Timing of tracheostomy in adult patients

Potential ramifications are alarming

EDITOR—In their systematic review and meta-analysis Griffiths et al conclude that tracheostomies should be performed earlier in critically ill patients.¹ As ear, nose, and throat surgeons we find the potential ramifications quite alarming, primarily as undertaking this major operation is not without potential morbidity in patients who are already systemically unwell.^{2,3}

These patients are often anaemic and have coagulopathies and sepsis among other comorbidities—points that we thought the authors had not discussed, and factors that have implications for the surgical procedure. In our experience, many patients listed for tracheostomy from intensive care units are extubated or sometimes die while waiting for their procedure; if tracheostomy were performed immediately these patients would be subjected to unnecessary procedures. The postoperative sequelae of tracheostomy are a slow weaning process before patients can be decannulated, followed by potential tracheal stenosis.⁴

These patients may be able to leave intensive care earlier, but Griffiths et al do not say how long they then remain inpatients before discharge because of problems with rehabilitation with a tracheostomy tube in situ. Interestingly, although duration of ventilation and stay in the intensive care unit were shortened by an earlier tracheostomy, mortality was not altered and inpatient stay was likely to have been lengthened.⁵

The authors divided timing of tracheostomy into two groups, demarcated by seven days after intubation, making an artificial separation that seems invalid. It would have been more interesting to see whether increasing number of days have a proportional relation to the duration of ventilation and stay in intensive care on a linear scale. We advise great caution in interpreting the findings of Griffiths et al and about its impact on the delivery of the tracheostomy service in hospitals.

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Conclusions may be premature

EDITOR—Griffiths et al critically appraised all randomised clinical trials into the timing of tracheostomy in adult patients in intensive care units.¹ Of the identified trials, most were excluded for several reasons, leaving only five trials with a combined study population of 406 patients for review and meta-analysis. The overall results showed that length of stay in intensive care and duration of artificial ventilation was significantly lower in the “early tracheostomy” group; timing of tracheostomy did not alter mortality or the development of pneumonia.

Meta-analyses are usually performed when individual studies are too small to yield sufficient power to detect a difference between strategies. Meta-analyses also bear the risk, however, of generalising the differences between incorporated studies, resulting in unacceptable heterogeneity and overscoring or underscoring of the effect under study. This may occur especially when the number of studies is small, resulting in higher weight of each individual trial. In the analysis by Griffiths et al, the combined study population left for review or meta-analysis regarding the first two outcome measures (duration of mechanical ventilation and length of stay in intensive care) was only 332 (four studies) and 226 patients (two studies), respectively. In addition, one of the studies was characterised by an unexpected high mortality rate of the control group (the “late or no tracheostomy” group).²

Finally, it is important to appreciate that length of stay in intensive care is not only dependent on need for mechanical ventilation but also on whether a stepdown or a long term acute care facility is present or not. Especially in the latter study this may have influenced length of stay in the intensive care unit.³

At least three large randomised trials are being performed on the topic of early or late tracheostomy: the randomised controlled trial in the United Kingdom (TracMan), a multicentre study in the Netherlands, and a

study in France.⁴ We should await the results of these (hopefully sufficiently powered) studies before drawing firm conclusions on timing of tracheostomy.

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Authors' reply

EDITOR—Philpott et al comment that tracheostomies carry an operative risk that is amplified by the severe illness precipitating admission to the intensive care unit. We estimated this risk when planning the TracMan trial of early versus late tracheostomy by reviewing 110 published case series of tracheostomy. Average operative mortality was 0.3% (www.ics.ac.uk/default2.htm and follow links). The overall mortality in intensive care in patients receiving a tracheostomy was 38.1% and in UK intensive care units, 20.1% (data at link above).

Thus even a modest reduction in mortality in intensive care exceeding 0.3% absolute (1.4% relative) in the United Kingdom caused by early tracheostomy would improve overall outcome. As they point out, this mortality benefit would come at a cost, numerous patients who would probably have survived without a tracheostomy would receive one. Similar problems are well known in drug trials, where the utility of a drug is often determined by looking at the number needed to treat (NNT) compared with the number needed to harm (NNH). It is more unusual to apply these considerations to a surgical procedure.

Philpott et al point out that duration of hospital stay may increase in patients with a tracheostomy even if their stay in intensive care and duration of artificial ventilation is reduced. Schultz et al also point out that a tracheostomy, independent of a need for

1 Griffiths J, Barber VS, Morgan L, Young JD. Systematic review and meta-analysis of studies of the timing of tracheostomy in adult patients undergoing artificial ventilation. *BMJ* 2005;330:1243. (28 May).

artificial ventilation, may alter the length of stay in intensive care depending on the availability of intermediate level care. To avoid these problems we would have preferred to use length of stay in hospital, not in intensive care.

Unfortunately neither the published randomised controlled trials in the review nor our review of case series mentioned above provided data on length of stay in hospital. An additional problem arises when using average length of stay in intensive care or hospital as an outcome measure. It is not independent of mortality, deaths tend to reduce the length of stay, and so lengths of stay should ideally be reported for survivors and for all patients. In the randomised controlled trials we identified it was unclear if the results were for survivors or the whole group. These points are being tackled in both the TracMan and French tracheostomy timing studies.

Schultz et al point out the limitations of our meta-analyses. We agree with their view that firm conclusions on the timing of tracheostomy should await the results of large randomised controlled trials.

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Shaving for head lice is unnecessary and distressing

See pp 362, 384

EDITOR—Lwegaba argues that shaving can be a safer treatment for head lice than insecticides.¹ However, shaving for head lice is far from ideal in children.

Western Australia's Department of Health does not recommend shaving hair to remove head lice. Shaving is only a short term solution. It is unnecessary (other effective treatments are available), it can distress the child, and it is often the result of despairing parents receiving out of date advice. A more effective approach is to involve parents and other school community members in establishing a substantial ongoing head lice management programme in schools that provides accurate, research based advice on screening for and treating head lice and helps to interrupt the cycle of reinfestation that can be perpetuated in school communities.

Western Australia's Department of Health has produced a school head lice resource (www.health.wa.gov.au/headlice) to help school communities develop a head lice management programme for schools. The resource includes multiple templates and advice on how to implement the

programme. For example, the programme describes a step by step process to help ensure a prompt and effective response from parents to a school's requests for them to screen and treat their children. The health department also supports schools in implementing a programme by providing free information sessions on using the resource.

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Childhood obstructive sleep apnoea

Covariation is not proof of cause

EDITOR—Neuropsychological deficits may be more common among children with obstructive sleep apnoea but that does not prove how many of these deficits are caused by sleep apnoea. Muscular hypotonia and dyscoordination are common among children with neuropsychiatric disabilities, and can cause snoring and obstructive sleep apnoea. Kotagal cites a study by O'Brien et al, describing 35 children with obstructive sleep apnoea (mean age 6.7 years) and 35 closely matched controls.^{1 2}

This paper by O'Brien et al does not deal with sleep apnoea²; it shows that children who only snore a lot (without apnoea) have slightly more problems of different kinds even when the authors beforehand removed all children with attention-deficit/hyperactivity disorder or hyperactivity from the study.

Urschitz et al found that habitual snoring was significantly associated with hyperactive (odds ratio 2.4) and inattentive behavior (4.0).³ These associations were independent of intermittent hypoxia, which is why I believe that both snoring and obstructive sleep apnoea are markers for neuropsychological deficits but not necessarily causing the deficits.

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Hypertension was not mentioned

EDITOR—An important effect is missing in the editorial on childhood obstructive sleep apnoea by Kotagal.¹ The neuropsychological sequelae of this condition were mentioned, but another effect was not—namely, high blood pressure.

This omission is not uncommon as shown by a report on the global burden of hypertension.² Currently, there is good evidence—for example, the report by the Tucson Group³—that childhood obstructive sleep apnoea leads to raised blood pressure. Our unpublished data on childhood obstructive sleep apnoea and 24 hour ambulatory blood pressure show that 18% of children had nocturnal hypertension only. Childhood obstructive sleep apnoea was also associated with blood pressure dysregulation that could lead to lower blood pressure in a subgroup of children. Other cardiovascular consequences such as left ventricular hypertrophy and altered sympathetic activation were also identified. The report from our centre also showed that snoring children without obstructive sleep apnoea had higher blood pressure but not hypertension and decreased arterial distensibility compared with controls.⁴

The study quoted by Kotagal to support the use of comprehensive clinical assessment to diagnose childhood obstructive sleep apnoea included a sample of 41 children. He did not cite a meta-analysis that showed that history and physical assessment alone for the diagnosis of childhood obstructive sleep apnoea in snoring children gave a combined positive predictive value of 55.8% (95% confidence interval 42.1 to 69.9)—that is, not significantly better than chance.⁵ The comprehensive clinical assessment may be useful in a healthy population without the risk factors of regular snoring, observed apnoea, obesity, dysmorphic syndromes, attention-deficit/hyperactivity disorder, or daytime sleepiness. For children with risk factors, sleep polysomnography should be offered.

