

Courtroom Testimony by Psychologists on Eyewitness Identification Issues

Critical Notes and Reflections

Vladimir J. Konečni* and Ebbe B. Ebbesen*†

This article makes two major points in regard to expert psychological testimony on eyewitness identification. First, the attention devoted by psychologists to eyewitness identification issues is far out of proportion to the incidence of trials involving eyewitness identifications of criminal defendants; furthermore, the often-expressed concern over wrongful convictions is probably misplaced. Second, the experimental methods used in studies of eyewitness performance are fundamentally unsuited for drawing conclusions about actual witnesses. Hence, there is not an adequate scientific foundation for expert psychological testimony on eyewitness identification. Archival research is perhaps the most promising approach to the study of the criminal justice system.

INTRODUCTION

As is clear from reading McCloskey and Eggeth (1983a, 1983b) and Loftus (1983a, 1983b), and listening to the participants at the Johns Hopkins conference, the overwhelming majority of psychologists testifying in court on eyewitness identification issues are hired by the defense. The testimony typically consists of a general account (bolstered by quoting research findings) of the shortcomings and limitations of human perception and memory, the net effect being a contribution to the defense strategy of discrediting the eyewitness in the eyes of the jury, regardless of the actual merits and specific facts of the particular case in question.

The upshot of the present paper is that the practice of routinely giving such

* Department of Psychology, University of California, San Diego, La Jolla CA 92093.

† Ebbe B. Ebbesen did not participate at the Johns Hopkins conference but has contributed to the written version of this paper. This is because the San Diego Psychology-Law Research Project, on which some of the comments in the paper are based, has been a collaborative effort from its inception in 1974.

testimony is premature, given the present state of psychological knowledge, or, more specifically, given the methods by which such knowledge was obtained.

Before citing the reasons for this conclusion, however, we will attempt briefly to place the issue of psychological testimony regarding the eyewitness-identification problems in a somewhat broader context. In discussing this matter, it seems reasonable to ask, first, how frequent is eyewitness testimony?

EYEWITNESS TESTIMONY: BASE RATES

According to the California Bureau of Criminal Statistics (see Figure 1.2, p. 15, in Ebbesen & Konečni, 1982), there were 145,500 felony arrests in the State of California in 1977. Only 17% of these cases reached county superior courts, and of those that did, 86% resulted in a conviction, though less than 10% by a jury trial (a total of 2085 cases, or only 1.4% of all felony arrests). Thus, as is well known in other jurisdictions also, and for other years, jury trials are relatively rare events.

According to Wallace Loh (1981, p. 686) the "identity of the perpetrator is not the issue in the vast majority of trials"; he estimates that eyewitness evidence is used in only about 5% of criminal trials. Therefore, it could be inferred that only about 100 people were convicted in California in 1977 by juries which had heard eyewitness-identification testimony. It can be further estimated that in many of these cases there was at least some additional physical/circumstantial evidence to link the defendant to the crime, and/or more than one witness. Thus, in the most populous state in the Union, with some 150,000 felony arrests per annum, perhaps 40–50 individuals are convicted on the basis of eyewitness testimony alone. Only very few of these cases, of course, involve psychological testimony.

Contrary to what one may be led to believe by the size of the psycholegal literature on the topic, the phenomenon we are examining here is of miniscule proportions—fortunately so, if one subscribes to the conclusion eventually reached in this paper.

EYEWITNESS TESTIMONY: WRONGFUL CONVICTIONS

Psychologists (e.g., Loftus, 1983a, 1983b; Wells, this issue) often defend courtroom testimony on eyewitness-identification issues on the grounds of a deep concern for protecting the innocent from being convicted, for due process, and so on.

However, McCloskey and Egeth (1983b, p. 552) state that "documented cases of wrongful conviction resulting from mistaken eyewitness testimony obviously represent only a small fraction of 1% of the cases in which defendants were convicted at least in part on the basis of eyewitness testimony." If "a small fraction of 1%" is conservatively interpreted as, say, .3–.4%, and using as the computational basis the estimate given above that 100 people were convicted in 1977 by juries which had heard eyewitness-identification testimony, it follows

that in the State of California one person is wrongfully convicted approximately every 3 years because of mistaken eyewitness testimony.

Other aspects of the reality of criminal activity and the criminal justice system's functioning in the United States are instructive and noteworthy in regard to the concern about wrongful convictions that is often used to justify expert testimony on eyewitness identification.

(a) The Law Enforcement Assistance Administration and the Census Bureau (see Greenberg, Wilson, & Mills, 1982) estimate that less than 50% of felonies such as rape, robbery, assault, and burglary are reported to the police.

(b) The percentage of undetected and unarrested perpetrators of reported felonies has been estimated at over 70%.

(c) The percentage of cases in which persons arrested for committing a felony are released, or the charges are dropped, dismissed, or changed to a misdemeanor (often because of due-process technicalities) is enormous, perhaps as high as 85% (see Ebbesen & Konečni, 1982).

(d) It is commonly known that juvenile offenders get several "free" crimes, including felonies (see, for example, Wilson, 1983, p. 76).

(e) A very high proportion of those arrested for a felony have a prior criminal record, yet very few are given a prison sentence (about 1.5% of those arrested for a felony go to prison; see Konečni & Ebbesen, 1982a).

In short, the relatively very few documented wrongful convictions must be seen against the backdrop of a society and a criminal justice system which, by all accounts, underreport crime, underdetect and underarrest the reported perpetrators of crime, underprosecute persons arrested for felonies, and underpenalize those who plead guilty to, or are found guilty of, felonies, even when they have numerous prior felony convictions. In other words, it is quite a difficult task to become incarcerated in the United States.

That may be good or bad, depending on one's values; the point is that Loftus's and Wells's (this issue) concern over wrongful convictions and due process, and their defense of psychological testimony on such grounds, must be viewed in the more general context of the (intentional and unintentional) checks and balances that the system already has. One wrongful conviction every three years because of mistaken identification in a state the size of California (if the estimates given above are correct) may be one wrongful conviction too many, but most reasonable people would probably regard it as well within the domain of "acceptable risk"—acceptable because no workable system of justice is perfect (cf. McCloskey & Egeth, 1983b; Rembar, 1980). Psychological testimony hired by the defense to discredit eyewitnesses may unilaterally tip the balance—such as it is—that presently exists. As McCloskey and Egeth (1983b, p. 552) put it, "an increase in juror skepticism toward eyewitness testimony would decrease convictions of the guilty as well as convictions of the innocent".

PSYCHOLOGICAL TESTIMONY: INCENTIVES

Why are then exaggerated claims being made about the importance, ubiquity, and value of psychological testimony on eyewitness issues? Part of the an-

swer is clearly to be found in scientific and ethical convictions strongly held by psychologists who testify, however mistaken these may appear to others. Yet not to consider, at the same time, the incentive system under which we all function professionally is to show a blissful neglect of the sociology and economics of science.

When one debates the issues of psychologists testifying in court, it seems important to keep in mind the different criteria by which the value of such testimony can be judged. The criteria can be defined in terms of (a) ethical objectives (increasing justice, fairness, equity of treatment of various defendants), (b) group-professional objectives [increasing the reputation of psychology as a profession, which may or may not be related to (a)], and (c) personal objectives [which may be related to (a) and (b), but also include a highly individual component, by which we mean—to utter the taboo word, so thoroughly ignored at the Hopkins conference—the various forms of individual gain]. All of these objectives could be further divided into long- and short-term ones. So when one debates the value and contribution of psychological testimony—when, for example, McCloskey and Egeth (1983, p. 551) state that “expert psychological testimony about perception and memory in eyewitnesses should be offered only if there is clear evidence that such testimony has salutary effects”—the question of which criteria are used should immediately arise.

Without belaboring these somewhat obvious, though rarely discussed, issues, it would appear that the behavior of the would-be expert witnesses may be motivated, at least to some extent, by the following considerations:

(a) “Basic” memory/perception research programs are easily converted into “applied” eyewitness-issues programs, often with a minimal financial and intellectual investment, the change often being not in the location of the research, subject population, type of paradigm, research design, dependent measures, and so on, but only in the kind of stimuli presented (and then with stimulus values often not being in the range found in real-life legal situations);

(b) Such “applied”-sounding research programs have—in the funding climate of the last 10 years—been more likely to bring grant funds and lead to the acceptance of articles for publication;

(c) The research results, grants, and publications in the legal-sounding areas establish the psychologist’s credentials as an “expert”; testimony in court brings visibility, prestige outside a narrow professional area, and, yes, fees.

All this is simply to say that in addition to ethical and group-professional considerations, psychologists who testify as expert witnesses have vested interests regarding court testimony and the research programs and results it is based on. These vested interests are certainly no greater, however, than those judges have in being reluctant to allow experimental psychologists to step on their turf. Of course, for their part, defense attorneys have an interest in building a strong record of successful defenses, and thus using all available tools (some of which were so soundly criticized by Goldman, in this issue, in his comments on zealous advocacy), including psychologists as expert witnesses, to lower the pro-conviction proclivities of juries. It would seem that a debate on the value of psychological testimony on eyewitness-identification issues would profit from such factors being openly acknowledged.

At this point, however, we would like to address what we consider to be the core of the research, legal, and ethical arguments regarding psychological testimony on eyewitness issues.

EXTERNAL VALIDITY OF THE EYEWITNESS-IDENTIFICATION RESEARCH AND THE FRYE TEST

The generalizability and external validity of practically the entire body of research on perception and memory, and its applicability to eyewitness identification and testimony, are at issue. Almost all the studies which form the basis for expert testimony in this area are simulations. Virtually none of these simulations have been validated in terms of the real-world situations, stimuli, and subject samples, especially simultaneously (thus allowing higher-order interactions to be demonstrated), either in general, or in reference to a particular trial, defendant, and crime (cf. Bersoff, this issue; Loh, 1981).

Eyewitness-identification research is by no means an exception in this regard. Similar doubts concerning generalizability can be reasonably held for research on jury decision making and many other relevant areas of cognitive and social psychology (e.g., Ebbesen & Konečni, 1980; Konečni & Ebbesen, 1982b; Olson, 1976). Sample-specific, stimuli-specific, task-specific, method-specific, and dependent-measure-specific findings abound in the literature—they may be said to be the rule, not the exception—not to mention various combinations of the above (usually subsumed under “context effects”—an umbrella term for things one does not understand, often because the effects are the result of higher-order interactions).

For expert testimony to be admitted (see Woocher, this issue), both the expert and the testimony must pass the four tests discussed in *Amaral*,¹ and one of these is that expert scientific testimony be “deduced from a well-recognized scientific principle or discovery . . . and the thing from which the deduction is made must be established to have gained scientific acceptance in the particular field in which it belongs,” or the *Frye* test.² The *Frye* case was concerned with the use of lie-detection apparatus, and the wording can almost certainly be assumed to include methodology, as well as “theory” and “principle.” And issues of methodology, clearly and prominently, include external validity and generalizability.

From this, one is led to two conclusions:

(a) The external-validity problems of the memory and perception research on which the expert psychological testimony on eyewitness issues is based are so glaring that this type of testimony, at the present time, does not pass the *Frye* test.

(b) These external-validity problems seem sufficiently important that they should give the would-be experts on these matters serious pause—on both ethical and scientific grounds—in deciding whether or not to testify.

To us personally, the generalizability and external-validity problems appear

¹ *United States v. Amaral*, 488 F.2d 1148 (9th Cir. 1973).

² *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923).

sufficiently severe that we would be willing to testify for the prosecution in a preliminary hearing in favor of not admitting—on detailed methodological grounds—the customary expert testimony on eyewitness issues. Much as one might deplore the courtroom “battles of experts” (of the type that psychiatrists, for example, regularly engage in), and the damage they may do to psychology, we think there is little to be gained, especially in the long run, by a knee-jerk boosterism of the discipline. Instead, we fully agree with McCloskey and Egeth (in their *American Psychologist* debate with Loftus) in their conclusion that psychology has so far (for numerous, perhaps understandable, reasons) gained far fewer brownie points in the public view than, say, physics, biology, or medicine, and can thus ill-afford further decreases in public esteem. Psychiatry somehow survived the highly embarrassing *Tarasoff*³ prediction-of-dangerousness case by pleading ignorance and running home to the vast, powerful, and generally respected medical establishment. In an analogous situation, under whose cloak would psychology hide?

WHAT IS TO BE DONE?

Various alternatives suggest themselves.

1. *Do Nothing*. Arguments against the psychologists’ simply refraining from giving courtroom testimony on eyewitness-identification issues (e.g., Loftus, 1983a, 1983b; Yarmey, this issue) are not particularly convincing. For example, Yarmey says (pp. 000–000): “If the experimental psychologist never acted until he was absolutely sure of the scientific results on issues of eyewitness identification he would never leave the laboratory.” Well, perhaps if we cannot be as sure as, say, physicists, astronomers, and engineers are most of the time, we should not leave the laboratory soon (our personal inclination would be to leave the laboratory, but in order to do more ecologically valid research, rather than testify). No intervention seems clearly preferable to an intervention of dubious value, especially when one is dealing with a highly sensitive area where people’s lives (the defendant’s, the victim’s, the potential future victims’) are quite literally at stake.

In any case, how do the advocates of testimony know that our discipline and our fund of findings are “ready,” *now*, to enter the courtroom? It would seem that their claims may have less to do with the actual, proven maturity of psychology and its findings than with these researchers-testifiers’ personal and professional maturity, stature in the field, and accomplishments: They are ready, so the results better be ready as well. Much as one may sympathize with such impatience, it is instructive to recall that Hugo Münsterberg—almost 80 years ago, and in the veritable Stone Age of cognitive and social psychology—felt as strongly that psychology (as represented by him, of course), was ready to enter the courtroom.

2. *Amicus Briefs*. The problems with the data we presently have, and the methods by which these data have been obtained, cannot be easily circumvented

³ *Tarasoff v. Regents of the University of California*, Sup. 131 Cal. Rptr. 14, 1976.

simply by switching to the friend-of-court approach. In fact, we find this solution quite unappealing. It would perhaps ease the conscience of experts, but would be unlikely to further justice, in that what is now, for all practical purposes, advocate testimony for the defense, would be paraded as neutral, objective, scientifically unimpeachable information. Entrenched views by few "recognized authorities" would be the only voices heard (as is now the case in many European countries which rely mostly on *amicus* briefs); and inviting a panoply of experts to submit briefs would most likely again result in a battle of experts, given the state of the data.

3. "*Concordance of Experts.*" Recent studies on this topic by Yarmey (this issue) and Yarmey and Jones (1982, 1983) are informative as far as they go, but their relevance is unclear. The concordance of experts may simply indicate that they are all aware that there is in the literature some evidence of a small advantage of, for example, the accuracy of same-race, as opposed to cross-race, identifications. What is essentially a weak and unreliable effect is thus translated—with the help of a research methodology employing a forced-choice format with four inaccurate, but differentially inaccurate, alternatives—into, say, 94% expert concordance. Had it been available, a fifth alternative, suggested by Lindsay and Wells (1983), would have perhaps been endorsed by 98% of the experts ("The Asian and white women will find the white man and the black man equally difficult to identify"). And an alternative which stated "The research results on this issue are hopelessly mixed" would perhaps result in 100% agreement. Thus, high expert concordance in this type of study does not necessarily mean that there is strong evidence of big, solid, reliable, generalizable, relevant effects, and therefore should not incline a would-be expert in doubt toward a positive decision to testify [Loftus's (1983a, 1983b) claims to the contrary notwithstanding].

Similarly, whether or not police officers and civilians are equally good/poor in identification is by no means a closed issue, and the results obtained so far may merely be reflecting the ways in which the cited studies were done. Laymen (as opposed to psychologists surveyed) may well turn out to be, in the end, correct in assuming that police officers are better than civilians at this task—when the studies are finally done in the field, in real-life conditions, where police officers' training, experience, focus of attention, better coping with fear, and so on, may give them an advantage.

4. *Staged "Crimes."* Staging quasicrimes for experimental purposes (as opposed to the laboratory studies of facial identification) has been often recommended (e.g., Buckhout, this issue; Clifford, 1979; Malpass, 1981; Malpass & Devine, 1980; Wells, 1978; Yarmey, this issue). This paradigm belongs to the family of approaches which attempt to be more "realistic" and less artificial (while still dealing with the issue in a general, abstract way). In a statistical and methodological sense, these studies ostensibly cut through the web of higher-order interactions (context effects) involving method, subjects, type of task, type of dependent measure, type of experimental design, and so on.

However, since the dimensions along which the experimenters attempt to achieve "realism" are often haphazardly chosen and a matter of convenience (for example, staging a quasicrime in front of 200 students in Psychology 1), the gains

of this approach are modest. In its defense, it should be noted that the choice of dimension(s) on which to attempt to achieve similarity to "real life" has to be haphazard, because there is little parametric knowledge of the factors and their combinations which most contribute to low external validity of laboratory experiments.

5. *Special-Purpose Experiments.* One way to increase the external validity of simulations and reduce the degree of unjustified extrapolation is to do small-scale, precise, situation-, task-, and population-specific studies, hand-tailored to specific real-world issues. An expert witness who has agreed to testify in a particular trial, for example, may conduct a study that addresses the narrow issues that arise in that trial, given the crime in question, witnessing conditions, line-up conditions, and so on. Such a study would be blatantly atheoretical and rather uninteresting for a broader audience of psychologists (unlike Loftus, 1983a, 1983b, we think that one cannot have one's cake and eat it too, at least not for long, or repeatedly). The results of this type of study would be pertinent for presentation in expert testimony in a particular trial, though they, too, could not be assumed to be reusable in another trial (i.e., to generalize), except through a massive and systematic data-collection effort in numerous trials.

Special-purpose experiments, especially if conducted after a psychologist has been hired (most likely by the defense), give rise to a further conflict of ethical and practical concerns. An experiment intentionally geared toward a particular outcome is not worthy of its name; an experiment which produces results unfavorable to the defense would most likely lead to the expert's contract being prematurely terminated. Therefore, such experiments would probably best serve justice by providing support for *amicus* briefs.

6. *Archival Research.* In the long run, the approach that appears the most promising to us is archival analysis (we used it to study bail and sentencing decisions, the processing of mentally disordered sex offenders, and so on). The application of this methodology is painstaking—one codes information (from files, hearings, etc.) relevant to hundreds of predictors in literally thousands of cases—but in return can hope to achieve a thorough understanding of what goes on, and how decisions are made, in the real-world legal system. The decision models that emerge (on the basis of extensive regression and log-linear analyses) are often surprisingly simple and elegant, and relatively strong causal statements can sometimes be made.

The contents of prosecutor files can be similarly analyzed.⁴ The doctoral dissertation of Robert W. Root, our graduate student at U.C.S.D., involves the coding of some 400-odd predictors in 1,000 randomly chosen prosecutor files pertaining to adult felony arrests in San Diego County in 1979. The variables, all coded in great detail, are (a) crime-related factors (e.g., type of crime, arrest

⁴ Prosecutor files are not in the public domain, but permission to do the research on them can be obtained after the customary legwork. For example, our psychological research group at U.C.S.D. has been certified as a *bona fide* research body by the Attorney General of the State of California under Section 13202 of the California Penal Code and granted access to confidential information by the Bureau of Criminal Statistics.

circumstances, use of weapons, victims, witnesses, etc.); (b) suspect-related factors (e.g., age, sex, race, physical appearance, employment status, marital status, prior criminal record, etc.); (c) evidence- and identification-related factors (e.g., eyewitnessing conditions, the content of witnesses' statements, line-up conditions, corroboration of witness accounts, physical and circumstantial evidence, and so on); (d) procedure-related factors (e.g., bail status, etc.); and (e) additional factors coded from prosecutor notes, summary sheets, and charge sheets (e.g., contacts with defense attorneys, plea-bargaining information on a step-by-step basis, etc.). On the basis of such coding, prosecutorial decision making can be mapped in considerable detail, notably in terms of the use of evidence and plea-bargaining.

This type of analysis is aggregate and so cannot help expert testimony in a particular case (this is true for any nomothetic approach), but it can obtain important base-rate information and place eyewitness issues in a broader context. One can compare the details of cases that are and are not prosecuted, that do and do not go to trial, that do and do not contain eyewitness identification and expert testimony, with varying amounts of different types of evidence, and with different outcomes in terms of jury verdicts, plea bargains, and sentences. This work can also perhaps help the area of expert testimony on eyewitness issues in the negative sense. If eyewitness identification and testimony turn out not to matter all that much, and if psychological courtroom testimony on eyewitness issues turns out to matter even less, perhaps one can put this venerable area of research to rest three-quarters of a century after Münsterberg.

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