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SUPREME COURT OF THE STATE OF CONNECTICUT

S.C. # 20727

EDGAR TATUM

v.

COMMISSIONER OF CORRECTION

**BRIEF AND APPENDIX FOR *AMICI CURIAE* THE
INNOCENCE PROJECT AND THE CONNECTICUT
INNOCENCE PROJECT IN SUPPORT OF PETITIONER-
APPELLANT AND URGING REVERSAL**

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STATEMENT OF ISSUES

1. Whether *State v. Guilbert* 306 Conn. 218 (2012) and *State v. Dickson*, 322 Conn. 410 (2016) should apply retroactively on collateral review as watershed rules of criminal procedure? (pages 9-18)
2. Whether the facts of Mr. Tatum's case warrant retroactive application of *Guilbert* and *Dickson*? (pages 18-19)

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STATEMENT OF INTEREST OF AMICI CURIAE¹

The Innocence Project is an organization dedicated to providing pro bono legal and related investigative services to indigent prisoners whose actual innocence may be established through post-conviction DNA evidence. To date, the work of the Innocence Project and affiliated organizations has led to the exoneration of 375 individuals whose post-conviction DNA testing has shown were wrongly convicted. *DNA Exoneration in the United States*, INNOCENCE PROJECT (2022), <https://innocenceproject.org/dna-exonerations-in-the-united-states/>. The Connecticut Innocence Project (CTIP) is a specialized unit of the State of Connecticut Division of Public Defender Services. Like the Innocence Project, the mission of CTIP is to identify, review and investigate cases of wrongful convictions within Connecticut, and obtain the release of wrongly convicted persons from prison. The Innocence Project and CTIP have a compelling interest in ensuring that criminal trials reach accurate determinations of guilt and promote justice. Because wrongful convictions destroy lives and allow the actual perpetrators to avoid taking responsibility for their actions, the amici's objectives help to ensure a safer and more just society.

In addition to its work on individual cases, the Innocence Project also seeks to prevent future wrongful convictions by researching the causes of wrongful convictions and pursuing reform initiatives designed to enhance the truth-seeking functions of the criminal justice system. Nearly 70 percent of individuals exonerated by DNA testing were originally convicted based, at least in part, on the testimony of eyewitnesses who turned out to be

¹ This brief has not been authored, in whole or in part, by counsel to any party in this appeal. No party or counsel to any party contributed money intended to fund preparation or submission of this brief. No person, other than the Amici Curiae, their members or their counsel, contributed money that was intended to fund preparation or submission of this brief.

mistaken. *Id.* The majority of these mistaken eyewitness identification cases involved in-court identifications. *Id.* Thus, inasmuch as mistaken eyewitness identifications are a principal cause of wrongful convictions and in-court identifications are a common feature of these cases, the amici have a compelling interest in the adoption of a legal framework that reduces the risk of a finding of guilt based on an in-court misidentification.

PRELIMINARY STATEMENT

The Innocence Project and Connecticut Innocence Project respectfully submit this amicus curiae brief in support of Defendant-Appellant, urging this Court to apply its prior decisions in *State v. Dickson*, 322 Conn. 410 (2016) and *State v. Guilbert* 306 Conn. 218 (2012) retroactively, and reverse the court below.

First, the court below erred in declining to apply *Guilbert* and *Dickson* retroactively. Those decisions created watershed rules of criminal procedure which implicate the fundamental fairness and accuracy of trials, thus satisfying the federal standard set out in *Teague v. Lane*, 489 U.S. 288 (1989). This argument is supported by an overwhelming body of social science research that demonstrates the unreliability of in-court eyewitness identifications and the danger of misidentifications in leading to wrongful convictions. Further, Connecticut law requires retroactive application of those rules here, regardless of what federal law standards may say about them. The Court has made clear that the due process clause of the state constitution provides more protection than its federal counterpart. Connecticut courts should therefore apply the watershed exception more broadly and find that *Guilbert* and *Dickson* apply retroactively under state constitutional law. Indeed, *Teague* is broadly applied in other jurisdictions. Connecticut should follow suit.

Second, the facts of Mr. Tatum's case are so egregious that they warrant reconsideration under the new rules established by *Guilbert* and *Dickson*. Of the two eyewitnesses in Mr. Tatum's case, one identified someone

other than Mr. Tatum from a photo array and the other could not identify anyone at all prior to the unconstitutional in-court identification. Notably, *Dickson* itself analyzed the identification procedure that took place in *Tatum* and found that it was fundamentally unfair. Justice thus demands the court apply these rules retroactively to Mr. Tatum’s case in particular.

STATEMENT OF FACTS AND PROCEEDINGS

Amici incorporate by reference the facts and procedural history set forth in the Petitioner-Appellant’s brief.

ARGUMENT

I. This Court Should Hold that *Guilbert* and *Dickson* Apply Retroactively

A. Watershed Rules of Criminal Procedure are Applied Retroactively on Collateral Review

Teague v. Lane authorizes this Court to apply the new standards for evaluating eyewitness identification evidence that it adopted in *Guilbert* and *Dickson* retroactively because those standards implicate the fundamental fairness and accuracy of criminal convictions. *See* 489 U.S. 288 (1989). Although new constitutional rules of criminal procedure generally do not apply to cases on collateral review, *Teague* carves out an exception for “watershed rules.” *Id.* at 314. Watershed rules of criminal procedure are those that implicate the “fundamental fairness that is implicit in the concept of ordered liberty” and “without which the likelihood of an accurate conviction is seriously diminished.” *Id.* at 313-15; *see also Thiersaint v. Comm’r of Correction*, 316 Conn. 89, 108 n.8 (2015). Watershed rules include those that “raise the possibility that someone convicted with use of the invalidated procedure might have been acquitted otherwise.” *Casiano v. Comm’r of Correction*, 317 Conn. 52, 63 (2015). This exception should be applied here.

B. *Guilbert* and *Dickson* Introduced a Watershed Rule of Criminal Procedure that Implicates the

Fundamental Fairness and Accuracy of Criminal Proceedings

i. The risk of wrongful convictions stemming from unreliable eyewitness identifications implicates the fundamental fairness and accuracy of criminal proceedings.

State v. Guilbert articulated a watershed rule of criminal procedure under *Teague*. In *Guilbert*, the defendant was convicted of murder, in large part based on the testimony of multiple witnesses identifying him as the shooter. 306 Conn. 218, 220-22 (2012). Mr. Guilbert appealed, arguing that the trial court improperly granted the state’s motion to preclude the defense from introducing expert testimony on the reliability of eyewitness identifications. *Id.* at 220. This Court ruled that the lower court abused its discretion in excluding the defendant’s expert. It held that expert testimony on the reliability of eyewitness identification is admissible where the court determines that the expert is qualified and that her testimony is relevant and helpful to the jury. *Id.* at 226.

More fundamentally, *Guilbert* recognized the threat misidentifications pose to fundamental fairness. The Court emphasized the “near perfect scientific consensus” that an array of variables, not all of which were accounted for by prior decisions of this Court nor necessarily within the knowledge of the average juror, are likely to cause mistaken identifications. *See id.* at 234-36. The Court explained that “the traditional methods for alerting juries to the fallibility of eyewitness identifications—cross examination, closing argument, and generalized jury instructions on the subject” are inadequate. *Id.* at 243. Relying on “scientifically valid studies,” the Court concluded that “mistaken eyewitness identification testimony is by far the leading cause of wrongful convictions.” *Id.* at 248-50.250 n. 30 (*e.g.* erroneous identifications have accounted for up to 86 percent of convictions of

persons ultimately exonerated by DNA testing (citing 42 AM. CRIM. L. REV. 1271, 1275 n.17 (2005)).

In defining the watershed exception, *Teague* emphasized the need to avoid wrongful convictions:

One of the two principal functions of habeas corpus was to assure that no man has been incarcerated under a procedure which creates an impermissibly large risk that the innocent will be convicted . . . all new constitutional rules which significantly improve the pre-existing factfinding procedures are to be retroactively applied on habeas. 489 U.S. at 312 (quoting *Desist v. United States*, 394 U.S. 244, 262 (1969) (Harlan, J., dissenting)). The holding in *Guilbert* represents an effort by this Court to ameliorate the significant risk of inaccurate guilty verdicts caused by eyewitness misidentification. It is thus a watershed rule of criminal procedure. In combatting the effects of misidentifications, the *Guilbert* rule significantly improves factfinding procedures in the interest of fundamental fairness. See *State v. Harris*, 330 Conn. 91, 120 (2018) (“using the *Guilbert* framework would enhance the accuracy of the constitutional inquiry into the reliability of an identification”). Accordingly, *Guilbert* should be applied retroactively under the watershed exception.

ii. In deeming first time in-court identifications unduly suggestive, *Dickson* also serves to prevent wrongful convictions, implicating the fundamental fairness and accuracy of criminal proceedings.

State v. Dickson also introduced a watershed rule of criminal procedure under *Teague* because it, too, went to the heart of the fairness of the proceedings. 322 Conn. 410, 414-15 (2016). In *Dickson*, the in-court identification of the defendant was preceded by an unsuccessful attempt to identify the defendant in a photographic array. *Id.* at 416-17. The *Dickson* Court held: “Any first time in-court identification by a witness who would

have been unable to reliably identify the defendant in a nonsuggestive out-of-court procedure constitutes a procedural due process violation.” *Id.* at 426 n.11. The Court reasoned it was “hard-pressed to imagine how there could be a more suggestive identification procedure than placing a witness on the stand in open court, confronting the witness with the person who the state has accused of committing the crime, and then asking the witness if he can identify the person who committed the crime.” *Id.* at 423-24.

