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When lack of information leads to apparent paradoxes and wrong conclusions: analysis of a seminal article on short falls

Leila Schneps

The seminal article 1991 by Chadwick et al

One of the most frequently-cited pieces of scientific research in the field of SBS is a 1991 article by Dr. D.L. Chadwick et al, entitled *Deaths From Falls in Children: How Far is Fatal?*[1] It is difficult to overestimate the influence of this short but striking article, which has been cited countless times and is referred to by all doctors who deal with head trauma in children.

The authors of the article present data observed over a 42-month period in the Children's Trauma Center serving San Diego County, California. During this time, a total of 317 children under 15 years old were brought into the trauma center because of injuries attributed to falls. Thirty-four cases in which no information about the height of the fall was known were dropped from the study, which concentrated on 283 cases in which specific circumstances were provided by the person bringing the child to the center. These 283 cases and the fatality outcomes were divided into categories as follows:

- Fall height of 1-4 feet, 100 cases, 7 fatalities
- Fall height of 5-9 feet, 65 cases, 0 fatalities
- Fall height of >9 feet, 118 cases, 1 fatality

The article gives the situations to which the short fall deaths are attributed as one fall on stairs, two from the arms of an adult and four from furniture. It gives a brief description of three of the cases (concerning children aged 6 weeks, 13 months and 11 months) and of the single long fall fatality (11 months).

After providing this information, Dr. Chadwick and his co-authors go on to discuss the possibility that the caretaker's description of a short fall to explain a child's injuries may not be truthful. They note that one can often reliably diagnose child abuse in cases where the victim presents multiple other injuries in different stages of healing, or multiple impact sites

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[1] D.L. Chadwick, S. Chin, C. Salerno, J. Landsverk, L. Kitchen, Deaths from Falls in Children: How Far is Fatal? *J. Trauma* 31 No. 10, 1991, 1353-1355.

on the head observed in cases where the given explanation is a single short fall (not a fall on stairs, which is considered as a sequence of short falls). They observe that many such injuries were observed in the seven children whose deaths were attributed to short falls, giving the following following table:

• Old fractures: 2
• Bruises on trunk or extremities: 3
• Genital injury: 2
• Two head impact sites: 2
• No associated injury: 2

This collection of additional data, obtained from examination and autopsy of the deceased children, adds great weight to the statement that many badly hurt children brought to the hospital by caregivers who attribute their injuries to a short fall are actually abused.

The next part of the article addresses the problem of children whose injuries strike the examining doctor as ``discrepant'' or not compatible with the caretaker's account of what caused them. In such cases, ``inflicted injury is often diagnosed when the clinician can state with a high level of certainty that the single injury seen in a child could not possibly have been produced by the event described by the caretaker''.

This assertion may give one pause. How does a clinician reach a high level of certainty? In order to state that a given injury cannot have been produced by a certain cause, he or she needs to be certain that such a thing is not merely rare, but has actually never occurred. Such a claim must necessarily be based on large-scale studies, since studies of just a few individuals are unlikely to reveal rare events. If a clinician is certain that a short fall cannot be fatal, then he or she will diagnose abuse any time a child dies after a short fall, so it is clearly of major importance to know whether it is possible for a short fall to be fatal or not. The remainder of Chadwick's 3-page article focuses essentially on attempting to settle this question in the negative by citing a number of previous studies.

The vast database of the National Electronic Injury Study of the Consumer Product Safety Commission (NEISS) was investigated in a study by T. Sweeney[2] which concluded that children can die from falls from heights of as little as 1 foot. However, Chadwick et al dismiss these results on the grounds that the NEISS database itself may not be reliable, in that unrecognized cases of inflicted injury may be included in the database as short falls.

A detailed study by J. Hall et al[3] reviewing records in the Medical Examiner's Office of Cook County, Illinois, which found 18 cases in which fatal head injury was attributed to a fall of 3 feet or less, is also dismissed by Chadwick on the same grounds, namely that "their work omits the detailed information necessary to exclude inflicted injury, and many of those cases might be in that category".

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[2] T.B. Sweeney, X-rated playgrounds? [Commentary], *Pediatrics* 64:961, 1979.

