Short falls and shaken baby syndrome: numerical and reasoning errors

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1. Introduction

The medical literature on the subject of short falls in children has come to play a role far broader than the strict topic of the physical damage that they can cause. There is a good reason for this: it is widely considered that short falls are by far the most frequent explanation given by child abusers to explain injuries observed on the victims. Chadwick et al [Chad91] explain that child abusers favor short falls over long falls as a supposedly plausible explanation due to the fact that long falls generally take place outdoors, and there is always the possibility of a bystander testifying that no long fall took place at the stated time and place. Curiously, Chadwick also states that there is a widespread belief that short falls can cause serious harm, although studies such as Hall et al [Hall89] have shown that on the contrary, people rarely believe that a short fall can be extremely dangerous, and delays in treatment due to this belief cause fatalities from short falls which could otherwise have been avoided. In any case, whatever the reasons, the assertion that child abusers frequently resort to histories of short falls is certainly correct.

It is this observation that has led to the high importance of short fall studies in the study of pediatric trauma, because it raises the crucial question of whether any caregiver who brings a child to the hospital with serious or fatal injuries and explains that they are due to a short fall is or is not telling the truth. Ideally, methods would exist to distinguish the nature of lesions produced by short falls from those produced by inflicted injury. However, no reliable methods exist for this at present; it is an established fact that short falls can occasionally 'mimic' the symptoms of abusive head trauma (see for example [Atkin17]). Thus the best we can do is to resort to statistical databases covering a significant population of children and attempt to use them as well as possible in order to determine whether short falls can truly cause serious or fatal damage in children, and if so, how many such events we can expect to see in a given population each year, and then attempt to diagnose injured children on a case-by-case basis. However, this is not what actually happens. In reality, if a child presents with certain injuries due to an alleged fall witnessed by a single caregiver only, the history is rejected and the caregiver generally arrested for abuse.

The literature has focused to a large extent on the specific phenomena of subdural hematoma (SDH), retinal hemorrhage (RH) and cerebral oedema, a trio of symptoms which are identified with "shaken-baby syndrome" (SBS), also called "abusive head trauma" (AHT). Because the names of these syndromes, which in principle describe a set of symptoms, actually contain the presumed cause of the symptoms (in other words, because these symptoms are considered pathognomonic for child abuse), the question of whether they can or cannot in fact have other causes such as short falls becomes more than a mere diagnosis, but an actual question of truth. It is this that explains the extreme importance of the medical literature on short falls, not only in diagnostics, but also in the judicial arena.

With such a responsibility on their shoulders, the authors of short fall studies have a duty to be as thorough and as precise as possible, since any error or bias in such studies may lead directly to serious miscarriages of justice. The purpose of the present chapter is to examine the situation concerning these studies. We focus above all on the governmental health policy statement put out by the Haute Autorité de Santé ([HAS2017]) on Shaken Baby Syndrome, which exerts a strong influence on French policy with respect to suspected SBS, providing "recommendations" that are in fact obligations to treat the symptoms of SBS as necessarily a consequence of child abuse, with severe sanctions for practitioners who do not abide by them. We also refer frequently to the article [LV2011] by many of the same authors, on which [HAS2017] is to a large extent based, but which is much more detailed. These articles cite others, some of whose statements we will also examine. Two articles, [Will91] and [Chad91] are of particular importance, in that they are used and cited repeatedly in the judicial context. Each of these articles contains serious flaws in reasoning and in mathematics, which are reproduced each time they are cited, and each is worth a detailed analysis in itself to explain and reveal these flaws. Such an analysis was given for the article [Chad91] in [Sch19]. A corresponding analysis of [Will91] is given as an Appendix to this chapter.

In our analysis of the statements made in these publications, we are led to cite several other articles and studies that give contradictory results, and which are for the most part not cited there, leading to inaccurate statements as well as concerns about bias and cherry-picking. In the interests of justice, it is essential and indeed urgent to give a detailed analysis of the manner in which statistics are cited, used and interpreted in the HAS report and the articles on which it relies.

2. Common types of error

The problems and inaccuracies in the statistical arguments used in the articles we consider are of various types, although one underlying main error lies at the basis of nearly all of them: namely *the absence of crucial information which, if present, could radically change if not even actually invert a conclusion that appeared obvious from the given data*. This situation is known as Simpson's paradox, and follows the paradigm illustrated in the following riddle based on a real situation.

> Two treatments (A and B) for kidney stones were proposed. The treatments were both tested on cohorts of 350 patients. In presenting rival applications for funding, Treatment B claimed superiority over Treatment A by the following statement: "Treatment B significantly helped 83% of patients whereas Treatment A significantly helped only 78%." The conclusion that Treatment B is superior to Treatment A appears self-evident.
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> However, Treatment A included the following statement in its application: "Treatment A caused significant improvement in 93% of large kidney stone cases whereas Treatment B helped only 87% of these patients; Treatment A also caused significant improvement in 87% of small kidney stone cases where Treatment B helped only 69% of these." This statement appears to be in contradiction with the previous one, yet both are factual.
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> The paradox is explained by the following additional piece of information: the cohort on which Treatment A was tested contained very different proportions of small and large kidney stone cases from the cohort given Treatment B. The A cohort contained 87 large kidney stone cases vs. 270 in the B cohort, and the A cohort contained 263 small kidney stone cases vs. only 80 small ones in the B cohort. Treatment B helped only 87% of large kidney stone cases (234/270) as opposed to Treatment A's 93% (81/87) and only 69% of small kidney stone cases (55/80) as opposed to Treatment A's 73% (192/263), yet in all Treatment B helped 289 out of 350 patients and Treatment A only 273. This information shows that the higher percentage of patients cured by Treatment B is an artifact of the distribution of the cohort, and that with identically distributed cohorts Treatment A would be superior. Thus the seeming obvious conclusion of Treatment B's superiority from the initial fact of its overall higher success rate changes completely after further information is provided.

Many studies of SBS and short falls suffer similar problems to this example, with crucial missing information concerning issues such as population sizes, expected values, age categories, or unmentioned common causes explaining some of the observed correlations. Other disquieting features that can be noted in the articles in question are circular reasoning, misunderstanding or misinterpretation of statistical observations, and comparison of dissimilar cohorts. Finally, there is a strong tendency to ignore or explain away studies or case histories providing counterexamples to some very peremptory assertions. The errors and problems that we illustrate in the following section are of four basic types: (i) faulty reasoning from statistics; (ii) heavy reliance on certain flawed articles, with consequence reproduction of the flaws; (iii) strongly-worded assertions that rely on certain publications while completely ignoring or even explicitly denying the existence of others that provide counterexamples; (iv) misquoting or misinterpretating statements even from those articles that are cited. The discussion below is organized by theme, each theme corresponding to a specific issue or assertion made with respect to SBS and/or short falls.

3. Specific issues concerning SBS and short falls

In this section we examine several of the issues that pit against each other on the one hand defenders of the assertion that the symptoms of Shaken Baby Syndrome indicate child abuse with rigorous certainty, and on the other hand those who consider that such certainty is very far from justified by the existing medical literature.

The article [LV2011] begins by warning that *"il est souvent difficile de différencier traumatismes crâniens infligés et traumatismes crâniens accidentels"*. This is certainly the key issue from a judicial point of view, since a wrong diagnosis may lead to a wrong accusation of child abuse, followed by a wrong condemnation that constitutes a miscarriage of justice, and further causes unwarranted traumatism for the entire family of the accused, including the unharmed siblings. The explicit intention of "protecting children" should give pause to certain medical practitioners intent on imprisoning parents on the unique presence of subdural hematoma (with but sometimes even without retinal hemorrhage), without the slightest evidence of abuse either by physical examination or by witnesses.

3.1. Circular reasoning. After their initial wise warning, [LV2011] goes on to cite numerous studies correlating incidents of shaking and/or inflicted head injury with various factors. This already leads to a question: if it is so difficult to distinguish between inflicted and non-inflicted injury, then how can we be sure that the cases in the studies are true cases of shaking? In particular, it is dangerous to rely too heavily on confessions, for a number of different reasons:

i) Parents are often told during interrogations that unless they tell the truth, the doctors will not be able to properly treat the child, sometimes pressuring them into inventing incidents in order to make sure the child is seen.
ii) Parents are often told during investigations that even a simple gesture of lifting an infant rapidly without supporting its head can cause severe injury; then when the parent is led to examine each of his or her own gestures and admit that they may have lifted the child up rapidly, or lifted it up in one arm, this is taken as a confession. Subsequently, since the parent's explanation does not correspond to the injuries, the parent is accused of not telling the whole truth.
iii) Conversely, parents who have exercised violence on an infant may resort to a confession of shaking as a way of diminishing their guilt, believing that shaking is considered a less harmful

or less criminal act than actual blows. This fact was observed in the early 1970's by Norman Guthkelch, considered one of the original discoverers of SBS, who noted that parents who brought children with subdural hematoma to his hospital in northern England freely mentioned having given their child "a good shaking". This gesture, which appears frequently in English and American literature, was considered very light, indeed more an expression of anger and annoyance than an actual punishment.

[LV2011] then gives a detailed analysis of when to diagnose a case of SBS, with the diagnosis being qualified as "possible", "probable" or "highly probable or even certain" according to the symptoms and injuries observed on the child, but also taking into account such features as "a clinical history that is absent, incoherent or changes over time", as well as the possibility of statements by an eyewitness. However, included in the list of elements that encourage suspicion of SBS is "a clinical history that is incompatible with the observed lesions". While perfectly reasonable on the surface, this criterion again comports a serious risk of circular reasoning, since if a medical professional belongs to the camp that decrees that certain symptoms can be caused only and uniquely by shaking, then he or she will automatically consider any other cause or explanation for the symptoms as "incompatible with the observed lesions".

The same risk of circular reasoning holds for the uncertainty described above regarding the possibility of a short fall causing serious injury to an infant. If a medical professional adopts the rigid position every time a claimed short fall causes severe injury it is in fact a case of child abuse[1], then in every case of serious harm caused by a short fall, the clinical history will be said to be incompatible with the lesions. For this reason, as explained above, the possibility of a short fall causing fatal injuries is an essential subject of investigation.

3.2. Existence of fatal short falls. Many studies conclude that short falls of less than 5 feet (1.5 meters) cannot cause serious injury in children, let alone death. [Chad91] asserts that "Falls of less than 4 feet are often reported in association with children's head injuries that prove to be fatal, but such histories are inaccurate in all or most such cases." Speaking of severe injuries and deaths observed in his hospital-based study, [Will91] concludes that his data "leads one to suspect that many if not all of these injuries attributed to falls of low height represent child abuse". These two studies are still cited very frequently, including in judicial situations, to shed doubt on the notion that short falls can cause serious injury, subdural hematoma, retinal hemorrhage, cerebral oedema, or death.

Yet deaths from short falls have been observed; in particular, there have been cases with independent witnesses, falls in public places, and even some incidents inside hospitals. In October 2019, a 4-year-old boy died of head injuries after falling out of a little red wagon in a dental clinic[2]. Hall et al found two instances of fatal short falls in medical facilities: "One normal child fell off a doctor's chair and developed a subdural hematoma, and another child fell while running down the hall in a hospital and developed a subdural hematoma, both with resultant herniation" [Hall90]. These cases form part of the vast study [Hall89], drawing on the medical examiner's records covering all of Cook County, Illinois (population circa 5 million) over a 4-year period. During this period, Hall et al found 18 cases of children who died from falls of less than 3 feet; all had lethal head injuries but no

[1] This view is actually quite widespread and based on publications; see for example the much-cited [Chad91], and the article [Will91] analyzed in the Appendix

[2] https://apnews.com/1c971bf0697d43539b20c05bb89e4c18

other injury, and child abuse in these cases was ruled out by an intensive medical and police investigation. The Hall study concludes that it is a dangerous error to spread the idea that short falls are not dangerous. "The myth that all minor falls are benign must be expunged; some can be serious. All parents and medical personnel must be aware of symptoms denoting severe injury after a 'minor fall' […] Abuse does need to be ruled out, but falls *regardless of height* are potentially fatal" [Hall90].

The Hall study and its claim that short falls can indeed cause serious injury received a great deal of criticism. In a strongly-worded Letter to the Editor published in the Journal of Trauma, M. Joffe [Joffe90] insists that that "Severe injuries are extremely uncommon after minor falls" and accuses the Hall study of insufficiently investigating the possibility of child abuse. In their response [Hall90], also published as a Letter to the Editor, Hall et al counter: "All children had not only a complete investigation by the local police department but also by an investigator from the medical examiner's office. All children had a complete post-morten plus full body X-rays. […] It is the practice of this office to rule 'out' (not 'in') abuse in all suspicious cases; conclusions were based upon the investigation and not the reverse […] We cannot conceive of a more independent nor complete investigation." Clearly the practice he points out of "ruling in" abuse in any doubtful situation is a key point to focus on in the search to avoid miscarriages of justice.

Eleven years after the Hall study, Plunkett [Plunk01] published an account of 18 cases of witnessed, outdoor short fall fatalities, each one described in detail including causes and symptoms. This study proves incontrovertibly that there do exist exceptional cases in which short falls can cause extreme injury and death. Nevertheless, in discussing whether short falls can be fatal, [LV2011] manages to dismiss nearly all of Plunkett's cases as valid examples for a variety of reasons: in some cases the multiple witnesses were all family members (who are invariably assumed to be plotting together to hide abuse), in others a full autopsy was not performed, in yet others children were swinging or rocking before their fall, etc. These reasons do not show that Plunkett's examples are not examples of fatal short falls; rather, they show that those who believe that short falls cannot cause serious injury will not accept any example of such an event short of its being literally been shot on camera. It so happened that in one of Plunkett's cases, concerning a 23-month old girl, the child's fatal short fall was actually caught on film by a grandmother watching her grandchildren at play. Due to this fact, this single case is accepted in [LV2011] as the only "incontestable head injury due to a fall from a low height". We note that the word "incontestable" is rather indicative, in this context, of an avowed intention to contest the results of Plunkett's study insofar as possible. It goes without saying that if the grandmother in question had not been holding a camera, her testimony and the testimony of the child's older brother who was playing with her would have been rejected along with all the others, as coming from family members.

