# Susceptibility to memory distortion: How do we decide it has occurred?

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When given suggestive information, some people can be led to believe that they had experiences that they did not actually have. For example, they may come to believe falsely that they got sick eating particular foods as children, and as a result of that belief they may avoid the foods. But how do we know that someone has developed a false belief or memory in this research? The criteria we choose when classifying whether someone has fallen for the suggestion are somewhat arbitrary. We reanalyze our prior data, using different criteria for deciding that a person fell for the suggestion (called a "believer") or did not (called a "non-believer"). Changing criteria obviously affects the percentage of people who are called susceptible and could conceivably affect the conclusions reached about that group. Comparisons between false memories and true memories could differ, too, depending on how memories are defined.

A great deal of work has shown that human memory is susceptible to myriad types and levels of distortion (e.g., Loftus, 1997; Loftus & Ketcham, 1994; Schacter, 1995, 2001). For instance, research has shown that memory can be altered by postevent suggestions, that is, by new information, presented after the to-be-remembered event. This information can include leading questions (Loftus, 1975), statements made by people in positions of authority (Loftus, 1979), or comments made by fellow observers (Gabbert, Memon, Allan, & Wright, 2004).

In response to postevent suggestions, small details of memory can be changed (Loftus & Palmer, 1974; see Ayers & Reder, 1998, for a review). In addition, entire, detailed false memories can be planted using such methods as imagination (Garry, Manning, Loftus, & Sherman, 1996; Mazzoni & Memon, 2003), dream interpretation (Mazzoni, Lombardo, Malvagia, & Loftus, 1999), doctored photographs (Wade, Garry, Read, & Lindsay, 2002), or, in the case of our own recent studies, false feedback presented to participants as if we had special knowledge about them. Moreover, such

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false beliefs and memories can have consequences for people (Bernstein, Laney, Morris, & Loftus, 2005).

But how do we know when someone has fallen for a suggestion and that his or her memory has changed? This issue has been addressed in several studies (e.g., Lindsay, Hagen, Read, Wade, & Garry, 2004; Miller & Wolford, 1999). We also raised this issue briefly in a recent set of studies (Bernstein et al., 2005). Using a simple false feedback procedure, we obtained data showing that adults can be given false memories of getting sick on specific food items as children. We then showed that the participants who fell for the suggestion (whom we will call believers) were more reluctant to eat the foods later on.

We discussed two separate potential definitions of believers. Both of these definitions were based on participants' movement, from pretest to posttest, along the 8-point scale of a modified life events inventory (LEI, a questionnaire in which participants are asked to rate their confidence that a number of events happened to them in their childhood; Garry et al., 1996). In the first, we defined believers as those whose confidence that the event had occurred merely increased from pretest to posttest. However, we suggested that this definition may have been too liberal, and we ultimately discussed the data using a stricter definition. According to this stricter definition, believers were those whose confidence increased from pretest to posttest *and* who reported at the end of the study that they had a "memory" or "belief" for the event. This judgment was obtained using the "memory or belief?" form (MBF), in which participants were given items (life events) and then asked to explain whether they had specific memories of the events, only general beliefs that they occurred, or neither. By including this measure, we had a more conservative definition of believers. But what if we had defined believers differently? In this article we explore the question of how we decide who has fallen for a suggestive influence and who has not. Obviously this issue must be thought through before we can ask, "What kinds of people are susceptible to false memories?" The answers provided by social science research might be different depending on how believers are defined in the research paradigm.

**How should believers be defined?**

One paradigm for studying false memories is the DRM procedure (after Deese, 1959; Roediger & McDermott, 1995). At the end of a typical DRM study, participants often are presented with a recognition test that contains words that they listened to previously (e.g., *bed, nap, pillow*), new words, and related but previously unstudied critical lures (e.g., *sleep*). Participants who recognize the critical lures on this test sometimes are classified, simply and confidently, as having false memories of having encountered these words during the study phase. But certain authors (e.g.,

Miller and Wolford, 1999) have suggested that DRM studies should not be framed in these cut-and-dried terms, and it is worth exploring the nuances of defining false recognitions in the context of the paradigm. Many subsequent DRM experiments have used additional strategies to isolate the false memory phenomenon. One such method is to ask participants whether they have a conscious recollection of having encountered the word or whether they just seem to know it was presented, without any associated perceptual detail ("remember" vs. "know"; Gardiner & Java, 1993; Tulving, 1985). Another technique is to warn participants about the paradigm and urge them to avoid false recollections when tested (Gallo, Roberts, & Seamon, 1997; McDermott & Roediger, 1998) or to assess their confidence in their judgments of whether the word is old or new (Neuschatz, Payne, Lampinen, & Toglia, 2001, Experiment 2).

