

Do Justice and Let the Sky Fall

Elizabeth F. Loftus and Her Contributions to Science, Law, and Academic Freedom

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CHAPTER
2

Tracking the Birth of a Star



Gordon H. Bower

I first met Beth Loftus (formerly Fishman) when she entered Stanford's PhD psychology program in 1966. She had been admitted as the sole woman in the cohort of young men brought into the mathematical psychology program. Beth fit the criteria as an ideal applicant because she had a joint major in psychology and mathematics from UCLA, a combination eminently suited for our program. Mathematical learning theory was (and probably still is) a field dominated by males—the power players were Bill Estes, Duncan Luce, George Miller, Bob Bush, Pat Suppes, Dick Atkinson, Bennett Murdock, Frank Restle, and Eugene Galanter. Beth was distinctive and unique: Not only was she well trained in higher mathematics, she was also a very motivated, enthusiastic, engaging, and efficient worker. Moreover, overriding the stereotype threat under which she operated, Beth's intellectual skills and competence quickly commanded respect from her peers and professors. Beth's peers in the mathematical psychology program during that period were a formidable group of soon-to-be stars of that field such as Rich Shiffrin, David Rumelhart, Bob Bjork, Jack Yellott, Mike Humphries, and Bill Batchelder.

Beth started with an interest in human learning, a major area of specialization in our program. She was a student in my graduate seminars on human learning. In one of those seminars, I assigned students the exercise of helping me critically read and review a few of the flood of manuscripts I was receiving as a reviewer for several professional journals. Subgroups of students and I would read a given manu-

script, meet to analyze it critically, and then they would present the substance of the findings, the theoretical explanation, and their review to the larger seminar group for discussion. Then we would come to a consensus view of the manuscript. I would take intensive notes and then write my final review of each manuscript for the action editor.

Because the students were learning their theory and experimental design principles in the front lines of our science, this exercise proved to be a valuable educational experience. The subgroups took their assignments seriously, and woe befell those who overlooked a flaw in experimental design or theoretical argument that the rest of us might catch. Moreover, their opinions and decisions had real consequences for the quality of work being published in our science. The exercise was clearly more consequential than handing in one more set of hypothetical homework problems. Beth later told me that those weekly exercises were some of the more meaningful and educational tasks for the students.

In those days, most of the students in the program were working with their professors on the popular Markov models for standard laboratory memory tasks like paired-associate learning, free recall, and short-term memory (see, e.g., the topics in our standard textbook by Atkinson, Bower, & Crothers, 1965). The classic paper, held up as the ideal prototype for students' aspirations, presented the model of short-term memory written by Beth's adviser, Richard Atkinson, and her fellow student, Richard Shiffrin (1968). But almost singular in her ways, Beth did not start down that career path. She had little interest in creating another Markov model that might provide a good fit to laboratory data from nonsense syllable learning. Rather, she wanted to work on intellectual puzzles closer to real-world situations.

In Stanford's psychology program at that time, the only available option close to her goal was for Beth to work on the computer-assisted instruction (CAI) projects that Pat Suppes and Dick Atkinson were conducting alongside their primary interest in mathematical learning theories. So Beth signed on to conduct research within CAI, joining the group of mainly Education-school students working with Suppes and Atkinson. This led to her first CAI publication (Fishman, Keller, & Atkinson, 1968) on teaching word spelling to elementary school children in local schools. Her second publication (Suppes, Loftus, & Jerman, 1969) continued in a slightly different line—arithmetic problem solving in fifth-grade children. Her dissertation, done in 1968 (published as Loftus & Suppes, 1972), was part of a larger project investigating mathematical problem solving in the elementary school curriculum. In that investigation, she and Pat Suppes were successful in identifying and measuring the many aspects of math word problems that contributed to the problems' difficulty, such as the length and complexity of sentences describing the problem, the number of steps in the solution path, the number of different operations to be performed, and so on. The collection of factors they measured were successful in predicting problem difficulty, with a multiple correlation of .73 in predicting the percentage of students who would solve the problems.

