Reviewer’s report

Title: Comparison of videolaryngoscopy and direct laryngoscopy by German paramedics during out-of-hospital cardiopulmonary resuscitation. An observational prospective study.

Version: 0 Date: 04 Nov 2019

Reviewer: Jeffrey Jarvis

Reviewer's report:

The authors of this observational study describe the difference in glottic view and intubation success among German paramedics during cardiopulmonary resuscitation. This is a timely topic as various video laryngoscopes are hitting the market at prices affordable to many EMS agencies. Their conclusion was that glottic view was better with glidescope VL (GVL) while first pass success (FPS) is not different.

I have two primary concerns with this study and several editorial concerns. First, I'm not clear that this paper significantly adds to our body of knowledge on this topic. There are many papers that have studied the same thing and come to the same conclusion. To the best of my knowledge, the population of German paramedics is a novel aspect of this paper, however. Perhaps the authors could focus their manuscript on how this aspect is important to the results?

Second, I feel the authors outcomes are in reverse order of importance. Glottic opening is, at best, a surrogate marker for ease of intubation. It is only important to the extent that it may impact intubation success. This paper directly measures intubation success, therefore glottic opening is not the most important measure studied. While it may be interesting and useful to include, intubation success is far more clinically important.

Finally, I feel there is substantial literature already demonstrating that video laryngoscopy improves glottic opening. The literature is mixed on whether it improves intubation success, particularly FPS. I've added several citations from my collection at the end of this review that I'm familiar with that may help make this point.

I'll make further, more minor editorial comments to follow.

Introduction. The readability is quite difficult, and I would recommend heavy editing with a focus on establishing one or two main points.

Page 5, line 9-11. This line is out of place relative to the rest of the introduction and adds confusion while interrupting the readability of the document.

Page 5, line 11-13. I do not believe these claims can still be made in the aftermath of the AIRWAYS-2 and PART trials, both large and well-done RCTs directly comparing cardiac arrest outcomes with ETI compared with iGel and King LT, respectively.
Page 6, line 7-8. You mention endobronchial intubation, but I believe you mean endotracheal.

Page 6, lines 11-12. I recommend rewording your goal. It currently reads like you are looking for evidence to justify a bias, but I don't think that's your intent. I would recommend simply stating you are investigating the effect of video vs direct laryngoscopy by paramedics in cardiac arrest on FPS and glottic opening.

Methods

Page 6, lines 19-23. I am not clear on how the four GVL devices were distributed throughout the system. Was it at random, based on convenience, or was their some type of schedule to try and assure even experience with it throughout the system?

Also, it isn't clear why a paramedic would choose VL vs DL. "Intubation should be performed… depending on availability". Were both DL and VL available simultaneously when VL was available? If so, was it at the paramedic's discretion which to use or was the protocol that VL was mandatory for first attempt when available? This section is important in understanding how the two groups differ. This is particularly important given that hyperacute angled devices, like the GVL, require a different technique than DL, making training much more important.

You have several repetitive sentences throughout the document.

More description is needed on the contents of the training. Specifically, the authors should address how the technique differs between GVL and DL and whether they were trained to use a rigid stylet with the GVL.

It isn't clear what the temporal relationship between training and availability of the device was. This is important because if a medic was trained on the device but then didn't have an opportunity to use it for several months or longer, the effect of the training would be substantially lessened. Finally, was there any post-training evaluation performed to determine medic competency prior to trial initiation?

The authors mention a new SOP and instruction manual. It would be helpful to understand the contents of these documents.

The definition of an intubation attempt is not clear. If the medic inserted the blade but did not attempt to pass a tube, was that an attempt? A relatively standard definition of an attempt is anytime a blade passes the teeth, regardless of attempt to pass the tube. This removes substantial ambiguity. How were the paramedics trained on the definition of an attempt?

How was success determined? It appears it was likely self-reported by the medic on post-event documentation. Was there any independent confirmation, such as review of waveform EtCO2 by the investigators? Was there independent confirmation by the EMS physician arriving on scene or in the ED?
What data was captured and available to the authors? Was the only data source the questionnaire or was the medical record interrogated? Was there any waveform review?

Results:

In general, it is unnecessary to repeat all results in both prose and table. For these results, the tables are a much more efficient means of communication. I would include only the main findings (no difference in FPS and improved glottic view with GVL) and refer the reader to the figures and tables.

Figure 1 indicates 134 CRFs returned. How many patients underwent intubation without a corresponding CRF returned? This is important because it gives the reader an idea of whether there might be a bias of the medic to only return CRFs for successful encounters.

Discussion

Your lead sentence is not supported by your evidence, in fact, the exact opposite could just as easily be true.

I again have difficulty following the prose in this section. There are several redundant sentences and several times there are sentences referencing a paper without any indication of why it might be relevant to the current paper.

The conclusion should be more concise and not include commentary or potential explanations that are better located in the discussion. Finally, the conclusion should not include any statements not supported by the current paper. The final sentence, for example, is unsupported.

I would suggest a better conclusion is something like "We found no difference in FPS between GVL and DL during CPR by paramedics despite better glottic visualization with the GVL".

Bibliography:

VL improves view but doesn't improve FPS:


VL better than DL in FPS:


VL less compression interruptions than DL:


Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

**Quality of written English**
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

**Declaration of competing interests**
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal