Letters to the Editor

Discussion of "The Polygraph and Psychiatrists"

Dear Sir:

Dr. Melvin G. Goldzband's recommendations, in the March 1990 issue of this journal, that a psychiatrist not render opinions of a suspect's truth or deception is sound advice [*1*]. A psychiatrist's training is in psychiatry, not in detection of deception. This lack of expertise is especially evident when Dr. Goldzband attempts to refute the theory, validity, and reliability of the polygraph technique.

Certainly, Dr. Goldzband must realize that his psychiatric patients are not a representative sample of the average subject's feelings about taking a polygraph examination. The findings from six independent surveys conducted between 1973 and 1983 on a total of 1165 job applicants who were administered a polygraph screening examination reported that 94% were not offended in any way by the examination, 93% believed the test was fair, and 89% believed the test was not an invasion of privacy [*2*].

Dr. Goldzband states that establishing the validity of the polygraph is "not easy because of the heated disagreement between the foremost researchers in the field." He then presents opinions of Leonard Saxe, David Lykken, and Paul Ekman. Of these three "researchers," only Lykken has ever researched the polygraph, and when he did, he found 98 and 100% accuracy rates (1959, 1960). The heated disagreement is not between researchers; it is between people who are not trained in, or do not understand, the polygraph technique and researchers who have been trained in the polygraph technique. If the author had even peripherally searched for favorable validity studies, he would have found that the vast majority of field research done on the polygraph technique reports an accuracy in the 85 to 95% range [*3*]. The few maverick studies that have reported lower accuracy rates involved student examiners or did not come close to duplicating actual field procedures [*4,5*].

In his book *A Tremor in the Blood*, Lykken presented his own implausible theory explaining the polygraph technique [*6*]. Goldzband, in believing Lykken, states, "Polygraphers and their advocates seem to believe that human emotional responses are linear in the same way that the physiologic responses charted by the polygraph are linear." This theory should have been credited to Lykken, because no polygraph examiner would agree with it. Lykken also was successful at spreading the myth that the polygraph is a "painless third degree." A properly conducted polygraph examination is nonaccusatory—it involves no interrogation whatsoever. If Goldzband is concerned about the admissions a subject makes following a polygraph examination, his comments should address interrogation, not the polygraph technique—they are two separate and distinct procedures.

A second misrepresentation is made by Goldzband, although perhaps not intentionally, in his failure to differentiate clearly between multiple-issue screening examinations and specific-issue polygraph examinations. The theory, format, and purpose of these two types of examinations are completely different, and very few statements which accurately apply to multiple-issue examinations also apply to specific-issue examinations. Unless one understands these distinctions, inaccurate assumptions will be made.

In his discussion of usefulness, Goldzband relates information learned through an interview with William Fedor, a deputy director within the U.S. Department of Defense (DOD). Fedor is either misquoted or was misinformed when he stated, "We [DOD] never said it was scientifically valid but, rather, that it was useful." In 1983 the Department

of Defense published a lengthy report in which the abstracts of 43 validity and reliability studies were reviewed [7]. The report states that "the research produces results significantly above chance."

The misinformation contained in this article offers the strongest support of Goldzband's position that psychiatrists should not detect deception. Evidently Dr. Goldzband took at face value Lykken's, Saxe's, and Eckman's misstatements about the polygraph technique. If psychiatrists are so easily swayed and duped into believing distorted facts and erroneous descriptions, they certainly should not be rendering opinions regarding truth or deception.

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Author's Response

Dear Sir:

A quick telephone call to Robbie Bennett, manager of the National Office of the American Polygraph Association, reveals that Brian Jayne is a member of the association's Public Relations Committee and, indeed, a representative spokesperson for the organization. I must confess to a feeling of *deja vu*, recalling a former time when, as an antitobacco physician, I had to plough through affectively and defensively similar material from the Tobacco Institute!

Probably my best approach in responding to Mr. Jayne's somewhat heated letter is to attempt to detail his objections in order. Therefore, I must point out my gratification at his initial agreement with me that psychiatrists ought not to be in the lie detection business. That, of course, was the central thrust of my article. The concept of psychiatrists attempting to be in that business by using polygraphs is, in my opinion, unethical and certainly unprofessional because the instruments are coercive and scientifically unproven, if not unsound. It is that latter aspect which causes Mr. Jayne to voice considerable disagreement.

More *deja vu* follows when I read his comments about the surveys of the average subject's feelings about having taken polygraph tests. Some people jump naively at the opportunity to take these tests in order to validate (they believe) their innocence or worthiness as potential employees. Many others resent it greatly, but they certainly are not going to risk their newly found jobs by complaining about the methods used in hiring them. I am reminded of the anecdote about Freud's leaving Vienna after the Nazi *Anschluss* in 1938. The Germans bowed to the pressures of his influential English and French friends but insisted that, before his release, he attest to the fact that he was not harmed. Freud wrote, "I can recommend the *Gestapo* to anyone!"

Mr. Jayne states that Lykken was the only valid researcher of polygraphy of the series I quoted. It is probably best to leave it to the other distinguished scientists to defend their own excellent reputations. When Jayne writes of quarrels among researchers who do not understand polygraphic technique, he is only repeating the material I discussed in my paper. There are a number of techniques, each with its proponents who have strong academic or scientific credentials and who decry the use of the other techniques, just as Mr. Jayne seems to be doing. In sum and substance, I believe that he is validating my viewpoint about polygraphy and its lack of scientific basis. His quote on accuracy rates reflects the *usefulness* of the instrument, not its scientific validity.

I am unable to comprehend Mr. Jayne's statement that polygraphic examination involves no interrogation whatsoever. The so-called techniques about which the experts battle are, in fact, interrogation variables. They certainly do not argue about the machine's squiggles on the graph.

Finally, I stand by the quote from William Fedor of the U.S. Department of Defense (DOD), who reviewed an earlier draft of my paper which also contained the quotation. I do not believe that Mr. Fedor was misinformed. Instead, I believe that the DOD's report to which he referred was, again, about the instrument's *usefulness* rather than its scientific basis. The validity and reliability studies indicated that it was, indeed, a very good way to get frightened and naive people to confess to their misdeeds. As Mr. Fedor indicated, "You wouldn't believe the information people would begin to tell us." It had nothing to do with the scientific basis for its possible success.

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Discussion of "Lingual Markings of Anterior Teeth as Seen in Human Bite Marks"

Dear Sir:

We have read the article in the July 1990 issue of the *Journal of Forensic Sciences*, titled "Lingual Markings of Anterior Teeth As Seen in Human Bite Marks." The author, Dr. Sperber, has proposed a mechanism of biting which purports to explain both the activity of the jaws during biting and the reasons for the patterned injury left on skin by the bite. We believe that there are misstatements in the paper and invite Dr. Sperber to clarify his explanation.

The detailed dynamics of a bite are probably unique in each instance and therefore difficult to duplicate. However, in order for any markings to appear on the skin of the victim, some resistance must be present to the biting force generated by either arch or both arches. When one bites human skin, only three such opposing forces or resistances are available:

(*a*) the teeth of the opposing arch,
(*b*) the pressure exerted by the tongue on tissue which has been gathered into the mouth of the biter, and
(*c*) some of the underlying substance of the victim's body, whether hard or soft tissue.

In other words, the teeth must "push" against something in order to mark the skin. Dr. Sperber is certainly correct in stating that the lingual surfaces of the maxillary teeth might mark when a person bites in centric occlusion. However, in such a situation the facial surfaces of the mandibular teeth would be likely to mark also. Figure 1*a* is a sketch of such a bite. In that situation, the lingual surfaces of the mandibular teeth are "unopposed"

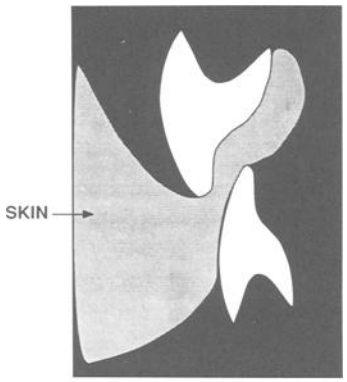

FIG. 1*a*—*A bite in which the maxillary linguals and mandibular facials are likely to mark.*

unless the biter has taken a sufficient amount of the victim's skin into his mouth and the biter's tongue is pushing against that skin. Figure 1*b* is a sketch of this situation.

At the cessation of biting, the victim's skin "relaxes" and "unfolds," allowing the imprint to be analyzed. The cingulum areas of both the maxillary and mandibular teeth will point towards the center of the bite. If the lingual surfaces of only one arch mark, the cingulum areas will still point toward the center of the bite. If the facial surfaces of either arch (or both) mark, the gingival areas will point toward the perimeter of the bite. Figure 2*a* depicts the result of the bite in Fig. 1*a*, and Fig. 2*b* depicts the result of the bite depicted in Fig. 1*b*. In order for the facial surfaces of either arch to mark, the biter must have forced his jaw or jaws against the underlying tissue of the victim. The degree of force exerted will determine the clarity of the mark. Dr. Sperber is also correct in asserting that facial surfaces of the maxillary teeth might mark if the biter overprotrudes and pinches the skin between his upper and lower teeth.

In reviewing the illustrations accompanying Dr. Sperber's article, one finds that, in his Fig. 4, the biter was apparently able to gather quite a bit of skin into his mouth in order

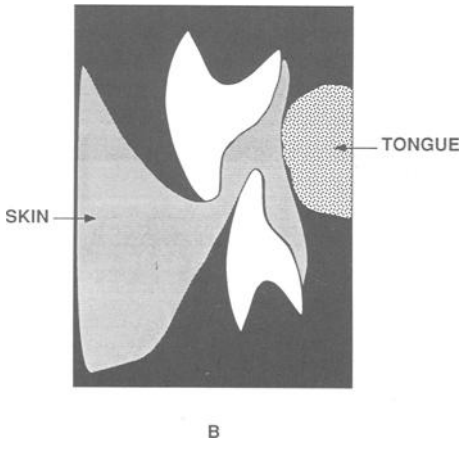

FIG. 1*b*—*A bite in which sufficient tissue is gathered into the biter's mouth for the mandibular linguals to mark also.*

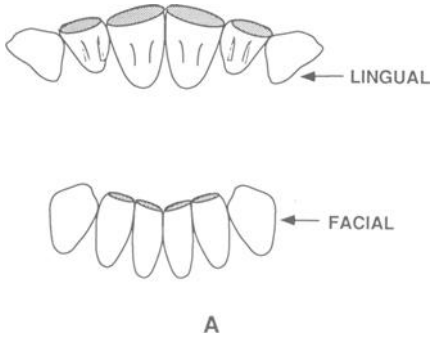

FIG. 2*a*—*Markings on the skin left by the lingual surfaces of the maxillary anterior teeth and the facial surfaces of the mandibular anterior teeth.*

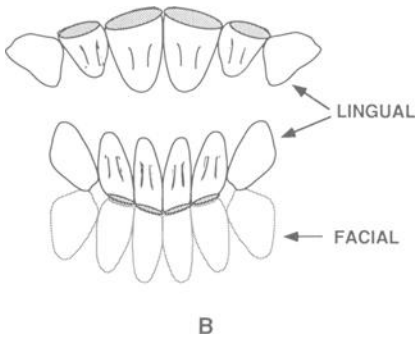

FIG. 2*b*—*Imprint left by the lingual surfaces of the anterior teeth of both arches and illustration of the markings that the facial surfaces of the mandibular anterior teeth might also possibly leave.*

to leave such clear lingual markings. The same situation is apparently the case in Dr. Sperber's Fig. 5, although on many victims it might be difficult to gather sufficient skin into the mouth from that area of the body. In Dr. Sperber's Fig. 6, on page 841, however, we feel certain that there is a discrepancy. It appears in that photograph that the maxillary "cingula" point toward the periphery of the bite. We would argue that these are actually the gingival areas of the *facial* surfaces of these teeth.

As for Dr. Sperber's Fig. 2, on page 839, we contend that he has misinterpreted the markings left by that bite. He attributes the maxillary marks to the lingual surfaces of those teeth. Careful analysis reveals the possibility that those marks may not be such. Although the cingula do point toward the center of the bite, at their gingival extent they curve incisally—an anatomic feature not in keeping with human teeth. We contend that those markings are in fact an injury to the tissue caused by the rapid withdrawal of the victim's skin from between the jaws of the biter—a common reflexive action. In addition to the two central incisor markings, there is a partial marking from the left lateral incisor and a clear mark associated with the tip of the left cuspid (see our Fig. 3).

Although in the past we have referred to such marks as "drag marks," "skipping," or "linear striations," we did not mean to imply that in every case the biter dragged or skipped his teeth across the victim's skin. However, let there be no misunderstanding of our contention that these marks are unquestionably the result of movements (other than chewing) occurring between the jaws of the biter and the skin of his victim. We have never attempted to link a bite mark to the biter's state of mind. We concern ourselves

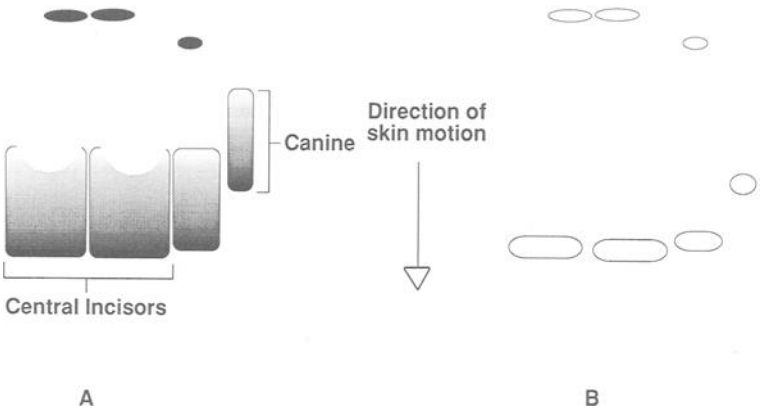

FIG. 3—(a) *A sketch of the photograph in Dr. Sperber's Fig. 2, at page 839, with the salient features labeled. The arrow depicts the direction of the relative motion of the skin during the dynamic interplay between the teeth and the skin surface. An area of traumatized skin results—the bite mark.* (b) *Overlays of the position at which the teeth of the biter first contacted the skin of the victim.*

with the dynamics of bite mark production. Forensic odontologists agree on the uniqueness of each person's dentition; therefore, in the dynamic interplay between teeth and skin there can be an infinite number of possibilities.

As scientists, we should not use a very limited set of circumstances to support theories that exclude other demonstrable facts. Such simplistic interpretations of complex matters have a way of returning to haunt one in the courtroom.

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Acknowledgment

The authors wish to thank Raymond Calvert, of the Department of Learning Resources, Louisiana State University School of Dentistry, for his assistance in producing the illustrations on the MacIntosh computer.

Author's Response

Dear Sir:

In reply to Drs. West and Barsley, I am happy to have this opportunity to address their concerns.

My article demonstrated and explained why the mandibular teeth usually mark in better detail than the maxillary teeth. I cited a reference, *Outline of Forensic Dentistry* (J. Cottone and M. Standish, 1981, p. 119), in which it was stated that the reason this occurred was because the mandible moved and the maxilla did not. I disagreed with this contention. I stated in p. 843 of my article that "lingual markings of maxillary and mandibular teeth are caused by the *relationship* between the upper and lower arches during biting." The phrase, "during biting," quite obviously includes any and all of the dynamics that occur during biting. In their letter, West and Barsley refer to "drag marks,"

"skipping," and "linear striations." They state, "let there be no misunderstanding of our contention that these marks are unquestionably the result of movements (other than chewing) occurring between the jaws of the biter and the skin of his victim." Of course, other markings occur during biting! They miss the entire point of the article! I stated that the lingual markings demonstrated in the article "should not be classified as drag marks, linear striations, or other similar lesions." In this statement alone, I am distinguishing between the types of lesions that one may see in a variety of cases. I am certainly not eliminating movement, as they contend.

They claim that there is a discrepancy in Fig. 6 of my article. They believe that the maxillary markings "are actually the gingival areas of the facial surfaces of these teeth." This is an interesting point, except that neither their sketches nor their letter explains how the facial gingival aspects of the maxillary teeth could be reflected in the skin in the absence of clear incisal edge markings. Obviously, the maxillary markings are diffuse and indistinct and therefore are subject to many interpretations. If a majority of credible experts feel that these maxillary markings are facial rather than lingual, and they can demonstrate this phenomenon through experimentation, then certainly I will be persuaded that the facial surfaces, to the exclusion of the lingual surfaces, caused these marks. In any case, the mandibular teeth mark well and the maxillary teeth do not, which is the subject of the article.

They also call for a clarification of Fig. 2. This photograph demonstrates good incisal markings of the mandibular teeth with a dark diffuse representation of the maxillary teeth. Regardless of how one interprets the gingival contours or the various other maxillary structures, it is still a fact that these markings are a result of lingual markings from the incisal edges of the maxillary teeth. If this bite had occurred as the good doctors claim, through "the rapid withdrawal of the victim's skin from between the jaws of the biter," one would expect to see some mandibular "scrape lesions" as well. Since it is apparent that the skin did not drag over the incisal edges of the mandibular teeth, then one is led to conclude that the maxillary markings were caused by the surface of the skin moving past the lingual aspects of the maxillary incisal edges. The bottom line, here again, is that the *incisal* edges of the maxillary teeth did not mark as distinctly as the mandibular because the bite did not occur in a protrusive relationship. This photograph (Fig. 2) was included with the others to show the wide variety of maxillary lingual markings.

