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Review Article

Videolaryngoscopy versus direct laryngoscopy for adult patients requiring tracheal intubation: a Cochrane Systematic Review†

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†This review is an abridged version of a Cochrane Review previously published in the Cochrane Database of Systematic Reviews 2016, *Issue* 11, *DOI: CD011136 (see* www.cochranelibrary.com *for information)*.[1] Cochrane Reviews are regularly updated as new evidence emerges and in response to feedback, and Cochrane Database of Systematic Reviews should be consulted for the most recent version of the review.

Abstract

Difficulties with tracheal intubation commonly arise and impact patient safety. This systematic review evaluates whether videolaryngoscopes reduce intubation failure and complications compared with direct laryngoscopy in adults. We searched CENTRAL, MEDLINE, Embase and clinicaltrials.gov up to February 2015, and conducted forward and backward citation tracking. We included randomized controlled trials that compared adult patients undergoing laryngoscopy with videolaryngoscopy or Macintosh laryngoscopy. We did not primarily intend to compare individual videolaryngoscopes. Sixty-four studies (7044 participants) were included. Moderate quality evidence showed that videolaryngoscopy reduced failed intubations (Odds Ratio (OR) 0.35, 95% Confidence Interval (CI) 0.19-0.65) including in participants with anticipated difficult airways (OR 0.28, 95% CI 0.15-0.55). There was no evidence of reduction in hypoxia or mortality, but few studies reported these outcomes. Videolaryngoscopes reduced laryngeal/airway trauma (OR 0.68, 95% CI 0.48-0.96) and hoarseness (OR 0.57, 95% CI 0.36-0.88). Videolaryngoscopy increased easy laryngeal views (OR 6.77, 95% CI 4.17-10.98) and reduced difficult views (OR 0.18, 95% CI 0.13-0.27) and intubation difficulty, typically using an 'intubation difficulty score' (OR 7.13, 95% CI 3.12-16.31). Failed intubations were reduced with experienced operators (OR 0.32, 95% CI 0.13-0.75) but not with inexperienced users. We identified no difference in number of first attempts and incidence of sore throat. Heterogeneity around time for intubation data prevented meta-analysis. We found evidence of differential performance between different videolaryngoscope designs. Lack of data prevented analysis of impact of obesity or clinical location on failed intubation rates. Videolaryngoscopes may reduce the number of failed intubations, particularly among patients presenting with a difficult airway. They improve the glottic view and may reduce laryngeal/airway trauma. Currently, no evidence indicates that use of a videolaryngoscope reduces the number of intubation attempts or the incidence of hypoxia or respiratory complications, and no evidence indicates that use of a videolaryngoscope affects time required for intubation.

Key words: anaesthesia; hypoxia; intubation; laryngoscopes

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Editor's key points

- The authors examined the evidential support for the hypothesis that videolaryngoscopy reduces the incidence of intubation failure and other complications.
- They found evidence supporting a reduction in the incidence of intubation failure when using a videolaryngoscope, particularly in the context of a difficult airway.

Recent UK data suggest that tracheal intubation is used for airway management in 38.4% of general anaesthetics, estimated at 1.1 million procedures per yr.[2] Intubation with direct laryngoscopy, requires flexing the lower cervical spine and extending the upper cervical spine to create a 'line of sight', and a Macintosh blade is commonly used to retract the tongue to enable passage of a tracheal tube. Failed or difficult intubation is associated with complications, including increased risk of hypertension, desaturation, unexpected admissions to the intensive care unit (ICU) and death.[3–5] Such difficulties during routine intubation occur in 1–6% of cases and failed intubation in 0.1-0.3% of cases[6 7] but are much more common in ICU and the emergency department.[8] Intubation difficulties may arise from restrictions in neck flexion, narrow jaw opening, enlarged tongue, poor tissue mobility, or cervical instability and, in the UK, the 4th National Audit Project (NAP4) showed that delayed and failed intubation were important precursors of major airway complications.[8] A recent large observational cohort study identified 93% of difficult intubation as unpredicted[9]; and predictive tests, for example the Mallampati or Wilson index test[10 11] have low sensitivity and positive predictive value.[7]

Alternatives to the Macintosh blade rely on fibreoptic or digital technology to transmit an image from the tip of the laryngoscope to an eyepiece or monitor, where it is viewed by the intubator. For this review, we are interested in rigid videolaryngoscopes, which use a blade to retract the soft tissues and transmit a video image to a screen attached to the end of the handle or to a monitor. This design enables a lighted view of the larynx without direct 'line of sight' and can therefore assist when difficulty is encountered (or predicted) with direct laryngoscopy. Studies suggest that use of a videolaryngoscope improves the view of the larynx during laryngoscopy[12 13] and videolaryngoscopes therefore provide the possibility of more successful intubation for patients in whom direct laryngoscopy is difficult. They also may be used after unsuccessful attempts to intubate with direct laryngoscopy. Whilst the use of videolaryngoscopes may aid visualisation, evidence is required to establish if this equates with increased success of tracheal intubation with reduced complications. Our primary objective was to assess whether videolaryngoscopy for tracheal intubation in adults reduces the risk of complications and failure compared with direct laryngoscopy. Our secondary aim was to assess the benefits and risks of these devices in selected populations, such as adults with obesity, critically ill patients in the ICU and emergency setting, and those with a known or predicted difficult airway. We did not intend to compare video devices directly. The finished work was published in the Cochrane Library in 2016[1]; an abridged version is presented here, with the full detailed review being available on line for further reference.

Methods

Protocol

This paper reports an abridged version of a previously published Cochrane systematic review,[1] itself based on a protocol previously published in the Cochrane Database of Systematic Reviews.[14] We prepared this manuscript according to guidelines published by Cochrane,[15] the PRISMA statement for systematic reviews and meta-analysis,[16] and the *British Journal of Anaesthesia* guidelines.

Information source

We searched the Cochrane Central Register of Controlled Trials (CENTRAL, Issue 2, 2015), MEDLINE (1970 to February 2015) and Embase (1980 to February 2015). We applied the Cochrane highly sensitive filter for randomized controlled trials in MEDLINE and Embase. We searched the trial registry database www.clinicaltrials.gov (accessed 19 August 2014) for ongoing trials. We carried out forward citation tracking of all included studies and backward citation tracking of identified review articles. We used no restriction on language of publication. The search strategy used for MEDLINE can be found in the review protocol.[14]

Eligibility criteria

We included randomized controlled trials (RCTs), with a parallel or cross-over design that compared the use of any model of videolaryngoscope with a Macintosh blade in participants aged≥16 yr who required tracheal intubation during general anaesthesia.