Although the opinion mentioned in a footnote that its holding requiring prescreening of first time in-court identification does not fall within the watershed rule exception, this comment was dicta and should be reconsidered. *See id.* at 451 n. 34. The Court relied on *Beard v. Banks*, 542 U.S. 406, 419-20 (2004) for the proposition that “the fact that a new rule removes some remote possibility of arbitrary infliction of the death sentence does not suffice to bring it within *Teague’s* second exception.” The Court should find that *Beard* is not applicable here. *Dickson’s* holding is not merely an “incremental change,” and it does much more than remove a “remote possibility” of wrongful convictions. As this Court wrote, “[t]he influence of improper suggestion upon identifying witnesses probably accounts for more miscarriages of justice than any other single factor—perhaps it is responsible for more such errors than all other factors combined.” *Dickson*, 322 Conn. at 425-26 (quoting *United States v. Wade*, 388 U.S. 218, 229 (1976)). Given the gravity of this risk, procedural rules preventing such identifications are an absolute prerequisite to fundamental fairness and accuracy in criminal proceedings.

iii. Social science demonstrates that eyewitness identifications are so suggestive, unreliable, and impactful on juries that they undermine notions of fundamental fairness and accuracy in criminal proceedings.

A rich body of scientific research establishes the principle that unreliable identifications, such as those targeted by *Guilbert* and *Dickson*, pose a serious threat to the accuracy of trial outcomes. Indeed, as this Court acknowledged in *Guilbert*, research demonstrates that “mistaken eyewitness identification testimony is by far the leading cause of wrongful convictions.” 306 Conn. at 249; *see also Dickson*, 322 Conn. at 425 (“this court previously has recognized that mistaken eyewitness identifications are a significant cause of erroneous convictions.”). National statistics collected by the Innocence Project show that mistaken eyewitness identifications contributed to at least 69% of the wrongful convictions in the United States overturned by post-conviction DNA evidence. *See* <https://innocenceproject.org/dna-exonerations-in-the-united-states/>. The issue of inaccurate eyewitness identification testimony thus strikes at the heart of whether a criminal proceeding is fair and accurate.

There can be no more fundamental rules than those aimed at reducing the number of wrongful convictions, such as the rules adopted by the *Guilbert* and *Dickson* Courts. *See Guilbert*, 306 Conn. at 249; *Dickson*, 322 Conn. 410. Memory decays over time—quickly, continuously, and permanently—making eyewitness’ memories weak and prone to suggestion. *See* Kenneth A. Deffenbacher et al., *Forgetting the Once-Seen Face: Estimating the Strength of an Eyewitness’s Memory Representation*, 14 J. Experimental Psychol. Applied 139, 139, 143, 147-48 (2008); *State v. Henderson*, 27 A.3d 872, 907 (N.J. 2011). “The pressure of being asked to make an identification in the formal courtroom setting and the lack of anonymity, create conditions under which a witness is most likely to conform his or her recollection to expectations.” Evan J. Mandery, *Due Process Considerations of In-Court Identifications*, 60 Alb. L. Rev. 389, 417-418 (1996). In a criminal proceeding, “eyewitnesses may identify the defendant out of reliance on the prosecutor and in conformity with what is expected of them rather than because their memory is reliable.” *Com. v. Crayton*, 470 Mass. 228, 237 (2014). In fact,

eyewitness identification is “among the least reliable forms of evidence.” See Daniel S. Medwed, *Anatomy of a Wrongful Conviction: Theoretical Implications and Practical Solutions*, 51 Vill.L.Rev. 337, 358 (2006). Given this scientific consensus, the introduction of unreliable eyewitness identifications in criminal proceedings “creates an impermissibly large risk that the innocent will be convicted.” *Teague*, 489 U.S. at 312. Constitutional rules preventing the introduction of this type of unreliable evidence, such as those adopted in *Guilbert* and *Dickson*, therefore are precisely the type of rules that fall within *Teague*’s watershed exception.

C. This Court Should Apply *Guilbert* and *Dickson* Retroactively as a Matter of Connecticut Law

i. This Court’s precedents suggest that *Guilbert* and *Dickson* should be applied retroactively on collateral review.

In *Thiersaint v. Commissioner of Correction*, this Court adopted the *Teague* framework, enabling Connecticut courts to apply procedural rules retroactively where they implicate the fundamental fairness and accuracy of trials. 316 Conn. 89, 113 (2015). The *Thiersaint* Court concluded that it would conduct its own independent analysis of *Teague* rather than follow federal precedent, thus enabling courts to opt for a broader application of the rule where appropriate. See *id.* Such an application of *Teague* is warranted here because the rules from *Guilbert* and *Dickson* are watershed rules of criminal procedure that should apply on collateral review.

In *Casiano v. Commissioner of Correction*, 317 Conn. 52 (2015), this Court applied *Teague* and found that a new rule announced in *Miller v. Alabama*, 567 U.S. 460 (2012), constituted a watershed rule of criminal procedure. As a result, the Court held that the rule applied retroactively to petitioner’s collateral state habeas proceeding. *Casiano*, 317 Conn. at 1031. The Court’s reasoning was that the rule was “central to an accurate

determination” that the sentence imposed was fair and proportionate. *Id.* at 1042.

Likewise, the rules in *Guilbert* and *Dickson* are both “central to an accurate determination” of Mr. Tatum’s innocence and “implicate the fundamental fairness of a trial.” *See id.* at 1402; *Teague*, 489 U.S. at 311-13. The *Guilbert* and *Dickson* rules should be applied here because they “raise the possibility that someone convicted with use of the invalidated procedure might have been acquitted otherwise.” *Casiano*, 317 Conn. at 63; *see also State v. McCleese*, 333 Conn. 378 (2019) (Ecker, J., dissenting) (finding that failing to apply *Casiano* would result in a “miscarriage of justice” because “[t]he *Casiano* watershed designation and its constitutional entailments cannot be ignored[.]”). *Id.* at 431, 469. Proper identification procedures are, at their core, central to an accurate verdict in a case involving eyewitness identification evidence. Ignoring such issues because they arise on collateral review ignores the prior efforts of this Court, made in the name of fundamental fairness, to get this central issue right.

ii. Connecticut courts should apply the watershed exception more broadly than federal courts.

This Court’s most recent decision regarding the proper assessment of eyewitness identification evidence further reinforces the idea that *Guilbert* and *Dickson* should be applied retroactively here. In *State v. Harris*, 330 Conn. 91, 96 (2018), this Court endorsed the *Guilbert* framework and modified the existing test for assessing eyewitness identifications “to conform to recent developments in social science and the law.” *Id.* at 115. *Harris*, a 2018 case coming after *Dickson*, held that the due process guarantees of the Connecticut Constitution provide broader protection than the federal constitution with respect to the admissibility of eyewitness identification testimony. The decision made clear that a procedure that is constitutional by federal standards may still be impermissible under state law. These same

principles that animated the *Harris* decision argue for the retroactive application of *Guilbert* and *Dickson* here. Fundamental fairness requires nothing less.

Several recent dissents urge this same result. In *Thiersaint*, Justice Eveleigh and Justice McDonald disagreed with the majority’s conclusion that *Padilla v. Kentucky*, 559 U.S. 356 (2010) did not apply retroactively to the defendant. *Thiersaint*, 316 Conn. 89 (2015). The dissent reiterated that the Court is “not bound by a strict adherence to *Teague*” and thus need not apply its exact framework to state habeas proceedings. *Id.* at 165. After examining the approaches taken by the Nevada and Idaho Supreme Courts, the dissent argued that this Court “should exercise independent judgment on the basis of the unique requirements of our state constitution, judicial precedents and statutory framework.” *Id.* at 168. The same logic applies here.

Likewise, the dissent in a recent case from the Supreme Court of Louisiana, *State v. Reddick*, also calls for a holistic approach to *Teague*. See 351 So. 3d 273, 292–94 (La. 2022) (Griffin, J., dissenting). Injustices from the past call for a remedy from the Court, and the notion that constitutional rights only matter to correct *future* injustices is illogical. See *id.* Further, “[t]he imperative to correct past injustices manifest in the deprivation of a constitutionally guaranteed right should not cede to reliance interests and administrative concerns[,]” and courts should not “perpetuate something we all know to be wrong only because we fear the consequences—and costs—of being right.” *Id.* at 297.

iii. Other jurisdictions also apply *Teague* broadly.

Just as this Court has on occasion taken a broader approach to the scope of *Teague*’s watershed exception, so too have the highest courts of other states. Indeed, it makes sense that states would take this approach to the watershed exception because *Teague*’s two driving justifications—comity and finality—have little force in the state system. See *Hughes v. State*, 901 So.2d

837, 863 (Fla. 2005) (Anstead, J., dissenting); *see also* Mary C. Hutton, *Retroactivity in the States: The Impact of Teague v. Lane on State Postconviction Remedies*, 44 Ala. L. Rev. 421, 436–37 (1993); Christopher N. Lasch, *The Future of Teague Retroactivity, or Redressability, after Dafforth v. Minnesota: Why Lower Courts Should Give Retroactive Effect to New Constitutional Rules of Criminal Procedure in Postconviction Proceedings*, 46 Am. Crim. L. Rev. 1, 34, 57 (2009). That is because in state collateral review, there is no fear of the federal system encroaching on the state system; it is also less complicated and disruptive to disturb a final conviction. *See* Hutton, *supra*, at 443–44, 449; Lasch, *supra*, at 57. Thus, with the significance of comity and finality diminished in the context of state collateral review, “get[ting] it right” is of heightened importance, and getting it right requires a broader approach to the watershed exception. *See Hughes*, 901 So.2d at 863.

