Reply from the authors

Editor—We thank Drs Shelly and Kinsella for their comments on our study.1 We are pleased to respond to the questions they raise.

1. Whether a more exact and precise definition of what constitutes a complication and morbidity should have been defined before commencement of the study.

As this was a prospective observational rather than an interventional study, the data collection sheet was designed to record current anaesthetic and surgical practice. We did not wish to tell thoracic anaesthetists in advance what we were interested in but left the question open in order to determine a definition of complications. We believe that this allowed a much wider collection of data than if we had specifically listed a set of arterial pressure values or heart rate values which in themselves would be open to debate and speculation as to their appropriateness and validity.

2. The wording of how major complication was defined in this paper compared with the original study publication.

We do not believe that ‘death within 30 days, treated cardiac arrhythmia or hypotension, unplanned intensive care admission, further surgery or inotrope usage’ is materially different from ‘significant arrhythmias requiring anti-arrhythmic, noteworthy haemodynamic instability requiring inotropes, severe respiratory complications requiring mechanical support, unexpected ICU admission, further surgery or 30 day mortality’ or that this constitutes an expansion of the criteria. In addition, this was not done in a subtle attempt to increase the net of major complications and this is borne out in their own conclusion that this made no difference to the actual reported rates of major complications. As stated, we did not specifically wish to ‘tie down’ thoracic anaesthetists to rigid definitions of what we defined constituted a major complication, so allowed them to decide what constituted a complication and we do not believe that this constituted a change in the original aims and objectives of the study. Once the study was completed, we were in a position to note the complications that occurred commonly, and these were grouped together in the composite outcome defined as major complications.

3. The final point specifically addresses the question of inotrope use.

We accept that this, in the large majority but not all cases, would mean the use of vasopressor agents rather than positive inotropic agents. No predefined arterial pressure was determined for significant hypotension requiring inotropic/vasopressor agents. This in itself would raise questions of how that level was defined and comparing whatever value was chosen to the patients pre-existing ‘normal’ arterial pressure. The final point raised relates to the ‘normal’ use of vasopressor agents in anaesthetic practice and speculates on whether this alone could contribute to the difference in the rates of major complications. Inotrope use was the lowest of the composite endpoints within major complications and therefore we do not believe a subset of anaesthetists using vasopressor agents as part of their normal practice would materially affect the outcome of the study. This is further supported by the strong links between the development of major complications and other factors such as age and ASA scoring, none of which would be affected by a variation in normal practice.

As we clearly stated in our publication, this is not a randomized controlled trial, and, therefore has the inherent weakness of an observational cohort study. The study alone does not allow a definitive conclusion to be drawn, such that total UK anaesthetic practice needs to be changed. However, we firmly believe that our study will form an important part of the basis for a future large randomized controlled trial to be conducted to provide a definitive answer to this clinically and economically important question.

Conflict of interest

None declared.

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doi:10.1093/bja/aer134

Jet ventilation and the completely occluded airway

Editor—We were impressed by the ingenuity of the ventilation ejector (DE 5) for applying expiratory ventilation assistance during jet ventilation that was recently described by Hamaekers and colleagues.1 Some years ago, we performed a similar benchtop experiment using a 14 G cannula connected to a three-way tap with the two other ports being connected to a Sanders injector and wall suction, respectively. We demonstrated that minute volumes of 10 litre min⁻¹ could be consistently achieved in a model lung with a simulated completely occluded airway by alternating between jet ventilation and suction-assisted expiration.²

Although we acknowledge that purpose-built devices have some advantages over cobbled-together equipment, this self-assembled equipment is not complicated to put together, is extremely inexpensive, and the concept is simple to grasp even in an emergency setting. We would be very interested to see how the DE 5 compared with the self-assembled equipment we describe in terms of minute volumes achieved and user-friendliness.

*Conflict of interest

None declared.
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doi:10.1093/bja/aer135

Reply from the authors
Editor—We thank Drs Waddilove and Kessell for their comments on our study.1 The letter published by Kessell in 19992 confirms some of our findings: in a completely obstructed upper airway, insertion of a second small-bore cannula for expiration is senseless if used for passive backflow, and suction can efficiently aid expiration. We wholeheartedly agree with the statement by Kessell that an active expiratory phase may help, by adequate minute volume ventilation, to bridge the time until a definitive airway can be established.

Several devices providing expiratory assistance have been published since introduction of the concept by Eger and Hamilton3 in 1958. In his letter, Kessell describes a simple system with a Sanders injector and suction tubing both connected to a transtracheal cannula by a three-way stopcock. This actually resembles a more sophisticated setup intended for small lumen ventilation introduced by Schapera and colleagues4 in 1994. They also proposed separate oxygen and suction tubing with injection and suction pressures and also inspiration and expiration times controlled by a computer.

In a can’t intubate, can’t ventilate situation, time is limited and, preferably, any device to be used should be readily available, easy, and, of course, safe to use. In the case of upper airway obstruction, a flow-regulated device (such as the DE 5) is safer compared with a pressure-regulated tool (such as a Sanders injector) because the injected volume of oxygen can easily be estimated. The use of a Sanders injector driven by wall pressure results in injection of highly compressed oxygen which will expand in the patient’s lungs.

The minute volume achievable with the self-assembled system proposed by Kessell might be higher compared with the DE 5, but at the price of using wall pressure (up to 5 bar) for inspiration and maximum suction pressure (up to −0.8 bar). Actually, for safety reasons, the DE 5 was designed to allow an adequate minute volume at much lower injection and suction pressures (typically below 0.15 bar injection and −0.4 bar suction pressure).

Conflict of interest
The research done in the manuscript was supported by funding from the European Union, OP-Zuid [31R104]. D.E. is the inventor of the Oxygen Flow Modulator (OFM) and receives royalty payments from Cook Medical. D.E. has applied for a patent on the DE 5.

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doi:10.1093/bja/aer136

Two curves theory does not clearly explain laryngoscopy and intubation
Editor—I congratulate Greenland and colleagues5 for their exploration of the mechanisms of direct laryngoscopy and intubation. Presentation of such findings contributes to a more complete understanding of a central procedure of airway management. However, the use of the curves theory does not clearly relate to the ease of laryngoscopy and intubation.

They state that ‘First successful laryngoscopy requires that the primary curve and the laryngeal vestibule align with the line of sight’.1 The primary curve described is related to the hard and soft palate. It is anatomically impossible for the primary curve to align with the line of sight and the laryngoscope does not align or affect the primary curve. The alignment of the described curves does not explain the mechanism of laryngoscopy and intubation. It is the tongue which blocks visualization of the glottis and the laryngoscope displaces the tongue.

The primary and secondary curves are anatomically non-continuous structures. The oesophagus separates these two curves. Furthermore, the two curves are not closely linked biomechanically. The primary curve (related to the head) extends on cervical vertebrae, and not the larynx or trachea (secondary curve). It is doubtful that the point of inflection described corresponds exactly to the same point of the laryngeal vestibule at all head positions.

In the calculation of the area posterior to the line of vision, the authors confuse the outline of the tongue with the primary curve. As demonstrated by their quoted pictures from Adnet and colleagues,1 2 the tongue does not always correlate with the primary curve. This confuses the explanation even further. The relationship between the alpha angle and ease of direct laryngoscopy is not directly intuitive.