Author's response to reviews

Title: Use of local anaesthetics and adjuncts for spinal and epidural anaesthesia and analgesia at German and Austrian University Hospitals: An online survey to assess current standard practice

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Author's response to reviews: see over
Dear ladies and gentlemen:

Thank you very much for the opportunity to revise the below mentioned manuscript, taking the valuable remarks of Dan Benhamou into consideration.

„Use of local anaesthetics and adjuncts for spinal and epidural anaesthesia and analgesia at German and Austrian University Hospitals: An online survey to assess current standard practice“

1. What was the goal of the study? This is not clearly stated. If the goal is to gain some knowledge on practice in these two countries, then the information might be useful at the national but not necessarily at the international level. It would be more logical to submit those data to a German Journal, except if the authors are in the position to explain the relevance of these data for an international readership. This should be added in the Introduction section.

We are not convinced by the argument that knowing the practice patterns of one country has only implications for this special country.

At least our practice and standard of care is increasingly influenced by survey results and observational studies that have been performed in other European countries; a contribution to the ongoing amalgamation process in medicine. This was the reason to report the findings in an attractive international journal. The open access policy is another worthwhile issue that makes a publication attractive in BMC Anesthesiology (Germany does not have an open access journal itself). Since we did not want to blow up the length of the introduction, we did not report on our motivation to publish in BMC Anesthesiology.

2. The authors evaluated only a limited fraction of German and Austrian institutions, namely only academic departments and it is not explained the authors restricted their analysis to this limited sample. Overall only 33 institutions responded, a number which in itself is limited and precludes analyses and comparisons.
We absolutely agree with this issue and we would assume that every organization committee of survey hopes for a good response rate. We are aware of the fact, that the number of included institutions is limited. Nevertheless, university hospitals are the main teaching hospitals in German speaking countries and according to this, the presented data indirectly reflect more that those 33 institutions, apart from the fact, that usually university hospitals lead the way concerning further development or changes.

However, we are convinced that it is far better to publish results and discuss the weaknesses rather than to put the results in a bin and deny a constructive discussion of the results.

Moreover data are surprisingly presented. For some surgical procedures, the authors indicate the percentage of patients who receive regional anesthesia and the type of drugs that are used but in some other circumstances we are given only the distribution of drugs that are used. For example, for short urological procedures, we are taught that 60 % of responding units are using regional anesthesia and that bupivacaine is the local anesthetic of choice. By contrast, for cesarean delivery, data on drugs are provided but the proportion of women who receive neuraxial anesthesia is not indicated. This would have been useful as previous papers showed than in the nineties, general anesthesia remained widely used for cesarean delivery in Germany. The reviewer believes that this is of more relevance to safety and good practice than the relative use of ropivacaine or bupivacaine. Similarly, having some knowledge on the proportion of women who receive epidural for labor pain relief would have been interesting.

As you can see from the displayed part of the web page, our intention concerning urology, gynaecology and obstetrics was the same. The arising problem is that not all of the hospitals provide regional anesthesia for e.g. short urological procedures, but all university hospitals provide regional anesthesia for spontaneous delivery as well as epidural and spinal anesthesia for caesarean section. This in turn leads to the problem you mentioned above, - namely, that we can calculate the number of hospitals using spinal anesthesia for short urological procedures (because no hospital regularly uses epidural in short procedures) and consecutively receive the number of hospitals using general anesthesia. So, in this case we can provide the percentage of patients who receive regional anesthesia. On the contrary, all university hospitals offer epidural for spontaneous delivery as well as spinal and epidural for caesarean section.

Hence, in both cases we did not ask for percentages but the emerging results allowed to draw the conclusion, that x% of the hospitals use regional anesthesia, while Y% do not, while a similar conclusion was not possible for other parts of the survey. Nevertheless, we agree with your statement, that the article definitely would benefit from additional information concerning the distribution of different kinds of regional anesthesia as well as concerning the distribution between general and regional anesthesia. Unfortunately, the need for such questions often arises after receiving the answers or after analysing the results. Therefore, our intention is indeed to use the present online survey as a basis for further online questionnaires.