Following these principles, other states have taken a broader approach to the watershed exception than that laid out by the U.S. Supreme Court. For example, in *Powell v. Delaware*, 153 A.3d 69 (Del. 2016), the Delaware Supreme Court retroactively applied a new state constitutional rule changing capital sentencing procedures in death penalty cases. *Id.* at 70–71. This rule fell within *Teague*’s watershed exception because the rule contributed to the reliability of the fact-finding process, and, without it, the likelihood of an accurate sentence was “seriously diminished.” *Id.* at 74–75.

Similarly, in both *Alford v. State*, 695 S.E.2d 1 (Ga. 2010) and *Talley v. State*, 640 S.E.2d 878 (S.C. 2007), courts retroactively applied the rule in *Alabama v. Shelton*, 535 U.S. 654 (2002), which extended the right to counsel to defendants who receive a suspended sentence, because it was a “bedrock procedural right.” *See Alford*, 695 S.E.2d at 4; *Talley*, 640 S.E.2d at 543–44. Reasoning that the right to counsel is a “bedrock procedural right” that implicates the fundamental fairness of a proceeding, both courts concluded that the rule was watershed and thus retroactively applicable on collateral review. *Alford*, 695 S.E.2d at 4; *Talley*, 640 S.E.2d at 544.

In *State v. Zuniga*, 444 S.E.2d 443 (N.C. 1994), the Supreme Court of North Carolina concluded that a new rule invalidating the state’s capital sentencing unanimity requirement (which requires capital juries to find unanimously that mitigating circumstances existed before they could be considered at sentencing) was watershed and thus retroactively applicable. *See id.* at 444. The court based this conclusion on the fact that the rule was a bedrock procedural element that is implicit in the concept of ordered liberty. *Id.* at 446–47 (quoting *Williams v. Dixon*, 961 F.2d 448, 456 (4th Cir. 1992)).

Connecticut should follow the approach taken by other states in broadly applying *Teague*’s watershed exception, to ensure the fundamental fairness of criminal proceedings in this State. In so applying the watershed rule, the Court should find that *Guilbert* and *Dickson* introduced watershed rules of criminal procedure that apply on collateral review.

II. The Facts of Mr. Tatum’s Case Warrant Retroactive Application of *Guilbert* and *Dickson*

Setting aside that *Guilbert* and *Dickson* articulated generally applicable watershed rules of criminal procedure, the facts of Mr. Tatum’s case independently warrant retroactive application of *Guilbert* and *Dickson* here. The identification procedures used in Mr. Tatum’s case were fundamentally unfair. Of the two eyewitnesses in Mr. Tatum’s case, one identified a different person from a photo array and the other could not identify anyone at all prior to the unconstitutional in-court identification. Given the supreme unfairness of allowing first time in court identification of Mr. Tatum by two witnesses who had been unable to identify him in any non-suggestive procedure, the Court should apply *Guilbert* and *Dickson* retroactively to Mr. Tatum’s case.

Indeed, in *Dickson*, this Court analyzed the first time in-court identification procedure that took place in the trial court and found that it was “unfair.” *Dickson*, 322 Conn. at 436. The Court found that: “The state is not entitled to conduct an unfair procedure merely because a fair procedure

failed to produce the desired result.” *Id.* The *Dickson* Court specifically referenced the absurdity of the holding in *Tatum*, stating that “it would be absurd to conclude that the state can simply decline to conduct a nonsuggestive procedure and then claim that its own conduct rendered a first time in-court identification necessary, thereby curing it of any constitutional infirmity.” *Id.* at 435-36. This Court has thus previously recognized the fundamental unfairness of the procedure used in Mr. Tatum’s case, a procedure that undermined the accuracy of his trial. Justice calls for retroactive application of *Guilbert* and *Dickson* to Mr. Tatum’s case.

CONCLUSION

We respectfully urge this Court to reverse the judgment of the Appellate Court and apply *Guilbert* and *Dickson* retroactively to correct this due process violation.

Respectfully submitted,

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Forgetting the Once-Seen Face: Estimating the Strength of an Eyewitness's Memory Representation

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The fidelity of an eyewitness's memory representation is an issue of paramount forensic concern. Psychological science has been unable to offer more than vague generalities concerning the relation of retention interval to memory trace strength for the once-seen face. A meta-analysis of 53 facial memory studies produced a highly reliable association ($r = .18$, $d = 0.37$) between longer retention intervals and positive forgetting of once-seen faces, an effect equally strong for both face recognition and eyewitness identification studies. W. A. Wickelgren's (1974, 1975, 1977) theory of recognition memory provided statistically satisfactory fits to 11 different empirical forgetting functions. Applied to the results of field studies of eyewitness memory, the theory yields predictions relevant to fact finders' evaluations of eyewitness credibility. A plausible upper limit for witness initial memory strength corresponds to a probability of .67 of being correct on a fair six-person lineup. Furthermore, not only can the percentage of remaining memory strength be determined for any retention interval, but this strength estimate can be translated into an estimated probability of being correct on a fair lineup of a specified size.

Keywords: eyewitness memory strength, forgetting of faces, retention interval, single-trace fragility theory

Unless the state possesses incriminating physical evidence, eyewitness identification testimony is crucial whenever the prosecution attempts to prove that the defendant and the perpetrator are one and the same. The reliability of an identification is affected by two classes of variables, system variables and estimator variables (Wells, 1978). System variables are those under the control of the criminal justice system, instructions given to eyewitnesses before they consider a lineup or photospread or the method by which members of the lineup other than the suspect are chosen, for instance. Estimator variables are those beyond the control of the criminal justice system and whose effects can only be estimated. These factors include, among many other estimator variables, the duration of the eyewitness's exposure to the perpetrator, lighting conditions at the crime scene, and retention interval, the length of the interval between observation of the suspect and test of the eyewitness's memory.

Given the controllability of system variables, a considerable amount of research has been focused on them, given the greater

promise that such research would lead to increases in the reliability of eyewitness memory testing procedures. Indeed, sufficient research progress on system variables had accumulated in the last 2 decades of the 20th century that the U.S. Department of Justice issued guidelines for the collection of eyewitness evidence based on these findings (Technical Working Group for Eyewitness Evidence, 1999). Police in a number of jurisdictions around the United States have already adopted these guidelines as standard practice.

Progress in producing forensically useful empirical generalizations has not been nearly as great in the case of estimator variables. Nevertheless, research on these variables may have the potential to produce not only greater understanding of situations in which eyewitnesses may experience perceptual or memorial problems but also to yield empirical generalizations that may assist the trier of fact (judge or juror) when he or she must assess the fidelity of an eyewitness's memory representation (cf. Wells, Memon, & Penrod, 2006).

In making this assessment, the key estimator variables are initial memory strength for the perpetrator's face and length of the retention interval. Many other estimator variables have their effect only as they affect initial memory strength. These variables include duration of exposure to the perpetrator, illumination conditions, presence or absence of other foci of attention (e.g., a weapon), eyewitness stress level, and whether the perpetrator is of a different race, among others. To make a proper assessment, the trier of fact would not only need to have an estimate of the witness's initial memory strength for the perpetrator and to know the length of the retention interval but also to understand the nature of the forgetting

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An earlier version of the meta-analysis included in this article and curve fits for two of the empirical forgetting functions included in Table 2 were presented at the Off the Witness Stand: Using Psychology in the Practice of Justice conference held March 1–3, 2007, in New York, NY.

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function for the human face. The forgetting function, of course, is the curve that specifies the strength of the memory representation over the retention interval. That is, the forgetting function specifies how rapidly memory strength, plotted on the ordinate of the graph, decreases as a function of time, plotted on the abscissa. Knowing the rate of memory strength loss and retention interval length allows one to specify the proportion of original memory strength remaining. To specify in absolute terms how much memory strength remains, one must know the initial memory strength, the “starting point” on the ordinate of the forgetting function.

Typically, the length of the retention interval for an eyewitness can easily be established to a reasonable degree of precision from information provided in police reports. Until now, however, psychological science has not had a means to provide at least a ballpark estimate of initial memory strength for a witness. Furthermore, cognitive psychologists have not established whether the forgetting curve for the human face is even of the same form as Ebbinghaus (1913) had determined. For that matter, it has not always been abundantly clear whether there even is a statistically reliable association between retention interval length and facial recognition memory (Deffenbacher, 1986). For example, in the period between 1970 (approximately the beginning of modern research on eyewitness testimony) and 1985, studies testing the effect of retention interval length on memory for the human face included a substantial minority reporting a null effect. An initial meta-analysis of this literature by Deffenbacher (1986) included 15 studies reporting a null effect out of a total of 33 studies, even though overall he found a highly reliable effect ($p < .0001$) of memory test delay on face recognition memory: The average z was 1.46, yielding a meta-analytic Z of 8.38 and an equivalent correlation of .25 (as retention interval or delay increased, forgetting increased). Including retention interval as part of a much more comprehensive meta-analysis than Deffenbacher’s, Shapiro and Penrod (1986) also documented statistically reliable effects of retention interval length on face recognition memory.

