Article type : Letter to the Editor

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**Commentary on**: Schuman MJ, Hutchins KD. Severe retinal hemorrhages with retinoschisis in infants are not pathognomonic for abusive head trauma. J Forensic Sci 2017;62(3):807–11.

Sir,

If one searches the terms “pathognomonic” AND “dogma” in pubmed, three articles are returned. One is a case report that highlights hypertrophic neuropathy in Noonan syndrome, and suggests that “NF1 diagnostic criteria need rethinking.” The second article entitled, “The Myth of the Fecalith” is a case report that “calls into question the venerable dogma surrounding the fecalith.” The third article is that of Shuman and Hutchins, recently published in the Journal of Forensic Sciences: “Severe Retinal Hemorrhages with Retinoschisis in Infants are [sic] Not Pathognomonic for Abusive Head Trauma”, which reports two cases that “challenge the dogma that severe retinal hemorrhages with retinoschisis are pathognomonic for AHT (abusive head trauma)”.

Words have meaning. Not lost on the reader is the presumption of blind faith attributed to a vague medical mainstream. In the article by Shuman and Hutchins, this is viewed as harmful, with the specter of wrongful incarceration palpable throughout the narrative. That this is

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concluded on the basis of anecdote and citations of low evidence quality is all the more remarkable, given the complexity of pediatric head trauma.

The data presented by Shuman and Hutchins leave substantial uncertainty in both of their cases. The "vascular malformation" is not independently interpretable as such from the images provided, while they appear to rely on CD31 and CD34 immunostains; these are not diagnostic of vascular formations of any kind (they simply stain endothelium). The term "vascular malformation" does not specify which malformation was identified (arteriovenous malformation, cavernous malformation, venous angioma, capillary telangiectasia, etc). This is not trivial, as each has a different clinical context. Any discussion of an association with subdural hemorrhage, retinal hemorrhage, retinoschisis, or, for that matter, rupture, is therefore conjectural. The fact that "arteriovenous malformation" (the only subtype of vascular malformation that may catastrophically bleed with any degree of plausibility) was not suggested, nor depicted, or even diagnosed by the neurosurgeon, leaves further doubt that some vascular lesion spontaneously ruptured. Based on the information provided, the reader is left with insufficient data on which to conclude a manner of death. Maybe the authors should take a second look at this too.

The second case is obviously a catastrophic accident. Noteworthy is that there was an occipital skull fracture which should have been described further. The presence of subarachnoid blood in the absence of cranial subdural blood, along with the fracture, tend to suggest impact injury and translational forces. That this was an accident is not a surprise. The diffuse spinal subdural hemorrhage, also not elaborated upon, suggests spinal trauma, especially in the absence of coagulopathy, suggesting in turn that the accident may have included injury mechanisms more severe than described in the narrative. Also, despite the title of the article, the gross pathology does not permit the interpretation of retinoschisis *per se* in either case, although given the severity of traumatic hemorrhagic retinopathy, traumatic retinoschisis would not be surprising.

The uncertainty of these cases notwithstanding, the unfortunate adversarial tone is what sets this article apart. The selective quote attributed to Dr. Levin and the American Academy of Ophthalmology (AAO) in the introduction appears as an attempt to target Dr. Levin personally and highlight what the reader should view as an unsupportable claim. Yet no meaningful data are provided that refute the quote. As one reads the paper and analyzes the citations, the AAO's position, and Dr. Levin's quote, are untouched. It should be obvious to anyone with minimal statistical literacy that two case reports are not a basis for refuting the collective experience

documented in the child abuse literature with regard to retinal hemorrhages and child abuse. Dubious too is the assertion of increased intracranial pressure in the experimental study by Coats et al, due to a 6-hour interval between experimental trauma and euthanasia, despite the fact that such data do not exist. This is, at its core, disingenuous. It leaves the reader with the impression of highly conjectural criticism, or a presumption of data in order to support a pre-established theory, one that trivializes the role of trauma in severe hemorrhagic retinopathy. It is striking that the wealth of data contrary to this theory and the general premise of the article are left out of the discussion ([1],[2],[3],[4]).

Three unfortunate comments might also be noted: i) “Had this fall not occurred in a public place, the findings would undoubtedly have resulted [sic] in accusations of abuse” (Really? How do you know?); ii) “The continued dogma that these findings are virtually pathognomonic of AHT is unsafe and can cause harm due to false allegations of child abuse” (where, exactly is the data to support this?); and iii) “most if not all of the studies that support the concept of diagnostic specificity have serious flaws in their methodology.” (Can you cite references that specifically support this with the rigor you claim is missing from the general child abuse literature?) These comments are indeed unfortunate in that they again accuse some unnamed purveyor of dogma as promoting false accusations, especially when they are not accompanied by citations. A level of naïveté is also assumed on the part of those who might give credence to any of those hundreds (perhaps thousands) of universally flawed studies of the past 157 years. A more objective tone would have been welcome, and badly needed in this topic.

Medical science is a dialectic between theory and observation. As such, it is prudent to avoid offensive rhetoric in articles purported to represent a form of scientific research, even if it consists of case reports that offer no quality evidence or even new information. The latter, by the way, is one of the items mentioned as flaws in some of the mainstream child abuse literature. Unfortunately, anecdotal case reports such as these give the impression that there is an erosion of the robust medical science that has accumulated in the documentation of child abuse, and manufacture controversy where none exists; but it is, after all, only a case report. In the end, the authors don’t inform the reader of anything, other than stains for endothelium are not diagnostic for anything other than, well, endothelium, and a witnessed fall is a witnessed fall.

**References**


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