There are also insufficiencies in the design and presentation of data. For example, who responded to the questionnaire? A single person, presumably the department head but how can we ensure that responses really reflect the real world. Was there a need to look at local statistics to define percentages or do the results reflect only the perception of the responder? Moreover, in some (if not most) institutions, the chairman does not practice in every subspecialty and has no clear knowledge of anesthetic attitudes in every subspecialty. How can he (she) answer precisely in this context?

I am not very familiar with the training guidelines in other countries. In German speaking countries especially university hospitals usually provide clinical intern guidelines, with respect to the fact, that they are, besides others, mainly responsible for training and education. Hence, we feel quite confident, that the questions are precisely answered. Further, it is unlikely that the e-mail correspondence is answered by a person not involved in the executive board of a department. In case there is ambiguity about the local standard of care usually the department head forwards the respective survey to the consultant being in charge of a clinical department. However, to be humble, we cannot be 100% confident that cheating has not influenced our results (which is likely to be true for every online survey or even pen-and-paper survey).

Questions are not precise enough. For example, for postoperative epidural analgesia in major procedures, the authors state that ropivacaine concentrations vary between 0.16 and 0.75%.
ously, this range does not reflect only the concentrations which are used for epidural infusion (or PCEA techniques) but also includes the concentration of the bolus injection. Information provided is thus difficult to analyze.

We fully agree that further insights regarding specific indications would be desirable. However, the primary intention was a rather broader survey among many disciplines rather than an in-depth look, e.g. at the gynaecology ward. Providing detailed insights for every surgical discipline would have led to a lengthy paper and the appropriate way in our view would be to split up the paper in separate ones. The latter, on the other hand, would have prevented a broader overlook.

7. Cesarean delivery is not separated in two major categories (i.e. scheduled and emergency) which is a clinically relevant distinction with different anesthetic strategies.

Indeed, we did not separate cesarean delivery in scheduled and emergency and we agree, that this is a clinically relevant distinction with different anesthetic strategies. Furthermore, your comment might be subsumed with your remark „11.8“. „Regarding cesarean delivery patients, it is also unclear as to how many patients receive a spinal anesthetic and how many receive an epidural."

As stated in 11.8, it was not the main goal of the study to determine the percentage of scheduled and emergency cesarean deliveries. Nevertheless, this is an interesting question, which might also be part of further online surveys, which however would require much more detailed questions concerning e.g. the definition of „emergency“ cesarean delivery in the various departments (hospitals).

8. Data presented in Figures and Tables are unclear. There are 4 Tables but it is unclear as to what they refer.

Please give more details on this issue. We have carefully checked the tables and were not able to find the inconsistencies as each table is mentioned in the "Results".

9. Also, Figure 4 is said to describe practices for vaginal delivery (see legend on Page 18) although most parts of the graph refer to high concentration bupivacaine or ropivacaine (0.5%), suggesting that this Figure rather refers to epidural for cesarean delivery.

Thanks a lot for this remark. This mistake must not have happened. Indeed, Figure 4 refers to the spinal anaesthesia for caesarean section and not to vaginal delivery. Accordingly, the legend of Figure 4 is revised.

10. The reviewer agrees that the various choices are more linked to local history and experience rather than in science as we know that the outcome benefits of regional anesthesia are very difficult to demonstrate and may be balanced by other problems. Thus providing a picture at a given moment in a limited number of institutions has a very limited usefulness.

We agree, that such an online survey can only be a snapshot. However, such surveys give valuable information over the course of time to gain an insight in the development concerning the use of techniques and / or substances.

We also agree, that the absolute number of institutions is limited. Nevertheless, as all the participants are university hospitals, which play a leading role as teaching hospitals and therefore reflect more than the single opinion of 33 anesthesiologists.

In summary, in our view it’s far better to know a little bit (based on the results or our survey) than to know nothing (without it).

But we fully agree that there is a constant shift in practice patterns that is hardly to pick up with surveys.

11. Additional questions and comments

11.1. The abstract states that 33 of the 39 hospitals responded. This is not indicated in the Results section of the article. Because 6/39 represent 15 % of institutions, it would be useful to know if non responding hospitals are “similar” to responding ones and if not how the differences should be taken into account in the analysis.
Thanks again for this useful hint. Of course it is absolutely necessary to provide information concerning the number of involved hospitals and respondents, respectively. An appropriate sentence is added at the beginning of “Results”.

“33 out of 39 German and Austrian university hospitals responded.”

Concerning the „similarity“ of the responding and non-responding hospitals: as all hospitals are university hospitals we don’t presume any difference, which could influence the results or the analysis to a greater extend.

11.2. The sentence on Page 8 (last sentence, bottom of the page) is in contradiction with the first sentence of the Conclusion: one states that there is a certain agreement while the other states that practices vary widely. Please modify and be consistent.

We apologize if we have provoked confusion and would be happy to receive constructive feedback on how to better phrase the sentence. We do not see any inconsistencies, since the first statement (“some consensus”) refers to the technique and regional anesthesia itself, while the other part (“differs to a large extent”) focuses on the local anaesthetics, concentrations of local anaesthetics used and/or adjuvants.

11.3. It would have been interesting to know why prilocaine is not widely used although it is available in the two countries.

We accede completely. The question why prilocaine is not widely distributed in Germany and Austria is indeed quite interestingly. However, this question will, besides others, be part of an already planned subsequent survey that should provide answers on questions that are not sufficiently answered.

11.4. Using a mixture of mepivacaine with bupivacaine is a surprising proposition and does not reflect the practice of most experts throughout the world.

We totally agree, that this combination definitely does not reflect the majority of anesthesiologists. Nevertheless, it is an interesting combination, which seems to be in regular use at a university hospital. Again, this is a good point and argument in favour of such survey. It may be an issue that can cause a letter to the editor and thus initiate a constructive and fruitful discussion. Simply stating that’s not the expert’s way of doing regional anesthesia does not add to the body of knowledge.

11.5. The paragraph (Page 9, bottom of the page) discussing the cardiovascular stability of ropivacaine is also surprising. The articles referenced to support this fact describe studies in isolated organs and do not take into account the much more important effect of local anesthetics on the sympathetic system. The reviewer is unaware of studies showing convincingly that spinal ropivacaine is better for high risk patients.

Thanks a lot for this note, which is really quite important for the reader. Indeed, human studies indicating the superiority of ropivacaine are rare and we did not provide any information to the reader. Therefore, we changed the above mentioned paragraph as follows:

“Interestingly, the majority of the replying clinics use bupivacaine 0.5%, even though several studies indicate the superiority of ropivacaine with regard to cardiac stability[14;15].”

Changed to:

Even though the reduced cardiovascular toxicity of ropivacaine has hardly been proved in randomised clinical trials[14;15], animal studies give evidence of its preponderance with respect to cardiac stability[16;17].

If there are still any further objections to this paragraph, - please let us know.

11.6. Why lipophilic opioids are used widely only in cesarean delivery patients is not discussed while it is likely that the use of such combinations would be useful for spinal or epidural in any other surgical procedure. A better knowledge on the barriers to the change in practice would have been interesting.
We fully agree with this point! However, as many other excellent remarks, this issue needs to be a questions of a further survey, that may have the focus to elucidate why a specific technique or drug (combination) is used.

11.7. The paragraph on UK practices (Page 11) is too long and has no real relevance to the present study. Moreover, the almost unique use of fentanyl is simply related to the fact that sufentanil is not commercially available in the UK.

Thanks a lot for his remark. Main parts of this passage have been deleted.

The original passage:

... has been replaced by:

... has been replaced by:

11.8. Regarding cesarean delivery patients, it is also unclear as to how many patients receive a spinal anesthetic and how many receive an epidural.

It was not a goal of the present study to determine the ratio of spontaneous delivery, caesarean section in spinal or epidural anesthesia or the percentage of emergency cesarean sections under general anesthesia.

We are convinced that most of the remarks have improved the paper. Unfortunately, regarding many other issues we can only hope that subsequent surveys will clarify the open questions. However, if there remain any questions or remarks that need to be answered we are willing to comment on it or revise it immediately.

Looking forward to hearing from you!

Yours sincerely

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