With results of these previous meta-analyses in hand, an immediate attempt to describe the forgetting function for once-seen faces might seem in order. However, there are good reasons to conduct a more up-to-date meta-analysis of face memory studies before searching for a suitable theoretical forgetting function. In the more than 2 decades since 1986, the published body of research findings concerning the effect of delay has increased by more than 60%. The number of null or negative (“negative” forgetting or reminiscence) results has also continued to increase.

A further concern is the proportion of studies that have been conducted in the context of the eyewitness identification paradigm rather than with the standard face recognition task in the tradition of cognitive psychology. The eyewitness identification paradigm usually exposes witnesses to a single target face (perpetrator) in a scripted scenario. Memory for this face is tested by embedding it in a 5- to 9-person live or photo lineup (target present) or by substituting someone else who is a match to the perpetrator’s description (target absent). Witnesses are asked to identify the perpetrator or to indicate that he or she is not present. The recognition memory task, on the other hand, exposes observers to a relatively large number of target faces. A recognition memory test typically includes the target set plus an equal number of unfamiliar distracter faces. Observers are exposed to faces serially and are to respond “yes” or “no” as to whether a given face has been seen

previously. It turns out that the proportion of eyewitness identification studies has more than doubled, increasing from 27% of published studies concerned with the effect of delayed memory test in Deffenbacher’s (1986) meta-analysis to 57% at present.

As a result, not only has there been a considerable increase in the proportion of studies with greater forensic applicability, but it is entirely possible that the effect size for retention interval could be different for eyewitness identification studies than for face recognition studies. Consider one possibility. Results of face recognition memory studies have typically been assumed to represent high estimates of the amount of facial memory obtaining in real-world eyewitness identification settings. If eyewitness identification studies did indeed produce lower estimates of initial memory strength than did face recognition memory studies, then there would be less room for the decline of any forgetting function to occur. Thus, retention interval effects might be less for studies in the eyewitness identification paradigm because of the greater probability of a restriction of range in possible loss of memory strength, as compared with face recognition studies. On the other hand, the direction of a difference in the effect size for retention interval could well be in the opposite direction. A number of published meta-analyses of the effects of other independent variables have yielded generally larger effects on memory for eyewitness identification studies than for laboratory face recognition studies. For instance, Deffenbacher, Bornstein, Penrod, and McGorty (2004) found a considerably larger negative effect of heightened stress on memory for witnesses in studies conducted in the more forensically relevant eyewitness identification tradition than for witnesses in face recognition studies.

For all these reasons, before attempting a theoretical description of the forgetting function for face memory, we deemed it advisable to conduct an up-to-date meta-analysis of the effects of retention interval on the strength of a witness’s memory representation for the once-seen face. We next present the methodology followed and the results obtained from this meta-analysis.

Meta-Analysis of Retention Interval Effects

Method

Sample characteristics. Clark (2005), Deffenbacher et al. (2004), and Reisberg and Heuer (2007) have all agreed that the legal standards for proffered scientific testimony established in *Daubert v. Merrell Dow Pharmaceuticals* (1993) have strengthened the legal system’s preference for meta-analytic conclusions based on a body of well-conceived, well-executed, and easily retrievable studies. Hence, we made the decision to include only published studies in our sample. A thorough search of relevant citation retrieval systems was made. These systems included PsycINFO, Medline, and Social SciSearch (the Social Science Citation Index). We also examined the citations in published research and in social science convention proceedings. The present study sample consists of 39 published articles, books, and book chapters. These sources, listed in Table 1, generated 53 independent tests ($N = 5,405$) of the hypothesis that longer retention intervals have a negative effect on memory strength for the once-seen face. Individual sample sizes ranged considerably, from a low of 8 to a high of 590 ($M = 101.98$). Retention intervals associated with these studies ranged from 1 s to 350 days.

Table 1
Effect Sizes for Proportion Correct Recognition Memory or Identification Accuracy

| Study | <i>n</i> | RI | <i>z</i> | <i>r</i> |
|-------------------------------------------------|----------|----------|----------|----------|
| Scapinello & Yarmey (1970) | 40 | 20 min | 0.00 | .00 |
| Smith & Nielsen (1970) | 144 | 10 s | 3.30 | .28 |
| Wallace et al. (1970) | | | | |
| Children | 200 | 5 min | 0.00 | .00 |
| Adults | 200 | 5 min | 0.00 | .00 |
| Goldstein & Chance (1971) | 52 | 2 days | 0.00 | .00 |
| Shepherd & Ellis (1973) | 36 | 35 days | 1.92 | .32 |
| Laughery et al. (1974) | 292 | 1 week | 0.00 | .00 |
| Chance et al. (1975) | 144 | 2 days | 0.00 | .00 |
| Egan et al. (1977) | 86 | 54 days | 1.65 | .18 |
| Davies et al. (1978) | 40 | 19 days | 1.96 | .31 |
| Walker-Smith (1978) | 8 | 19 s | 2.58 | .91 |
| Yarmey (1979) | 84 | 30 days | 2.32 | .25 |
| Ellis et al. (1980) | 48 | 1 week | 2.58 | .37 |
| Courtois & Mueller (1981) | 128 | 28 days | 2.76 | .24 |
| Deffenbacher et al. (1981): Control: 2 min/2 wk | 22 | 2 weeks | 1.96 | .42 |
| Krouse (1981) | 76 | 2.5 days | 2.58 | .30 |
| Mauldin & Laughery (1981) | 100 | 47.5 hr | 0.00 | .00 |
| Barkowitz and Brigham (1982) | 237 | 1 week | 2.58 | .17 |
| Brigham et al. (1982) | 88 | 22 hr | 5.65 | .60 |
| Shepherd et al. (1982) | | | | |
| Experiment 2 | 40 | 343 days | 3.58 | .57 |
| Experiment 3 | 104 | 90 days | -0.72 | -.07 |
| Krafka & Penrod (1985) | | | | |
| TP/no context | 24 | 22 hr | -0.22 | -.04 |
| TP/context | 20 | 22 hr | 0.45 | .10 |
| TA/no context | 21 | 22 hr | 2.16 | .47 |
| TA/context | 20 | 22 hr | 1.41 | .32 |
| Cutler et al. (1986): Experiment 2 | 287 | 23 days | 2.75 | .16 |
| Chance & Goldstein (1987) | 59 | 5 days | 0.00 | .00 |
| Cutler et al. (1987a) | 165 | 1 week | -1.60 | -.12 |
| Cutler et al. (1987b) | 290 | 12 days | -0.47 | -.03 |
| Peters (1988) | 212 | 6 days | 0.00 | .00 |
| Read et al. (1989) | | | | |
| Early rehearsal | 68 | 1 week | 2.63 | .32 |
| Late rehearsal | 68 | 1 week | -1.30 | -.16 |
| Ellis & Flin (1990) | 153 | 1 week | 1.96 | .16 |
| Podd (1990) | 90 | 2 weeks | 1.75 | .18 |
| Read et al. (1990) | 90 | 100 min | 0.00 | .00 |
| Goodman et al. (1991) | 48 | 4.5 days | 1.04 | .15 |
| Peters (1991) | | | | |
| Experiment 1 | 71 | 26 days | 0.00 | .00 |
| Experiment 3 | 64 | 13 days | 0.00 | .00 |
| Shepherd et al. (1991) | 96 | 1 month | 1.96 | .20 |
| Wixted & Ebbesen (1991): Experiment 2 | 195 | 2 weeks | 2.81 | .20 |
| Yarmey et al. (1996) | | | | |
| TP:5 min/24 hr | 69 | 24 hr | 1.45 | .17 |
| TA: 5 min/24 hr | 76 | 24 hr | 0.82 | .09 |
| Sh/TP: 5 min/24 hr | 69 | 24 hr | 1.29 | .16 |
| Sh/TA: 5 min/24 hr | 70 | 24 hr | 3.05 | .36 |
| Peters (1997): Experiment 2 | 96 | 6 months | 3.00 | .31 |
| MacLin et al. (2001) | 64 | 30 min | 1.34 | .17 |
| Memon et al. (2003) | | | | |
| Older adults: TP | 45 | 1 week | 1.86 | .28 |
| Younger adults: TP | 42 | 1 week | 0.40 | .06 |
| Older adults: TA | 42 | 1 week | 3.75 | .58 |
| Younger adults: TA | 42 | 1 week | -0.97 | -.15 |
| Yarmey (2004) | 590 | 4 hr | 0.00 | .00 |
| Brewer et al. (2006) | | | | |
| TP | 37 | 30 min | 0.00 | .00 |
| TA | 66 | 30 min | 0.00 | .00 |

Note. RI = length of delay between shortest and longest retention intervals. TP = target present lineup; TA = target absent lineup; Sh = showup.

Statistical procedures. As we always compared the longest and shortest retention intervals in each study to determine effect size, we selected z scores for a difference between proportions as the primary dependent measure. For the studies in our sample, a z score for a difference between proportions was occasionally reported or, more often, could be calculated post hoc. In instances in which a test of the hypothesis was reported as not statistically significant, but no statistics were cited, we followed the conservative procedure of entering a z of zero (Rosenthal, 1995). Otherwise, we entered a z score associated with the p value of the effect size estimate, 1.65 for $p = .05$, one-tailed, for instance.