In other types of studies, changes in memory are determined using confidence scales, often comparing pretest and posttest measurements. When memory accuracy or consistency is determined on a scale, as it is for all LEI-type procedures (e.g., Bernstein, Godfrey, Davison, & Loftus, 2004; Braun, Ellis, & Loftus, 2002; Garry et al., 1996), some DRM procedures (e.g., Miller & Wolford, 1999, Experiment 2), and some other types of procedures (e.g., Drivdahl & Zaragoza, 2001), a question arises about how to determine what constitutes a false memory. Is a one-point increase in confidence from pretest to posttest enough? This is a very liberal criterion, and it might create a problem because even control participants sometimes increase by one point. They might do so because of random fluctuation or the familiarity with the item that occurs the second time they are asked about it. If this minimal movement is caused by familiarity with the questionnaire, then it is meaningless in describing participants' memories. Previous research has shown that repetition increases familiarity, and this familiarity can increase belief (e.g., Begg, Anas, & Farinacci, 1992; Bernstein, 2005). And as Miller and Wolford (1999) pointed out, an assent to a critical lure on a DRM recognition test could well be the result of a guess or criterion shift and therefore should not automatically be interpreted as a false memory (cf. Gallo, Roediger, & McDermott, 2001). If this definition of *believer* is too liberal, then what definition should we use instead?

In this article we discuss four separate ways in which we can define believers (and some comparable nonbelievers) and true memories, all of which might have been used to describe a single set of data on false food beliefs and memories (i.e., Bernstein et al., 2005, Experiment 2). In order to provide a context for these definitions, we first give a brief overview of the method of the false food belief study. Finally, we present a reanalysis of the data using the various definitions and discuss the practical and theoretical implications of carving the data in different ways.

# EXPERIMENT

## METHOD

### Overview of the false food belief study

In our previous study, we suggested to 180 participants that they had gotten sick after eating either dill pickles or hard-boiled eggs as children (Bernstein et al., 2005, Experiment 2). The suggestion was delivered as a set of "personalized" feedback items that participants were told were the result of a number of questionnaires that they had completed previously but were in fact the same for everyone in the relevant experimental condition. Two separate suggestions (one about getting sick on dill pickles, the other on hard-boiled eggs) were used for two different groups of participants. Participants were randomly assigned to the two groups, and each group served as a control for the other. Response to the suggestion was measured by having participants complete the same modified LEI both before and after the test (with approximately 1 week between these two sessions).

In this study, the modified LEI was dubbed a food history inventory (FHI) and contained 24 food-related life events. It asked about experiences that participants might have had before the age of 10 (e.g., "Ate freshly picked vegetables," "Bought school lunch"). Participants were instructed to respond on an 8-point scale ranging from 1 (*definitely did not happen to them before age 10*) to 8 (*definitely did happen before age 10*). Embedded in this questionnaire were two critical items ("Felt ill after eating a dill pickle" and "Got sick after eating too many hard-boiled eggs"), which were in positions 9 and 16, respectively.

In addition, participants completed two separate consequence measures. In the first (intention to eat) measure, they were asked how likely they were to eat each of 37 different foods at a party, including two critical items (dill pickles and hard-boiled eggs) and other closely related foods. In the second (food preference) measure, they were asked how much they generally liked each of 64 different foods (also including the critical food items and close relatives). Both measures used 8-point Likert-type scales.

The very last questionnaire completed by participants was the MBF, which queried whether participants had a specific memory, from before the age of 10, for each of three items borrowed from the FHI. The second of these items was the critical item (pickle or egg, depending on group). For each item, the participant was instructed to write an *M* if he or she had a specific memory of the event, a *B* if he or she believed that the event had happened but lacked a specific memory of it, or a *P* if he or she was positive that the event had not occurred. In each case, the participant was instructed to write a sentence or two explaining why he or she responded the way he or she did.

### Definitions of believers

We explored four ways of defining what characteristics were required to classify someone as a believer of the suggested manipulation. See Table 1 for a summary of all four proposed definitions of believers and nonbelievers (as well as a definition of true memories, as described in the next section). Using the most liberal definition, we might define believers to include those who had any positive FHI movement from pretest to posttest (we call these liberal believers). By this defini-

Table 1. Proposed definitions of believers, nonbelievers, and true memories

| Type | Pretest FHI | FHI change | Posttest FHI | MBF |
|---|---|---|---|---|
| Liberal believers | — | Positive | — | — |
| Liberal nonbelievers | — | Stable or negative | — | — |
| Conservative believers | — | Positive | — | M or B |
| Conservative nonbelievers | — | Stable or negative | — | P |
| Low-start conservative believers | 1–4 | Positive | — | M or B |
| Ultraconservative believers | 1–3 | Positive | 6–8 | M or B |
| True memories | 6–8 | — | 6–8 | M or B |

*Note.* The cells represent the criteria for group membership. Where no criterion is listed, there is no requirement. B = belief; FHI = rating on the food history inventory; M = memory; P = positively not on the "memory or belief?" form (MBF).

tion, a believer might move from 1 to 8 on this scale but might also move from 1 to 2 or from 5 to 7 (for example). Those who stay at the same FHI rating or move down are labeled liberal nonbelievers.

As mentioned before, such movement might result from other phenomena besides a false memory. Therefore, a second, somewhat stricter definition of believers might include those who both increase their confidence from pretest to posttest (as defined by positive FHI movement) and report some sort of subjective memory at the end of the study. We call these conservative believers. This was the primary definition used in the Bernstein et al. (2005) article. In analyses reported in that article, the requirement for a subjective memory was met by participants reporting a belief or memory on the MBF. So, by the conservative definition, a believer might have been someone who moved from 3 to 5 on the FHI and then claimed on the MBF to have a belief that the critical event had happened, or someone who moved from 1 to 6 on the FHI and ended the study reporting a memory on the MBF.