Data collection and analysis

Two review authors independently screened titles and abstracts of search results to remove irrelevant studies. Two review authors then reviewed full texts of potentially relevant titles and identified studies that matched inclusion criteria. Data on study characteristics and outcomes were independently extracted from eligible studies by two of three investigators, to include data for the following outcomes.

Primary outcomes

1. Failed intubation or change of device required (failure as defined by the study authors)
2. Hypoxia between start of intubation and recovery from anaesthesia

Secondary outcomes

1. Mortality within 30 days of anaesthesia
2. Serious airway complications, including aspiration, within 30 days of anaesthesia
3. Laryngeal or airway trauma, including any one of damage to vocal cords, bleeding or dental injury
4. Patient reported sore throat: early (within two h of anaesthesia) and late (within 48 h of anaesthesia)
5. Hoarseness: early (within two h of anaesthesia) and late (within 48 h of anaesthesia)
6. Proportion of successful first attempts at tracheal intubation
7. Number of attempts at tracheal intubation

8. Total time for intubation and commencement of ventilation
9. Difficulty of intubation: assessed by observer or intubator, using locally derived or validated scales
10. Improved visualisation of the larynx: assessed using a validated classification system.[12 17 18]

Risk of bias within studies

We used the Cochrane risk of bias tool to assess the quality of study design and extent of potential bias and considered the following domains: sequence generation, allocation concealment, blinding of participants, personnel and outcome assessors, incomplete data, and selective outcome reporting.[15] It was not possible to blind the intubator to the intervention, nor to blind assessors of process measures. However, blinding of patients and post-intervention outcome assessors to the type of device was feasible.

Summary measures and synthesis of results

Data were analysed using Review Manager, version 5.3.[19] For dichotomous outcomes (e.g. failed intubation, hypoxia, mortality) we calculated odds ratios (OR) with 95% confidence intervals (CI). For continuous measures (time for intubation) we calculated mean differences (MD). We recorded some outcomes in short ordinal scales (i.e. number of attempts, intubation difficulty scores and scales of improved visualisation) and converted these to dichotomous data where appropriate. For multi-arm studies, we used an amalgamated comparison group (combining all videolaryngoscopes) compared with the control group, to create a single pair-wise comparison.[15] When it was not possible to amalgamate data without unit of analysis error, we included data from the videolaryngoscope group that would be closest to giving a result of 'no effect'; these decisions were then addressed in sensitivity analysis.

We carried out meta-analysis for outcomes for which we had comparable effect measures from more than one study and where measures of clinical, methodological and statistical heterogeneity indicated that pooling of results was appropriate. We classified levels of statistical heterogeneity using the I^2 statistic according to Higgins.[15] We considered that I^2 values<40% would not indicate important heterogeneity and above 75% would be substantial.[15] Our choice of a fixed-effect or random-effects statistical model for any meta-analysis was influenced by study characteristics, in particular the amount of methodological or clinical differences between studies. We used Mantel-Haenszel models for all dichotomous outcomes. For the continuous outcome, we used the inverse variance method.

We aimed to perform subgroup analyses to assess if results of meta-analyses differed according to: different designs of videolaryngoscope; anticipated or known difficult laryngoscopy; experience of intubator (an 'experienced' operator had to have performed at least 20 intubations with the devices); obese and non-obese participants; and the site of intubation (operating theatre, emergency department or the ICU). We performed sensitivity analyses to explore the impact of missing data on our results and decisions made during risk of bias assessment and analysis of data.

The quality of the evidence for each or our outcomes was assessed using the GRADE system.[20] A full account of how this was performed, and why evidece was downgraded, is in the original Cochrane version.[1]

Results

Study selection and characteristics

We identified 4920 titles and abstracts from database searches (10th February 2015) and through forward and backward citation tracking. After removal of duplicates, we screened 3412 titles and abstracts and assessed 275 full texts for eligibility. We identified 64 RCTs (with 7044 participants) to include in the review (Fig. 1).[21–84] Some designs of laryngoscope can be used with and without a camera attachment (such as Airtraq and Truview EVO2) and we excluded studies if direct vision without the camera attachment and separate screen was used, or in which it was unclear from the published report if the camera device and screen had been used; excluded studies are reported in the full version of the Cochrane review.[1] We identified five abstracts for which there was insufficient information,[100–105] and three full texts which required translations which we were unable to perform.[105–107] Through our clinical trial register searches, we also identified seven ongoing studies,[85–91] and a further eight studies for which data had not yet been published.[92–99]

One study took place in the ICU,[38] one in an emergency department,[84] and one in an out-of-hospital setting,[26] all with participants requiring emergency treatment. The remaining 61 studies took place in the hospital operating theatre setting with elective surgical participants. Two studies specified inclusion of only obese participants,[21 23] one study included only obstetric participants,[25] one study only participants with untreated hypertension,[35] and one study only participants from the burns unit.[82] We included three studies that used a double-lumen tracheal tube for intubation.[28 34 69] All remaining studies used a single-lumen tube. Nine types of videolaryngoscope design were used in the 64 included studies: GlideScope (Verathon UK, Amersham UK), Pentax AWS (Pentax, Tokyo, Japan), C-MAC (including the DCI laryngoscope which was its predecessor) with Macintosh blade (Karl Storz, Slough UK), McGrath Series 5 (Aircraft Medical, Edinburgh, UK), X-lite (Rush, Tuttlingen, Germany), C-MAC D-blade (Karl Storz, Slough UK), Airtraq (Prodol Meditec, Guecho, Spain), Truview EVO2 (Truphatek International Ltd., Netanya, Israel), and CEL-100 (Connell Energy Technology Co. Ltd., Shanghai, China). Most studies compared the use of GlideScope, Pentax AWS, C-MAC Macintosh blade and McGrath Series 5. We identified 17 studies conducted by a cross-over design[31 32 34 36 40 44 52 53 56 60 65 67 68 71 72 78 80] and 47 studies with a parallel design. Those studies described by study authors as cross-over designs used one type of laryngoscope initially to assess glottic view, followed by the other type of laryngoscope to assess glottic view and perform intubation. The exception to this was one study, which intubated participants after laryngoscopy with each device.[40] Participants in both cross-over designs were randomized by different orders of laryngoscope.

