

**IN THE SUPREME COURT
OF THE STATE OF CONNECTICUT**

S.C. 19035

STATE OF CONNECTICUT

v.

TROY ARTIS

**BRIEF OF AMICUS CURIAE
AMERICAN PSYCHOLOGICAL ASSOCIATION
IN SUPPORT OF DEFENDANT-APPELLEE
WITH ATTACHED APPENDIX**

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STATEMENT OF THE ISSUES

1. Did the Appellate Court majority properly determine that admission of the victim's out-of-court and in-court identifications following a suggestive police display of the defendant's photograph was a reversible due process violation under *Manson v. Brathwaite*, 432 U.S. 98 (1977)? (pg. 1 to10)

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INTEREST OF AMICUS CURIAE¹

The American Psychological Association, or APA, is the leading association of psychologists in the United States. A non-profit scientific and professional organization, APA has approximately 135,000 members and affiliates, including the vast majority of psychologists holding doctoral degrees from accredited U.S. universities. Among APA's purposes are to increase and disseminate knowledge regarding human behavior; to advance psychology as a science and profession; and to foster the application of psychological learning to important human concerns, thereby promoting health, education, and welfare.

APA has filed more than 145 amicus briefs in state and federal courts nationwide. These briefs have been cited frequently by courts over the years, including the U.S. Supreme Court. *See, e.g., Graham v. Florida*, 130 S. Ct. 2011, 2026 (2010); *Panetti v. Quarterman*, 551 U.S. 930, 962 (2007); *Atkins v. Virginia*, 536 U.S. 304, 316 n.21 (2002). The APA has filed amicus briefs in recent cases specifically involving the reliability and admissibility of eyewitness identifications. *See, e.g., Perry v. New Hampshire*, 132 S. Ct. 716 (2012); *Commonwealth v. Walker*, No. 28 EAP 2011 (Pa.).

¹ No counsel for a party wrote any part of this brief, and no party or counsel other than APA, its members, and its counsel contributed to the cost of the preparation or submission of this brief.

STATEMENT OF FACTS

APA adopts the statement of facts set forth in defendant-appellee Troy Artis's brief.

SUMMARY OF ARGUMENT

Psychological research on eyewitness identifications is reliable. *State v. Guilbert*, 306 Conn. 218, 236-237 (2012). The research is based on established scientific methodologies, operates within strict ethical boundaries, and is subject to vigorous peer review.

Extensive research in this field has yielded what this Court last year correctly labeled a “near perfect scientific consensus” that eyewitness identifications are sometimes mistaken, *Guilbert*, 306 Conn. at 234-235—quite possibly as often as one time in three. There is also a broad consensus that several factors can affect the accuracy of an identification. *See id.* at 237-239. The Appellate Court properly considered several of those factors here.

The State's sweeping attacks on the Appellate Court's ruling (and on psychological research and researchers) are addressed in detail below, but three overarching flaws bear mention at the outset. *First*, the State wrongly portrays the court as having given psychological research conclusive weight. *Second*, the State focuses on the very few voices that either dissent from the “near perfect scientific consensus” that this Court flagged, or criticize psychological research more generally. APA believes those voices are important, as criticism and dissent typically improve scientific research and thus boost human knowledge. But they *are* a very few voices, and they do not undercut the reliability of the psychological research or the consensus regarding the multitude of factors that affect the accuracy of eyewitness identifications. *Third*, the State all but ignores that the scientific consensus is mirrored by a judicial one: Courts throughout the country have acknowledged that the relevant research—and its conclusions regarding specific accuracy factors—are reliable

and properly considered by courts. The State provides no sound basis for this Court to reject its conclusions from last year and depart from this overwhelming judicial consensus.

ARGUMENT

I. *Guilbert* Correctly Held That Eyewitness-Identification Research Is Reliable

Accurate eyewitness identifications are a critical part of the truth-seeking process. But as courts have long recognized, eyewitness identifications can be *inaccurate*. See, e.g., *United States v. Wade*, 388 U.S. 218, 228 (1967) (“The vagaries of eyewitness identification are well-known; the annals of criminal law are rife with instances of mistaken identification.”). Empirical studies both old and new confirm this, documenting many wrongful convictions that rested on incorrect eyewitness identifications. See Garrett, *Convicting the Innocent* 8-9, 48 (2011) (of 250 wrongful convictions, 190 involved mistaken eyewitness identifications); accord Gross et al., *Exonerations in the United States, 1989-2012*, 40 *tbl.13* (2012); Borchard, *Convicting the Innocent* xiii (1932).

Consistent with these findings, field studies have revealed that the rate of inaccurate identifications of suspects may be as high as one in three. For example, an analysis of 1,561 line-up identification attempts found that 20 percent wrongly identified a “filler” and another 41 percent made no identification—meaning that at least 33.9 percent of the actual identifications were wrong (20 percent divided by 59 percent). See Wright & McDaid, *Comparing System and Estimator Variables Using Data from Real Line-ups*, 10 *Applied Cognitive Psychol.* 75, 77 *tbl. 1* (1996).² Controlled experiments involving real-world conditions have produced similar inaccuracy rates. See, e.g., Cutler & Penrod, *Mistaken Identification* 12 (1995) (35.8% inaccuracy rate).

² Accord Behrman & Davey, *Eyewitness Identification in Actual Criminal Cases*, 25 *Law & Hum. Behav.* 475, 482 (2001) (32.4% inaccuracy rate); Wright & Skagerberg, *Postidentification Feedback Affects Real Eyewitnesses*, 18 *Psychol. Sci.* 172, 175 (2007) (26.58%).

Last year, this Court joined others in concluding that the scientific research on eyewitness identifications “‘is ... reliable.’” *Guilbert*, 306 Conn. at 236. This conclusion was based on the fact that “[e]xperimental methods and findings have been tested and retested, subjected to scientific scrutiny through peer-reviewed journals, evaluated through the lens of meta-analyses, and replicated at times in real-world settings,” and the fact that “[c]onsensus exists among the experts ... within the ... research community” on the relevant findings. *Id.* at 237 (omissions in original) (internal quotation marks omitted).

This Court’s conclusions were correct; psychological research on eyewitness identification rests on a highly reliable methodological foundation. *First*, researchers use traditional scientific methods widely recognized to produce robust results. See Malpass et al., *The Need for Expert Psychological Testimony on Eyewitness Identification*, in *Expert Testimony on the Psychology of Eyewitness Identification* 3, 11-14 (Cutler ed. 2009); Wells et al., *Eyewitness Evidence*, 7 Psychol. Sci. in Pub. Int. 45, 49 (2006); accord Schmechel et al., *Beyond the Ken?*, 46 Jurimetrics 177, 179 (2006) (“Eyewitness reliability research uses methods accepted in all sciences.”). *Second*, eyewitness-identification research includes “hundreds of peer reviewed studies.” *Guilbert*, 306 Conn. at 236. The peer-review process provides an additional check on the methodological soundness of the research. See *Cutler & Penrod*, *supra*, at 66-67; Malpass et al., *supra*, at 14. *Third*, strict ethical standards governing the conduct of psychological research further reinforce its robustness. See APA Ethical Principles and Code of Conduct (e.g. Standards 8.10(a), 8.14(a)), available at <http://www.apa.org/ethics/code/index.aspx> (last visited Oct. 28, 2013). *Finally*, surveys of experts confirm the deep consensus as to eyewitness research’s core findings. See Kassin et al., *The “General Acceptance” of Psychological Research on Eyewitness Testimony*, 44

Am. Psychologist 1089, 1090, 1093-1094 (1989); Kassin et al., *On the “General Acceptance” of Eyewitness Testimony Research*, 56 Am. Psychologist 405, 410, 413 tbl. 5 (2001). This consensus is particularly noteworthy given the enormous volume of the relevant research. See Cutler & Penrod, *supra*, at 68.

The State’s challenges to *Guilbert*’s conclusion about reliability lack merit. The State argues, for example (Br. 25-26), that not all researchers adhere to the scientific method. If the State is suggesting that courts should scrutinize science and scientists closely, to ensure that only work of a certain standard receives weight, APA agrees. But the State’s authorities do not justify its implication that most or even many scientists practice shoddy science. Nor do they justify excluding scientific evidence entirely because of the practices of a few. Indeed, the State would surely oppose that approach, as prosecutors routinely rely on such evidence, including expert testimony about fingerprints and blood types.

Perhaps recognizing the far-reaching implications of its attack, the State asserts (Br. 26) that “the social sciences are particularly vulnerable” to “[d]epartures from the scientific method.” But none of its proffered reasons for this supposed vulnerability is valid. For example, social scientists *do* conduct “controlled and replicable experiments.” *Id.* As for both “incentives connected to professional advancement” and “the potential ... to impact law or public policy on controversial high-stake issues,” *id.*, social sciences are not on a unique footing. Many high-profile issues involving “hard” sciences are wrestled over by courts, such as the proper use of DNA (to convict as well as to exonerate the convicted).

The State also argues—relying on two newspaper articles—that “peer review offers no *guarantee* of scientific validity.” Br. 27 (emphasis added). That is true, but irrelevant. Psychological researchers, like other scientists, do not employ peer review because of any

“guarantees.” Rather, they do so because “submission to the scrutiny of the scientific community is a component of ‘good science,’ in part because it increases the likelihood that substantive flaws in methodology will be detected.” *Daubert v. Merrell Dow Pharms. Inc.*, 509 U.S. 579, 593 (1993). The fact that hundreds of studies and meta-analyses regarding the reliability of eyewitness identifications have been vetted through this peer-review process is further testament to the general acceptance and reliability of this field of science.

The State next asserts (Br. 28) that psychological researchers “contrive and overstate consensus.” The two New York trial-court cases it cites do not support that claim. The pertinent portion of the first case, *People v. Smith*, 2 Misc. 3d 1007(A), 784 N.Y.S.2d 923 (Sup. Ct. 2004), rested on another decision from the same court, *People v. LeGrand*, 747 N.Y.S.2d 733 (Sup. Ct. 2002). But New York’s highest court unanimously reversed *LeGrand* on precisely the consensus point the State now argues. *See People v. LeGrand*, 867 N.E.2d 374, 376, 380 (N.Y. 2007). In *People v. Banks*, 842 N.Y.S.2d 313 (Co. Ct. 2007), meanwhile, the judge did not find a lack of consensus as to any factor, but simply held that the defendant had not met his burden to prove consensus. *See id.* at 321-325.

APA’s point is not that there is perfect consensus in this area, or that the consensus has never been overstated. But the State is wrong to suggest that pervasive exaggerations exist, that there is great dissension among researchers, and hence that the relevant research should be ignored. As *Guilbert* recognized, the reality is that there is substantial, sometimes overwhelming, consensus regarding the findings noted by the Appellate Court.³

³ Evidently recognizing the problems that *Guilbert* poses for its position, the State asserts (Br. 24 n.12) that “*Guilbert*’s holding must be provisional, in that any purportedly scientific conclusion must ... be open to revisiting.” It is true that this Court can “revisit[],” i.e., overrule, any prior decision. But until that occurs (which the State does not request as to *Guilbert*), those decisions, including *Guilbert*, have full binding effect, and are not “provisional.”

II. Psychological Research Has Identified Several Factors That Can Affect The Accuracy Of An Eyewitness Identification

As this Court explained in *Guilbert*, the psychological research on eyewitness identifications has shown that several factors affect the accuracy of such identifications. These include factors that the Appellate Court looked to here, such as exposure duration, passage of time, intoxication, and witness stress.⁴ The research has also shown that the weak link between a witness's confidence in his identification and its accuracy becomes even weaker where (as here) the identification is tainted by a suggestive procedure.

As with reliability, the State's attacks regarding these factors are meritless. For example, the State (Br. 38-39) resists the common-sense conclusion that intoxication affects eyewitness accuracy, citing a single article for support. That article, however, was published in a flagship APA journal, undermining the State's claim that psychologists resist dissenting views. That aside, the study tested only the memory of intoxicated witnesses about aspects of a staged crime they had witnessed—*not* their ability to actually identify the "perpetrator" of the crime based on lineups or photo arrays. See Schreiber Compo et al., *Intoxicated Eyewitnesses*, 36 Law & Hum. Behav. 77, 79-81 (2012). In any event, at most the study shows that although the vast majority of the relevant research supports the Appellate Court's analysis on this point, there is not total unanimity. That would not undermine the court's decision to credit the consensus view, just as this Court did in *Guilbert*.

The State also strongly disputes the notion that, in general, eyewitness confidence does not correlate with accuracy. The relevant finding here, however, does not concern what is true in general. The relevant finding, rather, is that confidence is a poor predictor of

⁴ The State attacks the lower court for "unilaterally add[ing] stress as a negative factor." Br. 14. But both the U.S. Supreme Court and this Court have embraced the finding that stress affects accuracy. See *Perry*, 132 S. Ct. at 727; *Guilbert*, 306 Conn. at 237-238, 253.

accuracy *when the identification procedure was suggestive*—a finding confirmed by one of the State’s own authorities. See Lindsay et al., *Accuracy and Confidence in Person Identification*, 9 Psychol. Sci. 215, 218 (1998), *cited in* State Br. 32 (“[S]uggestive interviewing procedures before or after an identification is made[] can *dramatically weaken* the accuracy-confidence relationship.” (emphasis added)); accord Wells & Quinlivan, *Suggestive Eyewitness Identification Procedures and the Supreme Court’s Reliability Test in Light of Eyewitness Science*, 33 Law & Hum. Behav. 1, 12 (2009).⁵ That is critical because courts consider witness confidence only at the reliability stage of the due process analysis—i.e., only *after* concluding that the identification procedure was suggestive. See *Perry*, 132 S. Ct. at 726. In other words, even if a sufficiently robust confidence-accuracy correlation existed in general, there are strong reasons to doubt that it exists as to identifications that, as in this case, were tainted by a suggestive identification procedure.

III. The State’s Remaining Attacks On Eyewitness Research Lack Merit

The rest of the State’s arguments regarding eyewitness identification fare no better than those already discussed. As an initial matter, the State repeatedly mischaracterizes how the Appellate Court here treated psychological and other social-science research. It contends, for example, that the court “elevated ... social science findings ... to conclusive and binding authority for trial courts.” Br. 12; see *also* Br. 24, 43. The State also makes vague assertions that with regard to social science, the court: imposed “bright line rules” (Br. 33, 39), a “blanket rule” (Br. 43), or “blanket presumptions” (Br. 38); made “categorical

⁵ The other authorities cited by the State (Br. 32-33) did not study identifications that involved the type of highly suggestive procedures at issue here. See Sporer et al., *Choosing, Confidence, and Accuracy: A Meta-Analysis of the Confidence-Accuracy Relation in Eyewitness Identification Studies*, 118 Psychol. Bull. 315, 315-316, 318-319 (1995); Brewer & Wells, *The Confidence-Accuracy Relationship in Eyewitness Identification*, 12 J. Experimental Psychol. Applied 11, 16 (2006).

claims” (*id.*); and gave certain reliability factors “assigned, fixed weights” (Br. 23). See also Br. 31 (“the majority’s doctrinaire conclusions”). That is all wrong. The court certainly *cited* psychological research in conducting its analysis, just as this Court and others have done. See, e.g., *Perry*, 132 S. Ct. at 727; *Guilbert*, 306 Conn. at 235 n.9. And of course the State does so in its own brief. But such citation is not the imposition of any rules, presumptions, or fixed weights, nor amounts to treating the research as “conclusive and binding authority.” Br. 12. The State’s mischaracterizations should not affect this Court’s review.⁶

The State’s attacks on eyewitness-identification research itself are equally infirm. The State contends, for example (Br. 28-31), that researchers in this area are “partisan.” What is meant by that term is not explained. But if it means that researchers are pro-defendant, that position is devoid of factual support and, in fact, quite mistaken. The research, as it relates to possible changes in the criminal law, is aimed simply at increasing the prevalence of accurate identifications. That goal is not pro-defendant; surely the State, and law enforcement generally, also want identifications to be as accurate as possible. See, e.g., National Institute of Justice, *Eyewitness Evidence* 1-2 (1999) (relying on social-science research to develop recommended practices).⁷

Another “partisanship” complaint raised by the State (Br. 30-31) is selective publication. As an initial matter, the specific examples the State discusses in making this argu-

⁶ The State similarly mischaracterizes the Appellate Court’s treatment of one of the reliability factors—confidence. The State repeatedly suggests that the court “removed the certainty factor” from the analysis, thereby “altering the due process test contrary to binding law.” Br. 23; accord Br. 15, 23, 43. The court, however, actually stated that “little weight should be accorded to Otero’s level of certainty.” 136 Conn. App. 568, 604 (2012). It did not say “no weight,” and it certainly did not say it was refusing to consider the factor at all. Instead, it simply applied the confidence factor to the facts of the case before it.

⁷ Illustrating how unbounded its attacks are, the State in a single paragraph attacks both researchers whose experiments “failed to isolate” one relevant factor from others (Br. 40), and those whose experiments did so, see *id.* (“Isolation of factors can distort results.”).

ment, namely blind-lineup administrators and sequential photo displays, have nothing to do with this case, and thus played no role in the Appellate Court's decision. More generally, the various studies and other publications that the State cites in support of its own arguments belie its claim of a widespread selective-publication phenomenon.

The State next argues that psychological researchers have been forced to “retreat[]” (Br. 31, 32) from supposedly categorical or “headline” (Br. 33) claims. The only such claim the State points to, however (see Br. 32), was made by a law professor, not a scientist. And while the professor cited a work by a leading researcher in the field, that work actually contradicted the professor's claim. Specifically, whereas the professor stated flatly that “‘psychologists agree that eyewitness identification of strangers is unreliable,’” State Br. 32 (quoting the law review article), the cited authority stated, to the contrary: “How reliable are eyewitnesses? Obviously this is a complex question that can only be answered by reviewing the major variables that have been shown to affect eyewitness accuracy.” Wells, *The Eyewitness*, in *The Psychology of Evidence and Trial Procedure* (Kassin & Wrightsman eds. 1985), at 43. This entire line of the State's argument, then, attacks a strawman.

This is not to say, of course, that eyewitness-identification researchers always get it right the first time, and thus never have to revisit past views or findings. But the State's suggestion that researchers in this field carelessly rushed to make numerous claims that later had to be retracted (let alone that such early claims influenced courts) is simply false.

Ultimately, the State's assault on the integrity of eyewitness researchers rests on selected portions of an article by Steven Clark. See State Br. 28, 30, 31. Far from supporting the State's sweeping claims, Clark's criticism was quite narrow: It was limited to recommendations regarding which police-identification procedures are optimal—an issue

unrelated to the overall reliability of eyewitness research, and that is not implicated here. See Clark, *Costs and Benefits of Eyewitness Identification Reform*, 7 *Perspective on Psych Sci.* 238, 238-239 (2012). Moreover, the debate that followed this article contradicts the State's charges of structural bias. Its very existence proves that researchers scrutinize their peers' work, and that published studies report adequate data to permit re-analysis.⁸

Lastly, the State argues (Br. 34-37) that eyewitness experiments fail to simulate real-world circumstances—in part because creating crime situations in an experimental setting is difficult and in part because college students, the usual subjects in controlled experiments, are poor proxies for real witnesses. The former argument fails because the memory process does not work differently in a controlled setting than elsewhere. See Malpass et al., *supra*, at 13. Moreover, experimental research has the advantage of allowing control for external factors; that alone makes its findings more reliable than the alternative.