What is the appropriate comparison group for conservative believers? In our previous study, we compared this group with both everyone in the same experimental condition who did not meet these criteria (called nonbelievers) and those who received the alternative feedback (nonexposed or control participants). Thus, participants who met the criteria for conservative believer after receiving the egg feedback were compared both with those who received the egg feedback but did not meet the criteria for conservative believer and with those who received the pickle feedback. There are advantages to both of these comparison groups. When compared to believers, the nonbelievers allow us to explore the effects of "buying" the experimental manipulation. The nonexposed participants served as controls in our study, so they are also an important comparison group. They allow us to compare our manipulation with a pure baseline. By using both comparisons, we can see whether mere exposure to the feedback (i.e., simply reading about the critical items without necessarily believing them) is enough to change participants'

feelings toward the manipulated food items and whether falling for the suggestion has an additive effect on top of any effect of mere exposure to the suggestion.

For this article we propose an additional, somewhat more specific comparison group for conservative believers. Because conservative believers are required to have a subjective memory (an *M* or *B* on the MBF) in addition to positive FHI movement, conservative nonbelievers are required to lack any subjective memory. That is, they must have answered *P* for their critical item on the MBF, indicating that they were positive the event never happened, in addition to not increasing their confidence on the FHI. Thus, a typical conservative nonbeliever might rate the critical FHI item as a 1 both before and after the feedback (or perhaps a 3 before and a 2 after) and then claim to be positive that the event had not occurred. This is a sensible comparison group because it restricts group membership for believers and nonbelievers similarly and gives us groups of comparable size and theoretical construct. But the disadvantage of this definition of nonbelievers is that it leaves out a substantial portion of our sample because many participants meet neither the criteria for conservative believer nor the criteria for conservative nonbeliever. For this reason, we propose discussing multiple comparison groups (e.g., both control participants and conservative nonbelievers) for a group of conservative believers.

A third definition of believers (called low-start conservative believers in Table 1) adds a requirement to those of the conservative believers: Participants must begin at pretest with an FHI rating in the lower half of the 8-point scale. This requirement ensures that participants are starting out reasonably sure that the event did not happen to them.

The fourth and final definition takes this one step further and is the most conservative. This definition takes into account the fact that the FHI does not provide respondents with an exact midpoint, which may lead them to treat ratings of 4 and 5 on the 8-point scale as equivocal. Thus, the final group, called ultraconservative believers, must start below this midpoint on the scale at pretest (with a rating of 1–3) and end above this midpoint at posttest (with a rating of 6–8). As with the previous definitions, ultraconservative believers must also wind up with a subjective memory (a *B* or *M* on the MBF). Rather than proposing additional groups of nonbelievers for these last two definitions, we suggest that these believers can be compared with the same groups of participants as conservative believers.

## True memories

The previous discussion outlined several definitions of believers and nonbelievers, that is, those who do and do not succumb to an experimental suggestion. In addition, we can compare those who fell for the experimental suggestion—those with false beliefs or memories—with participants who have true memories of the incident. But how do we decide when someone might have a true memory?

Like the decision of how to define a false memory, the decision of what constitutes a true memory is also somewhat arbitrary, and many definitions may be proposed. However, some do not delineate appropriate boundaries and are either too conservative or too liberal. Rather than outline numerous iterations, we will focus on one definition that strikes a reasonable balance.

Our definition of participants with true memories of getting sick on the key foods is straightforward. Recall that the FHI offers a participant the opportunity

to indicate his or her confidence on an 8-point scale that several events occurred during his or her childhood. Someone with a true memory presumably would indicate a high degree of confidence at pretest, that is, before exposure to the experimental feedback. But what constitutes a high degree of confidence? For our purposes, we categorize a pretest FHI rating of 6 or above as an indication of a true memory. Such a rating avoids the midpoint of the scale (ratings of 4 and 5). In addition, this confidence in the event's occurrence should persist at posttest because it would be difficult to contend that movement from an 8 all the way down to a 1 or 2 would demonstrate a true memory.

Such a designation still assumes that FHI ratings of 6, 7, and 8 are equivalent to each other. However, as mentioned before, even control participants fluctuate somewhat on scales such as the FHI with repeated administration. Our definition of true memories requires respondents to show a consistently high degree of confidence—operationalized by an FHI rating of 6 or above on both occasions—but it does not require that respondents maintain exactly consistent high ratings. Thus, a respondent who answers with an 8 and then a 7 is not automatically eliminated under this definition.

Finally, as with most of the believer definitions outlined earlier, our definition of true memories requires participants to have some sort of subjective memory of the critical event. Therefore, we can eliminate any respondent who entered a *P* (positive the event did not happen) on the MBF, even if he or she consistently rated his or her confidence in the upper portion of the FHI.

This definition of true memories overlaps slightly with our definitions of believers, particularly conservative believers. If, before being exposed to a manipulation, someone is confident that he or she experienced an event, then that person is technically a type of believer because he or she also accepts the feedback item as being true. However, if the event truly did occur, his or her belief is not false, and we should exercise caution before categorizing the person as we do our other believers.

## RESULTS

### Statistical basis for definitions

Our definitions thus far have been purely theoretical constructs, categorizing participants based on their responses to the measures we have devised (i.e., the FHI and MBF). However, statistically speaking the MBF is a rational complement to the change in FHI rating from pretest to posttest. Recall that the MBF allows three responses: *M* for a memory of the event, *B* for a belief the event happened, and *P* for being positive it did not happen. If we recategorize the MBF responses into "yes" (both *M* and *B*, $n = 89$, coded as 1) and "no" (*P*, $n = 91$, coded as 0), we can calculate the point biserial correlation between FHI change and the dichotomous MBF response. Indeed, a significant positive correlation exists, indicating that greater FHI change from pretest to posttest is related to a greater incidence of affirmative MBF responses ($r_{pb} = 0.34$, $p < .001$). When re-

peating this procedure among just the "yes" responders (coding *B* as 0 and *M* as 1), we find that FHI change does not differentiate between *M* and *B* responses ($r_{pb} = 0.06$, $p = .57$). Thus, there is a statistical basis for our definitions because FHI change is predictive of a subjective memory *or* belief of having gotten sick on a particular food as a child.

**Preliminary analyses**

Although our original study had two separate experimental groups (one that received feedback about dill pickles and another that received feedback about hard-boiled eggs), for our present purpose these groups are unimportant. Instead, what matters is whether participants believed the feedback that they received (regardless of whether that feedback told them that they got sick on dill pickles or hard-boiled eggs). Therefore, for this article we have collapsed the two groups across their two critical items. Thus, believers are those who believed their feedback (and nonbelievers are those who did not believe their feedback), no matter which feedback that was.

To reduce the complexity of the consequence measures used in our original study, for current purposes, we have culled them down to two index measures. The first, an intention-to-eat index, combines two separate items for each group: dill pickle spears and pickle slices for the pickle group (Cronbach's $\alpha = .69$) and hard-boiled eggs and egg salad for the egg group (Cronbach's $\alpha = .73$). Each participant's index score is the mean of his or her scores on his or her two critical items (i.e., the two pickle items if he or she received pickle feedback or the two egg items if he or she received egg feedback). The second index similarly combines one pickle item (dill spears, because no other closely related food behaved sufficiently similarly) and three egg items (hard-boiled eggs, deviled eggs, and egg salad; Cronbach's $\alpha = .81$) into a single food preference index.

**Definitions of susceptibility**

How do the four separate definitions of susceptibility proposed earlier affect our false memory and consequence data? Summary data for each definition are provided in Table 2. The first and most obvious effect of changing the definition of susceptibility is the number of participants who are classified as believers and nonbelievers. Although all 180 participants in the study can be classified as either believers or nonbelievers by the liberal definition, 57 participants (32% of our sample) qualify as neither believers nor nonbelievers by the conservative definition. With the stricter definitions of believers, even fewer participants meet the relevant criteria: 40 participants (22% of the sample) for the low-start conservative definition and just 18 participants (10%) for the ultraconservative definition. These differences will have a substantial effect on the relative power of analyses involving the different definitions.

Table 2. Summary data for all exposed and nonexposed participants, for each of four types of believers, and for those with true memories

| | *n* (% of sample) | FHI pretest (*M*) | FHI posttest (*M*) | FHI change from pretest to posttest | Intention to eat index | | Food preference index | | MBF | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | *M* | *SD* | *M* | *SD* | M | B |
| Exposed: pickle manipulation | 89 (49) | 2.25 | 3.13 | 0.88 | 3.85 | 2.39 | 4.00 | 2.53 | 9.0% | 32.6% |
| Exposed: egg manipulation | 91 (51) | 2.29 | 2.92 | 0.63 | 4.00 | 2.31 | 4.29 | 2.15 | 11.0% | 46.2% |
| Exposed: total | 180 | 2.27 | 3.03 | 0.76 | 3.93 | 2.35 | 4.14 | 2.34 | 10.0% | 39.4% |
| Nonexposed | 180 | 2.19 | 2.29 | 0.10 | 4.28 | 2.19 | 4.35 | 2.32 | — | — |
| Liberal believers | 68 (38) | 2.07 | 4.92 | 2.85 | 3.58 | 2.34 | 3.98 | 2.29 | 13.2% | 60.3% |
| Liberal nonbelievers | 112 (62) | 2.38 | 1.88 | −0.50 | 4.14 | 2.33 | 4.24 | 2.38 | 8.0% | 26.8% |
| Conservative believers | 50 (28) | 2.24 | 5.32 | 3.08 | 3.21 | 2.17 | 3.69 | 2.16 | 18.0% | 82.0% |
| Conservative non-believers | 73 (41) | 1.84 | 1.34 | −0.50 | 3.77 | 2.38 | 4.00 | 2.54 | — | — |
| Low-start conservative believers | 40 (22) | 1.95 | 5.15 | 3.20 | 3.18 | 2.26 | 3.88 | 2.19 | 15.0% | 85.0% |
| Ultraconservative believers | 18 (10) | 1.44 | 6.72 | 5.28 | 2.92 | 2.16 | 3.56 | 2.15 | 27.8% | 72.2% |
| True memories | 10 (6) | 6.80 | 6.70 | −0.10 | 4.65 | 1.96 | 4.47 | 2.76 | 40.0% | 60.0% |
| Adjusted believers (true memories removed) | 46 (26) | 1.87 | 5.13 | 3.26 | 3.09 | 2.11 | 3.61 | 2.13 | 16.7% | 81.1% |

*Note.* FHI = food history inventory; MBF = "memory or belief?" form.

The four definitions also show substantial differences in pretest and posttest FHI ratings. Some of these differences are expected outcomes due to the way that the different definitions were constructed, but they are worth examining nonetheless. According to the third data column of Table 2, the mean posttest FHI ratings for liberal believers and nonbelievers were 4.92 and 1.88, respectively. Conservative believers and nonbelievers gave mean posttest ratings of 5.32 and 1.34, respectively. Based on these values, the posttest FHI rating differences between believers and nonbelievers (between participants) were 3.04 points for the liberal definition and 3.98 points for the conservative definition. The magnitude of the difference between believers and nonbelievers increases when a more conservative definition is used. For a more extreme example, the difference between ultraconservative believers ($M = 6.72$) and nonexposed participants ($M = 2.29$) was 4.43 points. If ultraconservative believers are compared with conservative nonbelievers ($M = 1.34$), the difference is 5.38 points. These differences are particularly impressive because the maximum possible difference between the two ratings is 7 points (i.e., between 1 and 8 on the 8-point scale).

According to the fourth data column of Table 2 (FHI change), the within-participant differences between pretest and posttest FHI ratings for believers ranged from 2.85 points (for liberal believers) to 5.28 points (for ultraconservative believers). These changes can be compared with two different baseline ratings: all nonexposed participants (i.e., all participants' ratings on the item on which they were not manipulated) increased an average of 0.10 points, and all exposed participants (regardless of whether they believed their feedback) increased an average of 0.76 points. That is, participants who are not exposed to a particular feedback item tend not to increase their FHI rating of that item from pretest to posttest, and participants who are exposed to a particular feedback item increase an average of only three quarters of a point. But participants who are exposed to a feedback item and fall for the suggestion that the critical event happened to them increase on average between 2.85 and 5.28 points, depending on which definition of *believer* is used. Clearly, the ultraconservative believers increased their confidence substantially, a result that is partially masked by the additional respondents in the liberal believer group. Thus, the way susceptibility is defined has a large effect on the degree to which participants increase their confidence: Believers who were defined more strictly became much more confident that the critical event occurred.

How do our consequence results change depending on how we define believers? The primary change, again, is in the power of our analyses. That is, with more conservative definitions, we will necessarily have fewer group members and thus less power. Although we lose power, we gain changes in mean ratings. See Table 2 for the means and standard deviations for the consequence index measures. First, those who were not exposed to

the manipulation have mean ratings of 4.28 and 4.35 on the intention to eat and food preference measures, respectively. In comparison, those who were exposed but did not buy the manipulation (liberal and conservative nonbelievers) are less likely than their nonexposed counterparts to show willingness to eat or preference for the manipulated foods. Moreover, as expected, those who believed the suggestion show even less liking and willingness to eat the critical foods. However, once again, how much lower their ratings fall depends on the definition used. Whereas the liberal believers have mean ratings of 3.58 and 3.98 on the intention to eat and food preference indexes, respectively, the conservative believers have mean ratings of 3.21 and 3.69. The ultraconservative believers fall even lower, with means of 2.92 and 3.56 on the consequence measures. Thus, it appears that those who are classified by a stricter definition of a supposed false memory of a food-related illness exhibit greater consequences of that memory (exhibited as a decreased liking of and intention to eat dill pickles or hard-boiled eggs). This can also be seen in Figure 1.

**Memories versus beliefs**

There are also differences between the four definitions in the proportion of participants claiming to have a specific memory. (Again, these differences are, to some extent, determined by the definitions themselves.) Overall, 10% of participants indicated that they had memories of getting sick on their critical food item, and 39.4% had beliefs. Liberal believers

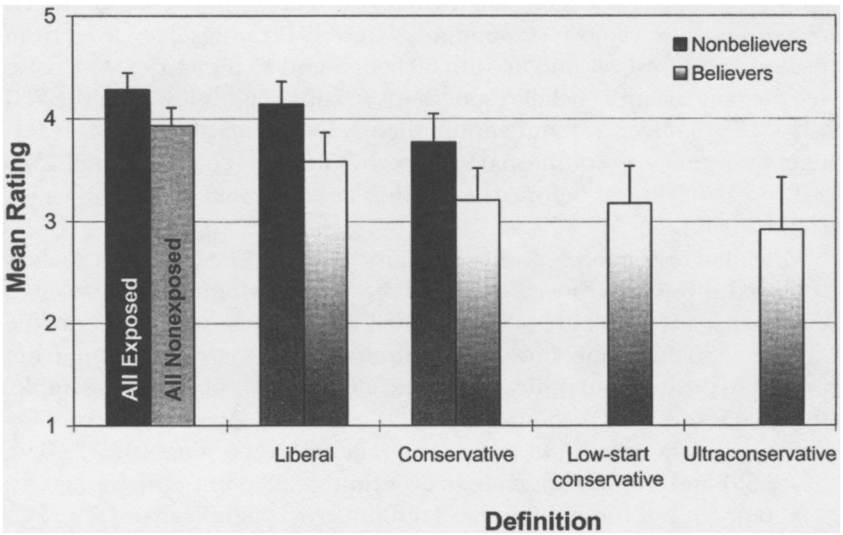


Figure 1. Participants' expressed willingness to eat foods (either pickle or egg related) that they were told they got sick on as children. Error bars represent standard errors of the mean

were somewhat more likely to report both memories (13.2%) and beliefs (60.3%) than the overall sample, whereas liberal nonbelievers were less likely to report either (8.0% and 26.8%, respectively). At the opposite end of the scale, ultraconservative believers, who were required to have a memory or belief to be labeled as such, reported a higher proportion of memories than any other group of believers (27.8%). Thus, the way susceptibility is defined also has an effect on the proportion of participants who report having memories.

Do those who claim to have a memory differ from those who claim to have a belief, in terms of their behavior on the FHI? As we saw before, FHI change from pretest to posttest was predictive of an *M* or *B* response but did not differentiate between the two. Despite our limited sample size, partitioning the data in different ways might reveal trends that otherwise would not be evident.

Initially, we can compare all the *M* ($n = 18$) and *B* ($n = 71$) responders. Those with a mere belief tended to start out lower on the FHI ($M = 2.63$ vs. $M = 3.22$ for memory responders) and end lower at posttest ($M = 4.06$ vs. $M = 5.00$), indicating less confidence that the event occurred. However, the groups do not appear to be significantly different on their pretest, posttest, and change FHI ratings (using the Welch statistic to account for unequal variances, all $ps > .20$). Keep in mind, though, that the only requirement for these participants was to answer *M* or *B* on the MBF; their FHI ratings were not restricted.

Thus, a logical next step is to further restrict the sample based on FHI responses. If we remove respondents whose FHI ratings decreased from pretest to posttest, we end up with 60 beliefs and 15 memories. Again, we see the same trend of belief respondents starting slightly lower on the FHI ($M = 2.23$ vs. $M = 2.53$) and ending slightly lower ($M = 4.20$ vs. $M = 4.93$) than their memory counterparts and exhibiting less change overall ($M = 1.97$ vs. $M = 2.40$). As before, these differences did not reach significance (all $ps \geq .40$).

One final restriction proves much more revealing. The previous analysis removed those who moved down on the FHI over time, but it retained the participants who did not change their rating. Thus, we can restrict the sample to include only those who both increased their FHI rating from pretest to posttest and indicated having a memory or belief. This sample, then, contains 41 *B* responses and 9 *M* responses. Again, as before, belief responders started out lower than memory responders ($M = 2.00$ vs. $M = 3.33$) and showed less change over time (2.88 points, on average, vs. 4.00 points), but these differences did not reach significance ($ps > .17$). However, the posttest FHI rating did differ significantly between groups. Respondents who said they had a belief at the end of the study gave an average posttest FHI rating of 4.88, whereas memory respondents wound up averaging 7.33 on an 8-point confidence scale; Welch$(1, 29.9) = 49.6$, $p <$

.001. Thus, it appears that when participants increased their confidence in the event's occurrence *and* reported having a specific memory, they actually increased their confidence substantially and ended up significantly more confident in the memory than their counterparts who responded with only a belief. If our sample were large enough, we could explore this ad infinitum; for instance, we could see whether the various categories of *M* and *B* responders differ on any of our consequence measures.

**What about true memories?**

As mentioned before, someone with a true memory may be thought of as a type of believer, and indeed, some categories of believers already outlined may overlap with those with true memories. However, true memory respondents are not believers in the way we conceive of believers (i.e., with an implied *false* preceding the term). What happens when the definitions overlap? Should we remove true memory respondents from our analyses of believers, and if so, how much difference would it make?

In order to answer these questions, we removed potential true memory respondents from the group of conservative believers ($n = 50$). This group was chosen because it was the primary definition used in the Bernstein et al. (2005) study. Recall that conservative believers are categorized by positive FHI movement from pretest to posttest and an *M* or *B* on the MBF. True memories, on the other hand, require a pretest and posttest FHI rating of 6 or higher and a subjective memory (*M* or *B* on the MBF). Based on these criteria, 10 out of 180 participants fell into this category for the current study. However, because conservative believers are already characterized by positive FHI movement and a subjective memory or belief, the only additional requirement for a true memory would be the pretest and posttest FHI values of 6–8. Thus, only four supposed true memory respondents actually overlapped with the subset of conservative believers, and they were removed for the present analysis (we refer to the resulting sample as adjusted believers, $n = 46$).

One initial observation from this analysis is that the FHI posttest mean rating decreases from 5.32 to 5.13, indicating that believers, as a group, have slightly less confidence in the event's occurrence when several strict true memory respondents are no longer included. Indeed, several studies have shown respondents to be more confident of their true memories than their false ones (Loftus & Pickrell, 1995; Pezdek, Finger, & Hodge, 1997). It appears as if memories for actual occurrences may have partially driven posttest FHI confidence for the initial group of conservative believers.

Next, we can evaluate changes in the study's consequence measures. During the second session of the study, participants indicated their willingness to eat and liking of several foods, including the critical items (pickle or egg) and related items. For both of these measures, the adjusted group showed a slight decrease in mean willingness to eat ($M = 3.09$) and their

preference for the critical and related food items ($M = 3.61$), compared to the parent group of conservative believers ($M = 3.21$ and $M = 3.69$ for willingness to eat and preference, respectively). This has the effect of increasing the intergroup difference between conservative nonbelievers ($M = 3.77$ and $M = 4.0$ for willingness to eat and preference, respectively) and adjusted believers when true memories are removed (see Table 2).

A final consequence of removing true memories involves the actual reporting of subjective memories by participants. The removal of true memories, as we defined them here, decreased the number of *M* and *B* responses each by two cases. Overall, the proportion of *M* and *B* responses was not greatly affected by the removal of true memories.

Indeed, this appears to be the case with the three outcome measures reported here. In our current illustration, the numerical differences that arise are not great enough to have any significant impact. However, it is plausible that under the right circumstances such a shift could yield a statistically significant difference between groups of believers and nonbelievers where previously there was none.

## DISCUSSION

We applied several novel definitions of believers and nonbelievers to our data and found that the conclusions that one reached about the data could change. For instance, we assessed posttest confidence, in terms of whether participants bought the experimental suggestion. The magnitude of the difference between those who did and those who did not believe the manipulation increased with a stricter definition of *believer*. In addition, the application of progressively stricter definitions led us to identify a class of participants who came to be quite confident over time that the false event in question had actually occurred. Finally, more restrictive definitions revealed greater consequences that resulted from the false memory.

How should we operationalize false memory, and how do we determine that it has occurred in our studies? Usually, we are looking for some sort of a "no" that turns into a "yes." In some cases, this may manifest as a declaration such as "I don't remember that happening" that transforms, over time, into a detailed account of an event. But, as we have seen, there are degrees of "yes." Some of our participants say they have a memory, but others just believe it might have happened. Some embellish with unique details, and others merely repeat what we told them. Some increase their confidence greatly, whereas others shift only a point or two. And of course, many respondents fall in between. How do we determine which are manifestations of the phenomenon we are studying (i.e., signal) and which are just noise?

We chose four separate definitions of believers and two companion definitions of nonbelievers to describe and analyze in the current article. Each

of these definitions represents specific theoretically important attributes of false memories, and the varied definitions cover a wide range of possibilities. But there are numerous other definitions that we might have included. For example, we might have required that every believer have a specific, detailed memory for the false event (i.e., that he or she responded with an *M* on the MBF). We chose not to use this criterion for two reasons. First, it would have reduced our sample to just 18 believers. Second, seven of those believers rated the critical item between 1 and 4 (i.e., unlikely to have occurred) on the postmanipulation FHI. This effect reinforces our claim that the use of two or more separate measures, with different rating systems, can lead to more converging evidence of false memories.

In our earlier article (Bernstein et al., 2005) we used a conservative definition of believers and comparison groups of those not meeting the criteria for this definition and those not exposed to the same manipulation. In the present analyses, we relied primarily on the same conservative definition of believers but with a new, more specific definition of nonbelievers. These two definitions provide groups of participants that we can be reasonably confident did and did not (respectively) fall for the suggestion presented in our false feedback.

**True memories**

We further adjusted our believers in the present work by eliminating participants who reported what we defined as a true memory of the critical event. Ultimately, this appeared to have little effect on our outcome measures, possibly because very few respondents ($n = 4$) were removed with our conservative designation.

Clearly, numerous characterizations of true memories are possible. We did not outline every possible definition, but if we wanted to characterize more true memories (in order to compare them with false beliefs), we could loosen our criteria. For instance, a requirement of only a 5 or higher on both FHIs would give us 19 true memories (10.6% of our total sample). However, we then run the risk of not accurately capturing true (and only true) memories. We could go to the other extreme and declare that only a rating of 8 indicates total confidence and thus denotes a true memory. Unfortunately, this severely limits the viable number of respondents, to the point at which analysis is impossible. Furthermore, some participants may not use the full range of a scale such as the FHI (e.g., see Chen, Lee, & Stevenson, 1995, for a study of cultural differences) and might be mistakenly eliminated under such a stringent criterion.

Instead, we could require the designation of *M* on the MBF, but again, by using such a method, we may be unintentionally excluding true memories in our overzealous conservatism. On closer examination, it appears that some participants may not understand the distinction between a memory and a belief. Our conception of memory and belief, as described on the

MBF, is similar to the remember–know judgment and corresponds to complete and partial memories discussed in the literature on false memories (see Lindsay et al., 2004). Whereas our participants' memories may not all be false, they can still be similarly divided into either a genuine belief of the occurrence, supplemented by unique information and sensory detail, or a mere acceptance of the incident. However, respondents often pair a *B* with a specific description of a unique event that would otherwise be classified as a complete episodic memory (e.g., "My dad bought me a hamburger with pickles and I felt sick afterward."). Alternately, other participants designate an *M* along with something like a vague declaration that they "guess it probably happened" (e.g., "I used to *love* dill pickles so as a child, I probable [sic] ate too many and got sick."). Should we, the experimenters, classify the participants and assign memories based on the offered exposition? Studies such as Lindsay et al. have done just that, with judges (blind to condition) categorizing respondents' memories. An additional rating, besides that of the participant, undoubtedly would prove informative. In the current study, a comparison of ultraconservative believers ($n = 18$) and true memories ($n = 10$) indicates that neither group is significantly more likely to define their subjective experience with an *M* or *B* on the MBF; $\chi^2(1, n = 28) = 0.44$, *ns*. Nor are they more or less likely to offer a free response description of a complete or partial memory. Thus, true and false memories may be difficult to distinguish, even when one uses MBF-type questionnaires.

