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Peer reviewed

The *Western Journal of Emergency Medicine* has received a detailed critique by Dr Christopher Greeley of the article, “Challenging the Pathophysiologic Connection between Subdural Hematoma, Retinal Hemorrhage, and Shaken Baby Syndrome” by Dr Steven Gabaeff, published in May 2011, Volume XII, Issue 2. The author’s response is even more detailed. *The Journal* recognizes that these 2 authorities are diametrically opposed in their opinions, and in the interest of fair academic discourse, we are publishing both the letter to the editor and response to the editor in electronic form for those interested in this highly contentious debate.

We leave it to the reader to judge the original article, its critique, and rebuttal, on their own merits.

The Editor

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Challenging the Pathophysiologic Connection between Subdural Hematoma, Retinal Hemorrhage, and Shaken Baby Syndrome

Gabaeff SC. Challenging the Pathophysiologic Connection between Subdural Hematoma, Retinal Hemorrhage, and Shaken Baby Syndrome. *West J Emerg Med.* 2011;12(2):144–158.

To the Editor:

As having board certification in both general pediatrics and child abuse pediatrics, and having experience and training in clinical research and medical literature appraisal, I read with great interest the “Special Contribution” by Dr Steven Gabaeff.¹ I appreciate the special relationship that the author has with the *Western Journal of Emergency Medicine* as having been instrumental in the rebranding from *The California Journal of Emergency Medicine*, past president of the California chapter of the American Academy of Emergency Medicine, and a current editorial board member. Given the complex and contentious nature of the subject matter, I am impressed that it took less than 4 weeks for a meaningful peer review to occur, for recommending revisions for the author, and for receiving those revisions.

I recognize that there are a number of medical professionals who disagree with some of the accepted clinical features of abusive head trauma (AHT) (formerly referred to as “shaken baby syndrome”) and I believe that critical scrutiny and lively debate of much of clinical medicine is a healthy and necessary endeavor. As a result, there exists a small cadre of professionals who have become denialists to many of the central tenets of AHT² and use various rhetorical techniques^{3,4} to further an ideology, and not to meaningfully contribute to the field.

Unfortunately, I fear the piece by Dr Gabaeff does not contribute to a substantive deconstruction of some of the basic tenets of child abuse pediatrics or further the discussion. I would like to point out some of the methodologic flaws the author makes so as to afford your readership a more accurate appreciation of this complex and often contentious field. Owing to space constraints, I cannot present a counterfactual argument for each of the presented hypotheses. I will limit my comments to highlighting certain rhetorical sleights that may mislead the reader, and provide some examples from Dr Gabaeff’s text.

Throughout the article, the author uses a common technique of preceding and/or following controversial and unsupported statements with cited comments or phrases. This technique gives the appearance of cited literature support for an unsupported opinion. The first example of this is when the author discusses the work of Dr Ommaya in whiplash forces on the brain and cervical spine of monkeys. The author writes, “With current technology, these neck findings following whiplash injury would be evident as soft tissue swelling from hematoma or edema on magnetic resonance image (MRI) and computed tomography (CT) of the neck.” This is placed before and after well-cited work by Dr Ommaya but is itself uncited, and in the pediatric population has been shown to be untrue.^{5,6} It is this sentence that is meaningful to clinicians, but it is this sentence that is unsupported. This “citation sandwich” is a common way in which unsupported opinions are given the veil of legitimacy by their proximity to cited and supported concepts. Another example of this is when the author describes the hypothesis that shaking an infant is dangerous. The author writes, “based on analysis of the force required to cause intracranial injury and the impact of shaking on the neck, without some findings of neck injury on imaging, intracranial pathology resulting from human shaking of a previously healthy child should be seriously called into question.” While this statement is uncited, it is preceded by a cited discussion of the G forces required to cause injury and followed by a cited

discussion of helmet forces, which occur during football collisions. Of note, the discussion of the forces generated in football collisions is an example of “irrelevant conclusion” (*ignoratio elenchi*). This technique is used to divert attention away from an underlying argument by introducing a tangential and irrelevant argument theme. The forces generated by the collisions of adults playing football are physiologically and biomechanically unrelated to the theory that shaking of an infant can result in retinal hemorrhages.

