Testing the reflection assumption: an examination of the external validity of published studies on lineup identification accuracy.

RUNNING HEAD: A Comparison of Eyewitness Ecology in the Laboratory and the Field

Testing the Reflection Assumption:

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Abstract

The purpose of this project was to draw attention to the problem of *eyewitness ecology*, a term that we use to refer to the relationship between the eyewitness and the criminal environment, which includes characteristics of perpetrators, witnessing conditions and the identification test. Our goal was to compare the range of eyewitness ecologies found in real world cases with those found in laboratory studies of eyewitness identification. Toward this end, we coded the characteristics of the published literature on criminal identification in the laboratory ($N = 290$). The results were compared to the characteristics of a stratified random sample of felony cases ($N = 721$) obtained from a large metropolitan district in the United States. The results provide a systematic overview of the methods and procedures that have been used to study eyewitness identification in the laboratory, and suggest areas in which further research is needed to better reflect the range of eyewitness ecologies that are found in actual criminal cases.

Testing the Reflection Assumption:

A Comparison of Eyewitness Ecology in the Laboratory and the Field

Experts in the area of psychology and the law are often called upon to share their knowledge and understanding of the eyewitness identification literature with the legal system. For eyewitness experts, this often means testifying for the defence regarding the factors that can negatively impact eyewitness memory (Kassin, Tubb, Hosch, & Memon, 2001; Kassin, Ellsworth, & Smith, 1989; Pezdek, in press), or making policy recommendations for the handling and preservation of eyewitness testimony (e.g., Wells, Small, Penrod, Malpass, Fulero, & Brimacombe, 1998). Inherent in these applications is the assumption that the research conditions adequately reflect the eyewitness context in "real world" cases. Stated another way, individuals testifying on how different factors may affect eyewitness performance are assuming that the empirical findings, the vast majority of which come from laboratory studies, will generalise to real world identifications (e.g., Pezdek, in press; Yarmey, 2001). Whilst this may be a reasonable assumption, it is one that has been given little systematic empirical treatment.

Many of the debates regarding the applicability of laboratory research findings to the legal system have revolved around the question of generalisability. In making generalisations from the literature to actual cases, some have argued that we are in a position to generalise because the laboratory methods used to study eyewitness identification are diverse enough to capture the essential characteristics of real world crimes (e.g., Deffenbacher, 1984; Haber & Haber, 2000; Loftus, 1983; Pezdek, in press; Yarmey, 1997; Yarmey, 2001), whereas others have cautioned that we must be sensitive to differences between the two settings with respect to the environmental and emotional

context in which eyewitness identifications are carried out (e.g., Konečni & Ebbesen, 1986; Elliott, 1993; Flowe, Finklea, & Ebbesen, in press; Malpass & Devine, 1981; Pachella, 1986; Yuille, 1993). Yuille and Wells (1991) have expressed the need for more comparisons between experimental research and field contexts so that the similarities and differences between the two can be enunciated. Although we would optimally want to know how memory performance between the two differs, a more basic and unanswered question is *how do conditions in the laboratory compare to those found in actual crimes?*

In the present project we sought to compare the range of circumstances in which eyewitness identification occurs in the lab and in actual criminal investigations. There are several motivations for examining the range of circumstances in which testimony is rendered. First, a quick search of U.S. appellate cases on Lexis/Nexis suggests that there is a wide range of real world contexts in which the laboratory studies on eyewitness identification have been applied. Second, the eyewitness context differs from the context in which the research was carried out in a large number of these cases. Third, expert witnesses typically testify in a given case about a number of factors that affect identification accuracy in the laboratory, not just about a single factor. Fourth, procedural recommendations derived from laboratory research (e.g., Wells, Small, Penrod, Malpass, Fulero, & Brimacombe, 1998) affect all cases in the legal system once they are implemented, not just potentially problematic cases in which eyewitnessing conditions are poor. For these reasons, examining the extent to which the laboratory studies capture the range of naturally and frequently occurring psychological processes in the field is an issue worth pursuing.

To be sure, maximising external validity is not the primary goal of all laboratory research in psychology. Creating artificial and highly controlled conditions that may very

well never exist outside of the laboratory are often necessary for the development and

testing of theory (Mook, 1983). However, it seems manifest that most researchers in the

eyewitness memory domain anticipate that their results might be applied in courtrooms or

play an instrumental role in public policy development. Here we wish to echo this point

using a statement made by Mook (1983) in his highly influential paper on the issue of

external validity in social research:

> Of course there are also those cases in which one does want to predict real-life
> behavior directly from research findings. Survey research, and most experiments in
> applied settings such as the factory or classroom, have that end in view. Predicting
> real-life behavior is a perfectly legitimate and honorable way to use research. When
> we engage in it, we do confront the problem of E[xternal] V[alidity]…" (p. 386).

Indeed, there are numerous examples, too many to document, of the application of

eyewitness memory research in the legal system (see Flowe, Finklea, & Ebbesen, in press).

Therefore, if only because there seems to be a demand for applying laboratory research

findings to the legal system, a key empirical issue for researchers is ascertaining how

various factors that can influence eyewitness memory have been represented in laboratory

studies. In so doing, we can identify areas where additional work seems to be needed in

order to increase the range of cases to which generalisation is possible.

The sampling of subjects and stimuli to represent the people and environments to which

research results will be applied is not a new problem, nor is it unique to the eyewitness

identification research domain (see Wells & Windschitl, 1999 for a discussion of stimulus

sampling in the social psychological literature). Egon Brunswik (see Hammond & Stewart,

2001) put forth the concept of "representative design" to convey his notion that

psychologists should design their experimental conditions in a manner that represents the

naturalistic conditions to which they wanted to generalise their results. Just as randomly

sampling participants produces results that fairly represent the population (in the long run), designing experimental conditions that are representative of naturalistic conditions increases external validity. Brunswik, by all means, was not calling for randomly selecting conditions from a population; rather, he was arguing that psychologists should particularise the conditions in the natural world to which generalisations were intended, and accordingly, design experiments that represent those conditions.

Our first objective in the present study was to analyse the overall distribution of the conditions found in the laboratory and archival cases. In this manner, eyewitness ecology, or the nature of the participants, eyewitnessing conditions, and identification procedures, was described. Second, the ecological conditions of laboratory and archival cases were compared to determine whether there are areas in which additional research seems warranted. For instance, how long are eyewitnesses exposed to the culprit in the archives and in the laboratory? How often are eyewitnesses subjected to violence? How often do eyewitnesses interact with the perpetrator before the onset of the crime? What is the lower and upper limit for the retention interval between the crime and the identification test? Does the retention interval length vary depending on the type of identification procedure employed? Answers to questions such as these can offer some guidance to researchers who are interested in utilising real world parameters to inform the types of procedures and methods that they use to study eyewitness identification in the laboratory.

