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: 29 June 2012

Reviewer: Richard Cooper

Reviewer's report:

On the whole, the paper is interesting, topical, timely and well-written. The authors are to be congratulated on undertaking the task of reviewing and organizing a large body of literature. The objective is well defined and the methods are appropriate.

The proposed clinical categories are reasonable. I'm not sure if it wouldn't have been better to look at studies reporting “consecutive patients” rather that “unselected” ones. They may be the same but the latter has a greater potential for reporting bias, always a challenge when evaluating new devices, frequently reported on by enthusiasts.

Do the authors have any recommendations for future evaluative studies on this emerging technology?

Discretionary revisions:

I am not convinced that “Rigid Indirect Videolaryngoscopy” adds much to the simpler and more commonly used “Videolaryngoscopy”.

Suggest modifying the sentence: “When these conditions are not met, for example...the failure rate of conventional direct laryngoscopy increases.” to “...the failure rate of intubation with conventional direct laryngoscopy increases.”

The use of videolaryngoscopy makes it clear that we must distinguish between seeing the target and successfully inserting the endotracheal tube. It is important to continually reinforce that distinction with precise terminology.

The statement regarding the best available evidence provided by Shiga et al. could be further clarified regarding the patients constituting their denominator. In their meta-analysis, they excluded patients whose airways were “anatomically abnormal” or in whom DL was deemed inappropriate.

Page 5: I would recommend rephrasing the sentence: “However, even given this broad definition of difficult laryngoscopy this still suggests an impressive overall success rate for [intubation by] direct laryngoscopy of over 95%” since laryngoscopy fails if it does not reveal the larynx, even though intubation may be successful. Furthermore, the statement that the standard is high should relate to the population investigated--in this case, patients who were deemed to be
suitable for DL. A relatively small proportion of patients would likely have been eliminated because their airways were managed otherwise (bronchoscopically, surgically, retrograde etc.) or intubation was avoided.

English language or easily accessible translation requires clarification.

Please confirm that the Storz DCI, V-MAC and C-MAC were regarded as identical devices. This is unclear because the term Storz Macintosh video laryngoscope is used but the company refers to this product as the V-MAC or the Video Macintosh.

Under Data Extraction: would suggest that Number of true difficult laryngoscopy be changed to “Number of difficult direct laryngoscopies (C&L > III)”

Page 10: Difficult Direct Laryngoscopy: this term was used for patients with C/L> III. I prefer the term “Failed Direct Laryngoscopy” because laryngoscopy failed to provide a view of the larynx. Difficult DL is commonly used, but I believe that it is used inappropriately. Please consider rephrasing this. “Failed Direct Laryngoscopy” really should be referred to as “Failed Intubation by DL.”

Minor essential revisions:

In the Methods, the authors should explicitly state whether the GlideScope Direct, the McGrath MAC or the C-MAC D-blade were included in/exclude from the study. Although one could take exception to the inclusion or exclusion of a wide variety of devices, I’m not clear why the Bullard Laryngoscope was included in this analysis. (It could be argued that if it was included, the search should have included similar devices such as the WuScope and the Upsher Scope, both of which would probably have been subsequently eliminated because of an insufficient number of recent publications.)

There appears to be an orphaned * after (until April 2011).

Page 8: First attempt is repeated twice on the last line of Data Extraction.

Page 9, line 3: separately is misspelled.

Maharaj has been misspelled on Table 3 (2nd and 3rd entry).

Page 9, “Other studies have suggested even lower PPVs for the Mallampati score” but only a single study is cited. Suggest including other studies or changing this to “another study”.

The paragraph “Evidence for the use of video laryngoscopy in unselected patients” is the perfect place to reinforce the distinction between improvement in rates of successful laryngoscopy and rates of successful intubation.

Suggest changing “Evidence for the use of VL in pt assessed to be at high risk of difficult laryngoscopy” to “Evidence...high risk of difficult direct laryngoscopy” and the same for the following sections. Where there are references to “suspected difficult laryngoscopy” and “true difficult laryngoscopy”, these should include the device used (e.g. Direct or even better, Macintosh DL although the latter is often
assumed rather than stated).

Major compulsory revision:

Page 7: Many of the comments in the paragraph Grading the view at laryngoscopy belong in the Discussion, rather than Methods. “Unfortunately the other grades of laryngoscopic view[s]...” “the concept of using a full view of the vocal cords as a desirable outcome measure when comparing direct laryngoscopy...is questionable. These grading schemes are designed and validated for direct laryngoscopy only; however, this measure is used throughout all of the studies as no alternate scheme exists.”

Consideration of the Mallampati grade as the sole predictor of a normal or difficult laryngoscopy is both inappropriate and arbitrary a point that the authors themselves state on the following page. I think that Shiga’s paper indicates that Mallampati alone lacks sensitivity and specificity in predicting an adequate laryngeal view by DL, yet more complex scores don’t perform much better. The authors have made a compromise which is justifiable but should be acknowledged as a limitation.

The proposed classification of IVL is reasonable but in the opinion of this reviewer, there is a potential advantage to distinguishing between VL with Macintosh style blades (C-MAC, McGrath MAC, GlideScope Direct) and those with angulated blades (GlideScope, McGrath Series5, Storz D-blade). It is possible that their performance is significantly different with respect to the laryngeal view and the ease of intubation. Further, this may be quite different for the four categories.

Both the Methods and Results sections contain a good deal of information that more appropriately belongs in the Discussion. The Methods should state what was done (not why) and Results should state their findings, not the reasons for the comparisons, attempts to explain them or conclusions drawn from them.

Discussion:

It is difficult to believe that the performance of these devices was assessed in appropriately experienced hands. In reviewing the literature, the competency of the operators is rarely stated and the number of prior uses does not adequately address skill level. Furthermore, there is often an assumption that an experienced anesthesiologist or laryngoscopist can be regarded as an experienced operator despite limited familiarity with a different technique. I believe that this is in fact a serious limitation of any such analysis and should be so stated. The same applies to recommendations of how this information should be used. The difference in outcomes relating to experience is highlighted in another study by the authors (26) where the results differed between two institutions involved in a trial.

Page 16: The comments regarding the description of laryngoscopy/intubation are pertinent not only to VL but all airway maneuvers. It can no longer be assumed that intubation is performed by a Macintosh blade and accordingly, it is important
to always state what devices were used, what view was obtained, what adjuncts were employed and how many attempts were required. Perhaps Adnet’s Intubation Difficulty Score should be advocated. It is probably just as relevant to VL as DL. Terms such as easy and difficult are somewhat subjective.

Reference 25 has been superseded by reference 13. I don’t think that there is a need to reference both the 1993 (25) and the 2003 (13) Practice Guidelines.

**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 was a former investor in the company that manufactured the GlideScope and have received travel subsidies from Verathon. I have not received honoraria or payment for consulting services. I do not feel that I have a COI.