changes associated with induction of anaesthesia in elderly patients undergoing high risk vascular surgery. ASA Abstracts 2012: A2792
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Airway management strategy for odentogenous abscess

Editor—In their study of patients with odentogenous abscesses and reduced mouth opening, Schumann and colleagues report successful tracheal intubation with the Glidescope device (Verathon Medical Europe, Ijsselstein, The Netherlands) for all 50 subjects randomized to this form of laryngoscopy; additionally, the Glidescope was used successfully for rescue in the group of patients randomized to Macintosh laryngoscopy but had failed. Readers may see this as good news for the Glidescope in managing difficult airways complicating dental abscess; some may extend this as good news for videolaryngoscopy in general. While this may be true, we have concerns that the authors’ overall approach may not be generally applicable for patients with this dangerous condition.

First, this study was performed by practitioners with experience in anaesthesia for head and neck surgery and with acquired skill in videolaryngoscopy. This is therefore a competent group and so their results may not be directly transferable to others, a form of ‘context sensitive airway management’. We feel that this factor should be emphasized more; experts do things that others cannot.

Secondly, the authors acknowledge that this is a high-risk group of patients to manage safely. The authors recognize this with description of a back-up plan, namely awakening the patient with subsequent awake fibreoptic intubation. The notion of planning for failure is a consistent theme throughout the NAP4 report. Planning is often, however, limited to the so-called ‘Plan A, Plan B’ approach. If these two plans share modes of failure which can spoil each technique, the notion of binary planning for airway management may not be sufficient, since factors leading to failure of Plan A may prevent effective execution of Plan B. For example, if Plan A (videolaryngoscopy) fails due to instrumental bleeding (a real possibility in the context of airway infection with swollen, hyperaemic tissues), Plan B (fibreoptic management) is also compromised. The two plans are too tightly linked or coupled to the same failure mode. So, for the authors, failure of both of their plans is possible as a consequence of both plans sharing a common failure mode, namely bleeding.

Thirdly, to the element of risk, we can add the element of uncertainty. In their study of airway management planning in dental abscess, Darshane and colleagues report that ‘problems did occur... and ‘these always came as a surprise requiring a total re-think’. This is a possibility for Schumann and colleagues. They may be confronted with a deteriorating, perhaps dangerous airway requiring immediate, reactive responses. Facemask ventilation may not be effective (efficacy of mask ventilation in the authors’ study was not reported), use of a supraglottic device may be impossible with high-grade trismus, and emergency cricothyroidotomy is associated with low success. Succinylcholine, used by the authors for airway control, may not wear off before hypoxia develops, especially in patients with high oxygen consumption (young patients with infection and fever). Darshane and colleagues call this ‘on the hoof’ airway management, stating it ‘must be considered a recipe for disaster’.

We suggest that safety is increased if the second (and even better, a third) plan has a method of execution whose risks are distinct from the other selected. For Schumann and colleagues, this could be insertion of a precautionary cricothyroid cannula, a proactive method of particular utility in patients with high-risk airways. This would plausibly increase safety not only for the intubation sequence, but also for tracheal extubation, a period of risk which is often underestimated. Postoperative bleeding is a concern for all the study patients, particularly for those whose mouth opening remained limited after anaesthesia. Indeed, airway bleeding is recognized as a major source of morbidity and mortality in the NAP4 report.

In general, odentogenous (dental) abscess with fascial space involvement are difficult to manage and those affecting the floor of the mouth are even more difficult and may be dangerous. Trismus is an extra and major complication. This may not resolve with anaesthesia and is associated with symptom duration, as the authors have usefully shown. We conclude that videolaryngoscopy may be safe and effective in this challenging group of patients, but only in the context of a robust, reliable airway management strategy. This would best be devised using proactive and reactive components whose effective implementation, if needed, use methods that are not compromised by sharing the same failure mode.

Declaration of interest

D.R.B. has received equipment for evaluation, teaching, and use from P3 Medical, Freelance Surgical, Storz Medical, Trucorp, Smiths Medical, Verathon, and Intavent Direct.

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Tracheal intubation in patients with odontogenous abscesses: plans and more plans

Reply from the authors

Editor—We appreciate the chance to respond to the letter of Cassellos and Ball in response to our paper.1 The authors are concerned that our approach may not be generally applicable for patients with this dangerous condition.

We agree with the authors’ concern, but it was at no point our intention to present a general plan for all patients with this condition. Our intention was to compare two devices, which might be used for intubation in this critical situation and possibly improve future management of this condition.

We excluded, as pointed out in our Methods section, patients with a mouth opening of <1.4 cm and patients with a reduced apnoea tolerance. Patients with odontogenous abscesses can present conditions for tracheal intubation that vary from very easy and unproblematic to almost impossible. Therefore, as we pointed out in our Discussion section, all these cases require a thorough plan before induction as presented by Darshane and colleagues2 in their ‘responsive contingency planning’.

Based on the history of the patient, clinical examination, and information from the responsible surgeon about spread and location of the respective abscess, the planning has to go from plan A to B or C or even further down the alphabet. For example, patients with a mouth opening of <1.4 cm would be intubated directly under local anaesthesia and preserved spontaneous ventilation. Moreover, it depends also on the local resources and experience. In ‘advanced’ cases, we have an experienced and cranio-maxillofacial surgeon in the room for emergency intervention. The higher the risk of the patient, the more we have to plan and find individualized plans and solutions. In selected cases, part of the plan might be to place prophylactically a percutaneous transtracheal catheter. However, we do not suggest doing this for every odontogenous abscess and we should not neglect complications of this technique and the difficulties we might have in some patients (with distorted anatomy) in placing such a catheter.

It is also true that we have to make a thorough plan for extubation, but this was not the focus of our study.

Overall, we can agree to the conclusion of Cassel and Ball that videolaryngoscopy may be safe and effective in this challenging group of patients in the context of a robust, reliable airway management strategy.

Declaration of interest

None declared.

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End-expiratory occlusion test: please use the appropriate tools!

Editor—We would like to comment on the article ‘End-expiratory occlusion manoeuvre does not accurately predict fluid responsiveness in the operating theatre’ published by Guinot and colleagues.1 In this study, the authors carry two main messages. The first is that the changes in stroke volume, measured by the CardioQ device during an end-expiratory occlusion (EEO) test, cannot predict fluid responsiveness, as assessed by the same device. The second message is that the changes in end-tidal carbon dioxide (etCO₂) during an EEO test do not predict fluid responsiveness either. These two messages are wrong for methodological and conceptual reasons.

Concerning the changes in stroke volume measured by oesophageal Doppler during the EEO test, the authors missed the physiological point that the CardioQ device is unable to correctly track the changes in stroke volume induced either by EEO or by volume expansion. Indeed, this device does not measure the changes in aortic diameter that physiologically accompany the changes in arterial pressure. Neglecting these changes, the device underestimates the changes in cardiac output resulting from changes in preload. This has been clearly demonstrated.2 This issue might have particularly influenced the findings in this study, given the changes in arterial pressure observed during EEO and after volume expansion.

Note that eventually, the EEO-induced changes in stroke volume predicted fluid responsiveness with an AUC under the ROC curve of 0.78 (95% confidence interval: 0.63–0.89). In fact, this result should be considered as fair (as declared by the authors themselves in the Results section, by the way), considering the methodological drawbacks of the test.

More importantly, the authors attempted to test the reliability of the EEO-induced changes in etCO₂. This concept does not make sense to us. Changes in etCO₂ can reflect the changes in cardiac output only if the ventilation is stable. Of course, interrupting mechanical ventilation for 15 s stops carbon dioxide...