Forty-seven studies included participants without a predicted difficult airway, and 15 of these used techniques to simulate a difficult airway for the purpose of the study. Six studies recruited participants with a known or predicted difficult airway, but others did not specify or included patients with both predicted and not predicted difficult airways. Forty seven studies specified that experienced anaesthetists performed laryngoscopies.[21–27 29–35 37 39 40 43–47 49–53 55 57–60 62–64 66–67 69 71 72 74 75 77 79 80 82 83] Five studies used anaesthetists who were described as novices or who were trained with manikins but had no patient experience.[38 41 42 78 81] Five studies used both novice and experienced anaesthetists.[28 48 54 68 84] Seven studies did not specify the experience of anaesthetists.[36 56 61 65 70 73 76] Detailed study

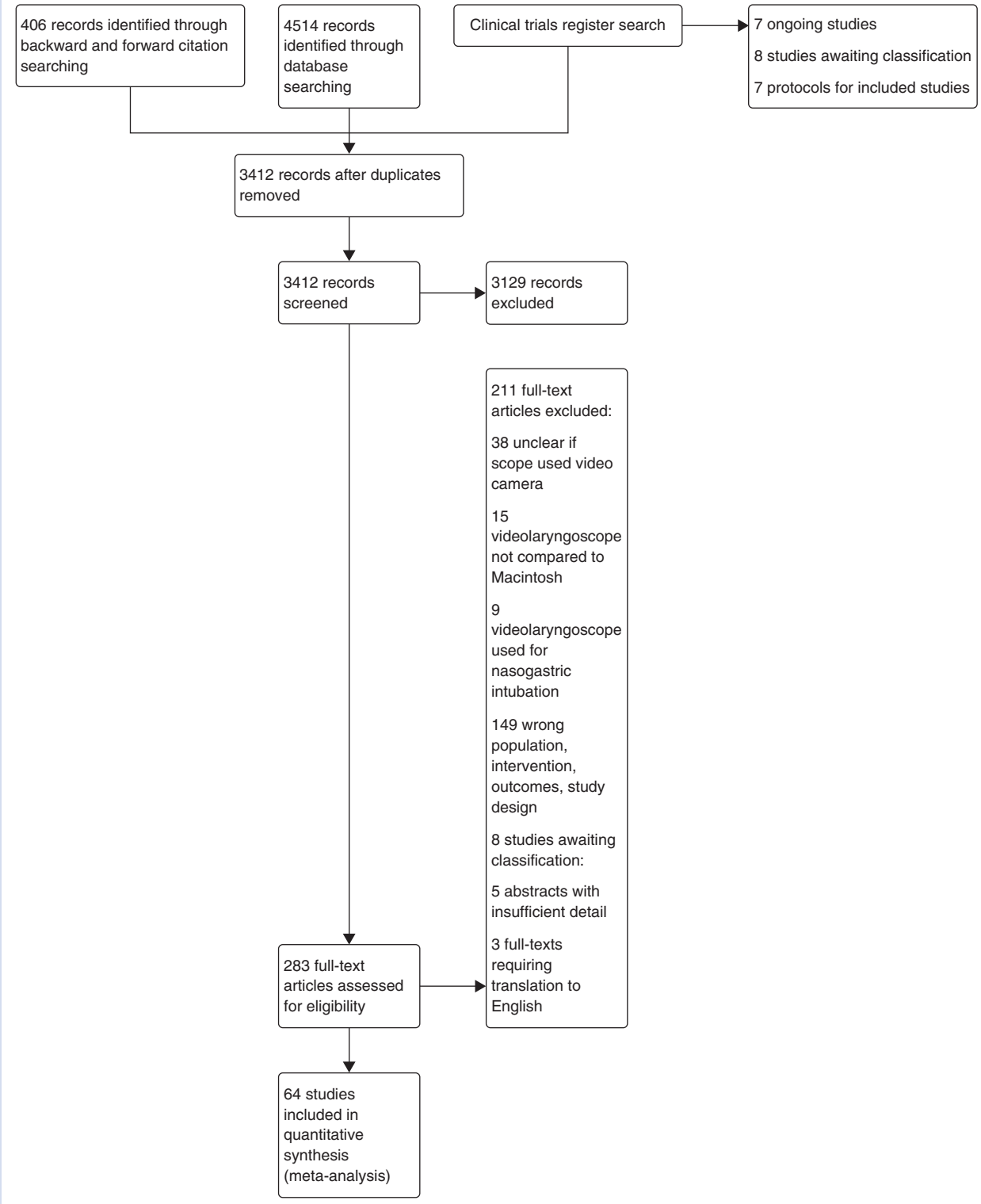


Fig 1 Flow chart of search strategy.

characteristics are reported in the full version of the Cochrane review.[1]

Risk of bias in included studies

All studies were described as randomized, with 36 studies providing sufficient detail of methods of randomization. Allocation concealment was poorly reported in studies. Few studies were prospectively registered with clinical trials registers and we were unable to make judgements on risk of selective reporting bias in unregistered studies. Performance and detection bias was high in all studies because it was not possible to blind the intubator and assessors of the primary outcome. There was a low risk of attrition bias in more than three quarters of studies and we were not concerned by influence of funding sources from videolaryngoscope manufacturers for most of the studies. We paid particular attention to whether the experience of the intubator in the videolaryngoscope and Macintosh group was equivalent within each study and believed there to be a low risk of bias for about 50% of studies. Study reports provided inadequate detail for many of our risk of bias criteria and therefore we were unable to make assessments for these studies (Fig. 2). A more detailed summary of the risk of bias per included study is presented graphically in the Supplementary Appendix S1.

Synthesis of results

Primary outcomes

Failed intubation. This outcome was defined within the review as the definition used by the study authors. The definitions are listed in the 'Table of Included Studies' in the published Cochrane version,[1] but typically included measures based on time (usually greater than 60 or 120 s) or on number of attempts (failure being usually defined as inability to intubate the trachea in two or three attempts). Thirty-eight studies with 4141 participants reported the number of failed intubations.[23–32 34 36 44 46 48 50 52–55 57–59 62 64–66 69 71 72 74 75 77–79 81–83] We excluded one crossover study[40] from meta-analysis which introduced too much performance bias to be equivalent to the others. Analysis demonstrated fewer failed intubations when a videolaryngoscope was used (OR, random-effects 0.35, 95% CI 0.19 to 0.65; I^2=52%; n=4127) (Fig. 3). Evidence from a funnel plot for this outcome suggested that there was no evidence of reporting bias (Supplementary Appendix S2).

Hypoxia. Eight studies reported on hypoxia,[23 27–29 50 55 71 79] and only three of these had event data.[27–29] Analysis showed no difference in hypoxia according to type of device (OR, random-effects 0.39, 95% CI 0.10 to 1.44; I^2=70%; n=1319) (Supplementary Appendix S3).

