Reviewer’s report

Title: The Role of Rigid Indirect Videolaryngoscopy in the Successful Orotracheal Intubation of Adults: A Systematic Review of Randomized and Non-Randomized Trials.

Version: 2 Date: 11 July 2012

Reviewer: Konstantinos Stroumpoulis

Reviewer’s report:

The study by Healy et al is a very interesting study which I enjoyed reading. This study presents some significant findings with probably important implications on the future use of videolaryngoscopes. However, several significant questions which have to be answered before any consideration for acceptance are raised.

My comments are the following.

Major Compulsory Revisions

Introduction


Methods

2. Page 7 para 2: The limitation of using such a strict measure of glottic view improvement is the risk missing a lesser, but perhaps clinically significant improvement in glottic view afforded by device use, for instance an improvement from a grade 3 to a 2a view of the glottis. It must be noted that the concept of using a full view of the vocal cords as a desirable outcome measure when comparing direct laryngoscopy with the variety of methods of indirect laryngoscopy is questionable. These grading schemes are designed and validated for direct laryngoscopy only… Hence you did not assess the overall improvement in glottic view described by many studies. Would you suggest that a C/L IV turned into a C/L IIA by IVL does not improve glottic view and therefore does not probably create better conditions for a successful intubation? I am well aware (as the majority of your readers) that a good glottic view does not necessarily equals an easy intubation, on the other hand you are making a long logical jump by accepting that all C/L grades greater than I provide unfavourable intubating conditions. I am certain that by making this assumption you missed a...
significant number of clinically significant improvement in glottic views. This constitutes a major limitation of your study. All data is available to you. Can you provide data concerning the performance of each IVL and the comparison between devices? If you have an IVL that converts C/L IV to C/L IIa in a significantly higher rate than the others this is important to know. An evidence-based review would have assessed these differences which are not subtle at all.

Page 8 Data extraction

3. The only measure you included for the prediction of a normal or a difficult airway was the Mallampati score. It is very well known that it has a low predictive validity and that is why other scores have been developed (El Ganzouri multivariate risk index, upper lip bite test etc). By using only Mallampati as a predictive tool you possible lost some other significant studies focusing on difficult airways such as the following:


Please consider including them.

4. On the other hand, you have included several manikin studies. Despite manikin studies may form an attractive choice for investigators for a number of reasons, particularly with respect to obviating the need for careful patient selection and recruitment, but also in terms of proving a safe, standardized and reproducible environment that will likely yield reliable and comparable data, several concerns are raised with the growing number of studies that are initially assessing airway management devices in a simulated setting and argue that many of the aforementioned reasons limit the degree to which finding generated are applicable and generalizable to human populations


Therefore there is a high probability that your sample is highly heterogeneous and your results not something that one could say representative. Please add this observation to your limitations section.

Minor Essential Revisions

Introduction
1. Page 4 para2: Indirect videolaryngoscopy (IVL) is a recent development. I would not characterize as a recent development a device that has been developed more than 10 years ago. Please rephrase.

2. Page 4 para2: This may be incorrect as an improvement in success may be limited by both use of unfamiliar equipment...please rephrase: unfamiliarity with equipment and difficulty in advancing the ETT. and difficulty placing an endotracheal tube out of the line of sight. However, this is not the case in IVLs since the ETT is placed under continuous direct vision (as all manufacturers suggest). I wonder whether the authors have ever used any of these devices. Please erase this comment.

3. Page 5: The definition of difficult laryngoscopy was broad and included all

4. Page 5: as many patients can easily be intubated blindly...do you mean by conventional means? (with a bougie, an ILMA, a fastrach etc?). Please elaborate.

Page 11
5. The evidence for efficacy....
there are no accepted or published success rates of direct laryngoscopy...
I beg to differ. There are studies. See my previous comment on the studies by Stroumpoulis and Marrel.

Page 13.
6. Of note, the evidence for the GlideScope is limited to a small study where the view was improved to a Cormack and Lehane view 1 in only 8% (4 patients).
I beg to differ.
Please search the literature a little more! See the studies by Stroumpoulis and Marrel. Please also consult the following study: Rope TC, Loughnan BA, Vaughan DJ. Videolaryngoscopy: an answer to difficult laryngoscopy? Eur J Anaesthesiol 2008; 25:434–435.

7. Discussion
Please relate your evidence with other relevant reviews.
Please comment more on your findings.

8. Overall comments
I believe that the use of the terms “recommend”, “recommendation” is rather strong especially when it is not coming from an executive committee or a working group. Please consider using another term.

9. Please include the aforementioned limitations throughout my comments to the appropriate parts of your limitations section.
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:  
I declare that I have no competing interests.