[3] J.R. Hall, H.M. Reyes, M. Horvat, The mortality of childhood falls, *J. Trauma* 29:1273, 1989.

Instead, Chadwick relies on five studies of falls which occurred in public places (hospitals or outdoors) rather than relying solely on caretakers' stories. Two of these studies observed a total of 180 small children who fell from heights of less than 4 feet while in the hospital, and found that they sustained only very minor injuries or were uninjured. Two more studied falls from buildings (one using cases from ten years of experience and the other a set of 66 cases) and concluded that only falls from the fourth floor or higher resulted in death. The fifth study observed 100 falls of children and adults, and apart from one death in an apparent 10-foot fall of a child which was unobserved, found that life-threatening injury required at least a 15-foot fall.

In contrast, write Chadwick et al of their own observations of the San Diego trauma center records, "the data in the present study show an astonishing concentration of risk of death in the group with the shortest falls". It is this contrast with the five studies above that lead them to their main conclusion:

> **"If the histories of short falls are accepted as correct, the conclusion would be reached 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. Since this conclusion appears absurd, it is necessary to seek another explanation[...] The best explanation of the findings is that for the seven children who died following short falls the history was falsified."**

Statistical errors and omissions in the Chadwick study

On first reading, Chadwick's conclusion seems straightforward. Experience and common sense tell us that child abuse exists, that short falls cannot cause more deaths than long ones, and that seven deaths out of 100 short falls is really too many. Add in the other injuries seen on the children, and Chadwick's conclusion can seem nothing short of inevitable. This is how it is taken by pretty much the entire body of medical professionals working in SBS. Nowadays, the diagnosis of inflicted injury arises automatically whenever a child presents serious injuries or dies following an event explained by the caretaker as a short fall. This diagnosis can lead immediately to arrest, investigation and trial, most often with immediate separation of the suspected parents from all of their children.

On a second, more careful reading of Chadwick's paper, however, a number of problems arise, all of them are based on the key issue of various types of invisible information left out of the analysis, whose presence could make a major difference to the conclusions.

1. Ages of the victims

The first example of important missing information is the ages of the children who died. Generally speaking, babies are more fragile than toddlers[4], toddlers more fragile than

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[4] "Despite similar injury severity scores, infants sustained more skull fractures than toddlers (71% v. 39%)", N. Ibrahim, J. Wood, S. Margulies, C. Christian, Influence of age and fall type on head injuries in infants and toddlers, Int. J. Dev. Neurosci. 30(3) (2012), 201-206.

schoolchildren, and children more fragile than most adults. But children who fall from heights greater than 10 feet must be climbing; they cannot be tiny pre-mobile babies. Most of the detailed information on the ages of the children who fell from different heights in Chadwick's study is absent. The only information given is that of the 317 children brought to the trauma center, there were 30 babies less than one year old, 145 toddlers aged 1-3, 61 children aged 4-6, 65 aged 7-12 and 16 aged 13-15. But we are not told the ages of the children who had short falls, or long falls, or the ages of each of those who died, apart from what can be gleaned from the brief description of a few cases mentioned above, namely that three of the short fall victims were aged 6 weeks and 11 and 13 months, and the single long fall fatality was 11 months old. This data is insufficient to draw any conclusions or to refine those of the article. However, were the complete data available, it could possibly lead to quite different conclusions.

To illustrate this, let's invent a purely theoretical, hypothetical but perfectly plausible set of data giving the ages of the children who fell, and those of all the fatalities. We naturally suppose that the great majority of babies fell from low heights, with only a tiny number being physically capable of climbing up to a dangerous place, and furthermore unsupervised. We also assume that few of the older children aged 4-15 are in the group where the height of the fall could not be determined, as children of this age can explain precisely what occurred. Finally, we place more toddlers in the long fall group than in the medium fall group, reflecting the fact that the average age of children who fall out of windows is known to be around 4, whereas medium falls tend to take place more on playgrounds, where children are generally somewhat older. The following table of hypothetical data respects these plausible hypotheses.