Studies involving college students, meanwhile, have yielded results similar to those of studies that used “real-world” subjects. See Appellee's Br. 39 (citing studies). Moreover, using college students would, if anything, *overestimate* the overall accuracy of eyewitness identifications, because “college students are among the best eyewitnesses in light of their general health, visual acuity, recall, and alertness.” *State v. Henderson*, 27 A.3d 872, 894 (N.J. 2011) (citing expert testimony); see also Wells & Quinlivan, *supra*, at 6. Any such bias would undermine—not support—the State's position here.

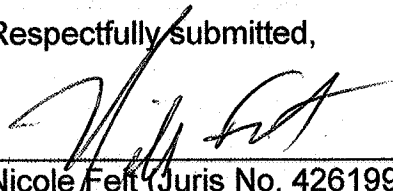
CONCLUSION

The judgment of the Appellate Court should be affirmed.

⁸ The State also makes unsupported, gratuitous comments about social science and its practitioners. See Br. 24 (“experts touting popular eyewitness science”), 25 (“fashionable social science claims”), 27 (“trendy ... social psychology findings”). Such derogatory remarks do not help this Court resolve the issues before it, and are unworthy of the State.

Dated: October 28, 2013

Respectfully submitted,



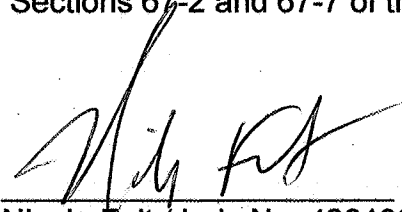
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CERTIFICATE OF COMPLIANCE

On this 28th day of October, 2013, I, Nicole Feit, certify that the foregoing amicus brief complies with the requirements set forth in Sections 67-2 and 67-7 of the Connecticut Rules of Appellate Procedure.



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APPENDIX

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2 Misc.3d 1007(A)

(Table, Text in WESTLAW), Unreported Disposition**(Cite as: 2 Misc.3d 1007(A), 784 N.Y.S.2d 923)****H**

People v Smith

2 Misc.3d 1007(A), 784 N.Y.S.2d 923

N.Y.Sup.,2004.

2 Misc.3d 1007(A), 784 N.Y.S.2d 923, 2004 WL 690321, 2004 N.Y. Slip Op. 50172(U)

***1** This opinion is uncorrected and will not be published in the printed Official Reports.

The PEOPLE of the State of New York, Plaintiff,

v.

Champagne SMITH, Defendant.

Indictment No. 7063/01.

Supreme Court, New York County.

March 26, 2004.

CITE TITLE AS: People v Smith

ABSTRACT

Crimes

Witnesses

Expert Witness

People v Smith (Champagne), 2004 NY Slip Op 50172(U). Crimes—Witnesses—Expert Witness. (Sup Ct, NY County, Mar. 26, 2004, Bradley, J.)

APPEARANCES OF COUNSEL

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Neighborhood Defender Service of Harlem, Of Counsel, Rick Jones, Esq. and Sean Maher, Esq., New York, for the defendant.

OPINION OF THE COURT

JOHN A.K. BRADLEY, J.

The defendant Champagne Smith moves to admit expert witness testimony concerning eyewitness testimony and identification. This court conducted a Frye (*Frye v. United States*, 293 F. 1013, (DC Cir., 1923)), hearing as to the admissibility of this testimony. Steven Penrod, Ph. D. testified on behalf of the defense and Ebbe Ebbesen, Ph. D. testified on behalf of the People. Both witnesses were erudite and made impressive and thorough presentations.

FACTUAL BACKGROUND

On November 17, 2001, the victim, 19 year old Nazon Powell, was walking with a friend on Lenox Avenue in Manhattan at 2:30 AM. A man and a woman were crossing West 138th Street northbound on the West side of Lenox Avenue, and defendant was crossing the same street in the opposite direction. Defendant allegedly approached the victim from behind several feet from the corner and shot him in the head, killing him instantly.

After being given the name of the defendant from friends and relatives of the victim, the Police spoke with three eyewitnesses to the shooting—the friend accompanying the victim, and the man and woman crossing the street contemporaneously with the shooter. The witnesses described the defendant as a black man, approximately six feet tall, with a thick nose and corn row braids, wearing a black, three quarter length pea coat style leather jacket, a red shirt, black or blue jeans and black Timberland boots. The witnesses observed the shooter walking with two other men, under *2 streetlights with a gun in his hand. They saw the shooter approach the victim and either saw or heard him shoot the victim and then flee.

Ten days after the incident, the male eyewitness picked defendant's photograph out of a six photo array. Defendant was arrested, admitted to having

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braids at the time of the incident, and admitted being in the vicinity of the shooting at the time of the shooting while wearing a black leather jacket, a red shirt, black sweat pants, and black Timberland boots. Three lineup procedures were conducted with the three eyewitnesses. The witnesses were told that the perpetrator may or may not be in the lineup (thus the witnesses recognized that one of their options was to simply state that the person that they had seen was not present in the lineup). Each of the eyewitnesses selected the defendant in the lineup procedures.

The defendant was indicted on a variety of charges, including Murder in the Second Degree. Various suppression motions were made and denied. Defendant made a motion to be permitted to present testimony at trial of an expert in eyewitness identification. Defendant initially advised that such expert would testify in various areas: (1) the effect of weapon focus on identification; (2) effect of stress on identification; (3) the suggestiveness of photo array and lineup; (4) the occurrence of post trauma amnesia in victims; (5) relation back of subsequent identification to the initial identification; (6) lack of correlation between confidence and accuracy in eyewitness identification; (7) the effect of post event information on identification; (8) effect of exposure duration on identification; (9) effect of color perception on identification; (10) double blind lineups; (11) cross racial identifications; and (12) psychological factors affecting perception and memory. After hearing argument from the parties, this Court denied the motion as to many of the proffered areas of expertise and ordered a Frye hearing as to the remaining six: weapon focus, stress, post event information, unconscious transference, confidence and accuracy non correlation, and confidence malleability. Defendant has withdrawn the motion as to stress.

Thus, the motion is presented for decision as to the following factors:

Weapon Focus: the focus or attention that an eyewitness gives to a perpetrator's weapon during the

course of a crime. See Steblay, Nancy Mehrkens, "A Meta-Analytic Review of the Weapon Focus Effect", *Law and Human Behavior*, vol 16, no 4 at 413 (1992) ("Steblay").

Post Event Information: the notion that eyewitnesses recollection of an event they witnessed is influenced by information obtained after the event. Kassir, Saul M., Tubb, V. Anne, Hosch, Harmon M., & Memon, Amina, "On the 'General Acceptance' of Eyewitness Testimony Research: A New Survey of the Experts", *American Psychologist*, vol. 56, no.5 at 8 [2001] ("Kassir")

Unconscious Transference: Somewhat tellingly, this Court struggles even to define this proposed area of expert testimony because the experts themselves appear similarly to be uncertain. Penrod testified that it is "hard to ... say exactly what the effect[s] size is or the conditions under which the effect is most likely to occur." Penrod Direct at 158-159. According to Penrod, unconscious transference refers to "the notion that people can misidentify, or they can, if you will, swap the context in which they have seen people, get confused about the context in which they have seen people and misidentify them as coming from the wrong situation or context." Penrod Direct at 155. According to Penrod, this can happen either before, after or during the event. Penrod Cross at *3 779. However, in *U.S. v. Haynes*, 172 F. 3d 54 (7th Cir.1999), Dr. Gary Wells testified that unconscious transference refers to confusing the people the witness saw at the crime scene because of the confusing events there, somehow swapping a bystander for the perpetrator. See also, Read, J.D., Tollestrup, P., Hammersley, R., McFadzen, E. and Christensen, A., "The Unconscious Transference Effect: Are Innocent Bystanders ever Misidentified?", *Applied Cognitive Psychology*, vol. 4, 3-31 (1990) (refers to an eyewitness's misidentification of an innocent bystander because of the witnesses exposure to the bystander in another context).

Confidence and Accuracy Correlation: the relationship between the accuracy of an eyewitness identification and the confidence the witness expresses in such identification. Wells, Gary L., Small, Mark,

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Penrod, Steven, Malpass, Roy S., Fulero, Solomon M. & Brimacombe, C.A.E., "Eyewitness Identification Procedures: Recommendations for Lineups and Photospreads," Law and Human Behavior, vol. 22, no. 6 [1-39] at 14. [1998].

Confidence Malleability: the notion that an eyewitness's confidence can be influenced by factors that are unrelated to identification accuracy. Kassir, *supra*.

LEGAL FRAMEWORK

In *People v. Lee*, 96 N.Y. 2d 157, 160 (2001) the Court of Appeals held that expert testimony about eyewitness identification does not per se invade the province of the jury and is thus not "inadmissible per se" but "the decision whether to admit it rests in the sound discretion of the trial court".

Dependent on the jurisdiction, there are two distinct legal approaches in place today for assessing the admissibility of expert testimony. Traditionally the courts had applied the so called *Frye* test, derived from *Frye v. United States*, 293 F. 1013 (DC Cir., 1923). In *Daubert v. Merrell Dow Pharmaceuticals*, 509 U.S. 579 (1993), the United States Supreme Court, in a case arising under the Federal Rules of Evidence, suggested a methodology which was subtly but profoundly different from *Frye*. New York has adhered to the *Frye* test. See *People v. Wesley*, 83 N.Y. 417, *Lee*, *supra*, *People v. Taylor*, 75 N.Y. 2d 277; *People v. Middleton*, 54 N.Y. 2d 42 .

Frye sets forth a four prong test for admissibility of scientific expert evidence. First, is the witness competent in the field of expertise that he purports to address at trial. The expert should possess the requisite skill, training, education, knowledge, or experience from which it can be assumed that the information imparted or the opinion rendered is reliable. *People v. Legrand*, 196 Misc.2d 179 (Sup.Ct., N.Y.Co., 2002, Fried, J.); *Mattot v. Ward*, 48 N.Y. 2d 455.

Second, the expert testimony must be based on a scientific principle or procedure which has been sufficiently established to have gained general acceptance in the particular field in which it belongs. *Legrand*, *supra*; *People v. Wernick*, 89 N.Y. 2d 111.

In other words, the trial court must determine whether there is general consensus among scientists in the relevant community that the proposed testimony is reliable. Here, in what may appear at first blush to favor admissibility but which in fact is illustrative of a large apparent flaw in the proffered methodology, the court is not considering the discipline of eyewitness identification in its entirety, but rather only the individual factors mentioned above.

*4 The burden is on the party tendering the testimony to lay the proper foundation that the processes meet this test, by offering judicial opinions, scientific or legal writings, or expert opinion other than that of the proffered expert. See *Cameron v. Knapp*, 137 Misc. 2d 373 (Sup.Ct., N.Y.Co., 1987).

The third requirement is that the proffered testimony be beyond the ken of the jury. *Legrand*, *supra*. The court must determine when jurors are able to draw conclusions from the evidence based on their day to day experiences, their common observation and knowledge, and when they would be benefitted by the specialized knowledge of an expert witness. This prong of the test has been resolved for purposes of the issues here by the Court of Appeals in *Lee*, *supra*, which stated that although "jurors may be familiar from their own experience with factors relevant to the reliability of eyewitness observations and identification, it cannot be said that psychological studies regarding the accuracy of an identification are within the ken of the typical juror." *Lee* at 162.

The fourth prong is that the expert's opinion be relevant to the issues and facts of the individual case. *Legrand*, *supra*. Here, there can be little question that eyewitness testimony is the centerpiece of the People's case, so that relevance is a foregone con-

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clusion.

So too can there be no question that the proffered experts, Ebbesen and Penrod, are highly qualified, skilled and articulate in this area. Dr. Ebbesen is a Full Professor of Psychology at the University of California, San Diego. He holds many academic honors and has received many research grants. He is a member of many important professional organizations and reviews for many prestigious journals and agencies. He has authored numerous publications and technical reports. Indeed, he was held to be qualified in these areas in the *Legrand* case. Dr. Penrod is a Distinguished Professor of Psychology at the John Jay College of Criminal Justice, City University of New York. He also holds academic honors and awards in the field of psychology and has received many research and program grants. He has authored innumerable publications and studies in different areas of psychology and is on several major departmental and university committees. Dr. Penrod is also a member of many prestigious professional organizations as well as on the editorial board of many psychological publications.

This leaves the single issue of whether the proffered expert testimony is considered generally reliable in the relevant scientific community.

DISCUSSION

The defense places great emphasis on the Kassin survey. However, as pointed out by the People and its expert, Dr. Ebbesen, and as found by Justice Fried in *Legrand*, this survey represented only a “‘very small sample of the thousands of researchers who do research on human memory’, thereby excluding a ‘highly published minority’ of researchers in the eyewitness identification discipline from participating in the survey. Even the defense experts, such as Penrod and Malpass, disagree as to the size and makeup of the relevant community. Justice Fried, in *Legrand*, went on to point out that there is a dispute as to what actually makes up the relevant psychological *5 community. *Legrand* at 191-92. It

also appears that potential significant respondents such as Ebbesen, Eliot, Egeth, and Yuille were not invited to respond to the survey, which would cast doubt on its objectivity (given the predilection of these particular people to be skeptical of the validity of this approach).

