Competing interests: none declared

Bruising in preschool children with special needs

The authors are to be congratulated for the systematic review “Are there patterns of bruising in childhood which are diagnostic or suggestive of abuse?” It is an extremely valuable piece of work, providing data for paediatricians working in the field of child protection.

Results show that non-abusive bruises are generally small, sustained over bony prominences, and found on the front of the body. Abusive bruises are away from bony prominences, and the commonest site is the head and neck (particularly face), followed by buttocks, trunk, and arms.

The paper highlights that children with significant motor delay would not be expected to have the same bruising pattern as their peers, but points out that most studies in the literature exclude children with neurodisability or diseases that predispose to bruising, to a varying degree.

Locally a small pilot study examining the bruising patterns in 14 children with a variety of disabilities aged 15 months to 4 years who attend two preschool special needs opportunity groups has shown interesting results. Thirteen of the 14 children (where there were no concerns about abuse) had bruises, one child with no bruises being the only child in the study who was unable to crawl. The other children were all cruising or walking, although one wore piedro boots, another had splints, and some had an unsteady gait. The average number of bruises was 5.7 (range 0–20) and size varied between 0.5 cm and 2.0 cm in diameter. Five children (35.7%) had bruising on their buttocks/bottom, four (28.6%) had bruising on their arms, and two (14.3%) had bruises on their face or neck—all areas usually associated with abusive bruising.

Bruising patterns in children with disabilities or special needs may be very different to their age matched peer group. This reinforces the need for a large case-control study to confirm and delineate this hypothesis. Such a study should obey strict inclusion criteria and exclude any children where child abuse is suspected. It should take into consideration the proposed mechanisms of causality of the bruising and the characteristics of the participants’ disability and developmental parameters.

The observations emphasise that in clinical practice we should evaluate bruising pattern according to the developmental level of the child rather than their age.

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The dental health of children with refractory epilepsy in a residential school

We were interested to read the recent review on “Dental disease in children with chronic illness” having recently undertaken a review of the dental health of the children at The David Lewis Centre School. This is a residential school for children with severe refractory epilepsy and associated neurodisability. There are 63 children aged 7–19 years. Over 50% of the children require two or more antiepileptic drugs and other long term medications.

The children are at risk of poor dental hygiene because:

- Regular long-term medications may contain sugar
- Effective dental hygiene can be difficult to achieve for children with these impairments.

The David Lewis Centre has an on-site pharmacy; wherever possible tablets or capsules are dispensed. If liquids are necessary these are sugar free. A dental team experienced in working with children with severe learning difficulties reviews the dental health of all children every four months and offers advice to care staff about the best ways to maintain the children’s dental hygiene.

The David Lewis Centre led omits mention of key elements and, most importantly, misconstrues the trials’ results and consequently their clinical implications.

The main single shortcoming of the meta-analysis is its failure, by considering only pooled risk differences across studies, to relate the outcomes of individual randomised trials to the stringency of the indications used in determining trial eligibility. Although the authors note that “the inclusion criteria of the trials varied from mild and non-specific to severe and very strict”, they pursue this issue no further and appear not to recognise its importance in determining outcomes. Thus, for example, the results of tonsillectomy on children meeting any stringency criteria of our first trial were more distinctly favourable than the results of either tonsillectomy or adenotonsillectomy in children who met the somewhat less stringent criteria of our second trial, even though the latter criteria were considerably more stringent than those used in the studies conducted before 1970.

Another important shortcoming of the meta-analysis is its failure to consider, in addition to the mean numbers of episodes overall occurring after randomisation, the nature and severity of the episodes that occurred and how these varied among the trial participants. Thus, in our first randomised trial in the first year after randomisation, there was a 2.5-fold reduction among surgical subjects in the mean number of throat infection episodes overall (47 episodes in 38 surgical subjects versus 108 episodes in 35 control subjects), but a 4-fold reduction in the proportion of surgical subjects who had three or more episodes of any type (13% [5 of 38] versus 54% [19 of 35]) and a 14-fold reduction in the mean number of episodes rated clinically as moderate or

Reference


Effectiveness of tonsillectomy depends on stringency of indications

The meta-analysis of adenotonsillectomy trial results reported by van Staaij and colleagues, from which they concluded that the operation confers “an additional, but small, reduction of sore throat episodes...compared to watchful waiting”, fails short on numerous counts and misleads the unwary reader.