Although this research was successful in calibrating math and word problems for drill-and-practice exercises for fifth graders, Beth gained little satisfaction from such projects. In *Witness for the Defense* (Loftus & Ketcham, 1991), Beth wrote about her reactions to those projects:

It was tedious work, no doubt about it. The theoretical model had been set up years earlier by my Ph.D. advisor, and I was just one of several graduate students, each of us plugged into a specific slot, computing our statistical analyses, feeding our results into a common pot. It occurred to me that my particular job was a little like cutting carrots to put in a soup. To the left or the right of me were other students, equally frenzied and meticulous about cutting up their onions, celery, potatoes, chunks of beef, and then tossing them into the same huge pot. And I couldn't help thinking; all I've done is cut up the carrots. (p. 5)

The passage goes on to describe the beginning of her collaborative research with Stanford Psychology Professor, Jonathan Freedman. In that context, she was able, for the first time, to investigate her own research questions and not simply add her efforts to a large project in which she played a small part. Freedman and Beth began their collaborative research on semantic question answering, measuring how rapidly answers could be retrieved to various questions posed to a person's long-term, semantic memory. These results were interpreted as indicating something about the representation, organization, search, and activation of a person's long-term knowledge. They found, for example, that answers to category-then-property questions ("Name a *fruit* that is *purple*") were retrieved 250 milliseconds quicker than with the reverse ordering of the cues—indicating that people's semantic memory is likely organized around object categories rather than properties (Loftus & Freedman, 1970). They published several similar papers on semantic retrieval, which at the time was a very popular topic (Freedman & Loftus, 1971; Loftus & Freedman, 1972).

As she neared completion of her dissertation, Beth began looking for an academic job. She and I recall my telling her that the New School for Social Research in New York City was looking to hire a cognitive psychologist. Because she considered herself a specialist in human learning, she wondered whether she qualified for the job. I told her to apply and if she got the job, to shape it to fit her talents and interests. So she applied and obtained her first job at the New School, starting in fall 1970. That position had several advantages: First, her husband, Geoff Loftus, had received a postdoctoral fellowship with George Sperling at New York University, close by the New School; and second, Beth could continue her collaborative research with Jonathan Freedman, who had recently moved to Columbia University. Her research on semantic retrieval continued along a successful path for the next few years (Loftus, 1972, 1973; Loftus & Cole, 1974).

During this time, she had an encounter with an attorney friend that caused a major change in her attitude and approach to research. She'd been describing to him how her research had demonstrated that "*fruit - P*" retrieved an answer (*plum*) a quarter second faster than the reverse order, "*P - fruit*". The friend asked, "But how important to the human condition is the difference of a quarter second? Why not study some really significant problems such as the reliability of eyewitnesses testifying to crimes?" That and similar challenges caused an epiphany for Beth—as it came to be for many other research psychologists (e.g., Gruneberg, Morris, & Sykes, 1978; Neisser, 1978, 1982). Beth decided deliberately to aim her research increasingly at questions about the operation of memory in applied settings.

But even before that, already in 1971, there were intimations of Beth's interest and creativity in attacking applied questions. One of these was her little known paper on the strength of different cues in jogging people to remember their intentions (Loftus, 1971). This rarely cited paper is remarkable in two respects: First, her subjects were local inhabitants and staff members she encountered in and around the New School buildings and who she'd persuaded to answer a brief opinion survey; and second, it is one of the earliest papers in the nascent research area of "prospective memory." That topic was to mushroom into special prominence in memory research about 15 years later. So already in 1971, Beth was starting to use creative experimental methods to investigate novel, important questions in naturalistic field settings.

The first really applied issue Beth investigated was with John Keating (Loftus & Keating, 1974) on how to increase people's attention to, and their compliance with, public address emergency announcements, such as fire warnings to occupants of high-rise buildings. This research led to a series of collaborative papers published over succeeding years, detailing the efficacy of different forms of disaster announcements and how to reduce panic and instigate efficient responses among members of a threatened crowd (Keating & Loftus, 1975, 1977, 1981; Keating, Loftus, & Manber, 1983).

A later applied issue that attracted Beth's curiosity was the framing of questions in survey questionnaires used, for example, in collecting national health statistics or surveys of economic well-being (Feinberg, Loftus, & Tanur, 1985; Loftus, Feinberg, & Tanur, 1985). This was an area ripe for exploration with concepts of cognitive psychology, and especially memory, because so many survey questions ask people to remember how often they've done something. She was able to demonstrate, for example, strong anchoring influences on people's retrospective reports of the frequency of their physical complaints or their taking medications. Thus, after discussing a headache remedy (Anacin), the question "How many headache products would you say you tried—one, two, three?" elicited far lower estimates of headache remedies tried than did the same question but with higher suggested anchors—"one, five, ten?" (Incidentally, this was around the time when she appeared in a long-running television commercial for a headache remedy!) In later work, by checking health records, she and her colleagues were able to dis-

cover how impoverished were people's retrospective reports of many of their medical facts and, moreover, how different forms of memory questions led respondents to be more or less accurate. Beth also investigated methods for improving respondents' temporal dating of events from the past (Loftus & Marburger, 1983). Research in this area by Beth and her collaborators has had a substantial influence on the manner in which national survey questions are posed and their results interpreted.