The doctors conclude, "As scientists we should not use a very limited set of circumstances to support theories that exclude other demonstrable facts." The article did not "exclude other demonstrable facts." Rather, it eliminated an explanation that had appeared in dental literature for years, that the *movement* of the mandible explained the difference between maxillary and mandibular markings.

Apparently, they agree with the experimental protocol and the conclusion, because they do not critique this aspect of the article. They join other collegues, who have communicated with me, in agreeing with the basic premises of this presentation to the A.A.F.S. in 1989 and the published paper. In fact, they are the only ones, that I know of, who have taken issue with *any* part of the paper or presentation.

I am personally pleased that Drs. West and Barsley read this article and that it stimulated their thinking on this particular aspect of bite mark dynamics. Their sketches, however, are not a substitute for clinical demonstrations of the interaction between teeth and skin. I would encourage them to continue and add to the work that I have started, so that proper bite mark evidence interpretation can continue to evolve.

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On the Classification of Small Colorless Glass Fragments as to Their Sheet or Container Origin

Dear Sir:

In Vol. 31, No. 4, Oct. 1986, of this *Journal*, Ryland published the results of a study on the sheet/container classification of small colorless glass fragments using refractive index determinations, scanning electron microscopy/energy-dispersive X-ray analysis (SEM/EDX), and X-ray fluorescence spectroscopy (XRF). The study employed samples from 140 domestically collected sources, with approximately half of them sheet and half of them container glass. Using the X-ray intensity ratios of calcium/magnesium (Ca/Mg) and calcium/iron (Ca/Fe) as criteria, Ryland found that 93% of the samples were correctly classified, with 7% falling into a recognizable inconclusive category.

Koons, Fiedler, and Rawalt subsequently reported on the classification and discrimination of sheet and container glasses by inductively coupled plasma/atomic emission spectrometry (ICP/AES) in Vol. 33, No. 1, Jan. 1988, of this *Journal*. They examined 184 colorless container and sheet glasses, including 85 beverage bottles, 50 baby food jars, and 49 sheets of glass. One of the approaches for classification that they presented used the concentrations of six elements and resulted in correct classification of 180 of the samples, with 4 falling into an "unclassifiable" (inconclusive) category.

In an attempt to compare the procedures, Koons et al. applied Ryland's approach, based on ratios between magnesium, calcium, and iron, to the ICP data for these three elements in their samples. They stated that

> Ryland's procedure correctly identifies 128 of the 135 containers and 25 of the 49 sheets, with the remaining 7 container and 24 sheet glasses classified as "unclassifiable." From this limited comparison, it appears that the use of 6 elements rather than 3 increases the probability of correctly classifying glass fragments, particularly for sheet glass samples.

This statement was intended to show that the reliability of a classification method improves as the number of independent classifying elements is increased. However, it unfortunately raises the question of the reliability of the SEM/EDX/XRF approach in light of the projected 83% overall classification success and, more disturbing, the projected 51% correct and 49% inconclusive rates for the sheet glass samples.

In response to this failure of the method of Ca/Mg and Ca/Fe ratios when applied to the ICP data, we have done additional studies and would like to add several points of clarification to the earlier studies. In this report, we discuss the calculations used by Koons et al. in evaluating the SEM/EDX/XRF approach, thereby explaining their low projected classification success rate. Furthermore, we provide the results of reanalysis and classification of the samples of Koons et al. using the SEM/EDX/XRF method.

On the first point, the published comparison by Koons et al. was based on the calculation of correction factors, using Ryland's reported Ca/Mg and Ca/Fe X-ray peak intensity ratios determined from SEM/EDX and XRF analyses of National Bureau of Standards (NBS) Standard Reference Materials 621 and 1831 in relation to the certified concentrations of Ca, Fe, and Mg in those standards. These correction factors were then used to calculate the corresponding element ratio classification cutoffs to be used with the ICP data. The 2 standard glasses resulted in two values each of Ca/Mg and Ca/Fe cutoff ratios. Koons et al. adjusted their classification ratios so that no samples were misclassified. Specifically, among those samples having low Ca/Mg ratios, requiring consideration of the Ca/Fe values, there were several container samples which also had low Ca/Fe ratios and fell within the range of most of the sheet glass samples. To avoid misclassifying these samples as sheet glass, a low value for the lower limit of the inconclusive range for the Ca/Fe ratio was selected (a value about 30% lower than that calculated from the lower of the NBS standards). This resulted in 49% of the sheet glass

samples falling into the inconclusive range. Had Koons et al. selected a higher value for the sheet glass inconclusive cutoff, in line with that for the standard glasses, the results would have been 12 samples classified as inconclusive and 2 containers misclassified in the sheet glass category. Koons et al. reported elsewhere in their study that for samples having low iron concentrations (less than about 400 ppm), occasionally high ICP results were observed. They attributed this to contamination during sample preparation. Wolnik et al. [*1*], in a more recent study, attributed the same observation to iron inclusions in the glass. Regardless of its cause, the occasional high Fe results for samples having low Fe concentrations is a characteristic of the ICP data. The concentration of Fe in most container glass samples is low enough to be near the detection limit of the ICP method in use at that time. Hence, the heavy reliance on Fe values of Ryland's classification procedure makes it less useful for ICP data than for XRF data. Other classification schemes discussed by Koons et al. overcome this limitation by increased reliance on other trace elements such as barium, strontium, and manganese, which are routinely quantifiable by ICP but not by XRF.

Recently, we have reanalyzed 14 of the 17 container glass samples having low Ca/Mg ratios (that is, falling into the sheet glass range) and 43 of the 49 sheet glass samples from the study of Koons et al. using the SEM/EDX and XRF procedures described previously by Ryland. These samples included the "worst case" specimens falling close to the classification threshold values. The SEM/EDX-derived Ca/Mg ratios for all 57 of these samples are less than 15, which is in agreement with the projected ratios calculated from the ICP data. Sheet/container classification, using Ryland's published XRF Ca/Fe criteria, resulted in a correct classification of 12 of the container glasses and 39 of the sheet glasses, an inconclusive classification of 1 of the container glasses and 3 of the sheet glasses, and a slight misclassification of 1 of the container glasses and 1 of the sheet glasses. Widening Ryland's "unclassifiable" Ca/Fe peak intensity ratio range of 24.0 to 28.0 to a range of 23.0 to 31.0 solves the misclassification problems, leaving 4 sheet glasses and 3 container glasses in the "unclassifiable" category. This widening of the Ca/Fe peak intensity ratio range does not affect the classification of the samples previously reported in Ryland's study. Thus, actually subjecting the samples of Koons et al. to SEM/EDX/XRF analysis and classification results in 96% being classified correctly and only 4% falling into the recognizable inconclusive category, which is in line with Ryland's observations reported previously. Furthermore, only 8% of the sheet glass samples fall into the inconclusive range, not the 49% projected by Koons et al.

In examining our combined data and sharing the insight of quantitative elemental ratios relative to SEM/EDX/XRF elemental peak ratio intensities, the following points were noted.

1. Simple linear conversion of the XRF elemental peak-intensity ratio cutoffs to quantitative ICP elemental ratio cutoffs, as used by Koons et al., does not appear to be valid for the Ca/Fe ratios. Comparison of the SEM/EDX Ca/Mg X-ray intensity ratios with the ICP-derived concentration ratios shows good correspondence. However, the Ca/Fe ratios do not exhibit a good correlation between methods.
2. The discrepancy between XRF ratios and ICP ratios appears to be partially based on occasionally spurious high Fe levels and on the difficulty in making accurate Fe measurements (especially for samples having less than about 0.05% Fe) using the ICP method. Owing to the heavy reliance on Ca/Fe ratios in the XRF treatment, this can result in a widening of the unclassifiable range when applied to the ICP data. Changes in ICP instrumentation have lessened these problems in comparison with the problems associated with the data available to Koons et al. Small errors in Fe measurements by ICP do not affect the classification schemes reported by Koons et al. because of their heavier reliance on other elements and the proportionally less weight given to Fe.

3. One of the four problem sheet glasses when using the SEM/EDX/XRF classification approach on the samples of Koons et al. was misclassified using Ryland's reported maximum threshold of 28 for the Ca/Fe peak intensity ratio inconclusive range. Increasing the threshold to 31.0 solves the misclassification, placing all four sheet glasses into the "unclassifiable" range. This modification does not result in any change in the classification results previously reported for Ryland's 140 samples.
4. Two of the three problem container glasses when using the SEM/EDX/XRF classification approach on the samples of Koons et al. were American-produced Smirnoff vodka bottles. One of these glasses was misclassified when using Ryland's Ca/Fe peak intensity ratio minimum threshold value of 24.0 for the inconclusive range. The ICP data shows that both of these glasses have abnormally low Ca levels. The misclassification can be corrected in one of two ways.

   First, experience over the past few years indicates that one should pay close attention to the Ca/Si peak intensity ratio when using the XRF technique in an attempt to somehow recognize a lower Ca level. This situation obviously affects the Ca/Fe peak intensity ratio, which in turn affects the classification. The authors suggest that, if the Ca/Si peak intensity ratio drops below 0.50 and the Ca/Fe peak intensity ratio is close to the "unclassifiable" range of 31.0 to 24.0, an inconclusive opinion is in order. If this approach is incorporated into the XRF approach, the lower threshold does not have to be adjusted and three of the container samples of Koons et al. fall into the "unclassifiable" range with no misclassifications.

   Second, the lower threshold of the Ca/Fe peak intensity inconclusive range may be changed from 24.0 to 23.0. This again avoids the misclassification of the one problem container glass and does not result in any change in the classification results previously reported for Ryland's 140 samples.
5. The success rate for classification of the samples of Koons et al. when using the six-element ICP approach is 98% with no misclassifications, while that for the SEM/EDX/XRF approach is 96% with no misclassifications. This is in line with the reported success rate of 93% for Ryland's samples when using only the SEM/EDX/XRF technique, again with no misclassifications. These results support the suggestion by Koons et al. that increasing the number of independent classifying elements improves the classification scheme; however, the improvement is not quite as dramatic as those authors had originally predicted.
6. The determination of low Mg levels by either approach serves to segregate 80 to 88% of the container glass specimens in the two studies.

This limited investigation affirms that both the SEM/EDX/XRF and the ICP approaches can be reliable in the sheet/container classification of small colorless glass fragments. It should be noted, however, that the sheet glass population encountered in the studies by Koons et al. and by Ryland apparently represent only modern window glasses (after 1940). The recent procurement and analysis of several domestic "old" window glass samples [*2*] corroborates the expected low levels of Mg reported by the British [*3–5*]. As German et al. suggest, consideration of Fe levels in these low-Mg sheets should permit recognition of these unusual samples [*5*].

SEM/EDX/XRF offers the advantages of being nondestructive, relatively fast, and available to many forensic science laboratories. The degree of discrimination afforded is hampered by reduced precision in the comparison of trace elements (titanium, barium, arsenic, strontium, and zirconium) caused by geometric effects in small irregularly shaped fragments, as noted by Howden et al. [*6*]. On the other hand, ICP spectroscopy offers advantages in discrimination within groups of similar types of glass (such as manufacturer or plant differences) as a result of its quantitative analysis capability unhampered by sample shape effects and its greater multielement capability. It is, however, more time intensive, requires a larger sample size, and is destructive to the sample. Of course, one should not overlook the discrimination power of refractive index and density. The ad-

ditional elements determined quantitatively by ICP, with good precision and accuracy, make a useful extension of the comparison and classification of glass fragments using physical, optical, SEM/EDX, and XRF methods.