Their analysis of the trials conducted between the 1920s and 1960s not only overlooks previously published critiques of those trials, but fails to incorporate the sense of those critiques’ most telling criticisms, namely: the exclusion from most of the trials, on ethical grounds, of children thought to be severely affected; the inclusion of children who were only mildly affected and in whom, therefore, the benefits of surgery could at best be modest; and the limitations of some of the follow-up procedures, particularly, ascertainment of the numbers and types of succeeding episodes of throat infection. Relatedly, their analysis of trials conducted since the 1960s (each of which I discussed in my critique) omits mention of key elements and, most importantly, misconstrues the trials’ results and consequently their clinical implications.

The main single shortcoming of the meta-analysis is its failure, by considering only pooled risk differences across studies, to relate the outcomes of individual randomised trials to the stringency of the indications used in determining trial eligibility. Although the authors note that “the inclusion criteria of the trials varied from mild and non-specific to severe and very strict”, they pursue this issue no further and appear not to recognise its importance in determining outcomes. Thus, for example, the results of tonsillectomy on children meeting any stringency criteria of our first trial were more distinctly favourable than the results of either tonsillectomy or adenotonsillectomy in children who met the somewhat less stringent criteria of our second trial, even though the latter criteria were considerably more stringent than those used in the studies conducted before 1970. Another important shortcoming of the meta-analysis is its failure to consider, in addition to the mean numbers of episodes overall occurring after randomisation, the nature and severity of the episodes that occurred and how these varied among the trial participants. Thus, in our first randomised trial in the first year after randomisation, there was a 2.5-fold reduction among surgical subjects in the mean number of throat infection episodes overall (47 episodes in 38 surgical subjects versus 108 episodes in 35 control subjects), but a 4-fold reduction in the proportion of surgical subjects who had three or more episodes of any type (13% [5 of 38] versus 54% [19 of 35]) and a 14-fold reduction in the mean number of episodes rated clinically as moderate or

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We considered only pooled risk differences.

Authors' reply

First, controlled non-randomised studies are indeed less valid than randomised controlled trials. We therefore presented the results of the controlled non-randomised studies as circumstantial evidence in a separate table (table 3 of the article). Despite their shortcomings, the results of the more recent and methodologically sounder controlled non-randomised studies appeared to be surprisingly similar to those of the randomised trials.

Second, pooling of the results of the randomised trials was certainly justified in view of the lack of relevant heterogeneity among the studies. To take heterogeneity due to differences in design into account, we used a random effect model. Unfortunately, it was not possible to identify subgroups of children that might benefit (more) from the operation since the randomised trials published so far did not provide detailed results according to clinically relevant subgroups. We do, however, agree with Dr Paradise that there are subgroups of children that do benefit from (adeno)tonsillectomy.

Third, we indeed did not assess the influence of adenotonsillectomy on the severity of throat infections among trial participants. The main reason for this is the notion that any effect on the severity (and not the number) of the infections is likely to be attributable to information bias, caused by definition by the non-blinded nature of the studies. Parents of children in the watchful waiting group may be more likely to report a sore throat or upper respiratory infection as “severe” than parents of children in the surgical group, which would result in an overestimation of the effect of (adeno)tonsillectomy.

Finally, we agree that very frequent throat infections (seven or more in past year) is an adequate indication for tonsillectomy in children. That is why we excluded such children from our trial, which was published in 2004. Consistent with the results of the most recent trial by the group of Dr Paradise in “less severely affected children”, we found that in children with mild to moderate symptoms of throat infections, the likelihood of substantial benefit from tonsillectomy is indeed small. We therefore concluded that in these children the operation had no relevant clinical benefits to offer over a watchful waiting policy.

B van Staaij, M Rovers, A Hoes, A Schilder

References


A strategy to minimise the impact of maternal HIV

Stein and colleagues suggest that the “question that most urgently needs to be addressed is: what can be done to help infected women and their young children, and in particular, what intervention strategies are necessary to minimise the impact of maternal HIV?” One answer is wet-nursing.

Almost half of all children who acquire HIV do so while breast feeding.6 We suggest that donor funds be used to hire HIV negative women to work as wet-nurses for HIV positive mothers. This would protect infants against HIV while providing the unique benefits of breast milk. More importantly, perhaps, it would make wet-nursing a profession of high prestige and wet-nurses role models for adolescents. Few poor women know their HIV status.6 Wet-nursing would give them an incentive to remain HIV negative and counter monetary motives for risky sex.7 A wet-nursing profession would build female solidarity, increase female social power, and help integrate children of HIV positive mothers into society. It would also retain donor funds in poor communities and thereby alleviate poverty.

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