important because the analogy’s purpose is to teach the inexperienced. Therefore, what Dr Bone (or I, or any experienced practitioner) thinks the analogies convey does not actually matter. It matters what novices think, and if we want to teach them, it matters if the instructor’s beliefs are at odds with the student’s. Teaching right the first time can shorten the learning curve for novices, and may also decrease early errors. Presumably, once airway positioning has been mastered it does not matter what term you use to encapsulate your knowledge. However, this is about teaching novices.

Dr Bone stated that he was ‘sure that many people would consider the position required to sniff for smoke to be very different from that associated with sniffing the morning air’. This may be true; however, our pilot data showed that many positioned the head the same incorrect way regardless of whether we used to sniff for smoke or sniff the morning air. We also confirmed that, at least locally, both of these interpretations of Magill’s vague 1936 expression were used. Pilot data also suggested that it was not unusual for novices simply to be told ‘to put the patient in the sniffing position’ with no further instructions, whether verbal, written, or hands-on. Dr Bone is, therefore, absolutely correct that we cannot rely upon only written instructions to teach airway positioning. However, we make this exact point twice. We wrote: ‘the low percentage of correctly positioned airways confirms that learners need more than just verbal instructions. However, instructional time is limited. Therefore, we need to know where to focus attention, just as we need to avoid strategies that might worsen performance’ and that: ‘we provided no feedback or hands-on demonstration, as would hopefully occur during airway training. This was in order to isolate the effect of the written instructions’. In other words, our design mirrored our goal. We were studying how learners interpret these analogies, not teaching airway positioning. We agree that human volunteers may be superior to manikins for teaching. However, humans have anatomic differences. This would introduce another study variable. Awake humans also resist uncomfortable positions, or make slight movements before a photograph is taken. Of note, however, there is also a growing reluctance to allow novices to start their learning with humans. Simulators have been widely promoted but rarely tested. This study provided a useful critique of two common airway simulators.

This is a topic ripe for more study: we simply provided an initial step. Dr Bone rightly points out there may be fascinating lessons from other cultures and languages. Regardless, as scientists, we should update based upon the best available evidence. Airway instruction is too important to merely rely upon tradition.

Conflict of interest
None declared.

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Nerve injury by needle nerve perforation in regional anaesthesia

Editor—I read with interest the article by Steinfeldt and colleagues and congratulate them on their excellent research paper. However, I feel that this study requires further work before making a recommendation for clinical practice. First, the most important consideration is to avoid nerve puncture, while there is no major study to show that ultrasound is safer compared with other techniques, it has been shown that a larger diameter needle is better visualized by ultrasound.

Secondly, it is believed that the most damage to nerves is actually done by direct intra-neural injection and local anaesthetic application and it seems that Steinfeldt and colleagues are in an optimal position to take their work further. It would be of interest to extend their previous study protocol and to examine the histological difference after puncturing the nerve and injecting local anaesthetic into the nerve with the same 24 G and 19 G needles.

Conflict of interest
None declared.

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Editor—We thank Dr Sell for his interest in our work and his thoughtful suggestions with regard to eventual clinical implications and potential future studies. He raised two important aspects: first, needle visibility via ultrasound technology, and second, potential consequences of intra-neural injection of local anaesthetics. Although needles with larger diameter may, in general, be better visualized with ultrasound, recent engineering progress in both ultrasound technology and the ultrasound visibility of particular needles may have improved this aspect even with smaller needles. Irrespective of needle visibility, ultrasound technology carries important limitations that need to be acknowledged; that is, advancing a needle in a three-dimensional space using two-dimensional control. Thus, lack of a third dimension may result in incorrect needle placement regardless of needle visibility. To avoid adverse consequences of the conceivable worst-case scenario—needle-nerve perforation—we propose the use of smallest needle diameter that is feasible for the selected regional anaesthesia. Recently, a study of intra-neural local anaesthetic injection in a pig model showed that intra-neural application of local anaesthetics was associated with a marked inflammatory response within the nerve. However, no control with needle penetration or injection of liquid (e.g. saline) was used.
in control nerves. Therefore, eventual causality remains unclear. Interestingly, the authors made a behavioural assessment within 1 week and found no functional deficits, but the tests used lacked discrimination of motor or sensory deficits, paraesthesia, or painful neuropathies. Therefore, functional data should be interpreted with caution. Objective evaluation of nerve injury, for example, myelin damage, is missing. Behavioural assessment is challenging, especially in larger animals such as pigs. For rodents, for example, rats and mice, validated and reproducible instruments (i.e. hot-plate test, von Frey electronic, etc.) are available. On the basis of these many uncertainties, we feel that it is necessary to further clarify risks and consequences of various regional anaesthesia techniques before their acceptance and application in daily clinical practice.

**Conflict of interest**

None declared.

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**Emergence agitation in children**

Editor—I was interested in the article concerning emergence agitation in children after sevoflurane or desflurane anaesthesia. I anaesthetize a large number of pre-school children each year and my practice is mainly ENT. The authors found from their meta-analysis that propofol, ketamine, fentanyl, and preoperative analgesia all had a preventative effect on emergence agitation. I, like them, was slightly surprised that midazolam did not prevent emergence agitation. I would personally expect all agents such as sedatives, hypnotics, and analgesics to have a preventative effect on emergence agitation as they will all tend to give a smoother recovery.

I have long felt that a large number of pre-school children anaesthetized in my local hospitals suffer emergence agitation after operation when maintained with sevoflurane (we do not have desflurane available). For many years, I have maintained anaesthesia for these children, after induction of anaesthesia with either propofol or sevoflurane, with isoflurane. Isoflurane is possibly a slightly more difficult agent to use as it is more irritant to the airway than sevoflurane. I feel that the children maintained with isoflurane show less signs of emergence agitation. The children tend to wake slower than those maintained with sevoflurane but seem generally less distressed and their discharge from hospital is not delayed, compared with those maintained with sevoflurane.

As the authors mentioned, emergence agitation is distressing for recovery staff, ward staff, and parents and also puts the child at risk of self-harm and I feel that the simple substitution of isoflurane for sevoflurane for maintenance of anaesthesia decreases this risk. I have no evidence that isoflurane decreases the incidence of emergence agitation; indeed, some papers support the very opposite view but I would be interested if the authors came across any evidence to support or refute the suggestion that isoflurane leads to less postoperative emergence agitation?

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Editor—I thank Dr Huddy for his interest in our study. In response, two studies comparing sevoflurane and isoflurane have been published. These studies had divergent conclusions, despite similar methodologies especially concerning the preoperative pain management. However, the quantification of the emergence agitation was different, which may explain the conflicting results. It is established that the rapidity of the emergence is one of the factors favouring the agitation. Consequently, despite these conflicting results, isoflurane, because of its slower offset, may be considered as a prophylactic treatment of emergence agitation.

**Conflict of interest**

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