Yet it is worth pointing out that although "nanny cams" have filmed a fair number of incidents of violent shakings, several of which ended up in court, none of the victims was found to have any of the symptoms of SBS. In fact, there has never been a single independently witnessed case of a shaking leading to SBS. Applying a obvious double standard (which they justify by the urgent need to protect children), the authors of [LV2011] do not hesitate to refute this argument against SBS by declaring "Le fait qu'il n'y ait pas de témoin n'élimine pas la *possibilité* d'un secouement"[3].

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[3] Personal communication to the author

3.3. Frequency of fatal short falls. Everybody agrees that it is extremely rare for a child to die from falling off furniture or playground equipment, yet the studies cited above show that exceptionally, this actually can occur. In a large population, even a rare incident is pretty sure to occur sooner or later, so the key question concerning any given population is: How many such events can we expect to see over a given period of time?

In his letter to the editor [Joffe90], Joffe estimates that even if one accepts the 18 fatal short falls reported in the Hall study as accidental and not inflicted, given the census report of 292,692 children aged 1-4 in Cook county in 1980, and considering that toddlers of this age group sustain short falls 3-5 time per week (according to an informal survey of 10 parents performed by Joffe), the chance of a child dying from a short fall is less than 1 in 10 million. He concludes therefore that it is still much too rare an event ever be acceptable as an explanation for an actual fatality.

Hall et al respond by observing that 18 falls in 4 years equates to about 4.5 falls per year, so that in a population of 292,692 children the expected rate of fatal short falls is about 1 per 65,000 children per year. This argument is completely clear and obvious. How did Joffe reach the strange probability of 1 chance in 10 million?

Considering that 292,692 children fall on average 4x52=208 times per year , they will each fall about 832 times over the course of 4 years, so that the total number of short falls in the population aged 1-4 over the 4-year period is about 243 million; dividing this number by 18 produces a result of about 1 fatal short fall per 13 million short falls. This must certainly be where Joffe obtained his figure of "less than 1 in 10 million". In other words, Joffe is computing the probability that a child will die from a specific one out of several hundred short falls, rather than the probability that a child will at some point sustain a fatal short fall. This is an absurd reasoning error, since the important rate is not the fatality rate per fall, but the fatality rate per population of children. Since each child is said to sustain an average of 832 short falls over the 4-year period, Joffe's result should be further divided by 832 to obtain a rate of about 1 death per 16,260 children over a 4-year period, or about 1 death per 65,000 children per year, as noted by Hall et al.

Continuing to object to the results of the Hall study, Joffe cites two hospital studies of 246 and 363 children respectively which found no instances of serious injury at all. In a separate Letter to the Editor published in the same issue of the Journal of Trauma, R. Helfer [Helf90] describes a study of his own [Helf77], in which he found that out of 85 children who fell out of bed, "57 children had no apparent injury, 17 had small cuts and/or bloody noses, 20 had a single bump and/or bruise, and one child had a fractured skull with no serious sequelae. There were no children who had any central nervous system injury and no deaths […]" Helfer claims that "there is a vast discrepancy" between the Hall study and his own, and that he doubts that Hall was able to correctly rule out child abuse, since their "results are so strikingly different". Helfer does not appear to understand that given Hall's data over hundreds of thousands of children who sustained short falls, one would not expect to see any significant injuries in a random sample of 85 children. Indeed, the mere fact that, in a sample as small as 85 children who fell out of bed, one child fractured his skull would appear to indicate that more serious injuries could be quite frequent in a population of hundreds of thousands of children.

Hall et al correctly point out in their response [Hall90] that the hospital studies cited by Joffe and Helfer (and more generally all published hospital studies) concern populations so small that it would be unrealistic to expect to observe an event that occurs only once per 65,000 children. One would think this would be obvious, yet this error appears again and again in the published literature. [LV2011] also note that "in five studies, none of the 708 children who fell in a hospital setting died". Yet, as mentioned above, at least three cases of children dying from short falls while in hospital can be found in the published medical literature.

Hall et al explain that one significant difference between their study and hospital studies is that "ours is an autopsy study, while the others are from hospital admissions […]", in particular because autopsy records contain cases of short fall victims found dead on arrival and thus never admitted to hospital at all, so that they could not have been included in any hospital study. A 2006 report from the French Institut de Veille Sanitaire[4] reported all deaths of children from long falls in the Ile-de-France region during a 7-month period of the year 2006. In the section on hospital transfer, we read "Soixante et un enfants (95 %) ont été amenés à l'hôpital et 3 enfants (5 %), décédés sur place, ont directement été transférés à l'institut médico-légal. Au total, 7 enfants (11 %) sont décédés de leurs blessures, 3 sur place et 4 à l'hôpital." Thus a hospital study would give the fatality rate from long falls as 3/61~0.05 or 5%, whereas a database study would give the rate as 7/64~0.11 or 11%, more than the double.

In a study from 2008, Chadwick et al [Chad08] investigated the EPIC[5] database, which contains records of all deaths in the State of California, and found 13 deaths attributed to short falls among a population of 2.5 million children over a 5-year period. Chadwick et al exclude two cases in which the direct cause of death was considered to be suffocation following a short fall, two cases in which a heavy object fell on a child, and one case of a fall from the second story. He also (arbitrarily) excludes one fall from a height that is unspecified other than "short", and one that was a short fall from a parent's arms onto rocks; thus he retains a total frequency of 6/2,500,000 per 5-year period, or about 1 in 2 million per year (a figure which increases to 1 in a million per year if the four excluded short falls are put back into consideration, giving a fatality rate of 10/2,500,000).

Certainly, this frequency is much lower than Hall's 1 in 65,000. It is possible that the disparity might be explained by certain differences in the social and natural features of San Diego County as opposed to Cook County. For one thing, the latter is at a serious economic disadvantage compared to the former, with poverty being a recognized risk factor for short falls due to the need for parents to work and small children to be unattended or attended only by slightly older siblings. A much greater frequency of unattended children would lead to a greater frequency of untreated short falls, which could significantly increase the fatality rate. The same economic factor could be at the origin of the many delays in seeking care attested by Hall, especially considering the lack of health insurance among the poor and the extraordinary cost of medical treatment in the United States. Finally, far more dangerous weather conditions prevail in Illinois than in California, with copious sleet, snow and ice throughout the winter. Two of the deaths in Hall's series occurred when a parent carrying a baby slipped and fell on ice.

As a final point, in classifying the level of injury and fatality due to short falls, most authors not only ignore direct fatalities never admitted to hospital, but do not admit as fatal short falls those children whose lives were saved by having access to timely treatment. Hall et al themselves do not attribute the two hospital deaths mentioned above directly to the fall itself, so much as to the fact that "delays

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[4] Les chutes accidentelles de grande hauteur d'enfants en Ile-de-France, Nord-Pas-de-Calais et Provence-Alpes-Côtes-d'Azur, Institut de Veille Sanitaire, 2006.

[5] State of California Epidemiology and Prevention for Injury Control Branch database

in presentation were caused both by parents and emergency room personnel, who felt that minor falls were benign, and waited until late symptoms developed before becoming concerned". Indeed, they note that at least 20% of the fall deaths in the records could have probably been avoided if treatment had not been delayed. The enumerations of the number of fatal short falls in different studies are in fact skewed by the fact that some short fall injuries become fatal if left untreated, whereas others which might have been fatal are treated and never reach this stage.

3.4. Frequency of fatal long falls. Long fall fatality rates play a particular role in the study of SBS and short falls. In particular, the relative robustness of children, only a very small proportion of whom die from long falls, is used as evidence that short falls should be even less dangerous, with even lower harm rates.

