty judgments. None of the probing-behavior correlations were statistically significant. For 8 of the 10 coded behaviors, however, the sign of the probing-behavior effect was in the direction opposite from the behavior-veracity judgment correlation. Thus, the same behaviors apparently caused by probing were apparently associated with truthful attributions. Based upon these findings, the results of Kraut and Poe (1980), and research on impression management (e.g., Silverman, Rivera, & Tedeschi, 1979), Stiff and Miller speculated that communicators faced with negative probes may realize that message receivers are suspicious, and alter their behaviors accordingly in order to appear truthful.

Buller et al. (1989) extended upon the Stiff and Miller (1986) study by having respondents interact face-to-face, use probing during these interactions, and then make veracity judgments regarding their conversational partners. Probing caused sources to encode more speech errors, talk longer, and pause more. Also, individuals who had probed sources were more likely to rate the sources as honest than individuals who had not probed ( $r = .22$ ). Buller et al. (1989) interpreted their results as consistent with the Stiff-Miller (1986) study and the BAE. They argued that probing causes both deceivers and truth-tellers to present cues indicative of truthfulness. These cues, in turn, purportedly caused receivers to attribute honesty to sources whom they probed, because these sources encoded

behaviors linked to stereotypes of truth-tellers (see Buller et al., 1989, p. 167).

Similar results and interpretations were provided by Buller, Strzyzewski, and Comstock (1991). They coded 25 nonverbal behaviors, and found 6 significant main effects for probing on source behaviors. Probing resulted in longer turns and response latencies, and more pausing, speech errors, gestures, and laughing. Similar to Buller et al. (1989), truth judgments increased following probing ( $r = .57$ ). Buller, Strzyzewski, and Comstock (1991) also reported the only test to date of the first two links in the causal model specified by the BAE (i.e., probing leads to increases in source perceived suspicion, and perceived suspicion produces source behavioral adaptation). The effect for the first link was moderated by probe type. Coded probe skepticism was positively associated with perceived suspicion, but the effect for the probe manipulation was found to be nonsignificant. In examining the source-perceived-suspicion-leads-to-source-behavioral-adaptation link, 6 behaviors were found to be associated with perceived suspicion: illustrators ( $r = -.17$ ), body activity ( $r = -.13$ ), turn length ( $r = -.14$ ), laughing ( $r = -.19$ ), head shaking ( $r = .18$ ), and response latencies ( $r = .17$ ). Based upon these results, Buller et al. concluded that "as anticipated, perceptions of suspicion caused sources to manage their behavior" (p. 20), and "deceivers monitored receivers' reactions to determine whether their deceit was succeeding and, when suspicion was detected, they altered their behavior to appear truthful" (p. 18).

Most recently, Vrij (1995) investigated the effects of probing on nonverbal behaviors in a simulated police interview. Vrij reported that relative to baseline behaviors, probed subjects engaged in fewer head, trunk, and leg movements; smiled less; gazed more; and had more changes in voice pitch.

### Probing and Behavioral Adaptation

Close examination of previous findings indicates that data fail to provide compelling support for the BAE. If, when probed, both deceivers and truth-tellers strategically adapt their behaviors to appear more truthful, then the behaviors that change should be those that research-naive individuals stereotypically associate with deceptiveness. In order for this behavioral adaptation to account for the increase in honesty judgments that follows probing, however, the behavioral cues that are altered must include those that influence receiver truth-deception judgments. Hence, for the BAE to account for the probing effect, sources must change some of those behaviors that are both stereotypically deceptive and decoded-as-deceptive (Levine & McCornack, 1996a). Zuckerman, DePaulo, and Rosenthal's (1981) meta-analysis found that although the behaviors

stereotypically linked with deception correlate moderately ( $r = .44$ ) with those linked to judgments of deception, six of the stereotypical deception cues also cause deceptiveness judgments to increase (i.e., gaze, posture shifts, response latencies, speech errors, hesitations, pitch). Consequently if the BAE is a viable explanation for the probing effect, sources who are strategically adapting their behavior to appear honest and who also will be perceived as honest should use longer gazes, fewer posture shifts, shorter response latencies, fewer speech errors and hesitations, and a lower pitch.

The six behaviors specified by Zuckerman et al. (1981) related to both stereotypes about honesty and judgments of honesty are listed in Table 1, along with the behaviors found to change as a result of probing. As Table 1 displays, six of the nine behaviors found to vary as a function of probing vary in a fashion indicative of source deceptiveness rather than source honesty. Thus, two thirds of the cues purportedly indicative of sources' strategic behavioral adaptation are in the direction opposite of that predicted by the BAE.

To summarize, the BAE suggests a causal model in which probing leads message sources to perceive recipient suspicion, this perceived suspicion causes sources to adapt their behaviors to appear truthful, and this behavioral adaptation dupes message recipients. The evidence for this model is, at best, mixed. Research has not documented a significant relationship between probing and perceived suspicion (Buller, Strzyzewski, & Comstock, 1991), and the relationships between probing and behavioral changes have been nonsignificant (Stiff & Miller, 1986; Weiler & Weinstein, 1972), in the wrong direction (Buller et al., 1989; Buller, Strzyzewski, & Comstock, 1991), or generally consistent with the BAE (Vrij, 1995). The majority of behaviors that do change should lead to judgments of source deceptiveness, rather than the judgments of source honesty repeatedly observed by past researchers. Nevertheless, authors continue to cite the BAE as established fact (e.g., Burgoon & Floyd, 2000, pp. 245–246). Because only one in five studies have actually produced data consistent with the BAE, a search for an alternative explanation for the probing effect appears warranted.

### Confidence

One alternative explanation for the probing effect can be derived from the McCornack and Parks (1986) model of relational deception. McCornack and Parks found that as relationship involvement increases, people become increasingly confident in their ability to detect their partners' duplicities. These increases in confidence generate a "truth-bias," or tendency to judge all messages as truthful. Given this, confidence trig-

TABLE 1  
Cues Stereotypically Related to Truth-Deception, Decoded as Truth-Deception, and Produced by Probing

Cue	Stereotypes and truth judgments		Effects of probing			Effects of perceived suspicion
	Zuckerman et al., 1991	Stiff & Miller, 1986 <sup>a</sup>	Buller et al., 1989 <sup>b</sup>	Buller et al., 1991 <sup>c</sup>	Vrij, 1995 <sup>e</sup>	Buller et al., 1991 <sup>d</sup>
Gaze	+		<i>ns</i>	<i>nr</i>	+	<i>nr</i>
Posture shift	-	<i>ns</i>	<i>ns</i>	<i>nr</i>	-	<i>nr</i>
Response latency	-	<i>ns</i>	<i>ns</i>	+		+
Speech error	-	<i>ns</i>	+	+	<i>ns</i>	<i>nr</i>
Speech hesitation	-	<i>ns</i>	+	+		<i>nr</i>
Pitch	-				-	

NOTE: + indicates a positive relationship, - indicates a negative relationship; *ns* = nonsignificant main effects, *nr* = investigated but main effects not reported.

<sup>a</sup>N = 40.

<sup>b</sup>N = 148.

<sup>c</sup>N = 210.

<sup>d</sup>N = 210.

<sup>e</sup>N = 64.

gered by probing can be advanced as a potential explanatory mechanism underlying the probing effect.

Although researchers have not yet tested the relationship between probing and confidence, it seems plausible that such a link exists. Just as relational development produces confidence, which in turn produces a truth-bias, probing too may lead to increased confidence, which in turn produces a truth-bias. As receivers probe potential liars, they should gain more information upon which to base truth-lie judgments. As a function of having more information, and a belief in the efficacy of probing as a detection strategy, receivers should become more confident in their ability to make accurate judgments; confidence that ultimately results in truth-bias (DePaulo, Charlton, Cooper, Lindsay, & Muhlenbruck, 1997; McCornack & Parks, 1986; Levine & McCornack, 1992).

A model based upon confidence also provides an explanation for why probing appears to have little effect upon judgmental accuracy. Early probing researchers (e.g., Stiff & Miller, 1986) predicted that probing a source would provide a receiver with more information resulting in increased detection accuracy. This reasoning renders the repeated finding that probing does not increase accuracy counterintuitive. If, however, the accuracy boost generated by the information gained through probing is offset by a processing bias stemming from confidence, one would expect not to find an effect for probing upon accuracy. Consistent with this reasoning, research suggests that confidence and accuracy are unrelated (DePaulo et al., 1997; Vrij, 2000).

## STUDY 1

Previous researchers who have examined interrogative probing have argued that the increase in honesty ratings that occurs subsequent to probing stems from behavioral changes on the part of message sources. If behavioral adaptation causes the probing effect, one should find that when source behaviors are held constant, there will be no significant difference between sources who are probed and sources who are not probed in ratings of truthfulness. If, however, the line of reasoning regarding the confidence explanation is valid, behavioral adaptation on the part of the source should not be required to observe the probing effect. Thus, we expect to find that when source behaviors are held constant, sources who are probed will be rated as significantly more truthful than sources who are not probed.

Although data consistent with this hypothesis would be consistent with the confidence explanation, this would provide only indirect support for the confidence explanation. In order to provide a direct test of

the role of confidence, one would need to assess the effect of probing upon confidence, and the effect of confidence upon truth judgments. To the extent that the confidence explanation provides a viable account for the probing effect, we should find that individuals will be more confident in their judgments of sources who are probed than sources who are not probed, and confidence will be positively associated with judgments of honesty.

The type of probe that is used also may have an effect upon confidence. Stiff and Miller (1986) found a small but significant effect for the valence of probes, but did not have a no-probe control group. Later studies using only probe and no-probe groups found substantially larger effect sizes (Buller et al., 1989; Buller, Strzyzewski, & Comstock, 1991). Negative probes often imply suspicion and disbelief (Buller et al., 1989; Stiff & Miller, 1986). Given that suspicion may result in less confidence (Fein, Hilton, & Miller, 1990; Levine & McCornack, 1992; Stiff, Kim, & Ramesh, 1992), the use of negative probes may be significantly related to decreases in confidence. On the other hand, face-threatening, pointed probing is commonly believed to be more likely to trap deceivers (Buller et al., 1989). Thus, the use of negative probes may actually function to enhance confidence, by enhancing individuals' feelings that they indeed have "hit sources with their best shot." To explore this issue, our study tested if the type of probe used (i.e., positive-negative) influences the strength of the probing effect.

## Method

Study 1 occurred in several stages. First, respondents were videotaped discussing truthful and deceptive answers to an attitude questionnaire that they had completed. The probes were altered or removed from the tape to form the stimuli for four probing conditions in which source behaviors remained constant (i.e., positive, negative, neutral, and no probe). Next, the four tapes were pretested for realism. In the final stage, respondents each viewed one of the four tapes, and made judgments about the truthfulness of the sources on these tapes. All participants received extra credit in exchange for their participation.

*Phase 1: Interviews.* Seventeen undergraduate students (11 females, 6 males) from a large Midwestern (U.S.) university participated on a voluntary basis. Upon arriving at the experimental site, each respondent was told that the purpose of the experiment was to "examine the perceptions that people have of the communication behaviors of others." Respondents were then asked to complete a four-item questionnaire. Similar to other deception studies (e.g., McCornack & Parks, 1986), the questionnaire contained randomly selected items from the Mach IV scale



(Christie & Geis, 1970). Responses to these items were recorded on 10-point Likert-type scales.<sup>1</sup>

After completing the four items, an experimental assistant collected the questionnaire and changed two of the respondent's answers to answers that were five points different (i.e., half of the scale length) from the respondent's original true answers. Six different orderings of the items to be changed were used to minimize order effects.

The respondent was then informed that he or she would be interviewed regarding his or her answers on the altered questionnaire. The respondent was told to report his or her original answer for the two unchanged items during the interview. For the remaining two items, the respondent was told to report the answer that was changed (half the scale length different from his or her true answer). Each respondent was given 10 minutes to prepare his or her responses.

At the end of this time, an experimenter entered the room and sat facing the respondent. Each respondent was asked to state his or her attitude on each of the items and to briefly explain his or her answer. The experimenter was not informed as to which items were truthful (i.e., unchanged) and which items were lies (i.e., changed). This interview was videotaped from behind a one-way mirror. The camera was placed so that the respondent's entire body would appear, while the experimenter could not be seen.

During the interview, each respondent reported his or her answer and then briefly discussed reasons for his or her choice of that answer. Following each answer, the experimenter probed the respondent with a neutral probe, saying "tell me a little bit more about why you answered it that way." Following this probe, each respondent elaborated briefly upon his or her answer. At the conclusion of the interview, each respondent was debriefed and thanked for his or her participation.

*Phase 2: Creating experimental conditions.* From the original questionnaire protocol, one item was randomly selected as the stimulus item for the final test tapes. A single item was selected as the basis for all responses on the tapes in order to minimize potential effects due to variation in item content. Eight videotaped respondents were then randomly selected from the original sample of 17 interviewees with the constraint that there were 2 males who had lied, 2 males who had told the truth, 2 female liars, and 2 female truth-tellers. A master tape was then created, with each of these eight interviews (regarding respondents' answers on item one). The order of the eight interviews on the tape was randomly determined.

Following the creation of the "master" tape, three additional "stimulus" tapes were created. In the negative-probe stimulus tape, the audio-track of the tape was altered, so that the experimenter's original "neu-

tral" probe was replaced with a negative, face-threatening probe. The experimenter said, "I don't think you really circled that. Tell me a little more about why you answered it that way." In the positive-probe stimulus tape, the experimenter's original probe was replaced with a positive, face-supportive probe. The positive probe was "that sounds reasonable, but tell me a little more about why you answered it that way." In the no-probe control stimulus tape, the experimenter's probe was deleted and the tape spliced, so that each respondent's answer to the item and his or her explanation of the answer were joined together. This resulted in four tapes each corresponding to a probing condition: no probe, neutral probe (i.e., the master tape), positive probe, and negative probe.

*Phase 3: Pretesting of stimulus tapes.* The final step (prior to conducting the primary study) was to test each of the tapes for realism. This was to ensure that, despite the editing, the three altered tapes would not be perceived by naive observers as significantly different from the master tape (i.e., the original, unedited videotape of the eight respondents discussing item one, with the experimenter using neutral probes).

One hundred and thirty-four undergraduates (76 women and 58 men) from a large Midwestern university participated on a voluntary basis outside of class time. Respondents were informed that the purpose of the experiment was to "examine their perceptions of the communication behaviors of others." Respondents were randomly assigned to view one of the four videotapes (i.e., one of the three edited tapes or the master tape). After viewing the tape, respondents completed a six-item questionnaire designed by the experimenters to measure the realism of the test tapes. The questionnaire used Likert-type items with seven-point response formats. A sample item read "the interviews on this videotape seemed natural."

The internal consistency of the realism scale was tested using confirmatory factor analysis. The scale was found to be reliable ( $\alpha = .91$ ) and consistent with a unidimensional measurement model. The six items were then summed to create an overall index of realism for each respondent.

