

Eyewitness Researchers as Experts in Court: Responsive to Change in a Dynamic and Rational Process

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When we set out to revise the decade-old survey of eyewitness experts (Kassin, Ellsworth, & Smith, 1989), we did so for the explicit purpose of updating prior estimates of expert opinion so as to “encourage expert testimony that more accurately reflects the consensus of opinions within the scientific community” (Kassin, Tubb, Hosch, & Memon, May 2001, p. 405). The goal was not to weigh in on the “primal” question, long ago debated (see, e.g., Loftus, 1983; McCloskey & Egeth, 1983), of whether psychologists should testify—a question that American courts are increasingly settling in favor of such intervention—but rather to address the more discriminating, practically relevant question of which propositions pass the *Daubert* test and which do not.

McCullough (2002, this issue) seemed eager to engage in yesterday’s battles. At the outset, he characterized us as purporting to make a case in favor of the admission of eyewitness experts. That was not our aim. Believing that psychology’s most important contribution is to help reform eyewitness identification procedures (see Technical Working Group for Eyewitness Evidence, 1999; Wells et al., 2000), Kassin et al. (1989) self-consciously steered clear of absolutely endorsing or rejecting the participation of eyewitness experts, as did we. In our view, this question is increasingly being resolved in favor of experts at *Daubert* hearings all over the country, leaving a need to resolve the second-generation question concerning the content of that testimony, namely, which eyewitness phenomena are reliable enough for presentation to a jury and which are not. McCullough also asserted our desire to claim—to bolster a position we did not take on the primal issue—that potential jurors are ill informed on the subject of eyewitness memory. Nowhere did we make this assertion. In fact, we believe that the old debate about whether jurors are per se competent has also achieved second-generation status, which is why investigators seek to determine which eyewitness findings are known as a

matter of common sense and which are not.

McCullough’s (2002) commentary rests on a foundation of assumptions that are both naïve and erroneous. Let us begin with his overstatement of the purpose of the question we asked of experts, that they estimate whether they thought jurors believe the various propositions to be true as a matter of common sense. Critically, he states that “the way the opinions of potential jurors were arrived at was questionable” (McCullough, 2002, p. 376). Obviously, we did not intend for this question to serve as a surrogate measure of actual lay opinion. Kassin and Barndollar (1992) and others have already done that. Nowhere did we make this claim, as alleged, and nowhere did we draw attention to or discuss discrepancies between our experts’ opinions and their estimates of jurors’ opinions. The fact is, some experts, being good Bayesians, occasionally decline to testify about phenomena that they see as reliable but accessible to juries as a matter of common sense—hence, the value of that assessment (e.g., 90% saw alcohol effects as reliable, yet only 61% said they would testify as such; one possible reason is that 95% saw this effect as already known).

Let us move on to the erroneous assumption that stability in expert opinion from 1989 to 2001 is, to use McCullough’s (2002) word, “reassuring” (p. 377) and that change, in contrast, is “disturbing” (p. 377). To construe the survey results in this manner, one must assume that the scientific database of psychology is, or should be, static rather than dynamic—and, hence, that the second survey represented some form of test–retest reliability check on expert opinions of the past. This construal is evident in McCullough’s puzzling assertion that “if a high consensus of strong expert opinion were to change, this would suggest that experts might well have been wrong in the past” (McCullough, 2002, p. 377). To support this claim, he cited the finding that only 60% of our experts saw the effects of high stress on eyewitness performance as reliable, a reduction from the 71% rate previously found. Implying that this decline was itself an error, he cited Kassin and Barndollar’s (1992) finding that 82% of laypeople endorse the stress proposition and inferred that “it looks like laypeople are more likely to subscribe to the ‘correct’ view than experts are” (McCullough, 2002, p. 377).

This entire line of criticism is misguided. First, we expected to observe changes over time in expert opinion. The purpose of the update was to track such change in light of volumes of newly collected and published data. Like other sciences, psychology is dynamic. Hence, we expected, in light of post-1989 research, that experts would endorse the weapon focus effect more than in the

past, a prediction supported by a significant increase from 57% to 87% in the endorsement rate. As there has not been additional research on high stress, the nonsignificant decline from 71% to 60% shows that our experts—who were also, as reported, highly published researchers—were admirably responsive to the lack of empirical evidence rather than uniformly accepting of all eyewitness propositions. McCullough (2002) seemed to think otherwise, but lacking a strong empirical data base, experts were right, not wrong, to be more cautious about stress effects—despite the beliefs of Kassin and Barndollar’s (1992) lay participants.

Looking ahead, and on the basis of an article published near the time we administered our survey, McCullough (2002) surmised that expert opinion on the correlation between eyewitness accuracy and confidence seems bound to change. The weight of the extant literature suggests otherwise, but perhaps it will change some day. The study he cited (Lindsay, Read, & Sharma, 1998) may replicate across a range of conditions and the resulting speculation that laboratory experiments underestimate the correlation may turn out to be true. If so, on both counts, then future studies will force a revision of expert opinion—a revision that would indicate both the responsiveness of the scientific community and the rationality of the process by which it forms a consensus—hallmarks of success in the application of science, not failure.

Just a few years ago, astronomers using the Hubble space telescope discovered galactic clumps for the first time; recently, neuroscientists had to retract the once-firm belief that adult brain cells do not regenerate. Scientific knowledge builds incrementally as a function of new theories and conceptual paradigms, new technologies and discoveries, and access to new samples and populations. What professionals believe at one time differs, more or less, from opinions held earlier—and from those that will be held in the future. In light of this inherent feature of good empirical science, changes in consensus among eyewitness experts are to be expected and applauded.

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