Dear Sir,

Re The value of frenotomy for ankyloglossia from a parental perspective.

This was not a medical or scientific study. It was a consumer satisfaction survey, with the same credibility one might give to a survey of satisfaction with a washing machine or a restaurant.

Of note there is no control group. Further, in a radio interview one of the authors denied the need for a control group, thus bringing the credibility of the paper further into doubt. The primary conclusion that parents were satisfied demonstrates the importance of control groups. These parents had self-presented and paid for the procedure – strong biasing influences completely ignored in the study, and demonstrating the dubious nature of the conclusions.

The paper explores weak criteria such as parent satisfaction and impressions of its value.

The diagnostic criteria are poor and unsupported by imaging or dynamic studies, or measures of ongoing infant well-being such as weight gain. I note the lack of input by paediatric, ENT, or oral surgeons. Of further concern, given a recent HDC finding (16HDC01563) there was no reference to whether the infants had been given Vitamin K at birth – the age range is not stated but the paper refers to infants younger than six months and the mean age was 44 days, suggesting some infants could have been very young and within the time span when Vitamin K was relevant.

The reference to “five widely accepted randomised controlled trials regarding frenotomy and its effect on breastfeeding” was particularly amusing, given that these five trials involved only 302 cases. The conclusions that can be drawn from such low numbers beg the question – where was the adequate power analysis? Drawing conclusions about an invasive procedure from such tiny numbers is risky. Further, the NICE statement of “limited evidence suggests” is weak at best, and again shows the lack of credible support for widespread diagnosis and surgical treatment of tongue tie for breast feeding.

Whilst no serious complications were reported in the paper, these have occurred in New Zealand, resulting in emergency and or corrective surgery, blood transfusions and adverse findings by the Health and Disability Commissioner

My overall concern is that this paper, despite its poor methodology, is already being used to support what is widely regarded as unnecessary surgery for an over diagnosed problem.

Given the lack of credible scientific support this paper offers, I question the ethics of this study, in that it involved a surgical procedure in infants.

Yours faithfully,

Graham J Sharpe
Competing interests:
Nil.

Author information:
Dr Graham J Sharpe, Specialist Anaesthetist, Wellington.

Corresponding author:
Dr Graham J Sharpe, Specialist Anaesthetist, Wellington.
gjsharpe57@gmail.com

URL:

REFERENCES: