



Cricoid pressure: an alternative view

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Although the use of cricoid pressure (CP) seems to make intuitive sense, its scientific basis is weak at best and lacking at worst. It is based on studies of saline regurgitation in cadavers and a study of a small number of patients. More than 40 years after Sellick's description of CP, still no randomized controlled trial has been conducted to assess, let alone prove, the effectiveness of CP in preventing pulmonary aspiration of gastric content. Relying solely on CP as a preventive measure has numerous pitfalls. We possibly endanger more patients by interfering with optimal airway management than we save lives through prevention of aspiration of gastric content. It is potentially dangerous to consider CP to be effective in most cases and to become complacent about the many factors that contribute to regurgitation and aspiration. By today's standards, CP can hardly be considered an evidence-based practice.
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Probably the majority of anesthesiologists considers cricoid pressure (CP) the "linchpin of the rapid sequence induction,"¹ and would probably agree with the statement that the "... use of cricoid pressure to protect the airway in patients at risk for aspiration during induction of anesthesia and intubation of the trachea is and should remain the standard of care."² Such generally held conviction of the central role of CP begs several questions. One is: what is the basis of this conviction? Is it primarily scientific evidence or more tradition and ritual? Another question is: based on existing knowledge, is it still justified in 2005 to attribute CP this central role? I will approach these questions by initially tracing the origin of the technique of CP.

Background: Sellick's publication of 1961

In 1961, a publication by Sellick introduced the concept of CP.³ This single publication quickly resulted in a change of anesthetic practice worldwide within a surprisingly short

period of time and in the conviction of many anesthesiologists that CP is an integral part of a rapid sequence induction (RSI). Why did this publication achieve such enormous clinical impact? Was superior scientific quality the basis of its immediate and subsequent impact?

In his original publication, Sellick described the technique of CP during induction of anesthesia in 26 patients whom he considered at high risk for pulmonary aspiration. In 23 of them (forceps delivery 1, esophagoscopy for achalasia of the cardia 2, gastrectomy for adult pyloric stenosis 3, laparotomy for intestinal obstruction 17), no regurgitation or vomiting occurred before and during application of CP, and after its release following cuff inflation of the endotracheal tube. In the remaining 3 patients (forceps delivery, resection of carcinoma of the lower esophagus, laparotomy for relief of obstruction of the small intestine), the release of CP after endotracheal intubation was followed immediately by regurgitation of gastric or esophageal content into the pharynx. Sellick interpreted the latter as suggestive evidence for the effectiveness of CP in preventing regurgitation.

Sellick summarized his findings as follows: "Backward pressure of the cricoid cartilage against the cervical vertebrae can be used to occlude the oesophagus (a) to control regurgitation of stomach or oesophageal contents during

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induction of anaesthesia, or (b) to prevent gastric distention from positive-pressure ventilation applied by facepiece or mouth-to-mouth respiration." As these are far-reaching conclusions, the actual findings must be able to support them. Unfortunately, this is not the case.

First, the patients were positioned supine with a slight head-down tilt. In addition, the "head and neck were fully extended (as in the position for tonsillectomy)." This positioning of head and neck was expected to increase the anterior convexity of the cervical spine, stretch the esophagus, and prevent its lateral displacement during CP. However, such positioning is clearly contrary to today's recommended "sniffing position" during laryngoscopy and intubation, which involves flexion of the neck and extension of the atlanto-occipital joint.

Second, except for the statement that an "intravenous-barbiturate/muscle-relaxant technique permits rapid intubation and is the method of choice," there is no further reference to either the sequence of administration or the dosage of anesthetic drugs used for the induction of anesthesia. It is therefore impossible to judge the "quality" of induction of anesthesia and whether this was comparable between those patients who did and did not regurgitate.

Third, Sellick did not provide any quantitative data on the force applied during CP at the various stages of induction of anesthesia. The information was merely descriptive ("as anaesthesia begins, *pressure is exerted* on the cricoid cartilage"; "even a conscious patient can tolerate *moderate pressure* without discomfort"; "as soon as consciousness is lost, *firm pressure* can be applied"). Since Sellick stated that "the nurse or midwife accompanying the patient can be shown in a few seconds how to do it" ("it" being the application of CP), from what we know today,^{4,9} it is almost certain that the force applied during CP varied considerably between patients.

Fourth, no information on the possible effect of CP on laryngoscopy and intubation was provided. If CP had made laryngoscopy and intubation subjectively more difficult and such impression would have been reported, it is likely that the maneuver would have been viewed more critically.

Fifth, and most relevant in the context of any controversy on the effectiveness of CP, this study did not randomize patients to receiving or not receiving CP. At a time when the technique of CP had not yet been introduced into clinical practice and therefore not become standard of care, it would have been a golden opportunity to perform a randomized trial in patients considered at risk for aspiration, comparing the incidence of pulmonary aspiration in patients receiving CP with those not receiving it during induction of anesthesia. Such a randomized trial might have also settled the still unanswered question of whether or not the possible reduction in the incidence of pulmonary aspiration outweighs the possible increase in the incidence of hypoxic episodes caused by possibly more difficult laryngoscopy and intubation during CP.

Sixth, Sellick postulated that "extension of the neck and application of pressure on the cricoid cartilage obliterates the oesophageal lumen at the level of the body of the 5th cervical vertebra." This postulate was based on two lateral neck x-rays taken in a single anesthetized patient in whom a latex tube had been inserted into the esophagus. The tube was distended by contrast medium to a pressure of 100 cm H₂O. Following hyperextension of the head and application of CP, the x-ray showed obliteration of the lumen at the 5th cervical vertebra. Interestingly and unmentioned by the author, whereas the bony structures of skull, jaw, and cervical spine are clearly visible on the x-ray that demonstrates obliteration of the distended tube, the skeleton of the fingers which must have applied CP is completely missing. As a matter of fact, the x-ray did not provide any clue at all as to how the obliteration was achieved.

Seventh, Sellick concluded that CP "... can be used to occlude the oesophagus. . . to control regurgitation of stomach or oesophageal contents during induction of anaesthesia. . . ." This conclusion was based on the finding in those three patients who regurgitated after release of CP, and on findings in the cadaver showing that "... when the stomach was filled with water and firm pressure was applied to the cricoid. . . a steep Trendelenburg tilt did not cause regurgitation of fluid into the pharynx. Moreover, the flow of water from the pharynx could be controlled by varying the pressure on the cricoid cartilage."

As far as the cadaver study (studies?) is (are?) concerned, several questions remain unanswered. The wording "in the cadaver" leaves the possibility that the conclusion is based on the findings in a single experiment. Clearly, in such case, the findings would be of highly questionable clinical relevance. Furthermore, no quantitative data were provided with regard to the filling of the stomach (how much water was introduced to achieve what intragastric pressure) and the application of "firm pressure" to the cricoid.

Eighth, Sellick finally concluded that CP "... can be used . . . to prevent gastric distention from positive-pressure ventilation applied by facepiece or mouth-to-mouth respiration." This is pure speculation. The entire publication does not provide a single piece of evidence to support this conclusion. This aspect of CP was simply not the subject of the investigation.

In summary, this was a non-randomized, rather poorly controlled observational study in a relatively small number of patients. Lack of several pieces of essential information makes a useful interpretation of the findings basically impossible. Several of Sellick's conclusions were not supported by the findings. For all of those reasons, the publication was rightly published under the category of "Preliminary Communications."

Despite these major study deficiencies, the practice of CP was adopted rapidly and rather uncritically by the anesthesia community and soon became a standard of care during RSI of anesthesia. The practice seemed to be supported by subsequent findings in 1970 suggesting that, in cadavers, CP

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