Our second objective was to assess positive identifications of the suspect in the archival cases in relation to whether the identifications were cross-race, whether a weapon was present during the crime, whether the witness was exposed to violence, and whether a showup test (versus a lineup test) was conducted. Laboratory research indicates that these

conditions can negatively impact identification accuracy and eyewitness experts testify about these effects in the courtroom (Kassin et al., 2001; Pezdek, in press). However, the majority of field studies with actual eyewitnesses to crime have found no association between suspect identification rates and the presence of these conditions: For the cross race effect, Behrman and Davey (2001) found an association (however, their case sample was not random), whereas other studies have not found the association (Pike, Brace, & Kyman, 2002; Valentine, Pickering, & Darling, 2003; Wright & McDaid, 1996). For the weapon focus effect, Tollestrup, Turtle, and Yuille (1994) found a marginal reduction in suspect identifications in robbery cases when a weapon was present. Other archival research, however, did not find an association between weapon exposure and identification rates (Behrman & Davey, 2001; Valentine, Pickering, & Darling, 2003; Wright & McDaid, 1996). For stress, Wright and McDaid (1996), did not find that exposure to violence was associated with suspect identifications. Valentine, Pickering, and Darling (2003) found in their sample of archival cases that eyewitnesses who were assaulted had somewhat *higher* rates of suspect identifications compared to those who were not assaulted. Lastly, for showups, Behrman and Davey (2001) found that positive identifications of the suspect were significantly higher in showups compared to lineups. In contrast, Gonzalez, Ellsworth, and Pembroke (1993) found in their sample of real world identification procedures that positive suspect identifications were made less frequently in showups compared to lineups. We added to this growing body of field research by examining whether the factors that negatively influence identification accuracy in the laboratory might be moderated by duration of exposure and retention interval length. For instance, would increasing the duration of exposure to the culprit decrease the size of the cross race identification

correlation? Of course, in field studies it is not known whether the suspect is guilty; but if the present study found, for instance, that cross race participants identify the suspect less often only when the duration of exposure to the culprit is relatively short, this would at least suggest that perhaps further laboratory studies are needed to assess the boundary conditions for the effect.

Method

Literature Review

Sampling. A thorough literature search using the Psychological Abstracts Information Services (PsycINFO) was conducted, using key word and title word searches related to our endeavor (e.g., eyewitness, lineup). Our only criteria for inclusion were that study participants witnessed some sort of event and were presented with a lineup or showup task. From this search, the features of 290 studies from 200 peer-reviewed published papers involving 52,762 participants were coded (139 papers included 1 experiment, 42 papers included 2 studies, and 19 papers included 3 or more studies). The journals in which the studies most frequently appeared were as follows: 75 studies (26%) were published in the *Journal of Applied Psychology*, 49 in *Law and Human Behavior* (17%), and 28 (10%) in *Applied Cognitive Psychology*.

Of the studies, 44% presented the target in a video, 17% live in a lab, 13% live in an auditorium or a classroom, 11% in a photograph, 8% live in a natural setting, and 7% in a slide sequence. The target was portrayed under conditions that were not criminal in 41% of the studies (e.g., laboratory participants memorized a photograph, or field participants were asked to identify a customer or a researcher with whom they had previously interacted), and

the target committed theft in 32%, robbery in 20%, other types of crimes in 4%, assault in 1%, and committed rape in less than 1% of the studies.

   Coding Scheme. For purposes of generally describing the studies, we recorded the number and type of independent variables investigated, whether the independent variables were correlational, how the variables were manipulated (between or within subjects) and the setting in which the experiment took place (laboratory, home, store, or other natural setting). The number and type of participants recruited (children, college students, or other adults) and participant demographics (race, age and gender) were coded as the primary "eyewitness" variables. "Perpetrator" characteristics gathered from the studies included the number of targets shown, target physical features, and whether the target was disguised. For the witnessing condition variables, we coded whether participants were "victimized" (i.e., had something stolen from them by a confederate perpetrator during a live staged event), exposed to a weapon, witnessed the crime alone or in a group, and whether participants knew they were in a memory experiment prior to seeing the event. Critical event (i.e., the simulated crime, or other circumstances in which the target was portrayed) and target exposure duration (in minutes) were also recorded when available. Finally, characteristics of the identification task that we coded were: whether participants were asked to describe the target, type of identification task administered, the actual and functional size of the lineup, method of lineup presentation (simultaneous or sequential), whether the target was present, how foils were chosen (with or without pilot work), who constructed the lineup (experimenter, local police or other method), and the description and identification retention intervals.

When information was not provided in a given paper, no speculation was made regarding what the missing data values were. For congruency with the archival analysis, data regarding "culprit" characteristics were based on the total number of simulated perpetrators, and data pertaining to "eyewitnesses" were analysed based on the total number of simulated eyewitnesses.

Archival Review

Sampling. The characteristics of the laboratory studies were compared to a stratified random sample of 721 felony cases (robbery $n = 238$, rape $n = 301$, assault $n = 182$) that were referred for prosecution by the San Diego Police Department to the San Diego District Attorney's Office between January 1, 1991 and December 31, 1995. Of these cases, 65% ($n = 468$) were accepted for prosecution ($n = 208$, rape; $n = 176$, robbery; $n = 84$ assault), while in the others the issuing D.A. dropped the charges against the defendant.[2] These three types of crime were chosen because they were serious felony offenses that were likely to involve eyewitness testimony as a pivotal element. A suspect in each of these cases had been arrested by the police, and the case had been handed over to the prosecutor's office to determine whether charges should be filed. Rejected and accepted cases were included in the sample because we wanted to include a range of eyewitness ecologies. On one end of the spectrum, the rejected cases might largely represent cases in which the eyewitness ecology was likely to lead to poor memory formation, whereas on the other end of the spectrum, the accepted cases might largely represent circumstances in which the eyewitness ecology promoted stronger memory representations.

The testimony of 1,319 eyewitnesses is represented in this sample (403, 450, and 466 of the witnesses were from robbery, rape and assault cases, respectively). The number of

witnesses in each case ranged from 1-10, but typically the cases involved the testimony of 1 eyewitness (Robbery: 40% involved 1 eyewitness, range = 1-9; Rape: 75% involved 1 witness; range = 1-10; Assault: 42% involved 1 eyewitness, range = 1-10).

Coding Scheme. Similar to the literature review, case files were coded both at the defendant level and the individual eyewitness level. With the exception of the exposure duration variables (crime and target), lineup functional size, and weapon presence and use, the coding procedures previously described were utilized. For the exposure duration variables, because this information was not actually available, estimates of these time intervals were made when coders felt they could reasonably approximate it from the eyewitness statements (if any) described in the police crime/incident report. Coders estimated lineup functional size if a copy of the photographic lineup was in the case file. Functional size was estimated based on the number of persons in the lineup that fit the physical description of the culprit. Finally, for the weapon presence and use, an eyewitness' awareness of the threat or use of a weapon, rather than the actual presence and use of weapon, was coded from the archival case files.

For descriptive purposes, and to assist with the design of future experiments, a few additional variables were coded in the archival cases. These included viewing conditions (i.e., lighting and distance away from the perpetrator), whether the police reported that the witness was under the influence of alcohol and/or drugs, and distinctive physical features of the perpetrator noted by the police on the arrest report. These variables were not coded in the experimental studies because the information was usually not provided in the papers.

For the purposes of this study, a "case" was defined by a single defendant, and in the event a file involved felony complaints against more than one individual, the file was seen

as involving more than one case and was coded accordingly. Because defendants were often charged with more than one crime, cases were assigned to a crime category (rape, robbery, or assault) on the basis of the most serious charge on the felony complaint. The variables coded were taken from a number of sources in the case files, such as the crime report, arrest report, probation officer's report, and the investigator's follow-up report. As might be expected, the completeness and content of the case files varied from case to case (case files ranged in size from a single file folder to several large boxes); as a result, the number of responses across variables varied.

    Coder Reliability. Before coding information from the criminal case files, the 46 coders involved in this portion of the study were given detailed (written and verbal) instructions, as well as a number of practice cases to code. Intercoder reliability was measured by assigning one randomly chosen case to each of the coders at some point during data collection and observing intercoder agreement for the variables in this case. As might be expected given the extremely "low" or basic level of coding involved in this study, the agreement between the coders was extremely high. To measure intercoder reliability for variables that required estimations on the part of the coder, Cronbach's alpha was computed for each. The values for these alphas were .86 for exposure duration, .98 for description latency, and .89 for identification latency.

Data Analysis

    Comparisons between the studies and the archival cases were made at both the defendant and the eyewitness level. For categorical variables, chi-square analyses were utilized to determine the statistical relationship between the variables manipulated by researchers and those naturally occurring in the real world. Since the retention interval and

duration of exposure data were not normally distributed, the Mann-Whitney U statistic was used in analyses involving these variables.

Archival results were expected to differ for some variables depending on the type of crime committed (e.g., the majority of witnesses in rape cases are female). For these variables, separate analyses were conducted for each crime category (rape, robbery and assault).

Results

Independent Variables

Of the 290 studies, 72% employed a true experimental design, 23% included both experimental variables and correlational variables (such as participant age or personality traits), and the remaining 4% exclusively examined correlational variables. The studies typically manipulated 2 (mode) independent variables ($M = 2.57$, $SD = 1.61$, range = 1 to 14 independent variables). Table 1 presents the top ten independent variables found in the research literature. For every variable listed, except for "memory ability" (which was most often operationalized as a continuous variable), the mode number of design levels was 2.

Eyewitnesses

Gender. Participant gender was available for coding in 37% ($n = 108$) of the studies and for 95% ($n = 1,251$) of the archival witnesses. Across all crime categories, 44% of the research participants and 46% of the archival witnesses were male. Gender composition in the archives varied by case type. The majority of real world witnesses were male in robbery (65%) and assault (60%) cases, whereas the majority of the witnesses (84%) in rape cases were female.

Age. The majority of studies recruited college students to serve as witnesses ($n = 203$), and other adult populations ($n = 30$) and participants under the age of 18 ($n = 14$) were recruited less frequently (41 studies drew subjects from multiple populations and 2 studies did not report sampling information). Across research participants, 68% were college students, 22% were from other adult populations, and 9% were children. Participant age ranged from 2 to 94 years (89 studies reported this information). For archival eyewitnesses ($N = 1,133$), the age range was 4-86 years, with the average age being 29.28 years ($SD = 13.04$, median = 26).

Race. Race was known for 5294 research participants (34 studies reported participant race) and for 1,142 archival eyewitnesses. The distribution of eyewitness race in the archives significantly differed from the distribution found in the studies, $\chi^2 (4, N = 6436) = 1154.07, p < .0001$. Whereas 86% of laboratory witnesses were White, only 49% of real-world eyewitnesses were.[3] As for other racial categories, the background of participants in the studies was: 5% Black, 3% Asian, 2% Hispanic, and 2% were designated as belonging to other racial categories. In the archives, the backgrounds were 18% Black, 6% Asian, 26% Hispanic, and less than 1% were designated as belonging to other racial categories.

Relationship to Target. For assault, rape, and robbery archival witnesses, 92%, 79% and 21%, respectively, were in some way acquainted with the suspect. On the other hand, none of the witness subjects were previously familiar with the target. In a few of the papers, researchers indicated that when subjects volunteered to the experimenter that they were familiar with some members of the lineup, they were excluded from the data sample.

Alcohol and Drug Use. A total of 13% of all real world witnesses were under the influence of drugs ($n = 28$), alcohol ($n = 122$) or some combination of the two ($n = 26$)

during the crime. Most of these witnesses (73%) were from rape cases. A description of the

culprit was given to the police by 45% of witnesses who were under the influence ($n = 79$).

Additionally, 22% of the intoxicated witnesses were given an opportunity to identify a

suspect ($n = 39$); 44% of these cases involved defendants and witnesses previously

unacquainted. A total of 4 published papers examined the effects of alcohol (Dysart,

Lindsay, MacDonald, & Wicke, 2002; Read, Yuille, & Tollestrup, 1992; Yuille &

Tollestrup, 1990) or drugs (Yuille, Tollestrup, Marxsen, Porter, & Herve, 1998) on

identification accuracy.

Perpetrator

Gender. The gender of the targets significantly varied in the archives compared to the

research studies, χ^2 (1, $N = 1192$) = 57.73, $p < .001$. While both laboratory and actual

criminal cases involved primarily male targets, the laboratory studies utilized a greater

number of women ($n = 94$) compared to the archives ($n = 41$). When women were

employed as the target in the studies, 62% were portrayed in conditions that were

noncriminal, 34% engaged in simulated thefts, 2% in robberies, and 2% in other criminal

scenarios. With respect to the archives, 63% of the female defendants were alleged to have

committed robbery, 4% rape, and 32% assault.

Age. Information was provided in 106 of the studies regarding the general age range of

the culprit (rarely was the exact age of the culprit provided). A college student or an adult

under the age of 26 was the target most often in the studies (86%), followed by adults over

the age of 26 (13%). A child was the target in 1% of the studies. In the archives, the

defendants were on average aged 29 years ($SD = 9.10$, range = 16 to 79, median = 28 years,

$n = 628$).

Race. The distribution of target race ($N = 620$) in the archives significantly differed from the race distribution for targets found in the studies ($N = 236$, with 39% of the research studies reporting this information), χ^2 (4, $N = 856$) = 198.71, $p < .001$. In the archives, 30% of the defendants were White, whereas 82% of the targets in the studies were White. With regard to the other racial categories found for the targets in the studies, 58% were Black, 5% were Asian, 5% were Hispanic, and less than 1% categorized as "other". In the archives, the distribution was 33% Black, 3% Asian, 31% Hispanic, and 3% were categorized as "other".

Disguises. The description(s) of the culprit given by the eyewitness(es) included a disguise in 6% of the archival cases ($n = 44$). Caps and hats disguised the perpetrator's appearance most frequently (68%), followed by glasses (20%), bandanas (7%) and stockings (5%). The results for the empirical studies were similar. Disguises were used by simulated culprits in 8% of the studies ($n = 15$), with hats (47%), glasses (27%), hairstyle and eyeglasses (20%), and a mask (7%) serving as the disguises.

