A Response to a Critic of the Critics

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The author of the website *www.bitemark.org* posted an article regarding the admissibility of bitemark evidence in several cases in Texas, and spent some time discussing the supposedly 'asinine' nature of applying experimental scientific methodology to forensic science. The article makes the point that the scientific method should not apply to some disciplines, as they are not 'hard' sciences, like physics and chemistry. This commentary represents an example of why critics of forensic science find these disciplines particularly frustrating, in that they attempt to justify their forensic practice on the basis that they are somehow 'different' or 'immune' to good scientific practice. But there is no logical reason why forensic science and the scientific method should be mutually exclusive.

The author begins this part of his discussion by referring to the decision in *Coronado v Texas* [1], which held that not all 'science' should be considered equal in as far as its theories and practices. There is much to say about the *Coronado* ruling, most of which should be reserved for a different forum, however, suffice to say that the majority opinion in this case has created more problems for itself than it realises by attempting to draw a line between standards for 'hard' versus 'soft' sciences. Despite the fact it references *Daubert*[2] and *Kelly*,[3] both of which hold independently that in order for expert testimony to be considered reliable, and hence admissible: (1) the underlying scientific theory must be valid; (2) the technique applying the theory must be valid; and (3) the technique must have been properly applied on the occasion in

[1] *David Cesar Coronado v. The State of Texas*, No. 05-11-00605-CR, Texas App., 5th Dist.; Tex. App. LEXIS 9405 (2012)
[2] *Daubert v. Merrell Dow Pharmaceuticals Inc*., 509 US 579 (1993)
[3] *Kelly v. State,* 824 S.W.2d 568, 573 (Tex. Crim. App. 1992)

question, the court affirmed a stance taken in *Nenno*[4] that some expert evidence is exempt from this requirement. They then went on to say that this type of expert evidence, of which they offer no definition (other than to say that it would be of the type that is considered 'soft science' – whatever that term means), can affirm its reliability by proving that: (1) the field of expertise is a legitimate one; (2) the subject matter of the expert's testimony is within the scope of that field; and (3) the expert's testimony properly relies on or utilizes the principles involved in that field.

These three principles as held by the Texas Court of Appeals badly undermines the US Supreme Court's elucidation of the term 'reliable' in *Daubert*, which Texas itself adopted as precedent, beginning with *Robinson*[5] in 1995. Read carefully, the Court of Appeals application of these three factors in *Coronado* amount to nothing more than what has been termed 'counting expert noses'. They established that the field of forensic odontology was legitimate by referencing the existence of the American Board of Forensic Odontology, a body recognized by the American Academy of Forensic Sciences. By this reasoning, one could conclude that the science of astrology is also valid, as there is an Organization for Professional Astrology, yet most courts would have difficulty applying the very same reasoning in such a case. This practice harks back to the pre-*Daubert* days of *Frye*,[6] where experts could claim to be experts on the basis of 'pulling themselves up by their own bootstraps': in other words, they were essentially 'self-validating'. This was one of the primary reasons for the over-ruling of *Frye* at the federal level, to ensure that experts were subject to a form of accountability beyond that of their own professional affiliation. If you ask another astrologer whether their practice is legitimate, what does one imagine that they will say? Similarly, asking the American Board of Odontology whether it feels

[4] *Nenno v. State,* 970 S.W.2d 549, 561 (Tex. Crim. App. 1998)
[5] *E.I. du Pont de Nemours & Co. v. Robinson*, 923 S.W.2d 549 (Tex. 1995).
[6] See *Frye v United States* 293 F. 1013 (D.C.Cir. 1923)

that it is a legitimate forensic body seems ridiculous. In fact, so much so that the court in *Coronado*, like other before it, didn't even bother to ask, it just noted the existence of such an organization and therefore assumed that such a field was 'reliable' on this basis.

The court in *Coronado* heard about the existence of several professional organizations that recognize the practice of odontology, they heard about how bitemark analysis was performed, and agreed that the method used in the case before it was considered acceptable in as far as a recognized methodology. But it completely failed to consider whether this method actually yielded appropriate results that could be relied upon to any degree. So where was the analysis of the underlying basis for the discipline in *Coronado*? The answer appears to be: there wasn't any, because forensic odontology is a 'soft science'.

This distinction between 'hard' and 'soft' sciences is difficult to determine, and virtually all who purport that there is a difference then go on to say that the distinction is difficult to make, so much so that they avoid making it entirely, relying instead on the reasoning that it is 'obvious' that bitemark analysis is a 'soft science'. The author of the bitemark.org website maintains that 'soft sciences', at least forensic odontology and bitemark analysis in particular, are so called because science is observational, rather than experimental. But then the question arises – why is it one and not the other? Is it observational because there is no way to test its theories through experimentation, or because it just lacks experimental data? It seems that the category of 'observational' science is, in reality, just an excuse for a category of science that lack robust data to support their current practice. Astrology is also an 'observational' science in this respect: astrologers supposedly report on horoscopes by 'observing' positions of various celestial bodies, yet few would claim it would have a place in legal proceedings.

The notion that forensic odontology is observational, and therefore is different to the experimental sciences is largely true, but I would argue that this distinction is artificial and borne of excuses rather than amounting to true difference in scientific philosophy. How does one suppose that 'experimental' sciences became that way? Science almost always began through *observation* of a particular phenomenon. But note that the use of pure observation to verify scientific theories is basically pure *induction*: a methodology generally considered inappropriate, by itself, for the acquisition of 'scientific' knowledge. The true distinction between forensic science and hard science is that while much of forensic science started with observation, unlike physics or chemistry it failed to progress to the next stage: that of assessing whether the observations held for any given circumstance. For example, it was through observation that in the middle ages that the notion that eating eels was one of the causes of scurvy.[7] In reality, eating eel did not cause scurvy, although this belief was held for many years, until James Lind performed an *experiment* in which he attempted to treat sailors afflicted with scurvy with various cures, among one of which was citrus fruit. Through this experiment, considered widely to be the first clinical trial, it was suggested (although largely ignored for another hundred years) that a lack of some dietary factor was responsible for causing scurvy, rather than caused by eating a particular food. Since Lind's time, many clinical trials in both humans and animals have been conducted in order to verify the claim that scurvy is caused by a lack of vitamin C, to the point where the evidence is now virtually conclusive.

Take even the 'hardest' of 'hard' sciences: physics. It too began with observations that progressed through experimentation into standard theories that now hold almost uncontroversial weight. Newton first *observed* that a prism split white light into a beam of

[7] Crusaders noted that scurvy epidemics often arose during Lent. During this time, eel was traditionally eaten as a substitute for meat. See David Harvie's *Limeys, The True Story of One Man's War against Ignorance, the Establishment and the Deadly Scurvy*, Sutton Publishing, Phoenix Mill UK (2002).

varying colors we now refer to as the 'visible spectrum'. This suggested that white light was itself made up of a number of colors (all of them, in fact). How did he prove that this was not simply his unverified opinion? He conducted a series of systematic *experiments* that ended up proving he was right not only to his satisfaction, but to the rest of the learned world at the time.

The author at *bitemark.org* maintains that the appropriate view of forensic science is an *epidemiological* one: involving a retrospective analysis of results. The distinction between epidemiological and experimental science as applied to forensic disciplines is flawed here for various reasons. The most important reason this argument is flawed is because one cannot rely on historical instances in order to prove the reliability of forensic science because in each case, ground truth is never known with certainty. In 1991, Ray Krone was convicted on the basis of a bitemark, so in a retrospective analysis conducted in, say, 1996 we could chalk this up as proof that association between an individual and a bitemark is not only possible, but accurate. But eleven years later, in 2002, it became apparent that this conviction represented a grave error, Ray Krone was innocent. Forensic evidence cannot be validated as 'reliable' from judicial casework because the judicial outcome is usually dependent on that forensic evidence to start with: the question of forensic science's reliability becomes a self-fulfilling prophecy.

