JURY INSTRUCTIONS ON EYEWITNESS IDENTIFICATION EVIDENCE: A RE-EVALUATION
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Abstract: The primary contribution of this paper is to challenge the accepted wisdom that jury instructions are an ineffective safeguard against wrongful conviction caused by mistaken eyewitness identification. It argues that such a conclusion is based on an erroneous interpretation of the available experimental evidence and that, in fact, there are grounds for optimism about the effectiveness of jury instructions in educating jurors about the risks posed by eyewitness identification evidence and sensitising them to the factors relevant to its evaluation. In order to play a useful role in safeguarding against wrongful conviction, however, instructions need to be easily comprehensible; to reflect the relevant scientific findings; and be provided to jurors in writing (or an alternative format for those who would find written instructions inaccessible). The paper also makes a secondary contribution, which is to warn of the dangers of accepting uncritically the findings of mock jury research as the basis for legal policy formation.

I. INTRODUCTION

It can scarcely be debated these days that mistaken eyewitness identification evidence is one of the major causes – if not the major cause – of wrongful conviction.\(^2\) Indeed, the debate has long moved on to focus on the merits of various safeguards that might be put in place to minimise the risk of wrongful conviction occurring. These have included measures targeting the police investigation and the conduct of suspect-identification procedures,\(^3\) as well as measures aimed at regulating the use of eyewitness identification evidence at trial, such as exclusionary powers,\(^4\) expert testimony\(^5\) and instructions that alert jurors to the risks associated with eyewitness identification evidence.\(^6\)

It is the last of these that is the concern of this article. Jury instructions have come to be dismissed by many commentators as an inadequate safeguard against wrongful conviction resulting from mistaken eyewitness identification. As Dufraimont puts it, “a level of scholarly consensus has developed around the notion that jury instructions on eyewitness identification are basically ineffective”.\(^7\) This paper re-evaluates that claim and argues that those who have dismissed jury instructions as an effective safeguard have done so too hastily, as the evidence available from the relevant studies does not lead to that conclusion at all. That is not to say that jury instructions provide a complete solution to the problem of wrongful conviction.

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\(^1\) The research on which this article is based was financially supported by the Scottish Government as part of Lord Bonomy’s Post-Corroboration Safeguards Review, a review of safeguards against wrongful conviction following the abolition of the requirement for corroboration in Scottish criminal law. See POST-CORROBORATION SAFEGUARDS REVIEW: REPORT OF THE ACADEMIC EXPERT GROUP (James Chalmers, Fiona Leverick & Alasdair Shaw, eds., 2014). The author would like to express thanks to Alasdair Shaw, who provided invaluable research assistance, the other members of the project team (James Chalmers, Fraser Davidson, Peter Duff, Pamela Ferguson and Findlay Stark) and James Chalmers in particular for helpful feedback on an earlier draft of this article.

\(^2\) See infra notes 9-29 and accompanying text.

\(^3\) See infra notes 50-51 and accompanying text.

\(^4\) See infra note 53 and accompanying text.

\(^5\) See infra note 54 and accompanying text.

\(^6\) See infra note 55 and accompanying text.

based on mistaken eyewitness identification evidence; clearly they do not. The claim made in this article is that, contrary to the accepted view, there are grounds for optimism about the effectiveness of instructions in helping jurors to evaluate eyewitness testimony appropriately. Indeed, there may even be grounds for some degree of confidence.

As such, the article proceeds as follows. Part II establishes eyewitness misidentification evidence as a leading cause of wrongful conviction. Part III sets out exactly why it is such a problematic form of evidence. Part IV outlines the various safeguards that might be utilised to reduce the risk of wrongful conviction caused by mistaken eyewitness identification evidence. Part V considers in detail the available experimental evidence on the effectiveness of jury instructions. It concludes that the studies have generally been misinterpreted to suggest that jury instructions are an ineffective safeguard against wrongful conviction but that a careful reading of the studies does not support this conclusion at all. As such, jury instructions may have a more useful role to play than has hitherto been attributed to them. Part VI considers how the effectiveness of jury instructions on eyewitness identification evidence might be maximised. Part VII offers some concluding remarks.

II. EYEWITNESS IDENTIFICATION EVIDENCE AS A CAUSE OF WRONGFUL CONVICTION

It has long been recognized that mistaken eyewitness identification is one of the leading causes of wrongful conviction. It was one of the causes identified by Borchard’s pioneering study of wrongful conviction in 1932 and has consistently been identified as the leading cause (or one of the leading causes) in studies ever since. The most significant of these are the research projects based on the datasets held by the Innocence Project (founded in 1992 and based at Cardozo Law School) and the National Registry of Exonerations (NRE) (a joint project of the University of Michigan Law School and the Center on Wrongful Convictions at Northwestern University School of Law), both of which are based primarily or exclusively on DNA-based exonerations.

8 It should be said at the outset that jury instructions can only act as a safeguard in trials involving a jury. The safeguards appropriate to bench trials lie beyond the scope of this article.

9 Wrongful conviction is used here to mean the conviction of those who are (in all likelihood) factually innocent. Convictions might be quashed for reasons other than a belief that the appellant is factually innocent, such as a procedural irregularity that casts doubt on the integrity of the criminal justice process. It should also be said that in many cases factual innocence is near impossible to establish with 100 per cent certainty. For a discussion of these issues, see Keith A. Findley, Defining Innocence, 74 ALB. L. REV. 1157 (2010-2011).


11 See e.g. Jerome Frank & Barbara Frank, Not Guilty, 132-148 (1957) (following a study of 36 cases of wrongful conviction); Edward D. Radin, The Innocents (1964) (who identified single eyewitness identification as a leading cause of 80 cases of wrongful conviction); Hugo Adam Bedau & Michael L. Radelet, Miscarriages of Justice in Potentially Capital Cases, 40 Stan. L. Rev. 21, 57 (table 6) (1987) (who examined 350 cases of wrongful conviction spanning 1900-1985 and found mistaken eyewitness identification to be implicated in 56 of these); Arye Rattner, Convicted but Innocent: Wrongful Conviction and the Criminal Justice System, 12 L. & Hum. Behav. 283, 291 (1988) (eyewitness misidentification was present in 100 cases in his study of 205 wrongful convictions).

12 http://www.innocenceproject.org/

13 There is a detailed account of the history of the Innocence Project in Barry Scheck, Peter Neufeld & Jim Dwyer, Actual Innocence: When Justice Goes Wrong and How to Make it Right (2001).

14 www.law.umich.edu/special/exoneration/Pages/about.aspx
The Innocence Project focuses purely on DNA-based exoneration and, at the time of writing, had identified 330 such cases. Garrett analysed the first 250 of these, and found that eyewitness evidence had supported conviction in 76 per cent of cases, making it the leading contributory factor to wrongful conviction by far. The NRE is a wider dataset because it does not limit itself to DNA-based exoneration and, at the time of writing, contained 1,671 cases. A research project examining the causes of the first 873 exonerations (those registered from January 1989 to February 2012) reported that mistaken eyewitness identification was a “contributory factor” in 43 per cent of these. In fact, the figure is higher because, unlike Garrett, the researchers attempted to distinguish between genuine mistakes and deliberate misidentifications and classified the latter as “perjury” (which was a contributory factor in 51 per cent of cases in the sample). Including deliberate misidentifications in the figures adds a further 27 per cent of cases to the total.

The Innocence Project and the NRE focus on exonerations in the US but these findings are not confined to the US context. Mistaken eyewitness identification was the most common cause of wrongful conviction identified by the leading study of wrongful conviction in England and Wales. It has also been pinpointed as a leading cause of wrongful conviction in Canada, New Zealand and in continental European jurisdictions.

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15 Correct as of 6 October 2015: the cases are listed at http://www.innocenceproject.org/know/Browse-Profiles.php.
16 BRANDON L. GARRETT, CONVICTING THE INNOCENT: WHERE CRIMINAL PROSECUTIONS GO WRONG (2011). A shorter summary of his findings can be found in Brandon L. Garrett, Trial and Error, in WRONGFUL CONVICTIONS AND MISCARRIAGES OF JUSTICE 77 (C. Ronald Huff & Martin Killias, eds., 2013). An earlier study, based on the first 200 exonerations, is reported in Brandon L. Garrett, Judging Innocence, 108 COLUM. L. REV. 55 (2008). Others have analysed the Innocence Project data (see e.g. Rory K. Little, Addressing the Evidentiary Sources of Wrongful Convictions: Categorical Exclusion of Evidence in Capital Statutes, 37 SW. U. L. REV. 965 (2008)) but Garrett’s study is by far the most extensive.
18 Although it should perhaps be noted that Garrett’s analysis has been criticised in the respect that merely identifying that a particular type of evidence was led in a trial that resulted in a wrongful conviction does not tell us anything about the degree of influence that this evidence had on the outcome: see Simon A. Cole & William C. Thompson, Forensic Science and Wrongful Convictions, in WRONGFUL CONVICTIONS AND MISCARRIAGES OF JUSTICE 118 (C. Ronald Huff & Martin Killias, eds., 2013).
19 It encompasses cases where pardons were granted or where criminal charges were dismissed at the prosecutor’s motion after new evidence of innocence emerged, acquittals at retrials, a small number of “certificates of innocence” issued by courts and some posthumous exonerations: Gross & Shaffer, infra note 21, at 8.
20 Correct as of 6 October 2015: the cases are listed at: http://www.law.umich.edu/special/exoneration/Pages/browse.aspx
22 Id. at 40.
23 Id. at 40 (table 13).
24 Id. at 43.
25 Id. at 52.
III. Why is Eyewitness Identification Evidence so Problematic?

A vast amount of research into eyewitness memory has been undertaken by psychologists, and its conclusions are encapsulated by Wells et al., who state that “eyewitness testimony is among the least reliable forms of evidence and yet is persuasive to juries”. Most research studies have involved simulations (which vary according to the degree to which they replicate reality), which does need to be borne in mind when considering the validity of the findings although, as Cutler and Penrod point out, eyewitness performance tends to deteriorate the more ‘realistic’ the experimental conditions so, if anything, simulations may under-estimate the extent of the problem.

The psychological literature tells us that mistaken witnesses are often confident when they give evidence at trial, but that such confidence is not correlated with accuracy, which is worrying because (perhaps unsurprisingly) mock jury studies have shown that jurors place great weight on the degree of confidence that an eyewitness reports when giving his or her testimony. In Garrett’s sample of wrongful conviction caused by mistaken eyewitness testimony, the (mistaken) witnesses were all confident at trial and this is likely to have influenced the jury. In 92 of his 161 cases of mistaken eyewitness identification (57 per cent), however, the witnesses reported they had been uncertain at an earlier stage and in 34 of the trials (21 per cent) the witnesses even admitted at the trial that they had been uncertain earlier but even this clearly did not persuade the juries concerned that the identification was mistaken. Worryingly, almost all of the cases in Garrett’s sample involved defendants who looked unlike the person the witnesses first described as the perpetrator.

There is research to suggest that extreme stress (such as the stress that might be experienced by the victim of a violent attack) has a detrimental effect on the accuracy of eyewitness identification, although this is still subject to some debate, due in part to the

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31 Wells et al., supra note 30, at 605.
32 A useful discussion of the various methods and their limitations can be found in Anne Maass, Logic and Methodology of Experimental Research in Eyewitness Psychology, in Psychological Issues in Eyewitness Identification 279 (Siegfried L. Sporer, Roy S. Malpass & Guenter Koehnken eds., 1996).
34 Cutler & Penrod, supra note 30, at 112.
35 Brian L. Cutler, Steven D. Penrod & Hedy Red Dexter, Juror Sensitivity to Eyewitness Identification Evidence, 14 Law & Hum. Behav. 190 (1990). See also the review of the relevant literature in Cutler & Penrod, supra note 30, at 95 and Wells et al., supra note 30, at 620-623.
36 On the utility of mock jury studies: see infra notes 71-84 and accompanying text.
37 Cutler et al., supra note 35; Kenneth A. Deffenbacher & Elizabeth F. Loftus, Do Jurors Share a Common Understanding Concerning Eyewitness Behaviour?, 6 Law & Hum. Behav. 15 (1982) (see their findings in relation to question 7 of the questionnaire they distributed to study participants).
38 Garrett, supra note 16, at 64.
39 Garrett, supra note 16, at 49.
40 Garrett, supra note 16, at 64.
41 Garrett, supra note 16, at 49.
obvious difficulties involved in research design.\textsuperscript{44} Similarly, it has been suggested that the presence of a weapon reduces the accuracy of identification (the so-called “weapon focus effect”).\textsuperscript{45} The vast majority of the 190 mistaken eyewitness identification cases in Garrett’s sample (84 per cent) were ‘stranger’ rapes and Garrett speculates that the ‘stress/weapon effect’ might have been a factor here.\textsuperscript{46}

The psychological literature also indicates that cross-racial identifications are particularly problematic in terms of accuracy.\textsuperscript{47} As such, Garrett found that at least 49 per cent of the exonerees (mis)identified by eyewitnesses involved a cross-racial identification.\textsuperscript{48} Gross and Shaffer found that more than two-thirds of mistaken eyewitness identifications in sexual assault cases involved black defendants and, of these, 72 per cent (69 out of 96) involved an identification made by a white victim.\textsuperscript{49}

\section*{IV. Jury Instructions as a Safeguard}

The evidence presented in the preceding section leads to the conclusion that there is a need for at least some safeguards to operate within the criminal justice system to prevent wrongful conviction based on mistaken eyewitness identification. Various safeguards have been proposed and they fall broadly into two categories: those that target the investigation and those that target the trial. During the investigation, it is important that suspect identification procedures are conducted in a manner that minimises the risk of error and various guidelines have been developed in this respect.\textsuperscript{50} It has also been suggested that individuals should not be subjected to a lineup identification procedure at all unless there is at least a reasonable suspicion (based on other evidence) that they committed the offense in question.\textsuperscript{51} While not wishing to neglect the importance of measures targeted at the investigatory stage, the primary

\footnotesize{Identification Under Stress in the London Dungeon 123 APP. COG. PSYCH. 151 (2008). See also the discussion in Cutler & Penrod, supra note 30, at 103.\textsuperscript{43} It is questioned in e.g. Sven-Ake Christianson, Emotional Stress and Eyewitness Memory: A Critical Review, 112 PSYCHOL. BULL. 284 (1992). See also the discussion in Kapardis, supra note 33, at 45-46.\textsuperscript{44} Designing a realistic experiment that simulates the extreme stress of a violent attack is difficult to do in a way that meets ethical standards.\textsuperscript{45} Elizabeth F. Loftus, Geoffrey R. Loftus & Jane Messo, Some Facts About ‘Weapon Focus’, 11 LAW & HUM. BEHAV. 55 (1987); Nancy M. Steblay, A Meta-Analytic Review of the Weapon Focus Effect, 16 LAW & HUM. BEHAV. 413 (1992). See also the discussion in Cutler & Penrod, supra note 30, at 101 and Kapardis, supra note 33, at 48-49.\textsuperscript{46} Garrett, supra note 16, at 51.\textsuperscript{47} Christian A. Meissner & John C. Brigham, Thirty Years of Investigating the Own-Race Bias in Memory for Faces: A Meta-Analytic Review, 7 PSYCHOL. PUB. POL’Y & L. 3 (2001); Roderick Lindsay & Gary L. Wells, What Do We Really Know About Cross-Race Eyewitness Identification?, in EVALUATING WITNESS EVIDENCE (Sally M. Lloyd-Bostock & Brian R. Clifford eds., 1983). See also the summary in Cutler & Penrod, supra note 30, at 104.\textsuperscript{48} Garrett, supra note 16, at 73.\textsuperscript{49} Gross & Shaffer, supra note 21, at 49.\textsuperscript{50} See e.g. Wells et al., supra note 30, at 627-636 (setting out rules on who should conduct the lineup, the instructions that should be given on viewing, the structure of the lineup and obtaining confidence statements from witnesses); Andrew D. Rikard, How and Why New York Should Enact Mandatory Statewide Eyewitness Identification Procedures, 74 A.L.B. REV. 1525, 1549-1550 (2010-2011) (making recommendations on the composure and conduct of lineups); Richard A. Wise, Kirsten A. Dauphinaise & Martin A. Safer, A Tripartite Solution to Eyewitness Error, 97 J. CRIM. L. & CRIMINOLOGY 807, 856-865 (2007) (setting out ten guidelines for the conduct of identification procedures).\textsuperscript{51} Gary L. Wells, Y eeran Yang & Laura Smalarz, Eyewitness Identification: Bayesian Information Gain, Base-Rate Effect-Equivalency Curves, and Reasonable Suspicion, 39 LAW & HUM. BEHAV. 99 (2015).}
focus of this article is on measures that regulate the use of eyewitness identification at trial and there are essentially three main possibilities. The first is to utilise exclusionary powers to exclude entirely any eyewitness identification evidence that is particularly weak. The second is to use expert testimony to educate the jury on the risks associated with eyewitness identification evidence and the factors that might affect the accuracy of such identifications. The third is to achieve the same result but by way of an instruction given to the jury by the trial judge.

At this point it might be questioned whether any such measures are necessary, especially if the applicable guidelines on pre-trial identification have been followed. It has sometimes been argued that jurors are capable of using their everyday knowledge and experience to evaluate eyewitness identification evidence in the same way as they evaluate any other testimony and need be guided only by a general instruction about witness credibility. The difficulty here is that many of the findings of the scientific studies are counter-intuitive and are unlikely to be within the knowledge of the average juror (or indeed the average judge). One might point in particular to the lack of any correlation between the degree of confidence expressed by eyewitnesses at trial and the accuracy of their

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52 A fourth possibility is to require corroboration of identity in cases involving eyewitness identification. See Sandra G. Thompson, Beyond a Reasonable Doubt? Reconsidering Uncorroborated Eyewitness Identification Testimony, 41 U.C. DAVIS L. REV. 1487, 1541 (2008) (arguing for “the adoption of a corroboration rule in cases in which eyewitness identification testimony is offered”).