Distinctive Features. The police noted on the arrest report distinctive physical features for 33% of the defendants ($n = 241$). Of these defendants, 63% had one or more visible tattoos, 17% had prominent scars on their body, 11% had faces with tattoos, scars, or some other unusual feature, and 3% had missing, broken or gold-capped teeth. None of the papers in the literature mentioned whether their culprits had such features, though one study manipulated whether the target wore an Elvis wig or had a chipped tooth (Searcy, Bartlett, & Memon, 2000).

Witnessing Conditions

   Victimization. In the archives, 55% of the eyewitnesses were victimized, whereas 4% of research subjects were led to believe that they were victims of a crime. Real world witnesses were victimized 42%, 70%, and 55% of the time in assault, rape and robbery cases, respectively. In the studies, for ethical reasons, the victimizations were limited to theft.

   Critical Event. In the research studies, 67% of the participants knew that they were taking part in an experiment before viewing the target. Of those who did not know, 71% were presented with a noncriminal event and 97% were debriefed before the ID test was administered. Across participants who knew from the beginning that they were in an experiment, 77% were not told that they would be given an identification test until after the critical event was presented.

   Because the places in which the critical event occurred were so different for the archives compared to the laboratory studies, only descriptive information will be provided. The critical event was presented in the laboratory in 70% ($n = 209$) of the studies, a lecture hall setting in 21% ($n = 63$), the participant's home in 1 study, and in public places in 9% ($n = 27$) of the studies (9 studies presented the event in multiple settings). The crime location was known in 655 of the archive cases and took place in the following settings: at a residence (52%), on the street or in an alley (18%), in open public places (13%), in other outdoor settings (8%), in other indoor settings (3%), in multiple locations (3%) or in vehicles (1%).

   Alone or in a group. For 260 laboratory studies and 257 of the archive cases, information was available regarding whether the witnesses saw the crime by themselves or

with other witnesses. The critical stimulus was viewed by a lone participant more often in the studies compared to the archives (45% versus 18%), χ^2 (1, N = 516) = 44.41, $p < .001$. Furthermore, in 222 of the archival cases it was clear that 2 or more witnesses were previously acquainted with one another. None of the research investigations reported whether witnesses were acquainted with one another prior to seeing the event.

Event Exposure. Table 2 presents the event and target exposure durations for the studies and the archives by the type of event witnessed. For the archival cases in which event duration was estimable ($n = 507$), compared to the eyewitness studies ($n = 228$), event exposure time was considerably longer (Mann-Whitney U, $\underline{z} = -15.54$, $p < .0001$) and the data were encompassed by a broader range of duration. For the studies, mean exposure time was 2.61 min ($SD = 5.98$, range = .05 to 40 min, and median = 1.04 min). For the archives, the criminal event lasted on average 59.87 minutes ($SD = 390.75$, range = .08 to 7200, and median = 8 min). Additionally, as shown in Table 2, exposure duration varied depending on crime category for both the archives and the studies.

Target Exposure. As shown in Table 2, witnesses ($n = 404$) in the archives were exposed to the target for a longer duration compared to research participants (Mann-Whitney U, $\underline{z} = -17.89$, $p < .0001$). Duration of exposure to the target was reported by 205 studies; on average, research participants viewed the target for 2.26 min ($SD = 6.39$, range = .033 to 40 min, and median = 0.57 min). Archival witnesses viewed the culprit for 65.74 min on average[4] ($SD = 205.17$, range = 0.00 to 2423, and median = 7.8 min).

Violence. In the real world cases, 59% of eyewitnesses were exposed to violence (this variable was known for 1,251 witnesses). Of the witnesses exposed to violent events, 117 first interacted with the culprit during the violent act, whereas 176 first interacted with the

culprit before the violent act (for 46 witnesses, this variable was unknown). The remaining 399 eyewitness exposed to violence personally knew the culprit. None of the published papers examined the effect of prior exposure to the culprit before violence. Only 2 papers manipulated level of violence (Clifford & Hollin, 1981; Cutler, Penrod, & Martens, 1987).

Weapon Exposure. A much larger proportion of archival witnesses were exposed to weapons (54%) compared to research participants (8%), χ^2 (1, $N = 54081) = 3461.31$, $p < .0001$. A total of 20 staged crimes involved weapons. Of witnesses who saw a weapon, 60% viewed a gun, 15% a knife, 10%, a bomb, 5% a truck, and 5% a syringe (one study did not specify what the weapon was). Archival witnesses (N=1,046) viewed a greater variety of weapons than research participants did (12% saw a firearm, 7% a blunt instrument, 10% a knife, 1% a ligature, 37% the culprit's hands or feet were used as a weapon, 24% were exposed to a penis, and 9% fell into the other category, which included weapons such as rocks, bottles, and vehicles). Moreover, several archival witnesses ($n = 286$) saw more than one type of weapon, whereas none of the research witnesses did. Duration of weapon exposure in the archives ranged from 3 seconds to 15 hours, with eyewitnesses typically exposed for 1 minute.

Other Conditions. Other characteristics of the witnessing conditions, such as viewing distance and lighting conditions, were not available for analysis because they usually were not reported in the articles (information regarding viewing distance was given in 10 studies) or appeared to be homogenous across studies (only 2 papers manipulated lighting conditions and the remaining simulated crimes appeared to be conducted in well-lit areas). Out of the 1,191 archival witnesses for which lighting information was known, the following characterizes their lighting conditions: 35% were indoors in a well-lit area, 20%

were in the daylight, 18% were outdoors at night under street lighting, 15% were outdoors at night in the dark, 9% were indoors in a dimly lit area, and 3% were indoors in the dark. As for viewing distance, the closest contact witnesses had with the culprit was 36.25 ft on average (SD = 125.46, range = 0 – 2400, mode = 0 ft, with "0" indicating that the culprit and witness were in direct physical contact with each other).

Suspect Identification

Description. In just about half of the research studies (44%)**,** subjects were asked to give a description of the target. In the archives, description was given by 500 witnesses, 746 did not give a description, and 1 witness died before the investigators could ask him for one (this variable was known for 1,247 witnesses).

Identification Task Construction. For the studies, a total of 307 identification conditions were run (some studies ran more than one type of identification procedure). In 74% of the studies, the foils were selected for the lineup without pilot work. In these studies, the foils were selected using the "eyeball" technique; that is, in the view of the investigators, the foils appeared similar to the target. In the pilot tested lineups, the foils were selected on the basis of similarity ratings made by subject raters. Across all studies, the foils were selected by matching them to a description of the target in 20% of the studies, by matching them to the target's photograph in 26%, by using other strategies in 29%, and no information was provided in 21% of the studies. The lineups were mock witness tested in 23% of the studies to determine whether the fillers were adequate. The number of persons presented in the identification test ranged from 1 to 40, with 55% of the tests involving 6 persons. The functional size of the lineup was reported by 19% of the research studies, and ranged from 1 to infinity (in some studies, lineup member similarity was manipulated). Finally, the

position of the target (or target substitute) in the lineup was reported in 47% of the studies. For the studies in which this variable was known, 47% counterbalanced or randomized the position of the target, 29% utilized more than one position (typically 2 positions), and 24% used a single position. Typically, the target was placed in positions 2 or 4 (27% of the studies used one of these positions). Finally, the police constructed the lineup in 6% of the studies.

Of course, the police arranged all of the identifications in the archives. Unfortunately the case files did not provide us with information on how the police selected the foils for the lineups. The number of persons presented in the identification procedure ranged from 1 to 50, with 51% of all eyewitnesses presented with 1 person for identification. When photographic lineups were used, 6 persons were presented for identification (range = 1 to 9 persons) in 74% of the procedures. The functional size of the photographic lineups ranged from 1 to 9 persons. For 64% of the 6-person lineups, the functional size was estimated as 6 persons by the coders. The rate at which the target was placed in positions 1-6 was as follows: 10%, 31%, 21%, 17%, 16%, and 4%.

Identification Procedure. Because the methods used to identify suspects were so markedly different in the laboratory compared to the archives, no statistical analyses were conducted. In the archives, 31% of the witnesses ($n = 406$) were asked to identify the suspect and lineup tests were presented significantly more often to victims compared to eyewitness bystanders (67% versus 33%). Of those presented with a lineup, 344 positive identifications of the suspect were made.[5] Additionally, about 1% of the eyewitnesses in the archives who were given an identification test participated in the creation of a composite drawing, and none of the witnesses viewed mugshots prior to being given an identification

test. About 8% of the research participants viewed mugshots or a composite drawing before being given the identification test.

As Table 3 shows, while live showups were most often presented to real world witnesses (52%), photographic lineups were most often presented to research participants (77%). Live showups were viewed by 2% of research participants, no real world witnesses viewed video lineups or listened to voice lineups. The lineup faces were viewed simultaneously by 80% of research participants and 20% viewed the faces sequentially. The researchers manipulated whether the target was present in the lineup for 64% of the participants, only target present presentations were presented to 24%, only target absent for 8%, and both target present and target absent tests were presented to 4% of the participants.

Retention Intervals. The majority of descriptions were made on the same day as the incident in both the archival cases (97%) and the research studies (81%). Exact description retention intervals could be computed for 17,652 research participants (121/133 studies provided this information) and 139 archival witnesses. There was a significant difference between samples for when the descriptions were initially given (Mann-Whitney U, $\underline{z}$ = -11.66, *p* < .0001), with longer delays in real-world cases. The median description retention interval in the archives was 1.13 <u>hours</u> (*M* = 188.08, *SD* = 1030.58, range = 0 to 842.40 hours) and in the research studies it was 1.00 <u>min</u> (*M* = 29.35, *SD* = 114.36, range = 0 to 840 hours)

The identification retention interval could be computed for 43,420 research participants (234 studies provided this information) and 240 archival witnesses. The amount of delay between the crime and the identification opportunity was also significantly longer in the archive cases compared to the research studies (Mann-Whitney U, $\underline{z}$ = -14.47, *p* = .14). For

archival witnesses, the median retention interval was 36.00 hours (M = 383.08, SD = 1045.66, range = 0 to 8136 hours) and for research participants the median retention interval was 0.33 hours (M = 64.72, SD = 260.33, range = 0 to 3720 hours).

We also examined by lineup procedure the retention intervals found in the archives and lab. For showups, the median retention interval for the archival cases was 0.96 <u>hours</u> (M = 223.43, SD = 1243.42, range = 0 to 8136 hours) after the crime, whereas in the research literature it was 0.00 <u>minutes</u> (M = 0.23, SD = 0.05, range = 0 to 0.10 hours) after the crime (Mann-Whitney U, z = -17.89, p < .0001). For lineups, the median retention interval for the archival cases was 11 <u>days</u> (M = 561.98, SD = 847.44, range = 0.50 to 4992 hours) after the crime, whereas in the research literature it was 19.80 <u>minutes</u> (M = 70.22, SD = 284.88, range = 0 to 3720 hours) after the crime (Mann-Whitney U, z = -15.53, p < .0001).

Furthermore, had the 54 studies in which an exact identification retention interval was not provided been included in the analyses, the discrepancy between the archival and laboratory conditions would have been even greater. Participants viewed the target, completed a filler task, and then were given the identification test in 15 of these studies. In 34 studies, participants viewed the target, described the target, and then took the ID test. No information was given as to when the identification took place in 2 of the papers.

Witnesses in real world cases could potentially be asked to identify the culprit on occasions that follow their initial identification. The preliminary hearing typically occurred 30.52 days (SD = 29.77, range = 2.72 to 168.92, median = 17.65 days, n = 106) after the crime, and the trial typically occurred 149.45 days after the crime (SD = 90.95, range = 30.87 to 404.81, median = 103.06 days, n = 24). Out of the 796 witnesses involved in cases

accepted for prosecution, 51% testified at the preliminary hearing. In the 24 cases that went to trial, 97% of the witnesses testified.

Archival Identification Outcomes

Table 4 presents identification outcomes by the type of identification procedure that was conducted and relationship between the eyewitness and perpetrator. The results in this section will be restricted to cases in which the witness had no prior acquaintance with the perpetrator.[6] Furthermore, the actual guilt of the suspect and hence whether the eyewitness identification was in error is not known.[7]

For stranger identifications, positive IDs of the suspect were more frequent in showups (91%) compared to photographic lineups (75%), χ^2 (1, $N = 269$) = 12.42, $p < .001$. We compared suspect identification rates in photographic lineups and live showups, conditioning the data on factors shown by laboratory research to affect identification accuracy, including: own race bias, stress/exposure to violence, and weapon exposure. Additionally, we examined whether the effects of these factors on accuracy were moderated by memory strength, which was measured by the witness' duration of exposure to the culprit and by retention interval length between the crime and identification procedure. We coded face exposure duration (median = 5 min) into two categories, with durations up to 4 min coded as *short* ($n = 101$), and longer than 4 min coded as *long* ($n = 70$). ID retention interval was coded by categorizing as *short* identification procedures that occurred on the same day as the crime ($n=100$), and as *long* those that occurred on the day after the crime or later ($n=93$). Overall, a higher rate of positive suspect IDs was associated with a longer duration of exposure to the culprit's face (long = 93% versus short = 71%; χ^2 (1, $N = 171$)

= 12.92, $p < .001$) and with a shorter retention interval (short = 91% versus long = 74%; χ^2 (1, N = 193) = 10.62, $p < .01$).

For identification attempts for which the race of the eyewitness and suspect was known ($N = 211$), 52% were cross-race. Across all ID procedures, cross-race suspect identifications occurred at a lower rate (84%) compared to same-race suspect identifications (88%), though the difference was not statistically significant. Cross-race eyewitnesses were significantly less likely to ID the suspect in a photographic lineup compared to a live showup, χ^2 (1, N = 117) = 11.81, $p < .0001$, whereas no such difference was found for same-race eyewitnesses ($p = .61$).

For cases in which duration of exposure to the culprit was relatively short, the cross-race suspect identification rate across all identification procedures was significantly lower (53%, $n = 19$) compared to the same-race identification rate (87%, $n = 15$), χ^2 (1, N = 34) = 4.43, $p < .05$. With respect to longer durations of exposure, identifications of the suspect were not associated with whether the identification was cross-race (Cross: 90%, $n = 10$; Same: 100%, $n = 17$). Additionally, when the ID retention interval was long, the rate of cross-race suspect identifications was lower (67%, $n=34$) compared to same race identifications 100%, $n = 30$), χ^2 (1, N = 64) = 11.72, $p < .001$. When the ID retention interval was short, however, suspect identification rates did not differ for cross-race (91%, $n = 54$) and same-race (87%, $n = 31$) identifications.

The effect of violence on suspect identifications varied depending on how we operationalized "violence." Violence exposure (which included both witnesses who had violence perpetrated against them, or else who witnessed violence directed at someone else) did not have a significant effect on suspect identifications (exposed: 84%; not exposed:

81%), nor did physical injury to the eyewitness (injured: 85%; not injured: 81%). These results did not vary depending on whether a lineup or a showup was conducted. Differences between groups in viewing distance may account for why we did not find a significant effect of violence on identification outcomes: Of witnesses who attempted to identify a stranger-culprit, 84% of those experiencing violence were between 0-36 inches away from the culprit, whereas only 44% of witnesses who were not subjected to violence were in this range of proximity.

Across all stranger ID tests, suspect identification rates were lower if the eyewitness was exposed to a weapon (weapon exposed: 79%; not exposed: 90%), χ^2 (1, N = 230) = 3.39, p = .06. However, memory strength moderated the effect of violence on identification accuracy somewhat, regardless of how violence was measured. For brevity, we report the results for weapon exposure: Positive suspect identification rates were more similar if face exposure duration was long (weapon exposed: 92%; not exposed: 94%) compared to short (weapon exposed: 68%; not exposed: 83%), though the short rates did not significantly differ. Additionally, positive suspect identifications were higher for both weapon exposed and not exposed participants if the retention interval was short (weapon exposed: 87%; not exposed: 100%) compared to long (weapon exposed: 72%; not exposed: 73%).

Discussion

To recap, the archival analysis indicated that real world eyewitnesses were more often male in robbery and assault cases, and more often female in rape cases. The average age of the eyewitnesses was 30 years, and half of the witnesses were White. Except in the robbery cases, most of the eyewitnesses had some degree of familiarity with the suspect prior to the crime. Most of the time eyewitnesses saw the criminal event in a group of 2 or more

persons. More than half of the eyewitnesses interacted with the culprit before violence took

place. More than half of eyewitnesses were victimized. More than half were exposed to a

weapon. With respect to the culprits in real world cases, the majority were male and a third

of the culprits were White. About a third of the culprits had distinguishing features, such as

facial tattoos and gold-capped teeth. About a third of the eyewitnesses were given an

identification test, and most often, the eyewitnesses who were given the test were victims

rather than bystanders. The identification procedure most often used was a live showup

procedure. If a lineup was conducted, most often the suspect was placed in position 2.

While there was some difference between the real and experimental world for nearly

every characteristic we compared (the exceptions were target and eyewitness age and

gender), the discussion that follows will largely focus on the variables abut which experts

testify in the courtroom as being strongly related to identification accuracy (Kassin et al.,

2001; Pezdek, in press): cross race identifications, weapon exposure, exposure to violence,

culprit exposure duration, retention interval to identification, and the type of identification

task.

Meta-analyses of the cross-race effect find that cross-race manipulations significantly

decrease suspect identifications in the laboratory (Meissner & Brigham, 2003; Shapiro &

Penrod, 1986). Field studies have reported inconsistent findings, however, with some

researchers finding an association between cross race eyewitnesses and suspects and

identification outcomes (Behrman & Davey, 2001) and others not finding an association

(Pike, Brace, & Kyman, 2002; Valentine, Pickering, & Darling, 2003; Wright & McDaid,

1996). In the present study, the rate of cross-race suspect identifications was lower

compared to same race identifications in photographic lineups but not in showups. We

further found that memory strength affected the association, with suspect identifications in cross race lineups increasing as duration of exposure to the culprit increased. These results suggest that additional work is needed to identify the boundary conditions of the cross race effect.

The weapon-focus effect consistently reduces recognition accuracy, as reported by Steblay (1992) in her meta-analysis of the phenomenon. Our comparison of real world to simulated cases shows that weapons are more likely to be threatened and used in actual crimes. While a range of weapons was threatened against eyewitnesses in the real world, guns were the most frequently threatened (60%) in the pre-recorded simulations. In contrast, a perpetrator's hands and feet were the most frequently used weapons in the archives, with the use of a gun reported by about 1 in 10 eyewitnesses who were exposed to violence, a finding that is in keeping with the archival analysis conducted by Valentine, Pickering, and Darling (2003). We further found in the archives that suspect identifications decreased when a weapon was present, and that memory strength seemed to be associated with the effect. These results suggest additional avenues for laboratory research in which the effect of memory strength on identification accuracy is examined in relation to weapon exposure.

In a recent meta-analysis, Deffenbacher, Bornstein, Penrod, and McGorty (2004) found that heightened stress in laboratory studies significantly reduces face recognition accuracy. Similar to Wright and McDaid (1996), however, we did not find an association between eyewitness exposure to violence and suspect identification rates. There are numerous factors that probably interact with violence exposure that qualify its effect on memory accuracy. Perhaps for this reason expert consensus is relatively low regarding the reliability

of the relationship between stress and eyewitness memory (Kassin et al., 2001). For instance, we found that witnesses in the archives who experienced greater levels of violence also viewed the culprit from a shorter distance away than those who were not exposed to violence, a finding that has been demonstrated in a previous field study (Yuille & Cutshall, 1986).

Similar to other archival investigations, we found that suspect identifications decreased with longer retention intervals (Behrman & Davey, 2001; Tollestrup, Turtle, & Yuille, 1994) and that suspect identifications were more frequent at longer durations of exposure to the culprit (Valentine, Darling, & Pickering, 2003). The range of target exposure durations in the laboratory and in the field differed. Specifically, whereas the length of time that the culprit was in view was less than 1 minute for only 3 in 20 real eyewitnesses, this was the mode for subject eyewitnesses, with almost 3 in 5 given this limited viewing time.

The retention intervals used in the simulation studies for photographic lineups were also shorter than those found in the archives. While laboratory witnesses typically waited only about 19 minutes to identify the suspect from a lineup, archival witnesses waited about 11 days. However, while these longer retention intervals found in the real world might raise reasonable concerns regarding identification accuracy, archival witnesses were likely to be questioned about the incident within minutes of the crime. Turtle and Yuille (1994) raised the issue that the way in which real witnesses experience retention intervals might differ from laboratory witnesses. As a result, typical memory loss functions found in the laboratory might be different for real world witnesses. In support of this argument, Ebbesen and Rienick (1998) found that the accuracy of recalled events did not decay over time when participants were repeatedly questioned. In any case, additional research on eyewitness

memory performance in relation to the variables that intervene during the passage of time seems to lie ahead of us.