The 100 children brought to the trauma center for short falls, with average age 18 months, were:

27 of the babies under age 1
73 of the toddlers between 1-3

The 65 children who fell from medium heights, with average age 6 or 7, were:

1 baby under age 1
15 toddlers aged 1-3
49 children aged 4-15

The 118 children who sustained long falls, with average age 4, were:

2 babies under age 1
29 toddlers aged 1-3
87 children aged 4-15

The remaining 34 children (28 toddlers and 6 older children) who fell from unknown heights are not listed.

Let's also assume that all seven deaths occurred in babies aged 0-13 months, as we already know is the case for four of them.

Given this table of information, what would be the most likely explanation for the short fall and long fall fatality rates observed by Chadwick? The most natural conclusion would seem to be: *"Whether short or long, falls are much more dangerous and likely to be fatal in babies up to around age 1 than in older children. The reason there are so few fatalities in the long fall group is because most babies are more supervised and also unable to climb. In particular, even though the average age of children who fall out of windows is 4 years old, the only fatality was a child of 13 months."*

Let me immediately make it clear at this point that I am not suggesting that this is in fact the correct interpretation of Chadwick's observations. Indeed, **I am as far as possible from denying that child abuse exists and must be detected and stopped whenever possible;** furthermore the existence of additional injuries on the children is a key element, which we discuss further below. The above scenario is given to illustrate the fact that invisible information can have major significance and lead to very different but equally natural hypotheses explaining the data. A lot of pitfalls can be avoided by including all of the relevant statistics in any study.

2. *Number of short falls versus long falls in the general population of children*

The second example of missing information concerns the frequency with which small children actually fall. As every parent knows, short falls are extremely common, whereas long falls are very rare. Indeed, every child who sustains a long fall will automatically be taken to the hospital, so we may accept that the 118 children seen by Chadwick represent the true number of long falls in the population of San Diego County (though it might be slightly too low if some of the group for which fall heights were not specified were actually long falls). But children take insignificant tumbles all the time, so the situation for short falls is completely different. For the purpose of this text, we will count as "short falls" only those involving impact to the head.

How often does a typical child actually take such a fall? Thinking back over my own experience as mother to a family of active children, I can effortlessly remember several over a period of some fifteen or twenty years. I once saw my toddler daughter's stroller overbalance and crash to the ground with her in it after I lifted off a heavy bag of groceries that was hanging from the handlebar. The same toddler crashed her training wheel bike and fell onto the pavement on her face. My son also crashed to the pavement on his face after inadvisedly trying to leapfrog a low stone pillar. Another daughter was hit in the head by a swinging door she was playing with and required stitches. I myself remember a violent and unexpected crack in the back of the head which turned out to be because I had slipped and fallen on ice with such rapidity that I hit the ground before realizing I had fallen - I was carrying a baby at the time, and was lucky I went down onto my back. Two of my nieces suffered from an ill-understood brittle bone disease that had them in casts and wheelchairs

on a regular basis for several years, fortunately at an age where they were able to explain the simple household bumps that were causing their fractures (without which their parents would undoubtedly have been suspected of abuse). My mother fell down the stairs carrying one of my children. My nanny accidentally dropped a wriggly baby onto the staircase and watched in horror as she rolled all the way down; fortunately it was thickly carpeted. My nephew slipped on algae-covered rocks at the seaside and hit his forehead on a sharp point; more stitches. He also cracked his head on the bathtub while leaping from the toilet to catch onto the shower rod, presumably under the impression that he was a monkey in the jungle, and again at the swimming pool when he thought diving towards the side of the pool could be a fun idea.

I believe we are a pretty typical family, active but not daredevil. None of the injuries we sustained were at all life-threatening. It's fair enough to conclude from this that young children are pretty robust on the whole, and even an impressive bump or a bang on the head is not necessarily very serious, let alone fatal. On the other hand, given that I situate my family of children somewhere in the middle of an overarching scale ranging from kids on the one end who spend their time sitting on cushions and sewing fine seams (or more likely on couches holding remotes or smartphones) to those on the other who seem destined from birth to fly rockets or solo climb El Capitan, one can certainly state that the average number of short falls with head impact in children must be fairly high.