53 See e.g. Angela Baxter, Identification Evidence in Canada: Problems and a Potential Solution, 52 CRIM. L.Q. 175, 176 (2007) (suggesting that the trial judge’s discretion to exclude weak identification evidence could be a “more effective solution” to the problem of wrongful conviction caused by mistaken eyewitness identification evidence); Margery M. Koosed, Reforming Eyewitness Identification Law and Practice to Protect the Innocent, 42 CReIGHTON L. REV. 595, 629 (2008-2009) (arguing for a wider exclusionary power for unreliable eyewitness identification evidence); Kent Roach, Unreliable Evidence and Wrongful Convictions: The Case for Excluding Tainted Identification Evidence and Jailhouse and Coerced Confessions, 52 CRIM. L.Q. 210, 215 (2007) (arguing that an exclusionary power “could do much valuable work in excluding evidence that the experience of wrongful convictions suggests can be highly prejudicial”); Eva G. Shell, A Recipe for Mistaken Convictions: Why Federal Rules of Evidence Should be Used to Exclude Unreliable Eye-Witness Identification Evidence, 46 SUFFOLK U.L. REV. 263, 284 (2013) (arguing for the use of exclusionary powers “in recognition of the potential for jurors to become unreasonably swayed by this evidence despite substantial unreliability”).

54 See e.g. Koosed, supra note 53, at 619 (arguing that “[e]xpert testimony should be permitted in all cases where the perpetrator’s identity is a central issue, and there is little or no other independent evidence of the defendant’s guilt”); Michael R. Leippe, The Case for Expert Testimony About Eyewitness Memory, 1 PSYCHOL. PUB. POL’Y & L. 909, 924 (1995) (arguing that “eyewitness expert testimony is not only appropriate, but needed in certain circumstances to improve the likelihood of a valid jury decision”); Wise et al., supra note 50, at 823 (arguing for the admissibility of expert testimony when the primary or sole evidence against the defendant is eyewitness identification).

55 See e.g. Christian Sheehan, Making the Jurors the Experts: The Case for Eyewitness Identification Jury Instructions, 52 B.C. L. REV. 651, 683 (2011) (arguing for “a new model instruction to be given at the outset of criminal trials that involve eyewitness identifications”); Dufrainmont, supra note 7, at 325 (arguing that jury instructions represent the best way to educate juries about the frailties of eyewitness identification evidence in the vast majority of cases).


57 There is a growing body of evidence to suggest that judges hold incorrect beliefs about the factors affecting the accuracy of eyewitness testimony: see Richard A. Wise & Martin A. Safer, What U.S Judges Know and Believe About Eyewitness Testimony, 18 APPLIED COGNITIVE PSYCHOLOGY 427, 432 (2004); Svein Magnussen et al., What Judges Know About Eyewitness Testimony: A Comparison of Norwegian and U.S Judges, 14 PSYCHOLOGY, CRIME & LAW 177, 181 (2008); Richard A. Wise et al., A Comparison of Chinese Judges’ and U.S Judges’ Knowledge and Beliefs About Eyewitness Testimony, 16 PSYCHOLOGY, CRIME & LAW 695, 708 (2010).
identifications or to the detrimental effect of stress or the presence of a weapon on identification reliability. If it is accepted that there is a need to regulate the use of eyewitness identification evidence at trial, the next question is how this might best be achieved. A detailed evaluation of the relative merits of exclusionary powers, expert testimony and jury instructions lies beyond the scope of this article. It should be said at the outset that they are not necessarily mutually exclusive. All three may have an important role to play, depending on the circumstances of the case. The focus here, however, is on the last of the three possibilities – jury instructions.

Most US courts allow some form of instruction to be given to the jury that warns of the dangers of eyewitness identification evidence. Most commonly, the instruction given is based on that in United States v. Telfaire, where the court stated that:

In appraising the identification testimony of a witness, you should consider the following:
1. Are you convinced that the witness had the capacity and an adequate opportunity to observe the offender? Whether the witness had an adequate opportunity to observe the offender at the time of the offense will be affected by such matters as how long or short a time was available, how far or close the witness was, how good were lighting conditions, whether the witness had had occasion to see or know the person in the past.
2. Are you satisfied that the identification made by the witness subsequent to the offense was the product of his own recollection? You may take into account both the strength of the identification and the circumstances under which the identification was made.
3. Finally, you must consider the credibility of each identification witness in the same way as any other witness, consider whether the witness is truthful, and consider whether the witness had the capacity and opportunity to make a reliable observation of the matter covered in his testimony.

I again emphasize that the burden of proof on the prosecutor extends to every element of the crime charged, and this specifically includes the burden of proving beyond a reasonable doubt the identity of the defendant as the perpetrator of the crime with which the defendant stands charged. If after examining the testimony, you have a reasonable doubt as to the accuracy of the identification, you must find the defendant not guilty.

The Telfaire instruction is not without its flaws. It has been criticised on the basis that while it lists some of the factors that might contribute to mis-identification, it is vague as to their relevance, does not explain the way in which they can affect reliability and appears (erroneously) to associate confidence with identification accuracy. As such, some courts have developed more detailed instructions, occasionally going so far as to specifically alert

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58 See supra notes 33-37 and accompanying text.
59 See supra notes 45-46 and accompanying text.
60 See Koosed, supra note 53, at 617.
61 For a survey of different state practices in this respect, see Sheehan, supra note 55, at 670-673. For a survey of the approach of other common law jurisdictions, see Michael Bromby et al., An Examination of Criminal Jury Directions in Relation to Eyewitness Identification in Commonwealth Jurisdictions, 36 Common Law World Review 303 (2007).
63 See e.g. Duframont, supra note 7, at 306; Sheehan, supra note 55, at 680.
jurors to the risk of wrongful conviction associated with mistaken eyewitness identification evidence. In the scholarly literature, a majority view has developed that jury instructions about eyewitness identification evidence are an ineffective safeguard against wrongful conviction. Some have gone as far as to describe this view as a "consensus". This is perhaps to overstate the case, but it is, at the very least, a widely held belief. So, for example, in a review of the available evidence undertaken in 1995, Cutler and Penrod state that "we are forced to conclude that the judges' instructions do not serve as an effective safeguard against mistaken identifications and convictions". Roach suggests that jury warnings place "enormous faith in the ability of juries to follow such instructions, despite the fact that social science and common sense suggest that warnings may not always have their desired effect". Wise et al., in one of the most comprehensive reviews of safeguards against wrongful conviction based on eyewitness mis-identification, conclude likewise and argue instead for expert testimony as a more effective alternative.

Those who consider jury instructions ineffective tend to base their conclusions on a body of experimental evidence gained from psychological studies undertaken with mock jurors. A superficial reading of the relevant studies might well give the impression that jury instructions are an ineffective safeguard. It will be argued here, however, that closer examination of the studies reveals otherwise and it is to this that the article now turns.

V. THE EXPERIMENTAL EVIDENCE ON THE EFFECTIVENESS OF JURY INSTRUCTIONS

In this section of the paper, the experimental evidence on the effectiveness of jury instructions about eyewitness identification evidence will be evaluated. Before embarking on a survey of the research, however, it is necessary to say something about the methods typically used in the studies, all of which involve mock jurors. The paper will then go on to discuss the studies on eyewitness identification instructions before also considering the findings of studies in closely related areas.

A. MOCK JURY STUDIES AND THEIR LIMITATIONS

Mock jury studies can have a number of limitations that potentially affect their "external validity": the extent to which their findings are generalisable beyond the experimental

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65 See e.g. Dufraimont, supra note 7, at 301.
66 Two prominent dissenters are Dufraimont herself and Sheehan: see supra note 55 and accompanying text.
67 Cutler & Penrod, supra note 30, at 264.
68 Roach, supra note 53, at 214.
69 Wise et al., supra note 50, at 830-833 (quoting Cutler & Penrod, id.). For further examples of those who consider jury instructions an ineffective safeguard against wrongful conviction, see Jacqueline McMurtie, The Role of the Social Sciences in Preventing Wrongful Convictions, 42 A.M. CRIM. L. REV. 1271, 1276 (2005); Roger B. Handberg, Expert Testimony on Eyewitness Identification: A New Pair of Glasses for the Jury, 32 A.M. CRIM. L. REV. 1013, 1062 (1995); Thompson, supra note 52, at 1517; Baxter, supra note 53, at 182.
70 The relevant studies are discussed in Part V. infra.
These include inadequate sampling (especially the use of college students as ‘jurors’); inadequate trial simulation (such as a reliance on a transcript or study pack rather than a video or trial re-enactment); an absence of jury deliberation in the research design; the use of data collection techniques that do not reflect the reality of the juror’s task at trial (such as asking jurors to rate the probability of guilt on a scale); and participants’ awareness that they are role playing and that their decision has no real life consequences.

The extent to which each of these affects generalisability is contested by psychologists. In a meta-analysis that is unfailingly cited by those using the research methods in question, Bornstein argued that the use of student jurors and/or trial transcripts makes very little difference to research results. Others have questioned his conclusions, suggesting that this depends on the issue being researched.