However, the are some major flaws in the long fall literature. The first point is the fact noted above that the fatality rates from hospital obtained from hospital studies are skewed due to the absence of data on victims found dead on arrival. But another important point is that fatality rates from long falls are subject to immense regional variations, due to differences in features such as income distributions or urban architecture. Indeed, economic situations are directly linked to the availability of childcare and thus to the age and amount of time that children are left alone, or under the surveillance of an older child, or of a single adult simultaneously caring for a large number of other children. As for the architectural setting, falls from windows or balconies in urban areas where most children live in tall apartment buildings will tend to be longer than those in suburban or rural areas where children mainly live in lower buildings or individual houses. In fact, lumping all falls of over 10 feet together in a single category is likely to provide rather misleading results. In particular, a long fall fatality rate reported in a specific hospital study must not and cannot be taken as a rate that applies to the population at large.

In the much cited article [Chad91], the authors observed 118 hospital admissions in San Diego County, California, for long falls of 10 feet (3m) or more, and found only a single fatality over a 3.5-year period. However, the very detailed study by the Institut de Veille Sanitaire cited above gives the following figures concerning three regions in France over a 7-month period:

Région	Total chutes	Total décès	Amenés à l'hôpital	Décès à l'hôpital
Ile-de-France	64	7	61	4
Nord-Pas-de-Calais	24	3	23	2
PACA	18	0	18	0

While the number of falls in each region is correlated to population, the fatality rates are noticeably dissimilar between the first two regions and the third. The explanation for this is given in the report, which notes that in the southern PACA region, all falls without exception were from heights of between one and three floors, with an average height of 4.4 meters, whereas in the other two regions, fall heights varied between one and six floors[6] with a much higher average. This fact alone justifies the wide difference in fatality rates. Even if one uses the lower fatality rate per fall obtained by (artificially) restricting to children brought to hospital (4/61=6.5% rather than 7/64=10.9% in Ile-de-France, 2/23=8.7% rather than 3/23=13% in Nord-Pas-de-Calais), these figures are still 8-10 times higher than the figure of 1/118=0.8% reported by Chadwick. It is likely that the low rate of 1/118 is explained because the main risk factors (architecture, income level etc) in San Diego County are, as in the PACA region, less prevalent. But Chadwick et al are making a significant error in taking the figure of 1/118 as a general measure of the danger of long falls in children without taking important factors such as fall type, fall height and landing surface into account.

Their purpose of calculating the long fall fatality rate was of course to compare it with the observed fatality rate of short falls. Chadwick et al give this latter rate as 7/100, based on 100 children brought to the hospital for short falls over the course of the study. As in the case of long falls, the authors omit consideration of the population of children not brought to the hospital. However, whereas the error this may cause for long falls is relatively small (since the only children who sustain a major fall and are not brought to the hospital are those who are found dead on arrival), *the error it causes in the enumeration of short falls cases is gigantic, since the enormous majority of children who sustain a short fall are never seen in hospital at all.* The study observes the population of children under age 15 in San Diego Country, whose population numbered about 600,000 in 1990. Over 3.5 years this amounts to about 2,100,000 children, all of whom certainly sustained occasional short falls. Thus, we can in fact conclude from the Chadwick study that the frequency of fatal short falls among children under 15 is about 7/2,100,000. Even accepting the rate of 1/118 for the frequency of fatal long falls (although the true figure is certainly higher as observed above), *Chadwick's own observations imply that the likelihood of a child dying from a long fall would be some 2,500 times greater than from a short fall.* But since they do not take the other children into account, the article explains that since the comparison of 7/100 with 1/118 would yield the conclusion that "the risk of death is eight times greater in children who fall from 1 to 4 feet than for those who fall from 10 to 45 feet", and that since this conclusion is "absurd", paradoxical and highly suspicious, "the best explanation for the findings is that for the seven children who died following short falls the history was falsified".

In truth, the only absurdity here is the authors' apparent unawareness that short falls outside of hospitals must be counted in the short fall fatality rate. There is no paradox, but merely an invalid comparison of two numbers both of which are incorrect, one tremendously so. Yet the Chadwick study is cited in [LV2011] as evidence that short falls should not be accepted as explanations of serious injury in small children. As for the study by Hall et al indicating that the fatality rate of short falls among children is higher than Chadwick believes, it is not cited in [LV2011] at all. The erroneous conclusion of [Chad91] continues to play an important role in the setting up of hospital protocols around the world that require that caregivers who recount a history of a short fall for serious or fatal head injuries in children should automatically be arrested for child abuse.

3.5 Incomparability of different age cohorts. Another flaw often found in articles devoted to denying the risks of serious harm caused by short falls and attributing all such injuries to child abuse is the selective comparison or non-comparison of different age cohorts, or the blurring of results by not mentioning ages at all. [Chad91] in particular does not contain a table of the ages of all the

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[6] with the exception of 2 falls in Ile-de-France from the 11th and 16th floors respectively.

victims of the 7 short fall fatalities, are though some are mentioned in passing. Of particular interest are the ages of the two children in the study on whom no other injuries and no signs of child abuse could be detected, as these be the only true candidates for short fall injury. If we were to learn, for example, that these children were very young whereas the children who survived long falls were much older, we could draw a very different conclusion, namely that the findings are due to the greater fragility of the youngest infants.

Indeed, it is a known fact that *infants are at greater risk of harm than older children from short falls*. The relative size and weight of very young infants' heads is proportionately larger than for older children. The center of gravity of the human body moves progressively downwards as the body grows larger. It is nearest the head in the youngest babies, meaning that most falls at this age will result in head impact. Furthermore, the parachute reflex, in which the infant extends its arms and legs towards the ground to break the fall, does not develop until after the fifth month of life, which means that one can expect direct impact between the head and the ground when a very young infant falls even a short distance. In [Huelke1998] we read: "Contributing to specific head impact problems are the large head of the child, the relatively soft, pliable, and elastic bones of the cranial vault, and the fontanelles. As compared with the adult, these features make the head of the child less resistant to impact trauma. […] The reasons for this greater frequency of head injury in children can be demonstrated both anatomically and biomechanically." The Hall study [Hall89] indicated that "infants are much more likely to die from a fall than older children. The fact that these deaths were due to head injuries is consistent with the anatomic fact that younger children have softer cranial bones and thus less cerebral protection". A very precise study from Melbourne, [Crowe12], provides the following table obtained from a hospital study:

Table 1
Head injury severity by age group.