The major shift in Beth's research career took place in 1974, soon after she and Geoff had moved to the University of Washington. She published two articles that were to catapult her onto the national stage as an expert on eyewitness testimony. One was her article with John Palmer titled "Reconstruction of Automobile Destruction" (Loftus & Palmer, 1974); the other was her article "Reconstructing Memory: The Incredible Eyewitness" in *Psychology Today* (Loftus, 1974). Both articles demonstrated how easily eyewitnesses' reports about a car accident could be altered by very subtle changes in the wording of questions. The national response to those articles, and to the subsequent one on leading questions (Loftus, 1975), put Beth on a fast track to prominence as an expert witness in legal court cases. Important to note, Beth had the drive, stamina, and zealous work habits that allowed her to rise to the challenges of this new identity—lecturing throughout the country to many attorney groups, law school forums, police associations, and the entire spectrum of psychology conferences and conventions, writing popular and semipopular books, while continuing her production of a steady stream of basic research articles in the foremost journals of our field. Her productivity is nothing short of amazing.

While writing these remembrances, I asked myself why Beth was able so easily to investigate eyewitness memory for filmed accident or crime scenes when so many other memory researchers had not ventured into that territory, despite its obvious allure. I know the answer in my case, at least, and I suspect it is typical for my generation of memory researchers. For us traditionalists, the "Gold Standard" test of memory was to have participants attempt *full recall* of all the material studied, much as we examined full recall of a list of unrelated words presented to an experimental subject for study. But we simply had no good ideas about how to study full recall of a film's naturalistic events such as accidents or crimes. What would be the "memory units" to be recalled or counted, as our subjects tried to recall a staged event or accident film?

There are many hundreds of bits of information in a film of a naturalistic event, at multiple levels of generality, that one might look for in a person's recall. If we asked subjects to attempt their unaided recall, we had no good ideas on how to score the complex protocols that could result. Just consider that realistic protocols would surely include all manner of generalizations about the filmed events, personal evaluations, stylistic commentaries, paraphrases, inferences of all varieties, fragmentary sentences, combinations of sentences and propositional knowledge, ellipses, compressed summaries of multiple parts, warranted and unwarranted importations added to the story, and so on. In a word, it would

be a linguistic morass, and no one (least of all memory researchers) had very good ideas of how to code and score recall protocols from films—simply counting words won't do, even assuming words were spoken in the film. Those coding schemes were to come into the field somewhat later for meaningful texts (e.g., the text-analysis techniques of Kintsch, 1974, 1998; Meyer, 1975; van Dijk & Kintsch, 1983) and very much later still for scoring recall memory for filmed events (e.g., Berger, 1998; Magliano, Dijkstra, & Zwaan, 1996; Magliano, Miller, & Zwaan, 2001; Zacks & Tversky, 2001; Zacks, Tversky, & Iyer, 2001). Even today, most memory researchers view the analysis of full recall of films to be an intractable methodological problem.

It was Beth Loftus's insight to notice that one need not be able to score all of people's full recall of a film to be able to examine interesting questions about their memory of it. Rather, experimenters could confine themselves to asking pointed recognition-memory questions aimed at probing subjects' memory of specific details of the filmed event. In this manner, the experimenter could create films and, say, a confederate's recounting of it in order to see the impact of their joint manipulation on memory for specific details of interest. It was this very slight shift in defining the dependent variable (*viz.*, recognition memory for specific details) that opened the door for the stampede of other experimenters using Beth's method following her innovations. As this view implies, most of Beth's early experiments in this area never reported subjects' recall of the film—in fact, only rarely was there any reporting of subjects' overall recognition accuracy for large number of facts about the filmed event. Those measures were simply not relevant to answering the pointed questions that Beth was considering. Before that, traditionalists would have viewed testing only one or two critical recognition items from a whole mass of studied material as an egregious waste of a possibly rich data source.

