

Authors' Response

Sir,

We have had the opportunity to review the letter to the editor by Varun Arora et al. ("the Writers") concerning critiques in our article, "A New Digital Method for the Objective Comparison of Frontal Sinuses for Identification" (1), published in the *Journal of Forensic Science* in July 2009;54(4):761-72 ("the Article").

Forensic science, as with all sciences, benefits from critiques and criticisms of previous studies. It is through this process that science advances. The benefit, however, is dependent upon the critique providing something new to the field, unaddressed in the previous work, or results of a related experiment, all in a clear, coherent and accurate fashion.

The Authors appreciate and welcome criticisms and suggestions on ways to improve the proposed technique with regards to its validity and practicality; however, many of the arguments put forth in the critique are unclear and lack support. They also demonstrate a misunderstanding of several key points of our study. These concerns are addressed individually.

Under the heading "Use of Radiograph for two assessments," the Writers state that "to simulate the experiment properly, two radiographs with a proper time gap should have been used." The Authors do not dispute that ideally the experiment would have involved the use of a large set of skulls, each radiographed twice. This limitation is discussed at great length on page 769. As such, the Authors take issue with the suggestion that in some way this was a failure on the Authors' part to conduct the experiment "properly." There is nothing improper about conducting an experiment with limitations, so long as the limitations are clearly stated, and the results obtained are qualified accordingly.

Under the heading "Repeat Tracings," the Writers appear to be criticizing the use of replicate tracings to assess intra- and inter-observer reliability, suggesting that the proper method would have been to use two different radiographs. The Authors disagree with this suggestion. Using replicate tracings allowed the authors to isolate variation introduced by establishing the origin, tracing, and taking measurements in Adobe Photoshop[®], to effectively measure interrater and intrarater reliability. The use of Pearson's correlation coefficient was seen as an adequate statistical tool to demonstrate consistency in the application of the technique both between and within observers.

In the same vein, the Authors disagree with the statement: "... despite having significant differences between two pairs, there can be a strong correlation between two observations." Performing the technique twice on the same sample was intended for this very purpose to determine whether there was a strong correlation between two observations of the same sample. An assessment of intra- and inter-observer reliability determines whether the technique can produce consistent results between two observations by the same observer and between observations by two different observers, respectively. It does not, nor was it intended to, speak to the ability of the technique to draw conclusions regarding differences among pairs. The Writers' comments indicate a misunderstanding of the purpose of these assessments.

Under the heading "Method of Measurement," the Writers purport to have conducted an "experimental study" using the technique described in the Article. The Writers provide no particulars of their study, other than their conclusion, which calls

into question the validity of the Article. This is inconsistent with a scientific critique as it provides no way of assessing these data. For example, it is unclear to the Authors how "the shape of the sinus was [the] same [,] yet the size of the tracings did not match..." There is no mention of the magnitude of the size variation or discussion of whether this difference affected the ability to discriminate between pairs. Owing to this lack of information, it cannot be ruled out that the inconsistencies encountered by the Writers were the result of a failure to properly apply the technique.

Further, this paragraph also discloses concerns with how closely the Writers scrutinized the Article before preparing the Letter. The Writers state: "[The] [l]ocation of [a] reference point to start the measurements needs a good [,] objective [criteria]." On page 763 of the Article, under the heading "Tracing Procedure," the Authors describe, in detail, how an observer is to establish the origin (reference point) for measurement, using bony landmarks such as the superior margin of the orbits, nasion, nasospinale, frontal crest, and crista galli. It is difficult for the Authors to imagine more objective criteria than those proposed. The Writers provide no suggestions in this regard.

Under the heading "Methodology to calculate total difference (SS and DS)," the Writers express concern regarding the use of absolute values, stating that "...two differences in opposite directions may produce a net difference of zero... this possibility has not been taken into account." This argument is contradictory, because the entire purpose of using absolute values was to capture the variation that would otherwise be lost by having two numbers equal in magnitude but in "opposite directions."