Age group	*n*	Head injury severity		
		Mild, *n* (%)	Moderate, *n* (%)	Severe, *n* (%)
0–6 months	174	145 (83.8)	29 (16.7)	0 (0)
7–12 months	150	137 (91.3)	13 (8.7)	0 (0)
13–18 months	118	110 (93.2)	8 (6.8)	0 (0)
19–24 months	85	82 (96.5)	2 (2.4)	1 (1.2)
25–30 months	108	103 (95.4)	4 (3.7)	1 (0.9)
31–36 months	86	75 (87.2)	9 (10.5)	2 (1.1)

This table clearly indicates that falls and injuries in the 0-6 month age group are more frequent and have greater consequences than those in older infants and toddlers. Furthermore, we note that according to the descriptions given in the studies, many of the injuries classified as "moderate" in the Crowe study would have been classified as "severe" in the Williams study. Crowe et al explain that they use the term "moderate" for injuries with the following description: "a modified GCS of 9-12, indications of altered consciousness and reduced responsiveness and/or mass lesion and/or other evidence of specific injury on CT or MRI". Some of these injuries would have been classified as "severe" by Williams, whereas the term "severe" in the Crowe study requires a modified GCS of 3-8 representing altered consciousness or actual coma. Crowe et al further explain that while “reliance on the GCS as a marker of injury severity may lead to unreliable categorisation in young children" because “previous research has highlighted the limited value of the GCS as an index of brain injury” (in the 0-6 month age range), they were obliged to resort to this method due to a lack of radiology imaging in many cases. They specifically point out that in the 0-6 month group, “children with skull fracture all fell from a height of >1 meter,” showing that skull fractures occurred in the 0-6 month age group but were classified as moderate rather than severe, and that “the classification system used in this study meant that some children “with intracranial injuries significant enough to requre surgery” were classified as moderate. Finally, the Crowe study made sure that it was truly studying results of accidental injuries by specifically investigating child abuse in each case, and singling out the cases where it was suspected; one case in the 0-6 month group and one in the 6-12 month group[7].

In the study [Will91], Williams compared two cohorts of fall victims aged less than 3 according to whether the fall was corroborated (i.e. witnessed by more than one person or by an independent bystander) or uncorroborated (witnessed by a single caregiver). Unlike [Chad91], the Williams study does provide a breakdown of the fall type and injury severity by age, and in particular we note that more severe injuries from short falls occur in the group of infants aged 0-6 months than in the older groups. However, the article interprets this fact (and the rest of the observations given in the study; see the Appendix for a detailed analysis) by stating that the results “lead one to suspect that many if not all of the serious injuries sustained from short falls in the unwitnessed group represent child abuse”. Unfortunately, this conclusion does not seem to be borne out by the data any more than Chadwick’s (see the Appendix for a detailed analysis).

The article [LV2011] contains an entire section entitled “Traumatisme crânien minime : les chutes de faible hauteur”, which begins “En l’absence de définition formalisée du traumatisme minime, il a été décidé d’extrapoler à partir d’exemples de traumatismes considérés comme minimes, tout particulièrement les chutes de faible hauteur, pour lesquels des éléments de réponse existent dans la littérature scientifique”. Much as the term “shaken baby syndrome” contains the presumed cause in the name of the set of symptoms, the term “short fall” is taken here practically as a definition of “mild head injury”, a choice which amounts to a denial of the potential of short falls to cause serious injury and death. The identification of “mild head injury” with “short falls” is justified in the text by citing the erroneous conclusions of [Chad91] and [Will91].

In assessing the frequency of short fall fatalities, [LV2011] cites only Chadwick’s 2008 figure of 6/2,500,000 children in 5 years, or about 0.48 fatalities per million children per year, and not Hall’s figure of 1 fatality per 65,000 children per year. This is a clear example of cherry-picking to support a pre-selected hypothesis, the selfsame hypothesis that leads to the identification “short fall”=“mild head injury”.

[LV2011] cites as supporting evidence a set of five studies, containing a total of 708 children who sustained short falls, with no deaths. Only 94 of these children were under the age of 1. As mentioned above, a sample of 94 children aged less than 1 who sustained short falls serious enough

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[7] The article notes that (as also seen in other Australian studies), the low levels of inflicted injury are comparable to rates in Sweden and much lower than rates reported from the United States or the United Kingdom; suggested reasons are Australia’s “lower levels of teen pregnancy, stronger social welfare system and free and readily available access to health care”. The authors do not consider the possibility that some non-inflicted injuries are being diagnosed as inflicted in other countries.

to warrant a hospital transfer is far too small a sample to have a reasonable expectation of observing a phenomenon that may occur only once in tens or even hundreds of thousands of children each year.

Plunkett's article [Plunk01] detailing 18 cases of fatal short falls is dismissed on the grounds that the ages of the children ranged from 12 months to 13 years and therefore none was under age 1. Hall's study is unfortunately short on precise age details, although one fatal short fall of an 8-month old child is mentioned explicitly, but in any case this study is ignored by [LV2011]. If Chadwick had given the ages of the 2 children in his study with no other injuries or indications of child abuse who died following short falls, this would possibly have provided more cases, but these ages were again unfortunately omitted. It seems as though one of the main reasons for the shortage of cases of fatal short falls among children under 1 year old is the unfortunate lack of age information provided in the relevant articles.

3.6. SBS as a consequence of short falls. The governmental report [HAS2017] does not only deny the possibility of fatal short falls in infants under 1, but even insists that *"Il n'a été retrouvé dans la littérature aucun cas d'enfant de moins de 1 an ayant, après une chute de faible hauteur, l'association d'un HSD et d'HR".* This is in fact untrue; there are counterexamples in the literature. One appeared in the article by Gardner [Gard07] described below. More recently, a study by Atkinson et al. [Atkin17] presents eight cases of children who sustained witnessed falls (with an average of four witnesses) directly onto the occiput (as also in the case described by Gardner), all of whom were diagnosed with subdural hematoma and associated retinal hemorrage and all of whom had "a full investigation to exclude child abuse as cause of the injuries". According to the authors, "none of the children had a relevant prior medical history or a clinical history of either head trauma or suspected child abuse" and yet "the children's injuries all mimicked findings seen with AHT. If witnesses to the events had not been present, a high suspicion of abuse in these cases would have been justified." The mean age of the children in this study was 12.5 months. These studies lay particular emphasis on the presence of subdural hematoma following a backwards fall directly impacting the occiput. Neither Atkinson nor Gardner are cited in [LV2011] or in [HAS2017].

3.7. Retinal hemorrhage. Retinal hemorrhage plays a key role in the diagnosis of SBS, above all when it presents certain characteristics classified as "type 3" in [LV2011]: these present "multiples hémorragies de tous types (intra-, pré- ou sous-rétiniennes), tapissant toute la rétine ou éclaboussant la rétine jusqu'à la périphérie, associées à un placard hémorragique prémaculaire, uni- ou bilatéral (parfois évocateur d'un rétinoschisis hémorragique)." According to [LV2011], "Ces hémorragies de type 3 sont extrêmement évocatrices du SBS. Elles peuvent être considérées comme quasi pathognomoniques, ce d'autant plus si elles sont associées à un HSD, un oedème cérébral massif ou des lésions osseuses très évocatrices de maltraitance." The same principle is repeated in [HAS2017]: "Les HR sont quasi pathognomoniques du SBS quand elles touchent la périphérie de la rétine et/ou plusieurs couches de la rétine, qu'elles soient bilatérales ou unilatérales, avec parfois rétinoschisis hémorragique, pli rétinien périmaculaire." [LV2011] admits that type 3 RH "can be observed extremely rarely in violent, accidental HI [head injury] (road accidents)". [HAS2017] admits a very small list of other possible causes for type 3 RH: "Ce type de lésions peut se voir également après écrasement céphalique ou AVP [accident de la voie publique] à haute cinétique ou chute de grande hauteur (plusieurs étages)."

However, several counterexamples to this statement appear in the literature, and the doctors who observed them express vigorous objections to the doctrine that type 3 retinal hemorrhage are pathognomonic for SBS. A case study by Lantz [Lantz04] noted retinal hemorrhages in a child on whom a heavy television set fell while the father was alone at home with the children, but not in the same room. The retinal hemorrhages were immediately assumed to be the result of SBS and the child's 3-year old sibling was removed from the family. However this child, who had witnessed the accident, described it clearly to investigators, allowing them to reconstruct the accident and resulting in a withdrawal of the accusation of abuse. This striking study is probably at the origin of the addition of "écrasement céphalique" as a possible cause of type 3 retinal hemorrhage.

In [Lantz11], Lantz presented another case, this time of a 7-month old child who fell down the basement stairs and subsequently developed acute subdural hematoma and retinal hemorrhage whose description identifies it as type 3: "peripheral RHs were focally <0.1cm from the right ora serrata, whereas the left globe contained vitreal blood and extensive multilayered RHs extending 360º to the ora serrata". In the same study, Lantz presents a handful of other similar cases, obtained by searching through the medical literature, none of which are mentioned in [LV2011] or [HAS2017]. Lantz states openly that if it were not for the presence of multiple witnesses in each case, a diagnosis of child abuse would have been a foregone conclusion. Since similar accidents must also happen without multiple witnesses, it is certain that this situation must lead and has already led to mistaken diagnoses and miscarriages of justice. Aware of this issue, Lantz writes: "This case report refutes a pervasive belief that childhood low-height falls are invariably trivial events and cannot cause subdural bleeding, fatal intracranial injuries, and extensive multilayered RHs. The harmful and potentially devastating consequences for a caregiver or family facing a false allegation of child abuse obligate physicians to thoroughly investigate and accurately classify pediatric accidental head injuries".

Gardner [Gard07] presents a detailed study of a witnessed case of an 11-month old child falling directly backwards from a sitting position, with the occiput striking a carpeted floor. Gardner notes that this patient presented findings that closely mimicked SBS; a large acute left frontal subdural hematoma and "diffuse pre-/intraretinal hemorrhages, mostly posterior pole"; the pediatric ophthalmologist provided a fundus drawing depicting multiple hemorrhages. One week later, the same examiner observed "four quadrants of bilateral pre- and intraretinal dot, flame-shaped and white-centered hemorrhages - hemorrhages extend beyond equator - difficult to ascertain whether they extend to ora serrata". These retinal hemorrhages were also described as being "at different levels" and "multiple layers" that "extend to periphery", a description that corresponds to type 3. On suspicion of abuse, a skeletal survey of the child was done, but the results were normal. The family was also thoroughly investigated by police and social services, but no evidence of mistreatment was found; indeed the 5-year old sibling who observed the fall described it accurately and re-enacted it for the investigators with a stuffed animal. Of the five physicians treating the child, three believed the injuries were accidental and two believed they were inflicted. In the end, the court dismissed the charges. It goes without saying, however, that if the sibling had not been present or able to give such a convincing description, the story would have ended differently.

Gardner further compared American studies with two Japanese studies. One, by Aoki and Masuzawa [AM84] found 26 children aged from 3 to 14 months, who had subdural hematoma and retinal hemorrhage after a minor fall, and the other, by Ikeda et al [I87], found 14 cases. These authors were aware of the probability of child abuse and ruled out cases where there was any concrete evidence of it, retaining only cases for which they did "not believe the injuries were caused by battering or shaking" and that they were most "caused by a simple domestic head injury". Gardner notes that "The United States assumptions would require all the Japanese cases to be misdiagnoses and all 26-40 independent stories in Japan (which are similar to many of the United States stories) to be similar prevarications".

The case reported by Gardner, the videotaped Plunkett case, the Lantz case and 3 of the cases in [AM84] all prove that "the combination of intracranial bleeding and intraocular hemorrhaging has been noted to occur with witnessed short falls"; the Lantz case even includes retinoschisis and perimacular folds. *All of these thus constitute published counterexamples to the claim that type 3 retinal hemorrhage is pathognomonic for SBS*.

The case histories cited here and the many others to which they refer have been published for years, some even for decades. Yet in 2018 one of the authors of [LV2011] still wrote *"Une chute de moins d'1,5 m entraine exceptionnellement un HSD qui est* <u>*focalisé*</u> *et en regard d'une bosse et parfois d'une fracture. Une HR peut survenir mais là aussi exceptionnellement, elle est unilatérale, ne dépasse pas le pôle postérieur de la rétine, et est intra rétinienne"*[8] At this point, we are well beyond analysing mathematical errors or misinterpretations; the counterexample literature is simply being ignored in the governmental report, and also by the expert witnesses at SBS trials who are among its authors. Even if the authors have good reason to shed doubt on all of these case histories, the scientific method would demand that they should provide their analysis, not simply remain silent on the subject.

3.8. There is no controversy. We end this article with a short paragraph that would actually deserve a much longer discussion. One of the main weapons used by the supporters of a rigid diagnosis of SBS in the presence of SDH and RH is the denial in every forum (teaching, training of magistrates, published articles, courtroom testimony and private discussions) that there is any controversy whatsoever among medical professionals. A recent text by Leventhal and Edwards [LE2017] states in the introduction that one of their purposes is "to clarify that there is no significant controversy about the diagnosis of physical abuse and abusive head trauma in clinical medicine; rather, the existing controversy in the courtroom and media has been created by the use of scientifically unsupported explanations".

This claim of complete consensus in the medical community is doubly untrue. On the most obvious level, it denies the existence and ignores the publications of doctors who challenge the notion that aside from violent high-speed accidents, inflicted injury is the only possible cause of subdural hematoma and retinal hemorrhage found in infants.

But there is a more subtle and less visible controversy in the medical discussion of SBS (or AHT, abusive head trauma, as it is now called, due to the inclusion of impact as well as shaking in the presumed possible causes), which consists in contradictory statements that can be found by looking uniquely within the published consensus statements and recommendation reports. For example, in a text entitled *Consensus statement on abusive head trauma in infants and young children* [Choud17], which assures us that "AHT is a scientifically non-controversial medical diagnosis broadly recognized and managed throughout the world", the authors make the following short and simple claim: *"No single injury is diagnostic of AHT."* Yet as cited earlier, in [HAS2017] we read: *"Les HR sont quasi pathognomoniques du SBS quand elles touchent la périphérie de la rétine et/ou plusieurs couches de la rétine, qu'elles soient bilatérales ou unilatérales, avec parfois rétinoschisis hémorragique, pli rétinien périmaculaire",* which directly contradicts the statement of [Choud17].

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8 Private communication to the author.

Further, the authors of [Choud17] claim to "expose the fallacy of simplifying the diagnostic process to a 'triad of findings' - a legal argument and not a medically valid term". This famous "triad" refers to subdural hematoma, retinal hemorrhage and cerebral oedema. Choudhary et al. explain that "Efforts to create doubt about AHT include the deliberate mischaracterization and replacement of the complex and multifaceted diagnostic process by a near- mechanical determination based on the "triad" — the findings of subdural hemorrhage, retinal hemorrhage and encephalopathy. This critique has been sensationalized in the mass media in an attempt to create the appearance of a 'medical controversy' where there is none. The straw man 'triad' argument ignores the fact that the AHT diagnosis typically is made only after careful consideration of all historical, clinical and laboratory findings as well as radiologic investigations by the collaboration of a multidisciplinary team."

