PostScript

LETTERS

Best practice: migraine

In their otherwise very comprehensive review of this subject in your October issue of Education and Practice,¹ Drs Barnes and Jayawant limit themselves to only two aspects of management in the prophylaxis of migraine: drug treatments, and psychological/behavioural therapy.

While it may be difficult to find hard evidence for some of the environmental areas they omitted, some mention of avoidance of obvious trigger factors would surely have been in order. These may vary in importance from child to child, but hypoglycaemia/hunger, lack of sleep, stress, vigorous exercise, and obvious dietary triggers can each be relevant in individual cases and therefore worthy of common sense advice.

Of probably far greater importance as an omission from this paper, and one for which there is an excellent evidence base, is the use of dietary measures in prophylaxis. Egger *et al*, in their *Lancet* paper in 1983,² showed that a dietary approach could be very effective in a series of cases of severe intractable migraine, achieving a significant response in >80% of cases. They used an "oligoantigenic" or "few foods" diet followed by challenge with individual items of diet in a sequential manner. They found that a large number of "healthy" foods could be implicated in their cases, including wheat, cows' milk, other cereals, and fruits, with many children having multiple intolerances. In other words there is more to diet and migraine than simply excluding the 5 Cs (cheese, chocolate, coffee, coke, and citrus fruits).

They performed double blind challenges in a selection of their cases; these were confirmatory of the results of open challenge in the majority of cases.

Such an approach to management can in my experience be extremely helpful in individual cases of severe intractable abdominal or cranial migraine. It is quite feasible to administer on an outpatient basis with good support from an experienced dietician. When successful this approach may make drug treatment unnecessary. When not attempted it can render drug treatment ineffectual.

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Ankyloglossia intervention in outpatients is safe: our experience

Hall and Renfrew rightly describe the literature with relation to ankyloglossia as containing "little high quality objective

evidence"; they also describe the difficulties in study methodology in this setting with particular reference to concealing the diagnosis from parents in control studies.¹ With regards to intervention, they note that "...frenulotomy in the newborn is a low risk minor procedure, performed without anaesthetic" and that "...significant venous bleeding could occur if technique is not meticulous but we found no reports of serious adverse events".

Ankyloglossia intervention in the outpatient setting has been performed for over 20 years in Edinburgh without anaesthetic. We recently reviewed 28 months of patients (February 2002–June 2004) who underwent the procedure to assess the complication and success rates. We fully acknowledge that our study was neither controlled nor had objective measures of outcome; nevertheless, it is the only study we know of to specifically investigate the safety of outpatient intervention in this country.

Patients were identified from outpatient correspondence to general practitioners if they were initially referred for consideration of ankyloglossia as a cause of feeding difficulties. If appropriate the tongue tie was divided in the clinic by one of two consultant paediatric surgeons using the following method:

The baby is firmly swaddled in a blanket and held (by an experienced nurse) across the examination couch with his head towards the surgeon. The baby's head is steadied by the nurse who can also assist in pulling down the chin with her thumb to encourage the mouth to open. The surgeon's left index finger elevates the tongue placing the frenulum under tension. The transparent part of the frenulum is divided with iris scissors. Care must be taken to avoid cutting into the thicker posterior part of the frenulum which carries the blood supply. Bleeding should be minimal or nonexistent. The baby is fed by the mother in another room and checked again by the surgeon before leaving the clinic.

The mothers of the study patients were telephoned to complete a brief telephone questionnaire relating to possible complications and some subjective indicators of success after a minimum 14 day period.

Forty four (of a possible 51 study patients) were successfully contacted.

The mean age of infants on the day of tongue tie division was 49 days (SD 41, mean 38, mode 38) with the youngest infant being 3 days old and the oldest 202 days old. The study group consisted of 25 males (57%) and 19 females (43%).

The type of feeding before the procedure was documented and included all types of feeding attempted by the mother until the date of tongue tie division. Forty mothers were at least partly breast feeding, four infants were exclusively formula fed. Of the 40 breast fed infants, 35 (88%) had problems latching on. Thirty three (83%) of the mothers had sore nipples and 15 (38%) had mastitis. Thirty five (80%) of the 44 mothers noted an improvement in the ease of feeding after the procedure, with 28 (64%) also noting an improvement in the time taken for a feed. Three of the four formula fed infants were improved in both these areas.

With regard to the complication rate (table 1), any pain or bleeding after leaving the clinic was considered significant as well as any episodes of infection, any need to seek medical advice, and any repeat procedure required to release the tongue tie. The mean time of leaving the clinic after the procedure was 29 minutes (SD 21 minutes), with the shortest wait 0 minutes and the longest wait 120 minutes in an infant whose mother was awaiting delivery of a breast pump from stores. Excluding this infant, the longest wait was 60 minutes.

One infant had bleeding after leaving the clinic. The bleeding was minimal and self limiting. The baby did not require any further medical attention and had left the clinic 15 minutes after the procedure.

One infant was reported as being in pain after the procedure and was given a single dose of paracetamol with good effect. There were no incidents of infection and no requirement for further medical advice after the procedure. Two patients required a repeat procedure which was performed in the same way on a separate day. Both repeat procedures were successful.

This study shows that frenotomy in outpatients is both effective in releasing tongue tie and safe in terms of potential complications (9% overall complication rate). We suggest that the consideration of ankyloglossia is important in the differential diagnosis of an infant with difficulties feeding or a mother having pain breast feeding. If diagnosed in either of these situations, the ankyloglossia can be easily treated in the outpatient clinic setting with a low complication rate. We would recommend

Table 1 Complications after leaving clinic

Complications after leaving clinic	Yes	Yes (%)	No	No (%)	Total
Bleeding	1	2	43	98	44
Pain	1	2	43	98	44
Infection	0	0	44	100	44
Further medical advice needed	0	0	44	100	44
Repeat procedure needed	2	5	42	95	44
Total	4	9	40	91	44

the mother be encouraged to remain in clinic for a minimum of 30 minutes after the procedure during which time a test feed can be performed. Routine outpatient follow up is not required.

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Tongue ties and breast feeding

The humble tongue tie, like the foreskin, generates enormous quantities of hot air, with little evidence to support it. I welcome the decision by the *Archives of Disease in Childhood* to commission a Perspective on tongue tie¹ in the same month that NICE² has issued its positive guidance on the same topic.

Hall and Renfrew begin by stating their bias. Mine is that the *Archives of Disease in Childhood* rejected my two articles on tongue tie, a prospective series and a randomised control trial,^{3,4} both now published by other peer reviewed journals, but which the *Archives of Disease in Childhood* rejected following very poor quality "peer review", and compounded the error by referring me to my Medical Director as someone who was performing procedures which were unethical. This charge was rejected by my Trust.

The interesting question here is how to persuade an intelligent, professional body (in this case, paediatricians), that what they were taught has no evidence to support it. The old textbooks, as well as advocating bottle feeding, state that tongue ties essentially never cause a problem. Hall and Renfrew have looked at the new evidence and conclude that the textbooks are partially right—many babies with tongue ties can feed adequately or normally—but that the textbooks are wrong to say that tongue ties never cause a problem. Some babies do indeed have a problem which can be solved by a suitably trained person dividing the tongue tie. It is not necessary to look for tongue ties at birth, but if feeding problems are noticed, then anyone skilled in helping mothers to feed will need to exclude tongue ties as one of the many possible causes. Division is not advised at delivery, as babies can take several days to establish the correct feeding rhythm, but do not wait as long as us (three weeks) as many mothers may give up before then (we waited till the mothers asked for help, and accept that some (13/88, 15%) did give up before we could randomise them).⁴