This brings us to the issue of the nature of belief and memory. Believers of a false event and those with true memories may differ on our measures (namely, FHI confidence pretest and posttest), but their postsuggestion experience could be subjectively similar. Lindsay et al. (2004, p. 152) noted that "false memories were as compelling as memories of the true events" according to their measures. Their observation is made for a within-participant comparison of different memories, whereas our data evaluate a similar memory that appears to be theoretically true or false for different participants. Yet in both cases there is no bias according to type of belief or memory. We may have expected our true memory respondents to be more likely to answer *M* on the MBF than believers or to exhibit more complete memories. However, this was not the case. If true and false beliefs are actually experienced in the same way for participants, then perhaps removing true memories from our believer data is unnecessary. Theoretically, removal could end up reducing a dataset in quantity only, rather than quality. However, although the current analysis tentatively indicates that the removal of true memories from a set of false beliefs may have little effect, we contend that researchers in future work should theoretically and statistically justify keeping true memories in their sample of false memories.

Furthermore, future research should confront this question, especially in the context of autobiographical memories, and should not neglect true memory phenomena in favor of false. Rather, researchers should take the opportunity to tease apart any potential differences between beliefs and memories that may be true and those that are merely suggested. If investigators of false memory wish to define believers in such a way as to exclude true memories, our low-start conservative definition is one option. Before receiving any false feedback about a critical event, these participants begin reasonably certain that the event did not happen to them. After the feedback, they are reasonably certain that the event did happen to them, and they indicate having a belief or memory of the event.

**Limitations of the present study**

The current analysis does have certain limitations that are worth mentioning. First, we were constrained by the size of our dataset, which dwindled in the face of increasingly conservative definitions of believers, nonbelievers, and true memories. Therefore, we were unable to fully explore the potential ramifications of carving our dataset in myriad ways. We noted earlier that the primary result of more restrictive definitions was a loss of power, that is, fewer respondents were included in the dataset when a more conservative definition was used. This is an obvious consequence of such action, but that does not mean it is not worth consideration. An experimenter must decide on his or her priorities when power is at issue. Should one loosen the criteria in order to include more respondents in the analysis or run more participants through the protocol in order to gain power while keeping the desired definitions intact? A challenge exists in the need to balance theoretical constructs and real-world practicality, and one must decide how much noise he or she is willing to tolerate in the data, noise that may mask the true experimental effects. Furthermore, future researchers may want to use and report on more than one definition. Over time, this could help researchers pinpoint which definition (if any) might be the gold standard.

Another limitation of the present analysis is that the definitions laid out may not be directly applicable to other studies. Because the specific data that we used were from a false feedback study using an FHI and MBF, we consistently discussed our definitions in terms of participants' responses on those two measures. From a methodological standpoint, although many studies have used versions of the LEI (from which the FHI was derived; e.g., Braun et al., 2002; Garry et al., 1996; Heaps & Nash, 1999; Mazzoni et al., 1999; Mazzoni & Memon, 2003), no scale has been adopted as a standard in false memory research. Although such a standard would have obvious merit (i.e., facilitating comparisons across research studies), that issue was not the focus of the present discussion. Instead, we raised theoret-

ical issues that researchers might address within the confines of their own measures. For instance, a researcher might reconsider whether a 3-point confidence scale is sensitive enough to adequately classify participants' experiences. Or an experimenter might decide to include a pretest measure of confidence, in order to observe change over time, or supplement existing measures with a different type of rating system. Regardless of what particular scales or measures a researcher uses, the underlying concepts of the current discussion apply more broadly to memory research, and the definitions could easily be modified and used to describe the success of suggestibility measures in other types of scaled studies.

Finally, throughout our discussion of believers, nonbelievers, and true memories, we proposed numerous classifications. Yet even with numerous categories, there are still phenomena that we may have overlooked. What about a true memory that is not retrieved at pretest yet called up at posttest? As of yet, we have no way to adequately capture such a phenomenon. This occurrence, on paper, appears identical to that of an implanted false belief (e.g., moving from a 2 to a 7 on the FHI and giving a *B* on the MBF). We may attempt to control for such circumstances by probing for memory at pretest as well as posttest (in such a way as to disguise the intent of the study). However, although this may help in identifying some people, it would not solve the problem of a truly irretrievable pretest memory because a participant would not necessarily be able to answer with any more insight at that time. Instead, independent corroboration may prove useful, especially when dealing with instances of childhood events (i.e., asking parents). However, this is also not a complete solution because the critical event could have occurred without the parent's or accomplice's knowledge. One way to steer clear of such a problem is to avoid past autobiographical memory paradigms and favor those in which a memory is both constructed and tested under laboratory supervision (e.g., Thomas & Loftus, 2002).

Indeed, with a paradigm such as the one used by Bernstein et al. (2005), we must keep in mind the important caveat that we can only take our participants' responses at face value. The study described herein involves a manipulation pertaining to subjective autobiographical information, and we have no way of independently verifying the authenticity of an alleged memory. Therefore, our definitions must remain effectively quantitative, that is, based on numeric indicators of confidence on two separate occasions (FHI ratings). Although this is supplemented with a more qualitative measure (the MBF), such steps do not necessarily solve the problem entirely (e.g., participants' confusion regarding the applications of the terms *memory* and *belief*). Nonetheless, despite these limitations, the delineation of believers and true memory participants, as well as comparable nonbelievers, is an important distinction that merits further exploration.

## Notes

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