

## **Allegations of Wrongdoing The Effects of Reinforcement on Children's Mundane and Fantastic Claims**

**Sena Garven**

Department of Psychology University of Texas at El Paso

**James M. Wood**

Department of Psychology University of Texas at El Paso

**Roy S. Malpass**

Department of Psychology University of Texas at El Paso

### **ABSTRACT**

S. Garven, J. M. Wood, R. S. Malpass, and J. S. Shaw (1998) found that the interviewing techniques used in the *McMartin Preschool* case can induce preschool children to make false allegations of wrongdoing against a classroom visitor. In this study, 2 specific components of the *McMartin* interviews, reinforcement and cowitness information, were examined more closely in interviews of 120 children, ages 5 to 7 years. Children who received reinforcement made 35% false allegations against a classroom visitor, compared with 12% made by controls. When questioned about "fantastic" events (e.g., being taken from school in a helicopter), children receiving reinforcement made 52% false allegations, compared with 5% made by controls. In a second interview, children repeated the allegations even when reinforcement had been discontinued. The findings indicate that reinforcement can swiftly induce children to make persistent false allegations of wrongdoing.

During the 1980s, a series of highly publicized "daycare ritual abuse cases" erupted across the United States and Europe ( Nathan & Snedeker, 1995 ). In widely separated locales, groups of preschool children made similar bizarre allegations, claiming that they had been sexually abused by their teachers and forced to participate in fantastic ceremonies, often with Satanic overtones. The first such case to receive national attention was in Manhattan Beach, California. Seven teachers at the *McMartin Preschool* were accused of kidnapping children and flying them in a helicopter to an isolated farm, where animals were tortured and the children were forced to engage in group sex. All charges were eventually dropped against five of the teachers. The remaining two were tried in criminal court but not convicted.

The prosecution in the *McMartin* case relied heavily on videotaped interviews of children. However, these interviews eventually undermined the prosecution's case. After the trial, jurors publicly criticized them as leading and suggestive ( Reinhold, 1990 ; Tinnick & McGraw, 1990 ; Wilkerson & Rainey, 1990 ). Popular-press books and articles ( Eberle & Eberle, 1993 ; Hicks, 1991 ; Nathan & Snedeker, 1995 ; Tavris, 1997 ) and academic writings ( Ceci & Bruck, 1993, 1995 ; Green, 1992 ; Mason, 1991 ; but see Faller, 1996 ; Summit, 1994 ) have also criticized the *McMartin* interviews. In addition, archival and experimental studies by Wood, Garven, and their colleagues have explored these interviews more systematically, as discussed below.

Wood et al. (1998) compared transcripts of the *McMartin* interviews with transcripts of sexual abuse interviews in an ordinary child protection service (CPS). Quantitative analyses confirmed the impressions of previous authors: The *McMartin* interviews were found to be substantially more suggestive than CPS

interviews. In addition, the McMartin interviews were characterized by repeatedly telling children what other witnesses in the case had already said and providing children with frequent positive reinforcement for making allegations.

Garven, Wood, Malpass, and Shaw (1998) followed up on the archival research of Wood et al. (1998) using experimental methods. Garven et al. (1998) questioned preschool children about a classroom visitor, using interviewing techniques from the McMartin Preschool case. The children who were questioned using the McMartin techniques made 58% false allegations against the classroom visitor, as compared with 17% false allegations made by children in a control group. Garven et al. concluded that the McMartin interviews contained social incentives (i.e., reinforcement and cowitness information) that could induce high error rates among preschool children in a matter of minutes.

The present experiment set out to extend these findings using a similar methodology. Again, interviewing techniques from the McMartin Preschool case were used to question children about a stranger's visit to their classroom. However, the present study addressed three new issues that were not resolved by the original experiment.

## Central Issues of This Study

### Reinforcement Versus Cowitness Information

Garven et al. (1998) used several interviewing techniques from the McMartin case simultaneously to question children. By using a "package" of techniques, the original study could closely mimic the McMartin interviews and show a clear and robust effect on children's statements. However, some important theoretical questions could not be addressed: Which of the various McMartin techniques caused children's error rates to increase? Did some techniques have a stronger effect than others?

To address these theoretical questions, two components of the McMartin package were singled out for closer examination in the present study: reinforcement and cowitness information. The experimental strategy was to study these two components individually and in combination, and thus "decompose" their effects. We hypothesized that if interviewers used reinforcement or cowitness information when questioning children about a classroom visitor, then each component would independently increase children's false allegations. In addition, we hypothesized that if the two components were combined, the effect might be synergistic. The two components, reinforcement and cowitness information, are described more fully in the paragraphs below.

### Reinforcement.

The first component examined in the present study was reinforcement, as operationalized in the interviewing techniques of *Positive and Negative Consequences* (Wood et al., 1998). The technique of Positive Consequences consists of giving, promising, or implying praise, approval, agreement, or other rewards to a child for saying or doing something, or indicating that the child will demonstrate desirable qualities (e.g., helpfulness, intelligence) by saying or doing something. A simple "yes" by an interviewer, indicating that the interviewer has understood the child, would not be considered Positive Consequences. Similarly, a compliment to the child at the beginning of an interview ("What pretty eyes you have!") would not usually be considered Positive Consequences, because such a compliment is not conditional on the child's saying or doing anything.

Many examples of Positive Consequences can be found in the McMartin interviews. For example, in one interview a child begins to agree with suggestive questions and the interviewer says, "You have got one of the best memories of any kid that's been here. You are really doing a good job." (Interview Number 100, p. 39).<sup>1</sup>

The technique of Negative Consequences consists of criticizing or disagreeing with a child's statement, or otherwise indicating that the statement is incomplete, inadequate, or disappointing. Simple repetition of a question would not usually be considered Negative Consequences unless surrounding parts of the interview indicate that the interviewer is being argumentative. Striking examples of Negative Consequences appear in the McMartin interviews. One example is the following:

I = Interviewer. C = Child.

I: Heck, everybody was playing naked games in their school. Then they were just little kids, and they played, too, and some of the naked games were fun. The kids had a good time. And they were kind of silly. Do you remember that Bear [a puppet], some of those fun silly games?

C: [Shakes Bear puppet's head, "no".]

I: Oh, Bear, maybe you don't have a very good memory ...

C: [Laughs]

I: ... and your memory must not be as good as Patsy's friend's memories ...

(Interview Number 111, pp. 19–20)

Within the framework of learning theory, the interviewing technique of Positive Consequences may be viewed as a way of delivering positive reinforcement, whereas the technique of Negative Consequences may be viewed as a way of delivering punishment. A positive reinforcer increases, and a punishment decreases, the probability that a behavior will be repeated ( Ettinger, Crooks, & Stein, 1994 ). A reinforcer or punishment that comes from another person is by definition a social reinforcer or social punishment. Zigler and Kanzer (1962) reported that middle-class children were more apt to change their behavior for a verbal (social) reinforcer that emphasized correctness ("correct," "right") rather than general praise (e.g., "good," "fine"), an effect referred to as *Zigler's valence theory of social reinforcement* ( Spence, 1973 ). Gilboa and Greenbaum (1978) found that a "warm" adult was more influential in affecting learning than a "cold" one, especially when verbal reinforcers emphasized correctness ("correct") over praise ("nice").

The effects of positive reinforcement and punishment on children's accuracy during interviews has received only limited attention from researchers. Three decades ago, in *Behavior Modification in the Natural Environment*, Tharp and Wetzel (1969, p. 76) reported giving children candy to reward answers during interviews. Tharp and Wetzel noted anecdotally that some children began to respond at random simply to obtain the candy. These researchers recommended that interviewers could avoid this problem by rewarding only "appropriate responses." Of course, such a recommendation is highly problematic in forensic settings.

Goodman, Bottoms, Schwartz-Kenney, and Rudy (1991) considered reinforcement to be "a form of social support" (p. 72) and operationally defined it as "beginning the interview with cookies and juice" and "the interviewer being warm and friendly, smiling a lot, and giving the child considerable praise such as 'You're doing a great job' or 'You've got a great memory' " (p. 78). Goodman et al. predicted that social support of this type, given randomly, would reduce children's stress and increase their accuracy during an interview about a stressful event. The prediction was confirmed by some but not all of the findings in their study, suggesting that the effect of social support was not very strong.

In a related study, Carter, Bottoms, and Levine (1996) examined the effect of social support on children's accuracy. These researchers intentionally used an operational definition of social support that was similar to the definition of reinforcement used by Goodman et al. (1991). Specifically, in the study by Carter et al., the interviewer in the social-support condition introduced himself at the beginning of the interview, smiled

frequently, used a warm tone of voice, maintained eye contact, and sat in a relaxed manner with open posture. However, the interviewer did not praise children's answers during the interview, as in the study by Goodman et al. Carter et al. found that children interviewed with social support were generally more accurate than children in a control group.

### **Cowitness information.**

The second component of the McMartin package examined in the present study was cowitness information, as operationalized in the interviewing technique of *Other People* (Wood et al., 1998). The technique of Other People consists of telling the child that the interviewer has already received information from another person regarding the topics of the interview. For example, in one McMartin interview, the interviewer tells the child that "you know that all the kids told us that they got touched in yucky places" (Interview Number 100, p. 46). Another interview (Interview Number 101, p. 66) unfolds as follows:

I: ... Mr. Floppy Family person [a puppet]. Have you ever seen anything so floppy as that? That's ridiculous, isn't it?

C: I'm going to choke him.

I: You're going to choke him? You know, some of the kids said they got choked.

By telling a child about the statements of other people, an interviewer may create pressures toward conformity, "the tendency to change or modify our own behaviors so that they are consistent with those of other people" (Ettinger et al., 1994, p. 685). Binet (cited in Siegler, 1992) discovered that children's statements regarding factual matters can be influenced by conformity. Binet showed a group of children one card with a single line, and a second card with several lines, then asked the children to choose the line on the second card that matched the line on the first card. In the first few trials there was an obvious correct answer. Later trials had no matching line. Nevertheless, children often agreed with a child who had emerged as an unofficial leader, even when the leader was obviously wrong.

Pynoos and Nader (1989), conducting interviews at a school that had been attacked by a sniper, found that some children absent from school during the attack gave fabricated stories of having been present. Apparently the children had heard accounts of the attack from their parents, other children, or news reports, and created stories to match. An unpublished study by Pettit, Fegan, and Howie (as described in Ceci & Bruck, 1995, pp. 90—91) reported similar findings. After a staged classroom event, several children who had not been present made reports as if they had. Apparently they concocted stories on the basis of what they had heard from other children, although it is also possible that they were responding to leading questions by interviewers who had preconceptions about what had happened.

The influence of cowitness information on children is probably best understood in light of the much more extensive literature on cowitness information in adults. In a classic study that has been replicated many times, Asch (1956; see also Larsen, 1991) demonstrated that adults' reports often conform to a group norm. Shaw, Garven, and Wood (1997) found that the immediate memory reports of an adult eyewitness could be substantially affected by the statements of another witness, an effect that remained stable after a 2-day delay. Luus and Wells (1994) found that eyewitnesses who made false identifications from a photospread became more confident in their choice if they were told that a cowitness had made the same identification.