A one-way ANOVA was then conducted, with the four different test tapes as the independent variable and perceived realism as the dependent measure. The ANOVA found a marginally significant, and small difference between the four test tapes in terms of realism  $F(3, 130) = 2.64$ ,  $p = .052$ ,  $\eta^2 = .02$ ,  $r = .14$ ).

Upon examination of the means, it became apparent that the negative probe tape ( $M = 32.56$ ) seemed to be the cause of the difference (means in the other three cells ranged from 35.45 to 36.57). Given that the means for all of the tapes were over 32 (i.e., potential range for realism being 6 to 42), it is obvious that each of the test tapes was seen as fairly realistic

by the respondents in this sample. Based upon this, we opted to retain test tape 2 in the primary project.

*Primary study.* Three-hundred and thirty-seven participants were solicited on a voluntary basis from undergraduate classes at a large Midwestern university. Respondents included 129 men and 208 women.

*Procedures.* Each experimental session involved having a group of five to seven participants view one of the four test tapes. Upon arriving at the experimental site, each group was told that the purpose of the study was to investigate "how individuals perceive the communication behaviors of others." Each group was then randomly assigned to view one of the four probing tapes previously generated (i.e., neutral probe master tape, negative probe, positive probe, no probe). Fifty-seven respondents viewed the neutral probe tape, 100 respondents viewed the negative probe tape, 95 respondents viewed the positive probe tape, and 85 respondents viewed the no-probe tape. After viewing each of the eight videotaped interviews on the test tape to which they had been assigned, respondents were asked to rate their perceptions of the source's honesty on a single dichotomous "truth-lie" item. Subjects also were asked to rate how confident they were in their judgment on a 10-point scale. Perception of source honesty was computed as the proportion of truthfulness judgments to total judgments across the eight cases (0–100% honesty).

## Results

A one-way ANOVA was used to test the effects of probing on truth judgments (while holding source behaviors constant), with the test tape as the independent variable and perceptions of honesty as the dependent measure.<sup>2</sup> A set of contrast weights (+1, +1, +1, -3) were assigned a priori to reflect higher percentages of truth judgments in each of the three probing conditions than in the no-probe condition. The data were consistent with this hypothesis,  $t(325) = 3.61, p < .0001, r = .20$ .

We also tested for possible differences in the percentage of truth judgments between positive, negative, and neutral probe conditions. Results of the one-way ANOVA suggested a significant effect for probing condition on truth judgments,  $F(3, 325) = 4.67, p < .003, \eta^2 = .04$  (see Table 2 for means). Post hoc tests of between-group differences with the Tukey B procedure at  $p < .05$  found no significant differences between any of the tapes involving a probe.<sup>3</sup> Alternatively, planned  $t$ -tests showed that the mean in the no-probe condition was significantly less than each of the means in the three probing conditions (neutral,  $t[134] = 3.29, p < .001, r = .27$ ; negative,  $t[176] = 2.34, p < .02, r = .17$ ; positive,  $t[173] = 3.22, p < .002, r = .24$ ).

**TABLE 2**  
**Mean Honesty Judgments, Accuracy, and Confidence by Probing Condition in Study 1**

Dependent variable	Condition			
	Neutral	Negative	Positive	No probe
Truth judgments*	0.76	0.72	0.74	0.66
Accuracy	0.49	0.54	0.54	0.54
Confidence	59.8	57.6	59.9	57.3

\*ANOVA significant at  $p < .05$ .

Previous studies have found that the probing effect held for both truthful and deceptive messages. These findings were replicated; probing did not interact with message veracity,  $F(3, 325) = 1.37, p = ns, \eta^2 = .00$ . Also consistent with previous research, probing was not related to detection accuracy,  $F(3, 325) = 1.37, p = ns, \eta^2 = .00$ . Subjects in the neutral probe condition judged 49% of the messages correctly, whereas accuracy in the other three conditions was 54%.

We also predicted higher ratings of judgmental confidence in each of the three probing conditions than in the no-probe condition. This was tested with a similar one-way ANOVA using the same set of a priori contrast weights. The data were not consistent with this hypothesis,  $t(326) = 1.65, p = .10, r = .09$ . Cell means for truth judgments, accuracy, and confidence are presented in Table 2.

A final hypothesis predicted a positive correlation between confidence and the percentage of messages judged as truthful. The data were consistent with this hypothesis,  $r(328) = .30, p < .001$ . Further analysis, however, suggested that this effect was moderated by probing condition. Although the correlation between confidence and truth judgments was positive in all four experimental conditions, the magnitude of the correlations varied significantly across some of the conditions (see Table 3). Thus, interpreting these data as offering support for the third hypothesis must be tempered by the finding of a significant magnitude interaction between probing condition and confidence on truth judgments.

## Discussion

The goal of Study 1 was to examine a potential explanation for the repeated finding that interrogative probing increases perceptions of source honesty. Although previous researchers have argued that this increase stems from changes in sources' post-probe behaviors, results from

**TABLE 3**  
**Correlations Between Confidence and Truth-Bias in Study 1**

	<i>Condition</i>				
	<i>Neutral</i>	<i>Negative</i>	<i>Positive</i>	<i>No probe</i>	<i>Across conditions</i>
<i>r</i>	.40 <sub>a</sub>	.12 <sub>ab</sub>	.44 <sub>bc</sub>	.22 <sub>c</sub>	.30
<i>df</i>	54	96	95	80	328
<i>p</i> <	.001	.12	.0001	.03	.0001

NOTE: Correlations with the same subscript differ at  $p < .05$ .

Study 1 cast further doubt upon this explanation. Sources who were probed were perceived as significantly more honest than sources who were not probed, despite the fact that their behaviors across conditions were identical. This suggests that behavioral adaptation is not a necessary condition for observing the probing effect.

A confidence explanation derived from McCornack and Parks's (1986) model of relational deception was advanced as a plausible alternative to the behavioral adaptation model. The confidence explanation posited that confidence mediates the relationship between probing and truth judgments. The data clearly were inconsistent with this alternative. The predicted effects for probing upon confidence were nonsignificant. Moreover, the predicted effect of confidence upon truth judgments was moderated by probing condition. Thus, although confidence appears to play a role in the probing effect, it does not appear to form the basis for the probing effect.

Counter to arguments advanced by Buller, Stiff, and Burgoon (1996), these results also suggest that the very act of probing (rather than the type of probe used) may be the driving force behind the probing effect. In this study, tapes involving a negative probe were not perceived as significantly different (in terms of honesty) from tapes involving positive and neutral probes. Thus, the probing effect appears to be even more robust than previously thought. The probing effect occurs regardless of the type of probe used, regardless of any source behavioral changes produced by the act of probing, and is not dependent upon increases in confidence.

The failure of both the behavioral adaptation explanation and the confidence explanation to account for the data in Study 1 led us to search for a third alternative explanation for the probing effect. Study 2 argues for and provides a test of just such an alternative: a probing heuristic.

## STUDY 2

Deception research suggests that receivers may be decidedly unmotivated to detect deception. Individuals persistently ascribe truth to others' messages (Levine, Park, & McCornack, 1999; Zuckerman et al., 1981), even under conditions of high contextual suspicion (McCornack & Levine, 1990). This "truth-bias" is particularly strong among relational partners (McCornack & Parks, 1986; Levine & McCornack, 1992) and face-to-face conversational participants (Buller, Strzyzewski, & Hunsaker, 1991), and may derive from how people mentally represent information (Gilbert, 1991; Gilbert, Krull, & Malone, 1990).

