The evidence base for shaken baby syndrome

We need to question the diagnostic criteria

The phrase “shaken baby syndrome” evokes a powerful image of abuse, in which a carer shakes a child sufficiently hard to produce whiplash forces that result in subdural and retinal bleeding. The theory of shaken baby syndrome rests on core assumptions: shaking is always intentional and violent; the injury an infant receives from shaking is invariably severe; and subdural and retinal bleeding is the result of criminal abuse, unless proved otherwise. These beliefs are reinforced by an interpretation of the literature by medical experts, which may on occasion be instrumental in a carer being convicted or children being removed from their parents. But what is the evidence for the theory of shaken baby syndrome?

Retinal haemorrhage is one of the criteria used, and many doctors consider retinal haemorrhage with specific characteristics pathognomonic of shaking. However, in this issue Patrick Lantz et al examine that premise (p 754) and conclude that it “cannot be supported by objective scientific evidence.” Their study comes hard on the heels of a recently published review of the literature on shaken baby syndrome from 1966 to 1998, in which Mark Donohoe found the scientific evidence to support a diagnosis of shaken baby syndrome to be much less reliable than generally thought.

Shaken baby syndrome is usually diagnosed on the basis of subdural and retinal haemorrhages in an infant or young child, although the diagnostic criteria are not uniform, and it is not unusual for the diagnosis to be based on subdural or retinal haemorrhages alone. The website of the American Academy of Ophthalmology states that if the retinal haemorrhages have specific characteristics “shaking injury can be diagnosed with confidence regardless of other circumstances.” Having reviewed the evidence base for the belief that perimacular folds with retinal haemorrhages are diagnostic of shaking, Lantz et al were able to find only two flawed case-control studies, much of the published work displaying “an absence of … precise and reproducible case definition, and interpretations or conclusions that overstep the data.” Their conclusions are remarkably similar to those of Donohoe, who found that “the evidence for shaken baby syndrome appears analogous to an inverted pyramid, with a very small database (most of it poor quality original research, retrospective in nature, and without appropriate control groups) spreading to a broad body of somewhat divergent opinions.”

His work entailed searching the literature, using the term “shaken baby syndrome” and then assessing the methods of the articles retrieved, using the tools of evidence based inquiry. Reviewing the studies achieving the highest quality of evidence rating scores, Donohoe found that “there was inadequate scientific evidence to come to a firm conclusion on most aspects of causation, diagnosis, treatment, or any other matters,” and identified “serious data gaps, flaws of logic, inconsistency of case definition.”

The conclusions of Lantz et al and of Donohoe make disturbing reading, because they reveal major shortcomings in the literature relating to a field in which the opportunity for scientific experimentation and controlled trials does not exist, but in which much may rest on interpretation of the medical evidence.

If the concept of shaken baby syndrome is scientifically uncertain, we have a duty to re-examine the validity of other beliefs in the field of infant injury. The recent literature contains a number of publications that disprove traditional expert opinion in the field. A study of independently witnessed low level falls showed that such falls may prove fatal, causing both subdural and retinal bleeding. A biomechanical analysis validates that serious injury or death from a low level fall is possible and casts doubt on the idea that shaking can directly cause retinal or subdural haemorrhages. An important lucid interval may be present in an ultimately fatal head injury in an infant. Neuropathological studies have shown that abused infants do not generally have severe traumatic brain injury and that the structural damage associated with death may be morphologically mild. What is the relevance of the cranioocular injuries to corticospinal tracts, dorsal nerve roots, and so on that have been described? We do not know. What is the force necessary to injure an infant’s brain? Again, we do not know. While most abused children indisputably show the signs of violence, not all do. No one would be surprised to learn that a fall from a two storey building or involvement in a high speed road traffic crash can cause retinal and subdural bleeding, but what is the minimum force required? It is one thing clearly to state that a certain quantum of force is necessary to produce a subdural hematoma; it is quite another to use examples of obviously extreme force … and then suggest that they constitute the minimum force necessary.

Research in the area of injury to infants is difficult. Quality evidence may need to be based on finite element
modelling from data on infants' skulls, brains, and neck structures, rather than living animals. Any studies on immature animal models, if performed, will need to be validated against the known mechanical properties of the human infant. Pending completion of such studies, the reviews by Lantz and Donohoe are a valuable contribution and provide a salutary check for anyone wishing to cite the literature in support of an opinion. Their criticisms of lack of case definition or proper controls can be levelled at the whole literature on child abuse. If the issues are much less certain than we have been taught to believe, then to admit uncertainty sometimes would be appropriate for experts. Doing so may make prosecution more difficult, but a natural desire to protect children should not lead anyone to prolifer opinions unsupported by good quality science. We need to reconsider the diagnostic criteria, if not the existence, of shaken baby syndrome.

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Shaken baby syndrome
Pathological diagnosis rests on the combined triad, not on individual injuries

Shaken baby syndrome is a form of physical non-accidental injury to infants, characterised by acute encephalopathy with subdural and retinal haemorrhages, occurring in a context of inappropriate or inconsistent history and commonly accompanied by other apparently inflicted injuries. Injuries to the neck and spinal cord may also be present. Controversy surrounds the precise causation of the brain injury, the retinal and subdural haemorrhages, as well as the degree of force required and whether impact in addition to whiplash forces is needed. Although most discussion has concerned fatal injuries of this nature, not all are lethal, but they may be associated with subsequent neurological disability of varying severity.

Expert medical evidence about inflicted injury must have scientific validity, but applying the evidence based criteria appropriate to clinical practice entails some difficulties. In clinical practice medical management of defined clinical problems can be compared and best practice distinguished by clinical outcomes. Conversely, in inflicted paediatric injuries, one is presented with the outcome, investigation follows rather than precedes that outcome, and the history may be incomplete or deliberately misleading. A need exists for an impartial and intelligent assessment, but how may this be achieved in practice?

Because of the serious implications of diagnosing inflicted injury such as shaken baby syndrome, every case must be evaluated in detail, taking account of all the circumstances surrounding the injury and considering the pathological features in full, rather than attempting to evaluate the significance of each component.

Competing interests: JFG and JP have given evidence in criminal cases at the request of both the prosecution and the defence.


BMJ 2004;328:729-1
shaking must vary and nobody really knows how babies are injured, it may not be necessary to shake an infant very violently to produce stretch injury to its neuroaxis, a conclusion that is not supported by data in the paper and that has lead to considerable controversy among expert witnesses in court. It ignores the evidence for the force required to produce the triad of injuries, in fatal instances of shaken baby syndrome, obtained from evaluating the other components. Clearly, if “gentle” shaking were capable of causing fatal injury, such events would be an everyday occurrence. There is abundant evidence that minor head trauma, so common in the domestic context, is only very rarely associated with severe intracranial injury. Further confusion has been sown by a more recent contribution by Geddes et al. This describes the neuropathological findings in the brains of infants dying of non-traumatic cerebral hypoxia. Random examination of sections of dura showed intradural haemorrhage evident only at the microscopic level. On this basis they thought that all the components normally indicative of shaken baby syndrome might result from hypoxic damage alone, dural and retinal haemorrhage being due to brain swelling consequent on cerebral hypoxia. However, subdural haemorrhage in shaken baby syndrome is a macroscopic, not a microscopic, finding, and the comment on retinal haemorrhage has even less foundation in that no examination of the eyes was made.

As shown by Lantz et al in this issue, even when a particular detail has been claimed to be pathognomonic of shaken baby syndrome, the diagnosis should not rest on this feature alone (p 754). This careful case study reinforces the need for meticulous identification of the complexity of the injury and evaluating the findings against the validity of the explanation offered. It is also true that retinal haemorrhages can have causes other than shaking and that space occupying subdural haemorrhages causing death can occur in witnessed accidental injuries in children. However, of the patients Plunkett described, the youngest was 12 months old, which is outside the age group in which most cases of shaken baby syndrome occur.