The author further claims that because forensic events are unique, their circumstances can never be repeated, and so any experiment that attempts to duplicate one scenario is likely never to be applicable to any other. He appears to then make the leap that 'experimentation' would therefore be near useless. This is a very narrow approach to how science works. No experiment is ever perfect (that is why good scientists always express the likelihood of error, or at least discuss the possible limitations of their evidence in papers submitted for publication), nor does it always completely account for a single observed phenomenon. Modern science evolves through the accumulation of evidence, that when taken as a collective whole, allows theories to be verified, or discounted. Just because one experiment fails to account for every possible scenario does not mean that it is worthless. The calcium carbonate molecules in a test tube in Hungary are not the same calcium carbonate molecules in the test tube in Sweden, yet enough research has been done to categorically conclude that when combined with hydrochloric acid, carbon dioxide gas will reliably be produced. Chemists didn't verify this test with every sample of calcium carbonate, in all its various physical forms known to man, in order to establish that such a consequence could reliably be predicted.

It is true that forensic events are very difficult to reproduce in the lab. But again, no experiment in science ever attempts to replicate the exact conditions under which the world operates – this is already recognized as being beyond the realms of achievability in most areas of science. Nonetheless, experiments in bitemark analysis do generally try to replicate the conditions as best they can, in the experiments alluded to by the author by using actual models of dentition, biting into actual human flesh. Despite the fact that these conditions are not ideally representative of the real world, if we consider that the purpose of the bitemark experiments criticized in this article was to disprove that there was sufficient variation in bitemarks to be able to distinguish one from another, the laboratory conditions in these experiments actually favor the *null* hypothesis: in other words, these (non-realistic) conditions would assist in proving that there *is* sufficient variation. The author points out the fact that there is no 'struggle' by the victim, or that there is no 'vital reaction' in this experimental bite, but the absence of these conditions should make the resulting mark even more distinct, and *easier* to be related to a given dentition, not harder. While these conditions are not as relevant as it could be to real-life forensic work, they actually increase the likelihood that these bitemarks could be reliably related to dentitions in this experiment. Such bias 'towards the null' is often deliberately designed into scientific experiments, in order to further increase their overall validity. Unfortunately, despite the presence of factors which favor the null hypothesis, the experiments *still* demonstrate that distinction between similar dentitions is difficult, if not impossible, even on the basis of these 'near perfect' bitemarks.

The author maintains that the particular research group's methods in these experiments have not been verified as reliable. In actual fact, the method that they use to describe and compare the resulting bitemark with a dentition (a technique known as geometric-morphometric analysis) is supported as a method of geometric comparison by a wealth of mathematical and other scientific literature. Perhaps his concern is more that the method has not reliably been applied to bitemark analysis. This is arguably legitimate, however, an important distinction to note here is that they are not performing bitemark analysis *per se*, they are using a well-established method of geometric comparison to compare shapes of bitemarks to dentitions. The ability to distinguish and relate shapes of this nature is one of the fundamental tenets of bitemark analysis, but does not account for the whole practice itself, and it is only this particular aspect of bitemark analysis that they are attempting to challenge. How this then affects the overall theory of bitemark analysis is a conclusion that one is then free to draw oneself from these results.

This critical response to some of the only true 'scientific' research ever conducted on bitemark analysis belies the type of thinking that has got forensic science into trouble in the first place: a failure to understand the importance of scientific methods in modern scientific reasoning, and a perfunctory dismissal of its rightful place in forensic science. The distinction of 'soft' versus 'hard' sciences amount to nothing more than excuses for a lack of proper research data, and while judges may be happy with this distinction for the sake of advancing trials in a timely fashion, it is not one that the forensic science profession should be happy with if they are to truly consider themselves scientists.