However, in the HAS recommendations [HAS2017], we find the following instructions on diagnosis of AHT (called TCNA=traumatisme crânien non accidentel):

*"le diagnostic de traumatisme crânien non accidentel par secouement est **certain** en cas de:*
- *HSD plurifocaux avec caillots à la convexité (vertex) traduisant la rupture de veines ponts,*
- *ou HSD plurifocaux et HR quelles qu'elles soient,*
- *ou HSD unifocal avec lésions cervicales et/ou médullaires."*

To be sure, the HAS recommendations do point out the necessity of first excluding all differential diagnoses, which would diminish the "certainty" of the above diagnosis were it not that the list of alternative possible causes for multifocal subdural hematoma provided in the same text contains exactly one element: "*Seuls les traumatismes accidentels avec forte décélération (type accident de la route) peuvent entrainer des HSD multifocaux et une rupture de veines ponts",* while the only admitted alternative causes for type 3 RH are *"écrasement céphalique ou AVP* [accident de la voie publique] *à haute cinétique ou chute de grande hauteur (plusieurs étages)."*

In other words, no matter what the circumstances, clinical history or lack of other injuries, the diagnosis of TCNA is supposed to be "certain" in the presence of not *three but just two or even just one element of the famous triad,* as long as the SDH is multifocal and/or the RH of type 3. This hardly resembles a complex and multifaceted process; in fact it is precisely the type of "near-mechanical determination" denied in [Choud17].

It is not enough for articles to proclaim consensus, or to announce their conclusions as established fact. This would be justified only if the scientific work in the field could be said to be scientifically solid, free of error, and above any reproach of selection or other bias. We have shown here what many medical professionals already know, namely that this is far from the case. The controversy in the medical community exists, and until subdural hematoma, retinal hemorrhage and short fall injury in children have been investigated in large-scale independent studies with solid scientific methods, it will continue to exist. All of the high-profile controversy debated in the courtroom and reported in the media stems directly and uniquely from this.

Appendix: Analysis of the Williams study [Will91]

In [Williams91], Williams reports that 398 children were brought to a specific hospital with a history of falling, and that 159 of these children satisfying certain criteria (under age 3, unobstructed fall, witnessed fall, height of fall known, injuries known) were included in a study that separated them into two groups: in one group, containing 106 children, the falls were witnessed and corroborated by either two or more people or by a witness unrelated to the child, and in the other group, containing 53 children, the falls were witnessed only by a single person, the child's caregiver at the time of the fall. The results of the study are given in the following bar chart.

ries in Children from Witnessed Falls 1351

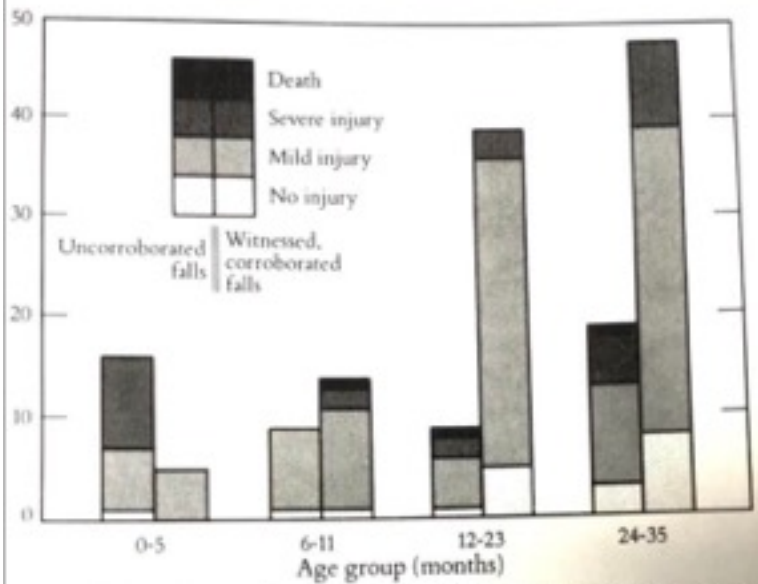

FIG. 2. Comparison of uncorroborated falls with witnessed, corrob-ted falls by age group and extent of injury.

Concerning the 106 children whose falls were corroborated by two or more people or by an unrelated witness, the following chart is given, correlating height of fall to level of injury.

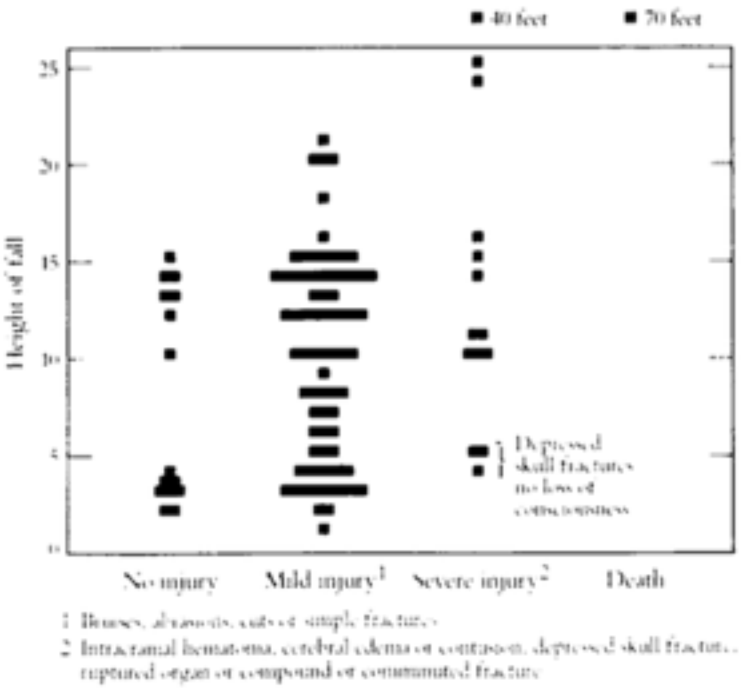

FIG. 1. Witnessed and corroborated falls; correlation of height of fall with extent of injury.

From the second chart, Williams draws the following conclusion: "Infants and small children are unlikely to be seriously or fatally injured in falls of less than 10 feet". This observation is surprising in view of the fact that *the chart shows 3 severe injuries in the group of 30 children who sustained falls of 5 feet or less.* This is a severe injury rate of over 10% among children brought to the hospital, a figure that hardly qualifies as rare[9].

It is regrettable that there is no information correlating age to fall height in the corroborated group, so that we cannot tell whether the greater injuribility from short falls is correlated with a younger age. As for the uncorroborated group, we read: *"When cases without a corroborated history are examined, at the beginning of the article that in the uncorroborated group, severe injuries occurred in 18 and death in 2 of 53 patients falling from less than 5 feet."* The bar chart actually shows that there were only a total of 18 cases of severe injury or death in the uncorroborated group, so that the 2 deaths in the sentence above must be included among the 18 severe injuries. This number also indicates that every single fall in the uncorroborated group was said to be from a height of less than 5 feet (although this is not actually stated in the article). This seems surprising, but it is at least partly explained by the fact that if the falls in question were unwitnessed, then many of them probably took place inside the home, and therefore cannot have been from a great height.