The pathological diagnosis of shaken baby syndrome requires careful evaluation of the character and extent of all components of the injury and should not rest merely on the presence or absence of one or more of the constituent lesions. The basic triad should have all the necessary features for confident diagnosis and the conclusion that undue force has been applied. Damage to the neck or spinal cord is further useful in confirming the presence of gripping injuries, while often absent, can provide further weight. Other inflicted extracranial injuries provide evidence of abuse even if they are not contemporaneous with the head injury.

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Risk assessment for spinal injury after trauma

The guidelines are simple and evidence based

About 600-700 people sustain acute traumatic injuries to the spinal cord in the United Kingdom each year. Previously published data indicate that the injury to the spinal cord remains unrecognised in 4-9% of individuals. Inadequate management of patients with injury to the spinal cord has the potential to lead to neurological deterioration, additional functional handicaps, and possibly medical litigation. Thousands of patients, however, routinely present to primary care centres every day with injuries to the neck and back. The immediate care and appropriate assessment of patients with spinal injury is a skill that is expected of all doctors. General practitioners and hospital doctors with little or no training and experience of caring for patients with trauma might have to help the victims of a recent accident. They will certainly have to advise patients who complain of spinal pain after injury. This article is written to guide clinicians in these situations.

The evidence base for this subject has improved recently with some large scale studies from North America. Several consensus guidelines have been published by the National Institute for Clinical Excellence and the British Trauma Society.


Letters

The evidence base for shaken baby syndrome

Response to editorial from 106 doctors

Editor—In challenging the diagnosis of shaken baby syndrome in their recent editorial Geddes and Plunkett make a number of serious errors in interpreting the research on this issue, and they display a worrisome and persistent bias against the diagnosis of child abuse in general.

In their opening sentence Geddes and Plunkett describe shaking a child to “produce whiplash forces that result in subdural and retinal bleeding,” omitting the most important element in this condition: brain injury itself. They elaborate that the “theory” of shaken baby syndrome rests on some core assumptions, including that “the injury an infant receives from shaking is invariably severe.” This is in conflict with the research of Alexander et al, Ewing-Cobbs et al, Kemp et al, and Jenny et al, who found that 30%–40% of newly diagnosed shaken baby cases had medical evidence of previously undiagnosed head injury.6 These infants had such mild or non-specific symptoms and signs that their trauma was previously not diagnosed. The diagnosis was ultimately made when the children had subsequent severe episodes of abuse, with computer tomographic evidence of both acute and older subdural haematoma and brain injuries.

Retinal haemorrhages

Geddes and Plunkett then consider retinal haemorrhages. Lantz et al, in the same issue, question the specificity of perimacular folds in abusive head trauma in infancy.7 They conclude from a literature review that there was no support for the contention that perimacular folds are pathognomonic for abusive head injury. Geddes and Plunkett applied these authors’ conclusions not only to perimacular folds but also to retinal haemorrhages.

Although research on the subject of inflicted childhood neurotrauma—over 600 peer reviewed articles—does not claim that retinal haemorrhages are pathognomonic for abuse, it does show that retinal haemorrhages are, overwhelmingly, more common in abuse than in non-inflicted injury. When massive retinal haemorrhages are seen in carefully studied children with non-inflicted major injuries, such as from motor vehicle crashes, crushing head injuries, as in Lantz et al’s report, and falls from various storeys, child abuse is not a consideration.

One study analysed these obviously non-inflicted injuries and compared them with abusive head injuries in children under 6 years of age. Severe retinal haemorrhages were seen in 5 of the 235 (2%) children in the non-inflicted group and in 18 of the 54 (33%) in the abuse group.7 Retinal pathology from major trauma mimicking shaken baby syndrome is old news.7–11 Its incidence is dramatically lower than that resulting from inflicted head injury and because of the obvious major trauma history it does not present a diagnostic dilemma.