Another methodologic flaw the author uses is “denying the antecedent.” This is a technique in which conclusions are made that are not supported by the presented evidence. The author writes, “On this basis, the consideration of intentional impact must be carefully evaluated to diagnose abuse, as it is clear that short falls in household situations are sufficient to cause not only ICT, but even death.” The citation for this is a review of 75,000 falls involving playground equipment reported to the US Consumer Protection Agency, of which 18 were fatal.⁷ In reading the “Methods” section of this citation, it is readily apparent that none of these were household falls and none involved children younger than 12 months. While this is an important article as support for consideration of falls as a cause of death in young children, to imply that it supports that a short household fall can kill an infant is misleading. Another example of denying the antecedent is when the author discusses the differential diagnosis of retinal hemorrhaging in infants. The author writes, “Lantz found from autopsy work on 425 eyes of the recently deceased that 17% exhibited RHs associated with a variety of diseases and conditions.” The citation for this is a single case report of a 14-month old child who had a crush injury to his head. His evaluation revealed “bilateral dot and blot intraretinal haemorrhages, preretinal haemorrhages, and perimacular retinal folds.” This is another important article but in no way supports the contention offered by the author. (Apparently, the author was intending to refer to Dr Lantz’s 2006 American Academy of Forensic Sciences presentation⁸ in which he described his experience with 111 people (16% of his total sample) with retinal hemorrhages, only 30 of whom were children. Of these 30, only 19 were younger than 1 year. Dr Lantz reported that 15 of these infants had retinal hemorrhages, which were from nonabusive causes.⁹ These data have not been published in peer-reviewed literature.

Another example of denying the antecedent in this piece is when the author discusses apparent life-threatening events (ALTE). The author hypothesizes that the symptoms associated with an ALTE (“seizures, decreased muscle tone [limpness], vomiting, failure to thrive, hydrocephalus, altered level of consciousness [LOC], color changes from hypoxic episodes, conventional or dysphagic choking, abnormal breathing patterns, and apnea”) could be the manifestations of a chronic subdural hematoma. Ironically, to support this contention, the author cites a 1968 cohort (pre-computed tomography [CT] technology) of 116 infants with “subdural effusions or hematomas” described by

Till.¹⁰ Of these 116 infants, nearly half had retinal hemorrhages, a number that “would have been undoubtedly higher if more time had been spent examining the fundi of these babies.”¹⁰ Till reports for the subdural collections “no satisfactory explanation in many cases, although trauma is an important factor in the majority.”¹⁰ It appears that the citation used to support Dr Gabaeff’s contention that the ALTE-like symptoms of a chronic subdural hematoma (SDH) can be spontaneous is that of a cohort of children many of whom likely had been abused.

Another subtle rhetorical technique used is the “straw man” argument. This is the most widely known rhetorical technique and involves constructing an opposing point of view in a manner that makes it seem unbelievable, and thus easily discountable. The author performs this when he refers to the large number of accidental falls that occur each day, and that “it is illogical to reflexively assume a different, sinister act has occurred in patients who are found to have SDH after an accidental fall. Rather, we should recognize that a very small subset of all accidental falls can and do result in serious brain injury. With a large denominator of accidental falls, the serious brain injuries can and do result from innocent, accidental mechanisms, and each of these cases most likely prompts a medical encounter.” This description makes the “pediatric child abuse specialist” seem irrational and thus unbelievable. In using this rhetorical sleight, one does not have to discuss the data that fatal falls from any height in children are exceedingly rare (55 per year in children younger than 5 years¹¹) nor outline the detailed protocols that hospitals and professional organizations^{12,13} have regarding the meticulous evaluation of suspect abuse. The straw man argument technique is intended to simply make the opposite position seem unfounded.

