CORRESPONDENCE

Abuse as a Cause of Childhood Fractures

by Oliver Berthold, PD Dr. med. Bernd Frericks, Dr. med. Thilo John, Dr. med. Vera Clemens, MPH,
Prof. Dr. med. Jörg M. Fegert, and PD Dr. med. Arpad von Moers in issue 46/2018

Little Significance

In the review by Berthold et al. (1) on childhood fractures, abuse is described as a potential cause. Altogether 86 full-text articles were analyzed, but only 40 references and 7 eReferences were listed. Unfortunately, the authors do not provide a critical and differentiated discussion of the various studies, but only use these articles to make primarily statistical statements about how likely it is that a specific type of fracture is the result of abuse. Consequently, the content of this review is of little significance for physicians actively involved in child protection. On the individual case level it is not helpful to know how often e.g. a humeral fracture is caused by abuse.

This also implies that—independent of a statistical analysis—all further information about individual fractures does not offer new insights. To the contrary, especially in the “Biomechanical aspects” section, specific fracture types are described sketchily or not at all, so that more questions are raised than answered.

As a matter of fact, the “Biomechanical aspects” section contains little biomechanical content. No associations between the type and/or magnitude of the mechanical impact and the presentation of the injury, but epidemiological data, such as the frequency of a suspected abuse-related etiology, is provided for fractures of various bones. The latter, however, does not allow to verify the plausibility of a claimed mechanism of injury, but this verification is required for each individual case encountered in clinical practice.

DOI: 10.3238/arztebl.2019.0146a

References

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Conflict of interest

The authors declare no conflict of interest.

Undifferentiated Description

In the review by Berthold et al. (1), the authors write that “a linear skull fracture without intracranial injury can even occur after a fall from a low height (the parental bed, for example) onto a hard surface”. This description is undifferentiated and shortened; thus, the statement is false. The evaluation of the question whether or not a history of an accident is credible requires from the evaluating physician extensive own or externally sourced forensic experiences. An example of the uncritical and thus counterproductive dealing with the literature is the way the study by

Atkinson et al. (2) is presented. Atkinson et al. classify their “accidents” according to reports of witnesses of the alleged mechanism of injury without scrutinizing these statements. But exactly this classification of the “accidents” is the responsibility of the attending physician, because in cases of abuse typically a “true” history cannot be obtained. A well-researched overview of falls in children under a wide variety of circumstances by Alexander et al. (3) is not cited at all. This study contradicts the above cited statement of Berthold et al. (1). A good, very useful and differentiated description (also of the biomechanical aspects) is provided by Bilo et al. in their book (4), which is mentioned but unfortunately not discussed in detail.

To be able to offer sound consultation regarding the pathogenesis of childhood fractures, thorough training and extensive experiences are required. In Germany, such specialists are found in forensic medicine (www.dgrm.de) and in the Child Protection Groups in the various Children’s hospitals which are almost always advised in forensic matters. These should be contacted with regard to fractures and not only for a multiprofessional diagnostic evaluation.

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Conflict of interest

PD Dr. Banaschak received fees from Springer-Verlag for being an author or co-author of a publication related to this topic.

Prof. Mützel declares no conflict of interest.

In Reply:

We would like to thank the authors from the field of forensic medicine for their critical feedback on our article (1).

The aim of our article was to create increased awareness in emergency departments and community-based medical practices of the possibility that fractures in children are the result of physical abuse (or neglect). Our intention was to help doctors working in these settings to ask the right questions and initiate a thorough

diagnostic evaluation, if required. It is definitely beyond the scope of such an article to already answer all these questions. This is the responsibility of specialized bodies, for example at the institutes of legal and forensic medicine. We pointed this out in our Introduction, in the section “What to do if indications of abuse are substantial” and in the Key Messages.

E-mails and telephone calls we have received after the publication of our review indicate that the goal of a critical assessment of childhood fractures has already been achieved at some sites.

Prof. Mützel and Dr. Banaschak describe our statement that linear skull fracture can occur after a fall from a low height as incorrect. To support this claim they cite a chapter from a book published in 2001. However, in the more recent edition published in 2009 (2), the occurrence of linear skull fractures after (partially in hospitals) observed falls from a height of 3 feet (approximately 91 cm, so-called “short falls”) is reported to be 0.8% to 1.3% (3). Furthermore, the force required to produce a skull fracture is described as “low to moderate“. It seems that at this point the misunderstanding occurs: As clinicians, these children are presented to us only after a selection process. Falls in children are everyday events. Approximately 1% of falls have consequences; thus, these cases will regularly occur in large cities, such as Munich, Cologne or Berlin, and be especially prevalent in large pediatric emergency departments. Of course, this alone does not allow to draw the conclusion that a linear skull fracture is unlikely caused by abuse. History, motor skills and all accompanying findings have to be as carefully evaluated as with other childhood fractures.

Likewise, the alleged improper dealing with the case series of Atkinson et al. (4) is something we cannot leave unchallenged. Banaschak and Mützel claim that Atkinson et al. had not scrutinized the information provided about the accidents. However, in the article’s Methods section the inclusion criterion is clearly stated: Apart from evaluation of a child protection physician, it was a requirement that in addition the examinations of the youth welfare office and the police had to arrive at the conclusion that it was not a case of abuse. We specifically included this study to illustrate how challenging the comprehensive evaluation of an injury can be in individual cases so that a multiprofessional evaluation of these cases (including forensic medicine) is crucial.

In their letter, Dr. Adamec and Prof. Graw criticize that we had analyzed more articles in full text than we cited. However, we think that this is per se a defining feature of selective reviews of the literature and meets the requirements of the *Deutsches Ärzteblatt* (the 40 most important references in the printed version and any indispensable further references as eReferences online).

That the authors of the letters regard our article as inadequate for physicians actively involved in child protection is in our view the result of the misunderstanding with regard to the target group for our article which we have already mentioned at the beginning of our reply.

Because of the set size limit for review articles, there was little room for biomechanical considerations which were only intended to demonstrate to the readers in medical practices and emergency departments which thoughts must precede the critical evaluation of the history of the fracture.

For further reading we referred, among others, to the work of Bilo et al. (5), cited also by Mützel and Banaschak.

Overall, we see in the correspondence the common goal that children with fractures nationwide require a careful diagnostic assessment to recognize or rule out potential abuse—a task which we think rather requires a combined effort than overemphasis on disciplinary boundaries.

DOI: 10.3238/arztebl.2019.0146c

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Conflict of interest

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