Evaluation of the efficacy of lidocaine and magnesium sulphate in reducing the haemodynamic effects caused after intubation/laryngoscopy

Avaliação da eficácia de lidocaína e sulfato de magnésio para reduzir os efeitos hemodinâmicos desencadeados pela laringoscopia/intubação

Dear Editor,

Firstly, we would like to thank the Brazilian Journal of Anesthesiology for publishing this study comparing the effects of lidocaine and magnesium sulfate and the researchers for their efforts in conducting this study.

The subject selection criteria plus the nature of prospective randomised double blind studies omit any question of bias.

However, we have identified several limitations of the method. Firstly, a single centre study with a small number of subjects is not representative of a larger population. Additionally, according to the power analysis there should have been at least 25 patients in Group L to make accurate conclusions about lidocaine – although it is arguable that a difference of one is a trivial.

It would have been helpful if the researchers had explained their choice of administration methods of the drugs further. It can be suggested that a lower total dose of lidocaine could have reached peak effects quicker with a bolus. Moreover, we wonder whether it was the intention of the researchers to give study drug infusions which would still be lowering blood pressure six minutes post OTI. We are curious to know how long it took after study drug infusion for the subjects’ blood pressure to return to normal.

It is known that ethnicity is one of the main risk factors for hypertension. It has been found that black people have a higher systolic and diastolic pressure than those with European origin. Therefore, a greater blood pressure in Group M may be due to more participants being of African origin. The authors have not supplied supplementary data indicating the ethnicities of participants which would be useful in assessing the contribution of ethnicity to the effect the drug has on blood pressure.

The authors state that their study ’’was performed with healthy patients’’ however table 1 in the paper by Mendonça et al. suggests that a proportion of participants took diuretics, angiotensin receptor blockers and ACE inhibitors. As the authors provide no supplementary data it is unclear if 15 participants were taking anti-hypertensive drugs or if a smaller number took multiple drugs. Furthermore, the paper does not explain why these participants were on anti-hypertensive drugs. They may have been administered medication to ensure the patients had a similar blood pressure pre-surgery. However, as hypertension is associated with left ventricular hypertrophy amongst consequences, it questions whether the study was ’’performed with healthy patients’’. The publication of supplementary material detailing the health of the patients would facilitate accurate interpretation of the results.

The title of the study does not clarify that its purpose is to compare the two drugs’ efficacy. A title explicitly stating the comparative nature of the study would be more appropriate.

The several drawbacks of this study include the lack of stratification of the results based on ethnicity and the health of participants and comprehensive explanations on their choice of methods. However, the unbiased nature of this study gives credit to its results.

Conflicts of interest

The authors declare no conflicts of interest.

References


Rowena Gnanapragasam a,∗, Ateka Gomaa a, Vinod Patil b

a Barts and the London School of Medicine and Dentistry, London, United Kingdom

b Barking, Havering and Redbridge University Hospitals NHS Trust, London, United Kingdom

∗ Corresponding author.
E-mail: r.s.gnanapragasam@smd16.qmul.ac.uk (R. Gnanapragasam).