In addition, research indicates that people are poor deception detectors (DePaulo, 1994; Levine et al., 1999; Zuckerman et al., 1981). Across numerous studies, detection accuracy tends to cluster around chance levels and rarely exceeds 70%. Even individuals in professions requiring frequent attempts at deception detection are not much better than chance at accurately discerning deception (DePaulo & Pfeifer, 1986; Ekman & O'Sullivan, 1991).

Social cognition research suggests that when individuals lack either the ability or motivation to process information, they often rely upon cognitive shortcuts that reduce complex problem solving to more simple judgmental operations (Chaiken, 1987; Cialdini, 1987). Such mindless decision rules have been labeled "heuristics" (Kahneman & Tversky, 1973; Tversky & Kahneman, 1974, 1981). Because of receivers' inability and lack of motivation to detect deception, researchers recently have argued that heuristics may play a major role in influencing receivers' veracity judgments (e.g., Fiedler & Walka, 1993; Levine & McCornack, 1992; O'Sullivan, Ekman, & Friesen, 1988; Stiff et al., 1989; Stiff et al., 1992). Heuristics that are believed to influence veracity judgments include the availability heuristic (O'Sullivan et al., 1988), the falsifiability heuristic (Fiedler & Walka, 1993), the infrequency heuristic (Fiedler & Walka, 1993), the representativeness heuristic (Stiff et al., 1989), and the truth-bias heuristic (Stiff et al., 1992).

When applied to deception, the availability heuristic implies that since people observe honest behavior more frequently than deceptive behavior, judgments of honesty are more available to them (O'Sullivan et al., 1988). Hence, individuals should be more likely to infer honesty than deception, regardless of the veracity of a given message. Similarly, the infrequency heuristic (Fiedler & Walka, 1993) holds that because lies are seldom discovered and are often unexpected, people often assume others are truthful. The falsifiability heuristic (Fiedler & Walka, 1993) involves judgments regarding the degree to which message content potentially is falsifiable: Individuals tend to believe that it is easier to fabricate

false facts than false feelings. Consequently, suspicious factual messages are more likely to be judged as lies than suspicious emotional messages (Fiedler & Walka, 1993). The representativeness heuristic is a decision rule regarding the likelihood that a particular event or behavior is an instance of a certain category of events or behaviors (Kahneman & Tversky, 1973; Tversky & Kahneman, 1974, 1981). In contexts involving deception, "the representativeness heuristic may be reflected in an individual's decision that nervous behavior is probably deceptive behavior" (Stiff et al., 1989, p. 560). The truth-bias heuristic involves the assumption that relational partners always will tell the truth (Levine & McCornack, 1992; Stiff et al., 1992). As Stiff et al. (1992) point out, "the simple decision rule 'my partner has been truthful in the past, therefore he or she is being truthful now' allows partners to make judgments of veracity without scrutinizing message content" (p. 328).

We believe that within contexts that provide for heuristic processing, people who utilize probing in order to detect deception may adopt a particular, peculiar heuristic that we have labeled *the probing heuristic*. The probing heuristic represents a variation on the representativeness and truth-bias heuristics, rooted in naive beliefs regarding deception and cognition. As McCornack (1997) recently has noted, laypersons and scholars alike share the belief that deceptive message generation requires greater cognitive load than truthful message generation. Particularly within contexts in which sources are suddenly and unexpectedly prompted, lying is considered a decidedly taxing cognitive event, so taxing that it will produce significant arousal and arousal-based behavioral leakage (see McCornack, 1997, for a critique of the cognitive load hypothesis). Because probing requires sources to generate answers to sudden, unexpected questions, and because lying is believed to be cognitively demanding, recipients may believe that sources will have grave difficulty spontaneously crafting lies immediately subsequent to probing. This belief in the relative nonfalsifiability of postprobe messages should give rise to a specific variation of the representativeness and truth-bias heuristics: Sources should be less likely to spontaneously generate false messages when confronted with sudden, unexpected questions. Although it represents an amalgam of previously documented heuristics, we believe that the probing heuristic is sufficiently particular so as to merit a unique, novel label.

The observable effect of the probing heuristic would be that within contexts that provide for heuristic processing, the act of probing should significantly increase ratings of source honesty, purely as a function of heuristic processing (i.e., independent from any significant changes in source behaviors). Consequently, the probing effect that previously has been observed may stem from the probing heuristic.

As Stiff et al. (1989; also Chaiken, 1987; Petty & Cacioppo, 1981) note, individuals adopting heuristics typically invoke these rules when they "are unmotivated, unable to process message content, or both" (p. 560). Thus, if the probing heuristic exists, individuals will be more likely to evoke it when they lack the ability or the motivation to carefully scrutinize messages for deception cues. Under conditions of high motivation and high ability, however, individuals should abandon the probing heuristic and actively appraise the veracity of others' messages, the result being a diminished or reversed probing effect.<sup>4</sup>

Study 2 was designed to test this line of reasoning by varying subjects' ability to process information, subjects' motivation to process information, and the occurrence of probing, and observing subsequent frequency of truth judgments. To the extent that probing is associated with reduction in the frequency of truth judgments under conditions that allow active-systematic processing, the existence of a probing heuristic provides a viable account for the probing effect.

### Ability

Consistent with Chaiken (1987) and Petty and Cacioppo (1981), Stiff et al. (1989) suggested that a person's "ability" to process a message is determined (in large part) by that individual's familiarity with information related to the context in which the deceptive event occurs. One type of "familiarity" relevant to deception contexts is receiver familiarity with the information presented by the source. Because people are able to process messages involving familiar information more actively than those involving unfamiliar information, individuals faced with messages involving unfamiliar information should be more likely than those confronted with familiar information to rely upon heuristics in rendering veracity judgments.

### Motivation

The work of Levine and McCornack (1991) suggests that "motivation" to detect deception is influenced by the degree of generalized communicative suspicion (GCS) possessed by individuals. GCS involves a predisposition toward believing that others frequently lie during conversations. Because people scoring high in GCS believe that others frequently attempt to deceive them, they tend to be more sensitive to situational information related to deceit (Levine & McCornack, 1991). Consequently, they also tend to be motivated to identify deception (McCornack & Levine, 1990).



## Hypothesis

The heuristic processing explanation for the probing effect predicts that if receivers are either unable or unwilling to systematically process messages, the act of probing will lead to an increase in ratings of source truthfulness (through the adoption of the probing heuristic). If, however, receivers are both motivated and able to scrutinize messages, then probing should have no effect. To the extent that being familiar with message content increases individuals' ability to process messages, and individuals high in GCS are more motivated than low GCS individuals to detect deception, the probing heuristic explanation would predict a three-way interaction between probing, familiarity, and GCS on the percentage of messages judged as truthful, such that sources who are probed will be perceived as more truthful than sources who are not probed when judged by low GCS individuals or by individuals who are not familiar with message content. Probing, however, will have no effect for high GCS individuals who are familiar with the message content.

## Method

Similar to Study 1, Study 2 occurred in several stages. First, respondents were videotaped discussing their answers to an attitude questionnaire that they had completed. From these interviews, two tapes were created. Next, the two tapes were pretested for realism. From these original two tapes, four test tapes were then constructed. In the final stage, respondents viewed these tapes, and made judgments about the truthfulness of the sources displayed on the tapes. Probing and respondent familiarity with message content was varied, and GCS was assessed.

*Phase 1: Interviews.* Four undergraduate communication majors (two females, two males) from a large Midwestern university participated on a voluntary basis. Upon arriving at the experimental site, each respondent was told that the purpose of the experiment was to "examine the perceptions that people have of the communication behaviors of others." Respondents were then asked to complete an eight-item questionnaire. Unlike Study 1, the questionnaire contained items regarding issues thought to be "controversial" on college campuses. Four of the items involved controversies specific to the campus at which the experiment was being conducted, whereas the remaining four items dealt with issues specific to a different campus.<sup>5</sup> Similar to Study 1, responses to these items were recorded on 10-point Likert-type scales. After completing the eight items, an experimental assistant collected the questionnaire, and

changed four of the respondent's answers to answers which were five points different (i.e., half of the scale length) from the respondent's original, true, answers. Four different orderings of the changed items were used to minimize order effects.

Each respondent was then informed that he or she would be interviewed regarding his or her answers on the altered questionnaire. Each respondent was told to report his or her original answer for the four unchanged items during the interview. For the remaining four items, each respondent was told to report the answer that was altered by the assistant. Each respondent was given 10 minutes to prepare his or her responses. At the end of this time, the experimenter entered the room and sat facing the respondent. Each respondent was asked to state his or her attitude on each of the items and to briefly explain his or her answer. The experimenter was not informed as to which items were truthful (i.e., unchanged) and which items were false (i.e., changed). This interview was videotaped. The camera was placed so that the respondent's entire body would appear, while the experimenter could not be seen. The camera was placed behind a one-way mirror.

During the interview, each respondent reported his or her answer and then briefly discussed reasons for his or her choice of that answer. Following each answer, the experimenter probed the respondent with a neutral probe, saying "tell me a little bit more about why you answered it that way." Following this probe, each respondent elaborated briefly upon his or her answer.

*Phase 2: Pretesting tapes for realism.* Two of the videotaped respondents (one male source and one female source) were selected randomly from the original sample of four. Two separate tapes were then created, one "probe" and one "no-probe." In the probe tape, the taped interviews with the two sources were combined, such that the male source presented his answers to each of the eight items (i.e., two familiar-true, two familiar-false, two unfamiliar-true, and two unfamiliar-false), followed by the female source discussing each of her answers. Experimenter probes were not deleted. For the no-probe tape, the tapes of the two sources were combined, and the audio-track was altered, so that (similar to the procedure employed in Study 1) the experimenter's original probes were deleted, and each source's responses to the items and his or her explanation of the answers were joined together.

The two tapes were then compared in terms of realism. This was to ensure that, despite the editing, the no-probe tape would be perceived by naive observers as not significantly different from the probe tape (i.e., the original, unedited videotape of the two sources discussing each of their eight responses).

Sixty-one undergraduates (48 women and 13 men) from a large Midwestern university participated on a voluntary basis outside of class time. Respondents were informed that the purpose of the experiment was to "examine their perceptions of the communication behaviors of others." Respondents were randomly assigned to view one of the two videotapes (i.e., master probe, master no-probe). After viewing the tape, respondents completed the same six-item questionnaire used to measure realism in Study 1.

The internal consistency of the realism scale was again tested using confirmatory factor analysis. The scale was found to be highly reliable ( $\alpha = .94$ ) and consistent with a unidimensional measurement model. The six items then were summed to create an overall index of realism for each respondent.

A *t*-test was then conducted, with the two different tapes as the independent variable and perceived realism as the dependent measure. The *t*-test found no significant difference between the master probe tape ( $M = 30.00$ ,  $SD = 7.68$ ) and the master no-probe tape ( $M = 29.67$ ,  $SD = 9.11$ ) in perceived realism  $t(1, 59) = .16$ ,  $p = ns$ ,  $r = .02$ .

*Creation of test tapes.* Following analysis of tape realism, four different test tapes were created: probe/familiar, probe/unfamiliar, no-probe/familiar, no-probe/unfamiliar. Both tapes involving probes were constructed from the interviews on the probe tape, whereas both tapes without probes were constructed from segments on the no-probe tape. In the probe/familiar tape, the four items (for each source) involving familiar topics were preserved, and the items involving off-campus information were deleted. Thus, the tape was comprised of eight responses total: four familiar items discussed by the female source (two truthful answers, two false answers), and four familiar items discussed by the male source (two truthful, two false). A similar procedure was then followed in the construction of each of the remaining three tapes, so that each of the final four test tapes involved the discussion of either eight familiar or eight unfamiliar items (four discussed by the female source, four discussed by the male source), and the presence or absence of experimenter probing.

*Primary Study.* One-hundred and thirty-six respondents were solicited on a voluntary basis from undergraduate communication classes at a large Midwestern university. Respondents included 80 women and 56 men.

*Procedures.* Each experimental session involved having a group of five to seven respondents view one of the four test tapes. Upon arriving at the experimental site, each group was told that the purpose of the study

was to investigate "how individuals perceive the communication behaviors of others." Each group then was randomly assigned to view one of the four test tapes previously generated (i.e., probe/familiar, probe/unfamiliar, no-probe/familiar, no-probe/unfamiliar). Thirty-nine respondents viewed the probe/familiar tape, 40 respondents viewed the probe/unfamiliar tape, 27 respondents viewed the no-probe/familiar tape, and 30 respondents viewed the no-probe/unfamiliar tape.

After viewing each of the eight videotaped interviews on the test tape to which they had been assigned, the tape was stopped, and respondents were asked to indicate if they thought that the individual on the tape was "lying" or "completely truthful." Perception of source honesty was computed as the proportion of truthfulness judgments to total judgments across the eight cases (0–100% honesty). Following completion of the eighth judgment, respondents were asked to complete the familiarity manipulation-check items and the GCS scale. The effectiveness of the manipulation of familiarity was tested using a four-item measure designed by the experimenters. Sample items included "I was familiar with the issues that the subjects on this videotape discussed" and "I had no prior knowledge of the topics that were discussed by the subjects on this videotape." GCS was measured using the 12-item GCS scale developed and validated by Levine and McCornack (1991). The GCS scale was designed to measure the general tendency to question the veracity of others' messages. Sample items include "I often feel as if people aren't being completely truthful with me" and "people seldom lie to me" (reflected item). Although the GCS scale has been found to be reasonably reliable ( $.70 < \alpha < .85$ ) and exhibits construct and predictive validity, its factor structure is somewhat unstable (Levine & McCornack, 1991).

Prior to their use, the GCS and manipulation-check items were tested for unidimensionality via confirmatory factor analysis. The procedure resulted in the elimination of three GCS items. The nine remaining items were summed as a measure of GCS ( $M = 29.79$ ,  $SD = 8.45$ ,  $\alpha = .83$ ). All four manipulation-check items were retained ( $M = 18.73$ ,  $SD = 7.94$ ,  $\alpha = .93$ ). The distribution of scores on the GCS scale approximated normality.

## Results

*Manipulation and validity check.* A one-way ANOVA was used to assess the quality of the familiarity manipulation. Subjects rated the content of the messages as significantly more familiar ( $M = 25.81$ ) in the two conditions involving discussion of familiar items than the two conditions involving unfamiliar items,  $M = 12.64$ ;  $F(1, 134) = 225.92$ ,  $p < .0001$ ,  $\eta^2 = .63$ ,  $r = .79$ , suggesting that the manipulation was effective. An examination of the distribution of scores, however, showed that the manipulation was more effective in the familiar condition than the unfamiliar condition.

**TABLE 4**  
**Correlations Among Familiarity, Truth-Bias, and Accuracy in Study 2**

<i>Across conditions</i>	<i>Familiarity manipulation</i>	<i>Manipulation check</i>	<i>Truth-bias</i>
Manipulation check	+ .79**		
Truth-bias	-.11	-.06	
Accuracy	+ .54**	+ .41**	-.32**
<i>Probing, high GCS cell</i>			
Manipulation check	+ .93**		
Truth-bias	-.41*	-.54**	
Accuracy	+ .67**	+ .67**	-.67**

NOTE: Overall  $df = 134$ , cell  $df = 17$ .