Literature on shaken baby syndrome

To discredit the literature on shaken baby syndrome over the past 30 years, Geddes and Plunkett rely on an article by Donohoe.12 In so doing they have erred in their assessment of the status of the science in the field.

Donohoe’s purpose was to examine trends in the quality of scientific evidence. Donohoe used evidence based medicine (EBM) criteria for weighting evidence to judge the comparative merit of published studies published before such criteria were widely embraced by authors, reviewers, and journals. He also plans to apply this process to more recently written articles. He explicitly did not challenge the existence of shaken baby syndrome and, to our knowledge, his review of more recent work has not yet been published. The cited paper reviewed studies published up to six years ago and purposely did not include research that has been published since that time.

One striking limitation of the Donohoe paper is that he used only the keywords “shaken baby syndrome” to search the literature whereas many of the articles on the subject use keywords such as “inflicted childhood neurotrauma,” “childhood head injury,” “cerebrocerebral trauma,” “inflicted traumatic brain injury,” as well as several others. We know of a number of qualified studies that were not included. If the search had been appropriately more inclusive, the resulting conclusions would likely have been quite different.

The application of EBM criteria to judge articles is intended to help physicians discern truth among competing works. The absence of clinical trials and definitive population based studies means lower EBM scores when the work is compared with more definitive work. Low EBM scores, in the absence of more highly regarded work, do not mean that the work is wrong, only that there is room for further research to learn more and that prior conclusions may not be definitive. Many aspects of clinical practice and medical knowledge have not been established with certainty by EBM criteria.

The comparative paucity of well-done population based cohort studies, in the face of a rather large literature of case reports, case series, cohort studies, and case-control studies underscores how hard research in this area is to complete. It also emphasises the need for more research and more government research assistance. Child abuse is a particularly difficult area in which to conduct research. Issues of informed consent, inadequacy of animal models, and the potential legal consequences of participation and telling the truth make this a complicated field.

Short falls in childhood

Geddes and Plunkett claim that “the recent literature contains a number of publications that disprove traditional expert opinion in the field” about short falls in childhood. However, they cite only two publications, and neither disproves the evidence presented in over 25 other studies of short falls in infancy and childhood.

Plunkett cites his own article on fatal falls from short distances in playgrounds, using archived data from various sources.13 His study has significant problems: the determination of the distances of the falls in the 75,000 cases presented relies on information supplied by the original sources of data and is thus open to question; no infants were studied; several of the falls were from 7 feet (that is, they were not “short” falls); several of the children had crush injuries or pre-existing conditions; and none of the children had “formal retinal evaluation.” Nevertheless, Plunkett and others assert that this study “proves” that short falls can kill and cause retinal haemorrhages.

Contact subdural and epidural haemorrhages may, however, result from short falls.14–15 They can occasionally cause severe
illness or death from space occupying lesions. Occasionally children with contact injuries due to short falls develop malignant cerebral oedema, and neck musculature? Although more dura, skull, bridging veins, cerebral cortex, cited is a review by Ommaya et al that from short falls was still exceedingly rare accept his conclusions despite these meth-

sis of the best literature on the subject. 

Biomechanical studies 

Geddes and Plunkett end by dismissing ani-

mal model studies unless they are “validated against the known mechanical properties of the human infant.” How are these properties to be known? How can an investor measure 

the tensile strength of the living infant dura, skull, bridging veins, cerebral cortex, and neck musculature? Although more appropriate studies of the mechanical properties of infant animal brain are beginning to be done,”2,10 no current studies reflect the response of infant animal brain tissue to harmonic forces, such as those likely occurring with infant shaking. Although more biologically faithful mechanical models of infants are being constructed,3,16 they will still only approach the response of living infants to shaking.

Assuming that shaking cannot cause infant brain injury, on the basis of current biomechanical studies is premature. Juxtaposed with these mechanical approximations, there is extensive clinical experience and an emerging literature of confessed shaking causing brain injury in infants.17

Conclusion 

Child abuse is an enormous social, medical, and mental health problem and its evaluation and treatment have far-reaching implications for children, families, and society. To provide optimal diagnosis and treatment, careful objective research and intellectual honesty are needed and must prevail over the entrenchment of ideological schools of thought and “winning” in court. Unfortunately, there remains considerable difficulty for some doctors to accept that children are abused. We must look at these cases using all of the information available, including collected clinical experience and the synthe-

sis of the best literature on the subject.18

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This letter is signed by another 105 doctors (see bmj.com for details).

Competing interests: None declared.


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Details of the other 105 signatories are available on bmj.com, as are details of references w1-w11.

Authors’ reply 

Editor—It is difficult to understand how Reece et al could interpret our editorial as displaying “a worrisome and persistent bias against the diagnosis of child abuse in general.” Child abuse exists, and we know and attest that it exists. The editorial does not discuss “child abuse in general.” 

Child abuse exists in many forms: our editorial addresses the diagnostic criteria for a specific type of abuse, the so-called shaken baby syndrome. We emphasise, as have Donohoe and Lantz et al,1 that the literature to support a diagnosis of shaken baby syndrome/inflicted head injury is based on imprecise and ill-defined criteria, biased selection, circular reasoning, inappropriate controls, and conclusions that over-

step the data. If it is the questioning of the criteria that is worrisome, we will continue to do so and to cause worry.

We encouraged the readers to evaluate critically the evidentiary basis for a diagnosis of shaken baby syndrome in the light of the questions raised by the two papers. Of course Donohoe’s study was limited and would retrieve only papers that included the words “shaken baby syndrome” in the title, key words, or abstract. The lack of scientific rigour that the identified is not restricted to infant head injury papers that specifically mention shaken baby syndrome. If Reece et al perform a critical review of the “number of qualified studies” that they assert would have been included by a wider search, they will encounter the same “data gaps, flaws of logic, and inconsistency of case definition” that were present in the literature studied by Donohoe. We would urge them to look again, for example, at the paper they cite by Alexander et al where they will find all the above shortcomings.

Finally, we are at a loss to explain or accept the authors’ statements in their penult-

imate sentence: “Unfortunately, there remains considerable difficulty for some doctors to accept that children are abused.”

If the authors are suggesting that we are among those doctors, or are encouraging others to be so, their argument is a willful misinterpretation of what we have written. When there is new evidence that challenges an established conviction, medicine has the responsibility to critically evaluate the data, and if verifiable, reflect that change. We must have no vested interest in yesterday’s belief. We are encouraging doctors to think clearly and critically, even in an area as emotive as child abuse. No more. And no less.

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Details of paper were incorrect 

Editor—Burkitt Wright et al have not attended one of my group’s communication skills courses; yet that doesn’t stop them from saying that patients valued forms of communication that are currently not emphasised in training and research, and did not intrinsically value others that are currently thought important, including provision of information and choice.1 Apart from the breathtakingly absurd suggestion that a qualitative analysis of views of 39 women with breast cancer should overturn painstaking research and survey findings gathered by many, their assertions are factu-

ally incorrect.

Firstly, we always ensure that patient needs inform the content of communication skills courses by involving patient groups and considering empirical research findings.

Secondly, patient centredness is a core component of our courses, which includes learning how to tailor information giving, providing choice if wanted, responding appropriately to patient led cues, and expressing empathy and respect.

Thirdly, each day CancerBACUP receives many calls from distressed patients and relatives made anxious and distraught by the lack of information they have received. We need trust, care, and respect, but no convincing evidence exists to show that those things in themselves are enough.

I am indignant that our work and that of others whom I respect receives such
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Meaning of signature must be made explicit

Editor—Reece et al's response to the editorial of Geddes and Plunkett claims to be a response of "106 doctors." What, precisely, do the 106 signatures attached to this letter signify? That all had reviewed the letter and were in full agreement with the entire content? That they agreed in general with the thrust of the letter? Or was this more a show of solidarity on the part of doctors who care deeply about the risks of shaking on children?

This needs clarification if the signatures are to carry any weight whatsoever. Science—evidence, even medical evidence—is not a popularity contest. The meaning of a signature must be made explicit for it to add weight to a document.

Each signature carries with it responsibilities of authorship. Reece's letter declared no competing interests, but all signatories would need to comply for this to be true.

Six of the signatories (Levin, Chadwick, Alexander, Barr, Jenny, and Reece) are medical practitioners on the International Advisory Board of the National Center on Shaken Baby Syndrome (www.dontshake.com). They participate in this group's conferences and are presumably compensated or reimbursed for this work: information requiring disclosure under BMJ guidelines.

The letter of Reece exemplifies a problem identified in my own paper—that the literature on shaken baby syndrome is polarised and based more on strong beliefs and opinions than strong data. Ten thousand signatures cannot change this.

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Competing interests: None declared.

Response to Reece et al from 41 physicians and scientists

Editor—Reece et al have implied that child abuse is a particularly difficult area in which to conduct research. This difficulty does not justify circular reasoning, selection bias, imprecise case definition, unscientific review publications, or conclusions that overstep the data.**

Geddes and Plunkett described the use of evidence-based medicine in evaluating the causes of head injury in infants and children.*** Evidence-based medicine is the conscientious, explicit, and judicious use of scientific evidence in making medical decisions and cautions against unscientific, untested reasoning and intuition based clinical applications. It integrates scientific principles and clinical experience with valid, current research.**

While much of clinical medicine still relies on observation, it is critical that these observations are verified and validated. Often, the clinician must be more deliberate about it than the experimentalist who uses a planned systematic approach. The clinical researcher may have to await the natural sequence of events—deducing relationships that lie below observed phenomenon, being more logical and less dogmatic, and avoiding the fallacy of mistaking correlation with causation.*** If the principles of science and evidence-based medicine are not critically applied to observational studies, a set of formulated beliefs among like-minded people may be reinforced, leading to misconceptions and misinterpretations. When this occurs, the primary principle of medicine—first, do no harm—may be violated.

Child abuse in any form is always unacceptable. However, if errors in diagnosis, false accusations, and wrongful convictions result from untested and unverified beliefs, then we have done harm.

Critically evaluating one's own understanding is far more constructive than...
Post-immigrant refugee medicine

Population mobility must be considered

Editor—Adams et al highlight the importance of pre-departure and migration history in post-immigration refugee medicine,1 but health professionals must also consider the ongoing reality of mobility in this population.

For example, a newcomer, in whom diabetes has been diagnosed during screening, happens to mention the recent death of her mother. This leads to the discovery of plans to travel back home to the Sudan and a possible provision of health advice, malaria prophylaxis, and a summary of drug treatment.

Population mobility in the context of refugees refers to the forced movement of people beginning before departure and continuing for years, sometimes a lifetime, as people search for a place to call home. Historically, refugee programmes have been designed before departure and consideration of health is lacking.2

Despite increasing focus on the mental health of refugee children, research data are lacking.3 Some researchers have found an increased risk of post-traumatic stress disorder, depression, and anxiety.4 Others found no differences between the incidence of psychiatric disorders in refugee children and the local population.5 Whether this reflects better assimilation of these children into the local population remains to be explored.

Children are worried not just about health but about loss of family members, loneliness, feeling cold, being depressed, lack of money, being bullied, language barriers, and being used as interpreters for their parents.1 They may not seek care for legal reasons or fear of persecution.1

Doctors need training in interviewing skills that explore these unique issues and awareness of locally available resources to act as advocates on their behalf. Collaboration between doctors and mental health, social, and education services is required. Children’s needs should not be seen in isolation but in the context of their families. The best way to help them is to help their families. A timely understanding of these needs will be critical in safeguarding our future.

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Competing interests: None declared.

Children’s needs should not be seen in isolation

Editor—Adams et al discussed the challenges of post-immigration refugee medicine.1 The physical and mental health needs of refugee children are unique. Children comprise nearly half of the refugee population in many countries and may arrive malnourished without any screening or immunisation. They need culturally sensitive dietary advice and information about sexual habits and avoiding drugs.2

Despite increasing focus on the mental health of refugee children, research data are lacking.3 Some researchers have found an increased risk of post-traumatic stress disorder, depression, and anxiety.4 Others found no differences between the incidence of psychiatric disorders in refugee children and the local population.5 Whether this reflects better assimilation of these children into the society or unknown variables remains to be explored.

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Millennium development goals: whose goals and for whom?

Editor—Millennium development goals are the most recent statement of commitment towards narrowing gaps between the developed and developing regions of the world.3 But how realistic are these goals?

Although goals help in making assessments of progress, they should not be blind to existing potentials for progress, which is conditioned by the existing status as well as the motivation of nations and states towards realising them. Unfortunately, millennium development goals are considered to be a tool for assessing accountability and highlighting a need for urgency that could violate the autonomy of nations and states. This raises the question of whose goals they are.

Often such initiatives are seen as global priorities, overriding local concerns. The best example is the vaccination initiative, which has consistently reflected failures by countries without the required infrastructure.5 In other circumstances, such externally aided initiatives are never integrated into the local health system to make the most of such intervention. In terms of measuring the extent of achievement of such goals, caution is advised in assessing progress conditioned by local realities that may not always be conducive to making the expected progress.

Finally, who benefits from the achievement of such goals needs to be made clear. Would there be any space to account for vulnerabilities resulting in achievement of such goals? If yes, the assessment of progress in achieving these goals needs to make adjustments for this to have a realistic evaluation of progress.

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Competing interests: None declared.

1 Haines A, Cassels A, Cao. The millennium development goals be attained? BMJ 2014;349:3947-8. (11 August)

Women in medicine

Doctors of both sexes are seeking balance between life and work

Editor—The Medical Women’s Federation supports Heath’s statement in her editorial that all occupations should seek to mirror the demography of society.6 Child care support at levels found in Scandinavian countries would greatly support women in medicine to achieve their potential. How-
The 41 signatories to the letter and competing interest statements

Competing interests: We are physicians and scientists who have a common interest in applying the principles of science and medicine to infant injury evaluation. Each of us either participated in writing this letter or reviewed it prior to submission. Each of us agrees with its content.

FAB, PDB, TC, BJC, DLC, VJMD, LJD, JEL, PEL, DM-P, MDN, AKO, JO, GR, SJR, MJS, PS, WS, JBS, ST, HW, and PW have consulted or testified for the prosecution and the defence as part of their official salaried responsibilities, as paid consultants or pro bono in fatal and non-fatal alleged child abuse cases.

RS has given evidence to the Courts (not for either prosecution or defence) and has been reimbursed by the public Legal Aid system.

JHD, GS, and LCT have testified for the prosecution in fatal alleged child abuse cases as part of their official salaried responsibilities.

JG, HG, RJ, REM, MM, KT, JBL, RR, JS, RU, CVE, and EW have consulted and testified for the defence pro bono and as paid consultants in fatal and nonfatal alleged child abuse cases.

TLB, KM and JN have no competing interests other than as stated in the first sentence above.

None of us answers, "Yes" to Questions 1, 2, 3 or 5 in the BMJ "Declaration of competing interest".
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Extra references (w1-7)