Lastly, the author also uses “converse fallacy of hasty generalization.” This is a technique in which a very specific premise is constructed and the conclusions are (mis)applied by generalization. This is a very common technique of rhetorical argument in which a single case report or instance is used to dispel an entire theory. The author uses this technique when he discusses the article by Rooks et al.¹⁴ This is a study of neuroimaging of newborn infants. Of the 101 infants undergoing neuroimaging, 1 (1%) had “a new frontal SDH on the 2-week MR imaging follow-up examination.” Rooks et al note that this neonate “had bilateral occipital and posterior fossa SDH on initial imaging at birth, confirmed on the 7-day follow-up MR imaging. He was also noted to have extra-axial collections of infancy. At 26-days postnatal age, the MR imaging demonstrated left frontal subdural collections that did not conform to CSF signal intensity.” This single case, that may have had something unique about it, is used to support a recommendation for a screening magnetic resonance imaging on all infants with “subtle behavioral abnormalities to prevent later accusations of abuse if complications arise.” (Of note, this infant was not described by Rooks et al as having hydrocephalus as Dr Gabaeff contends.)

A subtle variant of the converse fallacy of hasty generalization is to simply not provide literature support for a broad generalization. An example of this is when the author discusses the presence of retinal hemorrhages. He writes, “The American Academy of Ophthalmology has endorsed and taught the current corps of ophthalmologists that RH, schisis, retinal folds and vitreous hemorrhage are identified with intentional abuse when in fact these findings are more likely the consequence of metabolic catastrophe within the eye itself and unrelated to shaking forces as discussed above.” This sentence is uncited and nowhere in the article does the author refer to data on metabolic diseases and retinal findings. While case reports are quite rare of infants or children with Menke disease, von Willebrand disease, leukemia, and glutaria aciduria (to name a few) who have been noted to have retinal hemorrhages, the author’s sweeping generalization is simply unsupported by clinical practice or medical literature.

In closely appraising the “Special Contribution” by Dr Gabaeff, we see a number of concerning logical fallacies and rhetorical sleights of hand. While this piece is not a systematic review and simply represents the opinion of the author, much of what is written is intended to be used in legal proceedings, and to be cited as being from a peer-reviewed publication. The distinction between a methodologically rigorous systematic review and an opinion piece will be lost on many readers (and juries). The peer-review process is seen by many uninitiated readers as “validating as true.” As a sophisticated end-user of the medical literature, I am continually reminded it is ultimately up to me to critically scrutinize everything that I read and to assess the quality of methodology and data presented. Given the adversarial nature of some of the scholarship of AHT, I am very conscientious of many of the logical and rhetorical landmines readers can encounter. While it is I who ultimately assigns meaning and value to what I read, it is beholden to journals to maintain very high standard of quality and to not create artificial confusion where none exists. I fear the piece by Dr Gabaeff contributes little to the discussion and merely obfuscates the truth.

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In reply:

I welcome the opportunity to respond to Dr Greeley’s letter to the *Western Journal of Emergency Medicine*, criticizing the journal, the editorial staff, myself, and the content of what I have written.

The legal consequences of the misdiagnosis of accidents and medical problems as abuse are dreadful. The nonevidence-based “certainty” that retinal hemorrhage (RH) and subdural hematoma (SDH) are sufficient to diagnose abuse is expressed often, early, and with conviction by virtually all board-certified child abuse pediatricians, many radiologists, and most ophthalmologists. The reliance on these nonspecific findings as pathognomonic of abuse is the rule, not the exception. All other facts and circumstances in any specific case are subservient to the 2 nonspecific finding that were challenged in my article. Using these findings to accuse caregivers of abuse is backward thinking. The findings themselves, long established as inexact on their own and in combination, have been used to speculate

about intent, mechanism, and as the basis of abuse allegations. Clinging to dogma long since exposed as unreliable and scientifically invalid, and attacking the messengers exposing the flaws in that dogma, have been the modus operandi of the child abuse establishment, in this case represented by Dr Greeley's letter.

Dr Greeley recently presented a talk entitled "A Wolf in Evidence Clothing [sic]: Denialism in Child Abuse Pediatrics"¹ and gave a presentation in 2011 at the conference on abusive head trauma (AHT) in Hershey, Pennsylvania, that was titled "Deconstructing Donohoe: The Evidence Behind the 'Lack of Evidence.'" In each case, those who disagree with the child abuse establishment are referred to as "denialists" and their integrity and professionalism is attacked to blunt the impact of their analyses. Donohoe, who I cite, and whom Dr Greeley criticized, was singled out by him at a meeting of key members of that establishment precisely because Donohoe's criticism of the child abuse literature is so impactful to the current state of child abuse pediatrics.