To test the statistical reliability of any estimate of typical effect size, we calculated a one-sample t test and an associated 95% confidence interval (Rosenthal & DiMatteo, 2002). Given that r and d are more frequently encountered measures of effect size, and in the case of r , may be a more generally useful measure, we have reported mean effect sizes in terms of r and d as well. In the case of r , we first converted the z -score measures of effect size for each study to r by dividing z by the square root of n , a conversion formula recommended by Rosenthal and DiMatteo (2002). Each of these biserial correlation coefficients between retention interval (short or long) and memory accuracy was then normalized by conversion to the equivalent Fisher's z' score before averaging. Values of Cohen's d equivalent to the mean effect size expressed in terms of r were obtained by use of the expression $d = 2r(1 - r^2)^{-1/2}$.

Results and Discussion

For each study, we subtracted the proportion correct associated with the longer retention interval from the proportion correct associated with the shorter retention interval. Thus, a positive result represented positive forgetting, a loss of memory. A negative result represented negative forgetting, or reminiscence. When we report effect size in terms of r , then a positive r means that longer retention intervals were associated with more forgetting.

The unweighted mean r was .18, significantly different from zero, $t(52) = 4.78$, $p < .005$, with the 95% confidence interval (CI) extending from .10 to .26. The mean effect size for r in this instance is equivalent to $d = 0.37$, a small to medium effect size (Cohen, 1988). We should note that when all possible pairwise comparisons of retention intervals in studies that had more than two retention intervals were treated as independent effect sizes, the sample size increased from 53 to 78, and the mean effect size was .17 ($d = 0.34$), remarkably close to the results we obtained when only the longest and shortest retention intervals were compared.

We next applied a test of homogeneity of variances across the sample of weighted effect sizes to determine whether the degree of variability exceeded that expected on the basis of sampling error alone. A chi-square value of 23.19 ($df = 52$, $p > .05$) indicated that the degree of variability did not exceed that expected on the basis of sampling error. Strictly speaking, then, no moderator analyses were required. However, given our prediction that studies conducted in the context of the eyewitness identification paradigm might well show more or even less of an effect of retention interval on memory strength as compared with face recognition studies, we

nevertheless calculated mean effect sizes across 23 face recognition memory studies and 30 eyewitness identification studies. In the former case, the mean r was .21, $t(22) = 3.18$, $p < .005$, 95% CI = .08–.34; in the latter case, it was .16, $t(29) = 3.58$, $p < .005$, 95% CI = .07–.25. However, the difference between these two correlations was not significant by a two-sample t test, $t(51) = .21$, $p > .05$. Hence, nature of the research paradigm was not a moderator of average effect size.

Post hoc, it was suggested to us that a particularly strong moderator of the effect size for delay might be the duration of delay itself. In the third column of Table 1 (RI), we have included the length of delay between the shortest and longest retention intervals for each study in our sample. Noting that the most commonly encountered delay for British police has been a month (Pike, Brace, & Kynan, 2002), we estimated the average r to be .27, $t(6) = 3.39$, $p < .01$, 95% CI = .08–.44, for the seven studies with delays of a month or more. For studies with lesser durations of delay, we estimated the average r to be .17, $t(45) = 3.99$, $p < .005$, 95% CI = .08–.25. The difference in magnitude of these two correlations suggests that duration of the memory test delay itself might moderate effect size. This conjecture cannot be supported, however, because the difference between effect sizes at shorter and longer durations of delay was not significant, $t(51) = .38$, $p > .05$. Even so, it is interesting to note that the upper bound of the confidence interval for the studies with a maximum delay of a month or more was .44, as compared with a comparable figure of .25 for studies with a maximum delay of less than a month.

Thus, despite 22 of the sample of 53 effect sizes being null or negative, we have found a statistically reliable effect size estimate for the effect of retention interval on proportion of correct recognition judgments, a measure of memory accuracy for the human face. Furthermore, our effect size estimate does not vary as a function of whether it is a product of studies done in the face recognition memory paradigm or of studies conducted in the eyewitness identification tradition. Hence, it is reasonable to conclude that increased delay of a test for recognition memory for the once-seen face portends decreased probability of correct recognition judgments. This decreased probability presumably reflects loss of underlying memory trace strength.

Our estimate of the effect size for retention interval on memory for faces is also likely an underestimate of the actual value. The 28% of studies reporting forgetting effects that were not statistically significant but for which no statistics were cited resulted in our entering a conservative value of $z = 0.00$ in each instance. Most likely a small but positive amount of forgetting was actually exhibited by participants in such studies.

Our meta-analyses put us in a better position to specify what happens over time to a person's memory representation for an unfamiliar face. At least now we can say with some assurance that memory strength will be weaker at longer retention intervals than at briefer ones. However, our meta-analyses do not permit us to specify the shape of the forgetting function and answers to related questions, such as whether the memory representation will ever be truly lost, much less when. To address these questions, we would need to be able to specify a theoretical forgetting function that would satisfactorily fit empirical forgetting functions, particularly for studies in which facial recognition memory was tested at three or more retention intervals. The latter requirement would enable us to assess fit to nonlinear functions.

Finding a Theoretical Forgetting Function for the Human Face

Criteria

As indicated earlier, the trier of fact has had no useful way to estimate the initial strength of an eyewitness's memory representation for the once-seen face. Clearly, for it to have forensic applicability, any candidate theoretical forgetting function must (a) be able to provide an estimate of initial memory strength; (b) be accurate at predicting where future points will fall as retention interval increases, a strong test of the theory (Wixted & Carpenter, 2007); and (c) be able to satisfactorily fit group forgetting data, the form in which empirical forgetting functions exist in studies of memory for the human face included in our meta-analyses.

If a theoretical forgetting function is found that meets these criteria, eyewitness memory researchers should finally have evidence bearing directly on their belief that the forgetting function for the once-seen face is Ebbinghausian in nature. That is, 93% of experts in the field of eyewitness testimony research, when surveyed most recently (Kassin, Tubb, Hosch, & Memon, 2001), agreed that there was a research basis for the notion that the rate of memory loss for an event is greatest right after an event and then levels off over time. A still large majority (83%) of these same experts agreed that this generalization was reliable enough for psychologists to present in courtroom testimony. There has been little direct evidence provided to date, however, that the faith of these experts is justified when it comes to specifying the forgetting function for the once-seen human face. Consider the critique provided by Elliott (1993):

The Ebbinghaus forgetting curve . . . is another dubious metaphor for most eyewitness circumstances, both because the human face seems to have special properties as a stimulus, and because the retention intervals that are pertinent to identification scarcely ever include the very short ones where most forgetting presumably occurs. There is now a large enough number of results that are null or negative with respect to the Ebbinghaus hypothesis that their presence ought certainly to form part of any testimony that might be given: They should no longer be treated simply as error. (p. 429)

Selection of a Theory of Forgetting

The only theory meeting the first criterion for forensic applicability, provision of an estimate of initial memory strength at 0 s after stimulus cessation, is Wickelgren's (1972, 1974, 1975, 1977, 1979) single-trace fragility theory of recognition memory. Thus, Wickelgren's theory is the only one that we evaluate for its ability to meet the remaining two criteria. In its least complex version (Wickelgren, 1975, 1977), the form of the retention function is $m = Lt^{-D}e^{-It}$, where m represents memory strength at a given retention interval, t seconds after target stimulus exposure has ended; L is initial memory strength at 0 s after stimulus exposure ends; D is the rate parameter for a time-decay process, which is inversely proportional to the rate of memory consolidation; and I is the rate parameter for the loss of memory strength due to interference, which is directly proportional to the similarity of the target stimulus to subsequently encountered stimuli. Of course $e = 2.72$, the base of the natural or Napierian system of logarithms. It is important to note that Wickelgren (1974) proposed that at least for recognition memory, an interval-scale measurement of memory

strength (d') is possible by making relatively weak, yet plausible assumptions concerning how statistical decision theory would translate strength into yes–no decisions. For all practical purposes, then, both m and L are measured in terms of d' units.

Wickelgren's (1972, 1974, 1975, 1977, 1979) theory is unique in that rather than distinguishing between short- and long-term traces, it posits a single memory trace and two mechanisms productive of forgetting. An interference-free, time-decay process produces rapid forgetting in the first seconds and minutes of the retention interval because initially trace fragility is very high. As the neurophysiological process of consolidation begins to decrease trace fragility, however, the rate of forgetting slows in a negatively accelerated fashion, and less is forgotten per unit time. Consolidation, showing a negatively accelerated increase over time, is assumed to continue to decrease trace fragility and its susceptibility to the time-decay process throughout the life of the memory trace. The negative power component of Wickelgren's forgetting function, t^{-D} , would appear to be a plausible model of the negatively accelerated loss of trace fragility over time and therefore the continually decreasing amount of trace strength lost per unit time.

As the contribution of the time-decay process to forgetting declines in power function fashion with increases in the retention interval, the second process, a storage interference process, operating in a negative exponential fashion (e^{-It}), would be expected to increase its influence on the rate of forgetting at longer retention intervals. This prediction might explain a result noted by Deffenbacher, Carr, and Leu (1981), who found that for recognition memory of both faces and words, the amount of forgetting due to retroactive interference with an item's trace in storage increased over a 2-week retention interval.

We should note that a simpler version of Wickelgren's (1972, 1974, 1975, 1977, 1979) theory has been proposed (e.g., Wixted & Carpenter, 2007). This version, in effect, contains only two free parameters, initial memory strength and the rate of forgetting due to time decay. The version we have selected (Wickelgren, 1974, 1975, 1977) contains a third parameter, rate of forgetting due to interference generated subsequent to encoding of the stimulus. It would be prudent to justify the necessity of the additional free parameter.

We have found it necessary to retain the interference parameter to secure an adequate fit to empirical forgetting functions that included retention intervals greater than 1–2 weeks in length. The two-parameter version provides about the same degree of fit as the three-parameter one for retention intervals up to this length. At longer intervals, however, face recognition memory appears to require a source of forgetting in addition to time decay. Values of the time-decay parameter sufficient for a good fit at shorter intervals were not sufficient to account for the considerable additional forgetting at longer intervals. Indeed, a plot of log memory strength (d') against log time reveals a downward inflection in empirical forgetting functions that occurs between an interval corresponding to about 1 week and ones corresponding to a month or more (Deffenbacher, 1986). Interestingly, Valentine, Pickering, and Darling (2003) found in their analysis of 314 lineups conducted by the London Metropolitan Police that the probability of identifying the suspect decreased drastically in the interval between 1 week and 1 month, declining from .66 to .34. Finally, face recognition in a forensic context often includes an institutional source of interference subsequent to encoding of the perpetrator's

face, the exposure of the witness to mugshots before a memory test by means of a live or photographic lineup (Deffenbacher, Bornstein, & Penrod, 2006). This sort of interference could be problematic if the later lineup were a target-absent one.

Previous Tests of the Theory

In the first decade after the introduction of the single-trace fragility theory of forgetting in the 1970s, a modest amount of empirical support was generated. For instance, Wickelgren (1972, 1974, 1975) found that the theory provided an excellent fit to forgetting functions obtained for episodic memory representations for frequently encountered words and for pictures of commonly encountered objects. In the three publications just cited, Wickelgren reported a dozen experiments resulting in 35 separate r^2 statistics, averaged across 3–10 research participants in each instance. The median r^2 was .89, the proportion of empirical forgetting function variance accounted for by single-trace fragility theory. All these experiments used yes–no recognition memory tasks, with memory for verbal and pictorial materials being tested under a variety of conditions and measured at retention intervals up to 2 years in length.

To the best of our knowledge, there have been only two previously published attempts to fit any theory of forgetting to face recognition memory forgetting functions. Deffenbacher (1986) not only conducted a meta-analysis of memory for the once-seen face as a function of retention interval but also conducted a preliminary test of the ability of Wickelgren's (1972, 1974, 1975, 1977, 1979) single-trace fragility (power-exponential) theory to fit empirical forgetting functions for face recognition memory. He found that

Wickelgren's power-exponential theory provided relatively good fits to the functions from five different studies. Wixted and Ebbesen (1991, Experiment 2) showed that a simple power function was an excellent fit to their empirical forgetting function for face recognition memory tested at retention intervals ranging from 1 hr to 2 weeks in length.

Unfortunately, except for the single effort of Wixted and Ebbesen (1991, Experiment 2), neither Deffenbacher nor anyone else ever followed up these first curve-fitting forays with any further theory testing or development in regard to the forgetting of faces. Furthermore, neither Deffenbacher nor anyone else ever made a serious attempt to determine to what extent either Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory or any other theory of forgetting might have forensic application. In the next section, we attempt to remedy the first of these two deficiencies. We remedy the second deficiency in a subsequent section.

New Tests of the Theory

Table 2 illustrates the results of our fitting Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory to 11 empirical forgetting functions obtained from the face recognition memory and eyewitness identification literatures. These 11 data sets were obtained from studies that included at least three retention intervals, that obtained a significant effect for retention interval (positive forgetting), and for which sufficient information was provided to calculate d' values as a measure of memory strength at each of the tested retention intervals. Six of the data sets were

Table 2
Fit of Single-Trace Fragility Theory to Empirical Forgetting Functions

| Study | Observed and (predicted) d' memory strength after various delays | | | | |
|---------------------------------------|--------------------------------------------------------------------|-------------|-------------|-------------|-------------|
| | 0 s | 5 min | 2 days | 7 days | |
| Barkowitz & Brigham (1982) | (1.70) | 1.47 (1.47) | 1.19 (1.24) | 1.14 (1.18) | |
| | 0 s | 10 min | 2 days | 7 days | |
| Chance & Goldstein (1987) | | | | | |
| White faces | (2.49) | 2.12 (2.12) | 1.96 (1.82) | 1.61 (1.72) | |
| Japanese faces | (1.53) | 1.30 (1.30) | 1.02 (1.02) | 0.88 (0.76) | |
| | 0 s | 1 min | 2 days | 28 days | |
| Courtois & Mueller (1981) | (3.26) | 2.94 (2.94) | 2.41 (2.39) | 1.93 (1.95) | |
| | 0 s | 5 min | 1 day | 7 days | |
| Ellis & Flin (1990) | | | | | |
| 7 years/2-s encoding time | (1.13) | 0.98 (0.98) | 0.74 (0.84) | 0.70 (0.72) | |
| 10 years/2-s encoding time | (1.78) | 1.54 (1.54) | 0.98 (1.20) | 0.72 (0.62) | |
| 10 years/6-s encoding time | (1.98) | 1.72 (1.72) | 1.72 (1.47) | 1.23 (1.26) | |
| | 0 s | 3 min | 6 days | 35 days | |
| Shepherd & Ellis (1973) | (1.97) | 1.73 (1.73) | 1.24 (1.28) | 0.78 (0.74) | |
| | 0 s | 7 days | 30 days | 90 days | 350 days |
| Shepherd, Ellis, & Davies (1982) | (2.78) | 1.92 (1.92) | 1.62 (1.64) | 1.47 (1.17) | 0.00 (0.29) |
| | 0 s | 1 hr | 1 day | 1 week | 2 weeks |
| Wixted & Ebbesen (1991, Experiment 2) | (2.47) | 2.01 (2.01) | 1.75 (1.83) | 1.46 (1.57) | 1.41 (1.37) |
| | 0 s | 1 min | 7 days | 30 days | |
| Yarmey (1979) | (3.39) | 3.12 (3.12) | 2.44 (2.30) | 1.47 (1.50) | |

obtained from studies published since Deffenbacher's (1986) preliminary test of Wickelgren's theory of forgetting.

We should note that these 11 functions were of necessity fitted by eye so as to minimize the sum of absolute deviations of predicted and observed values. Least-squares or maximum likelihood estimates of parameter values were not possible, given that each forgetting function contained only three or four retention intervals and that the observed values at each retention interval were group d' scores.

Fortunately, we were able to begin our curve-fitting exercise by taking advantage of parameter values required to fit Wickelgren's (1975) data for frequently encountered English words and Ryback, Weinert, and Fozard's (1970) data for recognition of pictures of common everyday objects. We discovered, however, that the value of the time-decay parameter needed to fit our data for unfamiliar faces was only one-tenth that required for the data by Wickelgren (1975) and Ryback et al. (1970). The same value of the time-decay parameter (.025) provided good fits for 10 of the 11 forgetting curves. A value of .02 improved the fit slightly for the remaining study (Yarmey, 1979). Values of the interference parameter that we used here were up to an order of magnitude smaller (6×10^{-8}) than that required to fit the data of Wickelgren (1975) and Ryback et al. (1970), 6×10^{-7} . The forgetting data from Barkowitz and Brigham (1982), Courtois and Mueller (1981), and Shepherd, Ellis, and Davies (1982) and Chance and Goldstein's (1987) data from Caucasians viewing Caucasian faces were fit with the 6×10^{-8} value of the interference parameter, and the data from the remaining seven studies were fit by values of the interference parameter that were up to 16 times greater.

The values provided in the 0-s column of Table 2 are estimates of L , the initial memory strength parameter. Given that all the data for memory measurement as a function of retention interval were group, rather than individual, in nature, and given the lack of any previously established estimates of initial memory strength for unfamiliar faces, we obtained initial strength estimates by substituting for the predicted value of d' in Wickelgren's (1975, 1977) equation the observed value of d' obtained from the first retention interval at which face memory was measured and then solving for L . It should therefore not be surprising that the predicted and observed values match perfectly at the first retention interval for each forgetting function. Clearly, any statistical assessment of the adequacy of fit includes only the degree of fit at retention intervals subsequent to the first. This approach also permits assessment of how well the theory predicts where future points will fall as retention interval increases beyond Time 0.

A statistical assessment of the fit of Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory to the 11 empirical face forgetting functions was made by applying a chi-square goodness-of-fit test in each instance. In no instance was the chi-square test significant. Hence, in each case the null hypothesis that both observed and predicted values represent the same forgetting function could not be rejected. An omnibus chi-square test of the fit of retention interval data from all 11 functions (23 df) was also not significant. The quality of the curve fits by the power-exponential theory is especially encouraging when one notes that the observed values of d' are by necessity group scores rather than being based on individually computed scores such as Wickelgren obtained from continuous recognition memory experiments.

Thus, it can be said that Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory has met all three criteria set out earlier for any theory of forgetting to have potential forensic applicability. The theory provides an estimate of initial memory strength, predicts accurately where future points will fall on the forgetting function as retention interval increases, and satisfactorily fits functions based on group data. Before considering the forensic applicability of Wickelgren's (1975, 1977) theoretical forgetting function, however, we should first note some additional aspects of its theoretical utility.

Further Theoretical Observations

Faces as a stimulus category. Finding a theory that fits face recognition memory forgetting functions has been rewarding in terms of a number of additional insights gained, insights accrued beyond the mere promise of having a more precise accounting of the loss in fidelity for the memory representation of the once-seen face. One particularly intriguing finding is that not only is virtually the same value of the time-decay parameter required to fit the power-exponential theory to each of the 11 empirical forgetting functions but it is an order of magnitude smaller than the value required by Wickelgren (1975) to fit forgetting functions for common English words and pictures of common objects. Apparently, there is a more rapid rate of decline in trace fragility (a more rapid rate of increase in trace consolidation) for unfamiliar faces than there is for episodic traces of familiar English words and pictures of familiar objects. Thus, even though face recognition memory forgetting functions may be fit by the same theory as forgetting functions for words and objects, episodic memory for unfamiliar faces may decline more slowly. As Deffenbacher (1986) noted, perhaps this phenomenon should not be all that surprising, given selection pressures in the evolutionary history of our species to promote efficient processing of faces. After all, faces constitute a very important category of stimuli, providing a very rich source of socially relevant information, continually requiring all of us to make fine discriminations among them, and needing a relatively large quantity of human cortex to be devoted to their processing (e.g., Sergent, Ohta, & MacDonald, 1992). Our finding that memory representations for the unfamiliar face may be consolidated more rapidly than for certain other visual stimulus classes reinforces the notion that faces may not be unique stimuli but they are at least somewhat special (Ellis & Young, 1989).

Cross-race effect. A second interesting byproduct of our theoretical search is revealed as a result of our fit of the power-exponential theory to two forgetting functions provided by the data of Chance and Goldstein (1987; see Table 2). Here we have additional illumination of mechanisms underlying the cross-race effect, a forensically relevant phenomenon whereby once-seen faces of another race or ethnic grouping are discriminated from one another less well and later recognized less well than are once-seen faces of the observer's own race (Meissner & Brigham, 2001). Chance and Goldstein's observers were Caucasians exposed to Caucasian and Japanese faces, blocked in counterbalanced order by race of face. The statistically reliable cross-race effect obtained here was clearly due to superior encoding of the Caucasian faces. This effect was documented by an initial memory strength superiority of approximately 1 d' unit for Caucasian faces when encoded by Caucasian observers as compared with the initial

memory strength engendered by their encoding of Japanese faces. It is also of interest that even though Japanese faces were consolidated at the same rate as Caucasian faces, requiring the same value of the time-decay parameter for a fit of theory to forgetting function, the forgetting function for the Japanese faces required a value of the interference parameter 10 times as great, 10^{-7} versus 10^{-8} . This finding provides support for the view that same-race faces are more easily discriminated from one another than are other-race faces (Meissner & Brigham, 2001), providing an opportunity for them to be encoded in a more discriminating fashion, yielding thereby at least some of the encoding advantage for same-race faces. Same-race faces would therefore likewise be expected to withstand better the ravages of interference generated by subsequent encounters with other faces during any particular face's retention interval. Indeed, Meissner and Brigham (2001) found that cross-race effects were greater at longer retention intervals.

Target face exposure. The effect of increases in target face exposure time is illuminated by our fit of Wickelgren's (1972, 1974, 1975, 1977, 1979) theory to two forgetting functions yielded by the work of Ellis and Flin (1990). Examining the face recognition memory functions generated by two different groups of 10-year-old Scottish schoolchildren (Table 2), one group having an encoding time of 2 s per face and the other having encoding time of 6 s, we observed that the extra 4 s of encoding time provided an initial memory strength advantage of about 0.20 of a d' unit. This initial advantage leveraged a memory strength advantage of 0.50 of a d' unit at a 7-day retention interval. A recent meta-analysis by Bornstein, Deffenbacher, McGorty, and Penrod (2007) has confirmed that longer exposures of target faces are associated with both higher hit rates ($r = .23$) and lower false alarm rates ($r = -.12$). Clearly, more research is needed in an effort to try to establish just how much additional initial memory strength can be purchased by n additional seconds of exposure time.

Age differences. Still another phenomenon of face recognition memory is the age effect associated with efficiency of face processing, such that older children show superior memory for faces. It would appear that the age effect associated with the greater recognition memory shown by Ellis and Flin's (1990) 10-year-olds with 2 s of encoding time per face as compared with 7-year-olds with the same 2 s of encoding time is strictly due to the former being able to encode unfamiliar faces more effectively in the time available, yielding an initial memory strength advantage of 0.65 of a d' unit (Table 2). Up until the middle teen years, face recognition memory shows continual improvements in the ability to discriminate same-race faces from one another and later to recognize them (Chance & Goldstein, 1984). Thus, 10-year-olds have had another 3 years of fine tuning of their brain's perceptual learning "machinery," permitting enhanced ability to respond to more subtle differences among faces they typically encounter.

Failures to find retention interval effects. Finally, power-exponential theory can help us to understand at least one potential contributor to the frequent failure to find statistically significant retention interval effects for face recognition memory. Clearly, one factor in producing findings of a lack of a statistically reliable effect of forgetting occurs primarily when just two retention intervals are measured and both intervals are at points on the forgetting function between which little forgetting would be expected to occur. Although it is more informative to test retention at more

than two intervals, if only two intervals are to be tested, investigators should at least ensure that one occurs within minutes after encoding, given the relatively rapid roll-off in memory strength in the first minutes after encoding. After all, memory strength for the once-seen face loses 15% of its strength in the first 10 min of the retention interval. Consider an illustration provided by the data from Wixted and Ebbesen (1991, Experiment 2, 11-s stimulus duration condition); see Table 2. Note that there was about a 12% loss of original memory strength ($d' = 2.47$) between Day 1 and Day 7. However, the actual amount of original memory strength lost since encoding was about 41%. If one were to have only measured face recognition memory at the Day 1 and Day 7 intervals, one would have underestimated the actual amount of forgetting by a factor of more than three to one. Hence, Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory and its ability to provide an estimate of original memory strength permits us to see clearly that just where retention intervals fall on the theoretical forgetting function will affect the likelihood of finding statistically reliable amounts of forgetting.

Forensic Applicability of Power-Exponential Theory

As we indicated in the introduction to this article, to make a proper assessment of the strength of an eyewitness's current memory representation, a trier of fact needs to have an estimate of the witness's initial memory strength, to know the length of the retention interval, and to understand the nature of the forgetting function for the once-seen human face. As retention interval length can usually be specified with some precision, acquiring an estimate of the eyewitness's initial memory strength and a knowledge of the precise nature of the forgetting function represent the key forensic needs.

Let's consider the latter forensic need first. Psychologists are now able to provide a much greater degree of specific knowledge to the trier of fact as regards the nature of the forgetting function for human face recognition memory. It turns out that we can now offer the judge or juror an estimate of what proportion of memory strength, regardless of its initial value, remained at the time the eyewitness's memory for the perpetrator was tested. We can do this for three reasons. First, we have clearly demonstrated the ability of Wickelgren's (1972, 1974, 1975, 1977, 1979) power-exponential theory to fit forgetting functions that have included retention intervals ranging in length from 1 min to nearly 1 year. Second, we have demonstrated the remarkable constancy of the value of the time-decay parameter (.025) needed for a fit. Third, we have likewise demonstrated the relatively narrow range of values of the interference parameter needed for a fit, practically all falling within an order of magnitude of each other, 10^{-7} to 10^{-8} . For a conservative estimate of the proportion of remaining memory strength, we can simply plug into Wickelgren's (1975, 1977) equation given earlier in this article the values of the forensically relevant retention interval (in seconds), the value of the time-decay parameter at .025, and the value of the interference parameter at 10^{-8} ; if a case involves a cross-race identification, the interference parameter should be instead set at 10^{-7} . For a given retention interval, the resulting calculation yields the estimated proportion of initial memory strength remaining. Having an expert on eyewitness memory be able to testify to this estimate should aid the trier of fact considerably in his or her task of assessing the fidelity of an

eyewitness's memory representation. Knowing, for example, that at memory test, an eyewitness had only 50% of original memory strength remaining would represent a real improvement in specificity over what could be provided by an expert before the present. More valid assessments of eyewitness credibility can only increase the quality of justice rendered by a trier of fact.

Let's now return to the other key forensic need for triers of fact to be able to add precision to their assessment, the need for an estimate of initial memory strength for the perpetrator's face. Wickelgren's (1972, 1974, 1975, 1977, 1979) theory of forgetting is indeed unique among theories of forgetting in its specification of an estimate of initial memory strength. However, the forensic situation is neither a laboratory nor a field experiment and does not yield an estimate of initial memory strength.