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The Standardization of Handwriting Opinion Terminology

Sir:

(This article has been adopted as recommended guidelines in reports and testimony by the Questioned Document Section of the American Academy of Forensic Sciences and by the American Board of Forensic Document Examiners.)

Any attempt to standardize terminology evokes many reactions. Pet terms are called into question. Some feelings are hurt. People are expected to change a life-long collection of concepts, expressions, and thought processes. The authors of this paper strongly believe, however, that there is a need for some element of standardization, both to bring our profession in line with other forensic sciences and to make our expressions of opinions more universally understandable. Galinski and Nedobity state that "practically everyone today profits by the standardization of terminology, without necessarily being aware of it" [*1*]. We have attempted to avoid many of the pitfalls that normally go with such a project by using common expressions that are already familiar to a majority of document examiners.

The Case for Probability Statements

Document examiners should always begin their handwriting examinations from a point of complete neutrality. As an examination progresses, the examiner's mind goes through a process of detecting and comparing features of writing and, more importantly, of

evaluating their significance. The mind does not suddenly jump from the neutral zone (or zero point) to, for example, an identification, nor does it skip in uniform steps through a carefully defined scale. Instead, the mind moves steadily, but with some irregularity, through an infinite number of gradations of opinion toward an identification or an elimination until it reaches that point or until the "needle" stops short of that point. It is in those cases wherein the opinion is less than definite that careful attention is especially needed in the choice of language used to convey the weight of the evidence.

Document examiners have long debated the question of whether or not qualified opinions should be reported. Previous papers by McCarthy [*2*], McNally [*3*], Hilton [*4*], Cole [*5–8*], Schmitz [*9*], Duke [*10*], and others have amply covered both sides of the issue, and further debate of that subject is not the primary purpose of this paper. The fact is that a vast majority of document examiners now give qualified opinions. Recent studies by Decker [*11*] and Leung and Cheung [*12*] show that from 82 to 90% of the document examiners surveyed stated that they used qualified opinions. These surveys, as well as an informal survey by the authors of this paper, have also shown that the terminology used in reporting qualified opinions is both divergent and confusing. In some cases, several different terms are used to mean approximately the same thing, whereas in other cases different examiners use the same (or similar) terms to mean different things.

The need for some form of standardization in the choice of terminology used in reporting qualified opinions has long been recognized, especially by young examiners, who themselves are searching for the right words to express their opinions exactly. The problem is compounded by what could be called "probabilophobia" on the part of some examiners. Before there is any hope of standardization of terminology, we must recognize probability statements as legitimate and necessary forms of conclusions. Most of the arguments against probability statements stem from the erroneous conviction that probability is only a function of mathematics.

Our arguments for the use of probability statements are the following:

1. While probability is usually contrasted with certainty, both terms apply to judgments about things or events, not to the things or events themselves. Judgments with relatively complete confidence are termed **certainty**, while those with a lesser degree of confidence are termed **probability**. Wolf [*13*] suggests that we could understand this better if we regarded certainty as the limiting case of probability. Then we would have a continuous scale of probability, varying from the lowest to the highest degree of confidence. This should drive home the fact that even our so-called definite statements of identification are actually statements of probability.

2. Science and life itself are based on degrees of probability. Twentieth century philosopher Elton Trueblood [*14*] has stated,

> The fact that we do not have absolute certainty in regard to any human conclusion does not mean that the task of inquiry is fruitless. We must, it is true, always proceed on the basis of probability, but to have probability is to have something. What we must seek in any realm of human thought is not absolute certainty, for that is denied us as men, but rather the more modest path of those who find dependable ways of discerning different degrees of probability.

3. Probability is accepted not only by philosophers, but also by other scientists, including forensic scientists. If we are to fall in line with other scientists, we must use probability statements.

4. Probability statements and other qualifying terms are not new in handwriting examinations. We offer the following citations from books on the subject by Ames [*15*], Osborn [*16*], and Mitchell [*17,18*] published during the first quarter of this century:

> The degree of certainty of an expert's conclusion must, of course, be proportionate with his skill to detect, and with the skill of the perpetrator to conceal his identity [*15*].
>
> The degree of certainty of proof of forgery, or proof of identity through handwriting, necessarily differs enormously in differing circumstances and ranges all the way from mere conjecture to positive proof [Here a footnote is indicated, which reads, in part,] it seems that this kind of evidence, like all probable evidence, admits of every degree of certainty from the lowest presumption to the highest moral certainty [*American and English Encyclopedia of Law*, 2nd ed., Vol. 15, 1903, p. 283] [*16*].
>
> As with all other evidence, conclusions from the characteristics of handwriting can only be drawn with a degree of probability varying with the circumstances, and it is for the Court to decide what degree of probability should be attached to this or any other evidence [*17*].
>
> . . . there is an increasing tendency for the services of the trained student in handwriting to be utilized in the proper manner—that is to say, in pointing out the characteristics of the writing in question, and stating the degree of probability attaching to the conclusions drawn from them [*18*].

5. Probability statements are necessary in science (including handwriting comparisons) because evidence varies on a continuum in strength, clarity, and relative significance.

6. Probability, as used in handwriting opinions, is not a statistical measurement but a measurement of the examiner's confidence, based on scientific principles and experienced judgment, that the opinion rendered is correct. This is true because probability relates to qualitative as well as quantitative processes. Wolf [*13*] recognized this and stated that "not all cases of probability are really measurable, not even those cases in which differences of degree are readily distinguishable."

7. The courts need to hear the expert opinion, regardless of its degree of certainty, and they have not opposed the use of probability, having held in *Wantland v. State* [413 A.2d 1396 (1980)] that a medical expert need not have absolute certainty in expressing his opinion, but only a reasonable probability. Scott [*19*], in his scholarly treatise, lists several additional citations wherein the courts supported probability statements and other qualified expressions in testimony.

8. There is *no* connection between the degree of certainty an expert holds and legal standards of proof (such as "beyond a reasonable doubt" or "preponderance of evidence") [*20*]. Although Ellen [*21*] and Grant [*22*] wondered whether some degrees of certainty were sufficient for conviction, it is in fact the *courts* that will assign the weight the testimony deserves.

