Letters to the Editor

Drug Concentrations and Driving Impairment—Consensus Report

Dear Sir:

The National Institute on Drug Abuse sponsored a conference on drugs and driving in October 1983. The objective of the conference was to reach a consensus on several key issues associated with the state of knowledge about the relationship between body fluid concentration of drugs and degree of driving impairment. It was also of interest to ascertain whether a sufficient body of knowledge existed for an expert to form an opinion, which would meet the applicable standards of proof for legal proceedings that a person's driving ability was impaired based on body fluid concentrations of a drug. The consensus development panel report was published [*1*].

In November 1987 a questionnaire was mailed to each of the panel members. It contained the following questions:

1. Do you know of any studies, reports, or other evidence that could be used to correlate drug concentrations and driving impairment? Panel members comments were:

> I believe that issues relating to interindividual variation, polydrug abuse, chronic vs. acute use, and tolerance will continue to be confounding and prevent per se judgements. On the other hand, I believe enough is known to permit a reasonable expert opinion when drug levels are considered in conjunction with other information. (PIJ).

> I think if one picks a high enough level (15 ng/ml of THC in blood for instance), there would be general agreement among experts that impairment is probable. This is not a practical presumptive level on which to base legislation however, since most positives encountered in forensic situations will be lower than this concentration due to the metabolic elimination of THC from the blood and the difficulty in getting a sample after an accident or interdiction. (RLH).

> Many years of research were required to develop rebuttable presumptions for driving while under the influence of alcohol; the effects of other drugs on even simple physiological effects is still in its infancy. (RTC)

2. Is the text under "Determinations in Urine" still valid? Each respondent answered "yes."

Comment: The second sentence of that paragraph was "Inferences regarding the presence of systemic concentration of the drug at the time of driving or impairment from drug use are generally unwarranted." It was suggested that "in the absence of other clinical evidence" be added. (YHC).

3. Are the conclusions and recommendations of the consensus still valid? Each respondent answered "yes."

The following general comments were made:

> Valid analytical methods of necessary sensitivity now exist for the major drugs of interest. (KMD).

> The use of drugs and/or alcohol is only one, possibly not major, factor that influences drivers. We zero in on it because the research is fundable and we can promote laws. Important factors such as skill, equipment quality, motivation etc., are not amenable to law. (RBF).

Members of the consensus panel and respondents were: Robert V. Blanke, Ph.D., Richmond, VA; Yale H. Caplan, Ph.D., Baltimore, MD; R. Thomas Chamberlain, Ph.D., J.D., Memphis, TN; Kurt M. Dubowski, Ph.D., Oklahoma City, OK; Bryan S. Finkle, Ph.D.,

San Francisco, CA; Robert B. Forney, Ph.D., Indianapolis, IN; Richard L. Hawks, Ph.D., Rockville, MD; Leo E. Hollister, M.D., Houston, TX; Peter I. Jatlow, M.D., New Haven, CT; Roger P. Maickel, Ph.D., W. Lafayette, IN; and Arthur J. McBay, Ph.D., Chapel Hill, NC.

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Reference

[1] Blanke, R. V., "Consensus Development Panel: Drug Concentrations and Driving Impairment," *JAMA*, Vol. 254, No. 18, 8 Nov. 1985, pp. 2618-2621.

Authors' Response to Discussion of "Bite Mark Standard Reference Scale—ABFO No. 2"

Dear Sir:

We are responding to Dr. James Ebert's letter to the editor entitled "Discussion of 'Bite Mark Standard Reference Scale—ABFO No. 2'," which appeared in the March 1988 issue of this journal (Vol. 33, No. 2, March 1988, pp. 301-304). He is correct in his statement that we disagreed with his comments in his review of our paper to the extent that the article was published without regard to his suggested revisions. We did, in fact, carefully consider each of his comments in finalizing our paper but concluded in each instance that it did not add to the clarity or accuracy of our technical note dealing primarily with the techniques of bite mark photography. We were also influenced by the following statement in which he first applauds the development of the ABFO No. 2 scale then disqualifies himself as an expert in the actual techniques of bite mark photography: "I *do* think that the scale the authors have devised is just great—I would like to have a few of them myself, in case I ever got to photograph primary bite mark evidence. In my involvement with some 400 bite mark photograph cases, I have never had that opportunity." We did appreciate this reviewer's candor in admitting to his own inexperience in the practical aspects of experimental and clinical bite mark photography, but this admission did raise serious questions in our minds as to the credibility of his criticisms. Furthermore, it is important to point out that there was a second reviewer whose suggestions were incorporated into the published version of our paper. We feel it is appropriate to publish this second reviewer's comments here: "This is a very well-organized and written paper, on an important improvement in technical procedure. I do suggest consideration of the possible changes, indicated in pencil on the manuscript, during a final clean-up of the text."

It will be obvious to readers knowledgeable in bite mark photography who carefully read both our technical note and Dr. Ebert's letter why we chose to disregard his suggestions; but for the benefit of the casual reader and those who may not be familiar with these techniques, we feel compelled to respond directly to his letter and address each of his concerns individually.

First, he feels that our definition of "metric analysis" in the first paragraph of our paper is misleading. The use of the term *metric analysis* is so common to so many disciplines of scientific photography (including engineering, biology, anthropology, astronomy, and so on) that any further attempts at elucidation here beyond that already provided in our paper would be redundant and unnecessary. We fully recognize that human tissue is not a structurally stable substrate for a bite mark, but the prerequisite for any attempts whatsoever at bite metrology is a suitable scale. The scale we describe in this paper may not be the ultimate answer, but it has been widely accepted by practitioners in the bite mark field as a positive

step in the right direction. The following quotation from the frontispiece of Gomer T. McNeil's classic text *Photographic Measurements* summarizes our thoughts on this subject: "It is better to light one candle than to curse the dark."

Dr. Ebert further points out that ". . . a workable and adequate photo scale . . . ." is not often included in bite mark photographs and that odontologists and ABFO members rarely have an opportunity to photograph actual bite mark evidence. He also states that in perhaps 400 bite mark cases in the last ten years in which he has personally been involved ". . . both in (his) testimony and that from the 'other side,' actual measurements have rarely been very conclusive." We are hopeful that the availability and proper application of the ABFO No. 2 scale will help to rectify this unfortunate situation. If the decision of selecting a workable and adequate photo scale is left up to the whims of any photographer who happens to be on the scene at the time, as has often been the case in the past, we can hardly expect to see any significant improvements in the information content of bite mark photographs in the future.

He is critical of our technique of using a mirror to achieve parallelism between the scale and the film plane. We fully recognize that the surface containing the bite mark will in most cases be displaced from the plane of the scale and will more than likely be curvilinear in shape. Again, when making any attempt to quantify a bite mark by means of photography, or any other metrological technique, it is highly important to eliminate as many variables as possible. Our objective in suggesting the mirror as an autocollimator is to eliminate the variable of nonparallelism between the scale and the film plane. By so doing, the analyst can neglect this source of error entirely and direct his/her attention to those variables over which he/she has had no direct control.

He does not completely understand the ". . . use of a clip and arm jig to hold the ABFO No. 2 scale on or above the bite mark." The explanation is quite simple. Any attempt to hold the scale with thumb and fingers during actual photography is just not good photogrammetric practice. A mechanical device of the type described in our paper is required so the scale can be optimally aligned with the bite mark (or the portion thereof to be photographed) and clamped securely in that position. This preliminary procedure is especially important when a mirror is to be used for the purpose of aligning the camera with the plane of the scale by means of the autocollimation method discussed above.

Furthermore, he does ". . . not understand how shadows are going to be prevented by aligning a spotlight or flashlight along the same axis as the flash illumination." It is apparent that he either did not read our paper carefully or he is uninformed in the widespread use of modeling lights by photographers to *preview* the shadow formation on subjects illuminated by electronic flash sources, as our paper specifically states. Continuing to quote Ebert, "This would cause the same shadows that the flash would." This, of course, is the very idea behind modeling lights. We do not propose that the subject area be illuminated *simultaneously* by spotlight *and* electronic flash. Instead, the spotlight is to be used before photography as a means of *previewing* the effects of the flash on the subject matter.

He asks the question, "Are the accuracy figures the authors give derived from measurement of the printed ABFO No. 2 scale?" The answer to that question is *yes*.

He challenges the practicability of rectifying prints by tilting the enlarging easel using the ABFO No. 2 scale as a rectilinear reference. He is incorrect in his statement,

> Using a special scale and rectifying the resulting prints, however, does not correct in any way for the "topographic" variations in the subject—that is, those geometric properties that result from the photograph being taken of a real world "scene" on a curved area of the body, and therefore having areas that are varying distances from the principal point of the lens. The ABFO No. 2 scale does not help correct for those real "topographic" problems.

His phrase ". . . does not correct *in any way* for the 'topographic' variations in the subject . . ." is grossly misleading. Theoretically, perfect rectification is attainable in the plane

of the scale and *partial* or somewhat-less-than-perfect rectifications are possible on surfaces falling on either side of that rectified plane. The accuracies to which these out-of-plane surfaces are rectified depend upon their orientations and distances relative to the rectified plane and upon the optical geometry of the photographic setup. It is possible in practice to rectify preferentially a plane surface that falls outside of the plane of the scale if the spatial relationships between the two planes are known. We reported on these effects in our Bite Mark Breakfast talk in Philadelphia in February 1988. These, and other considerations in using the ABFO No. 2 scale, will be covered in a published paper which we are currently preparing. In actual practice, rectification *in the plane of the scale* can be achieved to an accuracy of approximately $\pm 1\%$ for oblique camera angles as large as 45°. This is not accomplished by seeing "how many oddly shaped objects that one can lay hands on in the dark can be placed under the enlarging easel without ever bringing it to *quite* the proper tilt" as Dr. Ebert suggests. Rectification is achieved in the darkroom by a much more scientific and systematic approach than the haphazard procedure described in his letter. A detailed description of the method we use is beyond the scope of this letter but will be included in our forthcoming paper.

His attention is finally directed to our Fig. 5 showing a gridded and ungridded photograph. He notes that the most deviant "square" was distorted by about 3% and asked the question, "Was it rectified?" The answer to this question is *no*. Gridding a photograph as described in our technical note is suggested as a simple alternative to the rectification procedure discussed above. The same limitations apply to both of these methods for surfaces that fall outside the plane of the scale.

In conclusion, a recent case in rural Kansas demonstrates the merits of ABFO No. 2. The pathologist at autopsy provided the crime scene officer with an ABFO No. 2 reference scale to use in photographing a bite mark. Having no special knowledge of its use, but based on his training and experience, he placed it in a plane coincident with the most common plane of the bite mark. Examining the resultant photographs, it was immediately evident to the author (T. C. K.) that there was significant angular distortion from camera placement error resulting in misrepresentation of the bite mark pattern. Following written instructions and with discussion over the phone, the officer was able to rectify the angular distortion in his darkroom without special equipment producing a verifiably acceptable bite mark photograph. Whether an odontologist chooses to work only with pattern recognition or include some metric analysis, use of the ABFO No. 2 reference scale can contribute significantly to his understanding of the bite mark and enhance the credibility of his conclusions.

We believe that, properly used, the ABFO No. 2 reference scale is a significant adjunct to forensic science, particularly in evidentiary close-up photography.

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