Reviewer's report

Title: Noninvasive pulse wave analysis for monitoring the cardiovascular effects of CO2 pneumoperitoneum during laparoscopic cholecystectomy

Version: 2 Date: 26 May 2014

Reviewer: Calin Mitre

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Reviewer's report about the study:
Title

'Non-invasive pulse wave analysis for monitoring the cardiovascular effects of CO2 pneumoperitoneum during laparoscopic cholecystectomy'

Péter Sárkány, Szabolcs Lengyel, Réka Nemes, Livia Orosz, Dénes Páll, Csilla Molnár and Bela Fulesdi

Type - Research article type.

1. Major Compulsory Revisions

Pag. 5 line 10 Please specific the value of PEEP if you had. Associated PEEP can modify the results;

Pag.5. line 11. Please change the term of "CO2 insufflation" with "CO2 intraperitoneal pressure". Insufflation means "the introduction of a flow of gas into a body cavity"

Pag.6. line 4.b Please provide results for the ejection duration hemodynamic parameter. Even if it's possible to calculate the ejection duration and ejection duration index from the PWA I didn't see any sustainable results about this parameter.

Pag. 7 line 2. I consider it would be better to evaluate the hemodinamyc results also after the period when the gases were closed.

Pag.7. line 14 Please consider if the number of females (much more than man) and the age (50 years) could influence the results.

Pag.8. line 1. Please consider if the central aortic pressure values increases only because of the peripheral arterial stiffness or also because blood is squeezed from the splachnic venous and preload (return) is augmented (IAP levels below 15 mm Hg)

Pag. 8. line 22, 23. Please reconsider the affirmation that "The decrease of augmentation index reflecting the stiffness of the peripheral vessels." In my opinion the decrease of augmentation index was about vasodilatatory effect of the anesthetic and …
Pag.9. line 20-25 please let me suggest to the author another point of view about the pneumoperitoneum hemodynamic effects: the results from this study show that the hemodynamic modifications, produced by the pneumoperitoneum, were good for the patients, because after the insufflations they were similar with the initial values before the induction of anaesthesia. That means the modification of pneumoperitoneum antagonized (cancelled) the decrease of the peripheral and central pressure and also decrease of the augmentation pressure produced by the induction of anesthesia.

The hemodynamic modifications produced by the pneumoperitoneum alone could be dangerous for some groups of patients, but if we associated general anesthesia, the results overlap with the canceled of the hemodynamic depression produced by the anesthetics and loss of the sympathetic tone.

This possible interpretation should be an explanation for the results from other studies (Safran D, et al. Surg Gynecol Obstet 1993;176:548-54.) which in the high severe patients (ASA III-IV), did not found any clinical modification about the pneumoperitoneum for abdominal laparoscopic surgery, contrary to expectations. Maybe it would be interesting for the author to compare the values before induction with the results obtained after insufflations.

This suggestion, if it is adopted by the authors, will change also the conclusions.

- Minor Essential Revisions
There are some spelling and grammar mistakes. I recommend a language correction.

- Discretionary Revisions
I agree to publish the comments entered here and answers to the questions constituting the report by the author on the site if the article is accepted

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests.