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Upper Limits of Eyewitness Identification Accuracy in Court

Ralph Norman Haber and Lyn Haber
Human Factors Consultants

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Address for Correspondence: Dr. Ralph Norman Haber, 313 Ridge View Drive, Swall Meadows, CA 93514. Tel: 760-387-2458; Email: Ralph@humanfactorsconsultant.com.

Abstract

Eyewitnesses in real life make erroneous identifications, as shown by compilations of a century's worth of newspaper accounts of erroneous indictments and/or convictions based on mistaken eyewitnesses, and based on published descriptions of exonerations since 1989 that show that the majority of the erroneous convictions resulted from erroneous eyewitness identifications. These accounts do not indicate an error rate, only a high number of instances. To estimate error rates with which eyewitnesses identify innocent suspects in police lineups and innocent defendants in court, we review and evaluate five independent lines of research. These include: (1) laboratory experimental research on face recognition showing the accuracy of recognizing unfamiliar faces seen just once before, in the absence of a crime; (2) laboratory experimental research from 1970 to the present in which subjects observe a crime and attempt an identification from a lineup; (3) field research studies from the showing the accuracy of the identification of a "perpetrator," in the absence of a crime; (4) military laboratory research data showing the accuracy with which soldiers can identify their interrogators (perpetrators) from a lineup 24 hours after intense and stressful questioning; and (5) analyses of police archival data showing the percentage of time that an eyewitness picks the person the police have placed in the lineup as the suspect. These data bases, taken together, establish an upper limit on eyewitness identification accuracy of less than 50% correct (correct identification rate and/or correct rejection of the lineup when the perpetrator is not in the lineup). They further indicate that most real eyewitnesses to real crimes are unable to achieve even this modest level of accuracy when they testify in court to an identification of a defendant as the perpetrator.

Upper Limits on Eyewitness Identification Accuracy in Court

The purpose of this article is to review the available evidence on the accuracy with which an eyewitness identifies the perpetrator of a crime from a lineup, and, based on that evidence, project the accuracy levels of real eyewitnesses when they testify in court to an identification of the defendant as the perpetrator. The photospread lineup is the principal technique used by police officials to elicit an identification of a suspect as the perpetrator (Behrman & Davey, 2001). Live lineups are rare, but current evidence suggests that identification accuracy is equivalent to photo-spreads, whereas live showups are more frequently used, but current evidence suggests lower accuracy levels than found with multi-person photospread and live lineups (Stablay et al., 2003).

Eyewitness identification accuracy levels, until quite recently, have not been directly investigated. Up to the past decade, the main source of data has been experimental laboratory research, using college students as subjects. The purpose of those studies has been almost exclusively to investigate different factors that affect the accuracy of identifications, not the absolute levels of accuracy. Even when reported, which is not always done, the absolute accuracy levels found in those studies have not used to estimate the accuracy with which real eyewitnesses can pick the suspect out of a real lineup administered by real police.

The purpose of this article is to estimate the accuracy rate with which an eyewitness to a crime is likely correctly to identify the defendant as the perpetrator of that crime. Since we already know that many factors and variables influence accuracy levels, our focus in this article is to estimate the upper limit that can be expected on identification accuracy under the factors known to be present. We believe that the public, the legal system, and law enforcement agencies have all assumed that the upper limit on identification accuracy, under optimal circumstances of observation, human memory, and police investigation procedures, is close to or at 100% correct. We will show in this article that this optimality assumption is grossly incorrect, due to normal limitations in human perception and memory, especially of strangers, and for additional limitations produced by fear and distress. Our goal is to provide estimates on what this upper limit might be, for the various conditions affecting the eyewitness in producing an identification of a perpetrator of a crime. These upper limit estimates are extracted from the research to be reviewed here such that they can be applied to the probability that the testimony of an eyewitness to the identification of the defendant as the perpetrator in court may be correct or erroneous.

We begin by reviewing a century's worth of accounts showing that erroneous eyewitness identifications have been used to indict and convict innocent suspects, and published exonerations from 1989 to the present based on DNA and other post-conviction evidence which show the majority of these convictions rested on erroneous eyewitness identifications.

Then we examine five independent and very different research data bases which we use to estimate upper limits for the accuracy rates of eyewitness identification.

The first provides estimates of the accuracy with which subjects in experimental research can recognize faces seen only once in non-crime settings. We consider this data base first, because it establishes a pure measure of the great difficulty of the identification task when the person to be recognized is a stranger, seen but once before. It is pure because identification accuracy is measured in the absence of the multitude of variables that further impede the accuracy of the eyewitness who observes a crime. The results of these studies provide an upper limit on the accuracy with which a witness can identify someone who is a stranger under otherwise optimal conditions.

The second data base contains the large corpus of laboratory accuracy rate research on college student eyewitnesses who observe a crime and identify the suspect in a lineup. These results, unlike from the first data base, include the basic ingredients of eyewitness identifications: the witness observes a crime, a person suspected of that crime is shown in a lineup; and the identification results allow examination of the factors that affect accuracy, including the methods used to elicit identifications. But these studies are missing the presence of the normally high levels of fear, terror and stress present in witnesses to most crimes: these laboratory ‘crimes’ are of necessity gentle. They also use almost exclusively a subject population that is not typical of most real live eyewitnesses. These accuracy results represent a different upper limit on eyewitness identification accuracy, because the subject/eyewitness observes and makes the identification under more idealized conditions than a real witness to a real crime.

The third data base contains accuracy rates from non-crime field lineup identification studies in which eyewitnesses identify persons they observed under focused attention. These experiments do not involve crimes, so there is no fear or stress, and they are designed to insure the witness’s focused attention, but they do correct two of the problems with the laboratory research: the subjects in the field studies are more representative of those who witness crimes; and, more importantly, the eyewitnesses do not know they are being tested or observed. This data base provides another upper limit on eyewitness identification accuracy of a stranger, but under focused attention and in the absence of fear and stress.

The fourth data base includes recent military research in which soldiers are being trained to resist interrogation. Each soldier is ‘captured’ and interrogated under intense conditions. After the interrogation, he is asked to identify his interrogator from a live lineup. The results from this research permit estimates of accuracy when the witness experiences great fear comparable to what eyewitnesses to violent crimes commonly feel. The two limitations are the atypical soldier population, and their lengthy, focused, well-lit view of their interrogators.

The last data base includes actual police records of real crimes in which real eyewitnesses make identifications from lineups constructed by the police that contain the suspect the police believe is the perpetrator. The data reported are the actual rates with which eyewitnesses pick the suspect from the lineup. These suspect identification rates are exactly the estimate sought: real eyewitnesses of real crimes identifying real suspects from real lineups administered by real police. However, since the police’s suspect may not always be the true perpetrator, the identification accuracy rate again provides only an upper limit, one dependent on the accuracy of the police in finding the suspect who is the true perpetrator.

Taken singly, none of these data sources can be used to estimate the accuracy of eyewitness identifications in real lineups. We review the problematic nature of each individual data base in some detail. Despite the limitations of each one considered separately, these five data sets can be viewed together to set upper limits on accuracy in real life circumstances. We will show that these limits converge toward a single value that can be applied to most eyewitness identifications based on observation of a real crime. The enormous advantage of estimates based on experimental work (the first four data bases above) is that the ground truth of the identification is known: every identification response made by the eyewitness can be scored as correct or incorrect. In the police archival data, in which the true perpetrator is unknown, true identification accuracy rate cannot be known and the results provide only an upper limit. However, because of the direct applicability of this fifth data base to testimony in court, this upper limit is the most limiting of all.

Examples of Individual Erroneous Indictments and/or Convictions

Do eyewitnesses in fact make erroneous identifications that result in indictments or convictions of innocent persons? The answer is yes. In addition to anecdotal examples, there are at least two large compilations of instances. The older sources of evidence are primarily from collections of newspaper articles about cases in which eyewitnesses were shown to be wrong in their identifications.

Gross (1987), drawing on Bochard (1932), Frank and Frank (1957), Radin (1964), Bedau (1967), Ferguson and Miller (1973), Loftus (1979), Wells and Loftus (1984), summarized 136 cases from 1900 to 1986 in which a person was identified as the perpetrator by a witness, charged with the commission of that felony, followed by a determination that the accused was innocent. With respect to the small size of the data base, Gross (1987) notes that with less rigorously conservative selection criteria and access to computer searches of older newspaper accounts, the number of such cases found in this 86 year time period would have been far higher.

Whether N = 136 is a small or a large number, it is not an error rate estimate. Gross (1987) observes that without knowing the overall number of eyewitness cases resulting in indictment or in conviction in the same time period, no estimate of an error rate for eyewitness accuracy in court can be calculated.

A more recent compilation of erroneous convictions was triggered by the first use of DNA to exonerate a convicted defendant in 1989. These collected cases since 1989 serve as a data base of mistaken convictions of a defendant, and from them, it is possible to determine the cause of the error. A number of analyses have been published, including Schenk, Neufeld, and Dwyer (2001), and Gross, et al. (2005).

Gross, et al. (2005) summarized the results of these individual court cases from 1989 through 2003 in which erroneous convictions have been overturned by new post-conviction evidence. In these 15 years, 328 cases of individual exonerations were reported, of which about half resulted from DNA evidence and the rest from other evidence that excluded the convicted defendant of the crime.

When Gross, et al. classified the cases by the cause of the erroneous conviction, they found that in 64% of the erroneous convictions (209/328), one or more eyewitnesses erroneously identified the (innocent) defendant. These 209 cases included most of the rape cases (88%) that were exonerated, and half the murder cases (49%). An erroneous eyewitness identification was the most prevalent cause of the conviction of an innocent defendant in the entire data base of 328 cases. These results are summarized in Table 1.

Insert Table 1 here

As with counting the newspaper articles from 1900-1986 on mistaken identifications, it is not possible to argue that 88% of all eyewitness identifications of rapists by their victims are erroneous, or that 64% of all convicted criminals identified by eyewitnesses are falsely identified. All we can tell from this data base alone is that a substantial number of erroneous identifications are being made. Gross argues from internal analyses of the data base that the underlying error rate for convictions of felons is orders of magnitude higher, so that these examples are just the tip of the iceberg, with many more not yet discovered.

The five data sets we next review permit estimates of eyewitness identification error rates. These estimates suggest that identifications of stranger-perpetrators are frequently wrong, and support Gross' iceberg model: erroneous eyewitness identifications account for an unseen but substantial percentage of our imprisoned innocents.

1. Laboratory Experiments on Recognition of Unfamiliar Faces

Hundreds of experiments on recognition memory have been published using faces as the stimuli (see Shapiro & Penrod, 1986 for an older and very thorough review which has not been updated). Since eyewitness identifications made from lineups usually depend on recognition from photographs of a stranger seen only once, these face recognition experiments using photographs as the stimuli provide an independent measure of the ability of witnesses to recognize from lineups a face they have seen only once before.

The typical recognition experiment includes a learning session followed by a testing session. During learning, subjects are shown a number of photographs of faces, one at a time. Some time later, in a testing session, some of the same faces are shown again, along with some faces never seen before, and the subjects are asked to indicate which of those had been shown during the learning session. We equate the learning session with the observation of a strange perpetrator, and the testing session with the presentation of the same perpetrator as a suspect in a lineup.

Every one of the 190 plus experiments examined by Shapiro and Penrod (1986) used only unfamiliar faces. We further restricted their data base to studies in which the subjects were adults, each photograph was presented singly for at least 5 seconds; and the perspective, pose, lighting, or expression was changed from the learning to the testing session. We found 18 experiments in their data base, and found that the accuracy rate of correct recognition was 59%. These conditions are most representative of the conditions present for an eyewitness when looking at photographs in a lineup.

Relative few experiments have used highly familiar faces in a recognition memory paradigm, either contrasted with unfamiliar faces or alone (Bredart & Devue, 2006; Bruyer & Lafalize, 1989; Burton, Jenkins, Hancock & White, 2005; Campbell, Coleman, Walker, Benson, Wallace, Michelotti & Baron-Cohen, 1999; Davies, Ellis, & Shepherd, 1978; De Jong, Wagenaar, Wolters & Verstijnen, 2005; Ellis, Shepherd & Davies, 1979; Ge, Luo, Nishimura & Lee, 2003; Geiselman, Tubridy, Bkynjun, Schroppel, Turner, Yoakum & Young, 2001; Klatzky & Forrest, 1984;: Leveroni, Seidenberg, Mayer, Mead, Binder & Rao, 2000; Stacey, Walker and Underwood, 2005).

For most of these, the familiar faces are usually selected from photographs of famous persons, faces that the subjects had presumably seen before the current recognition experiment. To verify that the faces are familiar, the subjects are asked whether they recognize the faces in the photographs, and in most experiments using familiar stimuli, data from familiar stimuli are used only if the subject reports that the face is familiar (which is close to 100% anyway). The procedures used are comparable to the studies considered by Shapiro and Penrod: familiar faces (sometimes mixed with stranger faces) are shown one at a time during the training session, and then these and another group of familiar faces (and sometimes mixed with stranger faces) are shown one at a time during the test session. The subject has to indicate which of the pictures in the test session had been shown in the training session, and which had never been shown.

Comparing the 59% correct recognition accuracy with unfamiliar faces contained in Shapiro and Penrod (1986), familiar faces in the above experiments are recognized correctly as having been presented previously in the learning session between 95% and 100% correct, and the familiar faces never shown in the experiment are rejected as not have been presented over 90% of the time (see Table 2). This suggests that if an eyewitness observed his close friend commit a crime, the probability is over 95% that he would identify him accurately from a lineup; and if that witness saw his friend playing golf, and then witnessed some one else commit a crime, the probability is over 90% that he would reject his friend as the perpetrator of the crime if shown his friend's photograph in a lineup.

Insert Table 2 here

The difference between recognition accuracy of familiar faces as compared to unfamiliar faces is the loss in accuracy in recognizing strangers. This "stranger effect" places an upper limit of about 60% on forensic eyewitness identification accuracy. The unfamiliar photograph recognition accuracy of about 50% is the best that eyewitnesses can be expected to achieve when attempting to identify a stranger seen only once before.

However, this roughly 60% accuracy is still an overestimate of an upper limit on eyewitness accuracy, because the face recognition experiments do not attempt to mimic most of the conditions present in real life or even laboratory tests of forensic identifications. No crimes are observed, no sudden actions, disguises or restricted views are employed, no stress, fear or victimization is created in the subject, attention is focused on viewing the faces, and the testing session usually follows the learning session with little or no time delay. Every one of these omissions would have reduced accuracy if they had been present (Haber & Haber, 2000).

2. Laboratory Experiments on Eyewitness Accuracy

Intensive research effort spanning over a half century has produced hundreds of published experiments on the accuracy with which subject-eyewitnesses to crimes can perceive, remember and identify perpetrators from lineups. Penrod (1995), Loftus and Doyle (1997), Haber and Haber (2000), and four meta-analyses by Stablay (1992; 1997; Stablay et al., 2001; 2003) provide recent reviews. Most research has focused on the discovery and manipulation of factors that negatively affect accuracy.

Wells (1978) divided these variables into two groups, based on whether the criminal justice system has any control over them. Those beyond control he called <u>estimator</u> variables: poor lighting, short duration of viewing, long distance, presence of weapons, inattention, violence of crime, victimization of witness, cross racial identifications, unfamiliar perpetrator, exposure to post event information, long delay between crime and lineup presentation, and repeated and pressured questioning. These variables are accidental properties of the crime, the witness, or the investigation.

Wells called the second group system variables. These factors are largely or completely under the control of the investigators of the crime, the criminal justice system. They include presentation of a lineup without an admonition regarding the suspect's possible absence, presentation of all lineup members simultaneously, use of a very small number of persons in the lineup (including just one), use of a lineup biased in its display of persons or their dress, non-blind presentation of the lineup, and presentation of a photograph of the suspect prior to the lineup test.

Haber and Haber (unpublished) explored a second data base, the large number of experiments that contained evidence of the accuracy that experimental subjects could correctly identify the perpetrator in lineups or reject lineups that did not contain the perpetrator. The experimental studies that comprise this second data base met the following criteria: they were published after 1970 through late 2005; the subject-witnesses were exposed either to a real crime, a realistic crime staged for the experiment, or a crime presented for the experiment by slides, film, or video; the perpetrator was a stranger to the witnesses; the witnesses were adults; the subjects made their identification by viewing either a photo-spread or a live lineup containing four or more people; the subjects responded with the equivalent of "yes," "no" or "none of these people" after being shown the lineup; the experimental manipulations avoided artifacts of ceiling or floor effects, or unusual levels of difficulty, and the experiment reported sufficient original data to permit computation of the percentage of subjects who made correct identifications, erroneous identifications, and missed identifications when the perpetrator was present in the lineup, and/or correctly rejected the lineup or erroneously identified someone when the perpetrator was absent from the lineup;

We selected all of the studies that met the criteria stated above from the bibliographies of four meta-analyses of identification accuracy (Steblay, et al., 1997; Steblay, et al., 2001; Steblay, et al, 2003; Shapiro & Penrod, 1986). We then searched the American Psychological Association PsycINFO online data base using the keyword "eyewitness testimony," which produced nearly 400 articles, chapters and books. "Eyewitness identification" produced fewer citations (and no new ones), and "eyewitness" alone included a landslide of irrelevancy. We then examined the reference lists and bibliographies of each of those 400 publications, from which we acquired 125 additional articles. We asked researchers and reviewers for suggestions of other studies, and used those that met the criteria. This analysis probably includes a very close to the complete population of such experiments.

We found 41 publications that met our criteria, and we included all of them in our analyses (indicated with an (*) in the reference list). Whenever a publication reported two or more experiments, in which different subjects were used and each experiment separately met our selection criteria, then we included each of the separate experiments in our analyses. The 41 publications contained 50 separate acceptable experiments.

The only dependent variables scored were the percentage of subjects who scored a correct identification, erroneous identification, missed identification or correct rejection of the lineup for perpetrator-present and perpetrator-absent lineups. We did not correct for sample size when averaging percentages, since nearly all samples were relatively large.

Table 3 reports the raw percentages of the subjects' responses, averaged across the 50 experiments, broken down by whether the perpetrator was present in the lineup (N = 41) or absent (N = 28), but otherwise averaged across all other conditions and across the 50 experiments.

Insert Table 3 here

When the perpetrator was present in the lineup, 48% of the subjects made correct identifications of the perpetrator, 31% erroneously identified an innocent foil as the perpetrator, and 21% erroneously responded that the perpetrator was not in the lineup.

When the perpetrator was absent from the lineup, 48% of the subjects correctly rejected the lineup as not containing the perpetrator, and 52% erroneously identified an innocent foil.

Overall, correct performance is 48%, averaging correct identifications and correct rejects across the perpetrator present and perpetrator absent conditions.

We examined the choices of estimator and system variables in the experiments to determine whether those choices would have pushed the accuracy levels higher or lower. This analysis was important and necessary because we had not controlled how these variables were set when we selected the 50 experiments.

We classified each experiment by the presence or absence of negative values of ten estimator variables: low lighting, short duration of viewing, weapon present, lack of initial attention, violent crime, victimization of witness, cross race, post-event information, multiple interviews with the witness, and long delay before showing the lineup. Of the 500 opportunities for these 10 negative estimator variables to be used in these 50 experiments, only 9 instances were found. These included a weapon present in 3 experiments, a violent crime in 2 experiments, high stress in 1 experiment, and the witness was victimized in 3 experiments. Thus, in 491 of the 500 opportunities (98%), the estimator variables were selected to be optimal for identification accuracy: the lighting was daylight, the viewing was more than sufficiently long, no weapon was present, initial attention was directed at the relevant action and people, and so forth. These experiments provide estimates of identification accuracy under idealized estimator conditions. Eyewitnesses to real crimes rarely experience such a combination of optimal conditions: therefore, a 48% accuracy level represents an upper limit. These experiments do not tell us how much more difficult are real life observations of crimes—they do not permit an estimate of how much that difficulty would depress the 48% accuracy result.

The same conclusion applies to examination of the presence of the system variables. Only five system variables were manipulated, including 12 experiments with no admonition bias, 41 with simultaneous presentation, 5 with biased selection of foils, 2 with non-blind administration of the lineup by the police, and 5 with multiple presentations of the suspect to the witness. Hence, of 250 opportunities to choose a non-optimal system variable, only 64, or 13% of the experiments were run under system variable manipulations known to depress accuracy. Wolgalter, Malpass and McQuiston (2004) presents evidence regarding actual usage of these system variables. They report that most eyewitnesses to actual crimes are asked to make identifications under far less than optimal conditions, so that these research results again provide estimates of identification accuracy under conditions favoring higher accuracy, rather than under realistic conditions.

The laboratory research experiments involve other components that probably elevate accuracy and certainly make generalization to real life identification questionable. The studies in this data base create little, if any, fear or stress in the subjects; the subjects know they are in an experiment, for which they usually receive course credit; and they are not a representative sample of typical eyewitnesses. Real eyewitness identifications, under real life conditions, cannot be expected to identify correctly perpetrators 48% of the time.

<u>3. Field Experiments Using Lineups without Crimes</u>

Non-crime field studies have surmounted two major objections to laboratory experiments. First, the subject population of witnesses sampled is a cross section of people, rather than exclusively college students; and, second, the witness does not know an experiment is being run. The two main drawbacks are that the field studies assess the accuracy of observation in the absence of a crime—that is, in a less dramatic or stressful circumstance, and the witnesses' encounters with the perpetrator are designed to engage their focused attention.

Cutler and Penrod (1995) review a number of field studies in which a range of people in real life circumstances are asked to make an identification from a lineup. In the typical paradigm, an unfamiliar "perpetrator" engages the attention of a clerk or teller for several minutes, usually by making a legal but unusual request or by acting strangely. Within several hours, an "investigator" appears, reminds the clerk or teller of the unusual earlier event, and asks that witness to view a photo spread lineup to try to identify the perpetrator.

Across the non-crime field studies reviewed, the average accuracy rate in picking the perpetrator in perpetrator-present lineups was 42%, with the erroneous identification responses distributed between picking an innocent person and rejecting all of the persons in the lineup as the perpetrator. When the perpetrator is absent from the lineup, 64% of the eyewitnesses correctly report that the perpetrator is not present, while 36% pick one of the innocent persons (erroneous identifications). The average accuracy across both types of lineup is 53%.

Since in these field studies there was no crime, no fear, no distraction of attention, no violence, no victimization, and no long delay in making the identification, and the identification procedures follow optimal system variable guidelines, nearly all of the variables that have been shown to affect eyewitness identification accuracy are set at maximal values to create accurate identifications. As with the laboratory studies, these results must be viewed as upper limits on eyewitness accuracy. Real eyewitnesses under real life conditions cannot be expected to be this accurate.

<u>4. US Military Research on Eyewitness Accuracy</u>

Morgan, et al. (2004) reported experiments that are part of a large program of research to improve the capability of military personnel to survive in captivity. His experiments added levels of stress and fear more equivalent to what is felt by real eyewitnesses and victims, so they include a important amount of realism missing from all of the research in the data bases examined so far.

As part of the interrogation training, soldiers were subjected to interrogation procedures, some of which were intense and highly stressful. An incidental question investigated in this research was whether the trainees could later identify their respective interrogators (perpetrators) in an in-person lineup. Each trainee (N = 509, tested individually) had been deprived of sleep and food for 48 hours prior to the interrogation sessions. After being “captured,” each trainee was questioned for over 30 minutes by an unfamiliar interrogator while standing face to face at a distance of only a few feet under good lighting. Half of the sessions were conducted under high stress with physical confrontation; the remainder without the physical confrontation and under a lower level of stress. The next day each trainee was shown a live six person lineup containing his interrogator among a group of foils (the foils were interrogators for other participants).

In these perpetrator-present lineups, the perpetrator was identified 68% in the low stress condition, with 29% erroneous identifications (picking someone who had never been present in the training), and 3% responses that the perpetrator was not present (see Table 4). In the high stress condition, the correct identification rate was less than half the low-stress level—only 32%, with 61% erroneous identifications of innocents, and 7% not-present responses.

Insert Table 4 here

Over two-thirds of these highly motivated eyewitnesses under strong stress were unable to identify their interrogator, with whom they had been face to face for over a half hour one day before. Even under only moderate stress, after a half hour of direct, highly attentive observation, nearly a third of the soldiers failed to identify their interrogator just one day later.

This data base fills in a large gap in the more “gentle” research considered in the preceding sections: here high stress and motivational levels are achieved and maintained, much more closely resembling those faced by real victims or eyewitnesses of traumatic crimes. Even so, the results on accuracy suggest they may be overestimates. First, the subject population is limited to physically fit young men with normal or corrected-to-normal vision and hearing, who are highly motivated to do well. Second, the witnesses’ observations of the perpetrator-interrogator occurred over a long period of time under good lighting and full attention. Third, the witness viewed the lineup the very next day. Fourth, the system variables were optimal, which is rare in actual practice (Wogalter, et al., 2004). Each of these factors, when present, has been shown to increase identification accuracy (Haber & Haber, 2000); each is frequently not present to help the eyewitness in a real life crime.

While it is difficult to quantify the amount of stress and trauma produced and experienced, and to equate different kinds of crimes and different kinds of witnesses, it seems reasonable that the military experiments more closely resemble the experiences of real life witnesses to real life crimes than do the laboratory experiments under protective conditions. This suggests that the low identification accuracy rate (32%) of a stressed and/or traumatized soldier/witness is a closer estimate of the accuracy of a real eyewitness to a violent crime.

5. Archival Police Records

When the police investigate a crime observed by an eyewitness, and have a suspect who may have committed the crime, they normally create a lineup including the suspect and ask the eyewitness to attempt an identification. Archival police records can be examined as data to determine the percent of time that the eyewitness in fact picked the person the police suspected of committing the crime. If the unlikely assumption is made that the suspect under suspicion is always the perpetrator, then the percent of the time eyewitnesses choose the suspect is a measure of the accuracy of eyewitness identifications.

Several analyses have been published based on archival police records, of which Behrman and Davey (2001) and Tollestrup, Turtle and Yuille (1994) provide the most detailed and largest. Both Behrman and Davey, and Tollestrup et al. used archival police data collected over a period of time, each from a single city (see also Cutshall and Yuille, 1989; Fisher, Geiselman and Amodor, 1989; and Sporer, 1992).

Behrman and Davey (2001) examined a sample of 271 felony crimes in which 689 attempted identifications of suspects were made by eyewitnesses from lineups in Sacramento, California (USA) over a 12 year period. From the subset of photographic lineups (by far the most common type used by the police), they found that 48% of the witnesses identified the suspect placed in the lineup by the police as the perpetrator.

In a similar type of analysis, Tollestrup et al. collected data from all of the police records of robberies and fraud reported over a two year period from a suburb of Vancouver, British Columbia, in which a witness (N = 166) was asked to make an identification of an unfamiliar suspect from a lineup. Tollestrup et al. report that 32% of eyewitnesses picked the suspect whom the police had placed in the lineup.

If the police suspect is always the perpetrator, then the 40% average of these two studies is the maximum accuracy level displayed by eyewitnesses to real crimes in these police data. However, there is no assurance that the suspect placed by the police in the lineup is the true perpetrator; nor is there a way to discover the percent of time the police have or do not have the true perpetrator. Consequently, an estimate based on archival data can only provide an upper limit on accuracy. To the extent that some persons suspected by the police are innocent of the crime, not all of the times an eyewitness picks the suspect will be correct. For example, if it is assumed that the police have the perpetrator in half the lineups and an innocent suspect in the other half, then half of the 40% identifications, or 20%, are correct identifications, and the rest are erroneous identifications.

These two archival analyses included as a variable the time delay between the crime and the presentation of the lineup. In Behrman and Davey, a delay of less than a week lead 55% to chose the police suspect; a delay of longer than a week reduced this to 45%. The delay effect was even greater in the Tollestrup et al. analyses: less than a week's delay lead to choosing the police suspect 57% of the time; a delay of longer than a week dropped this to 23%. While there is some laboratory experimental data on the effects of the delay between crime and lineup presentation, these archival data show the effect of more than a week's delay are much stronger, and much more relevant to real life.

Another important factor that inflates these accuracy scores concerns the system variables. Wells and others have shown, for example, that absence of a proper bias admonishment (e.g., the perpetrator may or may not be in the lineup, and you can answer that none of these people are the one I saw) can reduce identification accuracy to 0% (e.g., Wells & Bradfield, 1998; 1999). Further, non-blind administration of the lineups by the police (Garrioch & Brimacombe, 2001) and the other system variables are rarely set at optimal levels in current police practice (Wogalter, et al., 2004), even the lower scores found in this data set are overestimates.

Police archival data, of all the data bases considered, are the most relevant to the basic question under investigation: what is the accuracy of real eyewitnesses to real crimes making identifications from lineups administered by real police. The police archival data are based on real eyewitnesses to real crimes, making identifications from real lineups administered by real police. These data are limited in only two respects: some unknown percentage of time the eyewitness and the police both identify the wrong perpetrator (which inflates the estimated eyewitness accuracy score); and the choosing rate is inflated by the faulty bias admonishments, non-blind lineup administration of the lineup, and other faulty application of the system variables.

Discussion

Review of the Results from the Five Data Bases

We analyzed results from five independent data bases for evidence of the accuracy of eyewitnesses when making identifications of suspects in lineups.

The face recognition data base contains laboratory experiments designed to determine the accuracy with which people can correctly identify a picture of an unfamiliar person they have seen only once before. Across a variety of experimental designs and conditions, the average accuracy is about 60% correct. Because the accuracy was nearly perfect for pictures of persons familiar to the subject, the low accuracy level in the stranger experiments suggests that the ability to retain a memory for a stranger is quite poor. Since virtually all eyewitnesses to crimes are being asked to identify a stranger, this stranger effect is a major depressive force on eyewitness identification accuracy. The face recognition data provide a pure estimate of about 60% as the maximum accuracy with which witnesses can identify a stranger. The 60% accuracy will be reduced by all the factors concomitant to observing a crime, such as were variously present in the other data bases reviewed.

The laboratory lineup experiments data base includes 50 experiments in which subjects observed a crime, were shown a lineup containing a suspect, and asked if they could pick anyone as the perpetrator of the crime. Across all of the experiments analyzed, 48% accurately identified the perpetrator when he was in the lineup, and 48% rejected the entire lineup when the perpetrator was not present. However, a more realistic upper limit on accuracy has to be lower than 48%, because the estimator and system variables in these experiments were optimal. In real life, conditions less favorable to accuracy prevail.

The field experiment data base consisted of a number of non-laboratory experiments designed to simulate laboratory research using subjects more typical of eyewitnesses to crimes. Under conditions designed to promote attention and memory for a stranger "perpetrator," an average of 42% of the witnesses were able to pick the correct perpetrator from a lineup. Again, this sets an upper limit in the absence of a crime and stress, but is otherwise realistic in using a broader sample of witnesses, none of whom knew they were being tested.

The military interrogation data base provides data on the accuracy with which soldiers could identify their interrogators 24 hours after being intensively and stressfully questioned. The results, based on over 500 soldiers, showed that under high stress questioning, only 32% could correctly identify their "perpetrator," with nearly all of the remainder picking someone they had never seen before (erroneous identifications). Accuracy increased to 68% under lower stress (but still intensive) conditions.

It is likely that the high stress conditions here most closely resemble the stress and trauma experienced by actual victims of crimes, and by actual witnesses to violent crimes. When fear and trauma are high, 32% correct may represent the very best upper limit on eyewitness identification accuracy.

The police archival data base was examined because the results from these data, in which representative real eyewitnesses to real crimes attempted to identify real police suspects from real lineups created and administered by real police, are the best estimate available of the accuracy of an eyewitness who testifies to an identification in court. These results show the percent of the time the eyewitnesses picked the police suspects. In the two larger studies, those rates averaged about 40%--only less than half of the time did the eyewitnesses identified the person suspected by the police of having committed the crime. The actual accuracy shown in these real police data has to be lower, for two reasons. First, even if police work were always superb, it is unreasonable to postulate perfection: at least some of the time, the eyewitnesses are choosing an innocent police suspect. This percentage is unknown and unknowable. Second, because lineups are only rarely administered blind, and often without proper control of the other system variables, the upper limit on eyewitness accuracy from these data cannot even reach 40% at best, and is probably significantly lower.

<u>Convergence of Estimates of Accuracy Rates</u>

The estimated upper limits on eyewitness accuracy results from these five data bases are acquired under both low and high levels of stress. For the low stress level accuracy estimates, the face recognition is about 60%, the laboratory lineup studies about 50%; and field studies about 40%. The military interrogation under relatively low stress shows identification accuracy of 68%: an anomaly among these numbers. The two data bases providing high stress estimates include the military interrogation upper limit of just above 30%, and the police archival estimate of 40%.

Even with the low values of these upper limits on accuracy estimates, all of them were acquired under conditions that greatly simplified or compromised real life circumstances. The two that most closely approximate real life have limitations that had to have inflated their identification accuracy. The military interrogation provided ideal conditions for observation (including a half hour duration of direct observation under focused attention), and ideal conditions for identification, so that both conditions provide an overestimate compared to real life, particularly so for the low stress condition. The police archival data cannot be scored for correctness of identification, since some proportion of the suspects were innocent of the crime. That unknown proportion lowers the upper limit estimate by that amount from that data base.

These data indicate that eyewitnesses identify a perpetrator correctly less half the time under optimal conditions. For fearful crimes, for poor observation conditions, and for poor identification lineup conditions, identification accuracy is less than 40% correct.

Wells and Bradfield (1998; 1999) have shown that failure to adhere to proper system variable values can result in 0% identification accuracy; every witness can be led to make an erroneous identification. Real life identification accuracy can be improved upon only by instantiating the system variables so that police investigators rigorously follow the US Attorney General's Guidelines on handling eyewitnesses (Technical Working Group for Eyewitness Evidence, 1999). At present, many of the guidelines are not in place, with a known consequence of a reduction in accuracy of identifications. However, as the evidence from the five data bases shows, improvement of the system variables cannot increase eyewitness identification accuracy beyond about 30% for highly stressful crimes, to about 50% for less stressful ones.

Conclusion

The criminal justice system must recognize and accommodate to these low accuracy rates that are the result of processes endemic to witnesses—mainly fear and the stranger effect--and endemic to crimes--the remaining estimator variables. It is critical that police follow the proper system variables during the investigation, those comparable to the US Attorney's Guidelines (Technical Working Group for Eyhewitness Evidence, 1999), but that no matter how successful they are, the majority of identifications of defendants as the perpetrator testified to by eyewitnesses in court will still be erroneous. Gross et al. (2005) assumed that the 328 exonerations of erroneous convictions found between 1989 and 2003 are only the tip of an iceberg of thousands of innocent persons in prison. The data presented here suggest that the majority of convicted persons who had pleaded innocent and in their trial were confronted with primarily or exclusively eyewitness identification evidence, that they were erroneously rather than correctly identified as the perpetrator. Because juries tend to convict most defendants based only on an identification testified to by a single eyewitness (Devlin, 1976), these innocent defendants are convicted. They are a substantial percentage of the prison population, and the 328 who were exonerated after being convicted between 1989 and 2003 are indeed a tiny tip of a very large iceberg.

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References marked with an asterisk indicate studies included in the averages of percents.

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Table 1. Exonerations Between 1989 and 2003 of Innocent Persons Convicted Due to Erroneous Eyewitness Identifications (from Gross, et al., 2005)

	Number of Exonerations	Number of Erroneous IDs	Percent of Erroneous IDs
Murder	199	97	49%
Rape	120	106	88%
Other	9	6	67%
Totals	328	209	64%

Table 2. Correct Recognition of Familiar and Unfamiliar Faces

	Correct Recognition as Presented Before (Hit)	Correct Rejection as Never Presented before
Familiar Faces (see text)	95% to 100%	90% to 95%
Unfamiliar Stranger Faces (Shapiro & Penrod, 1986)	59%	68%

The Stranger Effect is 97% - 59% = 38%.

Table 3. Percent of Responses in Laboratory Experiments to Perpetrator Present (41 Experiments) and Perpetrator Absent Lineups (28 Experiments) From Haber & Haber, unpublished).

Perpetrator Present Lineups			Perpetrator Absent Lineups		Average
48%	31%	21%	48%	52%	48%

Table 4. Percent of Responses in Military Interrogation Experiments to Perpetrator Present lineups under low and high stress (N = 509). Data from Morgan, et al. (2004).

Low Stress Conditions			High Stress Conditions		
Hits	False Alarms	Misses	Hits	False Alarms	Misses
68%	29%	3%	32%	61%	7%

Table 5: Percent of Responses in Police Archival Data in which Witness Picks the Police Suspect in Sacramento (Behrman and Davey) and Vancouver (Tollestrup, et al)

	Sacramento	Vancouver	Average
Overall Mean	48%	32%	40%
Less than 1 Week Delay in testing	55%	57%`	56%
More than 1 Week Delay in Testing	45%	23%	34%