9. Probability statements lend credibility to expert opinions because they reflect caution, a conservative attitude in making very important judgments.

10. Our final argument is simply a quote from *Black's Law Dictionary* [*23*]:

> *Probability.* Likelihood; appearance of reality or truth; reasonable ground of presumption; verisimilitude; consonance to reason. The likelihood of a proposition or hypothesis being true, from its conformity to reason or experience, or from superior evidence or arguments adduced in its favor. A condition or state created when there is more evidence in favor of the existence of a given proposition than there is against it.

Suggested Terminology

Some guidelines are in order regarding the choice of terminology used in reporting our findings. McAlexander [*24,25*], Ellen [*21*], and Purdy [*26*] all have published papers giving suggested terminology, but the suggestions of all appear to be somewhat restricting or incomplete. Leung and Cheung [*12*] suggest a numerical scale that, in our opinion, simply will not work outside a relatively small area in which the criteria can be easily controlled and the consumer can be readily educated.

An infinite number of degrees of probability is possible between the indeterminable and certain poles of the opinion range; however, common sense dictates that we must limit the terminology we use in expressing our degrees of confidence in the evidence to terms that are readily understandable to those who use our services (including investigators, attorneys, judges, and jury members), as well as to other document examiners. We must be careful that the expressions we use in separating the gradations of opinions do not become strongly defined "categories" that will always be used as a matter of convenience; instead, these expressions should be guidelines without sharply defined boundaries. The authors agree with Purdy, however, that our reports "should follow the principles of brevity, clarity, precision and accuracy" [*26*].

Before getting into the suggested language of opinions, a few comments are in order regarding the word choices and sentence structures of the examples given. Some examiners feel that first person personal pronouns should not be used in reports covering the scientific examination of evidence and that, although the examiner is expressing his or her personal conclusion, the use of the pronouns "I" and "my" implies a totally subjective process and, further, implies that an equally competent examiner considering the same evidence might arrive at a different conclusion. On the other hand, other examiners feel that the use of first person pronouns is more direct in that it recognizes that handwriting examination is the most subjective of the forensic sciences, clearly assumes responsibility for the opinion, and leaves room for legitimate differences in the gradations of opinions by equally competent examiners. Since our main purpose is to suggest terminology that is readily understandable and both methods of reporting fulfill that requirement, examples of both are given. Also, these examples should not be regarded as the only ways to utilize probability statements in reports and testimony. In following any guidelines, the examiner should always bear in mind that sometimes the examination will lead into paths that cannot be anticipated and that no guidelines can cover exactly.

While the suggested language and definitions given follow closely those of McAlexander [*25*], the structure of the range of opinions is similar to that of the balanced scale of Purdy [*26*]. The following are recommendations for the terminology of opinions as well as examples and elucidating comments:

identification ***(definite conclusion of identity)***—This is the highest degree of confidence expressed by document examiners in handwriting comparisons. The examiner has no reservations whatever, and although he is prohibited from using the word "fact," he is certain, based on evidence contained in the handwriting, that the known writer actually wrote the writing in question.

Examples: It has been concluded that John Doe wrote the questioned material, or it is my opinion [or conclusion] that John Doe wrote the questioned material.

strong probability ***(highly probable, very probable)***—The evidence is very persuasive, yet some critical feature or quality is missing so that an identification is not in order; however, the examiner is virtually certain that the questioned and known writings were written by the same individual.

Examples: There is a strong probability that John Doe wrote the questioned material, or it is my opinion that John Doe very probably wrote the questioned material.

probable—The evidence contained in the handwriting points rather strongly toward the questioned and known writings having been written by the same individual; however, it falls short of the "virtually certain" degree of confidence.

Examples: It has been concluded that John Doe probably wrote the questioned material, or it is my opinion that John Doe probably wrote the questioned material.

indications *(evidence to suggest)*—A body of writing has few features which are of significance for handwriting comparison purposes, but those features are in agreement with another body of writing.

Example: There is evidence which indicates (or suggests) that John Doe may have written the questioned material, but the evidence falls far short of that necessary to support a definite conclusion.

Note: This is a very weak opinion, and a report may be misinterpreted to be an identification by some readers if the report simply states, "The evidence indicates that John Doe wrote the questioned material." There should always be additional limiting words or phrases (such as "may have" or "but the evidence is far from conclusive") when this opinion is reported, to ensure that the reader understands that the opinion is weak. Some examiners doubt the desirability of reporting an opinion this vague, and certainly they cannot be criticized if they eliminate this terminology. But those examiners who are trying to encompass the entire "gray scale" of degrees of confidence may wish to use this or a similar term.

no conclusion *(totally inconclusive, indeterminable)*—This is the zero point of the confidence scale. It is used when there are significantly limiting factors, such as disguise in the questioned and/or known writing or a lack of comparable writing, and the examiner does not have even a leaning one way or another.

Examples: No conclusion could be reached as to whether or not John Doe wrote the questioned material, or I could not determine whether or not John Doe wrote the questioned material.

indications did not—This carries the same weight as the **indications** term above; that is, it is a very weak opinion.

Examples: There is very little evidence of significance present in the comparable portions of the questioned and known writings, but that evidence suggests that John Doe did not write the questioned material, or I found indications that John Doe did not write the questioned material, but the evidence is far from conclusive.

probably did not—The evidence points rather strongly against the questioned and known writings having been written by the same individual, but, as in the **probable** range above, the evidence is not quite up to the "virtually certain" range.

Examples: It has been concluded that John Doe probably did not write the questioned material, or it is my opinion that John Doe probably did not write the questioned material.

Note: Some examiners prefer to state this opinion: "It is unlikely that John Doe wrote the questioned material." We see no strong objections to this, as "unlikely" is merely the Anglo-Saxon equivalent of "improbable."

strong probability did not—This carries the same weight as **strong probability** on the identification side of the scale; that is, there is a virtual certainty that the questioned and known writings were not written by the same individual.