There is, nevertheless, a way to yield a conservative estimate of initial memory strength for a typical eyewitness, conservative in the sense that the estimate would very likely represent an upper bound on initial memory strength for many forensic situations. The results of an interesting field experiment (Pigott, Brigham, & Bothwell, 1990) provide us with the opportunity. Pigott et al.'s (1990) participants were 47 Florida bank tellers, each of whom interacted with one of two men who attempted to cash a crudely altered U.S. Postal Service money order during a scripted 1.5-min encounter. More than 75% of these tellers had training in eyewitness techniques but were not made aware that their encounter with the perpetrator of attempted bank fraud was not genuine until after their recall and recognition had been measured 4 hr later. Averaged across two target-present and two target-absent lineups, their mean proportion correct was 0.55, which for a seven-alternative, forced-choice recognition memory task (six lineup members plus the alternative of rejecting the entire lineup) is equivalent to a d' score of 1.41 (Hacker & Ratcliff, 1979). It seems reasonable to account for the alternative of rejecting the lineup as an additional choice. After all, for a target-absent lineup, the correct choice is rejection of the lineup. If this had not been done in the present instance, the six-alternative, forced-choice d' would have been 1.30.

If we substitute 1.41 for the value of m in Wickelgren's (1975, 1977) equation and solve for L , we come up with a very plausible estimate of initial memory strength for the bank teller eyewitnesses of Pigott et al. (1990), $d' = 1.79$, equivalent to 67% correct on a seven-alternative, forced-choice task (Hacker & Ratcliff, 1979). After 4 hr, then, memory strength for the perpetrator of attempted bank fraud was just 79% of what it had been originally. Had the tellers not been tested until a week after the encounter with the perpetrator, memory strength would have been approximately $d' = 1.24$, equivalent to a predicted performance score of 49% correct—considerably lower, although still better than chance.

The d' value of 1.79 plausibly represents an upper limit for initial memory strength for eyewitnesses in many forensic situations, at least for those not having a highly distinctive or memorable perpetrator. That is, it is fair to say that the forensic scene for Pigott et al.'s (1990) bank tellers represents a close to optimal situation for an eyewitness. Consider that the perpetrator was in full view for 1.5 min, an amount of target exposure greater than the "critical value" of 1.0 min noted by Valentine, Pickering, and Darling (2003) in their massive study of lineups conducted by the London Metropolitan Police. The banks were well illuminated. The perpetrators were not disguised. There was no alternative focus of attention present that might ordinarily have been expected

to draw the teller's attention away from the perpetrator's face, such as a weapon; only the face of the money order proffered by the man trying to perpetrate bank fraud required some attention. Tellers should not have been operating under high stress levels, given the absence of any personal threat to them.

To the extent that one or more of the optimal conditions of the Pigott et al. (1990) study are not met in any given forensic situation, then, the predicted initial memory strength for an eyewitness should be less than the figure of 67% correct predicted for the Florida bank tellers in Pigott et al.'s field experiment. Until further research is conducted—testing three or more retention intervals and the effect of varying durations of target person exposure, target person distinctiveness, and illumination levels, for instance—conservative advice to a trier of fact would be that the typical eyewitness viewing a perpetrator's face that was not highly distinctive would be expected to have no more than a 50% chance of being correct in his or her lineup identification (six-person lineup) at a 1-week delay. Clearly the trier of fact would still need to consider other specific facts of the case to decide how much less than 50%, if any, the chance of a correct identification might be in these less than optimal witnessing conditions. Retention interval benchmarks other than 1 week can be readily calculated, of course, using Wickelgren's (1972, 1974, 1975, 1977, 1979) theory of forgetting and the data provided by Pigott et al. These calculations do assume that the lineup's construction and administration have been conducted fairly. However, a post hoc assessment of lineup fairness is relatively straightforward (Malpass, Tredoux, & McQuiston-Surrett, 2007).

To illustrate in particular the need for more research on the relationship of variations in target face distinctiveness to initial memory strength, consider the results of using power-exponential theory to predict initial memory strength for the eyewitnesses in the field studies of Read, Tollestrup, Hammersley, McFadzen, and Christenson (1990). Averaging across four different photo lineup conditions and 212 retail clerks, performance was at a level of 76% correct at a 48-hr retention interval, equivalent to a d' of 2.10. Using this value of memory strength to estimate initial memory strength, we find a d' value of 2.87 (91% correct), considerably higher than the 67% correct initial memory strength figure predicted for Pigott et al.'s (1990) bank tellers. Predicted performance level for Read et al.'s clerks would have been 73% correct at a memory test delay of 1 week, equivalent to a memory strength value of $d' = 1.98$. However, because only one perpetrator was used across the four different 48-hr retention interval conditions to which Read et al.'s clerks were exposed, it may well be that their higher performance level than that obtained by Pigott et al.'s (1990) eyewitnesses and those in other field experiments was due to the single perpetrator having a rather distinctive, and hence memorable, face.

Conclusions

Psychological science is now in a position to offer the trier of fact more than vague generalities regarding the relationship between retention interval and strength of the memory representation for the once-seen face. The results of our meta-analysis confirm that there is indeed a statistically reliable association between longer retention intervals and decreased face recognition memory, an association equally true of face recognition memory and

eyewitness identification studies. That is, there is an increase in positive forgetting as the delay increases between encoding of a face and test of one's memory for it.

The present meta-analytic review of the literature also provides support for Wickelgren's (1972, 1974, 1975, 1977, 1979) theory of forgetting from recognition memory, using data from studies of memory for once-seen faces that used diverse methodologies and a wide range of retention intervals. Fitting Wickelgren's power-exponential theory to 11 different empirical forgetting functions providing group data resulted in statistically satisfactory fits, fits predicting where future points will fall on the function as retention interval increases. In addition, power-exponential theory provides an estimate of initial memory strength. This latter feature of the theory, particularly useful when applied to the results of field studies, permits calculation of not only an estimate of initial memory strength (d') but also calculation of a strength estimate at any given retention interval. Hence, not only can percentage of initial memory strength remaining be determined at any retention interval, but the strength estimate at a particular retention interval can also be readily translated into a probability of being correct on a fair lineup of a specified size. Of course, to be practically useful, these estimates would need to be calculated for and clearly explained to the trier of fact by an eyewitness memory expert.

When considering the applicability of our findings, at least two concerns might be raised. First, throughout this article, we have assumed that the amount forgotten is a function of the current strength of the memory representation. It could be objected that what is remembered is also very much determined by retrieval conditions, such as the type of memory test. Recognition memory tests are often more sensitive measures of memory than are recall tests for the same material, for instance. Furthermore, the encoding specificity principle proposes that the amount of forgetting at retrieval is a function of the degree of match between encoding context and retrieval context. Although we do not deny the validity of these objections, we do not see them as problematic for the applicability of Wickelgren's (1972, 1974, 1975, 1977, 1979) theory to eyewitness memory. For one thing, the retrieval tasks in the studies we have reviewed, eyewitness identification memory tests and laboratory face recognition tests, differ somewhat but are still essentially tests of recognition memory. One of our moderator analyses showed that type of retrieval task, recognition memory or eyewitness identification, was not a moderator of effect size for the correlation between retention interval length and memory strength. In addition, even though the match of encoding and retrieval context is typically greater for recognition memory tasks than for eyewitness identification tasks, this difference did not affect the strength of the relationship between retention interval and memory strength, either. Furthermore, Wickelgren's theory was equally effective at predicting memory strength at any given retention interval for both eyewitness identification and face recognition memory studies.

A second concern that might be raised relates to the fact that our curve-fitting exercise could only be applied to 11 forgetting functions from just eight published studies. The robustness of the data sets underlying these 11 functions clearly depends on the overall quality of the eight published papers. Our considered judgment is that there is little to be concerned about in this regard. Quality of fit of theory and data shows no obvious relationship to any per-

ceived differences in quality of publication source or any minor differences in quality of methodology and data analysis.

In any event, psychologists interested in the psychology of testimony now have much more abundant direct evidence bearing on their belief that the forgetting function for the once-seen face is Ebbinghausian in nature (cf. Kassin et al., 2001): Rate of memory loss for an unfamiliar face is greatest right after the encounter and then levels off over time. Psychological science can now also provide to both these same psychologists and triers of fact rather more specific details concerning the decline and fall of a face's memory representation over time and succeeding facial encounters.

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CERTIFICATION

Pursuant to Connecticut Practice Book §§ 67-7A, 67-2A(g) and 62-7, the undersigned certifies that (1) on March 27, 2023, copies of the amicus brief and appendix have been delivered electronically to: James A. Killen, Office of the Chief State's Attorney, 300 Corporate Place, Rocky Hill, CT 06067, tel. (860) 258-5807, fax (860) 258-5828, DCJ.OCSA.Appellate@ct.gov and James.Killen@ct.gov; Kara E. Moreau and Emily C. Kaas Jacobs & Dow, LLC, 350 Orange Street, New Haven, CT 06511, Tel. (203) 772-3100, fax (203) 772-1691, kmoreau@jacobsllaw.com; ekaas@jacobsllaw.com; and Lisa J. Steele, Steele & Associates, P.O. Box 547, Shrewsbury, MA 01545-0547, steelellaw@earthlink.net; (2) the hard copies of the amicus brief and appendix being filed with the appellate clerk are true copies of the brief and appendix that were submitted electronically; (3) the amicus brief and appendix do not contain any names or other personal identifying information that is prohibited from disclosure by rule, statute, court order, or case law; (4) the word count of the brief is 3391 words, exclusive of the appendix, cover page, table of contents, table of authorities, statement of issues, signature block of counsel of record, certifications, and statement of interest of the amici required by §67-7A; (5) the amicus brief and appendix complies with all provisions of Practice Book § 67-2A; and (6) no deviations from Practice Book §§ 67-2A or 67-7A were requested or approved.

By: /s/ Robert J. Meredith (410126)

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