This question is not explicitly addressed in either of Chadwick's articles, apart from a passing allusion to a study by Kravitz et al which determined that while more than half of infants under the age of one year take at least one fall from a surface 3 or 4 feet off the ground, "none sustained fatal injuries" (Chadwick does not explain how many mothers were queried and over what time period, so we don't know whether "none" refers to 100 babies in one study or a million babies in a population). If we accept Kravitz's assertion that 50% of all babies take a short fall in the first year of life, then we should double it at the very least for toddlers and pre-schoolers. It is difficult to find any precise statistics on the matter, even if the web is filled with blog comments by parents whose children fall and hit their heads as often as several times a month. The vast majority of these falls cause little or no damage, and only a small fraction worry the caretakers enough for them to actually bring the child to the hospital.

Let's show, as we did for the missing age information, how using a plausible estimation of the number of short falls in children can make an enormous difference to Chadwick's assertions. Let's use the rough (and probably very low) estimate that the number of falls per year in children is equal to the number of children, meaning each child might be expected to take a short fall with head impact about once a year.

The population of San Diego County in the 1990s was about 2,800,000 individuals, with about 200,000 of them being children under 5 and 600,000 children under 15. The pediatric trauma center from which Chadwick obtained his data serves children under 15 in the entire county; any child under 15 brought to any hospital for head trauma would be sent there, so we can be certain that Chadwick's study did not miss any serious cases of falls in San Diego County. With the rate of one short fall per child per year, we are looking at a collection of

around 2.1 million short falls of children under 15 during the 3.5-year period covered by Chadwick's study; the seven fatalities he observed all come from this pool.

Long falls in children are very rare. Every child who takes a long fall of more than 10 feet is brought to the hospital, and all of them are in Chadwick's data set. So we can conclude that there were a total of about 118 long falls over 3.5 years among the children under 15 of San Diego country, as compared to around 2.1 million short falls among the same children over the same period. This means that the risk of death from long falls is correctly assessed by Chadwick as being around 1 in 118, whereas even if we assume that all the short fall histories were true, the short fall risk of death would be only about 7 in 2.1 million. Using the formula

(1/118) = (2,542) x (7/2,100,000),

we see that *Chadwick's own data should be interpreted to tell us that* ***if we accept the seven short fall histories as true, the risk of death from long falls is more than 2,500 times greater than the risk of death from short falls****, which is totally opposite to his absurd assertion that we would be led to the conclusion that* ***"the risk of death is eight times greater in children who fall from 1 to 4 feet than in those who fall from 10 to 45 feet"****.*

Chadwick's error here is that ***he calculates as though the 100 children who were brought to the trauma center for short falls are all children who experienced short falls, just as the 118 children brought in for long falls are all children who experienced long falls.*** He is clearly not aware that by ignoring the data of the number of short falls among children, he is making this implicit assumption, which is of course completely wrong! The number of children actually brought to the hospital for short falls is absolutely not relevant when considering the risk of death from short falls. The risk of death is the simply the number of fatalities per total number of short falls.

Before moving on, I want to be clear once again that I am not claiming the above statistical estimate of children's short-fall frequency is fully accurate, but simply showing what different conclusions can be reached when we are in possession of important statistical information that is missing from the article, and how ignoring it can lead to absurdly wrong statements such as "the risk is 8 times higher". The seeming paradox that led Chadwick et al to conclude that *all* the short fall stories were falsified is nothing but an optical illusion.

3. The true risk of death from short falls

It seems that the importance of knowing the true risk of death from short falls eventually came to Chadwick's attention, because nearly two decades after the article we are analyzing, he and some of his colleagues published another one, entitled *Risk of Death Resulting From*

Short Falls Among Young Children: Less than 1 in 1 million[5], that deals precisely with this question. The authors use the only approach that really makes sense, namely an examination of databases and of previous studies in order to determine the number of deaths in small children that can reliably be attributed to short falls without suspicion of abuse. The plan is a good one, but as we will see, this article suffers like the previous one from multiple problems with the way it was carried out.

A study of the California Epidemiology and Prevention for Injury Control Branch (EPIC) database covering the whole state of California yielded the following information. Over the five-year period 1999-2003, 34 deaths due to falls were recorded for a population of 2.5 million children aged 0 through 4 years, of which 13 were classified as short falls. Chadwick et al obtained the 13 corresponding death certificates and examined the causes of death in more detail. They excluded seven cases that are not considered real cases of short falls, leaving a total of six "ordinary" short falls that were considered legitimate causes of death according to the medical records, and not attributed to any abuse or inflicted injury. In the newer article, Chadwick et al use this data to calculate the risk of death from short falls, and show that it is extremely small: 6 such falls seen in 2.5 million children over a five-year period averages out to a rate of about 0.48 such deaths per million children per year, or barely 1 child in 2 million each year. This is the "less than 1 in a million" figure of the title: the conclusion of the article is that the true risk of death from short falls is at most 0.48 per million children per year.

However, there are numerous reasons to believe that this fatality rate of 0.48 per million children per year is too low. To begin with, it is unclear exactly why Chadwick discarded seven of the 13 deaths recorded in the EPIC database as short fall deaths. He gives valid reasons for some of the exclusions, such as cases that involved falling furniture rather than falling children, and one fall from a second story window. He further excludes two deaths attributed to suffocation following a fall, an explanation that seems somewhat mysterious. He excludes one further case where the fall height was not specified, even though it was classified as a short fall in the EPIC database; this one may truly have been a short fall, and excluding it may incorrectly lower the frequency. Finally, he excluded a case where a child fell from the arms of an adult onto rocks. This seems like a legitimate short fall - why exclude it? Is it because the description of the fall history would not be considered "discrepant" in this case? There seems to be a whiff of circular reasoning in the unexplained choice to discard this example.

The second, more important reason to believe that the 0.48 figure may be too low comes from the existence of other studies. Both the Chadwick papers cite two earlier large-scale database studies that give higher risk assessments; the NEISS study and the work of J. Hall et al (footnote 5). These studies, already dismissed by Chadwick in the 1991 article, are mentioned and dismissed once again in 2008. Since they both produce risk rates that are

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[5] D.L. Chadwick, G. Bertocci, E. Castillo, L. Frasier, E. Guenther, K. Hansen, B. Herman, H. Krous, Annual Risk of Death Resulting From Short Falls Among Young Children: Less Than 1 in 1 Million, Pediatrics 121 (2008), 1213-1224.

markedly different from the final rate of 0.48 per million children per year accepted by Chadwick, it is worth having a closer look at his reasons for rejecting them.

The NEISS database is specifically concerned with deaths involving commercial products. Over a twelve-year period, it lists 9 deaths from falling off playground equipment, in a population of 400,000 children under 5. As six of these cases were either from heights greater than 4 feet or had no witnesses, Chadwick retains only 3, and thus calculates the risk of deaths from short falls in children as 3 in 400,000 over 12 years, which comes to an average rate of 0.625 such deaths per million children per year. This might not seem significantly different from the rate of 0.48 emerging from the EPIC database - except that these fatalities are from playground falls alone! Indeed, the NEISS database contains only those, because it only examines deaths specifically associated to commercial products, and there are not many products that can cause death by falling. Chadwick actually points this out, noting specifically that the database "captures almost all product-related injuries" but "is not adapted for violence-related data acquisition" and "may fail to capture deaths resulting from short falls that are not involved with products". However, instead of noting that what these limitations imply is that the true risk of death from short fall is certainly greater than 0.625 per million children per year, he takes them as a reason to discard the NEISS data altogether as unreliable.

As for the Hall study, it examined the records of the Medical Examiner of Cook County, Illinois over a four-year period, and identified no less than 18 deaths. In his 1991 article, Chadwick dismissed this study with the brief sentence "Their work omits the detailed information necessary to exclude inflicted injury, and many of those cases might be in that category." In the 2008 article, he and his co-authors are even briefer; the article is mentioned only in a table containing seven studies specifically of short fall risk, and accompanied by the comment "Fall histories not validated". A commentary located underneath the table states "All seven of these studies indicated a very low frequency of short-fall death; however, none involved large populations, and their conclusions were not quantitative. All studies were based on clinical populations and contained cases with incorrect histories."