Secondary outcomes

Mortality. Only two studies reported mortality rates. One study[38] was based in the ICU and one[84] in the emergency department with no difference in the number of deaths according to type of device (OR, fixed-effect 1.09, 95% CI 0.65 to 1.82; I^2=29%; n=663) (Supplementary Appendix S4).

Serious airway complications. Only one study reported respiratory complications as an outcome with one recorded event of pneumothorax in the Macintosh group and none in the videolaryngoscope group.[30]

Laryngeal/Airway trauma. Twenty-nine studies reported data for laryngeal or airway trauma, or both. We performed meta-analysis of 22 comparisons with event data.[21 24 27–30 39 43 44 49 50 53–55 57–59 62 69 78 79 81] The result showed fewer trauma events when a videolaryngoscope was used (OR, random-effects 0.68, 95% CI 0.48 to 0.96; I^2=25%; n=3110) (Supplementary Appendix S5).

Sore throat or hoarseness. Seventeen studies with 2392 participants reported on sore throat or hoarseness, or both.[21 23 24 27 30 43 44 51 55 63–65 69 74 78 79 82] We constructed analysis for studies at two time points: in the post-anaesthesia care unit (PACU) and at 24 h. Six studies did not state when sore throat was assessed

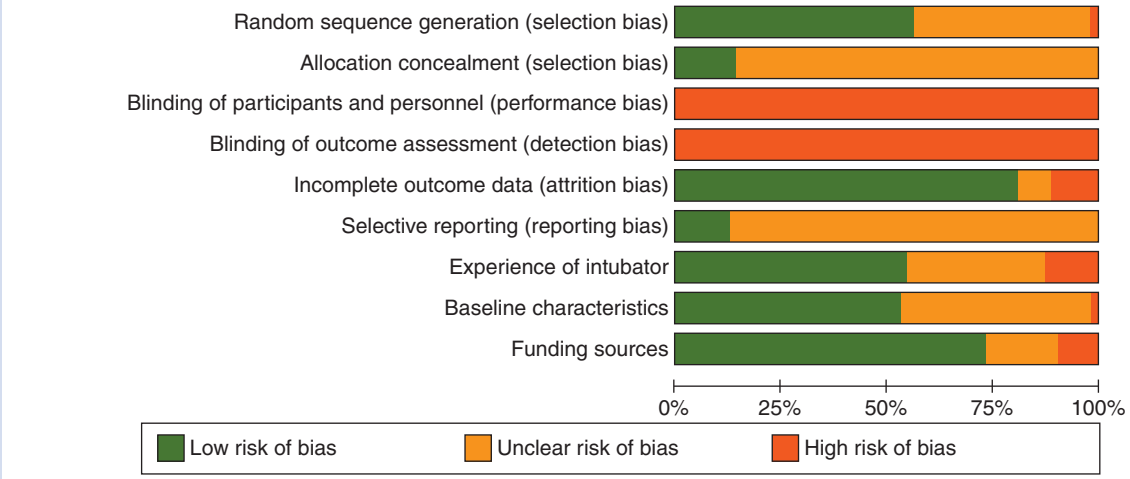
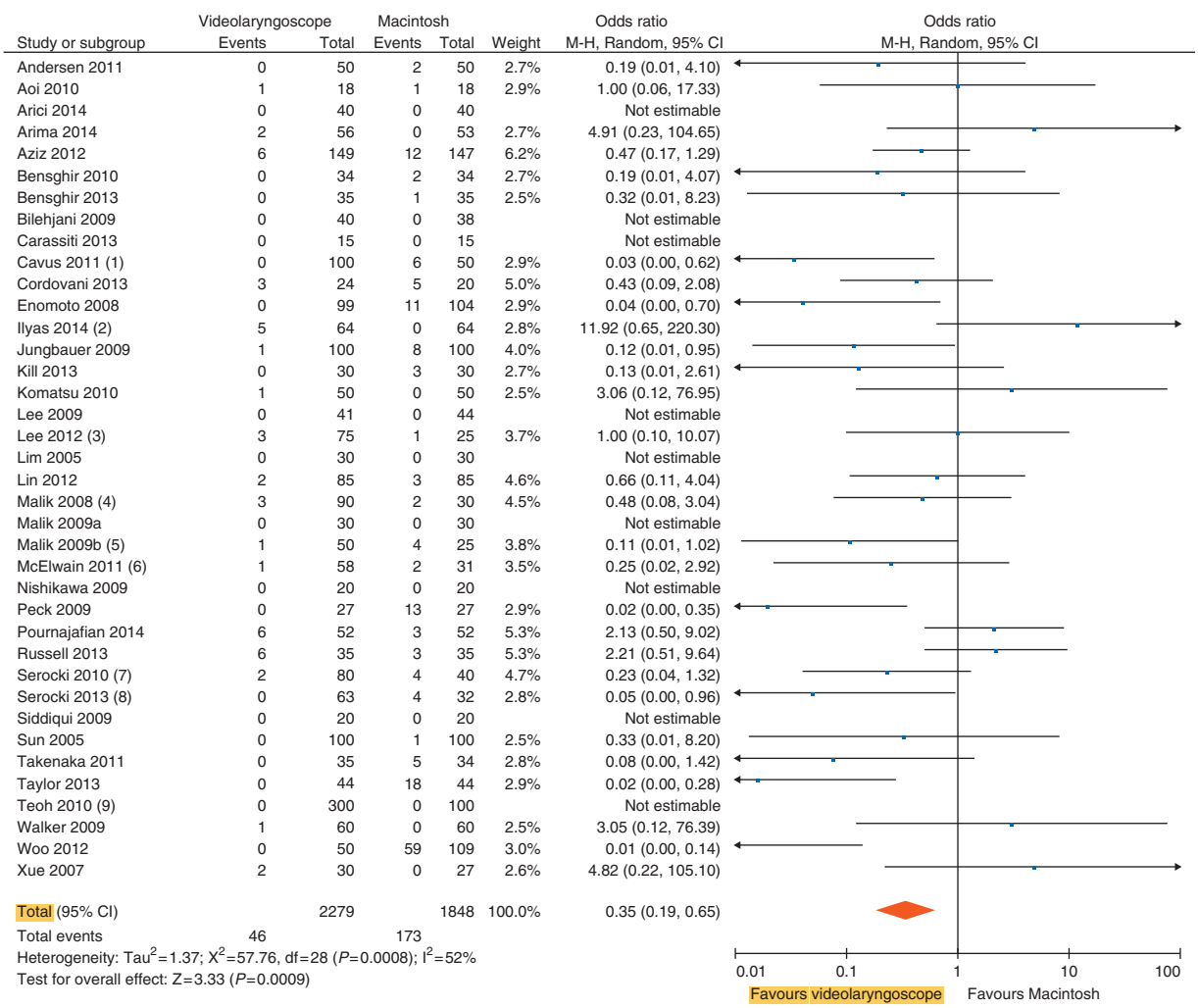


Fig 2 Risk of bias graph. Review authors' judgments about each risk of bias item presented as percentages across all included studies.