The general consensus among eyewitness experts (74% agreement) is that the use of showups as an identification task increases the risk of misidentification (Kassin et al., 2001). In a recent meta-analysis of photographic showup procedures, Steblay, Dysart, Fulero, and Lindsey (2003) found that showups yield equal hit rates but higher correct rejection rates compared to lineups. The latter effect was qualified by suspect-perpetrator similarity, as high similarity innocent suspects were more likely to be falsely identified from a showup. We found that *live* showups are the most frequently used identification task in the real world, presented to half of all eyewitnesses that were given an identification test. In contrast, the photographic lineup is most frequently used in our laboratory simulations, presented to 77% of subject witnesses. Only 2 experiments in the literature (Gonzalez, Ellsworth, & Pembroke, 1993; Yarmey, Yarmey, & Yarmey, 1996) compared eyewitness accuracy in live showups to lineups. Additionally, like Behrman and Davey (2001), we found that positive identifications of the suspect were significantly higher in showups compared to lineups. Additionally, in our archival sample, showups were typically administered within an hour of the crime, whereas lineups were conducted about 11 days later. These findings suggest that additional research is needed to examine the effects of live showup procedures under varying conditions on identification accuracy. This is potentially fertile ground for researchers who are interested in memory, as in a live showup there are cues, such as suspect gait and demeanour, that might affect memory retrieval in a manner that is different from a photographic showup.

While we feel that more attempts to mirror real world conditions would enhance the generalizability of laboratory studies, we acknowledge that not all conditions are equally feasible or ethical. Obviously, it would be much easier for investigator to consider the ethnicity of their eyewitnesses than to threaten them with a knife. However, experimenters such as Bothwell, Brigham, and Pigott (1987), Maass and Köhnken (1989), and Morgan et al. (2004) have employed creative (and still ethical) procedures to create a potentially more stressful eyewitnessing situation, and their work should encourage other researchers to do the same. Similarly, we are not suggesting that all variability in conditions be removed from empirical manipulations or that better reflecting conditions of the real world will address all concerns regarding the external validity of laboratory research. However, we do feel that in addition to utilizing multiple methodologies, such as laboratory studies, archival research, and studies with actual eyewitnesses, more realistic eyewitness simulations will strengthen our understanding of what factors influence the accuracy of eyewitness identifications and make us less likely to over-generalise empirical results to real world cases. In order to advance the development of theory in the eyewitness memory domain, both research high in experimental realism examining the functional relationships between variables and research high in mundane realism examining the parameters of those relationships are needed. One laboratory study we coded, for example, parametrically examined the effects of distance and illumination on eyewitness accuracy (Wagenaar & van der Schrier, 1996). However, before this research can be used in the courtroom (which the authors of the illumination study thought possible) further research is needed to determine how durations longer than 12 seconds (which was the exposure duration used in the illumination study) or how a live presentation of the target (instead of presenting a portrait,

which was the case for the illumination study) might alter the functional relationship observed between distance, lighting, and identification accuracy.

In the current study we have expanded previous archival research by describing the characteristics of real world cases to determine how well our research paradigms reflect these conditions. However, our review has been limited to mostly describing the effects of only one or two variables on suspect identifications. Clearly, there is not one typical crime situation, and variables do not affect eyewitness accuracy purely as main effects. Future comparisons, as well as experimental investigations, should be aimed at helping us better understand how different variables are related in the real world and how their co-occurrence is related to eyewitness accuracy.

While the goal of much of psychology is to develop universal theories of human behavior, just how context insensitive experiments in the eyewitness memory domain can be before they lose their real world value is an empirical issue. For example, does the videotaped showing of a staged crime to participants capture the essence of what it means to be a real eyewitness? Simulating a criminal event might be less important than trying to capture in the laboratory other features of witnessing that may be equally or more important. We might speculate that features such as high situational ambiguity (e.g., "Seriously, am I really being robbed?"), or the need to decide within seconds how to respond to the situation (e.g., Should I give up my person or property? Should I fight back or run?) are more important in terms of simulating the psychology of eyewitness identification than is staging for our subjects an event that portrays criminal activity being perpetrated against someone else.

Whether we are appropriately simulating witnessing is a reasonable question to ask, for unlike the real world cases, experimenters do not typically ask their participants how closely they paid attention, or screen out people who did not pay attention to or remember the crime (see Yuille & Cutshall, 1986, for an interesting discussion relating to how investigators might screen out witnesses who had a poor vantage point). To illustrate, Kurosawa (1996) used a typical staged crime scenario in a lecture hall setting and found that 33% of the participants could not recall seeing the staged incident. Additionally, 40-50% of the students were not able to say what the intruder did. Similarly, a study presenting a staged theft in a lecture hall setting reported that only 34 out of 147 students indicated they had witnessed the mock crime (Riske, Wallace & Allen, 2000). While the police may not extensively question or ask the witness to make an identification if they did not attend to the crime, in "live" staged crime experiments, the results are rarely conditioned on such factors. Additionally, these results from Kurosawa (1996) and Riske, Wallace, and Allen (2000) suggest that our crime simulations might not be as impactful as real criminal events; to our recollection, we coded only 3 studies (Hollien, Bennett, & Gelfer 1983; Leippe, Wells, & Ostrom, 1978; Hosch & Bothwell, 1990) reporting that subject witnesses to a staged crime attempted to intervene on behalf of the victim. While for obvious ethical reasons we cannot expose our participants to real criminal activity, studies such as these suggest that we should generalise laboratory results more cautiously, and that we should take seriously the issue of whether our simulations allow participants to be representative of real world witnesses. (See Leippe, Wells, and Ostrum (1978) and Malpass and Devine (1980) for their comments on creating crime simulations that are serious enough to involve participants.)

Our work has several limitations that must be addressed. First, one may question the generalisability of our sample of archival cases to other criminal cases on the basis of geographical considerations. While this issue cannot be addressed conclusively at this time, it is not entirely clear why one would expect characteristics such as exposure duration or retention interval lengths to differ in a systematic way in other places or at other times. Nevertheless, we strongly urge other researchers to conduct similar archival analyses in different jurisdictions to further clarify how our simulations could be improved by better reflecting actual eyewitnessing conditions. Second, we sampled from cases referred to the District Attorney for prosecution. The characteristics of cases that do not make it to this stage of the legal system may very well be different. Third, some may be concerned that a number of comparisons are based on coder estimations of time durations. Unfortunately, due to the nature of archival research, objective information of this sort is not always available. However, it is important to note that all coders were extensively trained and exhibited a high level of agreement when making these estimates. Additionally, the estimates significantly correlated with suspect identifications, thereby demonstrating the predictive validity of the time estimates. Fourth, there is also the question of whether missing data from the literature or from the archives may have affected our results in some way. Once again though, it is unclear that this missing information would drastically change the overall pattern of our results. Regarding this issue, we encourage all researchers in the field to provide as much detail as possible in their published reports regarding their simulations, especially for those factors (such as defendant and eyewitness ethnicity, exposure duration, retention interval, suspect lineup position, and how the lineup was constructed), which may affect accuracy either directly or indirectly. Fifth, construct