The Court also notes that the Kassin authors disclose that their survey was mailed to a 197 possible respondents, and only 64 responded, a 34.4% response rate.

The shortcomings of the Kassin survey are of particular note, for example, with respect to the confidence accuracy correlation. This particular factor would appear to be a prime candidate for appropriate expert testimony, because of its counterintuitive nature. In other words, the Court may presume that most lay jurors would consider eyewitness testimony from the perspective that confidence expressed by the witness in the identification had a high correlation to accuracy. Therefore, expert testimony to the effect that in fact the confidence accuracy correlation was low, would be extremely important. However, focusing on the confidence/accuracy correlation as presented in the Kassin survey, while 86.7% of the mere 34.4% of actual respondents agreed that confidence was not a good predictor of accuracy, of these 61 respondents, only 24 found this concept to be highly reliable, and only 16 found it to be generally reliable, with the remaining 21 respondents finding it to be less than generally reliable. Moreover, of the 86.7% who supported the confidence/accuracy non correlation, significantly fewer said they would so testify. Thus, as noted by Justice Fried in *Legrand*, the Kassin survey does not really support a finding that the confidence/accuracy non correlation is generally accepted.

Moreover, at least one experimental study (and one of the few that actually attempted to introduce several variables rather than isolating them out) has reached the precisely opposite, and intuitive conclusion. See Accuracy and Confidence in Person Identification: The Relationship is Strong When

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Witnessing Conditions Vary Widely. Lindsay, D. Stephen, Read, J. Don, and Sharma, Kusum. *Psychological Science*, vol. 9, no.3 (May 1998)(215-218).

This Court also finds significant that, unlike other recently propounded expert testimony concerning witness testimony and behavior, such as rape trauma syndrome, which are based on methodologies which study actual rape victims, with few exceptions the scientific analysis in the area of eyewitness identification is based on independent scientific experiments. The results of these experiments and analyses are then proffered as being indicative of and directly applicable to the efficacy of actual eyewitnesses at crime scenes, which are complete with an intense emotional background. There remains deep seated disagreement as to whether it is generally acceptable for experts to extrapolate their research findings from laboratory studies to the intense real life crime scenes which produce our in court eyewitnesses. See *Legrand*, supra, at 202, 203 and the articles cited therein. Compare Leippe, Michael R., "The case for Expert Testimony About Eyewitness Memory", *Psychology, Public Policy and Law*, vol. 1, no. 4 (909-959)(1995) at 919 ("Leippe").

As Justice Fried discussed in *Legrand*, experimental psychological studies utilize various techniques such as slide sequences, filmed events, and staged criminal events to replicate real life crime scenes. However, it has been noted that using these mechanisms does not qualify as a "forensically relevant paradigm" and may be of limited value for generalizing to real world, real criminal episodes, and life events. Yuille, J., Davies, Graham, Gibling, Felicity, Marxsen, D. and *6 Porter, S. Eyewitness Memory of Police Trainees For Realistic Role Plays. *Journal of Applied Psychology*, vol.79., no. 6, (931-936) at 932 [1994]. *Legrand* at 204,205 and articles cited therein. Even the showing of upsetting video tape or slides are obviously contrived situations inducing low stress, and a far cry from the types of stress encountered in an actual criminal in-

cident. McKenna, Judith, Treadway, Molly, McCloskey, Michael E., *Expert Psychological Testimony on Eyewitness Reliability: Selling Psychology Before Its Time*, *Psychology and Social Policy* (283-293) (1992) at 286.

Some of the experimental studies attempt to duplicate the trial model by utilizing mock juries. There are serious issues as to whether these studies reflect the tasks set before an actual trial jury. The experiments reflected in these studies generally do not involve live witnesses or follow up questions. There are no jury charges. The witnesses usually view a videotape and then pretend to be witnesses to the event they saw. See *Penrod Cross* at 500-506. The Court is uneasy about analogizing in this area when the defense expert acknowledged that "little is known about the empirical effect of trial simulation versus actual trials". *Penrod Cross* at 516-517.

Of course, the psychological community has been hamstrung to some extent by the ethical restraints in subjecting participants to levels of arousal as extreme as those present when a person is present at the occurrence of violent crime. (MacLin, Otto H., MacLin, Kimberly M. & Malpass, Roy S., "Race, Arousal, Attention, Exposure, And Delay: An Examination of Factors Moderating Face Recognition", *Psychology, Public Policy, and Law*, vol. 7, no. 1 at 149 (2001).

Those few studies which have actually been based on the analysis of actual criminal cases only heighten this Court's concern whether such analysis has reached the level to permit expert testimony on the issues proffered. See *Eyewitness Identification in Actual Criminal Cases: An Archival Analysis*, by Behrman, Bruce and Davey, Sherrie L., *Law and Human Behavior*, vol. 25. No.5. (2001); Tollestrup, Patricia, Turtle, John W. and Yuille, John C. *Actual Victims and Witnesses to Robbery and Fraud: An archival analysis*. (1994). These studies are based on archival analysis of actual criminal cases, comparing the eyewitness identifications to the strength of other extrinsic evidence in those cases. Aside from the fact that these studies are not yet suffi-

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ciently ubiquitous (as opposed to the numerous experimental analyses) to independently support any conclusion on the proffered evidence, they have raised issues as to the viability of the experimental findings. For example, Behrman and Davey concluded that the weapon focus effect was not significant, perhaps because anxiety levels in criminal incidents are already so high that the presence of a weapon does not create an additional distraction. They also concluded that the confidence accuracy correlation may be high.

Related to these concerns, and even more significant, the experimental methodologies are appropriately premised upon isolation to the extent possible of the particular factor being studied, so as to be certain that results reflect the impact of that factor alone, whether it be confidence/accuracy, confidence malleability, post incident influence, etc. Maass, Ann, *Logic and Methodology of Experimental Research in Eyewitness Psychology* (279-293) at 290 (experimental research seeks to reduce the number of variables to manageable size, excluding other variable.). See also Lindsay, *supra.*, at 215 (“[] researchers have used their methodological expertise to minimize variability in ability to identify the target by holding procedures constant across participants.”). It is this problem, which affects the admissibility of expert testimony as to eyewitness testimony *7 generally, which most troubles this Court. As noted by Justice Fried, while this rigorous scientific approach has produced greater control and internal validity, it “has caused experimental psychology in eyewitness identification to become generally associated with a loss of external validity and generalizability.” *Legrand* at 202.