Examples: There is a strong probability that John Doe did not write the questioned material, or in my opinion it is highly probable that John Doe did not write the questioned material.

Note: Certainly those examiners who choose to use "unlikely" in place of "probably did not" may wish to use "highly unlikely" here.

elimination—This, like the definite conclusion of identity, is the highest degree of confidence expressed by the document examiner in handwriting comparisons. By using this expression, the examiner denotes no doubt in his opinion that the questioned and known writings were not written by the same individual.

Examples: It has been concluded that John Doe did not write the questioned material, or it is my opinion that John Doe did not write the questioned material.

Note: This is often a very difficult determination to make in handwriting examinations, especially when only requested exemplars are available, and extreme care should be used in arriving at this conclusion.

In most instances wherein the opinion is less than definite, there is a necessity for additional comments, consisting of such things as reasons for qualification (if the available evidence allows that determination), suggestions for remedies (if any are known), and any other comments that will shed more light on the report. The report should stand alone with no extra explanations necessary.

Troublesome Expressions and Concepts

In sifting through around five dozen different expressions of conclusions used by document examiners participating in our informal survey and those of Decker [*11*] and Leung and Cheung [*12*], we found several expressions that were troublesome because of either bias, lack of clarity, or fallaciousness. Some of the terms are so blatantly inane (such as "make/no make") that they will not be discussed. Others are troublesome only because they are incomplete or misused. The following are some of these expressions:

possible/could have—These terms have no place in expert opinions on handwriting, because the examiner's task is to decide to what degree of certainty it can be said that a handwriting sample is by a specific person. If the evidence is so limited or unclear that no opinion—definite or qualified—can be expressed, then the proper answer is that outlined in the **no conclusion** section above. To say that the suspect "could have written the material in question" says nothing about probability and is therefore meaningless to the reader or to the court. The examiner should be clear on the different meanings of "possible" and "probable," although they are often used interchangeably in everyday speech.

consistent with—There are times when this expression is perfectly appropriate, such as when "evidence consistent with disguise is present" or "evidence consistent with a simulation or tracing is present," but "the known writing is consistent with the questioned writing" has no intelligible meaning.

could not be identified/cannot identify—These terms are objectionable not only because they are ambiguous, but also because they are biased in that they imply that the examiner's task is only to identify the suspect, not to decide whether or not the suspect is the writer. If one of these terms is used, it should always be followed by "or eliminate[d]."

similarities were noted/differences as well as similarities—These expressions are meaningless without an explanation as to the value or lack of value of the similarities or differences due to certain conditions which, likewise, should be spelled out. These terms should never be substituted for gradations of opinions.

cannot be associated/cannot be connected—These terms not only reflect bias but are also too vague and have no counterpart suggesting that the writer cannot be eliminated either.

no identification—This is another expression that could mean anything from a strong probability that the suspect wrote the questioned writing to a complete elimination.

It is not only confusing but also grammatically incorrect when used informally in sentences such as, "I no identified the writer" or "I made a no ident in this case."

inconclusive—This is commonly used synonymously with **no conclusion** when the examiner is at the zero point on the scale of confidence. A potential problem is that this term to some people means something short of definite (or conclusive), that is, any degree of probability, and the examiner should be aware of this ambiguity.

positive identification—On the surface, this term seems to suggest that some identifications are more positive than others.

[strong] reason to believe—There are too many definitions of *believe* and *belief* that lack certitude, and it seems more appropriate to testify to our conclusion (or expert opinion) than to our belief, so why use that term in a report?

qualified identification—We do not qualify our identifications. We do often qualify our opinions when the evidence falls short of an identification or elimination.

In addition to using terms which can be troublesome, we sometimes use reasoning which is faulty and which affects our judgment. One example of this is examiners who use the **strong probability** degree of certainty as a harbor of refuge when they are certain of authorship but feel that they would have difficulty in convincing a jury. Todd [*27*] states,

> If the premise is correct about the differences between an expert and a layman [that the expert can recognize and, in some instances, discount the effects of transitory or permanent factors that superficially change writing], then no doubt we will occasionally have a case with evidence that provides a basis for a positive conclusion, but which would not lend itself *readily* to illustration to others, especially to those on a jury.

We are justified in identifying writing when the evidence is so strong that we are completely convinced that the suspect wrote the questioned writing, regardless of whether or not we may be able to convince others who do not have our experience and expertise in this field. That should have no bearing whatever on our opinion.

The task of the document examiner is to evaluate the evidence presented for examination and render only an opinion that is justified by the nature and amount of evidence that is present. We should make no apology should that evidence fail to support a definite conclusion, as the insufficiency of evidence is not the fault of the examiner. However, a common and justified criticism of qualified conclusions in handwriting examinations is that an examiner will try to "sell" them as definite (or, at least, stronger) conclusions in subsequent testimony. McAlexander [*28*] states,

> The use of qualified opinions by document examiners puts an added burden on us to be extremely careful in testifying as to our findings. We owe it to ourselves, to our profession, and to the criminal justice system to be certain that our testimony reflects our written opinion exactly. Anything less is contemptible.

It should be explained to the court or jury that some doubt is present or else a definite conclusion would have been expressed. It should be further explained, however, that the doubt arises from the insufficiency of evidence rather than the presence of evidence that points to an opposite conclusion.

Another justified criticism of qualified conclusions is that they are articulated by different examiners in many different ways, and neither colleagues nor the users of our services have a clear understanding of what is meant by such conclusions. That is the issue that has been addressed in this paper.

Summary

The terminology used by document examiners in reporting results of handwriting comparisons differs greatly among examiners. In some cases, the same terminology may have different meaning to two different examiners; in other cases, different terminology may have the same meaning to two different examiners. This study is an attempt to get us all on the same track so that we all mean the same thing when we use terms that express our opinions and so that the language we use to express those opinions is clear, concise, and accurate.

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