It so happens that populationwise, Cook County is the second largest county in the United States, with about 300,000 children under 5. San Diego County is ranked fifth, so Chadwick is hardly fair in stating that the Hall study "did not involve a large population". Similarly, it is a bit absurd to complain that "the conclusions are not quantitative" in Hall's study, since the risk of death from short falls per million children per year can be calculated from Hall's data simply noting that 18 deaths per 300,000 children in four years corresponds to 60 deaths per million children in four years, or 15 deaths per million children per year, a figure that is *over thirty times higher* than Chadwick's rate of 0.48 per million children per year. Stating that the conclusion of the Hall study is "not quantitative" but adding that it nevertheless "indicated a low frequency of short-fall deaths" is, to say the least, ingenuous.

This impression is reinforced by an exchange of letters published in the *Journal of Trauma* shortly after the appearance of Hall's study[6]. At least three other s (not Chadwick) addressed letters to the editor raising the same doubts about Hall's work as Chadwick: essentially, that his results contradict previous studies, and that in order to exclude inflicted injury, it was insufficient to use merely the medical examiner's records together with a police investigation, but not the hospital records. The three earlier studies cited as contradicting Hall's work were respectively: one of 246 children under 5 who fell out of bed, another of 363 children under 5 who fell down stairs, and one of 85 children who fell off beds, gurneys or changing tables in hospitals. None of the children in these studies sustained any serious injury at all.

There is a fallacy here once again; **it is simply not true that Hall's results contradict those of the three smaller studies**. Applying his fatality rate of 15 deaths per million children per year to a group of 246 children[7], the expectation of seeing a fatal case is just 0.000246, which means there is a chance of just 0.0246 % - less than 3/100ths of one percent! - of actually seeing a fatality in a group of 246 children using Hall's risk rate. (The figure becomes 0.0363 % and 0.085% in the groups of 363 and 85 children.) Hall's fatality rate is perfectly coherent with the lack of fatal cases in the small studies.

As for his use of the medical examiner's records and police investigation, Hall responded to the letters to the editor with a letter of his own, in which he explained that each of the patients in his study "had a complete report prepared by an investigator from the medical examiner's office. This gave a detailed description of the cause of the accident and a summary of the [ambulance, pre-hospital and hospital] records. These reports were thus of superior quality to hospital records. 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-mortem plus full body X-rays […] We cannot conceive of a more independent nor complete investigation."

Given the fact that this published exchange of letters took place in 1990, just before the publication of Chadwick's 1991 article in which he claims that the work of Hall et al "omits the detailed information necessary to exclude inflicted injury, and many of those cases might be in that category", and his similar (but more summary) dismissal of the Hall study in 2008

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[6] M. Joffe, P. Diamond, Letter to the Editor, and R. Helfer, Letter to the Editor, and J. Hall, Letter to the Editor, *J. Trauma* 30:11, (1990), 1421-1423.

[7] One could object here that the children in these studies did not form a general population, since they were all victims of falls. However, there is no real need to differentiate between the population of children who have sustained short falls and the general population, since just about every child has sustained short falls. If we were considering long falls, this would be a legitimate point, and we would need to distinguish carefully between the rate of deaths among children who sustain long falls, which is high (1/118 according to Chadwick's data) and the rate of deaths from long falls among the general population of children (1/2.5 million children/5 years, or 0.08/million children/year, again according to Chadwick's data). Given that Chadwick gives 0.48/million/year as the risk of death from short falls, this shows that a random child runs a risk of dying from a short fall that is six times greater than their risk of dying from a long fall. This may seem like another of those surprising and paradoxical conclusions, but it is perfectly true, quite simply because short falls are so frequent and long falls so rare.

merely with the words "Fall histories not verified", **Chadwick's choice to dismiss this study and his description of his reasons for doing so do not appear justifiable**.

Certainly the risk rate of 15 deaths per million children per year in Hall seems very high. Hall admits that in spite of the best efforts of the police and the medical examiner, "it may be impossible to rule out all cases of abuse". At the same time, it is also the case that the risk can be different in demographically different areas; it is greater in urban areas with many buildings, in areas of greater poverty where children grow up with less supervision, and in places where there are natural dangers associated with geographic elements. Cook County is very different from San Diego; it has a significantly lower median income, another type of urban landscape, and also vastly different weather. (It is worth noting that at least two of the victims in Hall's study fell out of the arms of parents who slipped on ice, an unlikely circumstance in San Diego County.) The importance of area-based factors is borne out by a systematic review of studies and databases carried out in 2006 by Khambalia et al[8], covering 50 years' worth of data from around the world. They identify the above considerations as specific risk factors for fall injuries and give an overall figure for the risk of death from falls of any height in children aged 0-5 as 3 per million children per year.

4. Fallacy in the final conclusion

Even if we consider Chadwick's risk figure of 0.48 as acceptable, it seems to be used by the authors to reach an erroneous conclusion. Indeed, it is quite clear from their article that ***Chadwick et al interpret the figure of 0.48 deaths per million children per year as being so low and indicating an event so extremely rare that faced with an actual case of fatality due to an alleged short fall, the attending physician should consider the story as false, and assume that they are seeing a case of inflicted injury.*** In particular, they consider that the tiny death rate of 0.48 per million children per year justifies their earlier conclusion that the seven fatalities observed in the 1991 study are all cases with falsified histories.

However, that reasoning is wrong. It is a simple matter to use the risk rate of 0.48 in a probability calculation to see how many legitimate deaths of small children from short falls one would expect to see in San Diago County over a 3.5-year period. As mentioned above, the population of children under 5 in San Diego County in the 1990s was about 200,000, observed over 3.5 years. Using these figures and the binomial formula, we find that there is about a 29% chance of seeing at least one legitimate short fall fatality in the period and population under study - a probability that is definitely not so small as to be negligible! And if instead of the 0.48 per million per year figure we use Khambalia's estimate of 3 per million per year, the probability of seeing no true short fall fatalities falls to only 12%, the probability of seeing exactly one fatality is about 25%. What this means is that with an expected value of 3 fatalities per million children per year, the likelihood of seeing at least

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[8] A. Khambalia, P. Joshi, M. Brussoni, P. Raina, B. Morrongiello, C. Macarthur, Risk factors for unintentional injuries due to falls in children aged 0-6 years: a systematic review, *Inj. Prev.* 12(6) 2006, 378-381.

two fatalities when observing a population of 200,000 children over 3.5 years (or equivalently, observing 700,000 children) is nearly 62%[9]!

Given the fact that Chadwick's table listing the associated injuries among the seven fatalities observed in the 1991 study reveals exactly two cases with no associated injuries at all, this shows that it is absolutely unjustified to decree automatically, on statistical grounds and risk factors alone, that these two histories were falsified and that the parents are guilty of abuse. ***The error consists in assuming that it is incredibly unlikely to see an event that only occurs with a frequency of 0.48 per million children, forgetting that if one is actually observing a population containing a million children, it is actually quite likely that one will see one or two occurrences of the "rare" event.***

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It is obvious from Chadwick's tone, and that of his colleagues, that since the 1990s they have been engaged in an admirable battle to make sure that cases of child abuse are detected and recognized, and that children are saved from inflicted injuries and swiftly removed from dangerous situations. Hall, however, writes equally passionately about the danger involved in persuading people that short falls are benign and cannot be fatal. Indeed, several of the 18 children who died in Cook County did so because their caregivers delayed seeking treatment, due to a general impression that short falls cannot be very harmful: "Delays in presentation were caused by both parents and emergency room personnel who felt that minor falls were benign and waited until late symptoms developed before becoming concerned." Hall asserts that this represents a major problem: "The myth […] that all minor falls are benign must be expunged; some can be serious. Abuse does need to be reuled out, but *falls regardless of height are potentially fatal*."

Both sides are trying to protect children. There should be no controversy between them. If there is one, it is due to misunderstandings about what statistics really tell us, and errors in using them properly. In a world where statistics were well-gathered, well-understood and well-interpreted, this controversy would not need to exist at all.

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[9] Letting the short fall fatality rate p=0.48/1000000 and q=1-p, and considering the population of 200,000 over 3.5 years as equivalent to a population of 700,000 over 1 year, the probability that no fatalities will occur is given by q^700000=0.714 or about 71%, so the probability that one or more will occur is about 29%. Using Khambalia's figure p=3/1000000 and again setting q=1-p, we now find that the probability of seeing no fatalities is q^700000=0.122 or around 12% The probability of seeing exactly one fatality is 700000*q^699999*p=0.257, or about 25%, and the probability of seeing two or more is around 62%.