Common sense suggests that a myriad of factors may interact to affect the validity of actual crime scene identification. The confidence/accuracy correlation may or may not vary dependent on the age, race, or visual acuity of the observer, the level of stress, the presence of a weapon, the level of cognitive input at the point of observation, (e.g., loud

noises, strong smells, related distractions such as approaching vehicles, etc.), the historical observation skills and memory of the observer, and the duration of observation. Similar concerns affect confidence malleability, weapon focus, post event influence on testimony, and even unconscious transference. Yet eyewitness memory research fails to examine the extent that the relationship of any given variable or hypothesis will be affected by such other variables and their interaction at a given crime scene. Parametric research design, a methodology which would study and account for the interaction of such variables, is not used in the field of eyewitness reliability. See *Ebbesen Direct* at 43-60 and 126. This problem is, of course, highly relevant not only to the proffered testimony concerning the confidence/accuracy correlation, but to the other proffered areas as well. For example, in *Stebly's* meta-analysis (a meta analysis pools numerous studies on a particular variable to determine if there is an average effect of that variable) of weapon focus, she stated: “A desirable goal for future research is to clarify the interactive effect of arousal and attention in the weapon focus phenomenon. A variable related to both arousal and attention, crime scene complexity, could not be assessed with this data. It may be argued that real-life crime events include so many stimuli that the weapon focus effect becomes irrelevant or insignificant in magnitude.” *Nancy Steblay, supra.* (1992) at 422.

Indeed, as the authors of *A Critique of Theory and Method in Social Psychological Approaches to Legal Issues* (Konecni, Vladimir J. and Ebbesen, Ebbe B.), point out, the conflict is between the view that the real world is “ ‘additive’, that is, that factors occurring in it have only main effects and do not interact with each other. However, a more plausible view of the world is that it is highly ‘interactive’ ”. *Id.* at 489. The problem with each of the proffered aspects of expert testimony here is that they are studied and predicated under the ‘additive’ view.

An additional problem with the experimental mod-

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els is the presence in many of the forced choice model, where a subject must identify an individual. This differs from the real world where every person present at a criminal episode either can or cannot identify the perpetrator, and for those who can, they may or may not do so. In the forced choice experiments, those who because of inattention or lack of proper visual opportunity would have chosen in the real world not to have identified the perpetrator, must do so in the laboratory. See *Legrand* at 194,195.

Further, in the laboratory, witnesses are not worried about having to have their identifications withstand police skepticism and legal cross examination. One researcher has stated that real witnesses apply stricter standards to their own identifications and are less susceptible to bias *8 instructions, and that those who are aware that they are in a mere experiment have significantly higher false identifications. See Maass, A. Logic and Methodology of Experimental Research in Eyewitness Psychology, *Psychological Issues in Eyewitness Identification*, (279-93), at 291 (1996).

Another issued raised by the testimony at the hearing herein is that the factors measured in the experimental research differ from real world identification in a significant way. *Ebbesen Direct* at 31-36. A common method for measuring the accuracy of identification in the experimental models is by measuring the number of details or facts that are remembered by the subject vis a vis the total of such details available for observation, a fact controlled by the researcher. *Penrod Cross* at 931-933; *Ebbesen Direct* at 38-42.

In a real criminal episode, the number of material factors affecting identification is of course unknown, and the witness may remember any number of them, the important conclusion being that he or she can identify the alleged perpetrator, however many or few the remembered cognitive clues. The key, however, in the experiments is not what the subject remembers, but what the researcher believes he or she should remember (*Penrod Cross* at

840-945). This is particularly evident in the area of post event information. It is possible that the key fact behind the subject's identification might be an observation the researcher does not test for.

Further, in the experimental field, an error in identification is calculated as an error. In the real world, if the witness picks the wrong person out of a lineup, the error is not material as the police know the person selected is innocent. The potential witness is dismissed and no conviction is sought or obtained based on the false identification. It is only those who select the person in the lineup who is indeed a suspect who actually become witnesses. This illustrates the danger in extrapolating experimental error rates to the real world. See Wells, Gary L., What Do We Know About Eyewitness Identification. 48 *American Psychologist* 553-71 (1993). *Penrod* has acknowledged this discrepancy. See *Penrod Cross* 378-80. However, much of the historical data offered to this court is of course based on this flawed premise.

Moreover, much of the research directed to lineup procedures uses either (usually) target present or (rarely) target absent data, but not both. In the real world, although the police insert the suspect, the lineup in an absolute sense may be either. By not examining the relationship in experimental research between target present and target absent lineups (which probably need to include someone who closely resembles the purported perpetrator), the research again fails to seek truly to measure real world identification procedures. See *Penrod Cross* at 378-81; 396-397. Further, because it is unknown in the real world how many lineups are target absent and how many are target present, even those research experiments that use both techniques do not, because they cannot, mimic real world conditions by adjusting the ratio properly. *Ebbesen Direct* at 71-79. Finally experimental methodology cannot, in assigning quantitative value to confidence levels, mimic the real world where confidence is not quantitative but rather expressed verbally, emotionally and physically.

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In addition to the serious and troubling issues discussed above, there are other difficulties with permitting the individual aspects of expert testimony proffered.

CONFIDENCE/ACCURACY

***9** While the defense and its experts posit that at least at the experimental level the low correlation between confidence and accuracy is well established, this is far from clear, and consensus here is highly doubtful. As Justice Fried pointed out in *LeGrand*:

‘Deffenbacher reviewed 25 studies that collectively involved 43 assessments of the confidence/ accuracy relationship. In 22 of the assessments a positive correlation between confidence and accuracy was found, whereas in the remaining 21 assessments either no correlation or a slight negative correlation was obtained.’ *LeGrand* at 190, 191, quoting McKenna, J., *supra*, et al at 288 [1992].

It appears further to be the case that the level of correlation between confidence and accuracy varies dependent on the standard of measurement used in a particular study (there being no apparent consensus as to a standard of measurement). See Ebbesen, E., *supra*, 2000. Therefore, the strength of the relationship between confidence and accuracy depends upon the method of measurement chosen by the individual psychologist.

Additionally, focusing again on the Kassin survey, *supra*, aside from the fact that its own response rate was unimpressive, the survey was mailed to apparently a small sampling of the thousands of researchers who do research on human memory. Thus, an issue exists as to whether the chosen scientific community properly reflects in fact the proper scientific community.

There is also an issue as to the inclusion of both choosers and non choosers in experimental contexts, because real world identifications deal, of course, only with witnesses who actually purport to

identify a perpetrator. Some experimenters conclude that the confidence accuracy correlation is higher for choosers; others think this factor is irrelevant. See *Legrand*, *supra*. at 207, 208. Nevertheless, non choosers continue to be included in experiments on confidence accuracy correlation even though Penrod acknowledges this is problematic. Penrod Cross at 594-95, 640-46.