Footnotes
(1) Multi-arm study. Data combined for each videolaryngoscope group
(2) Two failed as a result of equipment failure, three failed due to difficulty passing tube
(3) Multi-arm study. Data combined for each videolaryngoscope group
(4) Multi-arm study. Data combined for each videolaryngoscope group
(5) Multi-arm study. Data combined for each videolaryngoscope group
(6) Multi-arm study. Data combined for each videolaryngoscope group
(7) Multi-arm study. Data combined for each videolaryngoscope group
(8) Multi-arm study. Data combined for each videolaryngoscope group
(9) Multi-arm study. Data combined for each videolaryngoscope group

Fig 3 Comparison: videolaryngoscope (experimental) vs Macintosh (control). Outcome: failed intubation.

and for the purpose of this analysis we included this data in the PACU group.[24 27 30 44 65 69] Analysis showed no difference in incidences of sore throat in PACU (OR, random-effects 1.00, 95% CI 0.73 to 1.38; I^2=24%; n=1548) or at postoperative day one according to type of device (OR, random-effects 0.54, 95% CI 0.27 to 1.07; I^2=74%; n=844) (Supplementary Appendix S6).

Six studies reported data for hoarseness. We combined data regardless of time of measurement. There were fewer incidences of hoarseness for those with whom a videolaryngoscope had been used (OR, fixed-effect 0.57, 95% CI 0.36 to 0.88; I^2=28%; n=527) (Supplementary Appendix S7).

Proportion of successful first attempts. We combined data from 36 studies for successful first attempt.[21 23–28 30 32 37–39 42 43 49 50 53–55 57–59 62 69 71–73 75–79 81–84] Analysis showed no difference in the number of successful first attempts according to type of device (OR, random-effects 1.27, 95% CI 0.77 to 2.09; I^2=79%; n=4731) (Supplementary Appendix S8).

Number of attempts. Thirty studies reported number of attempts as an outcome and we were able to combine data for 28 studies.[21 23 24 28 30 32 37–39 42 43 49 50 53–55 57–59 62 71–73 75 79 81–83] Analysis showed no difference between type of device for those

participants intubated in one attempt (OR, random-effects 1.25, 95% CI 0.68 to 2.31; I^2=79%; *n*=3346). We combined the data from studies reporting two, three or four attempts, and there was no difference between type of laryngoscope with additional attempts (OR, random-effects 0.89, 95% CI 0.47 to 1.70; I^2=79%; *n*=3346) (Supplementary Appendix S9).

Time for tracheal intubation. Fifty-five studies reported time for tracheal intubation. Of these, 18 were excluded from formal analysis because of unit of analysis issues. The remaining 37 studies included multi-arm studies with a total of 44 comparisons.[24 25 27–33 35 36 42–44 47 49–51 54 58 61 63–66 70 72–76 78–80 82–84] We identified an extremely high level of statistical heterogeneity (I^2=96%) when these 37 studies were combined, possibly explained by the various time points at which individual studies measured this outcome. Therefore, we have not presented an effects estimate for time for intubation (Supplementary Appendix S10).

Difficulty of intubation. Nineteen studies with 1765 participants reported difficulty of tracheal intubation.[21 23 24 26 29 33 37 39 44 45 54–59 62 70 77] Fourteen used the same validated scale of measurement (Intubation Difficulty Score (IDS) [23 24 26 28 37 39 44 55 57–59 62 70 77] of which we were able to combine seven studies.[24 29 39 57–59 62] Analysis demonstrated that a videolaryngoscope was easier to use when compared with the Macintosh, with 165 of 340 cases being given the lowest IDS score of 0 in the videolaryngoscope group, *vs* 31 of 228 cases in the Macintosh group (OR, random-effects 7.13, 95% CI 3.12 to 16.31; I^2=62%; *n*=568). Of the remaining studies that used an IDS scoring system, four reported a statistically significant result in favour of the videolaryngoscope. Five studies used alternative scales to IDS, with differences in direction of effect reported between studies.[21 33 45 54 69] (Supplementary Appendix S11).

Improved visualisation. Thirty six studies assessed visualisation using the Cormack and Lehane (CL)[12] scoring system and we were able to perform meta-analysis in 22 studies.[23–25 27–29 37–39 49 50 53–55 57–59 61 62 77 79 81] This showed a higher number of laryngoscopies achieving a grade 1 CL view (i.e. more than 50% of the cords were visible) when a videolaryngoscope was used (OR, random-effects 6.77, 95% CI 4.17 to 10.98; I^2=74%; *n*=2240). We combined data for CL grades 1 to 2 and for grades 3 to 4. This also showed more laryngoscopies achieving a CL grade 1 or 2 with a videolaryngoscope (OR, random-effects 5.42, 95% CI 3.70 to 7.95; I^2=5%; *n*=2240), and fewer videolaryngoscope laryngoscopies achieving a CL grade 3 or 4 (OR, random-effects 0.18, 95% CI .013 to 0.27; I^2=5%; *n*=2240). There were five studies that used the POGO (percentage of glottic opening) scoring method.[33 40 65 70 82] Combined results demonstrated extremely high heterogeneity (I^2=96%) and data were therefore not pooled (Supplementary Appendix S12–S14).

Additional analyses

Subgroup analysis

Videolaryngoscope design

Of four videolaryngoscope designs with enough data for meta-analysis, three (GlideScope, Pentax or McGrath Series 5) demonstrated no differences in the number of failed intubations compared with the Macintosh blade (GlideScope: OR, random-effects 0.57, 95% CI 0.25 to 1.32; I^2=24%; *n*=1306, Pentax: OR, random-effects 0.24, 95% CI 0.05 to 1.20; I^2=59% *n*=1086, McGrath Series 5 OR, random-effects 1.18, 95% CI 0.06 to 23.92; I^2=78%; *n*=466) while with the CMAC Macintosh blade there was a reduction in failed tracheal intubation (OR, random-effects 0.32, 95% CI 0.15 to 0.68; I^2=0%; *n*=1058) (Fig. 4).