There is a broader consensus over the lack of deliberation. As Shaffer and Wheatman put it, “perhaps the greatest limitation of mock-trial simulations is that the vast majority of them attempt to draw inferences from decisions rendered by nondeliberating mock jurors rather than deliberating mock juries” and the researchers go on to discuss some of the reasons why this might be the case. The deliberation process, they suggest, potentially iron out misunderstandings that might be held by individual jurors and jurors who hold prejudices (or who are disinclined to follow instructions) might not act in this way in a group situation where they have to articulate their reasoning to others. There is also a vast body of social psychological literature indicating that group decisions differ from individual decisions and research with real jurors has shown that deliberation does affect the verdict reached in a small but significant proportion of cases.

72 Discussed by Weiten & Diamond, supra note 71, at 75.
73 Id. at 77.
74 Id. at 78.
75 Id. at 79.
76 Id. at 81.
77 See e.g. Helen M. Paterson, David W.M. Anderson & Richard I. Kemp, Cautioning Jurors Regarding Co-Witness Discussion: The Impact of Judicial Warnings, 3 PSYCHOLOGY CRIME & LAW 287, 301 (2013).
82 Id. at 657-658.
83 This is summarised by Nuñez et al., supra note 80, at 443-446.
84 Harry Kalven & Hans Zeisel, The American Jury (1966). Kalven and Zeisel, in what was then a groundbreaking study, recorded the verdicts of juries in a sample of criminal trials in the 1950s. They found that the verdict preferred by the majority of jurors on the first ballot was not the eventual verdict in ten per cent of cases. See similarly Maria Sandys & Ronald C. Dilley, First-Ballot Votes, Pre-deliberation Dispositions, and Final Verdicts in Jury Trials, 19 LAW & HUM. BEHAV. 175 (1995).
Not all mock jury studies suffer from the limitations identified above to the same extent but, as this paper will go on to demonstrate, the concerns noted loom large in the jury instruction studies that are the focus here. Indeed, the majority of them have methodological flaws so serious that little reliance can be placed on them.

B. THE JURY INSTRUCTION STUDIES

An extensive search of legal and psychological databases identified five studies in peer reviewed journals that have assessed the effectiveness of jury instructions about eyewitness identification evidence. Any evaluation of these studies needs to keep in mind that the desired result of a jury instruction on eyewitness identification evidence is to induce what has been termed “juror sensitivity” and not “juror scepticism”, the latter being a general mistrust of eyewitness identification evidence even when this is not merited. As such, any experimental design that does not vary the strength of the eyewitness identification is unlikely to yield any valuable results. The usefulness of the study is also dependent on the quality of the jury instruction utilised. If a particular instruction is difficult to comprehend and/or inaccurate, this does not necessarily mean that all jury instructions are ineffective. It is worth noting the majority of studies used the Telfaire instruction which has been criticised for being vague and potentially misleading. Finally, it is worth repeating the point that studies vary in the extent to which the experimental design replicated the real life trial setting and studies that do not include an element of jury deliberation must be regarded with particular caution.

The earliest of the five studies is that of Katzev and Wishart, who asked 108 mock jurors (forming 30 mock juries) to watch a 40 minute mock burglary trial. The juries were divided into three groups. One received only the standard jury instructions with no summary of the evidence in the case or special instructions relating to eyewitness identification evidence. One received, in addition to this, a judicial summary of the evidence. The third group received the standard instructions, the judicial summary of the evidence and a short judicial instruction on eyewitness identification evidence. Jurors were asked to make a pre-deliberation decision about the guilt or innocence of the defendant, before deliberating in their jury groups and recording a post-deliberation group verdict. The authors found that providing a judicial instruction on eyewitness identification resulted in a significant increase

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85 One further study that might have been added to this list is Brian H. Bornstein & Joseph A. Hamm, Jury Instructions on Witness Identification, 48 COURT REVIEW 48 (2012). This study, however, was not published in a peer reviewed journal and it contains such limited details of its research methods that its generalisibility is impossible to evaluate.


87 Id. at 26.

88 Id.

89 See supra note 62 and accompanying text.

90 See supra note 63 and accompanying text.


92 Id. at 737. The instruction is reproduced at 737 (footnote 19).

93 Id. at 739.
in not guilty verdicts pre-deliberation. Post-deliberation, the number of not guilty verdicts was also slightly higher among those juries receiving the instruction on eyewitness identification evidence, although the finding was not statistically significant. There were, however, numerous weaknesses in the experimental design. It used college student subjects, there was no variation of the strength of the identification evidence and, as evidenced by the fact that post-deliberation 27 of the 30 juries returned not guilty verdicts, the overall evidence against the accused was very weak. As such, little can be usefully taken from the findings.

The next relevant study was carried out by Cutler et al., in which mock jurors were given a Telfaire instruction after a videotaped mock robbery trial in which a witness identified the defendant as the perpetrator. There were two versions of the experiment, in which the strength of the identification evidence was varied. In the first version, the defendant wore a hat, brandished a handgun and the eyewitness identified him as the perpetrator 14 days after the robbery. In the second version, the robbery wore no disguise, kept his handgun hidden and the identification took place two days after the robbery. The study found some evidence of increased juror sensitivity (as measured by the proportion of guilty verdicts returned) from hearing the instruction, although the effect was small and not statistically significant. Once again, however, the research methods used by the study cast a shadow over the usefulness of its findings. The 144 mock jurors were all college students, the experiment did not involve deliberation and, as Cutler, Dexter and Penrod themselves acknowledge, the poor quality of the Telfaire instruction may have been to blame for inducing unjustified scepticism in some participants.

A further study was undertaken by Ramirez et al., who conducted two separate experiments, both involving a video of a mock trial that was watched by college student subjects. The first – the robbery of a liquor store by a lone gunman – used very similar methods to Cutler et al. and found that the Telfaire instruction caused a significant scepticism effect – jurors hearing the instruction were less likely to convict in both the “good” and the “poor” identification conditions. The second compared the effectiveness of the Telfaire instruction with a re-written instruction in which the language was simplified and the content

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94 Id. at 740. It should be noted that table 1 of their paper (at 739) erroneously omits the word “not” from “not guilty” and is therefore a misleading representation of their results.
95 Id. at 736.
96 Id.
97 Id. at 740.
98 See the summary of the evidence given in the judge’s charge, which is reproduced in full at 738 (footnote 18).
99 Id. at 740.
101 The study also compared the effectiveness of jury instructions to that of testimony from a court appointed expert and its findings in this respect are discussed below: see infra notes 159-161 and accompanying text.
102 Id. at 1202.
103 Id. at 1205.
104 Id. at 1202.
106 Id. at 38.
107 Id.
108 Id. at 41.
revised to reflect more accurately the relevant experimental research. They found that there was little difference between the effectiveness of the Telfaire instruction and their re-written instruction in terms of the proportion of guilty verdicts returned. However, this finding must be regarded with caution. Like Cutler et al., the mock jurors were college students, the experiment did not involve deliberation and the evidence against the accused was very weak overall. Even in the “good identification conditions” version of the experiment, the only evidence against the defendant was that of a single eyewitness who viewed him for approximately 20 seconds while experiencing a traumatic event (the burglary of her house). Evidence was also presented at the trial that the defendant had an alibi – a friend testified that at the time of the burglary the two of them were together doing plumbing work in the friend’s kitchen. This does suggest that scepticism might, in fact, have been the most appropriate attitude in both the “good” and “poor” identification conditions, bearing in mind that the jurors were given the standard direction that they should convict only if they were convinced beyond reasonable doubt of the defendant’s guilt. It is also worth noting that the revised instruction did result in a significant improvement in recall of the instruction’s content and a “modest” increase in juror knowledge about the relevant issues.

An improvement in the realism of the experimental conditions can be found in the two experiments undertaken by Greene. Her first experiment involved a videotaped assault trial that was shown to college student “jurors” in which a person drinking in a bar was accused of throwing a bottle that hit and blinded another customer. No-one actually saw the defendant throw the bottle, but one of the bar staff testified that the defendant “might have done so”. There was no other evidence against the defendant. The strength of the identification evidence given by the witness was varied (in the “strong” version she had an unobstructed view and the bar was well lit; in the “weak” version the bar was dimly lit and her view was partially obstructed). The jurors were given a Telfaire instruction and were allowed 30 minutes of deliberation before reaching a “verdict”. In the second experiment, the conditions were identical, save for the fact that she, like Ramirez et al, used a revised instruction, rewritten to make it linguistically more comprehensible and to reflect more accurately the findings of relevant psychological research, and a shadow jury was used rather than college student subjects.

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110 Id. at 47. The re-written instruction is re-produced in Appendix B of the paper.
111 Id. at 47.
112 Id. at 50.
113 This was distinguished from the “poor” identification conditions version by the fact that the perpetrator was viewed for 20 seconds (rather than five), from five feet (rather than 15 feet), in good lighting conditions, and had no gun (as opposed to having a visible gun). The defendant was also identified two hours after the event (as opposed to two weeks) and he was picked out in both a photo ID and a lineup (as opposed to only in the lineup) (Id. at 48-49).
114 Id. at 48.
115 Id.
116 Id. at 57.
117 Both reported in Edith Greene, Judge’s Instruction on Eyewitness Testimony: Evaluation and Revision, 18 J. APPLIED SOCIAL PSYCHOLOGY 252 (1998).
118 Id. at 256.
119 Id.
120 Her revised instruction is reproduced at 263-264.
121 Greene, supra note 117, at 264. A shadow jury involves the use of real life jurors who have been summoned to court but not ultimately selected for trial.
Greene found that the Telfaire instruction caused a significant scepticism effect – when it was given the conviction rate decreased from 42 per cent to 6.5 per cent even for the strong identification evidence version of the experiment.\textsuperscript{123} It had no effect when the weak identification evidence was used, where the conviction rate was three per cent regardless of whether the jury had been given the instruction, but, as Cutler and Penrod point out,\textsuperscript{124} this was probably because the weak evidence was so weak that it would have been perverse to convict on it. In Greene’s second experiment, the revised instruction also induced scepticism rather than sensitivity. It resulted in a higher proportion of acquittals in the weak identification evidence condition (73 per cent compared to 42 per cent where no instruction was given and 41 per cent where a Telfaire instruction was given). However, it also resulted in a higher proportion of acquittals in the strong identification evidence condition (75 per cent, compared to 22 per cent for no instruction and 35 per cent for the Telfaire instruction).\textsuperscript{125}

Greene’s second experiment is without doubt the study that has used the most realistic experimental conditions, and it used a comprehensible and accurate jury instruction, and yet this still induced scepticism rather than sensitivity. This might imply that jury instructions on eyewitness identification evidence are of limited usefulness. However, her research design still had important limitations. It was a single experiment involving only 139 jurors where deliberation was limited to 30 minutes, after which jurors were asked to vote individually (rather than reach a collective decision). Most problematically, as noted above, the eyewitness identification evidence, even in the “strong” version of the experiment, was actually very weak. The only evidence against the defendant was the testimony of a single eyewitness who, when asked if the defendant was the person who threw the bottle, said only that the defendant “might have done so”\textsuperscript{126} As such, scepticism was entirely appropriate – especially given that, as in Ramirez et al’s experiments, the mock jurors were given the standard trial direction to convict only if they were convinced beyond reasonable doubt of the defendant’s guilt.\textsuperscript{127} It is worth noting that Greene’s rewritten instruction was extremely effective in improving juror understanding of the factors affecting the accuracy of eyewitness identifications – jurors who were given the rewritten instruction scored significantly better on this measure than jurors who were given no instruction or the Telfaire instruction.\textsuperscript{128}

Finally, a rather different research method was used in a study undertaken by Martire and Kemp.\textsuperscript{129} They used what they called a “real eyewitness design”\textsuperscript{130} where a first set of study participants acted as “witnesses” who were asked to view a video reconstruction of a robbery and then identify the perpetrator from a line-up. They then “gave evidence” and a second set of study participants acting as “jurors” were asked whether or not they believed them. This could then be compared to the true accuracy of the identifications. The jurors in the experiment were college students and they were divided into six groups where the experimental conditions were varied so that they watched either a “correct” or a “mistaken”

\textsuperscript{123} Id. at 258.
\textsuperscript{124} Cutler & Penrod supra note 30, at 260.
\textsuperscript{125} Greene supra note 117, at 266.
\textsuperscript{126} Id. at 256, emphasis added.
\textsuperscript{127} Id. at 264.
\textsuperscript{128} Id. at 259 (no instruction and Telfaire instruction), 267 (rewritten instruction).
\textsuperscript{129} Kirsty A. Martire and Richard I. Kemp, The Impact of Eyewitness Expert Evidence and Judicial Instruction on Juror Ability to Evaluate Eyewitness Testimony, 33 LAW & HUM. BEHAV. 225 (2009).
\textsuperscript{130} Id. at 226.
witness give evidence and they received either a jury instruction, a video of expert testimony on eyewitness identification, or no assistance at all. The researchers found that jurors were correct in their assessments 63.6 per cent of the time but that there was no significant difference between the jury instruction group, the expert testimony group and the control group: “the objective accuracy of the judgments they made were not found to be significantly associated with the type of instruction they heard”.

But the usefulness of this study is questionable at best. The number of mock jurors who witnessed each of the six possible scenarios was very small and the study design did not include any element of deliberation. In addition, the “witnesses” watched a video reconstruction rather than experiencing a real life event where environmental conditions and stress would most likely have played a part in the accuracy of their identification. Most importantly, the conditions in which the witnesses saw the perpetrator were not varied and therefore the only variables the jurors had to go on in determining accuracy were the witnesses’ reported confidence levels and their demeanour at trial. As such, little can be usefully concluded about the effectiveness of jury instructions on eyewitness identification evidence.

To summarise, all that can really be said on the basis of the experimental studies on the effectiveness of jury instructions about eyewitness identification evidence is that they are inconclusive. The studies are, as Dufraimont puts it, “few in number ... plagued with methodological problems and focus predominantly on the Telfaire instruction, which lacks the kind of informational content necessary to educate jurors about the frailties of eyewitness identification”. The two studies that evaluated the effect of a revised instruction (Greene’s experiment 2, Ramirez et al’s experiment 2) were, frustratingly, both hampered by the fact that the evidence against the defendant was weak even in the version of the experiment where it was supposed to be strong. Thus, while both reported a scepticism effect, scepticism was an entirely appropriate attitude. It is worth reiterating that both studies found that rewritten instructions improved juror comprehension of the relevant issues, compared to no instruction at all, or the Telfaire instruction.

C. STUDIES IN CLOSELY RELATED AREAS

Given the limited number of studies of jury instructions about eyewitness identification evidence, it is worth turning to some of the experimental evidence in closely related areas. Studies on the effectiveness of jury instructions in helping jurors to evaluate particular types of evidence other than eyewitness identification evidence are few and far between, most likely because few other types of evidence are so inherently problematic. Nonetheless, there are a few studies where the issues being investigated are sufficiently similar to be worth considering.

The first of these is a study by Paterson et al that examined eyewitness evidence but in a different context. The researchers examined the impact of a jury instruction about the effect of post-event discussion among eyewitnesses in a dangerous driving case. Mock jurors...
were given a trial transcript (of a dangerous driving trial) where eyewitness evidence was given by two witnesses.\textsuperscript{135} One gave evidence to the effect that she had seen the defendant using a cell phone. She did not mention this to the police in the statement she made to them immediately after the incident, but told them about it later after she had spoken to another witness to the event. The other witness mentioned the mobile phone in both her initial statement and in a later interview.\textsuperscript{136} Half of the participants were given a jury instruction that explained that eyewitness evidence can be unreliable and that reliability might be affected by the circumstances in which the witness observed the event. The other half were given the same general warning, but were also given a specific warning about the possible effect of co-witness discussion on memory.\textsuperscript{137}

In Paterson et al’s experiment, the specific warning did not induce general scepticism but instead resulted in a marked sensitivity effect – there was a significant reduction in belief of the testimony of the inconsistent witness when the specific warning was given, compared to the general warning condition.\textsuperscript{138} Such a reduction did not occur in relation to the consistent witness.\textsuperscript{139} This did not translate into a change in beliefs about the guilt or innocence of the defendant but, as the researchers suggest, this could be for any multitude of reasons, including the strength of the rest of the evidence in the case.\textsuperscript{140} That said, the findings must be still regarded with some caution. This was not the most realistic of experiments – the mock jurors were 80 college students, it involved a transcript rather than a video reconstruction and there was no deliberation.

Aside from this single experiment that has focused on eyewitness evidence in a different context, one might also look to studies that have evaluated the effect of jury instructions on other types of witness testimony. Two mock jury studies in particular are worth noting. The first is that undertaken by Bollingmo et al.,\textsuperscript{141} who evaluated the effectiveness of an instruction aimed at correcting the misconception that the extent to which a witness displays emotion provides a reliable cue to her credibility. Participants watched a five minute video of a mock police interview in which a woman gave an account of a rape.\textsuperscript{142} Experimental conditions were varied, so that (a) the witness displayed varying degrees of emotion when giving her statement and (b) the participants were either given an instruction warning of the danger of equating emotional condition with truthfulness or were not. The instruction was found to be highly effective at correcting any misconceptions held by participants in this respect, with the groups who were given the instruction reporting that they had given less weight to non-verbal cues and delivering higher credibility ratings of the witness than the group who were not.\textsuperscript{143} Notwithstanding, it does have to be said that the research methods used were not the most realistic in terms of simulating the trial experience. The 334 subjects were college students, there was no deliberation built into the experiment and the jury instruction was delivered verbally by the experimenter after the conclusion of the

\begin{itemize}
\item Id. at 292.
\item Id. at 293.
\item Id.
\item Id. at 297.
\item Id. at 299.
\item Id. at 300.
\item Id. at 63.
\item Id. at 67.
\end{itemize}
video.\textsuperscript{144} Given that the effect of the jury instruction was positive even in unrealistic conditions, these concerns might be seen as less pressing than in the eyewitness identification evidence experiments, where the less than realistic experimental conditions might well have been the reason why a positive effect was not produced.\textsuperscript{145} It does have to be said, though, that the experimental conditions were vastly simplified compared to a real life trial where jurors would be subject to a far greater volume and complexity of information than they were here, so it cannot simply be assumed that any positive effect would be replicated in the real life setting.

The other experiment worth noting was undertaken by Goodman-Delahutny et al., who examined the effectiveness of jury instructions in correcting common misconceptions about child memory of and responses to sexual abuse.\textsuperscript{146} The study participants were asked to read a transcript of evidence given by a child reporting sexual abuse. In the first experimental condition the transcript included a case summation by the trial judge but no information specifically relating to child witnesses.\textsuperscript{147} In the second experimental condition, the transcript also contained an instruction setting out the key scientific findings about children’s reactions to sexual abuse, their ability to recall experienced events and various other relevant matters.\textsuperscript{148} Jury instructions, the study concluded, significantly increased juror knowledge of the key scientific findings\textsuperscript{149} and resulted in an increased likelihood of a guilty verdict.\textsuperscript{150} Here too, as the researchers themselves acknowledge,\textsuperscript{151} the research methods used were less than realistic. Their subjects were 118 college psychology students, they relied on transcripts, rather than a trial simulation, and there was no deliberation built into the experiment.\textsuperscript{152} The effect of the instruction here was positive, and so the less than realistic conditions cannot be blamed for failing to produce an effect, but as with Bollingmo et al’s study it is impossible to know whether the same effect would have been produced in the more complex setting of a real life trial.

The other body of literature to which one might turn is the studies that have evaluated the effectiveness of expert testimony on eyewitness identification evidence. These have vastly outnumbered the studies of jury instructions in this context, as indicated by the fact that there exist several meta-analyses of the latter\textsuperscript{153} but none of the former. Martire and Kemp’s meta-analysis identified 24 experiments reported in peer-reviewed journals,\textsuperscript{154} some of which found that expert testimony induced a general scepticism effect but some of which found that it improved juror sensitivity.\textsuperscript{155} This might be seen as a cause for optimism, as the fact that expert testimony is capable of inducing sensitivity (albeit in experimental conditions

\begin{footnotesize}
\textsuperscript{144} Id. at 64.
\textsuperscript{145} See supra notes 91-132 and accompanying text.
\textsuperscript{146} Jane Goodman-Delahutny, Anne Cossins & Kate O’Brien, A Comparison of Expert Evidence and Judicial Instructions to Counter Misconceptions in Child Sexual Abuse Trials, 44 AUSTRALIAN & NEW ZEALAND J. CRIMINOLOGY 196, 208 (2011).
\textsuperscript{147} Id. at 204.
\textsuperscript{148} Id. at 206. There was also a third version of the experiment where the instructions were delivered by an expert witness (id. at 205) – this is discussed below (see text attached to note 162).
\textsuperscript{149} Id. at 208.
\textsuperscript{150} Id. at 211.
\textsuperscript{151} Id. at 213.
\textsuperscript{152} Id. at 204.
\textsuperscript{153} See e.g. Martire & Kemp, supra note 86; Leippe, supra note 54.
\textsuperscript{154} Martire & Kemp, supra note 86, at 25. The findings of the studies are summarised in their paper in tabular form at 31.
\textsuperscript{155} Id. at 31.
\end{footnotesize}
with all the generalisability caveats that this implies) suggests that appropriate jury instructions might do likewise. This especially as many of the expert testimony experiments used “court appointed experts” who were not cross-examined and thus the input provided to jurors was not dissimilar to that of a jury instruction.156

This does, of course, raise the question of whether expert testimony might be more effective than jury instructions as a safeguard against wrongful conviction based on eyewitness identification evidence. A detailed discussion of the relative merits of these two safeguards lies beyond the scope of this article.157 The claim being made here is not that expert testimony is ineffective in this context, but that jury instructions are effective, although it should be said that if confidence can be placed in the effectiveness of jury instructions, there are many reasons to prefer them over expert testimony, not least of which is cost, both in terms of time and money.158 Two experiments have directly compared the effectiveness of jury instructions and expert testimony in the context of eyewitness identification evidence (both of which were described above in the context of their findings on jury instructions).159 Neither found any evidence that expert testimony was a superior method of inducing sensitivity,160 although as both studies suffered from methodological flaws161 this finding does have to be regarded with some caution. A comparison of expert testimony and jury instructions in a different context – the study of child witness testimony in sexual abuse trials discussed above – also found both to be equally effective in correcting misconceptions.162

D. A SUMMARY SO FAR

As the discussion thus far has indicated, the experimental studies that have examined the effectiveness of jury instructions about eyewitness testimony are inconclusive. The limited number of studies that have been undertaken have mostly shown that jury instructions tend to result in increased scepticism towards all eyewitness identification evidence, regardless of its strength. This conclusion, however, has to be tempered by the fact that every single study – even the most realistic – suffered from serious methodological problems. Support for the effectiveness of jury instructions can be drawn from the expert testimony studies, some of which have been found to induce sensitivity, and (to a limited extent as it too had methodological flaws) from an experiment that examined eyewitness testimony and witness contamination. Finally, there is some cause for optimism in the fact that at least two studies (Greene’s experiment 2 and Ramire, Zemba and Geiselman’s experiment 2) have shown that a well-constructed instruction can improve juror appreciation of the factors affecting the reliability of eyewitness identification evidence and that jury instructions on witness credibility in other contexts (namely the effect of emotion on credibility and the factors

156 Leippe, supra note 54, at 934-939. It should perhaps be said that an expert by virtue of his or her qualifications, might be regarded by jurors as more persuasive than a trial judge.
158 Cutler et al., supra note 35; Martire & Kemp, supra note 129.
159 Cutler et al., supra note 35, at 1202; M artire & K emp, supra note 129, at 231.
160 See the discussion supra notes 100-105 and accompanying text (Cutler, Dexter & Penrod) and supra notes 129-132 and accompanying text (M artire & K emp).
161 Goodman-Delahunt, supra note 146, at 208, 211.
affecting the credibility of child sexual abuse complaints) have been effective in improving appreciation of the relevant issues. There are, in summary, grounds for cautious optimism that jury instructions on eyewitness identification can work – a conclusion far removed from the accepted scholarly wisdom that they are entirely ineffective.

Their effectiveness is, however, likely to depend on their content and on the manner in which they are presented. As such, it is worth turning to the broader body of research that has examined the factors that can improve the effectiveness of jury instructions.

VI. WHAT CAN BE DONE TO IMPROVE THE EFFECTIVENESS OF JURY INSTRUCTIONS?

What then can be done to maximise the effectiveness of jury instructions about eyewitness identification? Once again it is worth turning to the relevant experimental evidence and three factors in particular stand out: simplification of language; ensuring that the instructions accurately reflect the relevant considerations; and providing instructions in writing. Each will be examined in turn.

A. SIMPLIFICATION OF LANGUAGE

If jury instructions are to be effective, they need to convey information in a way that jurors can understand and utilise. Over-complex jury instructions are likely to be ineffective at best and counter-productive at worst. A vast body of experimental research exists that has assessed the extent to which juries comprehend the instructions they are given by trial judges and, as Comiskey puts it, these “have almost unanimously concluded that a jury’s ability to comprehend legal instructions is poor and that there is room for considerable improvement”.

To give some examples, Haney and Lynch, in a study of death penalty instructions in California, found that jurors were unable to apply them because they did not know what ‘mitigating’ or ‘aggravating’ meant. Rose and Ogloff tested Canadian mock jurors’ comprehension of an instruction on conspiracy and concluded that it was “abysmally poor”. In research undertaken with 48 real life criminal juries for the New Zealand Law Commission, Young et al. asked jurors about the instructions they had received (which

163 Duframont, supra note 7, at 297; Sheehan, supra note 55, at 687; QUEENSLAND LAW REFORM COMMISSION, A REVIEW OF JURY DIRECTIONS, para 5.77 (2009).
164 NEW SOUTH WALES LAW REFORM COMMISSION, JURY DIRECTIONS, para 1.68 (2012).
included instructions on the ingredients of the offence, the meaning of intent and the meaning of beyond reasonable doubt). They concluded that:\(^\text{169}\)

... there were widespread misunderstandings about aspects of the law which persisted through to, and significantly influenced, jury deliberations. Indeed, there were only 13 of the 48 trials in which fairly fundamental misunderstandings of the law at the deliberation stage did not emerge.

In the UK, Thomas was granted access in three English Crown Courts to jurors who had not been selected to sit on a trial.\(^\text{170}\) The jurors observed a simulated trial and were given instructions from a practising trial judge. In one court, jurors were tested on their understanding of an instruction they had received on the law of self-defense. While 68 per cent of jurors claimed that they had understood the instruction, when assessed objectively only 31 per cent actually had.\(^\text{171}\)

It has been suggested that there is no reason to be concerned about findings such as these because any difficulty individual jurors have in understanding instructions will be resolved during the deliberation process.\(^\text{172}\) Deliberation can undoubtedly affect trial outcomes, as noted earlier,\(^\text{173}\) and the studies that have examined the effect of deliberation on juror comprehension provide some support for its effectiveness in correcting mistakes.\(^\text{174}\) Its curative power should not, however, be over-stated.\(^\text{175}\) Deliberation will be effective in this respect only if, as Diamond puts it, “a significant proportion of the jurors begin deliberations with correct information; otherwise, deliberation may simply reinforce the inaccuracies of the majority”.\(^\text{176}\) In Rose and Ogloff’s study of the comprehension of the conspiracy instruction, deliberation made no difference\(^\text{177}\) and the inherent complexity of the instruction was the most likely reason why.

It might be questioned at this point how much of the preceding discussion is relevant to instructions on eyewitness identification evidence, which are relatively straightforward compared to some of the instructions that have been the subject of research. Juror comprehension levels have been shown to vary depending on the type of instruction,\(^\text{178}\) with instructions about procedural law generally being better understood than those about substantive law.\(^\text{179}\)

Even if this is the case, there is nonetheless no harm in ensuring that jury instructions about eyewitness identification evidence are made as linguistically straightforward as possible, while of course also retaining their legal integrity.\(^\text{180}\) This has been recognised in


\(^{171}\) Id. at 36.

\(^{172}\) See e.g. Young et al., supra note 169, at para 7.25.

\(^{173}\) See supra notes 80-84 and accompanying text.


\(^{175}\) Comiskey, supra note 166, at 641.

\(^{176}\) Diamond, supra note 71, at 565.

\(^{177}\) Rose and Ogloff, supra note 168, at 426.

\(^{178}\) Comiskey, supra note 166, at 642.


\(^{180}\) Simmonsen, supra note 158, at 1086.
other jurisdictions – the New Zealand Institute of Judicial Studies, for example, has employed editors with expertise in writing plain English in the preparation of the Criminal Jury Trials Bench Book. Indeed, experimental research has demonstrated that comprehension can be substantially improved by the use of simple language and straightforward syntax. Charrow and Charrow, for example, found that juror comprehension improved by between 35 and 41 per cent (depending on the measure of comprehension used) when they re-wrote 14 US civil jury instructions in simpler language. In the criminal context, other studies have achieved similar results. The re-written instructions in all of these studies were approved by trial judges, who checked that the re-write was legally acceptable.

B. THE CONTENT OF THE WARNING

If jury instructions are to be effective, they need to convey accurate information about, for example, the factors that have been shown to influence the accuracy of eyewitness identification. Instructions in some jurisdictions have been criticised on the basis that they either omit important information (such as the “weapon effect” or the particular difficulties involved in cross-racial identifications) or are actively misleading (for example by suggesting that the degree of confidence reported by the eyewitness at the time of the trial is an indication of accuracy).

Experimental research has also suggested that juries are more likely to follow instructions if it is explained to them why they are being given. In the present context, this

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181 As noted by the New South Wales Law Reform Commission, supra note 164 (referencing email correspondence between them and the New Zealand Institute at para 3.25 note 35).
182 For a review of the relevant research, see e.g. Rose & Ogloff, supra note 168, at 427-429; Joel D. Lieberman & Bruce D. Sales, What Social Science Teaches Us About the Jury Instruction Process, 3 PSYCHOL. PUB. POL’Y & L. 589, 626-627 (1997).
183 Robert P. Charrow & Veda R. Charrow, Making Legal Language Understandable: A Psycholinguistic Study of Jury Instructions, 79 COLOM. L. REV. 1306, 1331 (1979). The instructions were on issues including causation, witness credibility, expert evidence and negligence. The authors set out a method for improving comprehension at 1321-1328, which includes measures such as removing nominalizations, prepositional phrases, technical words, multiple negatives and embedded phrases.
184 See e.g. Laurence J. Severance, Edith Greene & Elizabeth F. Loftus, Toward Criminal Jury Instructions That Jurors Can Understand, 75 J. CRIM. LAW & CRIMINOLOGY 198 (1984) (the authors tested comprehension of instructions on the standard and burden of proof, intent, and the use of prior convictions and achieved significant improvements in comprehension following their re-write); Amiram Elwork, James J. Alfini & Bruce Sales, Towards Understandable Jury Instructions, 65 JUDICATURE 432 (1982) (the authors found that comprehension improved significantly when they re-wrote standard pattern instructions on a variety of issues including the meaning of beyond reasonable doubt, the definition of murder, the definition of insanity and the permitted use of expert evidence).
186 See supra notes 45-46 and accompanying text.
189 Nancy K. Steblay et al., The Impact on Juror Verdicts of Judicial Instruction to Disregard Inadmissible Evidence: A Meta-Analysis, 30 LAW & HUM. BEHAV. 469, 486 (2006); Ellsworth & Reifman, supra note 174 (see the studies discussed at 805); Saul M. Kassin & Samuel R. Sommers, Inadmissible Testimony, Instructions
implies that a jury instruction on eyewitness identification evidence ought to explain to the jury that people have been wrongly convicted on the basis of flawed evidence of this nature, or that errors in identification have occurred in the past, although the instruction would have to be carefully drafted so as not to induce undue scepticism.190

C. PROVIDING WRITTEN JURY INSTRUCTIONS

A final consideration is whether jury instructions should be provided in writing, something that has been advocated by numerous law reform bodies191 and researchers192 worldwide. In New Zealand the jury research project undertaken in 2001 resulted in the extensive use of written instructions in that jurisdiction193 and they are increasingly used in Canada194 and in some US jurisdictions.195

It has been argued that written instructions can lead to a number of benefits, including improvements in memory;196 improvements in comprehension;197 better quality deliberations (where more time is spent applying the law);198 reduced deliberation time (as juries spend less time trying to recall the instructions and any disputes about their content are quickly and easily resolved);199 and improvements in juror confidence and satisfaction.200 In relation to the first of these, Semmler and Brewer make the point that we are asking an awful lot of

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190 A good starting point is the instruction given by the trial judge in United States v. Burrous, 934 F. Supp. 525, 531-533 (E.D.N.Y 1996). The full instruction is attached as an appendix to the case report and states inter alia: “I want to caution you, first, that the kind of identification testimony you heard in this case must be scrutinized carefully. Scientific studies have amply demonstrated the dangers of mistake in human perception and identification. Of course, this does not mean that the identification in this case is incorrect. I merely tell you this so that you understand the importance of carefully evaluating the evidence here.”

191 See e.g. LORD JUSTICE AULD, REVIEW OF THE CRIMINAL COURTS OF ENGLAND AND WALES 533 (2001); New South Wales Law Reform Commission, supra note 164, at para 6.121; NEW ZEALAND LAW COMMISSION, JURIES IN CRIMINAL TRIALS para 314 (2001); VICTORIAN LAW REFORM COMMISSION, JURY DIRECTIONS (2009) (recommendations 43 and 44 discussed at paras 6.46-6.60).

192 An early advocate was Robert F. Forston, Sense and Non-sense: Jury Trial Communication, B.Y.U. L. REV. 601, 619 (1975) (“the advantages of using written instructions are dramatic”). More recently, see Comiskey, supra note 166, at 653; Devine et al., supra note 80, at 712; B. Michael Dann, Learning Lessons and Speaking Rights: Creating Educated and Democratic Juries, 68 IND. L.J. 1229, 1259 (1993).


194 Madge, supra note 193, at 820.

195 Id. at 820; Madge, supra note 165, at 451.

196 Madger, supra note 164, at 452; Y oung, supra note 193, at 684; Larry Heuer & Steven D. Penrod, Instructing Jurors: A Field Experiment With Written and Preliminary Instructions, 13 LAW & HUM. BEHAV. 409, 410 (1989); New South Wales Law Reform Commission, supra note 164, at para 6.119.

197 NEW SOUTH WALES LAW REFORM COMMISSION, JURY DIRECTIONS, para 10.13 (2008); Young, supra note 193, at 684.

198 Forston, supra note 192, at 619.


200 Forston, supra note 192, at 619; Heuer and Penrod, supra note 196, at 410; New South Wales Law Reform Commission, supra note 197, para 10.16.
jurors to retain the information provided by the trial judge – even the simplest charge is likely to run to several pages of instructions – and it may be that at least some barriers to increased comprehension may simply stem from limitations in working memory. Some possible objections to written instructions include the fear that they might increase deliberation time (because jurors become involved in time consuming arguments over how to interpret them); that they might be time consuming and burdensome for trial judges to produce, or that they assume a level of juror literacy that might not be borne out in practice.

The available research suggests that all (or, at worst, most) of the advantages are borne out in practice and that none of the disadvantages transpire. In England and Wales, a trial judge on the criminal circuit adopted the practice of giving written instructions to jurors in all cases for a period of several months. He concluded that this “seems to have almost eliminated requests from juries for reminders or further guidance on the law. Juries also seem to be reaching verdicts more quickly.” Marder notes that a US judge who has given each juror a written copy of her instructions for more than a decade, described the innovation as “wildly successful” and as “an inexpensive, effective way to virtually guarantee juror understanding of the law.” Admittedly these studies are anecdotal, unscientific and small scale, but their findings are supported by surveys of real life jurors. These have found that jurors who were not provided with written instructions thought that written instructions would have assisted them in their task and that jurors who did receive written instructions found them useful.

The anecdotal evidence is also supported by the findings of experimental research, the most extensive being that of Heuer and Penrod. In their study, 29 judges in Wisconsin randomly assigned their trials so that some juries received written instructions and some did not. After the trial was over the jurors were asked to complete questionnaires aimed at testing their understanding of the instructions they received (specifically those relating to the standard and burden of proof, the presumption of innocence, the evaluation of testimony and exhibits and procedural issues such as the allocation of responsibility for findings of law and

204 Madge, supra note 193, at 821.
206 As Madge himself accepts, supra note 193, at 822.
207 Brian L. Cutler and Donna M. Hughes, Judging Jury Service: Results of the North Carolina Administrative Office of the Courts J urors Survey, 19 BEHAVIORAL SCIENCES AND THE LAW 305 (2001) (surveying 1478 people who had served as jurors); Young et al., supra note 169, at para 7.60 (only 24 per cent of their sample of serving jurors did not think that written instructions would have been helpful; 62.2 per cent thought that they would have been helpful and 13.8 per cent gave no response).
208 Young et al., supra note 169, at para 7.59 (where the jury received written instructions “they were almost invariably appreciative”).
209 For an overview of the studies, see Comiskey, supra note 166, at 653-656; Ellsworth and Reifman, supra note 174, at 803-804; Lieberman and Sales, supra note 182, at 626-627.
210 Heuer & Penrod, supra note 196, at 409.
fact) and were asked a number of questions about their experience of jury service.\textsuperscript{212} The researchers also canvassed the views of the judges involved. They found no evidence of any of the potential drawbacks of written instructions. Written instructions made no significant difference to deliberation time\textsuperscript{213} and the judges involved reported that providing them was not burdensome or disruptive.\textsuperscript{214} In terms of the possible advantages, jurors reported that the written instructions were very helpful in settling any disputes that did arise.\textsuperscript{215} The researchers did not, however, find that the written instructions led to any improvement in the comprehension of legal concepts.\textsuperscript{216} Despite this, the researchers concluded that their results presented “a compelling case” for written instructions and that while they might not have all the advantages claimed of them they did have some clear benefits and they had no harmful consequences.\textsuperscript{217}

Heuer and Penrod are not alone in finding that written instructions did not lead to improvements in comprehension,\textsuperscript{218} but other studies have reported improvements. Kramer and Koenig, for example, found that jurors who received written instructions did score better on “true/false” tests aimed at measuring comprehension of a wide range of criminal jury instructions.\textsuperscript{219} Thomas’ research in England and Wales found that the proportion of jurors who were able to answer correctly two questions aimed at testing understanding of a self-defense instruction rose from 31 per cent to 48 per cent when a written instruction was provided,\textsuperscript{220} suggesting that at least some of the incorrect answers were due to failures of memory and not of comprehension.

In fact, a research design that failed to distinguish between memory and understanding may well account for Heuer and Penrod’s finding that written instructions did not improve comprehension. In their study the jurors completed comprehension questionnaires some time after the trial concluded,\textsuperscript{221} a limitation the researchers themselves acknowledge.\textsuperscript{222} Compare this to Kramer and Koenig’s research, where jurors were surveyed immediately after the conclusion of the trial (and where improvements in comprehension were reported). That said, the role that written instructions might play in improving

\textsuperscript{212} Id. at 416.
\textsuperscript{213} The mean reported deliberation time was 2.6 hours for written instructions and 2.7 hours for oral instructions (id. at 421).
\textsuperscript{214} Id. at 423. See similarly Madge, supra note 193, at 822; Leonard B. Sand & Steven Alan Reiss, A Report on Seven Experiments Conducted by District Court Judges in the Second Circuit, 60 N.Y.U. L. REV. 423, 453-456 (1985).
\textsuperscript{215} Heuer & Penrod, supra note 196, at 421.
\textsuperscript{216} Jurors were asked six multiple choice questions aimed at testing their comprehension and the mean correct scores were 6.7 for the written instructions and 6.8 for the oral instructions: see id.
\textsuperscript{217} Id. at 429.
\textsuperscript{218} See Reifman et al., supra note 179, at 549.
\textsuperscript{220} Thomas, supra note 170, at 38.
\textsuperscript{221} The questionnaires were sent out by post after the conclusion of the experiment: see Heuer & Penrod, supra note 196, at 417.
\textsuperscript{222} Id. at 423. To this might be added the issue that the survey was by way of a postal questionnaire to which the response rate was only 69 per cent (id. at 418).
comprehension should not be over-stated. Putting instructions in writing cannot compensate for instructions that are inherently unclear.223 As Lieberman and Sales put it:224

... presenting participants with written versions of unintelligible instructions cannot be expected to be beneficial. If a person does not speak a foreign language, it will not matter if they are given written or verbal instructions in that foreign tongue.

There are some practical issues. A copy of the written instructions should be given to each individual juror - otherwise there is a danger that the person with the written instructions dominates the discussion.225 There is the question of whether they should be provided before the oral instructions are given or afterwards. In favour of the former, this enables jurors to follow them as the charge is being given.226 In favour of the latter, there is the danger that the jury will not focus sufficiently on what is being said if they are distracted by the written copy.227 There is also the issue of juror literacy. It cannot be assumed that all jurors will have levels of literacy that would enable them to read a written text, so provision needs to be made in this respect by, for example, the use of recorded verbal or video instructions.

VII. Conclusion

The danger of wrongful conviction posed by mistaken eyewitness identification is a very real one, with such evidence being identified as the leading cause of wrongful conviction in the two major studies based on DNA exoneration cases. The need for measures to safeguard against wrongful convictions caused by mistaken eyewitness identification can scarcely be debated. Where there lies more disagreement, however, is in the most appropriate method of doing so.

Jury instructions warning jurors of the risks of mis-identification and alerting them to the factors relevant to evaluating eyewitness testimony have often been dismissed as ineffective. It has been demonstrated here, however, that this conclusion is based on a misreading of the relevant experimental evidence. While all of the studies in this area might purport to demonstrate ineffectiveness, they all have methodological flaws and a more informed reading of the studies (and the evidence from related areas) suggests that jury instructions can be effective in educating jurors about the risks associated with eyewitness identification evidence and in evaluating such evidence sensitively. In order to do so, however, they need to be expressed in language that jurors can understand, they need to accurately reflect the relevant scientific evidence, they need to indicate to jurors why they are being given and they should be provided in writing (or in a suitable alternative form to those who have literacy difficulties).

Two further concluding remarks are in order. First, while the focus here has been specifically on the role of jury instructions, the conclusions reached in this regard should not be taken to suggest that they are the only effective method of protecting against wrongful

223 A good example here is the conspiracy instruction used by Rose & Ogloff, supra note 168, where there was no difference in comprehension between those jurors given the oral and the written version.
224 Lieberman & Sales, supra note 182, at 628.
225 Forston, supra note 192, at 620.
227 Madge, supra note 193, at 821.
conviction caused by mistaken eyewitness identification. The important role played during the initial investigation by ensuring that identification procedures are conducted according to best practice guidelines should not be neglected.\(^{228}\) Likewise, in addition to jury instructions, there may also be an important role to be played at trial by exclusionary powers and expert testimony. These three measures should be seen as complementary, not as alternatives.\(^{229}\) Exclusion may be appropriate where identification evidence is so weak that no reasonable jury could find it credible.\(^{230}\) Allowing expert testimony (whether via a court appointed expert or by allowing both parties to lead their own expert witnesses) may be appropriate to deal with special circumstances (for example where an eyewitness is a child or has a learning disability) or where the findings of experimental research are contested (for example in relation to the effect of stress on the accuracy of identification).

Second, this analysis serves as a warning of the dangers of relying on lawyers' understanding of psychological research as a basis for policy formation. There is a danger that the findings of mock jury research are accepted uncritically and without due consideration of what can actually be drawn from them, given the research methodology concerned. With some notable exceptions,\(^{231}\) the community of legal scholars (and indeed some psychologists) has essentially coalesced around a view about the (in)effectiveness of jury instructions that was not supported by a proper reading of the studies. There are lessons for lawyers and psychologists here. While there is undoubtedly much of value to be learned about the design of legal processes from experimental psychology, lawyers need to be informed about the potential limitations of mock jury research and to evaluate critically the external validity of studies before relying on them as the basis for policy decisions. At the same time, if psychologists wish to exert an influence outside their own discipline, there is need to design experiments that reflect legal reality as far as possible and to be sensitive to what their findings imply for real trial processes.

\(^{228}\) See supra, note 50 and accompanying text.
\(^{229}\) On this, see Dufrainmont, supra note 7, at 325; Kossed, supra note 53, at 617.
\(^{230}\) For an argument to this effect, see Baxter, supra note 53, at 176.
\(^{231}\) See supra note 55 and accompanying text.