Finally, there appears to be a disparity between the confidence accuracy results achieved by measuring the correlation coefficient or the calibration curve, the latter finding a higher correlation. See Brewer, Neil, Keast, Amber, Rishworth, Amanda, The Confidence-Accuracy Relationship in Eyewitness Identification: The Effects of Reflection and Disconfirmation on Correlation and Calibration, 8 Journal of Experimental Psychology, no. 1 (44-56)(2002).

Thus, for all of the reasons reflected above, the Court concludes that the proffered evidence on the confidence accuracy correlation should not be admitted.

CONFIDENCE MALLEABILITY AND POST EVENT INFORMATION

These two areas of proposed expert testimony are interconnected. Post event information means information received by the witness after the incident in which the witness viewed the alleged perpetrator, including identification procedures such as photo arrays and lineups, and their impact on the witness's recollection of the identity of the alleged perpetrator. Confidence malleability means the ability of post event information to affect the witness's confidence in the accuracy of the identification. Under the Kassin survey, 93.7% of respondents found that post event information influenced witness identification. Kassin at 13, 17. Further, over 95.2% found the proposition that confidence can be influenced by factors unrelated to the actual identification to be reliable. However, ***10** only 82.5% said they would actually testify about postevent influence, and only 79% said they would testify about confid-

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ence malleability. Further, all of the criticisms of the Kassin survey discussed above are equally applicable here.

There also appears to be a divergence of opinion as to how post event information affects identification. One is that the original memory is replaced and the other is that a coexisting memory is introduced. (Penrod Cross 853-54, 940); Ebbesen Direct 141-145.). Understanding which of these theories is correct would be critical information for a jury in considering the expert testimony offered and its effect on their consideration of the identification. Of all of the areas of testimony offered, this is the most amorphous and difficult for the jury to parse. Did the witness receive such information, did they perceive it and how did they interpret it, did this information go to the heart of their identification, or a minor aspect thereof, what was the level of accuracy of the post event information and the impact of that fact upon the witness's actual identification. Also the jury would have to know the nature of the witness and any particular susceptibility (or resistance) to post event suggestion, and the position, and actual and perceived authority and competency of the transmitter of the post event information. See Penrod Cross 945-949.

Again the Court concludes that expert testimony in these area cannot be admitted.

UNCONSCIOUS TRANSFERENCE

In addition to the core definitional problems referred to above, there are additional problems which relate to the theory of unconscious transference. First, there are three potential hypotheses among researchers as to why such effect might happen. There is in fact even an issue as to whether the effect is conscious or unconscious. (Ebbesen Direct pp. 129-130, 136; Penrod Cross pp. 770-771). The inconsistency of the findings leads the Court to question whether there is in fact any understanding as to why it occurs, or whether it even does occur. There is no published meta analysis of unconscious

transference, and those experiments which have been conducted cast doubt on whether it really exists. See Read, J.D., Tollestrup, P, Hammersley, R., McFadzen, E., and Christensen, A., The Unconscious Transference Effect: Are Innocent Bystanders Ever Misidentified, 4 Applied Cognitive Psychology, 3-31 (1990) (only one in five experiments showed the effect). See also Ross, David F., Ceci, Stephen J., Dunning, David, Toglia, Michal P., Unconscious Transference and Mistaken Identity When a Witness Misidentifies A Familiar But Innocent Person, 79 Journal of Applied Psychology, 918-30 (1994) (contains a caveat that the results, not terribly emphatic in their own right, should not be held out as definitive or generally applicable.) Those other studies offered by Penrod, including his own, were either negative or inconclusive. Penrod Cross at 809-823; 787-788.

In *U.S. v. Langan*, 263 F. 3d 613, 619 (6th Cir.2001), the proposed expert who had performed experiments in the field of unconscious transference, noted that the "literature provides mixed and somewhat weak support for unconscious transference, and the empirical evidence for the [theory's] existence is rather meager". The Sixth Circuit noted that the defense expert's own articles recognized the "limited external validity" and "limited generalizability" of the research.[Id at 622.](#)

*11 WEAPON FOCUS

Again in addition to the difficulties discussed above, there are serious issues concerning the viability of expert testimony on weapon focus. Even in Steblay's meta analysis, only six of the nineteen tests, which made up the meta-analysis, found a weapon focus effect, although the mean effect size for the group of tests showed otherwise. Steblay, supra. at 413-14 (1992). While the mean effect size is not insignificant, a question is raised by the results: that a majority of the tests showed there was no weapon focus effect. As Justice Fried noted in *Legrand*, this calls into question whether the mean is truly representative of the group of studies which made up the meta analysis.

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In fact, these doubts are amplified when the studies are examined. Tollestrup found that the presence of a weapon did not hinder identification and the identifications were often more detailed when a weapon was present than in crimes in which there was no weapon. As Professor Rogers Elliot noted, the research does not yet tell us the circumstances under which presence should produce a moderate or small effect or which people are more susceptible to a weapon focus. Kassin, Saul M., Ellsworth, Phoebe C., and Smith, Vicki L., D, j... vu All Over Again: Elliot's Critique of Eyewitness Experts, Law and Human Behavior, vol. 18 no. 2 (203-210) at 205] [1994]. Focusing on the Kassin survey, while 86.7 % found the proposition that the presence of a weapon impairs the witness's ability to accurately identify the perpetrator to be reliable, only 15 respondents found it to be very reliable, and 27 respondents found it to be generally reliable. Again, substantially fewer respondents said they would actually testify about the alleged weapon focus effect. Even Penrod's own study of weapon focus found the effect not to be present. See Steblay at App B. at 423.

CONCLUSION

Excluding the proffered areas of expert testimony does not preclude the defense from using all of the issues raised in cross examination. Indeed, they may have expert assistance in that endeavor. That is a different proposition, however, from altering the time honored method by which juries assess the validity and strength of eyewitness testimony, their own life experience and cognitive powers. The Court of Appeals has ruled that expert testimony in these area may be appropriate. Indeed, although not before this court, the area of cross racial identification may indeed be ripe for expert testimony. But that is not before this Court today. The issues before the Court are not yet appropriate areas for expert testimony.

The defendant's motion is denied.

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N.Y.Sup.,2004.

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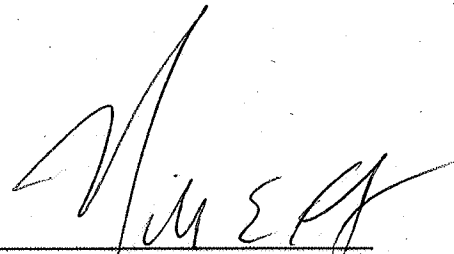
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