Donohoe² was cited in my article, and by many others, for his valid criticism of the child abuse literature. As the readers of my article might recall, Donohoe evaluated the child abuse literature from 1966 to 1998 and found significant weaknesses, concluding that there was inadequate scientific evidence to come to "a firm conclusion on most matters pertaining to SBS." He graded all of the child abuse literature at the lowest end of an accepted methodology quality scale. Appropriately, Donohoe called for controlled, prospective trials into shaken baby syndrome (SBS) and opined: "Without published and replicated studies of that type, the commonly held opinion that the findings of subdural hematoma and RH in an infant was strong evidence of SBS was unsustainable, at least from the medical literature."

Greeley attacked the scholarship of Donohoe in his "Denialism in Child Abuse Pediatrics" presentation and he stated that "Those who cite Donohoe as 'evidence based' are either inexperienced in medical literature appraisal or are being disingenuous; there is no third option."

Regarding the issues themselves, 6 questions remain critical to this debate. They sit at the core of the controversies in child abuse pediatrics and are the primary questions that must be answered to evaluate medical histories in potential abuse cases both for plausibility and probability. One could pose the questions central to an objective analysis and explore the literature, both old and new, to see if support for an alternative narrative, not abuse related, exists. Is the existing literature sufficient to create medical uncertainty or legal reasonable doubt regarding the allegations of abuse when these questions are asked? Does the literature in fact support the scientific invalidity of some of the core assumptions in child abuse pediatrics and their unreliability when used to prosecute alleged child abusers? Are innocent people being incarcerated with nonevidence-based assertions in medical records and in court?

The critical questions are as follows:

Can short falls cause serious injury?

Is chronic SDH likely to rebleed with relatively minor trauma?

Does increased intracranial pressure, from SDH, cerebral edema, infectious disease, hypoxic ischemic encephalopathy and other causes, without any evidence of shaking, cause retinal hemorrhage?

Can medical problems generate findings that can be misdiagnosed as abuse?

Is shaking biomechanically insufficient to cause brain hemorrhage?

Will extreme abusive shaking result in obvious neck damage?

As the number of studies supporting the affirmative response to these questions increases, the primary constructs of child abuse pediatrics are shown to be false. Even a cursory review of the literature reveals many studies that indicate the answer to these questions is a resounding "yes."

Plunkett³ in 2001 proved short falls cause serious injury. The 2009 article by Vezina⁴ showed that chronic SDH rebleeds occur with relatively minor trauma or no trauma. Aoki and Masuzawa's 1984 study⁵ shows that 100% of 26 children with SDH, not resulting from shaking, have retinal hemorrhage. Sirotinak and Frazier devote 2 chapters to "Medical Disorders that Mimic Abusive Head Trauma" in the text *Abusive Head Trauma in Infants and Children*,^{6,7} published in 2006. They discuss numerous infectious, hematologic, metabolic, accidental, and other disease entities that can mimic abuse. Prange et al⁸ in 2003 showed that human shaking is insufficient to cause brain damage. The study by Bandak⁹ in 2005 proved that any shaking sufficient to cause brain damage will cause severe and obvious neck damage.

Given these, and numerous other studies, showing the same things, how valuable is the highly restricted certification in child abuse medicine? Does the certification advance science or justice when those seeking certification are taught that they must answer "no" to these questions to be certified? Is there any latitude to disagree with the established dogma? If you do, do you risk being labeled an "outlier" or a "denialist" too?