Anticipated or known difficult intubations

There were fewer intubation failures when a videolaryngoscope was used with participants who had a predicted difficult airway (OR, random-effects 0.28, 95% CI 0.15 to 0.55; I^2=0%; *n*=830) or a simulated difficult airway (OR, random-effects 0.18, 95% CI 0.04 to 0.77; I^2=53%; *n*=810). There was no difference in failed intubation by type of device for participants with no predicted difficult airway (OR, random-effects 0.61, 95% CI 0.22 to 1.67; I^2=56%; *n*=1743) (Fig. 5).

Experience of intubator

We compared studies that included experienced personnel (i.e. ≥20 patient intubations with each device) with studies that used intubators who were inexperienced with the videolaryngoscope (<20 intubations; or unfamiliar with using double-lumen tubes for intubation). Studies with personnel experienced in both devices had fewer failed intubations when a videolaryngoscope was used (OR, random-effects 0.32, 95% CI 0.13 to 0.75; I^2=47%; *n*=1927), but there was no evidence of a difference in failed intubations when personnel were inexperienced with a videolaryngoscope (OR, random-effects 0.20, 95% CI 0.02 to 2.56; I^2=75%; *n*=346) (Supplementary Appendix S15).

Obese and non-obese participants

We identified two studies[21 23] that included obese participants. Only one study[23] included data for our primary outcomes and therefore it was not feasible to perform subgroup analysis against studies with non-obese participants.

Different sites of intubation

We identified three studies[26 38 84] that were in the emergency or prehospital setting. Only one study[26] included data for our primary outcomes and therefore it was not feasible to perform subgroup analysis against studies in the elective setting.

Sensitivity analysis

To investigate the robustness of the evidence, we conducted four separate sensitivity analyses: missing data, cross-over studies, multi-arm studies, and risk of bias.

- We removed studies for which we had been unable to judge whether data were complete and studies that had a participant loss of more than 10% or participant loss was unexplained.[15] Interpretation of effect estimates remained unchanged for all outcomes except for sore throat on postoperative day 1, for which the removal of one study[82] revealed fewer sore throats when a videolaryngoscope was used (OR, random-effects 0.45, 95% CI 0.22 to 0.90).
- After the removal of cross-over studies, there was no difference in the results for all outcomes except laryngeal/airway trauma which was no longer statistically significant (OR, random-effects 0.75, 95% CI 0.51 to 1.11).
- This review included a number of multi-arm studies that compared more than one videolaryngoscope against a Macintosh. We avoided unit of analysis errors in our primary analysis by selecting outcome data in the multi-arm studies for the videolaryngoscope design which had the lowest event rates. We used sensitivity analysis to assess the effect of this

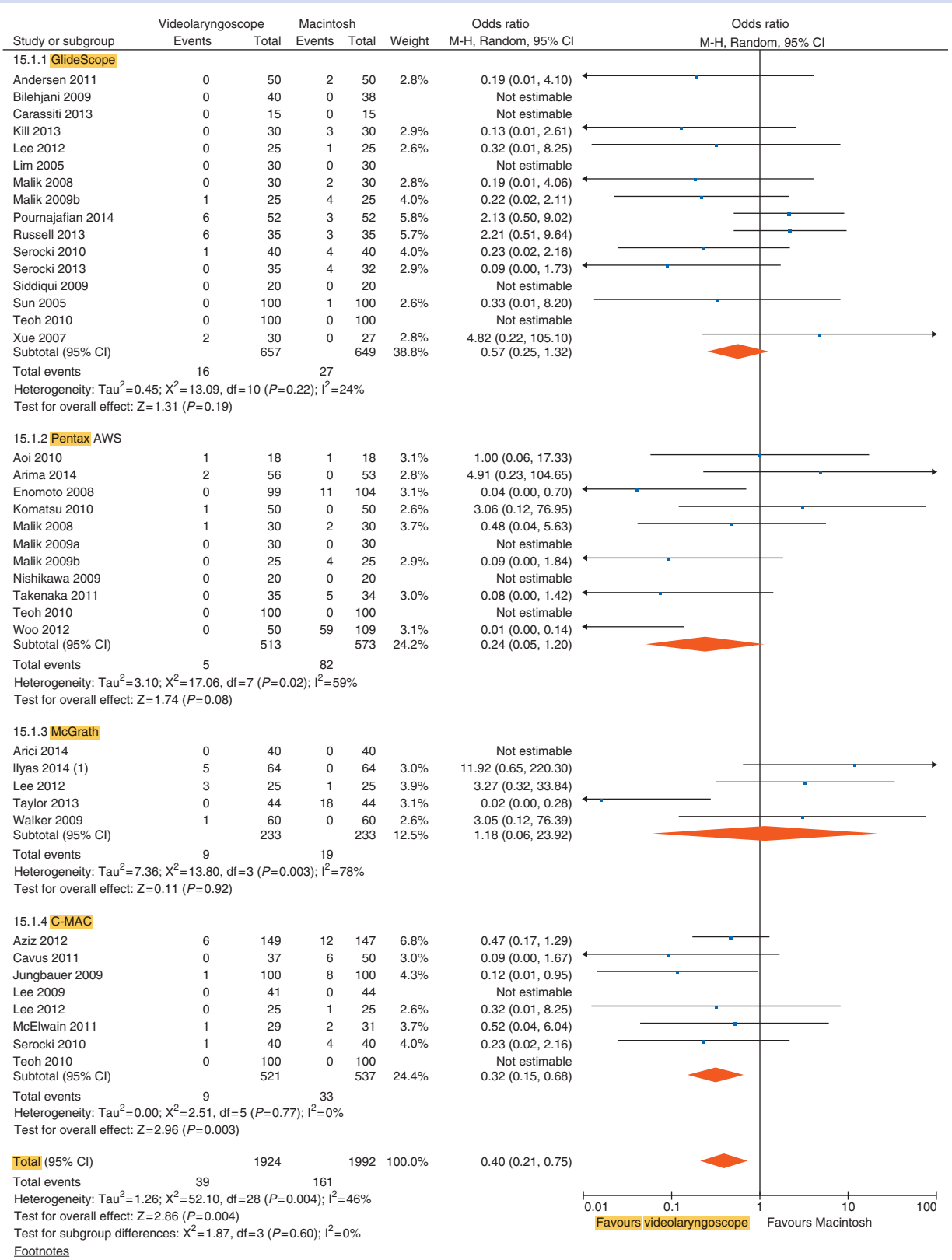
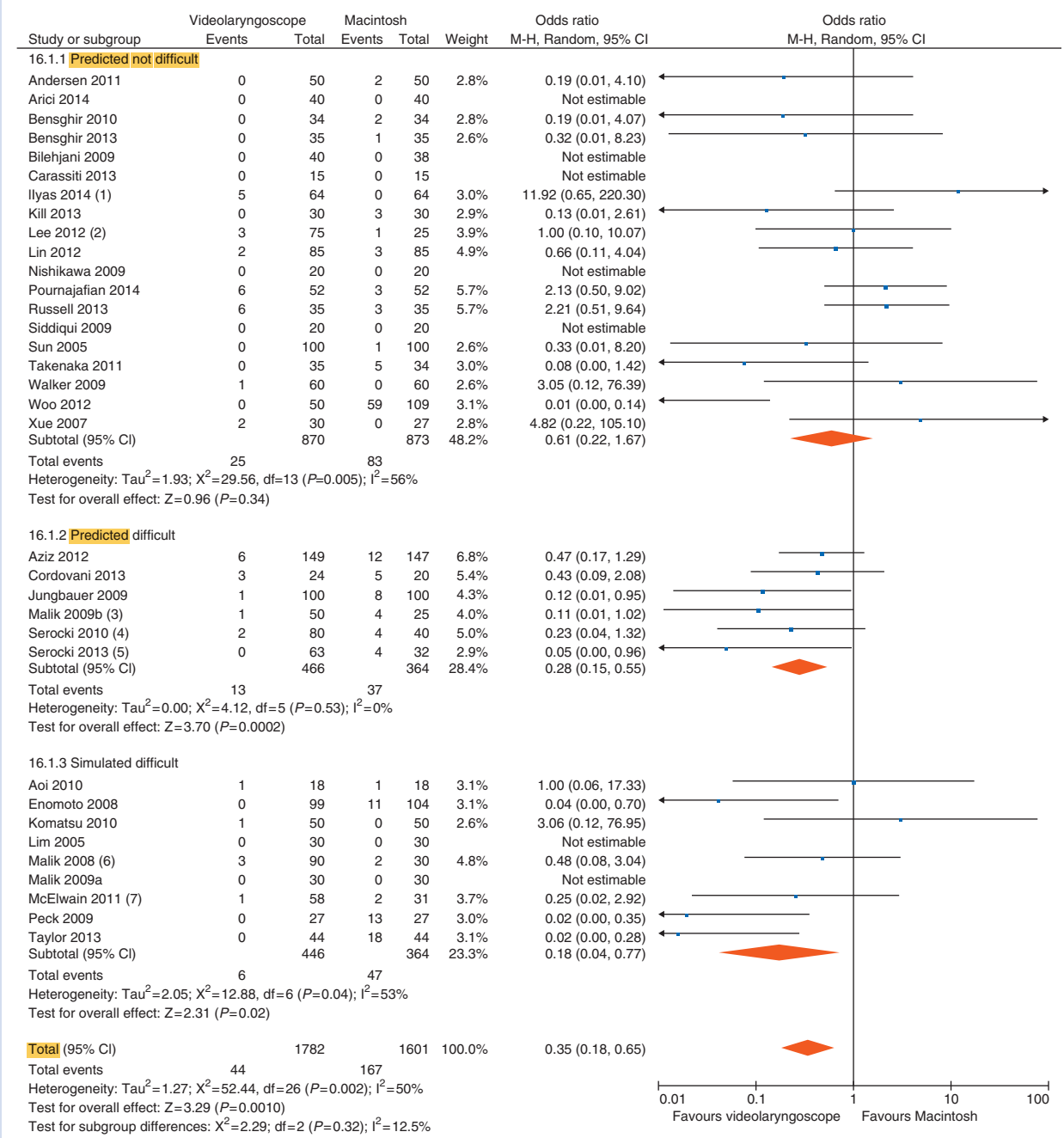


Fig 4 Subgroup analysis. Comparison: videolaryngoscope vs Macintosh. Outcome: failed intubation by scope.



Footnotes

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(2) Multi-arm study. Data combined for each videolaryngoscope group
(3) Multi-arm study. Data combined for each videolaryngoscope group
(4) Multi-arm study. Data combined for each videolaryngoscope group
(5) Multi-arm study. Data combined for each videolaryngoscope group
(6) Multi-arm study. Data combined for each videolaryngoscope group
(7) Multi-arm study. Data combined for each videolaryngoscope group

Fig 5 Subgroup analysis. Comparison: videolaryngoscope vs Macintosh. Outcome: failed intubation by airway difficulty.

decision by selecting outcome data for the videolaryngoscope design which had the highest event rates. The effect estimates remained unchanged for all outcomes except laryngeal/airway trauma which was no longer statistically significant (OR, random-effects 0.73, 95% CI 0.52 to 1.03).

- We considered the impact of our risk of bias assessments on our primary outcome of failed intubation. Removing studies which had an unclear or high risk of selection bias did not significantly affect the results (M-H OR, fixed-effect 0.41, 95% CI 0.26 to 0.63; 23 studies; 2811 participants). Similarly, removing studies which had a high risk of attrition bias resulted in no significant change in the effect estimate (M-H OR, fixed-effect 0.36, 95% CI 0.26 to 0.51; 34 studies; $n = 3624$).

Discussion

Summary of main results

We found 64 studies comparing videolaryngoscopy with direct laryngoscopy in patients requiring tracheal intubation for general anaesthesia. Analysis of 38 studies, including all types of videolaryngoscope, demonstrated statistically significantly fewer failed intubations when a videolaryngoscope was used. However, when analysis was carried out by type of videolaryngoscope, only the CMAC Macintosh blade showed a statistically significant decrease in failed intubations while for the GlideScope, Pentax or McGrath Series 5 we found no statistically significant difference. Failed intubations were significantly fewer when a videolaryngoscope was used in participants with an anticipated difficult airway (in most cases defined by a Mallampati score of 3 or 4) or a simulated difficult airway, whilst there was no difference in failed intubations in participants who presented without an anticipated difficult airway. (In this respect, we would note that there is significant risk of type 2 error in 'predicted easy' as failed intubation is infrequent and some studies include all patients at any elevated risk above 'normal'). We also found that there were fewer failed intubations using a videolaryngoscope when the intubator had equivalent experience with both devices, but not when the intubator was experienced with the Macintosh but not the videolaryngoscope.