Under the same heading, the Writers criticize using summation of differences between the measurements and propose that proportional difference be used instead. The Authors agree that a portion of the variation is lost by summing the differences (discussed at page 769 under "Discussion"), and it is possible that the use of proportional differences may increase the discrimination power of the technique. The Writers should consider applying this method in a follow-up experiment or request the raw data collected by the Authors to use.

It should be noted that this paragraph also contains an error, demonstrating a misunderstanding of our technique; the Writers state that 61 measurements are used when, in fact, only 59 are used (see page 763 of the Article under "Image Analysis"). This error provides evidence of the possibility that the Writer's may have obtained inconsistent results because of incorrect methodology. Again, inadequate information was provided about their "experimental study" to properly make this assessment.

This error is carried forward into the next paragraph, under the heading "Practicality/Complexity of Proposition." In this paragraph, the Writers criticize the Authors' choice of 61 [59] measurements and question why the Authors have not tried to reduce the number of measurements to a "reasonable level." Although the Writers cite three sources in support of their position, none of the articles use the Authors' technique, making it difficult to draw any conclusion with respect to the minimum number of measurements necessary for discrimination. As such, it is unclear to the Authors how the Writers determined what would be a "reasonable level." This statement also fails to appreciate that the Authors' experiment was an initial investigation into a new technique for identification and that refinement of the technique was clearly necessary before it could be used in casework.

Under the same heading, the Writers discuss the impracticality of applying the technique in a hypothetical scenario involving a mass tragedy of 50 victims. It seems that the Writers are suggesting that to identify an individual, 61 [59] measurements of all 50 victims need to be collected. This demonstrates a misunderstanding of the intensions of our technique. This would only be necessary if frontal sinus morphology was being used to identify all 50 victims as the sole means of identification, and each case required statistical support of a correct match, which is unlikely. If only a few of the victims were being identified based solely on frontal sinus morphology, then only measurements from their frontal sinus' would be necessary with the use of acceptable, previously established reference distributions (see page 770 for discussion on sex-specific and population-specific reference distributions). Our technique is intended for use when identification is being contested, and a statistical substantiation is required.

It is also unclear of the basis for the Writers' suggestion that "the authors should have marked the areas with maximum differences and similarities. Help of principal component analysis for the purpose could have been taken [sic]." The Authors appreciate the suggestion of a different statistical approach that could improve the technique; however, we feel that this is an interesting avenue for future research rather than a shortcoming of our previous research.

The authors also take issue with the statement in the final paragraph of the letter, wherein the Writers state: "In view of the above evidence, we find the technique[,] despite [having] been shown to be having [sic] promising results by the authors, impractical and difficult to learn with too many complexities." This statement is poorly articulated, and the apparent "evidence" they provided is not clearly outlined in the letter.

The Writers provide some interesting and potentially useful suggestions for future research, including principal component analysis and proportional matching. However, the Authors main-

tain the view that the concerns raised by the Writers were not fully supported. Specifically, the writers (i) failed to provided sufficient information of their unsuccessful "experimental study," especially details as to the problems encountered (were they the result of incorrect methodology?); (ii) demonstrated a poor understanding of the purpose and methodology of our technique; and (iii) seemed to pay little attention to limitations discussed in depth in our study.

The Authors' proposed technique for quantitative comparison of frontal sinus morphology allowed us to statistically demonstrate that the frontal sinus is adequately variable for reliable identification and was successful in providing statistical substantiation of a correct match versus nonmatch. We acknowledge that this work was a preliminary study, and there is need to further refine the technique to render it applicable in real casework. We also acknowledge the benefits of improving its practicality. The Writers seem to have some useful input on how to improve the technique; however, it is our view that many of their arguments lack support.

Reference

1. Cox M, Malcolm M, Fairgrieve SI. A new digital method for the objective comparison of frontal sinuses for identification. *J Forensic Sci* 2009;54:761-72.

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