Certainly, Williams' data gives rise to some legitimate questions:

1) Why did the study find that during the observation period, 16 infants aged 0-5 months sustained falls (or alleged falls) serious enough to warrant a hospital transfer while in the care of a single person, but only 5 in witnessed situations? And why is this imbalance inverted among older children, of which only 37 were brought to the hospital for unwitnessed falls as opposed to 101 in witnessed situations over the same time period?

2) Why were 18 serious injuries/deaths sustained in the unwitnessed group from falls described as under 5 feet, whereas only 3 serious injuries and no deaths were sustained from short falls in the witnessed group?

These questions are striking and legitimate, and any hypothetical answer would deserve to be investigated and supported with further data. But nothing more is done in the article, which simply terminates the above observations with the conclusion: *"That severe injuries and deaths from falls of five feet or less only occurred in the uncorroborated group leads one to suspect that many if not all of these injuries attributed to falls of low height represent child abuse."*

We claim that this conclusion is hasty and premature due to some important factors not being taken into account.

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[9] A similar observation can be made about the study cited in [Helf90] which observed 85 children who fell out of bed, and found nothing more than bloody noses and bruises except for a single child with a fractured skull. Rather than drawing the conclusion from this small sample that falling out of bed is basically harmless, the authors should realize that given the enormous number of children who fall out of bed, finding an injury as serious as a fractured skull in a sample of just 85 children indicates a risk that is non-negligible if the fall involves a head impact. And this is just from the height of a bed, whereas Williams studies falls from heights up to 5 feet.

Suppose first that we accept the author's hypothesis all or virtually all of the cases of severe injury in uncorroborated short fall cases are lies. To study the true incidence of short fall injury, these cases should then be removed from the bar chart. But what happens if we do this? We notice that the chart then becomes extremely unbalanced, with all severe injuries from falls of any height occurring uniquely in the witnessed group. This is tantamount to taking the attitude that a fall can produce a severe injury if and only it is independently witnessed. In other words, *the article's conclusion amounts to expecting to see a chart in which no child is ever severely injured in a fall while with a single caregiver, while accepting that 14 serious injuries and 1 death occurred from falls among 106 children in the witnessed group (of which 3 resulted from short falls).* This is an unjustified, unrealistic and unexplained expectation.

In fact, the article's statement that "severe injuries and deaths from falls of five feet or less only occurred in the uncorroborated group" is misleading and wrong, since 3 such injuries occurred out of 30 such falls in the corroborated group. In fact, *given the 10% rate of severe injuries among the 30 children hospitalized for short falls in the corroborated group, one could expect from this alone to see about 5-6 cases of severe injury in the uncorroborated group.* This already significantly weakens the conclusion that injuries from short falls are really due to child abuse in "many if not all" cases.

Still, we must consider the fact that there are not 5-6 cases of severe injury from short falls in the uncorroborated group, but 18. Before jumping to the conclusion that two-thirds of severe injuries from alleged short falls are due to child abuse, is there perhaps more information that, if taken into account, could at least partially explain the discrepancy?

There is indeed a factor of major importance which is never mentioned nor taken into account: the amount of waking time spent by a child in unwitnessed situations (i.e. with a single caregiver) versus witnessed situations (outdoors or with multiple family members or visitors). In fact, the expected numbers of short falls and severe injuries need to be rescaled in proportion to the amoung of waking time spent by a child in each age group in the two situations. In other words, for a given age group, it is incorrect to assume that in the absence of child abuse we should expect to see equal rates of short fall occurrence and equal rates of severe injury; in fact we should expect to see equal rates of short fall occurrence and equal rates of severe injury *per equal amount of time*. Children do not belong to the witnessed group or the unwitnessed group; they spend varying amounts of time in each situation. The more time they spend in the unwitnessed group, the more short fall occurrences and severe injuries will be seen in that group.

Consider the particular situation of the youngest group of infants aged 0-5 months. This is the only group in which unwitnessed falls outnumber witnessed falls and unwitnessed severe injuries far outnumber witnessed ones. The authors immediately jump to the conclusion of child abuse, failing to take the following fact into account: *Most newborns and pre-mobile infants spend the majority of their waking time alone with a single caregiver.*

In the earliest months of its life, the majority of moments when a tiny baby is likely to find itself in "witnessed" situations is during the early evenings when both parents and possibly older siblings are at home, but before the baby goes to sleep for the night. There are also occasional visits of relatives to the home, and walks outdoors and shopping expeditions, but from these one must discount the parts where the baby is asleep in its pram. Very young infants typically sleep about 16 hours per day, so their waking time totals about 56 hours per week, of which usually no more than 10-12 will be spent in witnessed situations. According to this estimate, about four-fifths of a very young infant's waking time is likely to be spent alone with a single caregiver.

Another particularity of the youngest age group is that these infants do not move around by themselves, so their falls will mainly be from arms or from furniture; in other words all falls in this group are short. In the complete absence of child abuse, then, we would expect to see about four times as many short fall occurrences and about four times as many severe injuries in the unwitnessed group than in the witnessed group of infants aged 0-5 months. (In fact, one could even suppose that more than four-fifths of short falls would take place in unwitnessed situations, given that it is easier for two people to take care of a very young infant than for one person, particularly during the performance of complex gestures like bathing or changing.)

This argument adequately explains the fact that 16 out of the 21 falls in infants aged 0-5 months occurred in the unwitnessed category. The number of short fall occurrences in the bar chart does not reflect any incidence of child abuse. The enormous increase in children brought to the hospital from witnessed situations in the older groups certainly reflects the much greater amount of waking time spent in witnessed situations by older babies and toddlers, as well as the fact that falls and injuries are more frequent in playgrounds, and that protocol in licensed day care centers attended by greater numbers of older children requires that children be checked at the hospital even for falls where the injuries appear to be minor.

If all the cases of serious injury in the age group 0-5 months were removed from the bar chart (under the assumption that they are not really short falls), we would find a nearly equal number of short falls in the witnessed and unwitnessed group. But this really makes no sense given how much more time infants spend in the unwitnessed situation.

What about the high rate of severe injury in the unwitnessed group of 0-5 month old children? In the lack of solid data about the frequency of severe injury from short falls in this group, it is difficult to make a rigorous statement. In the table from [Crowe12] given in section 3.5 above, there are 29 cases of moderate injury from short falls among 143 children, but as noted there, the description of the moderate injuries shows clearly that some of them would be classified as severe by Williams. Unfortunately, we do not know how many. We can assume that the rate of 10% among children brought to the hospital is a lower bound, since this is the rate observed by Williams himself for older children, and as explained earlier, babies are more subject to head injury. But what is the true rate? We simply don't know. Perhaps we would expect to see 2 children with such injuries among 16 brought to hospital, perhaps 3; we don't know. To properly compute the expected values, we would need better studies, better statistical analyses and better interpretation of the results. Nine cases of severe injury from short falls does seem excessive, and it is very possible that the difference is due to child abuse.

The point made here is not a denial of the existence and seriousness of child abuse. Rather, we are underlining the danger of taking the observation that among five infants brought to the hospital over a period of two years for witnessed short falls, none sustained serious injuries, and generalizing this observation to a belief that short falls in neonates are not dangerous and ***any*** *severe injury said to be from a short fall is almost certainly the consequence of abuse. We have shown here that it is statistically unreasonable and unjustified to assume that all cases of severe injury from short falls in* *infants are due to child abuse. This radical hypothesis is unjustified and untenable and leads to miscarriages of justice.*

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