I would ask two questions. Firstly, what do the authors mean by "precise case definition"? I think this is the identification of the problems caused by a symptomatic tongue tie, and suggest a triad of poor latch (which includes signs of frustration, like head banging), painful nipple feeding (including ulcerated, bleeding, cracked nipples, mastitis, and lipstick nipple), and frequent or continuous feeds. Secondly, I am keen to hear their

suggestions for objective measures of improvement in order to study "inter-observer reliability of pre- and post-intervention assessment". The *Archives of Disease in Childhood*, when pressed on this issue previously, could not suggest anything. Maternal pain scoring produces a quantifiable number, but is subjective. Test weighing babies is known to be stressful and is not practised in this country. However, the national or local rate of breastfeeding is well documented⁵ and should be an objective measure of pain-free, successful breastfeeding. This group of mothers are struggling to breast feed because of the pain and the time it takes. If the tongue tie is divided but there is no improvement in symptoms, there is no reason for the breastfeeding rate to be different from the national average or even lower. In both my studies^{3,4} and that of Masaitis and Kaempf,⁶ the breastfeeding rate at 3–4 months was at least 60% (twice the national average). Arguably, it may be that, like pain scores, all assessments should be subjective, as only the mother can really assess the quality of the feed.

Like the American Academy of Pediatrics Section on Breastfeeding Bulletin,⁷ Hall and Renfrew conclude that tongue ties which cause problems exist, and that these problems cannot be predicted by a static view of the tongue tie. The problems are relieved by division, which is simple, safe, and successful, and should be performed by a suitably trained health professional. These are all very reassuring conclusions to those involved in ensuring that as many babies as possible are able to breast feed successfully. Now that the *Archives* and NICE have concurred that the division of tongue ties in symptomatic babies is no longer unethical, I hope that more units will allow their Infant Feeding Specialists to be trained, so that mothers and babies in the future will be able to breast feed painlessly for as long as possible.

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Authors' reply

Tongue tie: more research needed

We welcome the interest in our review of tongue tie. Hansen *et al* confirmed the observation of others that division of tongue tie is a simple and safe procedure. We agree with Griffiths that although more remains to

be learned about tongue tie, the clinical practice and research described by him and his colleagues have sufficient support to justify it, and work on this topic is certainly not "unethical".

Griffiths asked two questions. First, he asked about case definition and partially answers his own question by suggesting a triad of features. We would put the question back to him and ask whether all three features are necessary and whether he considers that all three together are diagnostic or merely suggestive. We note that he does not actually refer at all in this triad to the physical appearance of the tongue; while we understand the reasons for this and commented on the difficulty of interpreting the static appearance of the tongue tie in our review, it would seem strange to ignore physical appearance altogether. We agree that maternal reports of pain are a crucial measure, subjective as this obviously is. Added to this it would seem essential to use independent blinded observers to assess feeding and any associated problems. The collection of serial observations of feeding, weight gain, infant distress, and maternal assessment over a period of time would identify whether changes are sustained. In addition, more work is needed to determine the place of dynamic studies using ultrasound or milk flow studies; these also would need analysis of inter- and intra-observer reliability.

We do not pretend that the design of any further research studies would be easy but, as Griffiths refers to the unit's breast feeding rate in support of his team's practice, one possible approach might be to compare departments, perhaps using a design similar to that undertaken by Kennedy and colleagues in their excellent trial of newborn hearing screening¹ (which coincidentally was also undertaken in Southampton).

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Reference

- 1 Kennedy CR. Neonatal screening for hearing impairment. *Arch Dis Child* 2000;**83**:377–83.

Postgraduate training in the UK: whither uniformity?

Like many others, the article by Bannon makes the mistaken assumption that medical career and training in UK is facilitated only by the official "RCPC approved" training pathways.¹ It totally ignores the aspirations and career plans of the huge workforce of doctors who are employed in so called non-training posts. In many places they equal or at times outnumber the Training Grades.

Perhaps it is time for all concerned, including the RCPC, to reflect on the fact that approval of training is not necessarily a matter of funding. Many Staff Grade doctors put in sterling performances, yet, due to a traditional dichotomy are never considered to be in "training" when compared to the Training SPRs.

Article 14 offers some hope for some of these doctors who have long years of experience, but for those at a much more junior level, the current changes will only add to the sense of being "left out" of the game. I find it really surprising that educators who spent their entire life designing new theories of