Dr Greeley's criticism of my article starts with innuendo that my efforts as president of the California chapter of the American Academy of Emergency Medicine in 2006, during which I initiated the effort to create a new top-tier, open-access journal of emergency medicine, created an "inside deal" that led to the publication of my article. This is unsupported and untrue. I chose *The Journal* because it offered open access that other professionals would have easy access to the material. Dr Greeley states that it took only 4 weeks to go through the peer-review process. In reality the article was submitted on December 16, 2009, some 1.5 years earlier, and went through

24 distinct drafts in response to peer review. The final version of the submission was turned around by *The Journal* in 8 weeks. The effort was coordinated by the editor and section editor to construct the message in a nuanced way, fully embracing and remaining sensitive to the controversy that the article would generate. The intent was to try to open the mind to possibilities beyond the dogma that sits at the core of child abuse pediatrics.

I am not alone in recognizing the dogmatic aspects of the positions held by Dr Greeley. A recent presentation by Dr Evan Matshes at the American Academy of Forensic Medicine in 2010 was introduced with this statement:

“For many years, the dogma of pediatric forensic pathology was ‘retinal and optic nerve sheath hemorrhages are pathognomonic of abusive head injury,’ including especially, the shaken baby syndrome (SBS).”¹⁰

And he ends with the following:

“Retinal hemorrhage and optic nerve sheath hemorrhage are not limited to children who die of inflicted head injuries; instead, they may be seen in a wide variety of situations, and may be linked to cerebral edema and sequelae of advanced cardiac life support.”

Dr Greeley prefaces his critique by claiming a “small cadre of . . . denialists” are furthering an “ideology,” using a variety of “rhetorical sleights” for which he provides examples.

First, he states that I have used the common technique of “preceding and/or following controversial and unsupported statements with cited comments or phrases,” the “citation sandwich.” The study of cognitive errors and logical fallacies, analyzed in depth by Croskerry,¹¹ lists numerous types of cognitive errors, and this is not among them.

The sandwich’s pieces of bread in this arcane metaphor, he argues, start with Ommaya’s 1968 study,¹² the entire basis for the theory of SBS. This study measured the whiplash forces that cause loss of consciousness in monkeys and then looked at autopsy findings in those that were rendered unconscious. Massive neck injury occurred whenever brain injury was present. The other piece of bread in Dr Greeley’s sandwich was the follow-up study by Ommaya and Gennarelli¹³ that demonstrated abnormal neurophysiology of the cervical spine after severe whiplash. This study followed 6 years later.

His criticism is that I have “sandwiched” between Ommaya’s 2 studies the idea that there would have been evidence of neck injury on computed tomography (CT) or magnetic resonance imaging (MRI) after a 600-g whiplash. Dr Greeley characterized this idea as “unsupported.” That is a false statement. Barnes,¹⁴ Bandak,⁹ and others,^{15–20} have stated the same thing for many years. I do cite these studies in my

article, something that he seems to have overlooked with the use of this culinary metaphor.

It is known how much force is needed to cause SDH and it is known how much force it takes for the neck to fail. The ratio is greater than 10 to 1. The neck, according to all biomechanical analyses, will fail well before the forces that can cause SDH in the head can form. I wanted the reader to consider that any baby allegedly shaken to unconsciousness, and with an SDH, would likely have neck findings on CT or MRI. It was written to suggest that the absence of neck findings may provide a basis to question the shaking component of SBS and consider other medical or accidental etiologies for the brain pathology.

Next, he cites what he says is an “irrelevant conclusion.” He declares that 26,000 measured helmet impacts during college football games are “unrelated to the theory that shaking of an infant can result in retinal hemorrhage.” He seems to miss the point I was making, which is that impacts above 85 g do not cause SDH (or retinal hemorrhage) and human shaking can only generate a force of 10 g to 14 g. This is about one tenth of the known thresholds for injury, established by the National Highway Transportation Safety Administration at 100 g, making shaking even more unlikely as mechanism for brain or eye injury. The football study *is* relevant to a discussion of force and I believe it is relevant to retinal hemorrhage too, since none of the athletes had retinal hemorrhages at forces greater than 100 g and since humans can only generate a fraction of that force.

The next methodical criticism is “denying the antecedent.” He defines this as “conclusions made that are not supported by the presented evidence.” Referring to the seminal study by Plunkett³ showing that accidental short falls from playground equipment can cause death, he himself cites a study that showed 18 of 75,000 falls (about 0.024%) resulted in death. That’s about 2 out of 10,000, a rate of serious injury more frequent than the commonly quoted “1 in a million” falls that will result in serious injury, promoted by Chadwick and his colleagues²¹ in 2008. The children in the study cited by Greeley were older than 1 year, with harder, more structurally solid skulls. They were less vulnerable to brain injury than infants. Children falling 5 feet or less from playground equipment can fall from similar heights at home, yet his “point” is that these household falls should be regarded as different. Biomechanically, a 5-foot fall on the playground and a 5-foot fall at home, are the same. Evidence of a 5-foot fall on the playground causing death to me, and others, is evidence that infants falling 5 feet at home can be killed as well. He states that “to imply that it [Plunkett’s article] supports a short household fall can kill an infant is misleading.” Really?

Furthermore, he fails to mention that serious injury from short falls, a much more common clinical event, well established by Greenes and Schutzman,^{22,23} occurs as frequently as 1 in every 6 frightening short falls that present in an emergency department (ED).

Another of his examples of “denying the antecedent,” reaching a false conclusion from evidence presented, is based on my selecting the wrong citation (not the wrong information) from a long list of articles by Dr Patrick Lantz, which I have in my computer files. Dr Lantz is a pediatric ophthalmologic forensic pathologist at Wake Forest University (Winston-Salem, North Carolina). Dr Greeley is right, I did intend to use Dr Lantz’s 2006 American Academy of Forensic Sciences presentation²⁴ in which he described his experience with 111 people (16% of his total sample) with retinal hemorrhages, of whom only 30 were children who had RH at autopsy from causes other than shaking abuse. The point being made, however, remains the same: a large percentage of all deaths from any cause, have RH at autopsy.

Dr Greeley then criticizes my use of Till as a reference. I had cited Till to validate the common symptoms of apparent life-threatening events (ALTE), I was describing the presentations and nothing more. This was something I was asked to do by the editors during our 1.5-year process.

The statement I made was as follows:

“When these infants present after an ALTE, they may have seizures, decreased muscle tone (limpness), vomiting, failure to thrive, hydrocephalus, altered level of consciousness (LOC), color changes from hypoxic episodes, conventional or dysphagic choking, abnormal breathing patterns, and apnea.”⁶⁰

Reference 60 was that of Till. Dr Greeley speculates that I intended to use this study to say that ALTEs can occur with a rebleed of chronic subdural hematoma. That is true, as Vezina⁴ showed, but I wasn’t using Till to make that point. And he cites the following quote from Till, which I had no intention of using, since I was focused on only the symptoms associated with an ALTE.

“Of these 116 infants [with subdural effusions-hygroma or hematoma-SDH] nearly half had retinal hemorrhages a number which “would have been undoubtedly higher if more time had been spent examining the fundi of these babies.” Till reports that the subdural collections have “no satisfactory explanation in many cases, although trauma is an important factor in the majority.”

It is my feeling that this supports my opinion (and Vezina’s) about the role of minor trauma in chronic SDH causing rebleeds. Dr Greeley then states that it

“appears that the citation used to support Dr. Gabaeff’s contention that the ALTE like symptoms of a chronic SDH can be spontane-

ous is that of a cohort of children many of whom likely had been abused.”

Dr Greeley’s comment, “whom likely had been abused,” inappropriately expands Till’s causality statement beyond trauma to “abuse,” when “no satisfactory explanation” is given.

Next, he raises the “straw man” argument. He writes, “This is the most widely known rhetorical technique and involved constructing an opposing point of view in a manner which makes it seem unbelievable, and thus easily discountable.”

He raises the straw man argument in reference to the following statement about accidental falls that I made.

“[I]t is illogical to reflexively assume a different, sinister act [occult shaking] has occurred in patients who are found to have SDH after an accidental fall. Rather, we should recognize that a very small subset of all accidental falls can and do result in serious brain injury. With a large denominator of accidental falls, the serious brain injuries can and do result from innocent, accidental mechanisms, and each of these cases most likely prompts a medical encounter.”

He himself acknowledges that 0.024% of all falls cause death. Many more cause serious injury. I said simply that “a very small subset of all accidental falls can and do result in serious brain injury.” I don’t see the straw man. I see 2 people saying the same thing: a tiny percentage of all short falls cause serious injury. He says that this idea “makes the ‘pediatric child abuse specialist’ seem irrational and thus unbelievable.”

Last, he invokes the “converse fallacy of hasty generalization” 3 times. This he defines as an “argument in which a single case report or instance is used to dispel an entire theory.” Well, if a single short fall kills a baby, I think any statement to the effect that short falls can’t cause serious injury becomes a deception. Even if it is “exceedingly rare,” as Dr Greeley suggests, it still occurs, and only those with serious injury present to the ED. If only the serious, frightening falls present, and each is incorrectly diagnosed as abuse on the basis of the “exceedingly rare” argument (a logical fallacy itself), then 100% of short fall accidents that have caused serious injury will be misdiagnosed as abuse.

He references my use of Rooks as another converse fallacy of hasty generalization, for reasons that are tangential as well. I cited Rooks to show that 46% of children are born with SDH. He seems to be implying I was citing Rooks to justify that the “single case” that she characterized as a “complication” is not a justification for screening neonates for perinatal SDH.

My point regarding screening, not based on Rooks, was that abnormal behaviors in the perinatal period, followed by

enlarging heads and vague neurologic symptoms, might indicate perinatal SDH and its complications and be a reason to screen symptomatic neonates.

That point was not based on a “single case” from Rooks but from a study by Zahl and Wester²⁵ in Norway that demonstrated that the number of children with complications is considerably higher. By looking for complications, Zahl and Wester showed that the equivalent of approximately 2,400 babies in the United States each year will develop hydrocephalus and hygroma, diagnostic signs of chronic SDH. My suggestion was that if the condition of these babies were identified early, or widespread screening of symptomatic neonates were done, it would (1) validate the complication rate of perinatal subdural hematoma and (2) spare innocent families the false accusations of abuse after an ALTE related to these complications.

His last example of the converse fallacy of hasty generalizations relates to this statement:

“The American Academy of Ophthalmology has endorsed and taught the current corps of ophthalmologists that RH, schisis, retinal folds and vitreous hemorrhage are identified with intentional abuse when in fact these findings are more likely the consequence of metabolic catastrophe within the eye itself and unrelated to shaking forces as discussed above.”

It is hard to see how this is an “argument in which a single case report or instance is used to dispel an entire theory,” but I can respond to Dr Greeley’s misunderstanding of the point I was trying to make.

The metabolic catastrophe I referred to is clearly hypoxic ischemic encephalopathy (HIE), the type of catastrophe that is seen daily in the EDs.

Dr Greeley’s narrow list of metabolic “diseases” (Menke disease, von Willebrand disease, leukemia, and glutaric aciduria), which he feels are adequate to rule out metabolic causes of bleeding, are almost never seen, and results are often not available before child abuse allegations have been made. Testing for them may create an illusion of differential diagnosis but does not change the frequency of HIE as a cause in intracranial pathology.

CONCLUSION

It was, and remains, my hope that some of the material herein and my article itself will penetrate the minds of the child abuse specialists who remain the linchpin, energy source, and ultimately, the key witnesses in court when prosecutors try to convict innocent caregivers of child abuse.

In lieu of reaching them, I hope that district attorneys, social workers, police, and judges will take the time to read about these issues. Understanding the issues in child abuse investigation and prosecution, independent of the child abuse

specialist, may be necessary to correct the injustices related to the misdiagnosis of child abuse. Recognizing misplaced “certainty” of abuse, when nonspecific findings are used to diagnose abuse, is within reach for nonmedical professionals. Any independent efforts to understand the issues related to the accurate diagnosis of abuse, I believe will lead to more objective and to just end results for all concerned.

Responses like Dr Greeley’s seem to indicate an intransigence to even consider alternatives. As more literature is published that undermines the dogma of child abuse pediatrics, it is neither academically appropriate nor fair to the falsely accused caregivers, families, and children to shield the past from new analyses that expose its flaws. Yet, it still seems clear that for many recognized and influential child abuse specialists this path of resistance must be followed and defended at any cost. Isn’t that true denialism?

Steven Gabaeff, MD

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