

hypoxemia under certain conditions. However, the relationship between pulmonary anesthetic administration and HPV remains controversial.

In conclusion, global effects of inhalation induction with sevoflurane at high concentrations may cause oxygen desaturation in lungs that are normally ventilated with 100% oxygen, potentially resulting in global inhibition of HPV. However, these conditions may be a consequence of global HPV and associated 100% shunt fractions. Thus, despite the high level evidence of no differences in outcomes following pulmonary and intravenous anesthesia, the hypoxic consequences of inhalation anesthesia require further clarification.

Consent

Written informed consent was obtained from the patient for publication of this case report. A copy of the written consent is available for review by the Editor of this journal.

Conflicts of interest

The authors declare no conflicts of interest.

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Anesthesia in a newborn with Klippel–Feil syndrome



Anestesia em recém-nascido com síndrome de Klippel-Feil

Dear Editor,

I read the case report of Altay et al.¹ about anesthesia management of a newborn with Klippel–Feil syndrome (KFS) with interest. The authors presented their case as “the youngest child with KFS on whom oral intubation was performed”. I appreciate the colleagues for their management of this challenging case, but there are some points that have to be discussed.

Altay et al. performed a successful intubation at first attempt with Direct Laryngoscopy (DL), which was consistent with the literature. According to the literature, KFS alone may not be a predictor of difficult airway management in infants. Naguib et al.² had reported a three-week-old boy diagnosed with KFS successfully intubated with DL.

Creighton et al.³ had reported 8 infants with KFS (6 of them had also cleft palate, most probably some of them were newborns) on whom oral or nasal intubation was performed with DL using regular laryngoscope. They performed awake DL successfully, despite the other present conditions that complicate intubation like cleft palate and lateral position in addition to KFS.

Recently we have reviewed the airway management and the success of DL in children with KFS⁴ and found that there is no report describing difficult mask ventilation or unsuccessful Laryngeal Mask Airway (LMA) insertion in the literature. Also, there is no report of an unsuccessful DL in infants with KFS. We think that the success rate of tracheal intubation with DL in early ages (probably before adolescence) seems to be increased when other predictors of difficult intubation does not accompany. These findings may encourage us for attempting DL in children with KFS alone, but accompanying airway anomalies are not rare in KFS and have to be investigated before anesthesia induction. Also, a previous successful DL does not ensure successful intubation because cervical fusion may become progressively worsen over time and DL may be challenging in older ages.

Another point: the authors mostly dwell on the airway management of the patient, but the anesthesia technique might be questionable. As, providing an adequate depth of

anesthesia to ablate the rise in pulmonary vascular resistance associated with surgical stimuli is one of the primary goals in anesthesia management of these patients, what was the reason to use sevoflurane as a sole anesthetic agent in a cardiac patient with persistent pulmonary hypertension, if early postoperative extubation was not planned?

Conflicts of interest

The authors declare no conflicts of interest.

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Effects of lidocaine and magnesium sulfate in attenuating hemodynamic response to orotracheal intubation: a single-center, prospective, double blind, randomized study



Os efeitos da lidocaína e do sulfato de magnésio na atenuação da resposta hemodinâmica à intubação orotraqueal: estudo unicêntrico, prospectivo, duplamente encoberto e aleatorizado

Dear Editor,

It was with great pleasure that I read the article “Effects of lidocaine and magnesium sulfate in attenuating hemodynamic response to orotracheal intubation: a single-center, prospective, double blind, randomized study”.¹ Concerned to clarify some points related to the statistical analysis and conclusion; here are some considerations to the authors:

1. The authors report a discrete statistical difference and this does not allow the article’s reader to come to conclusions: “There was a statistically significant increase in SBP ($p=0.018$) and DBP ($p=0.0467$) values measured post-TI (Fig. 2), but of little clinical importance”. The values should have been demonstrated in text because, as shown in Fig. 2, it is not possible to capture its

magnitude, so that the lack of clinical importance does not represent absence of biological relevance;

2. The data were partially or totally analyzed over time, and the patients were also submitted to anesthetics in addition to the medications tested, which may be in addition or not. It is known that magnesium sulfate has a prolonged clinical effect after venous use, whereas lidocaine has a short protective effect compared to magnesium. Thus, there are two factors that must be considered in this statistical analysis: time and treatment. The best statistical test to perform in this situation is two-way ANOVA.² The results analyzed as they are in the text may be erroneously positive and the possibility of a type I error in this research is clearly perceived;
3. If the authors consider the use of the Student’s *t* test as correct, or more appropriately in some cases the Mann–Whitney *U* test, according to the text, they should have corrected the *p* value with the procedure for multiple correction of hypothesis tests, instead of considering only 5% as the level of significance in all analyzes. The possibility of having a positive result in the statistical analysis occurring at random is 5%. The *p*-value correction would have reduced the probability of a random occurrence of the statistical result. Thus, the possibility of type I error in this research is clear;
4. The objective described by the authors was “to compare the effects of intravenous administration of magnesium sulfate versus lidocaine on this reflex hemodynamics after laryngoscopy and orotracheal intubation”. The authors’ conclusion was “magnesium sulfate and lidocaine have good efficacy and safety in hemodynamic control during laryngoscopy and intubation”, which is not in line with the proposed objective. It is necessary that the authors relate what were the efficacy variables and also the safety variables so that the conclusion is better understood. Noteworthy, the term *efficacy* should generally be used in research whose execution conditions are ideal, as with laboratory studies. This term should

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