Analysis of the other outcomes demonstrated statistically significantly fewer laryngeal/airway traumas and fewer incidences of postoperative hoarseness when a videolaryngoscope was used. However, as in all systematic reviews, the findings follow partly from the decisions made during the review process.[108] Here, the result for laryngeal/airway trauma was dependent on our decision to include cross-over designs and which data to use for included multi-arm studies. When using a videolaryngoscope, compared with Macintosh laryngoscopy there was statistically significantly higher number of laryngoscopies achieving a Cormack and Lehane grade 1 view, and a grade 1-2 view and fewer achieving a grade 3-4 view. The videolaryngoscope was easier to use than the Macintosh. Conversely, one could argue that the degree of heterogeneity (whether it arose from issues with definitions of outcomes, study protocols etc.) within the studies detailed in Figure 3 was too high to perform a meta-analysis at all. We opted to do so, but have drawn attention to the generally low quality of evidence throughout the presentation of this review.

There were only three studies reporting results that we were able to combine for hypoxia. For this outcome, there was no difference between type of device used. Similarly, there were few studies reporting on mortality and respiratory complications. The fact that most studies were performed in the elective setting where all these complications are uncommon or rare may influence these findings. There was no statistically significant difference in the incidence of sore throat either in PACU or at 24 h postoperatively. There was no statistically significant difference between devices in the proportion of successful first attempts, nor at those needing more than one attempt. There was a very high level of heterogeneity when studies that reported time for tracheal intubation were combined, possibly explained by the various time points used to measure this outcome and as a result, we did not present an effects estimate for this outcome.

It was not possible to blind personnel to the type of laryngoscope used; we believed that all studies were subject to a high level of performance bias owing to the potential for user preference. However, we considered other types of bias in our sensitivity analysis, and despite varied levels of bias across studies, results for our primary outcome of failed intubation were not affected by the quality of the evidence when combined in meta-analysis. When using GRADE to assess quality across the included studies, we believed that the unavoidable high level of performance bias in all studies should take preference when the risk of bias for this review was summarized (Supplementary Appendix S16). As a result, we downgraded evidence for each of our outcomes by one level for study limitations. We assessed the outcomes failed intubation, proportion of successful first attempts, and sore throat, to be moderate quality evidence. We included few studies that reported hypoxia, serious respiratory complications, or mortality, which introduced imprecision; we downgraded these outcomes to very low quality evidence. There were a large number of studies with substantial heterogeneity that reported time for tracheal intubation and we downgraded the evidence for this outcome to very low quality. Our findings are consistent with recent reviews[109–111] which indicate that this improvement is more pronounced in patients with a difficult airway,[109] and which recommend the use of videolaryngoscopes to achieve successful intubation in patients with a higher risk of difficult laryngoscopy.[112] Whether the evidence is sufficient to support videolaryngoscopy for all intubations will remain a matter for debate.[113]

Limitations

We excluded studies that had used particular devices (such as the Airtraq and Truview EVO2 laryngoscopes) and had not described in the study report whether these were used with a video/camera attachment; as we only intended to include studies where a screen (indirect view) had been used, we therefore excluded 38 such studies from the review. We encountered difficulty establishing the actual level of experience of personnel, either by the number of yr of relevant experience or by the number of experiences using each device; although we attempted to measure the review outcomes by level of experience, our results are only applicable according to our own interpretation of this. If future studies were to be performed with universally agreed outcomes and definitions of those outcomes, the 'ease of use' and value of the studies themselves and of future meta-analyses would be improved.

The use of videolaryngoscopes in particular clinical scenarios has not been sufficiently explored in this review, for example in the emergency setting during anaesthesia, and in the ICU, emergency department and outside hospitals. Also, we were not able to usefully distinguish performance differences

between different videolaryngoscopes, but it is unlikely that devices of such differing designs all perform equally. We re-ran the search in January 2016, before publication of the Cochrane version of this review, and are aware of additional published studies that have not been included here and ongoing studies identified in clinical trial register searches. This demonstrates continued research interest in this field, and incorporation of data from these studies, during a formal Cochrane update, may lead to changes in the results of this review.

Implications for research

Although there are a substantial number of studies in this systematic review, thus avoiding some of the difficulties of reviews with sparse data,[114] its conclusions must be limited by the variability in definitions used (for instance, for failed intubation), settings and devices. This has given rise to considerable heterogeneity and, taken together with the limited methodological quality of some of the studies, means that the results must be interpreted with caution. It is clear that future airway research should use standardised outcomes and procedures. Within perioperative care research, a recent attempt has been made to standardise definitions[115] and we would welcome a similar attempt in airway research. A forthcoming editorial in the Journal will also touch on this.[116] There is also a notable lack of studies in high-risk patients (those who are generally difficult to intubate, rather than rendered so by repositioning, the application of cervical collars etc.)[117] and patients in different (high-risk) settings such as the emergency department or ICU. Further studies directly comparing videolaryngoscopes of different types would also be welcome.

Conclusions and implications for practice

Our evidence suggests that videolaryngoscopes reduce intubation failure and make intubation easier, particularly in patients with a predicted or known difficult airway. Their use is likely to improve the glottic view and reduce the number of laryngoscopies in which the glottis cannot be seen, irrespective of predicted or known difficulty, and may reduce the incidence of laryngeal/airway trauma. We found no evidence to indicate that the use of a videolaryngoscope would result in fewer attempts to intubate. We were not able to establish whether intubation is likely to take less or more time with a videolaryngoscope, nor whether this would result in fewer incidences of hypoxia or respiratory complications.

Authors' contributions

Study design: A.S., S.L., T.C.
Study conduct: A.S., S.L., T.C., J.P., A.B.
Data analysis: S.L., A.S., T.C.
Writing paper: S.L., A.S., T.C., O.S-R.
Revising paper: all authors

Supplementary material

Supplementary material is available at *British Journal of Anaesthesia* online.

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Declaration of interest

One review author (T.C.) was paid for lecturing, several yr ago (>36 months), by Intavent Orthofix and the LMA Company. This company manufactures and distributes several supraglottic airway devices and one videolaryngoscope: AP Venner. T.C.'s department has received free or at cost airway equipment from numerous 'airway' companies for evaluation or research. He and his family have no financial investments and no ownership of any such company of which he is aware. He spoke at a Storz educational meeting in 2015, and the company paid the costs of travel to this meeting and accommodations. He received no financial benefit from the meeting and was not paid to speak. T.C. has no other known conflicts of interest. All remaining authors have no known conflicts of interest in this review.

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