\* $p < .05$ .

\*\* $p < .01$ .

Every subject in the familiar condition reported familiarity above the midpoint on the check scale and the mean was only 2 scale points below the maximum value possible. In the unfamiliar condition, however, the mean was only 3 points below the midpoint. Thirty percent of the subjects ( $N = 21$ ) in the unfamiliar condition score reported being familiar with the issues (i.e., a score above the midpoint). These subjects were excluded from the primary analyses.<sup>6</sup>

Correlations among the familiarity manipulation, the manipulation-check scale, the number of truth judgments, and accuracy provided an additional validity check. If familiar receivers were more able to process the messages for veracity, the manipulation and manipulation-check scale should be positively correlated with detection accuracy and parallel. The data were consistent with this reasoning; manipulated familiarity,  $r(134) = .54$ ,  $p < .001$ ; reported familiarity,  $r(134) = .41$ ,  $p < .001$ . This provides strong evidence that familiar subjects were better able to systematically assess message veracity.

A potential confound was that sources might have been able to lie more convincingly regarding familiar topics. If this was so, we would expect positive correlations between familiarity and the number of truth judgments. The data were not consistent with this speculation; manipulated familiarity,  $r(134) = -.11$ ,  $p = ns$ ; reported familiarity,  $r(134) = -.06$ ,  $p = ns$ . Instead, as expected, the effects for familiarity on truth judgments were confined to the critical probing, high GCS cell; manipulated familiarity,  $r(15) = -.41$ ,  $p < .001$ ; reported familiarity,  $r(15) = -.54$ ,  $p < .001$  (see Table 4). These results suggest that familiarity did in fact lead to increased ability and that potential confounding due to differential source behav-

iors was unlikely. Correlations among familiarity, truth-bias, and accuracy are presented in Table 4.

*Test of the probing heuristic.* Because one of the independent variables (i.e., GCS) was continuous, the hypothesis was initially tested using regression analyses. Possible two-way interactions and the predicted three-way interaction were modeled with product terms (i.e., multiplying raw scores). The percentage of truth judgments to total judgments made by each subject (i.e., truth-bias) served as the dependent measure.

The triple product reflecting the three-way interaction between probing, familiarity, and GCS was significantly related to the number of truth judgments made,  $\beta = -.20$ ,  $F(1,134) = 5.70$ ,  $p < .01$ . None of the main effects or two-way interactions were statistically significant. Moreover, the full regression model including the three main effects, the three two-way interactions, and the three-way interaction did not account for more variance than did the three-way interaction alone (difference in  $R^2 = .016$ ,  $F(6, 128) = 1.57$ ,  $p = ns$ ).

As product terms provide relatively little information regarding the specific form of the three-way interaction, the specific nature of the interaction was investigated with zero-order correlations and Fisher's  $r$  to  $z$  transformations. GCS was dichotomized with a midpoint split. Separate probing-truth-bias correlations were calculated in each of the four quasi-experimental conditions. Contrary to our expectations, probing was not significantly associated with the number of truth judgments made in either of the low GCS conditions (unfamiliar,  $r[37] = -.01$ ,  $p = ns$ , familiar  $r[48] = -.02$ ,  $p = ns$ ). In the high GCS/unfamiliar cell, probing was positively, but not significantly, related to truth-bias;  $r(8) = +.18$ ,  $p = ns$ . In the high GCS/familiar cell in which heuristic processing should be abandoned, probing was significantly, substantially, and negatively related to truth-bias;  $r(14) = -.61$ ,  $p < .006$ . This correlation differed significantly from the correlations in the other three conditions ( $z = 2.02$ ,  $2.05$ , and  $1.67$ , respectively).

It is possible that the unanticipated negative effect for probing on truth-bias was an artifact of the increased accuracy in the familiar condition. Previous research has found a negative association between truth-bias and accuracy (DePaulo et al., 1997; McCornack & Parks, 1986; Levine & McCornack, 1992). Although it is usually assumed that truth-bias lowers accuracy, accuracy could lower truth-bias in the current design. Because there was a fixed distribution of truths and lies, major gains in the ability to accurately detect would necessitate a downward shift toward the ratio of truth judgments to total judgments toward the objective .50 in the stimulus materials. The data are consistent with this reasoning. As reported in Table 4, familiarity increased accuracy, accuracy and truth-bias

were negatively related, but familiarity did not directly affect truth-bias. To see if the accuracy gain resulting from the familiarity manipulation could account for the probing results, the regression analyses were re-done with accuracy included as a predictor. In this analysis, the probing main effect, all two-ways involving probing, and the previously significant three-way interaction all were  $F < 1.00$ . Thus, the negative effect of probing is likely a spurious effect attributable to accuracy and familiarity.

As in previous studies, probing was unrelated to detection accuracy,  $F(1, 128) = 0.60, p = ns, \eta^2 = .00$  (no-probe 59%, probe 57%). The only factor to affect accuracy in Study 2 was familiarity. Subjects in the familiar conditions ( $M = 69\%$ ) were significantly more accurate than in the unfamiliar conditions ( $M = 48\%$ );  $F(1, 128) = 55.46, p < .001, \eta^2 = .29, r = .54$ .

## Discussion

Study 1 found that the probing effect occurs even when message source behavioral variation is controlled. It was reasoned, therefore, that the locus of explanation likely resides in receiver message processing, and may involve a unique form of cognitive heuristic. One way to test a heuristic explanation for the probing effect is to examine whether this effect fails to occur when people should not be heuristically processing. Although this may strike some readers as reverse logic, previous studies have examined those conditions under which the probing effect does occur, and in each case, heuristic processing was likely. Consequently, the goal of our second study was to document a condition under which the probing effect might not hold, by manipulating factors that should influence message recipient heuristic processing.

Unfortunately, the results of Study 2 were ambiguous with respect to the heuristic processing explanation. The probing effect appeared unexpectedly to reverse sign in the high GCS, familiar condition in which systematic processing was likely, but this finding did not hold when statistically controlling for accuracy. The lack of a probing effect when controlling for accuracy might be interpreted as consistent with heuristic explanation because the probing effect was not observed under conditions conducive to systematic processing. However, inconsistent with previous probing research, the probing effect was not evident in conditions in which heuristic processing was likely. Specifically, there was no probing effect for low GCS subjects.

There are two probable explanations for the lack of a probing effect found in the low GCS conditions. First, in retrospect, we might not expect the probing effect to be strong with low GCS subjects. Low GCS individuals already are truth-biased, and although probing should have some effect, its effects also should be attenuated by a ceiling effect. Specifically, those who did not witness probing were in the lower 70% range

in terms of truth judgments. Second, both Study 1 and Buller et al. (1989) found the size of the probing effect to be between  $r = .20$  and  $.22$ ; effect sizes that are not large. That these findings were not replicated by a study with substantially lower power is not particularly surprising. The power analyses for the nonsignificant probing correlations ranged from  $.14$  to  $.41$ . The power for the probing effect (across familiarity) for the low GCS subjects was approximately  $.50$ .

These findings are best interpreted as neither consistent nor inconsistent with the heuristic explanation. The findings should not be interpreted as inconsistent with Study 1 because the effects sizes obtained in Study 2 did not differ significantly from those of Study 1 or Buller et al. (1989). At the same time, the failure to replicate the probing effect in conditions in which it should have been observed precludes interpreting the data as consistent with the heuristic explanation.