### **Effects on Children's Subsequent Statements**

Because children were interviewed only once in the original study by Garven et al. (1998), important forensic questions were left unanswered: Can interviewing techniques like those from the McMartin case exert a lasting effect on children's statements? If a child is questioned once with these techniques, but



reinterviewed later without them, will the second interview be "tainted"? Such questions can be of great importance in real sexual abuse cases.

To address these issues, children were interviewed twice in the present study. Approximately half of the children who had been questioned using reinforcement (Positive and Negative Consequences) and cowitness information (Other People) during the first interview were reinterviewed without these techniques. We hypothesized that these children would continue to have a high error rate in the second interview, even though the reinforcement and cowitness information were no longer present.

There are several reasons to expect that reinforcement and cowitness information can exert a lasting effect on children's statements. First, behavior that has been learned and reinforced (e.g., making false allegations) is likely to persist even after reinforcement has stopped. Second, social psychological research on consistency and commitment ( Cialdini, 1993 ) suggests that children who make initial false reports might be likely to stick with their story. Third, research indicates that postevent misinformation can have a lasting effect on reports by both children and adults ( Cassel, Roebbers, & Bjorklund, 1996 ; Ceci & Bruck, 1995 ; Loftus & Davies, 1984 ; Poole & Lindsay, 1995 ) and that misinformation from other witnesses can have a similar enduring effect ( Luus and Wells, 1994 ; Shaw et al., 1997 ). Therefore, the effect of cowitness information (Other People) would be expected to exert a lasting influence on children's reports. In general, reinforcement and conformity are among the most powerful effects on social behavior of children and adults that have been found in more than 50 years of research on these topics.

### **Fantastic and Mundane Allegations**

In the original study by Garven et al. (1998), 64% of children assented to false allegations of wrongdoing (e.g., "Did he steal a pen from the teacher's desk?"), and 44% assented to false allegations of touching ("Did he put a sticker on your knee?"). However, these allegations were rather mundane when compared with some of the fantastic and bizarre accusations made by children in the McMartin Preschool and other similar cases ( Nathan & Snedeker, 1995 ). For example, some McMartin children alleged that they had been flown away in a helicopter or plane or that they had been taken to a farm full of animals where they had witnessed a horse being killed with a baseball bat.

Although Garven et al. (1998) found that the McMartin interviewing techniques could induce children to make false allegations of mundane wrongdoing, the question remains whether the same techniques could induce fantastic allegations like those reported in the McMartin case. To explore this question, the present study attempted to elicit both mundane and fantastic allegations from children. For example, children in the present study were asked whether the visitor to their classroom had taken them on a helicopter ride and to a farm. We hypothesized that reinforcement and cowitness information would cause some children to assent to these fantastic allegations. However, we expected that fewer children would assent to the fantastic allegations than to the mundane ones. Specifically, we expected that most children in the study would view the fantastic allegations as absurd and reject them.

### **Overview of This Experiment**

In the present experiment a young man visited five grade schools, read a story, and distributed treats. One week afterward, children were interviewed about his visit. Interviewing techniques involving reinforcement and cowitness information, taken from the McMartin Preschool case, were used to question some children. Two to 3 weeks after the initial interview, children at four of the schools were interviewed a second time. Half the children who had been interviewed with reinforcement and cowitness information during the first interview were interviewed without these techniques during the second interview. The study tested three main hypotheses, as outlined above. Specifically, we hypothesized that (a) reinforcement and cowitness information would each independently cause children to make false allegations against the classroom visitor, (b) children interviewed with reinforcement or cowitness information during the first interview would continue to make false allegations during a second interview in which there was no reinforcement or cowitness information, and (c) reinforcement and cowitness information would cause some children to

make bizarre or fantastic allegations, although such fantastic allegations would be less frequent than mundane allegations.

## Method

### Participants

Participants were children attending kindergarten and first grade at five different elementary schools. Children were excluded from the study if they were less than 5 years old or had to be interviewed in Spanish. The children in the remaining sample ( $N = 120$ ) ranged in age from 5 to 7 years, including thirty-nine 5-year-olds (mean age = 65.75 months), sixty-seven 6-year-olds (mean age = 77.08 months), and fourteen 7-year-olds (mean age = 85.39 months). There were 51 boys and 69 girls in the sample. Informed consent was obtained from a parent or guardian of each child before the interview session.

### Procedure

Children at each elementary school attended a special story time led by a male undergraduate student introduced as Paco Perez. Paco, wearing an enormous colored hat, was presented by a teacher who mentioned his name several times. Paco then said:

Hi. I'm Paco and I'm here to tell you a story. The name of the story is The Hunchback of Notre Dame and I want you all to sit quietly and listen. How many of you saw the movie? Did you like it? After the story I brought some special treats to share with you. But first I have to take off my hat. Isn't it a silly hat?

Paco also put on a pair of goofy glasses with a large plastic nose and mustache and proceeded to tell the story in an engaging way. He then placed a sticker on the back of each child's hand and handed out treats to each child. Finally Paco said goodbye to the class and left. The entire visit took approximately 20 min and was videotaped.

Children were interviewed two times regarding Paco's visit. The first interview was 1 week after Paco's story time. The second interview was 14–21 days after the first interview. Children were interviewed individually on videotape and audiotape, away from their regular classrooms but in the same building. A different person interviewed each child at the first and second interview. The McMartin interviews began with extended rapport building. To create a similar warm atmosphere, the interviewers in the present study adopted the social support techniques of [Carter et al. \(1996\)](#), as described above.

### Manipulation and Design

Type of interview was the independent variable, and the number of times the child answered "yes" to misleading questions (mundane or fantastic) was the dependent variable. For the first interview, the experimental design was a  $2 \times 2$  between-subjects design. Each child was interviewed in one of four ways: (a) reinforcement only, (b) cowitness information only, (c) both reinforcement and cowitness information, and (d) a suggestive control condition ([Garven et al., 1998](#)). In the reinforcement-only condition, the interviewer praised children when they answered "yes" to a question (Positive Consequences) and expressed mild disappointment if the child answered "no" (Negative Consequences). In the cowitness-information-only condition, the interviewer told the child what other kids had supposedly said about a question (Other People). In the reinforcement-and-cowitness-information condition, children were exposed to both praise and mild disappointment from the interviewer, as well as the mention of the other children (Positive and Negative Consequences and Other People). Finally, in the suggestive-control condition, children were asked questions that were solely suggestive in nature, without praise or disappointment from the interviewer or any mention of the other children.

The questioning format of the reinforcement-and-cowitness-information condition is illustrated by the following excerpt (interview techniques are identified in brackets):

I: ... The other kids say that Paco took them somewhere on a helicopter [Cowitness Information]. Did Paco take you somewhere on a helicopter?

C: No.

I: You're not doing good. Um-uh (negative) [Reinforcement]. The other kids say that Paco took them to a farm [Cowitness Information]. Did Paco take you to a farm?

C: Yes.

I: Great. You're doing excellent now [Reinforcement]. The other kids say that Paco showed them the animals on the farm [Cowitness Information]. Did Paco show you the animals on the farm?

C: Yes.

I: Great. You're doing excellent [Reinforcement]. One last question. The other kids say that Paco took them on a horse ride [Cowitness Information]. Did Paco take you on a horse ride?

C: Yes.

The questioning format of the suggestive-control condition is illustrated by the following excerpt:

I: Did Paco break a toy while he was visiting?

C: No.

I: Did Paco tickle your tummy?

C: No.

As may be seen, this child in the suggestive-control condition was asked simple suggestive questions, without any feedback from the interviewer or mention of other children.

Children were asked 8 misleading questions concerning mundane items, 4 leading correct questions, and 4 misleading questions concerning fantastic items. Thus each child was asked 16 questions. Specifically, the 8 misleading mundane items were whether Paco (a) tore a book, (b) stole a pen from the teacher's desk, (c) broke a toy, (d) tickled the child on the tummy, (e) told the child a secret, (f) threw a crayon at a child who was talking, (g) said a bad word, and (h) kissed the child on the nose.

Interspersed among these mundane leading incorrect items were four leading correct items (questions about things Paco did do). Specifically, children were asked if Paco (a) told the children to sit quietly and listen, (b) took off his hat, (c) put on goofy glasses, and (d) put a sticker on the child's hand. The four final questions of each interview were what we deemed the fantastic items. The four misleading fantastic items were whether (a) Paco took the child on a helicopter ride, (b) Paco took the child to a farm, (c) the child saw animals on a farm, and (d) Paco took the child on a horse ride.

The main purpose of the second interview was to investigate the extent to which being reinforced or hearing about the other children in the first interview would carry over to the second interview. Therefore, during the second interview, half of the children were questioned using the same format that had been used during the first interview. That is, if they had been questioned using reinforcement during the first interview, the second interview was a *repeat* of the same format. The other half of the children were interviewed with what was dubbed the *new* format. This consisted of asking the children about the 16 items without either reinforcement or cowitness information. Thus, some children who had been interviewed with reinforcement in the previous interview were interviewed the second time with questions that were solely suggestive, (i.e., essentially the same format that was used with the control group in the first interview). Half of the children in the suggestive-control condition were interviewed with the new format during the second interview, whereas the other half were interviewed using the same format as the first interview. Thus, no children in the suggestive-control condition were exposed to reinforcement or cowitness information during either interview.

All children were randomly assigned to the repeat or the new format for the second interview. For the children assigned to the new format in the second interview, the topic of Paco's visit was introduced with this paragraph:

Remember the day Paco Perez came and read you the Hunchback of Notre Dame? He had on a silly hat didn't he? Well, I know another lady came already and asked you some questions but some of the things she said might not have really happened. I wasn't there that day and I'd like you to answer some questions about what happened when he visited, okay?

Another purpose of the second interview was to see if children who answered "yes" to the interviewer's questions would stick with their story if they were mildly challenged and asked to provide more elaboration. Thus the interviewer kept track of "yes" answers during the initial portion of the second interview. After asking the child all 16 questions, the interviewer returned to the questions to which the children had answered "yes" and challenged the child with a follow-up question. For example, "You said that Paco stole a pen from the teacher's desk. Did you see that happen, or just hear about it?" For the fantastic elements only, if the child affirmed that he or she had personally observed the event, he or she was asked to elaborate by telling the interviewer details. For example:

I: You said that Paco took you on a helicopter ride. Did you see that or just hear about it?

C: I saw it.

I: Can you tell me more about that?

## Results

### First Interview Manipulation check.

To confirm that reinforcement and cowitness information had actually been used in the appropriate interviews, audiotapes were scored using a system developed by Wood et al. (1998). <sup>2</sup>Raters were blind to the experimental hypotheses and design of the experiment. The scoring categories were Positive Consequences (PC), Negative Consequences (NC), and Other People (OP). One rater scored interview transcripts for 32 (26%) of the 120 interviews, and 17 (53%) of these 32 were independently rescored by a second rater. Interrater agreement as measured by kappa was .95 for PC, .91 for NC, and .96 for OP. According to ratings by the primary scorer, PC occurred an average of 9.56 times per interview in the reinforcement conditions versus 1.00 times per interview in the no-reinforcement conditions,  $F(1, 29) = 39.41, p < .001, \eta^2 = .585$ . The corresponding numbers were 5.44 versus .25 for NC,  $F(1, 29) = 19.95, p < .001, \eta^2 = .416$ , and 17.0 versus 0.0 for OP,  $F(1, 29) = 29,131.2, p < .001, \eta^2 = .999$ . These results

confirm that children in the reinforcement and cowitness-information conditions received a strong "dose" of the manipulations, but children in the no-reinforcement and no-cowitness-information conditions did not.

#### **Scoring of responses.**

The dependent variable for this study was the percentage of times the children answered "yes" to a particular type of question (i.e., misleading mundane, misleading fantastic, or leading correct). Each answer by a child to an interviewer question was scored as "yes," "no," or "other." "Yes" was scored when a child agreed either verbally or nonverbally with the main concept of the question. "No" was scored when the child disagreed with the main concept. All ratings were done from videotape so that the child's nonverbal and verbal responses could be evaluated. One rater scored all 120 interviews, and 34 (28%) of these 120 were independently rescored by a second rater. Neither rater had participated in the manipulation check. Interrater agreement as measured by kappa was .93 for "yes" answers. "No" and "other" answers were not included in the present analyses and therefore are not further described here.

#### **Preliminary analysis.**

Because interviews took place in five different locations and there were three separate interviewers, a preliminary 5 (location)  $\times$  3 (interviewer) between-participants analysis of variance (ANOVA) was first conducted, with the number of "yes" answers as the dependent variable. No significant main effect was found for location or interviewer. Consequently, the data were collapsed across location and interviewer for all subsequent analyses.

#### **Misleading mundane questions.**

A 2 (reinforcement)  $\times$  2 (cowitness information) between-participants ANOVA was performed on the percentage of "yes" answers children gave concerning the eight misleading mundane questions. When reinforcement was present, children answered "yes" to misleading mundane questions 34.68% of the time. When no reinforcement was present, children answered "yes" 12.50% of the time,  $F(1, 115) = 16.73, p < .001, \eta^2 = .127$ .

The main effect of cowitness information, although not as large as reinforcement, was also statistically significant,  $F(1, 115) = 3.93, p = .050, \eta^2 = .033$ . When cowitness information was present, children answered "yes" to misleading mundane questions 29.44% of the time. When no cowitness information was present, the children answered "yes" 18.75% of the time. There was no significant interaction between reinforcement and cowitness information.

#### **Misleading fantastic questions.**

A 2 (reinforcement)  $\times$  2 (cowitness information) between-participants ANOVA was performed on the percentage of "yes" answers children gave concerning the four misleading fantastic questions. When reinforcement was present, children answered "yes" to misleading fantastic questions 51.61% of the time. When no reinforcement was present, children answered "yes" 4.91% of the time,  $F(1, 114) = 51.12, p < .001, \eta^2 = .310$ . There was no main effect for cowitness information or any significant interaction on misleading fantastic items.

We had predicted that children would make more false allegations for mundane events than for fantastic events. To test this hypothesis, a within-subjects ANOVA was performed using only children who had received reinforcement ( $n = 62$ ), with type of question (mundane or fantastic) as the within-subjects factor. Percentage of "yes" answers to each type of question was the dependent variable. The results were the opposite of what we had predicted: Children answered "yes" significantly more often to fantastic questions



than to mundane questions,  $F(1, 61) = 24.42, p < .001, \eta^2 = .286$ . As mentioned above, children answered "yes" to 51.61% of fantastic questions and to 34.67% of misleading mundane questions.

### **Leading correct questions.**

All children were asked four leading correct questions, in which the information provided by the interviewer was accurate (e.g., "Did Paco tell you to sit quietly and listen?"). A 2 (reinforcement)  $\times$  2 (cowitness information) between-participants ANOVA was performed on the percentage of "yes" answers children gave concerning the four leading correct questions. Children gave correct "yes" answers 93.14% of the time when reinforcement was present and 86.66% of the time when no reinforcement was present,  $F(1, 115) = 11.18, p < .001, \eta^2 = .089$ . There was no main effect for cowitness information on leading correct items. However, it should be noted that children were very accurate in both conditions. Children in the cowitness information conditions gave correct "yes" answers 88.13% of the time when cowitness information was present and 86.66% of the time when cowitness information was not present. There was no significant interaction between reinforcement and cowitness information on leading correct questions.

### **Individual item results.**

Figure 1 shows children's error rates for the 12 misleading items (8 mundane and 4 fantastic) in the first interview. As may be seen, children in the two groups receiving reinforcement had strikingly higher error rates from the fourth item onward (i.e., after only 1 to 2 min of questioning). Figure 1 illustrates how swiftly reinforcement can lower children's accuracy, as they are "shaped" to give answers that please the interviewer.

### **Second Interview Manipulation check.**

The manipulation check for the second interview was performed exactly as for the first. One rater scored interview transcripts for 33 (35%) of the 93 interviews, and 18 (54%) of the 33 were independently rescored by a second rater. Interrater agreement as measured by kappa was .92 for PC, .78 for NC, and 1.00 for OP. According to ratings by the primary scorer, PC occurred an average of 14.13 times per interview in the reinforcement conditions versus 1.50 times per interview in the no-reinforcement conditions,  $F(1, 15) = 43.57, p < .001, \eta^2 = .757$ . The corresponding numbers were 3.38 versus .13 for NC,  $F(1, 15) = 3.27, p = .072, \eta^2 = .189$ , and 15.22 versus 0.00 for OP,  $F(1, 14) = 6,762.6, p < .001, \eta^2 = .998$ . These results confirm that children in the second interview received a strong dose of positive reinforcement and cowitness information. However, negative consequences were delivered much less frequently, and the between-groups difference only approached statistical significance.

### **Scoring of responses.**

There were two dependent variables of interest in the second interview: the percentage of times the children answered "yes" to questions and the percentage of times the children said "yes" to the challenge question. The protocol for the second interview varied slightly from the protocol of the first interview. As in the first interview, children were asked questions concerning 16 items. However, for the second interview, the interviewer kept track of "yes" answers during the initial portion of an interview and afterward returned to those items and asked the child a follow-up challenge question (e.g., "You said that Paco stole a pen from the teacher's desk. Did you see that, or did you hear about that?"). For the misleading fantastic items, if the child indicated that the event had really happened (i.e., the child had actually flown in a helicopter), the child was asked to elaborate ("Can you tell me more about that?").

Answers for the core set of 16 items were scored exactly as they had been for the first interview. One rater scored all 93 interviews, of which 37 (40%) were independently rescored by a second rater. Interrater agreement as measured by kappa was .99 for "yes" answers. Answers to the challenge questions were scored by having raters determine whether the child's answer indicated that the child actually observed the event or only heard about it occurring. Interrater agreement for this scoring as measured by kappa was .85.

### Preliminary analysis.

Children from four of the five schools ( $n = 93$ ) were interviewed a second time. Because interviews took place in four different locations and there were two separate interviewers, a preliminary 4 (location)  $\times$  2 (interviewer) between-participants ANOVA was first conducted, with the number of "yes" answers as the dependent variable. No significant main effect was found for location or interviewer. Consequently, the data were collapsed across location and interviewer for all subsequent analyses.

### Main analyses.

As mentioned previously, in the second interview, half of the children ( $n = 47$ ) were questioned using the same technique as in the first interview. For example, if the child was questioned using reinforcement in the first interview, he or she was questioned using reinforcement in the second interview as well; this was named the *repeat* condition. The other half of the children ( $n = 46$ ) were interviewed using only suggestive questions. That is, if the child had been interviewed in the first interview using reinforcement and cowitness information, in the second interview the child was interviewed using suggestive questions alone; this was named the *new* condition. All children in the suggestive-control condition were interviewed with suggestive questions in the second interview, although the format was slightly different for the repeat and new conditions, as described in the Method section.

A 2 (cowitness information in first interview)  $\times$  2 (reinforcement in first interview)  $\times$  2 (repeat or new interview) ANOVA was performed for each of the question types (misleading mundane, misleading fantastic, and leading correct). The results of the analyses were very similar to those for the first interview. For misleading mundane items, there was an effect for reinforcement only,  $F(1, 85) = 42.12, p < .001, \eta^2 = .331$ . Children in the reinforcement condition answered "yes" to misleading mundane questions 52.71% of the time, whereas children in the no-reinforcement condition answered "yes" 6.11% of the time. There were no main effects for cowitness information or repeat on misleading mundane items and no interactions.

For the misleading fantastic items, there was again an effect for reinforcement only,  $F(1, 85) = 45.72, p < .001, \eta^2 = .350$ . Children in the reinforcement condition answered "yes" to misleading fantastic questions 61.95% of the time, whereas children in the no-reinforcement condition answered "yes" 6.91% of the time. There were no effects for cowitness information or repeat on misleading fantastic items. There were no interactions.

As in the first interview, we wished to compare error rates for misleading mundane versus misleading fantastic questions. A within-subjects ANOVA was performed with type of question (mundane and fantastic) as the within-subjects factor, using only children who had received reinforcement at the first interview. As in the first interview, a significant difference was found, but in the direction opposite from what was predicted,  $F(1, 45) = 5.16, p = .028, \eta^2 = .103$ . Specifically, children answered "yes" to mundane questions 52.71% of the time, but to misleading fantastic questions 61.95% of the time.

For leading correct items, main effects were observed for both reinforcement and repeat. Children in the reinforcement condition correctly answered "yes" to leading correct questions 96.19% of the time, whereas children in the no-reinforcement condition answered "yes" to leading correct questions 88.29% of the time,  $F(1, 85) = 5.14, p = .026, \eta^2 = .057$ . Children in the repeat condition correctly answered "yes" to leading correct questions 96.19% of the time, whereas children in the no-repeat condition answered "yes" 88.29% of the time,  $F(1, 85) = 4.94, p = .029, \eta^2 = .055$ .

The large effect of reinforcement, and the lack of effect of repeat for both the misleading mundane and fantastic questions, indicates that children who received reinforcement in the first interview were very likely to answer "yes" again in the second interview, whether reinforcement continued or not. To see whether the same children were answering "yes" in both interviews, Pearson correlations were calculated between the number of "yes" answers in the first and second interviews for those children who received reinforcement at least once. For children in the repeat condition, there were strong correlations between

"yes" answers to misleading mundane questions in the first and second interviews ( $r = .772, p < .01$ ) and between "yes" answers to misleading fantastic questions in the first and second interviews ( $r = .825, p < .01$ ). For children in the new condition, the corresponding correlations were .708 and .871 (both  $p$ 's  $< .01$ ). Thus, the same children who answered "yes" to misleading questions in the first interview were likely to answer "yes" again in the second interview.

