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LETTERS TO THE EDITOR

**Letter to the Editor
commenting the study
published in the journal by
Ascedio Jose Rodrigues et al.
(Rev Bras Anesthesiol
2013;63(4):358–361),
regarding flexible bronchoscopy
intubation**



**Carta ao editor comentando estudo publicado
na revista por Ascedio Jose Rodrigues et al.
(Rev Bras Anesthesiol. 2013;63(4):358–361), a
respeito de intubação com broncoscópio
flexível**

Dear Editor,

I read the article by Ascedio Jose Rodrigues et al. published in the RBA (Rev Bras Anesthesiol 2013;63(4):358–361), and I would like to expose some opinions about the awake flexible bronchoscopy intubation (FBI).

As highlighted in the study, FBI is not a good option for the situation “can’t intubate, can’t ventilate” (CICV), a situation that involves major threat to life.¹ Fiberoptic intubation is safe, but requires time and skill and is not suitable in CICV situation that requires immediate restoration of ventilation.

I never applied blockade (with needles) for FBI, and the reason is that if the patient is able to open his mouth enough for intraoral access to perform the glossopharyngeal nerve block, by injection in the caudal portion of the posterior tonsillar pillar, this patient probably has an easy intubation and does not require FBI. More importantly, due to the proximity of the carotid artery, there is a possibility of intra-arterial injection, or worse, hematoma of the posterior region of the tongue, which would transform a difficult case in an impossible case.

Regarding superior laryngeal nerve block, it is only feasible in patients who do not need it; that is, in lean patients with well-defined anatomical landmarks. In obese patients, or patients using cervical collar or with cervical trauma, or those with short, thick neck (“taurine”), precisely those that would benefit from awake fiberoptic intubation, this blockade is no longer a good option.

I perform blockades “without needles” in my patients. In order to apply local anesthetics, I ask the patient to poke his tongue and then I hold it with a gauze. On each side, I apply two puffs of 10% lidocaine in the palatoglossal arch in an attempt to block the glossopharyngeal nerve in order to minimize the gag reflex; followed by an additional puff in the soft palate. After about 3 minutes, I place a gauze soaked in 10% lidocaine in the piriform fossa, behind the base of the tongue bilaterally. The objective is to block the nerves due to their proximity to the mucosa saturated with concentrated local anesthetic solution.²

Regarding bite block application, I recommend using one of oropharyngeal intubation cannulas. Although there are several on the market, I have available only the VBM® and VAMA® (Valentin Madrid). The main difference between them is that VAMA® pushes the soft palate backward, while VBM pushes the tongue forward.³ With the use of these tubes, fibroscopy is **MUCH easier** (my emphasis): the fibroscope does not deviate from midline and is directed to the epiglottis, for anesthetic supplementation through the working channel of the fibroscope.⁴ With the use of these cannulas there is no need to disconnect the intermediate 22 mm device from tracheal cannula. I start with a careful titration of sedatives and administration of supplemental oxygen early in the procedure, in order to make the experience less unpleasant for the patient.

Conflicts of interest

The author declares no conflicts of interest.

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Marcelo Sperandio Ramos

Hospital Universitário, Universidade de São Paulo, São Paulo, SP, Brazil

E-mails: marcelo.ramos@hu.usp.br, marcelosramos@terra.com.br

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Comparison between continuous thoracic epidural and paravertebral blocks for postoperative analgesia in patients undergoing thoracotomy: meta-analysis of clinical trials[☆]



Comparação entre bloqueios peridural e paravertebral torácicos contínuos para analgesia pós-operatória em pacientes submetidos a toracotomias: meta-análise de ensaios clínicos

Dear Editor:

The article entitled "Comparison between continuous thoracic epidural and paravertebral blocks for postoperative analgesia in patients undergoing thoracotomy: a systematic review", recently published in the *Brazilian Journal of Anesthesiology*, demonstrates the authors' concern to show the therapy effectiveness for anesthetic management of postoperative pain in chest surgeries.¹

Reading the scientific article arouses great interest to readers; however, some points need to be considered, such as: the software used for calculations, the sensitivity analysis method by successive meta-analysis, the use of mixed-effect model analysis, and the search to identify statistical heterogeneity.

The software used in the search was described in the sections Method and References, but the latter is incorrect, and it is impossible to identify the place where it is available and to have access to the software for future searches similar to this.

The method of successive meta-analysis was used by the authors at some point of the systematic review execution to perform the sensitivity analysis; however, the outcome of this analysis was not reported in the results or discussion, which did not clarify its real contribution in this systematic review. This method allows the identification of a likely source of statistical heterogeneity and the exclusion or not of the article, in an attempt to consolidate the results.²

The authors reported the use of random and fixed effect model for meta-analysis calculation; however, the random model was chosen to calculate the meta-analysis whenever I^2 was greater than 30%. In the analysis of variables "assessment of pain at rest after 24h and "incidence of hypotension", the value of I^2 was lower than that proposed by the authors, not matching the research method description, and the results were also described by the random instead of fixed effect method. The article does not indicate whether this description of the results was due to consensus decision of the authors or a failure to conduct the research.

The authors considered the presence of heterogeneity as a research bias when they reported "(...) these results may have been biased by the included studies heterogeneity"; however, the presence of heterogeneity does not indicate bias in a systematic review. Tests of heterogeneity are used to determine whether differences between the included studies are genuine (heterogeneity) or if it occurred randomly during the analysis (homogeneity).³ If the differences occurred randomly, the results found in systematic reviews have more credibility, and if heterogeneity is found, the reasons should be carefully evaluated by the authors to consolidate the results and not only be considered a research bias.

It is noticed that the statistical heterogeneity present in most analysis was little explored by the authors, and it is possible to disagree with part of their conclusion that says: "From this systematic review, it is clear that epidural analgesia is associated with a higher incidence of arterial hypotension and urinary retention when it is used for lateral pain control after thoracotomy in adult patients, with evidence level 1A", as level 1A requires minimal or absent heterogeneity or that it is properly explored while performing a systematic review.

In short, I congratulate the authors for the article, which brings important results for the understanding of post-operative pain in thoracic surgery. Systematic review conclusions are less incisive regarding the clinical significance of its results when those of the included studies differ from each other.³

Conflicts of interest

The authors declare no conflicts of interest.

[☆] Universidade Federal de Alagoas, Maceió, AL, Brazil.