Although the results of Study 2 proved uninformative regarding the heuristic explanation, the effects of familiarity on accuracy represent a potentially important finding. Specifically, the data suggest that familiarity with message content is a rather strong predictor of detection accuracy. Participants in the familiarity condition were, on average, 21% more accurate than those who were in the unfamiliar condition, and the familiarity manipulation accounted for almost 30% of the variance in detection accuracy. This suggests that people rely on prior knowledge to detect deception when message content is familiar, and that people can detect deception at rates substantially greater than chance in situations in which prior knowledge is useful.

### STUDY 3

Although the results of Study 2 were not inconsistent with a heuristic explanation of the probing effect, neither did the data provide support for the probing heuristic. Particularly perplexing was the finding that low GCS subjects failed to display any type of probing effect. However, as noted previously, the lack of a probing effect for low GCS subjects may have been attributable to low power or restriction in range. To test this possibility, and to replicate the results of Study 1, we conducted an additional study.

As in Study 2, Study 3 tested the probing effect under conditions in which heuristic and active processing were likely. Our manipulations of ability and motivation differed from those used in Study 2. Ability was controlled with a nonverbal training manipulation, and motivation was varied with differential experimental instructions. To the extent that the probing effect is a function of heuristic processing, we expected to repli-



cate the probing effect under conditions in which ability and motivation were relatively low. When participants had the ability and motivation to actively process messages, however, we expected the probing effect to dissipate. Further, we anticipated that truth-bias would be higher when heuristic processing was likely.

## Method

*Participants.* The participants were 143 undergraduate students at a large Midwestern university. The sample included 50 men and 93 women. The mean age was 20.4 (range 18 to 41). All participants received extra credit in exchange for their participation.

*Design.* Study 3 used a 2 (probe, no-probe)  $\times$  2 (heuristic processing, active processing) independent groups design. The probing manipulation was conducted by exposing participants to either the no-probe tape or the neutral probe tape used in Study 1. The procedures in the heuristic processing condition mirrored those of Study 1.

The active processing condition required that participants have the ability and motivation to process the messages for veracity. Previous research has shown that nonverbal training in authentic deception cues enhances ability in deception detection (e.g., Fiedler & Walka, 1993), and that motivation to detect deception can be manipulated through experimental instructions (e.g., DePaulo, Kirkendol, Tang, & Obien, 1988). Hence, in Study 3 subjects underwent a brief deception detection training procedure prior to viewing videotapes and making judgments, and also received experimental instructions designed to enhance motivation.

*Procedures.* Each experimental session involved having a group of 10 to 12 participants view one of the two experimental tapes (i.e., probe/no-probe). Upon arriving at the experimental site, each group was told that the purpose of the study was to investigate "how individuals perceive the communication behaviors of others." Each group then was randomly assigned to view either the probe or the no-probe tape. Each group also was independently randomly assigned either to the heuristic or the active processing condition. In the active processing conditions, both the ability and motivation inductions took place prior to the viewing of the tapes.

The ability training procedure consisted of providing participants with a list of six reliable indicators of deceptive behavior, based upon prior research (i.e., deTurck & Miller, 1985). The six characteristics were described in detail, along with examples of each, and repeated multiple times to ensure retention. Participants also were provided with a list of

**TABLE 5**  
**Truth-Bias and Accuracy by Probing and Processing Type in Study 3**

	<i>Heuristic</i>		<i>Active</i>	
	<i>Probe</i>	<i>No probe</i>	<i>Probe</i>	<i>No probe</i>
Truth-bias	70%	61%	48%	46%
Total accuracy	49%	56%	48%	50%
Truth accuracy	68%	65%	46%	46%
Lie accuracy	29%	47%	50%	54%

cues that were stereotypically believed to be indicative of deception, but that have proven to not be reliable indicators. Participants were instructed to focus exclusively upon the six reliable behavioral cues during their videotape viewing, and were instructed to ignore the stereotypical cues that are unrelated to deception.

Motivation was manipulated through verbal instructions that described how individuals who are good at detecting deception tend to possess a wide range of desirable social skills. Participants were told that this experiment constituted "a test of the degree to which they were a socially perceptive person." Participants were then strongly encouraged by the experimenter to try their best to detect deception.

Participants in the heuristic processing condition received neither the ability training nor the motivational instructions. They were only instructed only to make veracity judgments, and began viewing the interviews immediately.

After viewing each of the eight videotaped interviews on the test tape to which they had been assigned, respondents were asked to rate their perceptions of the source's honesty on a single dichotomous "truth/lie" item. Perception of source honesty was computed as the proportion of truthfulness judgments to total judgments across the eight cases (0–100% honesty).

## Results

*Manipulation and validity checks.* To test the effectiveness of the ability training, participants were given a knowledge test after viewing the last taped segment and completing the dependent measures. The participants were provided with a list of 12 behavioral cues, and were asked to check all behaviors that were authentic deception cues. Their responses were scored for the percentage correctly identified.

The training manipulation was assessed with a 2 (probing)  $\times$  2 (processing) independent groups ANOVA with the knowledge score as the dependent variable. Participants receiving the nonverbal training ( $M = 82.7\%$ ) scored significantly higher on the knowledge test than did the untrained participants,  $M = 61.8\%$ ,  $F(1, 139) = 55.72$ ,  $p < .001$ ,  $\eta^2 = .29$ ,  $r = .54$ . Neither probing nor the probing by processing interaction affected knowledge scores ( $F_s < 1$ ).

Several studies have tied truth-bias to heuristic processing (Levine et al., 1999; Stiff et al., 1989). As a additional validity check, we tested if those in the heuristic processing condition were more truth-biased than those in the active processing condition. Consistent with our expectations, those in the heuristic processing condition were truth-biased ( $M = 66\%$ ) whereas those in the active processing condition ( $M = 47\%$ ) were not,  $F(1, 139) = 50.40$ ,  $p < .001$ ,  $\eta^2 = .25$ ,  $r = .50$ . Means are presented in Table 5.

*Test of the probing effect.* We predicted that the probing effect would hold in the heuristic condition but not in the active condition. We further expected participants to be more generally truth-biased in the heuristic condition than the active condition. To test these predictions, we employed an a priori contrast of -2, -2, +1, +3. The -2 contrast weights were assigned to the two (probing and no-probe) active processing cells indicating relatively lower truth-bias and no effect for probing. The +1 weight was assigned to the heuristic no-probe cell and the +3 to heuristic probe cell to reflect the predicted probing effect in the heuristic conditions. The a priori contrast was statistically significant,  $t(139) = 7.67$ ,  $p < .001$ .

The predictions were further assessed with analyses of simple effects. As anticipated, the probing effect was evident in the heuristic processing condition. The sources were judged as more honest when probed ( $M = .70$ ) than when not probed,  $M = .61$ ,  $t(72) = 2.04$ ,  $p < .05$ ,  $\eta^2 = .06$ ,  $r = .23$ . Also as expected, probing had no effect in the active processing condition, probe  $M = .48$ , no probe  $M = .46$ ,  $t(67) = 0.54$ ,  $p = ns$ ,  $\eta^2 = .00$ ,  $r = .06$ .

*Accuracy.* Exploratory analyses investigated the effects of processing and probing on detection accuracy. Participants were slightly more accurate in the active processing condition ( $M = 52\%$ ) than the heuristic condition ( $M = 49\%$ ) and with no probes ( $M = 56\%$ ) than with probes ( $M = 50\%$ ). However, neither effect was statistically significant;  $F(1, 139) = 2.94$ ,  $p = ns$ ,  $\eta^2 = .02$ ,  $r = .14$  and  $F(1, 139) = 1.58$ ,  $p = ns$ ,  $\eta^2 = .01$ ,  $r = .10$ .