The mark of a major advance in psychology is that it becomes “obvious” once it is pointed out; we slap our foreheads and exclaim, “Of course, why didn't I think of that?” Beth's insight in making a small but crucial change in experimental method—selecting only a few specific facts of a film to be varied and then measuring recognition memory for them—was one of those simple methodological innovations that immediately attracted the research community to her paradigm. Moreover, her techniques for inserting misinformation into a person's memory of a filmed event were ingenious—by exposing subjects to the account of another witness, or having an examiner ask preliminary questions containing subtle pre-suppositions (Loftus, 1975). These methods were so simple but powerful in distorting subjects' memory that everyone could appreciate their significance. The clear implications for applications of the methods were also appealing to laboratory memory researchers.

Furthermore, Beth has always had an engaging, informal style of conveying her findings and she explains them using easily accessible ideas characterized by catchy phrases such as “destructive overwriting” versus “coexistence” of an earlier with a later memory of an event, or the “mentalmorphosis,” that is, the “inte-

gration” or “blending” of the memories of an original and a subsequent event (e.g., Loftus, 1981).

I recall asking her once about this (I paraphrase): “Isn’t your misinformation effect just another example of the old idea of retroactive interference? That is, subjects first see ‘INTERSECTION has a STOP sign’ (A–B association), then later hear an informant claiming that ‘INTERSECTION had a YIELD sign’ (A–C), so having learned that A–C association interferes with later retrieval of the original A–B association. In addition, your catchy term ‘destructive overwriting’ of memories seems to be just the old idea of unlearning from interference theory, wherein learning A–C weakens an earlier A–B association in an absolute sense (e.g., Barnes & Underwood, 1959; Postman, 1971).”

In her inimitable style, Beth thought a moment and produced a most insightful answer (again, I paraphrase): “Technically, you’re right. But strategically, for getting people interested in the distortion of eyewitnesses’ memories, interference theory with its stable of laboratory experiments has no ‘sex appeal.’ These findings with these formulations are breathing new life into some old ideas that are once again exciting researchers and reaching a public audience. The misinformation paradigm provides a useful test ground for examining the suggestibility of people’s memories.”

At the time, I didn’t fully appreciate her reply, because I thought that connections to earlier research should always be acknowledged to the world. But to what world? Other researchers? The funding agencies and lay public that supports our research? In retrospect, I think Beth’s reply was right on the money (no pun intended). A measure of the pragmatic worth of some ideas is how much they influence the research of others, and how easily they diffuse out into the popular press and the lay audience for general use. After all, it is their taxes that support our laboratory research.

Psychologists appreciate that Beth’s findings are fascinating and easy to describe to our nonpsychologist acquaintances and they find such research very worthwhile. The findings are far more engaging than just a few more facts about lab subjects’ loss of access (to A–B, in a modified-free-recall test) due to learning a second list (A–C) of arbitrary nonsense syllable pairs. Consider a personal example: As a memory researcher, I am sometimes asked to talk about memory research to lay audiences. I can rarely get through such talks without giving special prominence to Beth’s findings on eyewitness memory errors, and memory distortions due to misinformation, imagination inflation, and implanting totally false new memories. Those tend to be the more attention-grabbing parts of my popular talks to lay audiences. In this respect, Beth’s research has rescued me many times from boring my lay audiences. For that I (and I’m sure other researchers) can be profoundly grateful to her.

Beth’s early research on distortions in eyewitness memories ushered her into a deep involvement with legal issues, where she has often provided testimony as an expert witness in court cases. These contacts have been extremely valuable because the legal issues she encountered there led her into more court-related re-

search. This includes her work on clarifying the judge's instructions to a jury, on the impact of discredited information on mock jury decisions, the impact of a witness's recovered memory of an assault (compared to a persistently available memory of it) on the evidential weight mock jurors give to that testimony, the impact of memory testimony elicited under hypnosis, the impact of level of detail versus vagueness in a witness's memory, or people's accuracy in judging whether they had years before remembered or forgotten a given fact. Her research along these lines showcases an excellent use of the "real world" (if criminal court cases can be so considered), bringing these applied issues into controlled laboratory settings to investigate. But important to note, Beth takes the further crucial step of moving her laboratory results back out to the real world in her lecturing and writings for lawyers and judges and her testimony in court cases.

Over the years, Beth and I would frequently meet at psychology conventions or whenever she'd be visiting Stanford, and we enjoyed discussing our respective researches. Over time those conversations (and e-mails) evolved into an informal mentoring relationship that has proved very satisfying to me. We would discuss her concerns about challenges to her findings and approach, her standing in the field, what tenure or career issues she should be considering at a given time, what to do about various job offers that came her way, and so on.

One persisting issue in our conversations that has continued over the past 10 to 15 years is her concern with the many criticisms and occasionally vicious attacks on her and her work by her legal opponents or from those disputing her skepticism about recovery of "repressed memories" of childhood abuse (see, e.g., Loftus & Ketcham, 1994; Tavris, 2002, and this volume). In the early years, these attacks caused her some distress and self-doubt. I could only offer her sympathy and general support in these battles—to "stick to her guns," not to overextend her findings or let herself be pushed into a more extreme position on an issue than she was comfortable with, to look for the concerns motivating her critics, and to try to educate rather than counterattack. Throughout the past decade, she has withstood many such threats, legal suits, and assaults on her expertise, education, character, ancestry, grant support, access to research venues, academic positions, and subconscious motivations. The large "silent majority" of memory researchers who agree with Beth's basic position on most scientific issues greatly admire her steadfast courage in holding her scientific ground while surviving a barrage of attacks from some irate therapists and their clients. She is an outstanding example of a scientist who has the pluck, moral backbone, and scientific certitude to stand up to her critics and emerge from the battles even stronger than when she started.

Another issue that used to vex her was whether her applied, popular research and extensions would be viewed as less valuable to "rigorous" experimental psychologists. That is, should she be aiming to publish more theoretical articles like the famous one she wrote with Allan Collins in the *Psychological Review* (1975)? After all, that's how her esteemed Stanford peers such as Rich Shiffrin, Dave

Rumelhart, Bob Bjork, and John Anderson were earning solid academic reputations. Beth worried that her applied research might prevent her from earning the respect and recognition in her field that brings the accolades that all academics covet—research grants, honorific awards, good job offers, election to distinguished positions in professional organizations, and so on.

I recall advising Beth not to worry about her reputation among academic memory researchers or hard-nosed experimental psychologists, because her applied research and legal consulting were having a major impact in many worlds far beyond academic psychology. If truth be told, many experimental psychologists would be very gratified if their research could have half the real-world impact and international recognition that Beth's work has received.

I recall saying something like this (paraphrased): “This kind of applied research may not get you elected to the National Academy of Sciences, but you shouldn't care about that. What's important is that your heart is in it, you love it, your research is of high quality, and you're having a huge impact inside and outside our field. Better that your contributions be respected and admired by 10,000 applied psychologists, lawyers, and judges than by 10 theoretical psychologists.”

But happily, I was terribly wrong in my forecast. Over the past 10 years, worldwide recognition of her research has showered upon her many honors and awards far beyond what most academic researchers ever achieve in their careers. Significant ones were her election to the presidency of the American Psychological Society (APS) and her selection to William James Fellow status in APS (“For significant lifetime contributions to the basic science of psychology”). In 2003, Beth along with Steve Ceci received the coveted award for “Distinguished Scientific Contributions for the Applications of Psychology” from the American Psychological Association.

The capstones to her brilliant career occurred with her election in 2003 to the American Academy of Arts and Sciences and in spring 2004, her election to the august National Academy of Sciences, recognized as the most prestigious scientific honor that can be bestowed on a psychologist. Moreover, her candidacy within the National Academy rose with extraordinary speed, from her nomination to a majority vote within the psychology and social science sections, and thence to the ultimate election by the entire membership of the Academy. (This is a process that ordinarily takes several years of balloting.)

Her election to the National Academy doubtless put the *coup de grâce* to Beth's longing for respectability of her research. In addition, in 1995 she was honored with the famous Grawemeyer Award for Psychology, receiving the \$200,000 unrestricted prize for “*her research on memory and how it can be altered. Her work has implications for law and for psychotherapy, particularly methods of probing memory.*”

As frosting on the cake, a recent study was published in the *Review of General Psychology* in which the authors used several objective criteria (e.g., citations, awards, textbook mentions) to evaluate the impact of the scholarly work of thousands of psychologists (Haggbloom et al., 2002). In that assessment, Beth ranked

58th and was the *top-ranked woman* among the 100 most influential psychologists of the 20th century. I think we can all agree that she might now lay aside concerns about the respectability of her research.

From her earliest days at Stanford, Beth Loftus has always shown signs of stardom. Tracking her rising star for the past 40 years has been fascinating to behold. She has made her success in spectacular fashion, and she's done it on her own terms. I am immensely proud to have been associated with her.

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