validity may be compromised in archival research if the people reporting the data are not objective (Wiener et al., 2002). Our observations, for instance, are based on observations made by police officers, probation officers, attorneys, and other legal officials who completed the case files. Sixth, while the power of the statistical tests is obviously sensitive to the size of the sample, the overall pattern of results presented here cannot be explained merely as false alarms. Seventh, unlike laboratory research, we cannot control extraneous influences that may have affected identifications in the archives. Having said that, it is precisely the effects of extraneous influences on eyewitness testimony that inspired us to do the field study in the first place. Extraneous influences abound in the natural world, and therefore, should lead us to exercise caution in generalising laboratory research findings to the legal system. Eighth, we coded only a subset of the literature on face memory, concentrating our efforts on the studies that simulated an eyewitness memory task. As a consequence, the number of studies that have investigated the effects of various phenomena (e.g., source monitoring, subject age, retention interval) on remembering is underestimated in our survey of the literature. However, had we included in our analysis the face memory papers that did not explicitly simulate an eyewitness memory task, the differences between the characteristics of the laboratory studies and the real world identifications in all probability would have been far greater. Finally, we have no conclusive way of determining whether the suspect in the lineup was in fact the perpetrator. All that we could offer were correlations between the strength of other types of evidence in the case and suspect identification rates to allow the reader to estimate identification accuracy.

The view that eyewitness testimony is probative has rested in part on the untested assumption that our simulations are generalisable to actual eyewitnessing situations. The

results of this research project suggest that we need to broaden the range of conditions employed in the laboratory to increase the range of cases to which the laboratory results apply in the legal system. This research project also proposed new avenues for eyewitness identification laboratory research by identifying conditions in the real world cases that we have not yet represented in the laboratory, such as the effects of exposure to the culprit before the onset of the crime and the need for live showup research. Finally, we hope that our study will prompt additional investigations into the ecology of real world eyewitnesses. This type of research has to potential to inform both theory and applied practice.

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Footnotes

[1]Though see Weiner et al. (2002) for a content and methodological analysis of *Law and Human Behavior* article abstracts. Additionally, Small (1993) examined the extent to which articles appearing in *Law and Human Behavior* from 1986 to 1991 proposed theoretical explanations for phenomena.

[2]Recommended guidelines for the collection of eyewitness evidence impact police handling of all cases, regardless of whether the D.A. issues charges. Therefore, any attempt to determine how well laboratory research conditions generalise to real crime situations should also include cases that do not reach the prosecution stage.

[3]In describing the racial backgrounds of research participants, eyewitnesses, and defendants, we adopted the racial nomenclature that legal officials used in the case files.

[4]That the mean duration of exposure to the culprit is longer compared to the mean crime duration is not unusual, as some witnesses viewed the culprit immediately before and after the crime.

[5]Only the characteristics of the first identification test for each eyewitness were included in our analyses. We found that 25 eyewitnesses in the archives (6%) were presented with more than one opportunity to identify the suspect.

[6]We repeated this analysis for acquaintance ID's, and did not find that duration of exposure (p = .43) or length of retention interval (p = .52) affected the results. Additionally, identification outcomes within the acquaintance sample did not differ depending on defendant/eyewitness race, or violence level.

[7]For descriptive purposes, the following was found for the stranger ID cases in which the suspect was positively identified: 41% had physical evidence implicating the suspect

(compared to 48% of lineup rejections), 73% of the suspects had a prior record (compared to 50% of lineup rejections), 20% admitted entirely to committing the crime (compared to 10% of lineup rejection suspects), 50% pled guilty (compared to 50% of lineup rejections), and 30% were arrested at the scene of the crime (24% of suspects who were rejected were arrested at the scene). At least one of these factors was present in 92% of the positively ID-ed suspect cases (compared to 76% of the cases in which the suspect was rejected).

Table 1

Top 10 independent variables investigated by the research studies.

	Independent Variable	Number of Experiments
1	Presence or Absence of Lineup Target	156
2	Simultaneous or Sequential Lineup	43
3	Source Monitoring	42
4.5	Subject Age	41
4.5	Target Characteristics	41
6	Lineup Member Similarity	35
7	Lineup Instructions	34
8	Mnemonic Techniques/Context Reinstatement	28
9	Retention Inverval	26
10	Memory Ability	21

Table note: *Source monitoring* refers to studies that investigated the effects of misleading information (e.g., mugshots, composite drawings, the presence of bystanders) on identification accuracy.

Table 2

Descriptive data for critical event and target exposure durations (in minutes) for the

archives and the research studies by crime category.

	assault	rape	robbery	theft	other crime	none[b]
Critical Event Exposure Duration						
Experiments						
M	0.39	1.25	0.61	1.56	2.21	4.47
SD	0.17	1.06	0.49	0.93	1.44	9.28
range	0.15 to 0.57	0.50 to 2.00	0.06 to 1.50	.06 to 3.17	0.17 to 4.00	0.05 to 40.0
median	0.41	1.25	1.00	1.5	3	1.00
N	8	2	39	75	7	88
Archives						
M	13.93	131.53	11.6			
SD	51.93	611.94	39.91			
range	0.08 to 540	0.50 to 7200	0.25 to 450			
median	10	10	5			
N	156	201	150			
Target Exposure Duration						
	assault	rape	robbery	theft	other crime	none[b]
Experiments						
M	0.39	1.13	0.56	0.79	1.23	4.34
SD	0.18	n/a[a]	0.46	0.64	1.02	9.56
range	0.15 to 0.57	n/a[a]	0.03 to 1.50	0.05 to 3.17	0.17 to 3.00	0.03 to 40.0
median	0.42	n/a[a]	0.43	0.57	1.20	1.00
N	4	1	39	71	6	83
Archives						
M	9.71	133.02	16.43			
SD	30.73	292.54	42.25			
range	0.00 to 330	0.00 to 2423	0.23 to 231			
median	2	15	5			
N	146	179	78			

a. Descriptive statistics could not be computed because *n* = 1. b. Studies in which the to be
remembered stimulus was not portrayed in a criminal situation.

Table 3

Distribution of identification tasks for the witnesses in the archives and participants in the research studies.

Procedure	Real World Identifications (N = 406)	Experimental Identifications (N = 55,678)[a]
Mugshots	<.01	.03
Photo Showup	.00	.05
Live Showup	.51	.02
Photo Lineup	.40	.77
Video Lineup	.00	.09
Live Lineup	.03	.02
Voice Lineup	.00	.01
Other	.06	.00
Unknown	.00	<.01

a. Some research participants viewed mugshots and were then run in another identification procedure.

Table 4

Identification outcomes in the archives by test procedure and eyewitness/suspect level of

acquaintance.

Outcome	Live Lineup $n = 12$	Live Showup $n = 207$	Photo Lineup $n = 164$	Other $n = 23$
Suspect ID	11 (7)	189 (142)	127 (85)	17 (11)
No ID made	1 (0)	8 (8)	22 (19)	2 (1)
Other	0 (0)	10 (6)	15 (9)	4 (0)

Table note: Numbers in parentheses indicate number of known stranger identifications. For instance, there were 11 live lineup suspect IDs, 7 of which were stranger IDs. "Other" identification outcomes include multiple IDs and foil IDs.