Significant effects, however, were evident for truth and lie accuracy. Participants correctly identified truths more often in the heuristic conditions ( $M = 67\%$ ) than in the active processing condition ( $M = 46\%$ ),  $F(1,$

139) = 36.46,  $p < .001$ ,  $\eta^2 = .21$ ,  $r = .46$ . Lies were identified with greater accuracy in the active condition ( $M = 52\%$ ) than the heuristic condition,  $M = 37\%$ ,  $F(1, 139) = 11.98$ ,  $p < .001$ ,  $\eta^2 = .07$ ,  $r = .26$  and in the no-probe condition ( $M = 50\%$ ) than in the probe condition,  $M = 39\%$ ,  $F(1, 139) = 7.27$ ,  $p < .008$ ,  $\eta^2 = .05$ ,  $r = .22$ .

## GENERAL DISCUSSION

Several studies have found evidence of a "probing effect." Message sources who are probed are seen as more honest than sources who are not probed, independent of actual honesty, participant-observer status, relationship between source and message recipient, and receiver suspicion.

Explanations for why this effect occurs have proven controversial. Although some scholars have argued that probed sources adapt their postprobe behaviors to look more honest, their data do not support this claim. There is no reliable evidence suggesting that probed sources act honest following probing, and results from our Studies 1 and 3 suggest that the probing effect occurs even when behavior changes are held constant.

Because the probing effect appears to occur independent of source behavioral displays, we reasoned that the explanatory mechanism must involve receiver cognitive processes. This led us to posit the heuristic processing account that was tested in Studies 2 and 3. Specifically, we expected the probing effect to occur in situations in which heuristic processing was likely, but that the probing effect would be absent in situations in which recipient processing could be characterized as active, systematic, and central. Consonant with our heuristic account, both Studies 2 and 3 found no evidence for the probing effect under conditions conducive to active processing. Alternatively, the probing effect was evident in the heuristic processing condition of Study 3, but not Study 2.

Taken together with previous studies, results from these three studies cast doubt upon prior explanations for the probing effect. Although previous researchers have argued that the probing effect stems from source behavioral adaptation, these results suggest that behavioral adaptation is not necessary in order to observe the probing effect, and that the occurrence of the probing effect seems to depend upon factors that should influence heuristic processing of information (e.g., nonverbal training, motivation to detect deception). Although the results of Study 2 were ambiguous, the results of Study 3 were consistent with the heuristic processing explanation. We hope that future research will attempt to replicate the results of Study 3.

Future research investigating cognitive processing explanations for the probing effect needs carefully to consider conceptual and operational issues relating to ability and motivation.

For example, ability might require that message judges merely have the cognitive capacity to scrutinize messages, or it might involve the capability to accurately distinguish truths from lies. Although our initial thinking about ability included both aspects, the results of Study 3 suggest that ability in the form of increased accuracy may not be necessary in order to overcome the probing effect.

These results do not suggest that source behaviors within contexts involving deception play no role in influencing veracity judgments. However, these results are informative regarding the failure of source behaviors to mediate the effect for probing upon receiver honesty judgments, and may signal the need for a revised view of how humans make veracity judgments.

For the most part, previous researchers have assumed a strong correspondence between actual source behaviors and judgments about sources. Message recipients are presumed to pay close attention to the verbal and nonverbal behaviors of message sources and make truth or lie judgments based upon these observed behaviors. For example, when recipients are inaccurate in detection, researchers have argued that they simply are focusing their attention upon the wrong cues (e.g., Miller & Stiff, 1993).

The current findings, as well as other recent research (e.g., Granhag & Strömwall, 2001), suggests the possibility of a more fundamental disconnect between the overt behavior observed and the behavior as perceived or interpreted. For example, Granhag and Strömwall, (2000, 2001), found that although most people expect liars to be less verbally and nonverbally consistent than truth-tellers, there is substantial variation in perceptions of consistency of behaviors. People base veracity judgments on their perceptions of source behavioral consistency, but they also disagree about what is seen as "inconsistent." Rather than judgments regarding source behaviors stemming predominantly from observable and objective actions of the source, judgments regarding source behaviors appear to derive (to a substantial extent) from psychological processes in message receivers that are at least somewhat independent of actual source behaviors.

This may explain the probing paradox we faced at the beginning of our research. Studies had found that probed sources tended to have longer response latencies, more speech errors, and more pauses (Buller et al., 1989, 1991). These same behaviors have been linked to judgments of deceit (Zuckerman et al., 1981). How could deceptive-acting sources consistently be judged as more honest? The traditional view (e.g., Miller & Stiff, 1993) argues that if sources are judged as more honest, then they

must have been acting more honest. The alternative we are suggesting is that under some conditions, receiver veracity judgments may have very little to do with actual source behaviors. We think the studies presented here are consistent with this alternative view, we find these results to be consistent with other recent research (e.g., Fiedler & Walka, 1993; Granhag & Strömwall, 2000), and we believe that the challenge confronting future researchers in this domain is to further investigate how receiver psychological processes affect veracity judgments and detection accuracy.

## NOTES

1. The Machiavellianism items included: (a) Most people are basically good and kind; (b) It is possible to be good in all respects; (c) It is hard to get ahead without cutting corners here and there; and (d) Generally speaking, people won't work hard unless they're forced to do so.

2. Prior to testing the hypotheses and research question, a 4 x 8 (probing condition x message source) mixed ANOVA was conducted to test for treatment x source interactions on truth judgments. The results indicated that the interaction between test tape and message source was not significant,  $F(21, 2275) = 1.43, p = ns, \eta^2 = .01$ . Given that no interaction was found between test tape and message source, we were able to sum across sources in testing the hypothesis, thus providing a more parsimonious analysis. Also, no systematic order effects were apparent, and source and receiver sex did not moderate the effects of probing. A similar 4 x 8 (test tape x message source) mixed ANOVA was conducted to test for treatment x source interactions on confidence. The results indicated that the interaction between test tape and message source was not significant,  $F(21, 2282) = 1.09, p = ns, \eta^2 = .01$ .

3. The differences between any of the conditions involving a probe were also nonsignificant when tested with more liberal *t*-tests.

4. Active processing could result in a reversed probing effect (i.e., probing causes sources to be perceived as less honest) if the probing prompted diagnostically useful or incriminating message content. This possibility, however, is not possible in the current design because source behaviors were held constant.

5. The familiar and unfamiliar controversial issues included: (a) George Perles should be fired from his position as head coach at Michigan State University; (b) Gay fraternities should be allowed to be part of Michigan State University's Greek system; (c) Some required courses at Michigan State University should be presented entirely on videotape to help cut costs; (d) *Playboy* should be allowed to recruit women on campus at Michigan State University this year; (e) The city of Bloomington should use an incinerator to get rid of PCBs; (f) The mayor of Bloomington was correct in vetoing the proposed plan to build a mall on the north side of town; (g) Sycamore Hall on the Indiana University campus should be converted from an Administration building back to student housing in order to improve the campus parking situation; and (h) The Indiana University Speech Communication Department should keep their forensics team under direct faculty control.

6. The participants eliminated from the analyses were distributed evenly across probing conditions, and probing was not related to the efficacy of the familiarity manipulation,  $t(134) = 0.03, p = ns$ . Analyses were conducted with these participants included, and accuracy was the only significant predictor of truth-bias.

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