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Anaesthetic implications for adenotonsillectomy are important

EDITOR—Kotagal correctly mentions that adenotonsillectomy is performed as a potential treatment for children with obstructive sleep apnoea and highlights some of the perioperative risks.¹ We write to emphasise this, since these children may present important challenges to safe and effective anaesthetic management.

Under anaesthesia, children with obstructive sleep apnoea have impaired ventilatory responses to carbon dioxide compared with controls.² This may account, in part, for the increase in respiratory complications recorded in these patients. Children aged 3 years or younger, and those with cardiac or neurological problems are at extra risk of such postoperative complications.³

On a practical note, administration of atropine at anaesthetic induction has been reported to reduce respiratory complications.⁴ For children with "severe" obstructive sleep apnoea, having their operation in the morning is associated with less postoperative hypoxaemia than that seen in those operated on in the afternoon.⁵

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Author's reply

EDITOR—Reimer says that covariance does not prove causation. I would remind him of the longitudinal, case-control study of Gozal, in which elementary school children with obstructive sleep apnoea who had adenotonsillectomies showed significantly improved academic performance compared with children with the same condition who did not.¹

Ng et al question the value of the clinical assessment, citing the meta-analysis of Brietze et al.² Three of the 12 studies cited in this meta-analysis were questionnaires pertaining to sleep history, three were retrospective, one excluded upper airway anomalies and abnormal facial morphology, and one used an invalid polysomnographic scoring criterion for paediatric obstructive sleep apnoea (apnoea length 10 s rather than 5 s; I therefore question these authors' conclusion that clinical assessment is unreliable). The comments of Singh and Marnane on bmj.com also resonate with the need for developing an algorithm based upon history and the clinical examination, as well as updating polysomnographic scoring guidelines.³

I agree with Ball and Jefferson about the additional perioperative risk posed by underlying cardiac and neurological disorders during adenotonsillectomy.

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Baby growth needs proper re-evaluation

EDITOR—Discussion of the forthcoming World Health Organization growth charts and their possible adoption in the United Kingdom is welcome but needs to be focused on the issues.

Wright asks whether the growth charts that we currently use are inaccurate, particularly in applying them to breastfed babies.¹ Although this is an interesting and important question, a more illuminating one might be: "Are we using growth charts appropriately?" If all who plot infant growth were aware that the shape of breastfed baby growth centiles differs from those of artificially fed infants, and if routine weight monitoring were matched with routine assessment of skilled breast feeding, current charts would probably be perfectly adequate. As Wright notes, the babies whose data are included in the new chart received ongoing support for breastfeeding and therefore show the pattern that infants can attain when their mothers are supported appropriately.

UK mothers seem to pick up the idea that their baby's growth should follow a particular chart centile, and even minor fluctuations are of concern. At the same time, if growth falters slightly for a breastfed baby, the first thought is to reach for a bottle. Neither of these common perceptions is seriously challenged by the information given to each mother in the parent held child health record, or by consistent messages from health visitors.

The major value of the WHO growth chart may be in sparking a re-evaluation of the mediocre weight monitoring practice often found in the UK, which frequently undermines breastfeeding. This is a long-standing issue and deserves serious attention.^{2,3} Otherwise UK infants are likely to remain at risk of inappropriate supplementation of breastfeeding due to poor weight monitoring and poor standards of professional breastfeeding support.⁴

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Teaching students in general practice can affect patients' blood pressure

EDITOR—An increasing amount of medical student teaching is being based in the community, as it is becoming increasingly recognised that this is where most consultations take place. Benson et al report on combining quantitative and qualitative methods in exploring the views of patients participating in teaching.¹ They start their discussion by saying that participation in teaching by general practitioners and student doctors had no negative impact on patients.

Patients have always been an essential part of medical education, but we cannot assume that they choose to participate.²⁻⁴ In our randomised controlled trial in 22 teaching practices we calculated the influence of the presence of doctors in training on the blood pressure of patients.⁵ We found that in teaching practices the presence of a doctor in training had a notable effect when an experienced general practitioner measured a patient's blood pressure. We came to the following conclusions.

Firstly, although most patients accept that doctors in training must develop their clinical skills, the presence of a student might add to the arousal that the medical environment already imposes on a patient.

Secondly, blood pressure measurement may be useful in investigating whether a doctor in training is a neutral bystander or an obtrusive observer provoking a reactive increase in a patient's blood pressure.

Thirdly, doctors should be cautious in initiating or adjusting antihypertensive treatment when blood pressure readings are obtained in the presence of a student.

Blood pressure measurement, arousal of the medical environment, and observer effects of students may have nothing to do with enablement or satisfaction, the scientific community being comfortable with the "objective," "accepted," "measurable," and "quantitative" blood pressure variables. In our study, however, we found that the presence of students affected the blood pressure of patients.

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