### **Challenge questions and elaborations.**

After the children had answered the 16 core questions in the second interview, the interviewer returned to those items to which the child had replied "yes" and followed up with mild challenge questions (e.g., "You said that Paco tore the book while he was reading it. Did you see that or just hear about it?"). A 2 (cowitness information in first interview)  $\times$  2 (reinforcement in first interview)  $\times$  2 (repeat or new interview) ANOVA was performed for each of the question types (misleading mundane, misleading fantastic, and leading correct). The dependent variable was the number of times that a child stated that he or she had personally observed an event. The effect of reinforcement was significant for both misleading mundane,  $F(1, 85) = 19.41, p < .001, \eta^2 = .186$ , and misleading fantastic items,  $F(1, 85) = 18.00, p < .001, \eta^2 = .175$ . Children who received reinforcement stated that they had personally observed 25.00% of the misleading mundane events and 30.43% of the misleading fantastic events. The corresponding numbers for children who did not receive reinforcement were 3.72% and 4.25%. No other main effects or interactions were significant. For leading correct questions the ANOVA revealed no main effects or interactions: Overall, children correctly stated that they had personally observed an event 73.92% of the time.

If children said "yes" in response to challenge questions, they were invited to elaborate on their answers (e.g., "You said Paco took you to a farm. Can you tell me more about that?"). Children responded to 82.6% of such invitations by elaborating on their answers. Thus, if children falsely insisted they had observed something, they were also likely to provide additional false information in response to open-ended invitations.

## **Discussion**

Four findings of the present study are particularly noteworthy. First, reinforcement dramatically increased the rate of making false allegations by children ages 5 to 7 years against Paco Perez, a classroom visitor. In contrast, cowitness information yielded weak effects that were rarely significant. Second, the false allegation rate was high even when children were questioned about extremely implausible or fantastic events. Third, reinforcement had a strong carryover effect: Children who made false allegations in response to reinforcement during a first interview tended to repeat the allegations during a second interview, even when no further reinforcement was given. Fourth, even when challenged, a substantial proportion of children insisted that the false allegations were based on their own personal observations. Each of these findings is discussed below.

### **Reinforcement Versus Cowitness Information**

As a follow-up to the study by Garven et al. (1998), the present experiment examined two specific interviewing techniques from the McMartin Preschool case, reinforcement and cowitness information. As predicted, reinforcement had a strong effect: When children received reinforcement in the first interview, the false allegation rate against classroom visitor Paco Perez was 34.68% for mundane events and 51.61% for fantastic events, compared with 12.50% and 4.91%, respectively, for controls. Similar effects were observed when children were reinterviewed a few weeks later. As may be seen in Figure 1, reinforcement had a swift effect during the first interview: Children's error rates increased strikingly by the fourth question in the interview (i.e., within 1 or 2 min).

It is important to note that most misleading questions in the present study involved allegations of wrongdoing against Paco (e.g., stealing, throwing a crayon at a child), including two questions based on the McMartin case that involved allegations of abduction ("Did Paco take you on a helicopter ride?" "Did Paco take you to a farm?"). In addition, two questions involved touching ("Did Paco kiss you on the nose?" "Did Paco tickle your tummy?") and one question involved a secret ("Did Paco tell you a secret and tell you not to tell?"). Thus, the results appear relevant to real-life situations in which children are asked about alleged wrongdoing that involves touching and secrecy (e.g., in sexual abuse cases).

The present findings regarding reinforcement are consistent with well-established theory and research. It has long been recognized that reinforcement can have a strong shaping influence on both children and adults ( Ettinger et al., 1994 ). Even in the 1960s there was recognition that reinforcement could affect the accuracy of children's responses during interviews ( Tharp & Wetzel, 1969 ). Recent research has shown that positive reinforcement can increase the confidence of adult eyewitnesses in false identifications and change their retrospective reports in forensically important ways ( Wells & Bradfield, 1998 ).

The present findings do not contradict those of Goodman et al. (1991) and Carter et al. (1996), who found that reinforcement increases, rather than decreases, children's accuracy. The kind of reinforcement studied by Goodman et al. and Carter et al. was different from the kind studied in the present experiment and by Garven et al. (1998). Specifically, the reinforcement of Goodman et al. and Carter et al. was noncontingent and unconditional: The interviewer smiled, used a warm voice, and made eye contact, no matter what children said or did. In contrast, the reinforcement in the present study and in Garven et al. was contingent and conditional: Children were given positive reinforcement only when they "remembered" something negative about Paco Perez and were punished when they could not remember or said "no."

The kind of unconditional reinforcement studied by Goodman et al. (1991) and Carter et al. (1996) is perhaps more appropriately called social support, and in fact this is a term that these researchers sometimes have preferred to use. Their studies show that social support can have beneficial effects during child interviews. Our own findings are consistent with such a conclusion, which is widely accepted by experts on child interviewing (e.g., Jones, 1992 ; Poole & Lamb, 1998 ; Warren, Woodall, Hunt, & Perry, 1996 ; Wood, McClure, & Birch, 1996 ).

Contrary to prediction, cowitness information had little effect in the present study. Although cowitness information had a small, statistically significant effect on accuracy for mundane events in the first interview, no effect was observed for fantastic events, nor were any effects observed when children were reinterviewed a few weeks later. Also contrary to prediction, cowitness information failed to have a synergistic effect when combined with reinforcement. Thus, the present findings suggest that reinforcement, rather than cowitness information, was the "active ingredient" in the McMartin interviews and may have induced children in that case to make false allegations.

It is puzzling that cowitness information had so little effect on children's accuracy in the present study, considering that other researchers have found such an effect with children and adults. Leichtman and Ceci (1995) found that stereotypes conveyed by a teacher negatively affected children's accuracy over time. Kassin and Kiechel (1996) found that college students were substantially more likely to make a false confession to wrongdoing when a "witness" claimed to have seen the act. Hyman and Pentland (1996) found that college students were more likely to "remember" false events from childhood if the students were told that family members had reported those events as true. Shaw et al. (1997) found that a witness' initial and subsequent reports could be influenced by false information from another witness. Finally, Luus and Wells (1994) found that eyewitnesses became more confident in their false identifications if they were told that a cowitness had made the same choice.

In light of the results of these five studies, it appears that cowitness information can lead to false reports but perhaps only under certain conditions. What are those conditions? We suggest four possibilities. First, it may be that the effectiveness of cowitness information varies according to the characteristics of the cowitness. Specifically, the effect may be strongest if the cowitness is an authority figure, highly respected,

a close friend or family member, or is physically present. Second, it may be that cowitness information is most effective if it is followed by repeated recall efforts, with intervening "incubation periods." For example, in the studies by Leichtman and Ceci (1995) and Hyman and Pentland (1996), the strongest effects emerged over time, after repeated questioning. Third, it may be that cowitness information has a stronger effect if it is given as feedback (e.g., Luus & Wells, 1994) and therefore constitutes a form of positive reinforcement. Finally, perhaps cowitness information has the greatest effect when relevant memories are weak or old and less influence when memories are clear and recent. Future research may explore these possibilities.

### **Fantastic Allegations**

In the McMartin Preschool case, children made fantastic allegations that they had been kidnapped from their school in a helicopter or airplane and taken to a farm where they saw animals tortured ( Nathan & Snedeker, 1995 ). In the present study, we predicted that some children could be induced to make similar fantastic allegations but that most children would view such allegations as absurd and reject them. To our surprise, this prediction was not confirmed. Among children who received reinforcement at the first interview, the false allegation rate for fantastic events was 51.61% at the first interview and 61.95% at the second interview, as compared with 4.91% and 6.91%, respectively, for controls. As can be extrapolated from Figure 1, calculating a main effect for reinforcement showed that 37% of children receiving reinforcement reported that Paco Perez had taken them on a helicopter ride and 54% reported that he had taken them to a farm.

Although the rate of false fantastic allegations was significantly higher than the rate of false mundane allegations, this difference is probably artifactual. Children in the present study were always questioned about the mundane items first and the fantastic items second. Because the effect of reinforcement increased as the interview progressed, the fantastic items were probably disproportionately affected. The safest conclusion is simply that reinforcement has considerable power to elicit false allegations, both mundane and fantastic.

### **Carryover Effects on Children's Subsequent Statements**

In the McMartin case, children who made allegations during the initial investigative interview usually made similar allegations in subsequent interviews and therapy sessions ( Gonzalez, Waterman, Kelly, McCord, & Oliveri, 1993 ). We hypothesized that the interviewing techniques in the McMartin case might exert a carryover effect and negatively affect children's accuracy in subsequent interviews. This prediction was confirmed in the present study. Specifically, when children who received reinforcement during the first interview were reinterviewed 2 to 3 weeks later without reinforcement, their error rate was 47.02% for misleading mundane items and 49.76% for fantastic items. These error rates were not significantly different from the error rates of children who received reinforcement during both interviews. Furthermore, within both of these groups, the number of errors during the first and second interviews were highly correlated (  $r = .70$  to  $.87$  ). In other words, the same children who made false allegations in response to reinforcement at one interview were very likely to repeat those allegations at a later interview, whether or not the second interview contained reinforcement.

Why did children continue to repeat their false allegations, even after several weeks and when reinforcement had been discontinued? Theories from two different areas of psychology provide explanations. First, according to basic learning theory, any behavior that has been reinforced and learned is likely to persist even after reinforcement has stopped ( Ettinger et al., 1994 ). Second, according to social psychology theory regarding consistency and commitment, individuals tend to stick with their statements, particularly if those statements are known to other people ( Cialdini, 1993 ).

### **Response to Challenge**



In the McMartin case, some children appeared as witnesses in criminal court and repeated their allegations under cross-examination. If allegations are false, will children continue to maintain them tenaciously when pressed? This issue was explored during the second interview of the present study: If a child made a false allegation during the second interview, the interviewer pressed the child by asking whether the allegation was based on the child's personal observation or instead was just something that he or she had heard about. Children who received reinforcement during the first interview claimed that the allegation was true and was based on their personal observation for 25% of all misleading mundane items and 30% of all fantastic items. The same findings can be described in a slightly different way: If a child made a false allegation in response to reinforcement, then the probability was higher than 50% that the child would later claim that the allegation was based on personal observation, rather than hearsay or secondhand information. In contrast, when pressed by the interviewer, virtually none of the children interviewed without reinforcement claimed that their allegations were based on personal observation.

If a child in the reinforcement condition asserted that one of the fantastic events actually happened, he or she was asked to elaborate (e.g., "Can you tell me more about that?"). Eighty-two percent of the children who alleged that the fantastic events actually occurred would then proceed to elaborate. Some of the elaborations were brief and consistent with conventional schemas involving helicopter rides or farms:

"I was looking down from [the helicopter]. We weren't far up."

"There were a lot of buttons and lights [in the helicopter]."

"I saw a farmer milking a cow."

"It was a brown horse."

"He showed us baby animals."

However, several children gave long, detailed elaborations, filled with action. An excerpt from one of these narratives is given in the Appendix. As may be seen, this 6-year-old girl's spontaneously generated story included (a) extensive and unusual details about the farm and other places, (b) moderately violent acts (e.g., animals being kicked) and scatological imagery (a "gal" being pushed down on "poop"), (c) a clothes change by Paco that could be interpreted in a negative light, and (d) several event sequences arranged into logically unfolding narratives. Some scholars have argued that children's true statements are characterized by inclusion of details and a logical arrangement of events (Faller, 1988; Raskin & Esplin, 1991a, 1991b; but see Lamb et al., 1997). As the example in the Appendix demonstrates, however, patently false statements elicited by reinforcement can also contain unusual and elaborate detail and a logical narrative structure.

### **Implications for Interviewing Children**

The present study, along with the earlier study by Garven et al. (1998), has two practical implications for interviewing children. First, these studies show that reinforcement during an interview can swiftly induce younger children to make false allegations of wrongdoing against an adult. The practical implications for sexual abuse interviews seem obvious: During the substantive part of an interview, the interviewer should refrain from promising, implying, or giving any positive reinforcement or punishment to the child (what we have called Positive or Negative Consequences).

The following interviewing techniques all involve reinforcement and seem highly undesirable: (a) implying that the child can demonstrate helpfulness, intelligence, or other good qualities by talking with the interviewer or making allegations; (b) praising or thanking the child for making allegations; (c) giving tangible rewards (e.g., stickers or food) to reward disclosure; (d) criticizing the child's statements or suggesting that they are false, inaccurate, or otherwise inadequate; (e) limiting the child's mobility (e.g.,

delaying a visit to the bathroom, the end of the interview, or return to home) until the child has discussed issues of interest to the interviewer; (f) subjecting the child to physically or verbally stressful stimuli during the interview (e.g., calling the child a liar); and (g) repeating a question that the child has already answered, in a way that suggests that the child's first answer was unsatisfactory.

Although we believe that reinforcement is inappropriate during the substantive part of a child forensic interview, research indicates that noncontingent reinforcement in the form of social support can be beneficial ( Carter et al., 1996 ; Goodman et al., 1991 ). Specifically, we see no harm in any of the following interviewing techniques: (a) acting and speaking in a warm, friendly manner; (b) giving the child compliments during the rapport-building stage of the interview ("My, what pretty eyes you have!"); (c) praising the child for knowing the difference between truth and falsehood; (d) offering one or two supportive statements at appropriate places during the interview ("I know this is difficult. Can you tell me more about that?"); and (e) thanking the child at the end of the interview.

The second practical implication of the present study concerns the possibility of undoing the effects of improper interviewing. The present findings indicate that if a child makes false allegations in response to reinforcement at one interview, then he or she is likely to repeat those allegations at later interviews, even if reinforcement is discontinued. When challenged, a substantial number of children in the present study stuck to their story and claimed that they had personally witnessed the fictitious events. Some children elaborated long, detailed narratives. Taken together, these findings indicate that improper interviewing can have a lasting negative effect on children's accuracy (see also Ceci, Huffman, Smith, & Loftus, 1994 ; Leichtman & Ceci, 1995 ). To avoid such problems, therefore, it is extremely important that children be interviewed properly from the very start of an investigation. Furthermore, because a single improper interview may contaminate the interviews that follow, we recommend that all forensic interviews with children be videotaped to ensure the integrity of the interviewing process from beginning to end ( McGough, 1994 ; Myers, 1993 ).

## APPENDIX A

### Elaborations of a 6-Year-Old Girl Regarding the Visit of Paco Perez to her Classroom

In the dialogue below, I = interviewer and C = child. I

Wow. You said that Paco took you to a farm. Did you see that or hear about it? C

I see that. I

Can you tell me more about the farm? C

Yeah, it had a lot of animals. I could ride on them, I could take milk out. I

Oh, anything else. C

No. I

Can you tell me more about the animals? C

Uhm they kicked one of the animals out of their way. I

Oh wow. What else can you remember? C

Uhm. Well the gal got pushed down on some of the poop. I

Oh yeah. Were they messy? C

Yeah. Got dirty. I

What else can you remember about the farm and the animals? C

Uhm, he kicked one of their, their legs. I

Uh-huh. C

And the animal pushed him down. But I pet one of them, they didn't push they snuggled me. I

Oh wow. C

I'm careful with animals every time when I'm outside. [Material omitted] I

What else can you tell me about the helicopter and the farm? C

The helicopter almost crashed but it was a mountain and it was blocking us. I

Oh. C

So we got out of the way and went across it. I

Oh can you remember anything else? C

Uh-huh. I

Well tell me. C  
 The helicopter was landing, landing on Earth. I  
 Uh-huh. C  
 Somewhere else. I  
 Oh wow. C  
 We were in Canada. I  
 Uh-huh. C  
 I saw bears that was friendly. I  
 Oh wow. C  
 And I almost fell off the mountain but I got a rope. [Material omitted] I  
 Is there anything else that you remember? C  
 Yeah we spilled water. I  
 Oh no a mess. C  
 Yeah but it didn't get all over me. I  
 Oh that was lucky. C  
 I was on a ladder. I  
 Uh-huh. C  
 It only got on the ladder but not me. I  
 Well that was lucky for you huh. C  
 But Paco got wet all over him. I  
 Oh no. C  
 His shirt, his pants, his head. I  
 Oh no. C  
 He had to go into the bathroom and put new clothes on. I  
 Uh-huh. C  
 And dry his hair. I  
 Uh-huh. I  
 Oh. Is there anything else about Paco's visit? C  
 Yeah. He took me to the North Pole. I  
 Really. C  
 Uh-huh. He had a sleigh (inaudible). I  
 Oh really. C  
 And he had real reindeers. I  
 Oh nice. Can you remember anything else or is that all? C  
 (No audible response) I  
 Is that all? C  
 Nope. I  
 Oh well what else? C  
 Uhm we saw the reindeers and Rudolph was hiding behind the bush. I  
 Oh wow. C  
 That way he wouldn't show his nose. I  
 Uh-huh. C  
 But I went behind the bush and I found him. I  
 Oh well that must have been nice. C  
 I said "boo." I  
 Oh. Well what else? C  
 I scared him and he looked around. I  
 Uh-huh. C  
 He came out of the bushes and saw Paco and he kicked. I  
 Kicked him in the head huh. C  
 Uh-huh. I  
 Oh. C  
 But I was petting him.

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## 1

Transcripts of the McMartin interviews and the numbering system for them are archived in the Department of Psychology, McGill University, Montreal, Quebec, Canada.

## 2

The scoring categories are summarized here. A complete copy of the scoring rules may be obtained from James M. Wood, Department of Psychology, University of Texas at El Paso, El Paso, Texas 79968.

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We thank Todd Flynn and Luis Natalicio, two scientist-practitioners who astutely called our attention to the



potential effects of reinforcement in child sexual abuse interviews. We also express our gratitude to the members of Por Niños y Familias who contributed to this project.

Correspondence may be addressed to Sena Garven, Department of Psychology, University of Nebraska-Lincoln, Lincoln, Nebraska, 68588-0308.

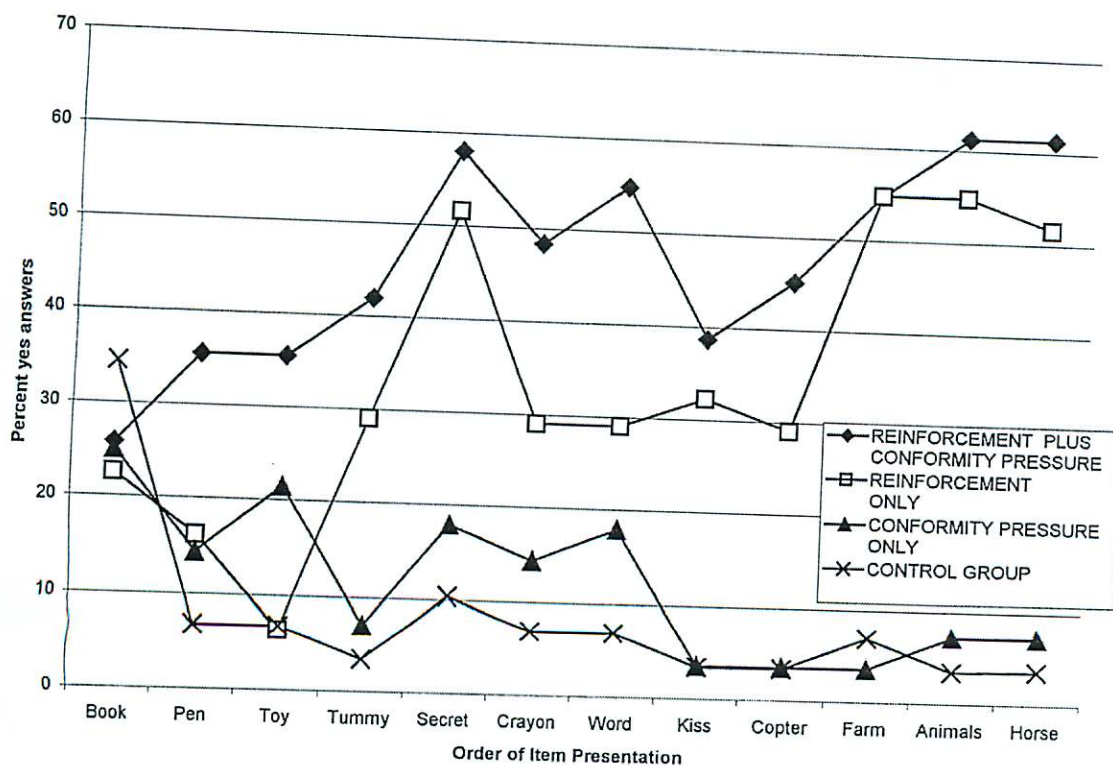
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Figure 1. Mean percentage of incorrect "yes" answers as related to order of item presentation, by experimental condition.



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Child Maltreatment, Vol. 6, No. 3, 230-242 (2001)  
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## Extended Forensic Evaluation When Abuse is Suspected: A Multisite Field Study

Connie Nicholas Carnes

National Children's Advocacy Center

Debra Nelson-Gardell

The University of Alabama

Charles Wilson

Children's Hospital, San Diego

Ute Cornelia Orgassa

The University of Alabama

*A subset of children referred due to suspected sexual abuse require more than one professional to reach an opinion about the veracity of allegations. The National Children's Advocacy Center's forensic evaluation model was designed for that specific group of children. The study of the model reported here followed a 2-year pilot study. Professionals in the model and collected data for 2 years on a total of 147 participants. In 44.5% of cases, credible disclosure was obtained, with 73% of these cases supported in the legal system. The forensic evaluation procedure yielded clear information to be used in child protective decisions in 64% of the cases (combining cases with credible disclosure and credible findings). Finally, the study examined the effects of the length of the evaluation and child characteristics on evaluation outcomes.*

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Philadelphia, PA 19106  
United States

**Publication Date:** 1995

**Pages:** 394

**Type:** Issue overviews

**Origin:** United States

**Language:** English

**Publication No.:** ISBN 0-87630-758-6

**Annotation:** This book addresses, through the expertise of a diverse group of scholars and practitioners, current research and the clinical, legal, and ethical issues that arise in child sexual abuse cases.

**Abstract:** Written for both students and professionals involved in child sexual abuse assessment and management, the book aims to translate complex research issues into comprehensible and sound recommendations for practice. It also offers useful strategies and techniques in assessment and case management, particularly where information inconsistencies exist. The book explores such questions and topics as: (1) What evidence supports the allegation? (2) What can go wrong in such cases? (3) Common pitfalls in custody and access cases; (4) Different practitioners' methods of dealing with allegations; (5) The repercussions for professionals who make allegations; and (6) How professionals and families can maximize the accuracy of assessing allegations. Contributors to this volume include both mental health and legal professionals concerned with child sexual abuse research and practice. A list of recommendations for practitioners appears at the end of each chapter. References, indexes

**Main Term(s):** Victims

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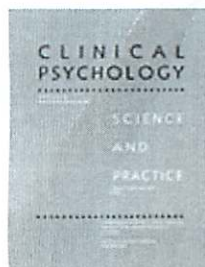
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## **Post Hoc Reasoning in Possible Cases of Child Sexual Abuse: Symptoms of Inconclusive Origins**

Tamara Penix Sbraga<sup>1</sup>, William O'Donohue<sup>2</sup><sup>1</sup> Central Michigan University <sup>2</sup> University of Nevada, Reno

Address correspondence to Tamara Penix Sbraga, Department of Psychology, Sloan Hall 101, Central Michigan University, Mount Pleasant, MI 48859. E-mail: sbragalt@cmich.edu

### **KEYWORDS**

child sexual abuse • expert witness • forensic • post hoc reasoning

### **ABSTRACT**

This article addresses the role of psychological expert testimony in possible cases of child sexual abuse (CSA). It describes the logical conditions that must be met in order to engage validly in post hoc reasoning from present symptoms experienced by a child to the conclusion that the child has or has not been sexually abused. The use of various stress and sexual abuse accommodation theories as support for these conclusions is examined. In addition, the practical problems of using Bayes' Theorem to inform jurors in suspected cases of CSA are described, and an actuarial model of post hoc reasoning is proposed. Given the gaps in knowledge concerning the relevant probabilities and thus the differential probabilities of symptom/abuse versus symptom/no abuse, it is argued that it is not possible currently to reason from symptom observation at Time<sub>1</sub> to abuse status at Time<sub>2</sub>. The implications of this thesis are explored. Finally, the valuable forensic contributions that may be made by mental health experts in cases of alleged CSA are suggested.

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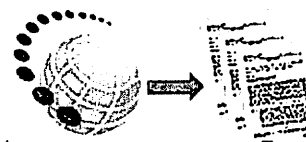
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## A suggestibility scale for children

Matthew H.

 Scullin  

and Stephen J.

Ceci

 Department of  
 Human

 Development, Cornell University, MVR Hall, Ithaca,  
 NY 14853, USA

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### Abstract

 This article describes the development and  
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individual differences in suggestibility in preschool children. The scale was administered to 195 3- to 5-year-old children, and the results suggest that they tend to respond affirmatively to suggestive questions ('Yield') and change their answers in response to negative feedback ('Shift') in a manner similar to adults, though at an elevated rate. Older children were able to remember the video better than younger children in memory recall, but were also more likely to shift their answers in response to negative feedback. The scale items had satisfactory internal consistency and were factor analyzed using the Varimax procedure. Yield and Shift were found to load on different factors, supporting Gudjonsson's (1984) view that there are at least two basic types of interrogative suggestibility [Gudjonsson, G. H. (1984). A new scale of interrogative suggestibility. *Personality and Individual Differences*, 5, 303–314]. Finally, preliminary validity analyses with an early version of the Video SSC reveals that it is predictive of suggestibility errors made in an independent study, using different procedures.

**Author Keywords:** Suggestibility; Childhood; Memory; Preschool age; Factor analysis; Rating scales

## Article Outline

1. Introduction
  - 1.1. Individual differences in interrogative suggestibility
  - 1.2. Studies of children using the GSS
  - 1.3. Construction of the video SSC
  - 1.4. Hypotheses
2. Method
  - 2.1. Participants
  - 2.2. Design and procedure
  - 2.3. Measures and scoring
    - 2.3.1. Memory recall
  - 2.4. The scale questions
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    - 2.4.3. Shift
    - 2.4.4. Total suggestibility
3. Results

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- 3.5. Group differences
- 3.6. Intercorrelations among measures

#### 4. Discussion

- 4.1. Directions for future research with the SSC

#### References

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Spotlight on Practice

**Allegations of sexual abuse by  
nonverbal autistic people via  
facilitated communication:  
testing of validity<sup>1, \*1</sup>**

M. Mary  
Konstantareas<sup>a</sup>,

<sup>a</sup> Social  
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**Abstract**

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**Objective:** The aim of the paper is to offer a comprehensive approach to establishing the validity of allegations of sexual abuse by nonverbal autistic children and adults produced through facilitated communication (FC). This approach is offered as an alternative to existing methodology that relies exclusively on the "message passing" task, and has been used to advantage in the courts.

**Method:** Three components to the battery are included: (a) specialized psychometric testing; (b) a variant of the message passing task; and (c) systematic analysis of the allegations themselves. Through the juxtaposition of the data from the different sources, conclusions can be reached as to the allegations' authorship.

**Results:** The results of a series of studies addressing the issue of validity of FC in general are briefly presented. Then a case presentation is offered to demonstrate how the technique can be employed to clarify allegations of sexual abuse. Elements of two other cases are also briefly discussed.

**Conclusions:** Facilitated communication has been heralded as a breakthrough, allowing nonverbal people with autism to express themselves. It relies on manual guidance by a facilitator. Its proponents' resistance to allowing the technique's validation relying on the paradigm of normal science has resulted in its broad dissemination without support. In the case of sexual abuse allegations thus far the "message passing" task has been used to assess their verity. The present methodology is offered as a more comprehensive alternative to "message passing," with relevance to other populations of nonverbal individuals.

## Résumé

**Objectif:** Le but de cet article est d'offrir une approche globale permettant d'établir la validité d'allégations d'abus sexuel par des enfants et adultes autistes ne parlant pas, à travers la communication facilitée. Cette approche est une alternative à la méthodologie existante, qui s'appuie uniquement sur la tâche de

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"transmission du message".

**Méthode:** Trois composantes sont incluses à la batterie: (a) un testing psychométrique spécialisé; (b) une variante de la tâche de "transmission message" et (c) l'analyse systématique des suspicions. À travers la juxtaposition des données, ainsi récoltées, on peut tirer des conclusions quant à savoir qui est l'auteur des allégations.

**Résultats:** Les résultats d'une série d'études se centrant sur la validité de la communication facilitée en général sont présentés. Puis une présentation de cas illustre l'utilisation de cette technique pour clarifier les allégations. Des éléments de deux autres vignettes sont discutés.

**Conclusions:** La communication facilitée a été annoncée comme une découverte, permettant à des personnes autistes ne parlant pas de s'exprimer. Elle s'appuie sur une guidance manuelle par un facilitateur. La résistance de ses défenseurs à autoriser la validation de la technique en se fiant au paradigme de la science normale a eu pour résultat sa large dissémination sans soutien. Dans le cadre des allégations d'abus sexuel la tâche de "transmission du message" a été utilisée pour évaluer leur véracité. La méthodologie présente est proposée comme une alternative plus globale que la "transmission du message", utile aussi à d'autres populations d'individus qui ne verbalisent pas.

## Abstract

**Objetivo:** El objetivo del trabajo es ofrecer un enfoque integral para establecer la validez de los alegatos no-verbales de abuso sexual en niños autistas y adultos obtenidos por comunicación asistida. Este enfoque se ofrece como una alternativa a la metodología existente que se basa exclusivamente en la tarea de "pasar el mensaje."

**Método:** Se incluyeron tres componentes a la batería: (a) pruebas psicométricas especializadas; (b) una variante de la tarea de pasar el mensaje; y (c) análisis sistemático de los alegatos en sí mismos. A través de la juxtaposición de los datos de las

diferentes fuentes, se pudo llegar a conclusiones sobre la autoría de los alegatos.

**Resultados:** Se presentan los resultados de una serie de estudios dirigidos a la validez de la comunicación asistida en general. Después se ofrece la presentación de un caso para demostrar cómo la técnica puede utilizarse para clarificar los alegatos de abuso sexual. También se discuten brevemente elementos de otros dos casos.

**Conclusiones:** La comunicación asistida ha sido proclamada como un avance que le permite a las personas autistas no-verbales expresarse. Se basa en la guía manual de un facilitador. La resistencia de los que la proponen para permitir la validación de la técnica confiando en el paradigma de la ciencia normal ha resultado en una amplia diseminación sin apoyo. En el caso de los alegatos de abuso sexual hasta ahora la tarea de "pasar el mensaje" ha sido utilizada para evaluar su veracidad. La presente metodología se presenta como una alternativas más integral que el "pasar el mensaje," en relación a otras poblaciones de personas no-verbales.

**Author Keywords:** Sexual abuse; Autistic disorder; Facilitated communication

**Index Terms:** child abuse; autism


## Article Outline

- Introduction
- Why the persistence of employing FC despite lack of validity?
  - Allegations of sexual abuse via FC
  - Assessing the validity of abuse allegations through FC
  - A comprehensive assessment protocol
  - The case of V. W.
- Discussion and conclusion
- References

\*1 Editorial Note: Assessing abuse allegations which refer to children with learning difficulties is a challenge to child protection services and specialist

professionals alike. This paper presents an approach to this problem in circumstances where facilitated communication is an added dimension.

<sup>1</sup> An earlier version of this paper was presented at the Conference on Child Abuse in Toronto, November 1993, and the Canadian Psychological Association Conference, Charlottetown, P.E.I., June 1995.

 Reprint requests should be addressed to M. Mary Konstantareas, Ph.D., Professor of Applied Developmental Social Psychology, University of Guelph, Guelph, Ontario, Canada N1G 2W1

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[FULL-TEXT ARTICLE](#)**Comment in:**[Child Abuse Negl. 2001 Oct;25\(10\):1395-6.](#)**A case study of child sexual false allegation.****Hershkowitz I.**

School of Social Work, University of Haifa, Israel.

**PURPOSE:** The objectives of the case study reported in this article were twofold. The first objective was to follow the path by which a naive suggestion made in the course of a mother-child conversation was transformed into an allegation of severe sexual abuse. The second objective was to analyze the child's interview scientifically and explore the limitations of scientific tools for detecting implausible allegations. **METHODS:** Independent case facts were collected and analyzed to determine whether the event described by the child was likely to have happened. The credibility of the child's account was assessed using **Criterion-Based Content Analysis** and the information provided in both the "implausible" and "corrected" statements was compared to quantify the fabricated details in the implausible statement. **RESULTS:** The event described by the child was "very unlikely to have happened" but the credibility assessment failed to detect its implausibility. Comparison of the two statements revealed that the child did fabricate central details but incorporated them into a description of an event she really experienced, and most of the information provided was truthful. **CONCLUSIONS:** The pressure to conform to suggestions can be irresistible, inducing some children to make false allegations of severe sexual abuse. Scientific tools designed for credibility assessment are limited and may fail to detect implausible statements especially when they incorporate information about genuinely experienced events.

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## FALSE ALLEGATIONS AND FALSE DENIALS IN CHILD SEXUAL ABUSE

Thomas D. Lyon

Harbor-UCLA Medical Center and Los Angeles County Children's Court

The amicus brief in the Kelly Michaels case ignores the risks that abused children will fail to reveal abuse unless direct and sometimes leading questions are asked. Although the brief correctly criticizes previous research for understating the risks that aggressive interviewing practices will lead young children to make false allegations of abuse, it overstates the likelihood that false allegations occur by overlooking the aspects of the Kelly Michaels case and the research it inspired that are unlike the typical abuse case. The author discusses factors that lead abused children to falsely deny abuse and that minimize the likelihood that nonabused children will allege abuse.

Interviewers who question children about suspected sexual abuse must avoid two types of error: eliciting a false report of abuse and failing to elicit a report of abuse when abuse in fact occurred. A decision regarding how to proceed is properly based in part on the risks that a particular method of interviewing will lead to a false allegation or a false denial. The decision is also dependent, however, on the interviewer's judgment regarding the damage caused by each type of error. If one seeks to avoid false allegations at all costs, then one simply does nothing that might prompt a child to assert falsely that abuse occurred. If one wishes to minimize false denials, on the other hand, one does everything one can to encourage a young child to acknowledge abuse.

That one's judgment about the relative risks of different types of interviewing practices is based on a mixture of empirical beliefs (how likely is this practice to result in error?) and value judgments (how harmful is it to commit each type of error?) muddies the debate over children's suggestibility. Although social science research may make it possible to estimate the rates of error, it does not address the value judgment regarding how different types of error should be weighed. When researchers focus only on risks of one type of error (whether it be false allegations or false denials), they preempt the value debate, substituting their own value judgments for those of policymakers.

The amicus brief in the Kelly Michaels case rightly criticizes previous research on children's suggestibility for understating the risks of aggressive interviewing practices with young children. However, it fails to acknowledge any middle ground; it ignores the likelihood that abused children will fail to reveal abuse unless direct and sometimes leading questions are asked. It fails to consider whether the Kelly Michaels case—a case involving alleged multivictim abuse by a preschool teacher—is representative of child sexual abuse cases in general, or whether the research inspired by cases like Michaels's accurately portrays the risks of false allegations of

---

I thank Karen Saywitz for the ideas she contributed to this article.

Correspondence concerning this article should be addressed to Thomas D. Lyon, who is now at University of Southern California Law Center, University Park, 699 Exposition Boulevard, Los Angeles, California 90089-0071.



abuse. The brief thus simplifies a difficult question—how best to interview young children about abuse.

### Underestimating False Negatives

The brief does not acknowledge seriously the possibility that abused children will not reveal abuse. The perspective taken is that the only mistake worth considering is the false allegation of abuse. Such a position makes it easy to recommend that interviewers simply avoid all leading questions. Moreover, a failure to understand children's reluctance to acknowledge that sexually abusive acts have occurred makes it easy to argue that when children are led in research to "remember" fictitious events, they can just as easily be led to "remember" sexual abuse that never occurred.

The only acknowledgment of the difficulty that truly abused children have in revealing their abuse occurs in passing, when the authors attribute to "some investigative and therapeutic interviewers" the view that it is "necessary to use all available strategies," including bribes, threats, and the induction of stereotypes, to convince children to reveal abuse (Bruck & Ceci, 1993/1995, p. 274). By attributing such a view to interviewers who elicit reports of abuse at all costs, the brief makes a straw man out of a serious claim: Abused children are unlikely to reveal their abuse readily, and specific ("leading") questions may be necessary when the interviewer has good reason to believe that an abused child is having difficulty disclosing.

Sexual abuse is embarrassing. Five- and seven-year-old girls undergoing genital examinations are unlikely to recall genital and anal touching accurately; most do not disclose that either occurred. A "leading" question is therefore required ("Did the doctor touch you here?") to elicit true reports, though many still (falsely) deny being touched (Saywitz, Goodman, Nicholas, & Moan, 1991). Children who have not been subjected to intimate touching or to physical or sexual abuse are less inclined to falsely affirm abuse than they are to agree to other types of misleading questions (Goodman, Aman, & Hirschman, 1987; Rudy & Goodman, 1991; Saywitz et al., 1991). Indeed, this is a point acknowledged elsewhere by Ceci and colleagues, who note that children will obviously be reluctant to state that someone took their clothes off and kissed them while they were naked, because such acts are naughty and embarrassing. Three-year-olds are reluctant to acknowledge that their parents kissed them in the bathtub if they are told that such behavior is naughty (Ceci, Leichtman, Putnick, & Nightingale, 1993). If 5- and 7-year-old children are reluctant to acknowledge having received a genital examination by a doctor, a socially sanctioned form of intimate touching, one suspects they might be even less willing to acknowledge "coercive, repeated abuse that can instill high levels of fear, shame, and mistrust" (Saywitz et al., 1991, p. 691).

Reluctance to report negative events that have in fact occurred suggests that children will be similarly reluctant to report negative events that are only imagined. Indeed, Ceci, Loftus, Leichtman, and Bruck (1994) found that "88% of children were more likely to assent to fictional neutral events (participant or nonparticipant) and positive events than to negative events" (p. 312). Bruck, Ceci, Francoeur, and Barr (1995) successfully influenced children's memory of a year-old inoculation, but only led children to *understate* their tears and hurt. Even without any feedback, children reported less hurt a year after the shot (though not less crying).



Children are often reluctant to reveal abuse because of their desire to protect the offender. Children are most often abused by a member of their family (Smith & Elstein, 1995; Langan & Harlow, 1994). Such cases constitute a majority of the cases heard in court (whether criminal, dependency, or family court). A child who is abused by a family member will often have mixed feelings regarding the offender and not wish to cause the offender harm. Even if the child is inclined to reveal, the nonoffending parent may not be inclined to support the allegation; in one study of maternal reactions to children's reports of sexual abuse, 24% were nonsupportive or rejecting of the child and her allegations, and 32% were ambivalent or provided inconsistent support (Everson, Hunter, Runyon, Edelsohn, & Coulter, 1989; it is possible that many of the allegations were false, but then one wonders who is coercing the child to make a false accusation). Sometimes, the offender explicitly asks the child to keep the abuse a secret. Ceci and Bruck (1993) reviewed research demonstrating that 4- and 5-year-olds will keep secrets when asked to do so by a wrongdoer. (None of this research is mentioned in their brief.) Even an implicit request may be effective; 5-year-olds in Clarke-Stewart's janitor study failed to report that the janitor was playing inappropriately when they believed that revealing the wrongfulness of the janitor's acts would get him in trouble (Clarke-Stewart, Thompson, & Lepore, 1989).

Research supports the commonsensical notion that children are reluctant to accuse their parent of wrongdoing. In one study (Bottoms, Goodman, Schwartz-Kenney, Sachsenmaier, & Thomas, 1990), 5-year-olds were reluctant to acknowledge that their mother had broken a Barbie doll (see also research discussed in Ceci et al., 1993). To my knowledge, experimenters have not encouraged children to make false allegations against family members. Bruck and Ceci (1993/1995) might argue that to do so would be unethical, but they should acknowledge that a failure to do so leads to an exaggeration of children's susceptibility to suggestive questioning in general and stereotype induction in particular.

Leichtman and Ceci's (1995) research on stereotype induction minimizes the likelihood that children's familiarity with and respect for the alleged wrongdoer militates against their suggestibility. First, facts are suggested about a virtual stranger, Sam Stone; unlike a family member (or even a nursery school teacher), he spent only 2 min interacting with the children. As one child later correctly recalled, "Sam Stone had come into the classroom and said hello and looked around [while the children were engaged in listening to a story read by the teacher], but that 'nothing happened'" (Leichtman & Ceci, 1995, p. 575). A child who knows next to nothing about Sam Stone is both less inclined to disbelieve a negative stereotype and less able to rebut such a stereotype with recollections of his or her own. Second, Sam Stone's mishaps are characterized as something to laugh about, thus minimizing the child's assumption that naming Sam Stone might get him in trouble.

Even if one assumes that children are fully motivated to reveal sexual abuse, a purely cognitive variable makes it difficult for young children to discuss abuse. Young children can often acknowledge what they cannot recall. Direct questions (which are answered "yes" or "no") are sometimes considered leading, because they tend to suggest a "yes" response. At the same time, direct questions minimize the need for the child to generate the to-be-remembered material, thus tapping recognition rather than recall performance. As Ceci and Bruck noted, "age differences in recognition memory are far less pronounced than age differences in free recall, and



at times these are nonexistent" (1993, p. 404). Hence, asking young children direct questions allows them to perform more like older children, eliciting more details about the to-be-remembered event (e.g., Baker-Ward, Gordon, Ornstein, Larus, & Clubb, 1993, in which "specific" questions plus "open-ended" questions led to 3 times as much information being produced by 3-year-olds as open-ended questions alone). As always, however, an increase in accurate information is accompanied by an increase in false affirmations as well.

Research on children who have in fact been sexually abused confirms their difficulty in revealing abuse. Sorenson and Snow (1991) found that 72% of abused children initially denied having been abused when questioned by a family member or as part of the investigative process. One might argue that some of the nondisclosers were not in fact abused, because the abused group included children whose abuse was confirmed only by a guilty plea (presumably in exchange for a more lenient sentence). Therefore, it is instructive to consider children who present with specific physical findings of abuse (e.g., sexually transmitted disease, recent or healed lacerations of the hymen and vaginal mucosa) but who have not reported abuse. Only about half of such children reveal abuse when questioned (Lawson & Chaffin, 1992; Muram, Speck, & Gold, 1991).

It is possible that abused children are so reluctant to reveal that in many cases no amount of suggestive questioning will be effective. It might even be the case that nonabused children in such circumstances are even more likely than abused children to report abuse. Although the latter possibility seems unlikely (because most of the pressures that discourage true reports also operate to discourage false reporting), we must be cautious in using the reluctance of abused children to disclose as justification for coercive methods of questioning.

Recognizing that a trade-off exists between false denials and false allegations, however, is preferable to pretending that error only occurs when someone is falsely accused. Abused children have difficulty in discussing abuse. Specific questions about abuse enable and encourage some abused children to reveal, but they also necessarily increase the risk of false allegations of abuse. I believe that "leading" questions (which directly ask the child about abuse) are appropriate when a child shows other evidence of being abused (a spontaneous disclosure, suspicious medical evidence, sexualized behavior) but does not freely recall abuse when asked for free recall.

Of course, my opinion reflects both my understanding of the risks of error and my value judgment regarding the appropriate weight attributable to each type of error. In part, this is because I work in dependency court, where our actions determine whether a child will be returned to the custody of a suspected abuser, rather than whether a suspected abuser—typically already separated from the child—should go to jail. I suspect that Bruck and Ceci (1993/1995) did not acknowledge the difficulty faced by sexually abused children in revealing abuse because of their own value judgment that false allegations should be avoided at all costs.

### Overestimating False Positives

The brief reviews the research of suggestibility in light of the facts of the Kelly Michaels case. Doing so evades the need to question seriously whether the case is representative of sexual abuse cases in general. The authors explain their focus on Kelly Michaels by pointing out that this case is only about Kelly Michaels (Bruck &

Ceci, 1993/1995). Surely they appreciate the precedential impact of a state supreme court opinion in a highly publicized case. Indeed, the authors' use of the Michaels case as a "window" through which child sexual abuse cases in general can be viewed (Ceci & Bruck, 1993, p. 403) evinces a belief that the facts of the case are common, if not typical.

As already noted, most cases of child sexual abuse are unlike the Michaels case in that children are most often abused by a member of their family. Closeness of the offender and the victim increases the child's resistance to report abuse, whether abuse in fact occurred or not. Stereotype induction is less likely to be effective; indeed, it is reasonable to suppose that interviewers are less inclined to demonize alleged offenders when the child has positive feelings for the accused. Family members are less likely to encourage the child to name a family member as an abuser.

Moreover, most cases of abuse involve a single victim (Smith & Elstein, 1995; Whitcomb et al., 1994). Multivictim cases present special problems. Parents of alleged victims are likely to share stories of abuse, as are the interviewers themselves. As soon as one child has disclosed abuse, interviewers are more inclined to single-mindedly seek allegations from other children, even if those children steadfastly deny that abuse occurred. The temptation is great to use other children's revelations in a coercive manner, either to convince the child that abuse occurred, or to make the child feel disloyal by failing to confirm abuse. Children who have neither revealed nor displayed any other signs of abuse will be vigorously questioned, increasing the potential for a large number of false positives. Repeated interviews regarding previously undisclosed allegations are more common; over time, various children name other children as participants. In the single-victim case, the chances for contamination of a child's responses by the responses of other children or the shared stories of parents and interviewers are minimized.

Other features of the Kelly Michaels case make it unusual. The average age of alleged victims in criminal child sexual abuse cases is 10 years (Whitcomb et al., 1994). I suspect that the average child in dependency court is somewhat younger than 10 but older than preschool age. Prosecutors have difficulty in obtaining convictions when the victim is a preschool child (Ceci, Leichtman, & White, in press), which naturally makes them less inclined to pursue such cases. Although prosecutors have an easier time in dependency court, preschool children make poor witnesses, even when thoroughly "prepared." Therefore, although the number of cases involving preschool children has increased in many jurisdictions, the most striking facts about the Kelly Michaels case concern the prosecutors' willingness to pursue the allegations, the children's ability to testify, and the jury's willingness to believe them.

In the Michaels case, multiple (and delayed) interviews took place regardless of the child's initial statements. Similarly, long delays occurred between the target events and the repetitive suggestive questioning in the Bruck et al. (1995) inoculation study and the Leichtman and Ceci (1995) Sam Stone study. In the typical case, a child's nondisclosure of abuse will mean that multiple interviews over a long period of time will not occur. If the child refuses to speak at the initial investigatory interview, removal of the child and initiation of dependency or criminal proceedings is unlikely (Hechler, 1988). This interview takes place soon after a child abuse report is filed, given the immediate risk of harm to the child (e.g., California Penal Code Section 11166). In turn, most reports are filed soon after suspicions of abuse first

arise (Whitcomb et al., 1994; median = 2 days), and those suspicions are most often attributable to a statement by the child (Whitcomb et al., 1994: 86% of the cases). Reports of multiple interviewing in the literature typically refer to cases in which the child *has* revealed, leading to criminal and dependency proceedings necessitating continued contact with the child.

It is unknown whether the coercive interviewing practices used in the Kelly Michaels case are typical. The methods violate both state and national codes of interviewing practices (Ceci, 1994), and "[e]ven those researchers who emphasize the strengths of children's memories are highly critical of interviewing tactics like those used in the Wee Care investigation" (Bruck & Ceci, 1993/1995, p. 307). Of course, it is entirely possible that most interviewers fail to live up to professional standards. However, even were we to judge by Ceci's own sample of transcripts sent to him by defense attorneys and law enforcement, two thirds fail to contain the "potentially suggestive and stereotype inducing" methods he condemns (Ceci, 1994, ms. p. 26). I also do not know how common it is for interviewers to ask the sort of "suggestive questions" used by Ceci and others in the most recent wave of research on children's suggestibility. It seems likely that interviewers do in fact ask a significant number of leading questions (Ceci, Leichtman, & White, in press; Goodman, Sharma, Golden, & Thomas, 1991; Pettit, Fegan, & Howie, 1990). What is leading and what is suggestive, however, is an important but often overlooked distinction.

One significant feature of the suggestive questions Ceci and others typically use is that they *tell* rather than *ask* the child what occurred. In the inoculation study (Bruck et al., 1995), children were "given multiple suggestions in repeated interviews" (Bruck & Ceci, 1993/1995, p. 281). In one suggestive condition, for example, the interviewer told the child that she worked with the pediatrician, that the research assistant (RA) worked with him as well, and that the RA was present when the child received a shot; for 65% of the children (who could not identify the RA in a photographic lineup), she pointed out a picture of the RA. In suggesting that the child had not cried and that the RA had in fact given the shot, the interviewer quoted the RA as remembering what had occurred. The questions asked presupposed the truth of the suggested material and requested elaboration. In the Sam Stone study of stereotype induction (Leichtman & Ceci, 1995), children were given four "suggestive interviews" (Bruck & Ceci, 1993/1995, p. 288). The first suggestive interview presented the child with physical evidence that a bad act occurred, and the interviewer asked the child to speculate as to who might have committed it. The second, third, and fourth interviews consisted of questions that presupposed that Sam Stone had in fact performed the bad acts. Children were not asked to affirm or deny whether Sam Stone committed the misdeeds but were given a forced-choice question regarding how the acts were performed by Mr. Stone. Similarly, in the "subtle intervention" (Bruck & Ceci, 1993/1995, p. 295) used by Poole and Lindsay (in press) in the Mr. Science study, parents were instructed to read a story to their child containing fictitious events. The three readings were not accompanied by any questions regarding whether the illustrated acts in fact occurred.

The importance of telling children what happened as opposed to asking them what happened is perhaps best illustrated by the pair of studies Ceci and colleagues performed in which young children were repeatedly asked whether particular events occurred. When preschool children were simply read short statements describing

events and asked whether they occurred (having been informed that some of them had and some of them had not), children did not increasingly assent to nonexistent events over time (Ceci, Crotteau, Smith, & Loftus, in press). (The initial rates of false affirmation were alarmingly high, however, hinting that something about the method is unusually suggestive.) In contrast, when preschool children were told that the events had in fact occurred and were helped to imagine surrounding details, the expected increase in false assenting over time occurred (Ceci et al., 1994).

In recommending that interviewers avoid leading questions and in equivocating about whether leading questions are ever justified (by crediting "some interviewers" with the belief that leading questions should be used as a "last resort"; Bruck & Ceci, 1993/1995, p. 307), the authors make no distinction between suggestive questioning and leading questioning. The possibility that the research methods explored in the brief may exaggerate children's suggestibility when asked leading questions is answered by the argument that the research is less coercive than the methods used in such cases as the Kelly Michaels case. That answer is satisfactory, however, only if one's choice of interviewing methods is limited to coercive bribes, threats, and stereotyping on the one hand, and open-ended questions on the other.

A final point about the applicability of the research discussed in the brief to actual court cases of sexual abuse concerns the potential for cross-examination to undermine the confidence of children falsely claiming that they were abused. It is important to note that the New Jersey Supreme Court's decision to order taint hearings in some child abuse cases is due to the supposition that some interviewing practices may give rise to "irreparably mistaken or false recollection" (*State v. Michaels*, 1994, p. 320). The justification for a taint hearing—at which a court may prevent the child witness from appearing before a jury—is that the usual means by which truth is tested by the judicial system are subverted by pretrial techniques that render the child unshakably certain that what he or she reports has in fact occurred. If only the child's report is altered by suggestion, and not the child's memory, then the child's testimony is susceptible to the usual procedure for assessing truth: The jury observes the witness's demeanor while he or she answers questions on direct- and cross-examination.

Ceci and Bruck (1993) have elsewhere made several points that highlight the potential significance of the defendant's opportunity to challenge the child's story through cross-examination. First, they acknowledged that "acquiescence to a leading question provided at the time of testing does not in itself imply that the misinformation contained in the leading question has been incorporated into memory" (Ceci & Bruck, 1993, p. 428). Therefore, taint hearings are not justified by high rates of false assenting alone; it is necessary to demonstrate that children actually believe what they endorse. Certainly, if a child's acquiescence is simply attributable to fear of the questioner, a good cross-examiner can evoke similar fear and compliance.

Second, the authors acknowledged that researchers currently disagree over whether suggestibility effects lead to erasure of the child's original memory for the to-be-remembered event, or whether the original memory remains intact (Ceci & Bruck, 1993, p. 432). Acknowledgment of this debate is nowhere to be found in the brief, which instead relies on anecdotal support for the proposition that children sometimes form illusory beliefs that are firmly held (Bruck & Ceci, 1993/1995, pp. 301–303). Although I find the anecdotes quite compelling, the frequency with which suggested events are believed has yet to be estimated, and the certainty with which

the events are believed is unknown. If the child is susceptible to doubts, a good cross-examiner can make such doubts apparent to the jury.

Third, when researchers "gently challenge" children's false reports, such reports tend to be cut in half (Leichtman & Ceci, 1995). If a single "you didn't really see him do this, did you?" is effective, one wonders what a slightly more persistent defense attorney might be able to accomplish. There is no rule against leading questions in cross-examination; indeed, good lawyers ask nothing but leading questions when confronting the opponent's witness. Moreover, as Ceci and Bruck (1993) acknowledged, "threats and bribes are not unique to prosecution interviews; similar examples can be found in defense interviews" (p. 423).

In sum, Bruck and Ceci (1993/1995) have written a persuasive brief that compellingly argues that young children are extremely suggestible. Unfortunately, the brief owes much of its persuasive appeal to its omission of any reference to an alternate perspective regarding how young children should be questioned about abuse. By dismissing the difficulty with which sexually abused children discuss their abuse, the brief minimizes the risk of false denial. By focusing on the facts of a case in which interviewers pursued allegations at all costs, by whatever means, the brief presents a distorted picture of the methods advocated by interviewers when questioning young children. My hope is that the effect of the brief (and the research it describes) will be to move child sexual abuse zealots toward the middle, but my fear is that it will only further polarize the debate.

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